

# Understanding the Effects of Granting Work Permits to Undocumented Immigrants

Ferran Elias, *Universitat de Girona*

Joan Monras, *Universitat Pompeu Fabra, Centre de Recerca  
en Economia Internacional, Barcelona School of Economics, and Center  
for Economic and Policy Research*

Javier Vázquez-Grenno, *Universitat de Barcelona and Institut  
d'Economia de Barcelona*

We study the regularization of 600,000 non-EU immigrants by the unexpectedly elected Spanish government following the terrorist attacks of 2004. By comparing non-EU to EU immigrants, we estimate that the policy did not lead to magnet effects. We then show that immigrant formal employment increased, while that of natives was not affected. However, there was a decrease in informal employment of both native and immigrant low-skilled workers. We document that tax revenues increased by around 4,000 euros per regularized immigrant—with no evidence of increased public expenditures. The evidence also suggests that the policy increased labor market opportunities for immigrants.

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## I. Introduction

Many countries host large numbers of undocumented immigrants.<sup>1</sup> By many accounts, the United States leads this ranking. According to the Pew Research Center, in 2017 there were as many as 10.5 million unauthorized immigrants on American soil, representing 23% of all immigrants.<sup>2</sup> These large numbers of undocumented immigrants have led recent US administrations, not without controversy, to consider either regularizing the status of these immigrants or deporting many of them to their countries of origin. However, despite the importance of these public debates, not enough is known about the effects of amnesty programs that grant work permits to unauthorized immigrants.

This paper fills this gap by analyzing a unique set of events in Spain that led around 600,000 working-age immigrants to gain legal working status between early February and early May 2005. Guided by a novel theoretical framework, we use several datasets to measure the effects of the policy on a diverse set of outcome variables, some of them understudied in the literature: migration flows, employment in the formal and the informal sector, wages in the formal sector, payroll and income taxes, and immigrant career paths.<sup>3</sup>

In the early 2000s, Spain experienced an immigrant boom, similar, although perhaps larger and occurring over a shorter period of time, to the experience in many European countries. From 1995 to 2004, the share of immigrants in the working-age population increased from less than 2% to around 10%. Many of these newly arrived immigrants lacked work permits. According to Domingo and Recaño (2005), close to 1 million immigrants—in a country of around 43 million inhabitants—were undocumented by 2004. However,

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<sup>1</sup> In this paper, “undocumented” refers to workers who were born outside the country in which they reside and do not have a work permit in the host country.

<sup>2</sup> See <https://www.pewresearch.org/fact-tank/2020/08/20/key-findings-about-u-s-immigrants/> (accessed September 2023).

<sup>3</sup> As we discuss in detail below, some papers study a subset of these outcomes using amnesty episodes in the United States and Colombia. Papers evaluating similar episodes in Europe concentrate exclusively instead on how amnesty programs affect immigrants. Our paper complements this evidence by studying a broader set of outcomes in an episode that provides particularly valuable variation.

their legal status changed abruptly in February 2005, when the newly elected government of the Spanish Socialist Workers' Party (PSOE) implemented a law that granted work permits to those satisfying two simple conditions: first, they needed to be offered a legal working contract by an employer for at least 6 months; second, they needed to be able to demonstrate residence in Spain since at least August 2004. As a result of the policy, the share of foreign workers registered in the social security system increased by around 3 percentage points overall.

The policy change was quite unexpected. The PSOE had won the general election in Spain only 3 days after the terrorist bombings of March 11, 2004, in Madrid, which killed nearly 200 people and is the largest terrorist attack in Spanish history. Before it, polls forecasted that the PSOE trailed the incumbent Popular Party (PP) by 7 percentage points. In fact, it was the mishandling of the crisis in the postattack days that caused the PP to lose this election, as explained in detail in Garcia-Montalvo (2011). It is very unlikely that a government led by the PP would have ever passed an amnesty program of this magnitude, since this party has traditionally adopted tough regulations on immigration.<sup>4</sup>

We begin our analysis by documenting that the amnesty did not seem to lead to magnet effects, at least in the shorter run. This is critical for interpreting what the policy actually meant. If there had been magnet effects, our estimates of labor and fiscal consequences would capture the effect of both regularizing undocumented immigrants and the increase in the supply of unauthorized immigrants. Without magnet effects, our estimates measure the consequences of granting work permits to undocumented workers who were already living and working in Spain. To shed light on this question, we leverage the fact that the amnesty affected only immigrants from outside the European Union. Hence, we compare immigrant stocks and growth rates from EU and non-EU countries around the policy change.<sup>5</sup> We do not detect any differential increase, at either the national or the local level, in the stock or growth rates of immigrants from outside the European Union, suggesting that the amnesty meant only a change in the right to work in the formal sector but did not affect overall labor supply in Spain.<sup>6</sup>

<sup>4</sup> While there had been previous immigrant regularizations in Spain, none compares (even slightly) in magnitude and importance to the labor market to the one introduced by the PSOE in 2005. See app. H (apps. A–H are available online) for a summary.

<sup>5</sup> In our setting, this exercise is possible because all immigrants, irrespective of work permit status, have strong incentives to register in the Municipal Registry of Population, and hence we can track them in our data.

<sup>6</sup> This result is consistent with the fact that the reform was approved only after a series of unexpected events and that work permits were not granted to recently arrived immigrants. Furthermore, the evidence is in line with previous work analyzing the magnet effects of the Immigration Reform and Control Act (IRCA) passed

To guide the empirical analysis of the potential effects of an immigrant amnesty program that grants work permits to workers already in the economy, we develop a model that expands recent work by Card et al. (2018) and Amior and Manning (2020). We assume that workers' labor supply schedules are upward sloping, which gives employers some degree of market power. The model assumes various labor types. At an upper level, we assume that high- and low-skilled workers are imperfect substitutes in production. Low-skilled workers with the legal right to work decide whether to work in the formal or informal markets. In contrast, undocumented low-skilled immigrants can supply their labor only in the informal market. We characterize this constrained choice in the labor supply decisions of unauthorized workers by assuming that undocumented workers' labor supply is more inelastic and limited to informal-type jobs. We also assume that formal and informal labor market input is imperfectly substitutable.

We use this framework to empirically analyze the Spanish amnesty program following three core ideas. First and most importantly, we assume that the amnesty program enlarged the labor supply choice set of undocumented immigrants who were, prior to the amnesty, forced to supply their labor endowment to the informal market. Second, given that the implementation of the amnesty was accompanied by an increase in work inspections against informality, we assume that the policy led to an increase in the costs of hiring workers informally. Third, we assume that the policy made jobs in the formal and informal sectors more similar from the viewpoint of workers, since a large portion of informal-type jobs in Spain combine some formal pay with supplemental informal rewards—an option that presumably became more complicated with the reform.

Empirically, we analyze the effects of the amnesty program on labor market outcomes and tax revenues by comparing Spanish provinces—which are a close approximation to local labor markets—that experienced a large entry of immigrants into the social security system with those that experienced a smaller one. Our main specification is a first-differences regression on detrended data, where the outcome is measured as the deviation from its linear pretreatment trend at the province level.<sup>7</sup> Therefore, the estimates are obtained from relating deviations from trend in outcomes of interest to the size of the relative entry of immigrants, also measured relative to trend, into the social security system. Moreover, we add several control variables that may be correlated with immigrant locations and may directly affect outcomes of interest. Among them we consider the size of the construction sector, the immigrant share, coastal dummies, and political alignment. We also report results from event study specifications that help to visualize our estimates, and we perform a

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in the United States in 1986 (Orrenius and Zavodny 2003). We also obtain this result when using synthetic control methods (see app. F, sec. F.3).

<sup>7</sup> This specification follows the recent work by Dustmann et al. (2022).

series of robustness checks and placebo exercises, which include estimates from a two-stage least squares (2SLS) regression. The results are very similar across all of these specifications and uncover six interesting facts, which we interpret through the insights of the model and which can be summarized as follows.

First, we document that for every 10 newly regularized immigrants—who on regularization necessarily entered the formal sector and who were working informally prior to the reform—only five formal jobs were retained. This confirms one of the predictions of the model. The policy change increases the pool of workers who can supply their labor endowment in the formal sector, but only a fraction of them end up in the formal sector, given that working informally is still an option.

Second, we estimate that the policy reduced employment among immigrants and native low-skilled workers in the informal sector. One of the defining aspects of the policy change is that work inspections increased substantially, and an effort was made to reduce the size of the informal sector. As a consequence, informal-type jobs became relatively more expensive, which explains the drop in low-skilled employment in the informal sector, even among natives. We can investigate the effect of the amnesty program on both formal and informal employment because we leverage the information from two different datasets: the standard Spanish Labor Force Survey (SLFS; in Spanish, Encuesta de la Población Activa, or EPA)—which captures both formal and informal workers, although they cannot be separately identified in the survey—and the Muestra Continua de Vidas Laborales (MCVL), which includes only formal employment.

Third, the model has ambiguous predictions on wages of low-skilled natives. We estimate that wages of low-skilled natives were positively affected by the reform, in line with the idea that the reform decreased firms' labor market power and that this force was stronger than competition effects.

Fourth, we show that wage and employment increased among high-skilled workers. This pattern likely reflects the substitution of low- for high-skilled workers given the cost increase implied by the movement of low-skilled labor from informal to formal jobs.

Fifth, we estimate that for each newly regularized immigrant, payroll tax revenues increased by around 4,000 euros per year at the province level.<sup>8</sup> In addition, we estimate that per each newly regularized immigrant, income tax revenues increased by around 440 euros per year. All together, this is a substantial increase in tax revenues that likely exceeded the costs of the reform, at

<sup>8</sup> Payroll taxes in Spain are around one-third of wages. Average wages (among all workers) before the policy change were almost 20,000 euros, while they are a bit lower for immigrants. Hence, our estimate indicates that payroll tax revenues increased by a bit less than would have been the case if firms were not paying payroll taxes for the newly regularized immigrants and then started to pay them the average salary.

least in the short run, given that undocumented workers in Spain already had access to public education and health care systems at the time. Indeed, we present evidence that school enrollment and hospital use were not affected by the regularization.

Last, we provide evidence in favor of one of the core ideas in our theoretical framework—namely, that the policy increased the labor market opportunities of undocumented immigrants by opening their access to the formal market. To do so, we analyze in detail the career path of the newly regularized immigrants. We focus our attention on undocumented immigrants who entered the social security system as housekeeping service workers, a sector that was characterized by a high prevalence of informality, a large number of undocumented immigrant workers, and one-employee firms (i.e., households employing housekeeping services). We show that immigrants who entered into social security as housekeeping workers stayed employed in the sector for about 6 months, which was a condition for the regularization process. Once they signed the contract and obtained the work permit, their labor market opportunities expanded. A large fraction of formerly undocumented immigrants moved into other sectors of the economy, most predominantly into the hotels and restaurants sector and the retail sector, as well as into larger firms. In numbers, we show that of the 100,000 immigrants who entered the social security system by mid-2005 as housekeeping workers, only half remained in this sector by the end of 2006. We observe these patterns in 2005, when the reform took place, but not in the preceding year, which gives us confidence that they are a consequence of the amnesty.

Overall, we contribute to the literature in two ways. First, we provide a conceptual framework to guide our empirical investigation, which can be used to analyze this and potentially other amnesty programs. We study how these policies expand labor market opportunities and hence reduce market power of employers of workers in the informal market. A number of papers investigate the role that imperfect competition plays in shaping labor market outcomes (Manning 2003), but within this literature only a handful of papers use imperfect competition frameworks to study immigration. Naidu, Nyarko, and Wang (2016) and Amior and Manning (2020) are, to the best of our knowledge, the two most important exceptions, although neither empirically analyze amnesty programs.

Second and perhaps more importantly, we use plausibly exogenous variation to analyze how the unexpected regularization of around 600,000 undocumented immigrants affected both the formal and the informal Spanish labor market. In addition, we provide evidence for workers of various skill levels. Moreover, we also show that the amnesty program improved public finances, since it increased payroll and income tax collection but did not increase the consumption of public services such as education and health care, which is a novel result in the literature. There are two recent papers closely related to ours, Bahar, Ibanez, and Rozo (2021) and Cascio and

Lewis (2019).<sup>9</sup> The former investigates the labor market effects of a large amnesty of Venezuelan workers in Colombia, using an empirical strategy similar to ours but focusing on a narrower set of outcomes and without a theoretical framework guiding their empirical investigation. The latter studies how the opportunity to apply for a green card in the United States (permanent residence) affected income tax revenues and receipt of public transfers (Earned Income Tax Credit). Relative to these papers, we investigate the effect of granting work permits to undocumented immigrants on many outcomes within the same institutional context, which allows us to paint a perhaps more complete picture of the consequences of regularization policies. On top of the outcome variables already mentioned and those analyzed in Bahar, Ibanez, and Rozo (2021) and Cascio and Lewis (2019), we also show that the policy did not lead to magnet effects and that recently regularized immigrants experienced an increase in their labor market opportunities. Our results may be particularly relevant for European countries that share labor and fiscal institutional details with Spain and similar immigrant compositions.

## II. Data and Context

### A. Data

We combine a number of different datasets to explore the consequences that the 2005 Spanish regularization of immigrants had on immigrant labor supply, tax collection, public expenditures, and different labor market outcomes of various other groups of workers, such as employment and wages. We provide a general overview of the data here and further details in appendix A.

First, we have aggregate administrative data on the number of social security affiliates by nationality and province of residence at a monthly frequency, provided by the social security system. This enables us to compute with precision the increase in social security affiliations around the amnesty. We use these data to measure the exposure of each province to the policy change.

Second, we have individual-level data from the SLFS, which provide information on both formal and informal employment. In these data we can identify native and immigrant workers of different skills, although we cannot identify whether the worker is working in the formal or informal sector or, in the case of immigrants, whether they have or lack work permits. The

<sup>9</sup> Besides these two papers, there are a number of papers that study the effects of amnesty programs on immigrant outcomes; among them are Dustmann, Fasani, and Speciale (2017), Pinotti (2017), Mastrobuoni and Pinotti (2015), DiPorto, Martino, and Naticchioni (2018), Devillanova, Fasani, and Fratini (2018), Amuedo-Dorantes and Bansak (2011), Amuedo-Dorantes, Bansak, and Raphael (2007), Kaushal (2006), Cobb-Clark, Shiells, and Lowell (1995), and Dolado, Duce, and Jimeno (1996). Relative to those, we study how amnesty programs affect the economy more broadly.

employment question does not mention informality. Hence, if someone has worked informally in the reference week, she should appear in the survey as working.<sup>10</sup> These data are a repeated cross section with quarterly frequency.

Third, we have individual-level data on the working history of a representative sample of workers, through the MCVL. These data are representative of the population of workers affiliated with the social security system, allows them to be tracked over time, and has information about their level of education. We classify workers with at least a university degree as high-skilled workers, and we label the rest of the labor force as low-skilled workers.<sup>11</sup> Among immigrants, only documented workers can be affiliated with the social security system. There are very few natives not covered in this dataset. While some natives work informally, most native workers sign, at some point during their working life, a legal contract, and hence they are sampled in the MCVL. Using these data, we can track newly regularized immigrant workers when they gain work permits and enter the social security system (which was a requisite for the regularization process). These data also contain information on formal workers' wages.

Fourth, we have aggregate administrative data on payroll tax revenues at the province level. These data report the total amount of taxes collected in each province for each of the labor market contribution types, called *regímenes* (regimes), available in Spain. These data cover public revenues coming only from payroll taxes. We have these data at a yearly frequency. Moreover, to show that granting work permits to immigrants has effects not only on payroll tax collection but also on income tax collection, we use data from the Spanish Tax Administration Agency on the number of filed income tax returns.<sup>12</sup>

Fifth, to determine whether the policy had effects on public spending, we use province-level data for the two main expenditure programs in Spain: education and health care. More specifically, we use yearly enrollment data in the public education system from the Ministry of Education, as well as data on hospital discharges, disaggregated by diseases, from the Hospital Morbidity Survey conducted every year by the Spanish National Institute of Statistics.

<sup>10</sup> The exact question in the SLFS is "Ha trabajado de manera remunerada la semana de referencia?" This can be translated as "Have you worked for a salary in the reference week?"

<sup>11</sup> We use the same criterion when we work with data from the SLFS.

<sup>12</sup> We decided not to use data on income tax revenues because of the temporal proximity between the regularization of 2005 and two tax reforms (2003 and 2007) that differently affected Spanish regions. In that sense, to isolate our estimates from potential confounding factors, we use the number of filed tax returns, since it is an indicator less affected by the reforms and therefore more accurate to analyze how the regularization of immigrants could have been affected income tax revenues. Given the particularity of the Spanish tax system, we do not have information for four provinces from two regions (Basque country and Navarra).

Finally, we have data on the universe of people living in Spain from the Municipal Register. These data cover both documented and undocumented immigrant workers. Undocumented immigrants are fully covered in these data, since registration in the Municipal Register grants them access to health care services and education. Moreover, local administrations also have incentives to track the number of people living in each municipality, since it affects the amount of transfers received from upper tiers of government. One of the main limitations of this dataset is that skill information is recorded very imperfectly. Hence, we use these data only for total population.

### B. Policy Change

In this section we explain the four main characteristics of the amnesty: its size, who benefited from it, its enforcement mechanisms, and that it could hardly have been anticipated a few months before its approval.

First of all, in February 2005, the Spanish government made effective a policy to regularize a large number of undocumented immigrants who were already living in the country. Around 600,000 immigrants gained a work permit, and the share of immigrants registered in the social security system increased from around 6% to around 9% in a very short period of time, as can be seen on the left of figure 1.

Second, the beneficiaries of the amnesty were immigrant workers who were already living in Spain and who had real connections with the Spanish labor market. In particular, the law offered a period of 3 months (between February 7 and May 7, 2005) to give work permits to workers who complied with the following two criteria: (1) the worker had to be in the Municipal

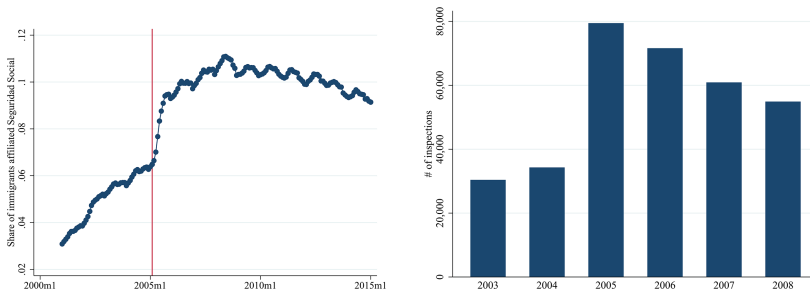


FIG. 1.—Social security registration and inspections related to foreign workers. The graph on the left shows the (monthly) share of immigrants registered in the social security system as a proportion of total affiliation with the social security system. The graph on the right shows the number of work inspections related to foreign workers. Source: Ministry of Labor and Social Security.

Registry of Population prior to August 7, 2004,<sup>13</sup> and (2) the employer needed to offer a legal working contract for at least 6 months.<sup>14</sup>

Third, the policy included enforcement mechanisms. In fact, the number of work inspections related to foreign workers more than doubled between 2004 and 2005, as can be seen on the right of figure 1. This is something that likely affected native workers in the informal economy as well, as we will see both in the theoretical section and in the empirical results.

Fourth, at the time it was difficult for anyone to foresee that an amnesty program of this magnitude was likely, as it was approved after a series of unexpected events that led the PSOE to surprisingly win the elections on March 14, 2004. According to a poll conducted between late January and early February 2004, the vote forecast for the two main political parties in Spain was 42.2% for the conservative PP and 35.5% for the PSOE. The PP is the party that has traditionally adopted tougher regulations to limit immigration. Therefore, it is very unlikely the amnesty program would have been approved if they had won the elections. Yet something completely unexpected occurred on March 11, 2004, just 3 days before the election. Early that morning, several terrorists attacked a number of commuter trains in Madrid, killing almost 200 people in the deadliest-ever terrorist attack on Spanish soil. Following the attacks, the 3 days leading to the general election were chaotic. Initially, the government tried to blame Euskadi Ta Askatasuna (ETA), the Basque terrorist group responsible for most terror attacks in Spain since the late 1960s. However, the indiscriminate nature of the bombings, causing many civilian victims, did not match with the *modus operandi* of most ETA attacks. The PP government was concerned that if the attacks had been committed by an Islamic terrorist organization, voters could perceive it as a retaliation for Spanish involvement in the Iraq war, a decision of the PP that was hugely controversial at the time. To avoid further stoking this controversy, the government delayed official statements on who was responsible for the attacks, even suggesting that perhaps ETA was behind them.

The attacks and the government's handling of the crisis changed the outcome of the election and unexpectedly gave power to the PSOE, as Garcia-Montalvo (2011) shows by comparing the voting behavior of Spanish nationals living abroad (who had cast their votes before the attacks took place) with postattack voting (Spanish residents) from this election and prior ones. The PSOE obtained 42.6% of the popular vote, while the PP had only 37.7%, in sharp contrast to the forecast of just a few weeks earlier.

<sup>13</sup> This criterion was subsequently relaxed, accepting registration by default (*empadronamiento por omisión*) on presentation of any official document proving that the immigrant had been in Spain in August 2004.

<sup>14</sup> There were some exceptions for the agricultural, construction, restaurant and hotel, and domestic service sectors, as well as for part-time workers.

### III. Theoretical Framework

In this section, we introduce a theoretical framework that helps to guide our empirical work of section IV. The model represents a Spanish province. This is similar to a commuting zone in other contexts. Spanish provinces are relatively well-connected local labor markets. Unlike commuting zones, they are not defined by commuting behavior but rather by historical delineations.

We model each province as a small open economy producing a freely traded good. We assume a representative firm at the local level. The representative firm combines high-skilled ( $L_H$ ) and low-skilled ( $L_L$ ) work in their production. In turn, low-skilled work is a combination of formal ( $L_F$ ) and informal ( $L_I$ ) low-skilled workers. Informal low-skill workers, in turn, can be decomposed into those who have work permits but decide to work informally ( $L_D$ ) and those who lack work permits ( $L_U$ ). High- and low-skill work is imperfectly substitutable (with elasticity of substitution  $\sigma$ ), and formal and informal workers are also imperfect substitutes (with elasticity  $\sigma_L$ ), as can be seen in the two-nest production function introduced below. Low-skilled workers with work permits decide whether to supply their labor formally or informally.

We assume that the amnesty program that we later analyze empirically affected the economy in a number of dimensions. First, a number of undocumented workers gained work permits and hence could start working in the formal market. Second, we think about the enforcement against informality that accompanied the policy change as making jobs in the formal sector and informal sectors more similar and hence substitutable from the viewpoint of workers with work permits deciding whether to supply their labor endowment in formal- or informal-type jobs. Finally, the policy change lead to an increase in the cost of hiring workers informally. It is worth emphasizing that this framework can potentially be used to analyze amnesty programs that are characterized by a different array of changes—for instance, situations where the enforcement toward informality does not change.

#### A. Labor Supply

There are two types of low-skilled workers. First, there are low-skilled workers with the legal right to work in Spain. We denote these by  $N$ . Second, there are immigrant undocumented workers, denoted by  $U$ . Low-skilled workers with work permits supply labor to either the formal ( $L_F$ ) or the informal ( $L_D$ ) sector, so necessarily  $N = L_F + L_D$ . Workers without work permits can supply labor only to the informal sector. We denote by  $L_U$  those among  $U$  who are working. The total number of workers in the informal sector is given by  $L_D + L_U = L_I$ , which includes workers who have work permits but supply labor to the informal market and workers without work permits.<sup>15</sup>

<sup>15</sup> For simplicity, we assume that workers with work permits and without work permits are perfect substitutes in the informal sector. We can relax this simplification and obtain similar results, albeit with more cumbersome algebra.

Low-skilled workers with work permits can decide to supply their labor in either the formal or the informal sector. Wages and tax rates are different in the two sectors. Taxes are paid by firms, as mandatory payroll taxes. Sector optimization choice leads to the following supplies of labor:

$$L_F = \frac{(\tau w_F)^{1/\epsilon_L}}{(\tau w_F)^{1/\epsilon_L} + (\tau w_D)^{1/\epsilon_L}} N, \tag{1}$$

$$L_D = \frac{(\tau w_D)^{1/\epsilon_L}}{(\tau w_F)^{1/\epsilon_L} + (\tau w_D)^{1/\epsilon_L}} N.$$

Note that these equations mean that labor supply curves are upward sloping with the labor supply elasticity governed by  $\epsilon_L$ . These equations can be microfounded with discrete choice-type models, as explained in Card et al. (2018).

Workers without work permits can supply labor only to the informal sector. They also behave according to an upward-sloping supply curve, which we express with the following equation:

$$L_U = (\tau w_U)^{1/\epsilon_U} U, \tag{2}$$

where  $U$  represents the total number of undocumented workers and  $L_U$  represents the workers actually employed. We assume that  $\epsilon_L < \epsilon_U$ , which captures the idea that workers with work permits have a more elastic labor supply curve.

The labor supply function of high-skilled workers, denoted by  $L_H$ , is governed by  $\epsilon_H$ , following

$$L_H = (\tau w_H)^{1/\epsilon_H} H, \tag{3}$$

where  $H$  denotes high-skilled population.

### B. Demand for Labor

Demand for labor comes from firm maximization. A representative firm produces according to the following production function:

$$Y = [L_H^\rho + L_L^\rho]^{1/\rho}, \quad \text{with } L_L = \left[ \alpha_F L_F^{\rho_L} + \alpha_I \underbrace{(L_D + L_U)^{\rho_L}}_{=L_I} \right]^{1/\rho_L},$$

where  $L_H$  indicates high-skilled workers and  $L_L$  is a composite of low-skilled workers that combines  $L_F$  formal workers with  $L_D$  documented workers in the informal sector and  $L_U$  undocumented immigrant workers. The terms  $\alpha_F$  and  $\alpha_I$  represent the productivity of formal and informal low-skill workers, respectively.

Profit maximization is given by the following program:

$$\max_{L_j, j \in \{H, F, D, U\}} pY - \sum_{j \in \{H, F, D, U\}} (1 + \tau_j)w_j L_j,$$

subject to the labor supply decisions expressed in equations (1)–(3), where  $\tau_j$  denotes the payroll tax paid when hiring factor type  $j$ . We assume  $\tau_H = \tau_F = \tau$ ,  $\tau_D < \tau$ , and  $\tau_U = 0$ ; that is, formal workers pay the full amount of payroll taxes, documented workers in the informal sector pay only a fraction of that amount, and undocumented workers do not pay any payroll taxes. For this maximization we assume monopsonistic competition—that is, firms take as given price indexes such as  $W = ((w_F)^{1/\epsilon_L} + (w_D)^{1/\epsilon_L})^{\epsilon_L}$ . Under this assumption, this maximization problem results in the following optimality conditions:

$$p \frac{1}{(1 + \epsilon_j)(1 + \tau_j)} \text{MPL}_j = w_j, \tag{4}$$

where  $\text{MPL}_j = \partial Y / \partial L_j$  is the marginal product of labor and  $w_j$  denotes the wage of each factor of production, which are paid a markdown over the marginal cost. We denote by  $w_j$  the wage received by the worker and by  $\tau_j$  the (payroll) taxes paid by the firm; hence,  $(1 + \tau_j)w_j$  is the total cost of one unit of factor  $j$ . Note that as  $\epsilon_j$  goes to zero, labor supply is more elastic, and hence the markdown on marginal products is smaller. The term  $p$  is the price of the good, which we assume is freely traded, and hence we can normalize to 1.<sup>16</sup>

### C. Equilibrium

The equilibrium in the labor market equates demands and supplies for each factor of production. This yields equilibrium relationships between the amount of employed workers of each type (which we denote by  $L_j$ ) and the total supply of high-skilled workers  $H$ , low-skilled workers with work permits  $N$ , undocumented workers  $U$ , payroll taxes  $\tau_j$ , labor supply elasticities, and the elasticities of substitution between the different factors of production. We illustrate these equilibrium conditions by showing the one for formal low-skilled workers here, and we relegate to appendix B the remaining conditions. By combining labor demand and supply for this factor type, we obtain<sup>17</sup>

$$\ln L_F = \delta_F - \frac{\sigma_L}{\epsilon_L \sigma_L + 1} (\ln(1 + \epsilon_L)(1 + \tau)) + \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1} \ln N, \tag{5}$$

<sup>16</sup> Marginal products of labor  $\text{MPL}_F = \alpha_F Y^{1/\sigma} L_L^{-(1/\sigma - 1/\sigma_L)} (L_F)^{-1/\sigma_L}$ . The others take similar expressions.

<sup>17</sup> From  $L_F = (\text{MPL}_F / ((1 + \epsilon_L)(1 + \tau)))^{1/\epsilon_L} (N/W)$ , we obtain that  $L_F^{1+1/\sigma_L \epsilon_L} = (\alpha_F Y^{1/\sigma} L_L^{(1/\sigma - 1/\sigma_L)} / ((1 + \epsilon_L)(1 + \tau)))^{1/\epsilon_L} (N/W)$ , which allows us to obtain expression (5).

where  $\delta_F$  combines aggregate output, aggregate labor, and various parameters that for simplification we take as fixed and where  $\sigma_L$  is the elasticity of substitution among low-skilled workers ( $\rho_L = (\sigma_L - 1)/\sigma_L$ ). Similarly, we can obtain that wages are given by

$$\ln w_F = \kappa_F - \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1} (\ln(1 + \epsilon_L)(1 + \tau)) - \frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N, \quad (6)$$

where  $\kappa_F$  is also a combination of aggregate output, aggregate labor, and various parameters that for simplification we take as fixed.

These expressions show that an increase in the supply of low-skilled workers with work permits ( $N$ ) leads to higher employment and lower wages, while a cost increase ( $\tau_j$ ) or an increase in the labor supply elasticity ( $\epsilon_L$ ) leads to lower employment and wages. Note in particular that we can recognize two effects. On the one hand, the term  $(\ln(1 + \epsilon_L)(1 + \tau))$  is a monopsonistic effect. It captures the idea that when labor supply schedules are more elastic, employers have less market power, and hence wages and employment are higher. On the other hand, the term  $\ln N$  is a competition effect. More workers in a market put downward pressure on wages.

#### D. Properties

Equations (5) and (6), as well as the equivalent ones for the other factors of production shown in appendix B, characterize employment and wage levels for each factor of production as a function of parameters of the model and population levels. This framework allows us to study the effect of an amnesty program.

Several governments have introduced amnesty programs. However, these policy changes are usually combined with other policies. Very often, regularizations of immigrant workers come together with increased border enforcement, as happened, for example, with the IRCA in 1986 (Hanson and Spilimbergo 1999; Orrenius and Zavodny 2003). In other episodes, governments have increased enforcement against informality at the same time that they have granted work permits, as in the Spanish case. Hence, to use this model to evaluate a particular policy change, it is important to first identify what the policy change meant.

In our case, we think that the policy change is well captured by exogenous changes in three parameters of our model. First, the policy decreased the number of workers who were undocumented ( $U$ ) and increased, by the same amount, those with legal work permits ( $N$ ).<sup>18</sup> Second, the policy increased enforcement against informality. This has two consequences. On the one hand, it increased the cost of hiring informal-type workers (i.e.,  $\tau_D$  increased). On the other hand, it made different low-skill jobs more similar, lowering firms'

<sup>18</sup> In the empirical section, we provide evidence that suggests that the overall supply of immigrants in Spain did not change as a result of the policy change.

ability to exert market power over low-skill workers, which in the model is captured by a decline in  $\epsilon_L$ . We can use this framework to derive the consequences of a policy change like the one implemented by the Zapatero government, which we summarize in the following proposition.

**PROPOSITION 1.** We define the policy change, which we denote by  $\partial\theta$ , as a policy that does the following:

1. Transforms undocumented workers into documented workers; hence,  $\partial U/\partial\theta = -\partial N/\partial\theta$ .
2. Increases the cost of informal work; that is,  $\partial\tau_D/\partial\theta > 0$ .
3. Makes the jobs in the formal and informal sector more similar to workers; that is,  $\partial\epsilon_L/\partial\theta < 0$ .

Under these assumptions, we have the following:

1. In the informal sector, wages of undocumented immigrant workers are lower than wages of documented workers, as long as  $\epsilon_U$  is sufficiently large.
2. Total tax collection increases with the amnesty program.
3. There is an ambiguous effect on wages of formal low-skilled workers.
4. Employment in the informal sector declines.
5. Employment in the low-skilled formal sector increases, but by less than the amount of low-skilled entrants into the formal sector.
6. Employment and wages increase for high-skilled workers.

*Proof.* See appendix C. QED

Proposition 1 states various results. First, it says that undocumented workers face fewer job opportunities, and hence employers can exert higher market power over them. Second, it says that payroll tax collection increases with the policy change. This is so mainly because undocumented workers enter the formal market and hence increase payroll tax collection. Third, low-skilled workers in the formal sector may see their wages increase. This is explained by the fact that the policy change makes working in the formal and informal sectors more similar, which reduces the ability of formal sector firms to extract surplus from workers, as illustrated in figure C.1 (figs. C.1, D.1–D.4, E.1, F.1–F.9 are available online). Fourth, the reform increases enforcement against informality and grants work permits to undocumented workers, which leads to a decline in informal sector jobs. Fifth, our framework shows that the increase in formal sector jobs is smaller than the increase in the number of workers who gain work permits. This result comes from the fact that the reform does not necessarily “kill” the informal labor market. Hence,

some of the undocumented immigrant workers can chose to remain in the informal market (at least after the initial 6 months where the employee had to offer them a formal contract). Finally, the amnesty program makes the low-skilled labor aggregate ( $L_L$ ) more expensive. Hence, employers substitute low- for high-skilled labor.

## IV. Empirical Evidence

### A. Main Identification Strategy

The circumstances that led to the amnesty program gives us a strategy for identifying the effects of granting work permits to undocumented immigrants on labor market outcomes and both payroll and income tax collection. If such effects exist, provinces with more regularized immigrants should experience relatively larger changes in outcomes. To the extent that the policy was not differentially implemented across provinces as a function of our outcomes of interest, our estimates can be interpreted as the causal effect of a policy that grants work permits to undocumented immigrants, as discussed in the preceding section.

The spatial distribution of immigrants across provinces was heterogeneous in 2002. Immigrants in Spain concentrate in coastal provinces with high levels of tourism and European retirees. This is the case of Alicante, the Balearic Islands, Girona, Tenerife, and Málaga. All of these provinces had immigrant shares above 8.5% in 2002.<sup>19</sup> Immigrants also concentrate in large cities, as happens in other countries (Albert and Monras 2022). In 2002, for example, Madrid and Barcelona had immigrant shares of 9.2% and 6.8%, respectively, numbers that have risen further in recent years. In contrast, in 2002 there were many provinces with extremely low levels of immigration: more peripheral provinces, such as Asturias, Coruña, or Lugo, in the north; Córdoba, Jaén, Sevilla, or Cádiz in the south; and provinces in central Spain all had immigrant shares that were 2–3 percentage points below the national average. In addition, most immigrants were low skilled, and the regularization rate was high across all provinces. Details on these statistics are provided in table F.1 (tables D.1, D.2, F.1–F.14, are available online).

Figure 2 depicts this spatial heterogeneity by dividing Spanish provinces by their median level of migration in 2002 and plotting the share of immigrants registered in the social security system as a proportion of the total affiliation with the social security system. The graph on the left shows that in high-immigration provinces, the share of foreign-born individuals registered in the social security system increased from around 7% to more than 10% in just a few months. In contrast, the policy change affected low-immigration provinces too, albeit with less intensity: the share of immigrants registered

<sup>19</sup> These immigrant shares include all foreign-born individuals (i.e., they include foreign-born people from EU and non-EU countries).

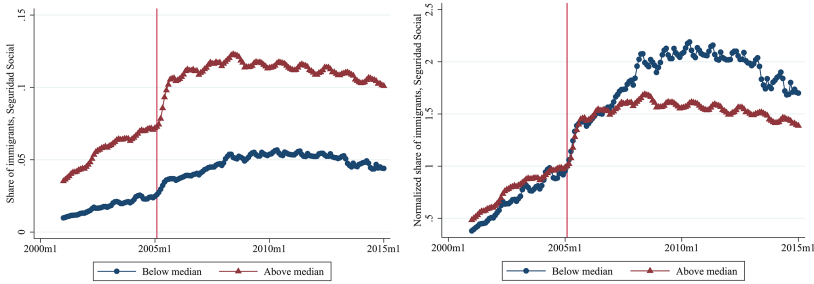


FIG. 2.—Social security registration and immigration reform. The graph on the left shows the (monthly) share of immigrants registered in the social security system as a proportion of total affiliation with the social security system in Spanish provinces above and below the median level of immigration (in 2002). The vertical line indicates the last period before the reform (2005m1). The graph on the right normalizes the one on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

in the social security system moved from around 3% to around 4% over the same period. The graph on the right in figure 2, where we normalize the two series to January 2005, shows that in fact the effect of the policy was similar across locations in proportional terms, which is consistent with the take-up rate among undocumented immigrants being similar across provinces.

Therefore, in our empirical strategy we exploit the time and spatial variation in the change of immigrants affiliated with the social security system across provinces to identify the effects on the outcomes of interest. We use two different specifications to quantify the impact of the policy change: a regression in first differences and an event study.

Intuitively, our difference-in-difference strategy tries to relate the size of the jump in the number of immigrants affiliated with the social security (as observed on the left graph of fig. 2) to the potential jump in the respective series for each of our outcomes of interest. A standard approach in these settings is to run a panel regression:

$$y_{c,t} = \delta_c + \delta_t + \beta T_{c,t} + \gamma X_{c,t} + \epsilon_{c,t}, \tag{7}$$

where  $T_{c,t}$  is the treatment variable, taking the value of the (potentially continuous) treatment in location  $c$  at time  $t$  and the value 0 before the treatment;  $\delta_c$  and  $\delta_t$  are location and time fixed effects; and  $X_{c,t}$  are time-varying controls.

In these specifications, the key identification assumption is that, conditional on the controls, we have parallel trends between treated and control units before the treatment and that those would have continued parallel in the absence of the treatment. Sometimes, however, even after controlling for

observables, there are systematic differences in trends across locations, but there is a clear jump in the series coinciding with the treatment, as occurs in the graph on the left of figure 2. In those cases, one can identify the size of the jump by controlling for linear location-specific time trends, as done in numerous prior work (see, e.g., Greenstone, Hornbeck, and Moretti 2010; Dustmann et al. 2022).<sup>20</sup>

However, a problem that is not always fully discussed in these settings is that oftentimes the treatment can affect not only the level of the outcome variable of interest but also its posttreatment trend.<sup>21</sup> This is problematic because including a location-specific time trend in panel regressions results in biased estimates in those cases. Intuitively, when estimating linear time trends in regressions that include posttreatment data, the effect of the treatment on the posttreatment trend affects the estimate of the linear location-specific trend, biasing the estimate of the effect of the treatment. In these situations, one way to recover the effect of the treatment of interest is to fit location-specific time trends before the treatment and then take first differences between the predicted values of the regression estimated on pretreatment data and the actual data. In appendix D, we provide more details using both Monte Carlo simulations (sec. D.1) and observed data (sec. D.2)—again, see Dustmann et al. (2022) for a recent paper that uses similar empirical strategies. It is worth noting that this approach not only recovers the effect of interest in the presence of potentially heterogeneous linear time trends across locations but is also robust to the policy change affecting postchange trends and is conservative in terms of inference. We can express this procedure as follows:

$$\Delta \hat{y}_c = \alpha + \beta \Delta \frac{\widehat{\text{Imm Soc Sec}}_c}{\text{Pop}_c} + \gamma X_c + \epsilon_c, \quad (8)$$

where the hat indicates that before taking the difference between the pre- and postpolicy period, we have removed linear-specific province time trends estimated using pretreatment data and a panel specification. The term  $\Delta \widehat{\text{Imm Soc Sec}}_c / \text{Pop}_c$  is the continuous treatment or shock variable that computes how many extra immigrants were affiliated with the social security relative to province-specific linear trends in immigrant affiliation. The subindex  $c$  indicates Spanish provinces. While we report results using various sets of controls, our baseline specification includes the fraction of immigrants from non-EU-15 countries in 2004, the prereform share of workers in the construction sector, political

<sup>20</sup> In this respect, we follow closely Dustmann et al. (2022; see fig. VII, p. 312). See also similar discussions in Meer and West (2016) and Monras (2019).

<sup>21</sup> The recent econometric literature on difference in differences has analyzed the biases of this point in a more general setting. Standard difference-in-difference estimators may fail to recover the effect of the treatment when the treatment has heterogeneous effects, which, e.g., can manifest as different effects of the treatment over time. See Chaisemartin and D'Haultfœuille (2020).

alignment, and coastal dummies. In the main regressions we consider 2002–4 as the preperiod and 2005–7 as the postperiod. We explicitly stop our analysis in 2007 to avoid the differential effect that the Great Recession could have had across Spanish provinces.

Second, we employ a dynamic difference-in-differences specification to estimate the precise timing of differential changes in outcomes for provinces with a relatively high increase in the number of immigrants affiliated with social security, relative to the base year 2004. Following this method, we can see whether there are systematic deviations from the trend across provinces in the variables of interest up to 2004, conditional on our controls, and visualize whether changes occur only after the intervention. For this exercise, we employ the following regression:

$$\hat{y}_{c,t} = \delta_t + \delta_c + \sum_{j=2002, j \neq 2004}^{2007} \beta_j 1[j = t] \Delta \frac{\widehat{\text{Imm Soc Sec}}_c}{\text{Pop}_c} + \sum_{j=2002, j \neq 2004}^{2007} \gamma_j 1[j = t] X_c + \epsilon_{c,t}. \quad (9)$$

That is, we estimate the interaction of year dummies, denoted by  $1[j = t]$ , and a shock variable. The coefficients of interest are the  $\beta_j$ , which we can plot in simple event-type graphs. We also interact the year dummies with the pretreatment control variables,  $X_c$ , described above. As in equation (8), the hat indicates that we have removed province-specific linear trends prior to the policy, as well as province and year fixed effects. We discuss several robustness checks in section IV.F and in appendix F.

### B. The Absence of Magnet Effects

A preliminary step in our analysis is to understand whether the policy change affected the overall supply of immigrants in Spain. This is a crucial point for the interpretation of the results. If there were magnet effects, the estimates would reflect both changes in the supply of immigrants and the consequences of granting work permits. Alternatively, if there were no magnet effects, the estimates would capture the effects of regularizing undocumented immigrants.

Formally, the policy gave work permits to immigrant workers in the informal sector who could prove they were living in Spain since at least August 2004. Moreover, its approval was unexpected, as explained in section II.B. These institutional details suggest that there should not have been supply effects, which is what we posited in assumption 1 of proposition 1. In this section, we investigate this issue empirically following two different approaches. The first one focuses on immigrant supply at the national level. The second tries to detect magnet effects at the provincial level, which is the source of variation we exploit in our main identification strategy.

First, we compare immigrant stocks and growth rates from countries affected by the amnesty program and immigrants from EU countries that were not affected by the policy. Spain was, and had been for many years, part of the Rome and Schengen treaties that allows EU citizens to freely move and work within the European Union without borders. Hence, we can compare whether there is a change in the stock of immigrants from countries of origin affected by the policy relative to the stock of immigrants from the European Union.<sup>22</sup>

To document the potential presence of magnet effects, we use data from the Municipal Registry, which, as argued in section II.A, captures both documented and undocumented immigrants. We start our empirical investigation by showing in figure 3A the stock of immigrants from the top four sending countries, three of which were affected by the policy and one of which was not. The graph does not show any significant change in the stock of immigrants from the United Kingdom (not affected by the policy), Ecuador (affected), Romania (affected), and Morocco (affected). This graph suggests that the policy did not lead to a substantial change in the (net) flow of immigrants from any of these countries.

We test more formally whether stocks of immigrants systematically diverged toward countries of origin affected by the policy change right after the reform using the following equation:

$$y_{ot} = \delta_t + \delta_o + \delta_o \times t + \beta \text{Affected countries}_{o,t} + \epsilon_{o,t}, \quad (10)$$

where  $y_{ot}$  are different measures of immigrant stocks or growth rates from origin  $o$  at year  $t$ ; “Affected countries” is a dummy variable that takes a value equal to 1 if the country is affected by the policy at time  $t$ ;  $\delta_o$  and  $\delta_t$  are country of origin and year fixed effects, respectively; and  $\delta_o \times t$  denotes country-specific linear time trends, which take into account the fact that stocks of immigrants from different countries of origin were in different trajectories.

In a first set of specifications, we investigate whether the amnesty program affected the composition of immigrants from the different countries of origin. To investigate this, we use as the dependent variable  $y_{ot} = \text{Immigrants}_{ot} / \text{Immigrants}_t$ , which measures the fraction of the overall stock immigrants from country  $o$  in each time period. Under the assumption that immigrants not affected by the amnesty continued to immigrate to Spain in the same

<sup>22</sup> Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia joined the European Union in May 2004. However, EU members could delay until 2011 the free mobility of workers with these countries (except Cyprus and Malta). Spain applied mobility restrictions until May 2006 to dependent workers (self-employed workers could already freely enter Spain in May 2004). Despite these restrictions on mobility, workers from new accession EU countries were not eligible for the amnesty program. Hence, we consider specifications that include or exclude immigrants from these countries as EU members when evaluating the effect of the amnesty program on magnet effects.

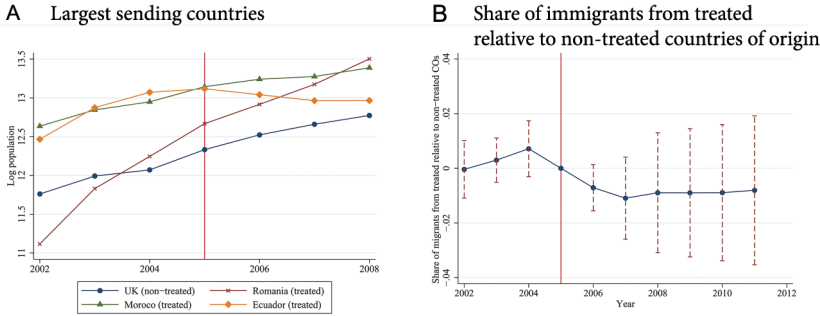


FIG. 3.—Absence of magnet effects. *A* shows the evolution of the (log) stock of immigrants from the four top-sending countries, three of which were affected by the policy change (labeled “treated”) and one of which was not (labeled “nontreated”). *B* shows an estimate of the share of immigrants (over all immigrants) from countries of origin affected by the amnesty program relative to unaffected countries. The vertical line indicates the last period before the reform (2005). These data are from the Municipal Register, which covers both documented and undocumented immigrants. The Municipal Register reports the number of individuals residing in municipalities on January 1 each year.

way as they did before the reform, then a change in immigrant composition,  $\beta \neq 0$ , would indicate that the reform affected migration decisions in countries affected by the policy change. If instead  $\beta = 0$ , then this is evidence that the policy did not lead to a change in the immigrant composition, which we interpret as an absence of magnet effects.

An alternative specification is to check whether immigrant growth rates diverged around the policy change for non-EU migrants relative to EU migrants. We investigate this using as the dependent variable  $\gamma_{ot} = \Delta \text{Immigrants}_{ot} / \text{Immigrants}_{ot-1}$ . In this case, if  $\beta \neq 0$ , then it would be evidence that the policy changed the growth rate of the arrival of immigrants from treated versus nontreated countries of origin. Instead,  $\beta = 0$  would suggest that the arrival growth rate of immigrants did not change differentially between non-EU and EU countries of origin.

Table 1 reports the estimates of  $\beta$ . We show two different specifications. Panel A includes only country and year fixed effects. Column 1 shows that there is no systematic change in the stock of immigrants from countries affected by the policy around the time of the change. In this first column, the sample years include 2002–9. In columns 2 to 4, we change the years selected by zooming into the year 2005, when the policy was implemented. As we zoom in, it is clear that there is no differential change in the stock of immigrants from affected and nonaffected countries. Figure 3*B* visualizes these estimates. Panel B of table 1 extends the specification used in panel A by allowing country-specific linear time trends. Estimates in panel B are similar to

**Table 1**  
**Estimates of the Effect of the Immigration Reform on Migrant Composition and Growth Rates**

	(1)	(2)	(3)	(4)
A. Composition, Country of Origin Fixed Effects (Dependent Variable: $\text{Immigrants}_{ot}/\text{Immigrants}_t$ )				
Affected countries	-.008 (.012)	-.007 (.011)	-.006 (.009)	-.006 (.007)
Observations	896	784	672	448
$R^2$	.919	.928	.952	.975
B. Composition, Country of Origin-Specific Trends (Dependent Variable: $\text{Immigrants}_{ot}/\text{Immigrants}_t$ )				
Affected countries	-.008 (.005)	-.011 (.006)	-.012 (.007)	-.008 (.005)
Observations	896	784	672	448
$R^2$	.992	.992	.991	.996
C. Flows, Country of Origin-Specific Trends (Dependent Variable: $\Delta\text{Immigrants}_{ot}/\text{Immigrants}_{ot-1}$ )				
Affected countries	-.129 (.038)	-.053 (.059)	.034 (.080)	-.036 (.061)
Observations	896	784	672	448
$R^2$	.793	.817	.858	.941

NOTE.—This table shows estimates of the effect of the regularization policy on the flow of immigrants from 112 countries of origin into Spain, comparing countries that were affected by the policy (non-EU countries) with those that were not affected by it (EU countries). The estimates in cols. 1–4 show different windows around the policy change: the sample in col. 1 includes the years 2002–9; col. 2, 2002–8; col. 3, 2002–7; and col. 4, 2003–6. Panel A is our baseline specification shown in eq. (10). Panel B also includes linear country of origin-specific time trends. Observations are weighted by the number of immigrants from each country of origin. The weighted mean of the dependent variable is 6.4%. Panel C uses the same specification as panel B, but with the immigrant growth rate by country of origin as the dependent variable. Robust standard errors clustered at the country of origin level are reported.

those in panel A, although sometimes marginally statistically significant. Its magnitude, however, is small, and the sign is always negative; hence, if anything the policy discouraged migration from non-EU countries.

Panel C in table 1 investigates whether the growth rate of the stock of immigrants changed differentially across countries as a result of the policy change. We investigate this point by using as the dependent variable in equation (10) the flow of immigrants between any two periods divided by the stock in the first period. Results suggest that immigrant flows did not change substantially around the policy change. If anything they seem to have declined, but only after 2008 and hence far from the policy change and outside the period we analyze later on.

The second approach that we follow to study magnet effects focuses on whether the policy led to significant changes in the share of immigrants across provinces. This exercise allows us to study whether magnet effects occurred at the province level and hence are correlated with changes in the share of immigrants affiliated with social security, our main regressor later on. To do so,

we use equation (8) to estimate the effect of the policy on the change in the share of foreign-born population from affected countries for total immigrants, immigrants in the labor force, and employed immigrants.

The results are shown in table 2. None of the coefficients are significant and all have a negative sign, indicating that the policy did not attract new immigrants from affected countries to the provinces where more immigrants were granted work permits.

Overall, figure 3 and table 1 show that the composition of immigrants in Spain did not tilt toward immigrants from non-EU countries of origin, nor did the growth rate of immigrant stocks change substantially between affected and nonaffected countries of origin around the implementation of the amnesty program. Moreover, table 2 shows that the policy did not increase the share of immigrants from affected countries in provinces where more immigrants were regularized. Based on this evidence, we conclude that the amnesty did not lead to magnet effects. Instead, the policy granted the right to work in the formal sector to a number of immigrants already living in Spain.

### C. Employment

We begin our exploration of the consequences that the regularization of almost 600,000 immigrants had on the labor market by documenting changes in employment rates. Employment rates are defined as the share of the working-age population who are actually working. Recall that we use data from two different data sources, the MCVL and the SLFS, which allows us to understand the effects that the amnesty had on both the formal and the informal labor market.

The results for employment in the formal labor market are presented in table 3. We report them for all workers (panel A) and for female workers (panel B). The first column of table 3 shows that the reform led to an

**Table 2**  
**Estimates of the Effect of the Immigration Reform on Share of Immigrants from Affected Countries**

	Dependent Variable: $\Delta$ Immigrant Population Share (Immigrant Population—OLS)		
	Total (1)	Labor Force (2)	Employment (3)
$\Delta$ Immigrants in social security/population	-.036 (.218)	-.267 (.241)	-.057 (.149)
$R^2$	.118	.153	.109
Observations	50	50	50

NOTE.—This table shows estimates of the effect of immigrant regularization on the different shares of the foreign-born population (total immigrants, immigrants in the labor force, and employed immigrants) from countries affected by the policy change. All shares are calculated as a proportion of the total population. Regressions are weighted by population. Estimates are based on a continuous difference-in-differences strategy, where province-specific prechange linear trends are removed. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. Robust standard errors are reported. OLS = ordinary least squares.

**Table 3**  
**Estimates of the Effect of the Immigration Reform on Formal Employment**

	Total Employment (1)	Natives (2)	Immigrants (3)	Natives		Immigrants	
				LS (4)	HS (5)	LS (6)	HS (7)
A. Formal Employment (MCVL—OLS; Dependent Variable: ΔFormal Employment/Population)							
All workers:							
ΔImmigrants in social security/population	.539 (.174)	.056 (.105)	.483 (.097)	.080 (.105)	-.024 (.010)	.462 (.094)	.021 (.005)
R <sup>2</sup>	.378	.235	.809	.243	.193	.801	.457
B. Formal Employment, Females (MCVL—OLS; Dependent Variable: ΔFormal Employment/Population)							
Female workers:							
ΔImmigrants in social security/population	.125 (.068)	-.058 (.037)	.183 (.043)	-.031 (.035)	-.027 (.010)	.173 (.041)	.009 (.004)
R <sup>2</sup>	.190	.241	.780	.234	.350	.774	.259
Observations	50	50	50	50	50	50	50
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.—This table shows estimates of the effect of immigrant regularization on formal employment. Panels A and B present estimates of formal employment for all workers and female workers, respectively. Estimates are based on a continuous difference-in-differences strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. Robust standard errors are reported. HS = high skilled; LS = low skilled; OLS = ordinary least squares.

increase in formal employment. This is one of the intended goals of the policy, since it moved informal immigrant workers to formality by granting them work permits. However, the estimate is quantitatively and statistically smaller than 1. Specifically, for each immigrant who entered the formal sector during the reform, there was an increase of a bit more of 0.5 jobs over the course of the subsequent two and a half years. This is in line with the predictions from the model: to the extent that the reform does not erase the informal market, some immigrant workers might return to work informally after obtaining the work permit.<sup>23</sup> The increase in total formal employment is driven, as expected, by an increase in immigrant formal employment (col. 3) and, in particular, immigrant low-skilled formal employment (col. 6). This aligns with the fact that immigration in Spain is strongly low-skilled biased, as shown in table F.1.

Moreover, the reform did not change the total number of native workers employed (col. 2) or low-skilled native workers (col. 4) in the formal sector.

<sup>23</sup> In sec. F.4 of app. F (see figs. F.5, F.6), we provide additional evidence to support this interpretation.

The last result is also consistent with the model predictions, which are ambiguous regarding this estimate. On the one hand, having more immigrants with the legal right to work in formal sector jobs puts pressure on native low-skilled employment. On the other hand, enforcement against informality reduces firms' market power to employ workers in the underground economy, which leads to increased formal employment. These two forces seem to cancel out which can explain an estimate in column 4 that is statistically indistinguishable from zero.

Additionally, we detect a small decline in native high-skilled employment, which comes entirely from high-skilled women (col. 5, panel B). In section IV.G, we show that one of the sectors in which the amnesty had a larger impact was housekeeping services. Thus, the reform increased the cost of home production and might have shifted the labor supply of some high-skilled women away from the formal labor market and into home production. This evidence is in line with what is reported by Cortés and Tessada (2011).

The results on formal employment suggest that we should find a decline in overall employment of around 0.5 workers per regularized immigrant. This is so because all of the immigrants who were regularized were already living in Spain for at least 6 months prior to the reform and needed a formal work contract for 6 additional months after the reform, yet 10 newly regularized workers lead to only five new formal jobs. We investigate whether indeed there is a decline in total employment by turning to data from the SLFS, which captures both formal and informal jobs. It is worth noting too that the model suggests that the overall decline in employment should be concentrated among low-skilled workers, since both the regularization and the enforcement policies against informality made low-skilled labor relatively more expensive.

In table 4, we quantify the effects of the policy change on total employment. As anticipated, we see that employment rates dropped as a consequence of the immigration reform. For each newly regularized immigrant, around 0.5 workers lost their jobs, which are necessarily informal sector jobs since formal sector employment increased.<sup>24</sup>

When we look at the split by skill groups and place of birth, we see that the overall loss of employment was concentrated among low-skilled workers in the informal sector. This evidence is consistent with two ideas: first, that the policy change increased enforcement against informality, and hence some native low-skilled workers working informally lost their informal jobs; second,

<sup>24</sup> In sec. F.6 of app. F, we show that employment losses of low-skilled natives are stronger in sectors with high concentrations of immigrants workers. This is consistent with the idea that the policy created some competition between natives and immigrants, even within sectors.

**Table 4**  
**Estimates of the Effect of the Immigration Reform on Total Employment**

	Total Employment: Formal + Informal (SLFS—OLS; Dependent Variable: $\Delta$ Total Employment/Population)						
	Total Employment (1)	Natives (2)	Immigrants (3)	Natives		Immigrants	
				LS (4)	HS (5)	LS (6)	HS (7)
$\Delta$ Immigrants in social security/population	-.520 (.215)	-.405 (.238)	-.116 (.156)	-.481 (.188)	.076 (.248)	-.373 (.153)	.257 (.084)
R <sup>2</sup>	.147	.178	.114	.276	.023	.247	.223
Observations	50	50	50	50	50	50	50
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.—This table shows estimates of the effect of immigrant regularization on total employment (formal and informal) of various types of workers. Estimates are based on a continuous difference-in-differences strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. Robust standard errors are reported. HS = high skilled; LS = low skilled; OLS = ordinary least squares.

that the policy made low-skilled informal-type jobs more expensive, and hence employers substituted away from these types of jobs.

Figure 4 displays the estimates of the event study specification, which shows visually the results reported in tables 3 and 4. The graphs on the left display results in the formal labor market for all workers, native workers, and immigrant workers. The graphs on the right show the estimates when jointly considering the formal and informal labor market. As can be seen, there is no sign of nonlinear differential pretreatment trends for any of the outcome variables. Similar evidence splitting the sample by skill level is shown in figures F.1 and F.2. It is worth highlighting that the standard errors reported in these and subsequent graphs are clustered at the province level, to account for potential serial correlation.

Finally, in section IV.F we describe a placebo exercise and robustness checks. The results are in line with the ones exposed in this section and give us confidence that the estimates reflect the effect of the policy change.

#### D. Wages

As we argue in the theoretical section, the effects on wages should mimic, in many respects, those of the employment rates. For instance, the model predicts ambiguous effects for labor types that experience increased competition with newly regularized immigrant workers, such as formal low-skilled workers. Instead, the model predicts positive wage effects for labor types that complement low-skilled workers and for workers who experience a reduction in employee labor market power. The effect of the various (sometimes opposing) forces operating in the model, may, however, generate differences in the estimates of the policy on wages and employment.

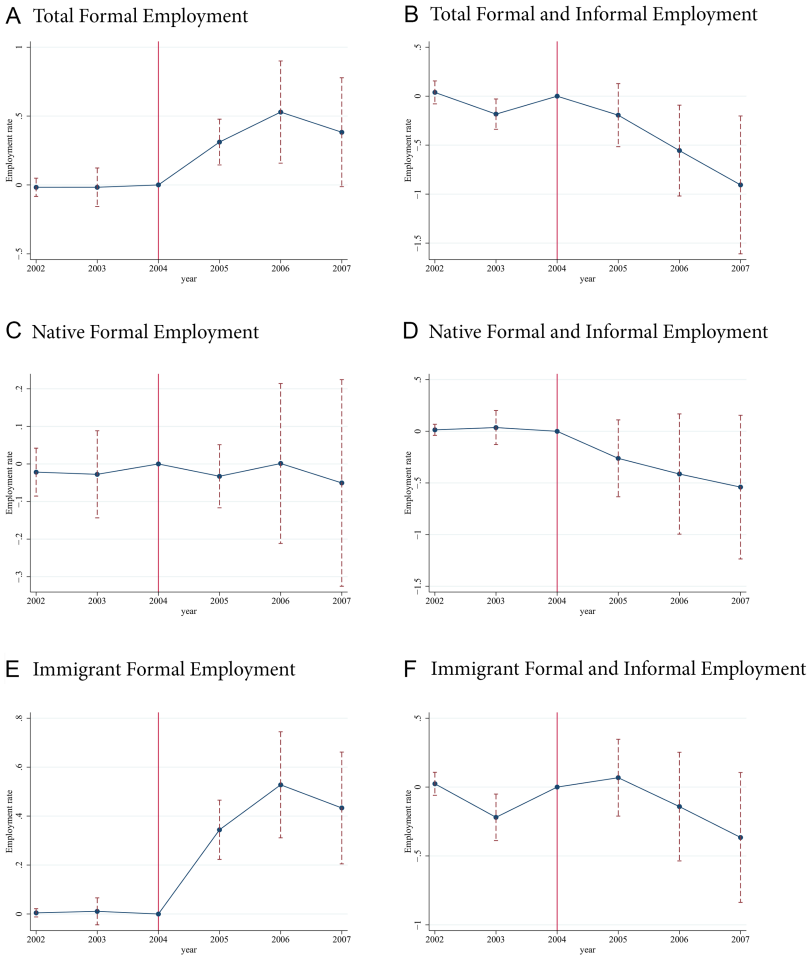


FIG. 4.—Effects on employment, dynamic difference in differences. This figure shows estimates from the dynamic difference-in-difference specification on employment. *A*, *C*, and *E* display results in the formal labor market. *B*, *D*, and *F* depict the effects on both the formal and the informal labor market. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical line indicates the last period before the reform. Source: Own elaboration based on MCVL and SLFS data.

In contrast to the evidence on employment, we can only use the MCVL to study the effect of the reform on wages. This means that all of our results describe what happened with wages of formal workers of various skills. To study the effect of the policy change on wages, we use the same empirical

strategy. Our measure of wages is “composition-adjusted wages.”<sup>25</sup> More concretely, we use a Mincerian regression allowing for specific returns across skills (low and high skilled) and allowing for pretreatment linear-specific trends at the province level. That is, we run the following regression:  $\log w_i = \beta_0 + \beta_1 X_i + \xi_i$ , where  $\log w_i$  is the log of the real daily wage of individual  $i$  and the vector  $X_i$  reflects individual characteristics, including skills, tenure, tenure squared, experience, experience squared, type of contract, and sector of activity for each skill level. In addition, we also include province and year fixed effects and province-specific linear trends. The assumptions that we make with this procedure are that the return to personal characteristics is equal across provinces and time, but we allow that different periods and different provinces may have different wage levels and wages may be evolving differently across provinces.

We present our main estimates in table 5. For all workers, the results show that wages increased following the policy change. The results are similar for native workers. Given that we have controlled for observable characteristics, the estimated changes in wages can only come from changes in the price of labor or changes in unobservable characteristics of those who are working.

Wages for high-skilled natives in high-immigration locations increased by 0.49 log points for a 1 percentage point increase in the share of immigrants registered in the social security system, while wages for low-skilled natives increased by 0.24 log points. Despite an overall negative effect, results are mixed for immigrants and less precisely estimated. The policy seems to have increased the wages of high-skilled immigrants substantially (although standard errors are large, reflecting the small number of high-skilled immigrants in our sample), while the wages of low-skilled immigrants decreased in the formal sector (although it was not statistically significant). This decrease probably reflects the changing composition of immigrant workers in the formal sector that occurred with the policy change.

The increase in wages of high-skilled natives, according to our model, reflects the increased demand for high-skilled labor once low-skilled labor becomes more expensive. Instead, the fact that wages of native low-skilled workers do not decline is in line with the fact that the policy change made formal- and informal-type jobs more substitutable, decreasing firms' market power over low-skilled workers with work permits. This force seems to be stronger than the potential pressure on formal low-skilled workers' wages coming from the increase in the number of low-skilled workers with work

<sup>25</sup> We consider the tax base of social security contributions divided by days worked each month as a proxy of daily wages. This is considered a proxy of wages because this tax base is bounded between a minimum and maximum amount that in 2005 stood at 598.2 and 2,813.4 euros per month. However, for a large majority of workers these limits are not binding.

**Table 5**  
**Estimates of the Effect of the Immigration Reform on Wages**

	Wages—OLS (Dependent Variable: $\Delta \log$ Wages)						
	Total (1)	Natives (2)	Immigrants (3)	Natives		Immigrant	
				LS (4)	HS (5)	LS (6)	HS (7)
$\Delta$ Immigrants in social security/population	.258 (.073)	.265 (.075)	-.219 (.344)	.238 (.083)	.493 (.214)	-.434 (.416)	1.827 (1.168)
$R^2$	.329	.369	.234	.340	.188	.228	.153
Observations	50	50	50	50	50	50	50
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.—This table shows estimates of the effect of immigrant regularization on wages. Estimates are based on a continuous difference-in-differences strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. Robust standard errors are reported. HS = high skilled; LS = low skilled; OLS = ordinary least squares.

permits. This result is similar to what we observed with formal native low-skill employment.

The estimates from the dynamic difference-in-differences specification are shown in figure 5. There are no significant nonlinear differential pre-treatment trends. The graphs visualize the positive effect on the wages for all workers and for natives (fig. 5A, 5B) and the evidence for immigrants (fig. 5C), which is in line with the results displayed in table 5. Similar evidence splitting the sample by skill level is shown in figure F.2.

### E. Public Finances

The results in this section show that the amnesty raised government net revenue. On the one hand, the reform increased both payroll and income tax collection. On the other hand, we show that the policy did not increase public spending in health care and education, the two largest government expenditures, which is not surprising since these services were already available to undocumented immigrants. Therefore, we can conclude that the reform improved public finances.

The results are shown in table 6. In total, as shown in column 9, each newly regularized immigrant increased payroll tax revenues by almost 4,000 euros. Columns 1–8 show the disaggregate estimates for each of the labor market contribution types (note that the sum of the coefficients shown in these columns adds to the one in col. 9). The increase in payroll tax collection comes from the increase in payroll tax revenues from the general regime (99% of the overall increase in total payroll tax collection), housekeeping regime (5%), and agrarian regime (5%), with some decline in other categories. The general, agrarian, and housekeeping regimes are the most commonly used by immigrant

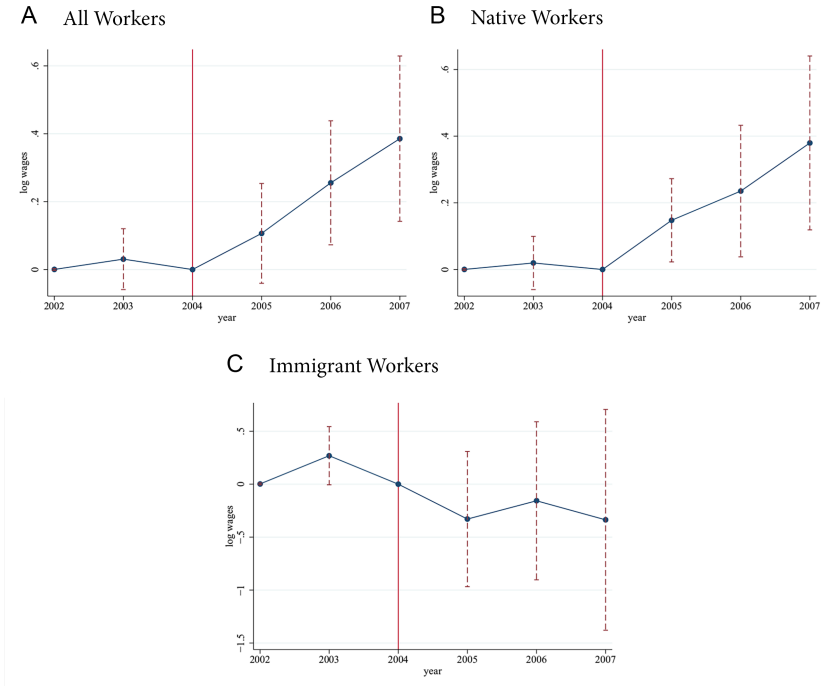


FIG. 5.—Effects on wages, dynamic difference in differences. This figure shows estimates from the dynamic difference-in-differences specification on wages in the formal sector. *A* displays results for all workers, *B* for native workers, and *C* for immigrant workers. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical line indicates the last period before the reform. Source: Own elaboration based on MCVL data.

workers. Overall, these estimates strongly suggest that the policy reform lead to a substantial increase in payroll tax revenues.

Figure 6*A* shows that there are no systematic nonlinear differences across provinces in total payroll tax revenues up to 2004. In 2005, when newly regularized immigrants started to contribute to social security, we see an increase that fully materializes in 2006 and 2007. The point estimates suggest that by 2006 and 2007 each newly regularized immigrant increased total payroll tax revenues by 4,000 and almost 6,000 euros, respectively. Figure 6*B* shows evidence for payroll tax revenues of housekeeping service workers. The contribution in that case reached its maximum in 2006, with a decline in 2007. This reflects the fact that some time after the regularization, immigrants started to move to other activities, as we explore in more detail in section IV.G. All of these results are in line with the results displayed in table 6.

The payroll tax is the most important channel through which the policy change affected tax collection. Additionally, we expect that some of the

**Table 6**  
**Estimates of the Effect of the Immigration Reform on Payroll Tax and Income Tax Revenues**

	Tax Revenues—OLS										
	Dependent Variable: Change in per Capita Payroll Tax Revenues by Labor Market Contract Type (in Euros)					Dependent Variable: Change in Number of Tax Returns					
	General Reg. (1)	Self. Emp. (2)	Agricult. (3)	Sea (4)	Coal (5)	Housekeeping (6)	Accident (7)	Unemp. (8)	Total (9)	Total (10)	Euros (11)
ΔImmigrants in social security/population	3,870 (1,116)	94.5 (54.0)	186.6 (47.2)	-3.9 (17.1)	12.1 (20.5)	186.0 (57.8)	-17.0 (29.7)	-446.8 (301.7)	3,882 (914.4)	.341 (.130)	439.8 ...
R <sup>2</sup>	.584	.194	.419	.225	.092	.700	.216	.497	.642	.291	...
Observations	50	50	50	50	50	50	50	50	50	46	...
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share of contribution (%)	99	2	5	0	0	5	0	-12	100	...	...
Immigrant share (%)	6.9	4.5	12.3	5.1	5.2	51.3	...	...	7.5	...	...

NOTE.—This table shows estimates of the contribution per regularized immigrant in each regime of social security in euros. Estimates are based on a continuous difference-in-differences strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. The coefficients in cols. 1–8 add to the coefficient in col. 9. Column 10 estimates are based on tax authorities' data on province-level filings (four provinces have decentralized income tax collection, for which we do not have data: Alava, Vizcaya, Guipuzkoa, and Navarra). Robust standard errors are reported. Immigrant share represents the percentage of immigrants in each regime (average 2002–7). OLS = ordinary least squares.

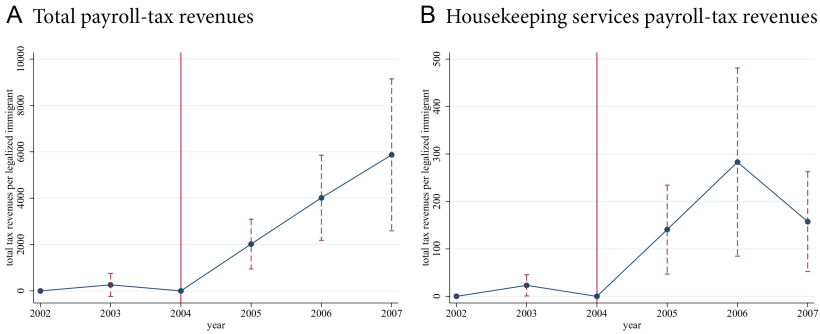


FIG. 6.—Effects on payroll taxes, dynamic difference in differences. *A* shows a dynamic difference-in-differences specification using the total payroll tax revenues in terms of population as the dependent variable and the size of the regularization in each province as an explanatory variable. *B* shows the same graphs but for payroll tax revenues in the category of housekeeping services. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical line indicates the last period before the reform (2004). Source: Own elaboration based on social security data.

immigrants who entered the formal labor market started paying income taxes, although in a smaller magnitude since the income tax is negligible at low income levels and most newly regularized immigrants entered formal employment with relatively low wages. To quantify how many extra workers started to pay income taxes, we estimate equation (8) using as the dependent variable the number of filed income tax returns divided by the total population.

We find that for 10 immigrants who gained work permits, 3.4 extra workers filed income tax returns (table 6, col. 10). This number represents around 60% of the new entrants to the formal labor market (see table 3, col. 1, panel A). To translate the change in the number of workers who file income tax returns into monetary terms, we use the distribution of wages of the immigrants who gained the legal status and the effective income tax rates by income brackets for these workers. We obtain an estimate of around 440 euros a year (see table 6, col. 11).<sup>26</sup>

Last, we provide evidence that the reform did not increase public spending. In table 7, we estimate equation (8) using as outcomes several variables

<sup>26</sup> The effective income tax rates by income bracket appears every year in the statistical yearbook of the income tax published by the Ministry of Finance (see <https://www.hacienda.gob.es/es-ES/CDI/Paginas/centraldeinformacion.aspx>; accessed September 2023).

**Table 7**  
**Effect of the Immigration Reform on Education Enrollment and Hospital Discharges**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Educational Enrollment—OLS							
	All	Kindergarten	Primary	Secondary	High School		
$\Delta$ Immigrants in social security/population	-.033 (.038)	-.012 (.027)	-.002 (.016)	-.012 (.010)	-.053 (.032)		
$R^2$	.091	.082	.155	.487	.172		
B. Hospital Discharges—OLS							
	All	Tumors	Mental	Circulatory	Respiratory	Birth	Other
$\Delta$ Immigrants in social security/population	.017 (.079)	.012 (.014)	-.004 (.006)	-.005 (.016)	.005 (.023)	.018 (.024)	.001 (.066)
$R^2$	.032	.061	.196	.065	.144	.072	.066
$\Delta$ Immigrants	-.036	.025	.009	.006	.006	.018	-.075
Observations	50	50	50	50	50	50	50
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.—This table shows estimates of the effect of immigrant regularization on public education enrollment (panel A) and hospital discharges (panel B). Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. The category “other” in panel B includes metabolism, nervous system, infectious, congenital, perinatal, skin, genitourinary, blood, poisoning, digestive, articulate, and without-diagnosis diseases. Regressions are weighted by population. Controls include political alignment dummies, coastal dummies, share of construction, and share of immigrants from non-EU-15 countries at baseline. Robust standard errors are reported. OLS = ordinary least squares.

of enrollment in public education and hospital discharges. We use these outcomes because those are the ones available at the province level. We do not find any significant effect of the regularization on the two main public spending programs. In table F.13, we explore whether the policy change affected expenditures at the autonomous community level, even if we can only use variation from 17 regions. The results are in line with those of table 7 and suggest that the regularization did not lead to large changes in public spending.

Overall, the results presented in table 6 and figure 6 suggest that the policy was effective in one of its main goals: it helped to raise tax revenues.

#### F. Placebos and Robustness Checks

In this section, we describe a few more empirical exercises that highlight the robustness of the results. First, we perform a placebo exercise where we estimate the effect of the policy change on changes in outcomes around 2003; that is, we see whether the policy implemented in 2005 affected outcomes before and after the year 2003. We expect from this exercise a number of estimates that are not distinguishable from zero.

We present the placebo estimates in table F.4. Panel A shows estimates on formal employment using the MCVL. All estimates are small and statistically indistinguishable from zero. Panel B shows estimates from the SLFS. Again, all estimates are statistically insignificant. Panel C shows a very similar thing for wages with the exception of immigrant wages, which increased in high-immigration provinces around 2003 relative to low-immigration ones. Panel D shows estimates on payroll tax collection of a number of categories. The overall tax collection did not increase differentially across provinces around 2003. The estimates for the other categories are also small and, in general, statistically indistinguishable from zero. The few estimates that are statistically different from zero are in categories that were not used by immigrants during the reform, such as self-employment, or are small in magnitude, as in the housekeeping regime.

Second, we reproduce our main results using a shift-share instrument, as explained in appendix E. As can be seen in panels E in tables F.5–F.11, our 2SLS estimates show that our baseline results are similar to the ones shown in the main text, reducing the risk that our findings are driven by other confounding factors or endogeneity concerns.

A third way in which we can gain confidence on our main estimates is to check whether they are sensitive to different specifications. In particular, we investigate two different additional specifications for every outcome variable in our main analysis. First, we show our results without controls. Second, we present estimates excluding the four largest provinces: Madrid, Barcelona, Valencia, and Sevilla. We also present these results in tables F.5–F.11. The estimates are broadly similar to the ones presented in the previous sections.

### G. Newly Regularized Immigrants' Labor Market Experience

In the theoretical model, we characterized the amnesty reform as an increase in the labor supply choice set of undocumented immigrants. In particular, we assumed that these workers had a more elastic supply curve after the reform and hence were less vulnerable against the market power that employers could exert over them. In this section, we provide evidence based on the work histories of immigrants after regularization that is consistent with this mechanism.<sup>27</sup>

In particular, we follow the working histories of immigrants after they enter the social security system, which we can do with the MCVL data. Moreover, we can leverage one of the particularities of the policy change—namely, the fact that immigrants required an employer to offer them a work contract for 6 months. This requirement likely pushed immigrant workers to stay in the same job they had while being undocumented for around 6 months, after which they could then more freely choose their employer.

To study this, we focus our attention on the immigrant workers who entered the social security system for the first time between February and August 2005 using the labor market contribution type designed for housekeeping services and stayed in the social security system until 2009. We focus on these workers because the prevalence of informal work in this sector was, prior to the policy change, extremely high. The regularization process was an opportunity for many of these workers to gain work permits and potentially a path toward other types of occupations.<sup>28</sup>

Figure 7 shows six graphs that illustrate the effect of the reform on labor market choices of housekeeping service workers. Figure 7A shows the total number of affiliates in the social security system that use the contribution type associated with housekeeping services. This number fluctuates around 60,000 prior to the amnesty program. Coinciding with the policy change, this number jumps immediately to almost 160,000. After 6 months, the number of affiliates decreases to a plateau of around 100,000 workers.

Among the newly regularized workers who remained in the social security system until 2009 (and hence for whom we can reconstruct their working

<sup>27</sup> In table F.14, we show, with a 2006 survey on immigrant workers (see app. A, sec. A.5), that the immigrants who lack work permits are paid around 75% of the wages of immigrants with work permits doing similar type of jobs, which is also consistent with the idea that having work permits attenuates the labor market power that firms can exert on immigrant workers. For two papers making a similar point, see Naidu, Nyarko, and Wang (2016) and Ortega and Hsin (2022).

<sup>28</sup> Housekeeping workers needed to work at least 30 hours per week to gain the work permit. Hence, we can consider the workers who entered the social security system after the regularization in this way as full-time workers, with little room for informal side payments (at least when working under legal labor contracts). These 30 hours per week did not need to be in the same household. Workers could be working in several places to reach these 30 hours.

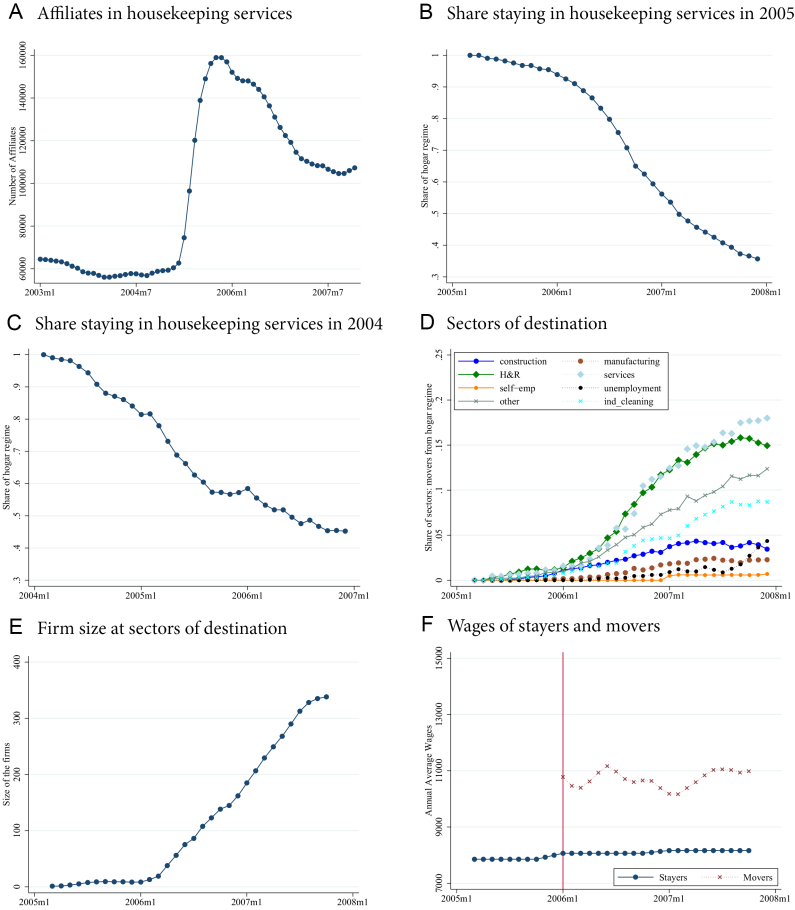


FIG. 7.—Newly regularized immigrants and sector switching. *A* shows the total number of affiliates in the housekeeping labor contract (*regimen del hogar*). *B* shows the fraction of immigrants who remained in housekeeping services among the immigrants who entered the social security system with the regularization in the housekeeping sector and continued in the sector throughout the period. *C* shows the fraction of immigrants who remained in housekeeping services among the immigrants who entered the social security system one year before the regularization in the housekeeping sector and continued in the sector until 2007. *D* shows the sectors where immigrants who entered the social security system with the regularization using housekeeping services contribution types and continued in the social security throughout the period move to. *E* shows the average size of the firm where immigrants who entered the social security system with the regularization using housekeeping services contribution types and continued to work throughout the period were working. *F* shows the difference between annual average wages of regularized immigrants who move away from housekeeping to others sectors of activity and the annual average wages of workers regularized through the housekeeping sector who remain in the sector. The vertical line indicates the beginning of 2006, where we observe that most movements took place.

history), we can track their sector of activity in each month. Figure 7B shows the fraction of these newly regularized workers who remained in the housekeeping sector. It is striking that the fraction who remained in housekeeping services stayed very close to 1 for the entire 2005. These workers necessarily had a 6-month work contract with the families through which they had entered the formal market. However, around the beginning of 2006, many of these workers started to move to other sectors of the economy with, as we will argue, better labor market opportunities. These patterns, as we show in figure F.7, are similar across all sectors.

The evidence in figure 7B is in sharp contrast to the working history of (the much smaller number of) immigrants who entered the social security system in 2004 as housekeeping service workers. These immigrants did not use the amnesty program to enter the social security system. In contrast to what was required by the reform of 2005, immigrant workers in the housekeeping sector did not have incentives to remain with their employer for 6 months. As can be seen in figure 7C, immigrants entering the system in 2004 switched much more smoothly from housekeeping services to other sectors than immigrants who entered through the amnesty of 2005.

Next, we investigate the destination sectors of immigrants entering the social security system through housekeeping services and the characteristics of the new jobs available to them. Figure 7D shows this evidence. Most of these immigrants entered the hotels and restaurants sector and other services, although many also entered other sectors. Figure 7E shows the average size of the firms where these workers were working. When employed in housekeeping services, the employer is typically a family that usually hires one person to either clean the house or take care of the young or elderly. As they move to other sectors, the average size of the employer increases substantially. Figure 7F shows that the wage of movers was around 30% higher than the average wage of those regularized immigrants who stayed in the sector. Therefore, these graphs suggest that the housekeeping workers who left the sector moved into larger firms and higher-paying jobs. The same figures for all new immigrants in the social security system and for two other sectors with a high presence of undocumented migrants—albeit with lower informality, like the construction sector and the hotels and restaurants sector—are shown in section F.9 of appendix F. Broadly, the graphs show similar patterns.

Overall, we view this as evidence that the policy change allowed immigrant workers to enter the formal market economy and explore jobs beyond the ones they had prior to the regularization, something that likely contributed to their assimilation to the host country, as argued using data for the United States and variation from IRCA in Bratsberg, Ragan, and Nasir (2002). We interpret these findings as suggesting that market power against undocumented workers was reduced as a consequence of the policy, in line with evidence from developing countries (Naidu, Nyarko, and Wang 2016).

## V. Conclusion

This paper studies the consequences of a large amnesty program in Spain. To do so, we combine detailed geographic data on tax revenues and labor market outcomes, and we show that the regularization of around 600,000 immigrants, combined with increased enforcement against informality, increased formal employment. Around half of the previously undocumented workers stayed in the formal sector. However, this was accompanied by a decrease in employment in the informal sector. Effects on wages are small but if anything positive, reflecting the complementarities between immigrant and native workers and the reduction of market power.

The regularization program raised government net revenue. On the one hand, tax revenues increased because the employers of formerly undocumented immigrants started paying payroll taxes, and the regularized immigrants might have started paying income taxes if their wages were high enough. On the other hand, we do not detect any increase in spending on public education or health care, which is not surprising because unauthorized immigrants already had access to these services.

Finally, we report evidence that the amnesty program increased the labor market opportunities of immigrant workers. After regularization, they started changing sectors and moving to larger and higher-paying firms.

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