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Anthony Edo

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# The Impact of Immigration on Native Wages and Employment*

Anthony EDO†

**Abstract**

This paper investigates the immigration impact on native outcomes using micro-level data for France. I find that immigration does not affect the wages of competing natives, but induces adverse employment effects. This finding is consistent with a wage structure that is much less flexible in France. The quality of the data allows to dig more deeply into the interpretation of the immigration impact. First, I show that immigrants displace native workers because they are more willing to have bad employment conditions. Second, I find that natives on short-term contracts, who are less subject to wage rigidities, do experience wage losses due to immigration.

**Keywords:** immigration, wage rigidities, employment, naturalization

**JEL Classification:** F22, J31, J61

**Résumé:** cet article est destiné à étudier l'impact de l'immigration sur les salaires et l'emploi des natifs en France. Nos estimations indiquent que l'immigration n'affecte pas les salaires des natifs avec lesquels les immigrés sont substituables. Ce résultat est en accord avec la forte rigidité salariale qui caractérise le marché du travail français. En revanche, ce papier met en lumière un effet négatif de l'immigration sur l'emploi des natifs. La qualité des données utilisées permet d'étudier les mécanismes sous-jacents à cet effet. En particulier, nous montrons qu'à niveaux de productivité comparables, les immigrés sont plus enclins à accepter des conditions d'emploi difficiles. Les entreprises tendent donc à substituer des immigrés aux natifs pour bénéficier de cette main d'œuvre plus attrayante.

**Mots clés:** immigration, rigidités salariales , emploi, naturalisation

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†Paris School of Economics and University of Paris I Panthéon-Sorbonne. Centre d'économie de la Sorbonne, 106-112 Boulevard de l'Hôpital, 75647 Paris, France. Tel. : +0033144078105. E-mail : Anthony.Edo@univ-paris1.fr.

# 1 Introduction

One commonly heard concern about immigration is that the native population suffers from the competition with migrants (Zimmermann, Bauer, et Lofstrom, 2000). This sentiment is consistent with some empirical studies which find evidence of a negative impact of immigration on the wages of competing native workers (see the studies by Borjas (2003, 2008) for the United-States and Puerto-Rico; Aydemir et Borjas (2007) for Canada and Mexico; Steinhardt (2011) for Germany; Bratsberg et Raaum (2012) for Norway). Other studies have also find depressive employment effects due to immigration (Angrist et Kugler, 2003; Glitz, 2012). In this article, I take a fresh look at the immigration impact by investigating how immigration can affect the labor market competition, and *in fine* the outcomes of natives.

The sentiment that immigration hurts the outcomes of natives is likely to be based on the belief that migrants are more willing to accept bad employment conditions. More generally, it might be that immigrants exhibit some attractive characteristics for firms, so that employers have incentives to substitute immigrants for natives. In order to examine this new aspect of the immigration impact, this analysis exploits a very rich dataset available for France. It provides a wide set of demographic, social and employment characteristics at the individual level. These micro-level data are provided from 1990 to 2002, a period over which the share of migrants in the labor force increased from 6.5% to 8.5%.[1]

The first contribution of the paper is to show that immigrants are more willing to accept lower wages and more painful working conditions than equally productive native workers. The richness of the French data allows to show that foreign-born workers exhibit a 2-3% lower wage and they are more likely to do late hours, and work at night or on the weekends. One of the reasons for this discrepancy between natives and foreign-born workers is that immigrants have lower outside options. For instance, immigrants have a limited access to the labor market – with a restricted access to public sector jobs (Math et Spire, 1999), as in the United States (Bratsberg, Ragan, et Nasir, 2002) – and to welfare state benefits (Math, 2011).

This set of results suggests that immigrants have specific characteristics that should make them relatively more attractive for firms compared to natives with similar productivity. Yet, this dissimilarity between natives and immigrants should have strong implications in terms of immigration impact on native outcomes. In particular, immigration should enhance the labor market competition and strongly depress the outcomes of equally productive native workers. In order to examine this implication, I use the skill-cell approach by Borjas (2003) since it allows to capture the own-effect of immigrants on the outcomes of competing natives.

[1]Over this period, the average number of new entrants by year is around 145,000 (Thierry, 2004).

However, the focus on the French labor market may not lead immigration to cause wage adjustment due to rigid institutions (minimum wage laws, strong trade unions and generous unemployment benefits). The empirical analysis indeed finds that immigration does not affect wages.[2] However, since migrants are more likely to accept worse employment conditions, immigration tends to decrease the employment of competing native workers – *i.e.* immigrants displace native workers. The baseline estimate implies that a 10% increase in the share of immigrants relative to the native workforce in an education-experience cell decreases the employment rate of male natives by about 3% in the short-run.

This paper goes beyond these average effects in two important ways. First, the quality of the French data allows to shed light on the important role played by the type of employment contract (short-term/long-term) in shaping wage rigidities. I find in particular that the natives who have short-term contracts (*i.e.* the natives who should not be subject to wage rigidities) rather experience huge wage losses due to immigration. Conversely, the insensitivity of wages to immigration is even more striking for those with long-term contracts.

On the other hand, I use the heterogeneity of migrants with respect to their nationality and show that migrants who obtain French citizenship no longer depress native employment. Instead, the aforementioned negative effects on native employment are completely attributable to the presence of non-naturalized immigrants. This second set of results supports the idea that the displacement mechanism operating between immigrants and natives lies in heterogeneous behaviors among workers (due to lower outside options among immigrants) – *i.e.* the fact that immigrants are more willing to accept bad employment conditions than equally productive natives. Indeed, the migrants who became French citizens have similar behaviors to natives since the naturalization leads to higher outside options, such as superior employment opportunities (Bratsberg, Ragan, et Nasir, 2002; Fougere et Safi, 2009) or equal access to social benefits with natives. Consequently, employers no longer have any incentive to replace native workers by the naturalized immigrants.

In order to support that the differential impact of immigrants on native employment hinges only on their citizenship status (naturalized/non-naturalized), I use matching techniques. I thus create two groups of immigrants which differ only in their citizenship and compare their impact on native employment. The main result still hold: the subsample of naturalized migrants which has similar characteristics to non-naturalized migrants does not impact native employment.

The remainder of this paper proceeds as follows. Section 2 discusses the expected effects of an immigration shock on the French labor market. The third section describes the data and methodologies used in the paper. Section 4 investigates immigrant-native dissimilarities in wages

[2]This result supports and generalizes those of Glitz (2012) for Germany.

and working conditions. This section also reports the estimated impact of immigration on native outcomes. The sixth and seventh sections provide two empirical extensions by underlining the importance of job contracts and migrant nationality in shaping the immigration impact. The last section concludes.

# 2 The Theoretical Effects of Immigration

The impact of migrations on the labor market is usually studied within the framework of a competitive model of labor demand where wages are perfectly flexible. In the short run, a competitive model suggests that higher levels of immigration should lower the outcomes of competing workers and increase those of complementary workers. In the long-run, these models predict that the host country's wage is independent of migration. The physical capital response to immigration will offset the fall of the capital-labor ratio. In the long-run, the economy therefore returns to its pre-immigration equilibrium, where wage and employment levels are exactly the same as they were prior to the immigrant influx. However, although an inflow of migrants should not affect the average level of native outcomes in the long run, some native workers will gain from immigration (complementarity effect), while others will lose (substitution effect).

Nevertheless, these theoretical results are unlikely to apply to France due to labor market frictions. In comparison to the United States, Card, Kramarz, et Lemieux (1999) report evidence that France has a variety of institutional features that prevent wage adjustment. Among the most prominent characteristics that may prevent the decline of wages are the high minimum wage, the strong power of unions and the importance of income support programs for unemployed individuals. In France, employers should therefore be unable to lower wages when marginal productivity drops due to immigration shocks.

Within the framework of downward inflexible wages, if natives and immigrants are complements, an immigration shock should increase native wages and employment (as predicted by the standard competitive model). In fact, if institutional factors resist the downward wage pressure, it is very likely that they allow for upward adjustments. However, if natives and immigrants are substitutes, immigration should increase the level of unemployment in the economy (Saint-Paul et Cahuc, 2009).

The immigration impact on native outcomes hinges on the degree of substitution between natives and immigrants. In this regard, prior empirical studies have reached mixed conclusions (Borjas, 2003; Ottaviano et Peri, 2008, 2012; Borjas, 2008; Borjas, Grogger, et Hanson, 2012). For instance, Ottaviano et Peri (2008, 2012) provide evidence that comparably skilled immigrants and natives are imperfect substitutes in production for the United States.[3] However, the results of imperfect substitutability tend to be sensitive to the selected sample and strategies of identification (Borjas, 2008; Borjas, Grogger, et Hanson, 2011, 2012). While Ottaviano et Peri (2008, 2010 with D'Amuri, 2012) find an estimated elasticity of substitution between natives and immigrants around 20 for the United States and ranging between 16 and 21 for Germany; Edo et Toubal (2013) find an elasticity of substitution equal to infinity for France. Across a wide variety of specifications and samples, Edo et Toubal (2013) particularly show that the hypothesis that immigrants and natives with similar education-experience profiles are perfect substitutes in production cannot be rejected.[4]

As a result, an immigration supply shock is expected to have a very limited impact on the French wage structure. An inflow of migrants should thus be translated into an equal rise in the number of unemployed people. Yet, if immigration increases the level of unemployment, the short-term impact of migrants on native employment is unpredictable here. Two scenarios are possible. New migrants could *(i)* directly become unemployed or *(ii)* hurt native employment.[5] In effect, the non-adjustment of wages should prevent the newcomers from finding a job (scenario *(i)*). Thus, immigrants would become mechanically unemployed and would not affect native employment.

Immigration could also have a short-run depressive impact on native employment through displacement effects. In particular, if immigrants exhibit some attractive characteristics for firms (while they are identical to natives in all other respects), they should be substituted for natives in the production process. In this regard, the remaining of the paper argues that the immigrant population differs from the native population in important ways. These dissimilarities will make immigrants relatively more attractive for firms, and will therefore lead immigration to depress the employment of equally productive natives through a substitution mechanism.

# 3 Data & Methodologies

## 3.1 Data, Variables & Sample Description

The empirical study is based on the French annual labor force survey (LFS) covering the period

[3]This finding is also reported by D'Amuri, Ottaviano, et Peri (2010), Felbermayr, Geis, et Kohler (2010) and Brücker et Jahn (2011) for Germany, Gerfin et Kaiser (2010) for Switzerland and Manacorda, Manning, et Wadsworth (2012) for the UK.

[4]In appendix (section A), I follow the study by Edo et Toubal (2013), and estimate the substitution elasticity between natives and immigrants for our period of interest 1990-2002.

[5]First, notice that these scenarios are not exclusive to each other. Second, in both cases, newcomers impose a cost on society in terms of foregone output. But in scenario *(ii)*, immigration leads to an additional cost in terms of unemployment benefits (D'Amuri, Ottaviano, et Peri, 2010).

1990 through 2002. This survey is carried out by the French National Institute for Statistics and Economic Studies (INSEE - *Institut National de la Statistique et des Etudes Economiques*). First, this section describes the data and sample used to perform the study. Then, it presents the two sets of variables used to investigate *(i)* immigrant-native dissimilarities in employment conditions and *(ii)* the labor market impact of immigration.

### 3.1.1 Data & Sample Selection

The LFS records much information about a random and representative sample of around 150,000 individuals per year. Constructed from repeated cross sections carried out in the same way over 13 years, the pseudo panel includes demographic characteristics (nationality, age, gender, and marital status), social characteristics (educational attainment, age of completion of schooling, and family background), as well as employment status, occupation, earnings, number of hours worked a week, etc.

In accordance with the literature on migration, I define an immigrant as a person who is foreign-born outside France. Certain immigrants may thus have become French through citizenship acquisition while others have remained non-French (or non-naturalized). The data provide detailed information on individual nationality (more than 80 countries) and distinguish naturalized immigrants from others.

The employment survey gives human capital characteristics for each respondent, such as their education level, their age, and the age when they completed their studies. The education level divided into six categories from high college graduate to no diploma. According to the International Standard Classification of Education (ISCED), those six levels of education respectively correspond to *(1)* the second stage of tertiary education, *(2)* the first stage of tertiary education, *(3)* post-secondary non-tertiary education, *(4)* (upper) secondary education, *(5)* lower secondary education and *(6)* primary & pre-primary education.

Individuals with the same education, but a different age or experience are unlikely to be perfect substitutes (Card et Lemieux, 2001). Hence, I distinguish individuals in terms of their labor market experience. Following Mincer (1974), work experience is computed by subtracting for each individual the age of completion of schooling from reported age.[6] This measure differs from the one used in the migration literature since the age of completion of schooling is usually unavailable.[7]

[6]The age of completion of schooling is usually considered as a proxy for the entry age into the labor market – *i.e.* the starting point from which an individual begins to accumulate work experience. For a few surveyed individuals, the age of completion of schooling is very low, between 0 and 11 inclusive. Since individuals cannot start accumulating experience when they are too young, I have raised the age of completion of schooling for each surveyed individual to 12 if it is lower.

[7] Empirical works rather assign a particular entry age into the labor market to the corresponding educational

Finally, I follow most empirical studies and restrict my attention on men[8] in the labor force (employed and unemployed individuals) aged from 16 to 64, who are not enrolled at school, who are not self-employed (farmers and entrepreneurs), and have between 1 and 40 years of labor-market experience.

### 3.1.2 First Set of Variables

A first set of variables is used to investigate immigrant-native dissimilarities in employment conditions. For each worker, the survey reports the monthly wage net of employee payroll tax contributions adjusted for non-response, as well as the number of hours worked per week. I use these information and compute the hourly wage for each worker to investigate wage inequalities. For 11% of workers (who present unusual working hours), I use the number of hours worked during the previous week to compute their hourly wage. Since wages are reported in nominal terms, they need to be adjusted for inflation. The French Consumer Price Index computed by the INSEE is thus used to deflate all wages with 2000 as the reference base period.

The survey also provides original information on working condition. It records whether employed individuals work at night (from midnight to 5am), at late hours (from 8pm to midnight), on Saturdays and Sundays. More precisely, the survey provides the frequency of those specific working conditions whether they are usual, occasional or never realized. I use these variables to build three dummies indicating if an employee usually works at night, at late hours or on the weekend (Saturdays or Sundays).

The richness of the French micro-level data allows to control for many variables that should affect immigrant-native inequalities. In addition to human capital information, the survey contains job characteristics. For each worker, the type of employment (public/private), the working time structure (full-time/part time) and the type of contract (short-term/long-term) are given. The data also provide an original variable indicating the entry year into a firm for each worker. I use this variable to compute the job tenure of workers. Occupations and regions of residence are also provided for each individual. The French LFS has the advantage to record 360 occupations. Finally, the LFS also reports family and social characteristics related to the number of children in

category.

[8]Women are generally excluded from samples for two reasons. First, they have to face more frequent periods of inactivity or unemployment, so that the correspondence between their potential and effective experience tends to collapse. It is therefore difficult to make any sensible inference based on these grouped data. Second, "the inclusion of working women in the analysis introduces selection issues that are difficult to address and resolve" (Borjas, 2013). These issues have been widely emphasized and studied by the literature on labor supply (see, for instance, Heckman, 1993).

the household, the marital status (single/couple) and the occupational category (over 29) of the respondent's father.

#### 3.1.3 Second Set of Variables

This paper adopts the skill-cell methodology from Borjas (2003) to investigate the labor market impact of immigration. This methodology aims at dividing out the national labor market into skill-cells. The cells are built in terms of educational attainment $j$, experience level $k$, and calendar year $t$, each of them defines a skill group at a point in time for a given labor market. Individuals are then clustered into these skill-cells according to their education-experience profile so as to compute the labor market outcomes of natives and the immigrant share in the labor force.

This paper uses four different labor market outcomes: the average monthly and hourly wages, the employment rate to population and the employment rate to labor force.[9] The first group of outcomes is devoted to capturing the price of the native labor force, while the second group is a proxy for the labor quantity supplied by natives on the market. These variables are computed using a personal weight provided by the INSEE to attenuate potential measurement errors.

While the average monthly wage is computed for full-time native workers, the calculation of the average hourly wage also includes part-time workers. To compute the average hourly wage in the cell $(j, k, t)$, I independently calculate the average monthly wage and the total amount of hours worked in each skill-group.[10] Both wages are adjusted for inflation.

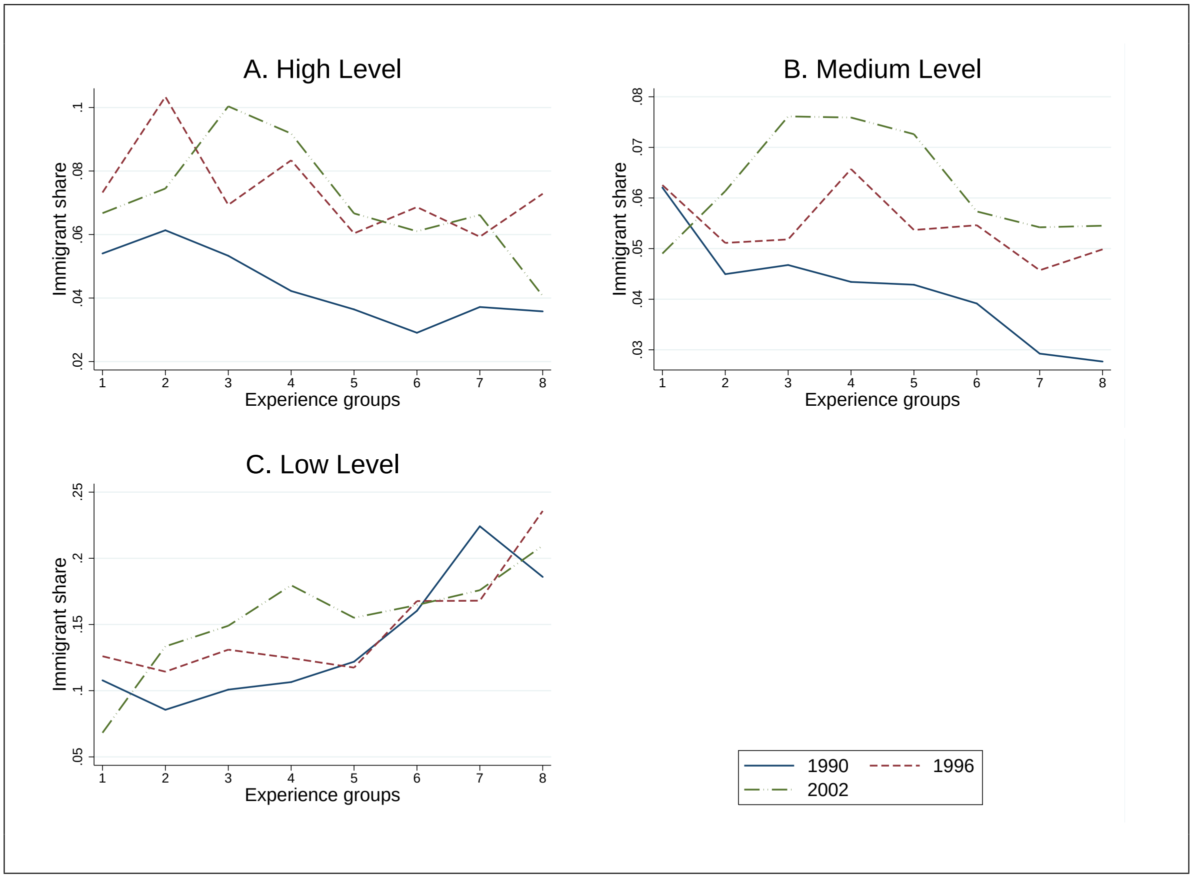
The employment rates are computed using the employment status of individuals. They are respectively equal to the employment of full-time native workers as a percentage of the overall native population aged from 16 to 64 (employed, unemployed and inactive) and as a percentage of the native labor force (employed and unemployed). The second ratio is a better measure of labor market opportunities. However, the comparison of these ratios will inform us on the immigration impact on the participation rate of natives (equals to the employment rate to population divided by the employment rate to labor force).

Following Borjas (2003), the immigrant supply shock experienced in a particular skill-cell with educational attainment $j$, experience level $k$ at year $t$ is measured by $p_{jkt}$, the percentage of total

[9] The content and trend of the four dependent variables are reported in the appendix for the skill-cells in the following calendar years: 1990, 1993, 1996, 1999 & 2002 (Tables 6, 7, 8 & 9). For each year, I also provide the number of observations which was used to compute the dependent variables. For Tables 8 & 9, I give the number of full-time native workers which was used to compute the numerator of the two employment ratios.

[10] This procedure reduces the loss of observations. Although some workers do not report their wage income, they always state their number of hours worked.

**Figure 1: Immigrant Share per Cell in 1990, 1996 & 2002**




**Notes.** The Figure illustrates the supply shocks experienced by the different skill-cells between 1990 and 2002. Experience groups denoted 1, 2, 3,..., 8 correspond respectively to an experience level equal to 1-5, 6-10, 11-15, ..., 36-40 years. The population used to compute the immigrant share includes men participating in the labor force aged from 16 to 64, not enrolled at school and having between 1 and 40 years of labor market experience. Self-employed people are excluded from the sample.

labor supply in a skill group coming from immigrant workers:

$$p_{jkt} \quad = \quad M_{jkt}/\left(N_{jkt} + M_{jkt}\right) \ ,$$

with $N_{jkt}$ and $M_{jkt}$ respectively the number of male natives and immigrants in the labor force located in the *schooling-experience-time* cell $(j, k, t)$. As well as native outcomes, the immigrant share is computed using a personal weight. The immigrant supply shock for each skill-cell is computed on the basis of 31,309 to 34,994 individual observations per year, of which between 8.0%

and 8.8% represent immigrants.

The graphs in Figure 1 illustrate the share of foreign-born workers for three education levels (high, medium and low) and three years (1990, 1996 & 2002).[11] Eight experience groups are defined, each spanning an interval of 5 years. The figure shows that immigration greatly increased the supply of the high- and medium-educated populations. These supply shifts did not affect all age groups within these populations equally. The immigrant supply shock experienced in the highly and medium-educated groups particularly increased in cells with more than 10 years of experience. The figure also indicates that immigrants are overrepresented in the low-educated segment of the labor market. However, this schooling group did not experience important supply shocks due to immigration.

## 3.2 Empirical Strategies

This paper uses important specificities of the immigrant population to investigate how immigration can affect the outcomes of equally productive natives. In order to do so, I first exploit Mincerian equations to examine the labor market dissimilarities in employment conditions between natives and immigrants. Second, I use the skill-cell methodology, introduced by Borjas (2003), to measure the labor market impact of immigration.

### 3.2.1 Extended Mincerian Equations

The study of labor market inequalities requires focusing on a non-randomly selected sample, that of workers. Yet, the productivity and behavior of workers may be different from that of individuals who are not included in this specific sample. Thus, the estimates of wage and work conditions inequalities may be biased due to a selectivity problem (Heckman, 1979; Blackaby, Leslie, Murphy, et O'Leary, 2002). The Heckman two-stage estimation procedure is undertaken to address this potential issue. The vector of selection variables has to contain at least one element that is excluded from the second-stage regressions (Sartori, 2003). Satisfactory identification requires data on factors that affect the labor market participation but do not directly wages. Following Glewwe (1996), I use marital status, family size and family background as identifying instruments.[12]

In order to capture the (unexplained) wage differential between natives and immigrants, I use the following Mincerian equation:

[11] In the appendix, Table 10 completes Figure 1 by providing the distribution of male natives and immigrants in the labor force per group of education over time.

[12] More specifically, I use the number of children in the household, a variable indicating whether the individual is single or not and a vector of father's occupation.

$$ln\left(w_{iort}\right) = \alpha_0 + \alpha_1 I_i + \alpha_2 H_i + \alpha_3 J_i + \zeta_o + \zeta_r + \zeta_t + \xi_{iort}\,. \qquad (1)$$

The dependent variable is the hourly wage logarithm for each individual $i$, in occupation $o$ and region $r$ at time $t$. The immigrant status of an individual is captured by the term $I_i$ which is a dummy variable indicating if the employee is an immigrant. The term $H_i$ is a vector of control variables containing the human capital characteristics for individual $i$ such as the age of completion of schooling, the labor market experience and its square. Job characteristics $J_i$ control for job tenure and its square, part-time employment, the type of job contract, public sector jobs and types of work (nights and weekends). In order to control for occupation-specific factors, we also add a vector of occupational dummies $\zeta_o$. We also include region and time dummy variables, respectively denoted $\zeta_r$ and $\zeta_t$, as geography and cyclical effects might affect individual wages. The error term $\xi_{iort}$ will be corrected for heteroscedasticity by the White method.

However, the prevalence of a high minimum wage in France should lead to a censoring problem and bias the estimates of $\alpha_1$. The discontinuity of the hourly wage distribution is addressed using Tobit estimation. For each year of the survey, different censoring values for the hourly minimum wage are thus used.

In order to investigate immigrant-native disparities in work conditions, three dummies indicating if an employee works *(i)* at night, *(ii)* at late hours or *(iii)* on the weekend are used as dependent variables. Then, I can estimate the three probit equations to examine whether those specific working conditions are, *ceteris paribus*, more widespread among immigrant workers. Compared to equation (1), I include two additional covariates: the number of children who live in the household and a dummy variable indicating whether the individual is single or not.

### 3.2.2 The Skill-Cell Methodology

I use the skill-cell methodology to examine the immigration impact on native outcomes. This methodology is the most suitable to investigate how the outcomes of natives can react due to an increase in the number of comparably skilled immigrants.[13]

The skill-cell methodology is based on the following equation:

$$y_{jkt} \;=\; \alpha + \beta\left(p_{jkt}\right) + \delta_j + \delta_k + \delta_t + \delta_j \times \delta_t + \delta_k \times \delta_t + \delta_j \times \delta_k + \xi_{jkt}\,, \qquad (2)$$

where $y_{jkt}$ is the labor market outcome at period $t$ for native men with education $j$ and expe-

[13]The present paper therefore focuses only on the partial elasticity of native outcomes to immigration (See Edo et Toubal (2013) for a complement study on the overall labor market impact of immigration in France).

rience $k$ and $p_{jkt}$ is the immigrant share. In addition to including the vectors of fixed effects for schooling $\delta_j$, experience $\delta_k$ and time $\delta_t$, this model also contains a full set of second-order interactions for schooling by time, experience by time and schooling by experience. The linear fixed effects in equation (2) control for differences in labor market outcomes across schooling groups, experience groups, and over time. Interactions $\delta_j \times \delta_t$ and $\delta_k \times \delta_t$ control for the possibility that the impact of education and experience on outcomes changed over time, whereas $\delta_j \times \delta_k$ control for differences in the experience profile by schooling group. $\xi_{jkt}$ is a remainder error term. The standard errors will be corrected for heteroscedasticity and clustered around education-experience groups to adjust for possible serial correlation.

The skill-cell approach identifies the labor market impact of immigration by examining how the evolution of outcomes within skill-cells has been affected by differences in the size of the supply shocks. The fact that migrants may not be randomly distributed across skill-cells would lead to biased estimates of the parameter of interest $\beta$. Suppose that the labor market may attract foreign-born workers mainly in those skill-cells where wages and employment are relatively high. There would be a spurious positive correlation between $p_{jkt}$ and the labor market outcomes of natives (Borjas, 2003). As a result, an instrumentation strategy would be necessary if the basic estimates from the skill-cell approach indicate that $\hat{\beta} > 0$. If the estimates rather indicate that $\hat{\beta} < 0$, the correction of the (upward) bias would induce the true immigration impact to be more negative. Within that case, the endogeneity of the immigrant share is therefore less problematic. In the remaining of this paper, I will use the fact that the estimates of $\beta$ has to be interpreted as lower bounds of the true impact of immigration to reinforce my empirical results.

In addition, the estimates are very likely to be sensitive to how skill groups are defined (Aydemir et Borjas, 2011). The dimension of the education-experience cells requires to trade off cell sample sizes against the number of observations available to run regressions. A finer (broader) classification of education-experience level grid drives up (down) the sample size, but reduces (increases) the number of observations in each cell. Yet, a small sample size per cell tends to attenuate the impact of immigration because of sampling error in the measure of the immigrant supply shift $p_{jkt}$ (Aydemir et Borjas, 2011). Hence, I build three samples with different structures of education-experience cells.

The baseline sample combines three categories of educational attainment $j = 3$ and eight experience groups $k = 8$, so that the labor market is divided into 24 segments.[14] In order to build the three education groups, I simply merge the two highest levels of education [Second stage of tertiary education - First stage of tertiary education], the two medium ones [Post-secondary

[14] In their empirical study, D'Amuri, Ottaviano, et Peri (2010), Felbermayr, Geis, et Kohler (2010) and Gerfin et Kaiser (2010) also use three education groups.

non-tertiary education - (Upper) secondary education] and the two lowest ones [Lower secondary - Primary education and Pre-primary education]. Regarding the experience dimension, eight groups of experience are generally chosen (Borjas, 2003; Ottaviano et Peri, 2008, 2010 with D'Amuri, 2012; Ortega et Verdugo, 2011), each spanning an interval of 5 years of experience [1-5; 6-10; 11-15; 16-20; 21-25; 26-30; 31-35; 36-40].

The two alternative samples make up four experience groups $k = 4$, but one of them contains three education classes $j = 3$ while the other contains six $j = 6$. Following Felbermayr, Geis, et Kohler (2010) and Gerfin et Kaiser (2010), I categorize individuals in four rather broad experience groups, each spanning an interval of 10 years of experience. This classification should attenuate the impact of any potential bias regarding the experience measure, and in particular, the fact that employers may evaluate the experience of immigrants differently from that of natives.

The sample with six rather narrower education levels is built to test the possibility of an educational downgrading among immigrants. Indeed, immigrants could accept jobs requiring a lower level of education than they have (Dustmann, Frattini, et Preston, 2013). Therefore, within a broad education group, immigrant workers could compete with the less educated natives of the cell. In this case, the labor market segmentation along three (broad) education levels could fail to appropriately identify groups of workers competing for the same jobs. Hence, a more detailed education partition with six education groups should allow to deal with the impact of immigrants on equally educated native workers. In particular, if immigrants downgrade upon arrival, the estimated effect on native outcomes should differ from a sample with six education groups to a sample with only three.

To sum up, the baseline sample divides the labor market into 24 ($j = 3 \times k = 8$) skill-cells, while the two alternative samples divide it into 12 ($j = 3 \times k = 4$) and 24 ($j = 6 \times k = 4$) segments.

# 4 The Econometric Analysis

## 4.1 Labor Market Conditions between Natives & Immigrants

This section underlines the prevalence of heterogeneous behaviors among workers through evidence of wage and work condition inequalities between natives and immigrants. The *left-hand side* (first two columns) of Table 1 report estimates of $\alpha_1$ from equation (1) for two specifications: one correcting for selection and the other for censoring (around 15,000 observations are left-censored).

The estimates indicate a negative wage premium of 2-3% for immigrants – *i.e. ceteris paribus*, immigrant wages are lower than those of natives by around 2-3%. This is in accordance with other findings for France (Algan, Dustmann, Glitz, et Manning, 2010). Table 1 also presents estimation of the inverse Mills ratio. The positive and significant selectivity term suggests that if those who are out of work were to find work, they would have lower earnings than individuals with similar characteristics who already have a job. This result is supported by the fact that individuals who do not enter the labor market are less productive on average.

Labor market disparities between natives and immigrants are also marked in terms of working conditions. The *right-hand side* of Table 1 reports the likelihood of working at night, at late hours and on the weekend for migrants. Each specification corrects for sample selection bias. The estimated coefficients are always significantly positive, implying that migrant workers are more likely to experience difficult working conditions. As expected, the negative inverse Mills ratios indicate that workers always prefer not to experience those specific work conditions. The finding of immigrant-native disparities in work conditions is consistent with Coutrot et Waltisperger (2009). For France, they show with a subjective survey that, *ceteris paribus*, immigrants are more exposed to painful and tiring occupations than natives.

The fact that foreign-born individuals are more likely to have bad employment conditions reflects the prevalence of heterogeneous behaviors:[15] immigrants are more willing to accept lower wages and harder working conditions than native workers. Some justifications for this conjecture lie in the fact that immigrants tend to have lower outside options compared to similar natives, with both lower reservation wages and bargaining power. On the one hand, Constant, Krause, Rinne, et Zimmermann (2010) provide evidence for Germany of an increase in the reservation wage (*i.e.* the crucial wage above which an individual is willing to accept job offers) from first- to second-generation migrants (the latter belonging to the native population). Changing frames of reference from one migrant generation to the next are identified as a potential channel through which this phenomenon may arise. Moreover, the eligibility to social welfare benefits that ensures a minimum income (or "social minima") is limited for immigrants in France (Math, 2011).[16] This eligibility condition may also affect their reservation wage negatively.

On the other hand, the bargaining power on the labor market is very likely to be lower for immigrants. First, the probability of finding a job is lower for migrants due to limited access to the labor market (as in the United States; see Bratsberg, Ragan, et Nasir (2002)). Due to legal reasons, access to a number of jobs in the public sector requires the possession of the French citizenship. In this regard, Math et Spire (1999) have documented that immigrants have access to only 70%

[15]It is also likely that a component of the unexplained wage differentials between workers may be related to discrimination, or racial disadvantage.

[16]Although five years of residence are required since 2003, the eligibility to "social minima" required three years over the period 1990-2002.

Table 1: Immigrant-Native Employment Condition Disparities (1990-2002)

| | Dependent Variable | | | | |
|---|---|---|---|---|---|
| | Log Hourly Wage | | Night Work | Late Hours | Weekend |
| Immigrants | -0.02*** | -0.03*** | 0.04** | 0.08*** | 0.08*** |
| | (-8.13) | (-11.92) | (2.24) | (5.12) | (6.42) |
| Inverse Mills Ratio | 0.02*** | - | -0.03*** | -0.04*** | 0.00 |
| | (14.09) | | (-3.25) | (-4.07) | (-0.66) |
| Adj. R-squared | 0.36 | 0.40 | 0.36 | 0.30 | 0.28 |
| Observations | 336,599 | 349,462 | 372,713 | 282,995 | 380,089 |
| Control Variables | | | | | |
| Human Capital | Yes | Yes | Yes | Yes | Yes |
| Job Characteristics | Yes | Yes | Yes | Yes | Yes |
| Family Characteristics | - | - | Yes | Yes | Yes |
| Occupation Dummies | Yes | Yes | Yes | Yes | Yes |
| Region Dummies | Yes | Yes | Yes | Yes | Yes |
| Time Dummies | Yes | Yes | Yes | Yes | Yes |
| Estimation Procedures | | | | | |
| Heckman | Yes | No | Yes | Yes | Yes |
| Tobit Estimation | No | Yes | - | - | - |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** Estimations are conducted on full-time and part-time male workers who have between 1 and 40 years of experience. On the *right-hand side*, the dependent variables are dummies equal to one when the employee works at night, at late hours or on the weekend and to 0 otherwise. Both parts of the table include the same regressors except for the *right-hand side* which contains an additional set of variables related to family characteristics: the number of children and the marital status. Human capital control variables include schooling, experience and its square. Job characteristics contain the job tenure, part-time, long-term contract and public sector dummies, as well as two additional dummies indicating if an employee works at night and on the weekend. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

of all available jobs in the economy. Second, conditions to renew a work permit or obtain French citizenship strongly require a job to attest to a high level of social and economic assimilation.[17]

In addition, a sociological work (Sayad, 1999) underlines the fact that immigrants are forced into a sort of "social hyper-correctness" which makes them less inclined to complain about their condition. This can be viewed as an alternative motivation to understand why immigrants are willing to endure worse employment conditions than any native would agree to.

As a result, natives and immigrants tend to be dissimilar in terms of labor market behaviors. Since immigrants have poorer outside options, they are more willing to accept both lower wages and harder working conditions compared to natives. Consequently, immigrants should be relatively more attractive for firms.[18] This should lead immigration to strongly increase the labor market competition between workers and depress the outcomes of competing natives. In particular, within a framework of wage rigidities, a strong displacement effect may arise after an influx of migrants.

## 4.2 Estimation of the Immigration Impact

Table 2 reports the estimates of coefficient $\beta$ for the main sample and various specifications. Since all the regressions are based on annual variations, the estimates capture the short-run effects of immigration. Having data from 1990 to 2002, setting $j = 3$ (education groups) and $k = 8$ (experience groups), the estimates of Table 2 are based on a perfectly balanced sample of 312 observations.

Table 2 is duplicated in the appendix for the two alternative samples (Table 12 & table 13). Tables 12 and 13 respectively provide estimates from a balanced sample of 156 (3 education groups$\times$4 experience groups$\times$13 years) and 312 (6 education groups$\times$4 experience groups$\times$13 years) observations. As mentioned above, four dependent variables are used: the log monthly wage (column 1), the log hourly wage (column 2), the log employment rate to population (column 3) and the log employment rate to labor force (column 4). As in Borjas (2003), regressions are weighted by the number of male natives used to calculate $y_{jkt}$.

The estimates reported in Tables 2, 12 & 13 show a robust adverse impact of immigrant flows on the labor market employment of natives, but not on their wages. First, this indicates that the immigration impact on native wages hinges on labor market rigidities. Secondly, the estimates report evidence of a strong displacement effect. This corroborates the idea that immigrants and

[17] See Fougere et Safi (2009) for detailed information on the French citizenship acquisition.

[18] The relative attractiveness of immigrants is supported by Sa (2011). She shows that immigrants are less likely to be unionized, less informed about the employment protection legislation, and less likely to claim their rights.

Table 2: The Impact of the Immigrant Share on Native Outcomes (Baseline Sample)

| **Specification** | Dependent Variable | | | |
|---|---|---|---|---|
| | Monthly Wage | Hourly Wage | Employment Rate to Population | Employment Rate to Labor Force |
| **1.** Baseline Regression | -0.41 | -0.40 | -0.36** | -0.32** |
| | (-0.90) | (-0.90) | (-2.57) | (-2.73) |
| **2.** Unweighted Regression | -0.52 | -0.47 | -0.46** | -0.34** |
| | (-1.12) | (-0.99) | (-2.61) | (-2.55) |
| **3.** Include Log of Natives as Regressor | -0.42 | -0.40 | -0.34** | -0.31** |
| | (-0.89) | (-0.86) | (-2.50) | (-2.65) |
| **4.** Experience $\in ]5; 35]$ | -0.02 | -0.00 | -0.32* | -0.29* |
| | (-0.05) | (0.00) | (-1.76) | (-1.89) |
| **5.** $t = 6$ | -0.55 | -0.51 | -0.36* | -0.34* |
| | (-0.98) | (-0.93) | (-1.86) | (-1.90) |
| **6.** High-Skilled | -0.03 | -0.56 | -0.16 | -0.16 |
| | (-0.05) | (-0.79) | (-1.10) | (-1.58) |
| **7.** Medium- and Low-Skilled | -0.66 | -0.68 | -0.35* | -0.32* |
| | (-1.02) | (-1.10) | (-2.08) | (-2.00) |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the coefficient of the immigrant share variable from OLS regressions where the dependent variables represent a measure for native outcomes. The first group of outcomes captures male native wages (columns 1 & 2), whereas the second group measures their labor market opportunities (columns 3 & 4). These variables are computed for each education-experience group at time $t$ which composed the baseline sample (3 education groups$\times$8 experience groups$\times$13 years). Except for specification 6, all regressions include education, experience, and period fixed effects, as well as interactions between education and experience fixed effects, education and period fixed effects, and experience and period fixed effects. *Upper part*: there are 312 observations for each specification, except for the $4^{th}$ and $5^{th}$ where there are respectively 234 and 144 observations. *Bottom part*: there are respectively 104 and 208 observations for specifications 6 and 7. Unless otherwise specified, each regression is weighted by the number of male natives used to compute the dependent variable. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

natives are dissimilar and heterogeneous in terms of behavior – *i.e.* migrants are more willing to accept lower wages and harder working conditions.

The first specification (row 1) reports the baseline estimates of $\beta$. For the three samples, the estimated coefficients are not significant when the dependent variables capture the level of wages.

Conversely, the estimated impact of immigration on the employment rates of natives is significantly negative. This negative effect is even more significant when the employment rate to labor force is used. This suggests that the share of immigrants has a very limited impact on the participation rate of natives: immigration does not discourage natives from seeking a job.

The estimates from the first specification (column 4) reported in Tables 2, 12 & 13 respectively imply that a 10% rise in the immigrant labor supply decreases the native employment rate to labor force by 2.7% (0.32*0.84), 5.9% (0.70*0.84) and 3.8% (0.44*0.84).[19] Notice that both alternative samples indicate a stronger negative impact on native employment. The effect of the immigrant share even doubles from Table 2 (3 education groups×8 experience groups×13 years) to Table 12 (3 education groups×4 experience groups×13 years). Such a fluctuation from one sample to another is due to measurement errors. A large number of cells causes an attenuation bias which becomes exponentially worse as the size of the sample used to compute the immigrant share declines (Aydemir et Borjas, 2011). Finally, the negative effect on native employment persists even when the sample with six education groups is used. This illustrates that the displacement mechanism is not driven by an educational downgrading among immigrants.

The remaining rows of the tables conduct several robustness tests to determine the sensitivity of the baseline result to alternative specifications. In the second specification, the estimated coefficients come from regressions which are not weighted by the sample size used to compute $y_{jkt}$. The third row addresses the problem that differences in the immigrant supply shock $p_{jkt}$ over time may be either due to a positive change in the number of migrants, or to a negative change in the number of native workers occupying an education-experience cell. In order to control for the fact that the evolution of the immigrant share can also be driven by the native labor supply, I therefore include the log of the number of natives in the workforce as an additional regressor.

Both wage and employment levels of the youngest and the oldest workers are strongly volatile from one year to another due to measurement errors (Tables 6, 7, 8 and 9 in the appendix). Thus, I run regressions without the first and last experience groups (specification 4). Notice that the estimated coefficients are no longer significant for the tables in the appendix. This is explained by the fact that specification 4 is not suitable for the samples with four experience groups: when two middle experience groups out of four are excluded, the number of cells, and therefore observations, is mechanically halved.

Finally, specification 5 removes the year 1990 and merges the following pairs of years: 1991/1992,

[19] In order to convert $\hat{\beta}$ to an elasticity, it has to be multiplied by $1/\left(1+m_{jkt}\right)^2$ with $m_{jkt} = M_{jkt}/N_{jkt}$. The mean value of the relative number of immigrants $m$ is about 9.1% over the period. Hence, $\hat{\beta}$ needs to be multiplied by 0.84 $\left(1/\left(1+0.091\right)^2\right)$. See Borjas (2003) for further details and a formal derivation.

1993/1994, 1995/1996, 1997/1998, 1999/2000 and 2001/2002. This leads to a substantial increase in the sample size per skill-cell, attenuating measurement errors (Aydemir et Borjas, 2011). If the variance tends to be higher, the results in row 5 suggest that the previous estimates may not be affected by a potential attenuation bias.

The last two rows provide estimates from regressions within schooling categories, so as to determine whether the results are being driven by particular groups. One consideration motivates this disaggregation: the willingness to accept worse employment conditions might be lower among highly skilled immigrants, compared to medium- and low-skilled ones. Specifications 6 and 7 respectively investigate the immigration impact within the high schooling group and within both the medium & low ones.[20] However, the estimates from specifications 6 and 7 must be interpreted with caution. First, the number of observations to run regressions declined dramatically. Second, for the samples with three education groups (Tables 2 & 12), specification 6 cannot control for changes in the return to experience or education over time.

The results still do not provide robust evidence of any detrimental effect on native wages. Moreover, the estimates tend to show that highly educated immigrants do not depress the employment of comparably skilled natives. Conversely, they indicate that the sample of medium- and low-educated immigrants is the group that is driving much of the analysis.[21] These results suggest that *(i)* the displacement mechanism tends to operate only within the group of medium- and low-educated individuals and *(ii)* high-skilled immigrants are relatively less attractive for firms.

All in all, the share of immigrants, and therefore immigration, does not affect the wages of competing native workers, but induces adverse employment effects. Already discussed, the potential endogenous selection of migrants into skill-cells would lead to upward biased estimates of $\beta$. Therefore, if this bias was addressed, the aforementioned conclusions drawn would be strengthened: the negative effect on native employment should be even stronger, implying a much more powerful displacement mechanism.

# 5 The Sources of Labor Market Rigidities

The estimates from Table 2 indicate that labor market rigidities play an important part in shaping the immigration impact on native outcomes. When wages cannot react due to labor supply shocks, the level of (un)employment tends to adjust. In order to examine whether this

[20] More precisely, Tables 2 & 12 (samples with three education groups) report the estimated impact of immigration within the highest education group (specification 6) and the two lowest (specification 7), while Table 13 (sample with six education groups) focuses on the two highest (specification 6) and the four lowest (specification 7).

[21] The fact that low-skilled native workers tend to be the most affected is consistent with Borjas (2003). He shows that immigration has reduced US wages, particularly for low-skilled natives.

finding is robust, this section focuses on the sources of wage rigidities. This investigation requires to find a subsample of native workers for which wages can be manipulated by the employers. While the average immigration impact on wages is insignificant, this specific sample should therefore experience wage losses due to immigration.

An important source of wage rigidities may lie in the type of employment contract (short-term/long-term). In France, there are two main types of contracts: the CDI, *Contrat à Durée Indeterminée* (long-term contract) and the CDD, *Contrat à Durée Determinée* (short-term contract). While the CDI is a permanent job contract with no end-date, the CDD is a fixed-term contract for a specific duration of employment.[22] Employers can easily manipulate the wages of CDD workers after each renewal, either for the same employee (within the limits of two renewals) or different ones. This possibility is supported by the fact that CDD workers are very likely to be less protected by labor unions and less eligible for unemployment benefits. Hence, contrary to workers who have long-term contracts, CDD workers are very likely to experience wage fluctuations after labor supply shocks. Immigration should therefore decrease the wages of workers with short-term contracts. Conversely, CDI workers should be protected from a decrease in their wages.

Table 3 provides the estimated immigration impact on the outcomes of native workers who have a CDD (*left-hand side*) and a CDI (*right-hand side*) for the baseline sample. Two dependent variables are used: the log monthly wage, the log hourly wage. The variable of interest $p_{jkt}$ is identical to the one used previously.

The *upper part* of the table uses the main specifications from Table 2. As expected, an immigration supply shift lowers the wages of competing native workers with a CDD. The baseline estimate implies that a 10% increase in supply reduces the monthly wages[23] of native workers with a short-term contract by 25%. This magnitude is higher than Borjas's findings for the United States, where he shows a wage adjustment by 3% to 4%. Two major considerations can explained this sizeable gap and the high magnitude of our estimated coefficients. First, our disaggregation induces a huge decline in the number of observations used to compute the dependent variables for the CDD workers. As Aydemir et Borjas (2011) pointed out, this induces sampling error and introduces some noise in the measure of wages. Second, native workers with short-term contracts tend to experience huge wage variations from one year to another by around 20%.

Although the estimated wage effect on CDD workers is probably not well identified, our results

[22] Over the 1990-2002 period, 9% of native workers had a CDD and out of these 9%, 80% had a contract duration of less than 1 year. A short-term contract (traditional CDD or temporary job contract) may be used to replace an employee who is absent, to cover changes in business activity or for seasonal work.

[23] The fact that hourly wages seem less sensitive to immigration suggests that migrant flows may not affect the number of hours worked by natives.

Table 3: The Immigration Impact on the wages of Natives with Short and Long-term Contracts

| | Short-Term Contract Workers | | Long-Term Contract Workers | |
|---|---|---|---|---|
| **Specification** | Monthly Wage | Hourly Wage | Monthly Wage | Hourly Wage |
| **1.** Baseline Regression | -3.21** | -2.19 | -0.17 | -0.23 |
| | (-2.42) | (-1.57) | (-0.38) | (-0.50) |
| **2.** Unweighted Regression | -3.54* | -2.47 | -0.35 | -0.40 |
| | (-1.95) | (-1.59) | (-0.76) | (-0.81) |
| **3.** Include Log of Natives | -3.38** | -2.24* | -0.17 | -0.22 |
| as Regressor | (-2.77) | (-1.87) | (-0.37) | (-0.47) |
| **4.** Experience $\in ]5; 35]$ | -4.43* | -3.52 | 0.25 | 0.19 |
| | (-2.08) | (-1.55) | (0.50) | (0.37) |
| **5.** $t = 6$ | -3.37* | -2.01 | -0.34 | -0.39 |
| | (-1.99) | (-1.06) | (-0.56) | (-0.61) |
| **6.** Experience $> 10$ | -5.70*** | -5.00** | 0.11 | 0.15 |
| | (-3.29) | (-2.88) | (0.22) | (0.30) |
| **7.** Experience $> 10$ & | -6.42*** | -5.83*** | -0.08 | -0.03 |
| Private Sector Only | (-5.58) | (-3.79) | (-0.13) | (-0.06) |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the estimated impact of immigration on the wages of native workers who have short-term contracts (*left-hand side*) and long-term contracts (*right-hand side*). *Upper part*: while there are 312 observations for specifications 1, 2 & 3, there are respectively 234 and 144 observations for specifications 4 and 5. *Bottom part*: there are 234 observations for specifications 6 and 7. Unless otherwise specified, each regression is weighted by the number of male natives used to compute the dependent variable. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

(presented in Tables 2 & 3) indicate that CDD native workers experience wage losses due to immigration. This result is supported by the *right-hand side* of the Table 3. It rather shows that immigration does not affect the wages of CDI workers. Having a permanent contract therefore protects from any downward wage pressure.

Specifications 5 and 6 remove from the sample the native workers who tend to be greatly affected by two other sources of wage rigidities: the high level of the minimum wage and the prominence of the public sector.[24] Specification 5 thus focuses on cells with more than 10 years of experience,

[24]The public sector might be much less competitive than the private one. Over the period, the average share of

excluding the groups most affected by the minimum wage. In addition, row 6 eliminates public workers from the sample. As expected, the estimated immigration impact on the wages of native workers who have a CDD is much more negative and becomes strongly significant. Moreover, the insensitivity of wages to immigration is even more striking for the natives with long-term contracts.

All in all, the estimates are perfectly consistent with section 4.2. When wages can be manipulated by the employers, immigration causes wage losses. These results also indicate that the type of job contract is a main determinant of wage rigidity.

# 6 Migrant Heterogeneity & Native Employment

The baseline results (section 4.2) report evidence of a detrimental average impact of immigration on native employment. Until now, this paper have considered all migrants as a homogeneous population. But are they all the same?

Actually, immigrants are very likely to be heterogeneous with respect to their nationality in terms of outside options. Naturalized migrants, in particular, should have both higher reservation wages and more bargaining power. First, immigrants who obtain the French citizenship are henceforth treated in the same way as native-born citizens in terms of the law. Actually, naturalized immigrants are eligible to all the social benefits, they no longer have constraints to renew their work permits and they fill all the requirements to have access to public jobs. Also, naturalization may foster their feeling of integration and/or modify immigrant beliefs and perceptions. Hence, naturalized immigrants are likely to have reference standards which are very close to those of natives (Constant, Krause, Rinne, et Zimmermann, 2010) and might no longer be forced into a sort of "social hyper-correctness" (Sayad, 1999).

With higher outside options, naturalized immigrants may tend to adjust their behaviors to those of natives.[25] Therefore, they should be less attractive for firms than non-naturalized immigrants. As a result, if the aforementioned displacement effect is indeed due to outside option (and cultural) differences between natives and immigrants, the naturalized immigrants should have a very limited impact on native employment. Conversely, the negative immigration impact on employment should be exclusively driven by non-naturalized immigrants. In order to investigate this strong implication, I thus divide the share of migrants between the share of naturalized immigrants $p^{ned}_{jkt}$ on the one

native workers in the public sector was around 20%.

[25] In line with this idea, Bratsberg, Ragan, et Nasir (2002) for the United States and Steinhardt (2012) for Germany demonstrate an immediate positive naturalization effect on wages and an accelerated wage growth in the years after the naturalization event. Similarly, Coutrot et Waltisperger (2009) also find evidence that the work conditions of naturalized immigrants tend to be closer to those of natives.

hand and that of non-naturalized immigrants $p_{jkt}^{non-ned}$ on the other hand.[26]

However, the estimated impact of $p_{jkt}^{ned}$ on native employment may prove to be spurious because of a selection problem. Indeed, the sample of naturalized immigrants is likely to be selected among the immigrant population based on certain specific characteristics (Bratsberg, Ragan, et Nasir, 2002; DeVoretz et Pivnenko, 2005). Immigrants who happen to be naturalized may be different than the others in terms of education level, experience, occupation, region of residence, etc. Consequently, it might be that if the naturalized immigrants do not affect native employment, this is not because they have similar outside options to natives, but because of these specific characteristics. In order to address this potential problem, the study requires to compare the impact on native employment of two immigrant groups which are identical except for their citizenship status (naturalized/non-naturalized). In effect, a different estimated impact of these two groups on native employment would indicate that citizenship matters in shaping the labor market immigration impact.

In order to find a group of naturalized immigrants which differs from the non-naturalized ones only in their citizenship, I use the propensity score matching (PSM) method.[27] It allows to decompose the naturalized population into two subsamples: the naturalized individuals who are very unlikely to be selected among the immigrant population (NS) and the naturalized individuals who should be (S). Here, the first group of naturalized immigrants (NS) should be similar to the non-naturalized immigrants in terms of education level, experience, occupation, etc., except for the citizenship status. Then, so as to estimate (and compare) the impact of these different immigrant groups on native employment, I compute the following immigrant shares : $p_{jkt}^{non-ned}$, $\left(p_{jkt}^{ned}\right)_{NS}$ and $\left(p_{jkt}^{ned}\right)_{S}$.[28]

An identification problem due to measurement errors might still affect the estimates. As the immigrant population is divided into groups, the number of observations per cell tends to decrease. Yet, this may lead to an attenuation bias due to sampling error in the measure of the immigrant

[26] $p_{jkt}^{ned} = \left(M_{jkt}^{ned}/\left(M_{jkt}+N_{jkt}\right)\right)$ and $p_{jkt}^{non-ned} = \left(M_{jkt}^{non-ned}/\left(M_{jkt}+N_{jkt}\right)\right)$, with $M_{jkt}^{ned}$ and $M_{jkt}^{non-ned}$ respectively the number of naturalized immigrants and non-naturalized immigrants in the cell $j$,$k$,$t$. Table 11 in the appendix reports the share of naturalized and non-naturalized individuals in the immigrant labor force over time (1990-2002). It shows a sufficient variation of the number of naturalized non-naturalized immigrants to identify the impact of each share on native employment.

[27] The implementation of the PSM is detailed thoroughly in the appendix. First, I describe the PSM procedure and detail the variables used to compute the probability to be naturalized among immigrants. Then, I implement the matching procedure with some tests for the matching quality. Finally, I explain why the usual limitations inherent to the PSM technique are unlikely to bias our results.

[28] Over the period, the number of immigrants is around 38,000, while there were 9,000 naturalized and 29,000 non-naturalized individuals. Here, the PSM implementation leads to splitting the naturalized immigrants into 4,200 and 4,800 individuals to compute $\left(p_{jkt}^{ned}\right)_{NS}$ and $\left(p_{jkt}^{ned}\right)_{S}$.

Table 4: The Impact of Naturalized and non-Naturalized Immigrants on Native Employment

| | Raw Estimates (1st Set of Regressions) | | Estimates after Matching (2nd Set of Regressions) | | |
|---|---|---|---|---|---|
| **Specification** | $p_{jkt}^{non-ned}$ | $p_{jkt}^{ned}$ | $p_{jkt}^{non-ned}$ | $\left(p_{jkt}^{ned}\right)_{NS}$ | $\left(p_{jkt}^{ned}\right)_{S}$ |
| **1.** Baseline Regression | -0.35*** | -0.02 | -0.35*** | 0.00 | -0.04 |
| | (-2.86) | (-0.07) | (-2.87) | (0.01) | (-0.09) |
| **2.** Unweighted Regression | -0.40*** | 0.08 | -0.40*** | 0.13 | 0.04 |
| | (-2.89) | (0.23) | (-2.86) | (0.31) | (0.09) |
| **3.** Include Log of Natives | -0.34*** | 0.04 | -0.34*** | 0.00 | 0.08 |
| as Regressor | (-2.86) | (0.13) | (-2.92) | (0.00) | (0.17) |
| **4.** Experience $\in ]5;35]$ | -0.33** | 0.23 | -0.34** | 0.08 | 0.32 |
| | (0.88) | (-2.11) | (-2.17) | (0.19) | (0.67) |
| **5.** $t = 6$ | -0.37** | 0.01 | -0.36** | 0.14 | -0.12 |
| | (-2.11) | (-0.01) | (-2.11) | (0.18) | (-0.21) |
| **6.** $t > 1992$ | -0.34** | -0.16 | -0.35** | -0.26 | -0.08 |
| | (-2.08) | (-0.42) | (-2.10) | (-0.54) | (-0.16) |
| **7.** $t = 4$ | -0.53** | -0.05 | -0.51** | 0.34 | -0.32 |
| | (-2.25) | (-0.06) | (-2.16) | (0.28) | (-0.39) |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the estimated impact of immigration on the native employment rate to labor force. Each line provides the estimates from two OLS regressions, one before (*left-hand side*) and one after (*right-hand side*) the matching procedure. *Upper part*: while there are 312 observations for specifications 1and 2, there are respectively 234 and 144 observations for specifications 3 and 4. *Middle part*: there are respectively 240 and 144 for specifications 5 and 6. *Bottom part*: there are respectively 156 and 312 observations for specifications 7 and 8. Unless otherwise specified, each regression is weighted by the number of male natives used to compute the dependent variable. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

supply shift (Aydemir et Borjas, 2011). In order to increase the number of naturalized immigrants per cell, and *in fine* limit the attenuation bias, two specifications are introduced. Since the number of naturalized immigrants increased significantly after 1992 (Table 11), specification 5 is added to focus on an alternative time span from 1993 to 2002. The second specification (row 6) removes the year 1990 and transforms the time span into four periods.[29]

[29] I merge the following years: 1991/1992/1993, 1994/1995/1996, 1997/1998/1999, 2000/2001/2003.

Table 4 shows the estimated impact of immigration on the native employment rate to labor force by decomposing the effect of naturalized and non-naturalized immigrants. It is divided into two parts: the *left-hand side* provides the raw estimates (with the raw immigrant shares: $p_{jkt}^{non-ned}$ and $p_{jkt}^{ned}$), while the *right-hand side* shows the estimates after the implementation of the matching procedure.

Firstly, all the estimates find that native employment is completely insensitive to the share of naturalized immigrants. Even the second set of estimates (*right-hand side*) indicates that immigrants who differ from the non-naturalized individuals only in their citizenship do not hurt native employment. Secondly, compared to the baseline estimates (section 4.2), the negative effect of $p_{jkt}^{non-ned}$ on native employment is slightly higher and much more significant. This indicates that the adverse impact of immigration is completely attributable to the presence of the non-naturalized immigrants.[30]

The competition is only heightened between workers with different outside options (here, between natives and non-naturalized immigrants). Consequently, native workers are only displaced by the non-naturalized immigrants.

# 7 Conclusion

This paper presents new evidence on the question of how immigration can decrease the outcomes of competing native workers. The prevalence of heterogeneous behaviors between natives and immigrants is identified as a major rationale for these effects. Immigrants are more willing to accept both lower wages and harder working conditions. For France, I have shown that while immigration has no impact on the wages of competing native workers, it causes adverse employment effects. The dissimilarities between natives and immigrants therefore lead employers to replace native workers.

The richness of the French data allows to go beyond these average effects. First, I provide evidence that the type of employment contract is an important source of wage rigidities. Contrary to workers with long-term contracts, those with short-term contracts tend to experience strong wage cuts due to immigration. Secondly, the analysis shows that immigrants who acquire the French citizenship do not have any adverse impact on native employment. Conversely, the detrimental employment effect of immigration is completely driven by the presence of non-naturalized immigrants. These results indicate that when migrants and natives share similar outside options, and *in fine* similar behaviors, immigration no longer affects native employment.

This last result has strong policy implications. Economic policies that affect the outside options

[30] More generally, all the previous estimates presented in Tables 2, 12 & 13 are robust to the decomposition naturalized/non-naturalized.

of immigrants should play a role in shaping the immigration impact on native outcomes. More specifically, economic policies implemented to protect natives, by limiting the access for immigrants to the labor market and social benefits, will tend to decrease their outside options and therefore prove to be counter-productive – *i.e.* in disfavor of natives. While they aim at protecting natives, these restrictions actually exacerbate the competition between immigrants and natives, and, in the end, enhance the negative effect of immigration on the level of outcomes. As a result, a way to attenuate the negative (partial) immigration impact on native outcomes would be to increase the outside options of immigrants by fostering their economic and cultural assimilation.

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# Appendices

## A Elasticity of Substitution between Natives and Immigrants

Within the context of the multi-level CES framework introduced by Borjas (2003), Ottaviano et Peri (2008, 2012) derive an empirical test of imperfect substitution between comparably skilled immigrants and natives. They look at the ratio of the wages of immigrants and natives, corresponding in a competitive market to their relative marginal productivities:

$$log\left(\frac{w^M_{jkt}}{w^N_{jkt}}\right) = log\left(\frac{\theta^M_{jkt}}{\theta^N_{jkt}}\right) - \frac{1}{\sigma_I}\cdot log\left(\frac{M_{jkt}}{N_{jkt}}\right), \tag{3}$$

where $w^M_{jkt}$ and $w^N_{jkt}$ gives respectively the real average wage of immigrants and natives in a particular skill cell with educational attainment $j$, experience level $k$, and observed in calendar year $t$. On the *right-hand side*, the first and second terms respectively capture the log relative immigrant-native productivity and the log relative number of immigrants.The parameter of interest is $\sigma_I$, namely the elasticity of substitution between immigrant and native workers.

Equation (3) can be estimated by replacing the relative productivity term by a vector of fixed effects and adding an error term. In the literature, one of the main debated point concerns the identification assumption on the relative productivity term. Ottaviano et Peri (2008) assume this term to be invariant over time. Thus, they use an interaction term between education and experience fixed effects. In their study for Germany, D'Amuri, Ottaviano, et Peri (2010) conversely use education, experience and time dummies to control for any systematic component of the relative efficiency parameter.

The present paper estimate the following econometric equations by using a comprehensive set of vector of fixed effects $\delta_{jkt}$:

$$log\left(\frac{w^M_{jkt}}{w^N_{jkt}}\right) = \delta_{jkt} - \frac{1}{\sigma_I}\cdot log\left(\frac{M_{jkt}}{N_{jkt}}\right) + \xi_{jkt}, \tag{4}$$

In order to estimate $1/\sigma_I$, I build three samples with different structures of education-experience cells.[31] The baseline sample combines three educational categories and eight experience groups (each spanning an interval of 5 years). The two alternative samples make up four experience

[31]See section 3.2.2 for detailed information on the selected samples.

Table 5: Estimates of the Substitution Elasticity Between Natives and Immigrants

| | Estimates of $1/\sigma_I$: | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **1.** Baseline Regression | -0.00 | -0.03 | -0.05 | -0.00 | -0.03 | 0.09 |
| | (-0.07) | (-0.90) | (-1.01) | (-0.07) | (-0.34) | (0.84) |
| **2.** Monthly Wage | -0.00 | -0.04 | -0.06 | -0.00 | -0.03 | 0.09 |
| | (-0.07) | (-1.02) | (-1.18) | (-0.07) | (-0.38) | (0.83) |
| **3.** Sample [$3 \times 4 \times 13$] | -0.06 | -0.09 | -0.12 | -0.06 | -0.12 | -0.11 |
| | (-0.78) | (-1.46) | (-1.64) | (-0.78) | (-1.25) | (-0.81) |
| **4.** Sample [$6 \times 4 \times 13$] | -0.03 | -0.04 | -0.07 | -0.03 | -0.10 | -0.06 |
| | (-0.60) | (-0.93) | (-1.36) | (-0.60) | (-1.20) | (-0.52) |
| $\delta_j$ (education dummies) | No | Yes | Yes | Yes | No | Yes |
| $\delta_k$ (experience dummies) | No | Yes | Yes | Yes | No | Yes |
| $\delta_t$ (time dummies) | No | No | Yes | No | Yes | Yes |
| $\delta_j \times \delta_k$ | Yes | No | No | Yes | Yes | Yes |
| $\delta_j \times \delta_t$ | No | No | No | No | No | Yes |
| $\delta_k \times \delta_t$ | No | No | No | No | No | Yes |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the estimates of the substitution elasticity between natives and immigrants. The main dependent variable is the relative log hourly average wage. The explanatory variable is the relative number of workers in each cell. For the specifications 1 and 2, we use the baseline sample (3 education groups×8 experience groups×13 years) which numbers 312 observations. For the two alternative samples (specifications 3 & 4), there are respectively 156 (3 education groups×4 experience groups×13 years) and 312 (6 education groups×4 experience groups×13 years) observations. Fixed effects are progressively added to test the sensitivity of our results. In order to estimate $1/\sigma_I$, we weight each regression by the total number of workers in a skill-cell. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

groups (each spanning an interval of 10 years), but one of them contain three education classes while the other contain six. Then, I follow most empirical studies and restrict my attention on men aged from 16 to 64, who are not enrolled at school, who are not self-employed (farmers and entrepreneurs), and have between 1 and 40 years of labor-market experience.

Table 5 reports the estimated values of $1/\sigma_I$ for various specifications including an increasing set of control dummies. The dependent variable is the relative log hourly average wage between groups of workers. As Ottaviano et Peri (2008, 2012), each regression uses the total number of observations used to calculate average wages as analytical weights. Standard errors are heteroscedasticity-robust and clustered around education-experience groups.

While the first specification uses the relative log hourly wage as dependent variable, the second specification uses the relative log monthly average wage as an alternative. The two last specifications test the sensitivity of the baseline estimates to different structure of education-experience cells. The specification 3 uses the sample combining 12 cells per year with three education levels, whereas the specification 4 combines 24 cells per year with six education level.

The results are unambiguous: the estimated coefficients are almost never significantly different from zero. In line with Borjas, Grogger, et Hanson (2012), this indicates that immigrants and natives are perfect substitutes in the production process.

# B Descriptive Statistics

Table 6: Average Monthly Wage of Full-Time Male Native Workers by Skill-Cell *(Constant Euros)*

| Level of Education | Years of Experience | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|---|
| High Level | 1 – 5 | 1511.9 | 1580.6 | 1951.0 | 1529.3 | 1659.9 |
| | 6 – 10 | 1702.5 | 2220.0 | 2189.7 | 1948.8 | 2035.0 |
| | 11 – 15 | 1989.8 | 2452.1 | 2820.6 | 2453.6 | 2440.7 |
| | 16 – 20 | 2142.1 | 2489.0 | 2530.2 | 2714.1 | 2880.9 |
| | 21 – 25 | 2492.1 | 2630.3 | 3102.1 | 2684.5 | 3000.2 |
| | 26 – 30 | 2584.6 | 2761.6 | 2916.0 | 2988.5 | 3137.1 |
| | 31 – 35 | 2565.7 | 3075.4 | 2980.1 | 3037.5 | 3407.8 |
| | 36 – 40 | 2661.7 | 3034.2 | 4516.5 | 3426.6 | 3373.9 |
| Medium Level | 1 – 5 | 849.4 | 1196.4 | 1127.3 | 1006.6 | 1110.7 |
| | 6 – 10 | 974.8 | 1133.6 | 1220.0 | 1164.6 | 1279.6 |
| | 11 – 15 | 1151.8 | 1330.2 | 1578.7 | 1296.4 | 1405.9 |
| | 16 – 20 | 1301.1 | 1467.3 | 1367.1 | 1440.0 | 1508.2 |
| | 21 – 25 | 1426.4 | 1532.4 | 1543.9 | 1565.1 | 1651.7 |
| | 26 – 30 | 1550.4 | 1588.7 | 1963.0 | 1643.9 | 1739.0 |
| | 31 – 35 | 1496.9 | 1683.0 | 1952.2 | 1776.6 | 1797.3 |
| | 36 – 40 | 1480.9 | 1887.4 | 2056.5 | 1775.1 | 1869.6 |
| Low Level | 1 – 5 | 714.2 | 826.8 | 808.8 | 858.3 | 963.5 |
| | 6 – 10 | 844.9 | 983.1 | 1017.2 | 1053.6 | 1115.6 |
| | 11 – 15 | 982.9 | 1101.9 | 1282.7 | 1140.2 | 1214.5 |
| | 16 – 20 | 1100.9 | 1202.8 | 1238.3 | 1246.3 | 1327.7 |
| | 21 – 25 | 1156.9 | 1299.5 | 1434.1 | 1319.8 | 1407.4 |
| | 26 – 30 | 1204.9 | 1364.6 | 1374.6 | 1423.6 | 1521.4 |
| | 31 – 35 | 1252.8 | 1665.8 | 1429.9 | 1517.8 | 1541.7 |
| | 36 – 40 | 1213.3 | 1408.7 | 1492.4 | 1523.4 | 1608.5 |
| Observations | | 25,312 | 26,383 | 26,775 | 26,392 | 26,852 |

Table 7: Average Hourly Wage of Male Native Workers by Skill-Cell *(Constant Euros)*

| Level of Education | Years of Experience | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|---|
| High Level | 1 – 5 | 8.9 | 9.3 | 11.5 | 9.3 | 10.6 |
| | 6 – 10 | 9.8 | 12.6 | 12.4 | 11.4 | 12.5 |
| | 11 – 15 | 11.6 | 13.8 | 15.4 | 14.0 | 14.5 |
| | 16 – 20 | 12.8 | 14.0 | 14.0 | 15.1 | 16.5 |
| | 21 – 25 | 14.3 | 15.0 | 16.9 | 15.2 | 16.8 |
| | 26 – 30 | 15.2 | 15.6 | 16.6 | 17.0 | 18.0 |
| | 31 – 35 | 14.6 | 16.8 | 16.4 | 17.7 | 19.5 |
| | 36 – 40 | 14.5 | 17.2 | 25.2 | 19.2 | 19.5 |
| Medium Level | 1 – 5 | 5.2 | 7.2 | 6.9 | 6.2 | 7.3 |
| | 6 – 10 | 5.9 | 6.8 | 7.3 | 7.2 | 8.3 |
| | 11 – 15 | 6.9 | 8.0 | 9.6 | 7.8 | 9.0 |
| | 16 – 20 | 7.9 | 8.8 | 8.3 | 8.7 | 9.7 |
| | 21 – 25 | 8.6 | 9.2 | 9.4 | 9.5 | 10.5 |
| | 26 – 30 | 9.4 | 9.5 | 11.9 | 10.0 | 11.2 |
| | 31 – 35 | 9.1 | 10.2 | 11.7 | 10.9 | 11.5 |
| | 36 – 40 | 9.0 | 11.7 | 12.3 | 10.8 | 12.0 |
| Low Level | 1 – 5 | 4.4 | 5.1 | 4.9 | 5.4 | 6.5 |
| | 6 – 10 | 5.2 | 6.0 | 6.2 | 6.6 | 7.4 |
| | 11 – 15 | 6.0 | 6.8 | 7.8 | 7.0 | 8.0 |
| | 16 – 20 | 6.7 | 7.3 | 7.4 | 7.7 | 8.7 |
| | 21 – 25 | 7.0 | 7.9 | 8.8 | 8.2 | 9.2 |
| | 26 – 30 | 7.2 | 8.3 | 8.3 | 8.8 | 9.9 |
| | 31 – 35 | 7.6 | 10.1 | 8.7 | 9.3 | 10.1 |
| | 36 – 40 | 7.5 | 8.5 | 9.1 | 9.4 | 10.4 |
| Observations | | 25,994 | 27,125 | 27,572 | 27,168 | 27,515 |

Table 8: Employment Rate to Population of Full-Time Male Native Workers by Skill-Cell *(%)*

| Level of Education | Years of Experience | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|---|
| High Level | 1 – 5 | 84.6 | 78.1 | 74.0 | 78.9 | 82.3 |
| | 6 – 10 | 92.8 | 87.1 | 89.5 | 90.2 | 90.3 |
| | 11 – 15 | 92.7 | 91.1 | 90.8 | 90.5 | 91.5 |
| | 16 – 20 | 95.9 | 89.1 | 91.1 | 90.4 | 90.5 |
| | 21 – 25 | 95.9 | 91.6 | 92.6 | 92.2 | 92.8 |
| | 26 – 30 | 91.6 | 90.7 | 90.6 | 89.0 | 91.4 |
| | 31 – 35 | 81.8 | 79.4 | 79.5 | 78.9 | 76.7 |
| | 36 – 40 | 53.9 | 50.0 | 47.3 | 43.7 | 48.1 |
| Medium Level | 1 – 5 | 73.7 | 62.8 | 61.3 | 61.7 | 72.7 |
| | 6 – 10 | 88.1 | 84.5 | 81.2 | 81.5 | 86.3 |
| | 11 – 15 | 91.4 | 89.3 | 86.9 | 86.0 | 89.2 |
| | 16 – 20 | 93.5 | 89.2 | 88.5 | 89.5 | 90.8 |
| | 21 – 25 | 93.2 | 90.9 | 89.7 | 88.3 | 89.6 |
| | 26 – 30 | 92.8 | 90.7 | 88.8 | 88.4 | 88.8 |
| | 31 – 35 | 88.0 | 84.3 | 83.6 | 84.4 | 87.6 |
| | 36 – 40 | 71.3 | 72.1 | 66.9 | 68.7 | 68.6 |
| Low Level | 1 – 5 | 38.5 | 34.0 | 29.4 | 29.0 | 36.7 |
| | 6 – 10 | 72.4 | 65.5 | 64.5 | 58.6 | 61.6 |
| | 11 – 15 | 82.5 | 75.7 | 70.1 | 68.7 | 73.0 |
| | 16 – 20 | 85.4 | 81.8 | 78.1 | 73.0 | 76.4 |
| | 21 – 25 | 86.1 | 82.3 | 80.3 | 78.5 | 79.6 |
| | 26 – 30 | 85.2 | 80.7 | 81.1 | 77.1 | 80.6 |
| | 31 – 35 | 84.7 | 79.9 | 77.7 | 77.5 | 77.2 |
| | 36 – 40 | 74.7 | 71.5 | 70.4 | 68.6 | 68.1 |
| Observations | | 26,060 | 27,216 | 27,650 | 27,200 | 27,552 |

Table 9: Employment Rate to Labor Force of Full-Time Male Native Workers by Skill-Cell *(%)*

| Level of Education | Years of Experience | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|---|
| High Level | 1 – 5 | 91.0 | 84.4 | 79.2 | 84.7 | 86.2 |
| | 6 – 10 | 96.9 | 92.3 | 93.5 | 93.1 | 93.5 |
| | 11 – 15 | 95.4 | 94.4 | 93.9 | 93.2 | 93.8 |
| | 16 – 20 | 98.3 | 92.4 | 93.9 | 93.8 | 92.2 |
| | 21 – 25 | 97.3 | 95.2 | 94.8 | 93.6 | 94.4 |
| | 26 – 30 | 94.9 | 94.7 | 92.7 | 93.8 | 94.2 |
| | 31 – 35 | 92.6 | 90.7 | 89.8 | 90.3 | 87. 9 |
| | 36 – 40 | 87.2 | 90.7 | 81.8 | 84.7 | 87.1 |
| Medium Level | 1 – 5 | 81.5 | 70.6 | 71.1 | 70.3 | 81.6 |
| | 6 – 10 | 91.0 | 87.1 | 84.5 | 84.9 | 89.4 |
| | 11 – 15 | 93.0 | 91.1 | 89.2 | 88.4 | 91.8 |
| | 16 – 20 | 95.1 | 91.5 | 90.3 | 91.5 | 93.1 |
| | 21 – 25 | 95.0 | 93.1 | 92.1 | 90.9 | 92.0 |
| | 26 – 30 | 95.2 | 93.7 | 91.6 | 92.2 | 92.9 |
| | 31 – 35 | 94.3 | 92.0 | 89.5 | 90.7 | 94.3 |
| | 36 – 40 | 90.7 | 92.3 | 85.9 | 86.4 | 88.4 |
| Low Level | 1 – 5 | 59.0 | 51.9 | 46.6 | 45.8 | 56.0 |
| | 6 – 10 | 77.8 | 72.4 | 70.1 | 64.2 | 70.2 |
| | 11 – 15 | 86.3 | 81.5 | 75.0 | 74.3 | 80.2 |
| | 16 – 20 | 89.9 | 86.0 | 82.6 | 78.8 | 83.0 |
| | 21 – 25 | 91.5 | 87.7 | 85.2 | 84.6 | 87.4 |
| | 26 – 30 | 91.0 | 89.0 | 86.3 | 84.7 | 88.9 |
| | 31 – 35 | 93.1 | 88.7 | 86.8 | 85.4 | 88.8 |
| | 36 – 40 | 91.4 | 87.2 | 85.1 | 84.5 | 87.0 |
| Observations | | 26,060 | 27,216 | 27,650 | 27,200 | 27,552 |

Table 10: Distribution of Male Individuals in the Labor Force by Level of Education and Year

| Level of Education | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|
| | | | **A. Natives** | | |
| High Level | 17.3 % | 19.9 % | 21.6 % | 24.0 % | 26.4 % |
| Medium Level | 45.4 % | 46.1 % | 46.3 % | 46.8 % | 47.3 % |
| Low Level | 37.3 % | 34.0 % | 32.1 % | 29.2 % | 26.3 % |
| Total | 100 % | 100 % | 100 % | 100 % | 100 % |
| | | | **B. Immigrants** | | |
| High Level | 9.7 % | 14.4 % | 17.6 % | 19.0 % | 20.4 % |
| Medium Level | 23.5 % | 24.4 % | 26.6 % | 30.0 % | 30.8 % |
| Low Level | 66.9 % | 61.1 % | 55.7 % | 50.1 % | 48.8 % |
| Total | 100 % | 100 % | 100 % | 100 % | 100 % |

Table 11: Distribution of Male Immigrants in the Labor Force by Nationality and Year

| | 1990 | 1993 | 1996 | 1999 | 2002 |
|---|---|---|---|---|---|
| Naturalized Immigrants | 6.5 % | 19.5 % | 24.9 % | 29.0 % | 31.7 % |
| Non-Naturalized Immigrants | 93.5 % | 80.5 % | 75.1 % | 71.0 % | 68.3 % |
| Total | 100 % | 100 % | 100 % | 100 % | 100 % |

# C Alternative OLS Estimates

Table 12: Impact of the Immigrant Share on Native Outcomes [$3 \times 4 \times 13$]

| | Dependent Variable | | | |
|---|---|---|---|---|
| **Specification** | Monthly Wage | Hourly Wage | Employment Rate to Population | Employment Rate to Labor Force |
| **1.** Baseline Regression | -0.38 | -0.35 | -0.62** | -0.70*** |
| | (-0.62) | (-0.57) | (-2.72) | (-4.52) |
| **2.** Unweighted Regression | -0.11 | -0.06 | -0.59 | -0.70** |
| | (-0.14) | (-0.07) | (-1.37) | (-2.49) |
| **3.** Include Log of Natives | -0.35 | -0.33 | -0.63** | -0.70*** |
| as Regressor | (-0.71) | (-0.61) | (-2.76) | (-4.52) |
| **4.** Experience $\in ]10;30]$ | 0.68 | 1.00 | -1.04 | -0.75 |
| | (0.53) | (0.69) | (-1.68) | (-1.51) |
| **5.** $t = 6$ | -1.16 | -1.08 | -0.52* | -0.67*** |
| | (-1.49) | (-1.33) | (-1.89) | (-3.32) |
| **6.** High-Skilled | 0.70 | -0.10 | -0.40 | -0.39 |
| | (1.51) | (-0.18) | (-0.92) | (-1.92) |
| **7.** Medium- and Low- | -1.03* | -1.09* | -0.50** | -0.64*** |
| Skilled | (-2.34) | (-2.16) | (-2.38) | (-4.20) |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the coefficient of the immigrant share variable from OLS regressions where the dependent variables represent a measure for native outcomes. The first group of outcomes captures male native wages (columns 1 & 2), whereas the second group measures their labor market opportunities (columns 3 & 4). These variables are computed for each education-experience group at time $t$ which composed the baseline sample (3 education groups $\times$ 4 experience groups $\times$ 13 years). Except for specification 6, all regressions include education, experience, and period fixed effects, as well as interactions between education and experience fixed effects, education and period fixed effects, and experience and period fixed effects. *Upper part*: there are 156 observations for each specification, except for the $4^{th}$ and $5^{th}$ where there are respectively 78 and 72 observations. *Bottom part*: there are respectively 52 and 104 observations for specifications 6 and 7. Unless otherwise specified, each regression is weighted by the number of male natives used to compute the dependent variable. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

Table 13: Impact of the Immigrant Share on Native Outcomes [$6 \times 4 \times 13$]

| | Dependent Variable | | | |
|---|---|---|---|---|
| **Specification** | Monthly Wage | Hourly Wage | Employment Rate to Population | Employment Rate to Labor Force |
| **1.** Baseline Regression | -0.19<br>(-0.38) | -0.14<br>(-0.28) | -0.45**<br>(-2.60) | -0.45***<br>(-3.04) |
| **2.** Unweighted Regression | -0.68<br>(-1.23) | -0.68<br>(-1.16) | -0.32<br>(-1.63) | -0.32*<br>(-1.96) |
| **3.** Include Log of Natives as Regressor | -0.40<br>(-0.72) | -0.34<br>(-0.61) | -0.28<br>(-1.67) | -0.37**<br>(-2.52) |
| **4.** Experience $\in ]10;30]$ | 1.36**<br>(2.78) | 1.77***<br>(3.71) | -0.45<br>(-1.45) | -0.37<br>(-1.42) |
| **5.** $t = 6$ | -0.49<br>(-0.79) | -0.35<br>(-0.61) | -0.51*<br>(-1.96) | -0.55***<br>(-3.03) |
| **6.** High-Skilled | -1.03<br>(-1.40) | -1.25<br>(-1.61) | 0.10<br>(0.62) | -0.06<br>(-0.64) |
| **7.** Medium- and Low-Skilled | -0.38<br>(-0.71) | -0.40<br>(-0.74) | -0.46**<br>(-2.41) | -0.41**<br>(-2.72) |

**Key.** ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.
**Notes.** The table reports the coefficient of the immigrant share variable from OLS regressions where the dependent variables represent a measure for native outcomes. The first group of outcomes captures male native wages (columns 1 & 2), whereas the second group measures their labor market opportunities (columns 3 & 4). These variables are computed for each education-experience group at time $t$ which composed the baseline sample (6 education groups $\times$ 4 experience groups $\times$ 13 years). Except for specification 6, all regressions include education, experience, and period fixed effects, as well as interactions between education and experience fixed effects, education and period fixed effects, and experience and period fixed effects. *Upper part*: there are 312 observations for each specification, except for the $4^{th}$ and $5^{th}$ where there are respectively 156 and 144 observations. *Bottom part*: there are respectively 104 and 208 observations for specifications 6 and 7. Unless otherwise specified, each regression is weighted by the number of male natives used to compute the dependent variable. Standard errors are adjusted for clustering within education-experience cells. t-statistics in parentheses are derived from heteroscedastic-consistent estimates of the standard errors.

# D Propensity Score Matching Procedure

The estimates of the impact of naturalized immigrants on native employment could potentially be biased due to systematic differences between the naturalized and non-naturalized groups. In order to address the selection problem into citizenship acquisition, a matching procedure is implemented. By excluding the naturalized individuals who are too dissimilar to non-naturalized immigrants, this procedure aims at creating two homogeneous groups which differ only in their citizenship.

## D.1 Propensity Score Estimation

The first step of PSM techniques is to estimate the probability of being naturalized for each immigrant (the so-called propensity score), which I do using a binary probit model and a vector of covariates $x$ to capture the most relevant differences between naturalized and non-naturalized immigrants. The sample used is the pooled cross-section from 1990 to 2002. The propensity score is computed from the following equation:

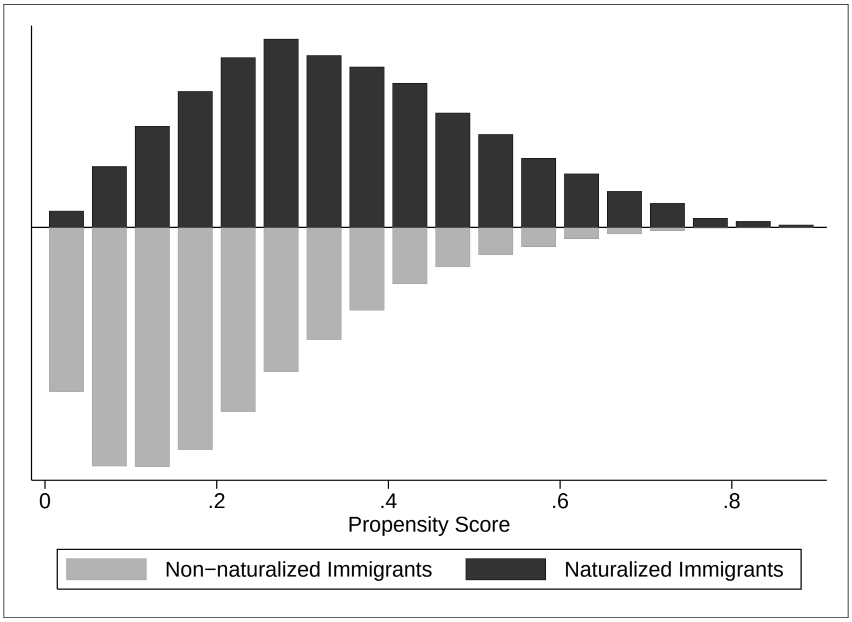
$$\mathbb{P}\left(N_{icfrt}=1\right)=\Phi\left(\rho_0+\rho_1 H_i+\rho_2 E_i+\rho_3 F_i+\rho_4 P_{rt}+\zeta_c+\zeta_f+\zeta_r+\zeta_t+\xi_{icfrt}\right) ,$$

where $\Phi(\cdot)$ is the cumulative normal distribution. The dependent variable $N_{icfrt}$ is a dummy variable indicating whether the immigrant $i$ is naturalized ($N_{icfrt}=1$) or is still non-naturalized ($N_{icfrt}=0$). The term $H_i$ is a vector of control variables containing human capital characteristics of individual $i$ like education, labor market experience and its square. The dummy $E_i$ indicates whether the individual is employed or unemployed. The family characteristics are captured by the vector $F_i$ which contains the number of children in the household and a dummy variable indicating whether the individual is single or not. Moreover, the naturalization decision may be influenced by the overall number of naturalized and non-naturalized immigrants in the region of residence (Fougere et Safi, 2009). Hence, both shares of naturalized and non-naturalized immigrants in population $P_{rt}$ are included. Occupational category dummies $\zeta_c$ are also added to capture specific effects related to the 30 broad job categories[32]. The term $\zeta_f$ is a vector of fixed effects containing the occupational categories of each individual's father. Finally, regional and time dummies are added since the naturalization decision may only concern a specific region and year.

In this respect, the estimated propensity score $e(x)$, is the conditional probability of being naturalized given the covariates; that is $e(x)=\mathbb{P}(N=1|x)$. Naturalized and non-naturalized immigrants selected to have the same $e(x)$ value will have the same distributions of $x$. Exact

[32] For unemployed individuals, the survey gives the last occupational category.

**Figure 2: Propensity Score Distribution among both Naturalized and non-Naturalized Immigrants**




**Notes.** The population used is men participating in the labor force aged from 16 to 64, not enrolled at school and having between 1 and 40 years of labor market experience. Self-employed people are excluded from the sample.

matching on $e(x)$ will, therefore, tend to balance the $x$ distributions in the two groups.

The propensity score distribution obtained from the probit estimation is represented in Figure 2. It indicates that the propensity score distribution differs considerably between the two groups of immigrants. As expected, it shows that non-naturalized (naturalized) immigrants exhibit a lower (higher) probability to be naturalized. The propensity score intervals of naturalized and non-naturalized immigrants lie respectively within the intervals $[0.005 - 0.890]$ and $[0.001 - 0.863]$. Hence, the common support (based on the MinMax criterion which consists in discarding all observations outside the common support region from the analysis) is given by $[0.001 - 0.890]$, resulting in a loss of six naturalized immigrants (over 8,578) and 53 non-naturalized immigrants (over 26,927).

## D.2 Matching Process

The second step is to implement a matching procedure to select the non-naturalized immigrants whose propensity scores are closest to those of naturalized immigrants. To do so, I use the most straightforward matching estimator: the nearest neighbor matching with replacement.[33] In this respect, an individual from the non-naturalized group is chosen as a matching partner for a naturalized immigrant who is the closest in terms of propensity score.

Since I do not condition on all the covariates, but on the propensity score, it is necessary to check whether the matching procedure can balance the distribution of the relevant variables in both groups of immigrants. The basic idea of all these approaches is to compare the situation before and after matching and to check whether some differences remain after conditioning on the propensity score. One suitable indicator to assess the distance in the marginal distributions of the covariates is the standardized bias[34] suggested by Rosenbaum et Rubin (1985). The standardized bias measure results show that the difference in the propensity score of unmatched immigrants is close to 7.5%. After matching, the bias significantly decreases and is equal to 1.2%. Although there is no clear indication of the success of the matching procedure, in most empirical studies a bias reduction below 3% or 5% is considered as sufficient (Caliendo et Kopeinig, 2008). In addition, the insignificant likelihood ratio tests and the very low pseudo R-squared (0.004) support the hypothesis that both groups have the same covariate distribution after matching. All these results therefore suggest that the sole difference between the two groups of immigrants lies in the fact that one of them is composed of individuals who have been naturalized.

However, the matching procedure was not completely successful for certain matching pairs, so that all the relevant differences between the two groups of immigrants may not have been captured by the covariates. Consequently, I restrict the subsample of matched individuals, by excluding the naturalized individuals matched with a propensity score distance higher than the mean distance. This leads to disregarding half of the 8,578 naturalized immigrants. Thus, I keep only the naturalized individuals who are strictly similar to the non-naturalized in the probability of being naturalized. In the end, the two groups of immigrants are identical: the only major difference between them is citizenship.

## D.3 Potential Limitations

Two limitations are generally raised to challenge the matching procedure. First, in the case at hand, the PSM technique assumes that never accepting bad employment conditions – *i.e.* the insensitivity of native employment to the presence of naturalized immigrants – and selection are

[33] Figure 2 suggests that the nearest neighbor matching algorithm without replacement would create poor matches due to the high-score individuals from the naturalized population, who would likely get matched to low-score individuals from the non-naturalized one. Therefore, the nearest neighbor matching is used with replacement so as to ensure the smallest propensity score distance between the naturalized and non-naturalized individuals.

[34] For each covariate, it is defined as the difference of sample means in the two groups as a percentage of the square root of the average of sample variances in both groups.

independent conditionally on the covariates (the conditional independence assumption). To put it differently, the decision to be naturalized should be random conditionally on the covariates. Yet, inasmuch the major variables influencing the selection are observed, the assumption that the insensitivity of native employment and selection are independent conditionally on these observables is plausible. However, it is impossible to certify that the naturalization decision is not due to unobservable variables which might be correlated with their willingness to accept bad employment conditions.[35]

In addition to independence, all individuals in both groups must be able to participate in all states to fill the common support condition. In this analysis, the number of observations deleted because of the common support requirement across different subsamples is low, so that this hypothesis tends to be satisfied.

Still, the insensitivity of native employment to $p^{ned}_{jkt}$ (Table 4) may be due to a positive correlation between naturalization and the level of integration. In this respect, the share of naturalized immigrants is rather likely to reflect the relative size of well-integrated immigrants. Hence, the relevant distinction among immigrants should be between well-integrated (old arrivals) and poorly integrated (recent arrivals) ones. Unfortunately, this cannot be investigated since the immigrants' year of arrival is not provided by the data. However, notice that the distinction between naturalized and non-naturalized individuals seems more accurate since naturalization gives individuals rights which are similar to those of natives. In this respect, naturalized immigrants should exhibit outside options closer to those of natives than well-integrated immigrants do. Furthermore, the possibility that the insensitivity of native employment is only driven by the fact that naturalized immigrants are better integrated is truly consistent with my explanations regarding the causes of the displacement mechanism. In fact, since well-integrated immigrants are likely to exhibit outside options and behaviors close to those of natives, employers may still not have any incentive to replace them.

[35]For instance, I do not include the country of birth to estimate the propensity score, whereas this variable may affect the naturalization decision and the willingness to accept or not bad employment conditions. Actually, this variable was not included since it is missing for 50% of the sampled naturalized immigrants. Nevertheless, notice that even when a vector of fixed effects for the country of origin is included in the propensity score equation, the econometric results (Table 4) remain unchanged.