



HAL
open science

Too Constrained to Grow Analysis of Firms' Response to the Alleviation of Skill Shortages

Sara Signorelli

► **To cite this version:**

Sara Signorelli. Too Constrained to Grow Analysis of Firms' Response to the Alleviation of Skill Shortages. 2020. halshs-02961493

HAL Id: halshs-02961493

<https://shs.hal.science/halshs-02961493>

Preprint submitted on 15 Oct 2020

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.



PARIS SCHOOL OF ECONOMICS
ÉCOLE D'ÉCONOMIE DE PARIS

WORKING PAPER N° 2020 – 62

Too Constrained to Grow
Analysis of Firms' Response to the Alleviation of Skill Shortages

Sara Signorelli

JEL Codes: J24, J23, J61

Keywords: Skill Shortages, Firm Output, Immigration, France



Funded by a French government subsidy managed by the
ANR under the framework of the Investissements d'avenir
programme reference ANR-17-EURE-001

Too Constrained to Grow

Analysis of Firms' Response to the Alleviation of Skill Shortages

Sara Signorelli*

Paris School of Economics

September 2020

Abstract

Skill shortages are a growing concern in the context of rapid technological change, raising the question of their cost for the economy and of the effectiveness of policies aiming to reduce them. This paper evaluates the effect of a French reform facilitating the hiring of immigrant workers with highly-demanded competencies on the firms that were constrained by the scarcity of these skills. The analysis is based on administrative data and applies a difference-in-differences strategy. Results show that the firms most exposed to the reform react by hiring more workers in tight occupations. This leads to higher growth in their revenues and value added, and generates some crowding-in of other types of employment. Conversely, productivity and capital stock do not react to the policy in the short run. Findings confirm that shortages in key-competencies significantly hamper firms' growth potential, while immigration appears to be a viable solution against them.

JEL Classification: J24, J23, J61

Keywords: Skill Shortages, Firm Output, Immigration, France

*I am grateful to Eric Maurin for his guidance and to Luc Behaghel and François Fontaine for their valuable advices throughout the project. I thank Jan Bakker, Cyprien Batut, Nicolas Chanut, Alan Manning, Sandra McNally, Guy Michaels, Steve Pischke, Paolo Santini, Matteo Sandi, Di Song Tan, Wiljan van den Berge and Alice Zulkarnain for the precious comments, as well as the seminar participants at the Paris School of Economics, London School of Economics, Polytechnic School of Lausanne (EPFL) and Centraal Planbureau (CPB); and the attendees of the 3rd Workshop on Migration, Globalization and the Knowledge Economy. This work is supported by the Labour Chair of the Paris School of Economics and by a public grant overseen by the French National Research Agency (ANR) as part of the “Investissements d’Avenir” program (reference: ANR-10-EQPX-17 - Centre d’accès sécurisé aux données – CASD), which enabled the access to confidential data. All remaining mistakes are my own.

1 Introduction

Labor market frictions are increasingly considered as crucial determinants of employment, wages and firm performance (Hyatt and Spletzer, 2013; Decker et al., 2018). Understanding their economic costs and identifying avenues to reduce them is thus of paramount importance. The idea that shortages in certain occupations can coexist with unemployment is not new. Blank, Stigler et al. (1957) already stated that "a shortage exists when the number of workers available (the supply) increases less rapidly than the number demanded at the salaries paid in the recent past." In the last 20 years, however, the debate has gained new relevance in the context of rapid technological change, which spurred a fast growing demand for technical competences.¹ This paper takes advantage of a natural experiment easing migrant hires within a list of tight occupations to identify how the sudden relaxation of specific skill shortages benefits firms that rely on these competences. Findings show that the policy fostered growth in firms' revenues, value added and demand for complementary inputs, allowing them to approach the frictionless benchmark. These results imply that attracting immigrants with rare competencies can be largely beneficial for the host economy.

The French reform under study reduces the administrative costs for hiring extra-European workers within a list of 30 technical occupations afflicted by skill shortages.² The bite of the policy is expected to be higher in sectors and localities where these constraints are more binding, either because of the types of competencies needed for production or because of the skill mix available in the area. Firms' exposure to the reform is thus defined based on the level of tightness in target occupations observed within the sector and commuting zone where the company is located. The analysis is based on administrative employer-employee data and applies a difference-in-differences strategy with continuous treatment, controlling for potential confounding factors such as shortages in other jobs and for sector-specific time trends. In a second step, the policy is used as an exogenous

¹The coexistence of high rates of unemployment and high rates of unfilled vacancies spurred the literature on skill-mismatch (Shimer, 2007; Şahin et al., 2014), which further linked this phenomenon to the technology driven obsolescence of some skills and fast growing demand for some others (Restrepo, 2015).

²Shortages are defined based on the ratio between the number of vacancies open within a given occupation and region, and the number of job seekers in the region that are qualified for the job. The occupations included concern mostly technical competencies for which at least an upper-secondary degree is required.

instrument in an 2SLS procedure that allows to recover the elasticity of firms' output and other inputs with respect to the supply shock in technical employment. Finally, the robustness of the results is tested using an alternative identification strategy based on a direct measure of sectoral demand shocks. The latter captures variations in the demand for different products driven by the GDP growth of France's major trade partners.³ This analysis allows to check whether more constrained firms under-react to positive shocks in demand before the reform, and reveals the extent to which they catch up after the policy change.

Results show that the most exposed firms react to the reform by increasing employment in the technical jobs of the list, both in absolute terms and relative to employment in other occupations. This increase is, as expected, mostly driven by the growth in employment of migrant workers. As a result, they achieve higher growth in size, revenues and value added, while they do not show any change in productivity nor in capital stock. I further detect some crowding-in of non-technical employment concentrated among executive jobs, while none of the other socio-professional categories experience crowding-out. The performance gains are larger for smaller and younger firms, while businesses with initially low levels of productivity and businesses located in slack labor markets see even a positive effect on productivity. This suggests that the policy played the role of an equalizer, since it was particularly beneficial for the most vulnerable companies, whose growth potential was significantly undermined by skill constraints. In terms of elasticities, a 1% increase in migrant employment within technical occupations increases firm size by 0.5%, revenues by 0.65%, value added by 0.5% and non-technical employment among executives by 0.6%. The analysis based on a direct measure of sectoral demand shocks yields very similar results, and reveals that the policy reduced constraints by about two thirds relative to the frictionless benchmark. Finally, regressions considering outcomes at the level of employment zones and sectors reveal that, in the first two years, the positive effects are also visible on aggregate, but they die out afterwards.

This paper relates to the literature studying the role of frictions in the labor market of high-

³Sector-specific demand shifts are computed as a Bartik instrument that weights the GDP growth of all countries other than France by the share of French exports in a given sector that are purchased by them.

skill workers (Hosios, 1990; Shimer, 2005; Chen, Chen and Wang, 2011). Bound et al. (2013) find that the local supply of engineers is slow to adapt to demand shocks, and that the growth in supply elasticity observed in the US over recent years was mostly driven by greater inflows of migrant workers. Jäger and Heining (2016) show that, following the sudden death of a worker, firms increase the wages of the other employees, especially when the occupational market in question is thin. Both these contributions refute the assumption of frictionless labor markets in the high-skill segment. Ryoo and Rosen (2004), on the contrary, find that individuals' choice to study engineering is highly responsive to labor market conditions and adapt quickly, suggesting that public policies aiming to increase training in technical competencies are unnecessary. I contribute to this body of work by quantifying the extent to which skill shortages in highly demanded competencies can hamper firms' growth potential; and by showing how reforms broadening skill availability can significantly increase market efficiency through the reduction of these frictions.

A related strand of the literature looks at the productive externalities generated by inflows of skilled migrants (Bound et al., 2015; Peri, Shih and Sparber, 2015; Peri, 2016). The analyses measuring the effect of these shocks at the firm level are still scarce, despite the fact that many of the evaluated policies directly aim to support businesses (Kerr, Kerr and Lincoln, 2015). In recent years, however, there has been some progress in this direction. Notably, Kerr and Lincoln (2010) and Beerli et al. (2018) find that high-skill immigration has positive effects on innovation at the firm level, in the US and Switzerland respectively. Giesing and Laurentyeva (2018) show a drop in firms' productivity in Eastern Europe following the massive emigration of skilled workers generated by the EU enlargement, and Mitaritonna, Orefice and Peri (2017) find that local immigration flows in France increase firms' total factor productivity and their capital stock. On the other hand, Doran, Gelber and Isen (2014) find that US firms obtaining one additional H-1B worker do not see any increase in innovation and may crowd-out domestic employment.⁴ My findings lay somewhat in-between, since they rule out crowding-out and show a positive effect on firm performance, but do not detect significant productivity gains nor changes in capital stock. In addition, the

⁴The H-1B is a large visa program in the US designed to encourage immigration of STEM workers and often used by the literature to identify how high skill migration affect the labor market. See for example Kerr and Lincoln (2010); Ghosh, Mayda and Ortega (2014); Peri, Shih and Sparber (2015); Mayda et al. (2018).

context of this paper allows to test the impact of migrant flows that are finely targeted towards the occupations facing hiring difficulties, as opposed to the more general increases in migrants with high levels of education that are typically considered in the literature.

I further contribute to the body of work looking at how sudden shifts in skill availability foster changes in the choices of technology used and in the demand for other labor and capital inputs according to the degree of complementarity between them (Lewis, 2011; Peri, 2012; Dustmann and Glitz, 2015). My findings are consistent with a production function characterized by approximatively constant returns to scale, where technical labor inputs are complementary to non-technical high-skill inputs, and capital is fixed (at least in the short run). Finally, this paper is a complement in scope to Signorelli (2019), where I analyze the effect of the same reform on the wages and employment opportunities of natives working in the occupations targeted by the policy, which face a direct increase in competition.

The remaining of the paper is organized as following: Section (2) describes the characteristics of the reform, Section (3) discusses the data, Section (4) lays out the empirical strategy, Section (5) presents the main findings, Section (6) describes the analysis based on the direct measure of demand shocks and presents the results, and Section (7) concludes.

2 The Reform

In France, the labor law gives priority to current residents and EU nationals in the hiring process. If an employer wishes to hire a non-European citizen that does not currently reside on French territory, he has to apply for a work authorization that is granted only if two conditions are met. First, the employer must prove to have searched extensively for a resident candidate. Second, the occupation under question must be subject to high labor market tightness as reported in the statistics collected by the French Employment Office.⁵ The tightness indicator varies by region and occupation, and measures the ratio between the number of available vacancies and the pool

⁵Institution in charge of supporting unemployed workers in their job search.

of unemployed workers possessing the required competencies for the job. A high level of tightness thus signals that a given position is hard to fill. Additionally, the authorities verify that the diplomas and work experience of the candidate fit the qualifications required by the occupation and that the wage offered by the firm is in line with the industry's standards. If all these conditions are met, the employer receives the authorization to hire the foreign candidate, and the latter can apply for an economic visa. The procedure is known to be lengthy and burdensome for firms. While the official time to process requests is set to a maximum of two months, it can take much longer in practice, especially when the institutional bodies involved in the final decision are in disagreement (OECD, 2012).

In January 2008, the French government introduced a legislative decree that facilitates the hiring of extra-Europeans within a list of 30 occupations characterized by structurally high levels of tightness. The new law states that, for the occupations concerned, the employer is not anymore required to prove the prior search for a priority candidate but is automatically granted the authorization to hire a migrant worker. This change represents a substantial reduction in the processing time and of the uncertainty surrounding the procedure. In a first step, the 30 occupations are defined at the national level, and in a second phase each region selects a subset of them that remains tight at the local level. Table A1 in appendix reports the full list of jobs included as well as the number of regions, out of 22, to which each of them applies, while Figure A1 displays their distribution across broad occupation categories and sectors. The type of competencies targeted by the intervention can be classified into 3 socio-professional levels: i) executives and engineers, ii) technicians and foremen, and iii) blue collar workers. The majority of the list concerns industrial technicians, mostly in mechanics and metal processing, electricity and electronics, and construction. A minority concerns engineers and executives (notably computer scientists), and skilled blue-collar workers (notably machinery operators). Figure A1.a shows that the jobs affected by the reform are the most prevalent in the heavy industry sector (over 20%), followed by wholesale and retail and IT services (around 15% each).

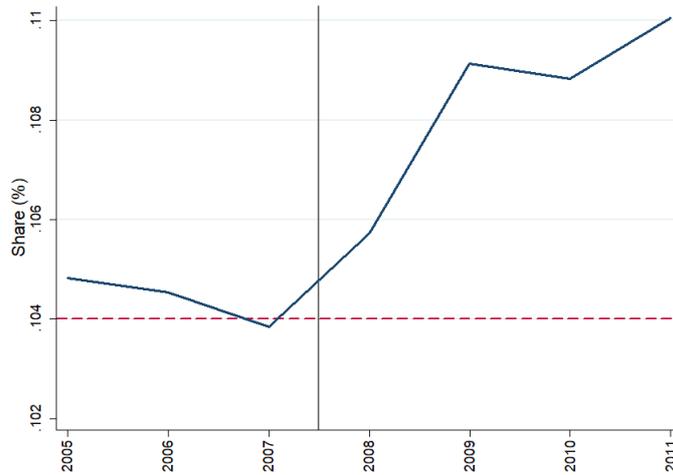
To identify structurally tight occupations, the policy makers relied on a list of indicators collected by the French Employment Office capturing the volume of available vacancies in a given

region and occupation, and the number of registered unemployed workers qualified for the job. The main indicator considered measures the ratio between the two (tightness index), but they also took into account the recent evolution in the number of both vacancies and unemployed people and the share of open ended contracts among vacancies. The latter was done to ensure the selection of valuable occupations with a structural lack of competent candidates, rather than jobs that people are just not interested in taking. I use the full set of measures considered by the policymaker to construct a composite index of tightness.⁶ Figure A2 in appendix shows the evolution in the average tightness index in reform and non-reform occupations during the years preceding the policy change, separately for each socio-professional level. We can see that the occupations in the list have structurally higher levels of tightness, at least among the executives/engineers and technicians/foremen categories, and that the difference became increasingly prominent during the period leading to the policy change (2003-2007). This is consistent with a context of fast growing demand for these skills that cannot be matched by supply at the current prices, as defined by Blank, Stigler et al. (1957). Given that the severity of these shortages vary across geographic areas as well as across sectors, firms are more or less constrained by these frictions depending on where they are located and what is their main economic activity. I exploit these differences in exposure in the identification strategy.

Figure 1 shows the evolution in the share of private sector employment made of reform jobs. Before the policy introduction, they accounted for about 10.5% of total employment and, if anything, they were slightly declining. In the four years following the reform they increase sharply and reach 11% by 2011. While I cannot claim that all of the increase is generated by the policy, especially given that the financial crisis that hit France in 2009 might have affected these jobs differently than others, this graph provides a first piece of descriptive evidence that the labor market did react to the regulatory change.

⁶The 6 indicators considered to define the reform are i) the tightness index, ii) the volume of job offers iii) its growth rate, iv) the volume of qualified job seekers v) its growth rate, vi) the rate of open-ended contracts within all vacancies. I standardize each of them as following: $z_m = \frac{x_m - \mu_m}{\sigma_m}$, where x_m represents each observation of the variable m , μ_m represents its average and σ_m its standard deviation. The composite index is then computed by taking the simple average over the standardized scores: $Index = \frac{1}{6} \sum_{m=1}^6 z_m$.

Figure 1: Share of reform occupations in employment



Note: The figure shows the evolution in the share of total employment in the private sector accounted for by the occupations of the reform, before and after the policy change.

3 Data

The analysis relies on data combining two administrative sources. The first one are the French payroll tax records (*DADS poste*), which contain information on each existing job within each private sector establishment in France, including the detailed occupation code, the salary and the number of hours worked. With this data, I can compute the volume of employment within the occupations of the reform and the volume of employment in non-affected jobs, and this for every establishment and across years. The data also includes an indicator for whether each employee is a French national or not. This allows me to test the first stage: that the reform impacts the size of migrant employment in the occupations concerned by the policy change. A caveat in this analysis, however, is that I cannot observe whether a foreign worker is hired through an economic visa, nor I can know his precise nationality or region of origin. Given that European workers are allowed to be employed in any job without restrictions and even extra-Europeans, if they are in France thanks to a family reunion visa, are allowed to work anywhere in the private sector, this indicator has to be taken as an imperfect proxy for the group directly affected by the reform.⁷

⁷The family reunion visa is the most common port of entry into France. Every year about 100 thousands new visas are issue for this reason while only 20 thousands are issued for economic reasons.

The second source contains the financial records of each private sector business (*FICUS-FARE*), which gives information on the value of sales, revenues and value added generated by the firm in a given year, as well as on the value of its capital stock, both tangible (machinery and buildings) and intangible (e.g. brand name and patents). Both administrative sources are collected yearly by the fiscal authority and thoroughly verified, since they are used to calculate the amount of income and capital taxes to be collected. Given that the first dataset is reported at the establishment level and the second is at the firm level (the legal entity), I restrict the sample to mono-establishment firms, such that the two datasets coincide. Firms with less than 20 employees are also dropped from the sample because they were not required to report detailed occupation codes before 2008, and so it is not possible to compute the volume of employment affected by the reform for them. Finally, the financial services sector has to be excluded because it does not appear in the financial records data. The period of analysis considered spans from 2005 to 2011.

In addition to the two main datasets, I use a subset of the payroll tax records allowing to follow a sample of employees over time (*DADS panel*) to construct the rate of job to job mobility within each employment zone.⁸ The latter is used in the analysis to capture the heterogeneity of the effect across geographic areas with more or less dynamic labor markets. Finally, I use the data containing the tightness indicators used to define the list of the reform to construct the measure of exposure to the policy change.

4 Empirical Strategy

The main analysis estimates the effect of relaxing skill constraints on firm performance and on demand for other inputs. This is interesting for multiple reasons. First, the observed degree of response reveals something about the costs that were associated with such frictions ex-ante: If firms were able to compensate for the lack of competent labor by hiring workers with similar characteristics and by training them in-house, we would expect the reform to generate some

⁸The DADS panel allows to follow 1/12th of individuals in the labor force through time and across employment spells.

substitution with other labor inputs and little difference in output. On the other hand, if the constraints were giving rise to bottlenecks in production, we would expect a much larger effect on output and possibly some crowding-in of complementary inputs. Second, the elasticity of output and other inputs with respect to the increase in employment in tight occupations reveals information about the structure of the underlining production function. Notably, it allows to say something about the shape of the returns to scale and about the degree of complementarity or substitutability between inputs. Third, by looking at the differential effect of the reform across local labor markets, it allows to infer the impact of the policy on spatial inequality in production and productivity.

In the ideal scenario, I would observe the degree of skill constraints specific to each firm, in order to get a firm-specific measure of exposure to the reform. However, given that the data on vacancies is only available at a more aggregate level, I need to find a proxy to measure the extent of skill shortages faced by different employers. I thus exploit preexisting variations in tightness of reform employment across sectors (ca. 70 categories) and commuting zones (ca. 350 units) where the business is located. Given that firms can switch sectors and move location, and that such changes can be endogenous to the policy change, I consider for each firm the sector and location of first appearance in the data and fix it throughout the period. Treatment exposure is computed as following:

$$Exp_{zs} = \sum_{o=1}^{30} \omega_{ozs} \Theta_{or} \quad (1)$$

Where exposure Exp_{zs} for employment zone z and sector s is calculated as the average tightness across each target occupation o observed in the region r where the employment zone z is located (Θ_{or}), weighted by its share in total reform employment observed in that sector and employment zone (ω_{ozs}). The indicator is computed on data from 2004 to 2007. The higher the value of Exp_{zs} , the more employers were constrained by the scarcity of these skills, thus the more the reform is expected to bite.

The main analysis relies on a difference-in-differences identification with continuous treatment. This approach yields an unbiased estimation of the effect of the reform under the assumption that employers more or less exposed to it would have evolved on similar trends in the absence of the policy change. There are many reasons that could cause the output of different sectors to follow different paths, especially in a period that includes the financial crisis. To control for these factors, I include sector x year fixed effects in all regressions. Variations in exposure to the reform are still identified thanks to geographic differences in the baseline tightness observed within sectors. It may still be the case, however, that firms located in areas with more severe skill shortages experience different paths in performance precisely because they have a harder time to access human capital and regardless of the effect of the policy. To maximize the comparability across geographic areas, I include a control for tightness in other occupations not part of the list, which is computed analogously to Equation (1). The common trend hypothesis is thus verified if firms operating in the same sector that are located in commuting areas with similar levels of overall tightness but different levels of shortages in reform occupations would have evolved similarly in absence of the policy change. The reduced form model can be formally written as following:

$$\ln(Y_{izst}) = \beta_0 + \beta_1 Exp_{zs} * post_t + \beta_2 Tight_{zs}^{ctr} * post_t + \gamma_i + \delta_{zs} + \omega_{st} + \epsilon_{izst} \quad (2)$$

Where $\ln(Y_{izst})$ captures the logarithm of outcome Y for firm i operating in sector s and employment zone z at time t . $post_t$ is a dummy equal to one for the years following 2008 and β_1 is the coefficient of interest. In the regression tables, I report two separate coefficients for the first two years of post-period (2008-2009) and the rest (2010-2011), to get a sense of the short and medium term effects. $Tight_{zs}^{ctr} * post_t$ controls for changes in trend explained by tightness in other occupations in the area. Finally, I include firm fixed effects (γ_i), employment zone x sector fixed effects (δ_{zs}) and sector x time fixed effects (ω_{st}). Standard errors are clustered at the employment zone x sector level and the model is estimated using OLS. Given the presence of firm fixed effects, the analysis is performed on the sample of firms already present in the dataset before the policy introduction and thus all the effects have to be understood in terms of intensive

margin. To capture the effect of the reform on firm entry and exit, in one subsection I also show the results obtained by aggregating the data into sector x employment zones cells.

This approach gives up some of the meaningful variation in reform exposure, such as the one across sectors, in order to ensure unbiasedness of the results. Consequently, the reduced form coefficients only capture a portion of the total effect of the policy, such that their sign and significance are more informative than their magnitude. For this reason, in a second step, I use the reduced form model as a first stage in a 2SLS procedure that recovers the elasticity of output and other inputs to changes in employment of reform occupations. The latter allows to recover more economically meaningful parameters. For this analysis, I exploit the fact that the policy creates quasi-exogenous variation in the size of firms' technical employment through the supply shock in migrant workers ($MigTech_{izst}$). In the second stage, the predicted value for the stock of migrant workers in technical occupations is regressed on the other outcomes. Formally:

$$\ln(MigTech_{izst}) = \beta_0 + \beta_1 Exp_{zs} * post_t + \beta_2 Tight_{zs}^{ctr} * post_t + \gamma_i + \delta_{zs} + \omega_{st} + \epsilon_{izst} \quad (3)$$

$$\ln(Y_{izst}) = \alpha_0 + \alpha_1 \ln(\widehat{MigTech}_{izst}) + \alpha_2 Tight_{zs}^{ctr} * post_t + \zeta_i + \sigma_{zs} + \phi_{st} + v_{izst} \quad (4)$$

To increase the strength of the instrument, the effect is only computed on the first two years following the reform and the sample is restricted to firms that employ at least one migrant in reform occupations in at least one year over the period. α_1 recovers the elasticities of interest. The exclusion restriction relies on the assumption that the effect of the reform on output and other inputs passes entirely through the channel of migrant employment in technical occupations. Given that the only legal change introduced by the policy is to make it easier for employers to hire foreigners in this set of occupations, the identification assumption seems reasonable in this context. In a robustness test, I instrument overall employment in reform occupations rather than only the migrant one, and preserve the entire sample. Finally, in the last section of the paper, I

show the results from an alternative identification strategy relying on a direct measure of sectoral demand shocks. This approach allows to test whether more constrained firms were under-reacting to increases in product demand before the policy change, and to measure the extent to which they catch up with the frictionless benchmark afterwards. More details on this procedure are provided in Section 6.

Table 1 reports the summary statistics characterizing the sample used for the analysis, which contains about 75'000 firms and a total of about 400'000 observations. On average, reform occupations constitute 9% of total firm employment and migrants account for 6% of the employees in these jobs. Firms employ on average 50 employees and have revenues amounting to 12 millions Euros.⁹ Firms operating in tighter markets employ a slightly higher share of reform occupations, among which there is a slightly higher proportion of migrant workers. Firms in markets with mid-level tightness obtain slightly higher revenues and value added per workers, but overall the performance across the three tertiles is fairly similar.

Table 1: Summary Statistics

VARIABLES	Overall Sample		Tertiles of tightness in reform occupations		
	mean	sd	1st	2nd	3rd
Share of reform occup. In empl.	0.09	0.17	0.08	0.08	0.13
Share of migrants in ref. empl.	0.06	0.19	0.05	0.06	0.07
Migrant reform empl.	0.39	3.10	0.33	0.31	0.58
Native reform empl.	4.84	16.68	4.98	4.45	5.79
Other employment	44.59	64.65	48.87	47.89	39.10
Wage in ref. empl. (€2010)	22.0	12.5	20.4	22.9	22.6
Wage in other empl. (€2010)	17.8	7.2	17.3	18.7	18.0
Firm size	52.43	79.65	56	56	48
Revenues ('000€2010)	12220	35517	11646	16349	9857
Revenues per worker ('000€2010)	234	712	202	309	208
Value added ('000€2010)	3167	7293	3118	3669	2970
Value added per worker ('000€2010)	62	135	58	70	61
Tangible assets ('000€2010)	4051	22519	4444	3842	4169
Intangible assets ('000€2010)	617	3817	470	942	483
Tightness in reform occupations (sec x zemp)	0.18	0.08	0.08	0.18	0.28
Tightness in other occupations (sec x zemp)	-0.04	0.07	-0.04	-0.06	-0.04
N. Obs.	414.573		141.919	139.064	133.185
N. firms	74.469		25.261	25.464	24.411

Figure A3 in appendix shows unconditional evidence for the first stage effect of the reform.

⁹Given that firms with less than 20 employees and multi-establishment businesses are excluded, the sample is representative for medium sized employers.

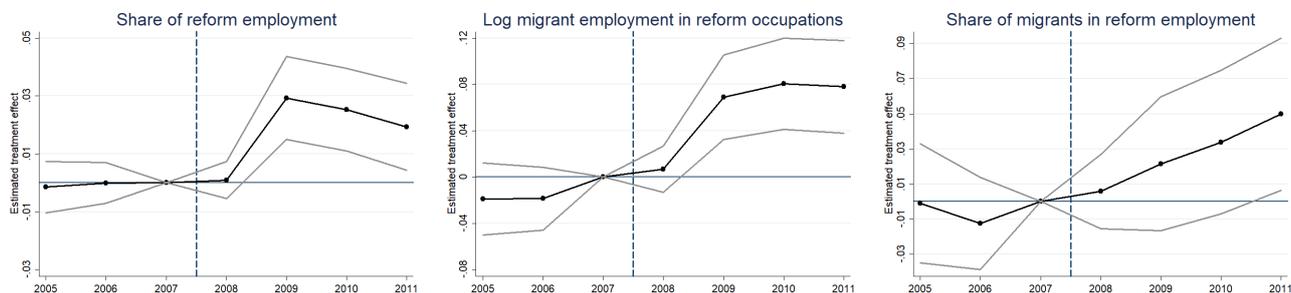
It presents the evolution of the size of reform employment (A3.a) and of the share of reform employment (A3.b) for groups of firms characterized by different levels of exposure to the policy. Firms suffering from severe shortages are also characterized by slightly higher growth rates in technical employment before 2008, which is in line with the fact that vacancies are growing faster than the number of available candidates. For this reason, the main specification includes some controls to ensure comparability. Nonetheless, the difference in trends prior to the reform is much smaller than the one observed afterwards, where greater exposure is strongly correlated with higher growth in targeted technical jobs. If we look at the share of total employment made of reform occupations, all groups of firms follow very similar paths before 2008, while the most exposed ones see a much larger increase after the introduction of the policy. This provides the first piece of evidence that constrained firms did take advantage of the policy by hiring more workers within target jobs. The next section presents the results from the main empirical analysis.

5 Results

5.1 Reduced Form Results

Figure 2 shows the event study graphs reporting the coefficients for the interactions between the exposure indicator and a battery of dummies for each year of the sample, which are obtained from regressions that include all the controls specified in Equation (2). The left panel reveals that, before 2008, the share of employment made of reform occupations evolved in parallel across firms with different degrees of exposure, while after the policy change more exposed firms experience a much sharper increase. The center panel shows a similar picture for the log of migrant employment in reform occupations, and the right panel confirms that previously constrained firms see a surge in the share of migrants occupying these jobs. All-together, these pictures corroborate the claim that firms located in markets suffering from more severe shortages in technical competencies react to the reform by hiring more workers possessing these skills. They also validate the common trend assumption required for difference-in-differences estimations.

Figure 2: Event study graphs on first stage effect



Note: The black line shows the coefficient obtained on the interaction between treatment exposure and each year fixed effects. The two grey lines report the 95% confidence intervals.

Table 2 reports the main coefficients obtained from the estimation of equation (2) on the first stage outcomes presented in Figure 2. Appendix Figure A4 and Table A2 show the same thing for additional first stage outcomes. To ease the interpretation of the magnitudes, I report at the bottom of each table the estimated marginal effect of the reform for a firm at the mean of the exposure distribution.¹⁰ As already visible from Figure 2, the more a firm is exposed to the reform, the more it increases its stock of employment in treated occupations and the share of migrants employed in these jobs. The increase in the stock of employment is significant both among migrants and natives in the short run, but in the medium run it remains significant only for migrants and gets stronger in magnitude, which is consistent with what we expect from the policy. The average wages in reform occupations decrease following the legal change while salaries in other occupations do not change. For an averagely exposed business, the reform increases the share of employment made of technical occupations by 0.4 percentage points (a growth of 6%), the share of migrants employed in technical occupations by 0.7 percentage points (a growth of 8%) and the number of migrants working in technical occupations by 1.6% in the medium run.

Next, I look at what happens to the main outcomes of interest: firm size, revenues, value added, productivity and demand for other inputs. Figure 3 reports the event study graphs. Here confidence intervals are larger since I am considering second order effects of the reform. Nonetheless, I find a significant increase in employment, revenues and value added after the

¹⁰A firm at the mean of the exposure distribution faces a level of tightness that is 0.18 standard deviations above average.

Table 2: Reduced form results on first stage

	(1)	(2)	(3)
	Share of reform empl.	Log migrant ref. employment	Share of migrants in ref. employment
VARIABLES	OLS	OLS	OLS
Exposure * 2008-09	0.0158*** (0.00431)	0.0494*** (0.0124)	0.0152 (0.0126)
Exposure * 2010-11	0.0233*** (0.00692)	0.0909*** (0.0189)	0.0418** (0.0189)
Observations	386,135	386,135	207,347
R-squared	0.862	0.749	0.655
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>			
Marginal effect ref. 2008-09	0.00285*** (0.000775)	0.00889*** (0.00223)	0.00274 (0.00226)
Marginal effect ref. 2010-11	0.00420*** (0.00125)	0.0164*** (0.00340)	0.00752** (0.00340)

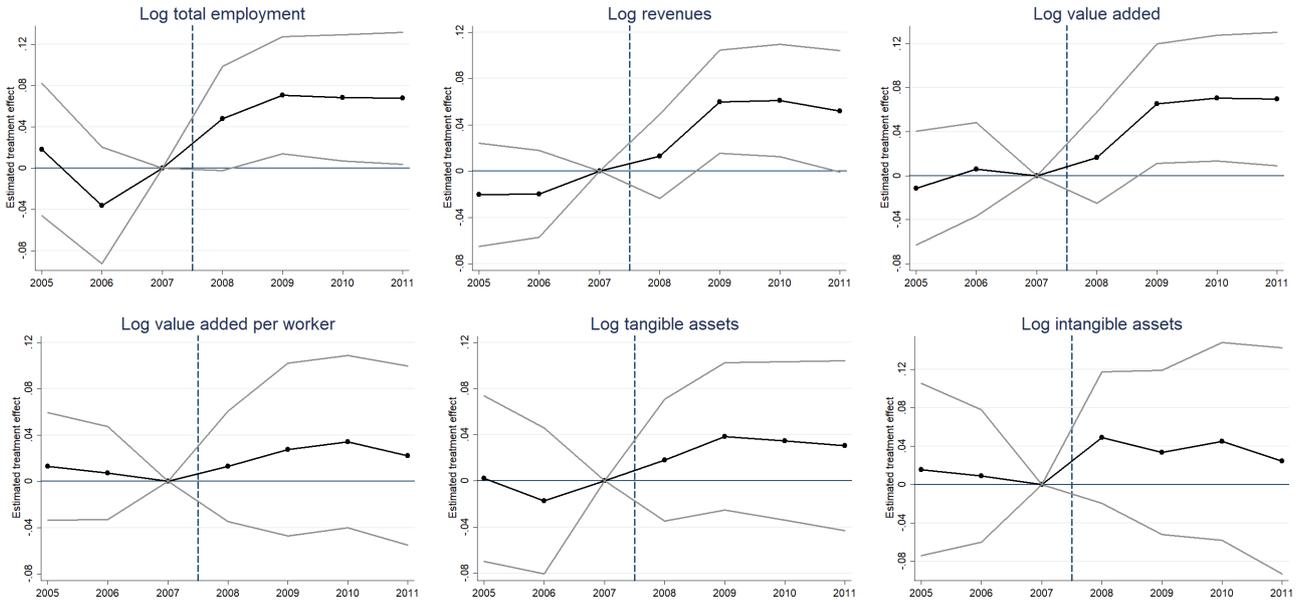
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered at the employment zone x sector level. Controls for tightness in other occupations interacted with post reform period, firm FE, employment zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. The logarithm of migrant reform employment has been taken on the value+1 to keep the zeros in the analysis.

implementation of the reform, while there are no differences in pre-trends in any of these measures. The evolutions observed after 2008 mimic the trend shown by the stock of migrant employment in reform occupations, consistent with the first being the trigger of the others. While productivity (measured through the value added per worker) and capital stock (both tangible and intangible) also follow a positive tendency after the introduction of the policy, here the effects are too noisy and thus not significant.

In Table 3 I report the short and medium term coefficients associated with treatment exposure as well as the coefficients associated with the controls for tightness in other occupations interacted with the post-reform periods. Once again, the estimated marginal effect for a firm with mean exposure is reported at the bottom of the table. The effect of relaxing skill constraints on firm size is positive and significant and seems to grow over time. The same is true for revenues and value added. In the medium run, firms with mean exposure grow their size by 1.4% and their revenues and value added by 1.3% thanks to the reform. Consistent with what observed in the event study graphs, the effect on the other outcomes is also positive but not significant. Interestingly,

Figure 3: Event study graphs on main outcomes



Note: The black line shows the coefficient obtained on the interaction between treatment exposure and each year fixed effects. The two grey lines report the 95% confidence intervals.

the coefficient associated with tightness in other occupations is either not significant or negative, except for a positive and significant coefficient on value added that disappears in the medium run. This refutes the hypothesis that what is observed is merely a mechanical effect explained by tightness itself rather than by the reaction to the reform. The magnitudes might seem rather small, but this is not surprising given that the reduced form approach picks up an average effect across sectors, some of which are much less affected than others, and partials out all the effect coming from differences across sectors. The analysis of the elasticity of the outcomes to changes in technical employment will provide more information on the fundamentals underlining these effects.

Finally, Table 4 explores the effect on the demand for other labor inputs. Column (1) pools together all the employment not included in the list of the reform while columns (2) to (5) break it down in the four socio-professional categories: executives, intermediate professionals, clerk and sales personnel, and blue collar workers. The first important finding arising from this analysis is that there is no crowding-out in any other type of employment. The latter goes against the hypothesis that firms suffering from shortages were able to compensate by hiring workers in other

Table 3: Reduced form results on main outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.0676*** (0.0224)	0.0514*** (0.0180)	0.0439** (0.0216)	0.0162 (0.0241)	0.0357 (0.0393)	0.0309 (0.0257)
Exposure * 2010-11	0.0765*** (0.0273)	0.0724*** (0.0241)	0.0741*** (0.0281)	0.0243 (0.0336)	0.0282 (0.0556)	0.0374 (0.0340)
Tightness in other occup * 2008-09	-0.0729* (0.0403)	-0.00586 (0.0363)	0.100** (0.0434)	0.0715 (0.0601)	-0.0481 (0.0732)	-0.0469 (0.0545)
Tightness in other occup * 2010-11	-0.117** (0.0513)	-0.0353 (0.0459)	-0.00854 (0.0517)	0.00414 (0.0833)	-0.231** (0.110)	0.0208 (0.0659)
Observations	385,623	372,559	369,143	361,774	335,565	363,560
R-squared	0.844	0.953	0.905	0.777	0.951	0.965
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>						
Marginal effect ref. 2008-09	0.0122*** (0.00404)	0.00925*** (0.00324)	0.00790** (0.00388)	0.00292 (0.00434)	0.00642 (0.00707)	0.00557 (0.00462)
Marginal effect ref. 2010-11	0.0138*** (0.00492)	0.0130*** (0.00433)	0.0133*** (0.00506)	0.00437 (0.00604)	0.00507 (0.0100)	0.00673 (0.00611)

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

Standard errors clustered at the employment zone x sector level. firm FE, employment zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011.

occupations and training them in-house. On the contrary, it seems that shortages played the role of bottlenecks in production, holding back firms from hiring workers in other jobs as well. When I look at which categories of jobs are complementary to the ones of the reform, I find that the largest positive reaction comes from executives, but intermediate professionals start to benefit as well from crowding-in the medium run. Low skill workers, both in administrative and blue collar jobs, are not affected by the sudden availability of technical workers. This results underline that, even though the occupations of the reform are rather skilled, they seem to be more complementary to other high-skill positions than to low-skill ones. This might be explained by the fact that additional technicians and engineers might require administrative managers to organize their work.

Tables A3, A4, and A5 in appendix test the robustness of the results to defining exposure at the region x sector level instead of doing it at the employment zone level. The latter has the advantage of coinciding with the level of variation observed in the tightness data, but the disadvantage of pooling together several distinct labor markets that might in practice face very different conditions. The results obtained are similar to the ones presented in the main analysis, except that the effect on capital stock, both tangible and intangible, becomes significant and

Table 4: Crowding-in of other employment

	(1)	(2)	(3)	(4)	(5)
	Log non-reform employment				
	Total	Executives	Intermediate professionals	Clerk and salesmen	Blue collar workers
VARIABLES	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.0431* (0.0249)	0.0787*** (0.0242)	0.0348 (0.0304)	0.0347 (0.0253)	0.0187 (0.0287)
Exposure * 2010-11	0.0217 (0.0300)	0.0667* (0.0350)	0.108** (0.0419)	0.0188 (0.0348)	0.0217 (0.0410)
Observations	385,372	386,135	386,135	386,135	386,135
R-squared	0.827	0.910	0.871	0.908	0.933
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>					
Marginal effect ref. 2008-09	0.00700* (0.00403)	0.0142*** (0.00436)	0.00627 (0.00547)	0.00625 (0.00455)	0.00337 (0.00517)
Marginal effect ref. 2010-11	0.00267 (0.00493)	0.0120* (0.00629)	0.0194** (0.00755)	0.00339 (0.00627)	0.00391 (0.00738)

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the employment zone x sector level. Controls for tightness in other occupations interacted with post reform period, firm FE, employment zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. The logarithm of native and total reform employment has been taken on the value+1 to keep the zeros in the analysis.

that the crowding-in is spread over more socio-professional categories. The magnitude of the effect for a firm with mean exposure is slightly larger as well. Taken together, the reduced form results indicate that skill constraints were imposing significant costs on the firms affected by them, impeding them to grow up to their full potential. The fact that I do not detect any substitution with other types of employment underlines that these positions are hard to fill with other types of workers, and that releasing such constraints generates positive spill-overs on the overall economy.

5.2 Heterogeneity

Tables A5 and A6 explore the heterogeneity of the effect across firm size and age, respectively. While size and age are significantly positively correlated, the correlation coefficient is only 0.15, signaling that these two dimensions identify different types of businesses. The sample of firms in the data is divided into two groups according to the number of employees that they declare on the first year of appearance, for the first table, and according to the number of years since firm creation, for the second table. In this way group assignment is fixed within a given entity and thus do not vary endogenously with the reform. The category of small firms includes establishments with an average of 28 employees, while large firms have an average of 66 employees. Young

firms have on average 11 years of existence, while old firms have on average 28 years. The main specification is then estimated on each subsample separately. Results show that the gains in performance are much larger for small and young businesses. In the medium term, the averagely exposed firm within the group of small firms sees a 2% increase in employment, a 1.9% increase in revenues and a 2.3% increase in value added thanks to the reform. For large businesses, the gains are only of 1% in both size and revenues and there are no significant improvements in value added. Similarly, young firms see an increase of about 2% in employment, revenues and value added, while old firms only experience an increase in revenues and value added of about 1% in the medium term, which is only marginally significant. In terms of productivity, however, there is still no significant effect of the reform even for small and young companies.¹¹ Finally, table A7 does a similar split of the sample along the productivity reported by each firm at their first appearance in the data (productivity is measured as value added per worker). Interestingly, firms with initially low and high levels of productivity benefit from the reform by a similar magnitude when it comes to employment size and revenues (about +1.3% in the medium term for the averagely exposed firm). Nonetheless, the effect on value added is more immediate for firms with low initial productivity, and the impact on productivity is positive and significant for them (+1.3%). Altogether these findings can be rationalized by the fact that, within a sector and employment zone severely affected by skill shortages, small, young and low productivity businesses are likely to be the ones having the most difficulty in accumulating the human capital that they needed. The constraints were more binding for them, and thus they are the ones benefiting the most from their alleviation.

Table A8 in appendix further explores the heterogeneity of the effect across labor market dynamism. Slack markets are identified through the rate of job to job mobility observed at the sector x employment zone level, which is computed using a subsample of the payroll tax data allowing to follow workers through employment spells.¹² Results show that constrained firms in slack labor markets benefit more than the average, and even experience significant growth in productivity thanks to the reform. In this sample, an averagely exposed firm sees 1.5% higher

¹¹The same is true for capital stock, where none of the groups see any increase in tangible or intangible assets. Results are available upon request.

¹²This dataset is known as the DADS panel and follows 1/12th of the French labor force across time and employment spells, allowing to construct rates of job to job mobility at different levels of aggregation.

growth in productivity in the medium run. The impact on employment size is of comparable magnitude with respect to the full sample, while the effects on revenues and value added are roughly the double. These results underline how a policy increasing access to scarce human capital through immigration is particularly beneficial for the most deprived geographic areas, where skilled native workers might be reluctant to move. This hints towards a reduction of spatial inequalities as a result of the reform. Taken together to the heterogeneity observed across firm size, age and initial productivity, these results show that the policy under study played the role of an equalizer of growth opportunities.

Finally, table A9 inquires whether the firms taking the most advantage of the reform are the ones that were already employing immigrant workers before the new policy, or the ones that had never done so. There are two competing hypotheses in this regard. One is that, given the presence of fix costs associated with going through the administrative process to hire extra-European workers, the reform allowed firms that had never employed foreign labor before to do so. According to this line of reasoning, firms already used to the system do not experience much of a change in 2008. The second one, however, goes in the opposite direction. The presence of foreign workers within the labor force might have increased firms' awareness of the legal change, and more generally might have played the role of attracting new migrants through network effects. In addition, firms already inclined to hire foreigners might be also more willing to take advantage of the policy. Table A9 reports the results obtained from triple-differences where the variable of exposure and the post-reform period are further interacted with a dummy equal to 1 if the business employs at least one foreign worker in any occupation during the period between 2005 and 2007. Results show that the effect of the reform on performance is mostly driven by firms that were already employing immigrant workers prior to the policy change. The network effect channel seems thus to dominate the possible fix costs involved when hiring a foreign worker.

5.3 Estimation of Elasticities

As previously discussed, the reduced form identification does not allow to measure the overall effect of the reform but only a partial one, which makes the sign and significance of the coefficients more informative than the magnitude itself. In order to recover more economically meaningful parameters, I use the main specification as a first stage in a 2SLS allowing to estimate the elasticity of output and other inputs to quasi-exogenous variation in employment in technical occupations, as described in Equations (3) and (4). Given that the policy has a direct impact only on the employment of foreigners within the occupations of the list, this is the variable that is instrumented in the first stage. To increase the strength of the instrument, I restrict the sample to firms employing at least one migrant in these jobs in at least one year over the full period. Tables 5 and 6 report the main coefficients as well as the conventional IV tests for each of the outcomes of interest. In Tables A10 and A11 I test that the results are robust to instrumenting total employment in technical occupations using the unrestricted sample, and in Tables A12 and A13 I check that results are robust to measuring exposure at the region x sector level.

Table 5: Elasticities of Main Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log N. migrants in reform empl.	0.494** (0.204)	0.646** (0.253)	0.515** (0.248)	0.163 (0.200)	0.569 (0.459)	0.409 (0.265)
Observations	67,909	68,705	67,713	66,510	62,719	67,349
R-squared	-0.564	-0.881	-0.390	-0.062	-0.230	-0.255
RMSE	0.283	0.346	0.345	0.287	0.496	0.332
K-P Wald F-statistic	14.70	13.45	14.91	15.76	12.64	13.97

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

Standard errors clustered at the firm level. Controls for tightness in other occupations interacted with post reform period as well as firm FE, employment zone x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located, and the post reform period. To increase the strength of the instrument the sample is restricted to firms counting at least one migrant worker in reform occupations in at least one year over the period, and to the years 2005-2009 (effect on first two years of reform).

The interaction between exposure and post-reform period appears to be a strong instrument given the F-stats obtained from the Kleibergen-Paap Wald statistic. Consistent with the reduced

form estimations, I find that the relaxation of constraints in this set of competencies induces growth in firm size, revenues and value added, while nothing changes in the demand for capital stock and other labor inputs, except for executives. The elasticities obtained are fairly high: a 1% growth in the stock of migrant employment in technical occupations generates a 0.5% growth in firm size and value added, a 0.65% growth in revenues, and a 0.6% growth in demand for non-reform executive jobs. Given that employment size and revenues increase by a similar magnitude, it can be deduced that the underlining production function presents close to constant returns to scale. Capital appear to be fixed in the short to medium run, and executive employment shows strong complementarity with the jobs of the reform.

Table 6: Elasticities of Other Labor Inputs to Reform Employment

	(1)	(2)	(3)	(4)	(5)
		Log non-reform employment			
	Total	Executives	Intermediate professionals	Clerk and salesmen	Blue collar workers
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS
Log N. migrants in reform empl.	-0.0682 (0.278)	0.587** (0.291)	0.504 (0.331)	0.117 (0.269)	-0.270 (0.330)
Observations	71,113	71,567	71,567	71,567	71,567
R-squared	-0.007	-0.408	-0.210	0.002	-0.058
RMSE	0.383	0.411	0.434	0.349	0.420
K-P Wald F-statistic	14.19	14.00	14.00	14.00	14.00

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

Standard errors clustered at the firm level. Controls for tightness in other occupations interacted with post reform period as well as firm FE, employment zone x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located, and the post reform period. To increase the strength of the instrument the sample is restricted to firms counting at least one migrant worker in reform occupations in at least one year over the period, and to the years 2005-2009 (effect on first two years of reform).

When I instrument for overall employment in reform occupations and preserve the full sample, I obtain similar results, with the same set of positive and significant coefficients but slightly higher magnitudes. Similarly, the elasticities obtained when the instrument is computed as the regional level are very similar in magnitude and significance to those derived with the preferred approach. Contrary to what observed in the reduced form results using regional exposure, here the elasticity of capital stock and of other labor inputs besides executives are not significant. These findings reinforce the claim that the technical occupations targeted by the policy are very important in the production process, and that firms constrained by the scarcity of these skills have to give up some

of their growth potential. Once again, however, productivity does not seem to be significantly impacted, at least during the four years following the legal change.

5.4 Aggregate Effects

Up until this point, the results presented are computed from variations within firms already present in the sample prior to the policy change, and have thus to be understood in terms of intensive margins effects. One might wonder whether the impact of the reform observed at the firm level translates into higher growth at the industry and local area level. This might not be the case if benefiting firms steal some market away from their competitors, such that the gains cancel out on aggregate. The reform is also likely to have an impact on firms' entry and exit, which cannot be captured using the approach of the previous sub-sections.

To answer these questions, I apply the same specification reported in Equation 2 on data aggregated into sector x employment zone cells. Instead of looking at the effect of the reform on firm output, I now look at what happens to the performance of the entire industry in a given local labor market. In addition to the main outcomes, I also measure the effect of the policy on firm survival rate and on firm creation rate. Results are reported in Table 7. During the first two years following the introduction of the reform, I detect an expansion of aggregate employment, revenues and value added of a similar magnitude to the one observed at the firm level. It seems thus that the gains are generalized and that there are no pro-competitive effects offsetting them. On the medium run, however, the coefficients become smaller and lose their significance, which suggests that the macro-level gains are not long-lasting. Interestingly, in the short run there is a positive effect on firm survival rate, which is in line with the improvement in firm performance, but also a significant decrease in firm creation. This might be driven by the fact that stronger incumbent companies make it harder for new entrants, which could explain why in the medium term the aggregate effects disappear. Taking stock of all the results, it appears that the policy increases the performance of smaller incumbent firms and of firms located in more disadvantaged areas, but raises the entry barriers for new businesses.

Table 7: Aggregate effects on main outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	firm survival rate	firm creation rate
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.0891** (0.0392)	0.0944** (0.0461)	0.0996** (0.0462)	0.0488 (0.0320)	0.0308* (0.0186)	-0.0396** (0.0194)
Exposure * 2010-11	0.0806 (0.0537)	0.0756 (0.0649)	0.0866 (0.0612)	0.00592 (0.0369)	0.0303 (0.0186)	-0.0152 (0.0197)
Observations	60,810	59,507	59,337	58,994	60,849	60,849
R-squared	0.944	0.943	0.934	0.763	0.236	0.276
<i>Calculated marginal effect of reform at mean exposure (tightness=0.16)</i>						
Marginal effect ref. 2008-09	0.0143** (0.00628)	0.0151** (0.00738)	0.0159** (0.00739)	0.00781 (0.00512)	0.00493* (0.00297)	-0.00634** (0.00310)
Marginal effect ref. 2010-11	0.0129 (0.00860)	0.0121 (0.0104)	0.0139 (0.00979)	0.000947 (0.00591)	0.00484 (0.00298)	-0.00244 (0.00316)

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

Regressions aggregating outcomes at the employment zone x sector level. Employment zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone.

Table A14 shows the aggregate effects on first stage outcomes, revealing that the increase in the stock of technical employment is long lasting and so is the number of migrants employed in these occupations. Nonetheless, on aggregate, there is no significant increase in the share of migrants occupying these jobs. This is a bit puzzling, but a possible explanation might be that the areas that were the most affected by shortages also manage to attract more native labor with these competencies, thanks to the reform. Finally, Table A15 computes the elasticities derived from the increase in technical employment at the macro-level and shows that a 1% increase in the latter generates a 0.7% increase in total employment, a 0.74% increase in revenues and a 0.79% increase in value added during the first two years following the policy change. These magnitudes are slightly larger than what observed at the firm level.

6 Identification of demand shocks

Skill shortages can be represented through a labor market where the supply curve becomes completely inelastic after a certain threshold L^{MAX} , as pictured in Figure A5 in Appendix. If a positive shock in labor demand pushes the equilibrium above this limit, we get an under-response in terms of quantity of labor and an over-response in terms of wages, relative to the frictionless benchmark.

While theoretically the persistence of such imbalances is not well defined, as the adjustment of wages should eventually clear the market (Saint-Paul and Cahuc, 2009), in the French case wages are known to be rigid and capacity constraints in the technical education tracks can explain the incapacity to quickly respond to changes in certain skill needs. In addition, rapid technological change is creating big shocks in the demand for certain technical competencies commonly known as STEM¹³, and the education systems in many developed countries have been slow at catching up with it. From a theoretical perspective, the migration reform helps to increase the labor supply elasticity above the threshold L^{MAX} , bringing it closer to the frictionless environment and therefore decreasing the severity of the shortage. Another way to evaluate the impact of the policy is thus to test whether firms that were the most constrained by these shortages reacted less to positive demand shocks before 2008, and whether the gap in response shrinks afterwards. This approach provides some insights on the extent of constraints reduction generated by the policy change.

For this analysis, I need to define an exogenous measure of changes in sectoral labor demand. To do that, I construct a proxy for changes in global demand driven by the performance of France’s major trading partners. For each tradable sector, I measure yearly variations in external demand as the average annual GDP growth of all countries excluding France, weighted by the average share of French exports going to each of them. Similar approaches are commonly used in the trade and migration literature.¹⁴ More formally, the proxy for sector-specific global demand changes is computed as following:

$$GD_{st} = \sum_{c=1}^N \frac{Exports_{sc,2002}}{Exports_{s,2002}} GrGDP_{ct} \quad (5)$$

Where the distribution of exports of a given sector s across countries c is computed using data from the 2002 UN Comtrade Database and the annual GDP growth of each country is taken from

¹³Acronym for Science, Technology, Engineering and Mathematics, which are the fields encountering the largest growth in labor demand as a result of the recent technological change. The occupations included in the French reform under study are largely related to these competencies.

¹⁴See for example Bertrand (2004); Hijzen and Swaim (2010); Brücker and Jahn (2011).

the World Development Indicators of the World Bank. Appendix Figure A6 shows the correlation between annual growth in the sectoral share of employment and annual changes in global demand as measured through Equation (5). The left panel present the relation observed over the pre-crisis period while the right panel looks at the post-crisis period. In both cases, the correlation is significant at the 1% level and it is slightly stronger in the second period (0.32) than in the first (0.25). Overall this picture validates the relevance of this measure as a proxy for changes in sectoral demand. I further use this proxy to estimate the following econometric model:

$$Y_{izst} = \beta_0 + \beta_1 GD_{st} * Tight_{zs}^{trt} + \beta_2 Tight_{zs}^{trt} * post_t + \beta_3 GD_{st} * Tight_{zs}^{trt} * post_t + \gamma_i + \delta_{zs} + \omega_{st} + \epsilon_{izst} \quad (6)$$

Which can be interpreted as a triple differences approach. β_1 identifies the difference in response to demand shocks before the reform for firms operating in more constrained markets. This coefficient is expected to be negative for technical employment and positive for technical wages, as shown in the left panel of the diagram in Figure A5. Indeed, if firms in these markets hit L^{MAX} earlier, they will have larger wage adjustments and smaller employment reactions. On the base of the findings from the previous sections, I also expect outcomes such as revenues and value added to react less to demand shocks in more constrained firms, since they are hindered by the skill shortages. β_3 captures the change in the elasticity gap after the policy implementation, and it is expected to be positive for technical employment and the other main outcomes and negative for technical wages. Indeed, the policy is expected to reduce the constraints faced by the most exposed firms, bringing them closer to the frictionless benchmark. If the magnitudes of β_1 and β_3 are equivalent in absolute value but with opposite signs, it would signal that the initial difference in elasticity generated by different degrees of skill shortages is entirely offset by the reform.

Table 8 shows the results for employment and wages of reform and non-reform occupations. As predicted by the conceptual framework, before 2008 the more a firm was constrained, the less

Table 8: First Stage Results from Global Demand Analysis:

	(1)	(2)	(3)	(4)
	Log total reform employment	Log other employment	Log wages reform occup.	Log wages other occup.
VARIABLES	OLS	OLS	OLS	OLS
GD * Exposure	-0.247*** (0.0565)	-0.0518 (0.0426)	0.104*** (0.0368)	-0.0150 (0.0144)
Exposure * post 2008	-0.725*** (0.161)	-0.0502 (0.112)	0.265*** (0.100)	-0.0317 (0.0390)
GD * Exposure * post 2008	0.150*** (0.0469)	0.0796** (0.0397)	-0.0709** (0.0348)	0.0133 (0.0138)
Observations	186,897	186,223	116,430	186,301
R-squared	0.878	0.830	0.764	0.801

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

Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Tightness in reform occupations is measured at the level of employment zone x sectors while global demand changes are measured at the sectoral level. The latter are proxied by changes in GDP growth of all other countries except France weighted by the share of imports of a given sector going to each of them.

its employment in tight occupations reacted in response to positive changes in global demand (Column 1). This gap shrinks by about two thirds after the reform, but it does not close completely. The opposite happens to the wages paid in these jobs: more exposed firms responded more strongly to increases in demand before 2008 and the gap gets smaller afterwards (Column 3). For the stock of employment in other occupations, I find no significant difference between more and less constrained firms before the reform, but the former do react more to positive shocks afterwards, consistent with the crowding-in effect documented in the previous analysis (column 2). Finally, there are no significant differences in wage elasticity for non-reform jobs, neither before nor after the crisis (column 4), which reinforces the claim that what is observed for the wages of tight occupations is linked to the reform and not to some general characteristics of more constrained firms that are observable across all types of jobs.

Table 9 presents the findings obtained from estimating Equation (6) on the main outcomes of interest. The coefficients are in line with what expected: skill-constrained firms used to benefit less from increases in product demand, both in terms of revenues and value added, and the gap shrinks by a bit more than two thirds after the policy implementation. In addition to what already observed in the previous sections, here it seems that skill constraints were holding back firm's

investments in both tangible and intangible assets, even if the coefficients are only marginally significant.

Table 9: Main Results from Global Demand Analysis:

	(1)	(2)	(3)	(4)	(5)
	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	OLS	OLS	OLS	OLS	OLS
GD * Exposure	-0.0959*** (0.0298)	-0.0912*** (0.0353)	-0.00171 (0.0295)	-0.116* (0.0636)	-0.0506 (0.0348)
Exposure * post 2008	-0.202** (0.0792)	-0.190** (0.0945)	-0.0151 (0.0806)	-0.259 (0.165)	-0.0584 (0.0868)
GD * Exposure * post 2008	0.0764*** (0.0285)	0.0694** (0.0336)	-0.00553 (0.0294)	0.104* (0.0605)	0.0571* (0.0328)
Observations	184,250	182,288	179,877	165,180	177,210
R-squared	0.946	0.901	0.778	0.942	0.972

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

Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Tightness in reform occupations is measured at the level of employment zone x sectors while global demand changes are measured at the sectoral level. The latter are proxied by changes in GDP growth of all other countries except France weighted by the share of imports of a given sector going to each of them.

Finally, as a robustness test, I reconstruct the measure of global demand shocks using value added trade data instead of gross export flows. The latter is computed by the World Input-Output Database and takes into account flows in intermediary inputs not captured by the gross export measure. It also has the advantage of calculating this value for many non-tradable sectors, but the level of aggregation across economic activities is larger and thus loses some meaningful variation. Results obtained with this alternative measure are reported in Appendix Tables [A16](#) and [A17](#), and broadly corroborate what shown in the preferred specification. With this approach, I obtain that the reduction in employment, revenues and value added constraints is even larger, approaching 80%.

7 Conclusion

This paper exploits the unique setting generated by a selective immigration reform that induced an increase in the inflow of migrant workers with a specific set of rare skills, to evaluate how firms

react to the relaxation of skill constraints. This question allows to get a sense of the costs that are associated with such shortages and of the features of the underlining production function. In addition, by evaluating the impact of the reform across firms and labor markets with different characteristics, this paper is able to say something about the re-distributive impact of this policy. Results show that firms previously constrained by the scarcity of these competencies react by increasing employment in reform occupations, which translates into higher employment in other jobs (crowding-in effect), higher revenues and higher value added. Productivity, on the other hand, does not react to the policy change. I can calculate that, thanks to the reform, constraints are reduced by about two thirds. The impact on the main outcomes is more pronounced for smaller and younger businesses, but the effect on productivity remains not significant for them. On the contrary, companies with initially low levels of productivity and firms located in more deprived labor markets do experience a significant increase in productivity. This reveals that the policy played the role of an equalizer, since it helped more the most vulnerable businesses. Results are initially visible at the macro-level as well, but they die out after the first couple of years.

These findings carry important policy implications. Skill shortages seem to be holding back the growth capacity of firms and immigration appears as a viable solution to relax them. While several Western countries have recently implemented different forms of selective immigration policies and debates are currently ongoing to reinforce these schemes, populist parties all across Europe and North America, often supported by citizens living in more deprived areas, are increasingly opposing all incentives meant to favor immigration, arguing that they deteriorate the labor market opportunities for natives. This paper shows that increasing the immigration of workers with rare and highly demanded competencies not only boosts the performance of domestic firms, but does so relatively more in the most disadvantaged areas, where we observe growth in productivity. This provides a strong argument for the reinforcement of this kind of policies. Nonetheless, the finding that the positive impact at the aggregate level vanishes in the medium term calls for more research to investigate how it could be sustained long-lastingly.

References

- Beerli, Andreas, Jan Ruffner, Michael Siegenthaler, and Giovanni Peri.** 2018. “The Abolition of Immigration Restrictions and the Performance of Firms and Workers: Evidence from Switzerland.” National Bureau of Economic Research.
- Bertrand, Marianne.** 2004. “From the invisible handshake to the invisible hand? How import competition changes the employment relationship.” *Journal of Labor Economics*, 22(4): 723–765.
- Blank, David M, George J Stigler, et al.** 1957. “The demand and supply of scientific personnel.” *NBER Books*.
- Bound, John, Breno Braga, Joseph M Golden, and Gaurav Khanna.** 2015. “Recruitment of foreigners in the market for computer scientists in the United States.” *Journal of labor economics*, 33(S1): S187–S223.
- Bound, John, Breno Braga, Joseph M Golden, and Sarah Turner.** 2013. “Pathways to adjustment: The case of information technology workers.” *American Economic Review*, 103(3): 203–07.
- Brücker, Herbert, and Elke J Jahn.** 2011. “Migration and wage-setting: reassessing the labor market effects of migration.” *Scandinavian Journal of Economics*, 113(2): 286–317.
- Chen, Been-Lon, Hung-Ju Chen, and Ping Wang.** 2011. “Labor-Market Frictions, Human Capital Accumulation, and Long-run Growth: Positive Analysis and Policy Evaluation.” *International Economic Review*, 52(1): 131–160.
- Decker, Ryan A, John C Haltiwanger, Ron S Jarmin, and Javier Miranda.** 2018. “Changing business dynamism and productivity: Shocks vs. responsiveness.” National Bureau of Economic Research.
- Doran, Kirk, Alexander Gelber, and Adam Isen.** 2014. *The effects of high-skilled immigration on firms: Evidence from h-1b visa lotteries*. National Bureau of Economic Research.

- Dustmann, Christian, and Albrecht Glitz.** 2015. “How do industries and firms respond to changes in local labor supply?” *Journal of Labor Economics*, 33(3): 711–750.
- Ghosh, Anirban, Anna Maria Mayda, and Francesc Ortega.** 2014. “The impact of skilled foreign workers on firms: an investigation of publicly traded US firms.”
- Giesing, Yvonne, and Nadzeya Laurentsyeva.** 2018. “Firms left behind: Emigration and firm productivity.”
- Hijzen, Alexander, and Paul Swaim.** 2010. “Offshoring, labour market institutions and the elasticity of labour demand.” *European Economic Review*, 54(8): 1016–1034.
- Hosios, Arthur J.** 1990. “On the efficiency of matching and related models of search and unemployment.” *The Review of Economic Studies*, 57(2): 279–298.
- Hyatt, Henry R, and James R Spletzer.** 2013. “The recent decline in employment dynamics.” *IZA Journal of Labor Economics*, 2(1): 5.
- Jäger, Simon, and Jorg Heining.** 2016. “How substitutable are workers.” *Evidence from Worker Deaths*.
- Kerr, Sari Pekkala, William R Kerr, and William F Lincoln.** 2015. “Skilled immigration and the employment structures of US firms.” *Journal of Labor Economics*, 33(S1): S147–S186.
- Kerr, William R, and William F Lincoln.** 2010. “The supply side of innovation: H-1B visa reforms and US ethnic invention.” *Journal of Labor Economics*, 28(3): 473–508.
- Lewis, Ethan.** 2011. “Immigration, skill mix, and capital skill complementarity.” *The Quarterly Journal of Economics*, 126(2): 1029–1069.
- Mayda, Anna Maria, Francesc Ortega, Giovanni Peri, Kevin Shih, and Chad Sparber.** 2018. “The effect of the H-1B quota on the employment and selection of foreign-born labor.” *European Economic Review*, 108: 105–128.
- Mitaritonna, Cristina, Gianluca Orefice, and Giovanni Peri.** 2017. “Immigrants and firms’ outcomes: Evidence from France.” *European Economic Review*, 96: 62–82.

- OECD.** 2012. “Renewing the skills of ageing workforces: the role of migration.” *International Migration Outlook*.
- Peri, Giovanni.** 2012. “The effect of immigration on productivity: Evidence from US states.” *Review of Economics and Statistics*, 94(1): 348–358.
- Peri, Giovanni.** 2016. “Immigrants, productivity, and labor markets.” *Journal of Economic Perspectives*, 30(4): 3–30.
- Peri, Giovanni, Kevin Shih, and Chad Sparber.** 2015. “STEM workers, H-1B visas, and productivity in US cities.” *Journal of Labor Economics*, 33(S1): S225–S255.
- Restrepo, Pascual.** 2015. “Skill mismatch and structural unemployment.” *Massachusetts Institute of Technology Job Market Paper*, 1–94.
- Ryoo, Jaewoo, and Sherwin Rosen.** 2004. “The engineering labor market.” *Journal of political economy*, 112(S1): S110–S140.
- Şahin, Ayşegül, Joseph Song, Giorgio Topa, and Giovanni L Violante.** 2014. “Mismatch unemployment.” *American Economic Review*, 104(11): 3529–64.
- Saint-Paul, Gilles, and Pierre Cahuc.** 2009. *Immigration, qualifications et marché du travail*. Documentation française.
- Shimer, Robert.** 2005. “The cyclical behavior of equilibrium unemployment and vacancies.” *American economic review*, 95(1): 25–49.
- Shimer, Robert.** 2007. “Mismatch.” *American Economic Review*, 97(4): 1074–1101.
- Signorelli, Sara.** 2019. “Do Skilled Migrants Compete with Native Workers? Analysis of a Selective Immigration Policy.” *PSE Working Paper Series*.

Appendix

Tables

Table A1: List of Occupations of the Reform

Occupation	Rank	N. of Regions (total=22)
Sales technician	Technician/Foreman	20
Sales repr. for intermed. goods and raw materials	Technician/Foreman	15
Executive of financial audit	Executive/Engineer	22
Computer scientist	Executive/Engineer	22
Senior computer scientist	Executive/Engineer	22
Marchandiser (design of stores and shelves)	Technician/Foreman	18
Operations manager in insurance	Technician/Foreman	15
Mechanic of construction machinery and agricultural machinery	Technician/Foreman	21
Operator of glass production	Blue collar	16
Installation driver for cement production	Blue collar	21
Driver of machinery for wood and furniture production	Blue collar	18
Artisan of wood and furniture production	Blue collar	18
Technician of industrial planning and methods	Technician/Foreman	14
Draughtsman manager mechanical construction	Technician/Foreman	13
Industrial draughtsman mechanic. construction and metallurgy	Technician/Foreman	13
Draughtsman manager in electricity and electronics	Executive/Engineer	22
Industrial draughtsman in electricity and electronics	Technician/Foreman	22
Production technician in mechanic. construction and metallurgy	Technician/Foreman	15
Quality-manager mechanic. construction and metallurgy	Technician/Foreman	18
Quality-manager in electricity and electronics	Technician/Foreman	5
Production technician of process industry	Technician/Foreman	11
Production technician of wood and furniture	Technician/Foreman	17
Installer and maintenance of elevators	Technician/Foreman	17
Compliance inspector	Technician/Foreman	13
Responsible for electronic maintenance	Technician/Foreman	5
Draughtsman of buildings and public work	Technician/Foreman	13
Construction surveyor	Technician/Foreman	10
Head of technical studies for buildings and public work	Technician/Foreman	21
Head of technical studies in construction	Technician/Foreman	1
Head of construction sites for buildings and public work	Technician/Foreman	21
Site supervisor for buildings and public work	Technician/Foreman	20

Table A2: Additional reduced form results on first stage

	(1)	(2)	(3)	(4)
	Log native reform employment	Log total reform employment	Log wages reform occup.	Log wages other occup.
VARIABLES	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.0689** (0.0271)	0.0804*** (0.0267)	-0.0911*** (0.0263)	0.00547 (0.00934)
Exposure * 2010-11	0.0728* (0.0434)	0.0957** (0.0430)	-0.121*** (0.0360)	-0.000500 (0.0119)
Observations	386,135	386,135	207,243	385,488
R-squared	0.871	0.884	0.768	0.831
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>				
Marginal effect ref. 2008-09	0.0124** (0.00488)	0.0145*** (0.00481)	-0.0164*** (0.00473)	0.000985 (0.00168)
Marginal effect ref. 2010-11	0.0131* (0.00781)	0.0172** (0.00774)	-0.0217*** (0.00649)	-9.00e-05 (0.00214)

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

Standard errors clustered at the employment zone x sector level. Controls for tightness in other occupations, firm FE, employment zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. The logarithm of native and total reform employment has been taken on the value+1 to keep the zeros in the analysis.

Table A3: First stage using regional level exposure

	(1)	(2)	(3)	(4)	(5)
	Share of reform empl.	Log migrant reform employment	Share of migrants in reform employment	Log native reform employment	Log total reform employment
VARIABLES	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.0113 (0.00757)	0.0995*** (0.0326)	0.0453** (0.0211)	0.0783 (0.0540)	0.107** (0.0516)
Exposure * 2010-11	0.0141 (0.0129)	0.155*** (0.0430)	0.0740*** (0.0266)	0.0990 (0.0901)	0.139 (0.0872)
Observations	408,158	408,158	209,555	408,158	408,158
R-squared	0.862	0.747	0.653	0.872	0.884
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>					
Marginal effect ref. 2008-09	0.00203 (0.00136)	0.0179*** (0.00586)	0.00815** (0.00380)	0.0141 (0.00971)	0.0192** (0.00930)
Marginal effect ref. 2010-11	0.00254 (0.00232)	0.0279*** (0.00774)	0.0133*** (0.00478)	0.0178 (0.0162)	0.0249 (0.0157)

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

Standard errors clustered at the region x sector level. Controls for tightness in other occupations interacted with post reform period, firm FE, region x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and region where the firm is located. Period of analysis: 2005-2011. The logarithm on native, migrant and total reform employment has been taken on the value+1 to keep the zero values in the analysis.

Table A4: Main outcomes using regional level exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.113*** (0.0379)	0.0904*** (0.0336)	0.0435 (0.0406)	-0.0592 (0.0444)	0.131** (0.0648)	0.0922* (0.0528)
Exposure * 2010-11	0.154*** (0.0471)	0.140*** (0.0478)	0.118** (0.0508)	-0.0817 (0.0636)	0.191* (0.0983)	0.141** (0.0673)
Observations	407,625	393,230	389,660	381,578	353,756	383,638
R-squared	0.842	0.954	0.905	0.775	0.950	0.964
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>						
Marginal effect ref. 2008-09	0.0204*** (0.00682)	0.0163*** (0.00605)	0.00784 (0.00731)	-0.0107 (0.00799)	0.0236** (0.0117)	0.0166* (0.00951)
Marginal effect ref. 2010-11	0.0277*** (0.00847)	0.0252*** (0.00861)	0.0213** (0.00915)	-0.0147 (0.0114)	0.0344* (0.0177)	0.0254** (0.0121)

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

Standard errors clustered at the region x sector level. Controls for tightness in other occupations interacted with post reform period, firm FE, region x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and region where the firm is located. Period of analysis: 2005-2011.

Table A4: Crowding-in of other employment using regional level exposure

	(1)	(2)	(3)	(4)	(5)
	Log non-reform employment				
	Total	Executives	Intermediate professionals	Clerk and salesmen	Blue collar workers
VARIABLES	OLS	OLS	OLS	OLS	OLS
Exposure * 2008-09	0.106*** (0.0366)	0.138*** (0.0403)	0.0860* (0.0503)	0.0447 (0.0447)	0.0906* (0.0473)
Exposure * 2010-11	0.116** (0.0466)	0.134** (0.0550)	0.161** (0.0781)	0.0672 (0.0627)	0.151** (0.0707)
Observations	408,158	408,158	408,158	408,158	408,158
R-squared	0.846	0.908	0.868	0.907	0.933
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>					
Marginal effect ref. 2008-09	0.0192*** (0.00658)	0.0249*** (0.00725)	0.0155* (0.00905)	0.00805 (0.00804)	0.0163* (0.00851)
Marginal effect ref. 2010-11	0.0208** (0.00840)	0.0242** (0.00991)	0.0290** (0.0141)	0.0121 (0.0113)	0.0272** (0.0127)

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

Standard errors clustered at the region x sector level. Controls for tightness in other occupations interacted with post reform period, firm FE, region x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and region where the firm is located. Period of analysis: 2005-2011. The logarithm on non-reform employment has been taken on the value+1 to keep the zero values in the analysis.

Table A5: Heterogeneity of main outcomes by firm size

VARIABLES	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		
	Log total employment				Log revenues				Log value added				Log VA per worker				
	small firms	large firms	small firms	large firms	small firms	large firms	small firms	large firms	small firms	large firms	small firms	large firms	small firms	large firms	small firms	large firms	
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	
Exposure * 2008-09	0.112*** (0.0391)	0.0410 (0.0252)	0.0694** (0.0275)	0.0431* (0.0230)	0.0865*** (0.0327)	0.0164 (0.0269)	0.00897 (0.0349)	0.0201 (0.0295)									
Exposure * 2010-11	0.114** (0.0464)	0.0546* (0.0313)	0.104*** (0.0382)	0.0522* (0.0304)	0.127*** (0.0439)	0.0381 (0.0370)	0.0249 (0.0506)	0.0215 (0.0375)									
Observations	165,783	219,773	159,519	212,976	158,037	211,043	153,696	208,015									
R-squared	0.663	0.875	0.934	0.957	0.853	0.912	0.778	0.778									

Calculated marginal effect of reform at mean exposure (tightness=0.18)

Marginal effect ref. 2008-09 0.0201*** (0.00703) 0.00737 (0.00454) 0.0125** (0.00495) 0.00776* (0.00415) 0.0156*** (0.00589) 0.00296 (0.00485) 0.00161 (0.00629) 0.00363 (0.00531)

Marginal effect ref. 2010-11 0.0205** (0.00835) 0.00983* (0.00563) 0.0187*** (0.00687) 0.00939* (0.00547) 0.0228*** (0.00790) 0.00686 (0.00667) 0.00448 (0.00910) 0.00387 (0.00675)

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. The table reports the results obtained from separate regressions run on the sample split in half across the firm size distribution.

Table A6: Heterogeneity of main outcomes by firm age

VARIABLES	(1) Log total employment		(2) Log total employment		(3) Log revenues		(4) Log revenues		(5) Log value added		(6) Log value added		(7) Log value added per worker		(8) Log value added per worker		
	young firms	old firms	young firms	old firms	young firms	old firms	young firms	old firms	young firms	old firms	young firms	old firms	young firms	old firms	young firms	old firms	
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	
Exposure * 2008-09	0.0978*** (0.0333)	0.0488* (0.0289)	0.0872*** (0.0283)	0.0280 (0.0223)	0.0671** (0.0331)	0.0297 (0.0267)	0.0287 (0.0343)	0.00468 (0.0304)	0.127*** (0.0420)	0.0442 (0.0346)	0.113*** (0.0376)	0.0508* (0.0304)	0.0972** (0.0423)	0.0675* (0.0373)	0.0265 (0.0439)	0.0226 (0.0417)	
Observations	153,944	231,622	150,230	222,274	148,572	220,514	145,075	216,642	0.825	0.858	0.946	0.959	0.767	0.785			
R-squared																	
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18)</i>																	
Marginal effect ref. 2008-09	0.0176*** (0.00600)	0.00879* (0.00521)	0.0157*** (0.00509)	0.00504 (0.00401)	0.0121** (0.00595)	0.00535 (0.00481)	0.00517 (0.00617)	0.000843 (0.00547)	0.0229*** (0.00756)	0.00623 (0.00623)	0.0204*** (0.00677)	0.00915* (0.00548)	0.0175** (0.00761)	0.0122* (0.00672)	0.00477 (0.00790)	0.00406 (0.00750)	
Marginal effect ref. 2010-11																	

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

Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. The table reports the results obtained from separate regressions run on the sample split in half across the firm age distribution.

Table A7: Heterogeneity of main outcomes by firms' initial productivity

VARIABLES	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		
	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	low prod. firms	high prod. firms	
Exposure * 2008-09	0.0714** (0.0317)	0.0589* (0.0323)	0.0557** (0.0243)	0.0451* (0.0257)	0.0654** (0.0294)	0.0266 (0.0293)	0.0712** (0.0318)	0.0266 (0.0293)	0.0654** (0.0294)	0.0266 (0.0293)	0.0712** (0.0318)	0.0266 (0.0293)	0.0712** (0.0318)	0.0266 (0.0293)	0.0712** (0.0318)	0.0266 (0.0293)	-0.0268 (0.0325)
Exposure * 2010-11	0.0706* (0.0394)	0.0756* (0.0390)	0.0720** (0.0329)	0.0714** (0.0341)	0.0805** (0.0393)	0.0720** (0.0329)	0.0742* (0.0432)	0.0720** (0.0329)	0.0805** (0.0393)	0.0720** (0.0329)	0.0742* (0.0432)	0.0720** (0.0329)	0.0742* (0.0432)	0.0720** (0.0329)	0.0742* (0.0432)	0.0720** (0.0329)	-0.0131 (0.0431)
Observations	181,020	204,534	177,474	195,024	176,249	192,831	172,606	192,831	176,249	192,831	172,606	192,831	172,606	192,831	172,606	192,831	189,107
R-squared	0.835	0.852	0.948	0.955	0.883	0.914	0.704	0.914	0.883	0.914	0.704	0.914	0.704	0.914	0.704	0.914	0.791

Calculated marginal effect of reform at mean exposure (tightness=0.18)

Marginal effect ref. 2008-09

Marginal effect ref. 2010-11

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. The table reports the results obtained from separate regressions run on the sample split in half across the firm productivity distribution as measured in the first period of appearance in the sample.

Table A8: Heterogeneity of main outcomes by labor market dynamism

VARIABLES	(1) Log total employment		(2) Log revenues		(3) Log value added		(4) Log VA per worker	
	Log j-j mobility		Low j-j mobility		Overall		Low j-j mobility	
	Overall	OLS	Overall	OLS	Overall	OLS	Overall	OLS
Exposure * 2008-09	0.0676*** (0.0224)	0.0665* (0.0386)	0.0514*** (0.0180)	0.0810*** (0.0300)	0.0439** (0.0216)	0.0894** (0.0356)	0.0162 (0.0241)	0.0598* (0.0354)
Exposure * 2010-11	0.0765*** (0.0273)	0.104** (0.0452)	0.0724*** (0.0241)	0.147*** (0.0411)	0.0741*** (0.0281)	0.174*** (0.0455)	0.0243 (0.0336)	0.0808* (0.0469)
Observations	385,623	95,525	372,559	93,360	369,143	92,631	361,774	91,632
R-squared	0.844	0.881	0.953	0.956	0.905	0.919	0.777	0.750

Calculated marginal effect of reform at mean exposure (tightness=0.18)

Marginal effect ref. 2008-09	0.0122*** (0.00404)	0.0120* (0.00695)	0.00925*** (0.00324)	0.0146*** (0.00541)	0.00790** (0.00388)	0.0161** (0.00641)	0.00292 (0.00434)	0.0108* (0.00636)
Marginal effect ref. 2010-11	0.0138*** (0.00492)	0.0187*** (0.00813)	0.0130*** (0.00433)	0.0264*** (0.00740)	0.0133*** (0.00506)	0.0312*** (0.00819)	0.00437 (0.00604)	0.0145* (0.00844)

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. Period of analysis: 2005-2011. Low j-j mobility areas consist in the half of the employment zones with the lowest level of job to job transitions in a given sector, which is a proxy of low dynamism of the labor market.

Table A9: Triple differences by presence of foreign workers before the reform

VARIABLES	(1) Log migrant reform employment	(2) Log total employment	(3) Log revenues	(4) Log value added	(5) Log VA per worker	(6) log intangible assets	(7) log tangible assets
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Employ migrant workers pre-reform	0.0221*** (0.00430)	0.0645*** (0.00658)	0.0395*** (0.00473)	0.0372*** (0.00563)	0.00287 (0.00546)	0.0275** (0.0111)	0.0383*** (0.00628)
Exposure * 2008-11	-0.0150 (0.0124)	0.0210 (0.0270)	0.0375* (0.0216)	0.0189 (0.0260)	-0.0124 (0.0279)	0.0195 (0.0534)	0.00977 (0.0320)
Exposure * employ migrants workers	0.0230 (0.0217)	-0.0271 (0.0309)	-0.00682 (0.0233)	-0.00551 (0.0266)	-0.0350 (0.0265)	0.0213 (0.0558)	-0.000509 (0.0309)
Exposure * employ mig. workers * 2008-11	0.147*** (0.0247)	0.0080** (0.0322)	0.0463* (0.0268)	0.0618* (0.0323)	0.0495 (0.0335)	0.0107 (0.0669)	0.0150 (0.0406)
Observations	359,281	358,949	347,765	344,807	337,786	314,672	339,617
R-squared	0.759	0.877	0.960	0.914	0.786	0.954	0.969
<i>Calculated marginal effect of reform at mean exposure (tightness=0.18), and for firms hiring migrant workers</i>							
Marginal effect ref. 2008-11	0.0264*** (0.00444)	0.0122** (0.00579)	0.00833* (0.00483)	0.0111* (0.00582)	0.00891 (0.00603)	0.00193 (0.0120)	0.00270 (0.00730)

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the employment zone x sector level. firm FE, employment zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located. These regressions perform a triple difference interacting firm exposure x the post-reform period x dummy equal to one if the firm employs at least one foreign worker before the policy change.

Table A10: Elasticities of outcomes instrumenting for total reform employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log total reform empl.	0.617* (0.318)	0.747** (0.337)	0.660* (0.360)	0.212 (0.330)	0.608 (0.604)	0.468 (0.367)
Observations	267,864	273,307	270,659	263,862	243,864	266,069
R-squared	-0.711	-1.056	-0.577	-0.085	-0.221	-0.271
RMSE	0.282	0.322	0.329	0.271	0.455	0.313
K-P Wald F-statistic	7.523	8.111	7.462	6.284	6.765	8.434

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

Standard errors clustered at the firm level. Controls for tightness in other occupations interacted with post reform period as well as firm FE, employment zone x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located, and the post reform period. To increase the strength of the instrument we restrict the sample to 2005-2009 (effect of first two years of reform).

Table A11: Elasticities of other labor inputs instrumenting for total reform employment

	(1)	(2)	(3)	(4)	(5)
	Log non-reform employment				
	Total	Executives	Intermediate professionals	Clerk and salesmen	Blue collar workers
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS
Log total reform empl.	0.472 (0.321)	0.911** (0.358)	0.399 (0.334)	0.422 (0.298)	0.162 (0.325)
Observations	273,307	283,992	283,992	283,992	283,992
R-squared	-1.056	-1.003	-0.111	-0.124	-0.026
RMSE	0.322	0.423	0.378	0.340	0.361
K-P Wald F-statistic	8.111	12.23	12.23	12.23	12.23

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

Standard errors clustered at the firm level. Controls for tightness in other occupations interacted with post reform period as well as firm FE, employment zone x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and employment zone where the firm is located, and the post reform period. To increase the strength of the instrument we restrict the sample to 2005-2009 (effect of first two years of reform).

Table A12: Elasticities of outcomes using regional level exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log migrant ref. employment	0.464** (0.227)	0.733*** (0.244)	0.461** (0.212)	0.0912 (0.170)	0.397 (0.412)	0.324 (0.246)
Observations	72,103	69,311	68,303	67,069	63,207	67,936
R-squared	-0.134	-1.140	-0.298	-0.021	-0.103	-0.147
RMSE	0.332	0.363	0.327	0.275	0.461	0.314
K-P Wald F-statistic	14.36	14.53	15.86	16.24	11.36	13.12

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

Standard errors clustered at the firm level, firm FE, region x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and region where the firm is located, and the post reform period. To increase the strength of the instrument the sample is restricted to firms counting at least one migrant worker in reform occupations in at least one year over the period, and to the years 2005-2009 (effect on first two years of reform).

Table A13: Elasticities of other labor inputs using regional level exposure

	(1)	(2)	(3)	(4)	(5)
	Log non-reform employment				
	Total	Executives	Intermediate professionals	Clerk and salesmen	Blue collar workers
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS
Log migrant ref. employment	0.307 (0.246)	0.472* (0.260)	0.271 (0.281)	0.226 (0.239)	0.323 (0.295)
Observations	72,195	72,195	72,195	72,195	72,195
R-squared	-0.117	-0.250	-0.045	-0.029	-0.119
RMSE	0.360	0.379	0.395	0.348	0.423
K-P Wald F-statistic	14.30	14.30	14.30	14.30	14.30

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

Standard errors clustered at the firm level, firm FE, region x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and region where the firm is located, and the post reform period. To increase the strength of the instrument the sample is restricted to firms counting at least one migrant worker in reform occupations in at least one year over the period, and to the years 2005-2009 (effect on first two years of reform).

Table A14: Aggregate effect on the first stage

	(1)	(2)	(3)
	Share of migrants in reform employment	Share of ref empl. In total empl	Log migrant reform employment
VARIABLES	OLS	OLS	OLS
Exposure * 2008-09	0.00880 (0.0140)	0.0315*** (0.00740)	0.122*** (0.0329)
Exposure * 2010-11	0.0236 (0.0205)	0.0532*** (0.0112)	0.193*** (0.0475)
Observations	52,684	60,849	60,849
R-squared	0.550	0.858	0.820
<i>Calculated marginal effect of reform at mean exposure (tightness=0.16)</i>			
Marginal effect ref. 2008-09	0.00141 (0.00224)	0.00504*** (0.00118)	0.0195*** (0.00526)
Marginal effect ref. 2010-11	0.00377 (0.00328)	0.00851*** (0.00180)	0.0309*** (0.00761)

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

Regressions aggregating outcomes at the employment zone x sector level. Employment zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured as the average tightness in reform occupations observed within the sector and employment zone.

Table A15: Aggregate Elasticities

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log migrant ref. employment	0.715** (0.357)	0.742* (0.384)	0.794** (0.389)	0.335 (0.255)	0.270 (0.745)	0.120 (0.504)
Observations	43,349	42,355	42,230	41,913	41,189	42,010
R-squared	-0.592	-0.475	-0.594	-0.297	-0.001	0.009
RMSE	0.335	0.383	0.389	0.249	0.581	0.428
K-P WF	13.44	15.11	15.30	15.48	13.69	14.97

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

Regressions aggregating outcomes at the employment zone x sector level. Employment zone x sector FE and sector x year FE are included but not reported. The excluded instrument is the interaction between exposure, measured as the average tightness in reform occupations observed within the sector and employment zone, and the post reform period. To increase the strength of the instrument the sample is restricted to the years 2005-2009 (effect on first two years of reform).

Table A16: First stage results for global demand analysis using value added trade

	(1)	(2)	(3)	(4)
	Log total reform employment	Log other employment	Log wages reform occup.	Log wages other occup.
VARIABLES	OLS	OLS	OLS	OLS
GD * Exposure	-0.0663* (0.0369)	-0.0326 (0.0269)	0.111*** (0.0299)	0.00277 (0.0102)
Exposure * post 2008	-0.171 (0.105)	-0.0293 (0.0698)	0.305*** (0.0852)	0.0167 (0.0286)
GD * exposure * post 2008	0.0440 (0.0315)	0.0438* (0.0256)	-0.0783*** (0.0271)	-0.000396 (0.00948)
Observations	386,135	385,372	207,243	385,488
R-squared	0.884	0.827	0.768	0.831

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

Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured at the level of employment zone x sector while global demand changes are measured at the sectoral level. The latter are proxied by changes in GDP growth of all other countries except France weighted by the share of value added imports of a given sector going to each of them.

Table A17: Main outcomes for global demand analysis using value added trade

	(1)	(2)	(3)	(4)	(5)	(6)
	Log total employment	Log revenues	Log value added	Log VA per worker	Log intangible assets	Log tangible assets
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS
GD * Exposure	-0.0773*** (0.0244)	-0.0539*** (0.0201)	-0.0537** (0.0235)	-0.0168 (0.0255)	-0.00936 (0.0459)	-0.0341 (0.0280)
Exposure * post 2008	-0.170*** (0.0618)	-0.134** (0.0536)	-0.137** (0.0640)	-0.0361 (0.0721)	0.00904 (0.120)	-0.0838 (0.0727)
GD * exposure * post 2008	0.0724*** (0.0235)	0.0427** (0.0193)	0.0426* (0.0223)	0.0152 (0.0228)	0.0131 (0.0439)	0.0281 (0.0265)
Observations	385,623	372,559	369,143	361,774	335,565	363,560
R-squared	0.844	0.953	0.905	0.777	0.951	0.965

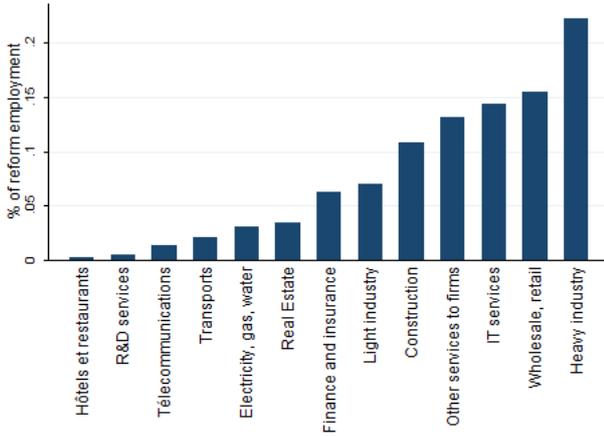
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered at the employment zone x sector level. Controls interacting tightness in other occupations x post reform periods as well as firm FE, empl. zone x sector FE and sector x year FE are included but not reported. Period of analysis: 2005-2011. Exposure is measured at the level of employment zone x sector while global demand changes are measured at the sectoral level. The latter are proxied by changes in GDP growth of all other countries except France weighted by the share of value added imports of a given sector going to each of them.

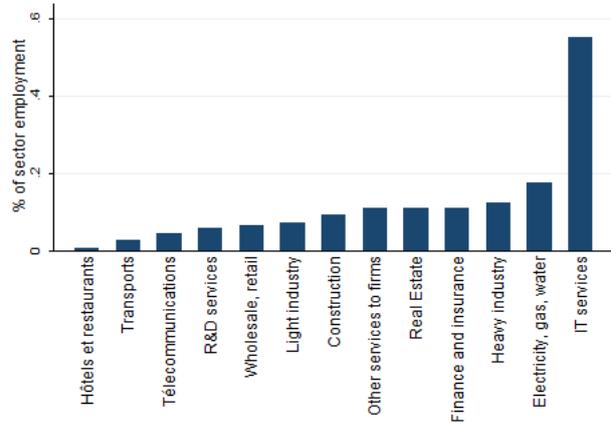
Figures

Figure A1: Characterization of the reform

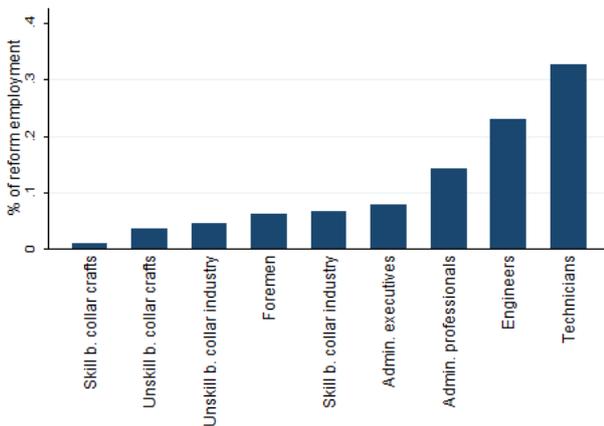
a) Sectoral distribution of reform employment



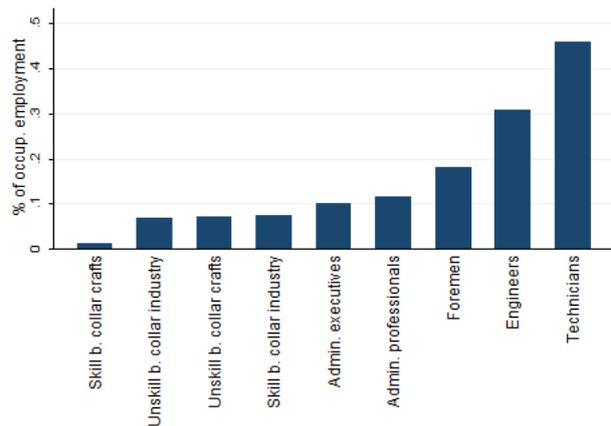
b) Sectoral exposure to the reform



c) Occup. distribution of reform employment

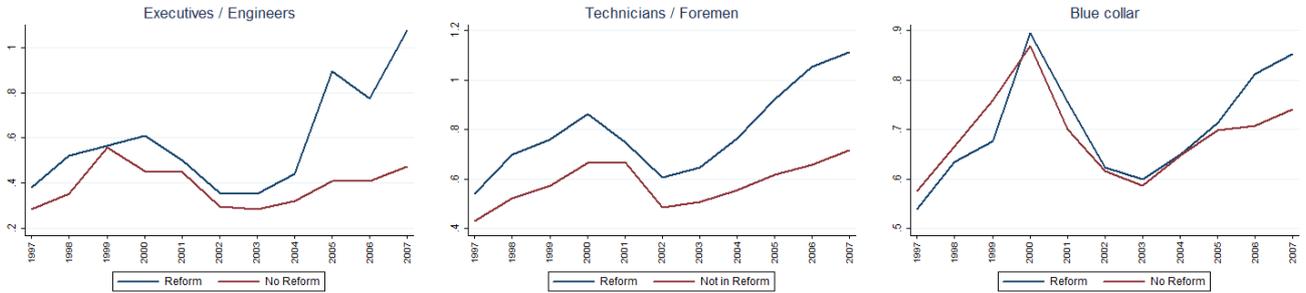


d) Occupational exposure to the reform



Author's calculations based on data from French Labor Force Survey 2004-2007. Panel a) shows how the total employment concerned by the reform is distributed across sectors. Panel b) shows how much of each sector's employment is concerned by the reform (measure of sectoral exposure). Panels c) and d) do the same across broad categories of occupations.

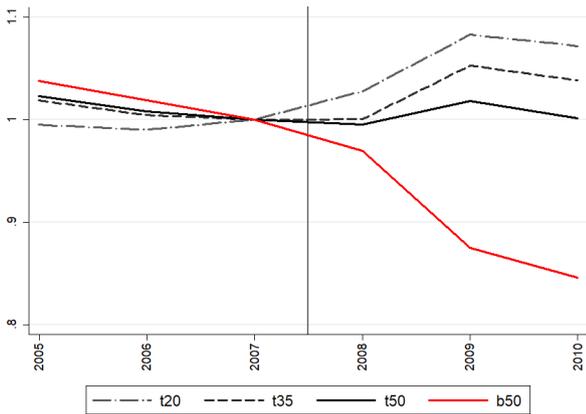
Figure A2: Growth in tightness index by reform status



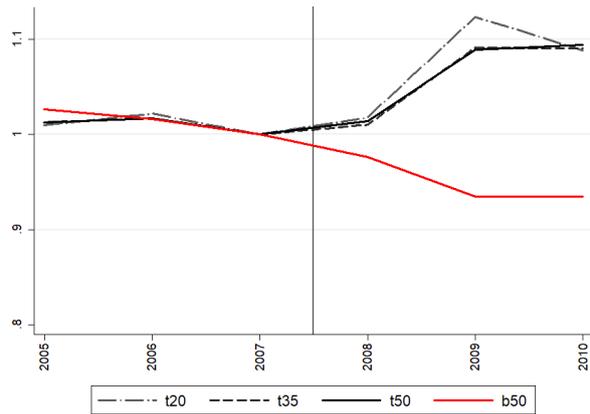
The figure reports the growth rate in the tightness index collected by the Employment office in reform and non-reform occupations within each group. They are both set to 1 in the initial year (1997).

Figure A3: Unconditional evidence on first stage

a) Growth in reform employment



b) Growth in share of reform employment



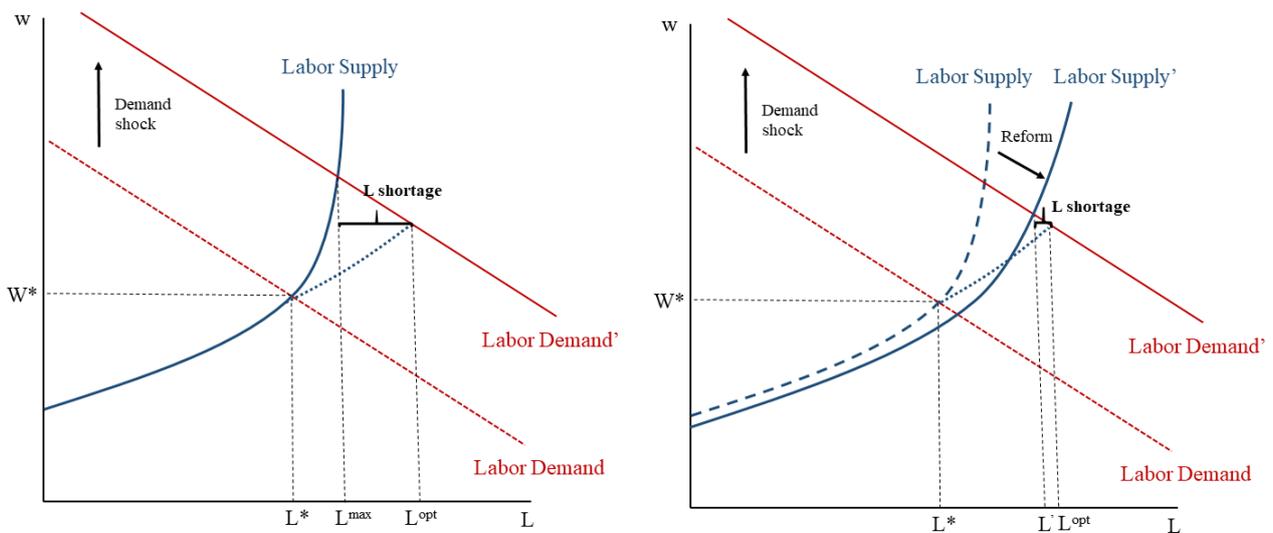
The graph shows the evolution of reform employment across different degrees of exposure to the reform (top 20%, top 35% top 50% and bottom 50% in the exposure distribution). The value is set to 1 in 2007 so that the value for the other years can be interpreted in terms of growth rates relative to 2007.

Figure A4: Additional event study graphs



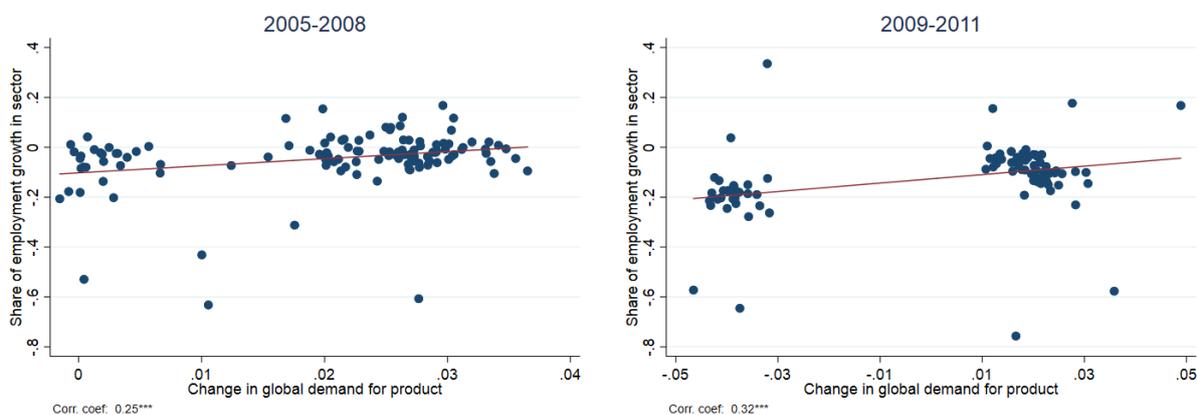
Note: The black line shows the coefficient obtained on the interaction between treatment exposure and each year fixed effects. The two grey lines report the 95% confidence intervals.

Figure A5: Conceptual Framework - Elasticity to Labor Demand Shocks



In our conceptual framework labor shortages are defined as the difference between the actual quantity of equilibrium L^{MAX} and the optimal quantity that we would obtain in a frictionless environment L^{opt} . The reform is expected to reduce such gap.

Figure A6: Validation of Proxy for Global Demand Changes



Each dot corresponds to a sector x year observation, changes in sectoral global demand are computed using Equation (5) and data from the UN Comtrade and the World Development Indicators. The sectoral growth in employment share is computed using the DADS poste data. Correlation coefficients with their significance levels are reported below each panel.