Understanding the Effects of Legalizing Undocumented Immigrants

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October 8, 2019

ABSTRACT

This paper studies the legalization of 600,000 non-EU immigrants by the unexpectedly elected Spanish government following the terrorist attacks of 2004. By comparing non-EU to EU immigrants we first estimate that the policy did not lead to magnet effects. We then show that the policy change increased labor market opportunities for immigrants by allowing them to enter sectors of the economy with fewer informal employment. We finally rely on cross-province comparisons to document that a) for each legalized immigrant payroll-tax revenues increased by at least 4,000 euros, b) the policy change *deteriorated* the labor-market outcomes of a *selected group* of low-skilled natives, *improved* the outcomes of high-skilled workers, and induced some low-skilled immigrants to move away from high-immigrant locations.

1   Introduction

Many countries host large numbers of undocumented immigrants.[1] By many accounts, the United States leads this ranking. According to the Pew Research Center, in 2017 there were as many as 10.5 million unauthorized immigrants on American soil, representing 2 percent of all immigrants.[2] 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.[3] 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.[4]

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 working-age 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 providing organizational structure, and in enforcing and monitoring the implementation of the policy.[5] For example, work inspections increased by an astonishing 132 percent, something that was widely announced at the time.[6]

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 and it was the mishandling of the crisis in the post-attack days that caused Rajoy's Popular Party to lose this election, as explained in detail in Garcia-Montalvo (2011). 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.[7]

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[1]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 in the host country.

[2]See https://www.pewresearch.org/fact-tank/2019/06/17/key-findings-about-u-s-immigrants/(accessed in October 2019).

[3]Data from Spanish Labor Force Survey (SLFS). See more details below.

[4]See Domingo and Recaño (2005).

[5]The government set up 742 information points across the country and reinforced administrative staff with about 1,700 additional employees (Finotelli, 2011).

[6]For a news report on the policy, see elpais.com/elpais/2005/05/07/actualidad/1115453817_850215.html (accessed in October 2019). The news from El Pais at the time had a special mention of the increase in work inspections. Data for work inspections can be found at http://www.mitramiss.gob.es/itss/web/Que_hacemos/Estadisticas/index.html(accessed in October 2019). The number of yearly work inspections before the policy was around 30,000. See Appendix C for more details.

[7]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 amnesty programs.

We begin our analysis by documenting exactly what the policy meant for the Spanish economy. The first step is to analyze whether there is any evidence that the policy lead to "magnet effects". At the time, one of the most publicly debated issues around this policy was whether it would lead to increased immigrant inflows. We leverage the fact that the amnesty program only affected immigrants from outside the European Union (EU) to provide evidence on this question, which is not only relevant for amnesty programs but more generally for any policy that may affect the well-being of immigrants in the host economy. The evidence strongly suggests that the policy change did not lead to magnet effects. We do not detect in the data any differential increase in the stock of immigrants from outside the EU relative to immigrants from the EU. This is true both when we focus on short- and longer-time horizons (i.e. the first year after the policy and up to four years after). Hence, the amnesty program meant only a change in the right to work in the formal sector and not a change in the total supply of immigrant workers.

The second step in our analysis is to understand how gaining work permits affected immigrant labor market opportunities. For this, we analyze in detail the career path of the newly legalized immigrants who entered the social security system as housekeeping services type workers, something that we can do with data from the *Muestra Continua de Vidas Laborales* (MCVL). Spain has six different labor market "regimes" in the Social Security. Most workers use the "General regime" but there are some sectors or occupations that use alternative contract types, which are usually referred as "special regimes". Among these special regimes there is housekeeping services, which is a sector of activity with very high levels of informality and hence very high levels of undocumented immigrants. Among the conditions of the amnesty program was that immigrants had to have an employer willing to offer them a contract for six months. Indeed, we document that immigrants who entered in the social security system as housekeeping workers stayed employed in the sector for about six months. After this initial period a large fraction of formerly undocumented immigrants moved into other sectors of the economy, most predominantly into "Hotels and Restaurants" and "Retail". In numbers, we show that out of the 100,000 immigrants who entered the social security system by mid 2005 as housekeeping workers, only half remained in this sector by the end of 2006. We also document a movement from one employee firms (i.e. households employing housekeeping services) to larger firms. We view this as evidence that gaining work enabled immigrant workers to enlarge their labor market opportunities.

We then turn to analyzing the effects of the amnesty program on tax revenues and labor market adjustments 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.[8] Our identification strat-

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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.

[8]We show that our results are very robust to a number of specifications. In Appendix B we show that our baseline results

egy relies on the fact that the policy was not anticipated, was implemented homogeneously across provinces, and linear-province specific time trends can account for secular differences across provinces in the outcomes of interest. Using administrative data aggregated at the province level, we first estimate that, for each newly legalized immigrant, payroll-tax revenues increased by 4,189 euros at the province level.[9] 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, which is in-line with the fact that newly legalized immigrants earned less than average workers in the economy or with the fact that there were other effects on the labor market (or a combination of both).

Second, we investigate the effect of the amnesty program on the labor-market outcomes of various types of workers. For this, we use two different data sets. We start by using the standard Spanish Labor Force Survey (SLFS, in Spanish the *Encuesta de la Población Activa*, EPA). This survey captures both formal and informal workers, although they cannot be separately identified in the survey.[10] Using these data we document that over the two years that followed the reform, for every 10 newly legalized immigrants, over 3 low-skilled natives lost their job while almost 3 additional high-skilled workers found a job. That is, overall native employment decreased while also becoming more intensive in high-skilled workers.[11] From the SLFS we cannot know however, whether the decline in low-skilled native employment was a result of further competition in the labor market – given that newly legalized immigrants could enter in new sectors of the economy after to the legalization – or whether it was the result of increased enforcement against informality – something emphasized in the policy change.[12]

By using data from the *Muestra Continua de Vidas Laborales* (MCVL) we can trace workers in the social security system, hence, formal workers. Using these data we document a number of interesting facts. First, we corroborate the evidence from the SLFS on immigrant employment. Among low-skilled immigrants, we obtain using data from the SLFS that over the two years following the reform, 4 immigrants lost their job for every 10 immigrants entering the social security data. These estimates imply that 6 formal immigrant-worker jobs were created as a result of the amnesty program. This number coincides exactly with the direct estimate from the MCVL. Second, we investigate what happened to formal sector native jobs. We document that the amnesty program did not affect formal native low-skill employment. Hence, all the lost jobs estimated using SLFS data are informal native low-skilled jobs. Hence, this suggests that the effects on native low-skilled

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 2SLS strategies to estimate all our coefficients of interest.

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

[10]Both, documented and undocumented immigrants, should be in principle 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, education, and other services. Moreover, undocumented immigrants did not have specific incentives to lie about their working status in the SLFS since the survey is not linked to any enforcement policies.

[11]We provide a detailed discussion of the magnitude of these estimates in section 3.3.5. In short, these estimates 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 Card and Lemieux (2001); Borjas (2003); Acemoglu et al. (2004); Monras (2015c); Neumark (2017).

[12]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.

employment are entirely driven by increased enforcement against informality rather than competition in the labor market for low-skilled jobs. Third, we document that there is a small loss in high-skilled native formal employment which is entirely driven by females. Our interpretation of this finding is that the legalization effectively increased the costs of housekeeping services by at least 22 percent (the size of the payroll tax for this type of contract which is smaller than the general regime pay-roll tax rate of 36 percent), which pushed some native high-skilled women out of the formal labor force and into home production. Given the unequal distribution of home production between males and females in Spain, it is not surprising that the lost high-skilled employment concentrated among women.

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 that those who lost their jobs had lower earnings than those who lost their jobs in previous years during comparable months. 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 the total 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 more than 1 log point for a 1 percent increase in the share of legal immigrants. This deterioration of labor market conditions for low-skilled immigrant workers may also explain why for each newly legalized immigrant in a location, 0.57 low-skilled immigrants relocated, in the two years following the legalization, suggesting that the policy increased internal mobility of mainly foreign-born individuals, perhaps less dependent on immigrant networks once they gained work permits.

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 distributional consequences – some native low-skilled workers probably lost, while highly skilled ones in general benefited, and not all immigrants who gained work permits kept their job in the subsequent two years after the reform.

Several empirical papers have studied amnesty programs in a variety of countries, although the focus is generally on immigrant outcomes. 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.[13] Similarly, Baker (2015) finds that the Immigration Reform and Control Act of 1986 significatively reduced crime in the U.S. There are also other papers that estimate the effects of gaining legal status on the general outcomes of immigrants (Dustmann et al., 2017a), 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 legalization (DiPorto et al., 2018; Devillanova et al., 2017; Amuedo-Dorantes and Bansak, 2011; Amuedo-Dorantes et al., 2007; Kaushal, 2006; Cobb-Clark et al., 1995).[14] In a somewhat closely related paper Dolado et al. (1996), study an amnesty program in Spain in the early '90s. Relative to this paper, our policy change was unexpected, which is important for our identification strategy and we can make use of both administrative payroll-tax data and much more detailed labor market data to obtain deeper insights on how the labor market adapted to the amnesty program, something that they could not do given the data available for their study.

Many of these amnesty programs, most famously the Immigration Reform and Control Act of 1986, combined 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 previous work, our case study generates arguably exogenous variation stemming from 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.[15]

In what follows, we introduce in Section 2 our data and explain the particular circumstances that led to the policy change. We then show empirical evidence on magnet effects, newly legalized immigrants' labor market experiences, public revenues, and labor market outcomes. We do this in Section 3. Section 4 offers our conclusions.

2 Data, Background, and the Policy Change

2.1 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. We use 50 Spanish

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[13]See also related evidence in Mastrobuoni and Pinotti (2015).

[14]A recent paper by Cascio and Lewis (2017) shows that the Immigration Reform and Control Act of 1986 redistributed resources toward high immigration locations since immigrants complied with tax payments. This redistribution stemmed from increases in transfers from programs like the EITC and increases in local tax revenues.

[15]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 et al. (1997), Card (2005), Lewis (2012), Glitz (2012), Monras (2015b), Llull (2017), and Borjas and Monras (2017).

provinces, i.e. all existing provinces except for the two Spanish enclaves in Africa (Ceuta and Melilla). Provinces are similar to Commuting Zones in the United States. Each province has a capital and most of the province is well connected to this capital which is usually the most important municipality.

Before describing the data in more detail, we start by providing an overview of the data at our disposal. First, we have aggregate administrative data on payroll tax revenues at the province level. These data report the total amount of taxes collected in each province for each of the labor market contract types, called "regimes", available in Spain. Among these regimes, we have housekeeping services. These data cover public tax revenues only coming from payroll taxes. We have these data at a yearly frequency.

Second, we have individual level data from the Spanish labor force survey. From these data we have standard information on employment. In these data we can distinguish native and immigrant workers of different skills. Among immigrant workers, the data covers both documented and undocumented workers, although we cannot tell from this individual-level data which immigrant worker is documented and which one is not. The employment question does not mention informality. Hence, if someone has worked informally in the reference week should appear in the survey as working.[16] This data is a repeated cross-section.

Third, we have individual-level data on the working history of a representative sample of workers. These data are representative of the population of workers affiliated to the social security system, i.e. it covers a representative sample of all legal workers in the formal sector. In these data we can track workers over time. Hence, we can track in this sample the newly legalized workers only when they gain work permits. These data has excellent information on wages.

Finally, we have data on the universe of people living in Spain from the Municipal Register. These data cover both documented and undocumented workers. Undocumented immigrants are fully covered in these data, since registration in the Municipal Register grants them access to health care services and education. Moreover, local administrations also have incentives to track the number of people living in each municipality since it affects the amount of grants received from upper tiers of government.

Hence, the main data limitations that we have are that a) we lack data on the wage of undocumented workers *before* the amnesty program, and b) we cannot distinguish documented and undocumented immigrant workers in the employment survey data. In what follows we explain each data set at our disposal in more detail.

2.1.1 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 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. Lastly, total payroll-tax collection statistics

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[16]The exact question in the SLFS is "Ha trabajado de manera remunerada la semana de referencia?", which can be translated as "Have your work for a salary in the reference week?".

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. Therefore, this detailed data set allows us to identify the effect of the policy change on payroll-tax revenues.

2.1.2 Employment and population data

Our main data set on employment and population is the SLFS. 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. We use the SLFS for the period from 2002 to 2007 and 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 B.3.

2.1.3 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.

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 *exact* same 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 of which 174,851 are native and 42,022 are foreign-born. 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.

2.2 Background and 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 being 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.[17] Similarly, in June 2002, the EU Summit in Seville agreed on tougher regulations to deter illegal immigration to Europe.[18]

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 has traditionally adopted tougher regulations to limit immigration in Spain. 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. 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. 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

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[17]See Real Decreto 864/2001.

[18]In Seville, 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).

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.[19]

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.[20] 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.

2.3   Affiliations to the Social Security

The 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 vacancies for which employers do not find resident workers, and, on the other hand, to increase the control over the concession of these authorizations."[21] The policy recognized 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,[22] and 2) the employer needed to show that it wanted the worker by offering a legal working contract for at least six months.[23] 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.[24]

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

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[19]For more details on voting intentions one week before election day, see Garcia-Montalvo (2011).

[20]Real Decreto 2393/2004.

[21]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."*

[22]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.

[23]There were some exceptions for the agricultural, construction, restaurant and hotel, 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, it was the employers instead of the foreign workers who were irregular, who had to submit the request for authorization and the job contract that linked them with the foreigner.

[24]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.

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.[25]

Figure 1 goes around here

As in many other countries, there is a lot of heterogeneity in relation to where immigrants cluster. On 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.[26] 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 goes around here

While the number of immigrants who were granted work permits among those who applied was very high and similar across all provinces (see Table 1), the legalization of around 600,000 immigrants likely had heterogeneous effects across space since immigrants are differentialy important as a fraction of the local labor force. 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 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 goes around here

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[25]In fact, there were 691,655 applications to the amnesty program, of which 578,375 (83.6 percent) were approved. Source: Anuario Estadístico de la Inmigración 2005 (Ministry of Labor and Social Security).

[26]These immigrant shares include all foreign-born individuals, i.e. it includes forwign-born people from EU and non-EU countries.

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. Therefore, it represents an exceptional opportunity to evaluate the consequences of this kind of immigration reforms. 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, suggesting that the take-up rate of the policy was similar across provinces and the fraction of undocumented immigrants was similar across provinces.[27] 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.

2.4 Identification Strategy

We are interested on the effect of the entry of immigrant workers into the social security on a number of outcomes of interest. Hence, a natural regression is as follows:

$$Y_{ct} = \delta_c + \delta_t + \delta_c * t + \beta * \frac{\text{Imm Soc Sec}_{ct}}{\text{Pop}_{ct}} + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is an outcome of interest, like the employment rate, in location c at time t, and where $\frac{\text{Imm Soc Sec}_{ct}}{\text{Pop}_{ct}}$ is the number of immigrants registered in the social security system as a fraction of total working-age population in each location and time period.

Equation (1) identifies β from changes over time within locations in the share of immigrants affiliated to the social security system. An OLS estimate of this regression would be unbiased if there is no correlation

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[27]Something related to that is the fact that, as can be seen in Table 1, the number of accepted applications in the legalization process, which we call the legalization rate, was very similar across provinces.

between the error term ε_{ct} and the share of immigrants in the social security system.

There are many reasons, however, why the share of immigrants in the social security system may be correlated to the error term. Perhaps if economic conditions are good in some region at a certain point in time, the share of immigrants affiliated in the social security increases and so does a labor market outcome of interest Y_{ct} like the employment rate. In this case, the OLS estimates would be upward biased. The OLS may be downward biased if in good economic conditions, natives' entry into the labor market displaces immigrants from the formal sector.

A first step towards identification is to concentrate on the variation generated by the natural experiment. This means comparing the mean of deviation of the average of the outcome variable from the national mean in the years before the policy to the years after the policy and relate this to the increase in social security affiliations that occurred as a consequence of the policy. In equations:

$$\Delta \bar{Y}_c = \alpha + \beta * \Delta \overline{\frac{\text{Imm Soc Sec}_c}{\text{Pop}_c}} + \varepsilon_c \tag{2}$$

where the upper bar indicates the average deviation of the variable from the national average in each year of the pre- and post-periods.[28] Using a specification that takes the average in all pre-change years and compares it to all post-change years is the recommended specification in Bertrand et al. (2004), especially in the presence of serial correlation.[29]

This regression identifies β by relating the change in the average of the outcome variable of interest between the pre- and post-shock periods to the change in the share of immigrants registered in the social security system induced by the policy. To the extent that a) the policy change was not anticipated, b) the compliance with the policy was similar across locations, and c) the trends of the outcome variable of interest were parallel across locations prior to the shock, then β measures the causal effect of legalizing immigrant workers on the outcome variable of interest.

We have argued that there are strong reasons to believe that the policy change was unanticipated. The policy change was introduced by a government that was unexpectedly elected following the largest terrorist attacks ever to take place in Spain. Hence, it is unlikely, and the data seems to confirm it, that the policy was not anticipated in any way, and hence a comparison between the pre- and post-period identifies the effect of the policy.

Whether the policy was complied by every immigrant is a priory uncertain. It could be that there are systematic differences across provinces in the fraction of immigrants that gained work permits, something that could, in principle, be correlated with changing labor market conditions at the local level. However, there are several pieces of evidence that suggest that this is not a major concern. First, Table 1 shows

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[28]More specifically, we compute for dependent and independent variables $\tilde{X}_{ct} = X_{ct} - \frac{1}{C}\sum_c X_{ct}$ for each year, where c indicates locations at time t. Then we take the average over the pre- and post-years: $\bar{X}_c^{Pre} = \frac{1}{3}\sum_{k=2002}^{k=2004} \tilde{X}_{ck}$, $\bar{X}_c^{Post} = \frac{1}{3}\sum_{k=2005}^{k=2007} \tilde{X}_{ck}$. Then, $\Delta \bar{X}_c = \bar{X}_c^{Post} - \bar{X}_c^{Pre}$. Note that these computations can be done using fixed effect reressions.

[29]This specification has two main advantages. First, it highlights the exact variation used to identify the parameter of interest, and two, it is very conservative in the estimate of the standard errors.

that the legalization rate is very similar across provinces. This measures the fraction of applications to the legalization program that were accepted. These small differences in the legalization rate across provinces suggest that the government was not endogenously more selective in approving the applications in provinces with specific labor market conditions. Second, as documented in Finotelli (2011), the government set up 742 information points across the country and reinforced administrative staff with about 1,700 additional employees, so it does not seem to be the case that it focused its monitoring efforts only in a selected group of provinces. Third, the increase in social security affiliations is proportional in high- and low-immigrant locations, as shown in Figure 2, something that would arise naturally if take-up or compliance rates of the policy were indeed similar across provinces.

The final concern in our identification strategy is that perhaps trends in outcome variables of interest are different across locations. As can be seen in Table 1, provinces with high shares of immigrants are substantially different than provinces with lower levels of immigration. For instance, high-immigration provinces tend to be larger and concentrated along the coast. If there are secular trends in the labor market that differ across provinces, we may be confounding the effect of the policy with the secular trends that differ across locations.

We address this third concern in two different ways. First, we remove linear specific trends at the province level prior to the policy change. The assumption here is that the trend in variables of interest would have continued had not been for the policy. This assumption is reasonable as long as we stop the analysis before the beginning of the Great Recession that clearly changed the trend in labor market outcomes. A second way in which we address this concern is by documenting that our results do not change when we exclude the largest provinces and when we include coastal dummies and other controls like local industrial composition that can be correlated with the location-specific business cycle.

Given these considerations, our main specification can be summarized in the following regression:

$$\Delta \widehat{Y}_c = \alpha + \beta \Delta \frac{\widehat{\text{Imm Soc Sec}}_c}{\text{Pop}_c} + \gamma X_c + \varepsilon_c \tag{3}$$

where the hat indicates that we have removed province specific linear trends *prior* to the policy, province and year fixed effects, and where X_c indicates control variables like political alignment, coastal dummies or the pre-reform share of the construction sector. In the main regressions we consider 2002 to 2004 as the pre-period and 2005 to 2007 as the post-period. We explicitly stop our analysis in 2007 to avoid the differential effect that the Great Recession could have had across Spanish provinces.

This identification strategy measures the average treatment effect. In Appendix A we discuss in more detail our identification strategy and we explain how we can estimate β using alternative estimators. In addition, we explain in detail why our preferred specification is the one explained in this section.

3 Empirical Evidence

3.1 What did the policy change imply?

The first step in the analysis is to understand exactly what the policy meant. Formally, the policy gave work permits to immigrant workers in the informal sector. However, such policy might have also have side effects with respect to immigration. First, it might have encouraged more immigrants to move to Spain, in the hope of higher chances for legal work status. This was, in fact, one of the most debated issues with respect to the policy and was often referred as magnet effects. More generally, whenever there is a policy that potentially affects welfare of immigrants in the destination country it is important to know whether immigrant flows towards the country react. We investigate this in section 3.1.1 by comparing immigrant stocks and net flows from countries affected by the amnesty program and EU-countries which were not affected by the policy.

A second important aspect of what the policy meant is it formally allowed undocumented immigrant workers to work in the formal sector. There is substantial variation across sectors in how easy it is to avoid controls from the authorities. Among the sectors with a higher prevalence of informal work is housekeeping services. Spanish labor market legislation has a special work contract type for workers in this sector which allows us to track how these workers entered the social security and how they moved to other sectors of activity and other types of employees. We document this in subsection 3.1.2

3.1.1 The Absence of Magnet Effects

One of the most controversial aspects of the policy was whether it would lead to magnet effects, namely, an increase in immigrant flows from the countries of origin included in the amnesty program. In our context it is important to know whether immigrant flows responded to the policy. If the total supply of immigrant labor in Spain did not change with the policy then the effect of the policy is simply to let immigrants enter the formal economy, potentially affecting tax revenues and labor market outcomes. If instead the policy change also affects the total flow of immigrants towards Spain, then this would imply that on top of letting undocumented immigrants enter the formal economy, the policy change also induces an immigrant-driven labor supply shock.

To investigate the presence of magnets effects, we rely on data from countries of origin that were not included in the legalization process. Immigrants from the European Union were not part of the legalization process because in 2005 Spain was, and had been for many years, part of the Rome and Schengen treaties that allows European citizens to freely move and work within the EU without borders. Hence, we can compare whether there is a change in the stock of immigrants from countries of origin affected by the policy relative to the stock of immigrants from the EU.

To graphically show the absence of magnet effects, we show in Figure 3 the stock of immigrants from the top four sending countries, three of which were affected by the policy, one which was not. The graph does not show any significant change in the stock of immigrants from the UK (non-affected by the policy),

Ecuador (affected), Romania (affected), and Morocco (affected).[30] This suggests that the policy did not lead to a substantial change in the (net) flow of immigrants from any of these countries.

Figure 3 goes around here

We can formally test that there is no change in the trend of immigrant stocks around the policy change from *all* countries of origin using data from the Municipal Registry, which should capture very well both documented and undocumented immigrants.[31] More specifically we can use the following equation:

$$\ln M_{o,t} = \delta_t + \delta_o + \beta \text{Affected countries}_{o,t} + \varepsilon_{o,t}$$

where $M_{o,t}$ is the total number of working-age immigrants from origin o at year t. "Affected countries" is a dummy variable that takes value equal to 1 if the country is affected by the policy at time t. If $\beta \neq 0$ then it would be evidence that the policy changed the total supply of immigrant labor. If instead $\beta = 0$ then, this is evidence that the policy did not lead to magnet effects.

Table 2 goes around here

Table 2 shows the results on magnet effects. Column 1 shows that there is no systematic change in the stock of immigrants from countries affected by the policy change around the time of the change. In this first column, the sample years include 2002 to 2009. In columns 2 to 4 we change the years selected, by zooming into the year 2005 when the policy change was implemented. As we zoom in, it is clear that there is no differential change in the stock of immigrants from affected and non-affected countries. Column 5 reports the estimates of the same regression in first differences (and hence (net) flows), where country of origin linear specific trends prior to the policy have been removed. Again the estimate is close to 0 and statistically insignificant. In sum, Table 2 suggests that the amnesty did not change the total supply of labor.

3.1.2 Newly Legalized Immigrants' Labor Market Experience

The amnesty program effectively gave the right to undocumented immigrant workers to access parts of the economy where, prior to the policy, they could not access. To study this, in this section we focus our attention on the immigrant workers who entered the social security system in the first half of 2005 using the labor market contract type designed for housekeeping services and stayed in the social security until 2009. Many people working in housekeeping services do so informally. The legalization process was an opportunity

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[30]Note that Romania was not part of the EU until 2007.

[31]Being in the Municipal Register allows individuals to access public health care and education and was not attached at the time, to being able to work in the country. From the view point of the local administrations, which handle the Municipal Register it is also in their interest to have everyone living in the municipality registered, since this is the basis to calculate the grants' amount from upper tiers of the government.

for many of these workers to gain work permits and potentially a path towards other types of occupations, which we can document because a sample of these workers enter the MCVL data.

Figure 4 shows five graphs. The graph on the top-left part of the figure shows the total number of affiliates in the social security system that use the contract type associated to housekeeping services. This number fluctuates around 60,000 prior to the amnesty program. Coinciding with the policy change this number jumps immediately to almost 160,000. The amnesty required employers of the newly legalized immigrants to offer them a work contract for at least 6 months. After these six months, the number of affiliates decreases to a plateau of around 100,000 workers.

Among the newly legalized workers who remained in the social security in 2009, we can track their sector of activity. The top-right graph of Figure 4, labeled as Panel B, shows the fraction of these workers that remained in the housekeeping sector. It is striking that the fraction who remained in the housekeeping services stayed very close to 1 for the entire 2005. These workers had necessarily work contracts with families that had helped them enter the formal market. Their wages were probably lower than the wages they could get in other sectors. For instance, in Panel E of Figure 4, we show that legalized immigrant workers who moved away from housekeeping sector, after 2006, had higher average wages than all workers legalized through the housekeeping sector.

Starting around the beginning of 2006, many of these workers started to move to other sectors of the economy. Panel C of the figure shows the sectors where these immigrant workers moved into. Most of them entered the Hotels and Restaruants (H&R), and other services, although many also entered other sectors. Panel D shows the average size of the firms where these workers were working. When employed in the housekeeping services, the employer is typically a family that usually hires one person to either clean the house or take care of either the young or the elderly. Therefore, these graphs suggest that the housekeeping workers that left the sector moved into large firms.

Overall, we view this evidence as suggestive that the policy change allowed immigrant workers to enter the formal market economy.[32] One way to understand the policy change documented in subsections 3.1.1 and 3.1.2 is through the lenses of the model presented in Liu (2019). In that model, a policy change may induce changes in entry mode and occupation choice. In our case, the policy change is a combination of increased enforcement against informality and a process of legalization of undocumented workers which in Liu's model will result in a shift towards occupations with lower prevalence of informal jobs, without necessarily an increase in overall migration.

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[32]Same figures for two other sectors with high presence of migrants, albeit lower informality, (Construction and H&R) are shown in Appendix B.4.

3.2 Public Finances

One of the most immediate consequences of the reform was that undocumented immigrants started to pay taxes and, in particular, payroll taxes. Thus, once we have showed that the policy did not affect the total supply of immigrants in Spain and after showing how migrants used the policy change to enter into the formal economy, it is worth studying the effect that the reform had on public revenues.

To show evidence on this, we first plot the level and proportional changes in total payroll-tax revenues. Figure 5 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 5, 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 5 goes around here

The graph on the right in Figure 5 normalizes the level of tax revenues in the province 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 location-specific linear trends leading to the policy change. When we do so, we obtain Figure 6.

Figure 6 goes around here

Figure 6 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 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 in part a result of the newly legalized immigrant workers moving towards better paid sectors that were not available to them.

To see whether these are large or small changes in total payroll tax 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 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 than 3 percentage points. Since, as we just showed, the increase in social security affiliations was about 1.5 percentage points on average, 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 within the same skill levels immigrants

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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 3.3.

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. As mentioned before, Spain has different regimes for different types of workers. Most workers are in the general regime, but there are also a number of special regimes. One that is used particularly by the immigrant population, as discussed in Section 3.1.2, is the housekeeping regime, which corresponds to housekeeping services.[33] We can use revenues from this contract regime to show that the change is indeed more pronounced, in this case, than in contract types used less often by immigrants.

<center>Figure 7 goes around here</center>

Figure 7 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 around 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. It is also remarkable that in 2007 payroll tax revenues from housekeeping services declined in high-immigrant locations. This is a result of the movement of immigrant workers to other parts of the economy.

We use the insights from Figures 5 and 6 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, following the identification strategy discussed in section 2.4. 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 6, we first remove linear specific trends at the province level and estimate the effect of the policy as deviations from these trends. We argued in section 2.4 that this produces unbiased estimates if the policy change was unexpected and the compliance with the policy was proportional across locations.[34]

The results are shown in Table 3. In Table A.1 of Appendix B we show, using slightly different specifications, the robustness of these results.[35] 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.

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[33] 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.

[34] The legalization rate with the policy seems to be proportional across locations, as shown in Table 1.

[35] 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 candidates to gain the legalization, at province level, to predict the actual share of immigrants affiliated to the social security.

Table 3 goes around here

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

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

where c indicates provinces. β estimates how much newly regularized immigrants contributed to total payroll-tax revenues changes. An estimate of $\beta = 1$ means that newly regularized immigrants contributed as much as previous immigrants and natives in the social security system. An estimate of less than 1 means that they contributed relatively less.

Figure 8 goes around here

The estimate that we obtain is 0.55(0.1) (with an R-squared value of 0.44), as shown in Figure 8. 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, public expenditure should be taken into account to evaluate the complete effect of the policy.[36]

Thus, from Table 3 and Figure 8 we learn that the policy helped increase tax revenues, but less than we might have expected from previous contributors 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.

3.3 Labor Market Outcomes

In this section we estimate the effect of the policy on labor market outcomes. We split the section into several outcomes: total employment (i.e. formal plus informal employment), formal employment, wages, and internal migration.

3.3.1 Total Employment: evidence using Labor Force Survey data

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

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[36]There is substantial heterogeneity across countries and over time in what being undocumented means. In the case of Spain in 2004, being undocumented meant lacking a work permit to work in formal jobs that granted access to programs funded by the social security, most importantly unemployment benefits and pensions.

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.[37] In this section we use data from the SLFS. These data capture both formal and informal workers. We cannot distinguish in the SLFS who is working formally or informally.

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 9 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.

Figure 9 goes around here

The graphs on the right of Figure 9 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 policy change decreased the employment rates of natives. It is worth noting that the gap in employment rates widens gradually. The graph on the right of Figure 9, Panel B, shows the normalized series for immigrants. Again, this series includes both documented and undocumented immigrants, since we use data from the SLFS. 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.

Table 4 goes around here

In Panel A of Table 4 we quantify the effects of the policy change on employment. We see that employment rates dropped as a consequence of the immigration reform. For each newly regularized immigrant who started to pay taxes, a bit under 0.5 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 9, the effects on employment are smaller during the first six months after the policy change. To get a sense of the magnitude of this estimate it is worth mentioning that it is in line with a the short-run labor demand elasticity is around 1.3 or that the inverse demand elasticity is around 0.8. We discuss this in a lot of detail

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[37]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.

in Section 3.3.5. 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.[38] See for example Monras (2015c), Neumark (2017), Borjas (2003), Acemoglu et al. (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, 0.37 low-skilled natives and 0.41 low-skilled immigrants lose their jobs, while 0.12 high-skilled natives and 0.16 high-skilled immigrants gain jobs. This shows that, the policy change had distributional consequences in terms of employment opportunities. Furthermore, in Table A.8 in the Appendix we show that employment losses of low-skilled natives are stronger in sectors with high concentrations of immigrant workers.

This evidence is consistent with two ideas. First, that the policy change increased enforcement against informality, and hence, some native low-skilled workers working informally lost their informal jobs. Second, it is also consistent with the idea that the policy created some competition between natives and immigrants, even within sectors. To investigate this we turn to formal employment data.

3.3.2 Formal Employment: evidence from Social Security data

To investigate formal employment we turn to data from the *Muestra Continua de Vidas Laborales.* Using the same specification than before we quantify in Table 4, Panel B, the employment effects that the reform had on formal employment. The first column of the table shows that the reform lead to an increase in formal employment. This is one of the intended goals of the reform, since it moved informal immigrant workers to formality by granting them work permits. The estimate that we obtain suggests that for each immigrant that entered the formal sector during the reform it lead to an increase of only .5 jobs over the course of the subsequent two years. Note that, in the absence of further labor market effects we would have obtained an estimate of 1.

The second column of Table 3, Panel B, shows that on average, the reform did not change the total number of native workers employed. The estimate is similar in magnitude to the estimate from the SLFS data. In the third column of the table we show that for each new legalized immigrant, there were almost .6 extra immigrants in the social security system. Again, this estimate coincides with the SLFS estimate. It means that .4 immigrant workers lost their informal sector job as a result of the reform. This is likely a consequence of the fact that the reform made formerly informal workers around 30 percent more expensive. The break-down by skill levels shows that indeed the employment effects among immigrants concentrate on low-skilled workers, since those are the bulk of the undocumented immigrant group. It also highlights an interesting fact. We detect a small but precisely estimated negative effect on the employment rate of native high-skilled workers in the formal sector.

To investigate further what could explain the negative estimate on native high-skilled workers in the

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[38]Note that labor demand elasticities could vary considerably accross countries. Dustmann et al. (2017b) 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 et al. (2016).

formal sector we re-estimate our effects on the employment rate of women. After all, an important fraction of newly legalized workers were housekeeping service workers and home production in Spain is very unequally distributed between men and women. We show these results in Table 4, Panel C. Estimates show that the formal native-high skilled employment loss is almost entirely driven by women. Moreover, when focusing on women we also see some employment losses among native low-skilled women, probably also a consequence of the increased cost of house keeping services. In either case, the estimates are small even if statistically different than 0. For each newly legalized immigrant less than .1 low-skilled and less than .05 high-skilled women dropped from formal employment.

We show the robustness of these results using alternative specifications and estimators in Tables A.3 and A.4.

3.3.3   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. We use "composition adjusted wages" to measure the unit price of labor.[39] 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 we allow that different periods and different provinces may have different wage levels and wages may be evolving differently across provinces. 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 B we show that results do not depend 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 10 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 in 2007 or 2008 than previous linear trends wold have predicted; hence the negative numbers in the series. This is apparent for native workers; differences are minimal in the case of immigrants.[40]

Figure 10 goes around here

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[39] 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.

[40] 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 9.

We quantify the insights of Figure 10 in Panel B of Table 4, and we provide a number of robustness checks in Appendix B.[41] 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. It is likely that most of the wage results for high-skilled individuals reflect changes in the price of labor coming from the increase in the relative demand, while changes for low-skilled individuals reflect selection.

Wages for high-skilled natives in high-immigration locations increased by 0.24 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 0.30 log points. The results are more mixed for immigrants. The policy seems to have increased the wages of high-skilled immigrants by more than 1.5 log points, while the wages of low-skilled immigrants decreased in the formal sector, if anything.

We investigate low-skilled native selection patterns more directly in Table 5. 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 5, and again, we provide alternative specifications in Appendix B.[42] In the second column, we show that low-skilled natives who entered the labor market in high-immigrant provinces during 2005, and thus were potentially 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.

<div align="center">Table 5 goes around here</div>

3.3.4   Internal migration

Not only employment rates 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

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[41]In Table A.5 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) using a 2SLS estimator that relies on the past shares of migrants candidates to gain the legalization, at province level, to predict the actual share of immigrants affiliated to the social security.

[42]See some robustness checks in Table A.7 in the Appendix.

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 2.1). 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 B.3, we show that we obtain the same results using administrative data from the Municipal Register.[43]

Figure 11 shows these series for natives (Panel A) and for immigrants (Panel B). 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. From the graphs of Panel B, 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 11 goes around here

The results are clear. After the reform, low-immigration provinces started to gain immigrant population. We quantify this in Table 6, and, as before, we provide estimates using alternative specifications in Appendix B.[44] 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 the various provinces more easily comparable.

Table 6 goes around here

Table 6 shows that for every newly legalized immigrant, 0.50 immigrants left high-immigration provinces. This is due to the outflow of low-skilled immigrants towards low-immigration locations, since, as we showed in section 3.1.1 total migrant flows to Spain seemed to be unaffected by the policy. For every newly legalized immigrant, 0.54 low-skilled immigrants left high-immigration locations, while 0.04 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

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[43]Using the Municipal Register has the advantage that there are no concerns about the coverage of undocumented immigrants. Both local administrations and undocumented immigrants have strong incentives to have everyone registered. Using the SLFS data has the advantage that we can split the population by education groups.

[44]In Table A.6 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 estimator that relies on the past shares of migrants candidates to gain the legalization, at province level, to predict the actual share of immigrants affiliated to the social security.

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.

3.3.5 Discussion of the magnitude of the estimates

At first sight, the estimates of the employment effects may seem large. They mean that the employment rate in high-immigration locations which was around 65 percent in early 2005 would have increased by around 2 percentage points more than it did. This would have implied that by 2007, at the peak of the employment rate series, the employment rate in high-immigrant locations would have been almost 70 percent instead of the 68 percent it reached. Given that the share of immigrants in the social security system increased by around 4 percentage points in high-immigrant locations, this suggests that the policy had an important effect on employment.

To rationalize these numbers, however, it is worth understanding in more detail what the policy meant for the labor market. First, the policy was in part an important increase in enforcement against informality which directly destroyed jobs of both immigrants and natives in the informal sector. Second, the policy implied a substantial increase in the cost of hiring low-skilled immigrants who were previously undocumented. The average size of payroll taxes is 36 percent of wages, and this tax is, at least on paper, paid almost entirely by the firm. This means that if wages received by the 4 percent of immigrants who entered the social security system did not change, the cost of hiring these workers would have increased by at least 36 percent. This is likely to be a lower bound. With the legal right to work, newly legalized immigrants started to be covered by the Spanish labor market regulation. Among the many rules governing the Spanish labor market are minimum wages which were, presumably, more likely to be binding for the immigrants who had just gained work permits. The cost increase of hiring formerly undocumented immigrant workers who were paid *below* the minimum wage was larger than the 36 percent pay-roll tax rate, since we need to take into account also the jump to the minimum wage. While we do not know what were the wages of undocumented immigrants prior to the reform, it is clear that there is a high concentration of newly legalized immigrants at the minimum wage, as can be seen in Figure 12.[45]

Figure 12 goes around here

Given all these changes it is difficult to compute with confidence the value of the implied labor demand elasticity. Knowing this value would be useful as we would be able to compare our estimates to prior literature. One way to approximate what the implied labor demand elasticity is as follows. On the employment side, the policy reform decreased employment in high immigrant locations by around 2 percentage points.

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[45]There is a small number of observations with yearly wage levels below the yearly income implied by minimum wages. Most of these observations simply reflect minimum wage workers that worked a large fraction of the year (and hence are part of our wage sample), but not every single week.

On the other hand, the increase in the cost of hiring can be approximated by an increase of at least 36 percent for 4 percent of the labor force. This is an increase in the cost of around 1.5 percent (0.36 x 0.04). Hence, the implied labor demand elasticity is 2 divided by 1.5 which is equal to around 1.3, or that the inverse demand elasticity is around 0.8. This is likely to be a lower bound given that a large fraction of the newly legalized immigrant workers jumped to minimum wage levels and hence the cost increase was higher than this 1.5 percent. Despite the concerns mentioned above, it is quite clear that the estimate of the local labor demand elasticity that we obtain is comparable to existing estimates in the literature.

4   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 most high-skilled natives and immigrants. In all, each newly legalized immigrant increased tax revenues by at least 4,000 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. Finally, we provided evidence that this amnesty program did not lead to magnet effects. Thus, abstracting from magnet effects and other dynamic aspects, we show that amnesty programs likely have positive effects for the overall economy, but also have distributional consequences between different types of workers.

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5   Figures

Figure 1: **Social Security Registration and Immigration Reform**
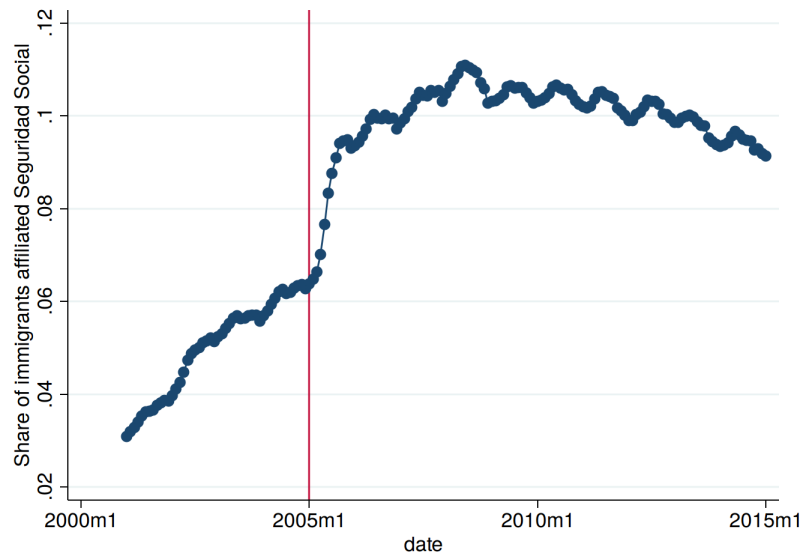
NOTE: This figure shows the (monthly) share of immigrants registered in the social security system. Source: Ministry of Labor and Social Security.

Figure 2: **Social Security Registration and Immigration Reform**
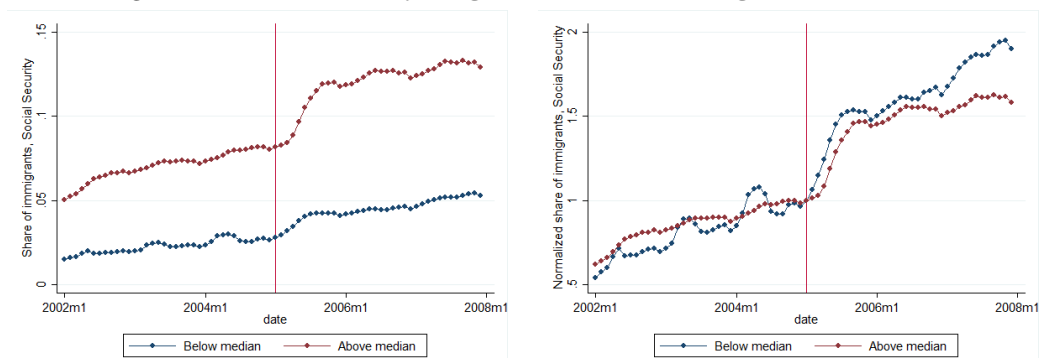
NOTE: The figure on the left shows the (monthly) share of immigrants registered in the social security system in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2005m1). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

Figure 3: **The Absence of Magnet effects**
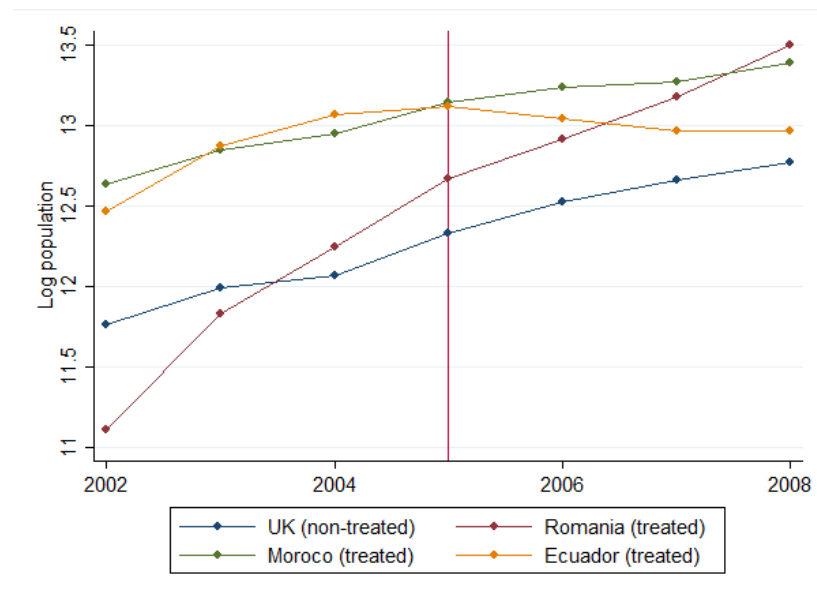
NOTE: This figures shows the evolution of the (log) stock of immigrants from the four top sending countries, three of which were affected by the policy change (labeled as "treated"), one of which was not (labeled as "non-treated"), using data from the Municipal Register.

Figure 4: **Newly Legalized Immigrants and Sector Switching**

Panel A

Panel B

Panel C
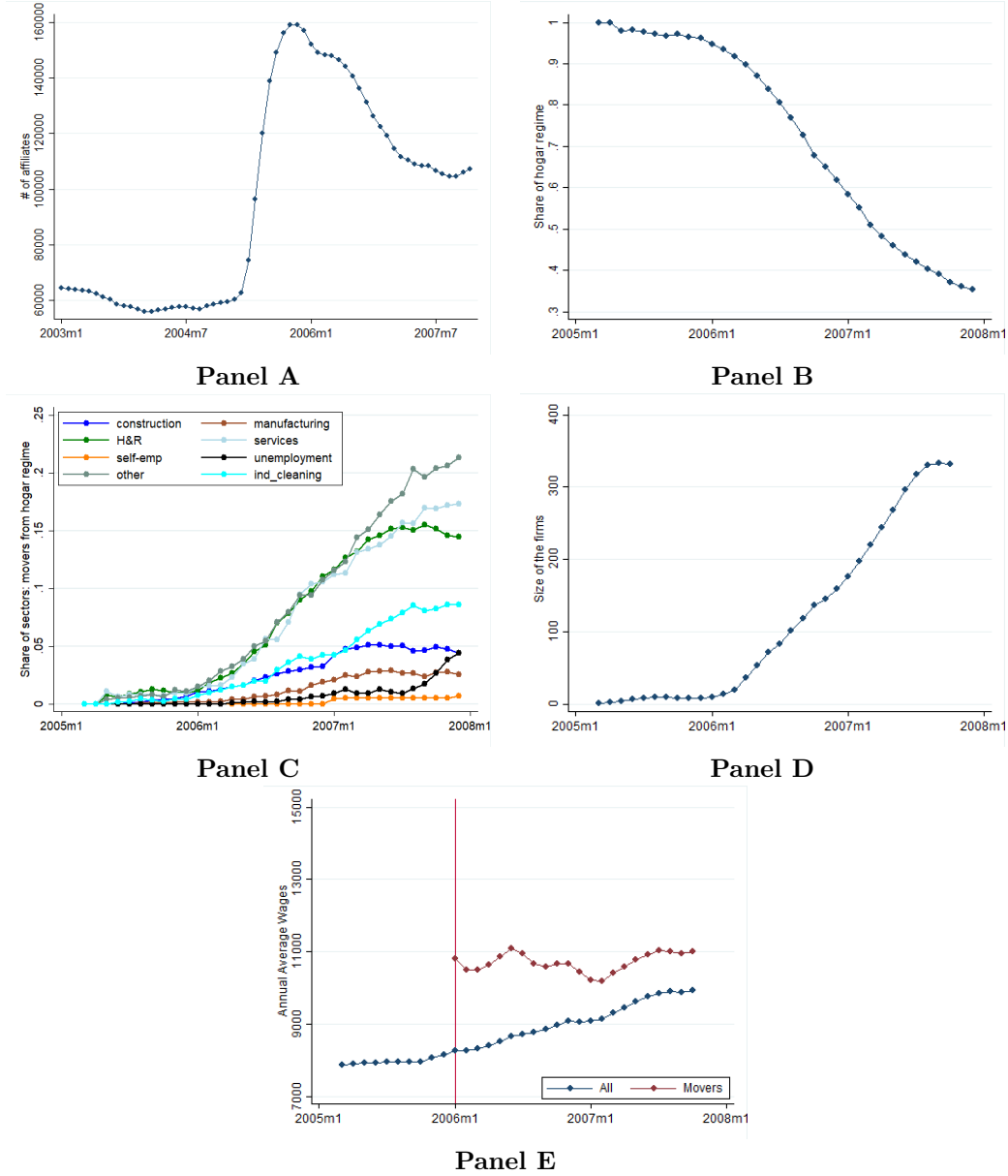
Panel D

Panel E

NOTE: Panel A shows the total number of affiliates in the housekeeping labor contract ("Regimen del Hogar"). Panel B shows the fraction of immigrants who remained in housekeeping services among the immigrants who entered the social security system with the legalization in the housekeeping sector and continued in the sector throughout the period. Panel C shows the sectors where immigrants, who entered the social security system with the legalization using housekeeping services contract types and continued in the social security throughout the period, move to. Panel D shows the average size of the firm where immigrants, who entered the social security system with the legalization using housekeeping services contract types and continued to work throughout the period, were working. Panel E shows the difference between annual average wages of legalized immigrants who move away from the housekeeping to others sectors of activity and the annual average wages of all workers legalized through the housekeeping sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

Figure 5: **Payroll Taxes and Immigration Reform**
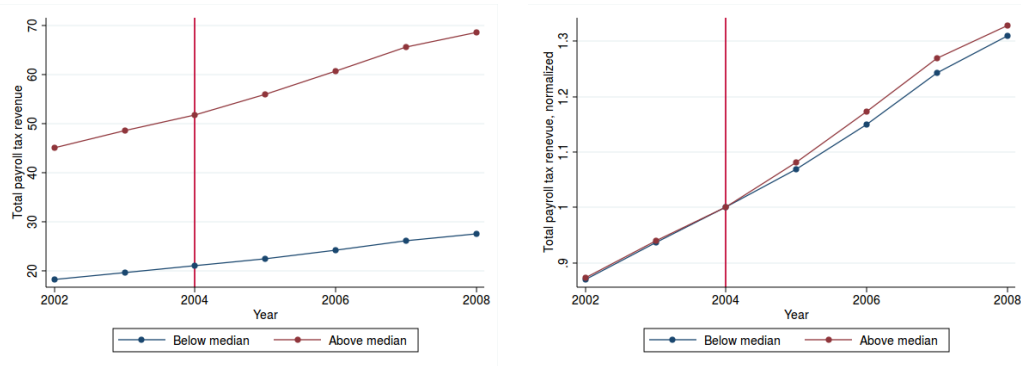
NOTE: The figure on the left shows the payroll-tax revenue in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2004). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

Figure 6: **Payroll Taxes and Immigration Reform, De-trended**
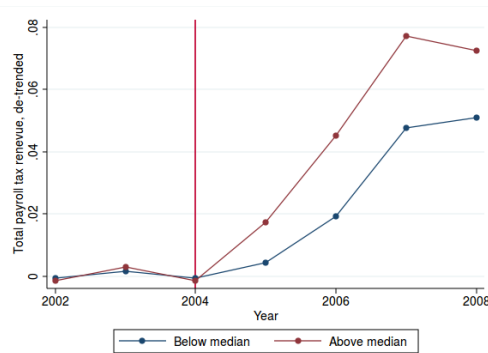
NOTE: This figure shows the de-trended series of total payroll-tax revenues. The vertical red line indicates the last period before the reform (2004). Source: Own elaboration based on Ministry of Labor and Social Security data.

Figure 7: **Payroll Taxes and Immigration Reform, Housekeeping Services**
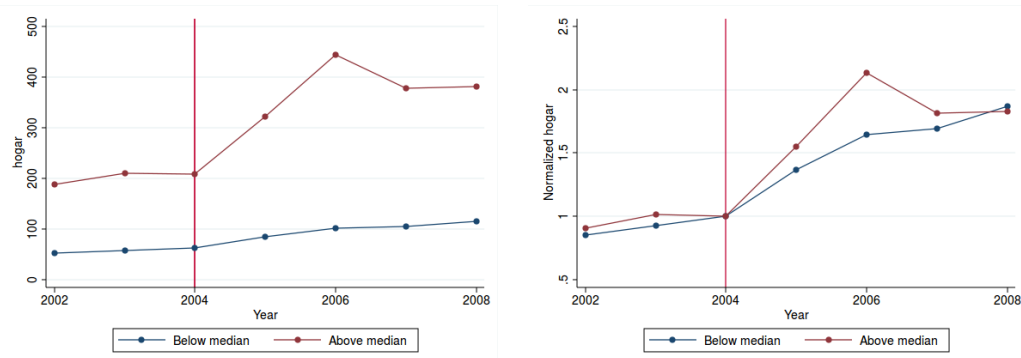
NOTE: The figure on the left shows the payroll-tax revenue from the housekeeping regime, in Spanish provinces above and below the median level of immigration (in 2002). The vertical red line indicates the last period before the reform (2004). The figure on the right normalizes the figure on the left, using the last observation before the policy intervention. Source: Ministry of Labor and Social Security.

Figure 8: **Payroll-Tax Revenues and Social Security Registration**
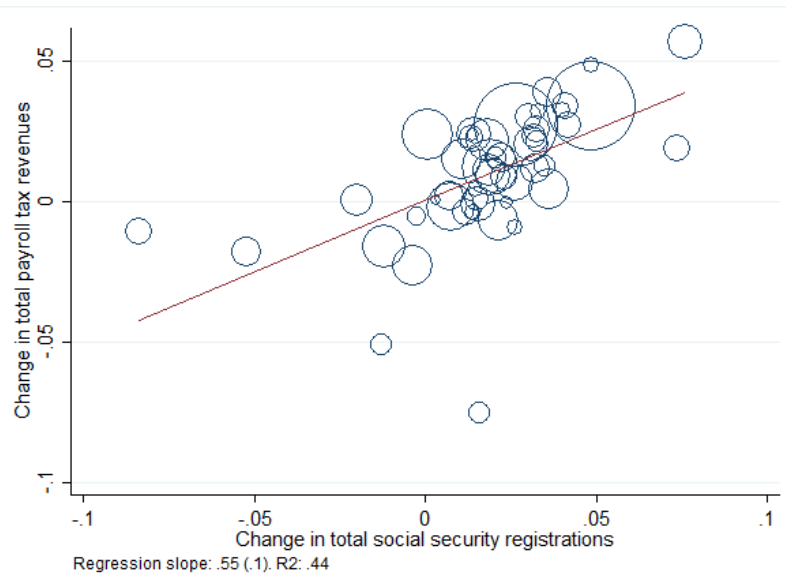
NOTE: This figure plots the de-trended change in total payroll-tax revenues against the de-trended change in total registration in the social security system between the periods 2002 to 2004 and 2005 to 2007. The size of the dots represents the population size of each province. Source: Authors' elaboration based on Ministry of Labor and Social Security data.

Figure 9: **Employment Rate**
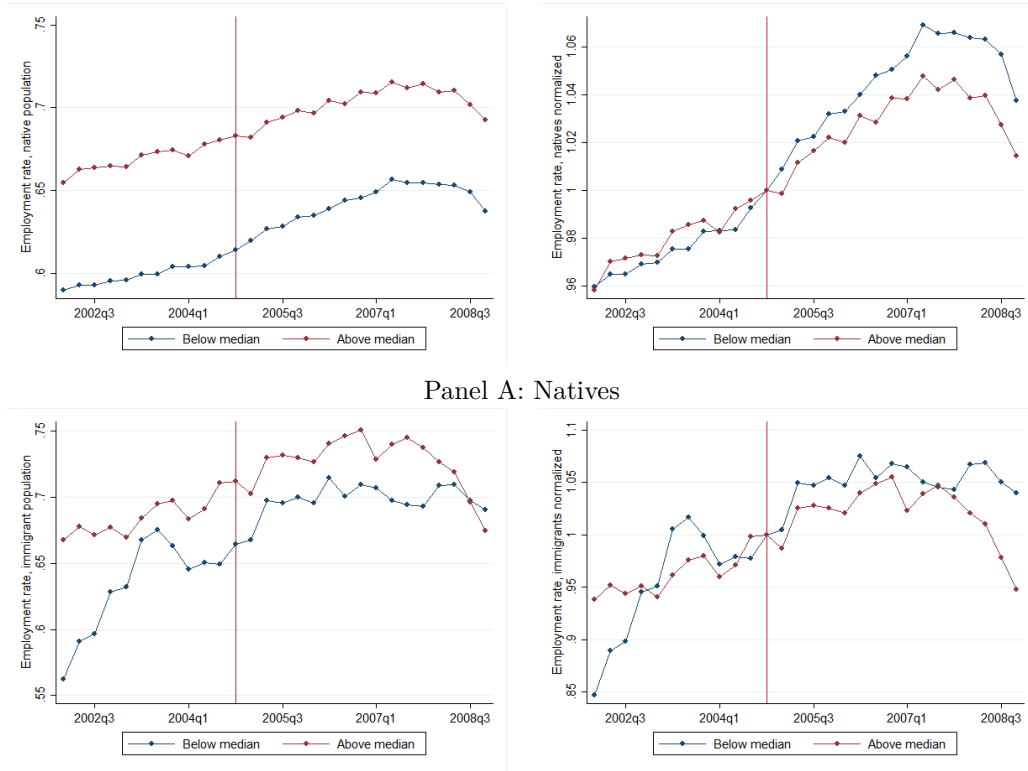
Panel A: Natives

Panel B: Immigrants

NOTE: The figure on the left shows the employment rate for Spanish and foreign-born workers, aged between 25 and 64 years old, 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.

Figure 10: **Composition-Adjusted Wages**
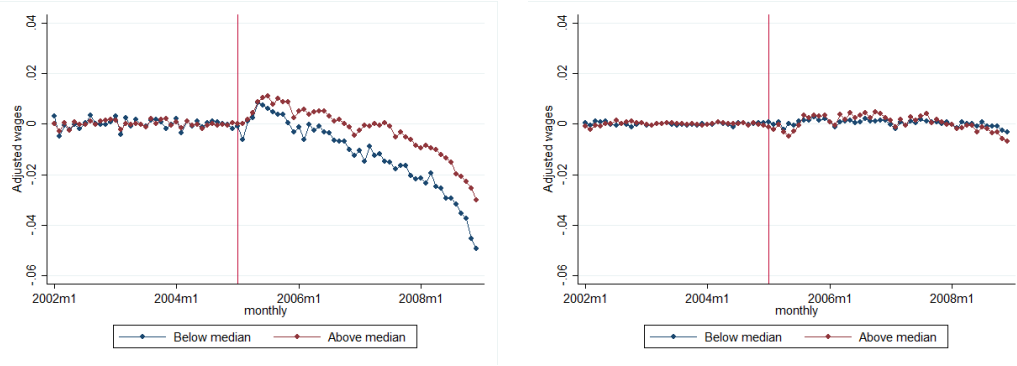
Panel A: Natives   Panel B**:** Immigrants

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.

Figure 11: **Spanish and Foreign-Born Population and the Immigration Reform**
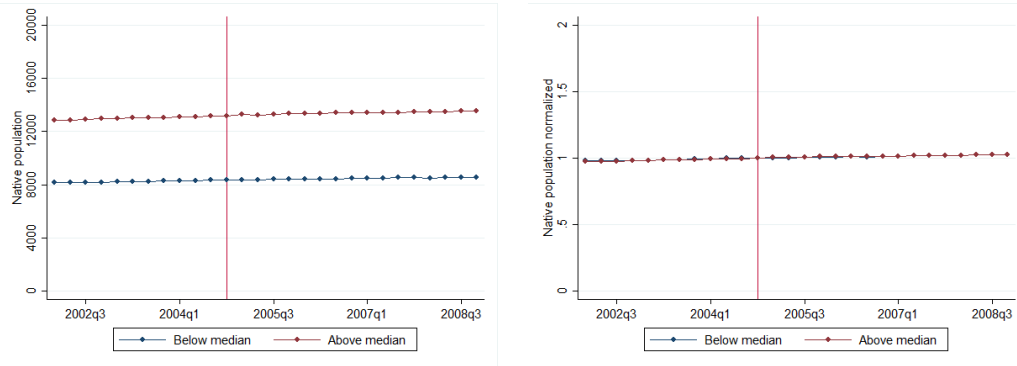
Panel A: Natives

Panel B: Immigrants

NOTE: The figures on the left show Spanish and foreign-born population, aged between 25 and 64 years old, 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.

Figure 12: **Newly Legalized Immigrant Wages and Minimum Wages**
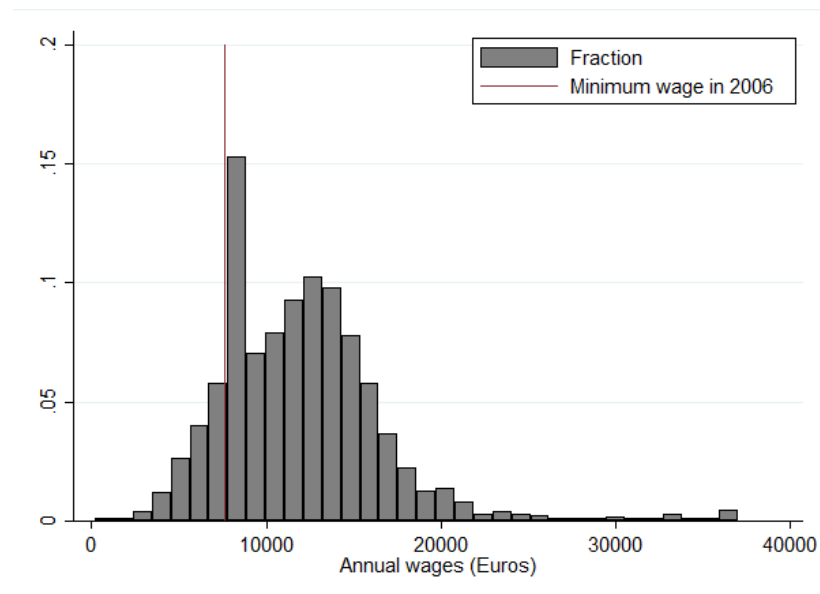
NOTE: This figure shows a histogram of the fraction of newly legalized immigrants in wage bins. The red vertical line indicates the minimum wage prevalent in 2006. Some workers can be paid below the minimum wage if they work less than the entire year or worked some months part-time. Source: MCVL

6 Tables

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

Province name	Immigrant share	Population	Legalization rate	Coast	Rank
Alicante	0.135	1595.2	0.803	1	1
Illes Balears	0.121	932.2	0.910	1	2
Girona	0.101	608.9	0.905	1	3
Madrid	0.092	5623.0	0.787	0	4
Tenerife	0.090	904.0	0.859	1	5
Málaga	0.086	1352.5	0.884	1	6
Almería	0.086	555.9	0.840	1	7
Las Palmas	0.082	965.3	0.805	1	8
Murcia	0.079	1248.1	0.880	1	9
Castellón	0.073	509.7	0.948	1	10
Barcelona	0.068	4979.4	0.843	1	11
Tarragona	0.067	642.7	0.849	1	12
Ávila	0.018	165.3	0.866	0	39
Salamanca	0.017	347.7	0.933	0	40
Asturias	0.016	1074.7	0.908	1	41
Cádiz	0.015	1148.3	0.783	1	42
Coruña	0.014	1116.4	0.785	1	43
Lugo	0.013	361.1	0.943	1	44
Sevilla	0.012	1770.8	0.820	0	45
Palencia	0.011	175.6	0.908	0	46
Badajoz	0.010	663.0	0.887	0	47
Jaén	0.009	649.5	0.813	0	48
Zamora	0.009	200.2	0.954	0	49
Córdoba	0.009	773.5	0.749	0	50
National average	0.042	42,133	0.836	–	

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. Legalization rate measures, at the province level, the ratio between workers legalized and applicants to the program. Source: Authors' elaboration based on Municipal Register and *Anuario Estadístico de la Inmigración* (Ministry of Labor and Social Security).

Table 2: **The effect of the policy on migrant stocks**

VARIABLES	(ln) Imm.	(ln) Imm.	(ln) Imm.	(ln) Imm.	Δ (ln) Imm.
Affected countries	-0.023	-0.011	0.000	-0.012	-0.001
	(0.134)	(0.127)	(0.113)	(0.087)	(0.047)
Observations	896	784	672	448	112
R-squared	0.978	0.979	0.982	0.991	0.000
Year FE	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes
Sample	2002-2009	2002-2008	2002-2007	2003-2006	2002-2007 de-trended

NOTE: This table estimates whether the effect of the legalization policy on the flow of immigrants from 112 countries of origin into Spain comparing countries that were affected by the policy (non-EU countries) and that were not affected by it (EU countries). The estimates in columns 1 to 4 show different windows around the policy change. Column 5 allows for a specific linear time trend in migrant flows prior to the policy change for each country of origin. Robust standard errors clustered at the country of origin level reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table 3: **Estimates of the Change in Payroll-Tax Revenues per Newly Legalized Immigrant**

	General Reg.	Self.emp.	Agricult.	Sea	Coal	Housekeeping	Accident	Unemp.	Total
Δ 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

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 pre-change 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 4: **Estimates of the Effect of the Immigration Reform on Employment and Wages**

	Panel A: Total Employment (Formal + Informal)						
		Δ Employment					
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Δ Immigrants	-0.495***	-0.254	-0.242	-0.371*	0.117	-0.405**	0.163*
in social security/pop.	(0.181)	(0.277)	(0.185)	(0.211)	(0.259)	(0.182)	(0.094)
Observations	50	50	50	50	50	50	50
R-squared	0.069	0.015	0.021	0.050	0.006	0.077	0.054
	Panel B: Formal Employment						
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Δ Immigrants	0.471***	-0.117	0.588***	-0.0519	-0.0650**	0.565***	0.0229***
in social security/pop.	(0.164)	(0.132)	(0.0898)	(0.144)	(0.0294)	(0.0874)	(0.00533)
Observations	50	50	50	50	50	50	50
R-squared	0.227	0.021	0.761	0.004	0.198	0.754	0.388
	Panel C: Formal Employment, females						
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Δ Immigrants	0.0895	-0.135***	0.224***	-0.0917*	-0.0430**	0.214***	0.0104***
in social security/pop.	(0.0564)	(0.0453)	(0.0396)	(0.0486)	(0.0196)	(0.0379)	(0.00299)
Observations	50	50	50	50	50	50	50
R-squared	0.072	0.162	0.710	0.090	0.195	0.697	0.249
	Panel D: Wages						
		Δ log wages					
	Δ Total log wages	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Δ Immigrants	0.244**	0.309***	-0.199	0.296***	0.235	-0.263	1.526
in social security/pop.	(0.106)	(0.114)	(0.260)	(0.101)	(0.227)	(0.272)	(0.920)
Observations	50	50	50	50	50	50	50
R-squared	0.143	0.204	0.015	0.201	0.021	0.023	0.029

NOTE: This table estimates the effect of immigrant regularization on employment. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change 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.

Table 5: **Native Selection**

	Always working (1)	Enter 2005 vs. Enter 2004 (2)	Lost job 2005 vs. Lost job 2004 (3)
	Δ (ln) wages low skilled natives		
Δ Immigrants	-0.064	0.739**	-0.388
in social security/pop.	(0.102)	(0.324)	(0.426)
Observations	50	50	50
R-squared	0.011	0.073	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 6: **Estimates of the Effect of the Immigration Reform on Internal Migration**

	Total	Low Skilled	High Skilled	Population
	Δ Immigrant population share			Δ Share of LS
Δ Immigrants	-0.500**	-0.535**	0.035	-0.392
in social security/pop.	(0.213)	(0.211)	(0.089)	(0.346)
Observations	50	50	50	50
R-squared	0.062	0.089	0.003	0.052

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.

Appendix

A Details on the Identification Strategy

A simple way to discuss our identification strategy is to follow the terminology of the randomized control trial's literature. The policy's treatment is "giving the right to undocumented immigrants to work". For undocumented immigrants, the compliance with the policy is to do the necessary paper work to obtain the work permit. There are good reasons to believe that the difference between the treated group and the group of compliants is negligible, as we explained in Section 2.4. However, it is worth explaining how we can compute the Intention To Treat (ITT) and Local Average Treatment Effect (LATE) using the data at our disposal. Crucially, and unlike it is standard in the randomized control trial's literature, we measure with error who belongs to the treatment group, while we measure without error the group of compliants. This data limitation implies that our ITT estimates will be downward biased and our LATE estimates will be upward biased. This explains why in the main text we prefer to report the average treatment effect.

To compute ITT estimates we can use the following equation:

$$\Delta \widehat{Y}_c = \alpha + \delta \frac{\text{Imm Candidates}_c}{\text{Pop}_c} + \gamma X_c + \varepsilon_c \tag{4}$$

where Imm Candidates$_c$ is the number of undocumented immigrants who were entitled to apply to the legalization program. There is not a data set available that directly measures Imm Candidates$_c$ without error (or in words, no one knew exactly how many immigrants had been in Spain for at least six months *and* were working). However, we can approximate this number by combining data from the Municipal Register, the SLFS and the social security. The Municipal Register has good information on the total number of immigrants (working and not working) by country of origin. The social security has information on the immigrants registered in the social security system by country of origin just prior to the legalization process. In order to be part of the program, immigrants had to have an employer willing to sponsor them, which essentially means that they had to be employed. From the SLFS we can compute the activity rate of immigrants from non-EU countries at the province level. We can then estimate the number of immigrants from non-EU countries that were in the labor market in each province. We can then subtract from this number the number of non-EU immigrants that were registered in the social security system. From this, we obtain an estimate of the number of immigrants who were candidates to be part of the program (denoted by Imm Candidates$_c$). Note, however, that we can only measure "Imm Candidates$_c$" with error since we assign the activity rate of both documented and undocumented non-EU immigrants to undocumented non-EU immigrants.

Measuring "Imm Candidates$_c$" with error *is* a problem for the standard ITT and local average treatment effect (LATE) estimates. Without measurement error, δ estimates the intention to treat. This is what usually happens in RCTs since the researcher randomly assigns the treatment to a group of individuals and

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this is what is best measured. With measurement error, we obtain a downward biased estimate of δ because of attenuation bias.[46]

To estimate the local average treatment effect we can use a two stage least square procedure, where the first stage is given by:

$$\Delta \frac{\widehat{\text{Imm Soc Sec}}_c}{\text{Pop}_c} = \alpha + \eta \frac{\text{Imm Candidates}_c}{\text{Pop}_c} + \varepsilon_c \tag{5}$$

In this case, η is the first stage estimate. If there is no measurement error and $\eta < 1$ then this means that the compliance rate is below 100 percent. In this case, we can use this first stage regression to estimate β in equation 3 and obtain an unbiased LATE estimate, which essentially scales up the least squares estimate. If there is measurement error, then $\eta < 1$ either because of non-compliance or because of attenuation bias in the first stage. In the latter case, the LATE estimate would be upward biased.

Figure A.1: **First stage results**

Note: Regression coefficient (se): .744 (.1605).
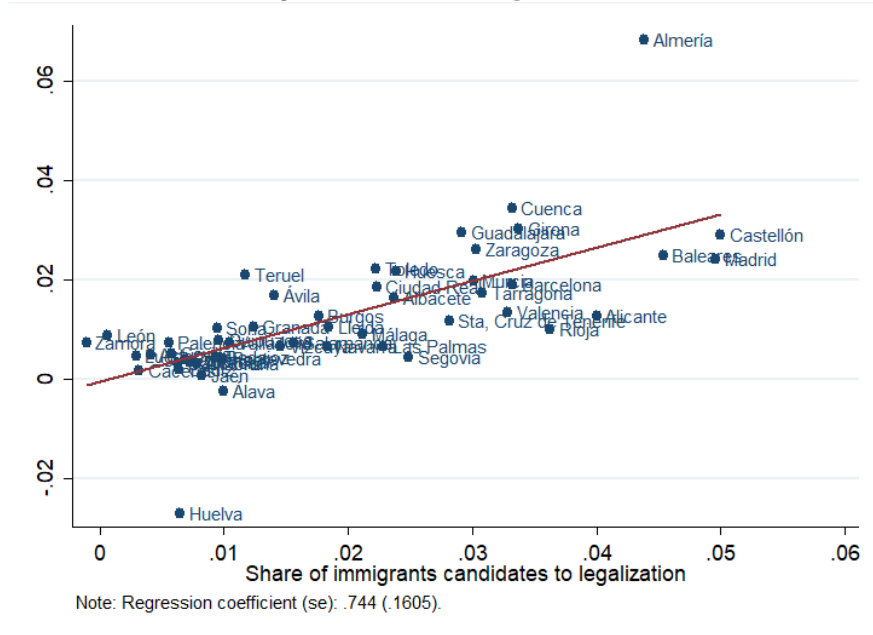
NOTE: This graph shows the first-stage regression of the change in social security affiliations (measured as deviations from linear province-specific trends) on our estimates of the share of immigrants who were candidates to obtain work permits. These estimates are based on the number of immigrants from candidate countries in the Municipal Register, the employment rate of immigrants from candidates countries from the SLFS, and the amount of immigrants from these countries already registered in the social security.

Figure A.1 shows the first-stage regression given by Equation 5. Our estimate of η is smaller than 1. This suggests that either we estimated $\frac{\text{Imm Candidates}_c}{\text{Pop}_c}$ with error (most likely), or that not everyone who could obtain work permits obtained them (something less likely given the effort of Spanish authorities).

All these considerations explain why we opted to report in the main text the estimate β using ordinary least squares as shown in equation (3). This estimate is the effect of the treatment. We report in Appendix

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[46]Under the assumption that measurement error is classical, which we think is a reasonable assumption.

the ITT estimates, and the LATE estimates.

B Robustness Checks

B.1 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 largest 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 reduced form and 2SLS as discussed in Appendix Section A. 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.1: **Payroll-tax revenue estimate**s

	General Reg.	Self. emp.	Agricult.	Sea	Coal	Housekeeping	Accident	Unemp.	Total
Panel A: Baseline									
Δ 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)									
Δ Immigrants	2,509***	97.06**	162.1**	-0.796	52.48	145.7***	-51.39	401.7*	3,316***
in social security/pop.	(869.5)	(39.15)	(62.81)	(22.07)	(41.88)	(51.78)	(36.69)	(202.0)	(880.3)
Observations	46	46	46	46	46	46	46	46	46
R-squared	0.294	0.068	0.276	0.000	0.019	0.423	0.062	0.085	0.445
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)									
Δ 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: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)									
Δ Immigrants	2,470***	28.09	77.88***	0.156	-0.307	139.5***	-5.177	-508.2***	2,202***
in social security/pop.	(412.0)	(28.76)	(25.02)	(10.66)	(9.400)	(10.55)	(16.91)	(133.8)	(382.2)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.641	0.155	0.249	0.159	0.088	0.889	0.168	0.664	0.615
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)									
Δ Immigrants	5,834***	66.33	183.9***	0.369	-0.725	329.4***	-12.23	-1,200***	5,201***
in social security/pop.	(1,240)	(61.89)	(41.74)	(23.61)	(20.86)	(60.87)	(36.83)	(421.3)	(981.5)
Observations	50	50	50	50	50	50	50	50	50
F-test of excluded instruments	37.980	37.980	37.980	37.980	37.980	37.980	37.980	37.980	37.980

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 pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix A. 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.2: **Estimates of the effect of the reform on employment**

	Δ Total Emp.	Δ Employment					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	-0.495***	-0.254	-0.242	-0.371*	0.117	-0.405**	0.163*
in social security/pop.	(0.181)	(0.277)	(0.185)	(0.211)	(0.259)	(0.182)	(0.0939)
Observations	50	50	50	50	50	50	50
R-squared	0.069	0.015	0.021	0.050	0.006	0.077	0.054
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	-0.529***	-0.348	-0.182	-0.330	-0.0175	-0.276	0.094
in social security/pop.	(0.190)	(0.253)	(0.149)	(0.251)	(0.228)	(0.172)	(0.133)
Observations	46	46	46	46	46	46	46
R-squared	0.066	0.025	0.011	0.036	0.000	0.036	0.024
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
Δ Immigrants	-0.539**	-0.382	-0.157	-0.604***	0.222	-0.403**	0.245**
in social security/pop.	(0.206)	(0.254)	(0.164)	(0.207)	(0.305)	(0.196)	(0.120)
Observations	50	50	50	50	50	50	50
R-squared	0.143	0.163	0.078	0.292	0.068	0.168	0.282
Panel D: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)							
Δ Immigrants	-0.404	0.144	-0.548**	-0.288	0.432*	-0.569***	0.021
in social security/pop.	(0.247)	(0.279)	(0.211)	(0.189)	(0.223)	(0.167)	(0.105)
Observations	50	50	50	50	50	50	50
R-squared	0.127	0.140	0.196	0.214	0.147	0.281	0.179
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)							
Δ Immigrants	-0.663*	0.235	-0.899**	-0.473*	0.708*	-0.934***	0.035
in social security/pop.	(0.384)	(0.449)	(0.417)	(0.273)	(0.392)	(0.335)	(0.159)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	25.460	25.460	25.460	25.460	25.460	25.460	25.460

NOTE: This table estimates the effect of immigrant regularization on employment. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix A. 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 formal employment**

	Δ Total Emp.	Δ Employment - All					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	0.423***	-0.126	0.549***	-0.0656	-0.0602**	0.527***	0.0219***
in social security/pop.	(0.152)	(0.122)	(0.0839)	(0.132)	(0.0274)	(0.0817)	(0.00505)
Observations	50	50	50	50	50	50	50
R-squared	0.213	0.027	0.762	0.008	0.200	0.755	0.391
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.449**	-0.136	0.585***	-0.108	-0.0282**	0.564***	0.0204***
in social security/pop.	(0.193)	(0.135)	(0.108)	(0.129)	(0.0123)	(0.105)	(0.00677)
Observations	46	46	46	46	46	46	46
R-squared	0.206	0.028	0.741	0.020	0.055	0.738	0.298
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
Δ Immigrants	0.494**	-0.00464	0.498***	0.0465	-0.0512***	0.477***	0.0217***
in social security/pop.	(0.197)	(0.134)	(0.0944)	(0.135)	(0.0160)	(0.0913)	(0.00562)
Observations	50	50	50	50	50	50	50
R-squared	0.274	0.192	0.803	0.213	0.399	0.800	0.439
Panel D: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)							
Δ Immigrants	0.0472*	-0.0532**	0.100***	-0.0300	-0.0232***	0.0958***	0.00457***
in social security/pop.	(0.0273)	(0.0225)	(0.0125)	(0.0218)	(0.00602)	(0.0120)	(0.00115)
Observations	50	50	50	50	50	50	50
R-squared	0.162	0.338	0.805	0.327	0.448	0.803	0.210
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)							
Δ Immigrants	0.117*	-0.132**	0.249***	-0.0745	-0.0575***	0.238***	0.0113***
in social security/pop.	(0.0624)	(0.0548)	(0.0315)	(0.0504)	(0.0183)	(0.0304)	(0.00271)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	46.150	46.150	46.150	46.150	46.150	46.150	46.150

NOTE: This table estimates the effect of immigrant regularization on formal employment. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix A. 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 formal employment, females**

		Δ Employment - Women					
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	0.0895	-0.135***	0.224***	-0.0917*	-0.0430**	0.214***	0.0104***
in social security/pop.	(0.0564)	(0.0453)	(0.0396)	(0.0486)	(0.0196)	(0.0379)	(0.00299)
Observations	50	50	50	50	50	50	50
R-squared	0.072	0.162	0.710	0.090	0.195	0.697	0.249
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.108	-0.107**	0.215***	-0.0904*	-0.0165	0.205***	0.0100**
in social security/pop.	(0.0765)	(0.0488)	(0.0536)	(0.0451)	(0.0110)	(0.0510)	(0.00417)
Observations	46	46	46	46	46	46	46
R-squared	0.086	0.100	0.634	0.088	0.038	0.620	0.189
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
Δ Immigrants	0.0930	-0.101**	0.194***	-0.0652	-0.0361***	0.184***	0.0104***
in social security/pop.	(0.0688)	(0.0444)	(0.0432)	(0.0423)	(0.0121)	(0.0409)	(0.00357)
Observations	50	50	50	50	50	50	50
R-squared	0.160	0.331	0.776	0.332	0.378	0.768	0.257
Panel D: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)							
Δ Immigrants	0.221***	-0.000490	0.221***	0.0369	-0.0374***	0.211***	0.0104***
in social security/pop.	(0.0773)	(0.0651)	(0.0381)	(0.0635)	(0.00790)	(0.0372)	(0.00204)
Observations	50	50	50	50	50	50	50
R-squared	0.229	0.192	0.700	0.218	0.528	0.695	0.413
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)							
Δ Immigrants	0.548***	-0.00122	0.549***	0.0916	-0.0929***	0.523***	0.0258***
in social security/pop.	(0.170)	(0.152)	(0.0653)	(0.149)	(0.0265)	(0.0630)	(0.00515)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	46.150	46.150	46.150	46.150	46.150	46.150	46.150

NOTE: This table estimates the effect of immigrant regularization on formal employment among females. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix A. 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 wages**

	Δ Total log wages	Δ log wages					
		Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	0.244**	0.309***	-0.199	0.296***	0.235	-0.263	1.526
in social security/pop.	(0.106)	(0.114)	(0.260)	(0.101)	(0.227)	(0.272)	(0.920)
Observations	50	50	50	50	50	50	50
R-squared	0.143	0.204	0.015	0.201	0.021	0.023	0.029
Panel B: Including women							
Δ Immigrants	0.213*	0.265**	-0.451	0.257**	0.226	-0.617*	0.751
in social security/pop.	(0.112)	(0.125)	(0.298)	(0.122)	(0.200)	(0.312)	(0.740)
Observations	50	50	50	50	50	50	50
R-squared	0.097	0.129	0.059	0.131	0.024	0.097	0.016
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.076	0.129*	-0.275	0.149*	-0.0366	-0.355	2.137
in social security/pop.	(0.0758)	(0.0757)	(0.247)	(0.0746)	(0.257)	(0.277)	(1.285)
Observations	46	46	46	46	46	46	46
R-squared	0.017	0.050	0.024	0.061	0.000	0.034	0.042
Panel D: All controls (pol. alignment; coastal dummies; construction sector pre-reform)							
Δ Immigrants	0.231**	0.309***	-0.417	0.292***	0.390	-0.523	2.597
in social security/pop.	(0.098)	(0.104)	(0.292)	(0.096)	(0.263)	(0.330)	(1.615)
Observations	50	50	50	50	50	50	50
R-squared	0.259	0.342	0.143	0.317	0.154	0.165	0.080
Panel E: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)							
Δ Immigrants	0.364***	0.452***	-0.216	0.387***	0.818***	-0.286	1.969
in social security/pop.	(0.101)	(0.0966)	(0.335)	(0.0924)	(0.247)	(0.371)	(1.457)
Observations	50	50	50	50	50	50	50
R-squared	0.340	0.436	0.112	0.370	0.245	0.122	0.052
Panel F: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)							
Δ Immigrants	0.409***	0.508***	-0.242	0.435***	0.919***	-0.321	2.211
in social security/pop.	(0.134)	(0.138)	(0.341)	(0.124)	(0.305)	(0.373)	(1.437)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	63.610	63.610	63.610	63.610	63.610	63.610	63.610

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. ITT and LATE estimates are explained in detail in Appendix A. 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: **Estimates of the effect of the reform on internal migration**

	Δ Immigrant population share			Δ Share of LS
	Total	Low Skilled	High Skilled	Population
Panel A: Baseline				
Δ Immigrants	-0.500**	-0.535**	0.035	-0.392
in social security/pop.	(0.213)	(0.211)	(0.089)	(0.346)
Observations	50	50	50	50
R-squared	0.062	0.089	0.003	0.052
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)				
Δ Immigrants	-0.360**	-0.368*	0.007	-0.071
in social security/pop.	(0.176)	(0.216)	(0.113)	(0.198)
Observations	46	46	46	46
R-squared	0.030	0.043	0.000	0.002
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)				
Δ Immigrants	-0.391*	-0.504**	0.113	-0.566
in social security/pop.	(0.206)	(0.243)	(0.113)	(0.359)
Observations	50	50	50	50
R-squared	0.106	0.152	0.176	0.230
Panel D: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)				
Δ Immigrants	-0.744***	-0.658***	-0.087	-0.769***
in social security/pop.	(0.246)	(0.210)	(0.095)	(0.241)
Observations	50	50	50	50
R-squared	0.234	0.241	0.170	0.370
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)				
Δ Immigrants	-1.221**	-1.079***	-0.142	-1.261***
in social security/pop.	(0.494)	(0.402)	(0.160)	(0.464)
Observations	50	50	50	50
F-test of excluded instruments	25.460	25.460	25.460	25.460

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 pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix A. 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.7: **Native Selection**

	Δ (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			
Δ Immigrants	-0.064	0.739**	-0.388
in social security/pop.	(0.102)	(0.324)	(0.426)
Observations	50	50	50
R-squared	0.011	0.073	0.016
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)			
Δ Immigrants	0.076	1.006**	-0.394
in social security/pop.	(0.094)	(0.389)	(0.461)
Observations	46	46	46
R-squared	0.014	0.097	0.013
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)			
Δ Immigrants	0.004	0.442	-0.949**
in social security/pop.	(0.103)	(0.363)	(0.448)
Observations	50	50	50
R-squared	0.383	0.209	0.121
Panel D: Reduced form (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)			
Δ Immigrants	-0.019	0.352	-0.453
in social security/pop.	(0.108)	(0.363)	(0.600)
Observations	50	50	50
R-squared	0.383	0.201	0.070
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)			
Δ Immigrants	-0.021	0.367	-0.440
in social security/pop.	(0.111)	(0.342)	(0.523)
Observations	50	50	50
F-test of excluded instruments	54.510	63.690	66.770

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. ITT and LATE estimates are explained in detail in Appendix A. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

B.2 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.8. The sum of the point estimates in this table should coincide with the estimate in column four of Table A.2. 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.8: **Estimates of the Effect of the Immigration Reform on Employment by Sectors**

|	Δ Employment Native Low Skilled		
	High-immigrant sectors	Low-immigrant sectors	Public administration
Panel A: Baseline			
Δ Immigrants	-0.288	-0.249	0.167
in social security/pop.	(0.186)	(0.182)	(0.117)
Observations	50	50	50
R-squared	0.031	0.028	0.037
Panel B: Without 4 main provinces (Mad., Bcn., Val., Sev)			
Δ Immigrants	-0.309*	-0.248*	0.229**
in social security/pop.	(0.166)	(0.141)	(0.093)
Observations	46	46	46
R-squared	0.037	0.031	0.072
Panel C: All controls (pol. alignment; coastal dummies; construction sector pre-reform)			
Δ Immigrants	-0.503**	-0.223	0.125
in social security/pop.	(0.194)	(0.140)	(0.089)
Observations	50	50	50
R-squared	0.250	0.213	0.291
Panel D: Reduced form all controls (pol. alignment; coastal dummies; construction sector pre-reform) (ITT)			
Δ Immigrants	-0.382**	0.049	0.062
in social security/pop.	(0.172)	(0.158)	(0.087)
Observations	50	50	50
R-squared	0.233	0.194	0.279
Panel E: 2SLS all controls (pol. alignment; coastal dummies; construction sector pre-reform) (LATE)			
Δ Immigrants	-0.625**	0.079	0.102
in social security/pop.	(0.251)	(0.247)	(0.129)
Observations	50	50	50
F-test of excluded instruments	25.250	25.250	25.250
	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. ITT and LATE estimates are explained in detail in Appendix A. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

B.3 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.2: **Spanish and Foreign-Born Population and the Immigration Reform, Natives and Immigrants**
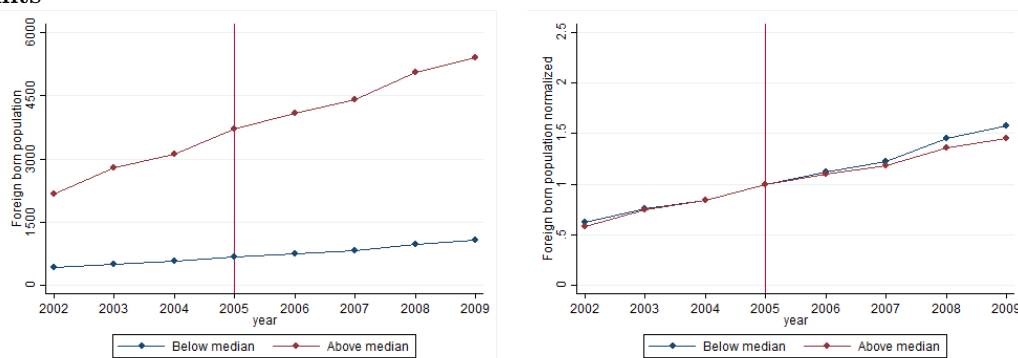
NOTE: The figures on the left show Spanish and foreign-born population in Spanish provinces above and below the median level of immigration in 2002. The vertical red line indicates the last period before the reform (2005). The Municipal Register reports about the number of individuals residing in municipalities 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.

B.4 Newly Legalized Immigrants and Sector Switching

Figure A.3: **Newly Legalized Immigrants and Sector Switching: Construction**

Panel A

Panel B
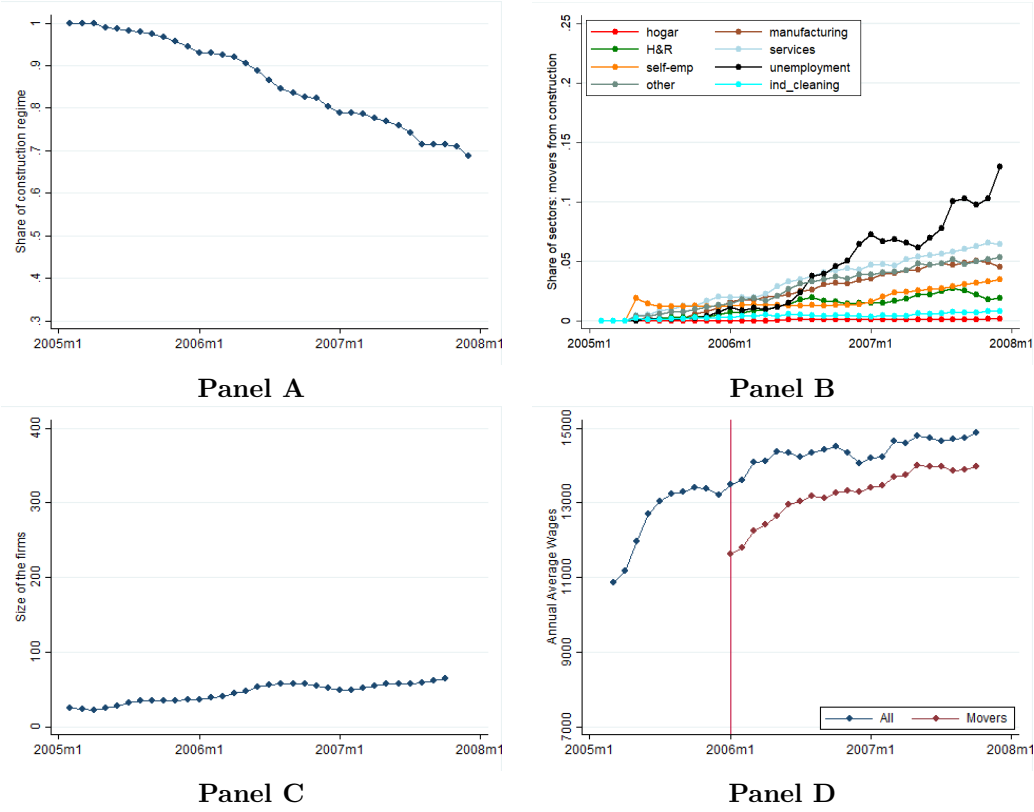
Panel C

Panel D

Panel A shows the fraction of immigrants who remained in construction among the immigrants who entered the social security system with the legalization in the construction sector and continued in the sector throughout the period. Panel B shows the sectors where immigrants, who entered into the social security system with the legalization and a contract in the construction sector and continued in the social security throughout the period, move to. Panel C shows the average size of the firm where immigrants, who entered the social security system with the legalization through in the construction sector and continued to work throughout the period, were working. Panel D shows the difference between annual average wages of legalized immigrants who move away from the construction to others sectors of activity and the annual average wages of all workers legalized through the construction sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

Figure A.4: **Newly Legalized Immigrants and Sector Switching: H&R**

Panel A

Panel B
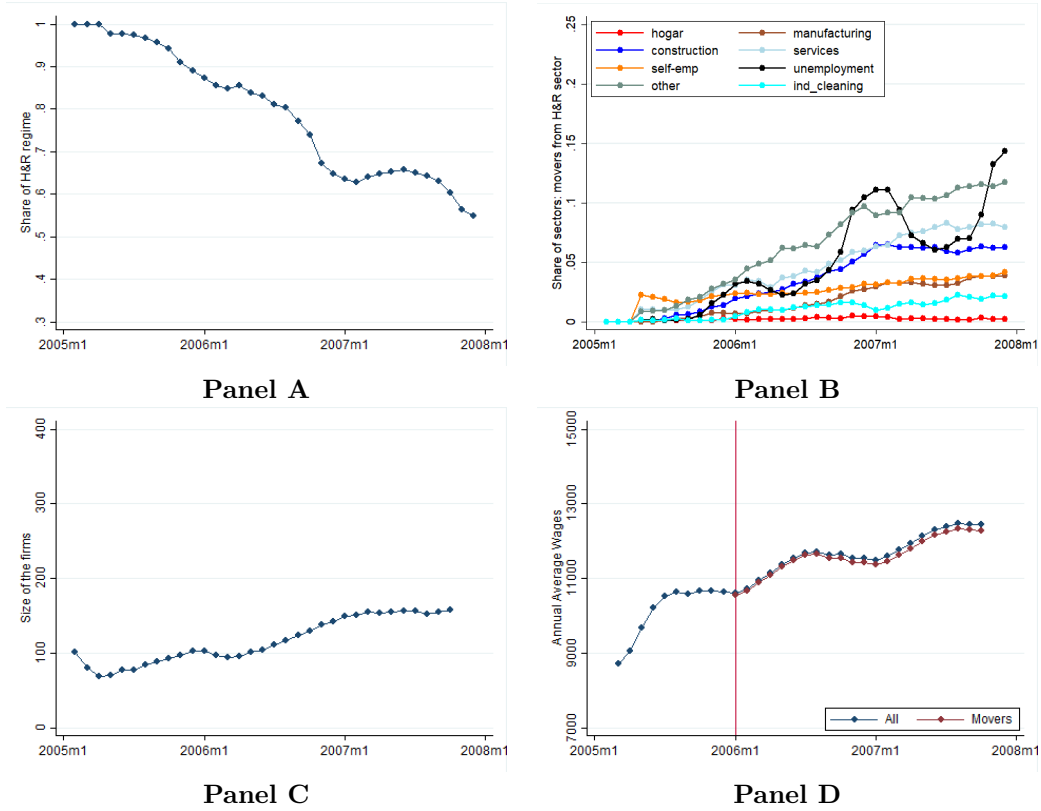
Panel C

Panel D

Panel A shows the fraction of immigrants who remained in H&R among the immigrants who entered the social security system with the legalization in the H&R sector and continued in the sector throughout the period. Panel B shows the sectors where immigrants, who entered into the social security system with the legalization and a contract in the H&R sector and continued in the social security throughout the period, move to. Panel C shows the average size of the firm where immigrants, who entered the social security system with the legalization in the H&R sector and continued to work throughout the period, were working. Panel D shows the difference between annual average wages of legalized immigrants who move away from the H&R to others sectors of activity and the annual average wages of all workers legalized through the H&R sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

C Work Inspections

Figure A.5: Number of inspections related to foreign workers

Source: Ministry of Labor and Social Security.

D 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.