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JEL Codes: J61, J62, J63

Keywords : Immigration, Employment, Wage, Occupations, France



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Do Skilled Migrants Compete with Native Workers? Analysis of a Selective Immigration Policy

by
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Paris School of Economics
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Abstract

The question of how migration flows affect the labor markets of receiving countries has been widely debated by the economic literature but still lacks consensus, and rare are the studies addressing it in the context of high-skill migration. In addition, several Western countries adopted selective immigration policies to offset domestic skill shortages, while little evidence exists on their effectiveness. This paper aims to bridge both gaps by taking advantage of a French reform making it easier for firms to hire high-skill migrant workers in a specific set of occupations lacking native candidates. The analysis uses a rich set of administrative employer-employee data and relies on a difference-in-differences approach. Results show that the reform boosted migrants' hires without causing any harm to native employment, thus increasing the stock of labor in targeted jobs. In addition, entry wages of foreign workers decreased twice as much as the ones of natives, suggesting that the latter are in part shielded from the additional competition. Finally, the paper estimates the elasticity of substitution parameter recovered using the reform as an exogenous instrument and shows that immigrants and natives are imperfect substitutes in production, even when they are employed in the same occupations and by the same employer.

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1 Introduction

The literature on the labor market effects of immigration mostly focuses on flows of uneducated migrants and refugees² and still lacks consensus on their impact on native employment and wages. This is in part due to the empirical challenges surrounding the estimation of the causal impact of immigration flows, given the pervasive endogeneity in mobility decisions (Dustmann, Schönberg and Stuhler, 2016)³. Among the papers basing their analysis on natural experiments, a common result is that native workers are in part shielded from the additional competition, as their labor market outcomes appear to suffer less than the canonical model would predict under the assumption of perfect substitution in production (Glitz, 2012; Foged and Peri, 2016; Dustmann, Schönberg and Stuhler, 2017). This has been explained by the fact that migrants often suffer from skill-downgrading and in general possess distinct abilities that result in differential task specialization with respect to natives (Peri and Sparber, 2009; Dustmann, Frattini and Preston, 2012; Foged and Peri, 2016). The majority of these works focuses on low-skill migrants, leaving open the question of whether the effect would be different in the context of an inflow of highly educated workers. In addition, by taking geographic areas and education levels as units of analysis, the reduced form results capture the aggregation of different adjustment mechanisms, including the specialization of natives and migrants in different jobs. Kerr and Lincoln (2010) and Mayda et al. (2018) are among the few analyzing the impact of high-skill migrants through changes in the H-1B visa policy in the US. They both find that there is no significant change in native employment and Kerr and Lincoln (2010) show a small and insignificant effect on wages.

The contribution of this paper is to look at the effect of a quasi-exogenous migration shock restricted to a set of high-skill professions on employment and wages of natives working in the same jobs and for the same employers, which allows to test whether perfect substitutability is observed in a context that rules out the possibility of differential occupational specialization. To our knowledge this is the first analysis measuring the degree of substitution between natives and migrants at the level of occupations within plants. The French reform under study lowers the employers' administrative cost for hiring extra-European citizens. The professions concerned are called "*occupations in tension*" and are defined based

²The large waves of low-skill immigration coming from Central America and entering the United States are arguably the most widely studied, with a focus on the negative wage pressure exerted on low-skill native labor. See for example Card (2001), Borjas (2003), Ottaviano and Peri (2012).

³Many papers rely on instruments based on past immigration waves (commonly called *Shift-Share*) or on geographic and educational differences in exposure (Altonji and Card, 1991; Card, 2001; Peri and Sparber, 2009; Peri, 2012; Cattaneo, Fiorio and Peri, 2015; Dustmann, Schönberg and Stuhler, 2017).

on the observed imbalance between the number of vacancies available and the number of unemployed candidates possessing the relevant qualifications in a given region. The types of competencies involved vary, but mostly require at least a post-high school technical specialization. One advantage of this reform is that it introduces a second list of occupations based on similar criteria, where the administrative procedure is lessened only for workers coming from a narrow set of origin countries. This provides us with a natural control group for the difference-in-differences analysis, under the assumption that the treatment intensity is much lower for this second set of jobs. In addition, the richness of the two administrative employer-employee datasets used makes it possible to test the effect on wages of new entrants and incumbent workers, both native and foreign, on the stock of employment and on hiring patterns, thus providing a rather complete picture of the different channels affected. Finally, in the last part of the analysis we estimate the elasticity of substitution parameter derived from a CES production function combining native and migrant labor inputs, following a similar method as in [Card \(2009\)](#), [Manacorda, Manning and Wadsworth \(2012\)](#) and [Ottaviano and Peri \(2012\)](#). However, while the unit of analysis of these papers are cells defined by age, education, and geographic location; we take advantage of the richness of our administrative data to look within establishments and occupation categories, which allow to measure the elasticity of substitution at the level where decisions take place: the firm. We then compare the results obtained from the OLS estimation with the ones resulting from a 2SLS procedure instrumenting the relative supply of migrant to native workers with the exogenous shock generated by the reform, which allows us to draw some conclusions on the potential biases incurred by the first method. Our results corroborate what found in [Card \(2009\)](#), where the IV strategy is based on a shift-share instrument.

We find that the reform increased the share of hires made of migrants by 13% in target occupations, and the probability of hiring a foreign employee by 16%, confirming that firms took advantage of the policy change. The recruitment prospects of natives remained unchanged, thus resulting in an overall increase of 1.1% in the stock of employment in these jobs. We observe a negative pressure on entry wages, as the canonical model predicts. The latter is however twice as important on the wages of migrant new hires (-13.6%) than on the ones of natives (-6.8%), revealing that the firsts are in part shielded from the additional competition. Furthermore, incumbent native workers do not seem to incur a wage loss while incumbent migrants see a 2% drop in salary, likely due to slower growth. The heterogeneity analysis indicates that the impact captured is mostly driven by occupations that were characterized by very high levels of tension before the reform. Finally, the elasticity of substitution in production between migrants

and natives obtained from the 2SLS procedure is much smaller than the one resulting from OLS, which suggests that the standard approach may lead to an over-estimation of the parameter. It is important to notice that the post-reform period is characterized by the hit of the economic crisis, which is visible by an overall drop in hires in 2009. While we argue that the econometric strategy can properly account for it, results have to be interpreted relative to this context, which may have exacerbated the negative pressure on wages due to the slack demand.

This analysis carries important policy implications. Several Western countries adopted various forms of selective immigration policies in recent years⁴. Furthermore, in the US and the UK there is an ongoing debate about a potential increase in selectivity in the attribution of economic visas, with additional weight put on the level of education of applicants. While these measures are widely discussed in the policy arena, there is still a lack of rigorous evidence on their effectiveness. The existing papers on the matter either adopt a macro-economic perspective (Constant and Zimmermann, 2005; Docquier and Rapoport, 2007) or take a descriptive approach (Mahroum, 2001; Devins and Hogarth, 2005; Wickham and Bruff, 2008; Ruhs and Anderson, 2010). This paper provides one of the first in depth empirical investigation of one of such policies in terms of take-up and labor market consequences for the population of native workers that face a direct increase in competition. While it does not address whether greater selectivity is desirable from the perspective of receiving countries, it does suggest that favoring inflows of high-skill workers with rare competencies can be effective against skill shortages and do not impose an excessive burden on native employees.

The remaining of the paper is organized as follows. Section 2 describes the reform, Section 3 presents the data and the empirical strategy, Section 4 shows the reduced-form results, Section 5 estimates the elasticity of substitution between migrants and natives, and Section 6 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 at the local prefecture. The latter has to transmit the request to

⁴For instance, Canada and Australia have a visa point system that encourages high-skill immigration, the H-1B visa procedure in the US incorporates country quotas to increase workers' origin diversity, and the UK presents visa facilitations for migrant workers possessing qualifications that are in shortage domestically.

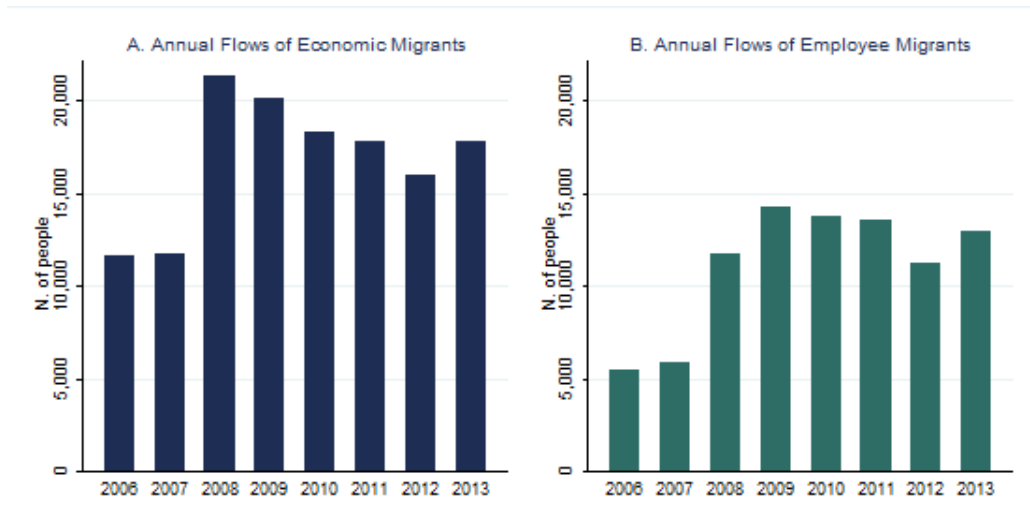
the Service of Foreign Labour, which grants it only if two conditions are met. First, the employer must prove to have searched extensively for a priority candidate before considering to hire a non-European. Second, the occupation under question must appear as in tension in the statistics collected by the French Employment Office. The tension indicator measures the ratio between available vacancies in each occupation and the pool of unemployed workers possessing the required competencies for the job. A high level of tension thus signals that the occupation is hard to fill. If both conditions are verified, the authorization is granted to the employer and the candidate is allowed to apply for an economic visa. Figure A1 in the appendix reports a diagram taken from [OECD \(2017\)](#) that illustrates all the administrative steps that need to be undertaken before hiring a migrant worker. The procedure is lengthy and burdensome for employers. All demands have to be submitted in paper format, and despite the fact that the official time to process requests is set to a maximum of two months, often it takes much longer in practice, especially when the institutional bodies involved in the final decision are in disagreement.

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 a high level of tension in the labor market. 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. In order to avoid abuses, the Service of Foreign Labour carefully checks that the qualifications and work experience of the candidate match the occupation for which he is hired and further controls that the contract conditions, notably in terms of salary, are in line with the standards for the position. The aim of the reform is to help firms recruiting for professions that suffer from a scarcity of domestic labor. In a first step the list was defined at the national level, and in a second phase each region selected a subset of occupations that remained in tension at the local level. As a result, only five jobs apply to the entire French territory, while the others are only valid in certain areas. Table A1 in the appendix reports the full list of target occupations and details the regions to which each of them applies. The main economic activities concerned are computer science (2 occupations), construction (4 occupations), electricity and electronics (4 occupations), and mechanical construction and metal processing (4 occupations); and the main job types involved are technicians, engineers and foremen⁵.

⁵Figure A2 in appendix details the exposure to the reform of different broad categories of occupations and sectors.

For the identification strategy we take advantage of the fact that the legal change under study is part of a larger effort to reform France’s economic migration policy. In particular, the working group in charge of the reform established at the same time an extended list of 150 occupations to be open without restrictions to European nationals coming from member states under transitory regimes⁶, which includes the 30 occupations open to extra-EU citizens and 120 additional ones. The definition of both lists is based on the same set of tension criteria⁷, such that the professions open only to EU States under transitory regimes provide a natural control group for the ones open to all nationalities, under the assumption that while they are similar in terms of pre-reform characteristics, the treatment intensity is much lower in the first than in the second. Despite the precise description of the indicators used by the working group, we were not able to find a threshold in the tension data determining the inclusion in the reform. This is due to the fact that the final lists were decided after a negotiation with the social partners, which introduced some degree of arbitrariness coming from different political arrangements (OECD, 2017). While this rules out the option of a regression discontinuity design, we use the tension data to ensure comparability between the treated and control groups in a difference-in-differences setting.

Figure 1: Economic Migration Flows



Source: National statistics on immigration published by MI-DGEF-DSED. Panel B is a sub-category of Panel A.

⁶At the time of the implementation of the reform this concerned only Romanian and Bulgarian nationals, which joined the European Union in 2007, but later it also included Croatians (EU members since 2013). Workers from these countries did not immediately obtain the right to work in all member states, and in France they continued to be subject to the same labor market restrictions of extra-Europeans during a probation period that lasted until 2014.

⁷To precisely measure tension in each occupation, the working group considered several indicators collected quarterly by the Employment Office: i) the ratio between job supply (vacancies) and demand (unemployed with relevant skills), ii) the volume of job supply, iii) the volume of job demand, iv) the evolution in the stock of demand and supply, v) the turnover rate of job seekers at the end of the month, and vi) the share of long term contracts within the job offers. All of the indicators are collected periodically for each of the 22 regions of metropolitan France and for 225 categories of occupations.

Figure 1 provides the first descriptive evidence that the labor market did react to the policy change by attracting larger inflows of migrant workers. Despite the fact that France is a country subject to important migration flows, only a small proportion of them is made of economic migrants: every year about 80% of the visas issued are for family reunification or study motives (about 160 thousands migrants in 2013), while only 10% are delivered for professional reasons (18 thousands migrants in 2013). However, the number of economic visas delivered nearly doubled between 2007 and 2008 (Figure 1, Panel A), mostly driven by a striking increase in the visa delivered to extra-Europeans coming in to work as employees for French firms (Figure 1, Panel B). In contrast, all the other categories of immigration do not show any significant change in size around the same years, as shown in Appendix Figure A3 and A4. These patterns are consistent with what is expected from the reform, since the only migrants directly impacted are the ones that apply to economic visas in order to work for domestic employers. The next section describes in details the data and empirical strategy used to recover the effect of the reform on employment and wages.

3 Data and Empirical Strategy

The two administrative datasets used in the analysis come from firms' declarations. The first one combines the declaration of labor movements (*DMMO*) with the survey on labor movements (*EMMO*), and reports information on all employees' entries and exits that took place in a given establishment over the year. The data contains all plants larger than fifty employees and a representative sample of the smaller ones. The second dataset is derived from the annual declaration of fiscal data (*DADS*) and contains employment and wage information on a random sample of 1/12th of all the private sector employees. The periodic reporting of this data is a legal requirement for firms and both datasets are further cleaned and verified by the statistical office attached to the Ministry of Labour. Both datasets allow to follow establishments over time but not workers, except for a short individual panel dimension included in the *DADS*, which allows to know the salary and occupation of a worker in the year preceding the report, if he was already employed by the same plant. For the rest of the paper we use the words "plant", "establishment" and "firm" interchangeably, always referring to the single physical location where workers perform their duties.

The *DMMO-EMMO* data reports the detailed occupation code, the nationality category of the em-

ployee (French, EU, non-EU citizen), and the nature of the contract of all new hires and terminations. While the data is originally reported quarterly, we summarize it by year to increase cell size and to avoid problems of seasonality. The DADS data contains the salary and number of hours worked of each employee in the sample, the detailed occupation code, and an indication of whether they were already working for the firm in the previous year, which allows to distinguish between entry and incumbent wage. In this data the nationality is a binary indicator of foreign workers, so EU citizens are included among the foreign population. Both datasets report the regional location, the sector, and number of employees at the establishment level. It is important to notice that workers' nationality is a proxy for individuals affected by the reform, since only non-EU citizens wanting to migrate into France with an economic visa and foreign residents that want to switch from a student to a working visa are concerned, while the holders of a family reunion visa are allowed to work in any occupation without restrictions. Nevertheless, given that the discrete jump in 2008 is only observed within economic visas, we can expect a discrete change in foreign entries within reform occupations to reflect the actual effect of the policy. The DMMO-EMMO data used in this study covers the period from 2004 to 2014, while the DADS 1/12th sample covers the period from 2004 to 2015. The agriculture and mining sectors are excluded because not concerned by either ones of the lists, and establishments with less than 20 employees are dropped from the DADS data because the detailed occupation codes are not reliable there. Both datasets are finally merged to a composite index of tension varying by occupation-region-year and constructed using the data from the Employment Office that served to define the reform⁸.

The econometric approach used to recover the impact of the policy is a standard difference-in-differences. The analysis is based on an unbalanced panel at the level of occupations x plants and treatment and controls are defined at the occupation x region level. A unit of observation is considered treated if the occupation figures in the list of the reform for the region where the establishment is located. The main identifying assumption is the absence of group-specific time shocks other than the reform itself, which insures that the trend observed in the control professions is a valid counterfactual. For this assumption to hold, the control group needs to present comparable characteristics, especially in terms of hiring difficulties. Given that the 30 occupations open to non-EU migrants (treated list) is a

⁸The index combines the following indicators: i) the ratio between job supply and demand registered during the reference period, ii) the volume of job supply, iii) the volume of job demand, iv) the evolution in the stock of demand and supply, v) the turnover rate of job seekers at the end of the month, and vi) the share of long term contracts within the job offers. Each indicator is translated into a standardized z-score using the formula $Zscore_i = \frac{x_i - \bar{X}}{\sigma_X}$, and the final index is computed as the average of the six z-scores. The latter captures all the information used by the commission to define the list of occupations for the reform.

subgroup of the extended list applicable to EU states under transitory regimes (EU list), and that both are defined using the same set of criteria, it seems natural to use the remaining occupations of the EU list as a control. Under the assumption that the hiring of Romanians and Bulgarians reacted similarly across both series of jobs, any difference observed in foreign hiring can be attributed to the greater treatment intensity of the treated list⁹. Given that the EU list is much larger, some of the jobs included there present much lower levels of tension with respect to the treated jobs, which may introduce a bias in the results if the tension differential is the source of asymmetric time-variant shocks. To reduce this risk, we exclude from the control group the occupations in the EU list that have a tension index below zero before 2008¹⁰. After this correction the average tension index is similar across both groups. The other threat to identification reside in the possibility of migrants changing strategically the occupation for which they apply in order to enter the list, or of employers changing the job description at the margin to be able to attract a broader pool of candidates. This would mean that in the absence of the reform the hiring of migrants in the control group would have grown faster because it would not have been "diverted" towards treatment occupations. While I cannot completely rule out this possibility, I observe carefully the hiring trends of control occupations at the moment of introduction of the reform, to check that there is no drop in foreign applications symmetric to the increase observed in treatment occupations. In addition, I exclude from the control group the occupations that are too similar in terms of skills required to some occupations in the treatment group, and that thus could be easily manipulated by employers or substituted by job seekers¹¹. The fact that the match between an applicant's qualifications and the competencies required for the position is carefully checked by the Office of Foreign Labour reduces the room for tampering.

The analysis on the DMMO-EMMO data focuses on the effect of the reform on the labor movements in and out of plants. In particular, it tests whether the legal change had an impact on the probability of hiring a migrant worker and on the share of migrant entries within each occupation. Further, it checks whether the first order impact crowded-out some employment opportunities for native workers

⁹Given that in the DMMO-EMMO data we can distinguish Europeans from non-Europeans, we test whether we find any significant effect on hiring of Europeans and we find none (results available upon request). This is an indication that the effect on Bulgarian and Romanian hiring is symmetric. Additionally, in the case where they were still hired more in the control list, then our measured treatment effect on natives would be a lower bound.

¹⁰Since the index is computed as an average of z-scores, a value below zero indicates that the job is in the bottom half of the tension distribution across all occupations in the economy.

¹¹To define similarity, I look at the first three digits of the four-digits occupation codes. For example, I exclude *Buyers and buyers' support* (control occupation) because too close to *Merchandisers* (treatment occupation), or *R&D technicians in electricity, electronics, and electro-mechanics* (control) because too similar to *design managers in electricity and electronics (treatment)*.

by looking at the same outcomes for this group. Finally, it measures whether the reform increased the precariousness of the employment contracts by looking at the effect on the share of temporary contracts for both migrants and natives. The model chosen to recover the effect of the reform follows a standard difference-in-differences specification with the addition of a few controls and several levels of fixed effects:

$$Y_{oirst} = \alpha + \beta_1 D_{or} + \beta_2 D_{or} * T_t + \beta_3 \mathbf{X}_{oirt} + \gamma_t + \delta_o + \rho_r + \sigma_s + \epsilon_{oirst} \quad (1)$$

Where Y_{oirst} captures the outcomes of interest within occupation o , plant i , region r , sector s and time t ; the treatment identifier D_{or} is a dummy varying at the region x occupation level and β_2 is the coefficient that directly measures the impact of the reform by estimating the differential trend observed in the treatment group after the reform ($D_{or} * T_t$). \mathbf{X}_{oirt} is a matrix of controls including the logarithm of plant size and the average tension in each regional occupation during the period preceding the reform, which allows to correct for any pre-treatment difference due to different levels of tension. Finally, the model includes year, occupation, region and sector fixed effects. In a robustness check we also test whether results are sensible to the introduction of additional levels of fixed effects. The standard errors are clustered at the plant level and the time period considered in the main regressions goes from 2004 to 2010. While the dataset would allow to analyze the effect up to 2014, the further away we get from the year of reform implementation, the harder it gets to interpret the coefficients are pure impact of the legal change. In addition, in 2011 the list was temporarily restricted by half, but was put back to its original state at the end 2012, thus including further complications for longer term analyses. However, in the graphic analysis we look at the full period available.

An important characteristic of all the flow outcomes is that they present a large number of zeros, since firms do not hire in all their occupations every year, especially when considering only the population of migrant workers. These zeros are important, because they signal the firm's choice of not to hire at that point in time, and therefore should be taken into consideration in the analysis. Consequently, linear estimators could lead to biased results. To solve for this issue, we follow the trade literature on gravity models, which developed unbiased estimators capable of taking into account the large number of zeros present in bilateral trade data (Beine, Bertoli and Fernández-Huertas Moraga, 2016), and that were further adopted by the migration literature (Alesina, Harnoss and Rapoport, 2016). In particular, for the estimation of the effect on the share of migrants in total hires and the share of temporary contracts, we

apply the fractional logit model suggested by [Papke and Wooldridge \(1996\)](#), which is a GLM estimator of the binomial family that uses a logit link function. This is variant of the Pseudo Poisson Maximum Likelihood estimator (PPML) proposed by [Silva and Tenreyro \(2006\)](#) that allow to accomodate fractional data¹². Finally, for the probability of hiring natives and migrants, we apply a logit binary model. The analysis based on the DADS data focuses on the impact of the reform on the total employment within occupations and plants, to check whether the effects observed on the flows are confirmed using stock data, and on the gross monthly salary of native and migrant workers, both overall and distinguishing between newly hired employees and incumbent workers. Given that here the outcomes do not suffer from the problem of zero-inflation, we use a linear estimator on the same difference-in-differences model presented in Equation (1) with the addition of plant fixed effects. Here as well we test the sensitivity of the results to different levels of fixed effects in a robustness test.

Table 1: Summary Statistics

<i>DMMO-EMMO Database</i>			<i>DADS Database</i>		
VARIABLES	Treatment mean/(sd)/sh. posit.	Control mean/(sd)/sh. posit.	VARIABLES	Treatment mean/(sd)	Control mean/(sd)
Share of migrant entries	0,03 (0,15) 5%	0,04 (0,17) 5%	Salary natives	3536 (2113)	3334 (2263)
N. of migrant entries	0,12 (1,62) 5%	0,10 (1,12) 5%	Salary native new entrants	3113 (1950)	2795 (2068)
Prob. of hiring a migrant	0,05 (0,22) 5%	0,05 (0,22) 5%	Salary native incumbent	3633 (2169)	3432 (2313)
Prob. of net migrant entry	0,04 (0,19) 4%	0,04 (0,19) 4%	Salary migrants	3598 (2273)	3034 (5782)
Sh. STC in migrant entries	0,35 (0,45) 2%	0,39 (0,47) 2%	Salary migrant new entrants	3191 (2034)	2664 (9385)
N. of native entries	2,10 (8,70) 57%	1,60 (6,51) 53%	Salary migrant incumbents	3756 (2361)	3185 (4316)
Prob. of hiring a native	0,57 (0,50) 57%	0,53 (0,50) 53%	N. of employees in occup.	2,2 (4,5)	1,9 (2,9)
Prob. of net native entry	0,36 (0,48) 36%	0,33 (0,47) 33%	N. entries from other occup. wn firm	0,72 (2,08)	0,63 (1,54)
Sh. STC in native entries	0,35 (0,44) 24%	0,37 (0,45) 23%	N. of exits to other occup. wn firm	0,23 (0,96)	0,20 (0,85)
Pre-reform average tension	0,19 (0,20)	0,15 (0,11)	Firm size	243 (661)	236 (661)
Firm Size	270,4 (699,1)	274,7 (724,0)	Pre-reform average tension	0,19 (0,20)	0,15 (0,11)
N. of observations	291 357	297 178	N. of observations	256'027	282'710
N. of occup. x establishments	154 788	160 138	N. of occup. x establishments	100'306	108'451
N. of establishments	62 546	61 613	N. of establishments	60'146	60'322

Note: All salary variables are expressed in terms of gross monthly salary in Euro 2010. Statistics refer to the unit of analysis occup. x plant x year. Period considered: 2004-2010, as in the regressions.

¹²In an earlier version of the paper we looked at the effect on the number of migrants and native hires in each occupation x plant using the PPML estimator. Results are consistent with the outcomes presented in this version but the absence of pre-reform common trend cannot be rejected. We thus decided to exclude them from the paper. These results are available upon request.

Table 1 presents the summary statistics of the main variables of interest across the two datasets, separately for treatment and controls. The two groups are similar across most characteristics. Migrants represent about 3% to 4% of total hires in both treatment and control occupations, and the probability of hiring a migrant in a given year is about 5%. The probability of net entry measures the likelihood of having more workers entering a given plant and occupation than exiting the same plant and occupation in a given year, and it is about 4% for migrants and 35% for natives. In addition, 5% of the occupations x plants considered hire at least one migrant in a given year while between 50% and 60% hire at least one native. This fact justifies the use of zero-inflated models for the flow outcomes. The share of temporary contracts (STC) in total hires is about 35% in both treatment and control occupations and is similar for migrants and natives. In the DADS data we observe about 2 employees per occupation within a plant, the gross monthly salary of natives is on average 3500 Euros in treatment occupations and 3300 Euros in controls, while foreigners have similar levels of salaries than natives within treatment occupations, they have a slightly lower salary than natives in control occupations. This signals that the jobs in question are relatively high skilled, and so are the migrants that are hired to do them. All the salary measures are expressed in terms of full-time equivalents, to avoid differences explained by hours worked, and are adjusted for inflation with 2010 as reference year.

Figure 2: Unconditional Growth Index in Hiring Composition

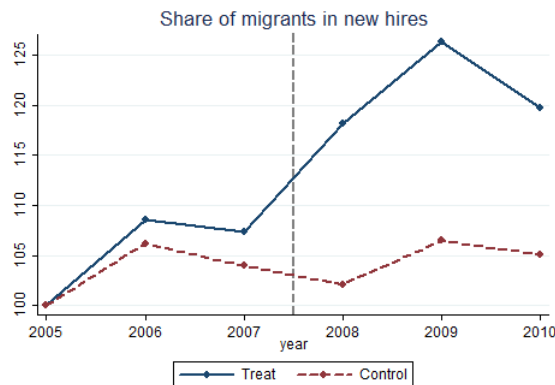


Figure 2 presents a graph showing the unconditional growth index in the share of migrant entries for both treatment and controls. The common trend seems to hold before the reform, even without conditioning neither for the level of pre-reform tension nor for the fixed effects. After the reform we see a clear increase in the share of migrants hired within treatment occupations while it remained stable within control occupations, which comforts the assumption of no strategic shifting in migrants

applications away from controls towards treatment jobs.

4 Results

4.1 Main results

Table 2 presents the main results obtained on employment flows. Column (1) presents the impact on the share of migrants in total hires, Columns (2) and (3) look at the effect on the probability of hiring natives, and columns (4) and (5) show the effect on the probability of hiring migrants. All the coefficients have to be interpreted in terms of incidence ratios. The reform increased the share of migrants in new hires by 13.5% on average over the three years following the reform, the probability of hiring a migrant in a given year rose by 16% and the probability of having a positive net entry of migrants increased by 13.7%. When looking at natives, the probability of entry and net entry are left unchanged by the rise in foreign inflows, so that the effect in column (1) can be seen as purely caused by higher migrant entries. The fact that native employment did not suffer from the reform is not surprising, since the occupations concerned were chosen because of their hiring difficulties, meaning that firms were employing less people than they would have liked in these professions. Given the legal requirements associated with the issuance of work permits, high levels of tension strongly increase the employment probability of migrants. On the other hand, treatment occupations appear to hire less migrants for a given level of tension, which can be explained by the fact that these jobs are slightly more skilled than controls and differ from the type of positions usually taken by foreigners. Finally, as predictable, large firms tend to hire more migrants, while it is not the case for natives. This can be due to the fact that it is very burdensome and costly for small companies to go through the procedure of work permit applications, so they tend to focus on local labor instead.

Table 3 presents the results obtained with the DADS data. The outcomes are inserted in the model in logarithmic form so that the coefficients can be interpreted as semi-elasticities. The results reported are based on a similar specification as the one applied to the flow data, with the only addition of firm fixed effects, and capture the total effect on both the extensive and intensive margins. Columns (1) and (2) present the results on employment stock, both in terms of number of employees and number of full-time equivalent workers, Columns (3) to (5) capture the effect on native salaries overall and disaggregated

Table 2: Main Results on Employment Flows

VARIABLES	(1)	Natives		Migrants	
	Share of migrants in hires	P. new hire	P. net positive entry	P. new hire	P. net positive entry
	FCT LOGIT	LOGIT	LOGIT	LOGIT	LOGIT
Treat	0.630*** (0.0925)	0.945 (0.0582)	1.096* (0.0600)	0.369*** (0.0473)	0.455*** (0.0682)
Treat x Post Reform	1.135*** (0.0369)	1.004 (0.0112)	1.009 (0.0115)	1.160*** (0.0305)	1.137*** (0.0338)
Pre-reform tension	1.231* (0.138)	0.991 (0.0383)	0.974 (0.0352)	1.375*** (0.142)	1.525*** (0.161)
log of firm size	1.046*** (0.00942)	1.010** (0.00461)	0.969*** (0.00415)	1.285*** (0.0135)	1.265*** (0.0136)
Constant	0.127*** (0.0170)	2.257*** (0.140)	0.645*** (0.0361)	0.107*** (0.0132)	0.0498*** (0.00699)
Observations	333,519	588,521	588,521	588,479	588,405
R2/ Pseudo R2	-	0.0285	0.0124	0.128	0.104

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

Standard errors clustered at firm level. All regressions include occupation, region, sector and year fixed effects. Coefficients reported in exponentiated form. Period of analysis: 2004-2010. The Fractional logit model used in column (1) is a glm estimator of the binomial family that uses a logit link function. It was first proposed by Papke & Wooldridge (1996) to accommodate zero-inflated fractional outcomes.

into new hires and incumbent workers, Columns (6) to (8) do the same for the salary of migrants. The first two columns confirm the results obtained on the movement data, showing that the overall stock of employment in treated occupations increased by 0.8% thanks to the reform, while the number of full-time equivalent workers increased by 1.1%. Native salaries did not show a significant loss overall, but the salaries of new hires did incur a drop of 6.8%. On the other hand, the salary of migrants suffered a significant loss of 3.5%, which can be disaggregated into a 13.6% decrease on the salaries of new hires and a 1.9% decrease for incumbent workers, which is likely to be the expression of slower growth, since wages are rarely decreased nominally within the same position. These findings are consistent with the canonical model of labor market equilibrium, where a positive supply shock generates a decrease in the price of labor. Nonetheless, the presence of such an important difference on the effect between natives and foreigners points towards the hypothesis that these two types of inputs may not be perfect substitutes in production, not even within the same occupation and firm. The salary regressions present very high R-squared, which can be explained by the battery of fixed-effects used.

Figure 3 provides the validation test for the common trend assumption on the main outcomes and shows how the effect evolved in the years following the reform. The same graphs for the additional outcomes not included here are reported in figure A5 in the appendix. The figures plot the coefficients and

the confidence intervals obtained from a flexible model interacting each year dummy with the treatment group indicator, showing the estimated difference in trends between treatment and control occupations in each year of the sample with respect to the last year preceding the reform. Here we include the entire period available in the data to give a sense on how the trends continued to evolve after 2010, which is the last year included in the regression analysis¹³. The difference between treated and controls was widely not significant before 2008, supporting the common trend assumption. Only in the probability of net positive entry of natives we see a slight decrease in the difference right before the reform. This may be due to some anticipation effects, even if the full-list of jobs was not made public before 2008. In a robustness test we show that the results hold if we exclude 2007 from the analysis. Finally, the effects on both employment and wages seem to be long-lasting, which can be interpreted as a success from the point of view of the objective to reduce the level of tension in these jobs.

Table 3: Main Results on Employment Stock and Salaries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All workers		Natives			Migrants		
	Log N. of employees	Log N. of FTE empl.	Log salary	Log salary new hires	Log salary incumbents	Log salary	Log salary new hires	Log salary incumbents
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat	-1.024*** (0.254)	-1.477*** (0.314)	-1.427*** (0.206)	-1.747** (0.739)	-1.484*** (0.0986)	0.173 (0.162)	-0.334 (0.214)	0.0826 (0.157)
Treat x Post Reform	0.00768*** (0.00262)	0.0106*** (0.00358)	-0.00323 (0.00268)	-0.0686*** (0.0133)	0.000850 (0.00275)	-0.0351*** (0.00721)	-0.136*** (0.0377)	-0.0191*** (0.00709)
Pre-reform tension	-0.00153 (0.0226)	0.00274 (0.0277)	0.00946 (0.0156)	-0.0384 (0.0461)	0.0127 (0.0161)	-0.00687 (0.0490)	0.0972 (0.167)	-0.0111 (0.0470)
Constant	1.925*** (0.254)	2.850*** (0.313)	8.432*** (0.204)	8.683*** (0.738)	8.537*** (0.0931)	7.285*** (0.135)	7.580*** (0.156)	7.499*** (0.139)
Observations	473,668	473,579	433,289	91,413	377,431	60,559	15,487	50,072
R-squared	0.492	0.466	0.713	0.761	0.733	0.771	0.800	0.806

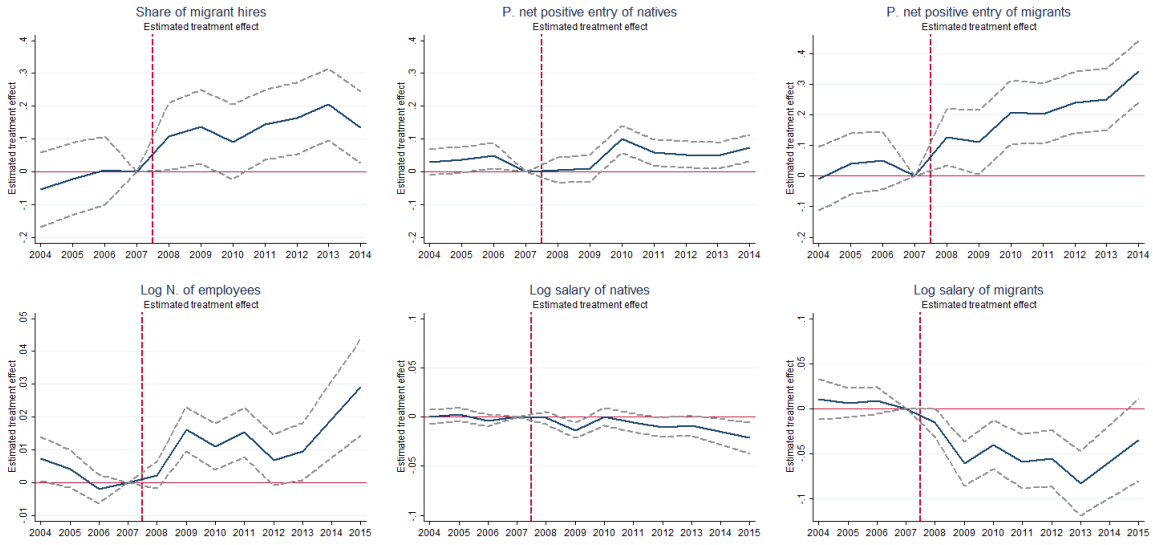
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered at the firm level. All regressions include firm, occupation, region, sector and year fixed effects. Period of analysis: 2004-2010. Column (2) expresses employment in terms of full-time equivalent. Salary outcomes are measured as the logarithm of gross monthly salary in 2010 Euros and are expressed in terms of full-time equivalent.

Table 4 presents the results on additional outcomes. The hiring data allows to distinguish between short term and open-ended contracts, so we construct a measure of share of short term contracts (STC) in new entries as a proxy for the quality of the jobs (see Column 1 and 2 of Table 4). The reform had no effect on the quality of jobs obtained by migrants and even decreased by 3.6% the proportion of STC in native hires, which contradicts the common hypothesis that greater immigration contributes to an increase in native precariousness. Finally, the short individual panel dimension of the DADS data allows

¹³Given that the reform was temporarily modified between 2011 and 2012 we exclude the post 2010 period from the econometric analysis.

Figure 3: Estimated Treatment Effect by Year



Note: The figure reports the estimated difference in trends between treatment and control occupations every year of the sample with respect to the last pre-reform year.

to recover whether the individual currently working in a given occupation was employed in a different occupation in the previous year, conditionally on having the same employer (see Column 3 and 4 of Table 4)¹⁴. It appears that the reform increased entries from other occupations within the firm in addition to raising hiring from outside, but did not create flight to other occupations in the firm, which could have been a possible native reaction to the increase in competition in treated jobs. In the next section we present the results of several robustness checks on the main outcomes of interest.

¹⁴Even though the DADS data does not allow to follow individuals through time but only firms, in any given wave it does provide information on what the individual was doing in terms of occupation and wage at year t-1 if he was already working for the current employer.

Table 4: Additional Outcomes

VARIABLES	(1)	(2)	(3)	(4)
	DMMO		DADS	
	Share of STC in native hires	Share of STC in migrant hires	Prob. entry from other occ. wn firm	Prob. exit to other occ. wn firm
	Frac Logit	Frac Logit	Logit	Logit
Treat	1.351*** (0.120)	1.256 (0.331)	0.433* (0.205)	3.785** (2.529)
Treat x Post Reform	0.964** (0.0154)	1.023 (0.0544)	1.040*** (0.0140)	1.005 (0.0197)
Pre-reform tension	0.664*** (0.0387)	0.393*** (0.0746)	1.033 (0.0460)	0.928 (0.0580)
Log firm size	0.958*** (0.00586)	0.901*** (0.0132)	1.165*** (0.00645)	1.362*** (0.0103)
Constant	4.357*** (0.382)	3.953*** (0.898)	2.522* (1.192)	0.0146*** (0.00974)
Observations	321,972	29,547	473 714	437,106
R2 / Pseudo R2	-	-	0.0345	0.0760

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

Standard errors clustered at the firm level. All regressions include occupation, region, sector and year fixed effects. DADS regressions also include firm FE. All coefficients reported in terms of incidence ratios. Period of analysis: 2004-2010.

4.2 Robustness checks and heterogeneity

Tables A2 to A8 in the appendix present the results of several sets of robustness tests. Tables A2, A3 and A4 perform two different placebo tests, introduce an alternative control group, and perform a propensity score correction on the employment flows outcomes (Table A2), on the main employment stock and salary outcomes (Table A3), and on salary disaggregated for new hires and incumbent workers and for natives and migrants (Table A4). The first placebo test checks the detected effect of a placebo-reform implemented in 2007 on the sample restricted to the years 2004-2007. A part from a negative and significant coefficient on the probability of net positive entry of natives, which was already visible from Figure 3 and may be explained with some anticipation effect, none of the other outcomes appear significant. The second placebo test considers only the observations in the control group and creates a placebo treatment indicator taking value one for the half of the observations with the highest pre-reform levels of tensions. We do this to test that the main results are not capturing some differential trend explained by skill-shortages themselves rather than by the reform. Here again the majority of the coefficients are insignificant. The only two of them significant at the 5% level are the share of migrant hires (negative) and the log of native salary (positive but very small in magnitude). Given that the sign of the coefficient is in both cases the opposite of what obtained in the main results, we are confident that if anything the latter are an upper bound. The test using an alternative control defines control

occupations as all private sector jobs within the top two quintiles of the tension distribution not included in the treatment list, regardless of whether they appear in the extended list or not. Results are similar to the ones from the main analysis. Finally, we compute the estimated propensity of entering the treatment list of occupations given the pre-reform level of tension, and we include the propensity score as additional control in the main regression. Again results are unchanged from the main analysis.

Tables A5 and A6 in the Appendix test how the main results vary when we add different levels of fixed effects, for the DMMO-EMMO and DADS data respectively. Here we have to go back to a linear model for the flow regressions to ensure computational feasibility. To ease the comparison with the main results we report the derived treatment effect in terms of percentage change from baseline at the bottom of the table (Table A5). For each of the outcomes and in addition to sector and year fixed effects that are included all along, in the first column we include only occupation and region fixed effects. In the second column we add firm fixed effects. In the third column we move to a full panel fixed-effect model since we include fixed effects for the main unit of analysis: occupation x plant. Finally, in the fourth column we include occupation, region, plant and sector x year fixed effects. The sign and significance remain the same across specifications for all of the outcomes, the magnitude generally gets smaller when sector x year fixed effects are included (except for the stock of employment from DADS), while the full fixed effect model increases the magnitude of the effect on the migrants flows, leaves it unchanged for the stock of employment, and decreases it slightly for the migrant salary.

Tables A7 and A8 in the appendix test the main specification in different sub-samples, for the DMMO-EMMO and DADS data respectively. The first column within every outcome excludes the year 2007 from the analysis to rule out any potential anticipation effect, the second column excludes the year 2009 to evaluate the results on the years less affected by the economic crisis, and the third column excludes the construction sector. Several of the occupations in the list specifically target this activity and some voices in the public debate expressed the fear that the policy may just have contributed to the legalization of previously illegal workers, defying the purpose of attracting new qualified labor into the country. The coefficients remain largely similar across all of the specifications. Only the effect on the probability of net positive entry of migrants becomes insignificant when the construction sector is excluded, but since the effect on the share of migrant hire and especially on the stock of employment and salary remain unchanged it may just be due to lack of power, given the very small proportion of positive values in the sample.

Finally, we look at the heterogeneity of the effects across some meaningful categories. Table 5 shows the heterogeneity analysis on the flow outcomes and table 6 does the same for the stock and salary outcomes. Panel A in both tables splits the sample between the seven regions that experienced a growth of economic visas above 50% between 2007 and 2008 and the rest of the country, based on the aggregate figures published by the Ministry of Interior¹⁵. Results using both datasets confirm that coefficients on all outcomes are much larger in magnitude and are more significant when the model is estimated on the sample of regions that present the highest jump in economic immigrants. In these areas, the share of migrant hires grew by 17.5%, the probability of hiring a migrant increased by 25% and the stock of employment in targeted jobs grew by 2.3%. The negative effect on wages is also more important in these areas, where the average native wage incurred a statistically significant loss of 1.5%. These results validate that the estimated impact of the reform using plant level administrative data reflects what is observed in the macro-level statistics.

Table 5: Heterogeneity of the effect on employment flows

	Share of migrant hires		P. net positive entry of natives		P. net positive entry of migrants	
	Low migration region	High migration region	Low migration region	High migration region	Low migration region	High migration region
<i>Panel A: by type of region</i>						
Treat * Post Reform	1.085 (0.0549)	1.175*** (0.0510)	0.993 (0.0144)	1.019 (0.0189)	1.039 (0.0477)	1.178*** (0.0473)
Observations	198,434	135,085	356,645	231,874	356,534	231,826
Pseudo R-squared	-	-	0.0144	0.0104	0.0858	0.0927
<i>Panel B: by tension in occup.</i>	Low tension	High tension	Low tension	High tension	Low tension	High tension
Treat * Post Reform	0.995 (0.0494)	1.261*** (0.0559)	0.986 (0.0162)	1.028* (0.0169)	0.976 (0.0443)	1.252*** (0.0514)
Observations	162,698	170,821	293,040	295,477	292,926	295,438
Pseudo R-squared	-	-	0.0115	0.0132	0.0905	0.118

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

Standard errors clustered at the firm level. All regressions include occupation, region, sector and year fixed effects. Period of analysis: 2004-2010. High migration regions are the regions where economic immigration grew by more than 50% between 2007 and 2008, low migration regions are the remaining ones. Low tension occupations are the 50% of occupations with the lowest level of tension before the reform while high tension are the other half.

The sample can further be split between the occupations characterized by very high levels of tension before the reform versus occupations where the tension was relatively lower (Panel B). To preserve the balance between treatment and controls, we divide both groups in two halves according to the distribution of the tension index and re-estimate the effect separately for high and low tension occupations. Not

¹⁵The seven regions with the largest growth in the flow of economic visas at the moment of the reform are Aquitaine, Corse, Ile de France, Languedoc-Roussillon, Midi-Pyrénées, Provence-Alpes-Côte d'Azur, Rhône-Alpes.

surprisingly, the occupations that reacted the most to the reform are the ones that were previously experiencing the most severe shortages of native labor. This indicates that the reform was successful in supporting the firms facing the highest hiring difficulties.

Table 6: Heterogeneity of the effect on employment stock and salaries

	Log salary natives		Log salary foreigners		Log N. employees	
	Low migration region	High migration region	Low migration region	High migration region	Low migration region	High migration region
<i>Panel A: by type of region</i>						
Treat x Post Reform	0.00441 (0.00333)	-0.0152*** (0.00458)	-0.0259*** (0.00966)	-0.0414*** (0.0106)	-0.00136 (0.00306)	0.0232*** (0.00481)
Observations	273,481	159,808	30,536	30,023	294,521	179,147
R-squared	0.701	0.715	0.747	0.784	0.501	0.487
<i>Panel B: by tension in occup.</i>						
Treat x Post Reform	-0.00440 (0.00396)	-0.00556 (0.00377)	-0.0210* (0.0114)	-0.0377*** (0.00949)	-0.00713* (0.00393)	0.0234*** (0.00382)
Observations	222,648	210,641	29,543	31,016	242,240	231,428
R-squared	0,755	0,765	0,808	0,817	0,550	0,661

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

Standard errors clustered at the firm level. All regressions include firm, occupation, region, sector and year fixed effects. Period of analysis: 2004-2010. High migration regions are the regions where economic immigration grew by more than 50% between 2007 and 2008, low migration regions are the remaining ones. Low tension occupations are the 50% of occupations with the lowest level of tension before the reform while high tension are the other half.

5 Elasticity of Substitution between Migrants and Natives

The canonical model of the labor market, notably presented in [Borjas \(1999\)](#), assumes that native and foreign workers active in the same skill segment can be seen as perfect substitutes in the firm production function. One of the theory's predictions is thus that a positive supply shock generated by an inflow of migrant labor exerts a negative pressure of equal magnitude on the wages of natives and foreigners, as the two inputs are assumed to be equivalent. Since the empirical literature often found results in contradiction with the assumption of perfect substitutability, several papers tried to measure this parameter using nested CES production functions. This paper follows a similar framework as in [Card \(2009\)](#), [Manacorda, Manning and Wadsworth \(2012\)](#), and [Ottaviano and Peri \(2012\)](#). However, while their estimates are done within cells of age and education, we compute the elasticity of substitution between migrants and natives within cells of occupations and establishments. By doing so we take advantage of the richness of our administrative data to estimate production technology at the level where production decisions actually

take place: the plant. In addition, the degree of substitution between labor inputs within occupations is a mechanism rarely explored by the literature and this level of analysis allows us to use the reform as an exogenous instrument and thus test the robustness of OLS estimations.

In our model the output Y_t of a representative firm is produced using labor inputs from different occupations o assembled through a CES production function:

$$Y_t = A_t \left[\sum_o \theta_{ot} \mathbf{L}_{ot}^{\frac{\sigma_o-1}{\sigma_o}} \right]^{\frac{\sigma_o}{\sigma_o-1}} \quad (2)$$

where σ_o is the elasticity of substitution between occupations and θ_{ot} is the occupation-specific efficiency parameter that may vary through time because of technological change, globalization etc. Without loss of generality we normalize $\sum_o \theta_{ot} = 1$. For simplicity, capital is assumed to be fixed and it is not reported. Further, the labor in each occupation L_{ot} combines inputs from native workers N_{ot} and migrant workers M_{ot} through a CES production function nested into the first level:

$$\mathbf{L}_{ot} = \left[\beta_{ot} M_{ot}^{\frac{\sigma_E-1}{\sigma_E}} + N_{ot}^{\frac{\sigma_E-1}{\sigma_E}} \right]^{\frac{\sigma_E}{\sigma_E-1}} \quad (3)$$

Where σ_E is the elasticity of substitution between natives and migrants, and β_{ot} is the relative efficiency of migrants with respect to natives within occupations, which can vary through time if there is a change in the relative quality of migrants, a change in discrimination biases, etc. By assuming that the marginal product of each labor input is equal to its marginal cost, we can derive the log-linearized wage equations of migrant workers as follows:

$$\ln(w_{ot}^M) = \ln(A_t) + \frac{1}{\sigma_o} \ln(Y_t) + \ln(\theta_{ot}) + \ln(\beta_{ot}) - \frac{1}{\sigma_E} \ln(M_{ot}) + \left(\frac{1}{\sigma_o} - \frac{1}{\sigma_E} \right) \ln(L_{ot}) \quad (4)$$

and of native workers analogously:

$$\ln(w_{ot}^N) = \ln(A_t) + \frac{1}{\sigma_o} \ln(Y_t) + \ln(\theta_{ot}) - \frac{1}{\sigma_E} \ln(N_{ot}) + \left(\frac{1}{\sigma_o} - \frac{1}{\sigma_E} \right) \ln(L_{ot}) \quad (5)$$

The relative wage of migrants to natives can thus be expressed with the following formula:

$$\ln\left(\frac{w_{ot}^M}{w_{ot}^N}\right) = \ln(\beta_{ot}) - \frac{1}{\sigma_E} \ln\left(\frac{M_{ot}}{N_{ot}}\right) \quad (6)$$

Where, net of changes in productivity captured by β_{ot} , the wage ratio of migrants to natives in each occupation depends on their relative supply, weighted by the inverse of their elasticity of substitution. To estimate equation (6) econometrically we have to set a restriction by forcing β_{ot} to vary additively by occupation and time, as done by [Manacorda, Manning and Wadsworth \(2012\)](#): $\beta_{ot} = f_o + f_t$. As previously explained, the richness of our data allows us to go one step further and look at the production process within each firm, instead of approximating the behavior of a representative firm. To do that we add establishment fixed effects and we obtain the following equation to be estimated:

$$\ln(W_{iot}^{M,N}) = \alpha_0 + \alpha_1 \ln(L_{iot}^{M,N}) + f_i + f_o + f_t + \epsilon_{iot} \quad (7)$$

Where $W_{iot}^{M,N}$ is calculated using the wage ratio of migrants to natives, and $L_{iot}^{M,N}$ is computed using the hiring ratio of migrants to natives within each plant i , occupation o and time t ¹⁶. α_1 is equal to $-\frac{1}{\sigma_E}$ and allows to recover the elasticity of substitution between migrants and natives. A common assumption in the literature is that, after controlling for changes in productivity through the different layers of fixed effects, the changes in relative supply of workers can be treated as exogenous and thus the relation can be estimated using OLS. However, there are reasons to believe that this might not be accurate, since relative wages might influence relative supply of workers through other channels than changes in productivity. For this reason, we compare the OLS results with an IV strategy using the reform as exogenous instrument for $L_{iot}^{M,N}$, as follows:

$$\ln(L_{iot}^{M,N}) = \beta_0 + \beta_1 D_{or} * Post2008_t + \beta_2 \mathbf{X}_{iot} + f_i + f_o + f_t + v_{iot} \quad (8)$$

¹⁶To increase the sample size of establishments x occupations for which we observe both migrants and natives, here we rely on the complete DADS dataset which includes the universe of private sector employees, instead of the 1/12th sample used in the reduced form analysis. Similarly, to increase the power in the estimation we extend the period of analysis to 2004-2012.

$$\ln(W_{iot}^{M,N}) = \alpha_0 + \alpha_1 \ln(\widehat{L_{iot}^{M,N}}) + \alpha_2 \mathbf{X}_{iot} + f_i + f_o + f_t + \epsilon_{iot} \quad (9)$$

The sample is restricted to the treatment and control occupations as defined in the previous sections, and therefore results are to be interpreted as the degree of substitution in these set of technical competencies.

Table 7: Elasticity of substitution between migrants and natives

VARIABLES	(1)	(2)	(3)
	Log ratio wages migrants to natives		log ratio new hires migrants to natives
	OLS	IV	FS
log ratio new hires migrants to natives	-0.0156*** (0.00312)	-0.110* (0.0646)	
Treat * Post reform (instrument)			0.183*** (0.0405)
Observations	12,568	12,568	12,568
R-squared	0.483	0.429	0.640
C-D Wald F-stat		22.49	
K-P rk Wald F-stat		20.31	
Estimated degree of substitution	64.1	9.1	
Year FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

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

All regressions include firm, occupation and year fixed effects. Period of analysis: 2004-2012.

Column (1) of Table 7 reports the results obtained with the OLS estimation, column (2) reports the ones from the IV estimation, and column (3) shows the first stage results. From the OLS estimation we obtain that an increase in the relative supply of migrant workers by 1% decreases their relative wage by 0.02%, which translates into an elasticity of substitution parameter of about 64 (arguably close to perfect substitution). The IV estimation results, however, show an elasticity of substitution of about 9, which is seven times smaller than the OLS. This suggests that linear models may lead to the over-estimation of the degree of substitution between migrants and natives, which appear to be imperfect substitutes in production even when they are employed by the same firm and in the same occupation. This may be explained by the fact that they can differ in their level of seniority, management responsibilities, precise task specialization or other dimensions that are too fine to be captured by the occupational classification. While the magnitude of the measured parameter has to be interpreted in the context of the specific set

of occupations concerned by the reform, the existence of a bias in the OLS estimation may extend to other contexts, and corroborates results from [Card \(2009\)](#), which found a similar bias using a shift-share instrument on education x city cells. The first stage shows that the reform significantly increased the relative supply of migrants to natives and that the instrument is strong according to conventional levels. Finally, Table A9 in the Appendix shows the reduced form results on both the relative supply and the relative wage of migrants to natives, which confirm what found in the main analysis.

6 Conclusion

This paper takes advantage of a migration reform introduced in France in 2008 to evaluate the impact of a quasi-exogenous increase in high skill immigrant workers on the labor market outcomes of natives working in the same jobs. The policy setting allows to identify very precisely the list of occupations affected by the legal change and provides a natural control group for a difference-in-differences identification. Additionally, the richness of the administrative data used makes it possible to measure the effect on hiring patterns, employment stocks and wages and to distinguish between the outcomes on natives and foreign workers. Finally, the paper exploits the exogenous source of variation in the relative labor supply of migrants with respect to natives to recover the elasticity of substitution parameter with an IV strategy and compare it with the same measure obtained through OLS.

The results from the main empirical analysis show that the hiring of non-European workers increased within targeted occupations, while native employment flows remained largely unaffected. The reform was thus able to alleviate the problem of skill-shortages, at least in the short run. We also detect a negative pressure on entry wages that is twice as large on foreign salaries than on natives ones, signaling that natives are in part shielded from the additional competition generated by the reform. The results are robust to a variety of tests and the heterogeneity analysis reveals that they are driven by occupations that were afflicted by severe hiring difficulties prior to the policy. The analysis on the elasticity of substitution parameter reveal that, in this particular set of occupations, natives and migrants are imperfect substitutes in production, even when they are employed by the same firm to do the same job. In addition, the standard OLS estimations seem to over-estimate the parameter of interest.

From a policy perspective, favoring high-skill immigration seem to be an effective tool to counter domestic skill shortages, at least in the short run, and the cost paid by native workers appear to

be limited. Further analysis should be undertaken to test whether in the long run this kind of policies generate any disincentives for the adaptation of the domestic education system, since they help providing the needed competencies from abroad. An important criticism of this policy reside in the fact that the list has remained unchanged up to this date, while the needs of the labor market have significantly evolved over the past ten years. The periodic adaptation of the list, as it is done in the UK for instance, might help maximize the support given to domestic firms and minimize the cost for local labor.

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Appendix

Tables

Table A1: List of 30 Occupations in the 2008 Reform (1st part)

Occupation	Rhône-Alpes	Provence-Alpes-Côte d'Azur	Poitou-Charentes	Picardie	Pays de la Loire	Haute Normandie	Basse Normandie	Nord Pas de Calais	Midi Pyrénées	Lorraine	Limousin
Technician of distant selling	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sales repr. for intermed. goods and raw materials	yes	0	yes	yes	yes	yes	yes	0	yes	0	yes
Executive of financial audit	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Computer scientist	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Senior computer scientist	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Marchandiser (design of stores and shelves)	yes	yes	yes	yes	yes	yes	0	yes	yes	yes	0
Operations manager in insurance	yes	0	yes	0	yes	yes	yes	0	yes	yes	yes
Mechanic of construction machinery and agricultural machinery	yes	yes	yes	0	yes	yes	yes	yes	yes	yes	yes
Operator of glass production	yes	0	0	yes	yes	yes	0	yes	0	yes	yes
Installation driver for cement production	yes	yes	yes	0	yes	yes	yes	yes	yes	yes	yes
Machinery specialist for wood and furniture production	yes	yes	yes	yes	yes	0	0	0	yes	yes	yes
Artisan of wood and furniture production	yes	yes	yes	yes	yes	0	0	0	yes	yes	yes
Technician of industrial planning and methods	yes	yes	yes	0	0	yes	0	0	yes	0	yes
Design manager mechanical construction	yes	yes	0	0	yes	yes	0	yes	yes	0	0
Industrial designer mechanic. construction and metallurgy	yes	yes	0	0	yes	yes	0	yes	yes	0	0
Design manager in electricity and electronics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industrial designer in electricity and electronics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Production technician in mechanic. construction and metallurgy	yes	yes	yes	yes	yes	yes	0	0	0	0	yes
Quality-manager mechanic. construction and metallurgy	yes	yes	yes	yes	yes	yes	0	yes	yes	0	yes
Quality-manager in electricity and electronics	yes	0	0	0	0	0	0	0	0	0	yes
Production technician of process industry	yes	yes	0	yes	0	yes	0	0	yes	0	0
Production technician of wood and furniture	yes	yes	yes	0	yes	yes	0	yes	yes	yes	yes
Installation and maintenance of elevators	yes	yes	yes	yes	yes	yes	0	yes	0	yes	yes
Compliance inspector	yes	yes	0	0	yes	yes	0	yes	yes	yes	0
Responsible for electronic maintenance	yes	yes	0	0	0	0	yes	0	0	0	0
Designer of buildings and public work	yes	yes	0	yes	yes	yes	0	0	yes	0	yes
Construction surveyor	yes	yes	0	0	yes	0	yes	0	0	0	yes
Head of technical studies for buildings and public work	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	0
Head of technical studies for the underground	0	0	0	0	0	0	0	0	0	0	0
Head of construction sites for buildings and public work	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	0
Site supervisor for buildings and public work	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	0

Occupations in bold are the ones at the national level. Source: Decree n.0017 of January 20th 2008

Table A1: List of 30 Occupations in the 2008 Reform (2nd part)

Occupation	Languedoc Roussil- lon	Île de France	Franche Comté	Corse	Champagne Ardenne	Centre	Brétagne	Bourgogne Auvergne	Alsace	Aquitaine
Technician of distant selling	yes	yes	0	0	yes	yes	yes	yes	yes	yes
Sales repr. for intermed. goods and raw ma- terials	yes	yes	yes	0	yes	yes	0	yes	yes	0
Executive of financial audit	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Computer scientist	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Senior computer scientist	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Marchandiser (design of stores and shelves)	yes	yes	yes	0	yes	yes	yes	yes	yes	yes
Operations manager in insurance	0	yes	yes	yes	yes	yes	0	yes	yes	0
Mechanic of construction machinery and agri- cultural machinery	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Operator of glass production	yes	yes	yes	0	yes	yes	yes	yes	yes	yes
Installation driver for cement production	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Machinery specialist for wood and furniture production	yes	yes	yes	0	yes	yes	yes	yes	yes	yes
Artisan of wood and furniture production	yes	yes	yes	0	yes	yes	yes	yes	yes	yes
Technician of industrial planning and methods	yes	yes	yes	0	yes	yes	0	yes	yes	0
Design manager mechanical construction	0	yes	yes	0	yes	yes	0	yes	yes	0
Industrial designer mechanic. construction and metallurgy	0	yes	yes	0	yes	yes	0	yes	yes	0
Design manager in electricity and electronics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Industrial designer in electricity and electronics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Production technician in mechanic. construc- tion and metallurgy	0	yes	yes	0	yes	yes	0	yes	yes	yes
Quality-manager mechanic. construction and metallurgy	0	yes	yes	0	yes	yes	yes	yes	yes	yes
Quality-manager in electricity and electronics	yes	yes	0	0	0	0	0	0	yes	0
Production technician of process industry	0	yes	0	yes	yes	yes	0	0	yes	0
Production technician of wood and furniture	yes	yes	yes	0	yes	yes	0	yes	yes	yes
Installation and maintenance of elevators	yes	yes	0	0	yes	yes	0	yes	yes	yes
Compliance inspector	0	yes	0	yes	0	yes	0	yes	yes	0
Responsible for electronic maintenance	0	yes	0	0	0	0	0	0	yes	0
Designer of buildings and public work	0	yes	yes	0	yes	0	0	yes	yes	0
Construction surveyor	0	yes	yes	0	yes	0	yes	0	yes	0
Head of technical studies for buildings and public work	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Head of technical studies for the underground	0	0	0	0	0	yes	0	0	0	0
Head of construction sites for buildings and public work	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Site supervisor for buildings and public work	yes	yes	yes	0	yes	yes	yes	yes	yes	yes

Occupations in bold are the ones at the national level. Source: Decree n. 0017 of January 20th 2008

Table A2: Placebo tests, alternative treatment and p-score correction on flow outcomes

VARIABLES	Share of migrant hires		P. net positive entry of natives		P. net positive entry of migrants	
	Placebo reform 2007	Placebo treatment	Placebo reform 2007	Placebo treatment	Placebo reform 2007	Placebo treatment
Treat * Post Reform	1.008 (0.0451)	0.917** (0.0403)	0.956*** (0.0156)	1.008 (0.0165)	0.956 (0.0386)	0.980 (0.0408)
Observations	201,859	163,186	348,264	297,168	348,144	297,052
Pseudo R-squared	-	-	0.0138	0.0125	0.107	0.110

VARIABLES	Alternative control	p-score correction	Alternative control	p-score correction	Alternative control	p-score correction
	FCT LOGIT	FCT LOGIT	LOGIT	LOGIT	LOGIT	LOGIT
Treat * Post Reform	1.104*** (0.0367)	1.136*** (0.0369)	1.001 (0.0118)	1.009 (0.0115)	1.108*** (0.0327)	1.137*** (0.0338)
Observations	392,620	333,517	668,860	588,514	667,729	588,400
Pseudo R-squared	-	-	0.0118	0.0124	0.104	0.104

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

Standard errors clustered at firm level. All regressions include occupation, region, sector and year fixed effects. Coefficients reported in exponentiated form. Period of analysis: 2004-2010. The Fractional logit model is a glm estimator of the binomial family that uses a logit link function. It was first proposed by Papke & Wooldridge (1996) to accommodate zero-inflated fractional outcomes. Placebo reform refers to regressions on the period 2004-2007 where 2007 is the placebo post-reform period. Placebo treatment refers to regressions where only the control occupations are included and placebo treatment is assigned to 50% of control occupations with the highest level of tension before the reform. Alternative control restrict the sample to occupations in the 4th and 5th percentile of tension distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). P-score correction refers to regressions controlling for the propensity to be treated given the pre-reform level of tension in occupation.

Table A3: Placebo tests, alternative treatment and p-score correction on stock and salary outcomes

VARIABLES	Log salary natives		Log salary migrants		Log N. employees	
	Placebo reform 2007	Placebo treatment	Placebo reform 2007	Placebo treatment	Placebo reform 2007	Placebo treatment
	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	0.00217 (0.00308)	0.00793** (0.00394)	-0.00204 (0.00756)	0.00806 (0.00830)	-0.00208 (0.00264)	0.00445 (0.00398)
Observations	257,358	223,463	37,483	34,130	282,634	246,429
R-squared	0.780	0.786	0.822	0.823	0.545	0.559
VARIABLES	Alternative control	P-score correction	Alternative control	P-score correction	Alternative control	P-score correction
	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	-0.00264 (0.00256)	-0.00327 (0.00268)	-0.0294*** (0.00768)	-0.0351*** (0.00722)	0.0116*** (0.00267)	0.00768*** (0.00262)
Observations	587,114	433,283	73,968	60,558	638,425	473,661
R-squared	0.697	0.713	0.774	0.771	0.495	0.492

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

All regressions include firm, occupation, region, sector and year fixed effects. Period of analysis: 2004-2010. Placebo reform refers to regressions on the period 2004-2007 where 2007 is the placebo post-reform period. Placebo treatment refers to regressions where only the control occupations are included and placebo treatment is assigned to 50% of control occupations with the highest level of tension before the reform. Alternative control restrict the sample to occupations in the 4th and 5th percentile of tension distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). P-score correction refers to regressions controlling for the propensity to be treated given the pre-reform level of tension in occupation.

Table A4: Placebo tests, alternative treatment and p-score correction on additional salary outcomes

VARIABLES	Log salary new entrants				Log salary incumbent workers			
	Placebo reform 2007	Placebo treatment	Alternative control	P-score correction	Placebo reform 2007	Placebo treatment	Alternative control	P-score correction
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
<i>Panel A: Natives</i>								
Treat x Post Reform	-0.00928 (0.0152)	0.0348 (0.0253)	-0.0594*** (0.0111)	-0.0685*** (0.0133)	0.00104 (0.00317)	0.00252 (0.00403)	-0.00123 (0.00257)	0.000821 (0.00275)
Observations	67,745	44,389	133,012	91,411	213,329	195,754	508,411	377,427
R-squared	0.831	0.834	0.726	0.761	0.793	0.797	0.724	0.733
<i>Panel B: Migrants</i>								
Treat x Post Reform	-0.0354 (0.0384)	-0.0198 (0.0462)	-0.0999*** (0.0376)	-0.136*** (0.0376)	0.00661 (0.00749)	-0.000539 (0.00844)	-0.0203*** (0.00739)	-0.0191*** (0.00709)
Observations	11,189	8,275	20,247	15,486	29,625	28,545	60,154	50,072
R-squared	0.842	0.859	0.784	0.800	0.851	0.844	0.819	0.806

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

Standard errors clustered at the firm level. All regressions include firm, occupation, region, sector and year fixed effects. Period of analysis: 2004-2010. Placebo reform refers to regressions on the period 2004-2007 where 2007 is the placebo post-reform period. Placebo treatment refers to regressions where only the control occupations are included and placebo treatment is assigned to 50% of control occupations with the highest level of tension before the reform. Alternative control restrict the sample to occupations in the 4th and 5th percentile of tension distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). P-score correction refers to regressions controlling for the propensity to be treated given the pre-reform level of tension in occupation.

Table A5: Flow results adding different levels of fixed effects

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Share of migrant hires				P. net positive entry of natives				P. net positive entry of migrants			
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat * Post Reform	0.0036*** (0.0012)	0.0039*** (0.0015)	0.0064*** (0.0017)	0.0026* (0.0015)	0.0011 (0.0025)	-0.0017 (0.0029)	-0.0051 (0.0037)	-0.0024 (0.0029)	0.0044*** (0.0010)	0.0038*** (0.0012)	0.0080*** (0.0016)	0.0026** (0.0012)
Observations	333,519	333,519	333,519	333,519	588,521	588,521	588,521	588,521	588,521	588,521	588,521	588,521
R-squared	0.038	0.303	0.001	0.304	0.016	0.213	0.014	0.214	0.039	0.195	0.002	0.196
Occupation FE	yes	yes		yes	yes	yes		yes	yes	yes		yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE		yes		yes		yes		yes		yes		yes
Firm x Occupation FE			yes				yes				yes	
Sector x year FE				yes				yes				yes
Mean baseline treatment		0.031				0.366				0.035		
TE in % change	12%	13%	21%	9%	0%	0%	-1%	-1%	13%	11%	23%	7%

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

Standard errors clustered at firm level. All regressions include sector and year fixed effects. Period of analysis: 2004-2010. The last row reports the treatment effect in terms of percentage change from the pre-reform levels to ease interpretation.

Table A6: Stock and salary results adding different levels of fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Log salary natives				Log salary migrants				Log N. employees			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	-0.0107*** (0.00256)	-0.00323 (0.00268)	0.000203 (0.00212)	-0.00377 (0.00282)	-0.0574*** (0.00677)	-0.0351*** (0.00721)	-0.0139*** (0.00511)	-0.0279*** (0.00786)	0.0076*** (0.0024)	0.0077*** (0.0026)	0.0076*** (0.0023)	0.0080** (0.0037)
Observations	433,289	433,289	433,289	433,289	60,559	60,559	60,559	60,559	473,668	473,668	473,668	473,579
R-squared	0.517	0.713	0.023	0.713	0.488	0.771	0.031	0.772	0.236	0.492	0.001	0.467
Occupation FE	yes	yes		yes	yes	yes		yes	yes	yes		yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE		yes		yes		yes		yes		yes		yes
Firm x Occup FE			yes				yes				yes	
Sector x year FE				yes				yes				yes

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the firm level. Sector and year fixed effects included in all regressions. Period of analysis: 2004-2010.

Table A7: Flow results using different sub-samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Share of migrant hires			P. net positive entry of natives			P. net positive entry of migrants		
VARIABLES	FCT LOGIT	FCT LOGIT	FCT LOGIT	LOGIT	LOGIT	LOGIT	LOGIT	LOGIT	LOGIT
Treat * Post Reform	1.140*** (0.0402)	1.120*** (0.0398)	1.067* (0.0409)	0.998 (0.0122)	1.020 (0.0129)	0.972** (0.0122)	1.127*** (0.0364)	1.151*** (0.0373)	1.013 (0.0347)
Observations	280,238	295,202	271,618	499,227	511,941	491,729	499,123	511,847	491,632
Pseudo R-squared	-	-	-	0.0123	0.0116	0.0114	0.102	0.105	0.0917
Exclusion of 2007	yes			yes			yes		
Exclusion of 2009		yes			yes			yes	
Exclusion of construction sector			yes			yes			yes

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at firm level. All regressions include occupation, region, sector and year fixed effects. Coefficients reported in exponentiated form. Period of analysis: 2004-2010. The Fractional logit model is a glm estimator of the binomial family that uses a logit link function. It was first proposed by Papke & Wooldridge (1996) to accommodate zero-inflated fractional outcomes.

Table A8: Stock and salary results using different sub-samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log salary natives			Log salary migrants			Log N. employees		
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	-0.00365 (0.00300)	-0.000271 (0.00297)	-0.00261 (0.00301)	-0.0409*** (0.00862)	-0.0236*** (0.00745)	-0.0251*** (0.00947)	0.00677** (0.00308)	0.00462* (0.00259)	0.00697** (0.00306)
Observations	368,930	373,131	355,921	49,744	54,428	41,586	401,847	410,356	384,227
R-squared	0.716	0.726	0.725	0.775	0.782	0.779	0.491	0.508	0.501
Exclusion 2007	yes			yes			yes		
Exclusion 2009		yes			yes			yes	
Exclusion construction sector			yes			yes			yes

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the firm level. All regressions include firm, occupation, region, sector and year fixed effects. Period of analysis: 2004-2010.

Table A9: Reduced form results from elasticity of substitution analysis

	(1)	(2)
	log ratio new entries migrants to natives	Log ratio wages migrants to natives
VARIABLES	OLS	OLS
Treat * Post Reform	0.122*** (0.0352) (0.184)	-0.0104** (0.00470) (0.0213)
Observations	16,782	98,314
R-squared	0.623	0.294
Year FE	Yes	Yes
Occupation FE	Yes	Yes
Plant FE	Yes	Yes
Tension and plant size controls	Yes	Yes

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
 All regressions include firm, occupation and year fixed effects. Period of
 analysis: 2004-2012.

Figures

Figure A1: Administrative Procedure to Hire a Foreign Worker

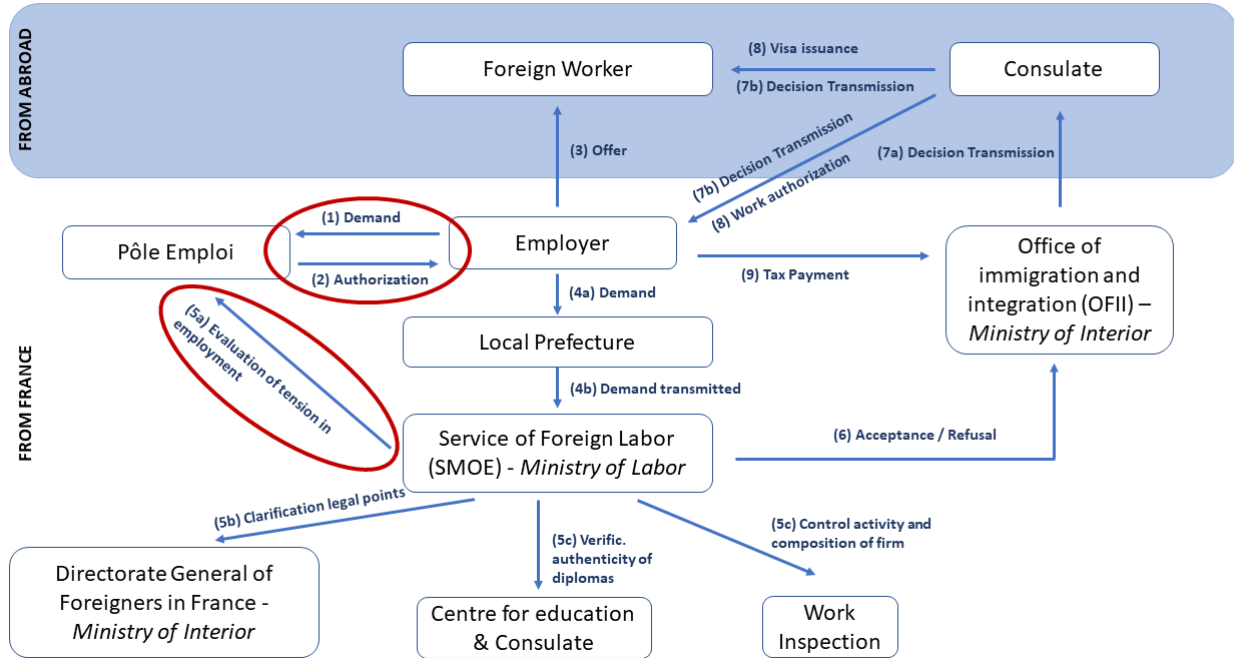


Diagram taken from OECD (2017). The red circles identify the administrative steps relaxed by the reform.

Figure A2: Share of Employment Concerned by the Reform by Broad Occupations and Sectors

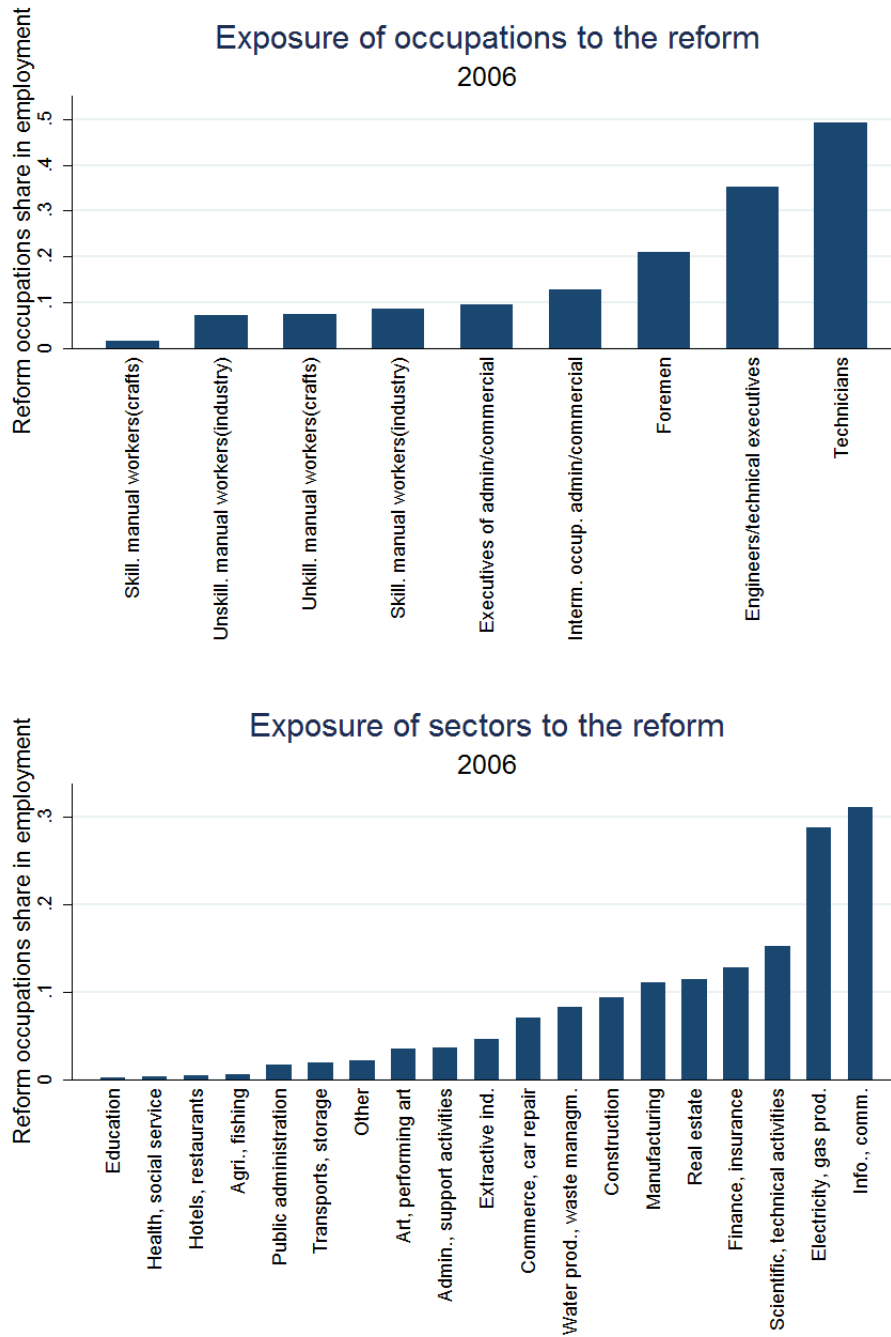


Figure A3: Overall Immigration Flows by Year

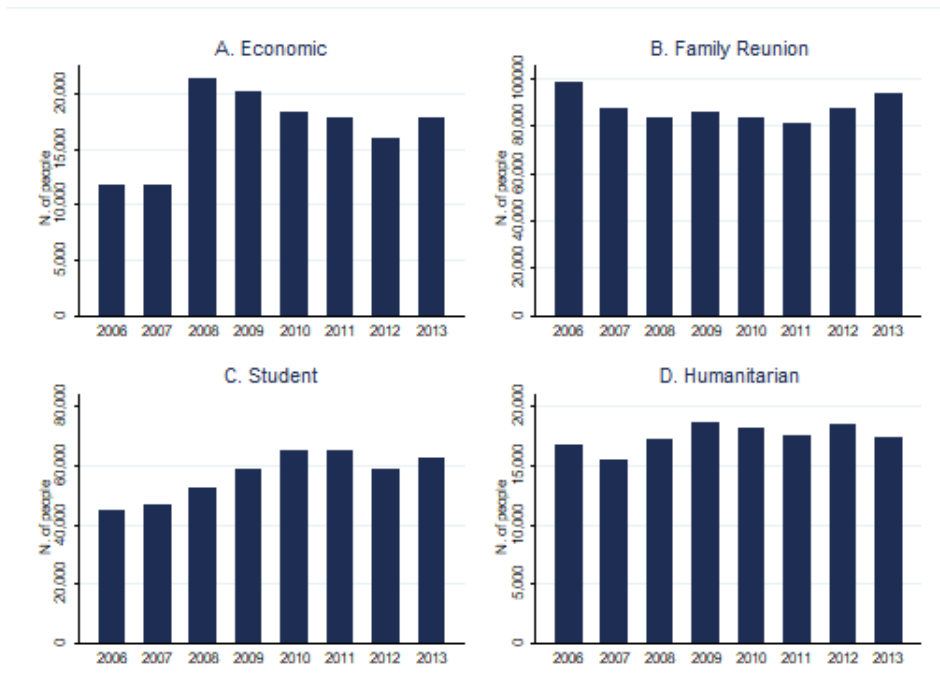


Figure A4: Economic Immigration Flows by Year



Figure A5: Additional estimated outcome trends

