Understanding the Effects of Granting Work Permits to Undocumented Immigrants

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July 18, 2022

Abstract

We study 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 estimate that the policy did not lead to magnet effects. We then show immigrant formal employment increased, while that of natives was not affected. However, there was a decrease in informal employment of both native and immigrant low-skilled workers. We document that tax revenues increased by more than 4,000 euros per legalized immigrant – with no evidence of increased public expenditures. We display evidence that the policy increased labor market opportunities for immigrants.

JEL Classification codes: F22, J31, J42, J46, J61, R11.

Keywords: Immigration, undocumented immigrants, public policy evaluation.

Acknowledgments: We are very thankful for Yan Hu's research assistance. This paper has benefited from discussions with Manuel Arellano, Samuel Bentolila, Paula Bustos, Susanna Esteban, Giovanni Facchini, Francesco Fasani, Osea Giuntella, Libertad González, Laura Hospido, Gordon Hanson, Gianmarco León, John Mclaren, Cristina Riquelme, and Sandra Rozo. We would also like to thank the audience at a number of conferences and seminars. Elias and Monras kindly acknowledge financial support from the W.E. Upjohn Institute for Employment Research for this project. Monras acknowledges support from the Fundación Ramon Areces and from the Spanish Ministry of Science and Innovation, through the Severo Ochoa Programme for Centres of Excellence in R&D (CEX2019-000915-S). Monras thanks the hospitality of the IES section at Princeton University. Vázquez-Grenno acknowledges financial support from the Spanish Ministry of Science and Innovation (PID2019-109813RB-I00 and PID2019-108265RB-I00) and the Catalan Government (Project No. 2017SGR796). All remaining errors are our own.

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 23 percent of all immigrants.[2] These large numbers of undocumented immigrants have led recent U.S. administrations, not without controversy, to consider either legalizing the status of these immigrants or deporting many of them to their countries of origin. However, despite its importance highlighted, among others, in these public debates, not enough is known about the effects of amnesty programs that grant work permits to unauthorized immigrants.

This paper fills this gap by analyzing a unique quasi-experiment in Spain that gave legal working status to around 600,000 working-age immigrants in 2005. There were two main conditions to get a working permit: first, to be offered a legal working contract by an employer for at least six months; second, to be able to demonstrate residence in Spain since August 2004. We provide a novel theoretical framework to analyze amnesty programs and use several datasets to measure the effects of the policy on a diverse set of outcome variables, some of them understudied in the literature: migration flows, employment in the formal and the informal sector, wages in the formal sector, payroll and income taxes, and career paths.

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. Many of these newly arrived immigrants lacked work permits. According to Domingo and Recaño (2005), close to 1 million immigrants –in a country of around 43 million inhabitants– were undocumented by 2004. However, their legal status changed abrutply in February 2005, when the newly elected government of the Socialist Party (PSOE) implemented a law that granted work permits to most of them. As a result, the share of foreign workers registered in the social security system increased by around 3 percentage points overall. This was attained thanks to the incentives of immigrants to take part of the legalization process and to the efforts of the Spanish authorities in enforcing and monitoring the implementation of the policy.[3] For example, inspections related with foreign workers increased by an astonishing 132 percent, something that was widely announced at the time.[4]

The policy change was quite unexpected. PSOE had won the general election in Spain only

[1] In this paper, "undocumented" immigrants refers to workers that were born outside the country in which they reside and that do not have work permit in the host country.

[2] See https://www.pewresearch.org/fact-tank/2020/08/20/key-findings-about-u-s-immigrants/ (accessed in July 2022).

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

[4] For a news report on the policy, see https://elpais.com/elpais/2005/05/07/actualidad/1115453817_850215.html (accessed in July 2022). 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 https://www.mites.gob.es/itss/web/que_hacemos/estadisticas/index.html(accessed in July 2022). The number of yearly work inspections before the policy was around 30,000. See Figure 1 for more details.

three days after the terrorist bombings of March 11, 2004, in Madrid, which killed nearly 200 people and is the largest terrorist attack in Spanish history. Before it, polls forecasted that PSOE trailed the incumbent Popular Party (PP) by 7 percentage points. In fact, it was the mishandling of the crisis in the post-attack days that caused PP to lose this election, as explained in detail in Garcia-Montalvo (2011). It is very unlikely that a government led by PP would have ever passed an amnesty program of this magnitude, since this party has traditionally adopted tough regulations on immigration.[5]

We begin our analysis by documenting that the amnesty did not lead to magnet effects. This is critical for a correct interpretation of what the policy actually meant. If there were magnet effects, the estimates would capture the consequences of both legalizing undocumented immigrants and of the increase in the supply of unauthorized immigrants. If there were no magnet effects, the estimates would measure the effects of granting work permits to undocumented workers that were already living and working in Spain. To shed light on this question, we leverage the fact that the amnesty only affected immigrants from outside the EU and, hence, we compare immigrant stocks and growth rates from EU and non-EU countries around the policy change.[6] We do not detect any differential increase in the stock or growth rates of immigrants from outside the EU relative to immigrants from the EU. This is true both when we focus on the short- and medium-time horizons (i.e. the first year after the policy and up to four years after). Thus, the amnesty meant only a change in the right to work in the formal sector, but did not affect overall labor supply in Spain. This result is consistent with the fact that the reform was only approved after a series of unexpected events and that work permits were not granted to recently arrived immigrants. Furthermore, the evidence is in line with previous work analyzing the magnet effects of the Immigration, Reform and Control Act (IRCA) passed in the U.S. in 1986 (Orrenius and Zavodny, 2003).

To guide our empirical analysis, we introduce a model to think about the potential effects of an immigration amnesty program such as the one introduced in Spain in 2005. We assume that the amnesty affected the economy in three ways. First, and most importantly, it enlarged the labor supply choice set of undocumented immigrants that were, prior to the amnesty, forced to supply their labor endowment to the informal market. Second, given that the implementation of the amnesty was accompanied with an increase in work inspections against informality, we assume that the policy lead to an increase in the costs of hiring workers informally. Third, we assume that the policy made jobs in the formal and informal sectors more similar from the view point of workers, since a large part of informal type jobs in Spain combine some formal pay with supplemental informal rewards – an option that presumably became more complicated with the

[5] 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 PSOE in 2005. See Appendix H for a summary.

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

reform.

We build on recent work by Card et al. (2018) and Amior and Manning (2020) and assume that workers labor supply schedules are upward sloping, which gives employers some degree of market power. The model assumes various labor types. At an upper level, we assume that high- and low-skilled workers are imperfect substitutes in production. In turn, low-skilled workers can supply their labor endowment in either the formal or the informal markets. The informal market is characterized by jobs where workers and firms do not fully contribute to the taxes associated to the labor market regulations. Given the conditions in the formal and informal market, low-skilled workers with the legal right to work decide whether to work in the formal or informal markets. In contrast, undocumented immigrants can only supply their labor in the informal market. We characterize this constrained choice in labor supply decisions of unauthorized workers by assuming that undocumented workers' labor supply is more inelastic. We also assume that formal and informal labor market input is imperfectly substitutable to employers.

The model predicts that an amnesty program affects the economy in various ways. First, formal employment increases, but by less than the number of workers that gain work permits. This is so, because the amnesty program is unlikely to make formal and informal jobs completely equivalent and hence some of the workers who gain the legal right to work still choose to supply their labor to the informal market. Second, there is an ambiguous effect on employment and wage outcomes of low-skilled workers who already had work permits prior to the policy change. On the one hand, by equating the conditions in the formal and informal economy, the policy reduces the market power of employers. This is a force towards higher low-skilled wages and employment. On the other hand, however, a larger pool of workers with the legal right to work in the formal sector puts pressure down on wages and employment. Hence, overall wage and employment effects of the amnesty program are an empirical question. Third, tax revenues increase, mainly a consequence of moving informal workers to the formal sector. Finally, the model predicts that the cost increase associated to moving a bigger part of the low-skilled labor market towards formality encourages firms to substitute towards more high skilled workers, which increases their wages and employment.

We then turn to empirically analyzing the effects of the amnesty program on labor market outcomes and tax revenues. We do so by comparing Spanish provinces – which are a close approximation to local labor markets – that experienced a large inflow of immigrants into the social security system with those that experienced a small inflow.

Our main specification is a first-differences regression on detrended data, where the outcome is measured as the deviation from its pre-treatment trend at the province level. In other words, we remove the variation associated with pre-treatment trends, and province and year fixed effects, and then first difference the data. Therefore, the estimates are obtained from relating deviations

from trend in outcomes of interest to the size of the relative inflow of immigrants into the social security system. Moreover, we add several control variables that may be correlated immigrant locations and may directly affect outcomes of interest. Among them we consider the size of the construction sector, the immigrant share, coastal dummies and political alignment.

We also report results from a event-study specifications and show that there are no significant effects before the intervention. In addition, we include estimates from a 2SLS regression in which we instrument the number of legalized immigrants with the number of immigrants that were entitled to apply to the legalization program. Furthermore, we perform a series of robustness checks and placebo exercises. The results are very similar across all these specifications and uncover six interesting facts, which we summarize as follows.

First, we document that for every 10 newly legalized immigrants – who upon legalization necessarily entered the formal sector and who were working informally prior to the reform – only 5 formal jobs were retained. This confirms one of the predictions of the model. The policy change increases the pool of workers that can supply their labor endowment in the formal sector, but only a fraction of them end up in the formal sector.

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

Third, wages of low-skilled natives in the formal sector were not substantially affected by the reform. This is in line with the model, which has ambiguous predictions on this outcome because there are two opposing forces influencing wages. On the one hand, the policy reduces the market power of employers. On the other hand, more low-skilled workers with permits put pressure on low-skilled wages.

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

Fifth, we estimate that, for each newly legalized immigrant, payroll-tax revenues increased by around 4,000 euros per year at the province level.[7] In addition, we estimate that, per each newly

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

legalized immigrant, income tax revenues increased by around 440 euros per year. All together this is a substantial increase in tax revenues that likely exceeded the costs of the reform, at least in the short-run, given that undocumented workers in Spain already had access to public education and health care systems at the time. Indeed, we present evidence that school enrollment and hospital use were not affected by the regularization.

Lastly, we provide evidence in favor of one of the key assumptions in the model. Namely, that the policy increased the labor market opportunities of undocumented immigrants and reduced labor market power of employers. To do so, we analyze in detail the career path of the newly legalized immigrants. We focus our attention on undocumented immigrants who entered the social security system as housekeeping service workers, a sector that was characterized by a high prevalence of informality, a large number of undocumented immigrant workers, and one employee firms (i.e. households employing housekeeping services). We show that immigrants who entered in the social security as housekeeping workers stayed employed in the sector for about six months, which was a condition for the legalization process. After this initial period, their labor market opportunities expanded. First, 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. Second, we document a movement from one employee firms to larger and higher paying firms. We observe these patterns in 2005, when the reform took place, but not in the preceding year, which gives us confidence that they are a consequence of the amnesty.

Overall, we contribute to the literature in two ways. First, we provide a conceptual framework to guide our empirical investigation, which can be used to analyze amnesty programs. We study how these policies reduce market power of employers of undocumented workers and potentially of low-skilled workers that can choose between working in the formal and informal sectors. A number of recent papers investigate the role that imperfect competition plays in shaping labor market outcomes (Manning, 2003), but, within this literature, only a handful of papers use imperfect competition frameworks to study immigration. Naidu et al. (2016) and Amior and Manning (2020) are, to the best of our knowledge, the two most important exceptions, though neither of them analyze amnesty programs.

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

two recent papers closely related to ours, Bahar et al. (2021) and Cascio and Lewis (2019).[8] The former investigates the labor market effects of a large amnesty of Venezuelan workers in Colombia. The latter how the opportunity to apply for a Green Card in the US (permanent residence) affected income tax revenues and receipt of public transfers (EITC). Relative to these papers, we investigate the effect of granting work permits to undocumented immigrants on many outcomes within the same institutional context, which allows us to paint a very intricate picture of the consequences of legalization policies. On top of the outcome variables already mentioned and those analyzed in Bahar et al. (2021) and Cascio and Lewis (2019), we also show that the policy did not lead to magnet effects and that recently legalized immigrants experienced an increase in their set of labor market opportunities.

In what follows, we introduce, in Section 2, our data and explain the particular circumstances that led to the policy change. We also present our identification strategy and show no evidence of magnet effects. Section 3 introduces a monopsonistic model of a local labor market that guides our empirical analysis. In Section 4, we show evidence on labor market outcomes, tax collection, and the newly legalized immigrants' labor market experiences, which we relate to our models' predictions. Section 5 offers our conclusions.

2 Data, Policy Change and Identification Strategy

In this section we explain the datasets that we use, we detail the characteristics of the amnesty program, we describe the identification strategy, and we show that the policy did not lead to magnet effects.

2.1 Data

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

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

[8] Besides these two papers, there are many more that study the effects of amnesty programs on immigrant outcomes, though we provide evidence for the effects on many outcomes, also on natives, using the same quasi-experiment and institutional context. Among them, Dustmann et al. (2017); Pinotti (2017); Mastrobuoni and Pinotti (2015); 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); Dolado et al. (1996)

Second, we have individual level data from the Spanish Labor Force Survey (SLFS). From these data we have employment information, covering both workers in the formal and informal sectors. In these data we can identify native and immigrant workers of different skills, although we cannot identify if the worker is working in the formal or informal sector, or, in the case of immigrants, if they have or lack work permits. The employment question does not mention informality. Hence, if someone has worked informally in the reference week she should appear in the survey as working.[9] This data is a repeated cross-section with quarterly frequency.

Third, we have individual-level data on the working history of a representative sample of workers, through the *Muestra Continua de Vidas Laborales* (MCVL). These data are representative of the population of workers affiliated to the social security system, allows to track them over time, and has information about their level of education. We classify workers with at least a university degree as high-skilled workers, and we label the rest of the labor force as low-skilled workers.[10] Among immigrants, only documented workers can be affiliated to the social security, hence, these data cover a representative sample of all the workers in the formal sector, which necessarily excludes undocumented immigrants. There are few natives not covered in this data set. While some natives work informally, most native workers sign, at some point during their working life, a legal contract. Using these data we can track newly legalized immigrant workers when they gain work permits and enter the social security system (which was a requisite for the legalization process). These data also contains good information on formal workers' wages. To get a sense of informal sector wages of immigrant workers – not covered in the MCVL –, we use the survey *Encuesta Nacional de Inmigrantes* (ENI), which is a nationally representative survey of the immigrant population in Spain conducted at the end of 2006.

Fourth, we have aggregate administrative data on payroll-tax revenues at the province level. These data report the total amount of taxes collected in each province for each of the labor market contribution types, called "regimes", available in Spain. These data cover public revenues only coming from payroll taxes. We have these data at a yearly frequency. Moreover, to show that granting work permits to immigrants not only has effects on payroll-tax collection, but also in income tax collection, we use data from the Spanish Tax Administration on the number of filed income tax returns.[11]

Fifth, to know if the policy had effects on public spending, we use province level data for the two main expenditure programs in Spain: education and health care. More specifically, we use

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

[10] We use the same criterion when we work with data from the SLFS.

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

yearly enrollment data in the public education system from the Ministry of Education, as well as data on hospital discharges, disaggregated by diseases, from the Hospital Morbidity Survey conducted every year by the Spanish National Institute of Statistics.

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

2.2 Policy Change

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

First of all, in February 2005, the Spanish government made effective a policy to legalize a large number of undocumented immigrants who were already living in the country. Around 600,000 immigrants gained a work permit, and the share of immigrants registered in the social security system increased from around 6 percent to around 9 percent in a very short period of time, as can be seen on the left of Figure 1. It is clear from the graph that the 2005 amnesty was special in magnitude as well as timing.

Second, the beneficiaries of the amnesty were immigrant workers who were already living in Spain and who had real connections with the Spanish labor market. In particular, the law offered a period of 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 prior to August 7, 2004,[12] and 2) the employer needed to offer a legal working contract for at least six months.[13]

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

Fourth, at the time it was difficult for anyone to foresee that an amnesty program of this magnitude was likely, as it was approved after a series of unexpected events that led PSOE to surprisingly win the elections on March 14th, 2004. According to a poll conducted in January

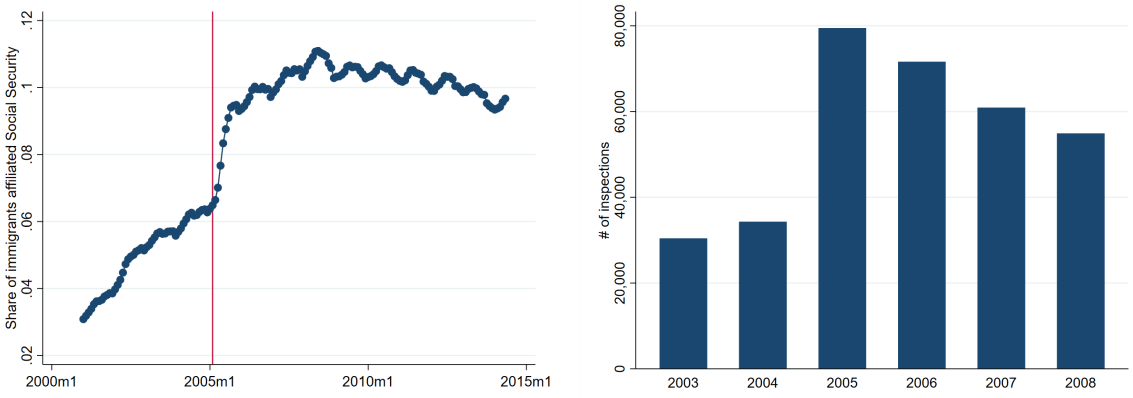
[12] This criterion was subsequently relaxed, accepting registration by default (*empadronamiento por omisión*) upon presentation of any official document proving that the immigrant had been in Spain in August of 2004.

[13] There were some exceptions for the agricultural, construction, restaurant and hotel, and domestic service sectors, as well as for part-time workers.

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

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

Figure 1: **Social Security Registration and Inspections Related to Foreign Workers**



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

2.3 Identification Strategy

The circumstances that led to the amnesty program gives us a strategy for identifying the effects of granting work permits to undocumented immigrants on labor market outcomes and payroll and income tax collection. If such effects exist, provinces with more legalized immigrants should experience relatively larger changes in outcomes.

Table 1 shows that the spatial distribution of immigrants was indeed heterogenous in 2002. Immigrants in Spain concentrate in coastal provinces with high levels of tourism and European retirees. This is the case of Alicante, the Balearic Islands, Girona, Tenerife, and Málaga. All these provinces had immigrant shares above 8.5 percent in 2002.[14] Immigrants also concentrate in large cities, as happens in other countries (Albert and Monras, 2019). 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. In contrast, in 2002 there were many provinces with extremely low levels of immigration: more peripheral provinces, such as Asturias, Coruña, or Lugo in the north; Córdoba, Jaén, Sevilla, or Cádiz in the south; and provinces in central Spain all had immigrant shares that were 2–3 percentage points below the national average. In addition, we can also see that most immigrants were low-skilled and that the legalization rate was high across all provinces.

Figure 2 depicts this spatial heterogeneity by dividing Spanish provinces by their median level of migration in 2002 and plotting the share of immigrants registered in the social security as a proportion of the total affiliation to the social security system. The graph on the left shows that, in high-immigration provinces, the share of foreign-born individuals registered in the social security system increased from around 7 percent to more than 10 percent in just three months. In contrast, 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 – where we normalize the two series to January 2005 – shows that, in fact, the effect of the policy was similar across locations in proportional terms, which is consistent with the take-up rate among undocumented immigrants being similar across provinces.

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

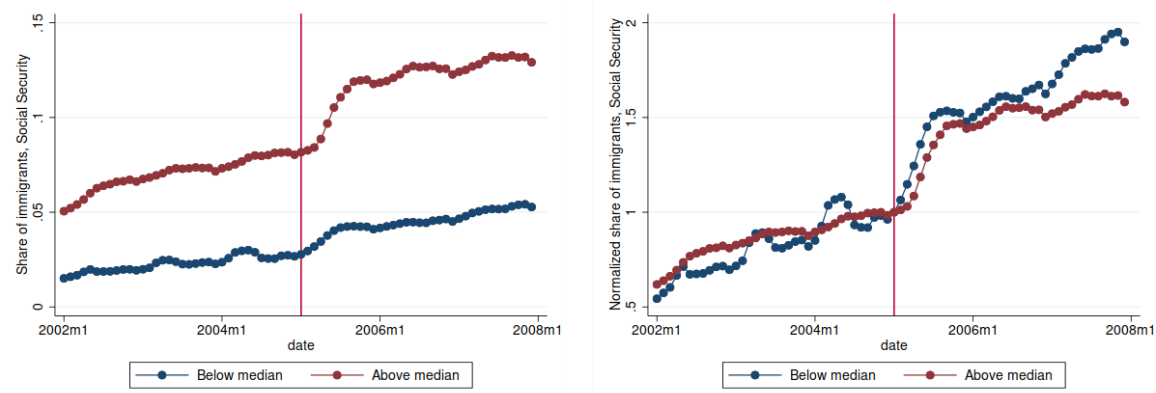
[14] These immigrant shares include all foreign-born individuals, i.e. it includes foreign-born people from EU and non-EU countries.

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

Province name	Immigrant share	Share Low-skilled among immigrants	Population in 1000s	Legalization rate	Coast	Rank
Alicante	0.135	0.854	1,595.2	0.803	1	1
Illes Balears	0.121	0.890	932.2	0.910	1	2
Girona	0.101	0.940	608.9	0.905	1	3
Madrid	0.092	0.751	5,623.0	0.787	0	4
Tenerife	0.090	0.759	904.0	0.859	1	5
Málaga	0.086	0.824	1,352.5	0.884	1	6
Almería	0.086	0.809	555.9	0.840	1	7
Las Palmas	0.082	0.878	965.3	0.805	1	8
Murcia	0.079	0.900	1,248.1	0.880	1	9
Castellón	0.073	0.939	509.7	0.948	1	10
Barcelona	0.068	0.698	4,979.4	0.843	1	11
Salamanca	0.017	0.804	347.7	0.933	0	40
Asturias	0.016	0.748	1,074.7	0.908	1	41
Cádiz	0.015	0.745	1,148.3	0.783	1	42
Coruña	0.014	0.829	1,116.4	0.785	1	43
Lugo	0.013	0.839	361.1	0.943	1	44
Sevilla	0.012	0.734	1,770.8	0.820	0	45
Palencia	0.011	0.921	175.6	0.908	0	46
Badajoz	0.010	0.745	663.0	0.887	0	47
Jaén	0.009	0.671	649.5	0.813	0	48
Zamora	0.009	1.000	200.2	0.954	0	49
Córdoba	0.009	0.797	773.5	0.749	0	50
National average	0.042	0.831	42,133.0	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).

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 as a proportion of total affiliation 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.

$$\Delta \widehat{Y}_c = \alpha + \beta \Delta \frac{\widehat{\text{Imm Soc Sec}_c}}{\text{Pop}_c} + \gamma X_c + \varepsilon_c \tag{1}$$

where the hat indicates that, before taking the difference between the pre- and post-policy period, we have removed three sources of potential bias. First, we control for province-specific linear trends prior to the policy. Thus, we will evaluate the effect of the amnesty by looking at the average changes in trends around the policy. Second, we add province fixed effects that absorb both observed and unobserved sources of potential bias that are fixed within a province over time. Third, we control for year fixed effects that account for shocks to outcomes by all Spanish provinces at a given point in time. Δ indicates that we have taken the difference between the pre- and the post- policy change period after detrending the data. $\Delta \frac{\widehat{\text{Imm Soc Sec}_c}}{\text{Pop}_c}$ is the "shock" variable that computes how many extra immigrants were affiliated in the social security relative to province-specific linear trends in immigrant affiliation. Therefore, β is the coefficient of interest. The subindex c indicates Spanish provinces.

A concern with our empirical strategy is that our estimates are at risk of contamination by other local area shocks to payroll taxes, employment and wages. For instance, provinces where more immigrants became formal workers could have also been experiencing a boom in the construction sector -which was growing very significantly at the time-, or they could have been receiving a larger inflow of immigrants. To mitigate this risk, we include control variables (X_c) in the specification such as: the fraction of immigrants from non EU-15 countries in 2004, the pre-reform share of workers in the construction sector, political alignment, and coastal dummies. 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.

Second, we employ a dynamic difference-in-difference specification to estimate the precise timing of differential changes in outcomes for provinces with a relatively high increase in the number of immigrants affiliated to social security, relative to the base year 2004. Following this method, we can test whether there are systematic differences across provinces in the variables of interest up to 2004 or not, and visualize if the changes occur only after the intervention. For this exercise we employ the following regression:

$$\ln y_{c,t} = \delta_t + \delta_c + \sum_{j=2002, j\neq 2004}^{2007} \beta_j 1[j=t] \Delta \frac{\widehat{\text{Imm Soc Sec}_c}}{\text{Pop}_c} + \sum_{j=2002, j\neq 2004}^{2007} \gamma_j 1[j=t] X_c + \varepsilon_{c,t} \tag{2}$$

This is, we estimate the interaction of year dummies, denoted by $1[j=t]$, and a "shock" variable. The coefficients of interest are the β_j. We also interact the year dummies with the pre-treatment control variables, X_c, described above. As in equation 1, the hat indicates that we have removed

province-specific linear trends prior to the policy, and province and year fixed effects.

As a robustness check of the first-differences specification (equation 1), we also report 2SLS estimates, where we instrument our main regressor using the distribution of immigrants in 2002, by nationality and by province. We explain with further details this strategy in Appendix D. In the tables displaying the results, we report estimates both from the first-difference OLS specification and from the 2SLS one. In addition, we perform a placebo exercise in which we estimate the effect of the policy on changes in outcomes around 2003. Finally, we also show estimates without controls and excluding the four largest provinces. As we will see in the empirical section, the results are not sensitive to any of these checks.

2.4 The Absence of Magnet Effects

A preliminary step of our analysis is to understand whether the policy change affected the overall supply of immigrants in Spain. This is a crucial point for the interpretation of the results. If there were magnet effects, the estimates would reflect both changes in the supply of immigrants as well as the consequences of granting work permits. Alternatively, if there were no magnet effects, the estimates would capture the effects of legalizing undocumented immigrants. Formally, the policy gave work permits to immigrant workers in the informal sector who could prove that they were living in Spain in August 2004. Moreover, its approval was unexpected, as explained in section 2.2. These institutional details suggest there should not have been supply effects. Nevertheless, in this section we further investigate this issue following two different approaches. The first one focuses on immigrant supply at the national level. The second looks for magnet effects at the provincial level, which is the source of variation we exploit in our identification strategy.

First, we compare immigrant stocks and growth rates from countries affected by the amnesty program and immigrants from EU-countries which were not affected by the policy. Spain was, and had been for many years, part of the Rome and Schengen treaties that allows EU citizens to freely move and work within the 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.[15]

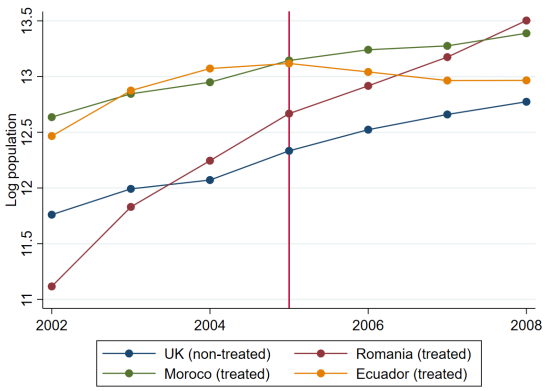
To document the potential presence of magnet effects, we use data from the Municipal Registry, which as argued in Section 2.1, captures both documented and undocumented immigrants. We start our empirical investigation by showing in Panel A of Figure 3 the stock of immigrants from the top four sending countries, three of which were affected by the policy, one which was

[15]Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia and Slovenia joined the EU in May 2004. However, EU members could delay until 2011 the free mobility of workers with these countries (except Cyprus and Malta). Spain applied mobility restrictions until May of 2006 to dependent workers (self-employed workers could freely enter into Spain already in May of 2004). Despite these restrictions on mobility, workers from new accession EU countries were not eligible to the amnesty program. Hence, we consider immigrants from these countries as EU members when evaluating the effect of the amnesty program on magnet effects. We show a robustness exercise that excludes those countries form our analysis in Appendix F.1.

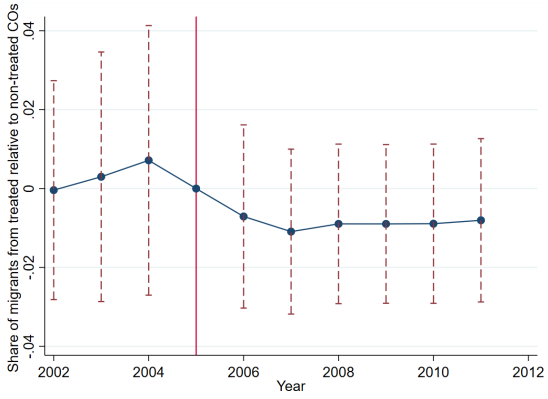
not. The graph does not show any significant change in the stock of immigrants from the UK (non-affected by the policy), Ecuador (affected), Romania (affected), and Morocco (affected). This graph suggests that the policy did not lead to a substantial change in the (net) flow of immigrants from any of these countries.

Figure 3: **The Absence of Magnet Effects**

Panel A: **Largest sending countries**



Panel B: **Share of immigrants from treated relative to non-treated countries of origin**



NOTE: Panel A of this figure 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"). Panel B of this figure shows an estimate of the share of immigrants (over all immigrants) from countries of origin affected by the amnesty program relative to countries not affected. The vertical red line indicates the last period before the reform (2005). These data are from the Municipal Register which covers both documented and undocumented immigrants. The Municipal Register reports the number of individuals residing in municipalities on January 1st each year.

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

$$y_{ot} = \delta_t + \delta_o + \delta_o * t + \beta \text{Affected countries}_{o,t} + \varepsilon_{o,t} \tag{3}$$

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

In a first set of specifications we investigate whether the amnesty program affected the composition of immigrants from the different countries of origin. To investigate this, we use as dependent variable $y_{ot} = \frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$, which measures the fraction of the overall stock immigrants that is from country o in each time period. Under the assumption that immigrants not affected by the amnesty continued to immigrate into Spain in the same way as they did before the reform, then a change in immigrant composition, $\beta \neq 0$, would indicate that the reform

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

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

Table 2 reports the estimates of β. We show two different specifications. Panel A includes only country and year fixed effects, and Panel B extends the specification by allowing country-specific linear time trends. Column 1 shows that there is no systematic change in the stock of immigrants from countries affected by the policy around the time of the change. In this first column, the sample years include 2002 to 2009. In columns 2 to 4 we change the years selected, by zooming into the year 2005 when the policy was implemented. As we zoom in, it is clear that there is no differential change in the stock of immigrants from affected and non-affected countries. Estimates in Panel B are similar to those in Panel A, although some times marginally statistically significant. Its magnitude, however, is small and the sign is always negative, hence, if anything, the policy discouraged migration from non-EU countries.

We visualize these results in Panel B of Figure 3, where we plot the estimates of the interaction between a time-invariant dummy that identifies "Affected countries" both before and after the policy and year dummies. Hence, it shows the year by year estimate of the difference-in-difference regression shown in equation 3. We observe that in the years prior to the policy the share of immigrants was increasing more or less at the same rate for immigrants from the treated countries of origin relative to non-treated. Hence, point estimates of the treatment dummy with year dummies lie around 0. If there had been strong magnet effects we would see a strong increase in the estimates that coincides with the timing of policy change. As can be seen in the graph, the relative share of immigrants from treated countries of origin was similar after the policy change.

Panel C in Table 2 shows the results of the alternative specification. It investigates whether the growth rate of the stock of immigrants changed differentially across countries as a result of the policy change. We investigate this point by using as dependent variable in equation 3 the flow of immigrants between any two periods divided by the stock in the first period. Results suggest that immigrant flows did not change substantially around the policy change. If anything they seem to have declined, but only after 2008, and, hence, far from the policy change and outside of the period we analyze later on.

The second approach we follow to study the magnet effects focuses on whether the policy led

Table 2: **Estimates of the Effect of the Immigration Reform on Migrant Composition and Growth Rates**

Panel A: Composition, country of origin fixed effects

Dep. Var.:	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$
	(1)	(2)	(3)	(4)
Affected countries	-0.008	-0.007	-0.006	-0.006
	(0.012)	(0.011)	(0.009)	(0.007)
Observations	896	784	672	448
R-squared	0.919	0.928	0.952	0.975
Year FE	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Country trends	no	no	no	no
Sample	2002-2009	2002-2008	2002-2007	2003-2006

Panel B: Composition, country of origin specific trends

Dep. Var.:	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$	$\frac{\text{Immigrants}_{ot}}{\text{Immigrants}_{t}}$
	(1)	(2)	(3)	(4)
Affected countries	-0.008*	-0.011*	-0.012*	-0.008*
	(0.005)	(0.006)	(0.007)	(0.005)
Observations	896	784	672	448
R-squared	0.992	0.992	0.991	0.996
Year FE	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Country trends	yes	yes	yes	yes
Sample	2002-2009	2002-2008	2002-2007	2003-2006

Panel C: Flows, country of origin specific trends

Dep. Var.:	$\frac{\Delta\text{Immigrants}_{ot}}{\text{Immigrants}_{ot-1}}$	$\frac{\Delta\text{Immigrants}_{ot}}{\text{Immigrants}_{ot-1}}$	$\frac{\Delta\text{Immigrants}_{ot}}{\text{Immigrants}_{ot-1}}$	$\frac{\Delta\text{Immigrants}_{ot}}{\text{Immigrants}_{ot-1}}$
	(1)	(2)	(3)	(4)
Affected countries	-0.129***	-0.053	0.034	-0.036
	(0.038)	(0.059)	(0.080)	(0.061)
Observations	896	784	672	448
R-squared	0.793	0.817	0.858	0.941
Year FE	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Country trends	yes	yes	yes	yes
Sample	2002-2009	2002-2008	2002-2007	2003-2006

NOTE: This table shows the estimates of 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. Panel A is our baseline specification shown in equation 3. Panel B includes linear country of origin-specific time trends. Observations weighted by the number of immigrants from each country of origin. The weighted mean of the dependent variable = 6.4%. Panel C uses the same specification as Panel B, but with the immigrant growth rate by country of origin as dependent variable. 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.

to significant changes in the share of immigrants across provinces. While the first set of evidence analyzed if magnet effects occurred nationally, with this method we can understand if magnet effects occurred at the province level and are correlated with changes in the share of immigrants

affiliated to social security, our main regressor. To do so, we employ specification 1 and the share of foreign-born population relative to three different variables: total immigrants, immigrants in the labor force and employed immigrants.

The results are shown in table 3. Panel A reports OLS coefficients and Panel B 2SLS estimates. None of the coefficients is significant and all have negative sign, indicating that the policy did not attract new immigrants from affected countries to the provinces where more immigrants were granted work permits.

Table 3: **Estimates of the Effect of the Immigration Reform on Share of Immigrants from Affected Countries**

	Δ Immigrant population share		
	Total	Labor Force	Employment
Panel A: OLS			
Δ Immigrants	-0.036	-0.267	-0.057
in social security/pop.	(0.218)	(0.241)	(0.149)
R-squared	0.109	0.148	0.102
Panel B: 2SLS			
Δ Immigrants	-0.254	-0.403	-0.184
in social security/pop.	(0.402)	(0.373)	(0.248)
F-test of excluded instruments	25.67	25.67	25.67
Observations	50	50	50

NOTE: This table shows the estimates of the effect of immigrant regularization on the different shares of the foreign-born population (total immigrants, immigrants in the labor force and employed immigrants) from countries affected by the policy change. All shares are calculated as a proportion of the total population. Regressions are weighted by population. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Panel A shows OLS and Panel B the 2SLS estimates using a shift-share instrument as explained in Appendix D. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Overall, Figure 3 and Table 2 show that the composition of immigrant in Spain did not tilt towards immigrants from non-EU countries of origin, nor did the growth rate of immigrant stocks change substantially between affected and non-affected countries of origin around the implementation of the amnesty program. Moreover, table 3 shows that the policy did not increase the share of immigrants from affected countries in the provinces where most immigrants regularized. Based on this evidence, we conclude that the amnesty did not lead to magnet effects and that it meant a change in the right to work in the formal sector and not a change in the supply of immigrant workers.

3 Theoretical framework

In this section, we introduce a theoretical framework that helps to guide our empirical work of Section 4. The model represents a Spanish province. This is similar to a commuting zone in other

contexts. Spanish provinces are relatively well connected local labor markets. Unlike commuting zones, they are not defined by commuting behavior, but rather by historical delineations.

We model each province as a small open economy producing a freely traded good. We assume a representative firm at the local level. There are four factors of production: **H**igh-skilled workers (L_H), low-skilled workers in the **F**ormal sector (L_F), **D**ocumented workers in the informal sector (L_D), and **U**ndocumented workers (L_U) who do not have work permits and hence necessarily work in the informal sector. Low-skilled workers with work permits decide whether to supply their labor in the formal or informal sectors. High- and low-skilled workers are imperfect substitutes (with elasticity of substitution σ) and formal and informal workers are also imperfect substitutes (with elasticity σ_L).

Spain has a relatively large informal sector. While there are some workers who only receive the returns to their labor endowment outside any type of labor market, very often informal workers receive part of their returns through part-time work and other arrangements, and, another part, informally – i.e. outside any contract. We consider these workers as informal workers. This is in contrast to full-time low-skilled workers fully paid and employed under regulated labor contracts.

We assume that the amnesty program that we later analyze empirically affected the economy in a number of dimensions. First, it reduced the supply of workers lacking work permits and increased the supply of those with work permits. Second, we think about the enforcement against informality that accompanied the policy change as making jobs in the formal sector and informal sectors more similar, and hence substitutable from the view point of workers with work permits deciding whether to supply their labor endowment in formal or informal type jobs. Finally, the policy change lead to an increase in the cost of hiring workers informally. We assume that the informal market only employs low-skilled labor.

3.1 Labor supply

There are two types of low-skilled workers. First, there are low-skilled workers with the legal right to work in Spain. We denote these by N. Second there are immigrant undocumented workers, denoted by U. Low-skilled workers with work permits supply labor to either the formal (L_F) or informal sector (L_D). Workers without work permits who do not have access to formal sector jobs can only supply labor to the informal sector. We denote by L_U these workers. Hence, the total number of workers in the informal sector is given by $L_D + L_U = L_I$, while the total amount of workers with work permits N is necessarily equal to $L_F + L_D$.[16]

Low-skilled workers with work permits can decide to supply their labor in either the formal or informal sectors. Wages and tax rates are different in the two sectors. Taxes are paid by firms, as

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

mandatory for payroll taxes. Sector optimization choice leads to the following supplies of labor:

$$L_F = \frac{(w_F)^{1/\epsilon_L}}{(w_F)^{1/\epsilon_L} + (w_D)^{1/\epsilon_L}} N, L_D = \frac{(w_D)^{1/\epsilon_L}}{(w_F)^{1/\epsilon_L} + (w_D)^{1/\epsilon_L}} N \tag{4}$$

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

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

$$L_U = (w_U)^{1/\epsilon_U} U \tag{5}$$

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

High-skilled workers', denoted by L_H, labor supply function is governed by ϵ_H, following:

$$L_H = (w_H)^{1/\epsilon_H} H \tag{6}$$

3.2 Demand for labor

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

$$Y = [L_H^\rho + L_L^\rho]^{\frac{1}{\rho}} \text{ with } L_L = [\alpha_F L_F^{\rho_L} + \alpha_I (L_D + L_U)^{\rho_L}]^{\frac{1}{\rho_L}}$$

where L_H indicates high skilled workers and L_L is a composite of low-skilled workers that combines L_F formal workers with L_D documented workers in the informal sector and L_U undocumented immigrant workers. α_F and α_I represent the productivity of formal and informal workers, respectively.

Profit maximization is given by the following program:

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

subject to the labor supply decisions expressed in equations (4) to (6), where τ_j denotes the payroll-tax paid when hiring factor type j. We assume $\tau_H = \tau_F = \tau$, $\tau_D < \tau$ and $\tau_U = 0$, i.e. formal workers pay the full amount of payroll taxes, documented workers in the informal sector pay only a fraction of that amount, and undocumented workers do not pay any payroll taxes. For

this maximization we assume monopsonistic competition, i.e., firms take as given price indexes such as $W = ((w_F)^{1/\epsilon_L} + (w_D)^{1/\epsilon_L})^{\epsilon_L}$. Under this assumption, this maximization problem results in the following optimality conditions:

$$p \frac{1}{(1+\epsilon_j)(1+\tau_j)} MPL_j = w_j$$

where $MPL_j = \frac{\partial Y}{\partial L_j}$ is the marginal product of labor and w_j denotes the wage of each factor of production, which are paid a mark-down over the marginal cost. We denote by w_j the wage received by the worker and by τ_j the (payroll) taxes paid by the firm, hence $(1+\tau_j)w_j$ is the total cost of one unit of factor j. Note that as ϵ_j goes to 0, labor supply is more elastic, and hence the markdown on marginal products is smaller. p is the price of the good, which we assume is freely traded, and hence we can normalize to one.[17]

3.3 Equilibrium

The equilibrium in the labor market equates demands and supplies for each factor of production. This yields the following equilibrium relationships between the amount of employed workers of each type (which we denote by L_j) and the total supply of high-skilled workers H, low-skilled workers with work permits N, undocumented workers U, and payroll taxes τ_j, labor supply elasticities, and the elasticities of substitution between the different factors of production.

Formal low-skilled workers (L_F)

Combining labor demand and supply for this factor type we obtain:[18]

$$\ln L_F = \delta_F - \frac{\sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau)) + \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1} \ln N \tag{7}$$

where δ_F combines aggregate output, aggregate labor, and various parameters that for simplification we take as fixed. Similarly, we can obtain that wages are given by:

$$\ln w_F = \kappa_F - \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau)) - \frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N \tag{8}$$

These expressions show that an increase in the supply of low skilled workers with work permits (N) leads to higher employment and lower wages, while a cost increase (τ_j) or an increase in the labor supply elasticity (ϵ_L) leads to lower employment and wages. Note, in particular, that we can recognise two effects. On the one hand, $\frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau))$ is a monopsonistic effect. It

[17] Marginal products of labor $MPL_F = \alpha_F Y^{\frac{1}{\sigma}} L_L^{-(\frac{1}{\sigma} - \frac{1}{\sigma_L})} (L_F)^{-\frac{1}{\sigma_L}}$. The others take similar expressions.

[18] From $L_F = (\frac{MPL_F}{(1+\epsilon_L)(1+\tau)})^{1/\epsilon_L} \frac{N}{W}$ we obtain that $L_F^{1+\frac{1}{\sigma_L \epsilon_L}} = (\frac{\alpha_F Y^{\frac{1}{\sigma}} L^{(\frac{1}{\sigma} - \frac{1}{\sigma_L})}}{(1+\epsilon_L)(1+\tau)})^{1/\epsilon_L} \frac{N}{W}$, which allows us to obtain the expression 7.

captures the idea that when labor supply schedules are more elastic, employers have less market power and, hence, wages are higher. On the other hand, the term $\frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N$ is a competition effect. More workers in a market put downward pressure on wages.

In Appendix B we show similar conditions for the other types of workers in the economy.

3.4 Properties

Equations (7), (8), and the equivalent ones for the other factors of production shown in the Appendix B, characterize employment and wage levels for each factor of production as a function of parameters of the model and population levels. This framework allows us to study the effect of an amnesty program.

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

In our case, we think that the policy change is well captured by exogenous changes in three parameters of our model. First, the policy decreased the number of workers who were undocumented (U) and increased those with legal work permits (N). Second, the policy increased enforcement against informality. This has two consequences. On the on hand, it increased the cost of hiring informal type workers, i.e. τ_D increased. On the other hand, it made formal and informal type jobs more similar from the view point of workers, which in the model is captured by a decline in ϵ_L. We can use this framework to derive the consequences of a policy change like the one implemented by the Zapatero government, which we summarize in the following proposition.

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

1. *Transforms undocumented workers into documented workers, hence $\frac{\partial U}{\partial \theta} = -\frac{\partial N}{\partial \theta}$*
2. *Increases the cost of informal work, i.e. $\frac{\partial \tau_D}{\partial \theta} > 0$*
3. *Makes the jobs in the formal and informal sector more similar to workers, i.e. $\frac{\partial \epsilon_L}{\partial \theta} < 0$*

Under these assumptions we have that:

1. *In the informal sector, wages of undocumented immigrant workers are lower than wages of documented workers, as long as ϵ_U is sufficiently large.*
2. *Total tax collection increases with the amnesty program.*
3. *There is an ambiguous effect on wages of formal low-skilled workers. Two forces are in place. On the one hand, the policy change increases the overall supply of formal workers,*

which puts pressure on wages. On the other hand, the policy decreases market power of firms, which tends to increase wages.

4. *Employment in the informal sector declines.*
5. *Employment in the low-skilled formal sector increases, but by less than the amount of low-skilled entrants into the formal sector.*
6. *Employment and wages increase for high-skilled workers.*

Proof. See Appendix C. □

Proposition 1 states various results. First, it says that undocumented workers face fewer job opportunities and hence, employers can exert higher market power over them. Second, it says that payroll tax collection increases with the policy change. This is so mainly because undocumented workers enter the formal market, and hence increase payroll-tax collection. Third, low-skilled workers in the formal sector may see their wages increase. This is explained by the fact that the policy change makes working in the formal and informal sectors more similar, which reduces the ability of formal sector firms to extract surplus from workers, as illustrated in Figure C.1 in the Appendix. Fourth, the reform increases enforcement against informality and grants work permits to undocumented workers, which leads to a decline in informal sector jobs. Fifth, our framework shows that the increase in formal sector jobs is smaller than the increase in the number of workers who gain work permits. This result comes from the fact that the reform does not necessarily "kill" the informal labor market. Hence some of the undocumented immigrant workers can chose to remain in the informal market (at least after the initial 6 months where the employee had to offer them a formal contract). Finally, the amnesty program makes the low-skilled labor aggregate (L_L) more expensive. Hence, employers try to substitute low- for high-skilled labor.

4 Empirical Evidence

In this section we investigate the empirical effects of the policy and test the predictions from the theoretical section. First of all, we estimate changes on employment and wages of both native and immigrant workers. We also report results by the skill of the worker.[19] Second, we quantify the impact of the amnesty program on tax collection and the use of public services. In particular, we focus on the two taxes most closely related to the labor market, the payroll tax and the income tax, and on education enrollment and hospital discharges. Lastly, we provide evidence of what we think is the main mechanism driving the results and a basic assumption in our model. Namely, that the amnesty program reduced the market power that employers could exert over undocumented immigrants.

[19] Remember that, 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.

4.1 Employment

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

Table 4: **Estimates of the Effect of the Immigration Reform on Formal Employment**

Panel A: Formal Employment (MCVL)							
All workers	Dep. Var.: Δ Formal Employment / pop.						
	Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A.1: OLS							
Δ Immigrants	0.540***	0.056	0.484***	0.080	-0.024**	0.463***	0.021***
in social security/pop.	(0.174)	(0.105)	(0.097)	(0.104)	(0.010)	(0.095)	(0.005)
R-squared	0.380	0.236	0.810	0.243	0.191	0.802	0.458
Panel A.2: 2SLS							
Δ Immigrants	0.528***	-0.035	0.564***	0.004	-0.039*	0.539***	0.025***
in social security/pop	(0.146)	(0.123)	(0.090)	(0.114)	(0.020)	(0.091)	(0.006)
F-test of excluded instruments	25.60	25.60	25.60	25.60	25.60	25.60	25.60
Panel B: Formal Employment, females (MCVL)							
Female workers	Dep. Var.: Δ Formal Employment / pop.						
Panel B.1: OLS							
Δ Immigrants	0.119*	-0.063	0.182***	-0.036	-0.027**	0.173***	0.009**
in social security/pop.	(0.071)	(0.039)	(0.043)	(0.035)	(0.010)	(0.041)	(0.004)
R-squared	0.182	0.230	0.779	0.229	0.349	0.772	0.261
Panel B.2: 2SLS							
Δ Immigrants	0.112*	-0.122**	0.234***	-0.079*	-0.044**	0.224***	0.011***
in social security/pop.	(0.062)	(0.056)	(0.034)	(0.048)	(0.018)	(0.034)	(0.003)
F-test of excluded instruments	25.60	25.60	25.60	25.60	25.60	25.60	25.60
Observations	50	50	50	50	50	50	50
Controls	yes	yes	yes	yes	yes	yes	yes

NOTE: This table shows the estimates of the effect of immigrant regularization on formal employment. Panels A and B present estimates of formal employment for all workers and female workers respectively. Panel A.1 and Panel B1 show OLS and Panel A.2 and Panel B.2 the 2SLS estimates using a shift-share instrument as explained in Appendix D. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Regressions are weighted by population. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

The results for employment in the formal labor market are in Table 4. We report them for all workers (Panel A) and for female workers (Panel B). Panels A1 and B1 show the OLS estimates and panels A2 and B2 report the 2SLS estimates. Since the OLS and 2SLS results are very similar, we focus the discussion on the OLS estimates.

The first column of Table 4 shows that the reform led to an increase in formal employment. This is one of the intended goals of the policy, since it moved informal immigrant workers to formality by granting them work permits. However, the estimate is substantially and statistically lower than 1. Specifically, for each immigrant that entered the formal sector during the reform,

Table 5: **Estimates of the Effect of the Immigration Reform on Total Employment**

Total Employment: Formal + Informal (SLFS)							
	Dep. Var.: Δ Total Employment / pop.						
	Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Δ Immigrants	-0.520**	-0.405*	-0.116	-0.481**	0.076	-0.373**	0.257***
in social security/pop.	(0.215)	(0.238)	(0.156)	(0.188)	(0.248)	(0.153)	(0.084)
R-squared	0.147	0.178	0.114	0.276	0.023	0.247	0.223
Panel B: 2SLS							
Δ Immigrants	-0.240	0.215	-0.456*	-0.395	0.610*	-0.618***	0.163
in social security/pop.	(0.397)	(0.540)	(0.267)	(0.361)	(0.321)	(0.218)	(0.161)
F-test of excluded instruments	23.52	23.52	23.52	23.52	23.52	23.52	23.52
Observations	50	50	50	50	50	50	50
Controls	yes	yes	yes	yes	yes	yes	yes

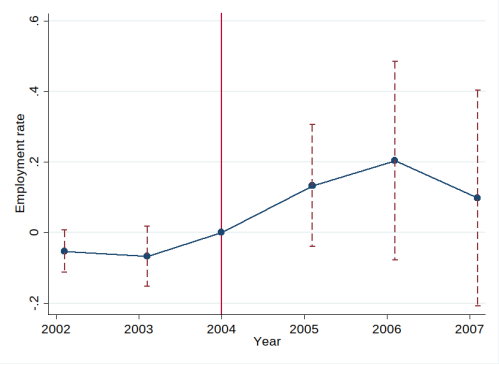
NOTE: This table shows the estimates of the effect of immigrant regularization on total employment (formal and informal) of various types of workers. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Regressions are weighted by population. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Panel A shows OLS and Panel B the 2SLS estimates using a shift-share instrument as explained in Appendix D. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

there was an increase of a bit more of 0.5 jobs over the course of the subsequent two years and a half, which is line with the predictions from the model. The rationale for that is that the reform does not erase the informal market, and some immigrant workers might return to work informally after obtaining the work permit. This effect is driven by an increase in immigrant formal employment (column 3) and, in particular, immigrant low-skilled formal employment (column 6). This is not surprising given that immigration in Spain is strongly low-skilled biased, as shown in Table 1.

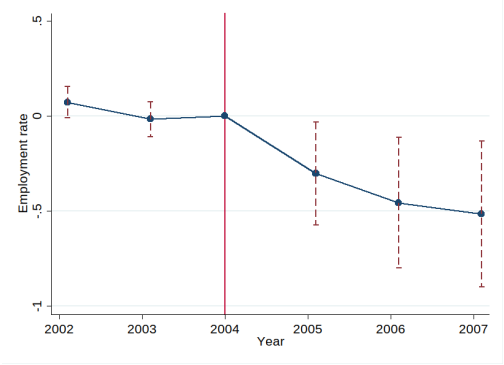
Moreover, the reform did not change the total number of native workers employed (column 2) or low-skilled native workers (column 4). The last result is also consistent with the model predictions, which are ambiguous regarding this estimate. On the one hand, having more immigrants with the legal right to work in formal sector jobs puts pressure on native low-skilled employment. On the other hand, enforcement against informality reduces firms' market power to employ workers in the underground economy, which leads to increased formal employment. These two forces seem to cancel out which can explain an estimate in column 4 that is statistically indistinguishable from 0.

Additionally, we detect a small decline in native high-skilled employment, which comes entirely from high-skilled women (column 5 Panel B). In section 4.4 we show that one of the sectors in which the amnesty had a larger impact was housekeeping services. Thus, the reform increased the cost of home production and might have shifted the labor supply of some high-skilled women away from the formal labor market and into home production. This evidence is in line with what is reported by Cortes and Tessada (2011).

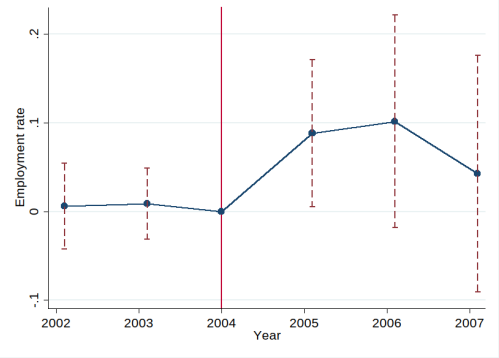
Figure 4: **Effects on Employment, dynamic difference-in-difference**



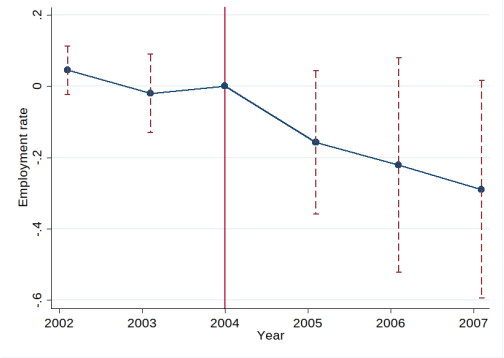
A. Total Formal Employment



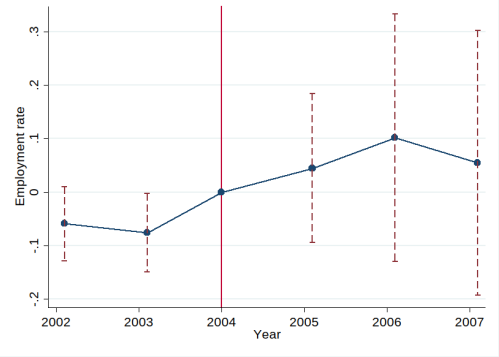
B. Total Formal and Informal Employment



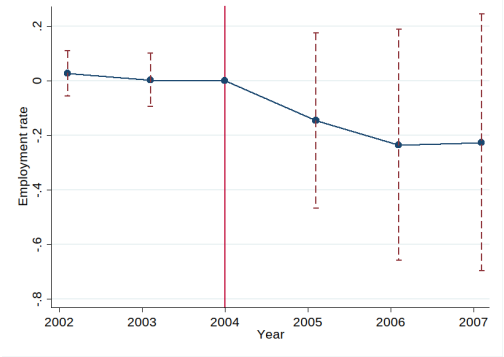
C. Immigrant Formal Employment



D. Immigrant Formal and Informal Employment



E. Native Formal Employment



F. Native Formal and Informal Employment

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

The results on formal employment suggest that we should find a decline in overall employment of around 0.5 workers per legalized immigrant. This is so because all the immigrants that were legalized were already living in Spain for at least 6 months prior to the reform and needed a formal work contract for 6 additional months after the reform, and yet 10 newly legalized workers lead to only 5 new formal jobs. We investigate whether indeed there is a decline in total employment by turning to data from the SLFS, which captures both formal and informal jobs. It is worth noting

too that the model suggests that the overall decline in employment should be concentrated among low-skilled workers since both the legalization and the enforcement policies against informality made low-skilled labor relatively more expensive.

In Table 5 we quantify the effects of the policy change on total employment. As anticipated we see that employment rates dropped as a consequence of the immigration reform. For each newly regularized immigrant, around 0.5 workers lost their jobs, which are necessarily informal sector jobs since formal sector employment increased.[20]

When we look at the split by skill groups and place of birth, we see that the overall loss of employment concentrated among low skilled workers in the informal sector. This evidence is consistent with two ideas. First, that the policy change increased enforcement against informality, and hence, some native low-skilled workers working informally lost their informal jobs. Second, it is also consistent with the idea that the policy made low-skilled informal type jobs more expensive, and hence employers substituted away from these type of jobs.

Figure 4 displays the estimates of the event-study specification. The graphs on the left show results in the formal labor market for all workers, immigrant workers, and native workers. Similarly, graphs on the right show the estimates when jointly considering the formal and informal labor market. As can be seen, there is no sign of pre-treatment trends for any of the outcome variables. Moreover, the figures confirm the results reported in tables 4 and 5, and it is visually evident that the impact of the amnesty happens only after its implementation. Similar evidence splitting the sample by skill level is shown in figures E.1 and E.2 in Appendix E.

Finally, in section 4.5 we present a placebo exercise and robustness checks. The results are in line with the ones exposed in this section and gives us confidence that the estimates reflect the effect of the policy change.

4.2 Wages

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

In contrast to the evidence on employment, we can only use the MCVL to study the effect of the reform on wages. This means that all our results describe what happen with wages of formal

[20] In Appendix F.3 we show that employment losses of low-skilled natives are stronger in sectors with high concentrations of immigrants workers. This is consistent with the idea that the policy created some competition between natives and immigrants, even within sectors.

workers of various skills. To study the effect of the policy change on wages we use the same empirical strategy. Our measure of wages is "composition adjusted wages".[21] More 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.

We present our main estimates in Table 6. Panel A reports results from the OLS specification, while Panel B displays estimates from the 2SLS strategy. The estimates from both methods go in the same direction, though the 2SLS coefficients are larger and more significant. The graphics in Figures E.3 and E.4 display the coefficients from the event-studies and show no evidence of pre-treatment trends.

Table 6: **Estimates of the Effect of the Immigration Reform on Wages**

	Dep. Var.: Δ log wages						
	Total	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Δ Immigrants	0.166	0.213*	-0.732***	0.189	0.394	-1.014***	1.775
in social security/pop.	(0.102)	(0.111)	(0.261)	(0.115)	(0.263)	(0.277)	(1.183)
R-squared	0.328	0.390	0.294	0.371	0.217	0.322	0.160
Panel B: 2SLS							
Δ Immigrants	0.417***	0.503***	-0.519**	0.458***	0.754***	-0.736***	0.933
in social security/pop.	(0.123)	(0.135)	(0.265)	(0.132)	(0.247)	(0.266)	(1.043)
F-test of excluded instruments	33.59	33.59	33.59	33.59	33.59	33.59	33.59
Observations	50	50	50	50	50	50	50
Controls	yes	yes	yes	yes	yes	yes	yes

NOTE: This table shows the estimates of the effect of immigrant regularization on wages. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Regressions are weighted by population. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Panel A shows OLS and Panel B the 2SLS estimates using a shift-share instrument as explained in Appendix D. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

For all workers, the results show that effects on wages were non-negative and, if anything, increased following the policy change. The results are similar for native workers. Given that we have controlled for observable characteristics, the estimated changes in wages can only come from changes in the price of labor or changes in unobservable characteristics of those who are working.

[21] 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.

Wages for high-skilled natives in high-immigration locations increased by 0.39 log points (almost statistically significant at conventional levels) for a one percentage-point increase in the share of immigrants registered in the social security system, while wages for low-skilled natives increased by 0.19 log points (again imprecisely estimated). The results are more mixed for immigrants. The policy seems to have increased the wages of high-skilled immigrants substantially (although standard errors are large, reflecting the small number of high-skilled immigrants in our sample), while the wages of low-skilled immigrants decreased in the formal sector. This decrease probably reflects the changing composition of immigrant workers in the formal sector that occurred with the policy change.

The increase in wages of high-skilled natives, according to our model, reflects the increased demand for high-skilled labor once low-skilled labor becomes more expensive. Instead the non-decline in wages of native low-skilled workers is in line with the fact that the policy change made formal and informal type jobs more substitutable, decreasing firms' market power over low-skilled workers with work permits. This force seems to counteract the pressure on formal low-skilled workers' wages coming from the increase in the number of low-skilled workers with work permits, similar to what happened with formal native low-skill employment.

4.3 Public Finances

The results in this section show that the amnesty raised government net revenue. On the one hand, the reform increased both payroll and income tax collection, as the model predicts. This is in line with the increase in formal employment caused by the policy, and the non-negative effects on wages. Thus, one of the immediate consequences of the reform was that employers of formerly undocumented immigrants started to pay payroll taxes, and newly regularized immigrants could now pay income taxes if their wages were high enough. On the other hand, we show that the policy did not increase public spending in health care and education, the two largest government expenditures, which is not surprising since these services were already available to undocumented immigrants. Therefore, we can conclude that the reform improved public finances.

The results of our main specification are shown in Table 7, Panel A.[22] In total, as shown in column (9) each newly regularized immigrant increased payroll-tax revenues between 4,000 and 5,000 euros. Columns (1) to (8) show the disaggregate estimates for each of the labor market contribution types (note that the sum of the coefficients shown in these columns adds to the one in column (9)). The increase in payroll-tax collection comes from the increase in payroll-tax revenues from the general regime (99% of the overall increase in total payroll-tax collection), housekeeping regime (5%), and agrarian regime (5%), with some decline in other categories. The general, agrarian, and housekeeping regimes are the most commonly used by immigrant workers.

[22] We discuss why this is our preferred specification in Appendix D.

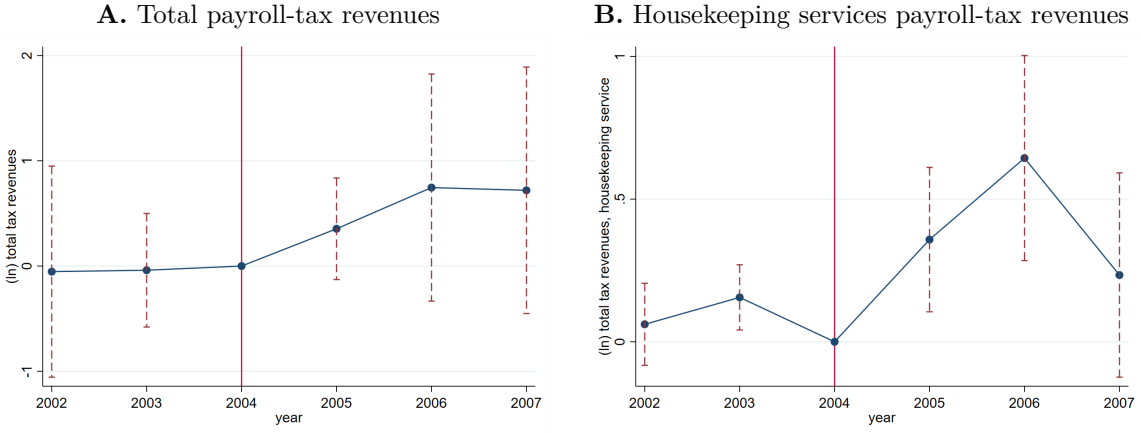
Overall, these estimates strongly suggest that the policy reform lead to a substantial increase in payroll-tax revenues. Panel B reports the 2SLS results, which are similar to the OLS ones.

Table 7: **Estimates of the Effect of the Immigration Reform on Payroll-Tax and Income Tax Revenues**

Dep. Var.:	Change in per capita payroll tax revenues by labor market contract type (in euros)									Change in number of Tax returns	
Panel A: OLS											
	General Reg. (1)	Self.emp. (2)	Agricult. (3)	Sea (4)	Coal (5)	Housekeeping (6)	Accident (7)	Unemp. (8)	Total (9)	Total (10)	Euros (11)
Δ Immigrants	3,870***	94.5*	186.6***	-3.9	12.1	186.0***	-17.0	-446.8	3,882***	0.341**	439.8
in social security/pop.	(1,116)	(54.0)	(47.2)	(17.1)	(20.5)	(57.8)	(29.7)	(301.7)	(914.4)	(0.130)	–
R-squared	0.584	0.194	0.419	0.225	0.092	0.700	0.216	0.497	0.642	0.291	—
Panel B: 2SLS											
Δ Immigrants	5,525***	47.01	189.8***	17.31	14.11	310.0***	3.961	-1,127**	4,979***	0.443***	504.0
in social security/pop.	(1,019)	(61.42)	(50.01)	(20.77)	(20.90)	(53.27)	(33.13)	(444.9)	(788.4)	(0.140)	—
F-test of excluded instruments	28.07	28.07	28.07	28.07	28.07	28.07	28.07	28.07	28.07	26.71	–
Observations	50	50	50	50	50	50	50	50	50	46	
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	
Share of contribution	99%	2%	5%	0%	0%	5%	0%	-12%	100%		
Immigrant share	6.9%	4.5%	12.3%	5.1%	5.2%	51.3%	—	—	7.5%	—	

NOTE: This table shows the estimates of 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. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Panel A shows OLS and Panel B the 2SLS estimates using a shift-share instrument as explained in Appendix D. The coefficients in columns (1) to (8) add to the coefficient in column (9). Column (10) estimates are based on Tax Authorities data on province level filings (4 provinces have decentralized income tax collection, for which we do not have data, Alava, Vizcaya Guipuzkoa, and Navarra). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level. Immigrant share represents the percentage of immigrants in each regime (average 2002-2007).

Figure 5: **Effects on Payroll Taxes, dynamic difference-in-difference**



NOTE: Panel A of this figure shows a dynamic difference-in-difference specification using the (ln) total payroll-tax revenues as dependent variable and the size of the regularization in each province as explanatory variable. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. Panel B shows the same graphs but for payroll-tax revenues in the category housekeeping services. 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 5, Panel A, shows that there are no systematic differences across provinces in total payroll tax revenues up to 2004. In 2005, when newly legalized immigrants started to contribute to the social security, we see a small increase that fully materializes in 2006 and 2007. The

point estimates suggest that by 2006 and 2007 a 1 percentage point increase in social security affiliates translates into a smaller than 1 percent increase in total tax revenues, reflecting that wages of newly legalized immigrants are lower than the average wage of workers in the social security pre-reform. Panel B shows similar evidence for payroll tax revenues of housekeeping service workers.

Table 8: **The Effect of the Immigration Reform on Education Enrollment and Hospital Discharges**

Panel A: Educational enrolment							
	All	Kinder	Primary	Secondary	High-school		
	(1)	(2)	(3)	(4)	(5)		
Panel A.1: OLS							
Δ Immigrants	-0.033	-0.012	-0.002	-0.012	-0.053		
in social security/pop.	(0.038)	(0.027)	(0.016)	(0.010)	(0.032)		
R-squared	0.091	0.082	0.155	0.487	0.172		
Panel A.2: 2SLS							
Δ Immigrants	-0.018	0.000	0.005	-0.012	-0.072***		
in social security/pop.	(0.042)	(0.035)	(0.014)	(0.013)	(0.027)		
F-test of excluded instruments	28.07	28.07	28.07	28.07	28.07		
Panel B: Hospital discharges							
	All	Tumors	Mental	Circulatory	Respiratory	Birth	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B.1: OLS							
Δ Immigrants	0.017	0.012	-0.004	-0.005	0.005	0.018	0.001
in social security/pop.	(0.079)	(0.014)	(0.006)	(0.016)	(0.023)	(0.024)	(0.066)
R-squared	0.032	0.061	0.196	0.065	0.144	0.072	0.066
Panel B.2: 2SLS							
Δ Immigrants	-0.036	0.025	0.009	0.006	0.006	0.018	-0.075
in social security/pop.	(0.084)	(0.017)	(0.009)	(0.023)	(0.026)	(0.027)	(0.079)
F-test of excluded instruments	28.07	28.07	28.07	28.07	28.07	28.07	28.07
Observations	50	50	50	50	50	50	50
Controls	yes	yes	yes	yes	yes	yes	yes

NOTE: This table shows the estimates of the effect of immigrant regularization on public education enrollment (Panel A) and hospital discharges (Panel B). Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. The category other in Panel B includes: metabolism, nervous system, infections, congenital, perinatal, skin, genitourinary, blood, poisoning, digestive, articulate and without diagnosis diseases. Regressions are weighted by population. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Panel A and Panel B show OLS and the 2SLS estimates using a shift-share instrument as explained in Appendix D. Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

The payroll tax is the most important channel through which the policy change affected tax collection. Additionally, we expect that some of the immigrants who entered the formal labor market started paying income taxes, though in a smaller magnitude since the income tax is negligible at low income levels and most newly regularized immigrants entered formal employment with relatively low wages. To quantify how many extra workers started to pay income taxes we estimate equation 1 using as dependent variable the number of filed income tax returns divided by total population (like our independent variable).

We find that for 10 immigrants that gained work permits 3.4 extra workers filed income tax returns (column 10, Table 7). This number represents around 60 percent of the new entrants to the formal labor market (see column 1, Panel A Table 4), since not all income levels need to file in income taxes. To translate the change in the number of workers who file in income tax returns in monetary terms, we use the distribution of wages of the immigrants who gained the legal status and the effective income tax rates by income brackets for these workers. This gives us an estimate of around 440 euros a year (see column 11, Table 7).[23]

Lastly, we provide evidence that the reform did not increase public spending. In Table 8 we estimate equation 1 using as outcomes several variables of enrollment in public education and hospital discharges. We do not find any significant effect of the legalization on the two main public spending programmes.

Overall, the results presented in Table 7 and Figure 5 suggest that the policy was effective in one of its main goals: it helped to raise tax revenues. We discuss robustness checks and placebo tests in Section 4.5. These exercises give us further confidence on our main estimates.

4.4 Newly Legalized Immigrants' Labor Market Experience

In the theoretical model, we characterized the amnesty reform as an increase in the labor supply choice set of undocumented immigrants. In other words, that these workers have a more elastic supply curve after the reform and, hence, are less vulnerable against the market power that employers can exert over them. In this section we provide evidence based on wages and the work histories of immigrants after legalization that is consistent with this mechanism.

First, we show that immigrants without work permits earn systematically less than documented ones. For that purpose, we use the ENI dataset, which we describe in more detail in section 2.1, and allows us to compute the wages of workers in the same occupations, some with the legal right to work in Spain and some without it.

In Table 9 we can observe that the ratio of wages between undocumented and documented immigrants is around 80%. Strikinlgy, the ratio is very similar when we consider various subgroups (economic sector, gender, age and time since arrival). While other explanations could also rationalize these numbers, like productivity differences, the stability of the results, and in particular, the results in sectors of activity where there is perhaps a low range of potential productivity differences -like housekeeping services, in Panel B-, makes us think that labor market power plays a significant role in explaining this result. In other words, immigrant workers without work permits, who are more likely to face constraints in their labor market supply decisions, end up having lower wages.

[23]The effective income tax rates by income bracket appears, every year, in the statistical yearbook of the income tax published by the Ministry of Finance, see https://www.hacienda.gob.es/es-ES/CDI/Paginas/centraldeinformacion.aspx (accessed in March 2022).

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

To study this, we focus our attention on the immigrant workers who entered for the first time in the social security system between February and August of 2005 using the labor market contribution type designed for housekeeping services and stayed in the social security until 2009. We focus on these workers because the prevalence of informal work in this sector was, prior to the policy change, extremely high. The legalization process was an opportunity for many of these workers to gain work permits and potentially a path towards other types of occupations.[24]

Table 9: **Wages of Documented and Undocumented Immigrants**

	Panel A: **All**			Panel B: **Housekeeping services**		
	Documented	**Undocumented**	**Ratio**	**Documented**	**Undocumented**	**Ratio**
	(1)	(2)	(3)	(1)	(2)	(3)
monthly wage (Euros)	1,034.0	842.3	0.81	805.8	692.90	0.86
hourly wage (Euros)	5.6	4.5	0.81	4.6	3.7	0.81
	Panel C: **Construction**			Panel D: **Hotels & Restaurants**		
	Documented	**Undocumented**	**Ratio**	**Documented**	**Undocumented**	**Ratio**
	(1)	(2)	(3)	(1)	(2)	(3)
monthly wage (Euros)	1,224.4	982.1	0.80	992.2	824.9	0.83
hourly wage (Euros)	6.5	5.34	0.82	5.5	4.5	0.81
	Panel E: **men**			Panel F: **women**		
	Documented	**Undocumented**	**Ratio**	**Documented**	**Undocumented**	**Ratio**
	(1)	(2)	(3)	(1)	(2)	(3)
monthly wage (Euros)	1,068.1	860.6	0.81	994.1	825.24	0.83
hourly wage (Euros)	5.8	4.7	0.80	5.5	4.4	0.81
	Panel G: **younger (less 35 y.o.)**			Panel H: **older (more 35 y.o.)**		
	Documented	**Undocumented**	**Ratio**	**Documented**	**Undocumented**	**Ratio**
	(1)	(2)	(3)	(1)	(2)	(3)
monthly wage (Euros)	1,037.5	853.8	0.82	1,030.0	826.9	0.80
hourly wage (Euros)	5.7	4.6	0.82	5.6	4.4	0.79
	Panel I: **new migrants (<3 years)**			Panel J: **old migrants (>= 3 years)**		
	Documented	**Undocumented**	**Ratio**	**Documented**	**Undocumented**	**Ratio**
	(1)	(2)	(3)	(1)	(2)	(3)
monthly wage (Euros)	1,033.2	822.2	0.80	1,036.4	860.4	0.83
hourly wage (Euros)	5.6	4.5	0.79	5.7	4.6	0.82

NOTE: This table uses data from the *Encuesta Nacional de Inmigrantes* to report immigrant wages as a function of their work permit status (Documented vs. Undocumented). The table provides estimates for different groups of workers, based on sector of employment, gender, age, and years since migration. The variable "Ratio" is the ratio of wages of undocumented to documented workers.

Figure 6 shows six graphs that illustrate the effect of the reform on labor market choices of housekeeping service workers. The graph on the top-left part of the figure, labeled as Panel A, shows the total number of affiliates in the social security system that use the contribution type

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

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. After six months, the number of affiliates decreases to a plateau of around 100,000 workers.

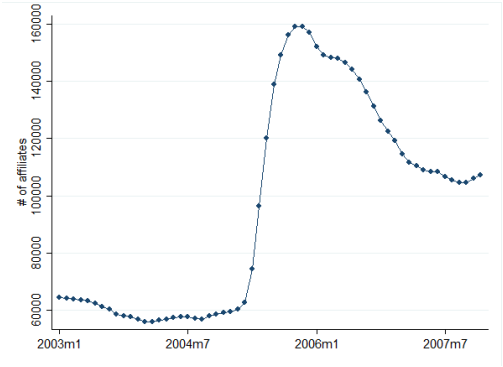
Among the newly legalized workers who remained in the social security until 2009 (and hence for which we can reconstruct their working history), we can track their sector of activity in each month. Panel B of Figure 6 shows the fraction of these newly legalized workers that remained in the housekeeping sector. It is striking that the fraction who remained in the housekeeping services stayed very close to 1 for the entire 2005. These workers necessarily had a 6 month work contract with the families through which they had entered into the formal market. However, around the beginning of 2006, many of these workers started to move to other sectors of the economy with, as we will argue, better labor market opportunities.

The evidence in Panel B of Figure 6 is in sharp contrast to the working history of immigrants who entered the social security system in 2004 as housekeeping services. These immigrants could not use the amnesty program to enter the social security system. In contrast to what was required by the reform of 2005, immigrant workers in the housekeeping sector did not have incentives to remain with their employer for 6 months. As can be seen in Panel C of Figure 6, immigrants entering the system in 2004 switched much more smoothly from housekeeping services to other sectors than immigrants who entered through the amnesty of 2005, and, overall, a smaller fraction of them had left the sector in the following 3 years (50 percent of stayers for those entering in 2004 versus less than 40 percent of stayers for those entering in 2005).

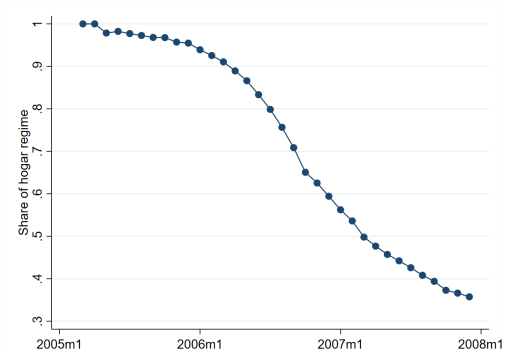
Next, we investigate the destination sectors of immigrants entering the social security system through housekeeping services and the characteristics of the new jobs available to them. Panel D of the Figure 6 shows this evidence. Most of these immigrants entered the Hotels and Restaurants (H&R), and other services, although many also entered other sectors. Panel E 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. As they move to other sectors, the average size of the employer increases substantially. Panel F shows that the wage of movers was around 30 percent higher than the average wage of those legalized immigrants who stayed in the sector. Therefore, these graphs suggest that the housekeeping workers that left the sector moved into larger firms and higher paying jobs. The same figures for two other sectors with high presence of undocumented migrants, albeit with lower informality, like Construction and Hotels and Restaurants, are shown in Appendix F.4. Broadly, the graphs show similar patterns.

Overall, we view this as evidence that the policy change allowed immigrant workers to enter the formal market economy and explore jobs beyond the ones they had prior to the legalization, something that likely contributed to their assimilation to the host country, as argued using

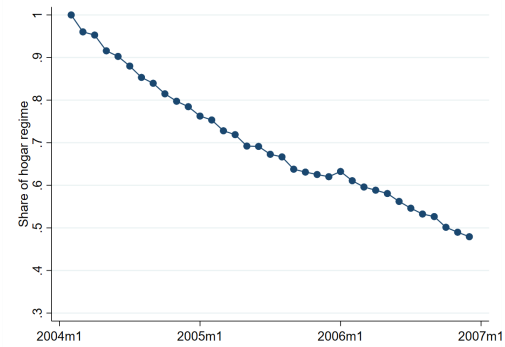
Figure 6: **Newly Legalized Immigrants and Sector Switching**



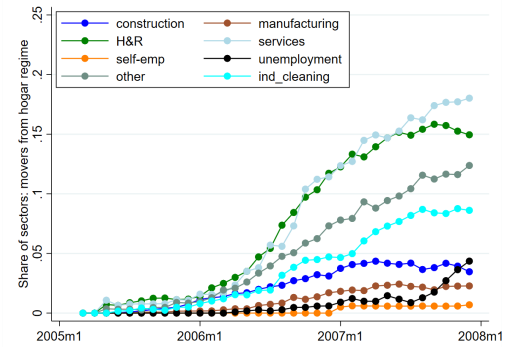
A. Affiliates in housekeeping services



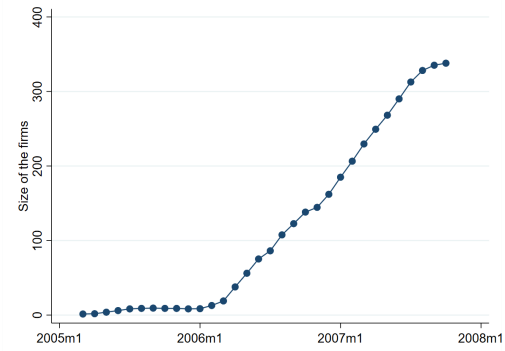
B. Share staying in housekeeping services in 2005



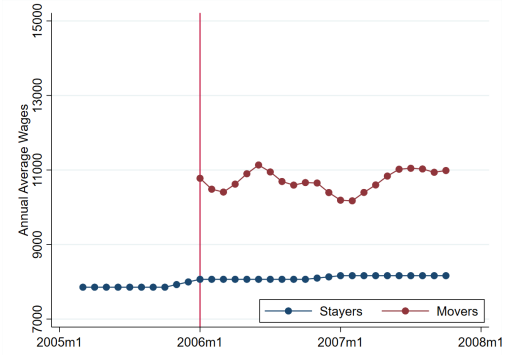
C. Share staying in housekeeping services in 2004



D. Sectors of destination



E. Firm size at sectors of destination



F. Wages of stayers and movers

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 fraction of immigrants who remained in housekeeping services among the immigrants who entered the social security system one year before the legalization in the housekeeping sector and continued in the sector until 2007. Panel D shows the sectors where immigrants, who entered the social security system with the legalization using housekeeping services contribution types and continued in the social security throughout the period, move to. Panel E shows the average size of the firm where immigrants, who entered the social security system with the legalization using housekeeping services contribution types and continued to work throughout the period, were working. Panel F 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 workers legalized through the housekeeping sector who remain in the sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

data for the United States and variation from IRCA in Bratsberg et al. (2002). We interpret these findings as suggesting that market power against undocumented workers was reduced as a consequence of the policy, in line with evidence from developing countries (Naidu et al., 2016).

4.5 Placebos and robustness checks

So far we have provided very similar estimates based on three different specifications: a first-difference OLS, a 2SLS strategy, and an event-study. In this section we perform a few more empirical exercises that highlight the reliability of the results. First, we perform a placebo exercise where we estimate the effect of the policy change on changes in outcomes around 2003. This is, we see if the policy implemented in 2005 affected outcomes before and after the year 2003. We expect from this exercise a number of estimates that are not distinguishable from 0.

We present these estimates in Table 10. Panel A shows estimates on formal employment using the MCVL. All estimates are small and statistically indistinguishable from 0. Panel B shows estimates from the SLFS. Again, all estimates are statistically insignificant. Panel C shows the exact same thing for wages.

Panel D shows estimates on payroll-tax collection of a number of categories. The overall tax collection did not increase differentially across provinces around 2003. The estimates for the other categories are also small, and, in general, statistically indistinguishable from zero. The few estimates that are statistically different than 0 are in categories that were not used by immigrants during the reform, such as self-employment, or are small in magnitude, as in the housekeeping regime.

A second way in which we can gain confidence on our main estimates is to check whether they are sensitive to different specifications. In particular we investigate 2 different additional specifications for every outcome variable in our main analysis. First, we show our results without controls. Second, we present estimates excluding the four largest provinces: Madrid, Barcelona, Valencia, and Sevilla. We present these results in Tables F.1 to F.7 in the Appendix. The estimates are broadly similar to the ones presented in the previous sections.

5 Conclusion

This paper studies the consequences of a large amnesty program in Spain. To do so, we combine detailed geographic data on tax revenues and labor market outcomes, and we show that the legalization of around 600,000 immigrants, combined with increased enforcement against informality, increased formal employment. Around half of the previously undocumented workers stayed in the formal sector. However, this was accompanied by a decrease of employment in the informal sector. Effects on wages were non-negative, though wages of high-skilled workers increased since

Table 10: **Pre-treatement Placebo Tests**

	(1)	(2)	(3)	(4)	(5)
Panel A: **Formal Employment - MCVL**					
Dep. Var.:			Δ Employment/pop		
		All Workers		Females	
	Total Emp.	Natives	Immigrants	Natives	Immigrants
Δ Immigrants	-0.076	0.069	-0.145	0.139	-0.102
in social security/pop.	(0.760)	(0.711)	(0.128)	(0.321)	(0.076)
R-squared	0.350	0.386	0.276	0.563	0.381
Observations	50	50	50	50	50
Panel B: **Total Employment (formal+informal) - SLFS**					
Dep. Var.:			Δ Employment/pop		
	Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS
Δ Immigrants	0.073	-0.016	0.089	-0.227	0.211
in social security/pop.	(0.448)	(0.353)	(0.182)	(0.412)	(0.202)
R-squared	0.157	0.179	0.081	0.093	0.158
Observations	50	50	50	50	50
Panel C: **Wages - MCVL**					
Dep. Var.:			Δ log wages		
	Total	Natives	Immigrants	Nat. LS	Nat. HS
Δ Immigrants	0.052	0.055	0.459	0.037	0.160
in social security/pop.	(0.108)	(0.095)	(0.329)	(0.090)	(0.279)
R-squared	0.289	0.328	0.240	0.360	0.034
Observations	50	50	50	50	50
Panel D: **Payroll-Tax Revenues**					
Dep. Var.:			Δ p.c. payroll-tax revenues		
	General Reg.	Self.Emp.	Agricult.	Housekeeping	Total
Δ Immigrants	432.7	225.3***	-142.2	31.59*	166.4
in social security/pop.	(629.6)	(77.00)	(135.3)	(16.70)	(732.5)
R-squared	0.150	0.284	0.194	0.720	0.065
Observations	50	50	50	50	50

NOTE: This table presents pre-treatement estimates of the effect of the immigrant regularization on our main outcomes of interest. In these placebo exercises, the pre-treatement is 2001-2002 and the pos-treatement period includes years 2003 and 2004, except for those estimates using data from MCVL for which the pre- and post-treatement periods are 2002 and 2003 respectively. Panel A presents the placebo tests for columns 1, 2 and 3 of Panel A and columns 2 and 3 of Panel B of Table 4. Panel B includes placebo tests for columns 1, 2 and 3 of Panel A of Table 5. Panel C presents the placebo tests for columns 1, 2, 3, 4 and 5 of Table 6. Panel D shows the placebo tests for columns 1, 2, 3, 6 and 9 of the Table 7. Regressions are weighted by population. Estimates are based on a continuous difference-in-difference strategy, where province-specific pre-change linear trends are removed. Controls include: political alignment dummies, coastal dummies, share of construction and share of immigrants from non EU-15 at baseline. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

the reform made hiring low-skilled labor more expensive.

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

Finally, we report evidence that the amnesty program increased the labor market opportunities

of immigrant workers. After legalization, they started changing sectors and moving to larger and higher-paying firms.

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Online Appendix, Not for Publication

A Data appendix

A.1 Ministry of Labor and 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 the province level. There are 50 provinces in Spain plus the cities of Ceuta and Melilla – two small cities in African territory that are part of Spain –, which we exclude from the analysis.[25] These data sets cover the period from 2000 to 2016. The social security registration data is available at a monthly frequency, while data on payroll-tax revenues is annual. Both the number of individuals registered in the social security system and pay-roll tax revenue data are also available disaggregated by labor market contribution type, in Spanish "regimes". There are several labor contribution types. Most workers are regulated under what is known as the general regime, which covers around 76 percent of the labor force. There are other labor contribution types that regulate particular sectors. These "special" types are the self employed, those employed in agriculture, in sea activities, in mining, and housekeeping services. These different types concentrated various degrees of immigrant workers, which help strengthen some of our results. While employment of immigrants is under-represented in the general and self employment regimes (69.6 % vs. 76.1% and 10.3% vs. 16.9%, immigrants and natives respectively) immigrant employment is clearly over-represented in the housekeeping and agricultural regimes (9.5 % vs. 0.7 % and 10.3 % vs. 5.8%, immigrants and natives respectively). In the housekeeping this over-representation is such that immigrants represents more than 50% of the total affiliates in this regime, while in total affiliation immigrants speaks for around 7.5%.

A.2 Continuous Sample of Employment Histories

We use Spain's Continuous Sample of Employment Histories (MCVL, for *Muestra Continua de Vidas Laborales*) to compute wages and formal employment. This is a micro-level 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 non-stratified 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 informa-

[25] Including Ceuta and Melilla does not change our results.

tion. 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 for two purposes. On the one hand, these data capture formal employment. On the other hand, it reports wages of the workers covered. With the objective of estimating the unit price of labor, we restrict the sample to natives and foreign-born workers, aged between 25 and 50 years old, who were employed at any point in our period of analysis (January 2002 to December 2007). In this analysis, we closely follow the sample of individuals constructed in de la Roca and Puga (2017), but we also include immigrant workers, women and extend our period of analysis to include 2002.

We also use these data to track working histories of immigrant workers entering the social security system at different points in time, in particular during the three months window of the legalization.

A.3 Spanish Labor Force Survey

We use the Spanish Labor Force Survey (SLFS, in Spanish *Encuesta de la Población Activa*, EPA) to measure overall employment. The SLFS is conducted every quarter by the Spanish National Institute of Statistics with a sample of around 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 50. The SLFS captures both formal and informal type jobs. In our sample both employment and unemployment rates are higher for foreign born individuals, 72.2% vs. 67.2% and 11.5% vs. 8.2% respectively. Moreover, employment and unemployment rates are even a bit higher when we focus on immigrants from countries candidate to legalize (73.2% and 12.1% respectively).

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.

A.4 Municipal Register

Municipal Register (*Padrón Municipal*) contains population residing in Spanish municipalities. The population counts refer to January 1st and are used to produce official population statistics. The Municipal Register includes information about country of birth, nationality, gender and date of birth. Since no proof of legal status is required, both regular and irregular immigrants are registered. Once immigrants arrive to Spain, they have strong incentives to register since

registration enables immigrants to access public services such as health care system or education. For the different exercises we use data from 2002 to 2008.

A.5 National Immigrant Survey

The main limitation of the MCVL is that it only captures workers in the social security system, and hence, provides information on wages only of documented workers working in the formal sectors. To investigate potential differences in wages of documented and undocumented immigrants we use the survey *Encuesta Nacional de Inmigrantes* (ENI). This is a nationally representative survey of the immigrant population in Spain. It was conducted between November of 2006 and February of 2007 and contains 15,465 observations. The survey records answers to a number of questions, including: legal status, household composition, socio-demographic characteristics of the interviewed individuals, migratory experience, socio-economic situation in the country of origin before migration, how they arrive to Spain, labor market and housing experience in Spain, relation with the origin country and social participation in Spain among others.

A.6 Other data sources

In addition, to confirm that the policy change did not increase public expenditures, we use data for the two main expenditure programs in Spain: education and health care. More specifically, to analyze if the legalization of immigrants implied additional costs for the public sector, we use yearly enrollment data in the public education system from the Ministry of Education, as well as data on hospital discharges, disaggregated by diseases, from the Hospital Morbidity Survey conducted every year by the Spanish National Institute of Statistics. Moreover, to show that granting work permits to immigrants not only has effects on payroll-tax collection, but also in income tax collection, we use data from the Spanish Tax Administration on the number of filed income tax returns.

B Equilibrium conditions for other types of workers

Informal documented low-skilled workers (L_D)

Doing similar steps we can obtain the equilibrium employment level of informal documented low-skilled workers:

$$\ln L_D = \delta_D - \frac{\sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau_D)) + \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1} \ln N - \frac{1}{1+\epsilon_L \sigma_L} \ln(1 + \frac{L_U}{L_D}) \qquad (9)$$

where δ_D combines aggregate output, aggregate labor, and various parameters as before, and

where $\tau_D < \tau$ are the taxes paid by informal workers, some of which, may be providing part of their labor through part-time work that is taxed. Alternatively, τ_D can also capture the risk of being caught by enforcement against informality.

Wages are given by:

$$\ln w_D = \kappa_D - \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau_D)) - \frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N - \frac{\epsilon_L}{1+\epsilon_L \sigma_L} \ln(1 + \frac{L_U}{L_D}) \quad (10)$$

Undocumented workers (L_U)

For the undocumented immigrants we can follow the same steps as above and hence, obtain:

$$\ln L_U = \delta_U - \frac{\sigma_L}{\epsilon_U \sigma_L + 1} \ln(1+\epsilon_U) + \frac{\epsilon_U \sigma_L}{\epsilon_U \sigma_L + 1} \ln U - \frac{1}{1+\epsilon_U \sigma_L} \ln(1 + \frac{L_D}{L_U}) \quad (11)$$

And wages:

$$\ln w_U = \kappa_U - \frac{\epsilon_U \sigma_L}{\epsilon_U \sigma_L + 1} \ln(1+\epsilon_U) + \frac{\epsilon_U}{\epsilon_U \sigma_L + 1} \ln U - \frac{\epsilon_U}{1+\epsilon_U \sigma_L} \ln(1 + \frac{L_D}{L_U}) \quad (12)$$

High skilled workers

Similar steps lead to the equilibrium employment and wages for high-skilled workers:

$$\ln L_H = \delta_H - \frac{\sigma}{\epsilon_H \sigma + 1} \ln(1+\epsilon_H)(1+\tau) + \frac{\epsilon_H \sigma}{\epsilon_H \sigma + 1} \ln H \quad (13)$$

And wages:

$$\ln w_H = \kappa_H - \frac{\epsilon_H \sigma}{\epsilon_H \sigma + 1} \ln(1+\epsilon_H)(1+\tau) + \frac{\epsilon_H}{\epsilon_H \sigma + 1} \ln H \quad (14)$$

C Proofs

The main proposition states that when the policy change is such that it:

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

Then:

1. In the informal sector, wages of undocumented immigrant workers are lower than wages of documented workers.

2. Total tax collection increases with the amnesty program.
3. There is an ambiguous effect on wages of formal low-skilled workers. Two forces are in place. On the one hand, the policy change increases the overall supply of formal workers, which puts pressure on wages. On the other hand, the policy decreases market power of firms, which tends to increase wages.
4. Employment in the informal sector declines.
5. Employment in the low-skilled formal sector increases, but by less than the amount of low-skilled entrants into the formal sector.
6. Employment and wages increase for high-skilled workers.

Proposition 1.1: In the informal sector, wages of undocumented immigrant workers are lower than wages of documented workers, as long as ϵ_U is sufficiently larger.

Proof. For this point, we need to combine the two wage equations:

$$p\frac{1}{(1+\epsilon_j)(1+\tau_j)}MPL_j = w_j$$

Now, given that the marginal product of labor for factor D and U is the same then:

$$\frac{w_D}{w_U} = \frac{1+\epsilon_U}{(1+\tau_D)(1+\epsilon_L)}$$

Hence, as long as τ_D is not too large, and given that $\epsilon_L < \epsilon_U$, we get $w_D > w_U$.

□

Proposition 1.3: There is an ambiguous effect on wages of formal low-skilled workers. Two forces are in place. On the one hand, the policy change increases the overall supply of formal workers, which puts pressure on wages. On the other hand, the policy decreases market power of firms, which tends to increase wages.

Proof. From:

$$\ln w_F = \kappa_F - \frac{\epsilon_L\sigma_L}{\epsilon_L\sigma_L+1}(\ln(1+\epsilon_L)(1+\tau)) - \frac{\epsilon_L}{\epsilon_L\sigma_L+1}\ln N \tag{15}$$

We want to compute that:

$$\frac{\partial \ln w_F}{\partial \theta} = -\frac{\partial(\frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau)))}{\partial \theta} - \frac{\partial(\frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N)}{\partial \theta} \tag{16}$$

Hence, we need to understand how the two terms change. We denote the first term as the monopsonistic effect and the second as the competition effect. The policy change makes ϵ_L smaller and increases the number of workers with work permits (N).

The monopsonistic effect is always positive: i.e., $-\frac{\partial(\frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau)))}{\partial \theta} > 0$. This is so because $\frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1}$ which is a value between 0 (as ϵ_L tends to 0) and 1 (as ϵ_L tends to infinity) decreases when ϵ_L decreases, and because $(\ln(1+\epsilon_L)(1+\tau))$ also becomes smaller when ϵ_L decreases. Hence, the first term, becomes smaller when ϵ_L decreases and, given the negative sign, the monopsonistic effect unambiguously becomes smaller as ϵ_L becomes smaller.

The competition effect tends to put pressure on formal low-skilled wages: The policy affects the second term of the previous equation in two different ways. On the one hand, $\frac{\epsilon_L}{\epsilon_L \sigma_L + 1}$ is a term between 0 (when ϵ_L goes to 0) and $\frac{1}{\sigma_L}$ (as ϵ_L goes to infinity). In other words, when ϵ_L becomes smaller the competition effect also becomes smaller. However, the policy also increases the number of workers with work permits N, which tends to put pressure on wages. Which one of these two forces dominates is a priori ambiguous. to investigate this further note that:

$$\frac{\partial(\frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N)}{\partial \theta} = \frac{\partial(\frac{\epsilon_L}{\epsilon_L \sigma_L + 1})}{\partial \theta} \ln N + (\frac{\epsilon_L}{\epsilon_L \sigma_L + 1}) \frac{\partial \ln N}{\partial \theta} = \frac{1}{(\epsilon_L \sigma_L + 1)^2} \ln N + (\frac{\epsilon_L}{\epsilon_L \sigma_L + 1}) \frac{\partial \ln N}{\partial \theta}$$

And so:

$$\frac{\partial(\frac{\epsilon_L}{\epsilon_L \sigma_L + 1} \ln N)}{\partial \theta} = \frac{1}{(\epsilon_L \sigma_L + 1)^2} \ln N - \frac{U}{N} (\frac{\epsilon_L}{\epsilon_L \sigma_L + 1}) \frac{\partial \ln U}{\partial \theta}$$

Which is positive as long as $\ln N > \frac{U}{N}(\epsilon_L \sigma_L + 1)\epsilon_L \frac{\partial \ln U}{\partial \theta}$ which is necessarily satisfied unless ϵ_L is too large.

Note that this effects can be analyzed with the following graph C.1 which shows the case when wages of formal low-skilled workers increase:

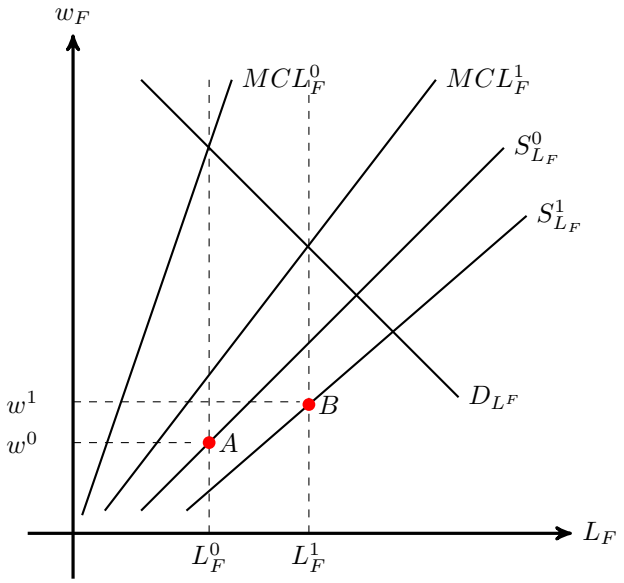
□

Proposition 1.5: Employment in the low-skilled formal sector increases, but by less than the amount of low-skilled entrants into the formal sector.

Proof. From:

$$\ln L_F = \delta_F - \frac{\sigma_L}{\epsilon_L \sigma_L + 1}(\ln(1+\epsilon_L)(1+\tau)) + \frac{\epsilon_L \sigma_L}{\epsilon_L \sigma_L + 1} \ln N \tag{17}$$

Figure C.1: Graphical representation of the model



We have that:

$$\frac{\partial \ln L_F}{\partial \theta} = -\frac{\partial(\frac{\sigma_L}{\epsilon_L\sigma_L+1}(\ln(1+\epsilon_L)(1+\tau))}{\partial \theta} + \frac{\partial \frac{\epsilon_L\sigma_L}{\epsilon_L\sigma_L+1}\ln N}{\partial \theta}$$

As before we have two terms. The first term increases when ϵ_L becomes smaller. The second term is affected by the policy change in two ways, the effect of the policy on ϵ_L and on N. Let's look at this in more detail:

$$\frac{\partial \frac{\epsilon_L\sigma_L}{\epsilon_L\sigma_L+1}\ln N}{\partial \theta} = \frac{\sigma_L}{(\epsilon_L\sigma_L+1)^2}\ln N - \frac{U}{N}\frac{\epsilon_L\sigma_L}{\epsilon_L\sigma_L+1}\frac{\partial \ln U}{\partial \theta}$$

Hence, this second term is also positive. To see that the increase in formal employment is less than one to one, it is worth remembering that not all the newly legalized immigrants will choose to enter the formal sector. Formal employment may also increase thanks to native low-skilled workers, but then they face two forces: first, the reduction in market power pushes them to work more; second, competition from newly legalized immigrant workers pushes in the other direction.

□

Proposition 1.4: Employment in the informal sector declines.

Proof. We can take the equilibrium levels of employment in the informal sector and obtain that:

$$\frac{\partial \ln L_D}{\partial \theta} + \frac{\partial \ln L_U}{\partial \theta} = -\frac{\begin{vmatrix} \Pi_{L_D\theta} & \Pi_{L_D L_U} \\ \Pi_{L_U\theta} & \Pi_{L_U L_U} \end{vmatrix}}{\begin{vmatrix} \Pi_{L_D L_D} & \Pi_{L_U L_U} \\ \Pi_{L_D L_U} & \Pi_{L_U L_U} \end{vmatrix}} - \frac{\begin{vmatrix} \Pi_{L_U\theta} & \Pi_{L_U L_D} \\ \Pi_{L_D\theta} & \Pi_{L_D L_D} \end{vmatrix}}{\begin{vmatrix} \Pi_{L_D L_D} & \Pi_{L_U L_U} \\ \Pi_{L_D L_U} & \Pi_{L_U L_U} \end{vmatrix}}$$

Note that the denominators are positive by the second-order condition at the maximum of the strictly concave profit function. Thus, the sign will depend on:

$$\Pi_{L_D\theta}(\Pi_{L_U L_D} - \Pi_{L_U L_U}) + \Pi_{L_U\theta}(\Pi_{L_D L_U} - \Pi_{L_D L_D}) \tag{18}$$

$$\Pi_{L_D L_D} = -\frac{(1+\sigma_L\epsilon_L)L_D + \sigma_L\epsilon_L L_U}{(1+\sigma_L\epsilon_L)(L_D+L_U)L_D} < 0$$

$$\Pi_{L_U L_U} = -\frac{(1+\sigma_L\epsilon_U)L_U + \sigma_L\epsilon_U L_D}{(1+\sigma_L\epsilon_U)(L_D+L_U)L_U} < 0$$

$$\Pi_{L_U\theta} = \frac{\sigma_L\epsilon_U}{1+\sigma_L\epsilon_U}\frac{1}{U}\frac{\partial U}{\partial \theta} < 0$$

$$\Pi_{L_D L_U} = -\frac{1}{(1+\sigma_L\epsilon_L)}\frac{1}{L_D+L_U} < 0$$

$$\Pi_{L_D\theta} = -\frac{\partial \epsilon_L}{\partial \theta}\frac{\sigma_L}{1+\sigma_L\epsilon_L}[-\sigma_L\epsilon_L \ln L_D + (1+\sigma_L\epsilon_L)\ln w_D \frac{1}{\epsilon_L}] -$$

$$-\frac{\sigma_L}{1+\sigma_L\epsilon_L}\left[\frac{\partial \epsilon_L}{\partial \theta}\frac{1}{1+\epsilon_L} + \frac{\partial \tau_D}{\partial \theta}\frac{1}{1+\tau_D} - \frac{\partial N}{\partial \theta}\frac{\epsilon_L}{N}\right]$$

Note that $\left[-\sigma_L\epsilon_L \ln L_D + (1+\sigma_L\epsilon_L)\ln w_D \frac{1}{\epsilon_L}\right] < 0$ for realistic values of L_D and w_D. Regarding the second element, $\left[\frac{\partial \epsilon_L}{\partial \theta}\frac{1}{1+\epsilon_L} + \frac{\partial \tau_D}{\partial \theta}\frac{1}{1+\tau_D} - \frac{\partial N}{\partial \theta}\frac{\epsilon_L}{N}\right]$ it will be positive as long as τ_D is sufficiently small. Then, $\Pi_{L_D\theta} < 0$

Thus, the sign of 18 will depend on the signs of $\Pi_{L_U L_D} - \Pi_{L_U L_U}$ and $\Pi_{L_D L_U} - \Pi_{L_D L_D}$. After some algebra, it can be shown that:

$$\Pi_{L_D L_U} - \Pi_{L_U L_U} = \frac{\sigma_L\epsilon_L}{(1+\sigma_L\epsilon_L)(L_D+L_U)} + \frac{L_D\sigma_L\epsilon_U}{(1+\sigma_L\epsilon_U)(L_D+L_U)L_U} > 0$$

$$\Pi_{L_D L_U} - \Pi_{L_D L_D} = \frac{\sigma_L\epsilon_L}{(1+\sigma_L\epsilon_L)(L_D+L_U)} + \frac{L_U\sigma_L\epsilon_L}{(1+\sigma_L\epsilon_L)(L_D+L_U)L_D} > 0$$

Then:

$$\frac{\partial \ln L_D}{\partial \theta} + \frac{\partial \ln L_U}{\partial \theta} < 0$$

Proposition 1.2: Total tax collection increases with the amnesty program.

Let's use T to refer to total tax collection. Thus:

$$T = (1+\tau)w_H L_H + (1+\tau)w_F L_F + (1+\tau_D)w_D L_D$$

Let's call $(1+\tau_i)w_i L_i = W_i$. Then:

$$T = W_H + W_F + W_D$$

When the policy change happens, the effect on tax collection can be summarized by:

$$\frac{\partial T}{\partial \theta} = \frac{\partial W_H}{\partial \theta} + \frac{\partial W_F}{\partial \theta} + \frac{\partial W_D}{\partial \theta}$$

Note that $\frac{\partial W_H}{\partial \theta} > 0$. We will divide the proof in two cases, depending on the sign of $\frac{\partial w_F}{\partial \theta}$.

Case 1: $\frac{\partial w_F}{\partial \theta} > 0$

Note that in this case, $\frac{\partial W_F}{\partial \theta} > 0$. Then $\frac{\partial T}{\partial \theta} > 0$ since:

$$\frac{\partial W_H}{\partial \theta} + \frac{\partial W_F}{\partial \theta} > \frac{\partial W_D}{\partial \theta}$$

Case 2: $\frac{\partial w_F}{\partial \theta} < 0$

We will analyze now $\frac{\partial W_F}{\partial \theta} + \frac{\partial W_D}{\partial \theta}$.

First, realize that:

$$\text{sign}(\frac{\partial W_F}{\partial \theta} + \frac{\partial W_D}{\partial \theta}) = \text{sign}(\frac{\partial \ln W_F}{\partial \theta} + \frac{\partial \ln W_D}{\partial \theta})$$

Hence, we can work with:

$$\ln W_F + \ln W_D = \ln L_F + \ln w_F + \ln L_D + \ln w_D + \ln(1+\tau) + \ln(1+\tau_D)$$

And we want to show that:

$$\frac{\partial \ln W_F}{\partial \theta} > \frac{\partial \ln W_D}{\partial \theta}$$

Start from:

$$\ln W_F > \ln W_D$$

Substituting W_F and W_D, we get:

$$\delta_F + \kappa_F - \frac{\sigma_L(1+\epsilon_L)}{1+\sigma_L\epsilon_L}\ln(1+\tau) + \ln(1+\tau) > \delta_D + \kappa_D - \frac{\sigma_L(1+\epsilon_L)}{1+\sigma_L\epsilon_L}\ln(1+\tau_D) -$$
$$- \frac{(1+\epsilon_L)}{1+\sigma_L\epsilon_L}\ln(1+\frac{L_U}{L_D}) + \ln(1+\tau_D)$$

$$\frac{\sigma_L(1-\sigma_L)}{(1+\sigma_L\epsilon_L)^2}\ln w_F L_F^{\frac{1}{\sigma_L}}\frac{\partial\epsilon_L}{\partial\theta} > \frac{\sigma_L(1-\sigma_L)}{(1+\sigma_L\epsilon_L)^2}\ln w_D(L_D+L_U)^{\frac{1}{\sigma_L}}\frac{\partial\epsilon_L}{\partial\theta} -$$
$$- \frac{\sigma_L(1+\epsilon_L)}{1+\sigma_L\epsilon_L}\frac{1}{1+\tau_D}\frac{\partial\tau_D}{\partial\theta} - \frac{1-\sigma_L}{(1+\sigma_L\epsilon_L)^2}\ln(1+\frac{L_U}{L_D})\frac{\partial\epsilon_L}{\partial\theta} -$$
$$- \frac{1}{L_D+L_U}\frac{\partial L_U}{\partial\theta} + \frac{L_U}{L_D^2}\frac{\partial L_D}{\partial\theta}$$

The second element on the right-hand side is positive and enters subtracting. Note that $\frac{L_U}{L_D^2} > \frac{1}{L_D+L_U}$, and hence the last two elements also enter the equation subtracting from the right-hand side. Moreover, the first and third element on the right-hand side can be combined. Thus, the equation above can be further simplified. Call A a positive constant. Hence:

$$\frac{\sigma_L(1-\sigma_L)}{(1+\sigma_L\epsilon_L)^2}\ln w_F L_F^{\frac{1}{\sigma_L}}\frac{\partial\epsilon_L}{\partial\theta} > \frac{\sigma_L(1-\sigma_L)}{(1+\sigma_L\epsilon_L)^2}\ln w_D(L_D)^{\frac{1}{\sigma_L}}\frac{\partial\epsilon_L}{\partial\theta} - A$$

Note that the element in the left-hand side is positive since $\sigma_L > 1$. Likewise for the first element in the right-hand side. Hence, ignoring A, the condition for the above equation to hold is:

$$\ln w_F L_F^{\frac{1}{\sigma_L}} > \ln w_D L_D^{\frac{1}{\sigma_L}}$$

In words, the increase in tax collection from the formal low-skilled labor market will outset the decrease in tax collection in the documented informal labor market as long as the first market is larger than second one. Thus, we have shown that:

$$\frac{\partial T}{\partial\theta} = \frac{\partial W_H}{\partial\theta} + \frac{\partial W_F}{\partial\theta} + \frac{\partial W_D}{\partial\theta} > 0$$

□

Proposition 1.6: Employment and wages increase for high-skilled workers.

Proof. Let w_L be the average wage in the low-skilled labor market -regardless of whether the workers is formally or informally employed. The reform increases the cost of low-skilled workers, $\frac{\partial w_L}{\partial \theta} > 0$, both because of the increase in tax enforcement ($\frac{\partial \tau_D}{\partial \theta} > 0$) and the employment surge in the formal low-skilled labor market ($\frac{\partial L_F}{\partial \theta} > 0$).

Thus, we will start by showing that $\frac{\partial L_H}{\partial w_L} > 0$. From the cost minimization problem:

$$\begin{aligned} \min_{L_H, L_L} \quad & w_H L_H + w_L L_L \\ \text{s.t.} \quad & [L_H^\rho + L_L^\rho]^{\frac{1}{\rho}} \geq Y \end{aligned}$$

Refer to L_H^* and L_L^* as the optimal values that minimize the problem. Then, the cost function is:

$$w_H L_H^*(w_H) + w_L L_L^*(w_L) = C(w_H, w_L, Y)$$

Note that L_i^* is a function of w_i because we are in a monopsonistic labor market. If we take derivatives with respect to w_H:

$$L_H^* + w_H L_H^{*'}(w_H) = C_{w_H}(w_H, w_L, Y)$$

Since the cost function is homogenous of degree 1, it can be rewritten as:

$$C(w_H, w_L, Y) = w_L C(\frac{w_H}{w_L}, 1, Y)$$

Then,

$$\frac{\partial L_H}{\partial w_L} = C_{w_H w_L}(w_H, w_L, Y) = -\frac{w_H}{w_L^2} C_{w_H w_H}(\frac{w_H}{w_L}, 1, Y) > 0$$

since $C_{w_H w_H}(\frac{w_H}{w_L}, 1, Y) < 0$ by concavity of the cost function.

Finally, the increase in w_H follows from the surge in demand for high-skilled workers.

□

D Alternative specifications

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 compliers is negligible, given the strong incentives for immigrants who were eligible to acquire legal status. 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 compliers. This data limitation implies that our ITT estimates will be downward biased and our LATE estimates may be biased, although we do not know the sign of this bias. This explains why in the main text we prefer to report the OLS estimate of the treatment effect.

To compute ITT estimates we can use the following equation:

$$\Delta \widehat{Y}_c = \alpha + \delta \frac{\text{Imm Candidates}_c}{\text{Pop}_c} + \gamma X_c + \varepsilon_c \tag{19}$$

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 this is what is best measured. With measurement error, we obtain a downward biased estimate of δ because of attenuation bias.[26]

To estimate the local average treatment effect we can use a two stage least square procedure,

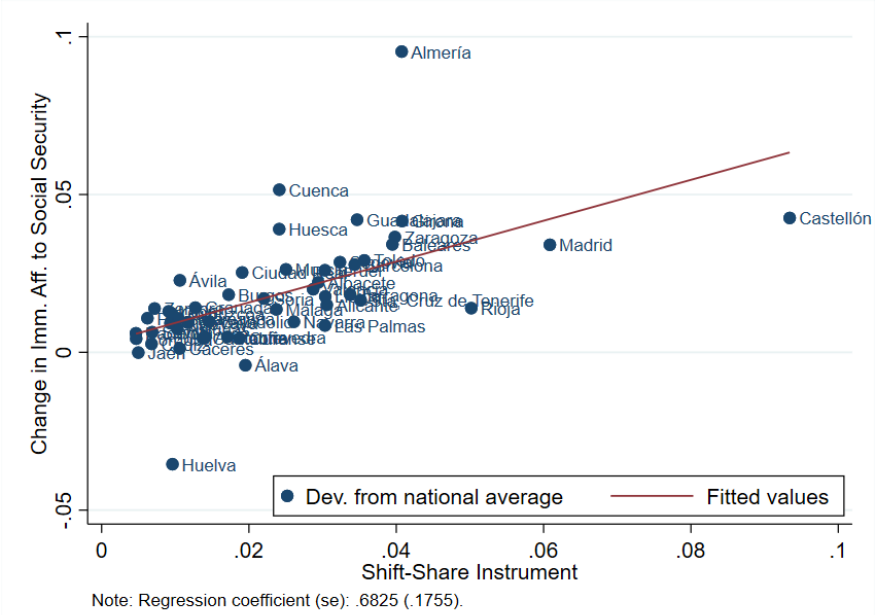
[26] Under the assumption that measurement error is classical, which we think is a reasonable assumption.

where the first stage is given by:

$$\Delta \frac{\widehat{\text{Imm Soc Sec}}_c}{\text{Pop}_c} = \alpha + \eta \frac{\text{Imm Candidates}_c}{\text{Pop}_c} + \varepsilon_c \tag{20}$$

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 1 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 2SLS will carry two source of bias: one η will be estimated too small, which will tend to make the 2SLS estimate too large. Second, δ will also be downward biased, which will tend to make the 2SLS LATE estimate too small. Which of these two forces dominates is unclear.

Figure D.1: **First Stage Results**



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.

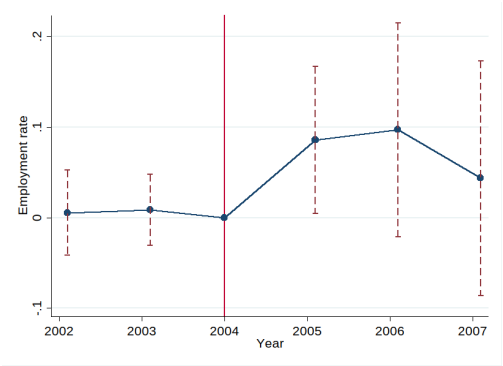
Figure D.1 shows the first-stage regression given by Equation 20. Our estimate of η is smaller than 1. This suggests that either we estimated $\frac{\text{Imm Candidates}_c}{\text{Pop}_c}$ with error (most likely), or that not everyone who could obtain work permits obtained them (something less likely 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 (1). This estimate is the effect of the treatment. We

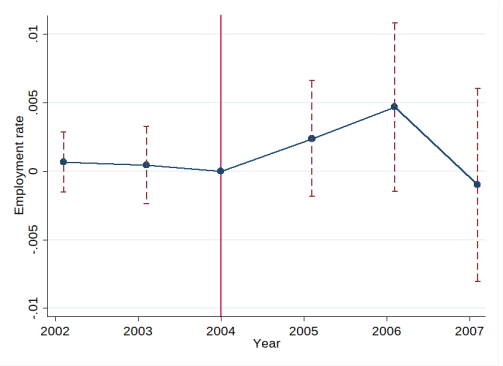
report in Appendix F.2 the ITT estimates, and the LATE estimates.

E Complementary Results

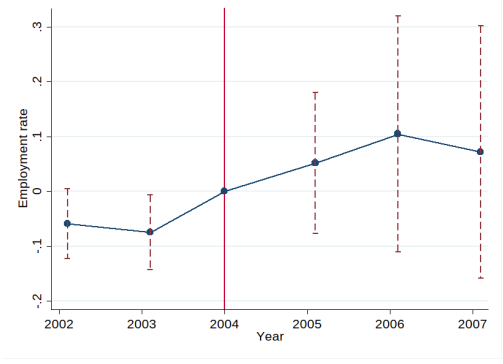
Figure E.1: **Effects on Formal Low- and High-Skilled Employment, dynamic difference-in-difference**



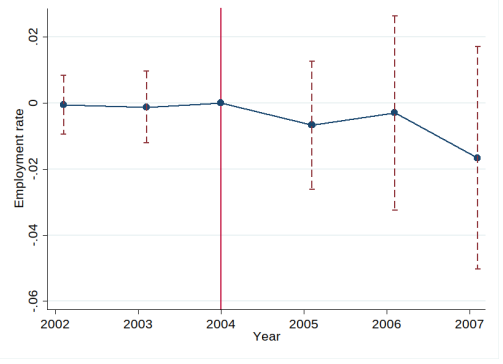
A. Immigrant Low-Skilled Formal Employment



B. Immigrant High-Skilled Formal Employment



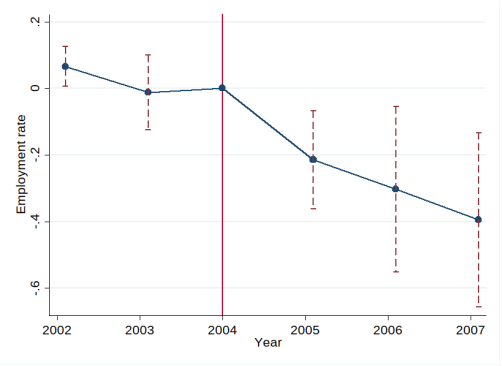
C. Native Low-Skilled Formal Employment



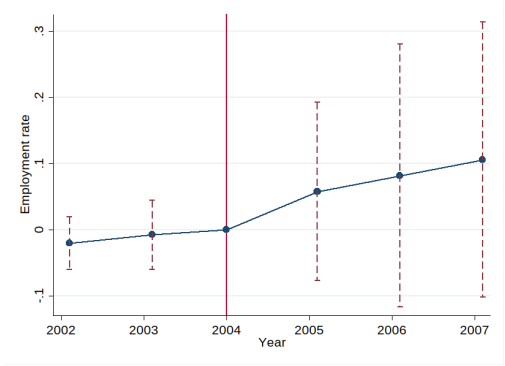
D. Native High-Skilled Formal Employment

NOTE: This figure shows estimates from the dynamic difference-in-difference specification on formal employment. Panels A and B display results for low- and high-skilled immigrant workers, respectively. Panels C and D for low- and high-skilled native workers, respectively. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical red line indicates the last period before the reform. Source: Own elaboration based on MCVL data.

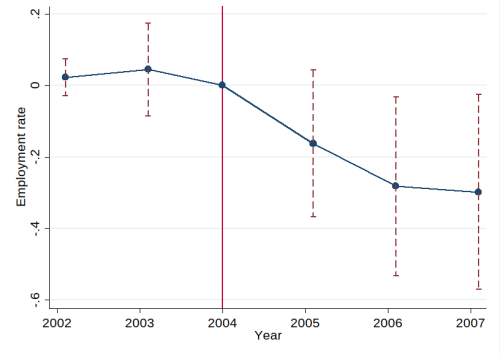
Figure E.2: **Effects on Formal and Informal Low- and High-Skilled Employment, dynamic difference-in-difference**



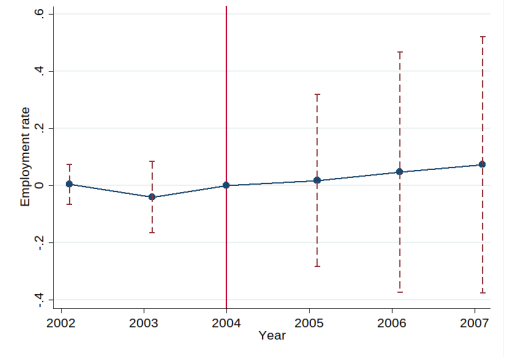
A. Immigrant Low-Skilled Formal and Informal Emp.



B. Immigrant High-Skilled Formal and Informal Emp.



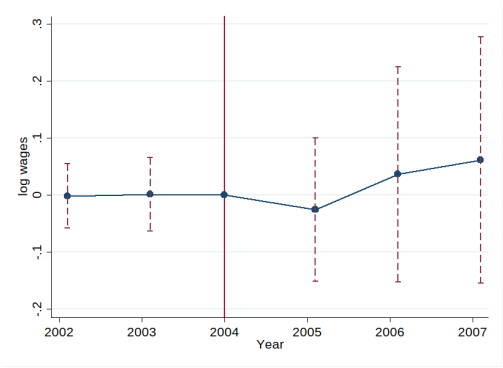
C. Native Low-Skilled Formal and Informal Emp.



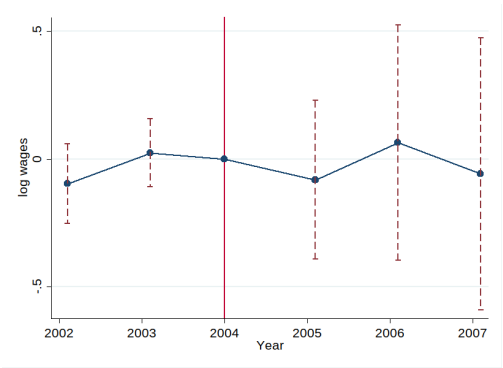
D. Native High-Skilled Formal and Informal Emp.

NOTE: This figure shows estimates from the dynamic difference-in-difference specification on formal and informal employment. Panels A and B display results for low- and high-skilled immigrant workers, respectively. Panels C and D for low- and high-skilled native workers, respectively. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical red line indicates the last period before the reform. Source: Own elaboration based on SLFS data.

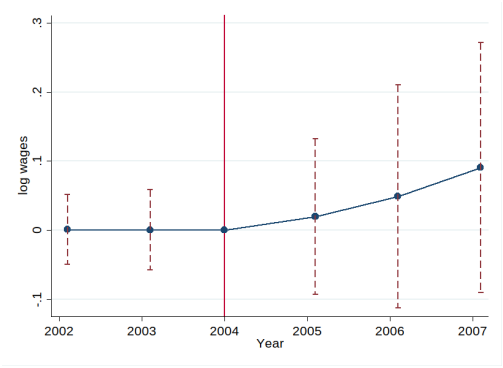
Figure E.3: **Effects on Wages, dynamic difference-in-difference**



A. All Workers



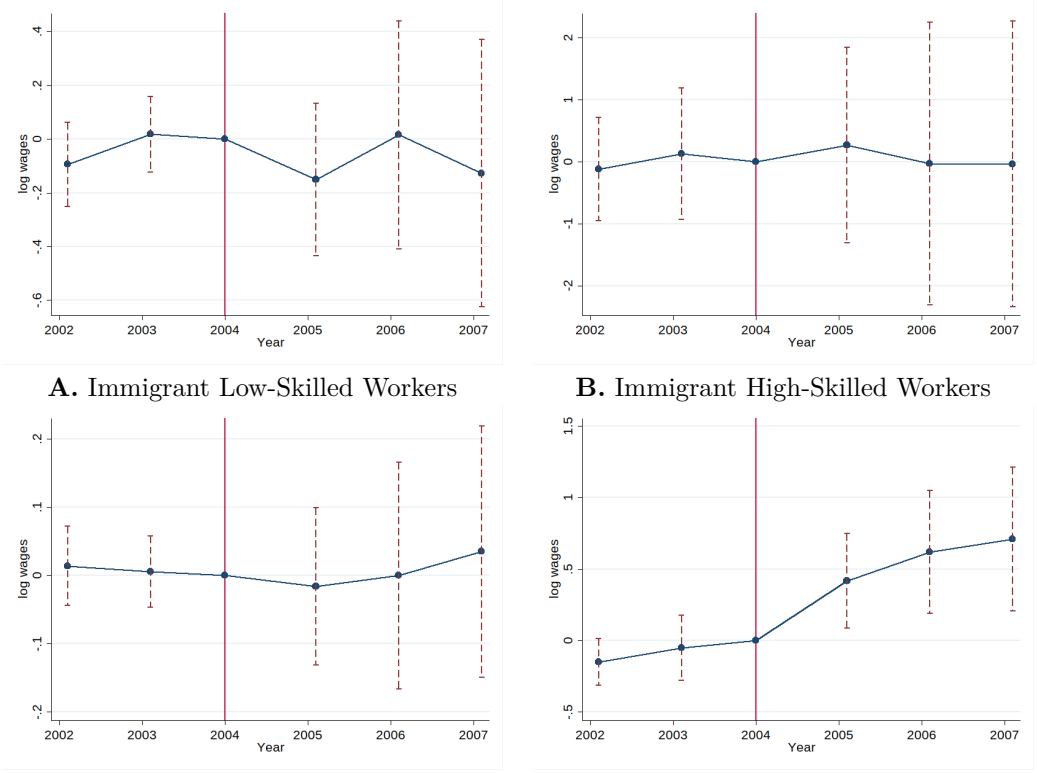
B. Immigrant Workers



C. Native Workers

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

Figure E.4: **Effects on Wages of Low- and High-Skilled Workers, dynamic difference-in-difference**



A. Immigrant Low-Skilled Workers

B. Immigrant High-Skilled Workers

C. Native Low-Skilled Workers

D. Native High-Skilled Workers

NOTE: This figure shows estimates from the dynamic difference-in-difference specification on wages in the formal sector. Panels A and B display results for low- and high-skilled immigrant workers, respectively. Panels C and D for low- and high-skilled native workers, respectively. Dashed lines indicate 95% confidence intervals of standard errors clustered at the province level. The vertical red line indicates the last period before the reform. Source: Own elaboration based on MCVL data.

F Robustness Checks

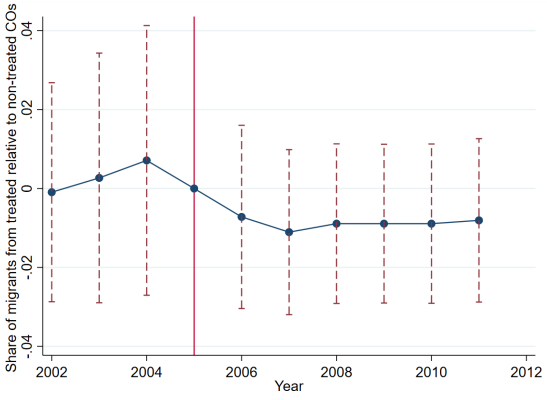
F.1 Magnet Effects: Additional results

In this section we investigate whether the results on the absence of magnet effects are affected by the new EU accession countries. As argued in the main text, Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia and Slovenia joined the EU in May 2004. However, EU members could delay, until 2011, the free mobility of workers with these countries (except Cyprus). Spain applied mobility restrictions until May of 2006 to dependent workers. Despite these restrictions, workers from new accession EU countries were not eligible to the amnesty program. Hence, given this particularity of non participation but also not completely free movement, it is worth checking whether results are different when excluding these new EU countries and also check how the main results look like when considering only these new EU countries as the control group.

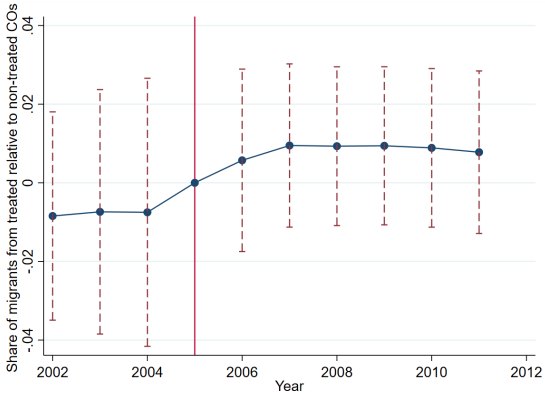
Figure F.1 shows two graphs. The first one repeats the right hand side graph of Figure 3 excluding new EU 2004 countries from the control group. The graph is very similar to the one in the main text. The graph on the right of Figure F.1, uses exclusively EU 2004 countries as control group. While there are some differences, between the two graphs, we cannot reject with confidence the null hypothesis that the amnesty program lead to magnet effects. As in the main text, if anything, it might have deterred new immigrants from entering Spain.

Figure F.1: **The Absence of Magnet Effects: The Role of New EU Countries that Accessed in 2004**

Panel A: **Treat. vs. Non-treat. (excl. EU 2004)**



Panel B: **Treat. vs. new EU 2004**



NOTE: Panel A excludes new EU 2004 countries from the control group. Panel B uses exclusively new EU 2004 countries as control group. Data come from the Municipal Register which covers both documented and undocumented immigrants.

F.2 Empirical Evidence: Additional results

In this appendix we present several robustness checks to our baseline estimates. First we re-estimate our baseline specifications witout controls. Second, we present our main estimates for an alternative sample of provinces that excludes the four largest provinces (Madrid, Barcelona, Sevilla and Valencia). Third, we also show the reduced form and 2SLS as discussed in Appendix Section D. 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 F.1: **Estimates of the Effect of the Reform on Payroll-tax Revenues**

	General Reg.	Self. emp.	Agricult.	Sea	Coal	Housekeeping	Accident	Unemp.	Total
Panel A: Baseline									
Δ Immigrants	3,870***	94.52*	186.6***	-3.915	12.07	186.0***	-16.95	-446.8	3,882***
in social security/pop.	(1,116)	(53.95)	(47.16)	(17.05)	(20.49)	(57.81)	(29.71)	(301.7)	(914.4)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.584	0.194	0.419	0.225	0.092	0.700	0.216	0.497	0.642
Panel B: Without controls									
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 C: Without 4 main provinces (Mad., Bcn., Val., Sev)									
Δ Immigrants	2,829***	142.1***	183.4***	-10.42	24.86	125.5**	-33.96	99.47	3,360***
in social security/pop.	(958.2)	(52.06)	(58.44)	(17.18)	(30.88)	(51.64)	(37.12)	(130.1)	(889.4)
Observations	46	46	46	46	46	46	46	46	46
R-squared	0.340	0.285	0.472	0.215	0.102	0.522	0.170	0.643	0.484
Panel D: Reduced form (ITT)									
Δ Immigrants	1,799***	15.31	61.80***	5.638	4.595	100.9***	1.290	-367.1**	1,621***
in social security/pop.	(361.5)	(22.27)	(19.19)	(6.825)	(7.51)	(18.45)	(11.57)	(146.5)	(305.9)
Observations	50	50	50	50	50	50	50	50	50
R-squared	0.611	0.148	0.256	0.236	0.092	0.828	0.210	0.624	0.597
Panel E: 2SLS (LATE)									
Δ Immigrants	5,525***	47.01	189.8***	17.31	14.11	310.0***	3.961	-1,127**	4,979***
in social security/pop.	(1,02)	(61.42)	(50.01)	(20.77)	(20.90)	(53.27)	(33.13)	(444.90)	(788.40)
Observations	50	50	50	50	50	50	50	50	50
F-test of excluded instruments	28.070	28.070	28.070	28.070	28.070	28.070	28.070	28.070	28.070

NOTE: This table shows the estimates of the payroll-tax 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 D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.2: **Estimates of the Effect of the Reform on Income Tax Revenues**

Dep. Var.:	Change in Tax returns	Computed change (in euros)	
	(1)	(2)	(3)
Panel A: Baseline			
Δ Immigrants	0.341**	196.7	439.8
in social security/pop.	(0.130)		
Observations	46		
R-squared	0.291		
Panel B: Without controls			
Δ Immigrants	0.321***	185.2	430.0
in social security/pop.	(0.099)		
Observations	46		
R-squared	0.211		
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)			
Δ Immigrants	0.317***	182.9	427.7
in social security/pop.	(0.115)		
Observations	42		
R-squared	0.317		
Panel D: Reduced form (ITT)			
Δ Immigrants	0.171**	98.6	279.8
in social security/pop.	(0.064)		
Observations	46		
R-squared	0.244		
Panel E: 2SLS (LATE)			
Δ Immigrants	0.443***	255.6	504.0
in social security/pop.	(0.140)		
Observations	46		
F-test of excluded instruments	26.710		

NOTE: This table shows the estimates of the contribution per regularized immigrant through the income tax. Column 2 uses the average wage and column 2 uses the entire distribution of wages of newly legalized immigrants to estimate the income tax contribution (from MCVL). Estimates are based on a difference-in-difference strategy, where province-specific pre-change linear trends are removed. ITT and LATE estimates are explained in detail in Appendix D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.3: **Estimates of the Effect of the Reform on Education Enrollment and Hospital Discharges**

Education enrolment (by level)							
	All	Kinder	Primary	Secondary	High-school		
Panel A: Baseline							
Δ Immigrants	-0.033	-0.012	-0.002	-0.012	-0.053		
in social security/pop.	(0.038)	(0.027)	(0.016)	(0.010)	(0.032)		
Observations	50	50	50	50	50		
R-squared	0.091	0.082	0.155	0.487	0.172		
Panel B: Without controls							
Δ Immigrants	-0.020	-0.003	-0.010	-0.017*	-0.035		
in social security/pop.	(0.035)	(0.023)	(0.011)	(0.010)	(0.027)		
Observations	50	50	50	50	50		
R-squared	0.006	0.000	0.016	0.031	0.055		
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	-0.042	-0.032	-0.001	-0.006	-0.051		
in social security/pop.	(0.043)	(0.033)	(0.016)	(0.012)	(0.036)		
Observations	46	46	46	46	46		
R-squared	0.108	0.128	0.193	0.545	0.168		
Panel D: Reduced form (ITT)							
Δ Immigrants	-0.024	-0.006	0.002	-0.012**	-0.033**		
in social security/pop.	(0.022)	(0.016)	(0.007)	(0.005)	(0.013)		
Observations	50	50	50	50	50		
R-squared	0.099	0.081	0.156	0.513	0.189		
Panel E: 2SLS (LATE)							
Δ Immigrants	-0.018	-0.000	0.005	-0.012	-0.072***		
in social security/pop.	(0.042)	(0.035)	(0.014)	(0.013)	(0.027)		
Observations	50	50	50	50	50		
F-test of excluded instruments	28.070	28.070	28.070	28.070	28.070		
Hospital discharges (by illness)							
	All	Tumors	Mental	Circulatory	Respiratory	Birth	Other
Panel A: Baseline							
Δ Immigrants	0.017	0.012	-0.004	-0.005	0.005	0.018	0.001
in social security/pop.	(0.079)	(0.014)	(0.006)	(0.016)	(0.023)	(0.024)	(0.066)
Observations	50	50	50	50	50	50	50
R-squared	0.032	0.061	0.196	0.065	0.144	0.072	0.066
Panel B: Without controls							
Δ Immigrants	0.023	0.010	-0.003	0.000	0.015	0.025	-0.025
in social security/pop.	(0.070)	(0.012)	(0.007)	(0.016)	(0.023)	(0.021)	(0.064)
Observations	50	50	50	50	50	50	50
R-squared	0.002	0.014	0.003	0.000	0.015	0.041	0.005
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.079	0.019	-0.005	-0.014	0.001	0.024	0.088
in social security/pop.	(0.098)	(0.015)	(0.007)	(0.017)	(0.026)	(0.020)	(0.069)
Observations	46	46	46	46	46	46	46
R-squared	0.111	0.056	0.156	0.068	0.059	0.269	0.114
Panel D: Reduced form (ITT)							
Δ Immigrants	0.012	0.012	0.0013	0.004	0.014	0.008	-0.018
in social security/pop.	(0.044)	(0.009)	(0.004)	(0.010)	(0.012)	(0.014)	(0.036)
Observations	50	50	50	50	50	50	50
R-squared	0.032	0.090	0.194	0.065	0.173	0.067	0.072
Panel E: 2SLS (LATE)							
Δ Immigrants	-0.036	0.025	0.009	0.006	0.006	0.018	-0.075
in social security/pop.	(0.084)	(0.017)	(0.009)	(0.023)	(0.026)	(0.027)	(0.079)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	28.070	28.070	28.070	28.070	28.070	28.070	28.070

NOTE: This table shows the estimates of the effect of immigrant regularization on education enrollment and hospital discharges 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 D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.4: **Estimates of the Effect of the Reform on Formal Employment**

		Δ Employment - All					
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	0.540***	0.056	0.484***	0.080	-0.024**	0.463***	0.021***
in social security/pop.	(0.174)	(0.105)	(0.097)	(0.104)	(0.010)	(0.095)	(0.005)
Observations	50	50	50	50	50	50	50
R-squared	0.380	0.236	0.810	0.243	0.191	0.802	0.458
Panel B: Without controls							
Δ Immigrants	0.458***	-0.072	0.530***	-0.039	-0.033***	0.509***	0.021***
in social security/pop.	(0.126)	(0.097)	(0.083)	(0.096)	(0.010)	(0.082)	(0.005)
Observations	50	50	50	50	50	50	50
R-squared	0.330	0.014	0.767	0.005	0.108	0.755	0.393
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.446**	-0.120	0.566***	-0.091	-0.030**	0.548***	0.019***
in social security/pop.	(0.169)	(0.115)	(0.106)	(0.112)	(0.011)	(0.104)	(0.0058)
Observations	46	46	46	46	46	46	46
R-squared	0.268	0.031	0.747	0.021	0.068	0.740	0.276
Panel D: Reduced form (ITT)							
Δ Immigrants	0.158***	-0.0106	0.168***	0.001	-0.012**	0.161***	0.007***
in social security/pop.	(0.053)	(0.039)	(0.029)	(0.037)	(0.006)	(0.028)	(0.002)
Observations	50	50	50	50	50	50	50
R-squared	0.208	0.230	0.679	0.228	0.212	0.672	0.397
Panel E: 2SLS (LATE)							
Δ Immigrants	0.528***	-0.035	0.564***	0.004	-0.039*	0.539***	0.025***
in social security/pop	(0.146)	(0.123)	(0.090)	(0.114)	(0.020)	(0.091)	(0.006)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	25.600	25.600	25.600	25.600	25.600	25.600	25.600

NOTE: This table shows the estimates of 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 D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.5: **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	0.119*	-0.063	0.182***	-0.036	-0.027**	0.173***	0.009**
in social security/pop.	(0.071)	(0.039)	(0.043)	(0.035)	(0.010)	(0.041)	(0.004)
Observations	50	50	50	50	50	50	50
R-squared	0.182	0.230	0.779	0.229	0.349	0.772	0.261
Panel B: Without controls							
Δ Immigrants	0.119**	-0.093**	0.212***	-0.057	-0.036**	0.202***	0.010***
in social security/pop.	(0.057)	(0.038)	(0.039)	(0.039)	(0.016)	(0.037)	(0.003)
Observations	50	50	50	50	50	50	50
R-squared	0.149	0.122	0.706	0.057	0.162	0.695	0.214
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	0.122	-0.080*	0.202***	-0.065	-0.015	0.193***	0.009**
in social security/pop.	(0.0802)	(0.045)	(0.053)	(0.039)	(0.011)	(0.050)	(0.005)
Observations	50	50	50	50	50	50	50
R-squared	0.128	0.080	0.624	0.069	0.032	0.613	0.156
Panel D: Reduced form (ITT)							
Δ Immigrants	0.033	-0.037**	0.070***	-0.024*	-0.013***	0.067***	0.003***
in social security/pop.	(0.022)	(0.015)	(0.011)	(0.014)	(0.005)	(0.010)	(0.001)
Observations	50	50	50	50	50	50	50
R-squared	0.122	0.277	0.739	0.258	0.381	0.736	0.230
Panel E: 2SLS (LATE)							
Δ Immigrants	0.112*	-0.122**	0.234***	-0.079*	-0.044**	0.224***	0.011***
in social security/pop.	(0.062)	(0.056)	(0.034)	(0.048)	(0.018)	(0.034)	(0.003)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	25.60	25.60	25.60	25.60	25.60	25.60	25.60

NOTE: This table shows the 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 D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.6: **Estimates of the Effect of the Reform on Employment**

		Δ Employment					
	Δ Total Emp.	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	-0.520**	-0.405*	-0.116	-0.481**	0.076	-0.373**	0.257***
in social security/pop.	(0.215)	(0.238)	(0.156)	(0.188)	(0.248)	(0.153)	(0.084)
Observations	50	50	50	50	50	50	50
R-squared	0.147	0.178	0.114	0.276	0.023	0.247	0.223
Panel B: Without controls							
Δ Immigrants	-0.482***	-0.272	-0.210	-0.317	0.0453	-0.383**	0.174**
in social security/pop.	(0.172)	(0.252)	(0.178)	(0.193)	(0.217)	(0.174)	(0.073)
Observations	50	50	50	50	50	50	50
R-squared	0.086	0.021	0.019	0.045	0.002	0.090	0.043
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)							
Δ Immigrants	-0.579***	-0.418*	-0.161	-0.324	-0.093	-0.246*	0.085
in social security/pop.	(0.171)	(0.229)	(0.131)	(0.227)	(0.193)	(0.134)	(0.081)
Observations	46	46	46	46	46	46	46
R-squared	0.104	0.045	0.010	0.042	0.006	0.039	0.011
Panel D: Reduced form (ITT)							
Δ Immigrants	-0.156	0.140	-0.295*	-0.256	0.395**	-0.401***	0.105
in social security/pop.	(0.296)	(0.358)	(0.159)	(0.280)	(0.180)	(0.132)	(0.121)
Observations	50	50	50	50	50	50	50
R-squared	0.148	0.127	0.186	0.230	0.080	0.310	0.139
Panel E: 2SLS (LATE)							
Δ Immigrants	-0.240	0.215	-0.456*	-0.395	0.610*	-0.618***	0.163
in social security/pop.	(0.397)	(0.540)	(0.267)	(0.361)	(0.321)	(0.218)	(0.161)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	23.520	23.520	23.520	23.520	23.520	23.520	23.520

NOTE: This table shows the estimates of 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 D. Regressions are weighted by population.Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

Table F.7: **Estimates of the Effect of the Reform on Wages**

		Δ log wages					
	Δ Total	Natives	Immigrants	Nat. LS	Nat. HS	Imm. LS	Imm. HS
Panel A: Baseline							
Δ Immigrants	0.166	0.213*	-0.732***	0.189	0.394	-1.014***	1.775
in social security/pop.	(0.102)	(0.111)	(0.261)	(0.115)	(0.263)	(0.277)	(1.183)
Observations	50	50	50	50	50	50	50
R-squared	0.328	0.390	0.294	0.371	0.217	0.322	0.160
Panel B: Without controls							
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	0.018	0.033	-0.467	0.058	-0.082	-0.628*	0.851
in social security/pop.	(0.076)	(0.074)	(0.309)	(0.074)	(0.223)	(0.342)	(0.960)
Observations	46	46	46	46	46	46	46
R-squared	0.001	0.003	0.050	0.009	0.003	0.078	0.015
Panel D: Reduced form (ITT)							
Δ Immigrants	0.270***	0.326***	-0.336	0.297***	0.488***	-0.477**	0.604
in social security/pop.	(0.0659)	(0.0687)	(0.208)	(0.0698)	(0.161)	(0.225)	(0.741)
Observations	50	50	50	50	50	50	50
R-squared	0.417	0.498	0.232	0.469	0.265	0.218	0.116
Panel E: 2SLS (LATE)							
Δ Immigrants	0.417***	0.503***	-0.519**	0.458***	0.754***	-0.736***	0.993
in social security/pop.	(0.123)	(0.135)	(0.265)	(0.132)	(0.247)	(0.266)	(1.043)
Observations	50	50	50	50	50	50	50
F-test of excluded instruments	33.590	33.590	33.590	33.590	33.590	33.590	33.590

NOTE: This table shows the estimates of 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 D. Regressions are weighted by population. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004). Robust standard errors are reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

F.3 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 F.8. The sum of the point estimates in this table should coincide with the estimate in column four of Table F.6. 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.

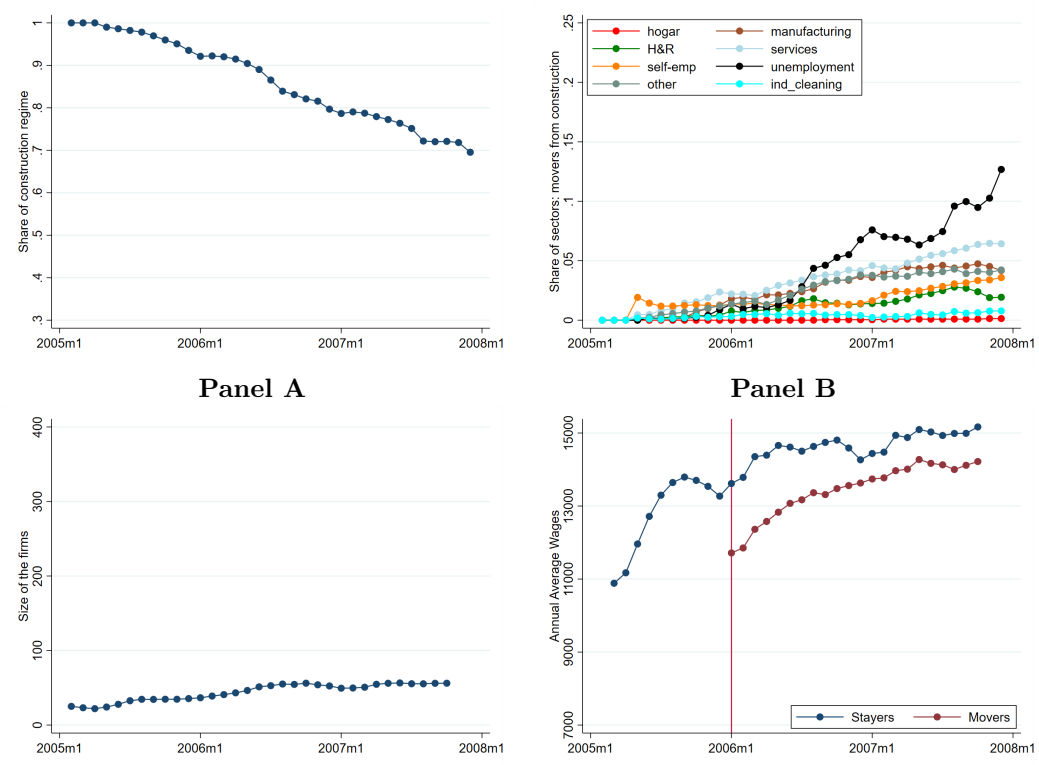
Table F.8: **Estimates of the Effect of the Reform on Employment by Sectors**

	Δ Employment Native Low Skilled		
	High-immigrant sectors	Low-immigrant sectors	Public administration
Panel A: Baseline			
Δ Immigrants	-0.349**	-0.157	0.060
in social security/pop.	(0.156)	(0.117)	(0.085)
Observations	50	50	50
R-squared	0.299	0.194	0.220
Panel B: Without controls			
Δ Immigrants	-0.139	-0.254*	0.094
in social security/pop.	(0.162)	(0.146)	(0.108)
Observations	50	50	50
R-squared	0.010	0.050	0.013
Panel C: Without 4 main provinces (Mad., Bcn., Val., Sev)			
Δ Immigrants	-0.150	-0.293*	0.151
in social security/pop.	(0.132)	(0.157)	(0.094)
Observations	46	46	46
R-squared	0.011	0.063	0.032
Panel D: Reduced form (ITT)			
Δ Immigrants	-0.321	-0.0910	0.186
in social security/pop.	(0.211)	(0.156)	(0.113)
Observations	50	50	50
R-squared	0.293	0.183	0.261
Panel E: 2SLS (LATE)			
Δ Immigrants	-0.497*	-0.141	0.288
in social security/pop.	(0.272)	(0.219)	(0.184)
Observations	50	50	50
F-test of excluded instruments	25.740	25.740	25.740
	Share in sector		
Immigrants	0.740	0.231	0.029
Natives	0.511	0.365	0.123

NOTE: This table shows the estimates of 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. Panels A, D and E include all controls (political alignment, coastal dummies, construction pre-reform and share non EU-15 immigrants in 2004).ITT and LATE estimates are explained in detail in Appendix D. Robust standard errors reported. * significant at the 0.10 level; ** significant at the 0.05 level; *** significant at the 0.01 level.

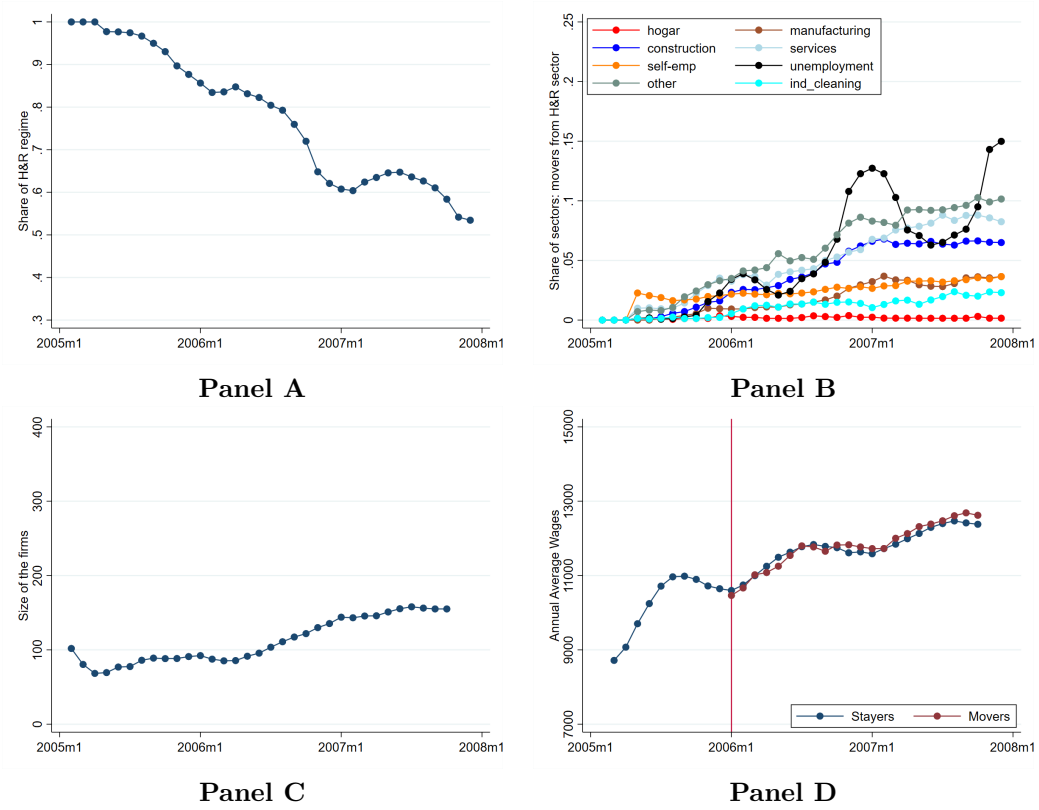
F.4 Newly Legalized Immigrants and Sector Switching

Figure F.2: **Newly Legalized Immigrants and Sector Switching: Construction**



NOTE: 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 workers legalized through the construction who remain in the sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

Figure F.3: **Newly Legalized Immigrants and Sector Switching: H&R**



NOTE: 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 workers legalized through the H&R who remain in the sector. The red vertical line indicates the beginning of 2006 where we observe that most movements took place.

G 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.

H Previous Immigrant Regularizations

Most immigrant regularizations before the reform that we study in this paper 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 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 one that took place in 1996 in which 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.