

Monopsony, Efficiency, and the Regularization of Undocumented Immigrants

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Abstract

In May 1981, President François Mitterrand regularized the status of undocumented immigrant workers in France. The newly legalized immigrants represented 12 percent of the non-French workforce and about 1 percent of all workers. Employers have monopsony power over undocumented workers because the undocumented may find it costly to participate in the open labor market and have restricted economic opportunities. By alleviating this labor market imperfection, a regularization program can move the market closer to the efficient competitive equilibrium and potentially increase employment and wages for both the newly legalized and the authorized workforce. Our empirical analysis reveals that the Mitterrand regularization program particularly increased employment and wages for low-skill native and immigrant men, and raised per-capita GDP by about 0.5 percent.

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Monopsony, Efficiency, and the Regularization of Undocumented Immigrants

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1. Introduction

Large numbers of undocumented immigrants reside in many industrialized countries. Over 10.5 million live in the United States, making up 23 percent of the foreign-born population and about 3 percent of the total population (Lopez, Passel, and Cohn, 2021). Similarly, between 4 and 5 million live in Europe (mainly in Germany, the United Kingdom, Italy, and France), making up nearly 20 percent of the foreign-born population and almost 1 percent of the total population (Connor and Passel, 2019).¹

The presence of a sizable undocumented population triggers economic shocks central to the debate over immigration policy—along with the inevitable questions of what to do about the current stock of undocumented immigrants and what can be done to halt the continuing inflow. Several countries addressed the question of what to do about the stock of undocumented immigrants by declaring amnesties that regularize their status. In the United States, the 1986 Immigration Reform and Control Act (IRCA) granted amnesty to 2.7 million undocumented persons and increased penalties for firms that hired undocumented workers. Other well-known regularization programs include the 2002 amnesty in Italy, which conditioned eligibility on being continuously employed during the three months prior to application (Devillanova, Fasani, and Frattini, 2018), and the 2005 amnesty in Spain, which required a job contract with an employer for at least six months to be eligible (Elias, Monras, and Vázquez-Grenno, 2024).

This paper uses the largest amnesty program implemented in French history to document how the regularization of undocumented workers affects labor market outcomes for *all* workers as well as aggregate output. On July 23, 1981, the newly elected government of President François Mitterrand proposed to regularize the status of undocumented workers who had entered the country prior to January 1, 1981, and had a work contract valid for at least a year. The program regularized 131,360

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¹ The “foreign-born” population in Europe includes only those persons born in countries outside the European Union or the European Free Trade Association.

immigrants. The regularized workers were predominantly male, low-skill, and lived disproportionately in the Île-de-France (Paris) region. They comprised 12 percent of the immigrant workforce, 2 percent of all workers in Paris, and nearly 1 percent of all workers in France.

The early studies that examined amnesty programs documented how IRCA affected the labor market outcomes of the newly regularized immigrants in the United States, typically finding higher earnings and job turnover rates, but ambiguous effects on employment rates (Borjas and Tienda, 1993; Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Amuedo-Dorantes and Bansak, 2011; and Pan, 2012).²

A few studies examine if the labor market effects of an amnesty spill over to *other* workers. Cobb-Clark and Kossoudji (1995) report that IRCA had a positive (but small) impact on the wage of manufacturing workers. Di Porto, Martino, and Naticchioni (2018) and Carrozo (2022) find that the 2002 Italian regularization program did not affect the wage of authorized workers (and provide mixed evidence on employment effects). In contrast, Elias, Monras, and Vázquez-Grenno (2024) document that the 2005 regularization in Spain did not affect native formal employment, but increased their wage. Finally, Chassambouli and Peri (2015) and Amior and Manning (2021) use search and monopsonistic models, respectively, to simulate the impact of regularization policies, and conclude that such policies are economically beneficial for natives.³

An important idea permeates the literature: Because of their irregular status, undocumented immigrants face restricted job opportunities and different labor market conditions than authorized workers. As the early discussion in Rivera-Batiz (1999, p. 96) noted: “Illegality allows employers to exert monopsonistic power over these workers because of their great fear of being reported to immigration authorities, which would lead to immediate deportation.”

We develop a theoretical framework where profit-maximizing monopsonistic firms combine the inputs of high-skill workers, low-skill authorized workers (both

² Amuedo-Dorantes, Malo, and Muñoz-Bullón (2013), Devillanova, Fasani and Frattini (2018), and Bahar, Ibàñez, and Rozo (2021) confirm these findings by examining amnesty programs in Spain, Italy, and Colombia, respectively.

³ The simulation results reported in Edwards and Ortega (2017), Peri and Zaiour (2021), and Ortega and Hsin (2017) show that legalizing undocumented workers would lead to a significant increase in U.S. gross domestic product (mostly due to productivity gains). Machado (2017) also simulates the impact of a legalization policy on the labor market and the social security system and concludes that the overall impact of such a policy on the welfare of natives is limited.

natives and legal immigrants), and low-skill undocumented immigrants. We use this model to examine the impact of an amnesty program on the wage and employment of *all* groups. Monopsony power in the undocumented labor market introduces an economic inefficiency, reducing the number of undocumented workers hired. We show that this inefficiency spills over to other sectors of the labor market, curtailing the hiring of all authorized workers below what would otherwise be optimal.⁴

A regularization program that reduces monopsony power in the undocumented labor market has two important consequences. First, it moderates the inefficiency, leading to an increase in the employment of undocumented workers. Second, the expansion may spill over to the labor market for authorized workers, increasing employment and wages as well. By reducing monopsony power in the undocumented labor market, a regularization program improves labor market efficiency and can generate a substantial increase in output, a “regularization surplus.” Our framework isolates the crucial characteristic of the labor market that would necessarily produce such a surplus: Firms must have some monopsony power over undocumented workers.

Our empirical analysis of the 1981 French amnesty exploits the geographic concentration of the regularized workforce in Paris to identify the impact on the employment and wages of natives, legal immigrants, and undocumented persons. We generally find positive effects for many groups, but particularly so for the male, low-skill workforce that included most of the regularized immigrants. The amnesty increased the employment rate of low-skilled French men in the Paris region by around 4 percentage points, and increased the wage of low-skilled French men by 3 to 5 percent.

We also estimate the aggregate impact of the regularization using data on regional per-capita GDP in France. Regularization increased per-capita GDP in Paris by 1.6 percent, implying an increase in French per-capita GDP of about 0.5 percent. This regularization surplus represents a *permanent* increase in aggregate income as it resulted from the fact that the regularization program moderated an existing inefficiency in the French labor market. This empirical estimate of the surplus coincides with the simulation estimate produced by a textbook supply-and-demand framework,

⁴ In related work, Elias, Monras, and Vázquez-Grenno (2024) also use a monopsony model to examine the impact of amnesty programs. They parameterize regularization as a shift in labor supply from the undocumented group to the authorized group (but hold output constant, thus limiting the scope for spillover effects). The difference between the alternative approaches is discussed further in the Mathematical Appendix.

where we interpret the area under the demand curve as total product and calculate the GDP implied by the expansion in employment induced by the regularization program.

A key insight from our analysis is that undocumented immigration introduces labor market inefficiencies because it increases the monopsony power of firms. These inefficiencies can spill over to other sectors and curtail employment opportunities for other groups. As a result, a regularization program that alleviates or removes this inefficiency can produce substantial economic gains.

2. The French “Exceptional Regularization”

2.1. Historical Context

François Mitterrand was elected the first socialist president of the Fifth Republic on May 10, 1981.⁵ The socialist platform contained 110 policy measures that were to be implemented after the election. None of them mentioned a potential regularization of undocumented immigrants, making it impossible to anticipate the “Exceptional Regularization” which almost immediately followed the presidential election.⁶

On July 23, 1981, the French government proposed to carry out a case-by-case regularization of undocumented immigrant workers (Garson and Moulrier, 1982, p. 18). Instructions for the regularization were described in an interministerial circular issued on August 11, 1981. Undocumented immigrants had to satisfy two main criteria to be eligible: they entered France before January 1, 1981, and they had a work contract valid for at least a year (or other proof of “stable employment”).⁷

⁵ Following the election of President Valéry Giscard d’Estaing in 1974 and the economic crisis caused by the first oil shock, the French government enacted a suspension of all labor immigration. This shift policy shift stabilized the population share of immigrants for more than two decades (it was 7.4 percent in 1975 and 7.3 percent in 1990).

⁶ The Mitterrand economic program also included large-scale nationalization in the industrial and banking sectors, the introduction of a solidarity tax on wealth, a sizable increase in the national minimum wage, the reduction of the workweek from 40 to 39 hours, and the hiring of 200,000 civil servants. The overall program “failed to conform to the Government’s expectations” (Sachs and Wyplosz, 1986, p. 277). By June 1982, the government had announced a spending and wage freeze to combat inflation. Section 6 shows that the labor market reforms (i.e. the rise in public sector employment and the minimum wage, as well as the reduction in the workweek) are not responsible for the observed correlation between employment and regularization.

⁷ As proof of employment, undocumented immigrants could present a letter from their employer, pay stubs (especially if the employee was registered under a different name), bank transfers, or checks. Unions could also assist in the process. Due to the increasing diversity of applications and the difficulty of proving stable employment, two circulars issued on October 22 and November 30, 1981, extended eligibility to interns, domestic and temporary workers, immigrants dismissed for seeking regularization, asylum seekers, pregnant women, and sick immigrants (Cealis et al., 1983, p. 15).

The deadline for applications was initially set for December 31, 1981, but it was eventually extended to February 15, 1982 (Tribalat, 1983, p. 114). Foreign workers applied by personally filing the forms at a designated government office. As soon as the request for regularization was accepted, the immigrant was given a one-year work permit. These permits could be renewed for an additional year, or extended for three years if the newly legalized immigrants had a job (Cealis et al., 1983, p. 15).

Employers were also granted a sort of amnesty until February 25, 1982. Those who cooperated in regularizing their undocumented employees were not prosecuted or forced to pay the arrears in social security contributions, and the fines for employing undocumented immigrants were reduced to 600 francs (90 euros) instead of 2,000 francs (300 euros). Beginning on February 26, 1982, employers faced higher penalties if they hired undocumented immigrants. The prison sentence was increased from 10-30 days to 2-12 months, and the fine was raised to 2,000-20,000 francs (300-3,050 euros). These sanctions would be imposed *each* time the employment of an illegal immigrant was brought to the authorities' attention.⁸

2.2. Socioeconomic characteristics of the regularized immigrants

By June 30, 1983, 149,226 undocumented immigrants had applied for legalization, and 131,360 of them were legalized (French Ministry of Social Affairs, 1984, p. 561). The regularized immigrants represented 11.8 percent of the non-French workforce and about 1 percent of the total workforce. Almost half (45.8 percent) originated in North African countries (Algeria, Morocco, and Tunisia). Portuguese and Turkish immigrants were the next largest groups, composing 12.7 percent and 8.7 percent of the legalized immigrants, respectively.

Figure 1 shows that almost two-thirds (62.9 percent) of the legalized immigrants resided in the Île-de-France (Paris) region.⁹ In contrast, only 14.7 percent resided in the

⁸ After the unexpected election of Socialist Prime Minister Lionel Jospin in June 1997, a second regularization policy was enacted. It regularized 62,702 undocumented immigrants but rejected the applications of 45,982. Most regularizations (76.2 percent) were approved based on family ties. Given its smaller scale and specific target population, the impact of this policy is not examined in this paper.

⁹ The regional distribution is based on a sample of 109,012 new legalized immigrants, which represent 83 percent of all regularizations (French Ministry of Social Affairs, 1984). The sample excludes 14,567 Algerian immigrants, 6,581 seasonal workers, and about 1,200 retail traders whose applications were also accepted during the amnesty program. According to a survey conducted by the French Ministry of Social Affairs (1984, p. 574), more than 80 percent of Algerian immigrants lived in the Paris region at the time of their legalization.

Marseille region and fewer than 5 percent resided in any of the other remaining regions. The uneven spatial distribution likely reflects the different economic performance of regions, as well as the settlement patterns of earlier immigrant waves. The geographic cluster of the legalized population in the Paris region is used as an identification strategy in the empirical analysis presented below.

To ascertain the socioeconomic characteristics of the legalized immigrants, the French Ministry of Social Affairs (1984) interviewed a sample of 8,938 legalized immigrants between October 1981 and July 1982. The survey found that 95.3 percent of the immigrants were employed when they were legalized. Most of them had been in France fewer than five years: 70 percent arrived after 1977.

The legalized immigrants were mostly men (82.5 percent). They were also very young: 80 percent were below age 32 and 17 percent were below age 22. The age distribution explains why 60 percent were not married and 64 percent had no children. Most of the legalized workers were low-skilled, employed in blue-collar occupations as unskilled industrial or craft workers (e.g., in the construction sector), agricultural workers, shop employees, or personal service workers (e.g., in the hotel and restaurant industry, or domestic services). Although the survey did not collect information on educational attainment, the large share of legalized immigrants employed in low-skilled occupations likely reflects their low education level.

To measure the relative size of the “supply shock” produced by the regularization program on the low-skilled segment of the local labor market, we divide the number of the regularized immigrants by the size of the low-skill male French workforce for each region in 1982.¹⁰ Figure 2 shows that the regularized immigrants represented 8.0 percent of the low-skill male French workforce in the Paris region, with the share falling by half for the Marseille region. The figure also shows that the regularization program had only a minor relative impact in the remaining regions.

2.3. Economic integration of the regularized immigrants

The literature suggests that the regularization of undocumented workers should expand their economic opportunities (e.g., Amuedo-Dorantes and Bansak, 2011; Deiana, Giua and Nisticò, 2022; Devillanova, Fasani, and Frattini, 2018; Kossoudji and Cobb-

¹⁰ The low-skill category refers to individuals having a primary education, which represented 54.8 percent of the working-age population in the 1982 French census.

Clark, 2002; and Pan, 2012). There exists only one article (Marie, 1984) documenting the economic integration of the new regularized immigrants in France, based on a representative sample of 3,200 regularized immigrants in the Paris region in December 1983. Those data, however, are no longer available for analysis.

Three conclusions emerged from the study. First, the new regularized immigrants did not encounter any impediments integrating into the French labor market. Their unemployment rate roughly two years after regularization (13 percent) was lower than the unemployment rate of immigrants originating outside the European Economic Community (16.5 percent). Moreover, the share of legalized immigrants who were employed remained high at 87 percent.

Second, the legalization policy had limited impact on occupational mobility. The immigrants had similar occupation distributions before and after regularization (Marie, 1984, p. 23). For instance, 15 percent of the sample worked in the hotel and restaurant industry before the legalization of their status, and the share remained at 14.9 percent after regularization. Similarly, 21.1 percent worked in manufacturing before the regularization, and the share was 21.8 percent after.

Finally, as Table 1 shows, the monthly wage of the regularized workers rose significantly. The fraction earning over 4,000 francs more than doubled from 11 percent to 25 percent, while the fraction earning less than 3,000 francs fell from 44 percent to 15 percent. The rise in the monthly wage can most likely be attributed to the minimum wage that employers must now pay the newly legalized workers, and to a decline in the monopsony power of firms.

3. Theory

The production technology uses three inputs: high-skill workers (L_H), low-skill workers authorized to work (L_A), and low-skill undocumented immigrants (L_U). Natives and legal immigrants make up the low-skill authorized workforce. The concave linear homogeneous production function is:

$$Q = f(L_H, L_A, L_U). \quad (1)$$

Production complementarities among the inputs may play a role in determining the impact of regularizing undocumented immigrants. The production function in (1) avoids building in complementarities through functional form assumptions. Instead, our

results follow from the concavity and linear homogeneity properties. Concavity implies that the Hessian matrix \mathbf{H} of the production function is negative semidefinite, while linear homogeneity implies that the matrix has rank $N - 1$, where N is the number of inputs.¹¹ It follows that $|\mathbf{H}| = 0$ and:

$$f_{ii} < 0 \quad \text{and} \quad \begin{vmatrix} f_{ii} & f_{ij} \\ f_{ji} & f_{jj} \end{vmatrix} > 0, \quad (2)$$

where $f_{ij} = \partial^2 Q / \partial L_i \partial L_j$. All inputs have diminishing marginal product ($f_{ii} < 0$), and all second-order principal minors are positive ($f_{ii}f_{jj} - f_{ij}^2 > 0$).

The production function in (1) assumes that low-skill native and low-skill legal immigrant workers are perfect substitutes. The Mathematical Appendix shows that potential complementarities between those two groups play only a minor role in the analysis. Although the generalization increases algebraic complexity, it does not alter any of the key insights.

The literature recognizes that undocumented immigrants face different labor market conditions than authorized workers because the undocumented have restricted job opportunities (Amior and Manning, 2021; Amuedo-Dorantes and Bansak, 2011; Elias, Monras, and Vázquez-Grenno, 2024; and the related work of Naidu, Nyarko, and Wang, 2016). It may be costly for them to participate in the open labor market and rent their skills to the highest-paying employer. Such exposure could lead to deportation.

There is heterogeneity within the undocumented population in how they perceive the cost of such detection. For some, the chance of getting caught and the cost of exposure may be relatively low. For others, the cost may be very high if, for example, the detection impacts the economic and social opportunities of family members. An undocumented worker may not quit his current job even if the employer were to cut the wage slightly, as entering the open labor market risks exposure to the authorities. Firms may also have to increase the wage if they wish to bring more undocumented immigrants “out of the shadows.” The fact that firms have a somewhat captive audience in the undocumented workforce and face an upward-sloping supply curve if they wish to

¹¹ We assume that the production function in (1) yields a unique solution to the canonical output-constrained profit-maximization problem. Barten, Kloeck, and Lempers (1969) show that the rank of the Hessian of such a production function must be at least $N - 1$. If the function is linear homogeneous, however, the Hessian is singular and cannot have rank N , so its rank must equal $N - 1$.

hire more undocumented immigrants is a key source of monopsony power in the undocumented labor market.

We derive our model in the general case where there may be some degree of monopsonistic competition for all types of workers. The supply function for group i is:

$$L_i = P_i w^{1/\epsilon_i}, \quad i = (H, A, U), \quad (3)$$

where ϵ_i ($\epsilon_i \geq 0$) is the reciprocal of the supply elasticity giving the number of type- i workers willing to work at the firm at a given wage, and measures the firm's monopsony power (Manning, 2003, p. 81); and P_i gives the "baseline" number of workers when the wage equals zero and supply is perfectly inelastic ($\epsilon_i = \infty$). It is convenient to rewrite the supply function in terms of the inverse supply curve:

$$w_i = P_i^{-\epsilon_i} L_i^{\epsilon_i}. \quad (4)$$

Our framework allows for the possibility that employers have market power over all labor inputs, but the firm will have greater monopsony power over undocumented workers (i.e., $\epsilon_U > \epsilon_H$ and $\epsilon_U > \epsilon_A$). In addition to the various factors that may generate upward-sloping supply curves for authorized workers (including imperfect information, heterogeneous worker preferences over job characteristics, and costs of moving across jobs), the undocumented labor market provides an additional reason: heterogeneity in how undocumented workers perceive the cost of detection and deportation.

We initially abstract from payroll taxes and other non-wage hiring costs. The first-order conditions to the profit-maximization problem require that the value of marginal product of type- i workers equals their marginal cost, or:

$$f_i = (1 + \epsilon_i)w_i = (1 + \epsilon_i)P_i^{-\epsilon_i} L_i^{\epsilon_i}. \quad (5)$$

Equation (5) shows the well-known wedge between marginal product and the wage in monopsonistic markets (i.e., $w_i = f_i/(1 + \epsilon_i)$), and this wedge is inversely related to the monopsony power that the firm has over a particular type of labor.

3.1. Regularization as a Reduction in Monopsony Power

A regularization program may affect several parameters in the model, including the extent of monopsony power in the undocumented sector, and impose new costs on

the hiring of the newly legalized workers (such as taxes that were previously unpaid). To isolate the link between regularization and labor market efficiency, we consider a policy that only reduces monopsony power in the undocumented labor market. We parameterize the policy as a decline in the value of the inverse elasticity ϵ_U .

The marginal cost of an undocumented worker ($MC_U = (1 + \epsilon_U)P_U^{-\epsilon_U}L_U^{\epsilon_U}$) is greater the higher the value of the labor supply elasticity:

$$\frac{dMC_U}{d\epsilon_U} = MC_U \left(\log \frac{L_U}{P_U} + \frac{1}{1 + \epsilon_U} \right) > 0. \quad (6)$$

Equation (6) is positive because actual supply L_U exceeds the baseline level P_U for any positive value of the undocumented wage. An increase in ϵ_U , which increases the marginal cost of hiring an undocumented worker, moves the monopsonistic labor market further away from the efficient level of hiring.

For expositional convenience, let R_ϵ denote a regularization policy that *reduces* ϵ_U . We denote the impact of this policy on outcome Y by dY/dR_ϵ (so that $dY/dR_\epsilon = -dY/d\epsilon_U$). It is also convenient to present the theoretical results using the concept of the elasticity of complementarity (Hicks, 1970; Sato and Koizumi, 1973). The elasticity of complementarity between inputs i and j is defined as $c_{ij} = f_{ij}f/f_i f_j$. The sign of c_{ij} indicates if inputs i and j are complements or substitutes (in the sense that an increase in the quantity of one input increases or decreases the marginal product of the other). The Mathematical Appendix shows that:

$$\frac{dL_U}{dR_\epsilon} = \frac{-\kappa_U}{\Delta} \frac{dMC_U}{d\epsilon_U} \left[(c_{HH}c_{AA} - c_{HA}^2) - \frac{1}{\theta_H\theta_A} (\theta_A c_{AA}\epsilon_H + \theta_H c_{HH}\epsilon_A - \epsilon_H\epsilon_A) \right] > 0, \quad (7)$$

where $\kappa_U = (f_H^2 f_A^2 / f^2) > 0$; θ_i is the output share of type- i workers (i.e., $\theta_i = f_i L_i / f$); and Δ is the determinant of the Hessian of the profit-maximization problem:

$$\Delta = \begin{vmatrix} f_{HH} - \epsilon_H f_H L_H^{-1} & f_{HA} & f_{HU} \\ f_{AH} & f_{AA} - \epsilon_A f_A L_A^{-1} & f_{AU} \\ f_{UH} & f_{UA} & f_{UU} - \epsilon_U f_U L_U^{-1} \end{vmatrix} < 0. \quad (8)$$

The second-order conditions require that $\Delta < 0$. Even though the production function is linear homogeneous, it is easy to verify that Δ is negative because the market imperfections introduce strict concavity into the profit function.

Equation (7) implies that a program that reduces the firm's monopsony power over undocumented immigrants (by lowering ϵ_U) increases the employment of such workers, moving the undocumented labor market closer to the efficient competitive level. The increased employment occurs because the regularization program reduces the marginal cost of hiring an undocumented immigrant.

Perhaps more important, this "local" improvement in labor market efficiency spills over to other sectors, increasing the employment of *all* other workers:

$$\frac{dL_H}{dR_\epsilon} = \frac{-\kappa_H}{\Delta} \frac{dMC_U}{d\epsilon_U} \left[\frac{\theta_U}{\theta_H} (c_{AA}c_{UU} - c_{AU}^2) + \frac{c_{HU}}{\theta_A} \epsilon_A \right] > 0, \quad (9)$$

$$\frac{dL_A}{dR_\epsilon} = \frac{-\kappa_A}{\Delta} \frac{dMC_U}{d\epsilon_U} \left[\frac{\theta_U}{\theta_A} (c_{HH}c_{UU} - c_{HU}^2) + \frac{c_{AU}}{\theta_H} \epsilon_H \right] > 0, \quad (10)$$

where $\kappa_H = f_H f_A^2 f_U / f^2 > 0$; and $\kappa_A = f_H^2 f_A f_U / f^2 > 0$. Because second-order principal minors are positive, the derivative dL_i/dR_ϵ will be positive as long as workers in group i ($i = H, A$) are not "strong" substitutes with undocumented immigrants.

In fact, some complementarity between undocumented immigrants and other workers is implied by the underlying technology. A weighted average of elasticities of complementarity must equal zero (Sato and Koizumi, 1973, p. 47):

$$\theta_H c_{HU} + \theta_A c_{AU} + \theta_U c_{UU} = 0. \quad (11)$$

The diminishing marginal product of undocumented workers ($c_{UU} < 0$) means that an average of the elasticities c_{HU} and c_{AU} must be positive. To simplify the exposition, we simply assume that c_{HU} and c_{AU} are both positive. More specifically, the low degree of substitution between undocumented and low-skilled authorized workers could be due to skill differences within the low-skill population (e.g., undocumented workers may have less work experience than low-skilled authorized workers.)

Equations (9) and (10) yield several important insights. First, the employment of both high-skill and low-skill authorized workers increases when firms have less market power over undocumented immigrants regardless of the extent of monopsony power outside the undocumented sector (i.e., regardless of the values of ϵ_H or ϵ_A). Second, the employment of all other workers must increase whenever there exist *any* production complementarities between undocumented immigrants and the other groups. Even the weakest complementarities guarantee that the efficiency gains produced by a reduction

in monopsony power in the undocumented sector spill over to the *entire* labor market. Indeed, while the assumption of complementarity between undocumented and authorized workers is sufficient to explain the positive impact of regularization policies, it is not necessary. A reduction in monopsony power can still lead to increased overall employment, even if labor inputs are slight substitutes. Third, regularization increases output because employment increases for all groups.¹²

The reduction in monopsony power also affects the wage. The upward-sloping supply curves for high-skill and low-skill authorized workers imply:

$$\frac{d \log w_i}{d R_\epsilon} = \epsilon_i \frac{d \log L_i}{d R_\epsilon} > 0, \quad (i = H, A). \quad (12)$$

A policy shift that reduces monopsony power in the undocumented labor market is predicted to not only increase the employment of high-skill and authorized workers but to raise their wage as well. Interestingly, the marginal reduction in monopsony power need not increase the wage of undocumented workers. In particular:

$$\frac{d \log w_U}{d R_\epsilon} = -\log \frac{L_U}{P_U} + \epsilon_U \frac{d \log L_U}{d R_\epsilon}. \quad (13)$$

The first term in (13) is negative because the profit-maximizing number of undocumented workers (L_U) exceeds baseline supply (P_U). The second term is positive because regularization increases the employment of undocumented workers. A reduction in ϵ_U , therefore, produces two conflicting effects. First, firms can offer a lower wage to hire the same (pre-existing) number of undocumented workers. Second, the reduction in monopsony power induces the firm to hire more workers in the undocumented sector, and firms need to raise the wage to attract those additional

¹² The Mathematical Appendix shows that these implications can also be derived in a more general framework that differentiates between natives and immigrants in the low-skill authorized workforce. Because adding a single input doubles the number of cross marginal products, we simplify by considering the nested system:

$$Q = g(L_H, L_D), \quad L_D = h(L_A, L_U), \quad \text{and} \quad L_A = j(L_N, L_M).$$

The top level combines high-skill workers and low-skill workers (L_D) to produce output. The next level defines L_D by combining the efficiency units of authorized low-skill workers and undocumented workers. The bottom level calculates L_A by using low-skill natives (L_N) and low-skill legal immigrants (L_M). The efficiency results discussed in the text carry over to the four-input case if undocumented immigrants are complements with authorized low-skill workers.

workers.¹³ Equation (13) implies that regularization will increase the undocumented wage if ϵ_U is sufficiently large (i.e., the greater the initial level of monopsony power in the undocumented market).

To focus on the efficiency consequences of regularization, we ignored the possibility that regularization may impose new costs on the hiring of undocumented workers. For example, firms must now comply with minimum wage legislation or start paying payroll taxes for those workers. Aggregate all these expenses into a “tax rate” that raises the cost of hiring an undocumented worker by $\tau_U \times 100$ percent, so that $MC_U = (1 + \tau_U)(1 + \epsilon_U)P_U^{-\epsilon_U}L_U^{\epsilon_U}$. A regularization program that *only* increases the tax rate τ_U obviously increases MC_U . Equations (7), (9), and (10) then trivially imply that regularization reduces employment for all groups and reduces output.

Suppose, however, that a regularization program both increases τ_U and lowers the supply elasticity ϵ_U . We can get a sense of the strength of the two opposing effects by examining a program that changes both parameters by the same relative amount. Consider a program $R_{\tau\epsilon}$ that raises the tax rate by k percent and lowers the supply elasticity by k percent. The impact on the marginal cost of an undocumented worker is:

$$\begin{aligned} \frac{dMC_U}{dR_{\tau\epsilon}} &= \frac{dMC_U}{d\log\tau_U} \frac{d\log\tau_U}{dR_{\tau\epsilon}} + \frac{dMC_U}{d\log\epsilon_U} \frac{d\log\epsilon_U}{dR_{\tau\epsilon}} = k \left[\frac{dMC_U}{d\tau_U} \tau_U - \frac{dMC_U}{d\epsilon_U} \epsilon_U \right], \\ &= k MC_U \left[\frac{\tau_U - \epsilon_U}{(1 + \tau_U)(1 + \epsilon_U)} - \epsilon_U \log \frac{L_U}{P_U} \right]. \end{aligned} \quad (14)$$

Equation (14) shows that a sufficient condition for regularization to *reduce* the marginal cost of an undocumented worker is that the supply elasticity be large (i.e., $\epsilon_U > \tau_U$). If firms have substantial monopsony power, a regularization program will have beneficial efficiency consequences even if firms must now pay the taxes and hiring costs they had avoided when the undocumented immigrants were working “off the books.”

It is worth emphasizing that the linear homogeneity assumption for the production function in equation (1) lies at the core of our analysis. It implies that undocumented workers are, on average, complements with other workers. Some recent

¹³ The textbook monopsony model typically compares the monopsony solution (where marginal cost equals demand) to the competitive solution (where supply equals demand) and concludes that the *elimination* of monopsony power increases both employment and the wage. We examine a marginal shift in monopsony power in a market that remains monopsonistic after the treatment. This marginal reduction increases employment but need not necessarily increase the wage.

studies linking the labor market impact of immigration and monopsony (Amior and Manning, 2021; and Amior and Stuhler, 2022) consider the possibility that immigrant workers are perfect substitutes with other low-skill workers.¹⁴ It is of interest to determine if this alternative assumption affects the efficiency results presented above.

The perfect substitution assumption imposes restrictions on the first and second derivatives of the production function. Suppose the production function is $Q = f(L_H, L_A + L_U)$. This specification implies that $f_A = f_U$; $f_{AA} = f_{AU} = f_{UU} < 0$; and $f_{HA} = f_{HU} > 0$. Moreover, linear homogeneity implies that $f_{HH}f_{AA} - f_{HA}^2 = f_{HH}f_{UU} - f_{HU}^2 = 0$ (as there are only two inputs that are not linearly related). It is easy to verify that imposing these restrictions in equations (7), (9), and (10) yields:¹⁵

$$\frac{dL_H}{dR_\epsilon} > 0, \quad \frac{dL_A}{dR_\epsilon} < 0, \quad \frac{dL_U}{dR_\epsilon} > 0, \quad \text{and} \quad \frac{dL_A}{dR_\epsilon} + \frac{dL_U}{dR_\epsilon} > 0. \quad (15)$$

Regularization reduces the marginal cost of an undocumented worker but does not affect the marginal cost of any other labor type. The number of undocumented workers hired rises because employers substitute towards the cheaper (and equally productive) low-skill input. This substitution effect reduces the employment of authorized low-skill workers. Note, however, that the *total* employment of low-skill workers rises (as that sector became more efficient). The employment of high-skill workers also rises because high- and low-skill workers are complements in the two-input linear homogeneous production function.

Although there is an overall efficiency effect when low-skill undocumented and authorized workers are perfect substitutes, the theory predicts that the employment of some groups will decline. The different predictions about the impact of regularization on different groups of low-skill workers can be used to infer the production interaction between low-skill undocumented and authorized workers in real-world settings.

In sum, a regularization policy that reduces the firm's monopsony power in the undocumented sector makes the entire labor market more efficient. Such a policy,

¹⁴ These studies also assume that employers do not wage discriminate in the low-skill sector, which would imply no wage gap between equally skilled undocumented and authorized workers. The evidence, however, suggests that undocumented workers suffer a wage penalty in the United States (Borjas and Cassidy, 2019; Ortega and Hsin, 2022; Pan, 2012).

¹⁵ The implied restrictions for the elasticity of complementarity are: $c_{AA} = c_{AU} = c_{UU} < 0$; $c_{HA} = c_{HU} > 0$; and $c_{HH}c_{AA} - c_{HA}^2 = c_{HH}c_{UU} - c_{HU}^2 = 0$.

therefore, has the potential to produce large economic gains. The size of this potential efficiency gain, a “regularization surplus,” is discussed in greater detail below.

3.2 Regularization in Competitive Labor Markets

In monopsonistic markets, the relatively low wage of undocumented workers arises from the market power given to firms by restrictions on the mobility of such workers. In a competitive market, the lower cost of hiring undocumented workers might instead arise because firms skirt the rules regulating legal exchanges in the labor market. For instance, firms might ignore minimum wage and overtime pay mandates or avoid paying payroll taxes. Let τ_i be the tax rate that captures these expenses for a type- i worker. If labor markets are competitive, the representative firm’s first-order conditions equating the value of marginal product to marginal cost are $f_i = w_i(1 + \tau_i)$.

The competitive equilibrium for each labor type occurs when aggregate demand (i.e., the sum of the marginal product curve across firms) equals aggregate supply as given by equation (4). The Mathematical Appendix shows that the predicted effects of a regularization policy that makes undocumented workers more expensive are:

$$\frac{dL_i}{d\tau_U} < 0 \quad \text{and} \quad \frac{dw_i}{d\tau_U} < 0, \quad (i = H, A, U). \quad (16)$$

A policy that raises the marginal cost of employing an undocumented worker reduces the demand for such workers. This reduction spills over to other sectors of the labor market if undocumented and authorized workers are complements.¹⁶ In the end, the rise in the cost of undocumented labor shrinks the *entire* labor market and fewer workers of all types are employed. This reduction in labor demand also reduces the wage w_i for all groups (as each group’s market supply curve is upward sloping).

The contrast between the impact of regularization in monopsonistic and competitive labor markets is striking. Whereas regularization in a monopsonistic framework expands the size of the market and increases employment for all groups and

¹⁶ If authorized workers and natives were perfect substitutes, authorized employment and wages would rise (due to the obvious substitution effect), but the employment and wage of undocumented workers would fall. Total employment would also fall and there would be no efficiency gains.

the wage of authorized workers, regularization in a competitive framework contracts the size of the market and reduces employment and wages for all groups.¹⁷

4. Data

The empirical analysis uses data from the French Labor Force Surveys (LFS) and the *Déclaration Annuelle des Données Sociales* (DADS). The LFS data allows us to study the impact of the regularization on employment, while the DADS data allows us to estimate its wage consequences.

4.1. LFS Data

The LFS data are collected annually in March.¹⁸ Before 1982, the LFS only contained information on a person's citizenship (rather than country of birth), making it impossible to distinguish native- from foreign-born persons. Instead, the LFS allows us to distinguish French citizens (which includes the foreign-born persons who acquired citizenship through naturalization) from non-citizens.¹⁹ We use the information on citizenship to measure the employment impact of the legalization program on the French population (which, by definition, includes native-born persons and naturalized immigrants). The LFS also contains information on such socioeconomic variables as gender, age, region of residence, employment status, and education.

To ensure a consistent definition of employment across survey years, we define employment as someone who is working at the time of the survey. We use the educational attainment variable to define two groups, workers who have completed high

¹⁷ We parameterized the regularization policy as a decline in the supply elasticity of undocumented workers. An alternative would be to model the policy as printing "papers" that allow some workers to instantly switch from the undocumented group to the authorized group (Chassambouli and Peri, 2015; and Elias, Monras, and Vázquez-Grenno, 2024). A regularization policy could then be interpreted as a mix of supply shocks: a negative supply shock in the undocumented labor market and a (numerically equivalent) positive supply shock in the market for legal immigrants. This alternative perspective suggests that complementarities between authorized and undocumented workers result entirely from the latter's lack of documentation. In other words, complementarities arise not because of characteristics embodied in workers themselves, but instead reflect the types of jobs that different groups perform. The Mathematical Appendix shows that this type of regularization policy need not increase native employment. Suppose, for instance, that there is greater complementarity between natives and undocumented immigrants than between natives and legal immigrants. Moving some workers from the "undocumented" to the "legal" column would then reduce native employment.

¹⁸ The INSEE introduced an additional representative survey in the month of October between 1977 and 1980. Unless otherwise specified, we do not use these additional surveys to be consistent with the data used in the latter years of the sample period.

¹⁹ In 1982, 30 percent of foreign-born persons were French citizens.

school (by passing a French exam named the “Baccalauréat” that gives access to college or an equivalent diploma) and workers who have not. Only 24 percent of French workers (aged 18-64) in the 1982 census had a baccalaureate degree (i.e., had passed the Baccalauréat exam). The educational attainment information is not available for many of the LFS respondents (between 25 to 56 percent) in the years 1975-1977. For the most part, we exclude these years from our analysis and start the empirical study of the employment effects of the legalization program in 1978.

We will also perform robustness tests by using the subsample of the least educated persons in the low-education group. This very low-educated sample is composed of individuals who have either no diploma or a diploma awarded at the end of elementary primary education (when they were 11 to 13 years old). This diploma is called CEP (*Certificat d'études primaires*) and was abolished in 1989. The 1982 census indicates that 45 percent of French workers were in this very low-educated group.

Our LFS sample consists of persons aged 18-64. We exclude persons who are self-employed (such as farmers or business owners), in military occupations, or enrolled in school. We also exclude Corsica from the analysis. The pre-1981 LFS data do not provide any information on a worker's earnings. To investigate the impact of regularization on French wages, therefore, we instead exploit the DADS data.

4.2. DADS Data

The DADS is an administrative file of matched employer-employee records collected by the INSEE. The data are drawn from mandatory reports filed by all French establishments. For each employee, the DADS reports gross and net real wages, number of days worked, and other relevant characteristics. The DADS only cover *legally* declared employees, exclude the self-employed, and does not contain any information for non-employed persons. Note that the design of the DADS prevents us from measuring what happened to the wage of undocumented immigrants after the policy was implemented (as they would not have appeared in the file prior to the regularization).

We use the panel version of the DADS from 1978 to 1988, which samples the French workforce born in October in even-numbered years (about 4 percent of all workers). The panel structure of our data allows us to track the same workers over time, holding constant the sample composition over the period of interest.

Because the DADS do not include information on a worker's education and citizenship, we use the Permanent Demographic Sample (*Échantillon Démographique Permanent* or EDP) which can be merged with the DADS. The EDP is a large-scale socio-demographic panel containing several variables relevant for our study, such as educational attainment, citizenship, and family characteristics (i.e., marital status and the birth year of a respondent's children). It covers a representative panel of individuals born between October 1 and October 4 each year. The merged DADS-EDP panel data allows us to classify French workers into different education groups.

We restrict our study of wage trends to full-time workers aged 18-64 (and again exclude Corsica from the analysis). Because the DADS excluded public sector employees in 1979 (and only partially included them between 1984 and 1987), we exclude public sector workers throughout the wage analysis. We aggregate across all job spells in a calendar year to calculate annual earnings for each worker and exclude observations that have extreme values (i.e., we exclude workers who are either in the top 0.5 percent or bottom 0.5 percent of the wage distribution). Finally, INSEE did not collect the DADS data in either 1981 or 1983. Our wage analysis, therefore, defines the pre-treatment period as 1978-1980, and the post-treatment period as 1984-1988.

5. The Labor Market Impact of Regularization

5.1. The Synthetic Control Approach: Graphical Evidence

A disproportionately large number of the legalized immigrants resided in the Paris region. We exploit this clustering to identify the economic impact of the legalization program. We first employ the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) to estimate the employment consequences of the regularization policy. Specifically, we compare employment outcomes in the treated region (i.e., Paris) to a synthetic region that mimics the pre-regularization employment outcomes of the treated unit.²⁰ In other words, the synthetic region approximates the post-1981 trajectory for the outcome of interest that would have been observed in Paris in the absence of the intervention.

²⁰ Our focus on a single treated region is similar to the approach used in other migration studies that employ the synthetic control method (see Bohn, Lofstrom and Raphael, 2017; Borjas, 2017; and Peri and Yasenov, 2019). Section 6.4 implements an alternative empirical strategy that exploits regional variation in the share of regularized immigrants to define a variable with different treatment intensity.

We construct the synthetic region by using the following set of predictor variables for the sample under study: the employment-to-population ratios between 1978 and 1981, and the change in employment and unemployment rates between 1979 and 1981.²¹ We initially exclude the Marseille region from the analysis as it may have been partly affected by the regularization program (see Figures 1 and 2).²²

Because the regularized immigrants were predominantly low-skill men, we initially focus on the employment response observed in the entire sample of low-educated men. Panel A in Figure 3 shows the trajectories of the employment-to-population ratio for the Paris and synthetic regions. The trends in the employment rate of low-educated men in the treated and synthetic regions are similar prior to 1981. The relative employment rate in the Paris region, however, increased sharply after the start of the regularization program. By 1983, the employment rate in the Paris region exceeded its pre-treatment level, while the employment rate in the synthetic control had fallen below its pre-treatment level. The employment rate in the two regions did not converge again until the late 1980s.

Panel A in Figure 3, therefore, suggests that the regularization policy had a positive impact on the employment of low-educated men in the first few years after the implementation of the program. The other panels of the figure show similar patterns for low-educated French men (Panel B), and low-educated non-French men (Panel C).

²¹ We tried alternative combinations of predictor variables to determine if our results were robust. For example, using the average participation rate over the pre-treatment period and the change in the participation rate between 1979 and 1981 as predictor variables produces similar empirical results. In addition, we find the same set of results if we increase the number of pre-treatment periods by including the October waves of the LFS conducted in 1977, 1978, 1979, and 1980 (thus doubling the number of pre-treatment periods). Finally, we also implement the augmented synthetic control method of Ben-Michael, Feller, and Rothstein (2021)—i.e., "a regression adjustments to attenuate the bias of synthetic control estimators in settings where the synthetic control counterfactual is constructed using untreated units with values of the predictors that do not closely reproduce the predictor values for the treated unit" (Abadie, 2021, p. 419). Our results and conclusions are fully robust to this alternative approach. The outcome variable is identical in the treated and synthetic control groups before treatment, while we observe a significant increase in the employment-to-population rate among low-educated men. The results are available upon request.

²² We use 19 French regions to build the synthetic region. When considering the baseline sample of low-educated men, the method assigns positive weights to Rhône-Alpes (0.547), Alsace (0.446) and Midi-Pyrénées (0.008). The method selects the same regions for the sample of low-educated French men but assigns different weight to each of them: Rhône-Alpes (0.805), Alsace (0.173) and Midi-Pyrénées (0.022). When looking at the sample of low-educated non-French men, the synthetic method uses a combination of all regions to create the control group, assigning the largest weights to Centre-Val de Loire (0.317), Franche-Comté (0.061) and Pays de la Loire (0.053).

To better illustrate the magnitude of these effects, Figure 4 shows the year-by-year difference in the employment rate of low-educated men between the Paris region and its synthetic counterpart. The regularization program increased the employment-to-population ratio of low-educated men by 3 to 6 percentage points by 1985. Given that the reform regularized the status of 2.0 percent of workers in the Paris region, this magnitude implies that a one percent increase in the number of authorized workers due to a legalization program increased the employment rate of pre-existing low-skill workers by about 1.5 to 3 percentage points.²³

5.2. Heterogeneity by Education

Figure 5 illustrates the employment trends across different education groups. It shows the yearly gaps in employment rates for three groups: those having only a primary education (or a very low level of education), those having less than a baccalaureate degree, and those having at least a baccalaureate degree (or a high level of education). Panel A of Figure 5 uses the entire sample of men, while Panel B uses the subsample of French men.

The results in both panels are very similar. The figures show that the employment gaps between Paris and the synthetic region before the amnesty program are essentially zero and show no pre-trends. Both panels also show that the positive employment impact of the policy is strongest for the least educated men and weakest for high-educated men. In particular, the gap between the actual and counterfactual employment rates widens from nearly zero to 7 percentage points for the least educated group and to 3 percentage points for the highly educated group.

5.3. Falsification Tests

Because “large sample inferential techniques are not well suited to comparative case studies when the number of units in the comparison group is small,” Abadie, Diamond, and Hainmueller (2010, p. 407) propose a falsification test based on the distribution of the (placebo) effects estimated for all units in the control group. The idea

²³ In unreported results, we also find a positive employment effect in the subsample of workers who were “likely undocumented” prior to the regularization program. The identification of this subsample of persons relies on imputation methods (Borjas, 2017, Albert, 2021; Amior and Manning, 2021) that exploit demographic information (such as receipt of particular types of public benefits, or employment in the public sector) to identify the subsample of persons most likely to have been undocumented.

of this “permutation” test is to reassign the treatment to each region in the control group, and to replicate the synthetic control approach. We can then estimate a placebo effect in the remaining 19 regions and determine if the estimated effect for the Paris region is extreme relative to the distribution of estimated effects for the other regions.

We again define the pre-treatment period as 1978-1981, the post-treatment period as 1982-1988, and use the same predictor variables as in the baseline analysis.²⁴ Figure 6 illustrates this empirical exercise by focusing on the employment rate of French men for the three education groups introduced above. Consider the permutation tests conducted in the low-educated sample. Panel A shows that the pre-treatment deviations in the employment rate between the treated region and the synthetic control are virtually zero. Nevertheless, no other placebo region experiences as large a post-treatment change as the Paris region did. This pattern is reinforced in Panel B, which focuses on the subsample of the least educated French men.

Finally, Panel C reproduces the falsification test using the sample of highly educated men. Contrary to the pattern in the samples of low-educated men, the permutation tests indicate that the pre-post deviations in employment rates in the Paris region are less exceptional and lie within the range of the placebo estimates. The data, therefore, seem to indicate that the regularization program had only a modest impact on the employment opportunities of high-educated French men.

5.4. Main Regression Results

We estimate the regression model:

$$ER_{rt} = \theta_r + \theta_t + \beta_1(\text{Paris} \times T_{1982-1983}) + \beta_2(\text{Paris} \times T_{1984-1988}) + \varepsilon_{rt}, \quad (17)$$

where ER_{rt} is the employment-to-population ratio in region r and year t ; θ_r is a vector of region fixed effects; θ_t is a vector of year fixed effects; and “Paris” is an indicator variable set to unity for the Île-de-France region. The regression interacts the Paris indicator with two post-treatment period fixed effects: $T_{1982-1983}$ and $T_{1984-1988}$ are equal to one in the years indicated in the subscripts and 0 otherwise. The model is estimated using annual observations between 1978 and 1988. The coefficient β_1 is the

²⁴ All results reported in this section are similar when including the October LFS waves conducted in 1977-1980 and when implementing the augmented synthetic control approach of Ben-Michael, Feller, and Rothstein (2021).

difference-in-differences estimator capturing the short-run change in the employment rate due to the legalization program, while β_2 measures the long-run impact.

Table 2 presents the estimates of β_1 and β_2 using the data for the treated and synthetic regions (so the regressions have 22 observations). We report several alternative specifications of the model, estimating the regression separately by gender, nationality group, and education. Panel A shows the estimates for the low-educated group, while Panel B reports the estimates for the high-educated sample.

The regression coefficients for low-educated men confirms the graphical evidence: the regularization program increased the employment rate gap between Paris and the synthetic region. The coefficient reported in column 2 indicates that the employment rate of low-educated French men increased by 4.5 percentage points in the Paris region in the first two years following the program.

The positive employment response is not restricted to low-educated men. Low-educated French women also experienced a positive employment gains (although the effects are not significant in the non-French sample). Finally, the regressions indicate that the employment impact of the legalization program was also positive for high-educated French men. These effects, however, are weaker than those observed for low-educated men and only significant between 1982 and 1983.

Table 3 moves beyond the synthetic control method and simply compares Paris to all other regions. These regressions use the entire data set of 20 regions and have 220 observations (20 regions, each observed 11 years).²⁵ The data confirm that low-educated men in Paris, the group and region most affected, experienced positive employment gains after the regularization program went into effect. The magnitude of the short-run effect is the same as that implied by the synthetic cohort analysis, about 4 percent. Although the regressions also reveal weaker positive employment effects for highly educated men, the effects are near zero (and insignificant) for women.

We also implemented the "more flexible and robust" synthetic difference-in-differences approach introduced by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021). This method attempts to match treated and control units on pre-treatment

²⁵ The regressions using the whole sample of regions also report the wild cluster bootstrap p -value of each estimated coefficient (Cameron, Gelbach, and Miller, 2008, p. 427). To be consistent with the construction of the synthetic control in Table 2, Table 3 excludes the Marseille region from the analysis.

trends allowing for a constant difference between treatment and control units.²⁶ We report the results using this alternative approach in Table 4. Our results again confirm the significant increase in the employment rate of low-educated men living in Paris after the treatment period relative to the other regions. The coefficient reported in column 1 indicates that the employment rate of low-educated men increased by 3.7 percentage points in the Paris region in the first two years following the program. Table 4 shows no significant effect for the other education and gender groups.

It is instructive to interpret the evidence through the lens of the theoretical framework presented in Section 3. The regularization of a sizable fraction of the low-skill male workforce in France led to an overall increase in the employment of low-skill men and to a smaller, but detectable, increase in the employment of women and high-skill workers. The expansion in low-skill employment is not consistent with a model where undocumented workers are hired in an open, competitive market. The evidence, therefore, strongly suggests that firms do have some monopsony power over undocumented workers. The “Exceptional Regularization” reduced some of that monopsony power and made the entire labor market more efficient.

5.5. The Impact on French Wages

We now provide an analogous regression analysis of the wage impact of regularization. Our theoretical framework predicts that a reduction in monopsony power in the undocumented labor market would increase the wage of undocumented workers (if monopsony power was sufficiently strong prior to regularization) and would also increase the wage of all other French workers. We use the DADS-EDP data to estimate the regression model that measures the wage impact:

$$\log w_{rt} = \theta_r + \theta_t + \alpha_1(\text{Paris} \times T_{1984}) + \alpha_2(\text{Paris} \times T_{1985-1988}) + \epsilon_{rt}, \quad (18)$$

where w_{rt} gives the wage in region r at time t . As noted earlier, the DADS did not collect data in 1981 and 1983. We simplify the exposition by excluding the year 1982 from the

²⁶ Specifically, Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021, p. 4089) note that this method “reweights and matches pre-exposure trends to weaken the reliance on parallel trend assumptions” (like the synthetic control approach), but it allows for treated and control units to be trending on different levels prior to a reform of interest. The method then applies a difference-in-differences estimation to the reweighted panel. We again exclude the Marseille region from this exercise.

analysis (when wage data was indeed collected) and compare wages between the pre-treatment period of 1978-1980 and the post-treatment period of 1984-1988.

Because the estimated wage effects are likely to be contaminated by changes in sample composition due to the entry of the legalized immigrants into the DADS-EDP file in the post-treatment years, we focus on French workers. Table 5 reports the estimates of α_1 and α_2 for low-educated French workers obtained from alternative regression models estimated separately by gender. The regressions contrast the Paris region with both the synthetic control and the whole set of regions (again excluding Marseille).²⁷

The cross-section estimates of α_1 and α_2 in columns 1-2 indicate that the average wage of low-educated French men in the Paris region *fell* after the policy change. As noted by Borjas and Edo (2021), the employment response likely changed the sample composition of earners, producing a selection bias in the estimates of α_1 and α_2 . If there is positive selection into employment and if regularization increased the employment of low-skill workers, the mean wage of the French men who entered the market after 1982 would be below the mean wage of the pre-existing workers. The changing sample composition mechanically reduces the observed mean wage in the market. This type of selection bias, therefore, produces a downward biased estimate of α_1 .

One possible way of correcting for selection bias (called the “identification at infinity” method in the literature) is to isolate a subsample of workers for whom selection into employment is unlikely to matter (Chamberlain, 1986; Heckman, 1990; Mulligan and Rubinstein, 2008; and Blau, Kahn, Boboshko and Comey, 2021). The employment probability of French men increases substantially with the presence of children (Borjas and Edo, 2021). We use the “identification at infinity” approach by re-estimating the model in the subsample of French men who had at least one child below age 18. Columns 3 and 4 show that the negative cross-section estimates of α_1 and α_2 turn positive and significant when we use a subsample of the male workforce that has a high level of labor force attachment regardless of any potential policy shock, thus reducing the possibility of selection bias.

Finally, Dustmann, Otten, Schönberg and Stuhler (2023) suggest that an alternative way of accounting for selection bias is to hold constant the sample

²⁷ The synthetic control method follows a similar approach to that used in the employment rate analysis. The predictor variables are the growth rate in the wage and employment rates of low- or high-educated male French workers between 1978 and 1980.

composition of native workers by exploiting the panel structure of the data. The regression coefficients reported in columns 5-6 show that the panel estimates of the short-run effect α_1 is significantly positive, and of similar magnitude to that estimated with the “identification at infinity” approach in columns 3-4.²⁸

Panel B replicates the wage regression analysis for the sample of French women, where selection biases might play an even larger contaminating role because the participation rate of women in France in 1982 was only 47.4 among low-educated women and 63.5 percent among high-educated women (relative to about 80 percent for men regardless of education). The identification at infinity method uses the subsample of women who are likely to have the strongest attachment to the labor market: unmarried women without children. Generally, the wage effects for women are much weaker than those found for men, except for the panel data analysis in column 5.²⁹

6. Sensitivity Tests

6.1. Concurrent Policy Shocks

Although the amnesty program was exogenous and unanticipated, it was accompanied by other labor market reforms introduced by the newly elected Mitterrand government. If these concurrent policy shocks also disproportionately affected the Paris region, the evidence reported in previous sections may not have been the result of the regularization program.

In particular, the new government hired 200,000 new civil servants (i.e., workers in the central or local administration). The implementation of the regularization policy could have motivated the government to disproportionately increase public employment in the Paris region to handle the regularization requests. The observed relative increase in total employment in that region would then have little to do with the impact of regularization on labor market efficiency. In addition, the national minimum

²⁸ The panel regressions restrict the analysis to native workers who remain continuously employed in the same region between 1978 and 1988, holding constant the regional composition of workers. This sample restriction implies that the number of observations used to compute the mean regional wage drops dramatically from an average of 26,486 in the repeated cross-sections to 7,750 in the panel for low-educated native men, and from 13,207 to 3,003 for low-educated women.

²⁹ We also estimated the wage regressions using the sample of high-educated French workers, and found little evidence that the relative wage of high-educated workers in the Paris region changed in the post-treatment period.

wage was raised by 10 percent in July 1981 (followed by several smaller increments) *and* the standard working week was reduced from 40 to 39 hours in February 1982.

Sachs and Wyplosz (1986) suggest that these reforms raised labor costs, while Crépon and Kramarz (2002, p. 1385) show that “changes in the legal standard workweek led to employment losses, contrary to the initial goals of these policies,” and that the adverse employment effects were more pronounced for minimum wage workers.

It is easy to illustrate that none of these reforms could be driving our results. Simply put, the geographic impact of these reforms did not coincide with the geographic impact of the regularization program—the identification strategy used in the previous section. The three panels of Figure 7 demonstrate that there is no relationship between the share of regularized immigrants and the growth rate in the number of male civil servants (Panel A), the share of male employees paid at the minimum wage (Panel B), or the share of men working at least 40 hours (Panel C). Although the correlation between the two variables is negative in Panels A and C and positive in Panel B, all the correlations are numerically close to zero and insignificant. It is therefore very unlikely that any of these policy shocks could explain why trends in employment and wages diverged between Paris and the other regions in the post-regularization period.³⁰

6.2. Paris and Marseille Regions

Up to this point, we have excluded the Marseille region from the analysis because the share of regularized immigrants in Marseille, though small, was not negligible. Nevertheless, viewing the Marseille region as an alternative treatment unit may help to reinforce the interpretation that the relative rise in the employment rate of low-educated men in Paris in 1982-1983 was due to the amnesty program (and not because the Paris region can be considered an outlier).

³⁰ The number of civil servants in the LFS increased by about 213,000 persons between 1981 and 1983, with 80 percent of this growth due to an increase in the number of high-educated workers, and 77 percent due to an increase in the number of female workers. The number of low-educated men employed in public administration fell by 16,000 persons. The rise in in public sector employment could not then mechanically lead to a rise in the employment of low-educated men. Similarly, given that most minimum wage workers are women, their employment opportunities should have been more significantly affected than those of similarly educated men (which contradicts the evidence). We also estimated the regression models excluding civil servants from the analysis. The regressions confirm that the regularization policy increased the relative employment of low-educated men in the Paris region. In fact, the estimated coefficients are close in magnitude to the baseline coefficients reported in Tables 2- 4.

Figure 8 uses the permutation analysis to illustrate how regularization affected the employment outcomes of French men with a primary education in Marseille. The pre-treatment employment gap between Marseille and its synthetic control is virtually zero before regularization. The relative employment rate of the least educated French men in Marseille, however, increased rapidly in 1982-1983. In 1982, the employment rate of that group is about 3.8 percentage points higher than in the synthetic region. Although this effect is significant at the 1 percent level, it is smaller than in the Paris region.

Table 6 reports the regression estimates of the relative gap in the employment rate of very low-educated French men in Paris and Marseille relative to both the synthetic control and all other non-treated regions. The coefficients in columns 2 and 4 (which measure the gap relative to all other regions) show that both Paris and Marseille experienced a rise in the employment rate of French men with a primary education in the first years after the regularization, and that this effect was weaker in the Marseille region. It is worth emphasizing that the finding of a small, but significant, employment effect in Marseille suggests that the stronger effect observed in Paris is not due to idiosyncratic post-1981 changes in economic conditions that happened to occur only in Paris. There is a common event—namely, the regularization of a large share of the low-skilled workforce—that reconciles the evidence.

6.3. Triple Difference Strategy

To further reduce concerns that our employment results may be driven by the entire package of reforms introduced shortly after the election of President Mitterrand, we now use an alternative strategy that relies on a triple difference design. This strategy takes advantage of the fact that regularized immigrants were mostly located in the Paris region *and* worked in only a few sectors (half of them worked in the construction sector, the hotel and restaurant industry, or domestic services).³¹

The triple-difference strategy has the advantage of controlling for all regional changes that could affect employment opportunities. Specifically, we estimate the following regression:

³¹ 30 percent of the newly regularized immigrants worked in the construction sector, 12 percent in the hotel and restaurant industry, and 10 percent in domestic services (Cealis et al., 1983, p. 39).

$$\frac{L_{srt}}{L_{rt}} = \beta(Treated_s \times Paris_r \times T_{1982-1988}) + \theta_{sr} + \theta_{st} + \theta_{rt} + \varepsilon_{srt}, \quad (19)$$

where L_{srt} is employment in sector s , region r , time t . The triple interaction term is the treatment indicator. The variable $Treated_s$ equals unity if the sector is construction, hotel or restaurant industries, or domestic services (and zero otherwise).³² The triple interaction in (19) then takes the value of one when the sector of activity is a treated sector and located in Paris after 1981, and zero otherwise. Equation (19) also includes vectors of sector-region fixed effects θ_{sr} , sector-time fixed effect θ_{st} , and region-time fixed effects θ_{rt} . The estimated parameter β then measures the employment trends in the treated sector (relative to the untreated sector) within the Paris region relative to other regions, before and after the regularization policy.

The various columns in the first row of Table 7 report the estimated β for alternative samples of workers. The estimated coefficient is significantly positive in the sample of low-educated French workers (with the effect being far stronger for low-educated French men). This finding indicates that the low-educated employment share in the treated sectors increased more than in the untreated sectors in the Paris region after the policy change. The asymmetric employment responses by gender and education are consistent with our previous findings. It appears that the positive employment impact of the regularization policy mostly took place in the regions and sectors most affected by the program.

Rows 2-4 of Table 7 report the regression coefficients when the treated sector is defined in a more granular fashion, as either construction, the hotels and restaurant industry, or domestic services. In each case, we again find that employment opportunities for low-educated men increased relatively more in the treated sectors in the Paris region after 1981. Finally, row 5 reports the results from a placebo test that uses the chemical and pharmaceutical sector as the treated group and the banking sector as the untreated group. The estimated coefficients are insignificant and close to zero, suggesting no differential change in employment between these two sectors in the Paris region after 1981. This result is reassuring and suggests that the previous positive

³² We exclude from the group of untreated sectors all workers in the agricultural, wholesale and retail sectors, as well as the textile and clothing industry, as these specific sectors were also partly affected by the regularization shock. We also exclude from the analysis the Marseille region as it was partially affected by the regularization policy, and public employees to increase the comparability across sectors.

employment responses in the treated region and sectors is not a statistical artifact and very likely attributable to the regularization of a large fraction of the low-skilled immigrant workforce.

6.4. Spatial Correlations as an Alternative Estimation Strategy

We have measured the employment impact of regularization by comparing the localities most “shocked” by the treatment to the rest of the French labor market. We now show that our results are robust if we instead use an event-study specification that exploits variable-intensity variation in treatment across regions. Consider the model:

$$ER_{rt} = \theta_r + \theta_t + \beta_1(R_r^{1981} \times T_{1982-1983}) + \beta_2(R_r^{1981} \times T_{1984-1988}) + \varepsilon_{rt}, \quad (20)$$

where R_r^{1981} gives the number of regularized immigrants in region r as a share of the low-educated French labor force in that region in 1981 (before the implementation of the policy). Equation (20) resembles a “spatial correlation” model, relating employment rates to the size of the regularization supply shock in the post-treatment period.³³ This approach is commonly used to measure the labor market impact of immigration, and requires that we account for the potential endogeneity of the regularization variable.

We use a shift-share instrument to correct for the endogeneity. The 1962 French census gives the baseline spatial distribution of low-educated non-French persons aged 18-64 for each of nine nationality groups.³⁴ We use these data to predict the regional allocation of the regularized immigrants. The predicted number of regularized immigrants in region r at time t is:

$$\hat{M}_{rt} = \sum_n \frac{M_r^n(1962)}{M^n(1962)} \cdot M^n(1983). \quad (21)$$

where $M_r^n(1962)$ gives the number of immigrants in 1962 from nationality group n residing in region r ; $M^n(1962) = \sum_r M_r^n(1962)$; and $M^n(1983)$ gives the total number of

³³ Cealis et al. (1983, p. 18) report the regional distribution of newly regularized immigrants as of August 1, 1983. Our results are robust to using alternative measures of the “regularization shock,” such as the share of newly regularized immigrants in the total labor force or in the immigrant workforce.

³⁴ The number of regularized workers is available for 11 nationality groups (Cealis et al., 1983, p. 17). We aggregate to 9 groups to homogenize with the census data: Algerian, Tunisian, Moroccan, Rest of Africa, Portuguese, Spanish, Turkish, Yugoslav, and rest of the world. The calculation excludes immigrants with a nationality that was unaffected by the policy (such as Belgian, Dutch, Italian or German).

regularized immigrants from group n . The instrument, denoted by \hat{R}_r^{1981} , is defined by dividing \hat{M}_{rt} by the regional native working-age population.³⁵

The key identifying assumption is that the employment rate would have evolved similarly in regions with higher and lower shares of regularized immigrants in the absence of the regularization policy. To provide visual evidence of the regional effect of the regularization policy on the overall employment-to-population ratio, we initially employ a flexible event-study design where we estimate the coefficients on the interactions between R_r^{1981} and each of the year fixed effects in our sample period.³⁶ We also include first-order region and year fixed effects, use \hat{R}_r^{1981} as an instrument, and standardize the estimated impact to zero in 1981.

Figure 9 displays the corresponding IV estimated effects of our event-study specification. The first important result is that the pre-treatment estimated coefficients are (with only one exception) insignificantly different from zero, suggesting no differential employment trends across regions prior to the reform.³⁷ Second, regions more exposed to the “regularization shock” experienced a faster rise in the employment rate immediately after the reform. Our baseline estimate indicates that a one-percentage point rise in the share of regularized immigrants increases the 1983 employment-to-population ratio by 0.7 percentage point. Finally, the post-1987 effects are again statistically indistinguishable from zero, indicating that the employment response dissipated over time.

Table 8 reports the estimated coefficients of β_1 and β_2 from equation (20) using various specifications. While columns 1-2 exploit the longer sample period between

³⁵ To show that the instrument is likely to satisfy the exclusion restriction imposed by the IV, we regressed the pre-regularization changes between 1978 and 1980 in (i) the overall employment rate, (ii) the employment rate of low-educated French men, (iii) the employment rate of low-educated French women, and (iv) the employment rate of high-educated French on the instrument \hat{R}_r^{1981} . In unreported results, we found that the estimated coefficients are imprecisely estimated (with corresponding p -values ranging from 0.75 to 0.95), indicating that the instrument is uncorrelated with pre-policy trends in employment outcomes.

³⁶ The numerator of the employment-to-population rate used as the dependent variable in the regressions is the number of working-age employed persons in region r at time t regardless of gender, education, and nationality. The focus on overall employment rates allow us to extend the pre-treatment period back to 1975.

³⁷ In unreported results, we found that including as additional controls the interactions between the year dummies and the initial regional shares of employment in the manufacturing and construction sectors led all estimated coefficients on the regularization shock to be insignificant before treatment (while their post-treatment values are positive and significant at the 5 percent level in the first years following the program).

1975 and 1988, columns 3-7 use our shorter baseline sample period 1978-1988 (which allows us to examine different education groups). Columns 1-4 show that regularization had a significantly positive impact on the employment-to-population ratio in the population. The IV estimated coefficients in column 2 and 4 are very close and implies that a one-percentage point rise in the share of regularized immigrants in a given region increased the employment rate in that region by 0.8-0.9 percentage point immediately after regularization.³⁸ The remaining columns of the table show that the positive employment response is stronger and more significant for low-educated men than for low-educated women, and there is no employment response among high-educated French persons.

The estimated coefficient in column 5 shows that a one-percentage point rise in the share of regularized immigrants increases the employment rate of low-educated French men and women in that region by 1.5 and 0.9 percentage points, respectively. These effects imply that the regularization of about 65,000 immigrants in the Paris region, which represent 2.8 percent of the local low-educated French workforce, would increase the employment rate of these two groups by between 2.6 and 4.1 percentage points. Despite the very different empirical methodologies, these magnitudes are quantitatively close to those reported in Table 2 using the synthetic control approach.

7. The Regularization Surplus

The canonical model of a competitive labor market predicts that a one-time supply shock generates an immigration surplus, an increase in the total income accruing to natives (Borjas, 1995). The regularization of undocumented immigrants when firms have monopsony power increases labor market efficiency. Regularization, therefore, also produces a surplus. We now examine the source of the “regularization surplus” and present the first estimates of its size.

It is easy to illustrate the surplus produced by a policy that regularizes undocumented workers and moves the labor market to a competitive equilibrium. Consider the market illustrated in Figure 10, where L now represents the number of

³⁸ Adding an interaction between \hat{R}_r^{1981} and a fixed effect for the years 1975-1979 or 1978-1979 does not affect our results. The estimated coefficients on this interaction term are never significant, indicating that French regions behaved similarly prior to the reform (i.e., there is no evidence of a violation of the “parallel trends” assumption in the pre-treatment period).

efficiency units (added across various groups). A competitive equilibrium is achieved when supply equals demand at point e^* , and L^* efficiency units are employed.

Monopsony power in the undocumented sector implies that fewer than L^* efficiency units are used in production. This inefficiency spills over to other types of workers particularly if undocumented and authorized workers are complements, reducing total employment to L^M . Total GDP in this inefficient economy is the area of the trapezoid A. Suppose that the regularization of undocumented workers *completely* rids the labor market of the monopsony power that was holding back employment in all sectors. Regularization then moves the economy to the competitive equilibrium, increasing GDP by the sum of the areas B and C.

We can use a standard back-of-the-envelope approach to calculate the size of the two areas and quantify the regularization surplus. Area B is conceptually equivalent to the immigration surplus for an economy that receives $(L^* - L^M)$ “immigrants”, while the rectangle C represents the total wages accruing to the new workers who entered the labor market after regularization. Specifically:

$$\frac{\text{Area B}}{Q} = -\frac{1}{2}sem^2, \quad (22a)$$

$$\frac{\text{Area C}}{Q} = \frac{w^*(L^* - L^M)}{Q} = sm, \quad (22b)$$

where Q is aggregate output, e is the inverse factor price elasticity ($e = d \log w / d \log L$); s is labor’s share of income ($s = wL/q$); and $m = (L^* - L^M)/L$, the “supply shock” of new workers who entered the labor market after the regularization program.

To approximate m , Table 9 estimates the impact of the regularization on the change in overall employment-to-population rate (including all persons regardless of age, gender, or nationality) by relying on the synthetic difference-in-differences approach (Arkhangelsky, Athey, Hirshberg, Imbens, and Wager, 2021).³⁹ Column 1 shows that the employment rate in Paris was 1.4 percentage points higher in the two first years following the program than it would have been in the absence of the policy. The share of the regularized workforce in the Paris region was 2.02 percent, such that

³⁹ As with the employment and wage analyses presented earlier, we exclude the Marseille region from the exercise.

the implied employment elasticity is 0.69 (0.014/2.02%).⁴⁰ Combining this elasticity with the share of the regularized workforce in France, the regularization program increased the French employment rate by 0.56 percentage point (or $0.69 \times 0.81\%$). Suppose the share of labor income is 0.7, and the inverse factor price elasticity is -0.3. The areas B and C would thus represent an increase of 0.0003 ($-0.5 \times 0.7 \times -0.3 \times 0.0056^2 \times 100$) and 0.3921 ($0.7 \times 0.0056 \times 100$) percent of GDP, respectively. This quantification exercise implies that the regularization program increased French GDP by about 0.4 percent.

Column 2 of Table 9 presents an alternative (and perhaps more direct) way of calculating the impact on output growth. It reproduces the same regression reported in column 1 by using the change in the log GDP per capita as an alternative dependent variable.⁴¹ The estimated coefficients indicate that the per-capita GDP growth rate increased in Paris immediately after 1981, but then declined and returned to a level comparable to that of the control group by 1984. This dynamic suggest an “excess” growth rate in the Paris region per-capita GDP of 1.6 percent between 1982 and 1983.

We can use this regression-based local estimate of the GDP effect to quantify the nationwide output response to regularization. Let us again define the output elasticity as the change in the growth rate of per-capita GDP induced by a program that regularizes one percent of the workforce. The share of regularized workers in the Paris workforce was 2.02 percent, so that the implied output elasticity is 0.79. The share of regularized immigrants in the total French workforce in 1981 is 0.81 percent. As a result, the regularization program increased French GDP by 0.64 percent ($0.79 \times 0.81\%$).⁴² This

⁴⁰ Specifically, the employment elasticity is the change in the employment rate induced by a program that regularizes one percent of the workforce.

⁴¹ The per-capita GDP data are reported in two publications available in INSEE’s archives. Donnellier, Maliverney and Montlouis (1987, p. 75) provide the regional per-capita GDP data between 1976 and 1984, and Dejonghe and Vincenau (1996, p. 150-151) cover the 1982-1990 period. Since these two series overlap between 1981 and 1984, we take the average of regional per-capita GDP in each year. We also deflate the time series using the French consumer price index provided by INSEE.

⁴² As a fraction of GDP, the regularization surplus is much larger than the immigration surplus typically reported in the U.S. literature. The surplus resulting from the immigrant supply shock that increased the size of the U.S. workforce by over 15 percent is only about 0.31 percent of GDP (Blau and Mackie, 2016, p. 171). It is important to note that the immigration surplus measures only the efficiency gain that accrues to natives (either workers or firms), while the regularization surplus accrues to everyone (firms, all pre-existing workers, and all new workers brought into the labor market by the increased demand and higher wages). It would be interesting to calculate the gains accruing to the various groups. This calculation, however, would require many more assumptions than those used to provide the back-of-the-envelope estimates discussed in the text.

estimate is very similar to the theory-based estimate based on the change in total employment (which relies on the textbook supply-demand analysis illustrated in Figure 10). In short, the exceptional regularization program increased French GDP by around 0.5 percent.⁴³

8. Conclusion

The presence of sizable numbers of undocumented immigrants often triggers a heated debate about what to do with the current stock of undocumented immigrants and what can be done to halt the inflow. One solution is to enact an amnesty that regularizes the status of the current undocumented population while tightening border enforcement and increasing penalties to discourage the continuation of the flow.

This paper documents the economic consequences of a large amnesty program implemented in France. In July 1981, the newly elected government of President François Mitterrand proposed to regularize all undocumented immigrants who satisfied two criteria: They entered France before January 1, 1981, and they had a work contract valid for at least a year. The regularized workers were predominantly male, low-skill, and lived disproportionately in the Île-de-France (Paris) region. The regularized immigrants composed 2.0 percent of workers in Paris, and nearly 1 percent of all workers in France.

It is often argued that undocumented workers face different labor market conditions than authorized workers because the undocumented have restricted job opportunities. It may be costly for them to participate in the open labor market and risk exposure to the authorities. These mobility restrictions give firms some monopsony power in the undocumented labor market.

We develop a theoretical framework that illustrates the economic inefficiency introduced by this market imperfection. Not surprisingly, the inefficiency reduces the number of undocumented workers hired. However, this inefficiency spills over to other sectors of the labor market, so that monopsony power in the undocumented sector may

⁴³ This result is consistent with the simulation results of Ortega and Hsin (2022). They find that granting legal status to undocumented immigrants in the United States (who account for about 5 percent of U.S. employment) could increase GDP by at least 1 percent. In their framework, the economic gain is due to a rise in the productivity of the newly regularized immigrants who can now perform jobs better suited to their skills.

curtail the hiring of both natives and authorized immigrants below what would otherwise be optimal.

A regularization program that reduces the firm's monopsony power will then increase the employment not only of undocumented workers, but also of both legal immigrants and natives. In short, by reducing monopsony power in the undocumented labor market, a regularization program improves labor market efficiency and can generate a substantial increase in output, a "regularization surplus."

Our empirical analysis of employment, wage, and output data in the French labor market confirms that the regularization program indeed had positive effects on the employment and wage of many groups, and particularly for male, low-skill workers. There was a sizable jump in the growth rate of per-capita GDP in the affected region, suggesting an increase in total French per-capita GDP of around 0.5 percent.

The implications of the analysis for the policy issues surrounding undocumented immigration are less transparent than the "regularization expands the economy" inference would make it seem. After all, the inefficiency would not have existed had there been no undocumented labor market in the first place. Moreover, amnesty programs may affect migration incentives in sending countries, perhaps creating new inefficiencies in the process. Finally, there are fiscal consequences, in terms of both social expenditures and tax revenues, that would need to be included in a full accounting of the costs and benefits of regularization policies.

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MATHEMATICAL APPENDIX

Suppose the canonical output-constrained profit-maximization problem (or, equivalently, the cost-minimization problem) with production function $g(x_1, \dots, x_N)$ has a unique solution. The matrix giving the Hessian of the production function is:

$$\mathbf{G} = \begin{bmatrix} g_{11} & \cdots & g_{1N} \\ \vdots & \ddots & \vdots \\ g_{N1} & \cdots & g_{NN} \end{bmatrix}. \quad (\text{A1})$$

Barten, Kloek, and Lempers (1969, p. 110) show that $\text{rank}(\mathbf{G}) \geq N - 1$. The matrix \mathbf{G} is singular if g is linear homogeneous. In this case, therefore, $\text{rank}(\mathbf{G}) = N - 1$ (De Boer, 1982, pp. 20-21).

A.1 Basic Model

The concave linear homogeneous production function is:

$$Q = f(L_H, L_A, L_U), \quad (\text{A2})$$

where L_H gives the number of high-skill workers; L_A the number of low-skill authorized workers; and L_U the number of low-skill undocumented workers. The Hessian \mathbf{H} of the production function has rank 2 and concavity implies that \mathbf{H} is negative semidefinite. Each input has diminishing marginal product ($f_{ii} < 0$), and the second-order principal minors of \mathbf{H} are positive (i.e., $f_{ii}f_{jj} - f_{ij}^2 > 0$).

The inverse supply function for group i is:

$$w_i = P_i^{-\epsilon_i} L_i^{\epsilon_i}, \quad i = H, A, U, \quad (\text{A3})$$

where ϵ_i ($\epsilon_i \geq 0$) is the reciprocal of the labor supply elasticity and P_i gives the “baseline” number of type- i workers. The first-order conditions to the firm’s profit-maximization problem are:

$$f_i = (1 + \epsilon_i) P_i^{-\epsilon_i} L_i^{\epsilon_i}, \quad i = H, A, U. \quad (\text{A4})$$

A.2 Regularization as a Reduction in Monopsony Power

To determine the impact of a change in ϵ_U , differentiate the first-order conditions in (A4). This yields the system of equations:

$$\begin{bmatrix} f_{HH} - \epsilon_H f_H L_H^{-1} & f_{HA} & f_{HU} \\ f_{AH} & f_{AA} - \epsilon_A f_A L_A^{-1} & f_{AU} \\ f_{UH} & f_{UA} & f_{UU} - \epsilon_U f_U L_U^{-1} \end{bmatrix} \begin{bmatrix} dL_H/d\epsilon_U \\ dL_A/d\epsilon_U \\ dL_U/d\epsilon_U \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ dMC_U/d\epsilon_U \end{bmatrix}, \quad (\text{A5})$$

where $MC_U = (1 + \epsilon_U) P_U^{-\epsilon_U} L_U^{\epsilon_U}$, the marginal cost of an undocumented worker. Note:

$$\frac{dMC_U}{d\epsilon_U} = MC_U \left(\log \frac{L_U}{P_U} + \frac{1}{1 + \epsilon_U} \right) > 0. \quad (\text{A6})$$

The second-order conditions require that:

$$\Delta = \begin{vmatrix} f_{HH} - \epsilon_H f_H L_H^{-1} & f_{HA} & f_{HU} \\ f_{AH} & f_{AA} - \epsilon_A f_A L_A^{-1} & f_{AU} \\ f_{UH} & f_{UA} & f_{UU} - \epsilon_U f_U L_U^{-1} \end{vmatrix} < 0. \quad (A7)$$

Although the production function has constant returns, it is easy to verify that $\Delta < 0$ because the monopsony market structure introduces concavity into the profit function.

The solution of the system in (A7) yields:

$$\frac{dL_H}{d\epsilon_U} = \frac{1}{\Delta} \frac{dMC_U}{d\epsilon_U} [f_{HA}f_{AU} - f_{AA}f_{HU} + f_{HU}f_A L_A^{-1} \epsilon_A], \quad (A8)$$

$$\frac{dL_A}{d\epsilon_U} = \frac{1}{\Delta} \frac{dMC_U}{d\epsilon_U} [f_{HA}f_{HU} - f_{HH}f_{AU} + f_{AU}f_H L_H^{-1} \epsilon_H], \quad (A9)$$

$$\frac{dL_U}{d\epsilon_U} = \frac{1}{\Delta} \frac{dMC_U}{d\epsilon_U} [(f_{HH}f_{AA} - f_{HA}^2) - f_{HH}f_A L_A^{-1} \epsilon_A - f_{AA}f_H L_H^{-1} \epsilon_H + f_H f_A L_H^{-1} L_A^{-1} \epsilon_H \epsilon_A]. \quad (A10)$$

It is convenient to rewrite equations (A8) - (A10) in terms of the elasticity of complementarity between inputs i and j . This elasticity is defined by $c_{ij} = f_{ij} f / f_i f_j$. Substituting this elasticity for the various f_{ij} in equations (A8) - (A10) yields:

$$\frac{dL_H}{d\epsilon_U} = \frac{f_H f_A^2 f_U}{\Delta f^2} \frac{dMC_U}{d\epsilon_U} \left[c_{HA} c_{AU} - c_{AA} c_{HU} + \frac{c_{HU}}{\theta_A} \epsilon_A \right], \quad (A11)$$

$$\frac{dL_A}{d\epsilon_U} = \frac{f_H^2 f_A f_U}{\Delta f^2} \frac{dMC_U}{d\epsilon_U} \left[c_{HA} c_{HU} - c_{HH} c_{AU} + \frac{c_{AU}}{\theta_H} \epsilon_H \right], \quad (A12)$$

$$\frac{dL_U}{d\epsilon_U} = \frac{f_H^2 f_A^2}{\Delta f^2} \frac{dMC_U}{d\epsilon_U} \left[(c_{HH} c_{AA} - c_{HA}^2) - \frac{1}{\theta_H \theta_A} (\theta_H c_{HH} \epsilon_A + \theta_A c_{AA} \epsilon_H - \epsilon_H \epsilon_A) \right], \quad (A13)$$

where $\theta_j = q_j L_j / q$. The sign of the derivatives in (A11) and (A12) can be established by using the theorem that $\sum_j \theta_j c_{ij} = 0$. Part of the bracketed term in equation (A11) can be rewritten as a principal minor:

$$\begin{aligned} c_{HA} c_{AU} - c_{AA} c_{HU} &= \left[\frac{(-\theta_A c_{AA} - \theta_U c_{AU})}{\theta_H} c_{AU} - c_{AA} \frac{(-\theta_A c_{AU} - \theta_U c_{UU})}{\theta_H} \right], \\ &= \frac{\theta_U}{\theta_H} (c_{AA} c_{UU} - c_{AU}^2) > 0. \end{aligned} \quad (A14)$$

Similarly, part of equation (A12) can be rewritten as:

$$\begin{aligned} c_{HA} c_{HU} - c_{HH} c_{AU} &= \left[\frac{(-\theta_H c_{HH} - \theta_U c_{HU})}{\theta_A} c_{HU} - c_{HH} \frac{(-\theta_H c_{HU} - \theta_U c_{UU})}{\theta_A} \right], \\ &= \frac{\theta_U}{\theta_A} (c_{HH} c_{UU} - c_{HU}^2) > 0. \end{aligned} \quad (A15)$$

Equations (9) and (10) in the text follow by substituting these expressions into (A11) and (A12).

A.3 Regularization in a Competitive Labor Market

Suppose the labor market is competitive and firms are homogeneous. Let τ_i be the payroll tax for a type- i worker and the tax is imposed on firms. Employers choose the profit-maximizing level of employment in each sector given the market-determined wages (w_1, w_2, w_3). The representative firm hires up to the point where $f_i = w_i(1 + \tau_i)$. Market equilibrium is defined by the intersection of the (tax-adjusted) marginal product and aggregate supply curve for each labor type:

$$(1 + \tau_i)^{-1} f_i = P_i^{-\epsilon_i} L_i^{\epsilon_i}, \quad i = 1, 2, 3. \quad (A16)$$

A regularization policy increases the hiring cost of undocumented workers (τ_U). This change leads to a system of differential equations closely related to (A5). The employment impact of a change in τ_U on the employment of type- i workers is similar to the impact described in equations (A11) – (A13), except that $dMC_U/d\epsilon_U$ is replaced by $dMC_U/d\tau_U = MC_U/(1 + \tau_U)$. Labor demand declines for all inputs because regularization increases the marginal cost of an undocumented worker, and the scale effect spills over to all labor markets given the complementarities between undocumented workers and other workers. Further, the upward-sloping supply curves ensure that the drop in demand lowers the wage of all groups as well.

A.4 Regularization as a Mix of Supply Shocks

An alternative way of modeling a regularization policy examines the impact of providing “papers” that allow some workers to instantly switch from the undocumented group to the authorized group. To evaluate the impact of a “supply shock” regularization policy, we allow for potential complementarities between the two groups in the authorized workforce: natives and legal immigrants. The key insights can be easily grasped by focusing on the low-skill labor market (so that the quantity of other inputs is held constant).

The linear homogeneous production function is $Q = f(L_N, L_M, L_U)$, where L_N gives the number of low-skill native workers, and L_M gives the number of low-skill legal immigrant workers. Consider what happens to *native* employment if we move a single worker from the undocumented to the legal immigrant sector. Differentiating the first-order conditions and using the theorem that $\sum_j \theta_j c_{ij} = 0$ yields:

$$\frac{dL_N}{dP_M} = \frac{-\bar{\kappa}}{\Delta} \frac{P_M^{-1}}{\theta_U} \epsilon_M \left[\frac{\theta_N \theta_U}{\theta_M} (c_{NN} c_{UU} - c_{NU}^2) + c_{NM} \epsilon_U \right] > 0, \quad (A17)$$

$$\frac{dL_N}{dP_U} = \frac{-\bar{\kappa}}{\Delta} \frac{P_U^{-1}}{\theta_M} \epsilon_U \left[\frac{\theta_N \theta_M}{\theta_H} (c_{MM} c_{UU} - c_{MU}^2) + c_{NU} \epsilon_M \right] > 0, \quad (A18)$$

where $\bar{\kappa} = f_N f_M^2 f_U^2 / f^2 > 0$; and $\Delta < 0$. The bracketed term in (A17) is positive if the elasticity of complementarity between natives and legal immigrants is not “too” negative, and the bracketed term in (A18) is positive if the elasticity of complementarity between natives and undocumented immigrants is positive.

The magnitude of the impacts in (A18) and (A19) depends on the size of the sector where the supply shocks are taking place (as measured by shares θ_M and θ_U , and

the baseline supplies P_M^{-1} and P_U^{-1}). To abstract from this scale effect, suppose that $P_M^{-1}/\theta_U = P_U^{-1}/\theta_M = P^*$, so that per-capita output shares are roughly equal for undocumented and legal immigrants. The impact of a policy R_P that moves a worker from the undocumented sector to the legal immigrant sector is:

$$\frac{dL_N}{dR_P} = \frac{dL_N}{dP_M} - \frac{dL_N}{dP_U} = \frac{P^* \bar{\kappa}}{\Delta} \left[\frac{\theta_N \theta_U}{\theta_M} [c_{NN} c_{UU} - c_{NU}^2] (\epsilon_U - \epsilon_M) + \epsilon_M \epsilon_U (c_{NU} - c_{NM}) \right]. \quad (A19)$$

The first term in (A19) isolates the effect resulting from differences in monopsony power between the two sectors. Regularization *reduces* the number of native workers employed if $\epsilon_U > \epsilon_M$. Equations (A17) and (A18) show why. The impact on native employment of a change in the number of type- i workers depends directly on ϵ_i . The reduction in native employment when an undocumented worker exits the market is large because ϵ_U is large. At the same time, the increase in native employment when a legal immigrant enters the market is small because ϵ_M is small. Native employment exhibits “excess sensitivity” to changes in the supply of undocumented workers.

Equation (A19) shows that the gap between the elasticities of complementarity c_{NU} and c_{NM} also determines the impact. The second term in (A19) will also be negative if $c_{NU} > c_{NM}$, so that there is greater complementarity between natives and undocumented immigrants than between natives and legal immigrants.

A.5 Generalizing the Production Function

The concave linear homogeneous production function is $Q = f(L_H, L_N, L_M, L_U)$. The group’s inverse supply function is $w_i = P_i^{-\epsilon_i} L_i^{\epsilon_i}$. The second-order conditions to the monopsonist’s profit-maximization problem require that:

$$\Delta^* = \begin{vmatrix} f_{HH} - \epsilon_H Q_H L_H^{-1} & f_{HN} & f_{HM} & f_{HU} \\ f_{NH} & f_{NN} - \epsilon_N f_N L_N^{-1} & f_{NM} & f_{NU} \\ f_{MH} & f_{MN} & f_{MM} - \epsilon_M f_M L_M^{-1} & f_{MU} \\ f_{UH} & f_{UN} & f_{UM} & f_{UU} - \epsilon_U f_U L_U^{-1} \end{vmatrix} > 0. \quad (A20)$$

The impact of a change in ϵ_U on undocumented employment is:

$$\frac{dL_U}{d\epsilon_U} = \frac{dMC_U}{d\epsilon_U} \frac{|\Delta_{44}^*|}{\Delta^*} < 0, \quad (A21)$$

where Δ_{44}^* is the principal minor obtained by deleting the fourth row and fourth column of the matrix in (A20). The second order conditions imply that $|\Delta_{44}^*| < 0$. A reduction in ϵ_U , therefore, increases the employment of undocumented workers.

The addition of a single input greatly complicates the algebra. We rely on both a property of the (negative semidefinite) Hessian of the production function and on a simplification of the technology to sign the other employment effects. First, note that all second-order minors of the production function have quadratic forms that satisfy:

$$[x \ y] \begin{bmatrix} f_{ii} & f_{ij} \\ f_{ji} & f_{jj} \end{bmatrix} \begin{bmatrix} x \\ y \end{bmatrix} < 0, \quad (A22)$$

for any vector $[x \ y] \neq 0$. Let $x = \sqrt{f}/f_i$ and $y = -\sqrt{f}/f_j$. Equation (A22) implies $(c_{ii} - 2c_{ij} + c_{jj}) < 0$. A sufficient condition for the inequality to be satisfied is $c_{ii} < c_{ij}$ (i.e., own elasticities are not only negative, but more negative than cross-elasticities). We impose this restriction in what follows.

Second, we use a nested version of the production function:

$$Q = f(L_H, L_D), \quad (\text{A23})$$

$$L_D = g(L_A, L_U), \quad (\text{A24})$$

$$L_A = h(L_N, L_M), \quad (\text{A25})$$

where L_D denotes the total efficiency units of low-skill workers and L_A the number attributable to low-skill authorized workers. The nesting restricts interactions among inputs. It implies that $c_{HD} = c_{HN} = c_{HM} = c_{HU}$ and $c_{AU} = c_{NU} = c_{MU}$. Further, the linear homogeneity assumption implies $c_{HD} > 0$. We assume that authorized low-skill workers are complements with undocumented workers ($c_{AU} > 0$).

We illustrate the use of these restrictions by showing how a change in ϵ_U affects the employment of low-skill native workers. This employment effect is given by:

$$\frac{dL_N}{d\epsilon_U} = \frac{1}{\Delta^*} \frac{dMC_U}{d\epsilon_U} \begin{vmatrix} f_{HH} - \epsilon_H q_H L_H^{-1} & f_{HM} & f_{HU} \\ f_{NH} & f_{NM} & f_{NU} \\ f_{MH} & f_{MM} - \epsilon_M f_M L_M^{-1} & f_{MU} \end{vmatrix}. \quad (\text{A26})$$

The sign of $dL_N/d\epsilon_U$ is the same as the sign of the determinant in (A26), denoted by Z . The cofactor expansion of this determinant is:

$$Z = f_{HU} \begin{vmatrix} f_{NH} & f_{NM} \\ f_{MH} & f_{MM} - \epsilon_M f_M L_M^{-1} \end{vmatrix} - f_{NU} \begin{vmatrix} f_{HH} - \epsilon_H f_H L_H^{-1} & f_{HM} \\ f_{MH} & f_{MM} - \epsilon_M f_M L_M^{-1} \end{vmatrix} + f_{MU} \begin{vmatrix} f_{HH} - \epsilon_H f_H L_H^{-1} & f_{HM} \\ f_{NH} & f_{NM} \end{vmatrix}. \quad (\text{A27})$$

Denote each term in the cofactor expansion by Z_1 , Z_2 , and Z_3 . Expressed in terms of elasticities of complementarity:

$$Z_1 = \kappa c_{HU} \left[c_{NH} c_{MM} - c_{MH} c_{NM} - c_{NH} \frac{\epsilon_M}{\theta_M} \right], \quad (\text{A28})$$

$$Z_2 = -\kappa c_{NU} \left[c_{HH} c_{MM} - c_{MH}^2 - c_{HH} \frac{\epsilon_M}{\theta_M} - c_{MM} \frac{\epsilon_H}{\theta_H} + \frac{\epsilon_M \epsilon_H}{\theta_M \theta_H} \right], \quad (\text{A29})$$

$$Z_3 = \kappa c_{MU} \left[c_{HH} c_{NM} - c_{NH} c_{HM} - c_{NM} \frac{\epsilon_H}{\theta_H} \right], \quad (\text{A30})$$

where $\kappa = f_H^2 f_N f_M^2 f_U / f^3 > 0$. Using the restrictions imposed by the nested specification and the property that $c_{ii} < c_{ij}$, it follows that the bracketed term in equation (A29) is negative. Similarly, the sum of (A29) and (A30) is:

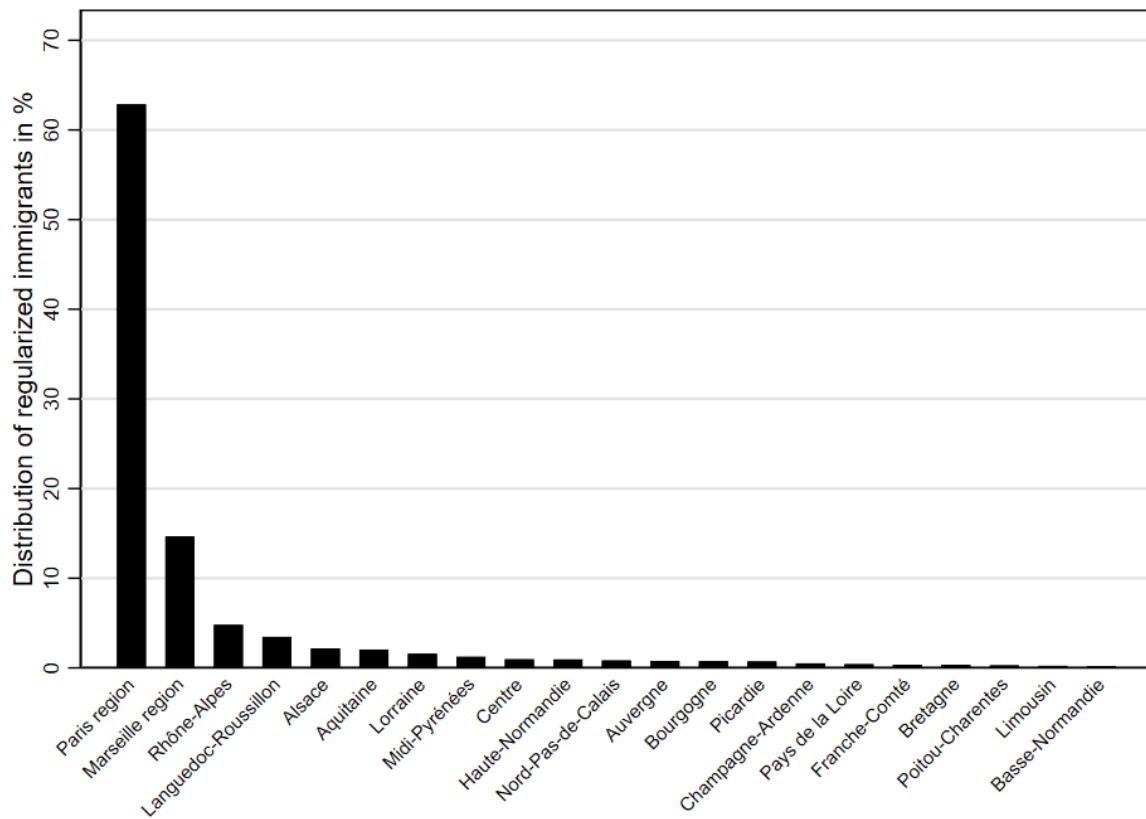
$$Z_2 + Z_3 = \kappa c_{NU} \left[\left(c_{HH} - \frac{\epsilon_H}{\theta_H} \right) (c_{MN} - c_{MM}) + c_{HH} \frac{\epsilon_M}{\theta_M} - \frac{\epsilon_M \epsilon_H}{\theta_M \theta_H} \right], \quad (\text{A31})$$

which is also negative. The determinant Z , therefore, is negative, implying $dL_N/d\epsilon_U < 0$. The same approach can be used to prove that both $dL_H/d\epsilon_U < 0$ and $dL_M/d\epsilon_U < 0$.

Suppose all low-skill workers are perfect substitutes and the production function is $Q = f(L_H, L_N + L_M + L_U)$. This specification implies that $c_{HN} = c_{HM} = c_{HU} > 0$; $c_{NN} = c_{MM} = c_{UU} = c_{NM} = c_{NU} = c_{MU} < 0$; and $c_{ii}c_{jj} - c_{ij}^2 = 0$. If we insert these restrictions into equations (A28) – (A30), it follows that $dL_N/d\epsilon_U > 0$. The employment effects in the perfect substitution case are:

$$\frac{dL_H}{d\epsilon_U} < 0, \quad \frac{dL_N}{d\epsilon_U} > 0, \quad \frac{dL_M}{d\epsilon_U} > 0, \quad \frac{dL_U}{d\epsilon_U} < 0, \quad \text{and} \quad \frac{d(L_N + L_M + L_U)}{d\epsilon_U} < 0. \quad (\text{A32})$$

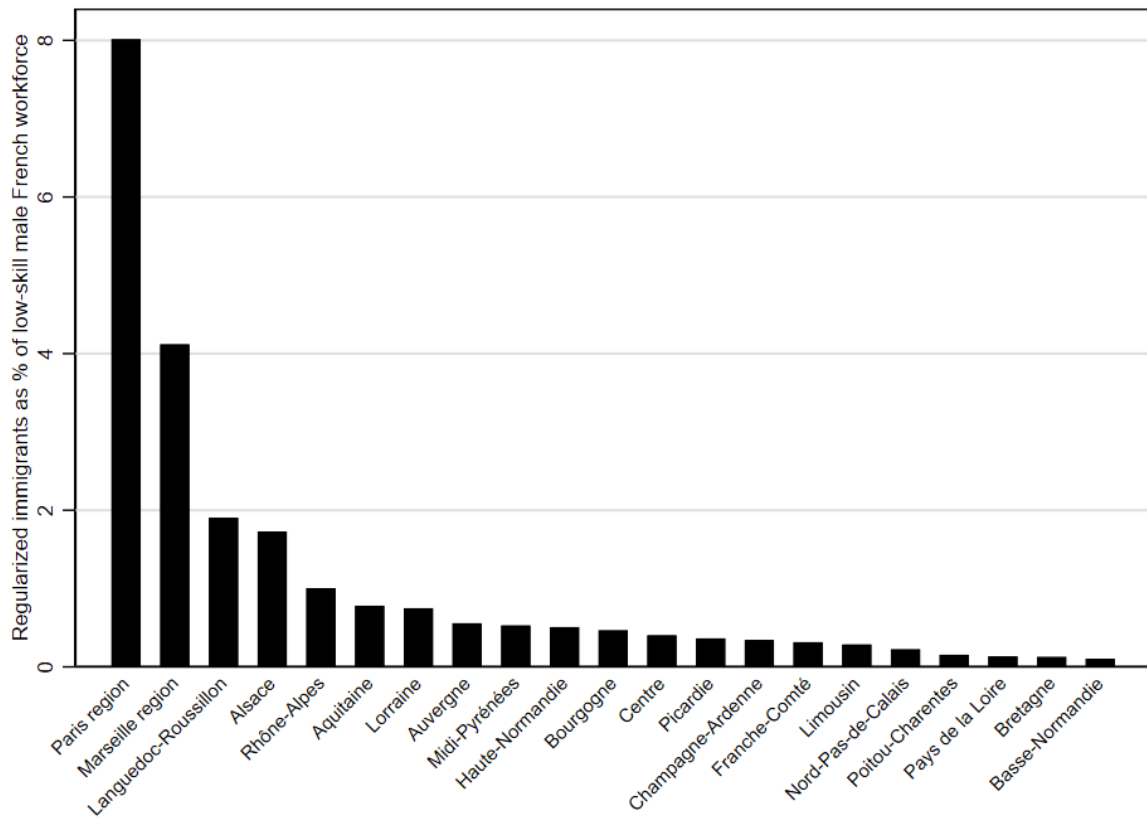
Figure 1: Distribution of the regularized immigrants across regions (in percent)



Notes: The sample consists of 102,012 immigrants regularized between 1981 and 1983. It excludes the Algerian immigrants, seasonal workers, and retail traders whose applications were accepted during the amnesty program. The Paris region refers to Île-de-France. The Marseille region refers to Provence-Alpes-Côte d'Azur.

Source: Cealis et al. (1983, p. 18).

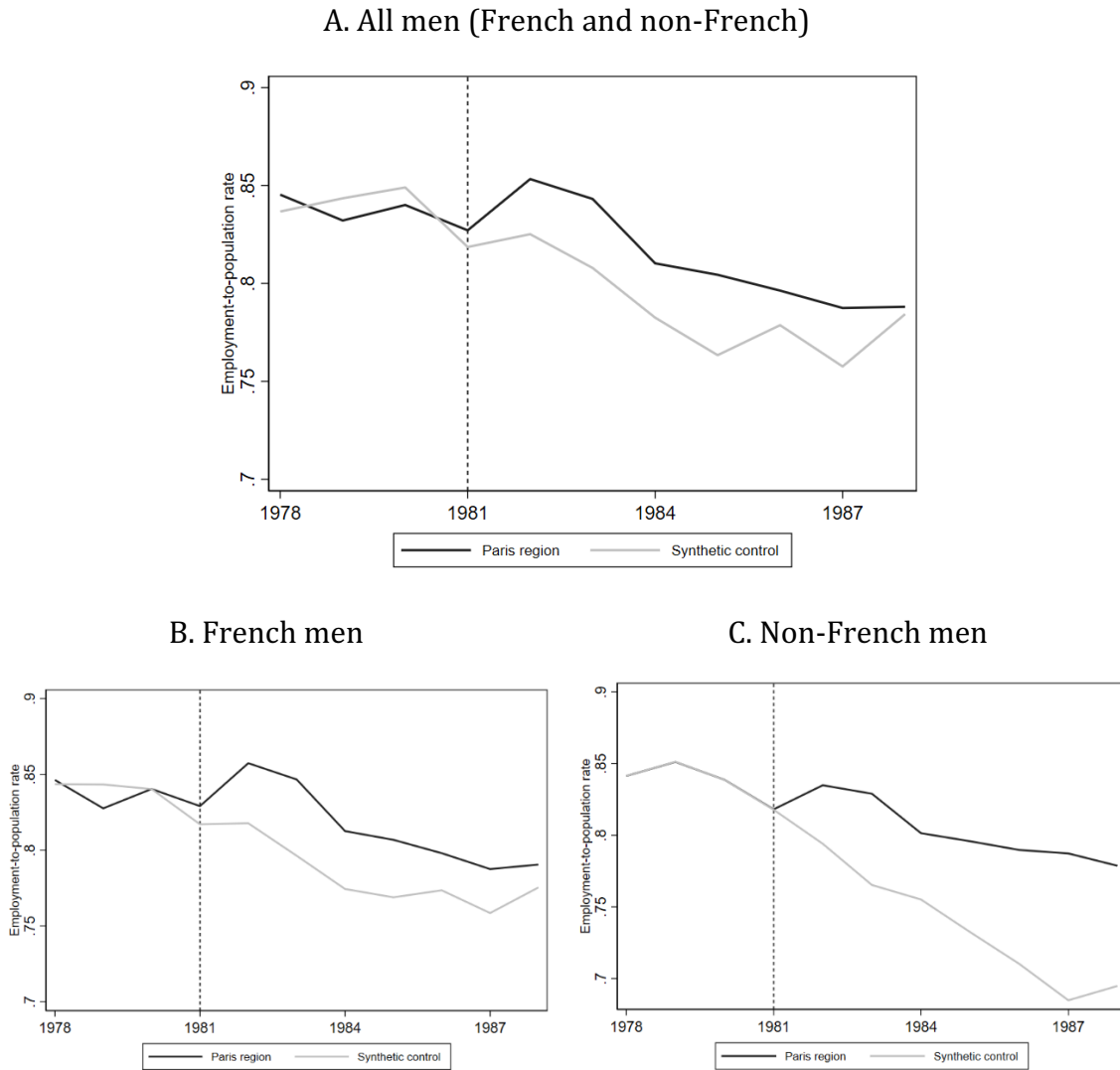
Figure 2: Number of regularized immigrants relative to low-skill male French workers across regions (in percent)



Notes: Each bar presents the number of immigrants regularized between 1981 and 1983 relative to the number of male workers having a primary education in 1982. The sample of regularized immigrants excludes the Algerian immigrants, seasonal workers, and retail traders whose applications were accepted during the amnesty program. The number of low-skill male workers is drawn from the 1982 French census and consists of men aged 18-64 who are in the workforce and not enrolled in school. The Paris region refers to Île-de-France. The Marseille region refers to Provence-Alpes-Côte d'Azur.

Source: Cealis et al. (1983, p. 18), and 1982 census.

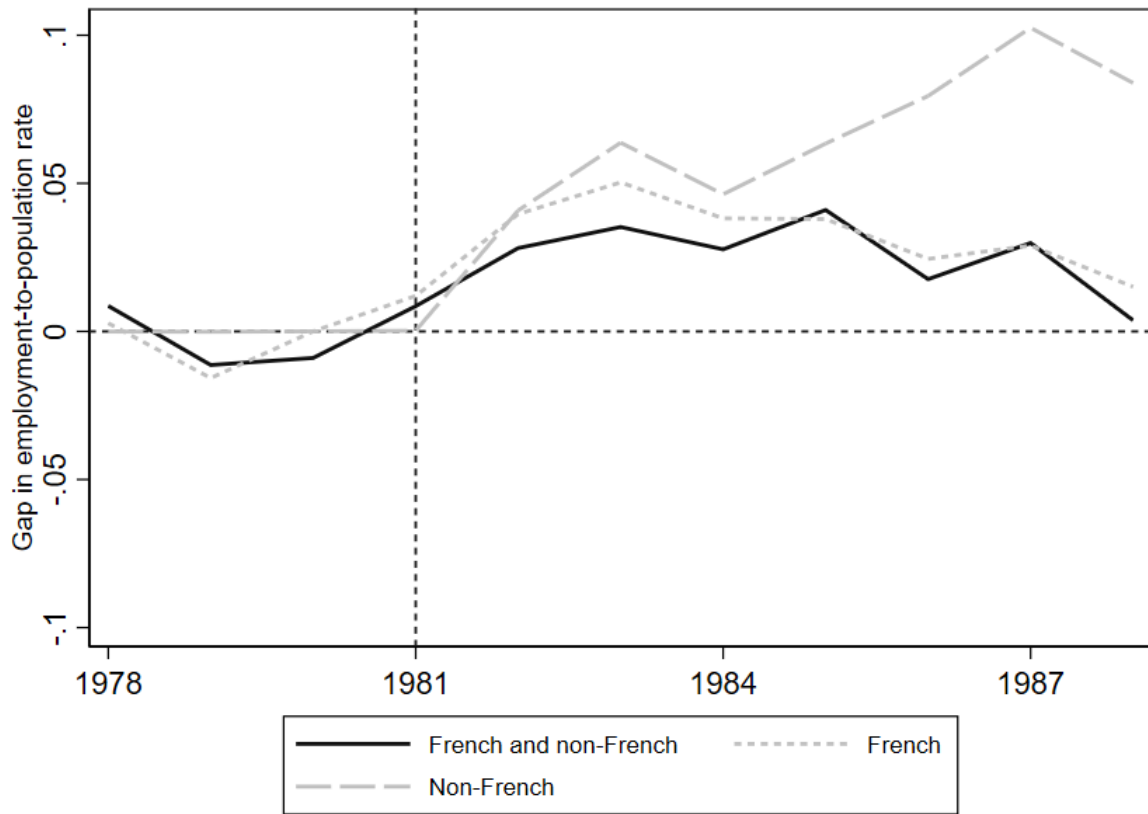
Figure 3: Trends in the employment rate of low-educated men in the treated and synthetic regions



Notes: The graphs show the evolution of the employment-to-population ratio of low-educated men in the Paris region and its synthetic counterpart over the 1978-1988 period. Panel A focuses on the employment rate for all men, while Panels B and C consider French and non-French men, respectively. The weights used to construct the synthetic control are chosen to minimize the distance with the Paris region in terms of employment-to-population ratio and growth in employment and unemployment rates between 1979 and 1981.

Source: French labor force surveys.

Figure 4: Yearly gaps in the employment rate of low-educated men between the Paris region and its synthetic counterpart, by nationality group

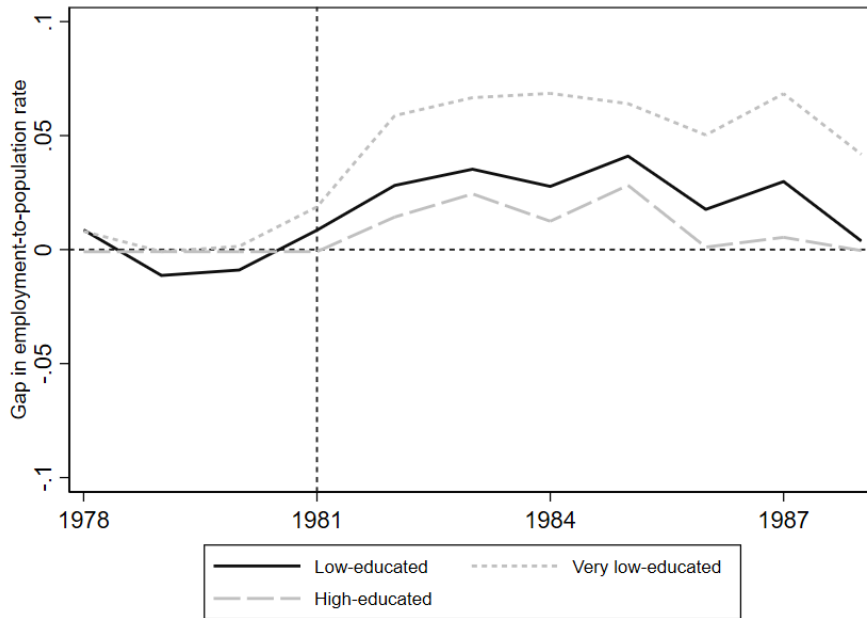


Notes: The figure shows the yearly gaps in the employment-to-population ratio of men between the Paris region and its synthetic counterpart over the 1978-1988 period.

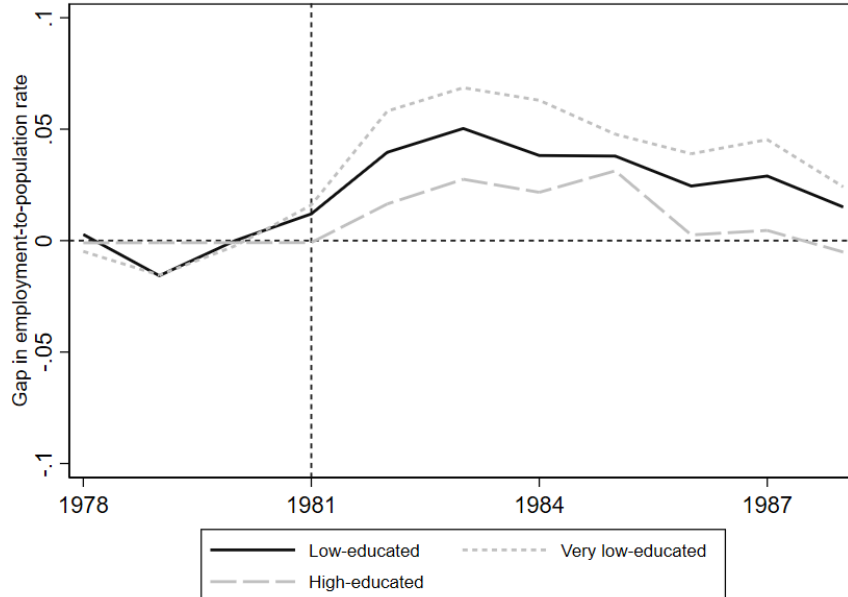
Source: French labor force surveys.

Figure 5: Yearly gaps in the employment rate between the Paris region and its synthetic counterpart, by education

A. Men

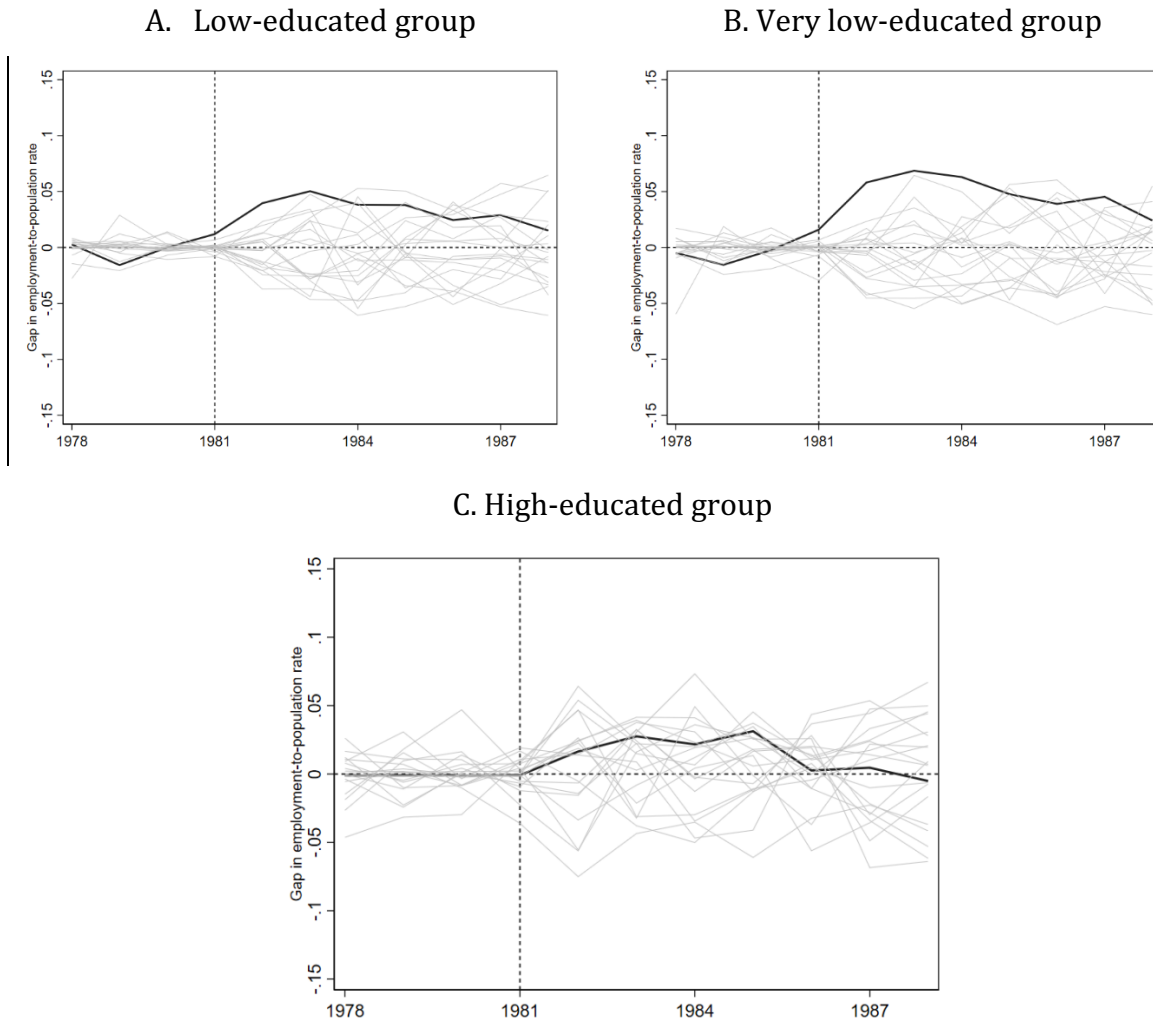


B. French men



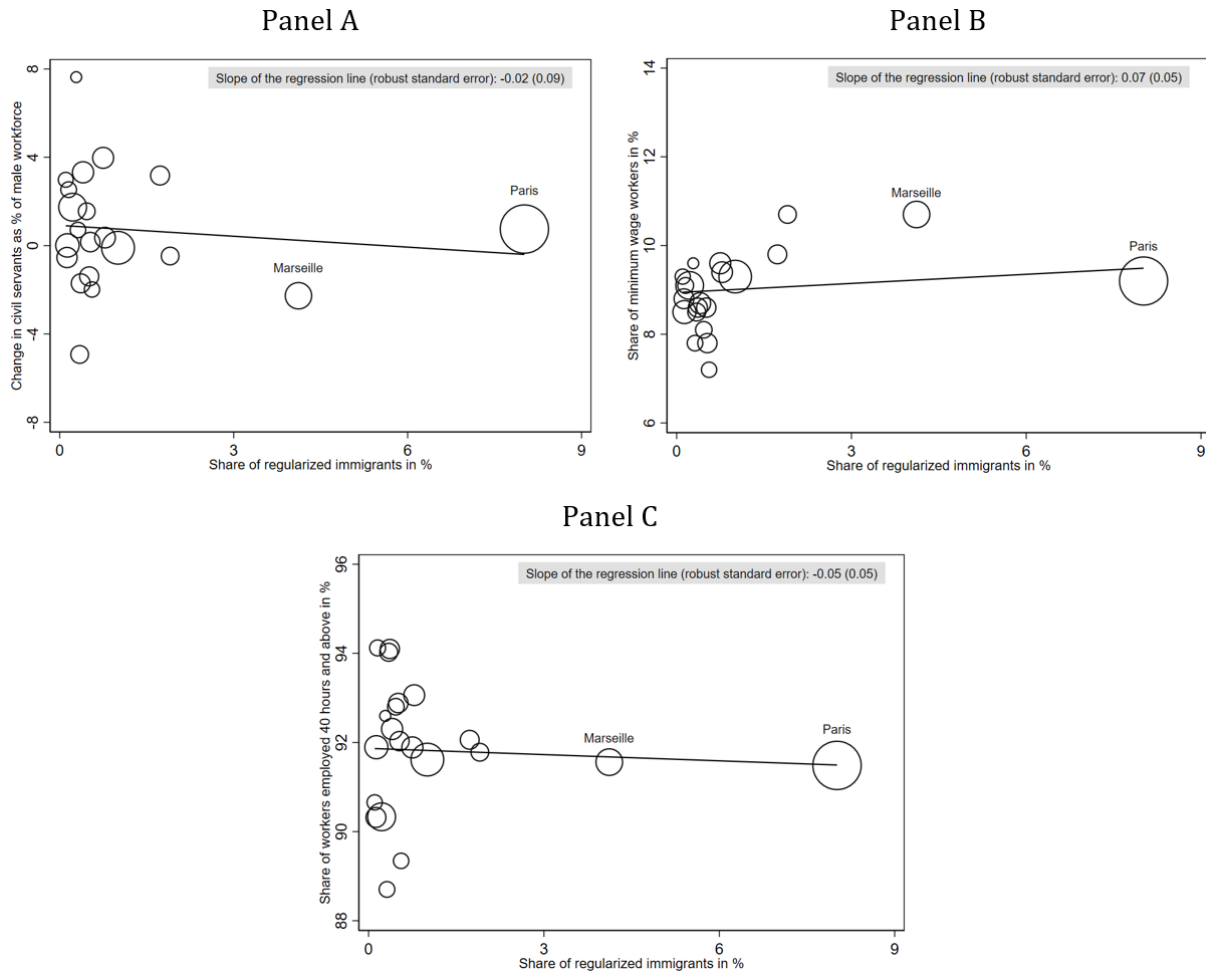
Notes: The figure shows the yearly gaps in the employment-to-population ratio of men between the Paris region and its synthetic counterpart over the 1978-1988 period for different education groups. Panel A uses the sample of all men, and Panel B uses the sample of French men. The low-educated category includes individuals having less than a baccalaureate degree. The very-low educated group refers to individuals having a primary education only. The high-educated group refers to individuals with a baccalaureate degree or more.

Source: French labor force surveys.

Figure 6: Permutation tests

Notes: Each graph shows the trend in the employment gap in the Paris region and the placebo gaps for the remaining 19 regions. The bold line represents the Paris region. Panel A uses the sample of French men having less than a baccalaureate degree; Panel B uses the sample of French men having a primary education only; and Panel C uses the sample of French men with at least a baccalaureate degree. Source: French labor force surveys.

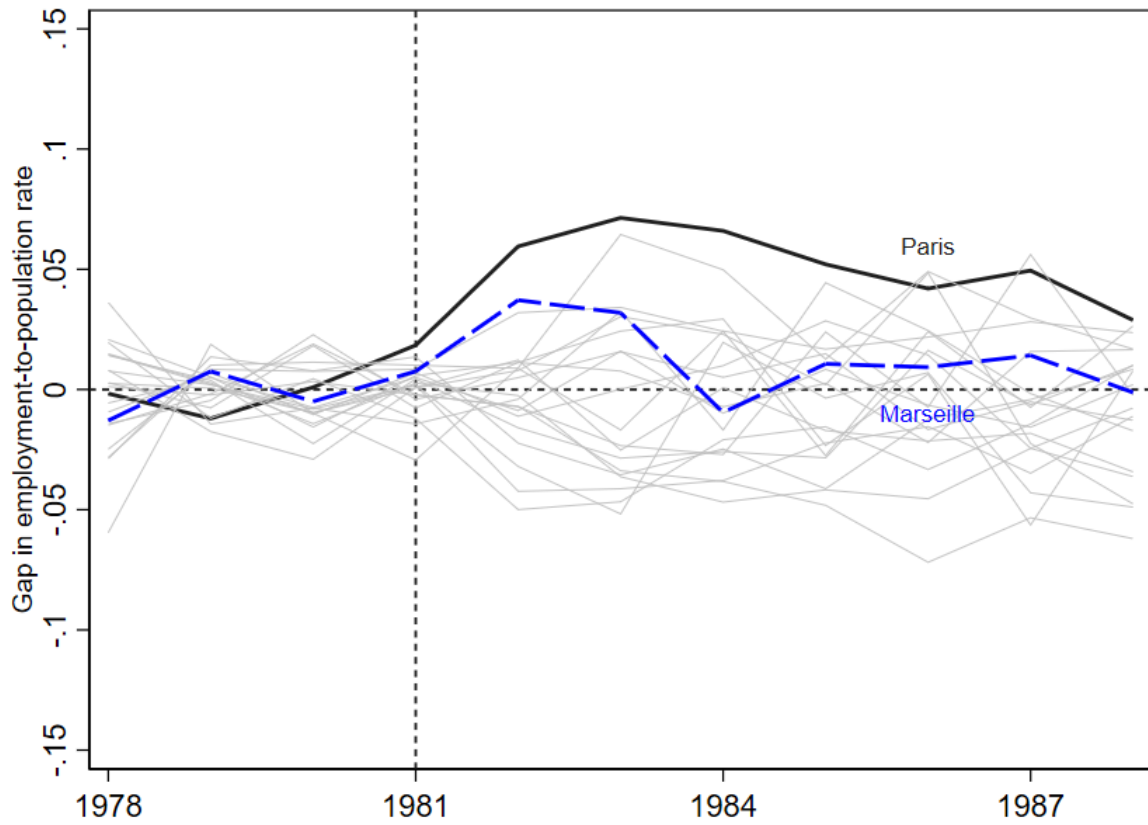
Figure 7: The share of regularized immigrants and the size of male workers in the public sector, paid at the minimum wage or working at least 40 hours



Notes: The unit of observation in the scatter diagrams is a region cell. In Panel A, the y-axis represents the change in the number of male civil servants (i.e., workers in public administration) between 1981 and 1983 relative to the number of male workers in 1981. In Panel B, the share of minimum-wage workers is defined as the size of full-time male workers earning less than 3,000 Francs per month relative to the size of the male full-time workforce. We use this wage threshold since Bourit, Hernu and Perrot (1983, p. 16) estimate that the minimum monthly wage (net of employee payroll tax contributions) for a full-time worker is 3,065 francs by the end of 1982. In panel C, the y-axis represents the share of male workers employed at least 40 hours per week. This threshold on hours worked follows Crépon and Kramarz (2002) and is consistent with the fact that the reduction of the workweek should only affect those workers employed exactly 40 hours and above. In the three panels, the x-axis represents the number of immigrants regularized between 1981 and 1983 relative to the number of male workers having less than a baccalaureate degree in 1982. The size of the circles is proportional to the size of the total workforce in the region.

Source: French labor force surveys, 1982 census, DADS in 1980.

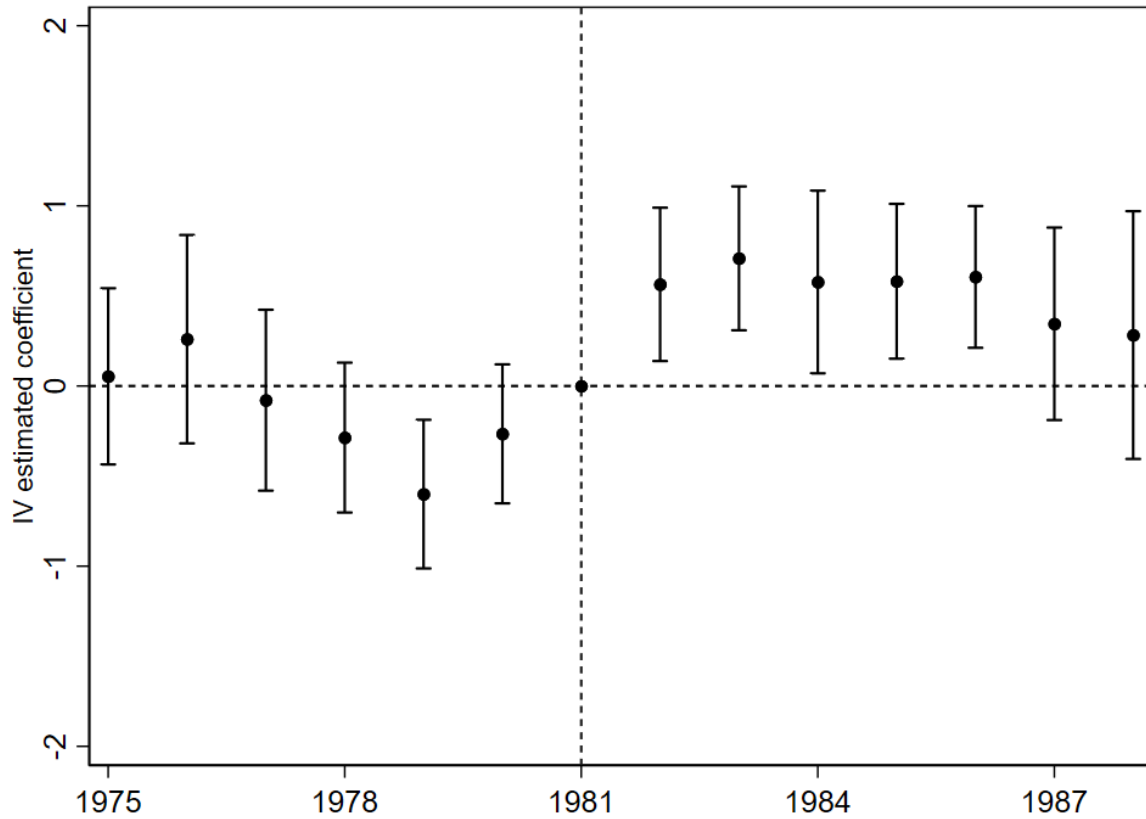
Figure 8: Impact of regularization in the Paris and Marseille regions



Notes: The graph shows the employment gap in the Paris and Marseille regions relative to their synthetic counterparts, as well as placebo gaps in the remaining 19 regions, using the sample of French men having a primary education only.

Source: French labor force surveys.

Figure 9: Impact of the share of regularized immigrants on the employment-to-population ratio



Notes: The graph plots the estimated IV coefficients of the interaction terms between the regional share of newly regularized immigrants and year fixed effects, and the corresponding 95 percent confidence intervals based on robust standard errors (vertical bars). We include region and year fixed effects. The year before the regularization policy (i.e., 1981) forms the excluded fixed effect, so the estimates are normalized to zero in that year. The regression has 294 observations (21 regions and 14 years) and is weighted using cell size.

Source: French labor force surveys and 1962 census.

Figure 10: The regularization surplus

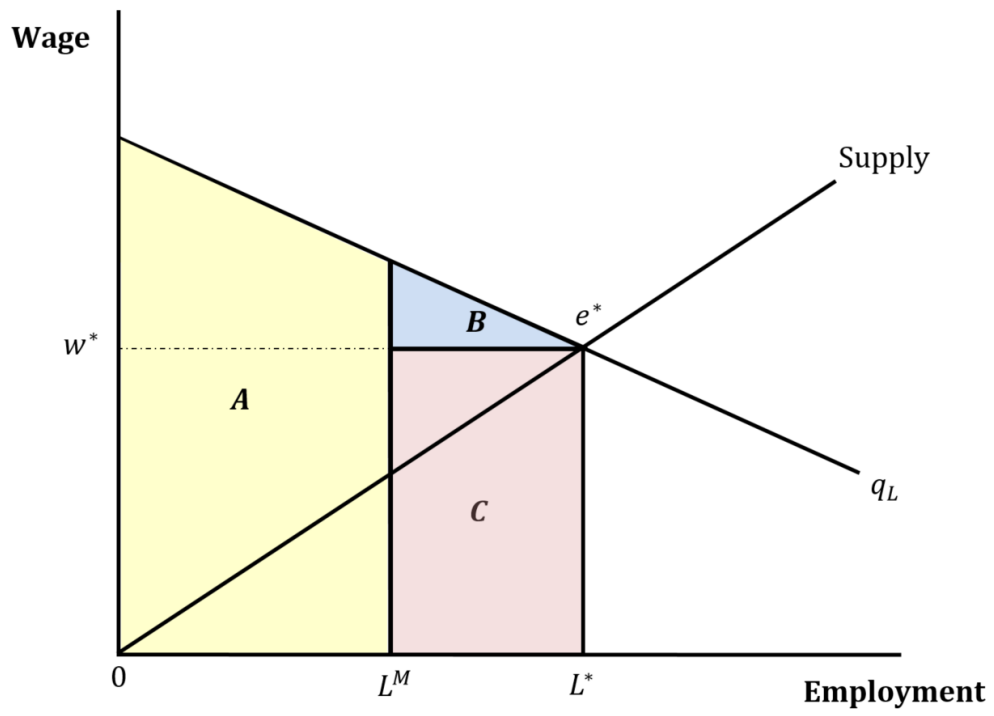


Table 1: Wage distribution of immigrants before and after regularization in the Paris region

	Before regularization	After regularization
	(1)	(2)
Less than 3,000 francs	44.2	14.7
3,000 - 3,999 francs	32.4	45.3
4,000 - 4,999 francs	11.4	25.1
More than 5,000 francs	1.7	12.2
No answer	10.3	1.8

Note: The table reports the wage distribution of immigrants before and after the regularization of their status across four (monthly) wage intervals. The median wage in France at the time was 4,830 francs (Bourit, Hernu, and Perrot, 1982).

Source: Marie (1984, p.25).

Table 2: Impact on the employment-to-population ratio using the synthetic region

	Men			Women		
	All	French	Non-French	All	French	Non-French
	(1)	(2)	(3)	(4)	(5)	(6)
A. Low-educated						
1982-1983	0.032** (0.01)	0.045*** (0.01)	0.052*** (0.02)	0.025* (0.01)	0.029* (0.01)	0.036 (0.05)
1984-1988	0.025** (0.01)	0.029*** (0.01)	0.075*** (0.02)	0.013 (0.02)	0.020 (0.02)	0.152** (0.07)
B. High-educated						
1982-1983	0.020*** (0.01)	0.023*** (0.01)	0.130*** (0.02)	-0.012 (0.01)	-0.016* (0.01)	0.207** (0.09)
1984-1988	0.010 (0.01)	0.012 (0.01)	0.130** (0.06)	-0.001 (0.01)	0.003 (0.01)	0.161** (0.07)

Notes: The data consist of annual observations for the Paris and synthetic regions between 1978 and 1988. The pre-treatment period has 4 years from 1978 to 1981, while the post-treatment period has 7 years from 1982 to 1988. The regressions have 22 observations. The table reports the estimated coefficients on the interaction term between the Paris indicator variable and the post-treatment fixed effects. Robust standard errors are reported in parentheses. All regressions include vectors of region and year fixed effects. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level. Source: French labor force surveys.

Table 3: Impact on the employment-to-population ratio using all regions

	Men			Women		
	All	French	Non-French	All	French	Non-French
	(1)	(2)	(3)	(4)	(5)	(6)
A. Low-educated						
1982-1983	0.037*** (0.01)	0.040*** (0.01)	0.031** (0.01)	0.014 (0.01)	0.014 (0.01)	0.025 (0.02)
<i>Wild bootstrap p-value</i>	<i>0.02</i>	<i>0.03</i>	<i>0.07</i>	<i>0.19</i>	<i>0.22</i>	<i>0.15</i>
1984-1988	0.044*** (0.01)	0.044*** (0.01)	0.060*** (0.01)	0.001 (0.01)	0.006 (0.01)	-0.006 (0.02)
<i>Wild bootstrap p-value</i>	<i>0.03</i>	<i>0.05</i>	<i>0.00</i>	<i>0.84</i>	<i>0.34</i>	<i>0.64</i>
B. High-educated						
1982-1983	0.021*** (0.01)	0.021*** (0.01)	0.083** (0.04)	-0.009 (0.01)	-0.011 (0.01)	0.072 (0.08)
<i>Wild bootstrap p-value</i>	<i>0.08</i>	<i>0.09</i>	<i>0.01</i>	<i>0.33</i>	<i>0.29</i>	<i>0.43</i>
1984-1988	0.010 (0.01)	0.010 (0.01)	0.087*** (0.03)	-0.001 (0.01)	0.001 (0.01)	0.095 (0.06)
<i>Wild bootstrap p-value</i>	<i>0.32</i>	<i>0.32</i>	<i>0.02</i>	<i>0.92</i>	<i>0.86</i>	<i>0.03</i>

Notes: The data consist of annual observations for each region between 1978 and 1988. The pre-treatment period has 4 years from 1978 to 1981, while the post-treatment period has 7 years from 1982 to 1988. The regressions have 220 observations. The table reports the estimated coefficients on the interaction term between the Paris indicator variable and the post-treatment fixed effects. Robust standard errors are reported in parentheses. All regressions include region and time fixed effects and are weighted by cell size. Wild bootstrap *p*-values in italics are computed using 1,000 bootstrap replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.

Source: French labor force surveys.

Table 4: Impact on the employment-to-population ratio using the synthetic difference-in-differences approach

	Men			Women		
	All	French	Non-French	All	French	Non-French
	(1)	(2)	(3)	(4)	(5)	(6)
A. Low-educated						
1982-1983	0.037** (0.02)	0.041** (0.02)	0.041 (0.11)	0.016 (0.02)	0.012 (0.02)	0.038 (0.12)
1984-1988	0.047** (0.02)	0.049** (0.02)	0.071 (0.06)	0.002 (0.02)	0.004 (0.02)	0.051 (0.19)
B. High-educated						
1982-1983	0.022 (0.02)	0.024 (0.02)	0.036 (0.48)	-0.012 (0.04)	-0.017 (0.04)	0.051 (0.32)
1984-1988	0.012 (0.02)	0.012 (0.02)	0.035 (0.34)	-0.002 (0.03)	-0.003 (0.02)	-0.032 (0.21)

Notes: The table relies on the synthetic difference-in-differences approach introduced by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) to estimate the impact of the regularization policy on the employment-to-population rate. We exploit annual observations for the Paris and untreated regions between 1978 and 1988. The pre-treatment period has 4 years from 1978 to 1981, while the post-treatment period has 7 years from 1982 to 1988. The table reports the estimated average treatment effects in Paris over the 1982-1983 and 1984-1988 periods. We use the “placebo method” to estimate the standard errors in parentheses. All results are based on 100 simulation replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.

Source: French labor force surveys.

Table 5: Impact on the wage of low-educated French workers

	Cross-sectional results					
	All workers		Identification at infinity sample		Panel results	
	Synthetic	All regions	Synthetic	All regions	Synthetic	All regions
	(1)	(2)	(3)	(4)	(5)	(6)
A. French men						
1984	-0.05*** (0.02)	-0.03* (0.02)	0.03** (0.01)	0.03* (0.02)	0.05* (0.03)	0.04** (0.02)
<i>Wild bootstrap p-value</i>	-	<i>0.29</i>	-	<i>0.32</i>	-	<i>0.28</i>
1985-1988	-0.05** (0.02)	-0.03** (0.02)	0.03* (0.01)	0.03** (0.01)	0.04 (0.03)	0.03 (0.02)
<i>Wild bootstrap p-value</i>	-	<i>0.13</i>	-	<i>0.05</i>	-	<i>0.17</i>
B. French women						
1984	0.01 (0.01)	-0.01 (0.01)	0.02 (0.02)	-0.03* (0.01)	0.09*** (0.02)	0.02 (0.02)
<i>Wild bootstrap p-value</i>	-	<i>0.56</i>	-	<i>0.22</i>	-	<i>0.43</i>
1985-1988	0.01 (0.02)	-0.01 (0.44)	-0.00 (0.02)	0.00 (0.01)	0.05* (0.03)	0.02 (0.01)
<i>Wild bootstrap p-value</i>	-	<i>0.44</i>	-	<i>0.93</i>	-	<i>0.41</i>

Notes: The data consist of annual observations for each region between 1978 and 1988. Because there is no wage data for the years 1981 and 1983, we also exclude the 1982 observation. The pre-treatment period has 3 years from 1978 to 1980, while the post-treatment period has 5 years from 1984 to 1988. The table reports the estimated coefficients on the interaction term between the Paris indicator variable and the post-treatment fixed effects. The synthetic control regressions have 16 observations, while the regressions using the sample of all regions have 160 observations. The Identification at infinity sample consists of male workers with at least one child in Panel A, and female single workers without children in Panel B. Robust standard errors are reported in parentheses. All regressions include region and time fixed effect and are weighted by cell size. Wild bootstrap *p*-values in italics are computed using 1,000 bootstrap replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level. Source: DADS-EDP.

Table 6: Impact on the employment-to-population ratio of low-educated persons in the Paris and Marseille regions

	Treated region			
	Paris		Marseille	
	Synthetic	All regions	Synthetic	All regions
	(1)	(2)	(3)	(4)
1982	0.058*** (0.01)	0.034*** (0.01)	0.038*** (0.01)	0.029*** (0.01)
<i>Wild bootstrap p-value</i>	-	0.195	-	0.350
1983	0.070*** (0.01)	0.053*** (0.01)	0.033** (0.01)	0.022* (0.01)
<i>Wild bootstrap p-value</i>	-	0.055	-	0.379
1984-1988	0.046** (0.02)	0.056*** (0.01)	0.006 (0.02)	0.004 (0.01)
<i>Wild bootstrap p-value</i>	-	0.003	-	0.476

Notes: The data consist of annual observations for each region between 1978 and 1988. The pre-treatment period has 4 years from 1978 to 1981, while the post-treatment period has 7 years from 1982 to 1988. The regressions in columns 1 and 3 compares the treated and the respective synthetic region and has 22 observations. The results in columns 2 and 4 are derived from a single regression that pools all regions and has 231 observations. The table reports the estimated coefficients on the interaction term between the Paris indicator variable and the post-treatment fixed effects. Robust standard errors are reported in parentheses. All regressions include region and time fixed effects and are weighted by cell size. Wild bootstrap *p*-values in italics are computed using 1,000 bootstrap replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.

Source: French labor force surveys.

Table 7: Triple Difference Strategy

Specification	Low-educated			High-educated
	All	French men	French women	French
	(1)	(2)	(3)	(4)
1. Treated v. untreated sectors	0.008**	0.013***	0.001	-0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Observations	5,506	5,394	4,695	4,521
2. Construction v. untreated sectors	0.007**	0.009***	0.004	-0.002
	(0.003)	(0.003)	(0.005)	(0.005)
Observations	5,066	4,954	4,258	4,085
3. Hotel/restaurant v. untreated sectors	0.009*	0.016***	-0.002	0.002
	(0.004)	(0.005)	(0.005)	(0.005)
Observations	5,066	4,956	4,259	4,123
4. Domestic services v. untreated sectors	0.007**	0.007***	0.000	-0.005
	(0.003)	(0.003)	(0.005)	(0.006)
Observations	5,066	4,956	4,259	4,123
5. Placebo test				
chemical/pharmaceutical v. banking	0.002	-0.001	0.006	-0.006
	(0.003)	(0.003)	(0.004)	(0.005)
Observations	438	422	408	408

Notes: The data consist of annual observations for each region-sector group between 1978 and 1988. The pre-treatment period has 4 years (from 1978 and 1981), while the post-treatment period has 7 years from 1982 to 1988. The dependent variable is the employment share across sectors in a given region at time t for low-educated workers (column 1), low-educated French men (column 2) or women (column 3), and high-educated workers (column 4). The table reports the estimated coefficients on a treatment indicator equals to one when the sector of activity is considered as treated, located in Paris after 1981, and 0 otherwise. All specifications exclude workers from the agricultural, wholesale and retail sectors, as well as the textile and clothing industry. Robust standard errors are reported in parentheses. All regressions include sector-region, sector-time and region-time fixed effects and are weighted by cell size. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.

Source: French labor force surveys.

Table 8: Spatial correlation estimates of the impact of regularization on the employment-to-population ratio

	All		All		Low-educated		High-educated
					French men	French women	French
	OLS	IV	OLS	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of regularized imm.							
× 1982-1983	0.62***	0.77***	0.76***	0.93***	1.47***	0.91***	-0.08
	(0.16)	(0.16)	(0.18)	(0.18)	(0.34)	(0.23)	(0.27)
<i>Wild bootstrap p-value</i>	0.16	0.01	0.13	0.00	0.00	0.09	0.82
× 1984-1988	0.72***	0.61***	0.86***	0.77***	1.33***	0.49**	0.00
	(0.13)	(0.14)	(0.14)	(0.15)	(0.30)	(0.23)	(0.21)
<i>Wild bootstrap p-value</i>	0.04	0.18	0.02	0.09	0.31	0.07	0.99
Kleibergen-Paap F-test	-	104.26	-	104.37	87.25	148.77	204.61
Sample period	1975-1988				1978-1988		

Notes: The data consist of annual observations for each region between 1975 and 1988 in columns 1-2, and between 1978 and 1988 in columns 3-7. The pre-treatment period has between 7 years (from 1975 and 1981) and 4 years (from 1978 to 1981), while the post-treatment period has 7 years from 1982 to 1988. The table reports the estimated coefficients on the interaction term between the regional share of newly regularized immigrants and the post-treatment fixed effects. The regressions have 294 in columns 1-2 (21 regions and 14 years) and 231 observations in columns 3-7 (21 regions and 11 years). Robust standard errors are reported in parentheses. All regressions include region and time fixed effects and are weighted by cell size. We instrument the share of newly regularized immigrants with the shift-share instrument computed using the 1962 French census. Wild bootstrap *p*-values in italics are computed using 1,000 bootstrap replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level.

Source: French labor force surveys and 1962 census.

Table 9: Impact on the change in employment and per-capita GDP growth rates

	Change in employment rate	Change in log GDP per-capita
	(1)	(2)
1982-1983	0.014** (0.01)	0.016** (0.01)
1984-1988	0.002 (0.01)	0.011 (0.01)
Implied elasticity	0.69	0.79

Notes: The table relies on the synthetic difference-in-differences approach introduced by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) to estimate the impact of the regularization policy on the change in employment-to-population rate in column 1 and log GDP per capita in column 2. The data consist of annual observations for each region over the 1976-1988 period. The pre-treatment period has 6 years from 1976 to 1981. The post-treatment period has 7 years from 1982 to 1988. The table reports the estimated average treatment effects in Paris over the 1982-1983 and 1984-1988 periods. We use the “placebo method” to estimate the standard errors in parentheses. All results are based on 100 simulation replications. ***, **, * denote statistical significance from zero at the 1%, 5%, 10% significance level. The implied elasticity gives the change in employment rate in column 1 and the log GDP per capita in column 2 induced by a program that regularizes one percent of the workforce. The share of regularized workers in the Paris workforce was 2.02 percent, so that the implied elasticities in columns 1 and 2 are 0.69 (0.014/2.02 %) and 0.79 (0.016/2.02 %), respectively.

Source: French labor force surveys and INSEE.