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From labor to cash flow? The abolition of immigration restrictions and the performance of Swiss firms

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KOF Swiss Economic Institute

**From Labor to Cash Flow?
The Abolition of Immigration Restrictions
and the Performance of Swiss Firms**

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From Labor to Cash Flow?

The Abolition of Immigration Restrictions and the Performance of Swiss Firms

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December 2016

Abstract

What is the effect of opening the labor market to foreign workers on the success of firms? We address this question by analyzing how firms in Switzerland were affected by the introduction of the free movement of persons with the European Union (EU) countries. This immigration reform granted all EU workers free access to the Swiss labor market. Our firm-level panel data models exploit the exceptional facts that the reform incidentally affected firms at different time periods and had a stronger effect on firms close to the border. We find that the reform increased employment, skill intensity, and sales of incumbent firms, especially for those that relied heavily on foreign workers and had reported that they suffered from skill shortages before the reform. In these firms, the reform also increased labor productivity. We explain these effects through the higher innovation performance of incumbent firms and the reallocation of economic activity into highly affected regions, as evidenced by the entry of new establishments and by the changes in establishment size within multi-establishment firms.

JEL Classification: J22, J61, F22

Keywords: Skilled immigration, immigration policy, free movement of persons, immigration restrictions, firm performance, labor mobility, firm relocation

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1 Introduction

Firms usually welcome unrestricted access to foreign workers. In fact, statements of business leaders often suggest that opening borders has substantial positive consequences on firms' performance.¹ Our knowledge on whether these claims are true, however, is surprisingly limited. The available evidence is restricted to studies that analyze the effects of policy variation in the H-1B visa program on US firms (Doran et al., 2015; Ghosh et al., 2014; Kerr and Lincoln, 2010; Kerr et al., 2015; Peri et al., 2015a,b). To the best of our knowledge, no further firm-level studies directly link immigration policies to firms' success. Indeed, despite the fact that firms arguably play a central role in determining the migration of skilled workers, "there is very little tradition for considering firms in analyses of immigration" more generally (Kerr et al., 2015, p. S148).

This study attempts to extend our knowledge on whether firms profit from open borders. It investigates the firm-level impacts of a very comprehensive reform: the gradual but eventually complete abolition 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 is the cornerstone of the EU's migration policy and allows its citizens to move freely within the territory of member states for the purpose of employment. Because the principle entails that member states do not legally discriminate between residents and EU citizens, Switzerland gradually removed all its prior legal barriers to immigration for EU workers in the course of its introduction from 1999 onward. The country experienced an extraordinary strong inflow of EU workers in the years following the reform.²

The exceptional feature of Switzerland's implementation of the free movement of persons was that the reform affected regions close to the border earlier and more heavily. This is because the reform not only gradually removed all prior barriers to migration for actual immigrants from EU/European Free Trade Association (EFTA) countries, but also all restrictions on employing *cross-border workers* (CBW). CBW are employed in Switzerland but live in neighboring countries, commuting across the border for work. CBW were already of sizable importance in Swiss labor markets close to the border of neighboring countries prior

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

²In the period between 2002 and 2014, total yearly net immigration was on average 0.8% and gross permanent immigration 1.7% of the permanent population. Around two thirds of all immigrants in the 2002–2014 period migrated to Switzerland for work reasons. Many of them were EU citizens. In the five years following the reform (2002–2007), the share of EU immigrants in gross immigration to Switzerland increased from less than 50% to more than 66%.

to the reform. In certain local labor markets, they represented more than one fourth of the total workforce. However, there were several administrative hurdles to employing CBW. Moreover, employment of CBW was limited to a clearly defined set of municipalities close to the border—the so-called “border region.”

The working restrictions for CBW were abolished gradually from 1999 onward in the course of the reform. In 2004, firms in the border region gained free access to CBW. The liberalizations increased the presence of CBW in Switzerland substantially in the years following the reform. However, they almost exclusively affected firms close to the border while leaving similar firms in regions further away unaffected. One reason is that employing CBW remained restricted to the border region until 2007. Another reason is that there are limits to the geographical distance that CBW are willing to commute. As a consequence, even many firms within the border region did not profit from the greater access to CBW because they were located too far away from the border.

Our identification strategy exploits this exceptional set-up. Using straightforward Differences-in-Differences (DiD) and event study regressions, we compare changes in outcomes for firms close to the border with changes in outcomes for firms outside the border region—not simultaneously affected by the reform—and with firms within the border region but further away from the border. We study the causal effect of the reform on the employment, sales, skill intensity, productivity, innovation, and outsourcing decisions of incumbent firms. The empirical analysis is based on 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. The use of panel data allows us to account for pre-existing differences across firms and regions and to distinguish the effects on incumbents’ establishments from effects driven by changes in the composition of establishments. This proves to be important because the reform increased the entry of establishments.

Our study belongs to very recent literature that exploits changes in policies for cross-border commuters in order to study the effects of immigration. [Dustmann et al. \(2015\)](#) analyze the consequences of a policy change that allowed Czech workers to search for jobs in German border municipalities without giving them residence rights. They show that the reform triggered a large increase in the share of CBW in the German border regions and led to a substantial displacement of resident workers in the short run. Contrasting results are presented in a paper concurrently written with ours by [Beerli and Peri \(2016\)](#). Applying a similar DiD identification strategy as we do, they show that introducing the free movement of persons in Switzerland increased the share of foreign workers in total employment

regions close to the border.³ Their results also suggest a limited impact of the immigrant inflow on the average wages and employment of residents, but positive effects on the wages of highly educated residents. An attractive feature of studying the effects of immigration using changes in commuting policies is that the increase in the aggregate number of immigrants and its unequal regional impact are a direct consequence of the exogenous change in immigration policy. This is in contrast to studies applying the traditional shift-share instrumental variable to isolate supply-driven variation in regional immigration flows. The instrument apportions national changes in the number of immigrants (the *shifts*) to regional labor markets using historical settlement patterns of immigrants across regions (Altonji and Card, 1991; Card, 2001). The validity of the approach hinges on the assumption that historical immigrant *shares* are uncorrelated to the current regional distribution of labor demand shocks.

Our study differs from those of Beerli and Peri (2016) and Dustmann et al. (2015) because of its focus on firms: while the two papers concentrate on the potential losers from changes in commuting policies, we focus on the expected winners. Moreover, the case studied here and the setting examined by Dustmann et al. (2015) are very different. In the German situation, the Czech workers were primarily unskilled. In contrast, Switzerland attracted a sizeable number of high-skilled workers in the period under consideration.⁴ In fact, the increased supply of skilled workers met firms' increasing demand for them. Before the reform, Swiss firms increasingly reported that they struggled to find suitable skilled workers.⁵ Our study is thus of direct policy relevance: it informs policy makers about the potential economic benefits of the principle of the free movement of persons in a situation where skilled workers are scarce and the reservoir of resident workers is close to exhausted. This is relevant not only because the working age population will begin to shrink as the baby boomer generation exits the labor markets in many developed countries, but is also

³The idea to exploit the changes in Switzerland's commuting policies in order to study the effects of immigration on the employment and wages of resident workers is advanced in two policy reports by Henneberger and Ziegler (2011) and Losa et al. (2014). In an unpublished paper, Bigotta (2015) examines how the abolition of the priority requirement in the course of the reform affected the duration of residents' unemployment.

⁴Between June 2002 and May 2008, 48% of all immigrants had a tertiary education. This share increased to 54% for immigrants in the period 2008–2014 (EU/EFTA immigrants: 57%). The share of tertiary educated immigrants clearly exceeded the corresponding share in same-aged cohorts of the resident population. See Beerli and Peri (2016) and Siegenthaler et al. (2016) for a discussion of the effects of the reform on the skill composition of immigrants.

⁵In 2001, the year prior to the reform, Switzerland's unemployment rate was only 2.5%. In the same year, the labor force participation rate of the population aged 15–64 was 81.2%—the second highest value among all Organization for Economic Cooperation and Development (OECD) countries. In a survey conducted in 2001 by the Federal Statistical Office, 36% of all firms reported substantial problems in finding qualified workers. Section C in the appendix provides an extended discussion of the labor market situation in Switzerland at the time of the migration reform.

pertinent against the background of mounting political and public opposition against the principle of the free movement of persons in several European countries in recent years. This opposition culminated in Great Britain’s decision to leave the EU.⁶

Our study contributes to the growing body of research examining the impacts of immigration on productivity, innovation, and production technology in the receiving country.⁷ Firm-level studies examining these links are still rather rare and almost all existing studies focus on the US.⁸ Ghosh et al. (2014), Kerr and Lincoln (2010), and Kerr et al. (2015) quantify the influence of changes in the number of admitted H-1B workers in the US on firms’ employment structures, size, inventions, productivity, and profits. These studies suggest that greater access to H-1B workers generally increases firms’ size, productivity, and innovation performance—effects that are strongly concentrated in firms that rely heavily on H-1B visas. Studies using more aggregate data⁹ or that focus on inventors (Moser et al., 2014) also tend to find positive impacts of skilled immigration on productivity and innovation. A recent paper by Doran et al. (2015) questions these conclusions, however. The authors find that winning an additional H-1B worker in the H-1B lotteries of 2006 and 2007 had no effects on firms’ patenting and size but crowded out resident workers, lowered the average earnings per employee, and increased firms’ profits.

Our results support the positive impact of opening borders on firms’ performance. We show that the reform triggered an excess inflow of foreign workers in local labor markets close to the Swiss border. This inflow increased the employment, skill intensity, and sales of incumbent firms. The magnitudes of these effects are economically relevant. In terms of full-time equivalent (FTE) employment, heavily exposed establishments grew 3.5–6.5% more than non-exposed establishments. Similar to Beerli and Peri (2016), we find limited evidence that natives were displaced. The size effect of the reform was particularly pronounced in firms that relied heavily on foreign workers and had reported that they were constrained by a lack of specialized personnel prior to the reform. These firms also experienced a substantial increase in value added per FTE worker. We show that the size effect of the reform is partly due to firms’ higher innovation efforts (i.e., an increase in R&D employment and the number of patent applications). We also find that the reform led to the reallocation of economic activity into the heavily affected regions. In particular, the reform resulted in the entry of

⁶In Switzerland, most voters accepted a referendum in 2014—the so-called stop mass migration initiative—which intends to restrict the free movement of persons.

⁷Kerr et al. (2015) and Lewis (2013) provide recent overviews of this literature.

⁸An exception is Paserman (2013), who examines how the surge in immigration from the former Soviet Union affected Israeli manufacturing firms in the 1990s.

⁹Hunt and Gauthier-Loiselle (2010), Ortega and Peri (2014), Ottaviano and Peri (2006), and Peri et al. (2015a,b) analyze the impact of (skilled) immigration on productivity and innovation using panel data of US cities, US metropolitan areas, US states and countries.

new establishments in these regions and multi-establishment firms decided to increase the size of heavily exposed establishments relative to less exposed establishments. Generally, our findings highlight the importance of the free movement of persons for firms' performance in a labor market characterized by shortages of skilled workers.

2 Immigration restrictions and firms' performance

This section uses a simple decomposition of a production function to motivate our empirical analysis on the effects of immigration restrictions on firms' performance. Note that the focus on the firm side disregards the role of immigrants as consumers. This seems appropriate, however, when analyzing changes in commuter policies. The reason is that CBW do not relocate to the country they work in and thus have a limited impact on local product or service demand. As pointed out by [Dustmann et al. \(2015\)](#), this is in fact attractive because it allows a more direct test of the theoretical models commonly used to study the impacts of immigration. These models generally do not consider the role of immigrants as consumers.

Consider the following Cobb-Douglas production function that is similar to those discussed by [Combes and Gobillon \(2015\)](#) and [Peri \(2012\)](#):

$$Y_{j,t} = A_{j,t}(s_{j,t}L(I_{j,t}, R_{j,t}))^\alpha K_{j,t}^{1-\alpha} \quad (1)$$

where $Y_{j,t}$ denotes firm j 's output in period t ; $0 < \alpha < 1$ is a parameter; $L(I_{j,t}, R_{j,t})$ is a labor composite aggregating firms' employment of immigrants, $I_{j,t}$, and resident workers, $R_{j,t}$; $s_{j,t}$ represents the average skills of the workers; and $K_{j,t}$ represents all other inputs that the firm uses in production (i.e., capital, intermediate inputs, and land). $A_{j,t}$ is the technological level of the firm. By rearranging terms, taking logs, and multiplying both sides with $p_{j,t}$, which is the average income of the firm per unit produced, we obtain:

$$\ln(p_{j,t}Y_{j,t}) = \alpha \ln L(I_{j,t}, R_{j,t}) + \ln(1 - \alpha)K_{j,t} + \ln s_{j,t}^\alpha p_{j,t} A_{j,t}. \quad (2)$$

Expression (2) shows that total output increases if a firm's total employment, use of other inputs, skill intensity, or technological level increases. It is conceivable that a reduction in immigration restrictions affects firm size through each of these components.

First, abolishing immigration restrictions can affect total output by increasing immigration, $I_{j,t}$, and, consequently, total labor input, $L(I_{j,t}, R_{j,t})$, if immigrants and resident workers are imperfect substitutes ([Ottaviano and Peri, 2012](#)). If immigrants and residents

are perfect substitutes, immigrants crowd out residents one-to-one and we observe no effect on total labor input and firm size through this channel.

Second, abolishing immigration restrictions may influence firms' use of $K_{j,t}$. On the one hand, immigrants could promote foreign direct investment (FDI) and imports of goods and services directly by reducing the costs of trade flows to and from their home country (see [Ottaviano et al., 2015](#), and the literature cited therein). Immigrant inflows may reduce firms' use of land by increasing house prices. More generally, reducing immigration restrictions may increase the overall attractiveness of a country's labor market for foreign firms and affect the staffing and offshoring decisions of domestic firms ([Kerr et al., 2015](#); [Olney, 2013](#); [Ottaviano et al., 2013](#)). On the other hand, immigrants could affect $K_{j,t}$ indirectly because they may either complement or substitute other production inputs ([Lewis, 2011](#)).¹⁰

Third, abolishing immigration restrictions could affect firm size by altering the skill intensity of the workforce, $s_{j,t}$. Whether a given policy change mainly attracts skilled or unskilled immigrants is, however, an empirical question. Theoretically, it depends on a variety of factors such as the skill-specific wage differentials between the source and destination country, and the change in the skill-group specific migration and hiring costs induced by the reform.

Finally, abolishing migration restrictions may affect the technological level, $A_{j,t}$, of firms in several ways. Most importantly, increased immigration flows may promote or dampen the adoption and diffusion of production technology ([Hornung, 2014](#); [Lewis, 2011](#)), support the exchange of ideas ([Bosetti et al., 2015](#)), stimulate innovation and the patenting activities of firms ([Hunt, 2011](#); [Hunt and Gauthier-Loiselle, 2010](#); [Kerr, 2013](#); [Kerr et al., 2014](#)), and may increase the diversity or the specialization of the workforce ([Alesina and Ferrara, 2005](#); [Parrotta et al., 2014](#); [Peri and Sparber, 2009](#)). More generally, the abolition of immigration restrictions could lead to positive agglomeration effects: regional labor markets could become thicker, the matching between workers and firms could become more efficient, and human capital externalities could be amplified ([Ciccone and Peri, 2006](#); [Moretti, 2011](#)).

Expression (2) is the basis of our empirical analysis of firms' output, employment, and labor productivity. Using a unified empirical framework for these variables is justified because it is straightforward to derive equations analogous to expression (2) for log employment and log value added per worker under the same assumptions about firms' production (see [Combes and Gobillon, 2015](#), for details). Because we estimate reduced form regressions where we

¹⁰Our Cobb-Douglas production function does not capture this indirect effect, however, because $K_{j,t}$ and $I_{j,t}$ are separable in production.

directly relate the outcomes to an indicator of the immigration reform, our regressions capture the net impact of the way in which abolishing immigration restrictions affected Swiss firms.

3 The immigration reform

The details of Switzerland’s introduction of the free movement of persons were specified in the Agreement on the Free Movement of Persons (AFMP) reached with the EU and EFTA member states in 1999. Introducing the free movement of persons was not an aim of the Swiss government owing to the anticipated political opposition against opening the borders. Rather, the AFMP was a political concession to the EU.¹¹ The AFMP detailed a step-wise introduction of the free movement of persons. Table 1 provides a simplified illustration of the reform. The table distinguishes three reform phases and two broad types of foreign workers: permanent resident immigrants and CBW. The shading of the table highlights the restrictiveness of the regulations for the respective category of workers.

The relevant details of the reform were announced in Switzerland during 1998. It is conceivable that the reform influenced the behavior of firms from this moment onward. Anticipatory effects prior to this moment appear very unlikely.¹² In fact, it remained quite uncertain even in 1998 whether and when the reform would actually take place. After being approved by the Swiss parliament and signed in 1999, the treaty also required the approval of the European parliament and of each EU member state. It also had to pass a referendum vote by Swiss voters (which took place in May 2000). The AFMP was enacted in June 2002—one-and-a-half years later than planned at the time of the initial announcement.

The AFMP gradually removed all prior legal restrictions on hiring and employing resident immigrants and CBW. The last column of the table shows that the residence status of resident immigrants was changed in three steps. The last step was the abolition of annual quotas for this category of immigrants. Likewise, many prior barriers on hiring and employing CBW were lifted between 1999 and 2004. A very important existing restriction, however, remained in place until 2007: employment of CBW remained restricted to the municipalities in the *border region* (BR). Figure 1 illustrates the geographical split of Switzerland into

¹¹The AFMP was part of a package of agreements negotiated at the same time. The EU saw the free movement of persons as a prerequisite in order to negotiate the six other agreements. The other agreements pertained to harmonizations in specialized fields such as air and land traffic, agriculture, research cooperation, granting firms better access to the common market, and reducing non-tariff barriers to trade.

¹²One reason is that the success of the bilateral negotiations was still uncertain prior to a breakthrough in the negotiations mid-1998. In 1997 and early 1998, several members of the Swiss parliament expressed their concerns that the negotiations could fail and asked the government about the alternatives if this should happen.

Table 1: The three phases of the immigration reform

Phase	Year	Event	CBW		Immigrants <i>Both regions</i>
			<i>Border region</i>	<i>Central region</i>	
<i>Pre-reform</i>	1995		Admission process, further restrictions	No access	Admission process, annual quotas
	1996				
	1997				
<i>Phase 1</i>	1998	Announcement	Anticipatory effects possible		
	1999	AFMP signed			
	2000	Referendum			
	2001				
<i>Phase 2a</i>	2002	AFMP enacted	Abolition of further restrictions		Higher annual quotas and further changes ¹
	2003				
<i>Phase 2b</i>	2004		<i>Free</i>		Abolition of admission process
	2005				
	2006				
<i>Phase 3</i>	2007	Full liberalization		<i>Free</i>	<i>Free</i>
	2008				
	...				

Notes: ¹ Extension of durations of different residency permits.
Allowance of family reunion for most permit categories.

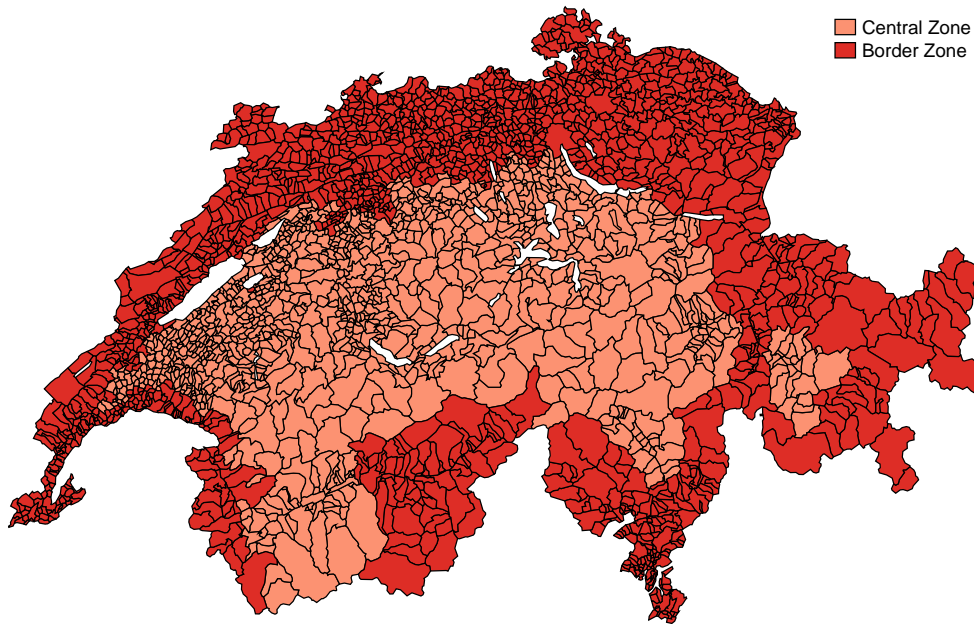
the BR and the rest of Switzerland, termed the central region (CR) henceforth.¹³ The BR had been defined in bilateral agreements between Switzerland and its neighboring countries signed between 1928 and 1973.¹⁴ The frontier between the CR and BR remained unchanged in the course of the reform. Moreover, it is specific to these contracts. It does not follow cultural or religious borders; nor does it follow cantonal or any other political borders other than municipal borders. The fact that the geographical restriction on employing CBW remained in place implies that the liberalizations in the working status of CBW between 1999 and 2004 only affected firms in the BR.

The liberalizations in the BR occurred in three steps (in 1999, 2002, and 2004). In the first phase after 1999, cantonal offices, which were responsible for handling applications for CBW, gained more discretion for doing so. According to Beerli and Peri (2016), certain cantonal offices exploited this to handle CBW applications in a more relaxed manner. Step 2a of the reform comprised the abolition of different administrative restrictions that had previously been maintained. From 2002 onward, CBW were no longer required to reside in the adjacent BR of the neighboring country for at least six months. New cross-border permits were also generally valid for five years now and were no longer bound to a specific job. Before 2002, cross-border permits ended with the termination of a work contract and

¹³See Section B in the appendix for more information of how municipalities were assigned to the BR and CR.

¹⁴The 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.

Figure 1: Municipalities in the border and central region in 2007

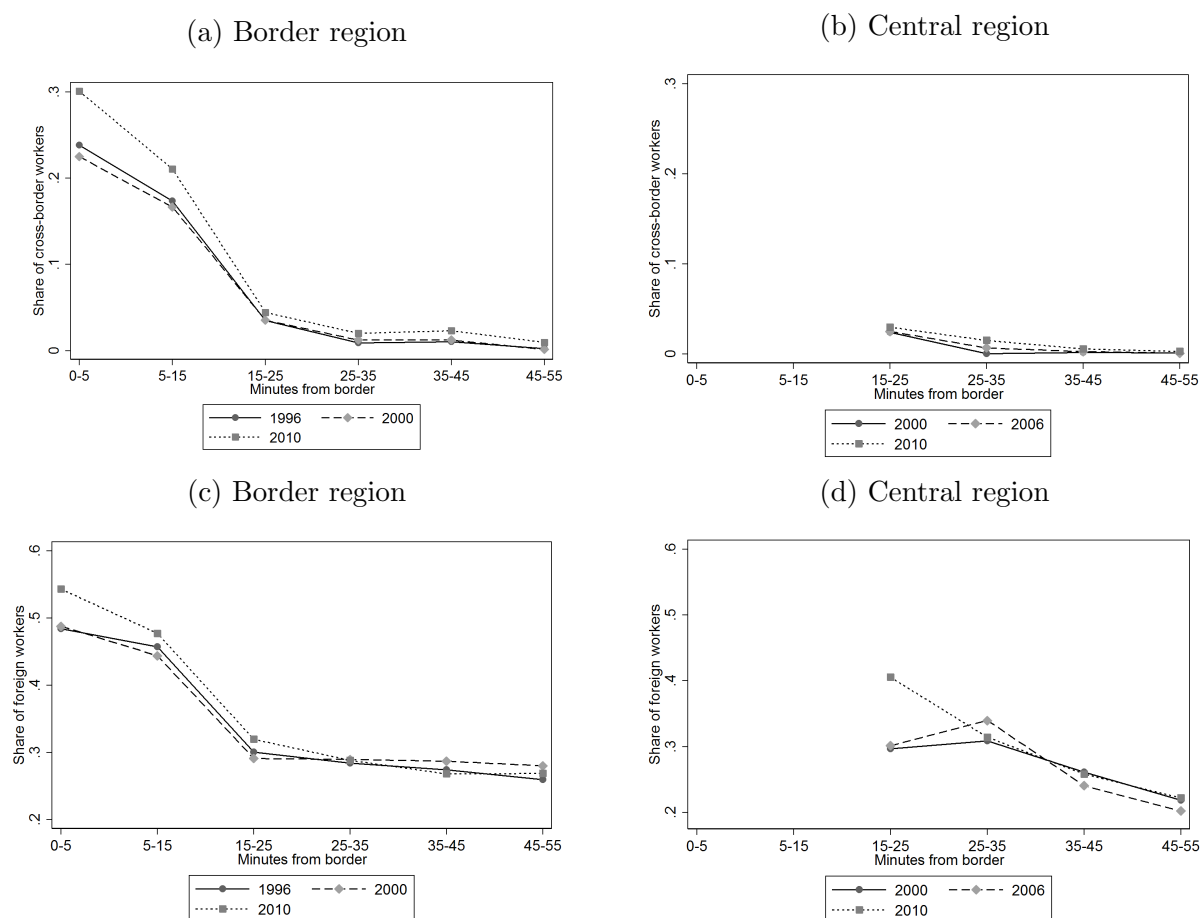


were formally limited to one year. Another liberalization in 2002 was that CBW were only required to commute to their place of residence weekly rather than daily as before. As a consequence, they were also granted the right to search for housing in Switzerland. Finally, from 2004 onward (phase 2b), Swiss firms no longer 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 bureaucratic admission process for CBW. It also created indirect recruitment costs because employing CBW was subject to a time delay during which firms did not know whether they would be able to fill their vacancies with the desired workers.¹⁵ With the abolition of the bureaucratic admission process in June 2004, hiring CBW became equivalent to hiring resident workers in the BR.

The number of CBW in the BR increased substantially in the years after the immigration reform. In the BR overall, the total number grew from 117,504 to 194,032 FTEs from 2000 to 2010. Many of the new CBW after 2000 were well qualified. In 2000, only 15% of all CBW

¹⁵When hiring CBW, firms were required to prepare an application detailing the job requirements of their vacancies and the working and contract conditions offered. Firms also had to demonstrate their search efforts within Switzerland. In practice, they had to have searched for a worker within Switzerland for a certain number of weeks without success before being allowed to apply for CBW. After this waiting period, the application had to be sent to the cantonal migration office. The processing of the application involved different governmental bodies and lasted one–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.

Figure 2: Share of cross-border workers and foreign workers in total employment in the border and central region, 1996–2010



Source: Wage structure surveys, various years. Firms that do not belong to the private sector are excluded from the sample. We exclude individuals where we do not know the residence status or occupation. Subfigures (c) and (d) pool CBW and permanent immigrants. The latter are foreign nationals with residency permits B, L, or C, granting one–five years of residence (L and B) or unlimited residence (C) in Switzerland.

in the BR had a tertiary degree. This share increased to 28% in 2010 and for the first time exceeded the corresponding share among Swiss nationals (see Table A.1 in the appendix for details). The number of CBW grew in many different occupations, but was very pronounced in “manufacturing and processing,” “machinery,” “logistic and staff duties,” and “research and development.”¹⁶

Another important characteristic of the increase in the concentration of foreign workers was that it was strongly concentrated in labor markets close to the border, as Figure 2 illustrates using data from the Swiss wage structure surveys. The figure plots the shares of CBW and foreign workers in total FTE employment, depending upon an establishment’s

¹⁶See Table A.3 in the appendix for a detailed overview.

commuting distance to the border. The figure also plots the shares separately for firms in the BR and CR.¹⁷ Subfigure 2a shows that the employment share of CBW increases from 18.9 to 24.2% in regions within 15 minutes travel duration to the border in the BR between 2000 and 2010. In this period, FTE employment of CBW grew from 98,000 to 160,000 in this region (see Table A.2). The change in the employment share of CBW between 2000 and 2010 becomes smaller, the further away from the border we go. It becomes close to 0 around 30 minutes away from the border. We observe the same qualitative pattern if we focus on the increase in the share of foreign workers (i.e., CBW plus permanent immigrants) in total FTE employment (subfigure 2c).

The strong spatial decay of the change in the concentration of foreign workers implies that the extent to which we can exploit phase 3 of the reform has limits. The reason is that most firms in the CR are not located close enough to the border to profit from the switch from no to free access to CBW that occurred in 2007. Figure 3 illustrates this by plotting the density of establishments in the BR and CR depending upon the commuting distance to the border. Further, the few firms located within, say, 35 minutes to the border experienced a small increase in the share of CBW in total employment (see Figure 2b). These factors explain why our empirical analysis mostly pools the reform effects for firms located 15–30 minutes away from the border in the BR and CR because it proved difficult to estimate separate effects for the two groups. Moreover, we primarily focus on how firms in the BR very close to the border were affected by the loosening of prior restrictions on CBW during phases 1 and 2 of the reform. Consequently, the main empirical advantage of the split into the BR and CR is that it delivers two natural control groups for these firms: firms in the CR and firms in the BR located too far away from the border to profit from the easier access to CBW.

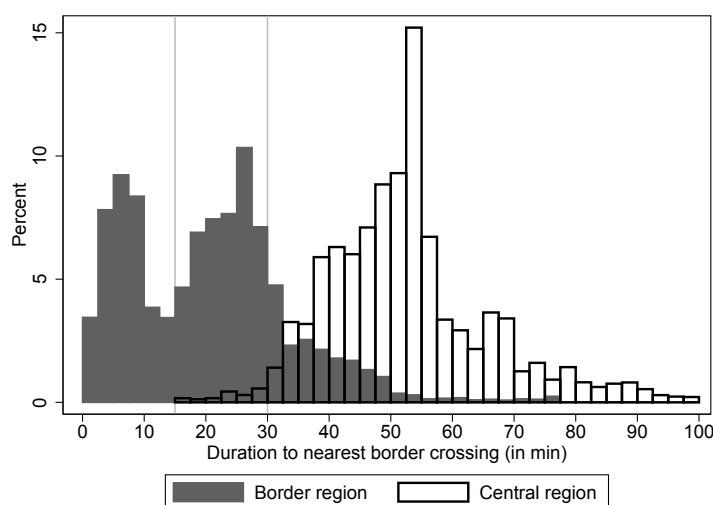
It is important to note that the AFMP also lifted all restrictions for Swiss residents to work as a CBW in neighboring countries. However, the growth in employment of CBW in the Swiss border region was about nine times larger than the growth of CBW residing in Switzerland who work in neighboring countries.¹⁸ This asymmetry reflects that nominal wages and the cost of living are much higher in Switzerland, making it very unattractive to live in Switzerland while working abroad.¹⁹ We thus interpret our results as reflecting

¹⁷Beerli and Peri (2016) present a very similar illustration.

¹⁸Table A.4 in the online appendix provides data on CBW on both sides of the border gathered by the Federal Statistical Office. 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). According to this data source, there were approximately 100,000 additional CBW working in Switzerland but living in neighboring countries in the same period.

¹⁹Data 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

Figure 3: Share of firms in the border and central region by time to nearest border crossing



Notes: The graph uses data from the 1998 BC. A small number of establishments located more than 100 minutes away from a border crossing were excluded.

the effects of immigration on the Swiss economy rather than as the effects of a larger labor market in the regions close to either side of the border.

4 Data and empirical strategy

4.1 Data

Our empirical analysis is based on three data sets. The first data source is seven waves of the Swiss Business Censuses (BC) conducted in 1991, 1995, 1998, 2001, 2005, 2008, and 2011. The BC cover the universe of private and public establishments in Switzerland. Approximately 4 million employed persons in approximately 389,000 workplaces are included in the census of 2008. The data are available as a panel data set and provide us, among other information, with the size (FTE employment) and the exact geographical location (geographic coordinates) of all establishments in Switzerland. The data are very reliable. Until 2008, the BC were used to update the Business and Enterprise Register of the Swiss Statistical Office and were based on mandatory surveys. Since 2011, the BC are based on register data. Many variables available for the earlier waves are no longer available because of this change. As a consequence, we had to update certain firm characteristics in 2011 using

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.

data from the same establishments in 2008. Our 2011 data thus contain only establishments that were already present in 2008. We exclude the agricultural sector from the analysis, mainly because it is not covered in all census waves.

The second data source used in the analysis 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 (1996, 1999, 2002, 2005, 2008, 2011, and 2013). The quantitative variables refer to the year *before* each survey took place. All surveys are based on a representative sample of privately owned firms with at least five FTE employees. The surveys cover the manufacturing, construction, and business services sectors in Switzerland and are disproportionately stratified with respect to firm size and two-digit industry affiliation.²⁰ The main advantage of the IS relative to the BC is that they provide very detailed information on the characteristics of the surveyed firms and a rich set of additional outcomes: FTE employment by the highest educational attainment of workers, sales, value added, product and process innovations and the number of patent applications filed. The main disadvantages are related to the fact that the surveys are voluntary: the data are potentially subject to reporting and measurement errors, there is attrition and non-response, and only 60% of the firms are available in more than one survey.

We complement these firm-level data using data from the Swiss wage structure surveys. The surveys 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 FTE workers from the manufacturing and service sectors in Switzerland. They cover between 16.6% (1996) and 50% (2010) of total employment in Switzerland. Participation is mandatory. The surveys contain extensive information on the individual characteristics of workers and their wages. They enable the disentanglement of CBW, resident immigrants and native workers and contain a firm identifier. The main disadvantages of the data set—which explains why we do not use it for our main empirical analysis—are that it contains limited information on firms and does not enable us to track firms over time.

4.2 Regression models

Our preferred empirical model exploits two elements of the immigration reform. First, firms in the CR could not resort to CBW until 2007, while firms in the BR gained easier access to CBW between 1999 and 2002 and full access in 2004. Second, the intensity of a firm’s exposure to the reform depends upon its distance to the nearest border crossing because of

²⁰The raw data contain answers for 1989, 2172, 2586, 2555, 2141, 2363, and 2034 firms for the surveys’ seven years respectively, representing an average response rate of 35%. The questionnaires can be downloaded from www.kof.ethz.ch/en/surveys.

limits to the commuting distance of CBW. Based on these considerations, we employ the following DiD model for a generic outcome, $Y_{i,m,t}$, of firm i in period t located in municipality m , belonging either to the BR or CR:²¹

$$Y_{i,m,t} = \beta_0 + \beta_{1,1}R_{m,t} + \beta_{1,2}R_{m,t} * D\{15 < d_{it} \leq 30\} + \beta_{1,3}R_{m,t} * D\{0 < d_{it} \leq 15\} + \theta r + \tau_t + \gamma_{i,m} + \epsilon_{i,m,t} \quad (3)$$

In the above model, $R_{m,t}$ represents the reform indicator that captures the differences between the BR and CR in the timing of the immigration reform. It reflects the timing of the legal changes caused by the reform, as shown in Table 2.²² d_{it} represents the travel duration (in minutes) between firm i 's location in period t and the nearest border crossing to one of Switzerland's neighboring countries. We construct d_{it} 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.²³ Further, the model contains a set of region dummies, θr ²⁴, and period fixed effects, τ_t , capturing all aggregate macroeconomic shocks common to all firms, such as changes in aggregate prices, foreign demand, and fiscal policy. Our preferred specification also accounts for firm fixed effects, $\gamma_{i,m}$ (discussed below). Finally, $\epsilon_{i,m,t}$ is the usual idiosyncratic error term that we assume to be strictly exogenous with respect to the covariates.

The central idea of our regression framework is to compare changes in outcomes in firms close to the border with changes in outcomes in firms in the other region that are not simultaneously affected by the reform, and firms within the same region that are “too far away” from the border to profit from greater access to CBW. We implement this idea econometrically by interacting the reform indicator, $R_{m,t}$, with two dummy variables that are 1 if the firm is located within a specific travel period to the nearest border crossing.

²¹In the BC, establishments are assigned to the BR and CR based on the municipalities where they are located. In the case of the KOF innovation data, we assign them depending upon the address the survey was sent to. In both data sets, we exclude a very small number of firms located in municipalities where we could not establish whether they belonged to the BR or CR.

²²In the IS, $R_{m,t}$ becomes 1 in the 2005 survey for firms in the BR and in the 2011 survey for firms located in the CR. The BC data refer to September 2001, 2005, and 2008. We thus set $R_{m,t}$ to 0 in 2008 in the CR and to 1 in 2011. The alternative, setting $R_{m,t}$ to 1 in 2008, yields qualitatively similar results; however, they are generally less precise. The alternative is also not supported by the analysis of the timing of CBW inflows into the CR.

²³The data on the location of border crossings in Switzerland necessary to construct d_{it} come from Henneberger and Ziegler (2011). Figure A.1 in the appendix, taken from their study, shows the locations of the border crossings. The data refer to the year 2010.

²⁴The regional dummies are constructed such that they absorb a fixed effect (FE) for each region defined by the interaction between the distance dummies and the BR/CR split. They are also included if we control for firm fixed effects because a small number of firms changes location.

Table 2: Specification of the reform indicator

Survey wave	IS	BC	$R_{m,t}^{BR}$	$R_{m,t}^{CR}$	$\mathbf{R}_{m,t}$		$R_{m,t}^{Event}$
	Year quantitative data refers to				BR	CR	
.	.	1991	0	0	0	0	0
1996	1995	1995	0	0	0	0	0
1999	1998	1998	0	0	0	0	0
2002	2001	2001	0	0	0	0	0
2005	2004	2005	1	0	1	0	1
2008	2007	2008	1	(0)	1	(0)	0
2011	2010	2011	1	1	1	1	0
2013	2012	.	1	1	1	1	0

As illustrated by Figure 3, these interactions partition the firm population into five groups of firms: three groups of firms in the BR, one of which is “highly” (0–15 minutes), one “slightly” (15–30 minutes), and one which is unlikely (more than 30 minutes) to be treated by the reforms between 1999 and 2004; and with two groups of firms in the CR (15–30 minutes and more than 30 minutes) with differing exposure to the liberalizations in 2007. We evaluate the effects of the reform for “highly” treated firms by comparing the evolution of outcomes of this group relative to firms in the CR (in which case the sum of $\beta_{1,1}$ and $\beta_{1,3}$ is the relevant treatment effect) with firms in the BR more than 30 minutes away from the border ($\beta_{1,3}$).

4.3 Identification strategy

Because most of our regressions contain firm fixed effects, our central identifying assumption when estimating equation 3 is that we would observe the same average within-firm change in each outcome in the five regions without the reform. As always in a DiD, this “common trend” assumption cannot be tested. However, we will assess the plausibility of the assumption in several ways. Most importantly, we will generalize the above DiD model to an event study to examine whether the effect is not present before the reform that was alleged to have caused it.

Because our preferred regression models account for firm fixed effects, $\gamma_{i,m}$, we control for pre-existing differences between firms and the regions they are located in. Such differences could have been a direct consequence of the long-established cross-border policy that restricted the hiring of CBW to firms in the BR. Further, the firm fixed effects make treatment and control units more comparable because they condition the results on the initial differences in the outcome variables across firms (such as size in the first period). In fact, however, the pre-reform differences in firms’ characteristics across regions are relatively

Table 3: Average firm characteristics in the relevant regions prior to the reform (survey waves 1996 and 1999)

Variable / duration (in min.)	Border region			Central region	Total
	≤ 15	15–30	> 30		
Firm age	45.2	45.3	46.4	51.3	47.1
Firms with foreign owners (%)	0.12	0.14	0.09	0.09	0.12
Manufacturers (%)	0.49	0.48	0.55	0.52	0.50
Firms with R&D expendit. (%)	0.43	0.51	0.48	0.42	0.46
Exporters (%)	0.54	0.51	0.49	0.43	0.49
FTE employment (ln)	3.89	4.05	3.85	3.82	3.92
High-skilled employees (%)	0.18	0.19	0.17	0.15	0.17
Wage/FTE employee (ln)	11.12	11.21	11.13	11.13	11.16
Labor productivity (ln)	11.68	11.82	11.74	11.73	11.75
Sales (ln)	16.15	16.50	16.13	16.10	16.25
Firm-year observations	947	1,431	621	1,130	4,129

Note: Entries represent averages per region of all firm-year observations in the estimation sample 1996–1999.

small. Using data from the IS, Table 3 shows that the regions are, for example, comparable in terms of average firm age, the share of firms in the manufacturing sector, the average export share in sales, the share of firms with nonzero R&D expenditures, and the share of employees with a tertiary degree. The regions are also comparable in terms of the average demographic and worker characteristics.²⁵

The main threats to a causal interpretation of our estimates are unobserved third factors (i) that are correlated with $R_{m,t}$ and (ii) that have an uneven impact on the within-firm change in $Y_{i,m,t}$ across the five regions of interest. Candidate confounding factors are unobserved industry-wide shocks to prices, demand, or productivity that may arise because of (pre-existing) regional differences in the sectoral composition or the input mix. In section 6.1, we provide extensive evidence that such unobserved factors do not confound our results.

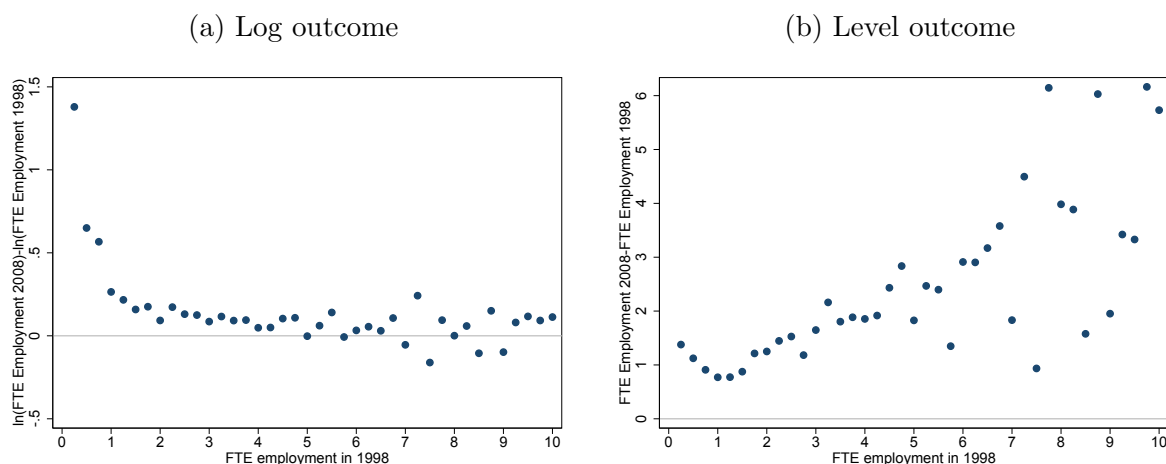
Our firm-level sample of most interest is that of private-sector establishments with at least five FTE workers. This is what we term the “preferred estimation sample” below. It is the firm population sampled in the IS. It is also our preferred sample in the BC. An important reason that we prefer this sample is that only few public firms and few small firms employed CBW prior to the reform.²⁶ Hence, we do not expect that these firms are strongly affected

²⁵See Table 2 in Beerli and Peri (2016).

²⁶The BC in 1995 provides data on the employment of CBW in each establishment. When restricting the sample to the relevant regions (i.e., establishments that are located within the BR and within 30 minutes to the border), the share of public sector firms employing at least one CBW is 4%. The corresponding figure is 13% in the private sector. Similarly, the average share of CBW in total FTE employment is 2.4% in firms with less than five FTEs. This share rises to 6% among firms with 5–50 FTEs and is 8.9% among firms with more than 50 FTEs. Only 5.4% of all microfirms employ CBW in 1995. This share is 57.2% among large firms.

by the reform. Another reason relates to the interval in which very small establishments can grow. Consider the extreme example of a new firm that is established with one worker. There are no limits for this firm’s growth, but it cannot shrink unless it goes out of business. The consequence is readily apparent in Figure 4a which relates FTE employment in 1998 to growth in FTE employment in the 1998–2008 period for very small establishments newly entering the BC in 1998, averaging all observations within an interval of 0.25 FTE workers. The binned scatterplot shows the mechanical negative correlation between the initial size and subsequent growth for establishments that initially have fewer than five FTE worker (see Mata, 1994). This negative correlation matters in our context because the firm size distributions are not identical in the five regions for all samples used in the estimation. Estimations that include the large number of micro-establishments present in the BC are thus estimated using FTE employment in levels because the asymmetry between initial size and subsequent change is less pronounced when using employment levels (see Figure 4b).

Figure 4: Relationship between establishment size in 1998 and growth 1998–2008 for establishments founded in 1998



In both firm-level data sets, we discard a very small number of extreme outliers that have a strong leverage on the estimated coefficients and the precision of the estimates.²⁷ Moreover, in most regressions, we restrict the sample to the cross section of firms existing in 1998. We do this because the reform led to an inflow of new establishments into the heavily affected regions, as discussed in section 5.5.2. The resulting changes in the composition of firms may bias our estimates of the reform effects on *incumbent* firms. To give a concrete example, note that the new entrants are likely to be smaller on average than incumbent

²⁷In the IS, we delete two firms with obviously wrong data on sales and employment in one year. In the BC, we compute deviations from within-firm means in FTE employment and discard 23 establishments with observations that lie above the 99.99% quantile or below the 0.01% quantile of the distribution of this variable.

firms. The entry of firms thus reduces the average firm size in the highly affected regions. As a consequence, a regression that does not account for firm entry is likely to underestimate the effect of the reform on the size of incumbents. One way to deal with this in the presence of panel data is to track the outcomes of a single cross section of firms. We focus on the cross section of firms existing in 1998.²⁸ These firms experience a differentially strong exogenous change in their expected future labor supply in the year when the reform is announced owing to their pre-determined location.²⁹

There are two further empirical issues that are worth discussing. First, the unit of observation in the IS is a firm and not an establishment (as in the BC). Most multi-establishment firms are thus assigned to a treatment or control group based on the location of their headquarters. Because the reform affects an establishment's, and not a firm's, possibility of hiring CBW, the results using the surveys may thus be biased downward because of classical measurement error in the reform indicator. This may also explain why firms with more than 500 FTE employees do not contribute to the precision of our results with the IS. Large firms are much more likely to be multi-establishment firms, increasing the potential for measurement errors for this subgroup.

Second, our regression models evaluate the reform effects using 15 and 30 minutes as duration thresholds. There are two reasons why we prefer the somewhat arbitrary dummy variable specification to alternative specifications (such as interacting $R_{m,t}$ with d_{it}). First, the dummy specification is non-parametric and hence does not impose an *a priori* restriction on the functional form of the reform effects across space. Second and more importantly, the dummy specification can be accompanied by a transparent graphical analysis of the data. The results when using alternative thresholds, or if we use a quadratic for the time to the border, are discussed in section 6.3.

²⁸Table A.15 presents the results for the most important outcomes if we focus on the cross section of 2001. These results are very similar to our baseline results.

²⁹In the BC, the cross section of firms in 1998 is just the firms that are present in 1998. In the IS, two groups of firms are assigned to the cross section of 1998. The first group is firms that are part of the IS in 1999 or the KOF survey on organizational change in 2000. Both surveys are based on a sample of firms drawn from the BC 1998. The second group is firms that answer at least one of the IS prior *and* posterior to the survey in 1999. We additionally delete a small group of firms that fulfill these criteria but were founded after 1998.

5 Main results

5.1 Employment of foreign workers

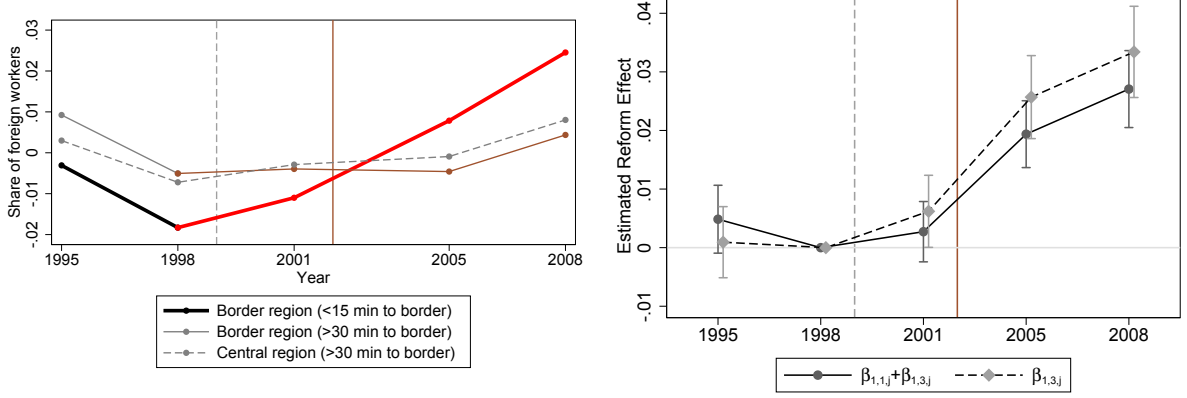
It is a central requirement for our empirical analysis that firms very close to the border were more strongly affected by the immigration reform. Figure 2 provides the first descriptive evidence that supports this idea with data from the wage structure surveys. Using the BC, we now study more formally whether we observe a disproportionate increase in establishments' share of foreign workers (i.e., resident immigrants plus CBW) in total FTE employment close to the border in the period following the announcement of the reform. Results using the count of foreign workers as the outcome are very similar and presented in Appendix E. Because the censuses in 1991 and 2011 do not contain information on the nationality of the workforce, the estimations are run on just five waves (1995–2008). As a result, we can only evaluate the extent to which the second phase of the reform affected immigrant employment. Table 4 presents the regression results. Observations are weighted using an establishment's average size as the weight. Inference is based on standard errors clustered at the establishment level (see Section 6.3 for a discussion). The bottom of the regression table provides the outcome's descriptive statistics.

The regression in the first column is a standard DiD regression using establishment-level data, containing fixed effects for each period and for each of the five regions that we are comparing. The sample is the entire universe of establishments in Switzerland. The estimated coefficient of $R_{m,t} * D\{0 < d_{it} \leq 15\}$ indicates that the reform increased the employment share of foreign workers by 2.7 percentage points in firms located between 0 to 15 minutes to the nearest border crossing. This effect is estimated using firms located within the same region but more than 30 minutes away from the border as a control group. By examining $\beta_{1,1} + \beta_{1,3}$, shown at the bottom of the table, we can also evaluate the reform impact relative to firms in the CR. The estimated reform effect is 2.1 percentage points using this control group. Figure 5a illustrates the results from this regression. It plots the evolution of the employment share of foreign workers in highly treated firms and in the two control groups. In order to improve the readability of the graph, we subtract the region-level mean of the foreign share from each observation and subsequently plot the averages of the region-demeaned data. We observe a sizeable increase in the foreign employment share between 2001 and 2005 in highly treated firms. In the two control groups, the foreign share clearly increases to a lesser extent.

In order to assess whether these visual differences are statistically significant, we cast the plotted data into the following event study DiD model:

Figure 5: Event study: share of foreign workers in FTE employment

(a) Region-demeaned foreign share, highly treated vs. control groups (all firms)
 (b) Event-study: highly treated vs. control groups



$$Y_{i,m,t} = \beta_0 + \sum_{j=-3}^{j=1} \beta_{1,1,j} R_{m,t+j}^{Event} + \sum_{j=-3}^{j=1} \beta_{1,3,j} R_{m,t+j}^{Event} * D\{0 < d_{it} \leq 15\} + \tau_t + \gamma_{i,m} + \epsilon_{i,m,t} \quad (4)$$

The model estimates the effects of phases 1 and 2 of the reform for highly treated firms relative to the two control groups, separately for each period from period $j = -3$ before the reform to period $j = 1$ after the reform. The event study reform indicators, $R_{m,t-j}^{Event}$, are 1 only in period $t - j$ and 0 otherwise (see Table 2). We omit the event-study indicator for the period 1998, $R_{m,t-2}^{Event}$, from the regression. All effects are thus estimated relative to the period the reform was announced.

Figure 5b shows the estimated reform effects and their 95% confidence intervals using this model. Firms in our high-treatment group and firms employment in both control groups had a similar within-firm change in the employment share of foreign workers in the 1995–1998 period: the confidence interval of the estimated reform effects include 0 in both cases. Between 2001 and 2005, we observe an excess increase in the employment share of foreign workers in highly treated firms relative to both control groups. Remarkably, the estimated reform effects are almost identical for both groups. The difference in the employment share of foreign workers between highly treated firms and the two control groups even becomes slightly larger from 2005 to 2008. The figure also provides weak evidence of reform effects during phase 1: the foreign share tends to increase slightly more in the high-treatment group relative to firms in the BR that are more than 30 minutes away from the border in

the 1998–2001 period. Yet, there is no statistically significant difference in the change of the share relative to the second control group.

Columns 2–5 of Table 4 show that the reform increased the employment share of foreign workers in all subsamples analyzed in this study. The estimated effects for highly and, in many cases, for slightly treated firms are positive and statistically significant relative to both control groups. Specifically, this holds (i) if we concentrate on the cross section of establishments that existed in 1998 (column 2); (ii) if we focus on our preferred sample (i.e., if we additionally discard firms with fewer than five FTE workers in 1998 and public sector firms, column 3); (iii) if we concentrate on within-establishment variation (by adding establishment fixed effects (FE), column 4); (iv) if we further restrict the sample to surviving firms (i.e., firms that are present in all waves of the BC between 1998 and 2008, column 5); or v) if we do not weight observations by firm size. The estimated effect is smaller, however, in the latter case, reflecting the impact of the reform on foreign employment is larger in large firms (see section 5.4). Column 7 looks at the staffing decisions of multi-establishments firms. In order to do this, we control for a full set of firm-period effects. The specification shows that the foreign employment share increased more in highly treated establishments compared to untreated establishments even when only comparing establishments within the same firm. Finally, the evidence that the reform affected firms’ recruitment of foreigners is even more striking if we fix FTE employment at the level of 1998. The reform increased the employment share of foreigners in highly treated establishments by 12 and 13.3 percentage points respectively, relative to the pre-reform size of a firm (column 6).

Overall, these results show that phase 2 of the reform increased the concentration of foreign workers in treated establishments. The reform effect was larger, the closer an establishment is located to the border. These conclusions are corroborated by similar results in Beerli and Peri (2016) estimated at the regional level using the wage structure surveys. In fact, we find almost the same reform effect on the employment share of foreign workers as Beerli and Peri (2016) if we adopt their municipality-level specification using the BC data. Their estimations add two further insights. First, differentiating the inflow of foreign workers into inflows of CBW and new resident immigrants, they show that around 80% of the increase in immigrant concentration in the region closest to the border is due to an increased presence of CBW. Second, exploiting the fact that the wage structure survey takes place every two years, they study how the different phases of the reform affected the immigrant share. Similar to our estimates, their results suggest that the strongest increase in the foreign employment share took place in the 2004–2008 period; namely, after the full liberalization of the CBW status. They also find a small excess increase in the immigrant

Table 4: The effect of the reform on the share of foreign workers in FTE employment

VARIABLES	(1) OLS All	(2) OLS CS 1998	(3) OLS CS 1998	(4) FE CS 1998	(5) FE Survivors	(6) FE unweigh- ted	(7) FE within firm	(8) FE CS 1998 rel. to 1998
$R_{m,t}$	-0.006** (0.003)	-0.004 (0.003)	-0.005 (0.004)	-0.005* (0.003)	-0.006* (0.003)	-0.001 (0.002)	0.009 (0.008)	0.013 (0.011)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.010*** (0.003)	0.008*** (0.003)	0.010** (0.004)	0.006* (0.003)	0.007** (0.003)	0.000 (0.002)	-0.003 (0.008)	0.031** (0.012)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.027*** (0.003)	0.020*** (0.003)	0.023*** (0.004)	0.024*** (0.004)	0.025*** (0.004)	0.012*** (0.002)	0.023*** (0.008)	0.120*** (0.027)
Observations	1,815,038	1,393,310	366,891	364,892	307,220	364,892	71,907	364,892
R-squared	0.093	0.094	0.123	0.852	0.851	0.804	0.786	0.554
Preferred sample	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Establishment effects	No	No	No	Yes	Yes	Yes	No	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Firm-period effects	No	No	No	No	No	No	Yes	No
$\beta_{1.1} + \beta_{1.2}$	0.003	0.004*	0.005*	0.000	0.001	-0.001	0.006	0.044***
$\beta_{1.1} + \beta_{1.3}$	0.021***	0.016***	0.018***	0.019***	0.019***	0.011***	0.032***	0.133***

Cluster-robust standard errors in parentheses

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

Dependent variable: Share of foreign workers

Estimation sample: 1995–2008

Descriptives of dependent variable: Mean: .1647, S.d.: .2939, Min: 0, Max: 1

Notes: With the exception of column 6, estimations are weighted using mean FTE employment of establishments as weights. The outcome in column 8 is FTE employment of foreign workers in period t relative to total FTE employment in 1998. The estimations in columns 2–8 are restricted to the cross-section of firms existing in 1998. The “preferred sample” is the sample of private sector establishments existing in 1998 with at least five FTE workers. Column 5 is restricted to firms existing in all periods from 1998–2008.

concentration in regions closest to the border in the 2000–2002 period, consistent with some effects of the reform during phase 1.

5.2 Firm size

Did the excess increase in the employment of foreigners affect the size of treated firms? Or did the foreign workers crowd out residents one-to-one with no impact on firm size? In order to study these questions, Table 5 contains several DiD models that use establishments’ FTE employment in levels and in logs as dependent variables. The estimations use all seven waves of the BC (1991–2011). The DiD model in the first column is estimated using the entire sample of establishments. The results indicate that the average establishment size grew more in highly treated firms than in firms more than 30 minutes away from the border in the BR. We also find evidence for positive impacts on the size of slightly treated firms with both control groups. However, we find no reform effect on highly treated firms if we use firms in the CR as the control group (as indicated by the estimate of $\beta_{1.1} + \beta_{1.3}$).

Nonetheless, the estimates in column 1 may not properly identify the effects of the reform on the size of incumbent firms. The reason is that the reform led to the creation of new establishments in the heavily affected regions. As explained in section 4.3, we thus restrict

Table 5: The effect of the reform on FTE employment (BC)

VARIABLES	(1) OLS All	(2) OLS CS 1998	(3) OLS CS 1998	(4) FE CS 1998	(5) FE CS 1998 log outc.	(6) FE Survivors	(7) FE Survivors log outc.	(8) FE within firm	(9) FE CS 1998
$R_{m,t}$	-0.377*** (0.062)	-0.357*** (0.078)	-0.516** (0.229)	-0.388* (0.203)	-0.023* (0.012)	-0.324 (0.212)	-0.017 (0.012)	-0.106 (1.235)	-0.731** (0.348)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.589*** (0.104)	0.913*** (0.147)	1.267*** (0.407)	0.908*** (0.327)	0.042** (0.019)	1.069*** (0.358)	0.047** (0.019)	2.275 (1.861)	1.222*** (0.395)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.390*** (0.110)	0.992*** (0.164)	2.158*** (0.474)	1.292*** (0.358)	0.055*** (0.019)	1.641*** (0.392)	0.068*** (0.019)	4.767** (2.274)	1.613*** (0.414)
$R_{m,t}^{CR}$									0.701* (0.423)
$R_{m,t}^{CR} * D\{15 < d_{it} \leq 30\}$									0.566 (1.331)
Observations	2,485,784	1,821,815	493,561	491,769	491,769	374,645	374,645	82,271	491,769
R-squared	0.001	0.001	0.002	0.903	0.910	0.914	0.925	0.476	0.903
Preferred sample	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment effects	No	No	No	Yes	Yes	Yes	Yes	No	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Firm-period effects	No	No	No	No	No	No	No	Yes	No
$\beta_{1.1} + \beta_{1.2}$	0.212**	0.557***	0.751**	0.520*	0.019	0.744**	0.030*	2.168	0.491*
$\beta_{1.1} + \beta_{1.3}$	0.014	0.636***	1.642***	0.904***	0.031*	1.317***	0.051***	4.660**	0.882***
$\beta_{1.1}^{CR} + \beta_{1.2}^{CR}$									1.267
$\beta_{1.1} + \beta_{1.2} + \beta_{1.1}^{CR} + \beta_{1.2}^{CR}$									1.758

Cluster-robust standard errors in parentheses

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

Dependent variable: FTE employment

Estimation sample: 1991–2011

Descriptives of dependent variable: Mean: 8.476, S.d.: 41.44, Min: .01, Max: 7318.

Notes: The regressions in columns 5, and 7 use log FTE employment as the outcome. In the log specifications, we weight observations using an establishment's average FTE employment as weight. The estimations in columns 2–9 are restricted to the cross-section of firms existing in 1998. The “preferred sample” is the sample of private sector establishments existing in 1998 with at least five FTE workers. Columns 6 and 7 are restricted to firms existing in all periods from 1998–2008.

the sample in the following columns to the cross section of establishments existing in 1998. If we do this, we find clear evidence for a positive effect of the reform on firm size, irrespective of whether we use the entire cross section (column 2) or if we use our preferred sample (i.e., private-sector firms with at least five FTEs in 1998, column 3). The specification in column 4, which accounts for establishment FE, indicates that establishments grew by 0.9 and 1.3 FTE employees because of the reform, depending upon the control group. These estimates translate into reform effects on establishment growth of 3.8% and 5.5% respectively because the average establishment size in this sample is 23.55 FTE workers.

The estimated reform effects are similar in size if we use log FTE employment as the outcome and weight observations by the average size of an establishment (column 5).³⁰ There is also strong evidence that the reform increased average establishment size if we restrict the

³⁰One concern with the specification in column 5 is that the sample contains a sizable amount of establishments with less than five FTEs in the years before and after 1998. The results may thus suffer from the mechanical negative correlation between initial firm size and subsequent growth discussed in the context of Figure 4a. In order to avoid this, we ran regressions only using establishments with at least five FTE employees in *all* periods. Reassuringly, this censoring has little impact on the estimated reform effects but in fact increases precision.

sample to surviving establishments. This holds both for the level (column 6) and the log outcome (column 7). Figure 6a illustrates the results from the regression in column 7. It plots the evolution of FTE employment separately for highly treated establishments and the two control groups in the BR and CR. We mirror the variation exploited in the regression by plotting the averages of firm-demeaned data. Figure 6b shows the results from the associated event study regression. We observe that the establishments grew at a similar pace in the periods before the announcement of the reform in the three regions. From 1998 onward, establishments in the highly treated region grow substantially more than establishments in the BR that are more than 30 minutes away from the border. The picture is similar if we use establishments in the CR as a control group. However, the excess growth is slightly less pronounced, especially in the first period after the announcement of the reform.

In column 8, we assess whether the reform affected the staffing decisions of multi-establishment firms. To this end, we include firm-period effects into the regression.³¹ We thus only compare establishments within the same multi-establishment firm that were differently exposed to the reform at a specific point in time. We find positive reform effects in this specification, too. This suggests that multi-establishment firms responded to the reform by increasing the size of establishments in which the access to foreign workers increased most.

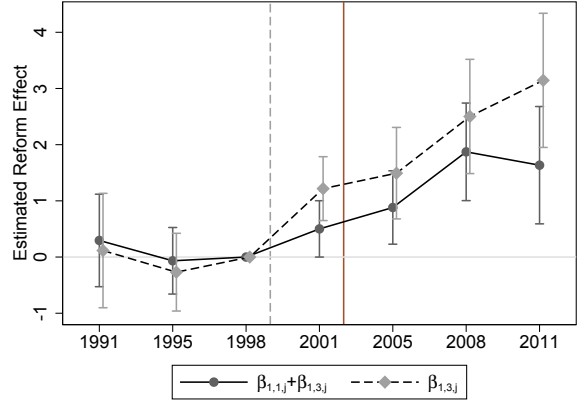
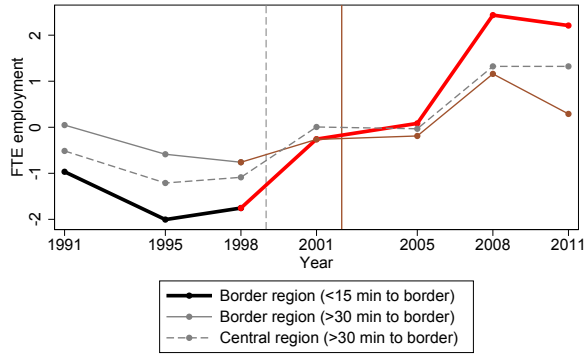
In the last column of Table 5, we test whether phase 3 had a different impact on firms in the CR as phase 2 had on firms in the BR by including a separate reform indicator for establishments in the CR, $R_{m,t}^{CR}$ (see Table 2). In this regression, and in analogous regressions for later outcomes, we generally cannot reject the null hypothesis that the reform effects were identical. These regressions thus validate our approach to pool the effects of phase 2 and phase 3 in an overall reform effect.

In columns 1-3 of Table 6, we present the results of our preferred FE regressions using the IS data. They confirm the results from the BC qualitatively. They are, however, more sensitive to the choice of the estimation model. The coefficients tend to be imprecisely estimated when using a level outcome (column 1). Our preferred specification for the IS is to use log employment. This is unproblematic in the case of the IS because micro-firms are not sampled, while taking logs accounts for the higher measurement error in the data. We present results both with (column 3) and without (column 2) weighting observations by firm size; however, we tend to prefer the latter because weighting observations gives a high weight to large multi-establishment firms that we potentially assign to the wrong region, as discussed in section 4.3. Column 2 suggests that the reform increased firms' FTE employment by 6.3% and 5.6%, depending upon the control group.

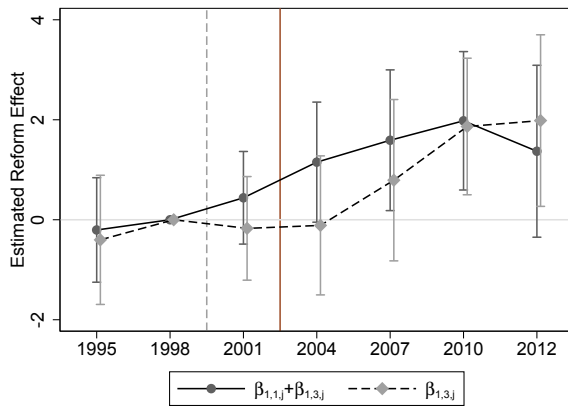
³¹We keep our preferred sample restrictions imposed in this specification. This ensures that we only compare establishments that existed already in 1998.

Figure 6: Event study: Firm size (FTE employment) of incumbent firms

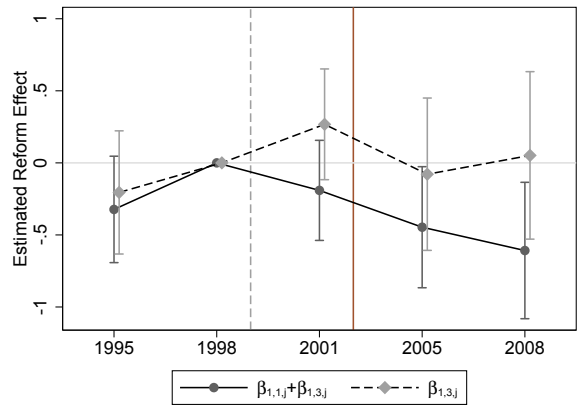
(a) Firm-demeaned FTEs, highly treated vs. control groups (BC) (b) Event-study: highly treated vs. control groups (BC)



(c) Event-study: employment share of workers with academic degrees



(d) Event study: Swiss workers



In the last three columns of Table 6, we study how the reform affected the composition of the workforce in terms of observed educational credentials. The table shows regressions that use the employment share of workers holding a specific educational degree as outcomes.³² In line with the observed change in the formal qualifications of CBW in Switzerland between 2000 and 2010, the reform mainly increased the employment share of tertiary educated workers. The event study associated with the regression in column 4 is shown in Figure 6c. Column 5 of Table 6 indicates that the employment share of workers with a secondary degree did not change much. Consequentially, the employment share of primary educated workers

³²Primary educated workers are apprentices and workers who have not attained a secondary school degree (i.e., workers on ISCED-levels 1 or 2). Secondary educated workers have completed an apprenticeship, have a secondary school degree (ISCED 3), or a post-secondary non-tertiary degree (ISCED 4). The last group consists of workers with a degree from a university or a university of applied sciences (ISCED 6 or higher).

Table 6: The effect of the reform on firms' FTE employment and the educational composition of the workforce (IS)

VARIABLES	(1) FE FTE level	(2) FE FTE log	(3) FE FTE log	(4) FE Share Bachelor or higher	(5) FE Share Secondary	(6) FE Share Primary
$R_{m,t}$	-16.950 (11.898)	-0.007 (0.019)	-0.081** (0.037)	0.150 (0.323)	-0.323 (0.836)	0.178 (0.794)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	58.015** (24.952)	0.079*** (0.027)	0.250*** (0.090)	1.053*** (0.381)	-0.187 (0.921)	-0.868 (0.855)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	26.896* (16.343)	0.063** (0.029)	0.126** (0.057)	1.200*** (0.418)	0.245 (1.105)	-1.557 (1.085)
Observations	10,429	10,429	10,429	10,036	10,036	10,036
R-squared	0.004	0.015	0.046	0.036	0.018	0.048
Number of firms	3,449	3,449	3,449	3,420	3,420	3,420
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	Yes	No	No	No
$\beta_{1.1} + \beta_{1.2}$	41.065* (16.343)	0.072*** (0.029)	0.169** (0.057)	1.203*** (0.418)	-0.510 (1.105)	-0.691 (1.085)
$\beta_{1.1} + \beta_{1.3}$	9.946	0.056**	0.045	1.350***	-0.078	-1.380

Cluster-robust standard errors in parentheses

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

Dependent variable: FTE employment

Estimation sample: 1995–2012

Descriptives of dependent variable (FTE in logs): Mean: 3.999, S.d.: 1.497, Min: 0, Max: 11.00

Notes: The dependent variables are FTE employment in levels (column 1), in logs (columns 2 and 3), and the share of FTE workers in total FTE employment with the respective highest educational attainment (columns 4–6). The “preferred sample” is the sample of surveyed firms existing in 1998. Weighted regressions use average FTE employment over the entire sample period as the weight.

declined (column 6). Overall, our results suggest that the reform raised the skill intensity of incumbent firms although it did not specifically target skilled immigrants.

A policy-relevant question is whether the inflow of foreign workers caused by the reform displaced native workers. We examine this question by examining the impact of the reform on FTEs by nationality. The analysis is restricted to the 1995–2008 period because information on the nationality of the workforce is available for waves 1995 to 2008 of the BC only. Our evidence on whether the immigrant inflow reduced the FTE employment of Swiss nationals is mixed. The event study results presented in Figure 6d reveal a decline in the FTE employment of Swiss workers in highly treated firms in the BR relative to untreated establishments in the CR after 2001, which is suggestive of displacement of Swiss workers. However, we do not find a comparable effect if we use untreated firms in the BR as a control group. The regression results, presented in Table A.6 in the appendix, do not provide a conclusive picture either. For instance, the estimated coefficients from our preferred model

suggest that the FTE employment of Swiss nationals was either unaffected or declined by 0.3% for any full-time job taken by a foreign worker, depending upon the control group.³³ Further, the reform effect on the employment of Swiss workers does not monotonically increase the closer we come to the border. If at all, the reform appears to have led to a decline in the employment of Swiss nationals close to the border and more than 30 minutes away from the border; however, we observe no negative impact in slightly treated firms. Overall, the results are consistent with some or no crowding out of Swiss nationals. There is no evidence for crowding in.

Our results on the effects on natives are in line with prior studies on the impacts of the recent immigration wave to Switzerland. These studies typically find small average negative wage and employment impacts on residents (Basten and Siegenthaler, 2013; Favre et al., 2013; Gerfin and Kaiser, 2010; Müller and Graf, 2015). The most important study in our context is Beerli and Peri (2016). Their in-depth analysis of the labor market effects of the reform reveals limited effects of the policy change on employment and hours worked of residents overall, and even positive effects for primary educated residents. They find strong evidence that the wages of tertiary educated residents increased as a consequence of the reform. Wages of other workers were not affected. The positive wage impacts on tertiary educated natives are consistent with their finding—and very similar results in Basten and Siegenthaler (2013)—that the new immigrants enabled skilled residents to move up the career and job ladder.

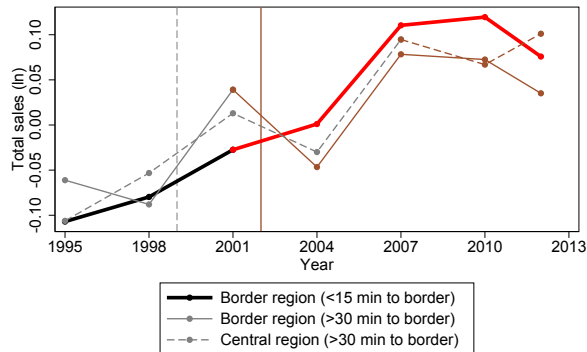
5.3 Sales and productivity

The results presented so far suggest that the introduction of the free movement of persons increased employment of skilled foreign workers. However, did the increase in firm size go hand-in-hand with growth in terms of sales and labor productivity? In order to gain insights into this question, Figure 7 uses data from the IS and plots the evolution of average firm-demeaned log sales in the three most important regions. Figure 7b contains the corresponding event study results. In the 2001–2004 period, when firms in the BR located close to the border were affected by the immigration reform, we observe that sales of highly treated firms grew while sales declined in the two other groups of firms. We observe a similar pre- and post-treatment trend in firms’ sales between highly treated firms and firms in the CR. The pre-treatment evolution in sales is slightly less similar if we compare highly treated firms and firms in the BR that are more than 30 minutes away from the border. The

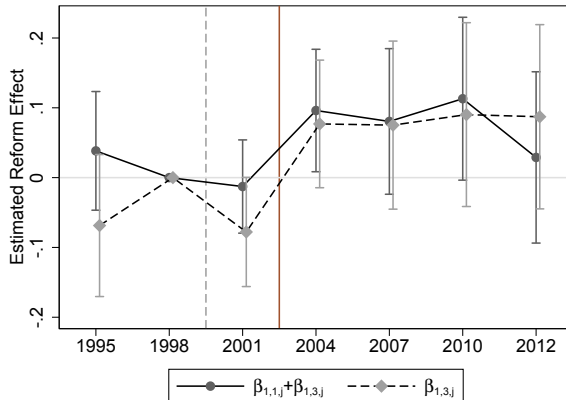
³³We compute these figures by relating the estimates in Table A.6 to the corresponding results in Table A.7.

Figure 7: Event study: Total sales

(a) Firm-demeaned log sales, highly treated vs. control groups



(b) Event-study: highly treated vs. control groups



post-treatment trends are, however, remarkably parallel, validating the use of this region as a second control group.

Columns 1–5 of Table 7 present the DiD regressions. Since the dependent variable is specified in logs, the estimated coefficients represent semi-elasticities. The simple models without firm fixed effects do not provide evidence that the reform had an impact on firms’ log sales, regardless of whether the estimation uses all firms (column 1) or only the cross section of firms in 1998 (column 2). According to our preferred specification in column 3 that accounts for firm entry and firm FE, however, the reform increased sales of highly treated firms. The estimated reform effects are 11% and 8.2%, depending upon the control group. The estimated reform effect becomes even larger if we weight observations by firm size (column 4).

Columns 6–9 of Table 7 examine whether the reform affected log value added per FTE worker.³⁴ The estimated treatment effects are close to 0 in many cases and mostly statistically insignificant. We even find evidence that the policy change *reduced* the average labor productivity of slightly treated firms in some specifications. The same conclusions hold qualitatively if we use the average wage per FTE worker rather than value added per FTE worker as an indicator of firm productivity (see section D of the appendix).

³⁴Value added is computed by subtracting intermediate inputs from total sales. The survey contains information on intermediate inputs as a share in total sales, as well as information on firms’ total sales.

Table 7: The effect of the reform on sales and average labor productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	OLS Sales	OLS Sales	FE Sales	FE Sales	FE Sales	OLS Produc- tivity	OLS Produc- tivity	FE Produc- tivity	FE Produc- tivity
$R_{m,t}$	-0.013 (0.067)	-0.083 (0.079)	-0.027 (0.026)	-0.135 (0.086)	-0.055 (0.036)	0.039 (0.026)	-0.005 (0.029)	-0.020 (0.024)	-0.015 (0.031)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	-0.091 (0.077)	-0.043 (0.087)	0.035 (0.033)	0.061 (0.084)	0.055 (0.037)	-0.035 (0.028)	-0.025 (0.031)	-0.054** (0.027)	-0.061** (0.031)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.062 (0.087)	0.076 (0.101)	0.110*** (0.038)	0.378** (0.151)	0.131*** (0.042)	0.024 (0.031)	0.015 (0.037)	0.031 (0.028)	0.026 (0.032)
$R_{m,t}^{CR}$					0.047 (0.048)				-0.009 (0.035)
$R_{m,t}^{CR} * D\{15 < d_{it} \leq 30\}$					0.140 (0.118)				0.233 (0.188)
Observations	14,643	9,749	9,749	9,749	9,749	12,534	8,303	8,303	8,303
R-squared	0.023	0.030	0.071	0.230	0.072	0.021	0.023	0.027	0.028
Preferred sample	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Firm effects	No	No	Yes	Yes	Yes	No	No	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	Yes	No	No	No	No	No
$\beta_{1.1} + \beta_{1.2}$	-0.104	-0.126	0.008	-0.074	0.000	0.003	-0.030	-0.074***	-0.076***
$\beta_{1.1} + \beta_{1.3}$	0.049	-0.007	0.082**	0.243**	0.076**	0.063*	0.010	0.011	0.011
Number of firms			3,342	3,342	3,342			3,064	3,064
$\beta_{1.1}^{CR} + \beta_{1.2}^{CR}$					0.187*				0.225
$\beta_{1.1} + \beta_{1.2} + \beta_{1.1}^{CR} + \beta_{1.2}^{CR}$					0.187*				0.149

Cluster-robust standard errors in parentheses

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

Estimation sample: 1995–2012

Notes: The dependent variable in columns 1–5 is the log of total sales. The dependent variable in columns 6–9 is log value added per FTE worker. The “preferred sample” is the sample of surveyed firms existing in 1998. Weighted regressions use average FTE employment over the entire sample period as the weight. The specification of $R_{m,t}^{CR}$ is shown in Table 2.

5.4 Heterogeneity

The average reform effects discussed so far hide interesting heterogeneity in the effects for different types of firms. To start with, we estimate the regression model in column 4 of Table 4 separately for different subgroups of firms. Panel A of Table 8 presents the results. The table reveals groups of establishments in which we observe a particularly sizeable increase in the employment share of foreign workers. These groups are large establishments (i.e., firms with more than 250 FTE workers in 1998), establishments that are part of a multi-establishment firm, manufacturers, and, in particular, high-tech manufacturers.

These groups also experience a particularly sizeable increase in establishment size as a consequence of the reform, as shown in Panel B of the table. The bottom of the panel contains the estimated reform effects (i.e., $\beta_{1.3}$ and $\beta_{1.1} + \beta_{1.3}$) scaled by the average firm size of each subgroup. The first three columns reveal that the impact of the reform is non-linear on the initial size of the firm. While there is no apparent impact on the growth of establishments with less than 50 FTE workers in 1998, firms with more than 250 FTE workers grow more than 11%. Even more marked is the size effect on multi-establishment firms, manufacturers, and, in particular, high-tech manufacturers. The latter grow by almost

Table 8: Heterogeneity of reform effects (BC)

	(1) FE Small establ.	(2) FE Medium- sized	(3) FE Large establ.	(4) FE Multi- establ.	(5) FE Manufac- turing	(6) FE High -tech	(7) FE Business services	(8) FE >25% foreigners
Panel A: Foreign share								
$R_{m,t}$	0.000 (0.002)	-0.013** (0.006)	-0.010 (0.018)	-0.007 (0.006)	-0.002 (0.007)	-0.004 (0.012)	-0.001 (0.003)	0.009 (0.006)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.000 (0.002)	0.009 (0.006)	0.021 (0.015)	0.012* (0.006)	0.002 (0.007)	0.011 (0.014)	0.003 (0.004)	0.000 (0.006)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.013*** (0.003)	0.024*** (0.006)	0.057*** (0.016)	0.032*** (0.008)	0.038*** (0.009)	0.063*** (0.014)	0.014*** (0.004)	0.041*** (0.006)
Observations	336,286	25,592	3,014	91,858	66,381	17,183	202,872	135,691
R-squared	0.827	0.880	0.892	0.869	0.864	0.870	0.847	0.730
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1,1} + \beta_{1,2}$	0.000	-0.004	0.011	0.005	0.000	0.007	0.002	0.009*
$\beta_{1,1} + \beta_{1,3}$	0.013***	0.011**	0.047***	0.025***	0.036***	0.059***	0.013***	0.050***
Firm size in 1998	12.70	96.87	525.51	15.56	15.41	33.45	6.10	13.83
Panel B: FTE employment								
$R_{m,t}$	-0.239*** (0.087)	-1.807 (1.522)	-9.381 (23.011)	-1.126 (0.752)	-1.053 (0.768)	-2.307 (2.748)	-0.314 (0.218)	-0.243 (0.451)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.474*** (0.125)	4.620** (2.339)	37.863 (31.925)	2.648** (1.117)	1.584 (1.187)	5.201 (3.913)	1.179*** (0.389)	1.196* (0.658)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.332** (0.151)	6.533*** (2.356)	69.174** (33.236)	3.837*** (1.226)	3.856*** (1.415)	12.041*** (4.511)	1.094*** (0.374)	2.825*** (0.652)
Observations	452,699	34,952	4,118	124,766	90,227	23,213	275,037	182,593
R-squared	0.653	0.653	0.868	0.917	0.928	0.933	0.834	0.915
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1,1} + \beta_{1,2}$	0.236**	2.814	28.482	1.522	0.531	2.894	0.865**	0.953*
$\beta_{1,1} + \beta_{1,3}$	0.093	4.726**	59.793**	2.711***	2.803**	9.734***	0.780**	2.582***
Firm size in 1998	12.70	96.87	525.51	15.56	15.41	33.45	6.10	13.83
$\beta_{1,3}$ scaled by firm size	0.026**	0.067***	0.132**	0.247***	0.250***	0.360***	0.179***	0.204***
$\beta_{1,1} + \beta_{1,3}$ scaled by firm size	0.007	0.049**	0.114**	0.174***	0.182**	0.291***	0.128**	0.187***

Cluster-robust standard errors in parentheses

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

Notes: Each panel contains individual FE regressions of our preferred specification and using our preferred estimation sample (private sector firms existing in 1998 with at least five FTEs in 1998) for each subsample mentioned in the table header. Small establishments have 5–50 FTE workers, medium-sized establishments 51–250 and large establishments more than 250 FTE workers in 1998. In accordance with the Eurostat definition, high-tech industries are NACE rev. 2 industries 20, 21, 254, 26–29, 30 without 301 and 325. Column 8 presents regression results for firms with a foreign worker share in FTE employment in 1998 of at least 25%. Establishments are assigned to each group based on the characteristics in 1998 with the exception of industry affiliation.

a third because of the reform. Another group that particularly resorts to foreign workers, and grows specifically because of the reform, is establishments that already had a workforce with more than a 25% foreign share in 1998 (column 8, panels A and B).

Figure 8 uses the IS data to provide further evidence on the heterogeneity of the reform effects, focusing on the impacts on sales and productivity. First, we estimate separate treatment effects for firms in industries that differed in their pre-reform share of CBW in total employment. Using the waves 1996–2000 of the wage structure survey, we differentiate industries with 0–5%, 5–10%, 10–15%, and more than 15% of cross-border share. We then interact indicator variables for each industry category with all reform coefficients in our preferred FE regression model. For each category, we plot the estimated reform effects evaluated relative to both control groups (by showing $\beta_{1,1} + \beta_{1,3}$ as well as $\beta_{1,3}$). Figure 8a shows that the positive sales effect of the reform is concentrated in industries with a large CBW share prior to the reform. In fact, firms in these industries also experienced productivity gains as a consequence of the reform (Figure 8c). These findings resemble the results

from studies examining the impacts of changes in the H-1B program on US firms. These studies typically find that changes in the number of H-1B visas mainly affect productivity in firms and regions that depend heavily upon H-1B workers (Ghosh et al., 2014; Kerr and Lincoln, 2010; Peri et al., 2015a).

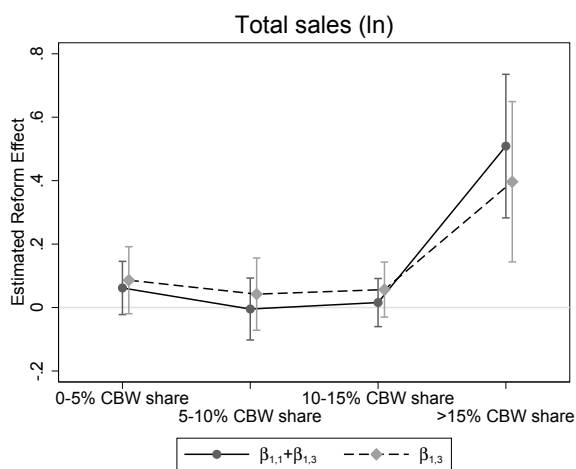
The second half of Figure 8 analyzes whether the reform had a particularly strong impact on firms that reported problems in finding suitable skilled workers prior to the reform. We expect that firms that perceived to have labor constraints prior to the reform profited particularly from the reform. In order to test this hypothesis, we exploit the fact that 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 three survey waves prior to the reform for each firm and subsequently build four dummy variables: firms that had “no shortage” are firms with a value below 2, “low shortage” firms are those with a value between 2 and 3, “medium shortage” firms have a value between 3 and 4, and “high shortage” firms have a value greater than or equal to 4. In line with our expectation, we find particularly positive sales effects of the reform among highly treated firms that suffered from substantial skill shortages prior to the reform (Figure 8b). These effects translate into positive effects on average labor productivity (Figure 8d).

Finally, Table 9 digs deeper into the question of whether the sales and productivity effects of the reform differed across firms using firm characteristics available in the IS. The table summarizes the results from regressions that estimate separate reform effects for the two groups of firms defined in each panel. We undertake this by interacting the reform indicator, $R_{m,t}$, with an indicator variable for one of the two groups. For each firm characteristic, we then assess whether $\beta_{1,1} + \beta_{1,3}$ and $\beta_{1,3}$ are statistically significantly different across the two groups. The first two columns suggest that the reform had a beneficial impact on firm size (in terms of FTE employment and sales) across a wide range of different firms (e.g., in manufacturing and service sector firms and in exporting and non-exporting firms). The productivity effects are, again, more heterogeneous. Table 9 highlights two further groups of firms that benefited in terms of labor productivity: foreign-owned firms and high-tech manufacturers. We find particularly positive reform effects in high-tech industries for most outcomes examined in this paper. Beerli and Peri (2016) also find particularly sizable positive wage effects on high-skilled residents in these industries.

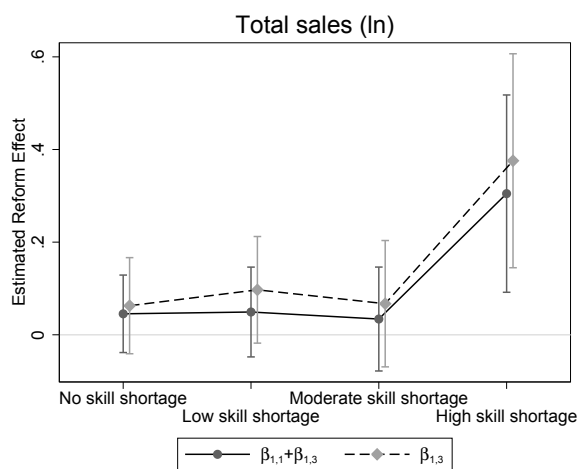
5.5 Mechanisms

The results so far suggest that opening the borders in Switzerland led to an increased presence of skilled foreign workers that had positive effects on firm size and heterogeneous

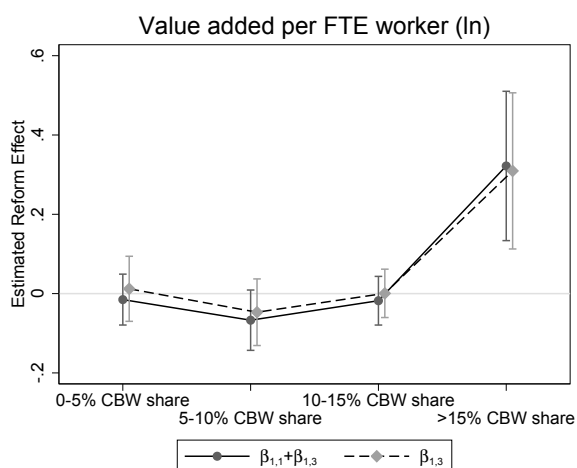
Figure 8: Reform effect by cross-border intensity of industry and pre-reform problems in finding skilled workers



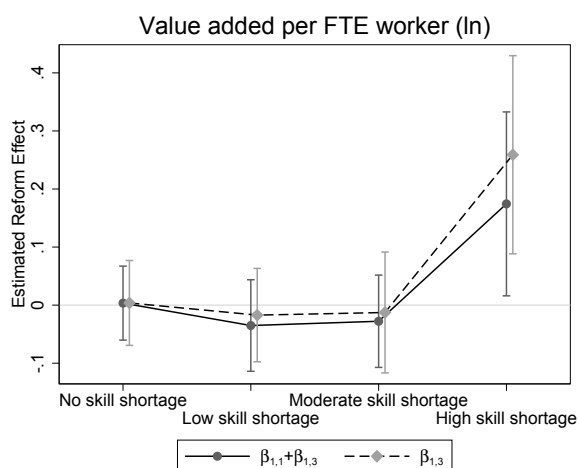
(a) Cross-border intensity of industry



(b) Skill shortage ex ante



(c) Cross-border intensity of industry



(d) Skill shortage ex ante

effects on labor productivity. One explanation for these findings supported by our analysis is that the reform helped certain firms to overcome skill bottlenecks that had constrained their expansion. This section discusses two further potential explanations: higher innovation performance and the relocation of economic activity into the regions with better access to foreign workers.

5.5.1 R&D employment and patenting

Our theoretical model and a growing empirical literature suggests that lifting immigration restrictions could affect firm expansion because it promotes the innovation activities of firms.

Table 9: Estimated reform effects by firm characteristics (IS)

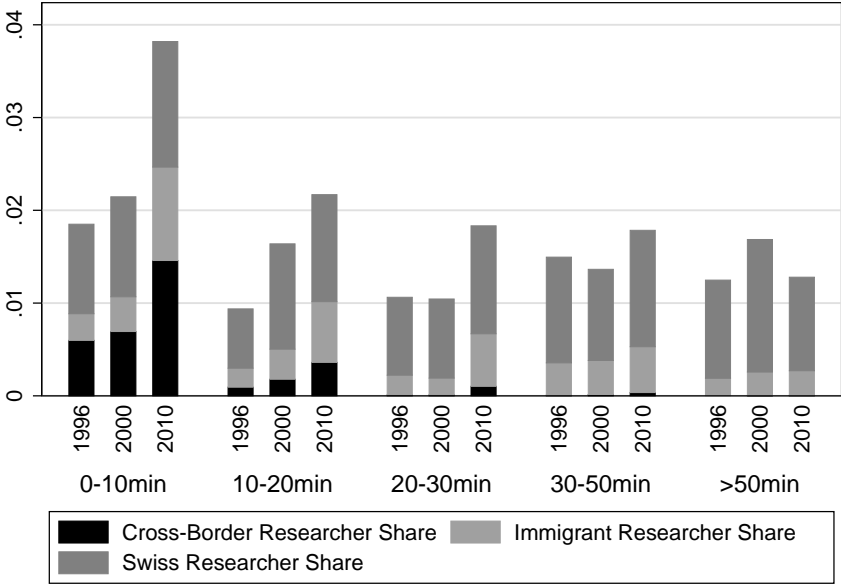
	(1)		(2)		(3)		(4)	
	FTE employment		Sales		Productivity		Patents (0/1)	
	$\beta_{1,1} + \beta_{1,3}$	$\beta_{1,3}$	$\beta_{1,1} + \beta_{1,3}$	$\beta_{1,3}$	$\beta_{1,1} + \beta_{1,3}$	$\beta_{1,3}$	$\beta_{1,1} + \beta_{1,3}$	$\beta_{1,3}$
A. Firm size								
Less than 100 FTEs	0.050*	0.071**	0.063	0.132***	-0.024	0.019	0.023	0.052***
At least 100 FTEs	0.070*	0.048	0.118**	0.068	0.067*	0.045	0.064**	0.054
p-value difference	0.64	0.69	0.38	0.41	0.04	0.64	0.18	0.98
B. Manufacturing versus services								
Services	0.086**	0.059	0.082*	0.083	-0.017	0.007	0.026	0.020
Manufacturing	0.030	0.066*	0.082*	0.132**	0.033	0.051	0.049*	0.083***
p-value difference	0.15	0.90	0.99	0.48	0.24	0.41	0.36	0.05
C. High-tech industries								
Low- and med-tech industries	0.050*	0.058*	0.044	0.084**	-0.022	0.011	0.038**	0.054***
High-tech industries	0.083*	0.087	0.249***	0.228**	0.148**	0.122*	0.040	0.054
p-value difference	0.49	0.65	0.03	0.17	0.01	0.14	0.96	1.00
D. Exporters vs. non-exporters								
Non-exporters	0.078**	0.047	0.076	0.072	-0.033	-0.015	0.029	0.026*
Exporters	0.048	0.101**	0.083*	0.150***	0.020	0.046	0.049*	0.080***
p-value difference	0.48	0.35	0.92	0.32	0.28	0.30	0.46	0.11
E. Foreign ownership								
Domestic firms	0.047*	0.066**	0.063*	0.099**	-0.011	0.008	0.020	0.042**
Foreign-owned firms	0.115*	0.066	0.189*	0.237**	0.168**	0.263***	0.082	0.064
p-value difference	0.33	1.00	0.23	0.27	0.02	0.00	0.27	0.75

Notes: The table shows reform effects estimated using our preferred FE model. The outcomes in columns 1–3 are measured in logs. In each panel, separate reform effects are estimated using an interaction term with the characteristic studied. Firm size is a dummy depending upon the average firm size in the sample period. In accordance with the Eurostat definition, high-tech industries are NACE rev. 2 industries 20, 21, 254, 26–29, 30 without 301 and 325. Exporters are firms with nonzero export shares prior to 2002. Foreign-owned firms are foreign-owned firms prior to 2002.

We now analyze this hypothesis. We first study whether the reform affected firms' employment of R&D workers. We undertake this using the wage structure surveys. The survey divides the labor force into 24 occupational categories (Table A.3 contains the list). One of them is whether a person works in "research and development." Using this information, we calculate the Swiss, foreign, cross-border, and overall researcher share in total FTE employment in every firm. The underlying sample consists of roughly 400,000 firm-year observations in the private sector firms with at least three FTE employees.

Figure 9 plots the share of cross-border researchers, Swiss researchers, and foreign researchers (i.e., permanent immigrants) in total employment depending upon the distance between a firm's location and the nearest border crossing. We observe very similar patterns as in Figure 2. In particular, CBW researchers play a more important role close to the border; moreover, their importance declines strongly with a growing distance to the nearest border crossing in every period. Further, the change in the CBW researcher share after 2000 becomes larger, the closer we come to the border. In fact, the researcher share in total employment does not change much in the 2000–2010 period in regions more than 30 minutes away from the border. Interestingly, the stronger increase in the CBW researcher share close to the border translates into a stronger increase of the total researcher share in total employment close to the border. This is suggestive of positive impacts of the reform on R&D employment.

Figure 9: The share of researchers in total FTE employment by firm’s distance to border crossing



Source: Wage structure surveys, various years. Firms that do not belong to the private sector are excluded from the sample. We exclude individuals where we do not know the residence status or the occupation.

In Table 10, we use the CBW, Swiss, foreign, and total researcher shares as outcome variables in order to conduct a more formal test. The regressions are estimated at the firm level. Because the wage structure surveys do not enable us to track firms over time, we cannot estimate FE regressions. As an alternative, we estimate a set of two-way fixed effects OLS regressions that, alternatively, control for time and region fixed effects (column 1), industry period and region effects, and time and zip code effects (column 3). The regressions clearly suggest that the reform promoted the employment of CBW in R&D. Moreover, they also suggest that the reform increased the total researcher share. Depending on the specification and control group, the estimated treatment effects range from 0.4 to 1.2 percentage points. In general, only approximately one third to one half of the increase in the R&D employment share is attributed to CBW. A substantial part also stems from increased employment of other (resident and newly immigrating) foreign R&D workers, suggesting crowding in of immigrant researchers. Most specifications provide no evidence for an effect on Swiss researchers.

We find substantial heterogeneity of the reform impacts across different firms. Consistent with our prior findings, the effects are greatest on the research departments in large

firms (column 6), in manufacturing firms (column 7), and in firms that have at least one researcher (termed R&D intensive firms, column 8). The reform did not increase the research departments in medium-sized companies (column 5) and there is mixed evidence on whether it had a positive impact in small firms (column 4).

In a further specification, we look at the count of FTE researchers per firm (column 9) as an outcome variable. In order to account for the substantial amount of firms without R&D workers and the long right tail of their distributions, we transform the outcome using the inverse hyperbolic sine (IHS). This transformation enables us to keep the zeros in the sample but normalizes the skewed distribution.³⁵ The number of researchers per firm increases by 2,5 to 3% depending upon the control group using this outcome.

Did the increased employment of R&D workers translate into inventions and innovations? We study this question in Table 11 using data from the IS. In columns 1–3, we examine how the reform affected the probability that a firm applied for at least one patent in the three years prior to the IS. The regressions suggest that the reform increased this probability. We find reform effects between 3.4 to 5.4 percentage points, depending upon the control group, the choice of the estimation sample, and the inclusion of firm fixed effects. Figures 10a and 10b illustrate the positive reform effect on the probability to apply for a patent. They also show that firms in the high-treatment region and in the two control regions have similar trends in their propensity to apply for patents in the periods prior and posterior to the reform. These results are also qualitatively confirmed if we use the IHS of the number of patent applications of a firm as an outcome, as is done in column 4 of Table 11. Estimating the reform effects for different subsamples of firms, it appears that the reform had a particularly beneficial impact on patent applications among exporters and manufacturers (see Table 9).

Another subgroup of firms where the reform may have had a stronger impact on inventions is firms that reported that they had suffered from a *shortage of R&D workers* prior to the reform. In column 5 of Table 11, we study this hypothesis by interacting the reform indicator with a variable termed $LS_{i,prior}^{R\&D}$. This dummy is equal to 1 if a firm reported substantial problems in finding R&D workers in either one or more of the three surveys preceding the reform, or 0 otherwise.³⁶ Contrary to our hypothesis, we do not find statistically

³⁵The 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 (see also Burbidge et al., 1988). The results are similar if we use the log of researchers (discarding observations with no R&D workers) or the log of 1 plus the number of researchers.

³⁶We average the responses to the relevant survey question on a 5-point Likert scale over the three survey waves prior to the reform for each firm and set $LS_{i,prior} = 1$ if the average is greater or equal to 4.

Table 10: The effect of the reform on the share of R&D workers in total FTE employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
				Small establ.	Medium establ.	Large establ.	Manufact- uring	Only R&D intensive	Researcher Count
Panel A: Cross-border researchers									
$R_{m,t}$	-0.001 (0.001)	-0.000 (0.000)	-0.001* (0.000)	-0.000*** (0.000)	-0.000 (0.000)	-0.001 (0.001)	-0.001* (0.001)	-0.002 (0.002)	-0.006*** (0.001)
$R_{m,t} * D(15 < d_{i,t} \leq 30)$	0.001** (0.000)	0.001 (0.000)	0.001*** (0.000)	0.000*** (0.000)	0.001*** (0.000)	0.001** (0.001)	0.002*** (0.001)	0.003* (0.002)	0.007*** (0.002)
$R_{m,t} * D(0 < d_{i,t} \leq 15)$	0.005** (0.003)	0.003** (0.001)	0.005*** (0.001)	0.002*** (0.000)	0.004*** (0.001)	0.013*** (0.004)	0.010*** (0.003)	0.014*** (0.004)	0.012*** (0.004)
$\beta_{1.1} + \beta_{1.2}$	0.000	0.000	0.000	-0.000	0.000***	-0.000	0.001	0.001	0.001
$\beta_{1.1} + \beta_{1.3}$	0.005*	0.003**	0.005***	0.002***	0.003***	0.011***	0.009***	0.012***	0.006*
Panel B: Foreign researchers									
$R_{m,t}$	-0.001* (0.001)	-0.001 (0.000)	-0.001* (0.001)	-0.001*** (0.000)	0.000 (0.000)	-0.003 (0.003)	-0.001 (0.001)	-0.001 (0.004)	-0.004* (0.002)
$R_{m,t} * D(15 < d_{i,t} \leq 30)$	0.002*** (0.001)	0.002*** (0.001)	0.002*** (0.001)	0.001 (0.001)	0.001 (0.001)	0.002 (0.003)	0.005** (0.002)	0.011 (0.007)	0.009* (0.005)
$R_{m,t} * D(0 < d_{i,t} \leq 15)$	0.004** (0.002)	0.002* (0.001)	0.004*** (0.001)	0.001*** (0.001)	-0.000 (0.001)	0.008* (0.004)	0.006** (0.002)	0.011 (0.007)	0.011** (0.005)
$\beta_{1.1} + \beta_{1.2}$	0.001**	0.001**	0.001**	-0.000	0.001	-0.001	0.004**	0.010	0.005**
$\beta_{1.1} + \beta_{1.3}$	0.003**	0.001*	0.003***	0.001	0.000	0.005*	0.005**	0.009**	0.007
Panel C: Swiss researchers									
$R_{m,t}$	-0.000 (0.001)	0.000 (0.001)	-0.000 (0.001)	0.000 (0.001)	-0.000 (0.001)	-0.006* (0.004)	-0.000 (0.002)	0.001 (0.007)	0.001 (0.004)
$R_{m,t} * D(15 < d_{i,t} \leq 30)$	0.003* (0.002)	0.002* (0.001)	0.003** (0.002)	-0.001 (0.001)	-0.000 (0.001)	0.006 (0.004)	0.005 (0.004)	0.020 (0.013)	0.010 (0.008)
$R_{m,t} * D(0 < d_{i,t} \leq 15)$	0.003 (0.002)	0.001 (0.001)	0.003** (0.001)	-0.000 (0.001)	-0.002 (0.001)	0.010 (0.004)	0.001 (0.002)	0.013 (0.007)	0.016** (0.005)
$\beta_{1.1} + \beta_{1.2}$	0.003*	0.003*	0.003**	-0.000	-0.001	-0.001	0.005	0.021	0.010
$\beta_{1.1} + \beta_{1.3}$	0.003	0.001	0.003**	-0.000	-0.002	0.003	0.000	0.013	0.017**
Panel D: Total researchers									
$R_{m,t}$	-0.002 (0.002)	-0.001 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.010* (0.006)	-0.002 (0.003)	-0.002 (0.010)	-0.005 (0.004)
$R_{m,t} * D(15 < d_{i,t} \leq 30)$	0.006*** (0.002)	0.005** (0.002)	0.006*** (0.002)	0.000 (0.002)	0.001 (0.002)	0.009 (0.007)	0.011** (0.005)	0.035* (0.019)	0.016* (0.008)
$R_{m,t} * D(0 < d_{i,t} \leq 15)$	0.012* (0.006)	0.006* (0.003)	0.012*** (0.004)	0.003** (0.001)	0.001 (0.002)	0.030*** (0.011)	0.017** (0.007)	0.037** (0.017)	0.030*** (0.008)
$\beta_{1.1} + \beta_{1.2}$	0.004**	0.004**	0.004**	-0.001	0.001	-0.001	0.009*	0.032**	0.010
$\beta_{1.1} + \beta_{1.3}$	0.010*	0.005*	0.011***	0.002	0.001	0.020**	0.014**	0.035***	0.025***
Observations	400,238	400,238	400,238	322,949	65,673	11,616	81,145	15,590	401,093
Region Dummies	Yes	Yes	No	No	No	No	No	No	No
Municipality dummies	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry period fixed effects	No	Yes	No	No	No	No	No	No	No
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Cluster-robust standard errors are in parentheses

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

Notes: The regressions use data from the wage structure surveys. The dependent variable of the regressions in columns 1–8 is the share of R&D workers in total employment in a firm or, if available, in the establishment of a firm. In column 9, the outcome is the count of researchers. Small establishments have up to 50 workers, medium-sized establishments 51–250, and large establishments more than 250 workers. R&D intensive companies have at least one researcher. All the regressions are weighted by the number of FTE workers in a firm. The coefficient of the share of cross-border researchers, Swiss researchers, and foreign researchers add up to the coefficient of the share of the total researchers in total employment. Standard errors are clustered at the zip code level.

Table 11: The effect of the reform on firm inventions and innovation (IS)

VARIABLES	(1) OLS Patents 0/1	(2) OLS Patents 0/1	(3) FE Patents 0/1	(4) FE Patent count (IHS)	(5) FE Patent count (IHS)	(6) FE Product innov. 0/1	(7) FE Process innov. 0/1
$R_{m,t}$	0.015 (0.013)	-0.013 (0.018)	-0.016 (0.016)	-0.045 (0.030)	-0.041 (0.036)	-0.047* (0.027)	-0.005 (0.030)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	-0.009 (0.014)	0.026 (0.017)	0.010 (0.017)	0.034 (0.036)	0.070* (0.040)	0.011 (0.027)	-0.006 (0.030)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.034** (0.016)	0.054*** (0.020)	0.054*** (0.018)	0.091** (0.037)	0.102** (0.046)	-0.003 (0.033)	0.068* (0.036)
$R_{m,t} * LS_{i,prior}^{R\&D}$					0.010 (0.096)	0.082 (0.052)	-0.009 (0.061)
$R_{m,t} * D\{15 < d_{it} \leq 30\} * LS_{i,prior}^{R\&D}$					-0.116 (0.148)	-0.029 (0.076)	0.079 (0.087)
$R_{m,t} * D\{0 < d_{it} \leq 15\} * LS_{i,prior}^{R\&D}$					0.019 (0.130)	0.145* (0.076)	-0.023 (0.100)
Observations	15,415	10,211	10,211	10,088	8,201	8,465	8,465
R-squared	0.004	0.004	0.011	0.005	0.006	0.020	0.046
Preferred sample	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	No	No	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1,1} + \beta_{1,2}$	0.006	0.013	-0.006	-0.011	0.029	-0.036	-0.010
$\beta_{1,1} + \beta_{1,3}$	0.049***	0.042**	0.038**	0.046	0.061	-0.050	0.063*
Number of firms			3,428	3,420	2,569	2,578	2,578
$\beta_{1,1} + \beta_{1,3} + \beta_{1,1}^{R\&D} + \beta_{1,3}^{R\&D}$					0.090	0.178***	0.031
p-value $H_0 : \beta_{1,1}^{R\&D} + \beta_{1,3}^{R\&D} = 0$					0.74	0.00	0.68

Cluster-robust standard errors in parentheses

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

Dependent variable: Patent applications in last 3 years (0/1)

Estimation sample: 1996–2013

Descriptives of dependent variable: Mean: .1379, S.d.: .3448, Min: 0, Max: 1

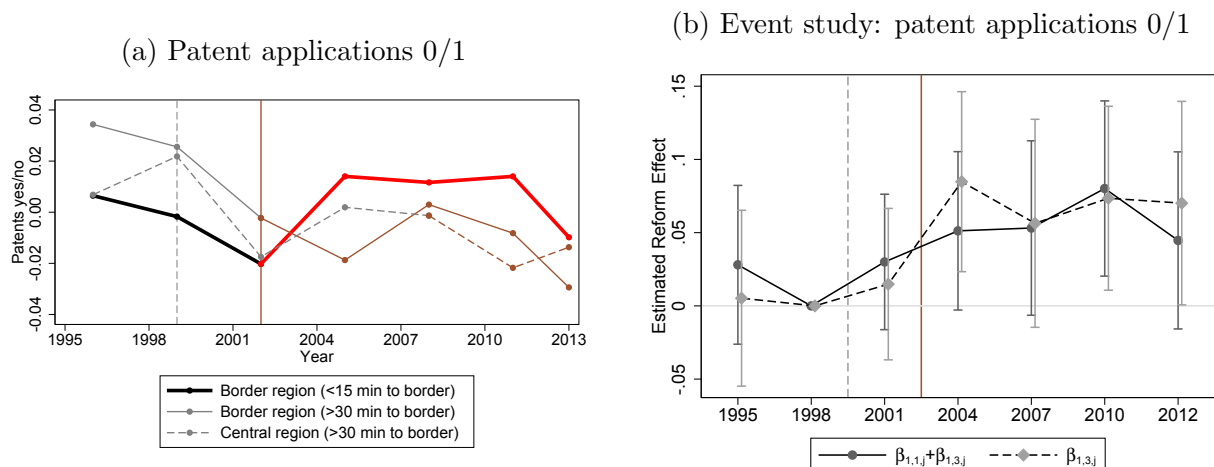
Notes: The dependent variable in columns 1–3 is a dummy equal to 1 if a firm filed at least one patent application in the three years preceding the survey. The dependent variable in columns 4 and 5 is the IHS of the number of patent applications. The dependent variables in columns 6 and 7 are dummies equal to 1 if a firm reported product or process innovations in the IS (see main text). The “preferred sample” is the sample of surveyed firms existing in 1998.

significant differences in the reform’s impact on the number of patent applications between firms that were and firms that were not constrained by lack of R&D personnel ex ante.

Columns 6 and 7, however, suggest that differences exist in the reform’s impacts between firms with and without constraints on R&D employment prior to the reform. In these columns, we look at the actual outcomes of the innovation process as collected in the IS. The outcome in column 6 is whether a firm reported product innovations in the three years prior to the survey. 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 regression shows that firms that suffered from problems in finding R&D workers had a higher probability of making product innovations when they obtained greater access to CBW. We find no impact of the reform on this outcome in firms that did not report a lack of R&D employment. In these firms, however, the reform appears to have increased the probability of process innovations, as shown in column 7. In the absence of prior constraints on R&D employment, better access to foreign workers appear to have led to the implementation of new technologies and production methods.

Overall, our results suggest positive effects from lifting immigration restrictions on innovation and patenting in Swiss firms. The results also add to an open debate on whether

Figure 10: Patent applications by region and distance to the nearest border crossing



inflows of skilled immigrants benefit high-skilled residents. Studies on the impacts of H-1B workers generally find no or positive impacts on high-skilled natives (Ghosh et al., 2014; Hunt and Gauthier-Loiselle, 2010; Kerr et al., 2015), with Doran et al. (2015) being the important exception. Similarly, Moser et al. (2014) show that patents filed by US inventors increased substantially after the immigration of Jewish chemists who fled from Nazi Germany. Peri et al. (2015b) find that inflows of science, technology, engineering and mathematics (STEM) workers are strongly positively related to the wages of college-educated natives across US cities. In contrast, Borjas and Doran (2012) show that US mathematicians were negatively affected by the strong influx of Russian mathematicians into the US after the collapse of the Soviet Union, both in terms of publications and academic positions. One explanation for the latter results, pointed out by Card and Peri (2016), is that the number of positions in top academic institutions and the number of papers in top academic journals are relatively fixed. In contrast, our results suggest that the immigration of foreign researchers to Switzerland was not a “zero sum” game where natives and residents competed for a fixed number of jobs. Rather, the evidence suggests that the number of jobs increased in parallel to the greater availability of workers. The next section presents evidence that this phenomenon is not specific to the labor market for R&D workers but may explain the absorption of the increased supply of foreign workers on the Swiss labor market more generally.

5.5.2 Firms’ location and outsourcing decisions

Did the reform lead to a reallocation of economic activity into the heavily affected regions, either between or within firms? Our evidence on the staffing decisions of multi-establishments firms provides first evidence along these lines, suggesting that there was a

within-firm reallocation of employment into establishments with better access to foreign workers. In this section, we extend our analysis on this question by examining the extent to which the reform influenced the entry and exit of firms and whether it affected firms' outsourcing decisions. We first turn to the impact on entry and exit. The theoretical framework presented in section 2 provides a motivation for this focus. Under nonzero profits, firms' location choices depend upon the same quantities as those that enter the productivity equation (see [Combes and Gobillon, 2015](#)).

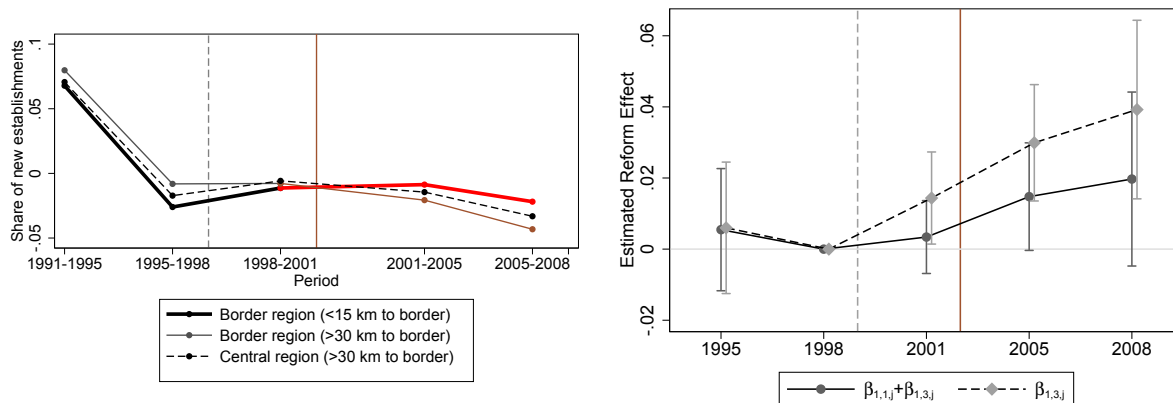
Using the BC data, Panel A of Table 12 estimates the impact of the reform on establishment entry. The estimations are run at the municipality level, cover the period 1991–2008, and include different sets of fixed effects. The outcome variable used is the number of establishments entering a municipality between waves $t - 1$ and t of the BC relative to the number of establishments in the municipality in $t - 1$. An establishment is considered a new entrant if its establishment identifier is new.³⁷ We find strong evidence for a positive reform effect on establishment entry into highly affected regions. Our preferred specification that accounts for municipality fixed effects (column 2) suggests that the reform increased the share of new establishments by 1.4 and 2.8 percentage points respectively, depending upon the control group. The results of this regression are illustrated in Figures 11a and 11b. The figures show that the difference in the treatment effects between the two control groups arises because of an increased entry of establishments in regions close to the BR relative to regions further away from the border in the BR in phase 1 of the reform. Running the regressions on establishment entry for each industry separately, it appears that the better access to CBW mainly promoted the entry of establishments in manufacturing, the construction sector, and in the “other community, social, and personal service activities” sector. These results are reported in columns 4–6 of Panel A of Table 12.

The impacts of the reform on establishment exits are analyzed in Panel A of Table 12. The outcome variable used is the number of establishment exits per municipality between two consecutive waves of the BC relative to the number of establishments in the first of the two periods. The estimations span the period 1991–2011. If at all, they suggest that the reform decreased the probability of establishment exits. The results are, however, somewhat sensitive to the choice of control variables. For instance, the negative effects are wiped out if we add NUTS-II region times period effects to the model (column 3). There is also no systematic evidence for the reform's impact on establishment survival if we restrict the

³⁷There are two reasons why we observe establishments with new establishment identifiers in the data. 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.

Figure 11: Entry of firms

(a) Region-demeaned share of new firms, highly treated vs. control groups
 (b) Event study: share of new firms, highly treated vs. control groups



estimation to those subsamples where the evidence of an effect on firm entry was most pronounced.

Our results generally suggest that the reform led to the entry of new establishments and had a limited impact on exits. Our results also suggest that the entries occurred simultaneously with the increase in the supply of workers. Such concurrency could explain why the absorption of the larger supply of CBW workers had limited negative impacts on resident workers. More generally, our results indicate that immigration policies can affect the composition of firms in local labor markets. This finding implies that the impact of immigration policies on aggregate productivity may go beyond the within-firm effects typically studied in prior papers. In this context, it is worth stressing that our estimations do not allow us to assess the macroeconomic effect of the reform on the number of establishments. The reason is that the entry of establishments to regions close to the border may have partially occurred at the expense of diminished entry into other regions.

Our results on firm creation and on the staffing decisions in multi-establishment firms indicate that greater access to a foreign workforce increases the attractiveness of the affected regions as a production location. These results suggest that the reform may have also affected the outsourcing decisions of firms. Arguably, outsourcing production to foreign workplaces would have been one of the main alternatives for firms if they had not found the workers they searched for. Along these lines, [Ottaviano et al. \(2013\)](#) present a model in which “hiring immigrants or offshoring productive tasks are alternatives that are simultaneously available to producers, and, in fact, may compete with one another or with hiring a native worker” ([Ottaviano et al., 2013](#), p. 1954). Indeed, in a survey among Swiss firms, one third of all

Table 12: The effect of the reform on establishment entry and exit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	OLS	FE	FE	FE	FE	FE	FE
				Manufac- turing	Construc- tion	Social personal services	Relocation of existing establ.
Panel A: Entry							
$R_{m,t}$	-0.013*** (0.004)	-0.014*** (0.004)	-0.010** (0.005)	-0.018*** (0.007)	-0.022** (0.009)	-0.012 (0.010)	-0.000 (0.001)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.021*** (0.006)	0.021*** (0.006)	0.022*** (0.005)	0.022*** (0.008)	0.036*** (0.011)	0.038*** (0.014)	0.002 (0.001)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.028*** (0.008)	0.028*** (0.008)	0.028*** (0.006)	0.042*** (0.011)	0.058*** (0.012)	0.043** (0.019)	-0.001 (0.002)
Observations	12,931	12,931	12,921	11,809	11,706	10,259	12,931
R-squared	0.252	0.371	0.404	0.074	0.035	0.107	0.017
Municipality effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-II-period effects	No	No	Yes	No	No	No	No
$\beta_{1.1} + \beta_{1.2}$	0.007	0.007	0.012**	0.004	0.014	0.027**	0.001
$\beta_{1.1} + \beta_{1.3}$	0.014*	0.014*	0.018***	0.024**	0.036***	0.031*	-0.001
Panel B: Exit							
$R_{m,t}$	0.001 (0.002)	0.001 (0.002)	0.003 (0.002)	0.002 (0.005)	-0.004 (0.004)	-0.001 (0.005)	0.001 (0.001)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	-0.002 (0.002)	-0.002 (0.002)	-0.005** (0.002)	-0.004 (0.005)	0.008 (0.006)	0.008 (0.005)	0.001 (0.001)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	-0.006** (0.002)	-0.006** (0.002)	-0.001 (0.002)	-0.004 (0.006)	0.002 (0.008)	-0.001 (0.006)	0.000 (0.001)
Observations	15,504	15,504	15,492	14,134	14,047	12,260	15,504
R-squared	0.332	0.413	0.438	0.125	0.082	0.088	0.371
Municipality effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-II-period effects	No	No	Yes	No	No	No	No
$\beta_{1.1} + \beta_{1.2}$	-0.001	-0.001	-0.002	-0.002	0.004	0.007	0.001**
$\beta_{1.1} + \beta_{1.3}$	-0.005**	-0.005**	0.001	-0.002	-0.001	-0.002	0.001

Robust standard errors in parentheses

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

Notes: All estimations are run at the municipality level using BC data. The dependent variable in Panel A: share of new establishments (number of establishments in t as a fraction of the number of establishments in $t - 1$). Estimation sample: 1991–2008. The dependent variable in Panel B: share of exiting establishments. Estimation sample: 1991–2011. Observations are weighted using the average number of establishments in the municipality as the weight.

firms would consider offshoring as an option if Switzerland restricted the free movement of persons in the future (BAK, 2013).

We use the IS in 2005 and 2011 to study the effect of the reform on outsourcing. In these surveys, firms were explicitly asked whether they outsourced different tasks (R&D, IT services, the production of final products, the production of intermediate products, and services such as cleaning) in the five years preceding the survey. Unfortunately, the surveys do not differentiate between outsourcing to domestic and foreign workplaces. We merge data from the KOF survey on organizational change conducted in 2000 with the two surveys in 2005 and 2011. The 2000 survey contained the same questions on outsourcing and was conducted on the same universe of firms.

The raw data reveal an interesting pattern. In the organization survey in 2000, 44.3% of the firms in the BR reported that they had outsourced at least one of the aforementioned tasks in the 1995–2000 period. This share declined to 34.7% in the 2000–2005 period. In the CR, however, the share of firms that engaged in outsourcing remained almost stable. The pattern reverses to some extent in the subsequent five-year period (i.e., between the 2000–2005 period and the 2006–2011 period). In particular, in the CR the share of firms that outsourced tasks declined by 10 percentage points—a decline that exceeds the one in firms in the BR by 3.3 percentage points. The timing of the changes in outsourcing propensity overlaps with the timing of the changes in the access to CBW between the BR and CR.

Table 13 uses a dummy variable for whether a firm engaged in outsourcing in the last five years as an outcome variable of a simplified version of our baseline regression model.³⁸ The regression does not interact the reform indicator with the distance to the border. The idea is to see whether the discussed differences in the change of the propensity for outsourcing between firms in the CR and BR are statistically significant. The point estimate is indeed negative and statistically significant in the OLS (column 1) and the FE (column 2) regressions. The latter estimate suggests that the reform reduced the probability that a firm has outsourced tasks in a five-year period by 11 percentage points. In columns 3–7 of the table, we analyze the type of outsourcing that drives this reduction. We observe that it is due to a decreased likelihood that firms outsourced basic tasks (the production of final and intermediate products, and services). We observe no effect on outsourcing of R&D and IT.

Our results on outsourcing have to be interpreted with caution. First, the reform effect on outsourcing does not increase with the distance to the border, as shown in the last column of Table 13. The effect is only observable relative to one of the two control groups of firms that we have used throughout the paper. Second, we lack data to assess whether the BR and CR would display similar trends for the probability to outsource in the absence of the reform. At best, our results are thus indicative of a reform effect on outsourcing.

³⁸Since the outcome is a dummy variable indicating whether firms engaged in outsourcing or not, we estimate linear probability models (LPM) in the table. The results are very similar and even more precise if we estimate them using a fixed effects logit model.

Table 13: The effect of the reform on firms' outsourcing decisions

VARIABLES	(1) LPM All tasks	(2) LPM All tasks	(3) LPM Produc- tion	(4) LPM Inter- mediates	(5) LPM Services	(6) LPM R&D	(7) LPM IT	(8) LPM All tasks
$R_{m,t}$	-0.064*	-0.107**	-0.081***	-0.068**	-0.079**	-0.031	-0.011	-0.128***
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	(0.038)	(0.045)	(0.031)	(0.030)	(0.036)	(0.021)	(0.035)	(0.048)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$								0.057 (0.058)
								0.048 (0.065)
Observations	2,995	2,995	2,995	2,995	2,995	2,995	2,995	2,995
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms		1,840	1,840	1,840	1,840	1,840	1,840	1,840
$\beta_{1.1} + \beta_{1.2}$								-0.071
$\beta_{1.1} + \beta_{1.3}$								-0.080

Cluster-robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether a firm outsourced tasks of the specified complexity within five years before the survey. The sample is from the KOF innovation surveys 2005 and 2011 and the KOF survey on organizational change 2000.

6 Robustness

6.1 Differential trends and unobserved shocks

A major concern with our results is that the effects studied are not caused by the immigration reform but by differential underlying trends in the outcomes across regions. In general, the results from the event studies suggest that this is not a relevant concern. We observe parallel trends in the outcomes prior and—sometimes even more so—posterior to the reform. It is thus not surprising that our results are robust to the inclusion of separate linear time trends for each of the five groups of firms that we are comparing (see Panel A of Table 14) and to the shortening of the estimation window to the 1998–2005 period.³⁹ Moreover, our results are also robust to controlling for separate time effects for the BR and CR (i.e., BR-period FE, Panel B of Table 14). These fixed effects account, among others, for potential differential trends in outcomes between the BR and CR. In fact, this specification only compares firms located within the same region that differ in their commuting distances to the border. Consequently, the specification shows that our results do not depend upon the classification of regions into the BR and CR because they hold if we only exploit a self-generated distance measure.

A further major concern with our results is that they are driven by other unobserved factors that affect differently the outcomes in highly treated firms and the two control

³⁹The shorter window limits concerns that our results are capturing an underlying trend rather than a discrete change (see Table A.9).

groups at the point in time when the reform takes place. Obvious candidates are other simultaneous changes in policies (e.g., changes in taxes) and unobserved industry-specific shocks (e.g., trade shocks) that affect differently regions close to and further away from the border; for example, because of differences in the industry mix. We address the concern that our results are confounded by unobserved third factors in four ways.

First, Panels C–E of Table 14 test whether the results from our preferred FE models are sensitive to controlling for a full set of industry-period (Panel C),⁴⁰ NUTS-II-period (Panel D), and canton-period fixed effects (Panel E). In the latter two specifications, the reform effects are effectively identified from the comparison over time of firms located in the same regional labor market (the NUTS-II region) or in the same canton. The inclusion of these fixed effects has very limited influence on the point estimates of the reform effects, indicating that unobserved regional or industry-specific shocks are not driving our results.

Second, we tested whether our results are driven by unobserved simultaneous trade shocks (e.g., an increased trade with the EU). In Table A.11 in the appendix, we show that our baseline results are not sensitive to absorbing all industry-specific shocks that affected firms’ differentially depending upon their initial export statuses. We also show that our results hold if we directly control for firms’ export shares in sales and control for the effects of fluctuations in the exchange rate.

Third, section F.3 presents the results of DiD regressions of the reform effect on firm size that do not rely on the comparison over time of firms that differ in their commuting distance to the border. In this section, we instead compare establishments that are all located within the most heavily affected regions (i.e., firms within 15 minutes to the border) and, in some specifications, even within the same municipality. Instead, establishments are assigned to a treatment or control group based on whether they employed CBW in 1995—information that is available in the BC in 1995. The approach follows the literature on the firm effects of the H-1B, which typically relies on pre-treatment employment of H-1B workers in order to gauge firms’ exposure to changes in the aggregate number of H-1B workers. The estimated reform effects on firm size using this alternative DiD strategy are both qualitatively and quantitatively in line with our baseline estimates.

In a fourth robustness exercise, we exploit the fact that firms were explicitly asked in the IS of 1996–2011 to evaluate whether 10 different policy-related factors had a substantial negative influence on their innovation efforts. We built 10 dummy variables from the original survey items and use them as outcome variables in our DiD models. The results are presented in Table 15. They indicate that there are, among other changes, no simultaneous changes in

⁴⁰The industry-period dummies are built at the level of the disaggregate sections of the NACE rev. 1.1 industry classification. This has 28 different industries.

Table 14: Robustness to region trends, industry-period effects, and region-period effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FE	FE	FE	FE	FE	FE	FE
VARIABLES	Foreign share	FTE count BC	FTE (ln) IS	Sales	Productivity	Share of HS workers	Patents 0/1
Panel A: Region trends							
$R_{m,t}$	0.005 (0.005)	0.083 (0.180)	-0.001 (0.021)	-0.017 (0.028)	-0.010 (0.028)	0.440 (0.454)	-0.020 (0.017)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	-0.012** (0.005)	-0.005 (0.330)	0.060* (0.034)	0.006 (0.047)	-0.060 (0.045)	0.014 (1.036)	0.039 (0.026)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.016*** (0.005)	0.451 (0.358)	0.104*** (0.034)	0.143*** (0.047)	-0.034 (0.049)	-0.045 (0.860)	0.065** (0.028)
Group-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	-0.006	0.078	0.059**	-0.012	-0.070*	0.454	0.020
$\beta_{1.1} + \beta_{1.3}$	0.021***	0.535*	0.102***	0.126***	-0.043	0.394	0.045*
Panel B: BR period							
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.006* (0.003)	1.232*** (0.388)	0.102*** (0.034)	0.056 (0.038)	-0.055* (0.030)	1.188** (0.489)	0.024 (0.020)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.024*** (0.004)	1.620*** (0.412)	0.086** (0.035)	0.130*** (0.042)	0.030 (0.032)	1.178** (0.509)	0.067*** (0.021)
BR-period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Industry period							
$R_{m,t}$	-0.004 (0.003)	-0.236 (0.201)	-0.004 (0.020)	-0.025 (0.026)	-0.020 (0.024)	-0.011 (0.349)	-0.015 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.001 (0.003)	0.630* (0.324)	0.072*** (0.027)	0.018 (0.033)	-0.059** (0.025)	1.227*** (0.419)	0.011 (0.016)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.018*** (0.003)	0.918*** (0.349)	0.047* (0.028)	0.091** (0.037)	0.025 (0.028)	1.254*** (0.445)	0.058*** (0.018)
Industry-period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	-0.003	0.394	0.067***	-0.007	-0.079***	1.216***	-0.004
$\beta_{1.1} + \beta_{1.3}$	0.014***	0.682**	0.043*	0.066*	0.005	1.243***	0.043**
Panel D: NUTS-II period							
$R_{m,t}$	0.001 (0.004)	-0.160 (0.275)	-0.037 (0.028)	-0.046 (0.031)	-0.009 (0.033)	0.307 (0.537)	0.007 (0.024)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.005* (0.003)	1.117*** (0.337)	0.093*** (0.031)	0.057 (0.036)	-0.053* (0.029)	1.263*** (0.463)	0.007 (0.019)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.017*** (0.004)	1.438*** (0.377)	0.077** (0.033)	0.114*** (0.041)	0.025 (0.032)	1.480*** (0.535)	0.057*** (0.022)
NUTS-II-period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.006**	0.957***	0.056*	0.011	-0.063*	1.570**	0.015
$\beta_{1.1} + \beta_{1.3}$	0.018***	1.279***	0.039	0.068	0.016	1.787***	0.064**
Panel E: Canton period							
$R_{m,t}$	-0.004 (0.004)	0.104 (0.323)	-0.004 (0.031)	-0.016 (0.040)	-0.002 (0.039)	0.246 (0.787)	-0.012 (0.040)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.011*** (0.003)	0.881** (0.385)	0.113*** (0.037)	0.087** (0.043)	-0.066* (0.034)	1.076** (0.528)	0.027 (0.023)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.017*** (0.004)	0.779* (0.455)	0.089* (0.046)	0.099* (0.055)	-0.001 (0.041)	0.622 (0.581)	0.087*** (0.029)
Observations	364,892	491,769	10,429	9,749	8,303	10,036	10,211
Canton-period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.008**	0.985***	0.110***	0.071	-0.068	1.322	0.015
$\beta_{1.1} + \beta_{1.3}$	0.014***	0.883*	0.085*	0.083	-0.003	0.868	0.075*

Cluster-robust standard errors in parentheses

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

Notes: Each panel contains separate regressions of augmented versions of our baseline model, estimated using firm fixed effects and the preferred estimation sample. In Panel A, we control for region-specific trends in the outcome in the five regions that we are comparing; in Panel B, for BR-period effects; in Panel C, for industry-period effects; in Panel D, for NUTS-II-period effects; and in Panel E for canton-period effects. Share of HS workers refers to the employment share of workers with academic degrees.

Table 15: Other simultaneous reforms? The reform and firms’ perceptions of policy-related obstacles to innovation

VARIABLES	(1) Insuff. external funding	(2) Taxes	(3) Techno- logy not accepted	(4) Regula- tion in dom. market	(5) Labor market regulation foreigners	(6) Environm. regula- tion	(7) Const- ruction laws	(8) Ease of copy- ing	(9) Restricted access to EU market	(10) Insuff. public research support
$R_{m,t}$	-0.014 (0.023)	-0.047** (0.020)	0.010 (0.020)	0.024 (0.021)	-0.019 (0.018)	0.002 (0.023)	0.042* (0.025)	-0.030 (0.027)	0.026 (0.023)	-0.008 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.033 (0.022)	0.025 (0.018)	-0.011 (0.020)	0.010 (0.021)	-0.027 (0.018)	0.017 (0.021)	-0.022 (0.024)	0.047* (0.026)	-0.011 (0.022)	0.006 (0.017)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.026 (0.025)	0.020 (0.022)	-0.008 (0.024)	-0.046* (0.024)	-0.040* (0.022)	-0.011 (0.025)	-0.020 (0.027)	0.074** (0.030)	0.007 (0.026)	0.004 (0.023)
Observations	8,544	8,542	8,496	7,686	7,685	7,683	7,684	8,553	7,687	7,137
R-squared	0.011	0.011	0.004	0.020	0.037	0.014	0.026	0.012	0.051	0.013
Number of firms	3,159	3,158	3,155	3,129	3,129	3,129	3,129	3,164	3,128	3,029
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.018	-0.022	-0.000	0.034	-0.046**	0.020	0.020	0.017	0.015	-0.003
$\beta_{1.1} + \beta_{1.3}$	0.011	-0.027	0.002	-0.021	-0.059**	-0.008	0.022	0.044	0.033	-0.004

Cluster-robust standard errors in parentheses

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

Notes: The table shows regressions of our baseline FE using the preferred estimation sample in the IS. The dependent variables are dummies equal to 1 if a firm reports that a specific factor has hampered their innovation activities.

taxes and market access to the EU that affect more heavily those firms that are also more heavily affected by the immigration reform. In fact, only one of the 10 dummy outcome variables covaries systematically with the reform indicator, $R_{m,t}$, across both control groups. It is the probability that firms perceive that their innovation activities are hampered by “labor market regulation for foreigners.” A reduction in this probability is obviously likely to be a direct consequence of the deregulations in the labor market caused by the immigration reform.

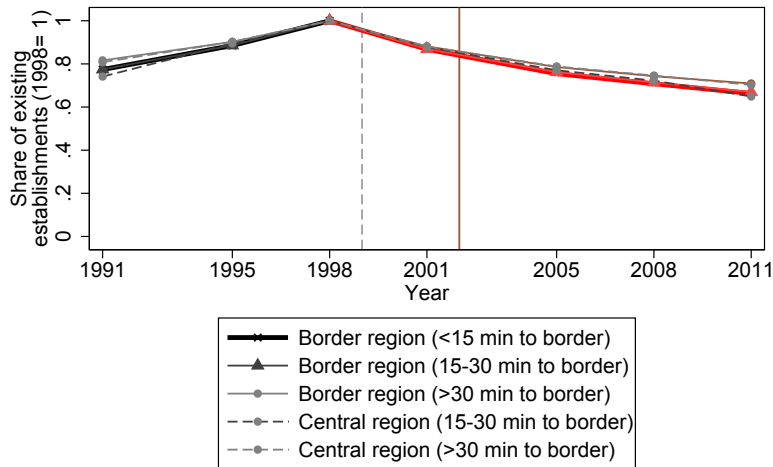
6.2 Selection

This section analyzes the sensitivity of our results to sample selection biases. We start by testing whether sample selection is related to the outcome variables conditional on the covariates. If it is unrelated, sample selection can be ignored. In the estimations using the IS data, we indeed cannot reject the null hypothesis that observations are “missing at random,” suggesting that sample selection is less of a concern (see Table A.13 in the appendix). We reach a different conclusion for the BC, however. Here, sample selection due to firm entry and exit is a concern.⁴¹

Because our primary focus on the cross section of firms in 1998 deals with the issues raised by firm entry, the main concern is that our FE regressions are driven by survivorship bias. This bias would mean that we attribute too much of the reform effect to occur within firms rather than to the change in firms’ composition. There are three pieces of

⁴¹The reasons for these differences may be due to the different sampling units (establishments vs. firms) or because there are more reasons for missing data in the IS than the BC. In the former, a firm is missing not only if it has gone out of business, but also if it was not sampled, did not respond to the survey, or did not respond to an individual survey item.

Figure 12: Entry and exit of establishment universe of 1998 (private sector establishments with at least five FTE workers)



evidence that suggest that we do not overestimate the reform’s within-firm effect in our FE estimations. First, there are only relatively small differences in the survival rate of establishments between the regions that we compare. In order to illustrate this, Figure 12 shows the region-specific share of establishments from the 1998 cross section that are present in each wave of the BC. These shares evolve very similarly in the regions of interest. Second, the evidence in section 5.5.2 suggests that, if at all, the reform had a *positive* impact on firm survival; namely, the treatment-control differences in firm exit rates would have been larger without the reform. If the reform helped firms to stay in business that would otherwise have ceased operating, our FE estimations potentially *underestimate* the within-firm impact of the reform on, for example, firm size. The reason is that treated firms that were induced to stay in business because of the reform are likely to underperform relative to firms that stay in the control groups. Finally, as we show in Table A.12 in the appendix, the estimated reform effects on the size of incumbent firms is positive and statistically different from 0 if we construct lower bounds on the average reform effect using a trimming procedure proposed by Lee (2009). We construct these bounds under the assumption that the reform lowered firm survival in the heavily treated region by the difference in the survival rate between treatment and control groups. This assumption is obviously inconsistent with the evidence presented in section 5.5.2; however, it can be considered because the survival rate is slightly lower among firms in the highly treated region in comparison with firms in the main control regions in the treatment period, as shown by Figure 12.⁴²

⁴²Among establishments in the highly treated region, 70.8% existing in 1998 still existed in 2008. This figure compares with 74.6% and 74.3% in the two control groups.

6.3 Further robustness tests

This section discusses four further robustness checks. First, we checked the extent to which our results may be driven by firms relocating within Switzerland. This may be relevant if, for instance, firms decide to locate in one region at the expense of another such that our regressions cumulate the positive reform impact on one region and the negative impact on the other. However, if we adopt the methodology from Table 12 to test this (column 7, Panel A of Table 12), we do not find evidence that the reform affected the relocation decisions of existing establishments. Our results are also practically unchanged if we assign firms to the location observed in 1998 over the entire sample period (see Table A.14).

Second, we checked whether our results hold if we regress the post-reform outcomes on differences in firms' exposure to the immigration reform while directly controlling for pre-reform outcomes. Such a regression only compares firms with similar pre-reform levels and trends in outcomes. Instead of a common trend assumption, this identification is based on an unconfoundedness assumption conditional on pre-treatment outcomes of firms. The results are remarkably similar to our baseline estimates using this alternative identification strategy (see section F.1 of the appendix).

Third, we changed the way in which we model that a firm's exposure to the reform is a function of its distance to the border. In section F.8, we estimate a very flexible model in which we assign firms to bins of 7.5 minutes to the border. We then estimate separate reform effects for each of these smaller bins. Following this, we estimate more parametric models in which we directly interact the reform indicator, $R_{m,t}$, with the time to the border ($d_{i,t}$) and its square. The estimations confirm all our main results qualitatively. They also show that the effects of the reform dissipate with a growing travel distance and become 0 at around 30 minutes travel distance to the border.

A final robustness check concerns our inference that is based on standard errors clustered at the level of individual establishments and firms. Abstracting from the firm-specific distance to the border, $d_{i,t}$, our regressor of interest, $R_{m,t}$, varies only between the BR and CR. If there is cross-sectional dependence in the errors between firms located in the same region conditional on the covariates, our standard errors may thus be subject to a Moulton (1990) bias and to the criticism by Bertrand et al. (2004) on the inference in many earlier DiD applications. We assessed the robustness of our inference in the IS by computing standard errors clustered at the industry level and at levels reflecting regional labor markets. All our main results are robust to using these other clustering levels (see section F.9). In many cases, standard errors are actually smaller using these alternatives.

7 Conclusion

This study sheds light on the effects of opening the borders to EU workers on immigration and on firm’s employment, sales, productivity, innovation, outsourcing, and location decisions. To this end, we exploit a unique quasi-experimental setting arising from the sequential introduction of the free movement of persons in Switzerland. The immigration reform lifted all prior restrictions on the mobility of EU workers and affected firms located close to the border earlier and more strongly.

Our DiD estimates suggest that the reform increased the employment share of foreign workers in incumbent establishments by at least 2 percentage points. The reform also increased the FTE employment of incumbent establishments. The estimated effects are economically considerable. The FTE employment of heavily affected establishments increased by 3.5–6.5% because of the reform. Sales of heavily affected incumbent firms grew by 8.2–11%. Further, there is only limited evidence that the new immigrants displaced Swiss workers. Although the reform did not specifically target skilled workers, it increased the share of high-skilled workers in firms’ workforces. The effects of the reform on expansion are observable across a wide range of firms. However, the reform appears to have particularly spurred the growth of (i) firms that reported they were constrained by a lack of specialized personnel prior to the reform, (ii) high-tech firms, and (iii) firms that already relied strongly on foreign workers and CBW before the reform. In these groups of firms, the reform also increased average labor productivity.

Our results suggest three mechanisms that contribute to the substantial effect of the reform on incumbent firms. First, the reform appears to have helped certain firms to overcome their prior skill shortages that had constrained their expansion. Second, the reform had a positive effect on the employment share of R&D workers and on the number of firms’ patent applications. This impact on firms’ innovation performance may have spurred firm expansion. Finally, the reform coincided with an economically sizeable decrease in the propensity of firms to outsource production and service tasks. It also promoted the entry of new establishments in the more affected regions and increased the size of heavily affected establishments relative to less affected establishments within the same multi-establishment firm. These results suggest that the reform led to the reallocation of economic activity into the heavily affected regions.

Overall, the results of our study and the complementary results in [Beerli and Peri \(2016\)](#) suggest that the absorption of CBW took place via job creation in incumbent and new firms. This led to no, or only small, negative wage and employment effects of the reform for residents. Moreover, our findings corroborate the claims of business leaders that unrestricted

access to skilled workers is important for the success of firms in a labor market characterized by shortages of skilled workers.

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Appendix

A Characteristics of cross-border workers

Table A.1: Characteristics of workers in border region, by residence status and year

Characteristics		Year			Change
		1996	2000	2010	2000–2010
<i>Tertiary education</i>	CBW	15%	17%	28%	11%
	Immigrants	12%	13%	23%	10%
	Swiss	18%	20%	26%	6%
<i>Secondary education</i>	CBW	47%	53%	49%	-5%
	Immigrants	32%	36%	39%	3%
	Swiss	66%	66%	63%	-3%
<i>Primary education</i>	CBW	38%	30%	23%	-7%
	Immigrants	56%	51%	38%	-13%
	Swiss	16%	14%	11%	-3%
<i>Average Wage (in CHF)</i>	CBW	5116	5567	6665	1098
	Immigrants	4811	5263	6685	1423
	Swiss	5817	6369	7212	843
<i>Average Age</i>	CBW	39.2	40.2	40.5	0.3
	Immigrants	38.0	38.1	39.1	1.0
	Swiss	39.7	40.1	41.6	1.5
<i>Average Tenure (in years)</i>	CBW	9.1	9.3	7.2	-2.1
	Immigrants	7.9	7.4	5.9	-1.5
	Swiss	9.1	9.1	8.4	-0.8
<i>Female</i>	CBW	31%	31%	34%	4%
	Immigrants	34%	35%	41%	6%
	Swiss	43%	42%	46%	5%

Notes: Immigrants are foreign nationals with residency permits B, L or C, granting 1–5 years of (L and B) or unlimited (C) residence in Switzerland

Source: Wage Structure Survey, own calculations

Table A.2: Cross-border workers and immigrants in border and central region, 1996–2010

		Year			Average annual change	
		1996	2000	2010	1996–2000	2000–2010
Border Region						
0–15 min	Cross-border workers	104363	97769	159638	-1649	6187
	(as % of employment)	19.9%	18.9%	24.2%	-1.6%	5.0%
	Immigrants	140884	140981	170378	24	2940
	(as % of employment)	26.9%	27.2%	25.8%	0.0%	1.9%
	Total employment	524238	518251	659824	-1497	14157
					-0.3%	2.4%
15–30 min	Cross-border workers	15488	15501	27662	3	1216
	(as % of employment)	2.3%	2.3%	3.3%	0.0%	6.0%
	Immigrants	180987	178148	226437	-710	4829
	(as % of employment)	27.0%	26.6%	27.1%	-0.4%	2.4%
	Total employment	670828	669460	834426	-342	16497
					-0.1%	2.2%
above 30 min	Cross-border workers	2276	4234	6732	490	250
	(as % of employment)	0.7%	1.4%	1.9%	16.8%	4.7%
	Immigrants	86118	84927	95339	-298	1041
	(as % of employment)	27.9%	28.2%	26.4%	-0.3%	1.2%
	Total employment	309027	301442	361016	-1896	5957
					-0.6%	1.8%
Central Region						
15–30 min	Cross-border workers	15	102	328	22	23
	(as % of employment)	0.3%	1.8%	3.3%	62.1%	12.4%
	Immigrants	2093	1416	2904	-169	149
	(as % of employment)	42.1%	25.3%	29.5%	-9.3%	7.4%
	Total employment	4968	5606	9834	159	423
					3.1%	5.8%
above 30 min	Cross-border workers	276	538	2026	66	149
	(as % of employment)	0.1%	0.1%	0.3%	18.2%	14.2%
	Immigrants	117166	117598	141304	108	2371
	(as % of employment)	22.0%	21.8%	21.9%	0.1%	1.9%
	Total employment	533418	539701	644704	1571	10500
					0.3%	1.8%

Notes: All counts are in full-time equivalents (FTE). Immigrants are foreign nationals with residency permits B, L or C, granting 1–5 years of (L and B) or unlimited (C) residence in Switzerland.

Source: Wage structure surveys, own calculations

Table A.3: Occupations of cross-border workers by year, private sector

	Cross-border workers				Change				Share of CBW in total employment (BR)				
	1996	2000	2010	2010	1996-2000	2000-2010	2000-2010	1996	2000	2010	2010	1996-2000	2000-2010
Manufacturing, processing	33334	30655	42650	42650	-2680	11995	11995	13.6%	13.4%	18.9%	18.9%	-0.3%	5.6%
Construction	17482	15584	19928	19928	-1898	4343	4343	11.4%	10.5%	13.0%	13.0%	-0.9%	2.5%
Machinery	10255	9965	15790	15790	-290	5825	5825	10.1%	9.3%	12.7%	12.7%	-0.8%	3.4%
Restoration, Crafts	650	90	469	469	-559	379	379	11.5%	4.8%	13.9%	13.9%	-6.6%	9.0%
Definition of corporate strategy	1052	1575	3651	3651	522	2076	2076	2.4%	3.3%	6.1%	6.1%	0.9%	2.7%
Accounting and personnel	3212	2863	5829	5829	-349	2965	2965	5.0%	4.6%	7.0%	7.0%	-0.3%	2.4%
Secretarial, office work	3187	3117	5193	5193	-70	2076	2076	4.0%	4.4%	7.0%	7.0%	0.5%	2.5%
Other administrative functions	4041	6014	10520	10520	1972	4507	4507	4.1%	5.4%	8.4%	8.4%	1.3%	3.0%
Logistic and staff duties	2317	2023	5100	5100	-294	3077	3077	8.1%	6.2%	11.4%	11.4%	-1.9%	5.2%
Evaluation, consultancy, certification	1394	2515	7116	7116	1121	4601	4601	2.3%	3.5%	5.5%	5.5%	1.2%	2.0%
Trade in Goods	1027	1188	2843	2843	161	1655	1655	4.0%	5.0%	8.6%	8.6%	1.0%	3.6%
Retail Sales of Goods and Services	6956	7279	12610	12610	323	5332	5332	5.5%	6.1%	7.5%	7.5%	0.6%	1.4%
Research and development	2769	3370	9615	9615	601	6244	6244	13.6%	15.3%	21.5%	21.5%	1.7%	6.3%
Analysis, programming, operating	3594	4361	8197	8197	767	3836	3836	9.9%	10.0%	11.6%	11.6%	0.1%	1.6%
Planning, designing, sketching	4309	3275	6733	6733	-1034	3459	3459	6.9%	6.7%	10.4%	10.4%	-0.2%	3.7%
Transport and communication	8067	7537	9510	9510	-530	1972	1972	10.6%	9.4%	11.0%	11.0%	-1.2%	1.6%
Security, surveillance	233	212	945	945	-21	734	734	4.6%	4.0%	6.7%	6.7%	-0.6%	2.6%
Medical, nursing and social functions	3588	5057	7677	7677	1470	2620	2620	5.9%	7.6%	7.8%	7.8%	1.7%	0.2%
Personal hygiene, dress care	691	1009	1260	1260	318	251	251	7.4%	10.3%	11.1%	11.1%	2.9%	0.8%
City cleaning, public hygiene	922	813	2070	2070	-109	1257	1257	4.0%	4.0%	5.3%	5.3%	0.0%	1.4%
Education	552	704	1959	1959	152	1255	1255	3.2%	4.0%	6.2%	6.2%	0.9%	2.2%
Hotel, catering trade, housework	7807	6966	11058	11058	-841	4092	4092	8.2%	6.6%	9.3%	9.3%	-1.6%	2.8%
Culture, information and recreation	566	468	1379	1379	-98	911	911	4.0%	3.3%	5.4%	5.4%	-0.7%	2.2%
Other	4119	865	1928	1928	-3254	1064	1064	7.9%	2.8%	7.9%	7.9%	-5.1%	5.2%
Total (BR)	122127	117504	194032	194032	-4622	76528	76528	8.1%	7.9%	10.5%	10.5%	-0.2%	2.6%

Source: Wage structure survey.

Table A.4: Cross-border workers residing in Switzerland and abroad

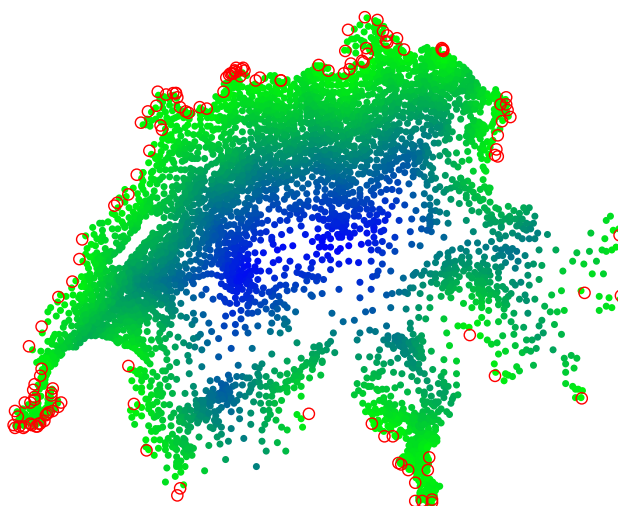
	3-Years Average, in Thousands					Average Annual Change
	1999- 2001	2002- 2004	2005- 2007	2008- 2010	2011- 2013	
Swiss border workers working in Switzerland and living abroad	NA	NA	10	8	15	0.63
Foreign border workers working in Switzerland and living abroad	144	167	188	221	261	7.81
Swiss border workers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

Source: Swiss Federal Statistical Office

B Border vs. central region and border crossings in Switzerland

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 differently classified to previous studies (e.g. [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'Iliez, 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 central region. The results are, however, not sensitive to the differential treatment of these municipalities compared to previous studies.

Figure A.1: The zipcodes of Switzerland and their distance to the nearest border crossing in 2010. (*Source: Henneberger and Ziegler (2011)*)



C 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 virtually 0% throughout most 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 a substantial 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 and Indergand, 2014; Puhani, 2005). 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 comparatively high GDP growth rates, falling unemployment, and very high employment growth relative to previous years. Even the Great Recession of 2007/2008 showed 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's had high employment growth despite solid real wage increases. Siegenthaler et al. (2016) dubbed this phenomenon the Swiss "job miracle".

D The reform effect on average wages

Table A.5: The effect of the reform on wages per FTE worker

VARIABLES	(1) OLS	(2) OLS	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE
$R_{m,t}$	0.049** (0.020)	0.051** (0.023)	0.044** (0.021)	0.045 (0.061)	0.044** (0.022)	0.054** (0.024)	0.058** (0.027)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	-0.030 (0.022)	-0.034 (0.024)	-0.056** (0.022)	-0.097* (0.051)	-0.056** (0.023)	-0.074*** (0.024)	-0.069*** (0.027)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.020 (0.025)	-0.011 (0.029)	-0.009 (0.025)	0.111 (0.075)	-0.029 (0.026)	-0.028 (0.027)	-0.021 (0.029)
$R_{m,t} * CB_{j,prior}$					0.002 (0.028)		
$R_{m,t} * D\{15 < d_{it} \leq 30\} * CB_{j,prior}$					0.003 (0.060)		
$R_{m,t} * D\{0 < d_{it} \leq 15\} * CB_{j,prior}$					0.130* (0.073)		
$R_{m,t} * LS_{i,prior}$						0.022 (0.047)	
$R_{m,t} * D\{15 < d_{it} \leq 30\} * LS_{i,prior}$						0.100 (0.073)	
$R_{m,t} * D\{0 < d_{it} \leq 15\} * LS_{i,prior}$						0.082 (0.099)	
$R_{m,t}^{CR}$							-0.024 (0.029)
$R_{m,t}^{CR} * D\{15 < d_{it} \leq 30\}$							0.216 (0.263)
Observations	13,250	8,846	8,846	8,846	8,845	7,718	8,846
R-squared	0.041	0.049	0.074	0.238	0.076	0.075	0.075
Preferred sample	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	No	No	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	Yes	No	No	No
$\beta_{1.1} + \beta_{1.2}$	0.019	0.017	-0.012	-0.052	-0.013	-0.021	-0.012
$\beta_{1.1} + \beta_{1.3}$	0.068***	0.040	0.035	0.156*	0.015	0.026	0.037
Number of firms			3,198	3,198	3,197	2,626	3,198
$\beta_{1.1}^{CR} + \beta_{1.2}^{CR}$							0.192
$\beta_{1.1} + \beta_{1.2} + \beta_{1.1}^{CR} + \beta_{1.2}^{CR}$							0.181
$\beta_{1.1} + \beta_{1.3} + \beta_{1.1}^{CB} + \beta_{1.3}^{CB}$					0.147**		
p-value $H_0 : \beta_{1.1}^{CB} + \beta_{1.3}^{CB} = 0$					0.05		
$\beta_{1.1} + \beta_{1.3} + \beta_{1.1}^{LS} + \beta_{1.3}^{LS}$						0.130	
p-value $H_0 : \beta_{1.1}^{LS} + \beta_{1.3}^{LS} = 0$						0.23	

Cluster-robust standard errors in parentheses

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

Dependent variable: Wage per FTE worker (ln)

Estimation sample: 1995–2012

Descriptives of dependent variable: Mean: 11.28, S.d.: .4954, Min: 4.624, Max: 17.69

Notes: The dependent variable is the log of the average wage per FTE worker, constructed by dividing the total wage bill by FTE employment. The “preferred sample” is the sample of surveyed firms existing in 1998. Weighted regressions use average FTE employment over the entire sample period as the weight. $LS_{i,prior}$ is a dummy indicating high skill shortage prior to the reform and $CB_{j,prior}$ is an indicator that the firm is in a cross-border intensive industry (see Figure 8 for details).

E Effects on FTE employment of foreign and Swiss workers

Table A.6: The effect of the reform on FTE employment of Swiss workers

VARIABLES	(1) OLS All	(2) OLS CS 1998	(3) OLS CS 1998	(4) FE CS 1998	(5) FE Survivors	(6) FE within firm
$R_{m,t}$	-0.250*** (0.068)	-0.381*** (0.092)	-0.230 (0.268)	-0.289 (0.221)	-0.307 (0.224)	-0.448 (1.504)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.279*** (0.075)	0.613*** (0.107)	0.494 (0.304)	0.447* (0.249)	0.544** (0.254)	1.524 (1.572)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	-0.054 (0.066)	0.228** (0.097)	0.233 (0.276)	-0.067 (0.229)	-0.046 (0.236)	2.013 (1.808)
Observations	1,815,038	1,393,310	366,891	364,892	307,220	71,907
R-squared	0.001	0.001	0.001	0.918	0.930	0.409
Preferred sample	No	No	Yes	Yes	Yes	Yes
Establishment effects	No	No	No	Yes	Yes	No
Period effects	Yes	Yes	Yes	Yes	Yes	No
Firm-period effects	No	No	No	No	No	Yes
$\beta_{1.1} + \beta_{1.2}$	0.029	0.232**	0.264	0.158	0.237	1.076
$\beta_{1.1} + \beta_{1.3}$	-0.304***	-0.154*	0.003	-0.357**	-0.353**	1.564

Cluster-robust standard errors in parentheses

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

Dependent variable: FTE employment of Swiss workers

Estimation sample: 1995–2008

Descriptives of dependent variable: Mean: 6.204, S.d.: 28.29, Min: 0, Max: 4826.

Notes: The regressions use FTE employment of Swiss nationals as outcome. The “preferred sample” is the sample of private sector establishments existing in 1998 with at least 5 FTE workers. Column 5 is restricted to firms existing in all periods from 1998–2008.

Table A.7: The effect of the reform on FTE employment of foreign workers

VARIABLES	(1) OLS All	(2) OLS CS 1998	(3) OLS CS 1998	(4) FE CS 1998	(5) FE Survivors	(6) FE within firm
$R_{m,t}$	-0.088*** (0.032)	-0.012 (0.040)	-0.047 (0.137)	-0.118 (0.122)	-0.086 (0.124)	0.322 (0.674)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.221*** (0.038)	0.361*** (0.056)	0.575*** (0.172)	0.393*** (0.147)	0.399*** (0.152)	0.323 (0.746)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.427*** (0.050)	0.864*** (0.082)	1.730*** (0.254)	1.339*** (0.201)	1.425*** (0.208)	2.633*** (0.918)
Observations	1,815,038	1,393,310	366,891	364,892	307,220	71,907
R-squared	0.003	0.003	0.009	0.890	0.891	0.549
Preferred sample	No	No	Yes	Yes	Yes	Yes
Establishment effects	No	No	No	Yes	Yes	No
Period effects	Yes	Yes	Yes	Yes	Yes	No
Firm-period effects	No	No	No	No	No	Yes
$\beta_{1.1} + \beta_{1.2}$	0.133***	0.349***	0.528***	0.275**	0.312**	0.645
$\beta_{1.1} + \beta_{1.3}$	0.339***	0.852***	1.683***	1.221***	1.339***	2.956***

Cluster-robust standard errors in parentheses

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

Dependent variable: FTE Employment of foreign workers

Estimation sample: 1995–2008

Descriptives of dependent variable: Mean: 2.050, S.d.: 15.51, Min: 0, Max: 3921.

Notes: The regressions use FTE employment of foreign nationals as outcome. The “preferred sample” is the sample of private sector establishments existing in 1998 with at least 5 FTE workers. Column 5 is restricted to firms existing in all periods from 1998–2008.

F Further robustness checks

F.1 Controlling for pre-reform outcomes

Our DiD estimations rely on the assumption of common trends in treatment and control group absent the reform. Because we have panel data, we can instead base the identification on an unconfoundedness assumption conditional on the pre-treatment outcome(s). In particular, we can regress the post-reform outcomes on the firms' exposure to phase 2 of the immigration reform while holding the pre-reform outcomes constant.

To understand how this approach differs from the DiD framework, it is best to consider what would happen if we just included pre-treatment outcomes in our DiD framework. In this case, one would assume a common trend between treated and control group while constraining the pre-treatment levels of the outcome to be identical. This is the same as just ignoring the differencing of DiD and to instead focus on the post-treatment comparison. Such an approach thus requires an unconfoundedness assumption conditional on pre-treatment outcomes. In fact, the unconfoundedness approach and the common trend assumption of the DiD approach are not nested, i.e., only one of them can be true (see [Imbens and Wooldridge, 2009](#); [Lechner, 2013](#), for extended discussions).

Table A.8: The reform effects in 2005 when conditioning on pre-treatment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
VARIABLES	Foreign share 2005	FTE level BC 2005	FTE level BC 2008	FTE log IS 2005	Sales 2005	Produc- tivity 2005	Share of HS 2005	Patents 0/1 2005
<i>BR</i>	0.010*** (0.002)	-0.314 (0.340)	-0.492 (0.407)	-0.020 (0.046)	0.017 (0.048)	0.084 (0.073)	1.669** (0.728)	-0.007 (0.034)
<i>BR</i> * $D\{15 < d_{it} \leq 30\}$	0.005** (0.002)	1.073*** (0.410)	1.387*** (0.465)	0.057 (0.044)	-0.041 (0.045)	-0.032 (0.074)	-0.435 (0.730)	0.005 (0.032)
<i>BR</i> * $D\{0 < d_{it} \leq 15\}$	0.044*** (0.003)	1.058*** (0.406)	1.690*** (0.494)	0.103** (0.045)	0.133** (0.055)	-0.037 (0.084)	-0.311 (0.824)	0.033 (0.035)
Dep. variable (1998)	0.477*** (0.007)	0.695*** (0.192)	0.861*** (0.148)	0.978*** (0.009)	0.985*** (0.010)	0.621*** (0.079)	0.762*** (0.078)	0.469*** (0.043)
Dep. variable (1995)	0.311*** (0.007)	0.277 (0.172)	0.200 (0.131)					
Observations	60,104	60,104	56,624	878	789	572	823	849
R-squared	0.605	0.816	0.789	0.913	0.917	0.328	0.601	0.261
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Past demand assessment	No	No	No	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.015***	0.759*	0.895**	0.037	-0.024	0.053	1.234**	-0.002
$\beta_{1.1} + \beta_{1.3}$	0.054***	0.744*	1.197***	0.083**	0.150***	0.047	1.358**	0.026

Cluster-robust standard errors in parentheses

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

Notes: Each column shows an individual OLS or FE regression on the outcome mentioned in the table header. For instance, the outcome in column 1 is the share of foreign workers in total employment in 2005. Share of HS workers refers to the employment share of workers with academic degrees. The controls for past demand in columns 4–8 are built from a qualitative survey question about firms' demand development in the main product market in the 1996–1998 period, contained in the survey 1999.

Table A.8 shows the results when comparing the post-treatment outcomes conditional on pre-treatment outcomes between the highly treated firms and the control groups. The outcome in column 1 is the employment share of foreign workers in 2005. The specification controls for the lagged dependent variables in 1998 and 1995, i.e., it also accounts for the firm-specific trend in the

outcome in the three-year period prior to the reform. It provides even stronger evidence than our baseline results that the reform increased the share of foreign workers in FTE employment. Using the same specification, the next two columns re-estimate the reform effect on the size of firms. The estimated effects are very similar in size and precision to those estimated using the DiD model. This holds both for the effects on firm size in 2005 (column 2) and 2008 (column 3).

The unconfoundedness approach works less well when using the IS. This is because the panel is highly unbalanced: conditioning on firms that we observe in consecutive surveys reduces the estimation sample substantially. We therefore only condition on the pre-reform outcome in 1998 rather than two pre-treatment outcomes. To control for the pre-1998 trends to some extent, we exploit that the survey in 1999 asked firms to assess how their demand developed in the main product market in the 1996–1998 period. We construct five dummies out of the original 5-level ordinal survey item and include them in the regression. Despite the small sample sizes, the estimated effects are in line with the estimates from our baseline DiD models. In particular, we find positive effects of the reform on log employment, the share of workers with tertiary degree, and sales.

F.2 Shorter estimation window

Table A.9: Baseline estimation using 1998–2005 as estimation period

VARIABLES	(1) FE Foreign share	(2) FE FTE level BC	(3) FE FTE (ln) IS	(4) FE Sales	(5) FE Produc- tivity	(6) FE Share of HS workers	(7) FE Patents 0/1
$R_{m,t}$	-0.004 (0.004)	-0.214 (0.253)	0.001 (0.033)	-0.008 (0.033)	-0.001 (0.048)	1.155** (0.497)	-0.042 (0.028)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.001 (0.004)	0.148 (0.290)	0.055* (0.033)	0.008 (0.031)	-0.051 (0.047)	-0.410 (0.514)	0.033 (0.026)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.022*** (0.004)	0.536* (0.288)	0.071** (0.033)	0.115*** (0.035)	-0.011 (0.053)	-0.514 (0.591)	0.084*** (0.028)
Observations	217,462	217,462	5,587	5,155	4,289	5,386	5,486
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	-0.003	-0.066	0.057**	0.001	-0.052	0.745*	-0.008
$\beta_{1.1} + \beta_{1.3}$	0.018***	0.321	0.073***	0.107***	-0.012	0.642	0.042*
Number of firms			3,134	2,958	2,594	3,065	3,098

Cluster-robust standard errors in parentheses

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

Notes: This table shows estimations of our baseline FE model using the preferred sample of establishments for each outcome mentioned in the column header. The estimation period is restricted to the 1998–2005 period. Share of HS workers refers to the employment share of workers with academic degrees.

F.3 Cross-border employment 1995 as measure of reform exposure

Our main estimations mainly rely on the assumption that there were no unobserved shocks affecting regions close to the border more heavily than regions further away in the relevant period. Rather than gauging the reform effects by comparing firms with different distance to the border, this section presents estimates of the reform effects on FTE employment by comparing firms that employed or did not employ CBW already in 1995. Information on employment of CBW is present

in the BC 1995 (but not in the following two BC). The estimations are restricted to the most heavily affected region (i.e., firms within 15 minutes to the border). A similar approach is also used in several studies that examine the impacts of H1-B visa on firms' success (Ghosh et al., 2014; Kerr and Lincoln, 2010; Kerr et al., 2015; Peri et al., 2015b). In these studies, firm-level exposure to the change in H1-B visa caps is measured through the dependence on H-1B workers prior to changes.

Table A.10 presents the results. The coefficient of interest is an interaction between a dummy variable that is one if a firm employed at least one CBW in 1995 ($CBW_{i,1995}$) and an indicator variable indicating the post-2001 period. The estimation is restricted to the period 1998–2011. Hence, employment of CBW in 1995 is information from a sample not used in the estimation. Column 1 shows that FTE employment of foreign workers increased by about 0.8 FTE more after 2001 in incumbent firms that employed at least one CBW in 1995 compared to firms that did not. This result even holds if we account for municipality-period fixed effects (column 2). In this specification, we only compare firms located in the same municipality at the same time period. Column 3 shows that firms with higher pre-determined exposure to the reform also grew more in general in the period after 2001—a result that is also robust to absorbing municipality-period fixed effects (column 4). In fact, the point estimate is also almost unchanged if we restrict the control group to firms that employed at least one foreign worker in 1995 (column 5) or if we only compare firms within 5 minutes commuting distance to the border (column 6). Columns 7 and 8 show that we do not find evidence that employment of Swiss workers changed differently between firms with and without CBW in 1995. This suggests limited displacement of Swiss workers in this subsample of firms.

Table A.10: Estimated reform effects on FTE employment using firm-level employment of CBW in 1995 to measure exposure to treatment

VARIABLES	(1) FE Foreign	(2) FE Foreign	(3) FE Total	(4) FE Total	(5) FE Total Foreign shr 1995 > 0%	(6) FE FTE ≤ 5 min	(7) FE FTE Swiss	(8) FE FTE Swiss
$CBW_{i,1995}I[\text{year} \geq 2005]$	0.674*** (0.099)	0.583*** (0.094)	0.850*** (0.172)	0.776*** (0.167)	0.913** (0.366)	1.180*** (0.383)	0.116 (0.088)	0.154* (0.087)
Observations	318,387	317,422	414,145	412,936	122,703	127,053	318,387	317,422
Establishment effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-period effects	No	Yes	No	Yes	No	No	No	Yes

Cluster-robust standard errors in parentheses

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

Notes: This table shows estimations of FE models for each outcome mentioned in the column header. The estimation period is 1998–2011 (total employment) and 1998–2008 (foreign and Swiss workers) and the sample is restricted to firms located within 15 minutes to the border or within 5 minutes to the border (column 5). In column 4, the control group only comprises of firms that employ at least one foreign worker in 1995.

F.4 Trade and exchange rate shocks

The panels in Table A.11 contain separate regressions of augmented versions of our baseline model. In Panel A, we include interactions between indicators of the firms' export status (i.e., whether a firm is a non-exporter, exports 0–50% of its sales, or 50–100% of sales) and industry-period effects. These fixed effects absorb all industry-specific shocks that affected firms' differentially depending on their initial export status. Firms are assigned to the exporter categories based on the reported export share in the first period they are observed. Panel B augments our baseline model with a firm's export share in sales, $EXSH_{i,t}$. In Panel C, we study the extent to which our results may be driven by movements in the real exchange rate. We thus include the average real trade-weighted exchange rate R_t into the baseline model. We interact the exchange rate with the indicators of the duration to the border. We also interact the exchange rate with the firm's export share (as it is observed in the first period that the firm is in the data), $EXSH_{i,t_0}$. This interaction captures the firm-specific exposure to exchange rate fluctuations in terms of sales. Finally, we interact this exposure measure with the duration dummies. The trade-weighted real exchange rate is taken from the Swiss National Bank and represents a weighted average of exchange rates taking into account real exchange rates of 24 trade partners of Switzerland.

Table A.11: Robustness to unobserved trade and to exchange rate shocks

VARIABLES	(1) FE FTE (ln) IS	(2) FE Sales	(3) FE Productivity	(4) FE Share of HS workers	(5) FE Patents 0/1
Panel A					
$R_{m,t}$	0.003 (0.020)	-0.034 (0.027)	-0.037 (0.025)	0.124 (0.309)	-0.017 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.062** (0.027)	0.021 (0.033)	-0.050** (0.025)	0.828** (0.377)	0.004 (0.016)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.053* (0.029)	0.108*** (0.038)	0.044 (0.029)	0.775* (0.428)	0.060*** (0.019)
Observations	10,097	9,477	8,091	9,741	9,897
Number of firms	3,326	3,237	2,979	3,305	3,310
Industry x Export cat. x period effects	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.065***	-0.013	-0.087***	0.952**	-0.013
$\beta_{1.1} + \beta_{1.3}$	0.055**	0.073**	0.007	0.899**	0.043**
Panel B					
$R_{m,t}$	-0.005 (0.020)	-0.023 (0.026)	-0.020 (0.025)	0.211 (0.327)	-0.016 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.072*** (0.027)	0.032 (0.033)	-0.052** (0.026)	0.995*** (0.381)	0.012 (0.017)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.065** (0.029)	0.106*** (0.038)	0.030 (0.029)	0.936** (0.413)	0.053*** (0.018)
$EXSH_{i,t}$	0.002*** (0.001)	0.004*** (0.001)	0.001 (0.001)	0.001 (0.011)	0.000 (0.000)
Observations	10,075	9,475	8,130	9,735	9,878
Number of firms	3,407	3,307	3,037	3,378	3,387
$\beta_{1.1} + \beta_{1.2}$	0.067***	0.009	-0.072**	1.206***	-0.004
$\beta_{1.1} + \beta_{1.3}$	0.060**	0.083**	0.011	1.146***	0.037*
Panel C					
$R_{m,t}$	-0.016 (0.020)	-0.037 (0.028)	-0.020 (0.027)	0.178 (0.325)	-0.018 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.080*** (0.028)	0.036 (0.034)	-0.055** (0.027)	0.997** (0.395)	0.009 (0.017)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.077*** (0.029)	0.126*** (0.039)	0.038 (0.030)	0.947** (0.414)	0.056*** (0.019)
$EXSH_{i,t_0} * R_t$	-0.007*** (0.002)	-0.017*** (0.003)	-0.010*** (0.003)	-0.051 (0.048)	-0.001 (0.002)
$EXSH_{i,t_0} * R_t * D\{0 < d_{it} \leq 15\}$	0.005* (0.003)	0.008** (0.004)	0.002 (0.004)	0.102 (0.085)	0.000 (0.002)
$EXSH_{i,t_0} * R_t * D\{15 < d_{it} \leq 30\}$	0.003 (0.003)	0.004 (0.004)	-0.001 (0.004)	0.021 (0.094)	0.000 (0.002)
$R_t * D\{30 < d_{it} \leq 15\}$	-0.004** (0.002)	-0.003 (0.002)	-0.001 (0.002)	-0.028 (0.022)	-0.000 (0.001)
$R_t * D\{15 < d_{it} \leq 30\}$	-0.002** (0.001)	-0.000 (0.002)	0.002 (0.002)	-0.010 (0.014)	0.000 (0.000)
Observations	10,098	9,478	8,091	9,742	9,898
Number of firms	3,327	3,238	2,979	3,306	3,311
$\beta_{1.1} + \beta_{1.2}$	0.063**	-0.001	-0.075**	1.176***	-0.009
$\beta_{1.1} + \beta_{1.3}$	0.061**	0.088**	0.018	1.126***	0.038*

Robust standard errors in parentheses

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

Notes: All panels contain separate regressions of augmented versions of our baseline model. In Panel A, we include interactions between indicators of the firms' export status (i.e., whether a firm is a non-exporter, exports 0–50% of its sales, or 50–100% of sales) and industry-period effects. $EXSH_{i,t}$ is a firm's export share in sales in period t . R_t is the average real trade-weighted exchange rate. The trade-weighted real exchange rate is taken from the Swiss National Bank and represents a weighted average of exchange rates taking into account real exchange rates of 24 trade partners of Switzerland. Share of HS workers refers to the employment share of workers with academic degrees.

F.5 Missing data

Table A.12 applies the trimming procedure of Lee (2009) to our baseline FE model using the preferred sample of establishments. For simplicity, we provide the estimation for both control groups separately and only use the BC in 1998 and 2008, i.e., we examine the reform impact on FTE employment of establishments existing in 1998 that survive until 2008. In the treatment group, 29.2% of the establishments exit, as shown at the bottom of the table. Columns 1–3 present the effects using firms in CR as the control group, columns 4–6 the equivalent effects using firms in BR more than 30 minutes away. Columns 1 and 4 provide the baseline effects without trimming the distribution of the outcome. To compute the lower bound effects in columns 2 and 5, the lower tail of the distributions of FTE employment are trimmed in the two control groups. The trimming proportion is directly related to the difference in the share of nonmissing observations in 2008 between the treatment and the two control groups. As shown at the bottom of the table, the trimming proportion implies that all establishments with less than 2.609 FTE workers (column 2) and 2.291 (column 5) are dropped from the control samples. The upper bound effects are computed by trimming the upper tail of the outcome distribution by the same proportion.

Table A.12: Lower and upper bounds for the size effect of the reform

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Control: CR Baseline	Control: CR Lower bound	Control: CR Upper bound	Control: BR > 30 min Baseline	Control: BR > 30 min Lower bound	Control: BR > 30 min Upper bound
Reform effect (1998 to 2008)	1.550*** (0.393)	0.981** (0.397)	3.368*** (0.352)	2.360*** (0.460)	1.839*** (0.465)	3.583*** (0.388)
Observations	70,138	68,112	68,122	45,928	45,196	45,202
R-squared	0.944	0.945	0.940	0.946	0.946	0.942
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes
Share of exiters treatment		0.292	0.292		0.292	0.292
Share of exiters control		0.254	0.254		0.257	0.257
Trimming quantile		0.051	0.949		0.047	0.953
Trimming value		2.603	75.715		2.291	78.967

Cluster-robust standard errors in parentheses

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

Notes: This table applies the trimming procedure of Lee (2009) to our baseline FE model using the preferred sample of establishments. See main text for details.

Table A.13: Robustness: Are observations missing at random?

VARIABLES	(1) FE Foreign share	(2) FE Foreign share	(3) FTE level BC	(4) FTE level BC	(5) FTE IS	(6) FTE (ln) IS	(7) FE Sales	(8) FE Sales	(9) Productivity FE	(10) Productivity FE	(11) Share of HS workers FE	(12) Share of HS workers FE	(13) Patents FE 0/1	(14) Patents FE 0/1
$R_{m,t}$	-0.006** (0.003)	-0.007** (0.003)	-0.598** (0.297)	-0.934** (0.367)	0.003 (0.019)	-0.007 (0.019)	-0.007 (0.026)	-0.027 (0.026)	-0.010 (0.025)	-0.020 (0.024)	0.142 (0.339)	0.149 (0.323)	-0.010 (0.016)	-0.016 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.006* (0.003)	0.006* (0.003)	0.891*** (0.338)	1.283*** (0.402)	0.066** (0.026)	0.079*** (0.027)	0.036 (0.033)	0.036 (0.033)	-0.066** (0.028)	-0.054** (0.027)	1.001** (0.395)	1.062*** (0.380)	0.010 (0.017)	0.010 (0.017)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.024*** (0.004)	0.025*** (0.004)	1.365*** (0.346)	1.783*** (0.420)	0.058** (0.028)	0.064** (0.029)	0.099*** (0.037)	0.110*** (0.038)	0.021 (0.030)	0.031 (0.028)	1.090** (0.437)	1.216*** (0.419)	0.050*** (0.019)	0.054*** (0.018)
$Missing_{i,t-1}$	-0.020*** (0.003)	-0.017 (0.012)	-2.629*** (0.241)	-4.986*** (0.402)	0.002 (0.009)	0.006 (0.011)	0.014 (0.011)	0.014 (0.015)	0.024** (0.011)	-0.001 (0.011)	0.070 (0.177)	0.312* (0.159)	0.007 (0.007)	-0.001 (0.007)
Observations	364,892	364,892	364,892	491,769	8,933	10,429	8,292	9,749	7,126	8,303	8,549	10,036	8,751	10,211
R-squared	0.852	0.852	0.926	0.903	0.015	0.015	0.064	0.071	0.027	0.027	0.031	0.037	0.009	0.011
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1,1} + \beta_{1,2}$	-0.001	-0.001	0.293	0.349	0.070***	0.072***	0.006	0.009	-0.077***	-0.074***	1.143***	1.210***	-0.001	-0.006
$\beta_{1,1} + \beta_{1,3}$	0.018***	0.018***	0.767***	0.849**	0.062**	0.057**	0.092***	0.083**	0.011	0.011	1.233***	1.365***	0.040**	0.038**
Number of firms					3,337	3,449	3,192	3,342	2,891	3,064	3,287	3,420	3,310	3,428

Cluster-robust standard errors in parentheses

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

Notes: This table examines whether observations are “missing at random” from the sample. This is tested by the means of simple t -tests on two variables added to our preferred specification. The first of the added variables, denoted $missing_{i,t-1}$ is an indicator variable showing whether a specific firm, observed in period t , has been part of the sample in the preceding period or not. The second variable, denoted $\tau_{i,t+1}$, is a variable counting the number of periods after period t that unit i is in the estimation sample. These tests were originally proposed by Verbeek and Nijman (1992) in the context of random effects panel estimation models. As noted by Wooldridge (2010), they also work for the case of fixed effects models. Share of HS workers refers to the employment share of workers with academic degrees.

F.6 Robustness to firm relocation

Table A.14: Assigning firms to location in 1998

VARIABLES	(1) FE Foreign share	(2) FE FTE level BC	(3) FE FTE (ln) IS	(4) FE Sales	(5) FE Produc- tivity	(6) FE Share of HS workers	(7) FE Patents 0/1
$R_{m,t}$	-0.005* (0.003)	-0.382* (0.205)	-0.008 (0.020)	-0.027 (0.026)	-0.019 (0.024)	0.022 (0.350)	-0.016 (0.016)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.006* (0.003)	0.876*** (0.328)	0.072*** (0.028)	0.034 (0.034)	-0.055** (0.026)	1.283*** (0.418)	0.012 (0.016)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.024*** (0.004)	1.261*** (0.357)	0.063** (0.029)	0.108*** (0.038)	0.030 (0.028)	1.267*** (0.441)	0.056*** (0.018)
Observations	364,892	491,769	10,429	9,749	8,303	10,036	10,211
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.000	0.494*	0.064***	0.007	-0.074***	1.305***	-0.003
$\beta_{1.1} + \beta_{1.3}$	0.019***	0.878***	0.055**	0.081**	0.011	1.289***	0.040**
Number of firms			3,449	3,342	3,064	3,420	3,428

Cluster-robust standard errors in parentheses

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

Notes: This table shows estimations of our baseline FE model using the preferred sample of establishments for each outcome mentioned in the column header. Firms are assigned to the municipality observed in 1998 throughout the entire estimation period. Share of HS workers refers to the employment share of workers with academic degrees.

F.7 Results for the cross section of 2001

Table A.15: Results using cross section of firms in 2001

VARIABLES	(1) FE Foreign share	(2) FE FTE level BC	(3) FE FTE (ln) IS	(4) FE Sales	(5) FE Produc- tivity	(6) FE Share of HS workers	(7) FE Patents 0/1
$R_{m,t}$	-0.005 (0.003)	-0.265* (0.145)	-0.017 (0.019)	-0.028 (0.024)	-0.014 (0.024)	-0.022 (0.311)	-0.011 (0.014)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.006* (0.003)	0.700*** (0.252)	0.067*** (0.026)	0.035 (0.031)	-0.028 (0.026)	1.231*** (0.381)	0.010 (0.015)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.024*** (0.004)	1.050*** (0.274)	0.060** (0.027)	0.087** (0.035)	0.031 (0.027)	0.940** (0.409)	0.056*** (0.017)
Observations	486,088	648,342	11,887	11,098	9,539	11,387	11,655
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.001	0.435*	0.050**	0.007	-0.042	1.209***	-0.002
$\beta_{1.1} + \beta_{1.3}$	0.019***	0.785***	0.043*	0.059*	0.017	0.918**	0.044**
Number of firms			3,997	3,865	3,593	3,941	3,979

Cluster-robust standard errors in parentheses

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

Notes: In this table, we re-estimate our baseline FE models using the cross section of firms existing in 2001 instead of those of 1998. Share of HS workers refers to the employment share of workers with academic degrees.

F.8 Different distance specifications

In this section, we evaluate whether our evidence is dependent on the way we treat the fact that the reform impact dissipates with growing distance to a border crossing. To this end, Table A.16 first shows the results of regression in which we directly interact the reform indicator $R_{m,t}$ with the duration to the border $d_{i,t}$ and its square $d_{i,t}^2$. The implied effects at different durations to the border are illustrated in Figure A.2. Figure A.3 in shows the estimated reform effects for all relevant outcomes when allocating firms into eight different distance bins of 7.5 min length. The results show that our main results do not depend on the exact choice of thresholds. They also provide a further motivation for the 30 min threshold. In almost all cases, we fail to identify a statistically significant reform effect on firms located more than 30 min away from the border.

Table A.16: Main results using a model with a quadratic in the travel time to the border

VARIABLES	(1) FE Foreign share	(2) FE FTE level BC	(3) FE FTE (ln) IS	(4) FE Sales	(5) FE Produc- tivity	(6) FE Share of HS workers	(7) FE Patents 0/1
$R_{m,t}$	0.030*** (0.005)	1.241*** (0.434)	0.084*** (0.031)	0.112*** (0.042)	-0.001 (0.035)	1.628*** (0.480)	0.041* (0.022)
$R_{m,t} * d_{i,t}$	-0.002*** (0.000)	-0.039* (0.022)	-0.003** (0.001)	-0.006*** (0.002)	-0.002 (0.001)	-0.026 (0.020)	-0.002** (0.001)
$R_{m,t} * d_{i,t}^2$	0.000*** (0.000)	0.000 (0.000)	0.000** (0.000)	0.000*** (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000** (0.000)
Observations	364,892	491,769	10,429	9,749	8,303	10,036	10,211
R-squared	0.852	0.903	0.013	0.071	0.025	0.036	0.010
Preferred sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of firms			3,449	3,342	3,064	3,420	3,428

Cluster-robust standard errors in parentheses

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

Figure A.2: Estimated reform effects using a model that has a quadratic in the travel time to the border

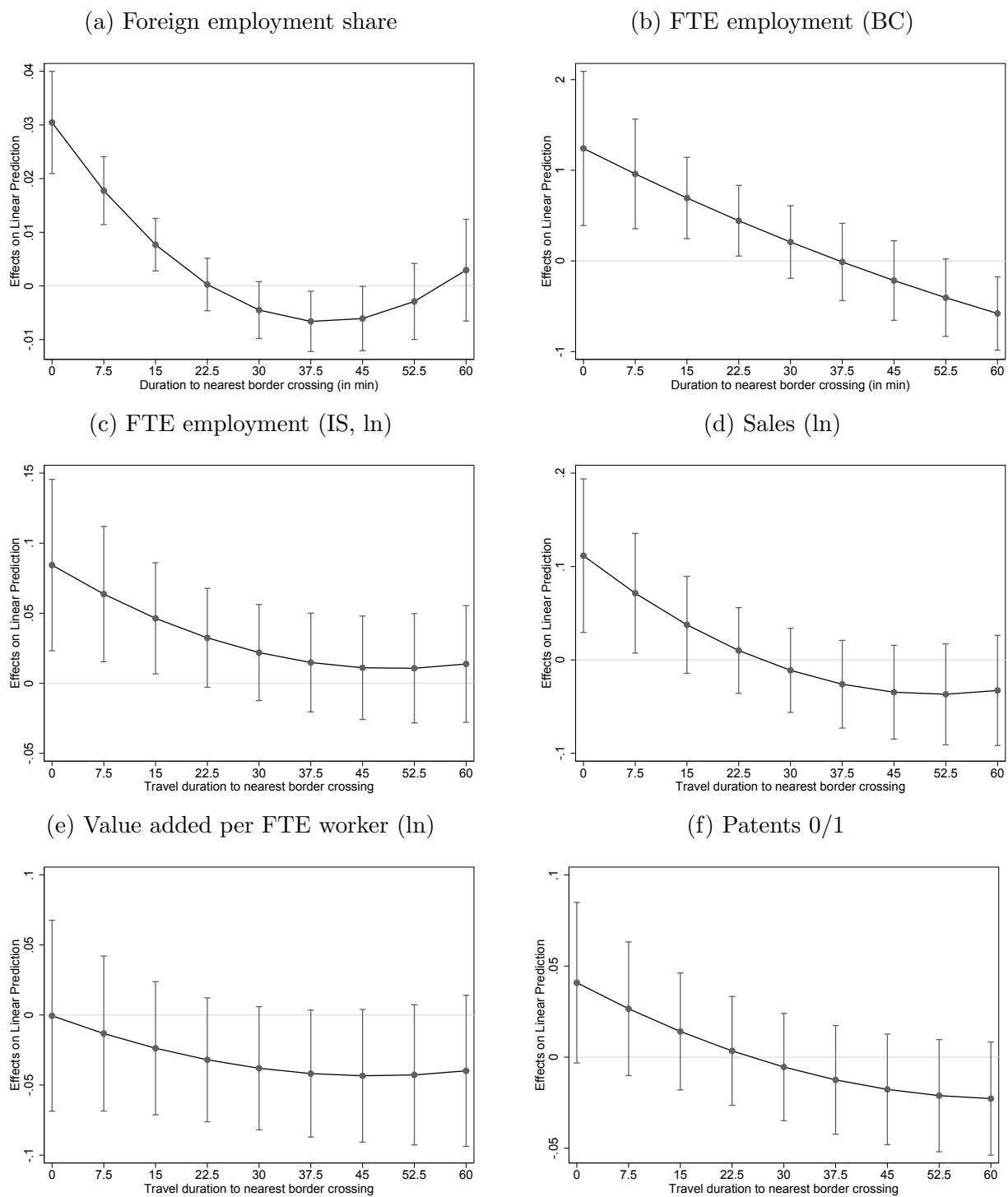
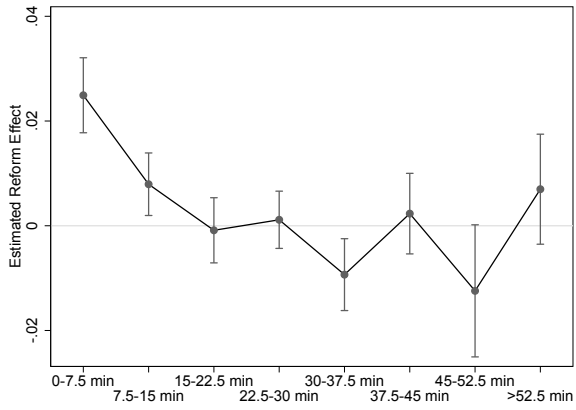
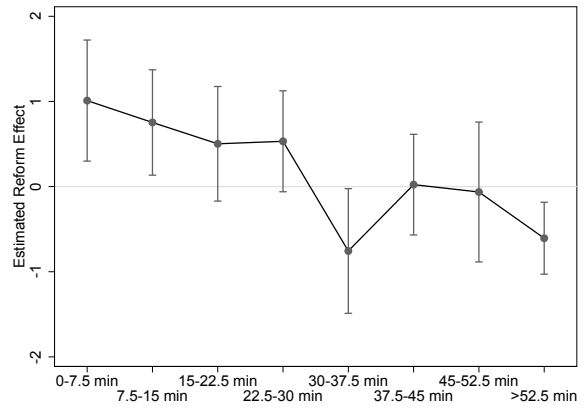


Figure A.3: Estimated reform effects by detailed duration to border

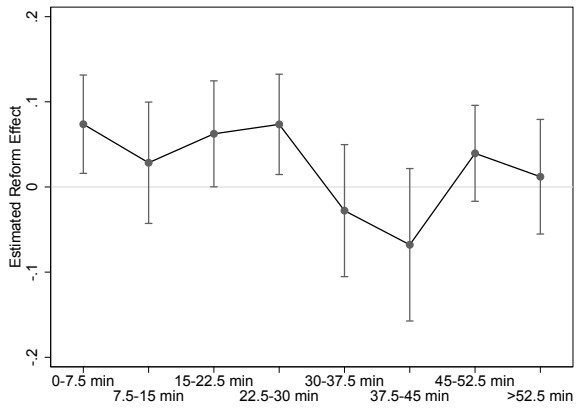
(a) Foreign employment share



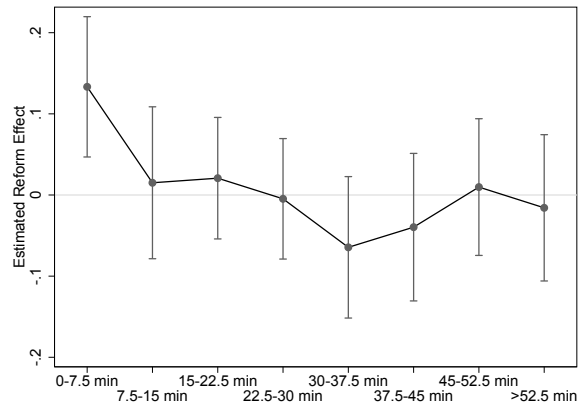
(b) FTE employment (BC)



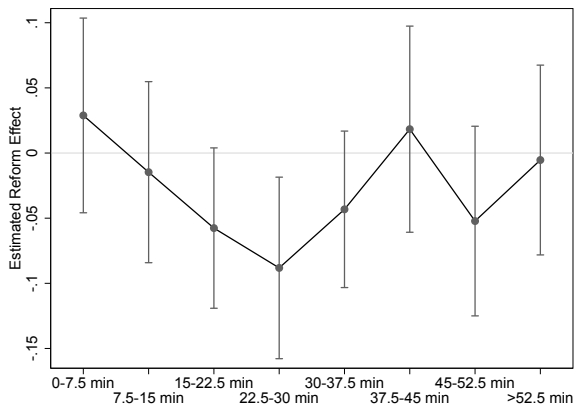
(c) FTE employment (IS, ln)



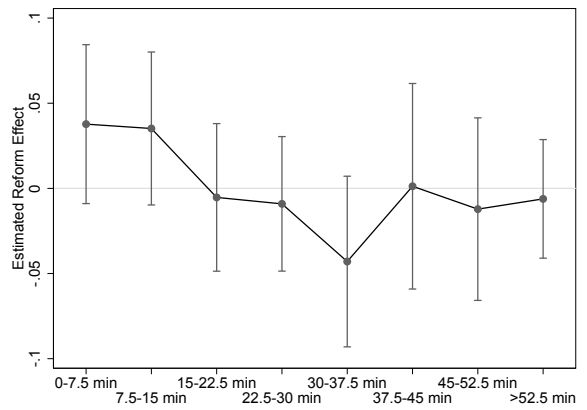
(d) Sales (ln)



(e) Value added per FTE worker (ln)



(f) Patents 0/1



F.9 Higher levels of clustering

Table A.17: Main results for IS when clustering standard errors on higher levels

VARIABLES	(1) FE FTE (ln) IS	(2) FE Sales	(3) FE Productivity	(4) FE Share of HS workers	(5) FE Patents 0/1
Panel A					
$R_{m,t}$	-0.007 (0.021)	-0.027 (0.026)	-0.020 (0.018)	0.150 (0.298)	-0.016 (0.014)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.079** (0.030)	0.035 (0.029)	-0.054* (0.027)	1.053* (0.526)	0.010 (0.015)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.063** (0.023)	0.110*** (0.028)	0.031 (0.029)	1.200*** (0.378)	0.054*** (0.019)
Observations	10,429	9,749	8,303	10,036	10,211
Number of firms	3,449	3,342	3,064	3,420	3,428
Canton-level clustering	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.072**	0.008	-0.074**	1.203***	-0.006
$\beta_{1.1} + \beta_{1.3}$	0.056**	0.082**	0.011	1.350***	0.038**
Panel B					
$R_{m,t}$	-0.007 (0.026)	-0.027 (0.022)	-0.020** (0.009)	0.150 (0.291)	-0.016 (0.014)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.079** (0.033)	0.035 (0.036)	-0.054*** (0.015)	1.053* (0.555)	0.010 (0.019)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.063*** (0.020)	0.110*** (0.031)	0.031 (0.024)	1.200** (0.490)	0.054** (0.024)
Observations	10,427	9,747	8,301	10,034	10,209
Number of firms	3,447	3,340	3,062	3,418	3,426
NUTS-II x BR clustering	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.072*	0.008	-0.074***	1.203***	-0.006
$\beta_{1.1} + \beta_{1.3}$	0.056**	0.082**	0.011	1.350**	0.038**
Panel C					
$R_{m,t}$	-0.007 (0.021)	-0.027 (0.040)	-0.020 (0.031)	0.150 (0.278)	-0.016 (0.017)
$R_{m,t} * D\{15 < d_{it} \leq 30\}$	0.079** (0.030)	0.035 (0.042)	-0.054** (0.023)	1.053* (0.599)	0.010 (0.014)
$R_{m,t} * D\{0 < d_{it} \leq 15\}$	0.063** (0.028)	0.110** (0.045)	0.031 (0.034)	1.200** (0.471)	0.054*** (0.018)
Observations	10,428	9,748	8,303	10,035	10,210
Number of firms	3,448	3,341	3,064	3,419	3,427
Industry-level clustering	Yes	Yes	Yes	Yes	Yes
$\beta_{1.1} + \beta_{1.2}$	0.072**	0.008	-0.074*	1.203*	-0.006
$\beta_{1.1} + \beta_{1.3}$	0.056**	0.082**	0.011	1.350***	0.038**

Robust standard errors in parentheses

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

Notes: All panels contain separate regressions of our baseline models. In Panel A, standard errors are clustered on the level of cantons. In Panel B, we cluster on the level of the seven large regional labor markets in Switzerland (NUTS-II regions), treating them as two regions if they contain both BR and CR. In Panel C, standard errors are clustered on the level of industries (NACE rev. 1.1, disaggregate sections). Share of HS workers refers to the employment share of workers with academic degrees.