Firm- and individual-level responses to labor immigration*

Cristina Bratu†
Uppsala University

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Abstract

Labor immigration is an important tool that countries can use to address labor shortages. The design of labor immigration policies may affect flows and the composition of immigrant workers, which can in turn have an effect on firms and workers in the host country. I quantify such effects by studying a major Swedish reform that made it significantly easier for firms to recruit non-Europeans. Using a difference-in-differences setup, I exploit variation in the strictness of immigration rules which affected industries differentially before and after the reform. Treated industries are predominantly lower-skilled, and concentrated in sectors like hotels and restaurants and retail trade sectors. Using linked employer-employee data, I study the effect of the reform on both firm-level and individual-level outcomes. I find that the mean earnings at firms in treated industries unambiguously increase. Firms also seem to take advantage of skill complementarities between natives and immigrants and intensify their overall hiring of high-skilled workers. Moreover, I follow native incumbents' employment and earnings over time and find heterogeneous effects along the skill and age dimensions.

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†cristina.bratu@nek.uu.se

1 Introduction

How should countries regulate labor immigration? Labor immigration policies around the world lie on a spectrum between being entirely immigrant-driven and entirely employer-driven.[1] In immigrant-driven systems, governments select individuals based on certain characteristics, such as education, age and experience, without imposing the condition for the applicant to already have a job. An example would be a points-based system, such as the one practiced in Canada. Employer-driven systems, whereby it is the firm that initiates recruitment, require individuals to have a job offer in hand before they are able to apply for a permit. Most countries impose a so-called labor market test that specifies the kind of occupations that are available for foreign recruitment, the necessary qualifications an individual needs to have before being eligible, or the salary threshold that needs to be reached in order to be granted a permit.[2] The design of such policies influences the flows and composition of workers that are recruited.[3] In turn, these are likely to have effects on the host economy. This paper aims to quantify the effects on domestic firms and individuals of a change in the Swedish system that removed all previous restrictions and made it entirely employer-driven.[4]

I study the effect of non-European labor immigration on firm- and individual-level outcomes in Sweden. I exploit variation in the strictness of the immigration system induced by a reform that differentially lifted restrictions across industries.[5] Immigration policy tends to move slowly, changes are often small and take the form of tweaks to an existing system. Instead, the reform studied in this paper meant that Sweden went from having one of the most restrictive labor immigration system to one of the most liberal in the OECD (OECD 2011). The group of treated industries is the group for whom restrictions were loosened the most. They are predominantly lower-skilled, in sectors such as hotels and restaurants, retail and construction. Therefore my results touch on the effects of low-skilled immigration. I compare the two groups using a difference-in-differences (DiD) design. I find that employees in firms in treated industries earn more on average. Treated firms hire more at both ends of the skill distribution, but hire more intensely at the upper end relative to the pre-reform period. Thus, firms seem to take advantage of skill complementarities between immigrants and natives. I further follow native incumbents' employment and earnings over time, allowing them to move across firms. This analysis yields estimates that are small in magnitude and weaker in terms of precision.

The shift from a highly restrictive system to a highly liberal one makes Sweden an ideal

[1] In the European Union, these refer to policies regulating immigration of third country nationals, referred to from now on as non-Europeans (non-EU).

[2] For example, the H1-B visa system in the US requires applicants to have at least a bachelor's degree in order to be eligible.

[3] See Czaika and Parsons (2017) for evidence on how different immigration systems affect the skill composition of workers.

[4] Before the change, the system was still employer-driven but many government actors were involved in the permit approval decision process.

[5] Industries are understood here as economic activities.

laboratory to study both the effect of immigration policies on the behavior of the main actors (here, the firms) and the subsequent effect of newcomers on firm and individual outcomes. The policy change took place in December 2008. As a result of the reform, firms were free to decide how many and what kind of non-European workers they needed to recruit from abroad.[6] Before the reform, firms that wanted to recruit in occupations that were not considered to be in shortage by the Public Employment Service had low chances of getting a permit approved. The reform eliminated the shortage criterion from the decision-making process. This gives rise to a situation where certain industries experience a more significant change to the policy environment, whereas for others the change is marginal. I use this variation to evaluate the effect of immigration on firm- and individual-level outcomes in a DiD setting.

In the first part of the analysis, I take the perspective of the firm. Firms became major players after the 2008 reform so it is natural to ask how they changed their hiring behavior in its aftermath. I use a matched employer-employee dataset that allows me to follow firms over time and study their hiring and separation patterns, size and average earnings. I find strong evidence that treated firms offer higher average earnings and weaker evidence that they grow. The fact that I can link employees to firms allows me to look at skill-specific hiring and separation and to study skill composition at the firm over time. I find that the firm hires more high-skilled workers relative to the pre-reform period, which results in a higher share of high-skilled at the firm. Since the newcomers go to predominantly low-skilled industries, these results are consistent with a skill complementarity framework.

I next follow the native individuals employed at these firms and look at their outcomes over time. Firms and individuals solve different optimization problems, so it may well be that what is beneficial to the firm is not necessarily beneficial to the individual. Using a similar design as for the firm-level analysis, I find that, on average, low-skilled individuals in treated industries are more likely to stay at the firms they are matched to in 2008 and also earn more (although results are imprecisely estimated). However, the average result masks heterogeneous effects along the age dimension. I find that young (below 40) low-skilled natives are less likely to be employed and may also earn less. I show the opposite holds for the group of older low-skilled, which suggests a higher degree of complementarity between older low-skilled natives and younger low-skilled immigrants. Surprisingly, non-European immigrants that had already been in Sweden at the time of the reform seem to benefit the most.[7] Previous literature tends to find the opposite (Card 2001, Ottaviano and Peri 2012).

The use of matched employer-employee datasets in the immigration literature is still relatively rare. Furthermore, unlike this paper, most studies that take the perspective of the firm focus on the effect of high-skilled immigration. There are two reasons for that: i) most studies

[6]The only two requirements placed on them was to advertise the vacancy for at least ten days at the Public Employment Service and to pay prevailing wages.

[7]A large share of this group is composed of older immigrants arriving from the Former Yugoslavia, Bosnia, Iraq, Turkey and Iran. Immigrants from these countries of origin have to a large extent been in Sweden for a long time.

use US data and examine workers on H1-B visas who are by definition high-skilled (Doran et al. 2014, Kerr et al. 2015)[8] and ii) the immigration inflows during the study period happen to be predominantly high-skilled (Mitaritonna et al. 2017).[9] One exception is Malchow-Møller et al. (2012) who look at the effect of low-skilled immigration on native wages at the firm in the Danish context. They find negative effects on native wages at the firm, particularly the low-skilled. They argue that the most likely mechanism is through a loss in bargaining power: if the newly-hired immigrants have worse outside options, the firm signals that it has an alternative pool of potentially cheaper workers to hire from, thereby reducing the bargaining power of incumbents. The effect is likely to be accentuated when the degree of substitutability between immigrants and natives is high. My work is complementary to this study since I also look at employment outcomes, hiring and separation rates, which paints a more complete picture of the different ways in which firms adjust to an increase in the potential pool of labor supply.

The variation I exploit allows me to study firms across different sectors. Previous studies often restrict their analysis to one sector (e.g. Bratsberg and Raaum 2012 look at construction workers, Mitaritonna et al. 2017 study manufacturing firms). I also make no significant firm size restrictions, whereas previous studies focus exclusively on large firms (e.g. Kerr et al. 2015).[10] My sample is primarily made up of small firms, with at most 50 employees, which is a more realistic reflection of the size distribution of firms in the economy. Small firms hire fewer immigrants per firm, but collectively they hire around the same amount as bigger firms. In Sweden, small firms also have the largest number of vacancies so they are potentially the most likely to benefit from a liberalized immigration system (IOM 2012).

The individual-level findings resonate with previous findings from the few existing studies that use longitudinal data. In the Danish context, Foged and Peri (2016) exploit a refugee dispersal policy to show that an increase in the number of predominantly low-skilled refugee-country immigrants pushed less educated native workers to change occupations towards occupations with less manual content. This result is especially salient when allowing individuals to move across establishments and municipalities. This implies that occupational adjustments

[8]This literature finds mixed results. Doran et al. (2014) find that new H1-Bs crowd out natives with similar observable levels of innovation and that employment at the firm at most modestly increases, with some evidence for at least partial crowding-out of other foreigners. However, they conclude that their results are not incompatible with the possibility that in the aggregate, H1-Bs might still lead to greater innovation and employment, since the crowded-out workers can move to firms where they can increase innovation. Kerr et al. (2015) study the effect of young skilled immigration on the hiring and separation patterns of firms. They find that the share of skilled workers at the firm increases with the share of young skilled immigrants at the firm, with the result being driven by an increase in the share of young skilled natives at the firm to the detriment of the share of older skilled natives. They interpret their results as evidence for a higher degree of complementarity between young skilled natives and young skilled immigrants.

[9]Mitaritonna et al. (2017) study French manufacturing firms that increase their hiring of foreigners following an increase in the (mostly high-skilled) labor supply of foreigners in the local labor market. They find that average wages of natives increase at the firm. They also show evidence for increased mobility of high-skilled natives, who move to firms that hire fewer immigrants.

[10]The only restriction I impose is for firms to have at least 2 employees in 2008.

are accompanied by movements across firms. They further find similar wage effects regardless of whether they allow individuals to move across firms. They thus conclude that the native unskilled that stay at the firm are those that are the most complementary to the newcomers in terms of task specialization. Although I cannot test the hypothesis that those that stay at the firm start performing different tasks than the newcomers, I do find that the low-skilled are more likely to stay at the firm and also earn more, which would be consistent with the same mechanism operating in this case. Bratsberg and Raaum (2012) follow Norwegian individuals in the construction sector over time. They exploit the fact that certain industries within the construction sector have licensing requirements and others do not, thereby generating exogenous variation in the inflow of immigrants across industries in the sector. They find that the wages of the highly-exposed natives are reduced and conclude that at least in the construction sector, immigrants and natives are close to perfect substitutes. In the sectors I study - predominantly services - I find (weak) evidence for a decrease in earnings only for the group of young low-skilled.

This is also the first paper that examines the effect of non-European labor immigration in Sweden.[11] With respect to the policy itself, I show that even in the presence of loose labor immigration restrictions, in absolute numbers, there are few firms in the private sector that take the opportunity. The main reason is that many occupations that are currently classified as being in shortage by the Public Employment Service are concentrated in the public sector, which I exclude from my analysis. However, those also tend to be occupations that require Swedish licenses (doctors, nurses, teachers).

Labor immigration at both ends of the skill distribution is an important tool that countries can use to address labor shortages due to increased population aging and age dependency ratios. Moreover, Sweden in particular has stopped giving permanent permits to refugees since 2015, and since family reunification is only granted to refugees with permanent permits, both of these channels are expected to contribute less to the total flows in the future. Hence, non-European labor immigration is likely to become an even more important channel in years to come. Understanding how firms and natives react is therefore crucial for the optimal design of labor immigration policy.

This paper is structured as follows: the next section gives the institutional background; section 3 lays down the empirical strategy; section 4 presents the firm-level results; section 5 describes the individual-level results. Finally, section 6 summarizes and concludes.

[11]Several reports (OECD 2011, Emilsson et al. 2014) provide descriptive-level evidence of the impact of the reform.

2 Institutional background

2.1 Pre-reform period

Non-Nordic labor immigration reached its peak in Sweden in the mid-1960s due to severe labor shortages that accompanied its post-war industrial boom.[12] Up until that point, workers entered either via bilateral agreements (1940s and 1950s) or as tourists (early 1960s). Tourists were allowed to search for jobs during the three months the visa lasted; if successful, they could switch to a work permit without leaving Sweden (Emilsson et al. 2014). Starting in 1966, however, the rules became more restrictive. Non-Nordic migrants were now required to obtain permits and housing before entering Sweden. Large flows continued to arrive despite the change until 1972, when the Swedish Trade Union Confederation (LO) sent out a circular to its unions asking them to reject applications for non-Nordic work permits (OECD 2011).

As a result, non-Nordic labor immigration continued to decrease until Sweden joined the EEA in 1994 and the EU in 1995. Afterwards, the highly restrictive rules applied only to non-European immigrants.

2.2 The December 2008 reform

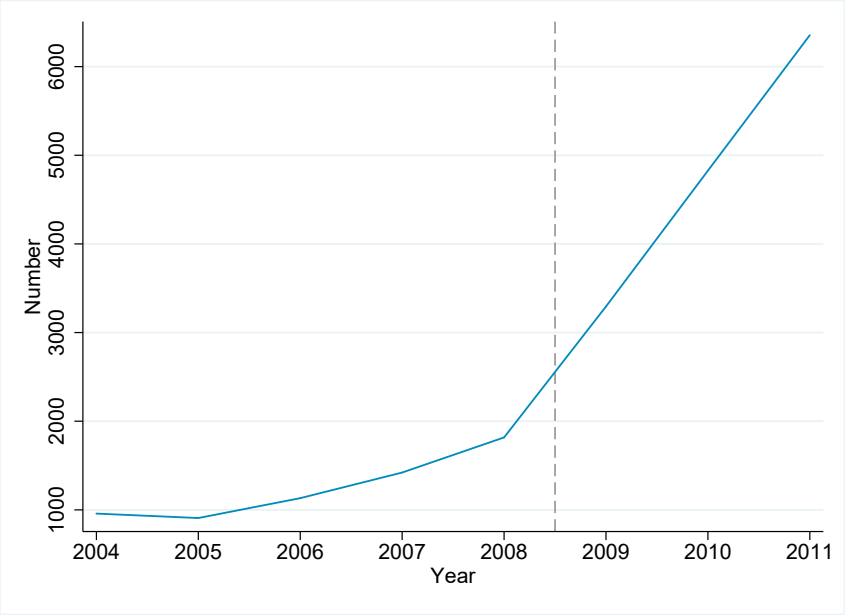
The system next went through a major change in December 2008, when the reform I study in this paper was introduced. Up until that point, employer organizations were demanding looser immigration restrictions because they could not find the right set of skills in Sweden. At the same time, unions were worried primarily about the effect of low-skilled immigration on domestic workers and they were resisting any change to the system. An investigation revealed severe shortages in certain sectors and regions and those that drafted the report advocated for a more liberal system (Ministry of Justice 2006). Two factors in particular helped pass the proposal: i) the fact that the 2004 and 2007 opening of the European borders did not result in a significant increase in flows that might have helped bridge the gap in skills that by now was acknowledged to exist and ii) the 2006 election whereby a center-right government came to power. The change in government gave a stronger voice to employer organizations to the detriment of trade unions (Cerna 2009). The reform made the system purely employer-driven and reduced the role of unions, who, up until that point, had a *de facto* veto on which permits were approved and which were rejected (OECD 2011). I expand on this point later in this section. The main requirements for eligibility on the part of the employer are to show that they are able to pay prevailing wages and that they advertised the vacancy for at least ten days at the Public Employment Service (PES).[13] The firm does not have to show proof of interviewing other candidates who respond to the job ad prior to recruiting a non-EU worker.

[12] With the 1954 Nordic Agreement, Nordic citizens were free to move across the Nordic countries without the need for residence and/or work permits.

[13] Prevailing wages are either in accordance with collective agreements or prevailing practice in the industry.

There are no skill requirements and no quotas. Figure 1 shows a significant increase in the number of work permits valid for at least a year granted before and after the reform.

Figure 1: Number of non-EU individuals on work permits



Notes: The figure plots the number of non-EU individuals on work permits that are valid for at least 12 months. Source: Statistics Sweden

In order to obtain a work permit, a non-European citizen needs to hold a job offer from a firm in Sweden.[14] Before the new rules came into place, in order for a work permit to be approved, the Swedish Migration Board was consulting a shortage list, drawn by the Public Employment Service twice a year and that identifies occupations for which there is a shortage of suitable job-seekers. The higher the score, the higher the shortage, and therefore the higher the probability of getting a permit approved. Each occupation gets a score from 1 to 5, where 5 means there is very low competition for jobs within that occupation.[15] The PES assesses the situation in the labor market by running an employer survey, where they ask around 12000 private firms about their recruitment needs, and by discussing with municipal authorities and county councils. The final score is a qualitative assessment of the information coming from these various sources.[16]

[14]If granted, a work permit is given for an initial period of maximum two years. During this time, the employee is tied to both the employer and the occupation for which the permit is given. If the employee wants to change employers regardless, they have to apply for a new permit, which implies that they need to have a new job at the time of application. If a worker loses their job, they have three months to find a new one. If by the end of this period they haven't found a new job, they have to leave Sweden. When the first permit expires, it is possible to apply for an extension of the permit, for an additional two years maximum. During this time, the worker is free to switch employers but not occupations. After four years, the worker can apply for permanent residence (OECD 2011).

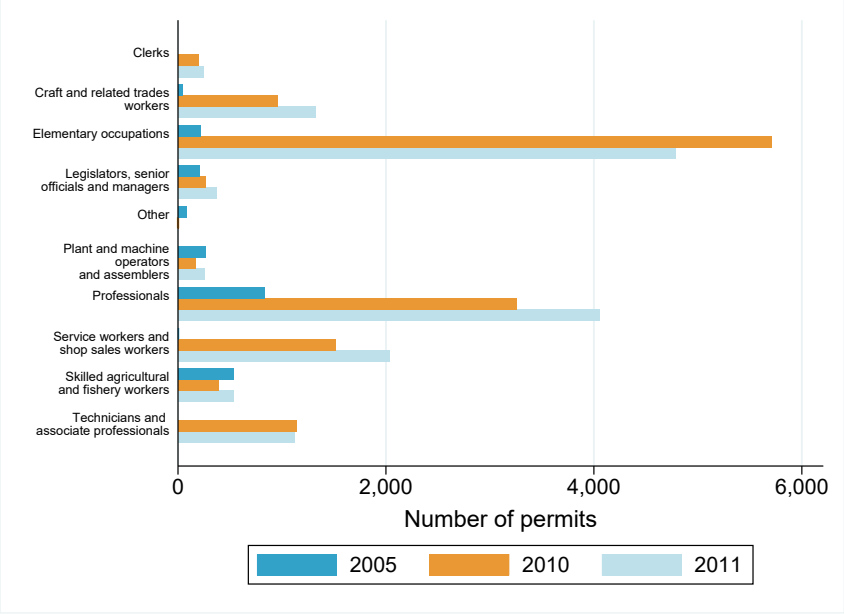
[15]Each regional PES office gives a score for each occupation that is relevant in their local labor market. The final score is a weighted average of all the regional scores.

[16]Author's communication with a PES employee.

The shortage list stopped being used as a criterion in judging work permit applications after December 2008 (IOM 2012). For firms that wanted to recruit in the pre-reform period in occupations high on the shortage list, the odds would have been high already in the pre-reform period. For firms that instead wanted to recruit in occupations lower on the shortage list, the chances would have been rather small. The December 2008 reform therefore removed institutional restrictions to a larger extent for firms that employ workers in occupations that the PES considers low in shortage. Regardless of the self-assessed need of the firm, a firm wanting to employ, for example, a cleaner, would have had a hard time doing so before the reform. That changed after the reform.

Figure 2 shows the number of permits valid for at least three months by occupation group.[17] There are two important things to note here: one, the number of permits granted in 2005 is much smaller than in 2010 and 2011 (even when we account for the fact that the data for 2005 cover only half a year), and two, the occupational distribution is skewed towards professionals in the pre-reform period. Whereas this group makes up a significant portion of workers in the post-period, the number of workers in elementary occupations is significantly larger.

Figure 2: Number of permits by occupation



Notes: The 2005 data only covers the months from January to July. Sources: Migration Board and Public Employment Service

In the aftermath of the reform, the fact that indeed workers started being recruited in occupations that the PES considers in low shortage, started to make headlines. Two main complaints were put forward soon after the reform was put in place. One, that the lack of

[17] The information for the post-reform period is of higher quality; for the pre-reform period, the only year for which this information is available is 2005; the data cover the months from January to July.

oversight left room for abuses of the system, and two, that there were too many workers in low shortage occupations and that unemployed domestic workers were cut out from access to these jobs as a result.[18] This debate led to an adjustment to the reform in 2012. Starting in January 2012, firms in certain industries have to go through additional hurdles in the application process.[19] For this reason my sample period ends in 2011.

Relative to all immigrants that come to Sweden for work purposes, the non-EU immigration channel has become more important in recent years. In 2008, non-EU workers represented 30% of all workers who are registered in Sweden for at least a year.[20] By 2011, that number almost doubled, to 57%.[21] These numbers represent a lower bound, since they only concern individuals who register in Sweden for at least one year.

3 Empirical strategy

3.1 Definition of treatment

As highlighted in the previous section, the reform I study meant a removal of all restrictions in place before with respect to recruiting foreign workers. For identification of the reform effect, I use the fact that the pre-reform restrictions were not affecting firms equally. Hence their removal gave rise to variation in the possibility to hire from abroad.

I use the 2009 report "Where are the jobs" (Public Employment Service 2009) to obtain the list of occupations and their respective shortage scores for 2008. Given that the reform took place in December 2008, 2008 is the last pre-reform year and 2009 the first post-reform year. I match the scores by occupation name to occupational codes (SSYK 1996).[22]

I first create a mapping between occupations and industries, which in turn allows me to create the index at the industry level.[23] I use information from the 2008 wage register (*Lönestrukturstatistik*) which covers a sample of private firms and the individuals matched to them. For each individual, I know their occupational and industry (SNI) codes. I calculate the index for each industry i using the following formula:

[18] However, recent evidence shows that reducing low-skilled immigration does not necessarily result in an increase in native employment (Clemens et al. 2018, Lee et al. 2017).

[19] The 5-digit-level industries concerned are primarily in the hotels and restaurants sector, construction, and retail trade. They now have to show evidence that they can provide a salary to the person they are planning on bringing for at least three months. Firms that have previously employed non-EU workers have to additionally provide the last three monthly tax account statements. Firms operating for less than a year are subject to the rules regardless of the industry they were operating in. Firms with more than 50 employees, however, are exempt from the rules (Migrationsverket 2018).

[20] Author's calculations based on data from Statistics Sweden. All workers include EU workers, including the self-employed, and non-EU workers.

[21] Relative to all flows, labor immigration represents a minor immigration channel. In 2008, it represented around 2.5% of all flows. In 2011, however, it made up around 9.4% of all flows. See also A.2.

[22] Most scores are given for 4-digit occupational codes, but a minority are given for 3-digit occupational codes. There are certain occupations that have the same SSYK code but in the list they are given different scores (e.g. bartenders/baristas). In that case I take an average of the scores corresponding to that code.

[23] I do this because my main dataset does not contain information on occupations.

$$index_i = \sum_o \alpha_{oi} \times score_o \tag{1}$$

where α_{oi} is the share of people working in occupation o in industry i; $score_o$ is the shortage score for occupation o. For those occupations that are not on the list, I impute the average score in the industry.[24] This formula has the advantage of capturing i) the fact that some occupations are more likely to occur in certain industries than in others and ii) the fact that certain occupations are given a higher score than others.

I calculate the index at the 5-digit industry level. I keep only those industries that employ at least 30 individuals. The sectoral distribution of the industries for which I am able to calculate the index versus the population is shown in Figure A.1 in the Appendix. The sectoral distribution is largely maintained.

I take the negative of the index obtained with the formula above for ease of interpretation: *the higher the index, the lower the shortage, the more affected the industry.* I standardize the index to have mean zero and standard deviation one.[25]

3.2 How well does the exposure measure predict non-EU hiring?

Firms in industries that were classified as being in lower shortage before the reform were more affected by the December 2008 reform than firms in industries classified as being in higher shortage. Given that fewer restrictions were put in place for the higher-shortage firms, these firms would have had the opportunity to hire from abroad even before the reform. We therefore expect an increased response for firms in the upper part of the distribution of the index.

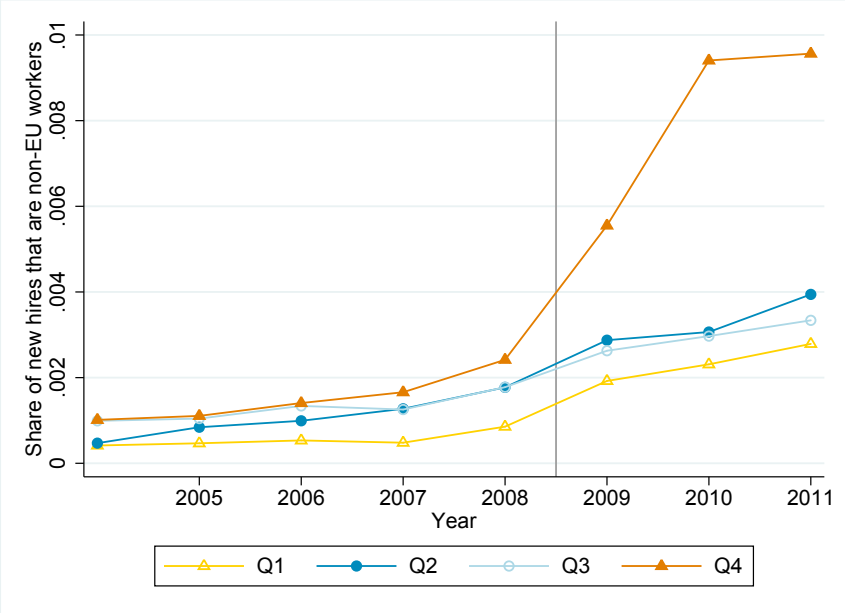
Figure 3a shows the share of newly-hired non-EU workers relative to all new hires, by year and quartile of the index. We see an increased response across all quartiles but a particularly high increase after the reform from firms in industries in quartile 4. Figure 3b shows that in the pre-reform period, firms in quartile 2 were doing the most hiring, with firms in quartile 3 and 4 doing roughly the same amount of hiring. Firms in quartile 1 hired the least, perhaps surprisingly. However, a lot of the high-skilled non-EU hires have short-term permits (valid for less than a year), in which case I wouldn't be capturing them. In the post-reform period, however, while firms in all quartiles hire more, firms in the fourth quartile hire significantly more compared to pre-reform, which is precisely what we would expect given the fact restrictions were lifted for industries in quartile 4 to a larger extent than for industries in the other

[24] Very few industries have no occupations on the list. I drop these.

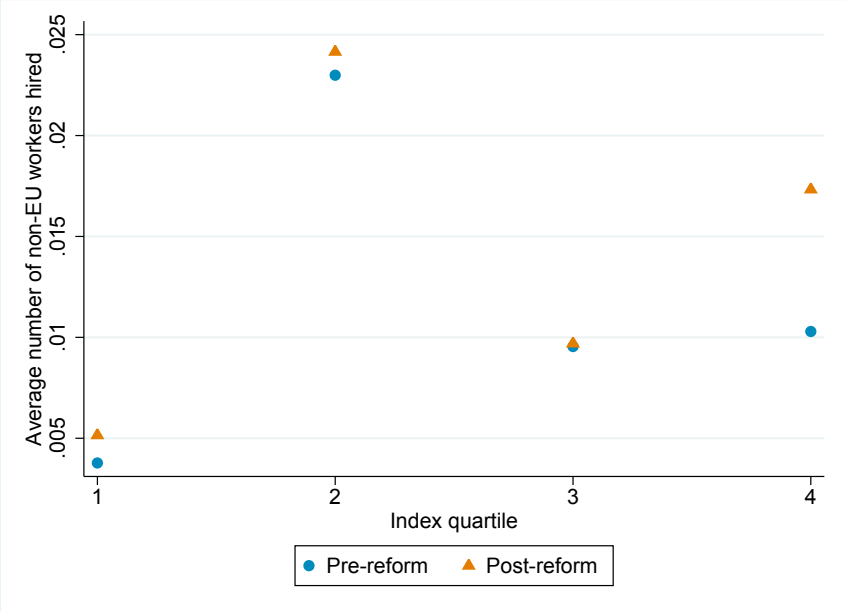
[25] One worry could be that the 2008 score doesn't capture well the overall pre-reform shortages in the labor market. An alternative would have been to use the average score over the entire pre-reform period. I collect yearly scores from the entire pre-reform period and construct an average score as well. As we can see in Figure A.3, the 2008 score is highly correlated with the average score. The pairwise correlations among the pre-reform years are also high (see Table A.1).

quartiles. Since firms in quartile 4 react the most, I categorize these firms as treated and all the rest as control.

Figure 3: Illustration of natural experiment



(a) Share of non-EU workers out of all new hires



(b) Number of non-EU hires, by quartile

Notes: Firms are grouped into quartiles depending on where the industry they belong to in 2008 lies in the index distribution.

3.3 Specification

3.3.1 Firm-level analysis

I use a difference-in-differences setup where the treatment group is made up of firms that are in the fourth quartile of the index distribution. As Figure 3 has shown, industries in the upper distribution of the index are those that respond the most to the reform. I estimate the following equation:

$$y_{ijt} = \alpha_i + \beta_t + \theta_{s,t} + \delta_j \times t + \gamma(I_{t\geq 2009} \cdot D_j) + \varepsilon_{ijt} \tag{2}$$

where y_{ijt} is the outcome in firm i in industry j and year t, α_i is a vector of firm fixed effects, β_t is a vector of year fixed effects, $\theta_{s,t}$ is a vector of sector-by-year fixed effects, where sectors are at the 1-digit level, and δ_j is a group-specific trend.[26] $I_{t\geq 2009}$ is an indicator for an observation after the introduction of the December 2008 reform, D_j is an indicator for being in one of the treated industries. ε_{ijt} is the error term. γ is the coefficient of interest. Assuming that the trends in the outcome would have been similar in firms in industries for which the policy environment changed to a larger extent than for firms in sectors for which it changed to a lesser extent, the estimate $\hat{\gamma}$ captures the causal effect of the reform.

3.3.2 Individual-level analysis

I follow largely the same strategy as in the firm-level analysis, with treated individuals defined as those in treated industries in 2008. I estimate the following equation:

$$y_{kjt} = \lambda_k + \tau_t + \eta_{s,t} + \mu_j \times t + \psi(I_{t\geq 2009} \cdot D_j) + \phi X_{kt} + \varepsilon_{ikt} \tag{3}$$

where the λ_k's are individual fixed effects, τ_t are year fixed effects, $\eta_{s,t}$ are sector-by-year fixed effects and X_{kt} is a set of time-varying covariates: age, age squared, labor market experience, labor market experience squared, a dummy for being married and a dummy for having children.

4 Firm-level analysis

4.1 Data and descriptive statistics

4.1.1 Identifying non-EU workers in the data

The data I use for my main analysis contains a variable that designates the reason for settlement in Sweden. I am interested in individuals whose first permit in Sweden is a work permit, i.e. in workers who have been recruited directly from abroad. I therefore take the

[26] Including additional county-by-year fixed effects changes the results little. They are available upon request.

matched employer-employee sample and find the first firm the non-EU worker works at in Sweden. I cross check this information with information on year of arrival. For around 95% of individuals, the first time they are observed with a firm corresponds to their year of arrival in Sweden or one year later. The latter is largely due to the fact that the register gets updated in November so for those arriving later they would show up in the register one year later. The remaining 5% could be due to measurement error. I am therefore capturing a lower number of workers given that some people already in Sweden could switch to work permits.[27]

In Table A.2, I show descriptive statistics for non-EU workers for the year they are hired. I divide them in four groups, by treatment status and by period of arrival. We can see that individuals in treated industries earned less than those in control industries even before the reform, but earnings dropped in both groups after the reform. The share of low-skilled increased by more than half in the treatment group (but there is a significant proportion of newcomers with missing skill information). Non-EU workers arriving after the reform in treated industries are to a large extent drawn from the lower part of the skill and earnings distribution. This is important to keep in mind when interpreting the results.

4.1.2 Sample restrictions and definition of key variables

I use a matched employer-employee panel for the period 2003-2011. In each year, I keep individuals aged 18-65 whose annual earnings are above the 10th percentile in the annual earnings distribution. This is so as to not consider individuals that are matched to a firm after only having worked a few hours at that respective firm. I impose two restrictions on the firm: 1) they have to exist in 2008 and 2009 and 2) they have to have at least two employees in 2008. The latter serves to remove the self-employed. However, I do include firms that are owned by self-employed who employ others. I restrict to private sector firms. I do the analysis at establishment level.[28]

For each firm, I measure *firm size* as the number of employees matched to the firm each year. I calculate *mean earnings* as the average annual earnings of the employees matched to a firm. I consider an individual as newly-hired if they are in the firm in year t but not in year $t-1$. I define the *hiring rate* as the number of new hires as a share of total firm size in 2008. A new hire is a non-EU worker if I have previously identified those workers as non-EU work permit holders following the steps outlined above. I consider a person to separate from the firm if they were observed in $t-1$ but not in t. *Separation rates* are calculated analogously to hiring rates.

[27] Students make up the group most likely to switch but I argue that they have a set of advantages with respect to their knowledge of the Swedish labor market that complete outsiders do not.

[28] I use establishment and firm interchangeably in the rest of the paper.

Table 1: Firm-level characteristics (all)

	Treated		Control	
	Mean	SD	Mean	SD
Firm size	11.799	34.319	16.005	86.840
Mean earnings	241.374	103.309	291.990	147.037
Non-EU	0.120	0.251	0.051	0.151
Women	0.560	0.330	0.216	0.261
Age	39.994	8.793	41.909	7.688
Low-skilled	0.738	0.274	0.811	0.264
Medium-skilled	0.128	0.175	0.096	0.160
High-skilled	0.127	0.204	0.086	0.190
Missing skill	0.008	0.051	0.007	0.048
Average skill content	1.708	0.358	1.917	0.170
Firm age	4.239	1.446	4.364	1.344
Primary sector	0.001		0.069	
Manufacturing	0.005		0.213	
Electricity, gas and water supply	0.000		0.016	
Construction	0.000		0.239	
Wholesale and retail trade	0.407		0.138	
Transportation and storage	0.006		0.118	
Hotels and restaurants	0.160		0.005	
Information and communication	0.005		0.004	
Financial, insurance and real estate activities	0.062		0.014	
Professional, scientific and technical activities	0.077		0.063	
Personal services	0.277		0.123	
Firm size ≤ 50	0.966		0.950	
Firm size 51-100	0.023		0.029	
Firm size 101-200	0.008		0.013	
Firm size >200	0.003		0.008	
Observations	59,868		86,071	

Notes: Firm-level characteristics refer to 2008. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Firm age data starts in 2003, so the oldest firm can be at most 5.

4.1.3 Descriptive patterns

Table 1 shows descriptives for all firms, by treatment status. Regardless of treatment, the majority of firms have fewer than 50 employees. There are

slightly bigger firms in the control group. The treated firms have employees that on average earn less than the employees in the control group. They are also slightly younger and predominantly female. Treated firms are found largely in the wholesale and retail trade, services and hotels and restaurants sectors.[29] More than twice as many employees in treated firms are born in a non-European country.[30] A higher share of employees in the treated firms are high-skilled.[31]

Table A.4 shows the same type of descriptives but only for firms that actually hire non-EU workers at least once during 2009-2011. The earnings, gender and age patterns remain for treated firms. Employees in treated firms earn almost twice as much on average than those in control firms. More treated firms that hire during 2009-2011 are small, relative to firms in the control group. Almost 60% of treated firms that hire non-EU workers are in the hotels and restaurants sector. More than half of the employees in treated firms are born in a non-European country. Note that firms in the control group also have a significant share of non-Europeans, which is in line with survey evidence on the likelihood of hiring from abroad that shows that firms with a larger pool of foreigners are more likely to hire foreigners (Winkelmann 2001).[32] Note that overall, firms in treated industries are more likely to have high-skilled employees (see Table 1), but the firms that actually take up the opportunity to hire non-EU workers are less likely to have high-skilled workers as compared to control firms that take it up (11% compared to 19%). This suggests that firms that take up the program are drawn from the lower part of the skill distribution among those eligible.

4.2 Results

Figure 3 showed descriptively that firms in industries in the highest quartile of the shortage index distribution hire the most non-EU workers after the reform. To probe that finding more formally, I estimate equation (2) for two outcomes: the number of newly-hired non-EU workers and the share of newly-hired non-EU workers relative to all the new hires. Table 2 shows the estimated coefficient on the interaction, γ, in equation (2) for these two outcomes.

[29]For the purposes of this table, I put the following sectors together: agriculture, forestry and fishing and mining and quarrying (primary); electricity, gas, steam and air conditioning supply and water supply, sewerage, waste management and remediation activities (electricity, gas and water supply); financial and insurance activities and real estate activities (financial, insurance and real estate activities); administrative and support service activities, public administration and defence, compulsory social security, education, human health and social woork activities, arts, entertainment and recreation and other service activities (personal services).

[30]Note that the non-EU definition here only looks at country of birth and not citizenship.

[31]In Table A.3, I show what predicts being in the treatment group, accounting for sector-by-year fixed effects.

[32]Another mechanism at work here could be that immigrant managers hire immigrant workers, as found in Åslund et al. (2014).

Column 1 says that firms in treated industries hire 0.009 non-EU workers more, which translates roughly into nine workers for each 1000 firms. The pre-reform average in the control group is 0.008 workers. Hence, the reform had the effect of doubling the number of workers in treated firms relative to the control firms. The share of non-EU workers relative to new hires increases by 0.2 percentage points. Given that the pre-reform average in the control group is 0.002, the share of non-EU workers relative to new hires also doubles. Below I test whether this effect is driven by a shift in the composition of hires or if the overall hiring rate also goes up.

In Figure A.4a I show the coefficients from running the same regression on number of non-EU hires separately for each firm size category. Firm size categories are calculated in 2008. We see that the bigger firms are affected the most in terms of number of non-EU hires. However, when looking at relative shares, we see that small and big firms do roughly the same amount of non-EU hiring (Figure A.4b). This result corroborates descriptive findings in OECD (2011) and Emilsson et al. (2014) that the reform provided the opportunity for smaller firms to recruit from abroad.

Table 2: Effects of December 2008 reform on non-EU hiring outcomes: Difference-in-differences with binary treatment

	Number of non-EU workers (1)	Share of non-EU relative to new hires (2)
Coefficient	0.009	0.002
	(0.003)	(0.001)
Observations	1,074,568	630,042
Clusters	449	449
Pre-reform average	0.008	0.002

Notes: Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Treatment is defined as being in the fourth quartile of the index. Pre-reform averages refer to the control group. Standard errors in parantheses and clustered at the level.

The reform thus resulted in firms hiring significantly more non-EU workers. The question that follows is whether the inflow of immigrants had an effect on firm outcomes. Table 3 shows the estimates of the coefficient on the interaction in equation 2 from regressions on firm size (measured as the log of the number of employees), the overall hiring rate, the overall separation rate and mean earnings (in logs). Firms in treated industries grow by around 2%, but this result is not significant. The reform has a positive effect on hiring rates and a negative effect on separation rates, but the coefficients are never significant. The average result on hiring is driven by an initial positive effect and a subsequent negative effect (Figure 4b),

whereas the yearly coefficients on separation rates are rather stable over time (Figure 4c). In sum, these results suggest that treated firms not only hire more non-EU immigrants, but they hire more overall, and keep their workforce to a larger extent. This results in an increase in firm size.

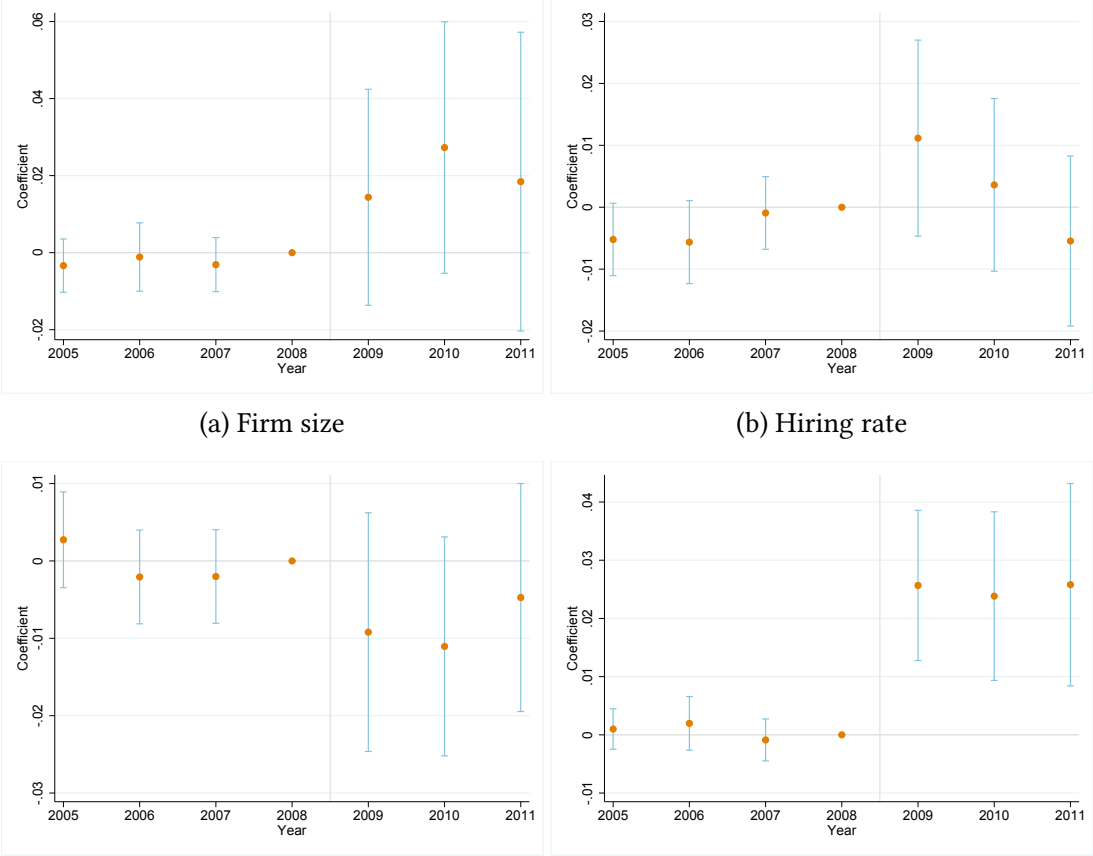
The most striking results in Table 3 are on mean earnings: they go up by around 2.5% on average, jumping in the first year after the reform then roughly stabilizing at the higher level (Figure 4d). Figure 5 shows that the average result is driven by the smallest firms.

Table 3: Effects of December 2008 reform on firm-level outcomes: Difference-in-differences with binary treatment

	Firm size	Hiring rate	Separation rate	Mean earnings
	(1)	(2)	(3)	(4)
Coefficient	0.020	0.010	-0.008	0.025
	(0.015)	(0.007)	(0.007)	(0.007)
Observations	1,074,568			
Clusters	449			
Pre-reform average	1.785	0.169	0.161	5.615

Notes: Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Treatment is defined as being in the fourth quartile of the index. Pre-reform averages refer to the control group. Standard errors in parantheses and clustered at the level.

Figure 4: Firm-level outcomes: yearly coefficients



(a) Firm size

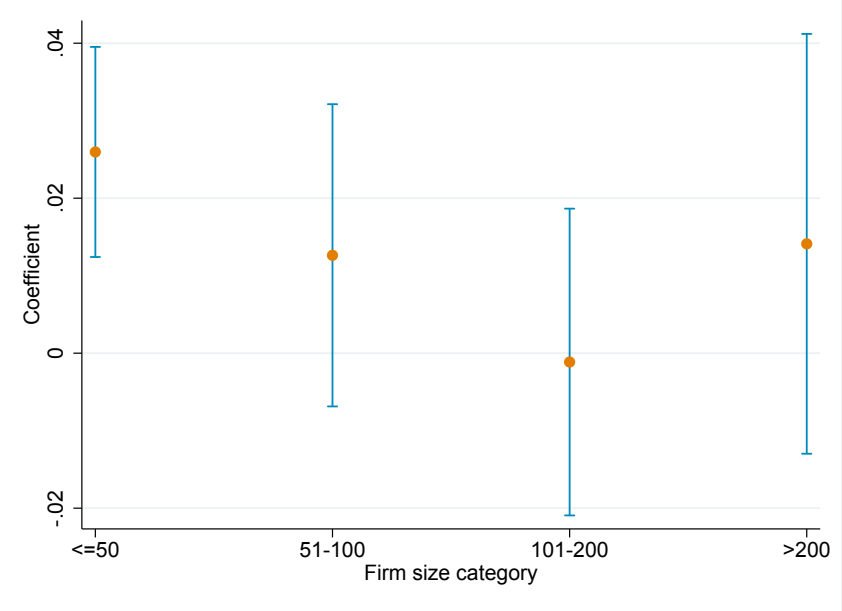
(b) Hiring rate

(c) Separation rate

(d) Mean earnings

Notes: Firms are restricted to exist both in 2008 and 2009 and to have at least 2 employees in 2008. Firm size and mean earnings are measured in logs. Regressions include year, firm, sector-year fixed effects and a group-specific linear trend. Standard errors are clustered at the 5-digit industry level.

Figure 5: Effect on mean earnings, by firm size



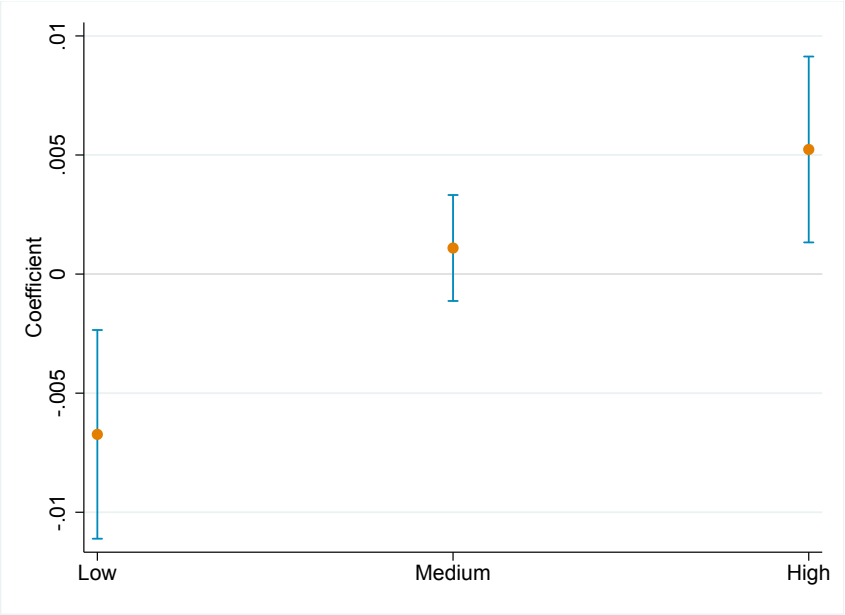
Notes: Estimates from regressions with group-specific linear trend. Firm size category is measured in 2008.

The average result on earnings could either be because incumbent workers are paid relatively more or because there is a change in the composition of workers at the firm. I therefore next look at how the skill composition changes at the firm. I run equation 2 on, respectively, the shares of low-skilled, medium-skilled and high-skilled. Figure 6 summarizes the results. We see the ratio of high-skilled to low-skilled going up (albeit insignificantly) with the share of low-skilled dropping by half a percentage point and the share of high-skilled increasing by around the same amount. This shift towards more high-skilled workers come from an increase in the intensity of hiring high-skilled workers, as Table 4 shows. Hiring rates go up for both types of workers, but firms are hiring slightly more high-skilled workers as a result of the reform. This is consistent with the share of high-skilled increasing at the firm. Separation rates also go down, more for the low-skilled, but results are insignificant for both groups.

Overall, even though not all coefficients are precisely estimated, these results all point in the same direction. Treated firms do better because of the reform: they hire more workers and fewer separate. Treated firms also take advantage of the skill complementarity between natives and immigrants and hire more highly-skilled as a result of facing fewer obstacles on hiring low-skilled from abroad. The group of small firms seems to benefit the most. These results need to be interpreted in light of the fact that I restrict firms to exist in 2008 and 2009. This means that I am not evaluating the effect of the policy on firm expansion.[33] Furthermore, these results are conditional on firm survival. However, they are not driven by firm survival. Estimating equation 2 on the probability of a firm to exist, the coefficient on the interaction term is small and insignificant (0.0004 with standard error 0.001).

[33] Olney (2013) finds that low-skilled immigration significantly increases the number of establishments within US cities.

Figure 6: Effect on skill composition



Notes: Estimates from regressions with group-specific linear trend.

Table 4: Effects of December 2008 reform on skill-specific hiring and separation outcomes: Difference-in-differences with binary treatment

	Hiring rate low-skilled (1)	Separation rate low-skilled (2)	Hiring rate high-skilled (3)	Separation rate high-skilled (4)
Coefficient	0.003	-0.004	0.004	-0.003
	(0.005)	(0.005)	(0.002)	(0.002)
Observations	1,074,568			
Clusters	449			
Pre-reform average	0.126	0.116	0.016	0.013

Notes: Regressions include year, firm fixed effects, sector-year fixed effects and a group-specific trend. Pre-reform averages refer to the control group. Treatment is defined as being in the fourth quartile of the index. Standard errors in parantheses and clustered at the level.

5 Individual-level analysis

As the previous analysis shows, the composition of workers in firms changes, which is reflected in the effect of immigration on average earnings. Positive effects at the firm may mask potentially negative effects on individuals, if these are pushed into worse-paying firms or into unemployment. In this section I study earnings and employment dynamics at the individual level, following domestic individuals that were employed in 2008 over time.

5.1 Data and descriptive statistics

I start with the sample of *low-skilled* domestic employees assigned to a firm from the firm-level analysis in 2008. I create a panel for the years 2004-2011 and follow these individuals over time, regardless of whether they stay at their 2008 firms or whether they are employed at firms that satisfy the restrictions in the firm-level analysis section. I keep the part of the individual histories when they are of working age (18-65).

Table 5 shows individual characteristics for 2008, by treatment status. Individuals in treated industries earn less than those in control industries, are to a larger extent born in a non-European country, are younger and more than twice as likely to be female. They are more heavily concentrated in service sectors, relative to control individuals who work primarily in manufacturing.

Table 5: Individual-level characteristics (low-skilled workers only)

	Treated		Control	
	Mean	SD	Mean	SD
Earnings	241.822	151.041	301.081	135.553
Non-EU	0.105	0.307	0.061	0.239
Women	0.567	0.496	0.224	0.417
Age	38.805	13.598	41.408	12.963
Average skill content	1.699	0.367	1.927	0.158
Primary sector	0.001		0.027	
Manufacturing	0.011		0.436	
Electricity, gas and water supply	0.000		0.022	
Construction	0.000		0.165	
Wholesale and retail trade	0.431		0.098	
Transportation and storage	0.027		0.102	
Hotels and restaurants	0.149		0.005	
Information and communication	0.004		0.005	
Financial, insurance and real estate activities	0.085		0.006	
Professional, scientific and technical activities	0.032		0.021	
Personal services	0.260		0.113	
Observations	502,146		1,026,513	

Notes: Individual-level characteristics refer to 2008. Treated individuals are those in industries that are in the fourth quartile of the index distribution. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each group.

5.2 Results

I look at four outcomes: annual earnings;[34] employment, defined as the probability of having non-zero income; firm stay, defined as the likelihood of being matched to the same firm as in 2008; and industry mobility, defined as the probability of being in an industry that is different from the industry in 2008.[35] The latter two serve to give an interpretation to the findings for earnings. If I find both earnings and firm stay to increase, that would suggest the adjustment happens at the firm. If instead earnings increase but firm stay goes down, then the adjustment happens by moving across firms.[36]

Given that the variation I exploit categorizes primarily low-skilled industries as treated, I focus on the sample of low-skilled natives. I also show results for different groups of low-skilled: young (aged below 40), older (aged above 40) and non-EU born who were already in Sweden. I show results for high-skilled for completeness.

Table 6 shows a summary of the results. In the first column, I show average results for the group of low-skilled. Overall, the results are small and insignificant, except for industry mobility, which goes down and is significant at the 10% level. Looking at columns (2), (3) and (4) reveals that these average results mask the fact that different groups are affected differently. The effects on the group of young low-skilled are consistently negative, regardless of the outcome. They earn less, are more likely to be unemployed, less likely to stay at their 2008 firms and less likely to switch industries. Only the result on employment is significant at the 5% level: young low-skilled domestic workers in treated industries are 0.8 percentage points less likely to be employed. Given that the group of newcomers largely falls in this same category of young and low-skilled, these results suggest a high degree of substitutability between the two. If this is the case, we would expect opposite effects for the group of older low-skilled. Column (3) shows that indeed, the earnings of the older group significantly increase by 2.7%. Older individuals are also more likely to be employed and to stay with their 2008 firms (these effects are insignificant, though).

The most surprising result is for the group of non-EU-born that were already in Sweden. Their earnings see an increase of 13% (significant at the 10% level). Those born in non-European countries are the least likely to switch industries and the most likely to stay with their 2008 firms (the latter effect is insignificant, though). Previous literature generally finds

[34]Note that most previous studies of immigration on individual outcomes look at hourly wages and not annual earnings (Bratsberg and Raaum (2012), Foged and Peri (2016)). One exception is Cattaneo et al. (2015).

[35]The industry codes change in 2007 such that for most codes there is no one-to-one mapping. In order to have the full industry history, I create a mapping between the previous version of the codes (SNI 2002) and the new version (SNI 2007). I pool all workers over the 2007-2009 period, as during these years I have both codes. This allows me to calculate relative frequencies, that is, to see the transitions between SNI 2002 and SNI 2007 that are most frequently encountered. In case I am left with industries without a mapping, I take the first transition that shows up int he correspondence tables provided by Statistics Sweden.

[36]The underlying mechanism could be the same in both cases. Foged and Peri (2016) find that low-skilled natives in areas with a higher share of immigrants transition to more complex jobs but only do so by moving across firms; they don't find this sort of task adjustment within firms.

that older immigrants tend to be hurt the most by new immigration (Card 2001, Ottaviano and Peri 2012). However, the older immigrants I look at here are potentially more successful than on average, given that they are already employed in 2008. They are also predominantly from countries of origin that have a longer history in Sweden (Former Yugoslavia, Bosnia, Iran). Moreover, whether these workers should be categorized as low-skilled is also not straightforward: immigrants often work in jobs for which they are overqualified if the education and experience they obtained abroad is not valued in the host country (Friedberg 2000, Joona et al. 2014). The current analysis also does not allow me to conclude anything about those that were unemployed in 2008 that would have worked in treated industries. These results are not inconsistent with that group being potentially more negatively affected. (as found, for example, in Dustmann et al. 2017).

If the mechanism underlying these results is skill complementarity, whereby the workers that are most complementary to the newcomers benefit the most, we should find larger effects on earnings for the high-skilled. The first row of column (5) in Table 6 shows that the average effect on the high-skilled is almost double the one on the low-skilled in column (1) but it is imprecisely estimated.

The individual-level analysis provides at least suggestive evidence for complementarities both within skill group (between low-skilled natives and low-skilled immigrants) and across groups (between low-skilled immigrants and high-skilled natives). However, not all low-skilled are equal. There is a smaller degree of substitutability between older low-skilled natives and immigrants than there is between younger low-skilled natives and immigrants, largely due to the age composition of the newcomers.

Table 6: Effects of December 2008 reform on individual-level outcomes

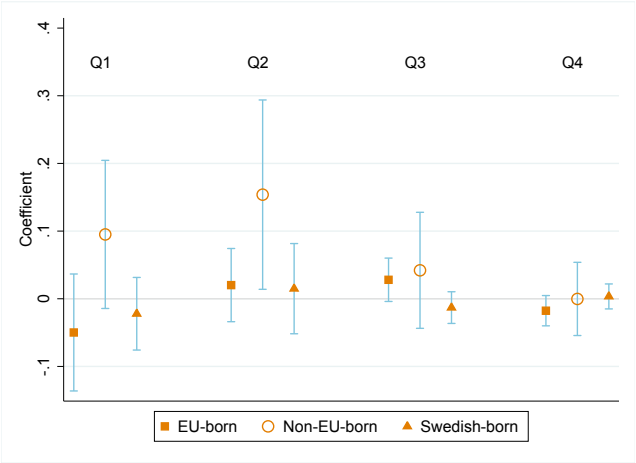
	Low-skilled	Young low-skilled	Older low-skilled	Non-EU low-skilled	High-skilled
	(1)	(2)	(3)	(4)	(5)
Earnings	0.009	-0.018	0.027	0.134	0.016
	(0.024)	(0.023)	(0.011)	(0.069)	(0.013)
Observations	11,382,943	5,420,355	5,962,588	817,813	2,222,149
Clusters	449	449	349	448	449
Pre-reform average	5.474	5.247	5.653	5.237	5.697
Employment	-0.002	-0.008	0.002	0.019	-0.003
	(0.004)	(0.004)	(0.003)	(0.015)	(0.003)
Observations	11,717,797	5,626,295	6,091,502	879,908	2,277,510
Clusters	449	449	449	448	449
Pre-reform average	0.976	0.962	0.988	0.925	0.973
Firm stay	0.003	-0.013	0.019	0.081	-0.014
	(0.021)	(0.021)	(0.014)	(0.056)	(0.019)
Observations	11,382,943	5,420,355	5,962,588	817,813	2,222,149
Clusters	449	449	449	448	449
Pre-reform average	0.773	0.688	0.840	0.719	0.673
Industry mobility	-0.054	-0.035	-0.068	-0.154	-0.028
	(0.032)	(0.024)	(0.028)	(0.052)	(0.028)
Observations	11,363,406	5,404,527	5,958,879	815,959	2,219,237
Clusters	449	449	449	448	449
Pre-reform average	0.228	0.291	0.179	0.268	0.321

Notes: Regressions include year, sector-year, individual-level fixed effects, time-varying covariates and a group-specific linear trend. Pre-reform averages refer to the control group. Treated individuals are those in industries that are in the fourth quartile of the shortage index distribution. Standard errors in parantheses and clustered at the 5-digit industry level. *, **, *** indicate significance at the 10, 5, and 1 percent level, respectively.

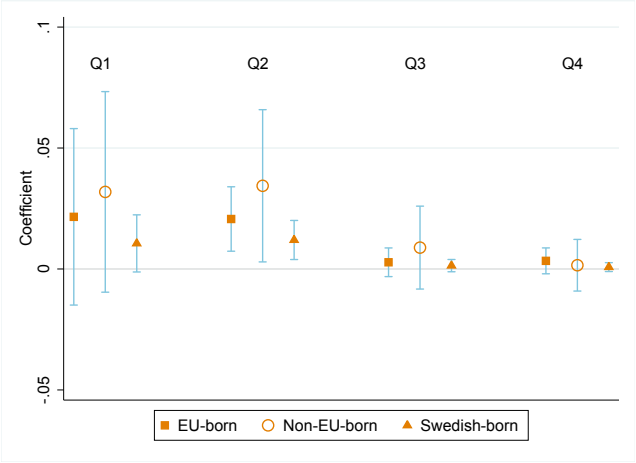
Given the fact that education levels are often poorly recorded for immigrants, and the fact that previous work has found effects of immigration along the wage distribution (e.g. Dustmann et al. 2012), I next do the following exercise. I break down the sample of individuals employed in the pre-reform period into earnings quartiles, depending on their position in the national earnings distribution. I then estimate equation 3 on three groups of workers, for each earnings quartile: Swedish-born, EU-born and non-EU born. Figure 7a plots the results for earnings and and Figure 7b for employment. We see that the previous result that

previous (non-EU) immigrants are the most positively affected remains (for both earnings and employment) and is concentrated at the bottom of the distribution. Those born in European countries and who were in the bottom quartile before the reform are the most hurt in terms of earnings but not in terms of employment.

Figure 7: Effect on earnings and employment, by earnings quartile



(a) Earnings



(b) Employment

Notes: Earnings are measured in logs. Regressions include year, firm, sector-year, individual-level fixed effects, time-varying covariates and a group-specific linear trend. Standard errors are clustered at the 5-digit industry level.

6 Conclusion

Many European countries are facing severe labor shortages across the skill distribution. Free mobility within the European Union has so far not been sufficient to eliminate these issues. Most states are in favor of opening up the borders to workers from non-European countries, and there is high competition for talent from abroad. In this paper, I show that low-skilled labor immigration also has mostly positive effects. I do so by exploiting variation induced by a major Swedish reform that lifted restrictions on hiring from abroad differentially across industries.

I first study firms and find that employees in firms in treated industries - those industries for which restrictions were lifted to a higher extent - have higher average earnings after the reform. This result seems to be driven by a change in composition at the firm, towards a more high-skilled workforce. Firms appear to take advantage of complementarities between natives and immigrants.

I also look at individuals and show some evidence that on average they earn more. Not all low-skilled workers are affected in the same way, however. The young and low-skilled, who are arguably competing the most with the newcomers, are more likely to become unemployed and may also earn less. An unexpected result of the individual-level analysis is that it is non-European immigrants that had already been in Sweden that benefit the most in terms of earnings, contrary to what previous literature has found. Further research is needed to probe that result and understand the reasons behind it.

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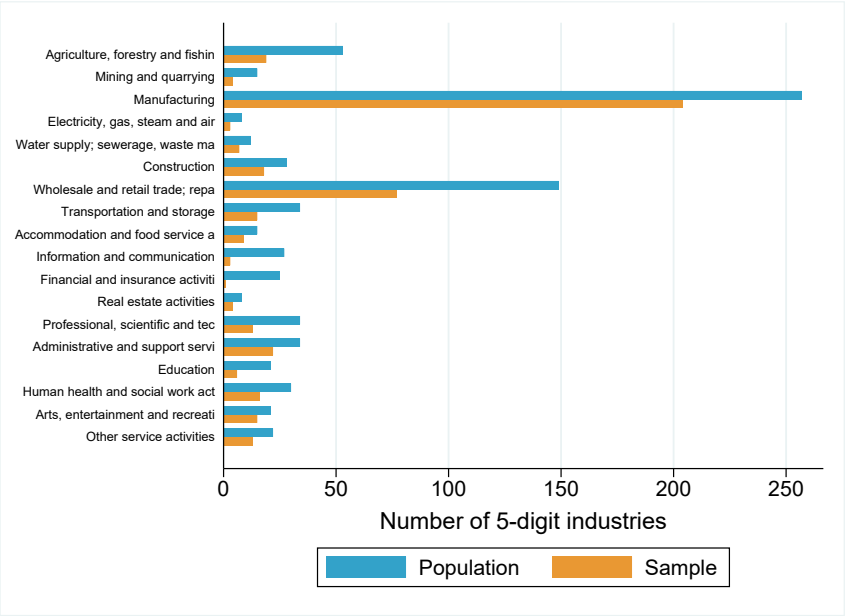
Appendix

Figures

Figure A.1: Sectoral distribution



Notes: This figure shows the number of 5-digit industries in each sector, overall (population) and considering only the 5-digit industries I can calculate scores for (sample).

Figure A.2: Share of immigrants by admission category

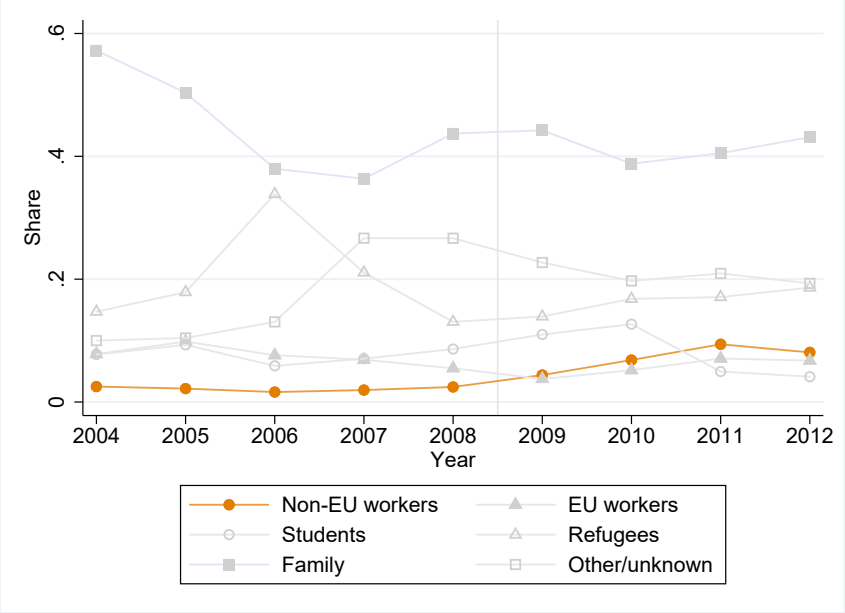


Figure A.3: 2008 shortage score against pre-reform average score

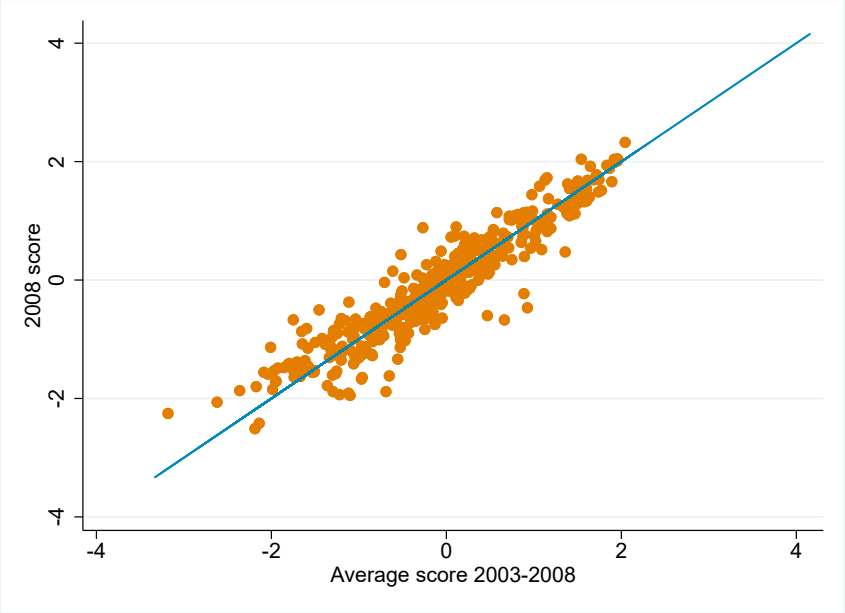
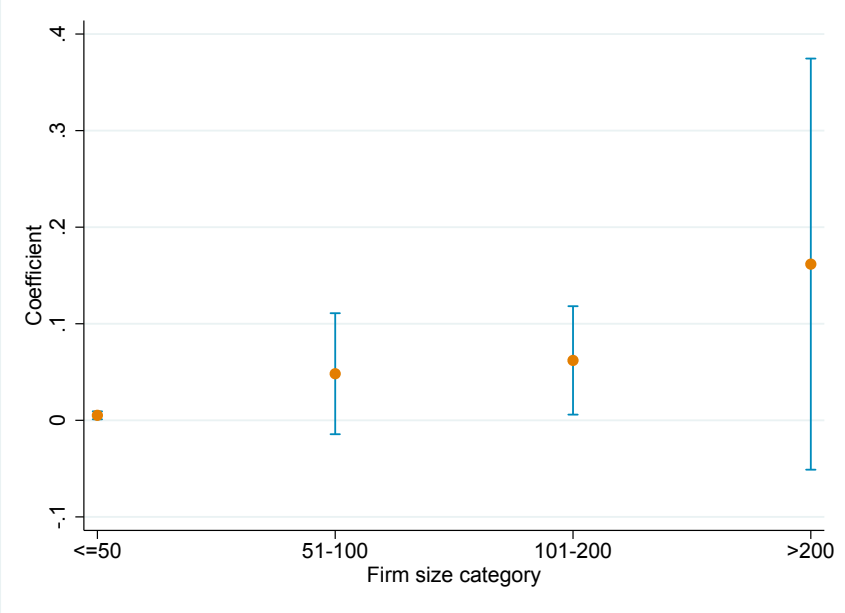
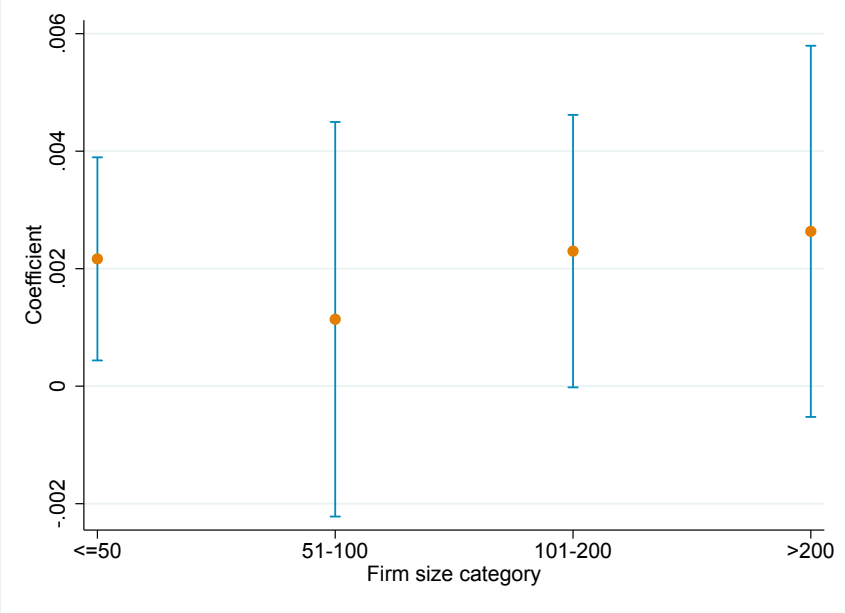


Figure A.4: Effect on non-EU hiring, by firm size



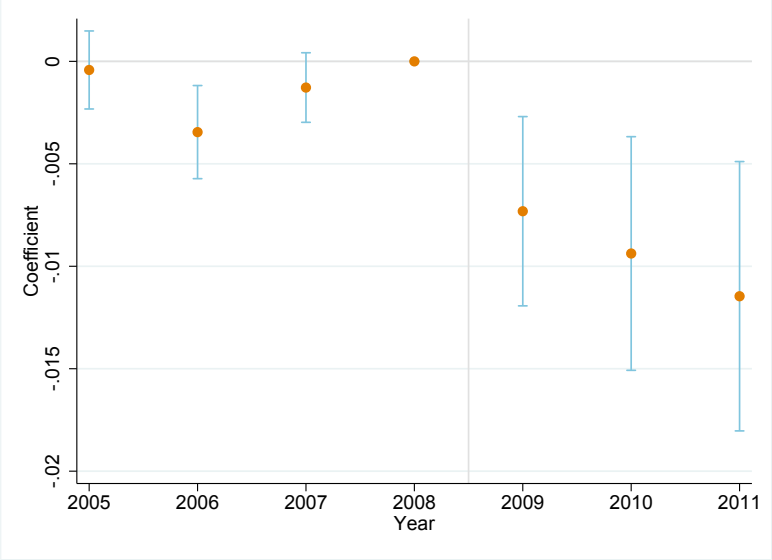
(a) Number of non-EU



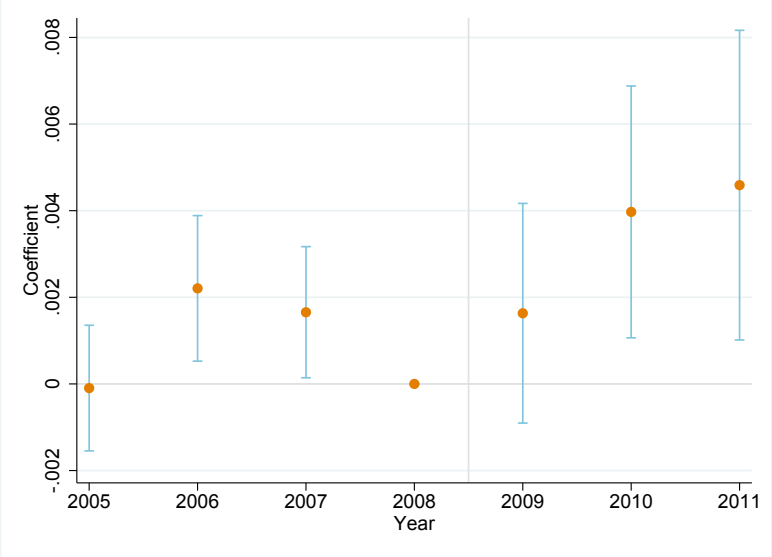
(b) Share of non-EU relative to all hires

Notes: Estimates from regressions with group-specific linear trend. Firm size category is measured in 2008.

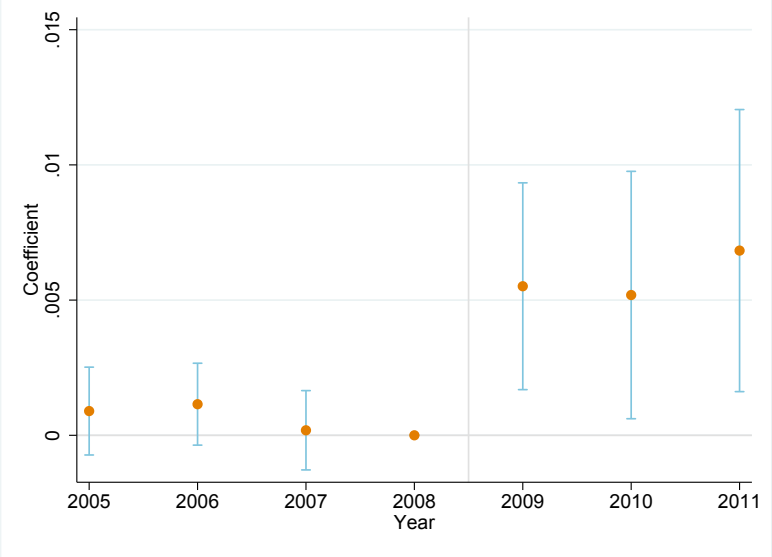
Figure A.5: Skill composition: yearly coefficients



(a) Low-skilled



(b) Medium-skilled



(c) High-skilled

Notes: Firms are restricted to exist both in 2008 and 2009 and to have at least 2 employees in 2008. Regressions include year, firm, sector-year fixed effects and a group-specific linear trend. Standard errors are clustered at the firm level.

Tables

Table A.1: Correlation matrix between shortage scores

	Yearly score					
	2003	2004	2005	2006	2007	2008
2003	1					
2004	0.909	1				
2005	0.804	0.901	1			
2006	0.754	0.839	0.920	1		
2007	0.675	0.766	0.811	0.933	1	
2008	0.706	0.808	0.842	0.896	0.917	1

Table A.2: Individual-level characteristics (non-EU workers only)

	Treated				Control			
	Pre-reform		Post-reform		Pre-reform		Post-reform	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Earnings	200.136	225.354	181.134	144.327	428.245	471.943	354.910	489.817
Women	0.262	0.440	0.218	0.413	0.210	0.407	0.165	0.372
Age	34.536	9.053	32.534	8.405	33.289	7.855	33.566	7.889
Low-skilled	0.168	0.374	0.255	0.436	0.072	0.259	0.129	0.335
Medium-skilled	0.103	0.304	0.092	0.290	0.047	0.211	0.053	0.223
High-skilled	0.200	0.400	0.153	0.360	0.388	0.487	0.364	0.481
Missing skill	0.529	0.499	0.499	0.500	0.493	0.500	0.455	0.498
Average skill content	1.413	0.346	1.343	0.328	1.926	0.195	1.922	0.165
Primary sector	0.000		0.003		0.017		0.030	
Manufacturing	0.029		0.008		0.401		0.392	
Electricity, gas and water supply	0.000		0.000		0.003		0.005	
Construction	0.000		0.000		0.041		0.098	
Wholesale and retail trade	0.107		0.101		0.071		0.083	
Transportation and storage	0.001		0.001		0.027		0.038	
Hotels and restaurants	0.502		0.530		0.001		0.005	
Information and communication	0.003		0.002		0.001		0.000	
Financial, insurance and real estate activities	0.027		0.014		0.002		0.005	
Professional, scientific and technical activities	0.034		0.023		0.267		0.193	
Personal services	0.297		0.319		0.170		0.151	
Observations	964		2,919		2,043		2,412	

Notes: Individual-level characteristics refer to the first year when the non-EU worker is hired. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each group.

Table A.3: Selection into treatment

	Coefficient	SE
Mean earnings (logs)	-0.091	0.026
Non-EU	0.115	0.036
Women	0.272	0.067
Age	-0.001	0.001
Low-skilled	0.024	0.041
Medium-skilled	0.015	0.063
High-skilled	-0.080	0.078
Number of establishments	0.000	0.000
Average skill content	-0.349	0.124
Firm size ≤ 50	-0.003	0.022
Firm size 51-100	0.004	0.020
Firm size 101-200	-0.010	0.017
Observations	771,902	
R-squared	0.561	

Notes: This table shows coefficients from a regression of treatment status on firm-level characteristics, for the 2003-2008 pre-reform period. The regression includes sector-by-year fixed effects. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Earnings are measured in logs. The omitted categories are missing skill and firm size greater than 200 for the skill and firm size categories, respectively. Standard errors in parantheses and clustered at the 5-digit industry level.

Table A.4: Firm-level characteristics (firms that hire at least one non-EU worker 2009-2011)

	Treated		Control	
	Mean	SD	Mean	SD
Firm size	31.511	115.076	157.234	617.656
Mean earnings	200.298	99.431	322.849	175.343
Non-EU	0.555	0.362	0.218	0.287
Women	0.395	0.274	0.253	0.236
Age	35.897	6.562	39.234	6.297
Low-skilled	0.722	0.246	0.652	0.289
Medium-skilled	0.110	0.138	0.120	0.126
High-skilled	0.110	0.182	0.194	0.240
Missing skill	0.058	0.118	0.034	0.095
Average skill content	1.377	0.330	1.910	0.185
Firm age	4.086	1.405	4.252	1.456
Primary sector	0.001		0.046	
Manufacturing	0.005		0.334	
Electricity, gas and water supply	0.000		0.006	
Construction	0.000		0.134	
Wholesale and retail trade	0.121		0.128	
Transportation and storage	0.002		0.067	
Hotels and restaurants	0.595		0.011	
Information and communication	0.001		0.000	
Financial, insurance and real estate activities	0.017		0.006	
Professional, scientific and technical activities	0.025		0.090	
Personal services	0.233		0.178	
Firm size ≤ 50	0.886		0.689	
Firm size 51-100	0.057		0.095	
Firm size 101-200	0.027		0.078	
Firm size >200	0.030		0.138	
Observations	1,618		1,092	

Notes: Firm-level characteristics refer to 2008. Treated firms are firms in industries that are in the fourth quartile of the index distribution. Low-skilled is defined as having at most 12 years of education and high-skilled as having 15 or above. Earnings are measured in thousands of 2014 SEK. Average skill content refers to the skill content of the occupations assigned to the industries in each quartile. Firm age data starts in 2003, so the oldest firm can be at most 5.