

# Understanding the Effects of Legalizing Undocumented Immigrants

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## ABSTRACT

This paper studies the legalization of 600,000 immigrants by the unexpectedly elected Spanish government following the terrorist attacks of 2004. We estimate that each legalized immigrant increased payroll-tax revenues by 4,801 euros. This takes into account both the direct impact of legalized immigrants and heterogeneous labor-market effects. The paper documents how the policy change *deteriorated* the labor-market outcomes of a selected group of low-skilled natives, *improved* the outcomes of high-skilled workers, and how some low-skilled immigrants moved away from high-immigrant locations. Taking into account both selection and internal migration is crucial to understand the consequences of amnesty programs fully.

**JEL Classification codes:** F22, J31, J61, R11.

**Keywords:** Immigration, undocumented immigrants, public policy evaluation.

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# 1 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 2014 there were as many as 11.1 million unauthorized immigrants on American soil, representing 26 percent of all immigrants.<sup>2</sup> These large numbers of undocumented immigrants have led recent U.S. administrations, not without controversy, to consider either legalizing these immigrants or deporting many of them to their countries of origin.

The U.S. is not alone in having undocumented immigrants. In the early 2000s, Spain experienced an incredible boom in immigration. From 1995 to 2004, the share of immigrants in the working-age population increased from less than 2 percent to around 10 percent.<sup>3</sup> Many of these newly arrived immigrants lacked work permits. According to some accounts, close to 1 million immigrants—in a country of around 43 million inhabitants—were undocumented by 2004.<sup>4</sup>

Despite these large numbers and the public policy debates around immigrant legalization, little is known about the effects of amnesty programs on the overall labor market. This paper fills this gap. In December 2004, the newly elected government of José Luis Rodríguez Zapatero passed a law that resulted in the legalization of around 600,000 immigrants already working illegally in Spain. This legalization meant that the number of workers registered in the social security system increased by around 3 percentage points overall. In fact, a large fraction of illegal immigrants became legal workers, something that was attained thanks to the efforts of the Spanish authorities in enforcing and monitoring the implementation of the policy. For example, work inspections increased by an astonishing 132 percent, something that was widely announced at the time.<sup>5</sup>

By many accounts, this policy change was completely unexpected. Zapatero had won the general election in Spain only three days after the terrorist attack of March 11, 2004, in Madrid which killed nearly 200 people, the largest terrorist attack in Spanish history. Before the attack, polls forecast that Zapatero trailed Rajoy by 7 percentage points. It is very likely that the mishandling of the crisis in the post-attack days caused Rajoy’s Popular Party to lose this election, as explained in detail in [Garcia-Montalvo \(2011\)](#), and it is very unlikely that a government led by Rajoy would have ever passed such a large amnesty program. Previous legalizations in Spain were much smaller, easier to anticipate, and not directed at workers but mainly at family reunification.<sup>6</sup>

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<sup>1</sup>In this paper, “undocumented immigrants” refers to workers that were born outside the country in which they reside and that do not have the legal right to work or stay in the host country.

<sup>2</sup>See [www.pewhispanic.org/interactives/unauthorized-immigrants/](http://www.pewhispanic.org/interactives/unauthorized-immigrants/) (accessed in February 2018).

<sup>3</sup>Data from Spanish Labor Force Survey (SLFS). See more details below.

<sup>4</sup>See [Domingo and Recaño \(2005\)](#).

<sup>5</sup>For a news report on the policy, see [elpais.com/elpais/2005/05/07/actualidad/1115453817\\_850215.html](http://elpais.com/elpais/2005/05/07/actualidad/1115453817_850215.html) (accessed in February 2018). The news from El País at the time had a special mention of the increase in work inspections. Data for work inspections can be found at [www.empleo.gob.es/itss/web/Que\\_hacemos/Estadisticas/](http://www.empleo.gob.es/itss/web/Que_hacemos/Estadisticas/) (accessed in February 2018). The number of yearly work inspections before the policy was around 30,000. See Appendix B for more details.

<sup>6</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 Zapatero. In fact, most reforms were not exclusively focused on immigrants’ working status and, thus, likely had smaller labor market effects. The 1985 legalization granted legal status to around 44,000 immigrants, irrespective on whether they were working or not. In 1991 another regularization approved almost 110,000 work and residence permits, a large fraction of which were granted on the basis of family reunifications—i.e., were not linked to labor

Thus, we can use this episode as a natural experiment to understand the effects of policies that grant work permits to immigrants already working illegally.<sup>7</sup> In this paper we show that the legalization of a large number of mainly low-skilled immigrants meant that a) newly legalized workers would contribute to the public system, thus increasing public revenues, b) immigrant low-skilled workers became around 30 percent more expensive—while still being cheaper than low-skilled natives—and at the same time closer substitutes to native low-skilled workers as they gained work permits. As a result the relative labor demand moved towards high-skilled workers, affecting indirectly public tax revenues.

We obtain these results and assess the effects of the amnesty program by comparing Spanish provinces that had large immigrant populations prior to the policy with those that had small immigrant populations, using a number of different specifications.<sup>8</sup> Using administrative data, we first estimate that, for each newly legalized immigrant, payroll-tax revenues increased by 4,189 euros at the province level.<sup>9</sup> That is only 55 percent of what we would have anticipated given the 3 percentage point increase in workers registered in the social security system as a result of the legalization. This suggests either that newly legalized immigrants earned less than average workers or that other workers were also affected by the policy change (or a combination of both).

Second, we investigate the effect of the amnesty program on the labor-market outcomes of various types of workers. We find that over the two years that followed the reform, for every 10 newly legalized immigrants, 4.5 low-skilled natives and 3.0 low-skilled immigrants lost their jobs, while almost 1 additional high-skilled native and almost 2 additional high-skilled immigrants found a job.<sup>10</sup> That is, overall employment decreased while also becoming more intensive in high-skilled workers.<sup>11</sup> This suggests that the legalization of mainly

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market participation. After the Spanish immigration boom started, in 2000, 150,000 immigrants obtained work/residence permits, and again a considerable fraction of these immigrants were not working. Finally, in 2001 there was another regularization process (known as *Regularización por Arraigo*) that regularized the working situation of around 235,000 immigrants, numbers that also include family reunifications (see CES, 2004). In all these regularizations, with the exception of a regularization that took place in 1996 where a labor contract at the moment of application was needed and which gave work permits to around 21,000 immigrants, there was no connection between the requirement to apply and the labor situation of the immigrants involved. Thus, their main intention was not to make workers already working illegally change their work status and make them contribute to public finances, but rather to accommodate immigrant families in the host country.

<sup>7</sup>We leave aside in this paper potential effects that amnesty programs may have on changing total immigration flows into a country. Some papers have analyzed this possibility. For instance, [Orrenius and Zavodny \(2003\)](#) examine the effects of IRCA 1986 on flows of undocumented immigration the U.S.-Mexican border.

<sup>8</sup>We show that our results are very robust to a number of specifications. In [Appendix A](#) we show that our baseline results reported in the main text, which mainly account for potentially different trends at the province level, are robust to changing the sample of provinces, to controlling for several confounding factors and to using immigrant-network 2SLS strategies to estimate all our coefficients of interest.

<sup>9</sup>Payroll taxes in Spain are around one-third of wages. Average wages before the policy change were almost 20,000 euros.

<sup>10</sup>From the SLFS data we know the employment rates of native workers and immigrant workers before and after to the policy change. Among immigrants *both* documented and undocumented workers are included in the sample since it is constructed using Population Censuses and Population Municipal Registry. Undocumented immigrants had strong incentives to register in the Municipal Registry given that this granted them access to health care and other services.

<sup>11</sup>To get a sense of the magnitude it is worth mentioning that, for every undocumented immigrant, labor costs to the firm increased by slightly more than 30 percent, due to payroll taxes. Given that legalized immigrants represented around 4 percent of low-skilled workers, this means that total low-skilled labor costs increased by around 1.4 percent (given a 35 percent increase in labor costs due to payroll taxes). Similarly, for every newly legalized immigrant around 0.5 low-skilled workers lost their jobs. Legalized migrants are around 4 percent of all low-skilled workers, so in total around 1.8 percent of low-skilled workers lost their job. Thus, this suggests that the short-run labor demand elasticity is around 1.3 or that the inverse demand elasticity is around 0.8. These numbers are in-line with a number of estimates in the literature on minimum wages, immigration, and labor markets in general. See for example [Monras \(2015c\)](#), [Neumark \(2017\)](#), [Borjas \(2003\)](#), [Acemoglu, Autor and Lyle \(2004\)](#), or [Card and Lemieux \(2001\)](#).

low-skilled immigrants had a detrimental effect on the employment outcomes of similar workers and a positive effect on the employment outcomes of complementary workers, consistent with the relative increase in the cost of low-skilled labor implied by the policy change.<sup>12</sup>

Instead, average wages of *both* high- and low-skilled natives increased as a result of the policy. While the increase in high-skilled wages is consistent with an increase in the relative demand for high-skilled labor, we explain the increase in low-skilled wages by selection on unobservables: low-skilled natives who lost their jobs belonged to the bottom end of the wage distribution of low-skilled workers. More concretely, we show that low-skilled natives who entered the labor market in 2005 earned more than previous entrants and those who lost their jobs earned less. Thus, low-skilled natives who entered the labor market in 2005 were positively selected and those who left the market were negatively selected. This, combined with the zero change in wages of those low-skilled natives who were always working during that period, explains all the change in average wages of native low-skilled workers in high-immigrant locations relative to low-immigrant locations and points to the importance of understanding selection patterns when assessing these types of policies. We also show that low-skilled immigrant wages slightly *decreased* and high-skilled immigrant wages *increased* by almost 1 log point for a 1 percent increase in the share of legal immigrants. This deterioration of labor market conditions for low-skilled workers may also explain why for each newly legalized immigrant in a location, 0.43 low-skilled immigrants relocated to other lower-immigration locations, while 0.07 high-skilled immigrants relocated to high-immigrant locations, suggesting that the policy increased internal mobility of mainly foreign-born individuals over the two years following the reform.

We argue in this paper that both selection in the labor market and internal migration are crucial for the right assessment of the policy, something that we view as an important methodological contribution of our paper. If we only had had payroll-tax revenue data, we would not have been able to take into account that the policy change induced internal migration, so that some of the newly legalized immigrant workers did not pay taxes in high immigrant locations but instead paid them in low immigrant regions. Similarly, if we only had had standard repeated cross-section data from labor-market surveys, it would have been difficult to take into account selection in the labor market. In particular we wouldn't have known whether those who lost their jobs or those who entered the labor market were a selected pool of workers, and thus we wouldn't have been able to infer the true changes in tax revenues from labor market data alone.<sup>13</sup> Instead, in this paper we can use the fact that our data records the working history of individual workers and the fact that payroll-tax revenues estimated from the labor market side need to match local estimates on payroll-tax revenues from administrative data (once selection is taken into account) to deal with the biases that both internal migration and selection patterns may generate.

Taking into account both selection and internal migration, we estimate that each newly legalized immi-

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<sup>12</sup>This evidence is consistent with the heterogeneous effects of immigrant inflows on native employment outcomes estimated recently in [Borjas and Monras \(2017\)](#). See also the work by [Llull \(2017b\)](#) in which heterogeneous effects across workers are reported.

<sup>13</sup>The estimated effects of the policy change on the labor market imply that, for each newly legalized immigrant, payroll-tax revenues should have increased by only 2,330 euros. This falls 1,859 euros short of the direct estimates obtained from payroll-revenue data. We show in detail how selection can explain this divergence.

grant increased payroll-tax revenues by 4,801 euros, or 15 percent more than the raw payroll-revenue data estimates suggest. Given that, prior to the policy change, undocumented immigrants already had access to public education and public health care, this estimate represents a net gain in terms of tax revenues.<sup>14</sup> This estimate would be even larger if we included income taxes that newly legalized immigrants started to pay.<sup>15</sup> Thus, these results highlight important public revenue losses associated with not granting work permits to immigrant workers already working illegally.

To the best of our knowledge, this is the first paper to combine public-revenue data and detailed labor-market data to account for the various channels through which amnesty programs can affect the economy. It provides the first account of the potential gains and losses that such policies may bring. On the one hand, it provides clear evidence that the policy succeeded in one of its goals: increasing tax revenues from workers who were already working but were not contributing to public finances. On the other hand, we show how the policy had important distributional consequences—low-skilled workers probably lost, while highly skilled native and immigrant workers benefited.

Several empirical papers have studied amnesty programs in a variety of countries. In a recent paper, [Pinotti \(2017\)](#) uses a sharp discontinuity design to show that legal status significantly reduces crime rates. While his identification strategy is convincing, it is not suited to studying the overall effects of the amnesty program on the labor market, as we do.<sup>16</sup> Similarly, [Baker \(2015\)](#) finds that the Immigration Reform and Control Act of 1986 significantly reduced crime in the U.S. There are also other papers that estimate the effects of amnesty programs on the general outcomes of immigrants ([Dustmann, Fasani and Speciale, 2017](#)), and more specifically their labor-market outcomes. Most of these papers show that the employment prospects of newly legalized immigrants improve as a result of the legalization ([Devillanova, Fasani and Frattini, 2017](#); [Amuedo-Dorantes and Bansak, 2011](#); [Amuedo-Dorantes, Bansak and Raphael, 2007](#); [Kaushal, 2006](#); [Cobb-Clark, Shiells and Lowell, 1995](#)).<sup>17</sup> In general, however, these papers make no mention of the potential consequences that these programs have on native workers' labor-market outcomes.<sup>18</sup>

Many of these amnesty programs, most famously the Immigration Reform and Control Act of 1986, combine the amnesty with increased border enforcement. Hence, there are also some papers that estimate the attracting or deterring effects that these programs have on prospective immigrants ([Hanson and Spilimbergo, 1999](#); [Orrenius and Zavodny, 2003](#)).

Relative to most of these papers, our case study generates arguably exogenous variation stemming from

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<sup>14</sup>During the period that we are analyzing, all immigrants enjoyed a generalized access to public services. To obtain access to these services, immigrants only had to be enrolled in the Municipal Registry of Population (*Padrón Municipal*) and, to do so, they did not have to provide any proof of their legal status.

<sup>15</sup>There are no direct data for this since income taxes differ across autonomous communities and are collected by the national government. Given that average wages of newly legalized immigrants were low, increases in revenues from income tax are unlikely to be very important economically.

<sup>16</sup>See also related evidence in [Mastrobuoni and Pinotti \(2015\)](#).

<sup>17</sup>A recent paper by [Cascio and Lewis \(2017\)](#) shows that the Immigration Reform and Control Act of 1986 redistributed resources toward high immigration locations. This redistribution stemmed from increases in transfers from programs like the EITC and increases in local tax revenues.

<sup>18</sup>[Dolado, Duce and Jimeno \(1996\)](#) study an amnesty program in Spain in the early '90s. Relative to this paper, we can make use of both administrative payroll-tax data and much more detailed labor market data to obtain deeper insights. These data were not available in their study.

the particular circumstances that led Zapatero to become the Spanish prime minister in 2004. Moreover, relative to other studies, ours is the only paper that combines detailed data on both public tax revenues and labor-market outcomes disaggregated at a fine geographic level, something that we show is crucial for the overall analysis.<sup>19</sup>

In what follows, we start, in Section 2, by describing the particular circumstances that led to the policy change and the data that we have at our disposal to conduct the analysis. We then show empirical evidence on both public revenues and labor-market outcomes. This is done in Section 3. In Section 4 we discuss the results and explain the various biases that potentially arise when working with tax-revenue data or labor-market data exclusively. In Section 4 we also show how we can combine these estimates to obtain more accurate results. Section 5 offers our conclusions.

## 2 Background, Data, and the Policy Change

Spain is among the countries with medium-high levels of immigration. More than 13 percent of its population is foreign-born, with Romania, Morocco, and Ecuador the top countries of origin. Relative to other European countries, such as Germany, this is a recent phenomenon. Immigrants started to arrive in Spain in large numbers in the late 1990s, and this flow continued through the 2000s, up to at least the beginning of the Great Recession in 2008.

Concerns about the arrival of large waves of immigrants intensified in the early 2000s. For example, a new law drafted in 2000 and put into effect in June 2001 recognized Spain as “a land of immigration” and subsequently established tougher conditions for immigrants to settle in Spain.<sup>20</sup> Similarly, in June 2002, the EU Summit in Seville agreed on tougher regulations to deter illegal immigration to Europe.<sup>21</sup>

Most of these efforts to deter further immigration were put in place by the Popular Party. This is the major center-right party in Spain, which ruled the country under the presidency of José Maria Aznar between 1996 and 2004. Like other center-right parties in Europe, this is the party that in Spain has traditionally adopted tougher regulations to limit immigration. The party won the general election in 1996 and consolidated its power in the 2000 elections with the majority of seats. From the beginning of his mandate, Aznar announced that he would seek to stay in power for only eight years. He was replaced as head of the party by Mariano Rajoy, already in his cabinet and, at the time, one of his closest ministers. Despite the large political protests against Spanish involvement in the Iraq war, the government and most of the people in Spain expected the Popular Party to continue in power after the March 2004 elections.

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<sup>19</sup>This paper is obviously related to the wider literature that uses geographic variation to estimate the labor-market effects of immigrant inflows. See Card (1990), Altonji and Card (1991), Borjas, Freeman and Katz (1997), Card (2005), Lewis (2012), Lull (2017a), Glitz (2012), Borjas and Monras (2017), and Monras (2015b).

<sup>20</sup>See Real Decreto 864/2001.

<sup>21</sup>In Seville, the host, Spanish Prime Minister Jose Maria Aznar, said that reducing illegal immigration was “the most important question in European politics at the moment,” and urged the EU to develop a “concrete timetable that will effectively give a very clear message that Europe is committed to combating illegal immigration.... We must combat these criminal organizations that traffic in illegal immigrants.” <https://migration.ucdavis.edu/mn/more.php?id=2661> (accessed in February 2018).

According to the CIS (*Centro de Investigaciones Sociológicas*), the vote forecast for the two main political parties in Spain (poll conducted in January 2004) was 42.2 percent for the Popular Party and 35.5 percent for the Socialist Party.

Yet something completely unexpected occurred on March 11, 2004, just three days before the election. Early that morning, several terrorists attacked a number of commuter trains in Madrid. Almost 200 people died in what was the largest-ever terrorist attack on Spanish soil.

The attack was, in many respects, larger than all the terrorist attacks that took place on Spanish soil from the early 1970s onward, mainly perpetrated by the Basque terrorist group ETA.<sup>22</sup> Following the attacks, the three days leading to the general election were chaotic. Initially, the government tried to blame ETA. One of the government’s concerns was whether the attacks had been committed by an Islamic terrorist organization, which could be perceived by voters as a retaliation for Spanish involvement in the Iraq war, a hugely controversial topic at the time. To avoid further stoking this controversy, the government delayed official statements on who was responsible.

The government’s handling of the three days after the terrorist attacks likely caused the Popular Party to lose the general election on March 14, 2004, 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 post-attack voting (Spanish residents) from this election and prior ones. [Garcia-Montalvo \(2011\)](#) concludes that the attacks ultimately changed the outcome of the election and unexpectedly gave power to José Luis Rodríguez Zapatero. The Socialist Party obtained 42.6 percent of the popular vote, while the Popular Party had only 37.7 percent, in sharp contrast to the forecast of just a few weeks earlier.<sup>23</sup>

Among the first laws that President Zapatero put in place was the legalization of a large number of undocumented immigrants. By December 2004, Zapatero had managed to pass new immigration guidelines that resulted in around 600,000 immigrants already in Spain obtaining legal status.<sup>24</sup> Thus, completely unexpected a few months earlier, a significant share of the Spanish immigrant population saw an extremely important change in their labor-market conditions. By gaining legal status, over the course of a few months a large number of undocumented immigrants gained a working status very similar to that of natives. On the following pages, we document this policy change in more detail.

## 2.1 Policy change

Less than a year after the election that brought Zapatero to power in March 2004, the Spanish government allowed a large number of immigrants, who were already in Spain and most of whom were working, to obtain a work permit. This policy became effective in February 2005 and had a huge impact on the share of migrants registered in the social security system.

The stated goal of the policy was “on the one hand, to speed up the [work] authorizations based on

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<sup>22</sup>See [Abadie and Gardeazabal \(2003\)](#) for an analysis on the economic consequences of ETA terrorism.

<sup>23</sup>For more details on voting intentions one week before election day, see [Garcia-Montalvo \(2011\)](#).

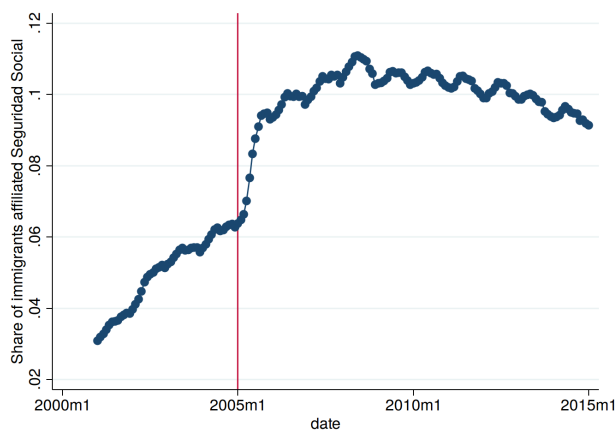
<sup>24</sup>Real Decreto 2393/2004.



vacancies for which employers do not find resident workers, and, on the other hand, to increase the control over the concession of these authorizations.”<sup>25</sup> The policy recognizes the “high number of foreign-born workers lacking a work permit” and offered a period of three 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 Registry of Population at least six months prior to February 7, 2005,<sup>26</sup> and 2) the employer needed to show that it wanted the worker by offering a legal working contract for at least six months.<sup>27</sup> In addition, the government announced that, by May 8, 2005, its policy would be inflexible with those firms employing undocumented immigrants. In fact, the number of work inspections related to foreign workers more than doubled between 2004 and 2005.<sup>28</sup>

When the policy went into effect, large numbers of immigrants took the opportunity to gain legal status. The simplest way to show this is to plot the share of immigrants among the total population registered in the social security system. This is shown in Figure 1. More specifically, Figure 1 shows how the share of immigrants in the social security system moved from around 6 percent to around 9 percent in the course of the period of legalization. This is a significant change and is the result of almost 600,000 immigrants throughout the entire country gaining work permits.<sup>29</sup>

Figure 1: **Social Security Registration and Immigration Reform**



NOTE: This figure shows the (monthly) share of immigrants registered in the social security system. Source: Ministry of Labor and Social Security.

As in many other countries, there is a lot of heterogeneity in relation to where immigrants cluster. On

<sup>25</sup>Real Decreto 2393/2004. In Spanish: “*Por un lado, agilizar las autorizaciones basadas en vacantes para las que los empresarios no encuentran trabajadores residentes, y, por otro lado, aumentar el control en la concesión de dichas autorizaciones.*”

<sup>26</sup>This criterion was subsequently relaxed, accepting registration by default (*empadronamiento por omisión*) upon presentation of any official document proving that the immigrant had been in Spain in August of 2004.

<sup>27</sup>There were some exceptions for the agricultural, construction, hotel and restaurant, and domestic service sectors, as well as for part-time workers. One of the main objectives was to grant work/residence permits to those irregular immigrants with real connections to the Spanish labor market. In order to ensure this, and unlike what happened in previous regularizations, in the Zapatero reform, the employers, and not foreign workers who are irregular, should be those who submit the request for authorization and the job contract that will link them with the foreigner that the regularization is intended for.

<sup>28</sup>According to the statistics of the Ministry of Labor and social security, the number of work inspections related to foreign workers increased by 132 percent between 2004 and 2005, from a baseline of 30,000 per year.

<sup>29</sup>In fact, there were 691,655 applications to the amnesty program, of which 578,375 (83.6 percent) were approved.



the one hand, immigrants concentrate in coastal provinces with high levels of tourism and European retirees. This is the case, for example, in Alicante, the Balearic Islands, Girona, Tenerife, and Málaga. All these provinces had immigrant shares above 8.5 percent in 2002. Immigrants also concentrate in large cities, as happens in other countries (Albert and Monras, 2017). In 2002, for example, Madrid and Barcelona had immigrant shares of 9.2 and 6.8 percent respectively, numbers that have risen further in recent years. On the other hand, 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. Actual numbers can be observed in Table 1.

Table 1: **Immigrant Shares across Selected Spanish Provinces**

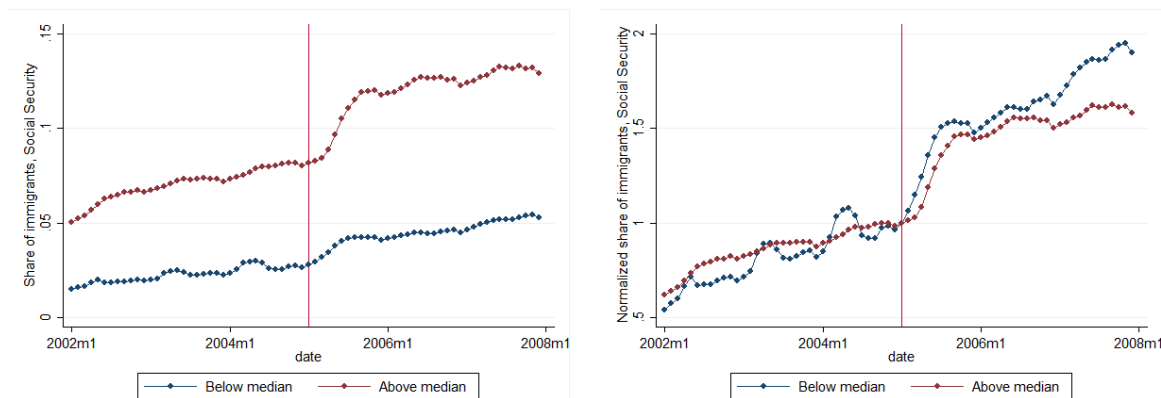
Province name	Immigrant share	Population	Rank
Alicante	0.135	1595.2	1
Balears. Illes	0.121	932.2	2
Girona	0.101	608.9	3
Madrid	0.092	5623.0	4
Tenerife	0.090	904.0	5
Málaga	0.086	1352.5	6
Almería	0.086	555.9	7
Palmas. Las	0.082	965.3	8
Murcia	0.079	1248.1	9
Castellón	0.073	509.7	10
Barcelona	0.068	4979.4	11
Tarragona	0.067	642.7	12
Ávila	0.018	165.3	39
Salamanca	0.017	347.7	40
Asturias	0.016	1074.7	41
Cádiz	0.015	1148.3	42
Coruña. A	0.014	1116.4	43
Lugo	0.013	361.1	44
Sevilla	0.012	1770.8	45
Palencia	0.011	175.6	46
Badajoz	0.010	663.0	47
Jaén	0.009	649.5	48
Zamora	0.009	200.2	49
Córdoba	0.009	773.5	50
National average	0.042	42,133.1	–

NOTE: This table shows the top and bottom dozen provinces out of the 50 total Spanish provinces by immigrant share in mid-2002. Population is measured in thousands. Immigrants are defined as foreign-born individuals. Source: Authors' elaboration based on Municipal Register.

Thus, the legalization of around 600,000 immigrants likely had heterogeneous effects across space. A simple way to view this spatial heterogeneity is to divide Spanish provinces by their median level of migration in 2002. This separates provinces into two groups: the first group (below the median), comprises those provinces that had fewer immigrants as a share of total population than that of the median province; the second group comprises provinces above that median. On the following pages, we show two types of graphs:

the first graph presents the raw data, which we show for provinces above and below the median; the second graph shows the raw data normalized by the value of the outcome variable in the period immediately before the policy change. These graphs allow us to visualize both the total and the proportional impact that the policy change potentially had across locations as a function of initial immigrant shares.

Figure 2: **Social Security Registration and Immigration Reform**



NOTE: The figure on the left shows the (monthly) share of immigrants registered in the social security system in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2005m1). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

Figure 2 depicts these two graphs for the share of immigrants registered in the social security system. The graph on the left in Figure 2 shows that, in high-immigration provinces, the share of foreign-born individuals registered in the social security system increased from around 7 percent to more than 10 percent in just three months. This is an extremely large increase, occurring in an extremely short period of time, which came from a policy change that was very unexpected. This represents an exceptional opportunity to evaluate the consequences of this immigration reform. As can be seen in the graph on the left of the figure, this policy change disproportionately affected initially high-immigration locations in Spain.

The graph on the left of Figure 2 also shows that the policy change affected low-immigration provinces too, albeit with less intensity. The share of immigrants registered in the social security system moved from around 3 percent to around 4 percent over the same period. The graph on the right in Figure 2 shows that, in fact, the effect of the policy was similar across locations in proportional terms. When we normalize the share of immigrants registered in the social security system to a value of 1 in the period right before the policy change (i.e., January 2005), we observe that the trends in high- and low-immigration provinces are similar, as is the dramatic rise in values observed both above and below the median. In both cases the share of immigrants registered in the social security system increased by around 50 percent.

These patterns suggest that the best way to analyze this policy change is through a continuous difference-in-difference estimator, where the intensity of the treatment is measured by the change in the share of immigrants affiliated with social security. This assumes that the legalization of undocumented immigrants affected all provinces in Spain but that it affected some more than others. We can rely on this variation,

and on the unexpectedness of the reform, to evaluate the consequences that legalizing immigrants had on the economy. We proceed to this goal in the following section. We conclude this section by describing in more detail the data used to generate Figures 1 and 2 and the data that we use throughout the paper.

## 2.2 Data

We combine a number of different data sets, from several sources, to explore the consequences that the 2005 Spanish legalization of immigrants had on payroll-tax collection and also on different labor-market outcomes such as employment, wages, and internal migration. Our unit of analysis is the province; in fact, we consider 50 Spanish provinces, excluding two Spanish enclaves in Africa (Ceuta and Melilla).

### 2.2.1 Social security data

We use two different data sets from the Ministry of Labor and Social Security: statistics of registration in the social security system and payroll-tax collection, both at province level. These data sets cover the period from 2000 to 2016. In the case of registration the frequency is monthly, while for payroll-tax revenues the frequency is annual. The number of individuals registered in the social security system is available for all contract types of social security, for natives and foreigners, on a monthly basis. Total payroll-tax collection statistics include contributions to different contract types existing in the Spanish social security system, contributions to unemployment insurance, and contributions to workers' accident insurance, on an annual basis. This detailed data set allows us to identify the effect of the policy change on payroll-tax revenues.

### 2.2.2 Employment and population data

Our main data set on employment and population is the Spanish Labor Force Survey (SLFS, or *Encuesta de la Población Activa* in Spanish). The SLFS is conducted, every quarter, by the Spanish National Institute of Statistics with a sample of some 65,000 households (about 180,000 individuals) and is designed to be representative of the Spanish population. The main goal of the survey is to reveal the characteristics of that population with regard to the labor market. Therefore, we use the SLFS for the period from 2002 to 2007. We focus our analysis on population aged 25 to 65.

We also use the SLFS to construct the provincial share of immigrants each quarter. In addition, and as a cross-check, we compute the same population shares using the Municipal Register of Population. We focus our analysis on the SLFS results for two reasons: 1) the SLFS allows us to compute these shares by skill level, and 2) the data are available at a higher frequency—quarterly instead of yearly. Results using the Municipal Register were deferred to Appendix A.2.

### 2.2.3 Wage data

We use Spain's Continuous Sample of Employment Histories (MCVL, for *Muestra Continua de Vidas Laborales*) to compute wages. This is a microlevel administrative data set obtained by matching social security,

income tax, and census records. It is a representative sample of the population that, in a given year, has any relationship with Spain’s social security system (individuals who are working, receiving unemployment benefits, or receiving a pension). The MCVL represents a 4 percent nonstratified random sample of this reference population, consisting of nearly 1.1 million individuals each year, and covers the period 2004–2015, with retrospective information going back further in time. The MCVL has longitudinal information. Individuals who are present in one wave of the MCVL, and remain registered in the system, continue in the sample for the next wave. Also, new individuals are added to the sample each year to ensure that it remains representative of the population. In particular, we use this data set with the objective of estimating the unit price of labor. We consider natives and foreign-born male workers, aged between 25 and 47 years old, who were employed at any point in our period of analysis (January 2002 to December 2007). In this analysis, we follow the sample of individuals constructed in [de la Roca and Puga \(2017\)](#), but we also include immigrant workers and extend our period of analysis to include 2002.

Altogether, our sample includes 216,873 workers. Natives compose the majority of the sample (174,851 natives and 42,022 foreign-born individuals). This sample has 10,009,971 monthly observations (8,602,570 natives and 1,407,401 foreign-born individuals). This means that, on average, each native is observed over a period of 49.2 months and each foreign-born individual is observed, on average, over a period of 33.5 months.

### 3 Empirical Evidence

In this section we estimate the effect of the policy change on public finances and on labor-market outcomes. This represents two ways of looking at the same issue, as can be seen through the following equation, which decomposes the change in total payroll-tax revenues per newly legalized immigrant using changes in the labor market:

$$\frac{\Delta \text{Total Payroll-Tax Revenue}_c}{\Delta \text{Documented Immigrant}_c} \approx \tau w_{c,imm} + \sum_{i,s} \tau \left( \frac{\Delta L_{isc}}{\Delta DI_c} \frac{E_{isc}}{L_{isc}} w_{isc} + \frac{\Delta E_{isc}}{\Delta DI_c} w_{isc} + E_{isc} \frac{\Delta w_{isc}}{\Delta DI_c} \right) \quad (1)$$

This decomposition shows that the change in total payroll-tax revenues in a location, denoted by  $c$ , (left-hand side of the equation) can come from either the incorporation of the immigrants into the documented labor force and their payroll-tax contributions ( $\tau w_{c,imm}$ )<sup>30</sup> or the indirect effect that this may have on internal migration ( $\Delta L_{isc}$ ), employment ( $\Delta E_{isc}$ ), and the effect on wages ( $\Delta w_{isc}$ ), potentially of different groups of workers, indexed by  $i$  (immigrants versus natives) and  $s$  (different skill groups). As we emphasize later, in the absence of selection on the labor market, this is an exact decomposition. Thus, differences in estimates of the left- and right-hand sides can be attributed to selection in the labor market.

Equation 1 guides our empirical analysis. We first estimate the left-hand side—i.e., how the policy change

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<sup>30</sup>Where  $\tau$  is the payroll-tax rate and  $w_{c,imm}$  represents the average wage of new legalized immigrants.

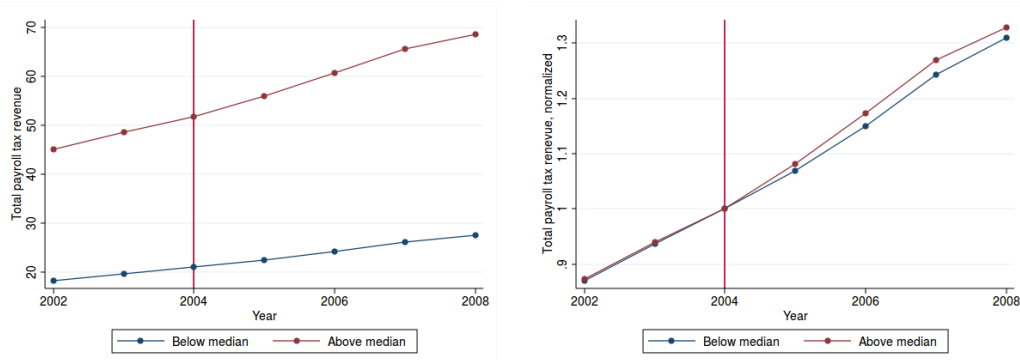
affected local public finances. We then turn to estimating each of the components of the labor market. The two estimates do not coincide, as we show in Section 4: we explain that selection is responsible for this discrepancy, and we propose ways to take it into account.

### 3.1 Public finances

One of the most immediate consequences of the reform was that undocumented immigrants started to pay payroll taxes. Thus, it is worth starting our analysis by looking at the effect that the reform had on public revenues. To do this, we first plot the level and proportional changes in total payroll-tax revenues.

Figure 3 shows that total payroll taxes in Spain generated around 70 billion euros in 2004. Provinces with high levels of immigration tend to be larger. Thus, the split between below- and above-median levels of immigration results in high-immigration provinces generating around 50 billion euros in 2004. The trend in total payroll-tax revenues was positive in the early 2000s. This was mostly a consequence of the high participation rates and low unemployment rates typical of a booming economy. In the graph on the left of Figure 3, we see that there is a small break in the trend in 2005 that coincides with the policy change. The break in the trend is in fact more pronounced in high- than in low-immigration provinces.

Figure 3: Payroll Taxes and Immigration Reform

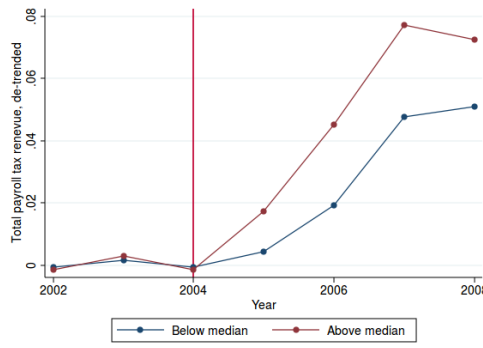


NOTE: The figure on the left shows the payroll-tax revenue in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2004). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

The graph on the right in Figure 3 normalizes the level of tax revenues to the year 2004. It is clear from the graph that, from 2005 on, the increase in total payroll-tax revenues is larger in high-immigration provinces. To help us clarify the magnitude of the change, it is useful to remove the linear trend leading to the policy change from the graph on the left. When we do so, we obtain Figure 4.

Figure 4 allows us to understand the effect on total payroll-tax collection of legalizing immigrants. We observe that, relative to trend, the reform increased by almost 2 percent of total revenue in high-immigrant provinces and by almost 1 percent in low-immigrant locations from 2004 to 2005. The increase continued in the following years. To see whether these are large or small changes in total revenue, it is worth comparing them to the change in the share of workers who registered in the social security system as a result of the

**Figure 4: Payroll Taxes and Immigration Reform, De-trended**



NOTE: This figure shows the de-trended series of total payroll-tax revenues. The vertical red line indicates the last period before the reform (2004). Source: Own elaboration based on Ministry of Labor and Social Security data.

reform. Figure 1 shows that the policy change increased the number of immigrants as a share of total population registered in the social security system by more 3 percentage points. Thus, every immigrant that obtained a work permit contributed around half as much as the existing population. This is not surprising, since immigrants in Spain tend to be less skilled than natives, and those that are within the same skill levels tend to earn less. Moreover, the reform may have impacted the labor market directly, affecting tax collected from different groups of workers. We investigate this further in Section 3.2.

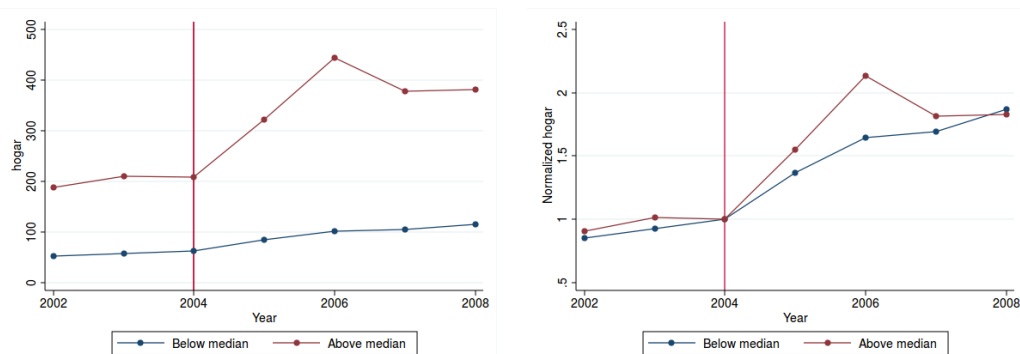
To gain further confidence that, indeed, these changes in total payroll-tax revenues are a consequence of the policy reform, it is worth zooming in on particular items of total payroll-tax collection. Spain has different regimes for different types of workers. Most workers pay in the general regime, but there are also a number of special regimes. One that is used particularly by the immigrant population (given the occupational distribution of immigrants relative to natives) is the housekeeping regime, which corresponds to housekeeping services.<sup>31</sup> We can use this contract regime to show that the change is indeed, in this case, more pronounced than in contract types used less by immigrants.

Figure 5 shows that payroll-tax revenues from housekeeping services increased by 50 percent in 2005 and by almost 100 percent in 2006 in high-immigrant provinces, while the increases were 40 and 50 percent, respectively, in low-immigrant locations. This is a remarkable increase, which is in line with the heavy presence of immigrants in this social security category.

We use the insights from Figures 3 and 4 to quantify immigrants' contributions to total payroll-tax revenue. First, we estimate the euro increase per regularized immigrant that followed the reform by directly translating the figures into regressions. In the regressions we use variation not just from high- relative to low-immigrant locations, but instead variation from the 50 Spanish provinces. In the regressions, the pre-period is 2002 to 2004 (both included) and the post-period is 2005 to 2007 (also both included). As in Figure 4, we first remove linear specific trends at the province level and estimate the effect of the policy as deviations from

<sup>31</sup>According to the statistics of the Ministry of Labor and Social Security, the share of workers affiliated with the “*régimen del hogar*,” or housekeeping regime, in 2004 was 0.7 for natives and 7.1 percent for immigrants.

Figure 5: Payroll Taxes and Immigration Reform, Selected Items



NOTE: The figure on the left shows the payroll-tax revenue from the housekeeping regime, in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2004). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

these trends. More generally, Figures 3 and 4 delineate our main identification strategy used throughout in the paper. In Appendix A, we show that our results are mostly unchanged by adding additional controls, removing the 4 biggest provinces from the analysis, or using migrant-network instruments.

Thus, to quantify the contribution per newly regularized immigrant, we can regress the de-trended series of payroll-tax revenues per capita on the de-trended series of immigrants registered in the social security system per person. The difference between the pre- and the post-periods will be the average contribution of each newly regularized immigrant across the 50 Spanish provinces. These results are shown in Table 2. In Table A.2 of Appendix A we show, using slightly different specifications, the robustness of these results.<sup>32</sup> In total, each newly regularized immigrant increased payroll-tax revenues by around 4,189 euros. This increase comes from the increase in payroll-tax revenues from the general regime, housekeeping regime, and agrarian regime. It suggests that the policy was effective in one of its main goals; i.e., it helped to raise tax revenues at the local level.

Table 2: Estimates of the Change in Payroll-Tax Revenues per Newly Legalized Immigrant

	General Reg.	Self. emp.	Agricult.	Sea	Coal	Housekeeping	Accident	Unemp.	Total
$\Delta$ Immigrants in social security/pop.	3,983*** (1,348)	65.7 (43.05)	146.4*** (50.92)	-11.4 (18.91)	46.4 (38.93)	233.8*** (75.00)	-44.2 (28.37)	-230.7 (456.0)	4,189*** (1,051)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.411	0.032	0.276	0.012	0.019	0.519	0.053	0.018	0.515

NOTE: This table estimates the contribution per regularized immigrant in each regime of social security in euros. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors are reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

To know whether increases of 4,189 euros are large or small, we run the following regression:

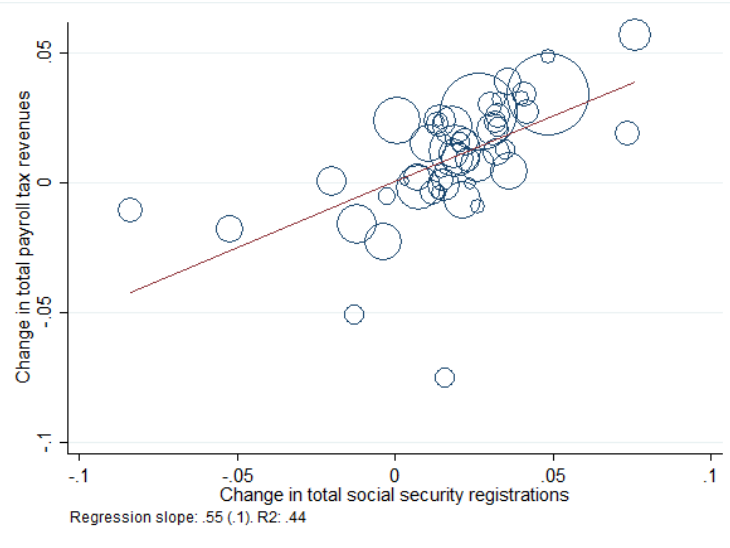
<sup>32</sup>More concretely we show that our results are almost unchanged when we 1) use an alternative sample of provinces (excluding the four biggest provinces in Spain); 2) include additional controls (political alignment, coastal dummies and the share of construction sector pre-reform) and, 3) use 2SLS relying on the past shares of migrants at province level to predict the actual share of immigrants affiliated with social security.



$$\Delta \ln \text{Total Payroll-tax Revenue}_c = \alpha + \beta \Delta \ln \text{Total affiliates to social security}_c + \varepsilon_c$$

where  $c$  indicates provinces and  $\beta$  estimates how much newly regularized immigrants contributed to total payroll-tax revenues. An estimate of  $\beta = 1$  means that newly regularized immigrants contributed as much as previous immigrants and natives. An estimate of less than 1 means that they contributed relatively less. This may be because newly legalized immigrants' wages were lower or because the regularization also affected the labor market (or a combination of both). We investigate this in detail in Section 3.2.

Figure 6: **Payroll-Tax Revenues and Social Security Registration**



NOTE: This figure plots the de-trended change in total payroll-tax revenues against the de-trended change in total registration in the social security system between the periods 2002 to 2004 and 2005 to 2007. The size of the dots represents the population size of each province. Source: Authors' elaboration based on Ministry of Labor and Social Security data.

The estimate that we obtain is 0.55(0.1) (with an  $R$ -squared value of 0.44), as shown in Figure 6. This means that for a 10 percent increase in the number of workers registered in the social security system as a result of the regularization process, total payroll-tax revenues increased by only 5.5 percent. A priori, it is not clear whether this brought additional net revenues to the government or not, since it depends on government expenditures. However, the largest government expenditures are in health care and education. Both public services were already available to undocumented immigrants so, in this particular case, public expenditure did not increase. In other contexts, this should be taken into account to evaluate the complete effect of the policy.

Thus, from Table 2 and Figure 6 we learn that the policy helped increase tax revenues, but less than we might have expected from previous payers to the social security system. This suggests that it may be particularly important to also investigate whether the policy had some unintended consequences on labor-market outcomes. We turn to this in the next section.

## 3.2 Labor market outcomes

In this section we estimate the effect of the policy on labor market outcomes. We split the section into employment, wages, and internal migration. These are the three components of the right-hand side of Equation (1).

### 3.2.1 Employment

We begin our exploration of the consequences that the legalization 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 that is actually working. We also differentiate between natives and immigrants and between different skill levels, as the reform might have affected each group differently.<sup>33</sup>

As before, the first step toward understanding whether the reform had an impact on employment rates is to differentiate between the provinces with high and low levels of immigration. Figure 7 shows these series for natives (Panel A) and for immigrants (Panel B). In the graph on the left of the figure, we observe how high-immigration provinces in Spain are also characterized by high levels of native employment. The difference is substantial, at around 10 percentage points, and it reflects the fact that the high-employment provinces of Madrid and Barcelona are among the high-immigration areas as well.

The graphs on the right of Figure 7 show the same series but normalizing the employment rate to 1 just before the immigration reform took place. It is apparent from the figure that, while the trends leading to the policy change were very similar between high- and low-immigration provinces, the two start to diverge in the first quarter of 2005 (especially visible in Panel A). Thus, it seems that the legalization of immigrants decreased the employment rates of natives. It is worth noting that the gap in employment rates widens gradually, perhaps showing that it took some time for immigrants to fully be able to compete with natives in the labor market. This is very much in line with the nature of the policy change. The policy legalized the status of immigrants who were presumably already working, given that they had to have a work contract for the six months following the reform. After this period they had similar work status to natives, and could, thus, find new jobs if they wished.

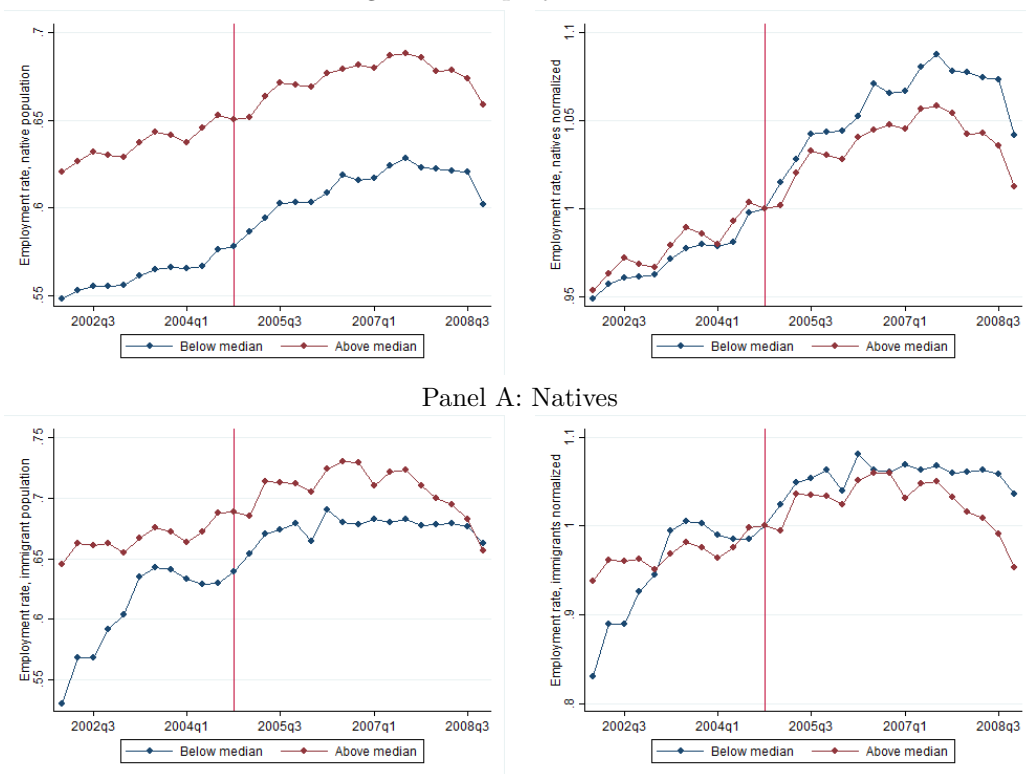
The graph on the right of Figure 7, Panel B, shows the normalized series for immigrants. This series is supposed to include both documented and undocumented immigrants. Thus, we *should not* expect to see an effect of the policy on employment rates caused by the mere fact that undocumented immigrants gained work permits. The series for immigrants seems to be a bit noisier than for natives, which is not surprising, given the smaller number of observations. If anything, it seems that employment rates of immigrants in high-immigration provinces declined slightly relative to low-immigration provinces.

We quantify the results shown in Figure 7 in Table 3. To do so, we compute the effect of the reform on total employment and then quantify how much of the effect on total employment comes from each of

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<sup>33</sup>We define high-skilled workers as workers having at least a university diploma, while we define low-skilled workers as having less than a university diploma.

Figure 7: Employment Rate



Panel B: Immigrants

NOTE: The figure on the left shows the employment rate in Spanish provinces above and below the median level of immigration in 2002. The vertical red line indicates the last period before the reform (2004q4). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: SLFS.

the different groups of workers. In this exercise, we take into account potentially different trajectories of the different provinces in Spain, which means that our estimates reflect the change occurring with the immigration reform relative to province-specific linear time trends. More specifically, we first run

$$\frac{\text{Emp}_{ct}}{\text{Pop}_{ct}} = \alpha + \delta_c + \delta_t + \delta_c * t + \varepsilon_{ct}^1 \quad (2)$$

where  $c$  indicates province,  $t$  indicates quarter, and  $\delta$  indicates fixed effects. This regression removes province-specific linear time trends (denoted by  $\delta_c * t$ ) as well as time and province fixed effects. This accounts for the potentially different levels of employment across provinces that are unrelated to immigration and removes potentially diverging secular trends on the evolution of employment across provinces. We apply this regression to the pre-immigration reform period exclusively, since trends after the reform are likely to be contaminated by the reform itself. We use these residuals to compute the level of employment rates before and after the immigration reform.

The second step is to do the same exercise of removing pre-reform trends for those registered in the social security system. In particular, we run the following regression:

$$\frac{\text{Imm Soc Sec}_{ct}}{\text{Pop}_{ct}} = \alpha + \delta_c + \delta_t + \delta_c * t + \varepsilon_{ct}^2 \quad (3)$$

Once we have removed province-specific trends from these series, we take the difference between the employment rates and immigrants registered in the social security system before and after the reform, and we run the following regression:

$$\Delta \frac{\widehat{\text{Emp}}}{\text{Pop}_c} = \alpha + \beta \Delta \frac{\widehat{\text{Imm Soc Sec}}}{\text{Pop}_c} + \varepsilon_c^3 \quad (4)$$

This regression isolates the variation of interest. In this setting,  $\beta$  measures the effect of the newly legalized immigrants on total employment. It has a simple interpretation: if  $\beta = -1$ , it means that each newly legalized worker prevents one other resident from working. To investigate the effect on particular groups of workers, we can simply substitute  $\text{Emp}_{ct}$  for the number of employed workers in the group of interest. Again, we consider 2002 to 2004 as the preperiod and 2005 to 2007 as the postperiod. In Appendix A we show that this strategy is robust to a number of possible concerns.<sup>34</sup>

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

	$\Delta$ Total Emp.	$\Delta$ Employment					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
$\Delta$ Immigrants in social security/pop.	-0.544*** (0.175)	-0.382 (0.252)	-0.163 (0.162)	-0.467** (0.224)	0.085 (0.246)	-0.339** (0.164)	0.176* (0.0937)
Observations	50	50	50	50	50	50	50
R-squared	0.087	0.034	0.010	0.077	0.003	0.059	0.062

NOTE: This table estimates the effect of immigrant regularization on employment. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

The results in Table 3 are clear. We see that employment rates dropped as a consequence of the immigration reform. For each newly regularized immigrant who started to pay taxes, 0.54 workers lost their jobs and thus stopped paying payroll taxes. This estimate represents the average effect over the two and a half years following the reform. As can be seen in Figure 7, the effects on employment are smaller during the first six months after the policy change. As mentioned earlier in the introduction, to get a sense of the magnitude it is worth mentioning that, for every undocumented immigrant, labor costs to the firm increased by slightly more than 30 percent, due to payroll taxes. Given that legalized immigrants represented around 4 percent of low-skilled workers, this means that total low-skilled labor costs increased by around 1.4 percent (given a 35 percent increase in labor costs due to payroll taxes). Similarly, for every newly legalized immigrant around 0.5 low-skilled workers lost their jobs. Legalized migrants are around 4 percent of all low-skilled workers, so in total around 1.8 percent of low-skilled workers lost their job. Thus, this suggests that the short-run

<sup>34</sup>Table A.3 presents estimates of this relationship for: 1) an alternative sample of provinces (excluding four main provinces); 2) including additional controls (political alignment, coastal dummies and the share of construction sector pre-reform) and, 3) for a 2SLS estimation, using the past shares of migrants at province level to predict the actual share of immigrants affiliated with social security.

labor demand elasticity is around 1.3 or that the inverse demand elasticity is around 0.8. These numbers are in-line with a number of estimates in the literature on minimum wages, immigration, and labor markets in general using US data.<sup>35</sup> See for example [Monras \(2015c\)](#), [Neumark \(2017\)](#), [Borjas \(2003\)](#), [Acemoglu, Autor and Lyle \(2004\)](#), or [Card and Lemieux \(2001\)](#).

When we look at the split by skill groups and place of birth, we see that low-skilled workers seem to lose while high-skilled workers seem to gain in terms of employment. For each newly legalized immigrant, 0.47 low-skilled natives and 0.34 low-skilled immigrants lose their jobs, while 0.09 high-skilled natives and 0.18 high-skilled immigrants gain jobs.<sup>36</sup> This shows that, while total employment decreased by around 0.5 workers, the policy change had important distributional consequences in terms of employment opportunities. Furthermore, in [Appendix Table A.1](#) we show that employment losses of low-skilled natives are stronger in sectors with high concentrations of immigrant workers. This is consistent with the idea that the policy created some competition between natives and immigrants, even within sectors, and arguably gives further credibility to our estimates.

These results are consistent with two ideas.<sup>37</sup> First, low-skilled immigrants and natives probably became closer substitutes with the policy change. This captures the idea that workers lacking a work permit cannot always access all types of jobs, which, in turn, may make workers of similar education levels imperfect substitutes (see [Ottaviano and Peri 2012](#); [Manacorda, Manning and Wadsworth, 2012](#)). Once immigrant workers obtained work permits, they likely became much closer substitutes to native workers of the same skill level, while still being relatively cheaper. This explains why employment rates of both natives and immigrants moved in the same direction in response to the policy change. Second, the policy made low-skilled workers relatively more expensive. A large number of low-skilled workers who were working illegally, and for whom firms were not paying payroll taxes, became legal workers. This increased the cost of hiring these types of workers by around 30 percent—i.e., the size of payroll taxes—and thus likely pushed firms into hiring more high-skilled workers, which explains the positive effects of the policy on high-skilled employment. It is worth keeping in mind the important monitoring efforts of the Spanish authorities at the time. As mentioned before, work inspections increased by 132 percent, which made sure that the policy was put into effect. This is important, since firms might have preferred to keep hiring workers illegally, but this option became more costly with the policy change.

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<sup>35</sup>Note that labor demand elasticities could vary considerably across countries. [Dustmann, Schonberg and Stuhler \(2017\)](#) show that employment effects are larger than wage effects in Germany, using an immigrant supply shock. See also the review of the immigration literature provided in [Dustmann, Schonberg and Stuhler \(2016\)](#).

<sup>36</sup>Both documented and undocumented immigrants are in the SLFS both before and after the legalization. While the Spanish National Statistics Office acknowledges that the response rate may be lower among undocumented immigrants than among documented ones, it is unlikely that this drives the employment results of low-skilled immigrants. First, the rotating nature of the survey means that few workers change in the sample right before the policy change and right after it. While clearly smaller, employment effects are perceptible right after the policy change. Second, newly legalized immigrants were required to have a work contract for at least 6 months. Thus, undocumented immigrants who were included in the legalization had higher employment rates than already legalized immigrants and a contract with an employer who was ready to do the required paper work.

<sup>37</sup>These ideas could be expressed with a spatial equilibrium model with many local labor markets and a common local production function with the following features: four factors of production (high- and low-skilled natives and immigrants) where immigrants and natives become closer substitutes as a function of the work status. This model would simply be a combination of elements in [Peri and Sparber \(2009\)](#) with standard spatial equilibrium models a la [Rosen \(1974\)-Roback \(1982\)](#).

### 3.2.2 Wages

Another labor-market outcome that might have changed as a consequence of the reform is wage levels. In this case, we are interested in knowing whether the reform changed the unit price of labor. Later, we investigate whether workers who lost or gained jobs were selected in particular ways. Wage changes can obviously help to explain why the contribution of newly legalized immigrants was proportionally less than we would have expected from the change in social security registration.

We use “composition adjusted wages” to measure the unit price of labor.<sup>38</sup> Concretely, we use a Mincerian regression allowing for specific returns across skills (low- and high-skilled) and allowing for 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 that different periods and different provinces may have different wage levels and wages may be evolving differently across provinces, and that the returns to characteristics are potentially different across skill levels. In the baseline results we use wages of working-age males, since there is usually less unexplained variance for this group of workers. In Appendix A we show that nothing depends on including or excluding women.

As before, we start by comparing high- and low-immigration locations and then quantify the results in a regression framework. Figure 8 shows the composition-adjusted wage series for both natives and immigrants. It is clear that, after the reform, wages in high-immigration locations seem to increase relative to low-immigration locations. Wages were increasing during this period, but the increases were less pronounced from 2005 to 2008 than previous linear trends predicted; hence the negative numbers in the series. This is apparent for native workers; differences are minimal in the case of immigrants.<sup>39</sup>

We quantify the insights of Figure 8 in Table 4, and we provide a number of robustness checks in Appendix A.<sup>40</sup> Table 4 shows that native workers’ wages increased following the policy change. 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. Given that employment effects are small for high-skilled individuals, whereas they are large for low-skilled individuals, it is very likely that most of the wage results for high-skilled individuals reflect changes in the price of labor, while changes for low-skilled individuals reflect selection. We return to this point in Section 4, see in particular Table 7.

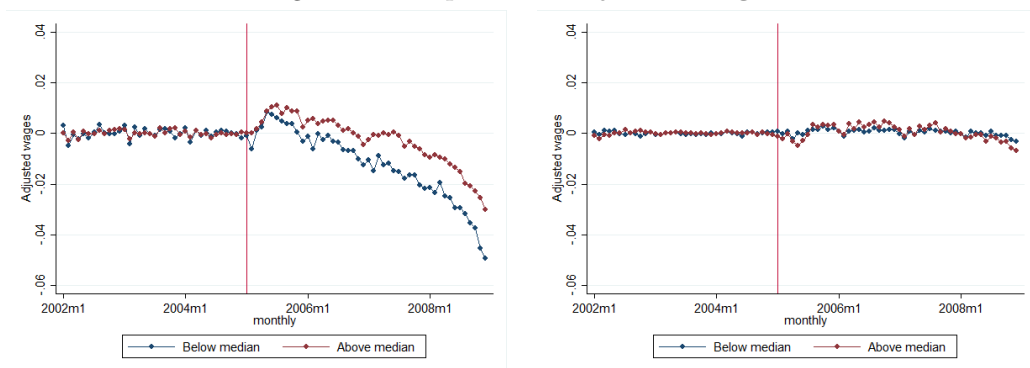
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<sup>38</sup>We consider the tax base of social security contributions divided by days worked each month as a proxy of daily wages. This is considered a “proxy” of wages since this tax base is bounded between a minimum and maximum amount that, in 2005, stood at 598.2 and 2,813.4 euros per month. However, for a large majority of workers these limits are not binding.

<sup>39</sup>Given that we use composition-adjusted wages (i.e., we remove observable characteristics and year and province fixed effects), the series that we show are equivalent to the normalized series shown in Figure 7.

<sup>40</sup>In Table A.4 we present estimates of this relationship: 1) for an alternative sample of provinces (excluding four main provinces); 2) including women in the sample; 3) including additional controls (political alignment, coastal dummies and the share of construction sector pre-reform) and, 4) for a 2SLS estimation, using the past shares of migrants at province level to predict the actual share of immigrants affiliated with social security.

Figure 8: **Composition-Adjusted Wages**



Panel A: Natives

Panel B: Immigrants

NOTE: The figure on the left shows the average composition-adjusted native wage in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2005m1). The figure on the right shows the same series for immigrant workers. Source: Authors' elaboration based on MCVL.

Table 4: **Estimates of the Effect of the Immigration Reform on Wages**

	$\Delta$ Total log wages	$\Delta$ log wages					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
$\Delta$ Immigrants in social security/pop.	0.244** (0.106)	0.310*** (0.113)	-0.052 (0.278)	0.275*** (0.093)	0.428* (0.223)	-0.118 (0.285)	0.998* (0.587)
Observations	50	50	50	50	50	50	50
R-squared	0.143	0.217	0.001	0.191	0.079	0.004	0.023

NOTE: This table estimates the effect of immigrant regularization on *log* composition-adjusted wages. Estimates are based on a continuous difference in difference strategy, where province-specific pre-change linear trends are removed. Robust standard errors reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Wages for high-skilled natives in high-immigration locations increased by 0.43 log points per percentage-point increase in the share of immigrants registered in the social security system, while wages for low-skilled natives increased by 0.28 log points. The results are more mixed for immigrants. The policy seems to have increased the wages of high-skilled immigrants by as much as 1 log point, while the wages of low-skilled immigrants decreased, if anything.

### 3.2.3 Internal migration

As we see from the decomposition of changes in total payroll-tax revenues shown in Equation (1), not only employment rate or wages may change as a result of the immigration reform but also internal migration may have responded. This could have happened as a consequence of natives relocating across Spanish provinces, or immigrants themselves relocating. In fact, immigrants are usually more mobile internally than natives, who may be more attached to the place where they reside (Cadena and Kovak 2016; Monras 2015a).

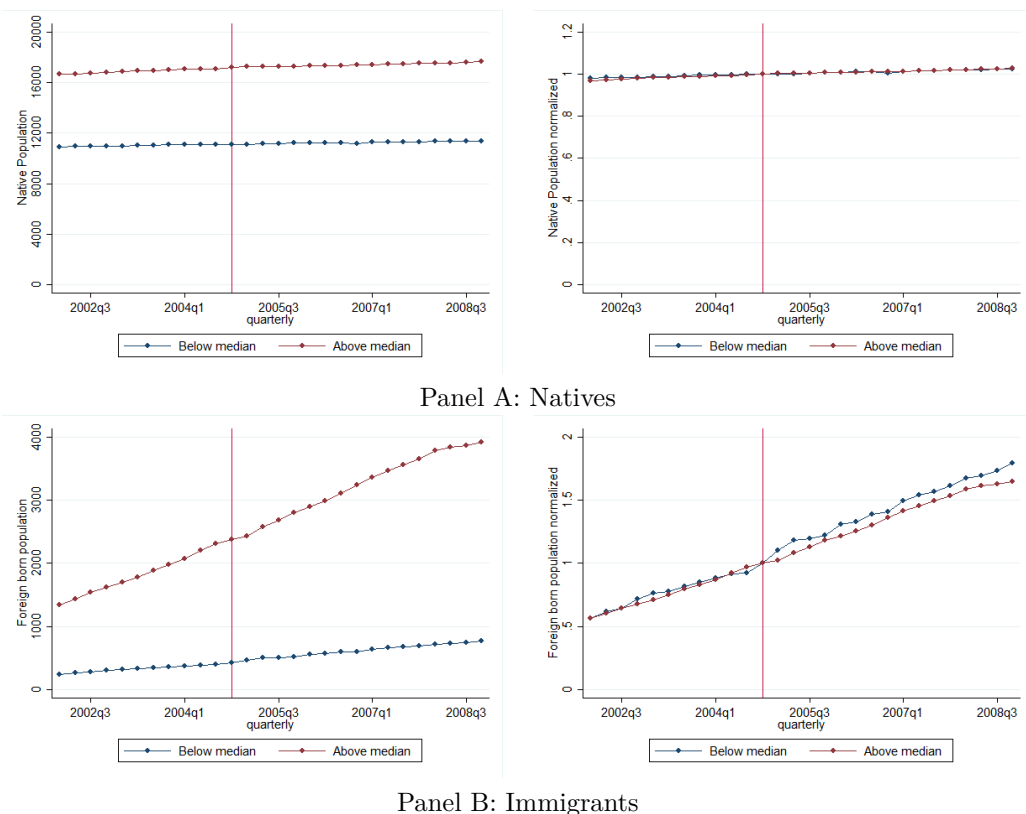
To investigate this, we proceed as in previous sections. We start by splitting the sample of Spanish provinces between high- and low-immigration provinces. We then plot the evolution of total native population and total immigrant population in the two sets of provinces. For this exercise, we use data from SLFS (see section 2.2). We base our main estimates on SLFS data instead of administrative data (Municipal Register)



because we can distinguish skill levels in SLFS data. In Appendix A.2, we show that we obtain the same results using administrative data.

Figure 9 shows these series for natives (Panel A) and for immigrants (Panel B). The graphs of Panel A show that native population did not change significantly around the time of the policy change. When we look at levels and when we normalize the series, we see that native population trends remain unchanged around the time of the policy change. The graphs of Panel B show that it is clear that more immigrants started to move to low-immigration provinces right at the time of the reform. This change is particularly apparent when we normalize the series in the graph on the right of Panel B.

Figure 9: Spanish and Foreign-Born Population and the Immigration Reform



NOTE: The figures on the left show Spanish and foreign-born population in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2004q4). The figures on the right normalize the figures on the left, using the last observation before the policy intervention. SOURCE: SLFS.

The results are clear. After the reform, low-immigration provinces started to gain immigrant population. We quantify this in Table 5, and, as before, we provide estimates using alternative specifications in Appendix A.<sup>41</sup> Specifically, we compute the share of immigrants across provinces and we de-trend the series as we did before. We prefer to use the share of immigrant population instead of levels of the different groups to make

<sup>41</sup>In Table A.5 we show that our results are similar when 1) we use an alternative sample of provinces (excluding four main provinces); 2) we include additional controls (political alignment, coastal dummies and the share of construction sector pre-reform), or 3) we use a 2SLS estimation, relying on the past shares of migrants at province level to predict the actual share of immigrants affiliated with social security.

the various provinces more easily comparable. We then run the following regression:

$$\Delta \frac{\widehat{\text{Imm}}}{\text{Pop}_c} = \alpha + \beta \Delta \frac{\widehat{\text{Imm Soc Sec}}}{\text{Pop}_c} + \varepsilon_c^4 \quad (5)$$

where, again,  $c$  indicates the various Spanish provinces and  $\beta$  estimates how many immigrants moved away from high- to low-immigration provinces for each immigrant that gained legal status following the reform. Preperiods are 2002 to 2004, and postperiods are 2005 to 2007.

Table 5: **Estimates of the Effect of the Immigration Reform on Internal Migration**

	$\Delta$ Immigrant population share			$\Delta$ Share of LS
	Total	Low Skilled	High Skilled	Population
$\Delta$ Immigrants in social security/pop.	-0.359* (0.201)	-0.432** (0.206)	0.073 (0.0862)	-0.370 (0.360)
Observations	50	50	50	50
R-squared	0.029	0.055	0.012	0.042

NOTE: This table estimates the effect of immigrant regularization on the share of foreign-born population. Regressions are weighted by population. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Table 5 shows that for every newly legalized immigrant, 0.36 immigrants left high-immigration provinces. This is due to the outflow of low-skilled immigrants. For every newly legalized immigrant, 0.43 low-skilled immigrants left high-immigration locations, while 0.07 high-skilled immigrants moved in. These estimates are relative to native population and thus implicitly assume that native population did not respond by moving across provinces. The last column shows that when instead of immigrants we use overall low-skilled population we obtain similar point estimates, suggesting that mostly immigrant workers account for internal migration responses. This is not surprising, since after the reform immigrant workers had work permits and were free to move internally, perhaps better profiting from employment opportunities outside traditional migrant networks.

## 4 Discussion

Given the results reported in Section 3, it seems quite clear that newly legalized individuals contributed positively to public finances, and that they changed some labor market outcomes for other workers. In this section, we discuss these results and what they mean for the overall evaluation of the policy change and our understanding of how labor markets work.

To start the discussion, note that we can compare the direct estimates of the effect of the policy change on payroll taxes with the implied increase in payroll taxes that would be suggested by our estimates of the effects of the policy on the labor market. For this, we can use the equation previously introduced:

$$\frac{\Delta \text{Total Payroll-Tax Revenue}_c}{\Delta \text{Documented Immigrant}_c} \approx \underbrace{\tau w_{c,imm}}_{\text{Direct contribution}} + \underbrace{\sum_{i,s} \tau \left( \frac{\Delta L_{isc}}{\Delta DI_c} \frac{E_{isc}}{L_{isc}} w_{isc} + \frac{\Delta E_{isc}}{\Delta DI_c} w_{isc} + E_{isc} \frac{\Delta w_{isc}}{\Delta DI_c} \right)}_{\text{Labor-market effects}} \quad (6)$$

Again, this equation simply says that the change in payroll taxes per newly documented immigrant can be decomposed in two blocks. The first block, which we can label as “direct contribution,” is simply the wage that a newly legalized worker receives,  $w_{c,imm}$ , multiplied by the payroll-tax rate he or she needs to pay,  $\tau$ . A newly legalized immigrant may have an impact on the labor market as well, as documented in Section 3.2. The labor market effect can be decomposed into the effect of newly legalized immigrants on various groups of workers. In particular, we have estimated the impact of the policy on high- and low-skilled natives and immigrants. This is the second block, which we have identified as the labor market effects of the legalization. There are two important points to keep in mind when using this decomposition. First, the decomposition uses average labor market outcomes, so it cannot capture particular selection patterns in employment changes. This explains why it is an approximation and not an exact decomposition. Second, the decomposition is in first differences. Hence, it does not matter whether we used males or all workers in estimating the wage effects, as long as wage changes for them move in parallel as suggested by the results shown in Appendix A.

We summarize all the results in Table 6, and we provide the equivalent results using alternative estimation strategies in Appendix A.4. In the first row, we show the distribution of newly legalized immigrants between high- and low-skilled individuals. For this we assume that for every 10 newly legalized immigrants, 9.3 are low-skilled while 0.7 are high-skilled. This follows the distribution of foreign-born workers, from countries whose migrants are candidates for the legalization, who appear in the MCVL for the first time between February and June of 2005.<sup>42</sup> The second row shows the migration estimates previously reported. As discussed, we estimate that, on average, for each legalized immigrant in a given location, 0.43 low-skilled immigrants leave and 0.07 high-skilled immigrants arrive. The third row displays the results on employment. We found that, for each newly legalized immigrant, 0.47 low-skilled natives and 0.34 low-skilled immigrants lose their jobs, while 0.09 high-skilled natives and 0.18 high-skilled immigrants obtain new jobs. The fourth row displays the results on wages. In this case, natives of all skill levels seemed to gain slightly, while low-skilled immigrant wages decreased by around 0.12 percent and high-skilled immigrant wages increased by around 1 percent.

In order to be able to use Equation (6), we also need some summary statistics on the key variables defining the labor market. We report this in the third block of Table 6. These numbers refer to the averages before the policy change—i.e., during the period 2002 to 2004, with the exception of the average wages of

<sup>42</sup>We cannot directly observe whether someone appearing in the MCVL was an undocumented immigrant, but it is very likely that immigrants appearing in this sample during the legalization period are mostly immigrants who gained work permits thanks to the policy change.

Table 6: **Evaluation of the Immigration Reform, Raw Estimates**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
<b>Assumed distribution of newly legalized immigrants</b>				
$\Delta$ Documented immigrants	0	0	0.93	0.07
<b>Estimates of the labor market effects</b>				
$\Delta$ Migration ( $\beta^{Mig}$ )	0	0	-0.432	0.073
$\Delta$ Employment ( $\beta^{Emp}$ )	-0.467	0.085	-0.339	0.176
$\Delta$ (log) Wages ( $\beta^{wage}$ )	0.275	0.428	-0.118	0.998
<b>Summary statistics</b>				
Employment rates	0.62	0.83	0.69	0.73
Average wages	17,131	23,759	14,082	19,892
Av. wages of new legalized immigrants	–	–	12,893	15,061
Employment distribution	0.46	0.14	0.05	0.02
<b>Estimates on payroll taxes by skill</b>				
Labor Change	-2,113	1,231	-3,283	1,764
Total Change	-2,113	1,231	1,054	2,157
Contribution per skill	-91%	53%	45%	93%
<b>Estimates of the effect on payroll taxes</b>				
Direct estimates payroll taxes			4,189 Euros	
Estimates of total effects, labor market			2,330 Euros	
Difference in estimates			- 1,859 Euros	

NOTE: This table reports the estimates of the effect of the policy on labor market outcomes, as reported in Tables 3, 4, and 5; some summary statistics on employment variables before the policy change, i.e., the years 2002 to 2004, inclusive; estimates on the contribution to changes in payroll-tax revenues by skill group; and total changes in contributions using predictions from labor market changes and direct estimates from payroll-tax revenue data reported in Table 2.

new legalized immigrants that refer to the year 2005. Both men and women are included in computing these averages. In order to use Equation (6) together with the estimates in Tables 3, 4, and 5, it may be useful to rewrite Equation (6) as follows:

$$\frac{\Delta \text{Total Payroll-Tax Revenue}_c}{\Delta \text{Documented Immigrant}_c} \approx \tau_{w_c, imm} + \sum_{i,s} \tau_{w_{isc}} \left( \beta^{Mig} \frac{E_{isc}}{L_{isc}} + \beta^{Emp} + \frac{E_{isc}}{L_c} \beta^{wage} \right) \quad (7)$$

where  $\beta$  indicates the estimates of the various labor market outcomes. Using Equation (7), we can build the fourth block of Table 6. In this block, we first report the contribution to the change in payroll taxes using the estimates of the policy on the labor market. We can compute this for each of the skill groups, using the average employment variables reported in the summary statistics block of Table 6. These computations suggest that, as a result of the labor market effect, low-skilled natives contributed 2,113 fewer euros per newly legalized immigrant. This reflects the negative effects that the legalization had on employment outcomes of low-skilled natives. Similarly, we estimate that the policy change increased high-skilled natives' contributions by 1,231 euros, decreased low-skilled immigrants' contributions by 3,283 euros, and increased high-skilled immigrants' contributions by 1,764 euros. Importantly, these numbers only reflect the effect of the policy on payroll-tax contributions through the effect on the labor market. To obtain the total effect, we need to take into account that the newly legalized immigrants started to contribute to payroll taxes. These computations are displayed in the second row of the fourth block of Table 6. Remarkably, the negative contribution of low-skilled immigrants now becomes positive since we add the direct effect of the policy. This also increases the size of the contribution of high-skilled immigrants. In total, the estimates from the labor-market data

suggest that payroll taxes should have increased by 2,330 euros per newly legalized immigrant at the local level. This contrasts with the direct estimates that we obtained using payroll-tax revenue data. With these data, we obtained that, on average, locations gained 4,189 euros per newly legalized immigrant.

In order to explain this divergence, we need to understand the assumptions behind the decomposition in Equation 7. If the wages of the low-skilled natives who lost their jobs as a consequence of the policy were equal to the average wages of other low-skilled workers then Equation (7) would be an *exact* decomposition and the estimates coming from payroll tax revenue data and the ones implied by labor market changes should coincide. This is unlikely to happen, since it is very likely that there is some type of selection. For example, it is not hard to imagine that those who lost their jobs were earning lower wages than the average low-skilled worker. In other words, it is conceivable that workers who lost their jobs were negatively selected.

We investigate low-skilled native selection patterns more directly in Table 7. In this table, we show the effect of the policy on three different wage measures. First, we show that the policy change did not affect natives who were already working and did not lose their jobs. Given the rigidities in the Spanish labor market, it is not surprising that wages under contract do not adjust significantly to changing conditions. This is shown in the first column of Table 7, and again, we provide alternative specification in Appendix A.<sup>43</sup> In the second column, we show that low-skilled natives who entered the labor market in high-immigrant provinces during 2005, and thus potentially were affected by the policy change, received significantly higher wages than similar natives in the preceding year. This suggests that only the most able among low-skilled workers could enter the labor market during the period of immigrant legalization. In the last column we show that, moreover, those low-skilled natives in high-immigrant locations who lost their jobs tended to earn lower wages than in other years. When comparing the wages of those who lost jobs in 2005 to those who lost jobs in 2004 in high-immigrant locations, we see that those who lost their jobs tended to have lower wages than in the preceding year. This is clear evidence that selection plays a key role in explaining the positive effects that the reform had on average low-skilled native wages.

Table 7: **Native Selection**

	$\Delta$ (ln) wages low skilled natives		
	Always working (1)	Enter 2005 vs. Enter 2004 (2)	Lost job 2005 vs. Lost job 2004 (3)
$\Delta$ Immigrants in social security/pop.	-0.064 (0.102)	0.739** (0.324)	-0.387 (0.426)
Observations	50	50	50
R-squared	0.011	0.052	0.016

NOTE: (1) Wages of those low-skilled natives who were working the entire 2004 and 2005. (2) Wages of those low-skilled natives who entered the labor market between February and December of 2005, against the wages of those who entered during the same period one year before. (3) Average wages (over the last six months) of low-skilled natives who lost their jobs between February and December of 2005, against the average wages of those who lost their jobs in the same period one year before. Robust standard errors reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Table 8 recomputes the contribution of each type of worker to payroll taxes following the legalization

<sup>43</sup>See some robustness checks in Table A.6 in the Appendix.

of immigrants by assuming that low-skilled natives and immigrants who lost their jobs earned around 34 percent less. This is the number that equalizes the estimates using labor market data and payroll-tax revenue data. This obviously changes the estimated contribution of the various groups. In this case, the decrease in payroll-tax contributions of low-skilled natives is estimated at 1,385 euros per newly legalized immigrant. This is the only group for which the policy change clearly reduces the total contributions. All the others contribute more to payroll taxes as a result of the policy, for various reasons. In the case of high-skilled natives and immigrants, this is the positive consequence that the policy had on both their wages and their employment outcomes. In the case of low-skilled immigrants, the increase in contributions comes from the newly legalized immigrants. In this case, the estimate is higher than in Table 6 because we assumed that those low-skilled workers who lost their jobs received lower wages than the average wages of low-skilled immigrants.

Table 8: **Evaluation of the Immigration Reform, Accounting for Selection**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
<b>Estimates on payroll taxes by skill</b>				
Labor Change	-1,385	1,231	-2,152	1,764
Total Change	-1,385	1,231	2,185	2,157
Contribution per skill	-33%	29%	52%	51%
<b>Estimates of the effect on payroll taxes</b>				
Direct estimates payroll taxes			4,189 euros	
Estimates of total effects, labor market			4,189 euros	
Difference in estimates			0 euros	

NOTE: This table reports estimates on three things: 1) the contribution to changes in payroll-tax revenues by skill group, 2) total changes in contributions using predictions from labor market changes under the assumption that low-skilled workers who lost their jobs are negatively selected, and 3) direct estimates from payroll-tax revenue data reported in Table 2. See more details in the main text.

The fact that we are taking migration into account in these computations is both right and misleading. It is right in the sense that migrants who leave a particular location stop contributing in that location, something that needs to be taken into account. But it is also a bit misleading because if these migrants moved to another location within Spain, they still contributed to payroll taxes. If all immigrants who leave high-immigration locations relocate within Spain, then in order to estimate the contribution of the policy change on payroll-tax contributions, we need to shut down the migration channel. We do this in Table 9.

Table 9: **Evaluation of the Immigration Reform, Accounting for Selection and Migration**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
<b>Estimates on payroll taxes by skill</b>				
Labor Change	-1,385	1,231	-1,153	1,377
Total Change	-1,385	1,231	3,184	1,770
Contribution per skill	-29%	26%	66%	37%
<b>Estimates of the effect on payroll taxes</b>				
Direct estimates payroll taxes			4,189 euros	
Estimates of total effects, labor market			4,801 euros	
Difference in estimates			612 euros	

NOTE: This table reports estimates on three things: 1) The contribution to changes in payroll-tax revenues by skill group, 2) total changes in contributions using predictions from labor market changes under the assumption that low-skilled workers who lost their jobs are negatively selected, and 3) direct estimates from payroll-revenue data reported in Table 2. In this table, we assume that all immigrant migration responses were from within Spain. See more details in the main text.

In this case, the contribution of low-skilled immigrants increases to 3,184 and the contribution of high-skilled immigrants decreases to 1,770 euros. This implies that the total change in contribution per legalized immigrant, once we take into account selection and internal migration, increases to 4,801 euros per newly legalized immigrant—i.e., 612 euros more than we obtained by simply looking at payroll-tax revenues. Table 9 highlights the importance of doing the complete exercise as we did. First, it is important to have data on payroll-tax revenues. This allows for a direct estimate of the contribution of newly legalized immigrants to total payroll-tax revenues at the local level. It also allows us to see whether the increase is larger or smaller than we would have anticipated by simply looking at the change in social security registration.

It is equally important to have detailed data on labor market outcomes. The fact that the contribution of newly legalized immigrants was only 55 percent of what we would have anticipated, given the change in the number of workers registered in the social security system, suggests either that newly legalized immigrants earned lower wages than already documented immigrants or that the policy also affected the labor market outcomes of other groups. By having detailed data on high- and low-skilled natives and high- and low-skilled immigrants, we could estimate the effect of the policy on the labor market.

Using the effect of the policy on the labor market, we could then predict the change in payroll-tax revenue, assuming that the workers who lost their jobs were not selected in any particular way. The fact that the direct estimates using payroll-tax revenue data do not coincide with the changes in payroll-tax revenues predicted based on changes in labor market outcomes suggests that, indeed, newly legalized immigrants received lower wages than already documented immigrants and that the low-skilled natives who lost their jobs because of the policy were negatively selected, as shown in Table 7 using information on entrants and exiters to the labor market. Thus, this comparison allows us to quantify the effects of selection on the labor market, something that is otherwise unobservable.

Once we know the role of selection, we can re-estimate the contribution of newly legalized immigrants by considering the idea that immigrants who decided to relocate to low-immigration locations in Spain still contributed to payroll taxes. This leads to our preferred estimate: each newly legalized immigrant increased payroll-tax revenues by 4,801 euros, on average. This suggests that the biases that arise from not taking into account selection and migration are substantial.

## 5 Conclusion

This paper studies the consequences of a large amnesty program in Spain. To do so, we combined detailed geographic data on tax revenues and labor market outcomes, and we show that the legalization of around 600,000 immigrants directly increased tax revenues because these workers started to pay taxes, but it also had consequences for the labor market. We show that newly legalized immigrants, who were disproportionately low-skilled, worsened the labor market outcomes of some low-skilled natives and immigrants and improved the labor market outcomes of high-skilled natives and immigrants. Most of the effects on natives come



from employment rates, whereas employment rates, wages, and internal migration all seem to change for immigrants. In all, each newly legalized immigrant increased tax revenues by at least 4,801 euros. We consider our estimate as a lower bound, since undocumented immigrants already benefited from most public services, such as education and health care, and also started to pay income taxes when they became legal—something that we cannot take into account with the data at our disposal. Thus, amnesty programs likely have positive effects for the overall economy, but also have important distributional consequences between different types of workers.

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# Appendix

## A Robustness Checks

### A.1 Employment by sectors

In this section we show the results of employment of low-skilled natives by sector of activity. We divide local economies into three sectors: 1) high-immigrant sectors, 2) low-immigrant sectors, and 3) public administration. High-immigrant sectors are defined as sectors where, among low-skilled workers, the share of immigrants working in the sector is larger than the share of natives in the sector. Low-immigrant sectors consist of all other sectors except for public administration. We distinguish public administration from the rest because it's the only sector in the economy where the share of immigrants is negligible: only 3 percent of all immigrants work in this sector, compared to more than 12 percent of all natives.

We show the effect of legalization on employment changes across sectors in Table A.1. The sum of the point estimates in this table should coincide with the estimate in column four of Table A.3. The results show how employment losses are concentrated in high-immigration sectors, and to a lesser extent in low-immigration sectors. These results suggest that natives and immigrants started to compete in the labor market once the legalization took place.

Table A.1: **Estimates of the Effect of the Immigration Reform on Employment by Sectors**

	$\Delta$ Employment Native Low Skilled		
	High-immigrant sectors	Low-immigrant sectors	Public administration
Panel A: Baseline			
$\Delta$ Immigrants	-0.349*	-0.274	0.163
in social security/pop.	(0.181)	(0.188)	(0.118)
Observations	50	50	50
R-squared	0.046	0.035	0.035
Panel B: Without 4 main provinces (Mad., Ben., Val., Sev)			
$\Delta$ Immigrants	-0.330*	-0.255*	0.224**
in social security/pop.	(0.168)	(0.140)	(0.0913)
Observations	46	46	46
R-squared	0.043	0.035	0.072
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)			
$\Delta$ Immigrants	-0.556***	-0.237*	0.120
in social security/pop.	(0.200)	(0.140)	(0.0884)
Observations	50	50	50
R-squared	0.248	0.222	0.300
Panel D: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)			
$\Delta$ Immigrants	-1.004***	-0.115	0.168
in social security/pop.	(0.344)	(0.306)	(0.144)
Observations	50	50	50
F-test of excluded instruments	21.290	21.290	21.290
	Share in sector		
Immigrants	0.740	0.231	0.029
Natives	0.511	0.365	0.123

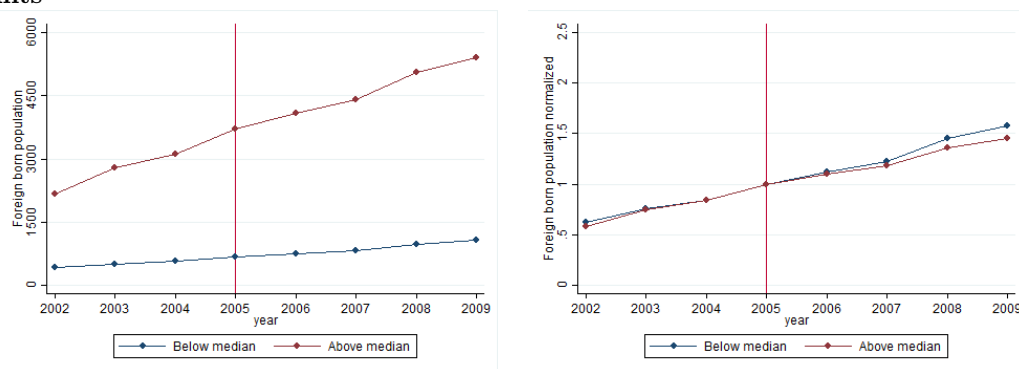
NOTE: This table estimates the effect of immigrant regularization on employment by sector of activity. High-immigrant sectors are Agriculture, Construction, Hotels and Services, and Other Services. Low-immigrant sectors are Industry (three subcategories), Transportation, and Finance. Regressions are weighted by population. Robust standard errors reported.

## A.2 Migration results

An alternative data source to measure internal migration is the Spanish Municipal Register of Population. This contains administrative data that record the location of residence of individuals living in Spain. These data have the advantage of being administrative data. However, using data from the Municipal Register, as opposed to the SLFS used in the main text, has two disadvantages: First, it is possible that people take some time to register in their new location once they have moved. Individuals have strong incentives to do so, since it gives them access to public education and health care, but there are mechanisms to obtain these services temporarily in locations other than the official residence. Second, in this data set, we cannot distinguish between high- and low-skilled workers.

It is reassuring that, using this alternative data set, we obtain very similar results compared to using the SLFS. In this appendix, we replicate the figures shown in the main text. We also check and can confirm that the estimation does not change significantly.

Figure A.1: **Spanish and Foreign-Born Population and the Immigration Reform, Natives and Immigrants**



NOTE: The figures on the left show Spanish and foreign-born population in Spanish provinces above and below the median level of immigration in 2002. The vertical red line indicates the last period before the reform (2005). The Municipal Register reports about the number of individuals residing in municipalities the first of January each year. The figures on the right normalize the figures on the left, using the last observation before the policy intervention. Source: Municipal Register.

### A.3 Empirical Evidence: Additional results

In this appendix we present several robustness checks to our baseline estimates. First we re-estimate our baseline specifications for an alternative sample of provinces that excludes the four main provinces (Madrid, Barcelona, Sevilla and Valencia). Second, we present an alternative specification that includes additional controls such as political alignment, coastal dummies and the share of construction sector for the pre-reform period (year 2004). Third, we also show the previous specification estimated with 2SLS. As an instrument for the actual share of immigrants affiliated with social security, we use the past immigration density at province level. Finally, in the case of wages, since we estimate our baseline results excluding women from the sample, in this section we also present estimates for our baseline specification, including in the sample men and women.

Generally speaking, our estimates show that our baseline results hold under alternative specifications, reducing the risk that our findings are driven by the presence of outliers, other confounding factors or endogeneity concerns.

Table A.2: Payroll-tax revenue estimates

	General Reg.	Self. emp.	Agricult.	Sea	Coal	Housekeeping	Accident	Unemp.	Total
Panel A: Baseline									
$\Delta$ Immigrants	3,983***	65.7	146.4***	-11.4	46.4	233.8***	-44.2	-230.7	4,189***
in social security/pop.	(1,348)	(43.05)	(50.92)	(18.91)	(38.93)	(75.00)	(28.37)	(456.0)	(1,051)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.411	0.032	0.276	0.012	0.019	0.519	0.053	0.018	0.515
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)									
$\Delta$ Immigrants	3,093***	73.42*	155.9***	-0.230	45.03	190.1***	-38.39	-46.29	3,472***
in social security/pop.	(947.0)	(43.53)	(56.24)	(19.29)	(35.76)	(62.00)	(33.24)	(327.8)	(819.0)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.596	0.035	0.287	0.115	0.019	0.682	0.062	0.118	0.650
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)									
$\Delta$ Immigrants	3,932***	94.87*	184.6***	-5.113	11.10	188.9***	-15.09	-454.1	3,937***
in social security/pop.	(1,243)	(52.81)	(46.15)	(17.77)	(21.12)	(62.67)	(32.87)	(317.1)	(1,026)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.537	0.193	0.394	0.161	0.088	0.662	0.171	0.488	0.598
Panel D: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)									
$\Delta$ Immigrants	6,820***	74.87	99.78*	14.97	11.19	430.6***	-35.63	-1,750***	5,666***
in social security/pop.	(1,146)	(110.0)	(59.70)	(39.27)	(43.29)	(67.19)	(63.40)	(462.7)	(974.6)
Observations	50	50	50	50	50	50	50	50	50
F-test of excluded instruments	21.290	21.290	21.290	21.290	21.290	21.290	21.290	21.290	21.290

NOTE: This table estimates the contribution per regularized immigrant in each regime of the social security in euros for different specifications. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors are reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.



Table A.3: Estimates of the effect of the reform on employment

	$\Delta$ Total Emp.	$\Delta$ Employment					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
$\Delta$ Immigrants	-0.544***	-0.382	-0.163	-0.467**	0.085	-0.339**	0.176*
in social security/pop.	(0.175)	(0.252)	(0.162)	(0.224)	(0.246)	(0.164)	(0.0937)
Observations	50	50	50	50	50	50	50
R-squared	0.087	0.034	0.010	0.077	0.003	0.059	0.062
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)							
$\Delta$ Immigrants	-0.581***	-0.407	-0.174	-0.366	-0.041	-0.271	0.097
in social security/pop.	(0.184)	(0.255)	(0.158)	(0.248)	(0.220)	(0.164)	(0.134)
Observations	46	46	46	46	46	46	46
R-squared	0.083	0.035	0.011	0.045	0.001	0.036	0.026
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
$\Delta$ Immigrants	-0.602***	-0.494**	-0.109	-0.678***	0.185	-0.366*	0.257**
in social security/pop.	(0.193)	(0.237)	(0.157)	(0.219)	(0.291)	(0.187)	(0.120)
Observations	50	50	50	50	50	50	50
R-squared	0.173	0.172	0.044	0.296	0.063	0.137	0.284
Panel D: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)							
$\Delta$ Immigrants	-0.775*	-0.693	-0.082	-0.954***	0.261	-0.231	0.149
in social security/pop.	(0.429)	(0.520)	(0.324)	(0.361)	(0.437)	(0.320)	(0.154)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	21.290	21.290	21.290	21.290	21.290	21.290	21.290

NOTE: This table estimates the effect of immigrant regularization on employment. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors are reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Table A.4: Estimates of the effect of the reform on wages

	$\Delta$ Total log wages	$\Delta$ log wages					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
$\Delta$ Immigrants	0.244**	0.310***	-0.052	0.275***	0.428*	-0.118	0.998*
in social security/pop.	(0.106)	(0.113)	(0.278)	(0.093)	(0.223)	(0.285)	(0.587)
Observations	50	50	50	50	50	50	50
R-squared	0.143	0.217	0.001	0.191	0.079	0.004	0.023
Panel B: Including women							
$\Delta$ Immigrants	0.213*	0.262**	-0.167	0.224**	0.328*	-0.295	0.941
in social security/pop.	(0.112)	(0.123)	(0.304)	(0.110)	(0.190)	(0.297)	(0.627)
Observations	50	50	50	50	50	50	50
R-squared	0.097	0.133	0.008	0.112	0.054	0.025	0.031
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
$\Delta$ Immigrants	0.0758	0.128*	-0.129	0.144*	-0.034	-0.189	1.210
in social security/pop.	(0.0758)	(0.0751)	(0.283)	(0.0730)	(0.230)	(0.293)	(0.922)
Observations	46	46	46	46	46	46	46
R-squared	0.017	0.050	0.005	0.059	0.000	0.011	0.024
Panel D: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
$\Delta$ Immigrants	0.231**	0.313***	-0.383	0.279***	0.501*	-0.527	1.749
in social security/pop.	(0.0983)	(0.106)	(0.306)	(0.0971)	(0.259)	(0.325)	(1.117)
Observations	50	50	50	50	50	50	50
R-squared	0.259	0.342	0.105	0.283	0.179	0.127	0.083
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)							
$\Delta$ Immigrants	0.291*	0.400**	-0.311	0.288*	1.094***	-0.429	-0.420
in social security/pop.	(0.171)	(0.162)	(0.609)	(0.155)	(0.305)	(0.617)	(1.773)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	21.290	21.290	21.290	21.290	21.290	21.290	21.290

NOTE: This table estimates the effect of immigrant regularization on *log* composition-adjusted wages. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors are reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Table A.5: **Estimates of the effect of the reform on internal migration**

	$\Delta$ Immigrant population share			$\Delta$ Share of LS
	Total	Low Skilled	High Skilled	Population
Panel A: Baseline				
$\Delta$ Immigrants in social security/pop.	-0.359*	-0.432**	0.073	-0.370
	(0.201)	(0.206)	(0.0862)	(0.360)
Observations	50	50	50	50
R-squared	0.029	0.055	0.012	0.042
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)				
$\Delta$ Immigrants in social security/pop.	-0.290	-0.322	0.032	-0.022
	(0.184)	(0.205)	(0.112)	(0.180)
Observations	46	46	46	46
R-squared	0.017	0.030	0.002	0.000
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)				
$\Delta$ Immigrants in social security/pop.	-0.265	-0.410*	0.145	-0.496
	(0.204)	(0.237)	(0.109)	(0.354)
Observations	50	50	50	50
R-squared	0.059	0.101	0.160	0.202
Panel D: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)				
$\Delta$ Immigrants in social security/pop.	-0.227	-0.255	0.028	-0.904*
	(0.404)	(0.404)	(0.141)	(0.517)
Observations	50	50	50	50
F-test of excluded instruments	21.290	21.290	21.290	21.290

NOTE: This table estimates the effect of immigrant regularization on the share of foreign-born population. Estimates are based on a continuous difference-in-difference strategy, where province-specific prechange linear trends are removed. Regressions are weighted by population. Robust standard errors are reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

Table A.6: **Native Selection**

	$\Delta$ (ln) wages low skilled natives		
	Always working (1)	Enter 2005 vs. Enter 2004 (2)	Lost job 2005 vs. Lost job 2004 (3)
Panel A: Baseline			
$\Delta$ Immigrants in social security/pop.	-0.064	0.739**	-0.387
	(0.102)	(0.324)	(0.426)
Observations	50	50	50
R-squared	0.011	0.052	0.016
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)			
$\Delta$ Immigrants in social security/pop.	0.076	1.006**	-0.394
	(0.094)	(0.389)	(0.461)
Observations	46	46	46
R-squared	0.014	0.064	0.013
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)			
$\Delta$ Immigrants in social security/pop.	0.004	0.442	-0.949**
	(0.103)	(0.363)	(0.448)
Observations	50	50	50
R-squared	0.383	0.181	0.121
Panel D: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform)			
$\Delta$ Immigrants in social security/pop.	0.038	0.433	0.012
	(0.120)	(0.415)	(0.703)
Observations	50	50	50
F-test of excluded instruments	60.690	60.550	55.560

NOTE: (1) Wages of those low-skilled natives who were working the entire 2004 and 2005. (2) Wages of those low-skilled natives who entered the labor market between February and December of 2005, against the wages of those who entered during the same period one year before. (3) Average wages (over the last six months) of low-skilled natives who lost their jobs between February and December of 2005, against the average wages of those who lost their job in the same period one year before. Robust standard errors reported. \* significant at the 0.10 level; \*\* significant at the 0.05 level; \*\*\* significant at the 0.01 level.

## A.4 Evaluation of immigrant reform: Additional results

This section evaluates the immigration reform considering the estimates from the alternative specifications presented in section A.3. More concretely, as we did in Tables 6, 8 and 9, here we compute contribution of low- and high-skilled natives and immigrants, taking into account the estimates of the alternative specifications presented in the previous section.

Table A.7: **Evaluation of reform raw estimates: additional results**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
<b>Panel A: Baseline</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-2,113	1,231	-3,283	1,764
Total Change	-2,113	1,231	1,054	2,157
Contribution per skill	-91%	53%	45%	93%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			4,189 euros	
Estimates of total effects, labor market			2,330 euros	
Difference in estimates			- 1,859 euros	
<b>Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-1,861	-3,94	-2,565	998
Total Change	-1,861	-3,94	1,773	1,391
Contribution per skill	-205%	-43%	195%	153%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,472 euros	
Estimates of total effects, labor market			908 euros	
Difference in estimates			- 2,564 euros	
<b>Panel C: All controls (political align.; coastal dummies; construction sector pre-reform)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-3,412	2,176	-3,441	2,807
Total Change	-3,412	2,176	896	3,200
Contribution per skill	-119%	76%	31%	112%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,937 euros	
Estimates of total effects, labor market			2,860 euros	
Difference in estimates			- 1,077 euros	
<b>Panel D: 2SLS all controls (political align.; coastal dummies; construction sector pre-reform)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-5,100	3,520	-2,182	1,176
Total Change	-5,100	3,520	2,156	1,569
Contribution per skill	-238%	164%	101%	73%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			5,666 euros	
Estimates of total effects, labor market			2,144 euros	
Difference in estimates			-3,522 euros	

NOTE: This table reports the estimates of the effect of the policy on labor market outcomes, as reported in Tables 3, 4, and 5; some summary statistics on employment variables before the policy change, i.e., the years 2002 to 2004, inclusive; estimates on the contribution to changes in payroll-tax revenues by skill group; and total changes in contributions using predictions from labor market changes and direct estimates from payroll-tax revenue data reported in Table 2.

Table A.8: **Evaluation of reform, accounting for wage selection: additional results**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
Panel A: Baseline				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-1,385	1,231	-2,152	1,764
Total Change	-1,385	1,231	2,185	2,157
Contribution per skill	-33%	29%	52%	51%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			4,189 euros	
Estimates of total effects, labor market			4,189 euros	
Difference in estimates			0 euros	
Negative selection of average wages			34%	
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-783	-394	-1,079	998
Total Change	-783	-394	3,258	1,391
Contribution per skill	-23%	-11%	94%	40%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,472 euros	
Estimates of total effects, labor market			3,472 euros	
Difference in estimates			0 euros	
Negative selection of average wages			58%	
Panel C: All controls (political align.; coastal dummies; construction sector pre-reform)				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-2,875	2,176	-2,900	2,807
Total Change	-2,875	2,176	1,437	3,200
Contribution per skill	-73%	55%	36%	81%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,937 euros	
Estimates of total effects, labor market			3,937 euros	
Difference in estimates			0 euros	
Negative selection of average wages			16%	
Panel D: 2SLS all controls (political align.; coastal dummies; construction sector pre-reform)				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-2,633	3,520	-1,127	1,176
Total Change	-2,633	3,520	3,211	1,569
Contribution per skill	-46%	62%	57%	28%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			5,666 euros	
Estimates of total effects, labor market			5,666 euros	
Difference in estimates			0 euros	
Negative selection of average wages			48%	

Note: This Table recomputes the contribution of each type of worker to payroll taxes following the legalization of immigrants by assuming that low-skilled workers (natives and immigrants) who lost their jobs tended to earn lower wages.

Table A.9: **Evaluation of reform, accounting for wage selection and migration: additional results**

	Natives LS	Natives HS	Immigrants LS	Immigrants HS
<b>Panel A: Baseline</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-1,385	1,231	-1,153	1,377
Total Change	-1,385	1,231	3,184	1,770
Contribution per skill	-29%	26%	66%	37%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			4,189 euros	
Estimates of total effects, labor market			4,801 euros	
Difference in estimates			612 euros	
Negative selection of average wages			34%	
<b>Panel B: Without 4 main provinces (Mad., Ben., Val., Sev)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-783	-394	-601	827
Total Change	-783	-394	3,736	1,220
Contribution per skill	-21%	-10%	99%	32%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,472 euros	
Estimates of total effects, labor market			3,779 euros	
Difference in estimates			307 euros	
Negative selection of average wages			58%	
<b>Panel C: All controls (political align.; coastal dummies; construction sector pre-reform)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-2,875	2,176	-1,681	2,042
Total Change	-2,875	2,176	2,656	2,435
Contribution per skill	-65%	50%	60%	55%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			3,937 euros	
Estimates of total effects, labor market			4,392 euros	
Difference in estimates			455 euros	
Negative selection of average wages			16%	
<b>Panel D: 2SLS all controls (political align.; coastal dummies; construction sector pre-reform)</b>				
	<b>Estimates on payroll taxes by skill</b>			
Labor Change	-2,633	3,520	-662	1,029
Total Change	-2,633	3,520	3,675	1,422
Contribution per skill	-44%	59%	61%	24%
	<b>Estimates of the effect on payroll taxes</b>			
Direct estimates payroll taxes			5,666 euros	
Estimates of total effects, labor market			5,984 euros	
Difference in estimates			318 euros	
Negative selection of average wages			48%	

Note: This Table recomputes the contribution of each type of worker to payroll taxes following the legalization of immigrants by assuming that low-skilled workers (natives and immigrants) who lost their jobs tended to earn lower wages and migrants who change their location within Spain continued contributing in their new locations.

## B Work Inspections

Figure B.1: Number of inspections related to foreign workers



Source: Ministry of Labor and Social Security.

## C Conditions for Work Permits

This section introduces the exact description (in Spanish) of the conditions for immigrants who were eligible to obtain legal work permits.

### **Disposición transitoria tercera. Proceso de normalización.**

1. En el plazo de tres meses desde la entrada en vigor del Reglamento de la Ley Orgánica 4/2000, de 11 de enero, sobre derechos y libertades de los extranjeros en España y su integración social, los empresarios o empleadores que pretendan contratar a un extranjero podrán solicitar que se le otorgue una autorización inicial de residencia y trabajo por cuenta ajena, siempre y cuando se cumplan las siguientes condiciones: a) Que el trabajador figure empadronado en un municipio español, al menos, con seis meses de anterioridad a la entrada en vigor del Reglamento de la Ley Orgánica 4/2000, de 11 de enero, sobre derechos y libertades de los extranjeros en España y su integración social, y se encuentre en España en el momento de realizar la solicitud. b) Que el empresario o empleador haya firmado con el trabajador un contrato de trabajo cuyos efectos estarán condicionados a la entrada en vigor de la autorización de residencia y trabajo solicitada. En el contrato de trabajo, el empresario se comprometerá, con independencia de la modalidad contractual y el tipo de contrato utilizado, al mantenimiento de la prestación laboral por un período mínimo de seis meses,

salvo en el sector agrario, en el que el período mínimo será de tres meses. En los sectores de la construcción y la hostelería, el cumplimiento del compromiso de mantenimiento de la prestación laboral de seis meses podrá llevarse a cabo dentro de un período máximo de doce meses. Cuando los contratos de trabajo sean a tiempo parcial, el período de prestación laboral se incrementará proporcionalmente a la reducción sobre la jornada ordinaria pactada en dicho contrato, en los términos que establezca el Ministerio de Trabajo y Asuntos Sociales. c) Que se cumplan los requisitos previstos en el artículo 50 del Reglamento de la Ley Orgánica 4/2000, de 11 de enero, sobre derechos y libertades de los extranjeros en España y su integración social, para el otorgamiento de una autorización para trabajar, con excepción de lo dispuesto en sus párrafos a), b) y g).

2. Con sujeción a los requisitos establecidos en los párrafos a) y c) del apartado anterior, y en idéntico plazo al establecido en éste, podrán solicitar igualmente la concesión de una autorización inicial de residencia y trabajo los extranjeros que pretendan desarrollar su actividad en el ámbito del servicio del hogar familiar, trabajando parcialmente y de manera simultánea para más de un titular del hogar familiar. Para ello deberán acreditar que reúnen los requisitos previstos por la legislación aplicable a los efectos del alta en el correspondiente régimen de Seguridad Social como empleados del hogar discontinuos y que van a realizar un número de horas de trabajo semanales no inferior a treinta, en el cómputo global. Las prestaciones laborales concertadas a estos efectos deberán de abarcar un período mínimo de actividad de seis meses. Los extranjeros que puedan desarrollar una actividad en el servicio del hogar familiar a tiempo completo para un solo empleador podrán obtener la autorización de conformidad con el apartado 1 de esta disposición, siempre que cumplan los requisitos establecidos en ella.

3. Sin perjuicio de lo establecido en la disposición adicional tercera de la Ley Orgánica 4/2000, de 11 de enero, y la disposición adicional cuarta de su Reglamento, el Ministerio de Administraciones Públicas podrá habilitar, mediante instrumentos adecuados previstos en la legislación vigente, otras oficinas públicas para la presentación de las solicitudes.

4. Las solicitudes basadas en lo dispuesto por esta disposición transitoria se tramitarán con carácter preferente. La presentación de la solicitud supondrá el archivo de oficio de cualquier otra solicitud de residencia o de residencia y trabajo para el mismo extranjero presentada con anterioridad.

5. La autoridad competente, a la vista de la documentación presentada, resolverá de forma motivada y notificará al empresario o empleador, en los casos del apartado 1, y al propio trabajador extranjero, en los casos del apartado 2, la resolución sobre la autorización de residencia y trabajo solicitada. Cuando la resolución fuese favorable, la autorización concedida estará condicionada a que, en el plazo de un mes desde la notificación, se produzca la afiliación y/o alta del trabajador en la Seguridad Social. La notificación surtirá efectos para que se proceda al abono de las tasas correspondientes. Resultará de aplicación lo dispuesto en la disposición adicional primera de la Ley Orgánica 4/2000, de 11 de enero, a los efectos del plazo para la resolución de las solicitudes.

6. Cumplida la condición de afiliación y/o alta, la autorización comenzará su período de vigencia, que

será de un año. Transcurrido el plazo de un mes desde la notificación de la autorización sin que se haya cumplido la condición señalada, la autorización quedará sin efecto. En este caso, se requerirá al empresario o empleador, en los casos del apartado 1, y al propio trabajador extranjero, en los casos del apartado 2, para que indique las razones por las que no se ha iniciado la relación laboral, con la advertencia de que, si no alegase ninguna justificación o si las razones aducidas se considerasen insuficientes, podrán denegarse ulteriores solicitudes de autorización que presente.

7. Durante el mes inmediatamente posterior a la entrada en vigor de la autorización, el extranjero deberá solicitar la tarjeta de identidad de extranjero, que será expedida por el plazo de validez de la autorización.

8. La concesión de la autorización determinará el archivo de los expedientes de expulsión pendientes de resolución, así como la revocación de oficio de las órdenes de expulsión que hayan recaído sobre el extranjero titular de la autorización, cuando el expediente o la orden de expulsión correspondiente esté basada en las causas previstas en el artículo 53.a) y b) de la Ley Orgánica 4/2000, de 11 de enero, sobre derechos y libertades de los extranjeros en España y su integración social. La denegación de la autorización implicará la continuación de los expedientes de expulsión y la ejecución de las órdenes de expulsión dictadas.