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

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Abstract

In recent years high-skill immigration has been often encouraged by governments aiming to support their economy, but its impact on native workers facing a direct increase in competition is still debated. This paper addresses the question by taking advantage of a reform facilitating the hiring of foreign workers within a list of technical occupations. The analysis relies on administrative employer-employee data and applies a difference-in-differences approach. Results show that the reform was successful in boosting migrants' hires without affecting native employment. Wages decrease following the supply shift but, in contrast with the standard model predictions, do so twice as much for migrants than for natives. I find that two channels explain this differential effect: imperfect degree of substitution in production and differences in bargaining power. Overall, this paper provides evidence that policies encouraging high-skill migration do not excessively harm the native labor force.

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1 Introduction

Several Western countries have adopted selective immigration policies over the past decades, but there is still a lack of consensus regarding their effectiveness and their consequences for incumbent workers.[1] In the COVID-19 aftermath, this question has become even more central in the policy debate, following the US decision to freeze the largest visa program for high-skill immigrants in an attempt to protect American jobs.[2] This paper addresses the issue by exploring whether a French reform that increased the inflow of migrants within a specific set of high-skill occupations generated any negative effect for the natives employed in the same jobs. Results suggest that French workers saw no changes in employment and were partly shielded from the negative wage pressure generated by the migration shock, such that foreign workers bore the majority of the cost. My contribution to the literature is to show that the wage burden imposed on natives is much lower than what is predicted by the canonical model in a context where the group facing the direct increase in competition can be precisely identified, and where there is little scope for migrants' skill-downgrading.[3] From a policy perspective, these findings imply that restricting skilled immigration might be an ineffective tool to support the native labor force.

The policy under study lowers the firms' administrative costs for hiring extra-European workers within a list of 30 occupations. Its aim is to help employers accessing the skills that they need. The professions concerned are selected based on their high level of labour market tightness: the observed imbalance between the number of vacancies and the number of available job seekers with the relevant qualifications. While the competencies demanded vary, they mostly require a technical specialisation from upper secondary or tertiary education. One advantage of this reform is that it introduces a second list of occupations selected based on the same criteria, but where the

[1] For 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.

[2] The H-1B visa program is the main channel used by US firms to hire high-skill immigrant workers. In 2019 about 190 thousands individuals were hired thanks to this system.

[3] Skill-downgrading refers to the fact that the first job occupied by immigrants in the host country often requires less qualifications than the one they possess. As a result, they do not directly compete with native workers with similar levels of education.

administrative procedure is only lessened for workers coming from Romania and Bulgaria.[4] This provides a natural control group for the difference-in-differences estimation, under the testable assumption that occupations in both lists were evolving comparably before the reform, and that the ones in the treatment list are exposed to a larger inflow of workers after the policy change. The analysis is based on two administrative employer-employee datasets. The first contain information on all recruitments and terminations carried out by firms over time, while the second reports the wage of all workers employed by them. Both sources include detailed occupation codes and distinguish between broad nationality groups, which enables the study of the differential impact of the reform on natives and migrants.

I analyse the results through the lenses of a standard model of occupational labour market equilibrium. At the starting point, the demand curve crosses supply in a portion where it is close to being perfectly inelastic, implying high levels of labour market tightness. In this setting, positive demand shifts increase wages but have a limited effect on equilibrium employment. The reform increases the elasticity of supply by broadening the pool of job seekers. As a result, the equilibrium employment in these jobs is expected to grow, driven by additional migrant hires, while the equilibrium wage is expected to decline. Under the assumption of homogeneity between migrants and natives employed in the same jobs, wages of both types of workers are expected to drop by a similar magnitude. Results show that the policy increases the probability of hiring a migrant, the number of newly hired migrants and the share of new hires made of migrant workers in targeted jobs, an indication that employers took advantage of the lighter regulations. In line with the model predictions, recruitment prospects of natives are mostly unaffected, resulting in a 1.4% growth of total employment in these occupations. Wages do decrease as expected, but do so twice as much for newly hired migrants (-14%) than for newly hired natives (-7%).

One possible explanation for this discrepancy is that migrants and natives are imperfect substitutes in production, even when they are employed in the same occupations, such that the supply shock generated by the reform creates greater competition for incumbent foreign workers

[4]European Union countries under a transitory regime preventing them from accessing freely the common labour market.

than for French nationals.[5] A second possibility is that there are rents in these markets such that natives can leverage their greater outside options to extract a larger portion of them.[6] To test these channels, I start by estimating the elasticity of substitution parameter that is derived from a CES production function combining native and migrant inputs. Here the policy is used as an exogenous instrument for the relative supply of migrant to native workers, which is in turn regressed on their relative wage using a 2SLS procedure. If the two inputs were perfect substitutes, a change in relative supply would have no effect on relative wages, which translates into an elasticity of substitution equal to infinity. I obtain a parameter that is above unity but well below infinity, hinting towards imperfect elasticity of substitution between native and migrant labour. In a second step, I check how the parameter changes in contexts where there is more or less scope for differential task specialisation within occupations and in contexts where natives have more or less bargaining power with respect to migrants. For the first, I find that migrants and natives are less substitutable within occupations involving a wider range of activities, where there is more scope for differential specialisation. For the second, I find that migrants and natives are more substitutable in contexts with high monopsony power and low occupational mobility, where the outside options of natives are as limited as those of migrants. I conclude that both imperfect degree of substitution in production and differences in bargaining power play a role in explaining the results. Finally, I test for potential composition effects by looking at whether workers that were employed in reform occupations right before the policy are more likely to change jobs after the reform (commonly known as native flight). Results do not show any change in mobility patterns.

This paper contributes to the literature on the labour market effects of immigration. While a large portion of existing work focuses on flows of uneducated migrants and refugees,[7] there is a growing strand of the literature looking at the effect of high-skill immigration, typically in the

[5]Tenured native workers may be promoted to supervisors of the newly hired migrants within a given occupation. More broadly, they may specialize in different tasks based on their comparative advantage, making them imperfect substitutes.

[6]Economic migrants have their visa tight to the firm and occupation for which they were granted access. It is therefore much harder for them to switch employer with respect to native workers. This fact supports the hypothesis that they have less bargaining power when it comes to wage setting.

[7]The 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 labour. See for example Card (2001), Borjas (2003), Ottaviano and Peri (2012).

context of H-1B visas in the US.[8] Peri, Shih and Sparber (2015) and Mayda et al. (2018) find that skilled migrants and natives tend to be either complementary or imperfect substitutes in production, which is in line with the findings of this paper. Peri, Shih and Sparber (2015) look at cities more or less exposed to an increase in H1-B visas while Mayda et al. (2018) compare for-profit and non-profit sectors after a reduction of visas' quotas that only affect the former. I differ from their analysis by focusing on the effect for workers that face a direct increase in competition from immigration, which is made possible by the narrow focus of the reform on specific occupations. By doing so I can isolate the impact on the group that is the most likely to suffer following the inflow of migrants. In addition, by comparing workers with different occupations within the same firms and locations, I abstract from aggregate mechanisms that might take place at the level of the sector or the city.

This analysis more broadly relates to the migration papers making use of natural experiments, which made some progress in the identification of causal effects in the context of the pervasive endogeneity of mobility decisions (Dustmann, Schönberg and Stuhler, 2016). A common result underlined by these contributions is that native workers with similar levels of education are in part shielded from additional competition since migrants often suffer from skill-downgrading that pushes them to specialise in lower-level jobs than those they are qualified for (Peri and Sparber, 2009; D'Amuri, Ottaviano and Peri, 2010; Dustmann, Frattini and Preston, 2012; Ortega and Verdugo, 2016). In the context considered by this paper, there is little scope for skill-downgrading, given that the match between the skill requirements of the position and the qualifications of the immigrant candidate is thoroughly examined by the local authorities before granting the visa.[9] I can thus go one step further with respect to previous findings and show that, even in the absence of such phenomenon, migrants and natives cannot be considered as homogeneous inputs, as it is imposed by the canonical model of Borjas (1999). Foged and Peri (2016) further find that low-skill natives react to a sudden inflow of refugees by moving to better-paid jobs, thus improving their

[8] See Kerr and Lincoln (2010), Kerr, Kerr and Lincoln (2015), Mayda et al. (2018), Mayda, Ghosh and Ortega (2016), and Peri, Shih and Sparber (2015) for studies on the US. See Beerli et al. (2018) for a study on Switzerland.

[9] In order to be granted a working visa, the foreign candidate needs to show that he possesses the diplomas and working experience corresponding to the occupation for which he's applying. Given that all occupations in the list require some level of technical specialization, this minimizes the scope for skill downgrading.

working conditions. Contrary to them, I do not find any evidence of a native flight effect in the context of high-skill migration.

For the estimation of the elasticity of substitution in production between migrants and natives, I follow a similar framework as in Card (2009), Manacorda, Manning and Wadsworth (2012), and Ottaviano and Peri (2012). While they estimate the parameter within cells of age and education, I compute it within cells of establishments and occupations and I take advantage of the reform to isolate exogenous shifts in the relative supply of these two worker types. Despite these differences, we obtain elasticities of similar magnitudes that corroborate the hypothesis of an imperfect degree of substitution in production. However, I avoid interpreting this result as purely driven by technology, given the presence of alternative mechanisms that could drive it (Dustmann, Frattini and Preston, 2012). I thus explore the role of differences in bargaining power, which is a mechanism that has been incorporated in search and matching models where natives and migrants have different reservation wages (Chassamboulli and Palivos, 2013; Nanos and Schluter, 2014; Moreno-Galbis and Tritah, 2016), but has been less studied in the context of natural experiments.

The remaining of the paper is organised as follows. Section 2 describes the reform, Section 3 presents the conceptual framework and lays-out its predictions, Section 4 discusses the data and the empirical strategy, Section 5 details the main results, Section 6 explores the channels behind the main findings, and Section 7 concludes.

2 The Reform

In France, the labour 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 authorisation that is only granted 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 "tight"

in the statistics collected by the French Employment Office. The tightness indicator measures the ratio between available vacancies in each occupation and the pool of unemployed workers possessing the required competencies for the job. High tightness thus signals that the occupation is hard to fill. Figure A1 in appendix reports a diagram taken from OECD (2017) that illustrates all the administrative steps that need to be undertaken by an employer before hiring a migrant worker. This procedure is lengthy and burdensome for firms. 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 2 months, it often takes much longer in practice.[10]

In January 2008, the French government introduced a legislative decree that facilitates the hiring of extra-Europeans within a list of 30 occupations characterised by a high level of tightness in the labour market. The new law states that, for the occupations concerned, the employer is no longer required to prove the prior search for a priority candidate but is automatically granted the authorisation to hire a migrant worker. To avoid abuses, the Service of Foreign Labour carefully checks that the qualifications and work experience of the candidate matches 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 labour. In a first step, the list is defined at the national level, and in a second phase, each region selects a subset of occupations that remains tight at the local level.[11] Table A1 in the appendix reports the full list of target occupations and details the number of 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), mechanical construction and metal processing (4 occupations); and the main professional categories involved are technicians, engineers and foremen.[12]

For the identification strategy, I 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

[10]This is especially the case when the institutional bodies involved in the final decision are in disagreement.
[11]Only five jobs apply to the entire French territory, while the others are only valid in certain areas.
[12]Figure A2 and A3 in appendix detail the exposure to the reform of different broad categories of occupations and sectors.

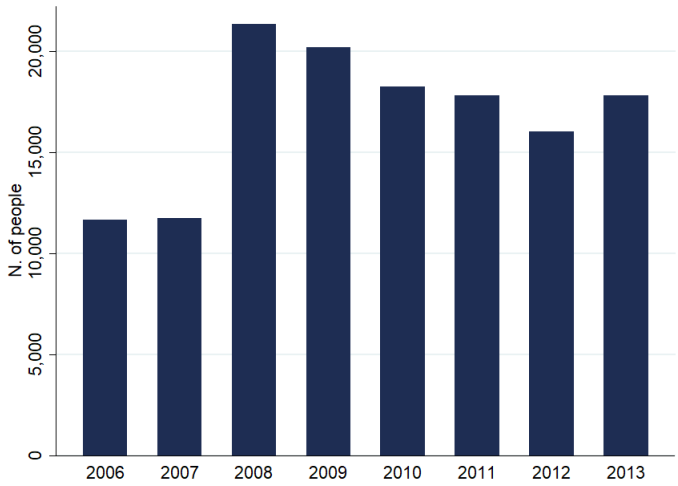
group in charge of the reform established at the same time an extended list of 150 occupations to be open without restrictions to European nationals coming from member states under transitory regimes.[13] The latter includes all 30 occupations open to non-EU citizens and 120 additional occupations. The definition of both lists is based on the same set of tightness criteria, such that the professions open only to the EU States under transitory regimes provide a natural control group for those 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.[14] Despite the availability of a precise description of the indicators used to define both lists, it was impossible to find a threshold in the tightness data determining the inclusion of an occupation in the reform. This is due to the fact that the final groupings were established 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, it increases the degree of comparability between the treated and control groups in a difference-in-differences setting.

Figure 1 provides the first descriptive evidence that the labour market did react to the policy change by attracting larger inflows of migrant workers. According to the aggregate statistics published by the Ministry of Interior, the number of economic visas delivered every year by the French government nearly doubles between 2007 and 2008, going from 12'000 to more than 20'000. In contrast, all other categories of immigration do not show any significant change in size around the same years, as shown in Appendix Figure A4. These patterns are consistent with what is expected from the reform, since the only migrants directly impacted are those that apply to economic visas in order to work for domestic firms.

[13] At 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 labour market restrictions of extra-Europeans during a probation period that lasted until 2014.

[14] To precisely measure tightness 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: Annual Flows of Economic Migrants



Source: National statistics on immigration published by MI-DGEF-DSED.

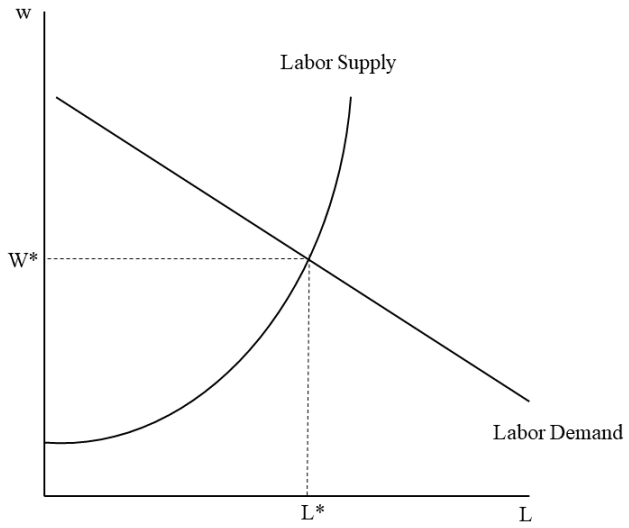
3 Conceptual Framework

The conceptual framework considers a standard model of occupational labour market equilibrium where employment and wages are jointly determined by the interaction between supply and demand. The starting point is a market characterised by a situation of tightness, which implies that a positive shock in demand has little effect on employment and mostly affects wages. The latter is represented by the fact that demand crosses the supply curve in a portion where it is close to being perfectly inelastic (Figure 2a). This corresponds to a context where most of the available labour with the required qualifications present in the country is already employed, while the high-skill content of these occupations makes it difficult to train additional workers in a short period of time (Saint-Paul and Cahuc, 2009). Descriptive evidence shows that the level of tightness in the occupations of the reform is positively correlated with their average wage, both in cross section (Appendix Figure A5) and across time (Appendix Table A2), suggesting that this could be an accurate representation of our setting. It is also similar to the migration model put forward by Borjas (1999), where is the total domestic labour supply in the country to be perfectly inelastic, under the assumption of no unemployment, no immigration, and no changes in demography.

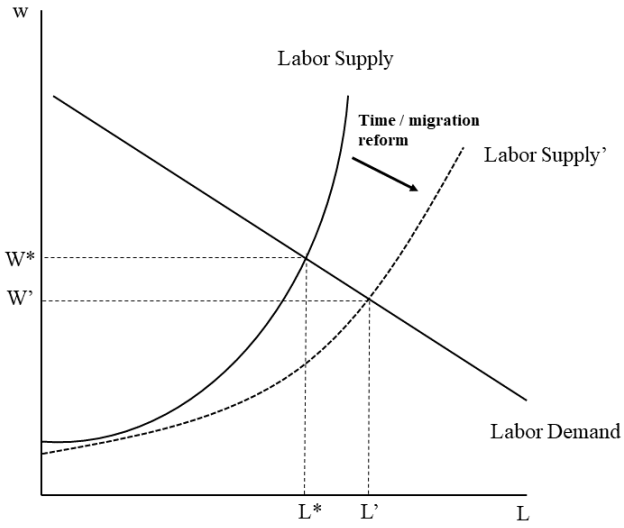
Figure 2b depicts the predicted effect of the reform. The latter is expected to increase supply

Figure 2: Conceptual Framework

(a) Setting before the reform



(b) Effect of the reform



elasticity at the top, which will, in turn, increase the equilibrium employment from L^* to $L^{'}$ and decrease the equilibrium wage from w^* to $w^{'}$. If we distinguish between native and migrant workers, the model predicts that the increase in employment is entirely driven by additional migrant hires in the short term, since native employment remains constrained. When it comes to the effect on wages, the negative pressure is expected to be equivalent in magnitude across both groups, since the model assumes homogeneity of labour across origins, thus perfect substitutability between them. This simple framework will serve as a benchmark to evaluate the empirical results.

4 Data and Empirical Strategy

The two administrative datasets used in the analysis come from firms' fiscal declarations. The first dataset combines the declaration of labour flows (*DMMO*) with the survey on labour flows (*EMMO*), and reports information on all employees' entries and exits that took place in a given establishment over the year. The data contains all private sector plants larger than 50 employees and a representative sample of the smaller ones. The second dataset is derived from the annual payroll tax data (*DADS*) and contains employment and wage information on a random sample

of 1/12th of all the private sector employees.[15] Both datasets allow to follow establishments over time but only the second allows to follow workers. For the rest of the paper the words "plant", "establishment" and "firm" are used interchangeably, always referring to the single physical location where workers perform their duties.

Both datasets report detailed occupation categories allowing to identify the jobs of the reform and a broad nationality indicator for each worker, as well as the regional location, the sector, and the number of employees at the establishment level.[16] The period considered in the regression analysis goes from 2004 to 2010, including 4 years preceding the reform and 3 years of post-reform effects. Nonetheless, in the event study graphs the post-trends are shown up to 2013, to give a sense of how the effect evolves in the medium term. The agriculture sector is excluded because it is not concerned by either one of the lists, and establishments with less than 20 employees are dropped from the DADS data because they were not required to report detailed occupation codes before 2008. Finally, both datasets are merged into a composite index of tightness that varies at the level of occupation-region-year and is constructed using the same data used to define the reform.[17] The final dataset is thus composed of an unbalanced panel at the level of occupations x plants.

The impact of the policy is recovered using a difference-in-differences identification. The treatment group includes the occupations that figure in the list of the reform at the national level, regardless of whether they apply to the region where the plant is located or not.[18] For the

[15]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.

[16]The DMMO-EMMO data allows to distinguish between French, European and non-European workers, while the DADS only distinguishes between French and non-French nationals. The occupational codes reported in the administrative data follow a different nomenclature with respect to the one reported in the legal text. I use an official crosswalk table provided by the French statistical office to translate the 30 job categories of the reform into 37 occupation codes appearing in the administrative data.

[17]This data is produced by the French Employment Office (*Pôle Emploi*). The 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.

[18]This can be considered as a measure of treatment exposure, given that not all occupations are "treated" in all regions.

common trend 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) are a subsample of the extended list applicable to EU states under transitory regimes (EU list), and that both are defined using the same criteria, the remaining occupations of the EU list are used as controls. Under the testable 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 list.[19] However, given that the EU list is much larger, it includes some jobs with much lower levels of tightness with respect to the treated jobs, which might introduce a bias in the results if the tightness differential is a source of asymmetric time-variant shocks. To reduce this risk, in the main specification I exclude control occupations that have a tightness index below average (computed over all occupations in the economy). After this correction, the average level of tightness is similar across treatment and control groups. In a robustness test, I check that the results continue to hold when the full EU list is included in the controls.

The second threat to identification resides in the possibility for migrants to change strategically the occupation for which they apply in order to enter the list, or for employers to change the job description at the margin to be able to benefit from the lighter regulation. This would mean that in the absence of the reform the hiring of migrants in the control group would have grown faster because they would not have been "diverted" towards treatment occupations. While this possibility cannot be completely ruled out, I limit the risk by excluding from the control group the occupations that are too similar in terms of skills required to some occupations in the treatment group, and that could be easily manipulated by employers or substituted by job seekers.[20] 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 limits the opportunity for tampering; and the descriptive evidence presented in Figure 3 further shows that the increase in the share

[19]Given that the DMMO-EMMO data allows to distinguish between European and non-European workers, I test whether there is any significant effect of the reform on EU hiring and find none.

[20]To define similarity, I consider occupations with the same first three digits out 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).

of migrants within treatment occupations does not seem to be accompanied by a decrease of it within controls.

Table A3 in Appendix shows the balancing test for outcomes in the treatment and control group before the reform. In the hiring outcomes there are some significant differences in levels but not in growth rates, while in the wage and employment stock outcomes there are no significant differences neither in levels nor in trends. Treated occupations are slightly tighter before the policy introduction (difference significant at the 10% level). These results support comparability in a difference-in-difference setting. Finally, the identification relies on the assumption that the financial crisis, which hit France around 2009, had a symmetric effect on treated and control occupations. In support of this hypothesis I show in a robustness test that controlling for sector-specific time trends leaves the majority of the results unchanged.

The main analysis is divided into two parts. First, it checks whether there is a first stage effect on the hiring of migrant workers both on the intensive and extensive margin. It further verifies that nothing changes for native hiring prospects and that equilibrium employment grows, as predicted by the conceptual framework. In a second step, it looks at how the wages of natives and migrants react to the growth in labour supply. The estimation is based on the following model:

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

where Y_{oirst} captures the outcomes of interest within occupation o, plant i, region r, sector s, and time t; the treatment identifier D_o is a dummy varying at the occupation level and T_t is a dummy equal to one for the years following the policy change. β_2 is the coefficient that directly measures the impact of the reform by estimating the differential trend observed in the treatment group during the post-implementation period ($D_o * T_t$). $\mathbf{X}_{oirt}$ is a matrix of controls including the logarithm of plant size and the average tightness in each occupation x region during the period preceding the reform, which allows to correct for any preexisting difference due to different levels

of tightness. Finally, the model includes the year, occupation, region, sector and firm fixed effects. The standard errors are clustered at the occupation x region level and the time period considered in the main regressions goes from 2004 to 2010. While the dataset would allow analysing the effect up to 2013, the further away from the year of reform implementation, the harder it gets to interpret the coefficients as the 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, the event study graphs show the outcomes over the full period available, to get a sense of how they evolved beyond 2010.

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, I 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 was further adopted by the migration literature (Alesina, Harnoss and Rapoport, 2016). In particular, for the estimation of the effect on the number of hires I use the Pseudo Poisson Maximum Likelihood estimator (PPML) proposed by Silva and Tenreyro (2006), and for the share of migrants in new hires I 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 a variant of PPML that can accommodate fractional data. Finally, for the probability of hiring natives and migrants, I apply a logit binary model. Since these estimators are very demanding on the data, the firm fixed effects are omitted from the first stage analysis. Nonetheless, in a robustness test, it is shown that a linear model including firm fixed effects gives results of similar magnitude with respect to the preferred specification. In the employment and wage regressions, I apply an OLS model where the outcomes are included in logarithmic form and firm fixed effects are included.

Table 1 presents the summary statistics of the main variables of interest across the two datasets,

Table 1: Summary Statistics

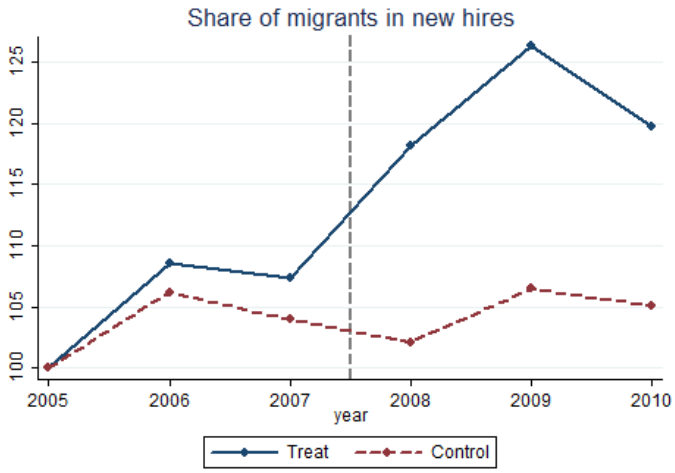
HIRING DATA	**Control** mean/(sd)	**Treatment** mean/(sd)	EMPL. & WAGE DATA	**Control** mean/(sd)	**Treatment** mean/(sd)
Share of migrants in hires	0.04 (0.17)	0.03 (0.15)	Salary natives	3384 (2295)	3513 (2050)
Native hires	1.61 (6.56)	2.00 (8.32)	Salary native new entrants	2889 (2170)	3112 (1915)
Migrant hires	0.10 (1.12)	0.11 (1.49)	Salary migrants	3168 (6149)	4101 (84620)
P. hiring a native	0.53 (0.50)	0.57 (0.50)	Salary migrant new entrants	2884 (10957)	3276 (2063)
P. hiring a migrant	0.05 (0.22)	0.04 (0.21)	N. of FT employees in occup.	5.1 (7.8)	5.6 (10.1)
P. net entry natives	0.33 (0.47)	0.36 (0.48)	Firm size	235 (658)	240 (652)
P. net entry migrants	0.04 (0.19)	0.03 (0.18)	Pre-reform average tightness	0.15 (0.11)	0.19 (0.20)
Firm size	274.2 (722.9)	266.9 (692.5)			
Pre-reform average tightness	0.15 (0.11)	0.19 (0.20)			
N. of observations	298'126	351'444	N. of observations	271'030	305'222
N. of occup. x establishments	160'746	186'165	N. of occup. x establishments	98'065	114'872
N. of establishments	61'870	68'192	N. of establishments	55'183	63'142

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 analysis.

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 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. The DADS data includes about 5 full-time employees per occupation within a plant, the gross monthly salary of natives is on average 3500 Euro in treatment occupations and 3400 Euro in controls. Migrants are paid slightly better than natives in treatment occupations and slightly worse than natives in control occupations on average, while the levels are comparable among new entrants. All the monetary values are expressed in terms of full-time equivalents, to avoid differences explained by hours worked, and are adjusted for inflation with 2010 as the reference year. The order of magnitude of wages confirm that the jobs in question are relatively high skilled, and so are the migrants that are hired to do them.

Figure 3 shows the unconditional growth index for the share of migrant entries in both treatment and controls. The common trend seems to hold before the reform, even without conditioning neither for the level of pre-reform tightness nor for the fixed effects. After the reform, there is

Figure 3: Unconditional Growth Index in Hiring Composition



The blue line shows the unconditional growth index in the share of migrants within new hires observed within treatment occupations. The red line does the same for control occupations. The index is set to 100 at the beginning of the period.

a clear increase in the share of migrants hired within treatment occupations while it remains stable within control occupations. The latter comforts the assumption of no strategic shifting in migrants' applications away from controls towards treatment jobs and signals that employers did react to the policy change by hiring more migrants.

5 Main Results

5.1 First stage: employment

Table 2 presents the main results obtained on employment outcomes. Column (1) displays the impact on the stock of employment in reform occupations, column (2) shows the share of migrants in hires, columns (3) and (4) look at the effect on migrant hires both on the extensive and intensive margin, and columns (5) and (6) do the same for natives. The coefficient of column (1) is a semi-elasticity while all the other coefficients have to be interpreted in terms of incidence ratios. The full set of controls is included in every regression. Results show that the reform increases the equilibrium employment in target occupations by about 1.4%. The latter is explained by a

15% growth in the probability of net positive hiring of migrants and a 50% growth in the number of migrants hired. When looking at natives, both the probability of net entry and the number of hires is left unchanged by the rise in foreign inflows. As a result, the share of hires made of migrants in these jobs grows by 16%. The fact that native employment do not suffer from the reform is not surprising, since the occupations concerned are chosen because of their hiring difficulties, meaning that firms were previously employing fewer people than they would have liked in these professions. These findings are in line with the predictions of the model and suggest that the reform was successful in increasing firms' access to scarce competencies.

Table 2: Main Results on Employment

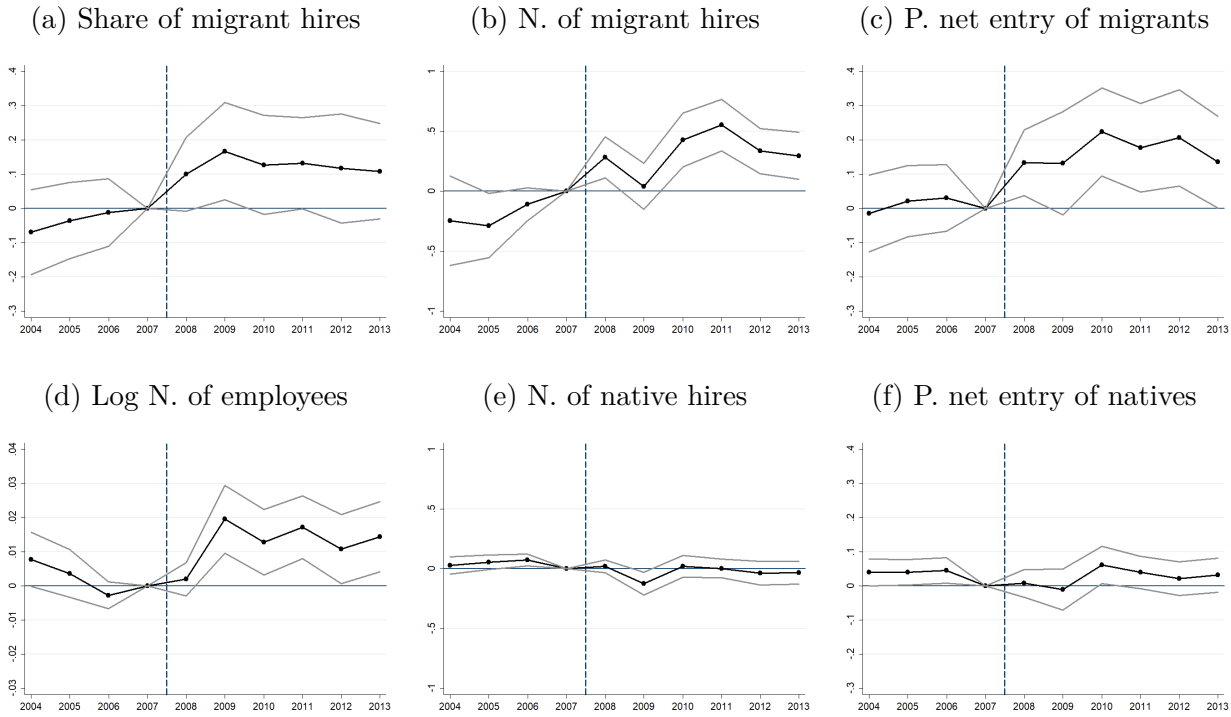
	(1)	(2)	(3)	(4)	(5)	(6)
	All workers		Migrants		Natives	
	Log N. of FT employees	Share of migrants in new hires	P. net positive hire	N. entries	P. net positive hire	N. entries
VARIABLES	OLS	Fract. Logit	Logit	PPML	Logit	PPML
Treat x Post Reform	0.0137***	1.160***	1.155***	1.502***	0.985	0.945
	(0.00479)	(0.0470)	(0.0502)	(0.202)	(0.0165)	(0.0327)
All controls	✓	✓	✓	✓	✓	✓
Observations	527,546	334,091	587,504	587,568	587,609	587,609
R-squared	0.451	-	0.104	0.073	0.0131	0.080

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as controls for pre-reform tightness in occupation x region and pre-reform firm size. Column (1) further includes firm fixed effects. Coefficients reported in exponentiated form except for column (1) where the coefficient can be interpreted as a semi-elasticity. Period of analysis: 2004-2010. The fractional logit model is a GLM estimator of the binomial family that a logit link function. It was proposed by Papke & Wooldridge (1996) to accomodate zero-inflated fractional outcomes. The pseudo-poisson maximum likelihood estimator was proposed by Silva & Tenreyero (2006) to accomodate zero-inflated continuous outcomes.

Figure 4 shows the event study graphs for the employment outcomes. It plots the coefficients and the 95% 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 the entire period available is included in the data to give a sense of how the trends continued to evolve after 2010, which is the last year included in the regression analysis. Treated and control groups follow similar trends before 2008 across all dimensions. After the introduction of the reform, treatment occupations hire significantly more migrants than the control ones, while there continue to be no difference in native hires. All the significant effects seem to be long lasting,

even if they decrease in magnitude towards the end of the period. Finally, as expected, flow outcomes react immediately after the policy introduction, while the stock of employment takes one additional year to react because it is more subject to inertia.

Figure 4: Event Study Graphs - First Stage



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

Table A4 and Figure A6 in Appendix report the regression results and event study graphs for additional employment outcomes. Similarly to the effect on net positive entry, there is a positive impact of the reform on the simple probability of hiring migrant workers and no change for the probability of hiring natives. The share of new hires made through short term contracts - a measure of job quality - does not change as a result of the reform, neither for natives nor for migrants. Finally I can look at what happens to European nationals, which are partially treated in both groups, since Romanians and Bulgarians gain access to both lists of occupations. I find no differential effect of the reform on the extensive margin, while there is a slight increase in the number of EU hires in treated occupations after the reform, which is however only visible during the year 2010 and then returns to zero. I can conclude that the observed growth in equilibrium

employment for the jobs targeted by the policy is almost entirely explained by additional extra-European hires, while native and EU employment prospects remain widely unchanged.

5.2 Second stage: wages

Table 3 presents the results for the effect of the reform on wages. All outcomes are inserted in a logarithmic form so that the coefficients can be interpreted as semi-elasticities. Columns (1) to (3) present the results on migrant wages, distinguishing between the overall effect, the effect on wages of newly hired workers, and the effect for workers that had been working for the same firm for more than a year. Columns (4) to (6) show the same outcomes for native workers.

Migrants' salaries decrease by 3.6% overall, which is driven by a 14.6% drop in the wage of new entrants and a 2% drop in the wage of incumbents. Native workers, on the other hand, do not experience a significant drop in average wages, while the wage of new recruits falls by 7.5%. Incumbent native workers do not seem to suffer at all from the additional competition.

Table 3: Main Results on Wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Migrants			Natives		
	Log salary	Log salary new entrants	Log salary incumbents	Log salary	Log salary new entrants	Log salary incumbents
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	-0.0360***	-0.146***	-0.0200***	-0.00490	-0.0749***	-0.000471
	(0.00727)	(0.0303)	(0.00693)	(0.00346)	(0.0168)	(0.00287)
All controls	✓	✓	✓	✓	✓	✓
Observations	65,521	16,934	53,901	483,124	102,055	419,754
R-squared	0.764	0.793	0.798	0.696	0.744	0.719

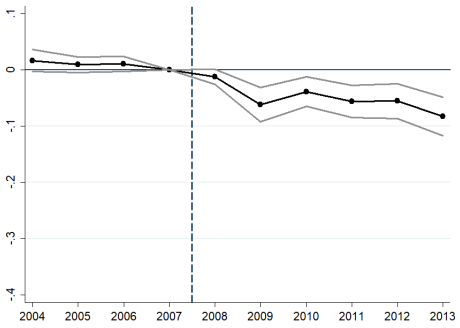
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include firm, occupation, region, sector and year fixed effects as well as controls for pre-reform tightness in occupation x region. Period of analysis: 2004-2010. Coefficients can be interpreted as semi-elasticities.

The finding that salary decreases in response to a positive shock in labour supply is in line with the predictions of the model. Table A5 in Appendix estimates the supply elasticity of wages in these occupations using a 2SLS model where the log of full-time employment is instrumented

by the interaction between the treatment group and the post-reform period. Results show that a 1% increase in employment decreases average wages by 0.7% and wages of new hires by 1.9%. These elasticities are large in magnitude, which is in line with a starting point characterised by a labour supply that is close to perfectly inelastic. Nonetheless, what is striking about these results is the fact that the impact on migrant wages is twice as important as the one detected for natives. This goes against the standard assumption of homogeneity between these two labour inputs when they are employed in the same occupations. In the next section, I explore two channels that can explain this differential effect.

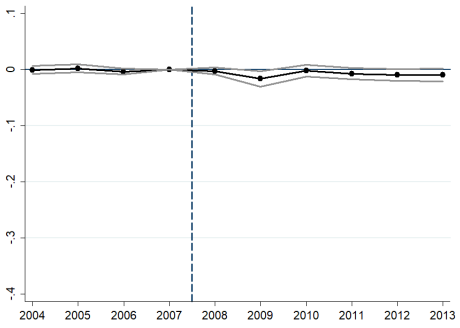
Figure 5: Event Study Graphs - Second Stage

(a) Log salary of migrants



(b) Log salary of migrant new hires

(c) Log salary of natives



(d) Log salary of native new hires

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

Figure 5 provides the validation test for the common trend assumption on the wage outcomes and shows how the effect evolved in the years following the reform. As in the employment outcomes, the difference between treated and controls was widely not significant before 2008, and

it becomes significant afterwards. The negative impact on hiring wages appears to be relatively short-lived, both for natives and migrants, but the effect on average wages is long lasting for migrants.

5.3 Robustness tests

Tables A6 to A10 in Appendix perform two different placebo tests and introduce two alternative control groups on employment outcomes (Tables A6 to A8), and on wage outcomes (Tables A9 and A10). The first placebo test checks the effect detected for a placebo-reform set in the years preceding the actual policy change. In this analysis 2007 is transformed to be a post-reform year and the model is estimated on the period going from 2004 to 2007. A negative and significant coefficient is detected on native hires and on the probability of hiring natives, which was already visible from Figure 4, and there is a slight positive effect significant at the 10% level on the number of migrant entries. The other flow outcomes show no change and none of the wage outcomes is significant, while the magnitude of all coefficients is close to zero. The same is true for the stock of employment in these jobs. The second placebo test only considers the observations in the control group and creates a placebo treatment indicator taking the value of one for the 50% of the observations with the highest pre-reform levels of tightness. This is done to ensure that the main results are not driven by differences in preexisting skill-shortages rather than by the reform itself. Here again the majority of the coefficients are insignificant and close to zero in magnitude. The share of migrant hires and the probability of hiring a migrant are negative and significant at the 5% and 10% level respectively, while native salaries are positive and significant at the 10% level but very small in magnitude (less than 1%). Given that the sign of these coefficients is the opposite of what was obtained in the main results, it can be argued that if anything the latter is a lower bound of the true effect. The third robustness test establishes an alternative control group including occupations in the top two quintiles of the tightness distribution but not included in the treatment list, regardless of whether they appear in the extended list of Romanians and Bulgarians or not. The results obtained are similar both in magnitude and significance to those reported in

the main analysis. Finally, in the last column, the full list of occupations open to Romanians and Bulgarians is included in the controls, without excluding those with low pre-reform tightness. Once again, results are qualitatively unchanged with respect to the main analysis.

The second set of robustness tests evaluates the sensitivity of the results to the inclusion of different levels of fixed effects (Tables A11 to A15). Here the estimation relies on an OLS model even for the flow outcomes, to ensure computational feasibility. To ease the comparison with the main first stage results, at the bottom of each table the treatment effect is translated in terms of growth rate relative to the pre-reform period. In addition to sector and year fixed effects, which are included all along, the first column includes only occupation and region fixed effects; the second column adds firm fixed effects; the third column moves to a full panel fixed-effect model by including occupation x firm fixed effects; and the fourth column includes occupation, region, firm and sector x post-reform period fixed effects. The sign and significance remain the same across specifications for most outcomes, the magnitude of the coefficients on employment outcomes gets smaller when sector x year fixed effects are included, while it becomes slightly larger for immigrant wages. The fact that results remain qualitatively the same even after controlling for sector-specific shocks supports the hypothesis that they are not driven by the financial crisis, which could have hit differently sectors more intensive in treated versus control occupations.

Finally, Table A16 to Table A18 look at the heterogeneity of the main effects across some meaningful sub-samples. Panel A in all tables splits the sample between the 7 regions that experience more than a 50% growth in economic visas between 2007 and 2008 and the rest of the country, based on the aggregate figures published by the Ministry of Interior.[21] 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 migrants. In these areas, the share of migrant hires grows by 20%, the number of migrant hires increases by 64%, and the stock of employment in targeted jobs grows by 2.4%, while the number of native recruitments remains unchanged. The negative effect on wages is also

[21]The 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.

more important in these areas, where the average native wage incurs a statistically significant loss of 1.5% and the average migrant wage drops by more than 4%. These results indicate that the estimated impact of the reform using plant level administrative data is consistent with what is observed in the macro-level statistics. The sample can further be split between the occupations characterised by very high levels of tightness before the reform versus occupations where the tightness is relatively lower (Panel B). To preserve the balance between treatment and controls, both groups are divided in halves according to the within-group distribution of the tightness index. In line with the conceptual framework, the occupations that react the most to the reform are those that were previously experiencing the most severe shortages of native labour. Consequently, they are also those experiencing the largest drop in migrant and native wages (-3.8% and -0.8% respectively). Finally, Panel C splits the sample according to the average pre-reform plant size. The overall increase in employment appears to be slightly bigger within large firms and, perhaps more interestingly, these establishments see a 12% decrease in the number of native entries, indicating that there is some employment substitution within this sample. The drop in wages of both natives and migrants is more important within large plants.

6 Channels Explaining the Differential Wage Effect

6.1 The elasticity of substitution between migrants and natives

The canonical model of the labour 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 labour 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 attempted 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, here the elasticity of substitution between migrants and natives is computed within cells of occupations and establishments. By doing so I take advantage of the richness of the administrative data to estimate production technology at the level where production decisions actually take place: the plant. In addition, the degree of substitution between labour inputs within occupations is a mechanism rarely explored by the literature. This level of analysis allows for ruling out the channel of skill-downgrading, which is the mechanism most commonly used to explain why natives and migrants with the same level of education do not directly compete with each other.

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

$$Y_t = A_t \Big[\sum_o \theta_{ot} \mathbf{L}_{ot}^{\frac{\sigma_o - 1}{\sigma_o}} \Big]^{\frac{\sigma_o}{\sigma_o - 1}} \tag{2}$$

where σ_o is the elasticity of substitution between occupations and θ_{ot} is the occupation-specific efficiency parameter that might vary through time (e.g., because of technological change and globalisation). Without loss of generality, θ_{ot} is normalized such that $\sum_o \theta_{ot} = 1$. For simplicity, capital is assumed to be fixed and it is not reported. Further, the labour 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:

$$\mathbf{L}_{ot} = \Big[\beta_{ot} M_{ot}^{\frac{\sigma_E - 1}{\sigma_E}} + N_{ot}^{\frac{\sigma_E - 1}{\sigma_E}} \Big]^{\frac{\sigma_E}{\sigma_E - 1}} \tag{3}$$

where σ_E is the elasticity of substation 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, for instance, there is a change in the relative quality of migrants or in discrimination biases. By assuming that the marginal product of each labour input is equal to its marginal cost, the

log-linearised wage equations of migrant workers can be derived as follows:

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

and of native workers analogously:

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

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

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

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. The canonical model assumes that migrants and natives working in the same jobs are perfect substitutes in production ($\sigma_E = \infty$), such that changes in their relative supply have no impact on their relative wages.

To estimate equation (6) econometrically, I have to introduce a restriction by forcing β_{ot} to vary additively by occupation and time, as done by Manacorda, Manning and Wadsworth (2012): $\beta_{ot} = f_o + f_t$. In addition, the richness of the administrative data allows to go one step further and look at the production process within each firm, instead of approximating the behaviour of a representative firm. To do that, establishment fixed effects are added to the model resulting in the following estimating equation:

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

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.[22] 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 the 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, I compare the OLS results with an IV strategy using the reform as an exogenous instrument for $L_{iot}^{M,N}$ in the first stage, as follows:

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

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

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 between migrants and natives in these set of technical competencies.

Column (1) of Table 4 reports the results obtained with the OLS estimation, column (2) reports those from the IV estimation, and column (3) shows the first stage results. The OLS estimation shows that a 1% increase in the relative supply of migrant workers decreases relative wages by 0.01%, which translates into an elasticity of substitution parameter of about 73 (arguably close to the perfect degree of substitution). The IV estimation results, however, show an elasticity of substitution of about 8, which is almost ten times smaller. This suggests that OLS models might lead to the over-estimation of the degree of substitution between migrants and natives, which

[22]For this exercise the two main datasets are merged together. To increase the sample size of establishments x occupations for which both migrants and natives are observed, here I rely on the complete DADS dataset which includes the universe of private sector employees, instead of the 1/12th sample used in the main analysis.

Table 4: Elasticity of substitution between migrants and natives

	(1)	(2)	(3)
	Log ratio migrant to native wages		log ratio migrant to native hires
VARIABLES	OLS	IV	FS
log ratio migrant to native hires	-0.0138***	-0.130*	
	(0.00374)	(0.0677)	
Treat * Post reform (instrument)			0.209***
			(0.0441)
Observations	8,827	8,827	8,827
R-squared	0.472	0.383	0.656
K-P rk Wald F-stat		22.38	
Estimated degree of substitution	**72,5**	**7,7**	
Year FE	✓	✓	✓
Occupation FE	✓	✓	✓
Plant FE	✓	✓	✓
Tightness and plant size controls	✓	✓	✓

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: 2004-2010. Standard errors are clustered at the firm level. The sample corresponds to the match between the employment flow dataset (DMMO-EMMO) and the wage dataset (DADS) at the level of occupation x plant. The IV estimation uses the interaction between treatment and post-reform period as the instrument for the ratio of migrant to native hires. Column (1) presents the OLS results, column (2) present the results obtained from the 2SLS procedure and column (3) reports the first stage.

appear to be imperfect substitutes in production, even when they are employed by the same firm and in the same occupation. Such an imperfect degree of substitutability may be explained by the fact that they can differ in their level of seniority, management responsibilities, precise task specialisation, or other dimensions that are too fine to be captured by the occupational classification. The first stage shows that the reform significantly increases the relative supply of migrants to natives and that the instrument is strong according to conventional thresholds. Interestingly, despite the fact that the elasticity of substitution of migrants to natives is estimated within occupations instead of education and age groups, and that the sample is restricted to the jobs concerned by the reform and their controls, the obtained substitution parameter is similar to that found by others in the literature: Manacorda, Manning and Wadsworth (2012) obtain 7.8 on the whole sample of workers in the UK, and Ottaviano and Peri (2012) find an elasticity of substitution of 11 among workers with high-school degrees, and of 14 among college graduates in the US. Finally, Table A19 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.

6.2 Complementarity in production versus bargaining power

According to the findings from the elasticity of substitution analysis, it appears that migrants and natives are imperfect substitutes in production, even when they are employed in the same types of jobs and by the same firm. Nonetheless, as underlined by Dustmann, Frattini and Preston (2012), this method yields the true technological parameter only under the assumption that workers can be properly assigned to skill groups based on observable characteristics (no skill downgrading) and that there is no other mechanism that can drive the results. Regarding the first point, the setting of this paper is much less subject to skill downgrading with respect to the analyses measuring the effect of large immigrant waves, since the occupations where migrants will be employed are defined ex-ante and, given the technicality of these jobs and the visa attribution procedure, it is ensured that they possess the relevant education background to fill these positions. A caveat of using occupations instead of education and age groups, however, is that they are not time-invariant so that natives might switch to other activities in response to the increase in competition. The reduced form results show that there is no negative effect on native employment in these jobs, which is inconsistent with a sizable native flight. The last section of this paper further tests the absence of composition effects by estimating the impact of the reform on natives' probability of changing employer or occupation, based on the individual panel dimension of the data.

Nevertheless, if the assumption of perfect competition in the product market is relaxed, there is a second mechanism that might explain the differential effect on wages: differences in bargaining power. One of the conditions attached to the economic visas in France is that foreign workers have to stay with the same employer and keep working in the same occupation. If a migrant wishes to change firm, he has to ask the new employer to support him in a new visa application. A similar procedure is required if he wants to change occupation, and if the new profession is not included in the list of the reform, he would have to comply to the stricter rules that apply to the general labour market. These elements restrict considerably the job to job mobility of migrants,

which gives employers greater bargaining power over how to split rents between profits and wages. Both a model with perfect competition in the product market and imperfect substitutability in production between migrants and natives, and a model with imperfect competition in the product market generating rents and different levels of bargaining power between migrants and natives yield the same predictions on the differential wage effect.

To get a sense of which one of these channels is driving the results, I split the sample in three different ways, two of which distinguish between contexts with more or fewer differences in bargaining power between migrants and natives, while the third distinguishes between occupations with more or less scope for complementarity in production. The first split is done according to the level of occupation mobility existing within a given job classification. Formally:

$$Omob_o = \frac{1}{4} \sum_{t=2004}^{2007} \frac{switchers_{ot}}{Empl_{ot}} \tag{10}$$

where $switchers_{ot}$ identify the number of workers with initial occupation o that switch to another occupation in a given year t, regardless of whether they also switch employer or not, while $Empl_{ot}$ represents the total employment in occupation o and year t. The rate of occupation mobility is then averaged over the period from 2004 to 2007. This measure captures the importance of native workers' outside options based on the easiness to reconvert to other professions. In a context where $Omob_o$ is low, migrants and natives have similar levels of bargaining power and therefore are expected to encounter a more similar effect on wages, while the opposite is true for high levels of $Omob_o$.

The second measure of bargaining power captures the extent of employers' monopsony power present in a given occupation. The latter is measured by the concentration of employment within firms as captured by the following Herfindahl Index:

$$Monops_o = \sum_{i=1}^{N} \Big(\frac{Empl_{oi}}{Empl_o} \Big)^2 \tag{11}$$

where $Empl_{oi}$ identifies employment in occupation o and firm i while $Empl_o$ is the total employment in occupation o. The indicator is computed on data for the years 2004 to 2007. The values of $Monops_o$ range between 0 and 1, where 0 corresponds to the case where there are a large number of firms, each concentrating a small proportion of the employment in a given occupation, and 1 corresponds to the case where one single firm concentrates all the employment of the occupation. In a context with high monopsony power, natives and migrants are expected to have similar levels of (low) bargaining power, and thus might have a more similar wage reaction with respect to contexts with less monopsony power on the firm side.

Finally, to capture the scope for an imperfect degree of substitution in production, the sample is split according to the level of task diversity comprised within a given occupation. Occupations that entail a wider variety of tasks allow, in principle, for greater differential specialisation of natives and migrants. For instance, we know that natives have a comparative advantage in tasks involving communication and thus might specialise in these activities within a given profession (Peri and Sparber, 2009). To calculate the extent of task specialisation of different occupations, I use data from the 2001 Dictionary of Occupation Titles (DOT), which lists 42 different activities and scores their importance for each detailed occupation category. Examples of activities include "Monitor Processes, Material, Surroundings", "Selling or Influencing Others", and "Developing Objectives and Strategies". These are general enough so that with only 42 tasks it is possible to describe all occupations in the labour force. This dataset is constructed based on characteristics of jobs in the US, so I have to assume that the same professions in France have similar features, at least in relative terms. The measure of task diversity is based on the following Herfindahl index:

$$TaskDiv_o = 1 - \sum_{k=1}^{N} \omega_{ko}^2 \tag{12}$$

where ω_{ko} is the weight of task k in occupation o. Given that the HHI is a measure of concentration, 1-HHI is measure of diversity.

Table A20 in the Appendix reports the correlation between these three measures. As ex-

pected, occupational mobility is negatively correlated with monopsony power, but the correlation coefficient is relatively low (-0.13), signalling that the two measures capture different aspects of native bargaining power. Furthermore, occupations more diverse in their tasks allow for greater occupational mobility and are less subject to monopsony power. Here as well, both correlation coefficients are below 0.3. Each of these three measures is used to split the sample at the median of their value. To preserve the balance between treatment and control observations, the split is done separately according to the median within each group. Table A21 reports the heterogeneity of the main results on wages across these groups, while Table 5 shows the heterogeneity of the elasticity of substitution analysis. In the latter, the instrument is sufficiently strong only across one of the two sub-samples, it is thus compared with column (1), which reports the coefficient for the full sample already reported in Table 4.

Table 5: Heterogeneity in the elasticity of substitution between migrants and natives

	(1)	(2)	(3)	(4)
	Log ratio migrant to native wages			
	Main	High monops. power	Low occup. mobility	High task diversity
VARIABLES	IV	IV	IV	IV
log ratio migrant to native hires	-0.130*	-0.116**	-0.118*	-0.196*
	(0.0677)	(0.0579)	(0.0610)	(0.103)
Observations	8,827	4,869	5,935	5,282
R-squared	0.383	0.495	0.433	0.215
K-P rk Wald F-stat	22.38	31.08	14.57	8.883
Estimated degree of substitution	**7.7**	**8.6**	**8.5**	**5.1**
Year FE	✓	✓	✓	✓
Occupation FE	✓	✓	✓	✓
Plant FE	✓	✓	✓	✓
Tightness and plant size controls	✓	✓	✓	✓

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: 2004-2010. The sample corresponds to the match between the employment flow dataset (DMMO-EMMO) and the wage dataset (DADS) at the level of occupation x plant. The IV estimation uses the interaction between treatment and post-reform period as the instrument for the ratio of migrant to native hires. Monopsony power is measured as the concentration (Herfindhal Index) of employment in a given occupation across firms. High monopsony power selects the 50% of the sample where employment is the most concentrated within few firms. Occupation mobility is measured as the probability of switching occupation for each occupation. Finally, task diversity is computed as the inverse of the concentration (Herfindhal Index) of different activities within each occupation. It is computed using DOT data, and high task diversity selects the 50% of the sample with the lowest concentration of activities.

Table A21 shows that the salary of migrants decreases by a similar extent in jobs with low and high monopsony power, while the coefficient on natives, even if never significant, appears to be more negative in magnitude in the sub-sample with high monopsony power. The negative effect on the salary of migrants is only present in contexts with low occupation mobility. Finally, and perhaps most interestingly, the negative pressure on migrant wages is similar across task diversity while it becomes negative and significant for natives in occupations with low task diversity. In this sub-sample, migrants see a decrease in their average wage of 3.7% and natives of 1.11%. This is in line with the channel of imperfect substitutability in production. Moving to the elasticity of substitution results, coefficients are quite similar across subgroups, but I obtain a higher degree of substitution between migrants and natives in contexts with high monopsony power and low occupation mobility, consistent with the hypothesis that when both natives and migrants have similarly low bargaining power, they suffer a more similar hit on wages. On the other hand, occupations with higher task diversity yield a lower degree of substitution, consistent with the hypothesis that there is more scope for complementarity in production in these jobs.

From this exercise, I conclude that both imperfect degree of substitution in production and differential bargaining power appear to play a role in explaining the fact that migrants' wages suffer more than natives' wages. The observation that there can be differential task specialisation of migrants and natives even when they are employed in the same jobs and by the same employers underlines that the extent of complementarity in production is even stronger than previously shown in the literature, at least within high-skill occupations with a technical content.

6.3 Composition effects

This subsection checks whether individuals directly exposed to the increase in competition generated by the reform are more likely to switch employer or occupation in response to it, which would imply that there is a change in the composition of natives that remain employed in the jobs of the list. Secondly, it measures the overall effect on their salary, regardless of whether they switch jobs or enter non-employment. To do that, I take advantage of the worker panel dimension

included in the 1/12th sample of the DADS to estimate the following model:

$$Y_{jt} = \beta_0 + \beta_1 D_o * T_t + \nu_j + \epsilon_{it} \tag{13}$$

where j is the subscript for individuals, β_1 captures the effect of the reform and ν_j represents individual fixed effects. The equation is estimated on two subsamples, implying different definitions of the treatment group D_o. First, it considers individuals that were employed in treated or control occupations in 2007 and looks at what happens to them in the years following the reform. In this case, the period of analysis goes from 2007 to 2010. Second, and to check the robustness of the effects, it does the same thing for individuals employed in treated or control occupations in 2005. In this case, the period of analysis goes from 2005 to 2010. The salary regressions are run on a balanced panel that assigns a wage of zero to workers that disappear from the sample.

Table 6: Individual panel results

	(1)	(2)	(3)	(4)	(5)	(6)
		Migrants			Natives	
VARIABLES	Log salary	P. changing firm	P. changing occup.	Log salary	P. changing firm	P. changing occup.
Panel A: Individuals employed in treat / control occup. in 2007						
treat*post2008	-0.0414	0.0152*	0.00981*	-0.00259	0.00342	-0.00115
	(0.0488)	(0.00796)	(0.00592)	(0.0340)	(0.00367)	(0.00327)
Observations	250,276	73,191	73,191	1,587,000	613,024	613,024
R-squared	0.371	0.345	0.343	0.402	0.322	0.315
Panel B: Individuals employed in treat / control occup. in 2005						
treat*post2008	-0.0858***	0.0108**	0.00947***	0.0205	0.00366	0.000990
	(0.0320)	(0.00469)	(0.00353)	(0.0289)	(0.00275)	(0.00182)
Observations	281,148	82,808	82,808	2,145,474	780,220	780,220
R-squared	0.388	0.275	0.280	0.394	0.255	0.250

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Treatment is assigned based on occupations that individuals held in 2007 (panel A) or 2005 (panel B). Standard errors are clustered at the occupation x region level. Fixed effects for individuals' initial occupation, region are included together with year fixed effects. Period of analysis: 2007-2010 (Panel A), 2005-2010 (Panel B). Columns (1) and (4) are estimated on a balanced panel sample where salary takes the value of zero if the individual disappears from the sample.

Results are reported in Table 6. In terms of the probability of switching employer or occupa-

tion, there is strictly no change for natives, which rules out the presence of any composition effect of the reform driven by native flight. For migrants, there is a slight increase of one percentage point in the probability of changing plant, which might be explained by the fact that migrants previously present in the country can now move to another employer more easily thanks to the simplified procedure introduced by the reform. Consistent with the main analysis, incumbent native workers do not see a drop in their average wages following the arrival of additional migrants. As shown in the reduced form results, only natives that are newly hired after the reform see a reduction in their salary. On the other hand, incumbent migrants do see a decrease in their average salary, especially in the sample considering individuals in treated occupations in 2005.

7 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 labour 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 information contained in the administrative data makes it possible to measure the effect of the reform on hiring patterns, employment stocks, and wages, and to distinguish between the outcomes for natives and foreign workers. In a second step, the paper exploits the exogenous source of variation in the relative supply of migrants with respect to natives to estimate their elasticity of substitution in production. Finally, the paper takes advantage of the individual panel dimension of the data to evaluate the overall impact of the additional competition on the earnings of incumbent workers and to test whether affected individuals have a higher probability of changing firm or occupation, which would change the composition of employment in target jobs.

The results from the main empirical analysis show that the hiring of non-European workers increases within targeted occupations, while native employment flows remain largely unaffected.

The reform was thus able to alleviate the problem of skill-shortages, at least in the short run. It further detects a negative pressure on entry wages that is twice as large on foreign salaries that on natives salaries, signalling 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 change. The analysis on the elasticity of substitution reveals that, in this particular set of occupations, natives and migrants cannot be considered as homogeneous inputs in production, even when they are employed by the same firm to do the same job. Nonetheless, this parameter reflects both the existence of an imperfect degree of substitution due to task specialisation and differences in bargaining power. Finally, native workers that were employed in reform occupations before the policy do not see any change in their probability of switching employer or profession, suggesting that the main results are not driven by compositional changes. Furthermore, their overall salary does not suffer any drop, consistent with the finding that only native workers hired after the implementation of the reform suffer wage losses.

From a policy perspective, favouring high-skill immigration seems to be an effective tool to counter domestic skill shortages, at least in the short run, and the cost paid by native workers appears to be limited. Further analysis should be undertaken to test whether, in the long run, these kinds of policies generate any disincentives for the adaptation of the domestic education system, since they help to provide the needed competencies from abroad. An important criticism of this policy resides in the fact that the list remained unchanged up to this date, while the needs of the labour market have significantly evolved over the past 10 years. The periodic adaptation of the list, as it is done in the UK, for instance, might help maximise the support given to domestic firms and minimise the cost for domestic labour.

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Appendix

A Tables

Table A1: List of 30 Occupations part of the Reform

Occupation	Professional category	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 elechtronics	Executive/Engineer	22
Industrial draughtsman in electricity and elechtronics	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 elechtronics	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 elechtronic 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: Correlation between change in tightness and change in wages

	(1)	(2)	(3)	(4)
	Reform Occupations (treat & control)		Other occupations	
	Monthly Salary	Log monthly salary	Monthly Salary	Log monthly salary
VARIABLES	Panel FE	Panel FE	Panel FE	Panel FE
Tightness index	37.89**	0.00985*	2.648	0.000537
	(16.89)	(0.00507)	(29.36)	(0.00590)
Observations	7,105	7,105	21,341	21,341
R-squared	0.029	0.034	0.006	0.016
Number of id	1,792	1,792	5,681	5,681

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Panel analysis of occupations x regions. Standard errors clustered at the occupation x region level. Period: 2004-2007. The tightness index is computed as a standardized score.

Table A3: Balancing test treatment and control groups

	Treat	**Control**	**Difference**
Panel A: hiring variables			
Share of migrants in hires	0.03	0.05	-0.02***
Native hires	1.94	2.05	-0.11
Migrant hires	0.08	0.13	-0.05*
P. hiring a native	0.58	0.57	0.01
P. hiring a migrant	0.04	0.06	-0.02**
P. net entry natives	0.37	0.34	0.03
P. net entry migrants	0.03	0.04	-0.01*
Growth share of migrants in hires	0.08	0.03	0.05
Growth native hires	0.06	0.07	-0.01
Growth migrant hires	0.21	0.11	0.1
Growth p. hiring a native	0.03	0.02	0.01
Growth p. hiring a migrant	0.17	0.07	0.1
Growth p. net entry natives	0.03	0.02	0.01
Growth p. net entry migrants	0.14	0.11	0.03
Firm size	252.9	256.6	-3.7
Pre-reform tightness	0.18	0.12	0.06*
N. occupations x regions	37	75	
Panel B: wage and employment variables			
Monthly salary natives	3235	2973	262
Monthly salary native new hires	2913	2707	206
Monthly salary migrants	4603	3218	1385
Monthly salary migrant new hires	2981	3260	-279
N. full time employees	5.5	5.3	0.2
Growth monthly salary natives	0.01	0.00	0.01
Growth monthly salary native new hires	-0.01	-0.01	0.00
Growth monthly salary migrants	0.00	0.00	0.00
Growth monthly salary migrant new hires	0.00	-0.03	0.03
Growth full time employees	0.01	0.01	0.00
Firm size	239.5	236.3	3.2
Pre-reform tightness	0.2	0.1	0.1*
N. occupations x regions	37	76	

Panel A reports outcomes from the DMMO-EMMO dataset while Panel B reports outcomes from the DADS dataset. Average computed within occupations for the period going from 2004 to 2007 (pre-reform). Salary outcomes are expressed in 2010 euros.

Table A4: Additional Flow Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Migrants		Natives		European Migrants		
	P. new hire	Sh. STC in new hires	P. new hire	Sh. STC in new hires	P. new hire	P. net positive hire	N. entries
VARIABLES	Logit	Fract. Logit	Logit	Fract. Logit	Logit	Logit	PPML
Treat x Post Reform	1.173***	1.090	0.990	0.993	1.033	0.979	1.147**
	(0.0525)	(0.0629)	(0.0185)	(0.0221)	(0.0468)	(0.0482)	(0.0691)
All controls	✓	✓	✓	✓	✓	✓	✓
Observations	587,568	27,779	587,609	323,337	587,459	587,451	587,459
R2 / Pseudo R2	0.128	-	0.0286	-	0.136	0.106	0.085

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as controls for pre-reform tightness in occupation x region and pre-reform firm size. Coefficients reported in exponentiated form. Period of analysis: 2004-2010. The fractional logit model is a GLM estimator of the binomial family that a logit link function. It was proposed by Papke & Wooldridge (1996) to accomodate zero-inflated fractional outcomes. The pseudo-poisson maximum likelihood estimator was proposed by Silva & Tenreyero (2006) to accomodate zero-inflated continuous outcomes.

Table A5: Estmated Supply Elasticity of Wages

	(1)	(2)	(3)	(4)
	Log wages		Log entry wages	
VARIABLES	OLS	IV	OLS	IV
log employment (FTE)	0.0405***	-0.698***	0.0815***	-1.878***
	(0.00127)	(0.209)	(0.00309)	(0.668)
Observations	638,163	638,163	111,584	111,584
R-squared	0.651	0.276	0.638	-2.497
K-P rk Wald F-stat		23.25		9.716

Period of analysis: 2004-2012 (2011 and 2012 are added in the post-period to increase instrument strength). The IV results are derived from a model where the log of full time employment is instrumented with the interaction between the treatment group and the post reform period. All the other controls from the reduced form analysis are included in both stages.

Table A6: Placebo tests, and alternative control groups on employment outcomes (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of migrants in new hires				Log N. FT employees			
VARIABLES	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control
Treat x Post Reform	1.025	0.879**	1.200***	1.195***	-0.000241	0.00881	0.0158***	0.0172***
	(0.0480)	(0.0530)	(0.0574)	(0.0432)	(0.00435)	(0.00638)	(0.00507)	(0.00413)
Observations	222,309	148,946	213,123	485,350	311,629	249,564	635,454	764,914
R-squared	-	-	-	-	0.512	0.533	0.463	0.404

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

Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as all the other controls. Coefficients in columns (1) to (4) are reported in exponentiated form while the ones in columns (5) to (8) can be interpreted as semi-elasticities. Period of analysis: 2004-2010. Placebo treatment 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 tightness before the reform. Alternative control group restrict the sample to occupations in the 4th anf 5th percentile of tightness distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). Full extended list as control keeps in the control occupations all the ones appearing in the extended Romanian and Bulgarian list instead of keeping only the ones with tightness above the mean.

Table A7: Placebo tests, and alternative control groups on employment outcomes (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N. migrant new entries				N. native new entries			
VARIABLES	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control
Treat x Post Reform	1.224*	0.953	1.538***	1.340**	0.948*	1.081	0.924*	0.964
	(0.141)	(0.140)	(0.212)	(0.171)	(0.0259)	(0.0705)	(0.0372)	(0.0260)
Observations	382,700	270,956	373.373	844,487	382,726	270,997	373,454	844,487
R-squared	0.061	0.048	0.090	0.130	0.084	0.080	0.097	0.086

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as all the other controls. Coefficients are reported in exponentiated form. Placebo treatment 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 tightness before the reform. Alternative control group restrict the sample to occupations in the 4th anf 5th percentile of tightness distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). Full extended list as control keeps in the control occupations all the ones appearing in the extended Romanian and Bulgarian list instead of keeping only the ones with tightness above the mean.

Table A8: Placebo tests, and alternative control groups on employment outcomes (3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P. new hire				P. net positive hire			
VARIABLES	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control
Panel A: Migrants								
Treat x Post Reform	0.972	0.860**	1.225***	1.201***	0.974	0.891*	1.175***	1.190***
	(0.0374)	(0.0520)	(0.0605)	(0.0482)	(0.0415)	(0.0625)	(0.0574)	(0.0452)
Observations	382,700	270,956	373,373	844,487	382,606	270,892	373,354	844,487
R-squared	0.133	0.144	0.140	0.124	0.109	0.111	0.113	0.0980
Panel B: Natives								
Treat x Post Reform	0.951***	0.989	1.005	1.016	0.955***	0.984	0.988	1.014
	(0.0153)	(0.0278)	(0.0218)	(0.0175)	(0.0151)	(0.0284)	(0.0196)	(0.0144)
Observations	382,726	270,997	373,454	844,487	382,726	270,997	373,454	844,487
R-squared	0.0304	0.0336	0.0311	0.0330	0.0138	0.0133	0.0133	0.0158

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as all the other controls. Coefficients reported in exponentiated form. Period of analysis: 2004-2010. Placebo treatment 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 tightness before the reform. Alternative control group restrict the sample to occupations in the 4th anf 5th percentile of tightness distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). Full extended list as control keeps in the control occupations all the ones appearing in the extended Romanian and Bulgarian list instead of keeping only the ones with tightness above the mean.

Table A9: Placebo tests, and alternative control groups on wage outcomes (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log salary migrants				Log salary natives			
VARIABLES	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control
Treat x Post Reform	-0.00692	0.00807	-0.0289***	-0.0359***	0.00256	0.00793*	-0.00266	-0.00791**
	(0.00663)	(0.00767)	(0.00871)	(0.00675)	(0.00299)	(0.00453)	(0.00377)	(0.00322)
Observations	40,289	34,808	69,606	97,352	284,080	226,043	582,866	701,044
R-squared	0.812	0.822	0.750	0.753	0.767	0.785	0.687	0.680

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include firm, occupation, region, sector and year fixed effects as well as all the other controls. Period of analysis: 2004-2010. Placebo treatment 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 tightness before the reform. Alternative control group restrict the sample to occupations in the 4th anf 5th percentile of tightness distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). Full extended list as control keeps in the control occupations all the ones appearing in the extended Romanian and Bulgarian list instead of keeping only the ones with tightness above the mean.

Table A10: Placebo tests, and alternative control groups on wage outcomes (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log salary new entrants				Log salary incumbent workers			
VARIABLES	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control	Placebo reform 2007	Placebo treatment	Alternative control group	Full extended list as control
Panel A: Migrants								
Treat x Post Reform	-0.0356	-0.0227	-0.103***	-0.122***	0.00187	0.000435	-0.0192**	-0.0246***
	(0.0296)	(0.0348)	(0.0274)	(0.0252)	(0.00665)	(0.00821)	(0.00761)	(0.00630)
Observations	12,111	8,658	13,941	25,936	31,708	28,871	55,998	79,716
R-squared	0.836	0.856	0.687	0.784	0.839	0.841	0.798	0.783
Panel B: Natives								
Treat x Post Reform	-0.00619	0.0349	-0.0590***	-0.0607***	0.000861	0.00184	-0.00125	-0.00453*
	(0.0137)	(0.0254)	(0.0150)	(0.0136)	(0.00289)	(0.00445)	(0.00263)	(0.00258)
Observations	75,131	45,347	111,041	152,888	234,527	197,489	501,925	608,316
R-squared	0.819	0.832	0.668	0.716	0.781	0.795	0.713	0.695

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

Standard errors clustered at the occupation x region level. All regressions include firm, occupation, region, sector and year fixed effects as well as all the other controls. Period of analysis: 2004-2010. Placebo treatment 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 tightness before the reform. Alternative control group restrict the sample to occupations in the 4th anf 5th percentile of tightness distribution and defines the control group as all the occupations that are not treated (regardless of whether they enter the extended list). Full extended list as control keeps in the control occupations all the ones appearing in the extended Bulgarian and Romaninan list instead of keeping only the ones with tightness above the mean.

Table A11: Robustness to different fixed effects - employment outcomes (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of migrants in new hires				Log N. FT employees			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	0.00401***	0.00362**	0.00559***	0.00247*	0.0150***	0.0137***	0.0179***	0.0111**
	(0.00155)	(0.00156)	(0.00185)	(0.00140)	(0.00409)	(0.00479)	(0.00477)	(0.00479)
Observations	334,091	334,091	334,091	334,091	527,546	527,546	527,546	527,546
R-squared	0.036	0.266	0.001	0.266	0.206	0.451	0.001	0.451
Effect as growth from pre-reform:	**14%**	**12%**	**22%**	**9%**				
Occupation FE	✓	✓		✓	✓	✓		✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓		✓		✓		✓
Firm x Occup FE			✓				✓	
Sector x post reform				✓				✓

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

Table A12: Robustness to different fixed effects - employment outcomes (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N. migrant new entries				N. native new entries			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	0.0421*	0.0347**	0.0532***	0.0231**	-0.108*	-0.0751	-0.113	-0.0741
	(0.0248)	(0.0151)	(0.0200)	(0.00923)	(0.0575)	(0.0566)	(0.0847)	(0.0511)
Observations	587,609	587,609	587,609	587,609	587,609	587,609	587,609	587,609
R-squared	0.019	0.217	0.001	0.217	0.054	0.302	0.005	0.302
Effect as growth from pre-reform:	44%	**36%**	**56%**	**24%**	**-5%**	-4%	-5%	-4%
Occupation FE	✓	✓		✓	✓	✓		✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓		✓		✓		✓
Firm x Occup FE			✓				✓	
Sector x post reform				✓				✓

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

Table A13: Robustness to different fixed effects - employment outcomes (3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P. new hire				P. net positive hire			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Panel A: Migrants								
Treat x Post Reform	0.00599***	0.00641***	0.0125***	0.00470***	0.00417***	0.00417***	0.00857***	0.00325**
	(0.00215)	(0.00179)	(0.00246)	(0.00152)	(0.00153)	(0.00136)	(0.00195)	(0.00130)
Observations	587,609	587,609	587,609	587,609	587,609	587,609	587,609	587,609
R-squared	0.059	0.218	0.002	0.218	0.038	0.171	0.002	0.171
Effect as growth from pre-reform:	**14%**	**15%**	**29%**	**11%**	**13%**	**13%**	**26%**	**10%**
Panel B: Natives								
Treat x Post Reform	-0.00271	-0.000932	-0.00143	-0.00469	-0.00436	-0.00476	-0.00774*	-0.00503
	(0.00446)	(0.00379)	(0.00538)	(0.00343)	(0.00365)	(0.00338)	(0.00464)	(0.00329)
Observations	587,609	587,609	587,609	587,609	587,609	587,609	587,609	587,609
R-squared	0.039	0.239	0.022	0.239	0.017	0.183	0.014	0.183
Effect as growth from pre-reform:	0%	0%	0%	-1%	-1%	-1%	-2%	-1%
Occupation FE	✓	✓		✓	✓	✓		✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓		✓		✓		✓
Firm x Occup FE			✓				✓	
Sector x post reform				✓				✓

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

Table A14: Robustness to different fixed effects - wage outcomes (1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log salary migrants				Log salary natives			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treat x Post Reform	-0.0537***	-0.0360***	-0.0119**	-0.0302***	-0.0128***	-0.00490	0.000110	-0.00588*
	(0.00870)	(0.00727)	(0.00588)	(0.00799)	(0.00391)	(0.00346)	(0.00266)	(0.00351)
Observations	65,521	65,521	65,521	65,521	483,124	483,124	483,124	483,124
R-squared	0.482	0.764	0.031	0.764	0.504	0.696	0.023	0.697
Occupation FE	✓	✓		✓	✓	✓		✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓		✓		✓		✓
Firm x Occup FE			✓				✓	
Sector x post reform				✓				✓

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

Table A15: Robustness to different fixed effects - wage outcomes (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log salary new entrants				Log salary incumbents			
VARIABLES	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Panel A: Migrants								
Treat x Post Reform	-0.166***	-0.146***	-0.112***	-0.152***	-0.0309***	-0.0200***	-0.0103*	-0.0152**
	(0.0188)	(0.0303)	(0.0231)	(0.0394)	(0.00793)	(0.00693)	(0.00603)	(0.00724)
Observations	16,934	16,934	16,934	16,934	53,901	53,901	53,901	53,901
R-squared	0.423	0.793	0.016	0.794	0.522	0.798	0.032	0.798
Panel B: Natives								
Treat x Post Reform	-0.0731***	-0.0749***	-0.0626***	-0.0847***	-0.00992***	-0.000471	0.000486	-0.00124
	(0.0125)	(0.0168)	(0.0146)	(0.0174)	(0.00336)	(0.00287)	(0.00240)	(0.00291)
Observations	102,055	102,055	102,055	102,055	419,754	419,754	419,754	419,754
R-squared	0.468	0.744	0.007	0.745	0.511	0.719	0.025	0.719
Occupation FE	✓	✓		✓	✓	✓		✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE		✓		✓		✓		✓
Firm x Occup FE			✓				✓	
Sector x post reform				✓				✓

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

Table A16: Heterogeneity by region, job tightness and plant size - employment outcomes (1)

	(1)	(2)	(3)	(4)
	Share of migrants in new hires		Log N. FT employees	
Panel A: by type of region	Low migration region	High migration region	Low migration region	High migration region
Treat x Post Reform	1.119**	1.199***	0.00756	0.0245**
	(0.0585)	(0.0722)	(0.00506)	(0.00949)
Observations	205,207	128,884	331,584	195,962
R-squared	-	-	0.453	0.456
Panel B: by tigthess in occup.	Low tightness	High tightness	Low tightness	High tightness
Treat x Post Reform	1.036	1.284***	-0.00307	0.0340***
	(0.0605)	(0.0622)	(0.00747)	(0.00640)
Observations	165,372	168,719	268,427	259,119
R-squared	-	-	0.519	0.557
Panel C: by plant size	small plants	large plants	small plants	large plants
Treat x Post Reform	1.120**	1.200***	0.0121**	0.0146**
	(0.0621)	(0.0637)	(0.00499)	(0.00695)
Observations	176,862	157,229	265,932	261,614
R-squared	-	-	0.491	0.408

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. Coefficients in column (1) and (2) are reported in exponentiated form and are estimated using the fractional logit model, coefficients in column (3) and (4) are estimated using OLS and can be interpreted as semi-elasticities. All regressions include occupation, region, sector and year fixed effects as well as all the other controls. 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 tightness occupations are the 50% of occupations with the lowest level of tightness before the reform while high tightness are the other half. Small plants are the 50% of plants with the smallest number of employees before the reform while large plants are the other half.

Table A17: Heterogeneity by region, job tightness and plant size - employment outcomes (2)

	(1)	(2)	(3)	(4)
	N. migrant new entries		N. native new entries	
Panel A: by type of region	Low migration region	High migration region	Low migration region	High migration region
Treat x Post Reform	1.123	1.644***	0.899**	1.004
	(0.126)	(0.235)	(0.0398)	(0.0463)
Observations	367,489	220,067	367,527	220,081
R-squared	0.073	0.081	0.080	0.087
Panel B: by tigthess in occup.	Low tightness	High tightness	Low tightness	High tightness
Treat x Post Reform	1.213	1.660***	0.993	0.914*
	(0.180)	(0.225)	(0.0398)	(0.0460)
Observations	297,437	289,938	297,553	290,052
R-squared	0.041	0.118	0.054	0.107
Panel C: by plant size	small plants	large plants	small plants	large plants
Treat x Post Reform	1.452**	1.500***	1.037	0.882***
	(0.273)	(0.186)	(0.0356)	(0.0411)
Observations	301,193	286,355	301,214	286,393
R-squared	0.035	0.099	0.058	0.090

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. Coefficients are reported in exponentiated form and are estimated using the PPML model proposed by Silva & Tenreyero (2006). All regressions include occupation, region, sector and year fixed effects as well as all the other controls. 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 tightness occupations are the 50% of occupations with the lowest level of tightness before the reform while high tightness are the other half. Small plants are the 50% of plants with the smallest number of employees before the reform while large plants are the other half.

Table A18: Heterogeneity by region, job tightness and plant size - wage outcomes

	(1)	(2)	(3)	(4)
	Log salary migrants		Log salary natives	
Panel A: by type of region	Low migration region	High migration region	Low migration region	High migration region
Treat x Post Reform	-0.0281***	-0.0418***	0.00144	-0.0155**
	(0.00907)	(0.0113)	(0.00370)	(0.00688)
Observations	33,504	32,017	307,976	175,148
R-squared	0.733	0.781	0.681	0.704
Panel B: by tigthess in occup.	Low tightness	High tightness	Low tightness	High tightness
Treat x Post Reform	-0.0190*	-0.0388***	-0.00550	-0.00794**
	(0.0111)	(0.00915)	(0.00568)	(0.00392)
Observations	31,664	33,857	247,054	236,070
R-squared	0.803	0.808	0.740	0.747
Panel C: by plant size	small plants	large plants	small plants	large plants
Treat x Post Reform	-0.0130	-0.0423***	0.00305	-0.0131***
	(0.00835)	(0.00982)	(0.00356)	(0.00500)
Observations	28,532	36,989	244,385	238,739
R-squared	0.839	0.715	0.757	0.621

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region 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 tightness occupations are the 50% of occupations with the lowest level of tightness before the reform while high tightness are the other half. Small plants are the 50% of plants with the smallest number of employees before the reform while large plants are the other half.

Table A19: Reduced form results from elasticity of substitution analysis

	(1) log ratio migrant to native entries	(2) log ratio migrant to native wages	(3) log ratio migrant to native entry wages
VARIABLES	OLS	OLS	OLS
Treat * Post Reform	0.140***	-0.0130**	-0.0664***
	(0.0385)	(0.00532)	(0.0170)
Observations	12,151	73,793	18,326
R-squared	0.634	0.291	0.325
Year FE	✓	✓	✓
Occupation FE	✓	✓	✓
Plant FE	✓	✓	✓
Tightness and plant size controls	✓	✓	✓

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: 2004-2010. The sample corresponds to the match between the employment flow dataset (DMMO-EMMO) and the wage dataset (DADS) at the level of occupation x plant.

Table A20: Correlation across bargaining power and task diversity measures

	Monops. power	Occup. mobility	Task diversity
Monops. Power	1		
Occup. mobility	-0.1284***	1	
Task diversity	-0.2297***	0.2815***	1

Table A21: Heterogeneity of wage effect across bargaining power and task diversity

	(1)	(2)	(3)	(4)
	Log salary migrants		Log salary natives	
Panel A: by monopsony power	low monopsony	High monopsony	low monopsony	High monopsony
Treat x Post Reform	-0.0339***	-0.0283**	0.000461	-0.00854
	(0.00860)	(0.0119)	(0.00394)	(0.00542)
Observations	34,013	31,508	243,985	239,137
R-squared	0.809	0.801	0.732	0.750
Panel B: by occupation mobility	Low occup. Mobility	High occup. Mobility	Low occup. Mobility	High occup. Mobility
Treat x Post Reform	-0.0414***	-0.0131	-0.00947**	-0.00193
	(0.00860)	(0.0130)	(0.00402)	(0.00611)
Observations	39,998	24,361	253,835	218,641
R-squared	0.786	0.806	0.693	0.760
Panel C: by task diversity	Low task diversity	High task diversity	Low task diversity	High task diversity
Treat x Post Reform	-0.0368***	-0.0301***	-0.0111*	0.000153
	(0.0116)	(0.00966)	(0.00579)	(0.00450)
Observations	24,404	38,810	212,295	244,716
R-squared	0.840	0.794	0.776	0.731

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at the occupation x region level. All regressions include occupation, region, sector and year fixed effects as well as all the other controls. Period of analysis: 2004-2010. Monopsony power is measured as the concentration (Herfindhal Index) of employment in a given occupation across firms. High monopsony power selects the 50% of the sample where employment is the most concentrated within few firms. Occupation mobility is measured as the probability of switching occupationo ver employment spells. Finally, task diversity is computed as the concentration (Herfindhal Index) of different activities within each occupation. It is computed using DOT data, and high task diversity selects the 50% of the sample with the lowest concentration of activities.

B Figures

Figure A1: Administrative Procedure to Hire a Foreign Worker

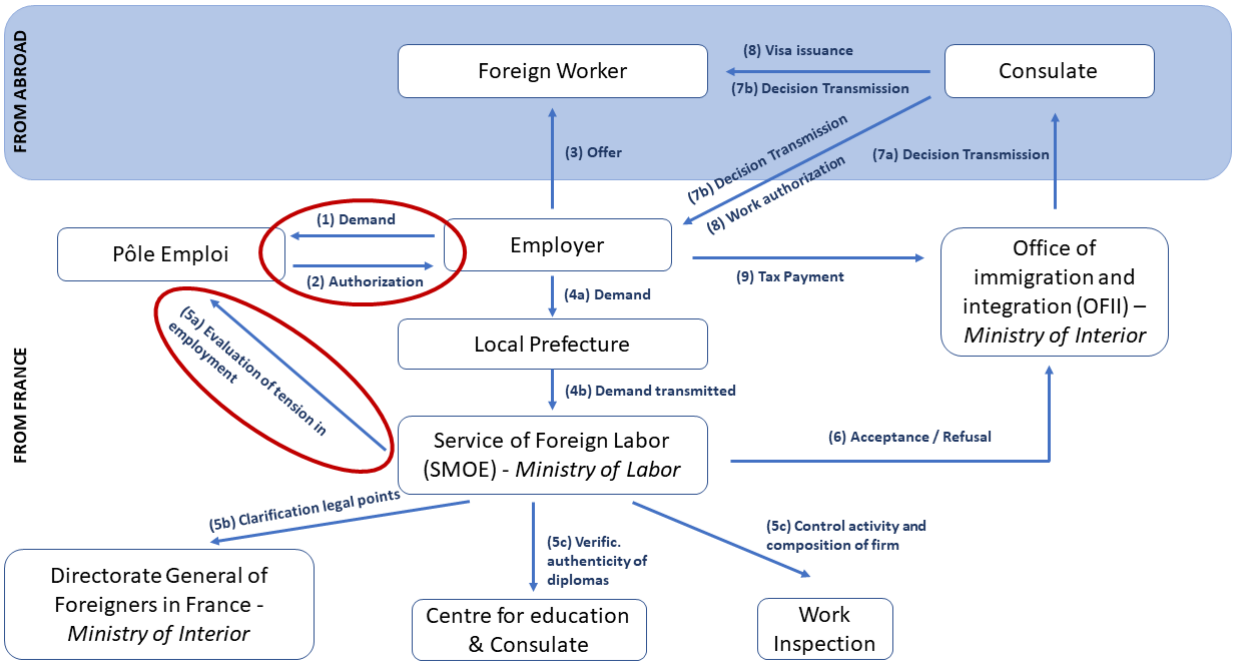
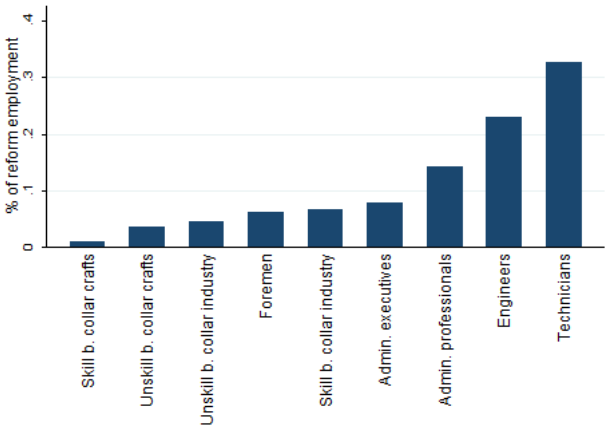


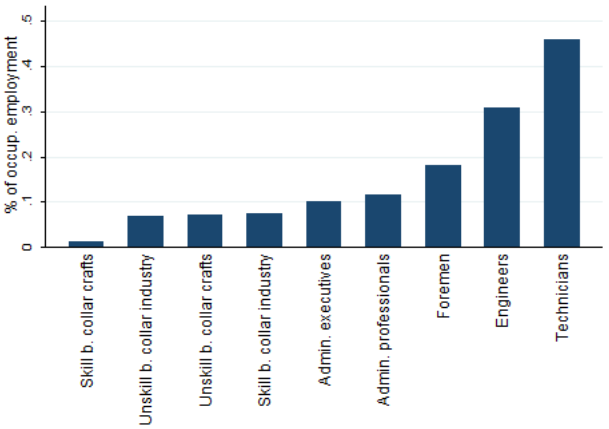
Diagram taken from OECD (2017). The red circles identify the administrative steps relaxed by the reform.

Figure A2: Occupational characterization of the reform

a) Occup. distribution of reform employment



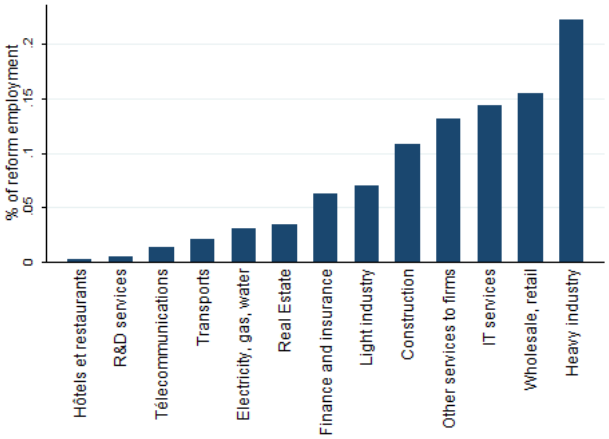
b) Occupational exposure to the reform



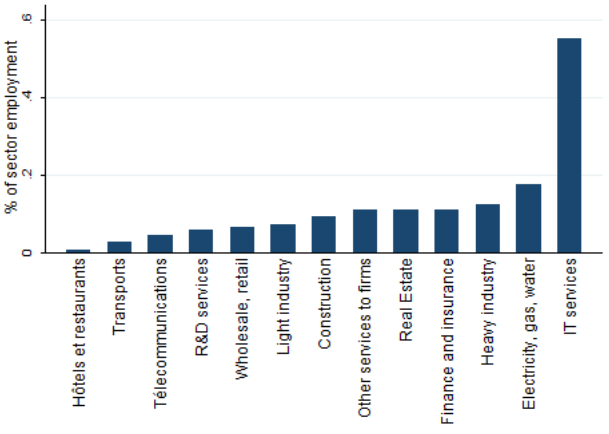
Author's calculations based on data from French Labour Force Survey 2004-2007. Panel a) shows how the total employment concerned by the reform is distributed across broad occupation categories. Panel b) shows how much of each occupation's employment is concerned by the reform (measure of exposure).

Figure A3: Sectoral characterization of the reform

a) Sectoral distribution of reform employment



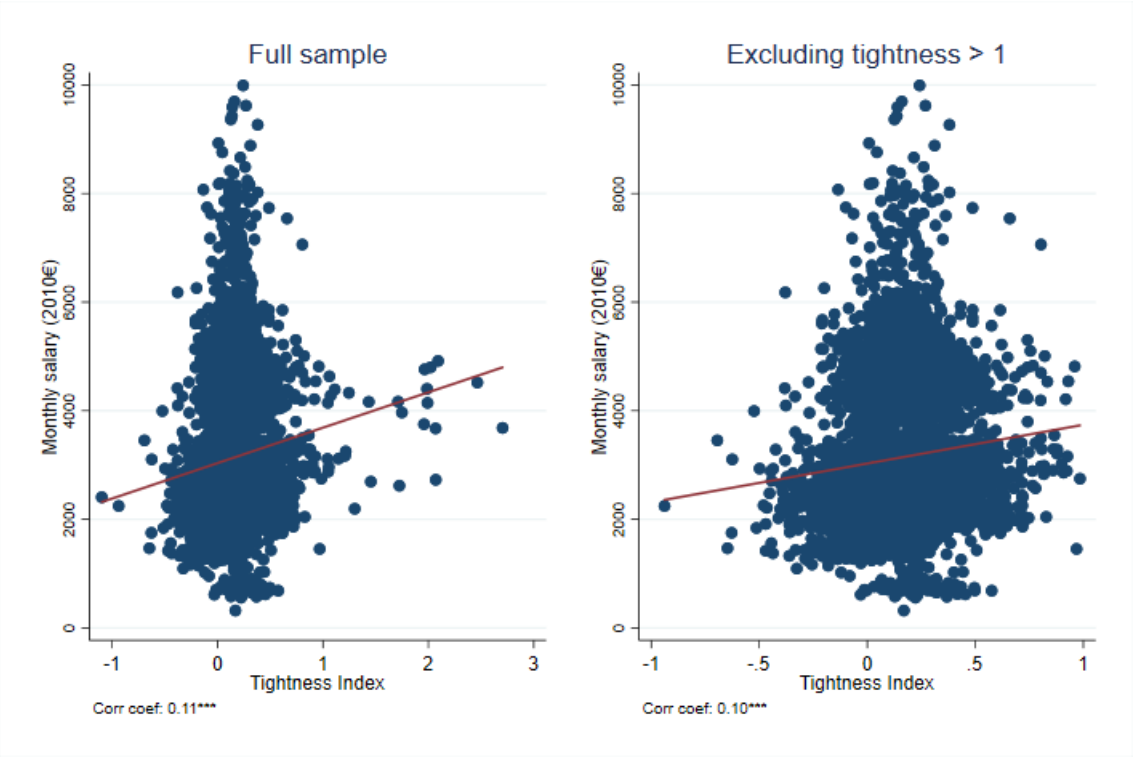
b) Sectoral exposure to the reform



Author's calculations based on data from French Labour 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 exposure).

Figure A4: Overall Immigration Flows by Year

Figure A5: Correlation between tightness and average wage



Source: author calculations based on tightness indicators produced by Pôle Emploi and average wages derived from DADS poste. Each dot corresponds to an occupation x region x year. Period considered: 2004-2007. The tightness index is computed as a standardized score. The right panel excludes observations where tightness is more than one standard deviation above the mean, to show that they are not the only ones driving the correlation.

Figure A6: Additional event study graphs