CRISEI

Centro di Ricerca Interdipartimentale in Sviluppo Economico e Istituzioni

Discussion Paper Series

The Labor Market Effects of an Unexpected Amnesty for Undocumented Workers

Salvatore Carrozzo

CRISEI - Università di Napoli - Parthenope

Università degli Studi di Napoli - Parthenope

CRISEI

The Labor Market Effects of an Unexpected Amnesty for Undocumented Workers

Salvatore Carrozzo*

* University Parthenope of Naples

The Labor Market Effects of an Unexpected Amnesty

for Undocumented Workers *

Salvatore Carrozzo [†]

Abstract

This paper studies the labor market effects of regularization programs on amnestied migrants' job competitors. Exploiting the 2002 unexpected Italian program, I find that the least productive job competitors experience a lower probability of being regularly employed in the very few months following the amnesty implementation. To explain the results, I develop a theoretical model showing that bundling strict application requirements and new penalties for hiring undocumented workers lowers the regular employment of amnestied workers' job competitors.

JEL Classification codes: F22, J23, J42, J46, J61

Keywords: Immigration, Amnesty, Labor Markets

[†]Department of Business and Economics (DISAE), University of Naples "Parthenope". Email: salvatore.carrozzo@collaboratore.uniparthenope.it.

1 Introduction

Over the last ten years, the discussion on granting an amnesty to undocumented migrants has been particularly lively in Western political debate since both the European Union and the US have experienced a surge in irregular migration[1]. In 2009, United Nations estimated around 50 million the total number of undocumented migrants around the world (UNODC, 2009). In particular, the number of undocumented migrants hosted in the EU-27 was between 1.9 and 3.8 million in 2008 (Kovacheva et al., 2011). Although regularization policies have a positive effect on the labor market outcomes of undocumented migrants (Amuedo-Dorantes and Bansak (2011), Amuedo-Dorantes et al. (2007), Bahar et al. (2021), Baker (2015), Cobb-Clark et al. (1995), Cascio and Lewis (2019), Devillanova et al. (2017), Di Porto et al. (2021), Freedman et al. (2018), Kaushal (2006), Mastrobuoni and Pinotti (2015), Monras et al. (2022), Pinotti (2017)), there are few papers focusing on the side effects of the amnesties on the labor market outcomes of amnestied workers' job competitors. Monras et al. (2022) do not find any meaningful effect of regularization on the formal employment and wages of amnestied workers' job competitors in Spain in 2005. They do find only a non-meaningful decrease in the total employment when they use a 2SLS strategy to identify the effect. Instead, Bahar et al. (2021) and Di Porto et al. (2021) find a small but meaningful negative effect on the employment of regularized workers' job competitors in Colombia in 2018 and in Italy in 2002, respectively. As of now, there is no clear evidence for possible side effects of regularization. To solve this puzzle, this paper presents a novel theoretical framework showing the trade-off between native and foreign workers and, then, assesses the model predictions exploiting the 2002 Italian amnesty program.

In the first part of the paper, I develop a model of profit maximization with formal and informal labor inputs nesting two other imperfect substitute labor inputs: foreigners and natives[2].

---

[1]See Orrenius and Zavodny (2004) for a discussion about the characteristics and the goals of an amnesty.

[2]The model is in the same fashion as De Paula and Scheinkman (2011) and Ulyssea (2018).

The model shows that bundling an amnesty with new penalties for hiring undocumented workers may lead firms to substitute native workers with amnestied workers in formal occupations and vice versa in informal occupations[3]. In particular, the change in the cost of employing undocumented migrants may lead to a shift in the demand for native and foreign workers in both formal and informal occupations. Finally, the model predicts that the substitution effect occurs between the least productive native workers and the amnestied workers.

To test the theoretical predictions in the second part of the paper, I exploit the 2002 Italian amnesty program which collected around .7 million applications and increased the penalties for hiring undocumented migrants. The Italian regularization is not the only case of massive regularization bundled with an increase in the penalties for hiring undocumented workers since Spain and the U.S. have implemented a similar program in 2005 and 1986 providing a residence visa to around .6 million and 3 million undocumented migrants, respectively. However, the Italian case is the only case where the announcement of the amnesty was very close to its implementation. Indeed, the policy was announced only two months before the implementation, while the Spanish and the American amnesties were announced eight and twelve months before the implementation respectively. The unexpected timing of the regularization is key to lower the probability of preventive behavior from involved economic agents before the opening of the amnesty window[4]. Therefore, the Italian case is a nice setting to evaluate the side effects of an amnesty program on the labor market outcomes of amnestied migrants' job competitors.

The empirical investigation uses a sample of individual data collected from social security records between 2001 and 2002. Data has information about the entire work history of each individual and includes information on socio-demographic characteristics, employment con-

---

[3]I define native workers as native-born workers or foreign-born workers holding a permanent visa, while I define foreign workers as foreign-born workers holding a temporary visa or without visa. In the model, I assume that foreigners holding a temporary visa do not compete for jobs with undocumented migrants

[4]For instance, workers might move across occupations before the opening of the amnesty window to offset potential negative effects on their labor market outcomes.

tracts, and the characteristics of the relative employer. I narrow the main analysis to just three months following the policy implementation since the short-run changes in the labor demand is the object of interest. However, I also provide some empirical evidences in the long run. The empirical model studies the relationship between the pool of regularized migrants and the labor market outcomes of their job competitors at the labor market level. The results show that native workers experience a lower probability of being formally employed following the program, while natives' wages increase at the market level and do not change at the individual level. This finding highlights that the most productive workers are less likely to experience negative effects following the amnesty, while the least productive workers are more likely to experience a lower probability of being employed with a formal contract. Finally, I show that the effect is greater within markets with a high concentration of low-skilled native workers.

This paper contributes to the discussion about the effects of amnesty programs on the job competition. The research has mainly focused on evaluating the impact of programs on the post-amnesty outcomes of regularized workers (Amuedo-Dorantes and Bansak (2011), Devillanova et al. (2017), Freedman et al. (2018), Kaushal (2006), Mastrobuoni and Pinotti (2015), Pinotti(2017) among others). Recently, three papers have included the effect on labor market outcomes of natives in the impact evaluation of an amnesty. Bahar et al. (2021) show the effect of the 2018 Colombian amnesty for Venezuelan refugees on the labor market outcomes of natives using monthly observations. They find a small negative effect on the formal employment of native workers and a null effect on wages. However, amnesty rules and the skill composition of Venezuelan workers differ from the Italian case. In particular, undocumented workers could apply for the amnesty without being already employed. Second, Di Porto et al. (2021) also use the 2002 Italian amnesty and social security data to show the effects on labor market outcomes. They do find a small effect on the employment of native workers. However, they do not explain

why firms decide to hire less native workers and they narrow the analysis at the firm level without showing the labor market dynamics. Finally, Monras et al. (2022) find a reduction in the employment of native workers and a non-meaningful positive effect on wages. Their analysis looks only at the provincial labor market without exploiting other dimensions, like firm size and industry. Therefore, the provincial level analysis might be not enough to detect negative effects. Furthermore, since the amnesty was implemented in February 2005 but announced in August 2004, an adverse effect of the regularization on the legal employment of amnestied workers' job competitors might have taken place before the policy implementation.

Finally, the theoretical framework contributes to the literature about labor substitution between formal and informal workers. Recent papers investigate the effects of an increase in sanctions for hiring undocumented workers and/or enforcing border patrols using search and matching models (Albert (2019), Chassamboulli and Peri (2015), Machado (2017), Ulyssea (2018)). Monras et al. (2022) is the only paper studying the effects of amnesty in a static framework. I complement their analysis by including an endogenous probability of detecting a worker with a specific productivity level. Furthermore, the theoretical framework developed in this paper considers an economy where firms exploit informal labor to hide part of the revenues. Since firms increase their profit margin by selling part of production without invoice, tax evasion, a large gap between the revenues and the number of employees may increase the probability of being inspected from the taxation authority. Therefore, I split the production function in two labor inputs, formal and informal labor, that produce an homogeneous good sold in two different markets, formal and informal.

The reminder of the paper is organized as follows. Section 2 develops the theoretical model. Section 3 sketches out the background of the amnesty. Section 4 presents the data and shows some descriptive evidence. Section 5 discusses the empirical strategy. Section 6 shows the

results. Section 7 provides some robustness checks. Finally, Section 8 concludes.

2 Theoretical Framework

2.1 The profit maximization model

This section offers a framework for understanding a firm's decision of declaring a worker when firms are free to hire foreign workers and experience higher penalties for hiring undocumented migrants than for hiring undeclared native workers. In this paper, I use the following definition: undocumented foreign workers are migrants working without a work visa; documented foreign workers are immigrants with a work visa; undeclared native workers are either citizens or permanent resident employed informally; and, declared native workers are either citizens or permanent residents employed formally. Given the higher labor cost of employing illegal migrants, amnesty predictably will end up increasing the legal employment of foreigners. However, the effect of the policy on native workers is not trivial. On one hand, the high demand of foreign workers might stem from a labor shortage in some occupations. In this case, the effect on incumbent workers would be null since firms regularize workers without substituting incumbent workers with foreign workers in legal employment. On the other hand, firms might substitute legal incumbent workers with amnestied workers since the labor cost of hiring an illegal worker is higher than the cost of hiring an undeclared worker. The following theoretical framework summarizes both intuitions showing how the substitution occurs.

I assume that a specific-market representative firm produces a homogeneous good sold both on the formal and on the informal market. The assumption of splitting the production in formal and informal production relies on the fact that the taxation authority is more likely to inspect firms with a large gap between formal production and total formal employment. A large gap

between formal sold quantities and total formal employment may stem from the employment of workers without a formal contract and, so, those firm may be more likely to get inspected. Therefore, firms employing informal workers may decide to sell products without invoice to prevent inspections. This assumption is quite realistic in low-added-value small and medium firms which represents the bulk of amnesty applicants and the bulk of fare dodgers.

The firm demands for formal and informal workers to produce in both markets. Each labor input contributes with its own productivity and labor inputs are imperfect substitutes in production. The labor supply is divided in two groups of workers, foreigners and natives, and is inelastic. Penalties for hiring undocumented workers are larger than the ones for hiring undeclared workers. Finally, since I am interested in studying the effect in the very short run after the policy implementation, labor and good prices are fixed[5].

I begin by defining the production function of both formal and informal output in the following way:

$$Y_I = F_I(\overline{\theta^M}, \overline{\theta^N}) = \left(\int_0^{\overline{\theta^M}} (H(\theta)^M)^\beta \, d\theta + \int_0^{\overline{\theta^N}} (H(\theta)^N)^\beta \, d\theta \right)^{\frac{1}{\beta}} \tag{1}$$

$$Y_F = F_F(\overline{\theta^M}, \overline{\theta^N}) = \left(\int_{\overline{\theta^M}}^1 (H(\theta^M)^\beta \, d\theta + \int_{\overline{\theta^N}}^1 (H(\theta)^N)^\beta \, d\theta \right)^{\frac{1}{\beta}} \tag{2}$$

The technology of both productions is represented by a constant elasticity of substitution (CES) production function where β represents the elasticity of substitution. Formal and informal productions, Y_F and Y_I, are function of a continuum of labor inputs with different productivity level. Assuming that foreign and native workers hold a productivity level between zero and one, the informal production is a function of the hours spent at work by all foreigners with

---

[5]For a discussion on the general equilibrium effects of the amnesty, I remind to Clark et al. (1995) and Monras (2020) among others.

a productivity level below $\overline{\theta^M}$ and all natives with a productivity level below $\overline{\theta^N}$, whereas a firm employs all the workers with a productivity level above those thresholds in the formal production[6]. The total amount of hours spent at work by each group of workers with a productivity θ, - where j is either M or N - is the following:

$$H(\theta)^j = h(\theta)^j L(\theta)^j \tag{3}$$

where $h(\theta)^j$ and $L(\theta)^j$ are the number of hours spent at work by each worker with a productivity θ and the relative labor supply of workers, respectively. I assume that hours are $h(\theta)^j = c * \theta^j$, where c is maximum number of working hours in a large enough time span[7]. The labor supply function of each θ is $L(\theta)^j = L^j * f(\theta^j)$, where L^j is the total labor supply of each group j and $f(\theta^j)$ is the corresponding density. Therefore, the most productive workers spend more time at work than the lowest productive workers.

The representative firm decides to declare workers when the net gain from hiring them without a formal contract is greater than hiring them with an informal contract. The cost of declaring a worker is equal to $w_I(1+t)H_F$, while the cost of hiring an undeclared worker is $w_I H_I + \alpha E[f^j; \theta^j]$. w_I is the unique wage rate in the market, t represents the contribution rate, α is the probability of being inspected, and, $E[f^j; \theta^j]$ is the expected fine when employing undeclared workers of group j with different productivity levels.

I assume that expected fines depend on the probability of detecting an undeclared worker during an inspection. Since the most productive workers spend more time at work, the probability of being detected is increasing in productivity[8]. I assume that the probability of detecting

---

[6]I assume that the representative firm has a full information on the productivity of each worker. In Appendix B, I present a model where the firm does not fully observe the productivity of each worker.

[7]In the empirical specification, the time span is one month.

[8]Employers do not know when the next inspection will be, if any. Hence, employers attach higher probabilities to the most productive workers since they will work more hours within the firm.

an informal worker during an inspection is linear in the productivity:

$$p(\theta^j) = \theta^j \tag{4}$$

The function of the expected fine is the following:

$$E[f^j; \theta^j] = \int_0^{\overline{\theta^j}} f^j p(\theta^j) L(\theta)^j \, d\theta \tag{5}$$

$L(\theta)$ is the sum of all workers with a specific productivity level. Finally, f^j is the fine for hiring an informal worker belonging to group j.

Finally, the firm maximizes the following profit function:

$$\max_{\overline{\theta^M}, \overline{\theta^N}} \quad \pi = p_1 F_I(\overline{\theta^M}, \overline{\theta^N}) + p_2 F_F(\overline{\theta^M}, \overline{\theta^N}) - w_I(1+t)H_F - w_I H_I - \alpha(E[f^M] + E[f^N]) \tag{6}$$

p_1 and p_2 are the prices of the informal and formal goods, respectively. H_F, H_I, $E[f^M]$ and $E[f^N]$ are also functions of $\overline{\theta^M}$ and $\overline{\theta^N}$, but I do not include the additional notation.

To solve the maximization problem, I assume, without loss of generality, that the labor supply of both foreigners and natives is distributed uniformly across productivity levels . The representative firm solves the problem by finding the marginal productivity thresholds, $\overline{\theta^M}$ and $\overline{\theta^N}$, such that firm declares all the foreign workers above $\overline{\theta^M}$ and all the natives above $\overline{\theta^N}$. The optimal productivity thresholds are the following:

$$\overline{\theta^j} = \frac{((p_1 Y_I^{1-\beta} - p_2 Y_F^{1-\beta}))^{\frac{1}{1-\beta}} c^{\frac{\beta}{1-\beta}}}{\beta^{1-\beta}(\alpha f^j - cw_I t)^{\frac{1}{1-\beta}}} \frac{c^{\frac{\beta}{1-\beta}}}{L^j} \qquad j = M, N \tag{7}$$

The ratio between $\overline{\theta^N}$ and $\overline{\theta^M}$ is:

$$\frac{\overline{\theta^N}}{\overline{\theta^M}} = \left(\frac{\alpha f^M - cw_I t}{\alpha f^N - cw_I t}\right)^{\frac{1}{1-\beta}} \frac{L^M}{L^N} \tag{8}$$

All pairs $(\overline{\theta_M}, \overline{\theta_N})$ that solve equation (8) maximize the firm's profit. An increase in the fine for hiring undocumented workers leads to a greater ratio. The increase might be due to a higher threshold for native workers and/or a lower threshold for foreign workers [9].

2.2 The increase in the fine for hiring undocumented foreign workers

Now, I assume that the two output levels are fixed before and after the policy implementation and equal to $\overline{Y^F}$ and $\overline{Y^I}$ [10]. I use (8) and (1) to find the thresholds. The following equation describes the closed form solution for both thresholds:

$$\overline{\theta_I^j} = \frac{\overline{Y_I}^{\frac{\beta}{\beta+1}} (\alpha f^{-j} - cw_I t)^{\frac{1}{1-\beta}} (\beta+1)^{\frac{1}{1+\beta}}}{cL^j [(\alpha f^M - cw_I t)^{\frac{\beta+1}{1-\beta}} + (\alpha f^N - cw_I t)^{\frac{\beta+1}{1-\beta}}]^{\frac{1}{1+\beta}}} \tag{9}$$

where f^{-j} is the fine for hiring undeclared native workers when I consider the productivity threshold of foreign workers, and otherwise.

To study the sign of the derivative with respect to f^M, I use logarithms to keep the notation shorter. I obtain the following derivatives:

$$\frac{\partial ln(\overline{\theta_I^M})}{\partial f^M} = -\frac{\alpha (\alpha f^M - cw_I t)^{\frac{2\beta}{1-\beta}}}{(\alpha f^M - cw_I t)^{\frac{\beta+1}{1-\beta}} + (\alpha f^N - cw_I t)^{\frac{\beta+1}{1-\beta}}} \frac{1}{1-\beta} \tag{10}$$

---

[9]In section 4, I proxy this ratio, named threshold gap, with both the employment and wage gap between foreigners and natives to present some descriptive statistics.

[10]This assumption is not so binding since the amnesty was unexpected and firms might have no time to react by changing the production, at least in the very short run.

$$\frac{\partial ln(\overline{\theta}_I^N)}{\partial f^M} = \frac{\alpha(\alpha f^N - cw_It)^{\frac{\beta+1}{1-\beta}}}{(1-\beta)(\alpha f^M - cw_It)[(\alpha f^M - cw_It)^{\frac{\beta+1}{1-\beta}} + (\alpha f^N - cw_It)^{\frac{\beta+1}{1-\beta}}]} \tag{11}$$

An increase in the penalties for hiring undocumented foreign workers leads to a higher productivity threshold for natives and a lower productivity threshold for foreigners. Firms react by regularizing more undeclared foreign workers and increasing the number of undeclared natives to keep on producing the same level of output in the formal and informal market[11].

2.3 The Quota level and the Amnesty

This subsection considers the case in which government implements only an amnesty without increasing penalties for hiring undocumented workers. The model considers two periods. In the first periods, the representative firm maximizes its profit by taking into account immigration quotas. In the second period, government allows employers to regularize their undocumented employees.

To solve the maximization problem in the first period, I assume that foreign workers filling the quotas are more productive than undocumented workers. In this way, the quota system sets exogenously the foreign productivity threshold, $\overline{\theta_Q^M}$. Therefore, the representative firms solves the maximization problem by choosing the native productivity threshold. The following equation describes the solution:

$$\overline{\theta^N} = \overline{\theta_Q^M}\left(\frac{\alpha f_M - cw_It}{\alpha f_N - cw_It}\right)^{\frac{1}{1-\beta}}\frac{L_M}{L_N} \tag{12}$$

In the second period, government implements the amnesty and firms may select a new

---

[11]The result does not consider the cost of regularizing a migrant since I am considering the increase in the fine and the amnesty jointly. In the case of no-zero regularization cost, the effect is smaller.

couple $(\overline{\theta^M}, \overline{\theta^N})$. However, firms do not have any incentive to deviate from the equilibrium $(\overline{\theta_Q^M}, \overline{\theta^N})$. Therefore, implementing an amnesty without changing the price ratio may not be enough to convince employers to regularize their undocumented employees.

3 Background

During the 1970s, Italy changed from being a sending country to being a receiving country (King, 1993). The surge of the gross domestic product and the contemporary reduction of the residence permits from France and Germany paved the way to an increase in the labor supply of foreigners. The total number of foreigners increased from 600,000 in 1991 to 1.5 million in 2002. Up to the late 1980s, a 1930 law and governmental instructions ruled the status and the relative rights of migrants in Italy. In 1986, the Italian Government changed the migration law to avoid huge inflows of undocumented migrants and to regulate the status of different types of migrants. Furthermore, a quota system was introduced to regulate the demand of foreign workers.

The quota system was not effective to prevent the entrance of undocumented migrants in Italy. As already explained in Pinotti (2017) and Cuttitta (2008), employers are used to exploit the quota system to regularize undocumented workers following a training period. Yet, quotas were too low to provide residence permits to all migrants working in the country [12]. Therefore, Italian Governments implemented four amnesties in 1990, 1995, 1998, and 2002 to provide a legal status to undocumented immigrants [13]. The largest regularization policy was in 2002 when around 700,000 undocumented migrants applied for the amnesty and around 640,000 of them were regularized. Around half of the amnestied foreigners was working in the private

---

[12]Pinotti shows that the applications were 610,239 and the quota was set to 170,000 in 2007. However, these numbers are underestimated since not all migrants have a sponsor in the country.

[13]Law 39/90, law 489/95, law 1998, law 189/02 and 222/02

sector.

Initially, the 2002 amnesty was targeted only at care-givers and domestic workers (Law 189/02). Yet, the Government was concerned about the possibility of cheating on the actual occupation of amnesty applicants since the immigration quota did not match the labor demand and the new migration law increased the penalties for both undocumented workers and their employers[14]. Casadio (2001) wrote that entrepreneurs said publicly they would hire undocumented migrants since immigration quotas were too low for the firms in the Northern and Central Italy and zero for the firms in the Southern Italy. Moreover, the new migration changed and added the following penalties: the administrative penalty for hiring undocumented migrants changed from a 3,000 euro for all the undocumented employees to a 5,000 euro per each worker; employing undocumented migrants became criminal offense punishable by imprisonment from three to twelve months for hiring ; extension of the time spent in jail for illegal migrants. Therefore, after the approval of the new migration law in July, government started to discuss about an amnesty for undocumented workers. The most amnesty-skeptic parties of the majority government, *Lega Nord* and *Alleanza Nazionale*, agreed on the characteristics of the amnesty, Law 222/02, only in September just a few days before the entry into force of the new penalties for hiring undocumented migrants (Law 189/02).

Undocumented foreign workers can be regularized by employers if they have been working within the same firm for at least three months before September 11 2002 and do not have any criminal record. Since most of undocumented worker never had a formal job in any register, *Prefetture*, provincial offices of the Minstry of the Interior, check whether migrants were in Italy just before the regularization and did not have another regular occupation [15]. To apply

---

[14]Pinotti (2017) shows that the share of males getting the residence permit as care-giver or domestic workers was anomalous with respect the actual supply of males in those occupations.

[15]The same controls applied in Spain where undocumented workers can submit the regularization application if and only if they had been residing in Spain before August 2004.

for the amnesty, employers have to fill a form with the personal information of both applicants and delivering it to the post offices between September 11 and November 11[16]. The regularization application must include also a payment of 700 euro fee and the employment contract. In particular, the employment contract must be an open-ended contract or a fixed-term contract lasting, at least, one year.

Table 1 shows a summary of the regularization. The total number of applications was around 700,000 and number of residence permits issued by the government was around 640,000. The rate of rejection is around 7% of the total applications and is quite common to all the amnesties. The largest part of rejections, 43%, were labeled as "archived". That case applied when the applicants, employer and employee, did not show up the day of the signature of the employment contract. However, immigrants could still get the residence permit if they could demonstrate that employers did not show up for a fair reason (death of the employer or layoff). Finally, 20% and 37% of the rejected applications ended with either a repatriation or a litigation, respectively.

Figure 1 shows the distribution of applications by province. The demand for the amnesty is higher among wealthier provinces. In particular, the North of Italy represents the largest share of the total applications. Figure 2 shows the ratio of the applications to the former legal foreign residents within the province. The Southern provinces display the largest values of the ratio which is an evidence of the ban of those provinces from the 2001 immigration quotas. In particular, the province of Salerno experiences 1.6 applications for the amnesty per former legal foreign resident.

Figure 3 shows the share of applicants by industry across regions[17]. The top-left figure shows the largest industry to apply for the amnesty within a region, while, the top-right and the

---

[16]Regularization involved several public offices as post offices, *Prefetture*, social security offices. These offices took a month for preparing the forms. Therefore, post offices started to collect applications only from October

[17]Figure uses data on social security records which do not collect information on blue collars of both agricultural and fishing industries.

bottom figures show the applications submitted by the second and the third largest industries, respectively. The applications from firms in the construction, hospitality, and manufacturing industries are more than 50% in almost every region. In particular, the percentage of applications submitted by construction companies is larger than 30% in 16 regions out of 20.

4   Data and Descriptive Statistics

I use the Work Histories Italian Panel (WHIP) dataset, a 1% sample of social security records of employees working for a private firm in the secondary and tertiary sectors. Unfortunately, I do not observe domestic workers and caregivers since they are treated as self-employed workers by the Italian social security system. Data are randomly picked by drawing people born in the same day and month over different years. Data are matched employee-employer and they include the socio-demographic characteristics of employees, information on labor contracts, the working location, and, the characteristics of the firms. Since individuals are followed over the entire working life, I observe individual legal employment spells.

I predict the foreign-born workers who are more likely to be regularized by following the rules of the amnesty since I do not observe the actual amnestied employees in the sample. Amnestied foreign workers must have a contract starting between September 2002 and November 2002. The contract must be either an open-ended contract or a fixed-term contract lasting at least one year. Finally, they must not have any formal contract in the three months before the regularization. Figure 4 shows the level of new contracts signed by non-EU workers by year and month. The spike shows the amnestied workers in September 2002.

To check whether I predict the right composition of amnestied workers, I perform a cross-validation with the statistics described in Di Porto et al. (2021) and Zucchetti (2004). Table 2 shows the share of amnestied employees by the main nationalities. The share in my sample,

last column, are very similar to shares in the first two columns. Figure 5 shows the ratio of the amnestied workers to the total employment within each province. The distribution looks similar to Figure 1 and Figure 3 across provinces. Finally, Table 3 shows the distribution of amnestied workers by industry and firm size. The shares of amnestied migrants are higher in both construction and manufacturing small firms, as in Di Porto et al. (2021).

I select only men to perform the empirical analysis since amnestied male foreigners are employed within male-dominated industries. I drop the public sector, the agriculture industry and the fishing industry since amnestied foreigners do not work in the public sector and social security records are not representative of blue-collar workers in the primary sector. Finally, I drop individuals belonging to the first and last percentile of wage distribution, individuals with an apprenticeship agreement and managers. The final sample is made of around 2 million observations from January 2000 to December 2002. The sample includes only 18-65 years-old men working in the private sector.

Table 4 shows descriptive statistics for natives, former-legal and regularized foreigners in September 2002. Foreigners earn less than natives and experience higher unemployment rates. Moreover, migrants are younger and work for smaller firms. Amnestied migrants are younger, earn less and work for smaller firms than the other two groups. Summary statistics highlight the fact that amnestied migrants are more likely to be less productive than the former legal ones.

In the subsection 2.2, I show that firms declare only the workers who are above a productivity level which is different for foreigners and natives. I define the ratio between these two productivity levels as the threshold gap. The employment and wage gaps between foreigners and natives are good proxies to check whether the threshold gap changes after the policy. Figure 6 shows a sharp drop in the employment gap by around .2 log points., while the wage gap increases by .6 log points. The next section sheds light whether the changes in both the employ-

ment and wage gaps depend only on the increase in the legal employment of foreign workers or depend also on a drop in the legal employment of low-productive native workers.

Unfortunately, I cannot perform an analysis on the informal workers since the social security records do not include informal workers by definition and the National Italian Statistical Institute (ISTAT) does not release the information on province of work in 2002[18]. Furthermore, the survey does not contain any information on the firm size. In the next section, I show that province and firm size are two key characteristics to perform the empirical analysis.

5 Empirical Strategy

The identification strategy of the causal effect of the amnesty on the labor market outcomes of regularized workers' job competitors follows two steps. First, I define markets where all firms have the same probability of being inspected, and, so, the same probability of applying for the amnesty. A wrong definition of the market might lead to an estimation bias. For instance, pooling together untreated with treated units might lead to toward-zero estimation bias. Second, I present an estimation model that allows me to control for several sources of endogeneity like seasonal effects, macroeconomic effects and market level effects. In the next subsection, I define the market.

5.1 Market Definition

I define a market as the collection of all the firms having the same probability of being inspected. Inspection probabilities affect the employment of both undocumented and undeclared workers within a firm since a higher likelihood of being fined increases the cost of hiring workers without a formal contract. Hence, firms are more likely to be homogeneous in the demand

---

[18]Researchers can only access to *standard use file* of Italian Labor Force Survey (ILFS) in 2002

of undeclared workers within markets where they experience the same expected penalty. For such reasons, each probability of being inspected defines a market where the labor demand of both undeclared and undocumented workers is likely to be homogeneous.

Unfortunately, I do not observe each market-specific probability of being inspected. To overcome this issue, I define a market by using the following firms' characteristics: province, industry and size. This assumption relies on the fact that the number of inspections is not homogeneous across markets, for instance firms are more likely to be inspected in the construction industry. Moreover, inspections are less likely when the number of inspectors is fixed and the share of small firms is larger. Therefore, firms are less likely of being detected when employing informal workers within provinces with a high fraction of small firms. To provide evidence of the relationship between inspections and amnesty applications, Figure 7 shows a positive correlation between the share of inspected firms and the share of applications for undocumented employees among Italian regions.

5.2 Identification

I use an event-study method to estimate the effect of the regularization program on the labor market outcomes of amnestied workers' job competitors. In the main analysis, I include only native-born workers since a bad prediction of not-amnestied workers might lead to an estimation bias. In the robustness section, I do include former legal workers to test the main results. I consider the months from May to August as pre-treatment months and the months from September to November as post-treatment months. I select only the months from May to August to test the pre-treatment effect since the amnestied workers should have been working for at least three months for the same firm before September 2002. For symmetry, I consider only three months following the beginning of the regularization program in the main specification.

18

The following equation describes the estimation model:

$$y_{i(c)\tau} = \beta_0 + \sum_{t=-3}^{3} \beta_t \frac{Amn_{cy}}{Imm_{cy}} 1\{m = t+8\} + \gamma_{cm} + \gamma_{cy} + \gamma_\tau + \varepsilon_{i\tau} \qquad (13)$$

where $y_{i(c)\tau}$ is the labor market outcome of the individual i belonging to the market (cell) c is employed at time τ. τ stands for the year-month variation. $\frac{Amn_{cy}}{Imm_{cy}}$ represents the ratio of amnestied workers to the total number of declared foreign workers within the market c at year y. In other words, this variable measures the change in the fraction of declared foreign workers after the amnesty. $1\{m = t+8\}$ is a set of month-specific dummies from May to November but August. The β_ts' coefficients are the marginal causal effects of an incremental unit in the share of amnestied migrants on the labor market outcomes of native workers within each month in 2002. γ_{cm}, γ_{cy}, γ_τ, are the market-year, market-month and year-month fixed effects, respectively. Finally, $\varepsilon_{i\tau}$ is an idiosyncratic term.

This model has two main advantages to estimate the amnesty effects. First, the monthly estimates test whether there is an anticipatory effect and there is a persistence of the treatment over time[19]. Second, the month by year setting allows me to control for both year and month fixed effects within every cell. The controls adjust the estimates for any kind of seasonal and macroeconomic trend within and between cells. Omitting one of those fixed effects might imply an omitted variable bias since cells might experience different flows of migrants within a year and across years.

A threat to the identification is the measurement issue of both the dependent and the independent variable. From the theoretical framework, an increase in the fraction of declared foreign workers affects the fraction of the declared native workers. The fractions are the ratio

---

[19]Since the increase in the penalties was well known, employers may decide to anticipate the effect by changing the composition of labor inputs before the start of the amnesty. However, there was no reason to do that since new penalties started from the beginning of the amnesty window. Therefore, any anticipatory change in the composition of labor inputs was not optimal.

of the declared native/foreign workers to the total labor supply of native/foreign workers. Yet, social security data collect only information on workers who have been declared for at least one month in a year. Therefore, the dependent variable is only a proxy of the actual fraction of declared workers since I do not observe the actual labor supply. In the same way, the amnesty share is only a proxy of the actual share of amnestied workers. However, Appendix 7.1 shows that estimates are, if any, a lower bound of the true parameters since the two proxies leads to an attenuation bias.

Another threat is the double counting of workers across markets. Since workers might work in different markets in a year, the imputation of unemployment spells to a specific market becomes problematic. To avoid that, I drop workers employed in more than one market within a year. The share of dropped workers is around 7% in 2001 and around 6% in 2002. Yet, the exclusion of workers who are more likely to move across markets might bias the estimates of effect of the amnesty on employment and wages. In particular, the effect on employment might be smaller including those workers who react faster to the decrease in the demand of legal employment within the affected markets, while the effect on wages is not trivial since workers might react to the drop in the legal labor demand by either moving away and getting higher wages or staying in the former working place and getting a lower wage. I deal with this issue in the Robustness Checks section.

6 Results

6.1 Employment Effect

Figure 8 shows the effects of the policy on the legal employment probability of native workers month by month. Before the regularization there is no effect on the employment probability,

20

while, after the regularization, the estimates are negative and different from zero. The effect is small and ranges between around .01 in September and around .04 in November. Table 5 shows that adding individual controls do not affect the estimate. Furthermore, the last two columns of Table 5 shows that the negative employment effect persists also in December.

Since this is the first paper evaluating the effect of an amnesty bundled with an increase in the penalties for hiring undocumented workers in the very short run, the size of the employment effect cannot be compared to other papers showing negative effects on the employment of native workers. However, the estimates do not fall so far from those founded in other papers exploiting different settings to identify the effect on native employment (Dustman et al. (2017), Edo and Rapoport (2019) among others). In particular, the effect is around -.22 using the share of amnestied workers over the total labor (Figure 10 in the Appendix A.9.1). Di Porto et al. (2022) find a similar effect even if the size seems to be smaller and noisier. The different results might stem from the fact that they compare firms which regularized workers with firms which have only submitted the demand. The change in the decision of regularizing undocumented workers might stem from different labor outcomes for natives such that they find more valuable not regularization the position. The time-varying decision on the regularization process may explain why their estimates are going towards zero following the opening of the amnesty. Finally, my results differ from Elias et al. (2019) since they do not find any effect on the formal employment of native workers. The null effect may stem from the timing of the amnesty since Spain announced the program less than one year before the implementation. Therefore, firms and workers may have adjusted their labor demand and supply before the opening of the amnesty window.

Table 5 shows the effect of the amnesty on the extensive margins of native workers. Yet, employers cannot fire workers in the very short run and without a fair reason due to labor market

rigidities. Therefore, a lower flow of new hirings or an increase in the early retirement is the only way to reduce the legal employment of native workers. Table 6 shows the effect on three age groups: young, middle-aged and old. The first column shows that youngsters, less than 29 years old, experience a negative effect after the amnesty. The second column shows a smaller effect for middle-aged workers. Finally, old workers experience the largest decrease. This is a first evidence of the negative effect on the employment of low productive declared workers since youngsters are more likely to be as productive as amnestied migrants and old workers are more likely to be less productive than migrants in hard labor jobs.

The middle-aged workers do not experience a decrease in the employment probability since they are the most productive group in hard labor jobs. However, employers might lower the labor cost of middle-aged native workers by employing them in the formal occupations for fewer hours. Table 7 shows the effect of the amnesty on the intensive margins by age groups and overall. The first column shows that the overall effect is positive but not different from zero. The other three columns show the effect on youngsters, middle-aged and old workers, respectively. Second and fourth columns show a negative effect but different from zero only for young workers. Instead, middle-aged workers experience an increase in the probability of being employed with a part-time contract. Therefore, employers lower the legal labor demand for young workers, push old workers to retire and lower the legal hours spent at work of middle-aged workers.

The Tables described above show the effect of the amnesty on both extensive and intensive margins of declared native workers. In particular, the amnesty has a negative effect on the employment of young and old workers and on the working hours of middle-aged workers. This finding is a first evidence of the crowding-out effect of the amnesty on the least productive workers. Yet, wages are the best proxies for the individual productivity. Therefore, I analyze

the effect on the average wage within cells in the next section.

6.2 Wage Effect

The theoretical model shows that firms decide to hire formally only the workers above a certain productivity threshold which is different for natives and foreigners. Since the model predicts that an increase in the penalties for hiring illegal migrants might lead to an increase in the legal employment of foreigners (lower productivity threshold) and a decrease in the legal employment of natives (higher productivity threshold), native workers just above the pre-reform marginal productivity are more likely to experience a lower probability of being declared. If it is the case, the average wage should increase within the cells since earnings are increasing in productivity [20].

Table 8 shows the effect of the amnesty on log wages. In the first column, there is an increase in wages after August even if estimates are statistically different from zero only from October. A percent increase in the share of amnestied workers leads to a surge of the average wage between .025 and .061 percentage points. To check whether the increase of wages depends on the crowding-out mechanism of the least productive workers, I include socio-demographic controls as a proxy for each worker-specific productivity level. In particular, I adjust the model for age and age squared and age, age squared and a dummy for region of birth in the second and the third columns, respectively. Estimates become smaller and less significant. This result is in line with other papers (Di Porto et al. (2022) and Elias et al. (2019)) showing that regularization policy has an effect on aggregate wages in the formal sector since firms substitute low-paid formal native workers with amnestied migrants.

Table 9 highlights that young and old workers experience a greater increase in the log wages

---

[20]Theoretical model assumes that the most productive workers spend more time on the job. To validate this assumption, Appendix 9.2 shows that after the amnesty implementation the average number of months spent at work increases for youngsters and elders.

than middle-aged workers. The evidence is in line with the employment effect since only the most productive young and old workers remain formally employed.

6.3   Analysis with Individual Fixed Effects

The introduction of the amnesty bundled with the increase in the fine for hiring undocumented migrants has a negative effect on the probability of being employed of low-skilled native workers and, as a consequence, has positive composition effect on wages. As already discussed, the composition effect gets smaller when controlling for individual characteristics. Yet, I do not observe all individual characteristics. Therefore, I move to individual panel analysis to better control for fixed unobservables that capture the individual productivity.

I use the panel dimension of data to check whether the employment effects persist and wage effects fade away after controlling for individual fixed effects. To do so, I select only the individuals who are in the data for 24 months between 2001 and 2002. The estimator is the following:

$$y_{i(c)\tau} = \beta_0 + \sum_{t=-3}^{3} \beta_t \frac{Amn_{cy}}{Imm_{cy}} 1\{m = t+8\} + \gamma_{iy} + \gamma_{im} + \gamma_{iy} * m + \gamma_\tau + \varepsilon_{im} \qquad (14)$$

$y_{i(c)m}$ is the labor market outcome of individual i within a market c at time τ, β_t is the parameter of interest which measures the effect of the amnesty on the individual outcomes, $\frac{Amn_{cy}}{Imm_{cy}}$ is the amnesty ratio, $1\{m = t+8\}$ is a set of monthly dummies. $\gamma_{iy}, \gamma_{im}, \gamma_{iy} * m, \gamma_\tau$ are individual-year fixed effects, individual-month fixed effects, individual-year fixed effects times a monthly trend, and the month-year fixed effects, respectively. ε_{im} is the error term.

Table 10 shows the estimates of the amnesty effect on the probability of being employed and on log wages in the first and second column, respectively. As predicted, the effect is negative

on the employment and null on the wage[21].

6.4 Heterogeneity Across Geographical Areas

The ratio of amnestied migrants to the total of former legal migrants was larger in the Southern regions than in Central and Northern regions [22]. This fact stemmed from a previous policy which forbade to firms in the Southern regions to hire foreign workers. The reason of such policy was the large share of low-skilled workers in those regions. Since the policy affects mainly low-productive workers, Southern regions should experience a greater effect.

Table 11 shows the effect of the amnesty on the probability of being declared and on wages within cells. The odd and even columns show the effect of the amnesty on the labor market outcomes for the employees working outside and within Southern regions, respectively. The employment effect is greater in the Southern regions, while the effect on the wage is greater elsewhere. Since the share of low-skilled workers is larger in the South, the cell-specific wage is more likely to increase in the Central and the Northern Italy while the crowding-out effect on the legal employment is more likely within Southern Italy.

7 Robustness Checks

7.1 Falsification Test

The amnesty ratio might proxy some time-variant cell characteristics which I do not control for. If that is the case, the effects of the amnesty should be similar over previous years. To check that, I impute the 2002 amnesty ratio to 2001 within the same cells and run the same estimation

---

[21]Including the individual fixed effects times the time trend, I get rid of one degree of freedom. For this reason, I drop the first coefficient

[22]The Central and Northern regions are: Emilia Romagna, Friuli Venezia Giulia, Lazio, Liguria, Lombardia, Marche, Piemonte, Toscana, Trentino Alto-Adige, Umbria, Valle d'Aosta, Veneto. The Southern regions are: Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sardegna, Sicilia.

model for the years 2001 and 2000. If it is a proxy for some time-variant characteristics, I should find the same results as in the main specification. Table 12 shows the effects of the amnesty on the probability of being declared and on the wage. The amnesty has a very small positive effect on the probability of being employed in the last two months of 2001, while there is no effect on wages. The effect on the employment has the opposite sign of the estimates in the main specification. Yet, the negative effect holds even subtracting the positive effect in 2001 from 2002 estimates[23].

7.2 Estimates on the Overall Sample

In the section 6, the results show the effects both on the employment and on the wage excluding the workers who change market within a year. This exclusion of around 5% of the sample might create a selection and invalidates the results. To demonstrate that the estimates are not affected by this selection, I keep all the observations and show the results for both outcomes[24]. Including all the observation creates an imputation issue for the unemployment spells since workers employed in more than one market might look for a job in two markets. To overcome this issue, I use a weighted least square estimator to take into account of the probability of looking for a job in each market. If a worker is unemployed, the weight is equal to the fraction of months worked in each market. If a worker is employed, the weight is equal to one. Figure 9 shows on the left the estimates of the employment effect and on the right the estimates of wage effect. Estimates are not different from the ones in Table 5 and Table 8. In particular, the point estimates are a little bit smaller for the employment and a little bit larger for the wage. Finally,

---

[23]A possible explanation is that amnestied migrants increased the supply of undocumented migrants in the last months of 2001 leading to lower wages. As a consequence, firms increase the share of undocumented workers and lower the share of undocumented natives. To check whether the 2001 affects the estimate in the main specification, I use the 2000 as the control year in Table A.3 in Appendix 9.3. The results still hold.

[24]I exclude the 1% of the sample which includes employees working at the same time in two markets for more than one month.

I can conclude that selecting workers who do not change market within a year does not affect meaningfully the estimates.

7.3   Inclusion of Former Legal Foreign Workers

So far, the analysis focuses on only the effect of the amnesty on native workers to understand whether the amnesty has a backlash effect on the labor market outcomes of this group. However, amnesty might affect also the legal employment of former legal foreign workers. In particular, there is a large strand of literature showing that migrants are more likely to share the same skills. Table 13 shows that the estimates do not change meaningfully when I consider only natives. In particular, the effect of the amnesty on employment is slightly larger since former legal migrants are very likely to be closer substitute to amnestied migrants than natives. Finally, the composition effect on wages is larger because not crowded-out foreign workers are more likely to be much more productive and, so, more likely of being in the upper part of the wage distribution.

7.4   Testing a different definition of labor market for a long-run analysis

The main analysis uses a definition of labor market based on the interaction between three characteristics: firm size, province and sector. This definition narrows the analysis to only three months following the policy implementation since WHIP data has information on the firm size only until 2002. To overcome the data issue, I narrow the definition of labor market using only the interaction between provinces and sectors to test how estimates change and to extend the analysis to the last two years included in the WHIP data, 2003 and 2004.

To perform the analysis in the following years, I cannot use August as base month since the months before August 2003 are already treated. Therefore, I look at the differentials between

January and the following months to see whether there is a negative trend in the labor market outcomes following the policy implementation. Another issue is the definition of the treatment since the 2003 market-specific share of amnestied migrants might be endogenous. Therefore, I use the 2002 specific-market share of amnestied migrants as a treatment variable.

Table 14 replicates the results for the 2002 using the new definition of labor market. Estimates are quite similar to the main estimates and show larger standard errors. In particular, a higher variability in the estimates confirms that firm size is an important factor to explain labor market outcomes.

Table 15 and Table 16 show the estimates for 2003 and 2004, respectively. As predicted, the effect of the regularization has a negative effect on the employment in 2003. The estimates of wages and months worked confirm that the low productive workers is the group affected by the policy. Finally, estimates become smaller in 2004 showing that an adjustment mechanism is taking place.

This analysis shows two results. First, using a different definition of labor market does not affect the estimates but estimates are noisier highlighting the role of the firm size in explaining the labor market outcomes. Second, estimates show that negative effects of the regularization on the employment of amnestied workers' job competitors last one year and half before starting to adjust. These results are in line with the spatial equilibrium dynamics of labor markets where workers experiencing adverse effects in a specific market are very likely to move in another market to offset the negative effects.

8   Conclusion

This article investigates the effect of massive regularization program on the labor market outcomes of amnestied workers' job competitors. To study this relationship, I exploit the 2002

Italian amnesty which granted a work visa to around 600,000 undocumented migrants and increased the penalties for hiring undocumented workers. Native workers experience a decrease in the probability of being employed in formal occupations following the policy implementation. The employment effect is heterogeneous across age groups. Indeed, old and young workers experience a decrease in the employment levels, while middle-aged workers experience a decrease in the total hours worked. Finally, the policy affects mainly the low-productive workers.

Results show that an amnesty program bundled with an increase in the penalties for hiring undocumented workers leads employers to prefer undocumented employees in formal occupations than native workers. The unexpected timing of 2002 Italian policy is best scenario to study the effects on the labor demand for native workers since workers are less likely to offset any negative effect in the very short run. Furthermore, the requirement of formal proof showing that foreign employees have been working for the same firm for at least three months before the beginning of the policy (fixed labor supply of foreign workers) and the requirement that employers are in charge of applying for the amnesty (monopsony power) lower the odds of a labor supply-driven effect.

A future research project should look at the effect of the amnesty on the probability of working informally for native workers. Nowadays, the data concerning undeclared workers is not available for each specific labor market. Therefore, this paper cannot provide any evidence in that sense. However, the negative effects on the legal employment for native workers might help to understand the increase in the vote for anti-immigration parties among low-productive workers.

References

[1] Albert, Christoph. 2021. The Labor Market Impact of Immigration: Job Creation versus Job Competition. American Economic Journal: Macroeconomics, 13 (1): 35-78.

[2] Almeida, R. and Ronconi, L. (2016), Labor Inspections in the Developing World: Stylized Facts from the Enterprise Survey. Ind Relat, 55: 468-489. https://doi.org/10.1111/irel.12146

[3] Amuedo-Dorantes, C. and C. Bansak, "The Impact of Amnesty on Labor Market Outcomes: A Panel Study Using the Legalized Population Survey," Industrial Relations, 2011, 50 (3), 443– 471.

[4] -,-, and S. Raphael, "Gender Differences in the Labor Market Impact of IRCA's Amnesty Provisions," American Economic Review, 2007, 97(2), 412–416.

[5] Bahar, D., A.M. Ibanez, and S. Rozo, "Give me your tired and your poor: Impact of a large-scale amnesty program for undocumented refugees," Journal of Development Economics, 2021, 151, 102652.

[6] Baker, Scott R. 2015. "Effects of Immigrant Legalization on Crime." American Economic Review, 105 (5): 210-13.

[7] Casadio, G. "Niente stagionali al Sud Usate i vostri disoccupati". Repubblica, 18 May 2001. `https://ricerca.repubblica.it/repubblica/archivio/repubblica/2001/05/18/niente-stagionali-al-sud-usate-vostri-disoccupati.html?ref=search`

[8] Elizabeth U. Cascio, Ethan G. Lewis, Distributing the Green (Cards): Permanent residency and personal income taxes after the Immigration Reform and Control Act of 1986, Journal of Public Economics, Volume 172, 2019, Pages 135-150, ISSN 0047-2727, https://doi.org/10.1016/j.jpubeco.2018.11.005.

[9] Chassamboulli, Andri and Giovanni Peri, The Labor Market Effects of Reducing the Number of Illegal Immigrants, Review of Economic Dynamics, October 2015, 18 (4), 792–821.

[10] Cobb-Clark, D. A., C. R. Shiells, and B. L. Lowell, "Immigration Reform: The Effects of Employer Sanctions and Legalization on Wages," Journal of Labor Economics, 1995, 13(3), 472–498.

[11] DE PAULA, Á. and SCHEINKMAN, J.A. (2011), THE INFORMAL SECTOR: AN EQUILIBRIUM MODEL AND SOME EMPIRICAL EVIDENCE FROM BRAZIL. Review of Income and Wealth, 57: S8-S26

[12] Devillanova, C., F. Fasani, and T. Frattini, "Employment of Undocumented Immigrants and the Prospect of Legal Status: Evidence from an Amnesty Program," Industrial and Labor Relations Review, 2017.

[13] Di Porto, E., E.M. Martino, and P. Naticchioni, S. Vannutelli, Back to Black? The Impact of Regularizing Migrant Workers, Working Papers. $https://www.dropbox.com/s/oznhto61t0kqfnr/DPMNV_JEEA_RR_2022_v8.pdf?dl = 0$

[14] Christian Dustmann, Uta Schönberg, Jan Stuhler, Labor Supply Shocks, Native Wages, and the Adjustment of Local Employment, The Quarterly Journal of Economics, Volume 132, Issue 1, February 2017, Pages 435–483, https://doi.org/10.1093/qje/qjw032

[15] Edo, Anthony, and Hillel Rapoport.2019. "Minimum Wages and the Labor MarketEffects of Immigration."Labour Economics, 61: 101753.

[16] Freedman, Matthew, Emily Owens, and Sarah Bohn. 2018. "Immigration, Employment Opportunities, and Criminal Behavior." American Economic Journal: Economic Policy, 10 (2): 117-51.

[17] Kaushal, N., "Amnesty Programs and the Labor Market Outcomes of UndocumentedWorkers," Journal of Human Resources, 2006, 16(3), 631–647.

[18] King, R. Recent immigration to Italy: Character, causes and consequences. GeoJournal 30, 283–292 (1993). https://doi.org/10.1007/BF00806719. 2021.

[19] Vogel D, Kovacheva V, Prescott H. The size of the irregular migrant population in the European Union – counting the uncountable? Int Migr. 2011;49(5):78-96.

[20] Machado, J. (2017). DEALING WITH UNDOCUMENTED IMMIGRANTS: THE WELFARE EFFECTS OF AMNESTIES AND DEPORTATIONS. Journal of Demographic Economics, 83(4), 445-492.

[21] Mastrobuoni, Giovanni, and Paolo Pinotti. 2015. "Legal Status and the Criminal Activity of Immigrants." American Economic Journal: Applied Economics, 7 (2): 175-206.

[22] Monras, J., Vázquez-Grenno, J., & Elias, F. (2022). Understanding the effects of granting work permits to undocumented immigrants (Working Paper).

[23] Orrenius, Pia M., and Madeline Zavodny. What are the consequences of an amnesty for undocumented immigrants?. No. 2004-10. Working Paper, 2004.

[24] Pinotti, P., "Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime," American Economic Review, January 2017, 107 (1), 138–68.

[25] Ulyssea, G. (2018). Firms, Informality, and Development: Theory and Evidence from Brazil. The American Economic Review, 108(8), 2015–2047. https://www.jstor.org/stable/26528024

[26] United Nations Development Program; Human Development Report 2009; Overcoming Barriers: Human Mobility and Development, New York 2009

[27] Zucchetti, E. (2004): 'I caratteri salienti della regolarizzazione in Italia'. In Zucchetti E. La regolarizzazione degli stranieri. Nuovi attori nel mercato del lavoro italiano. Milano, Franco Angeli.

Figure 1: Total number of Applications by Province

(5356,107191]
(3356,5356]
(1576,3356]
[163,1576]
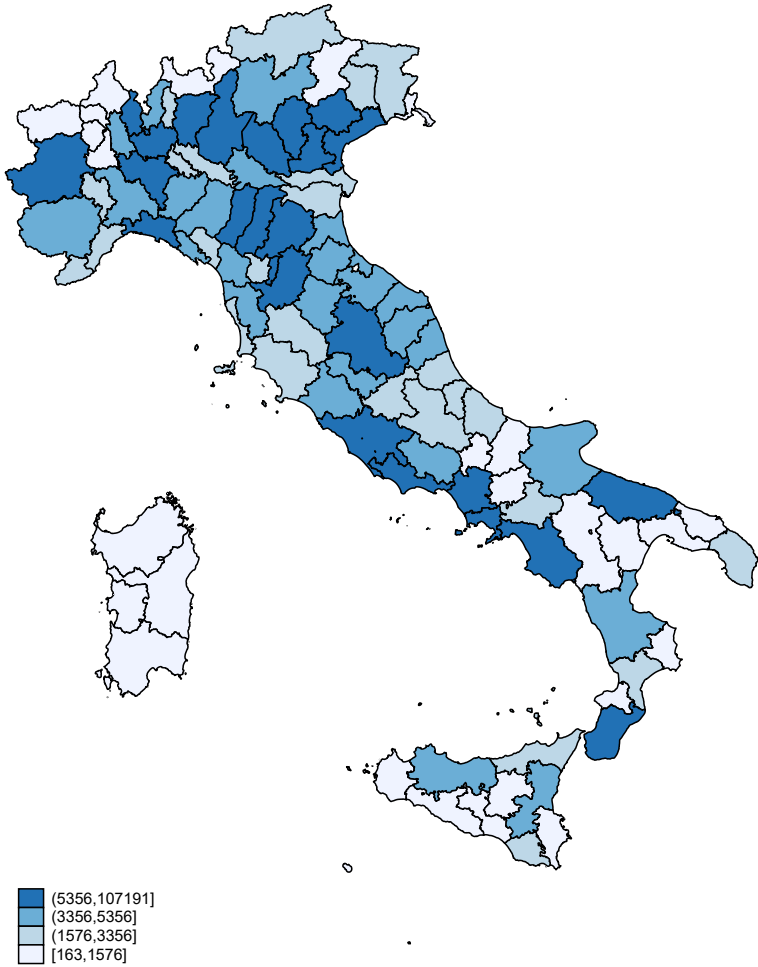
Notes: Figure shows the number of applications for the amnesty within each province.
Source: Zucchetti (2004)

Figure 2: Ratio of Accepted Applications to the 2001 Legal Foreigners within Province
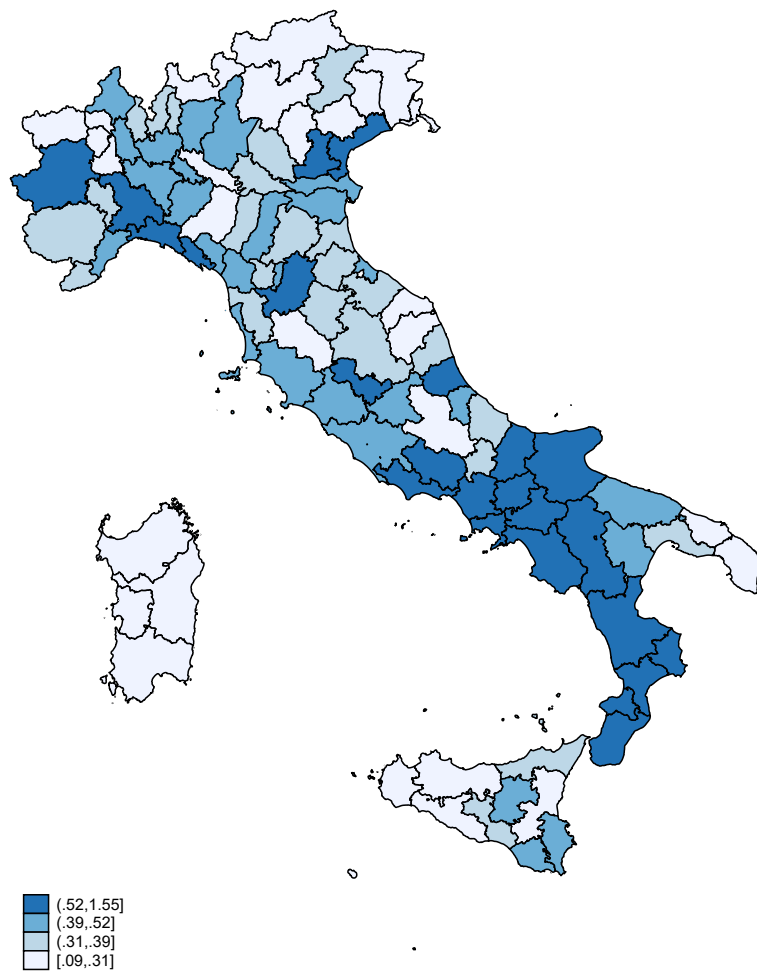
Notes: Figure shows the ratio of amnestied workers to the former legal foreigners within each province.
Source: Zucchetti (2004) and ISTAT

Figure 3: Demand for the Amnesty by Region and Sector
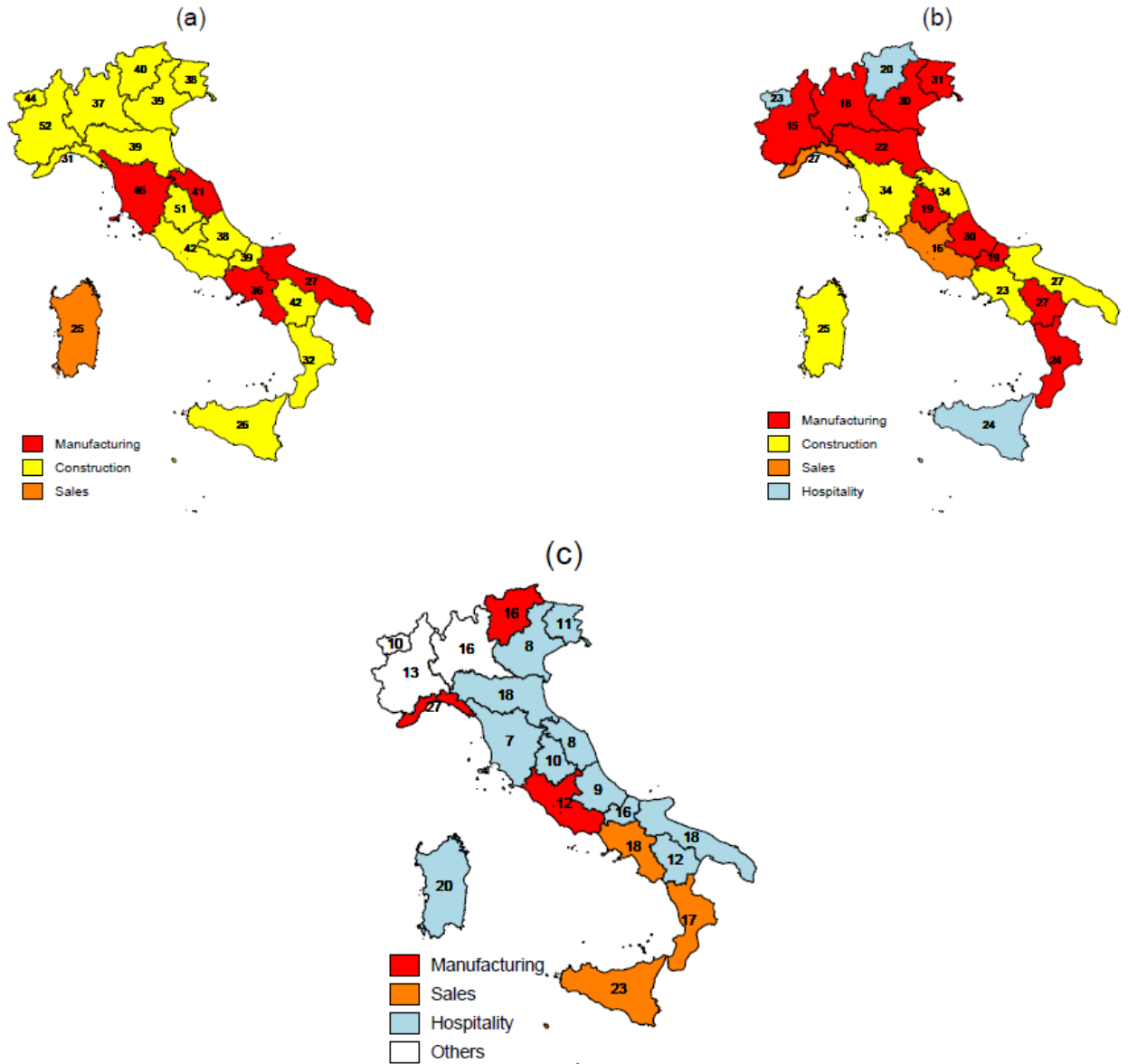
Notes: Figures show the share of applications by industry within regions. (a), (b), and (c) show the share of the first, the second and the third industry, respectively.
Source: Congia (2005).

Figure 4: New Contracts of Extra-EU Workers Between 2000 and 2004
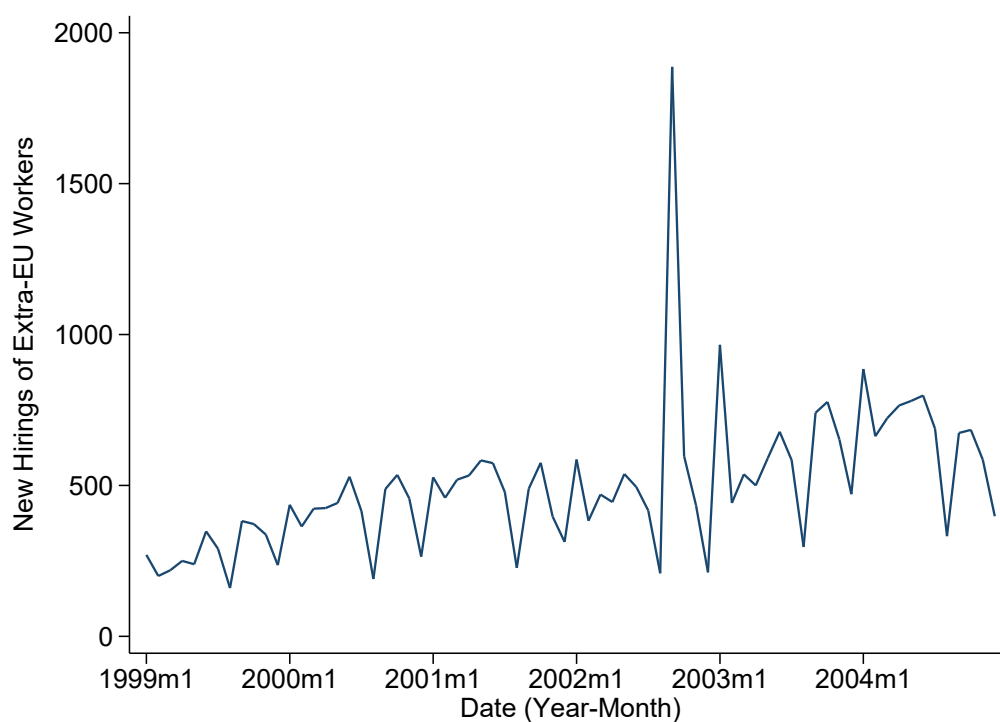
Notes: Figure shows the new hirings of non-EU workers from January 1999 to December 2004.
Source: WHIP.

Figure 5: Ratio between non-EU worker, regularized, workers and employees per thousands of contracts ($‰$)
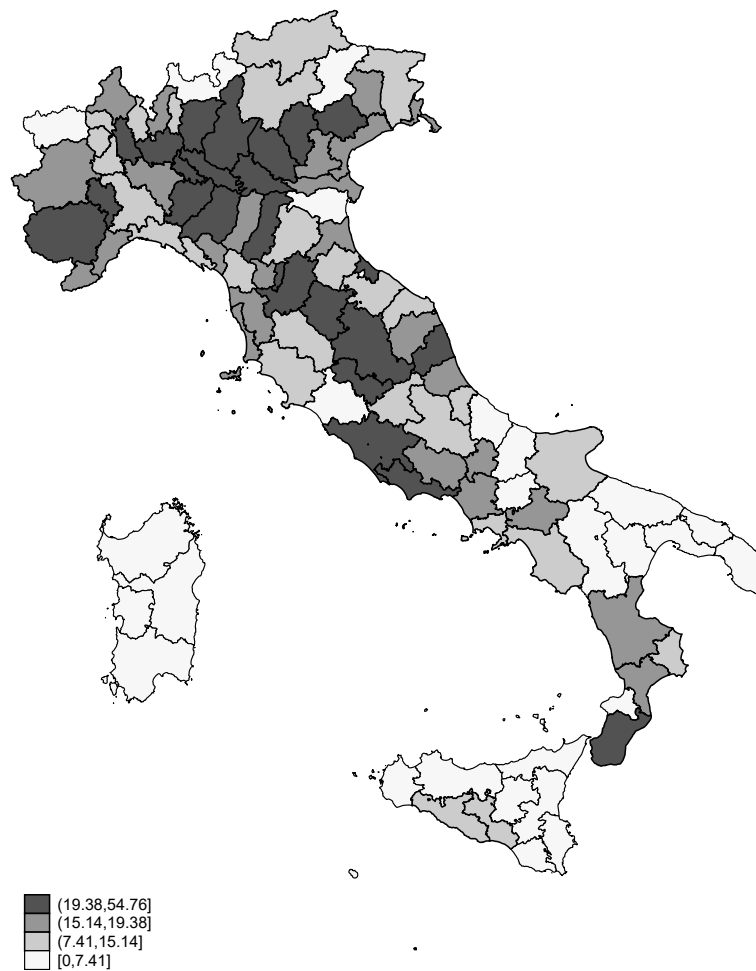
Notes: Figure shows ratio of amnestied workers to the total employment within each province.
Source: WHIP.

Figure 6: Foreign-Native Log Employment Gap and Log Weekly Wage Gap by Year and Month
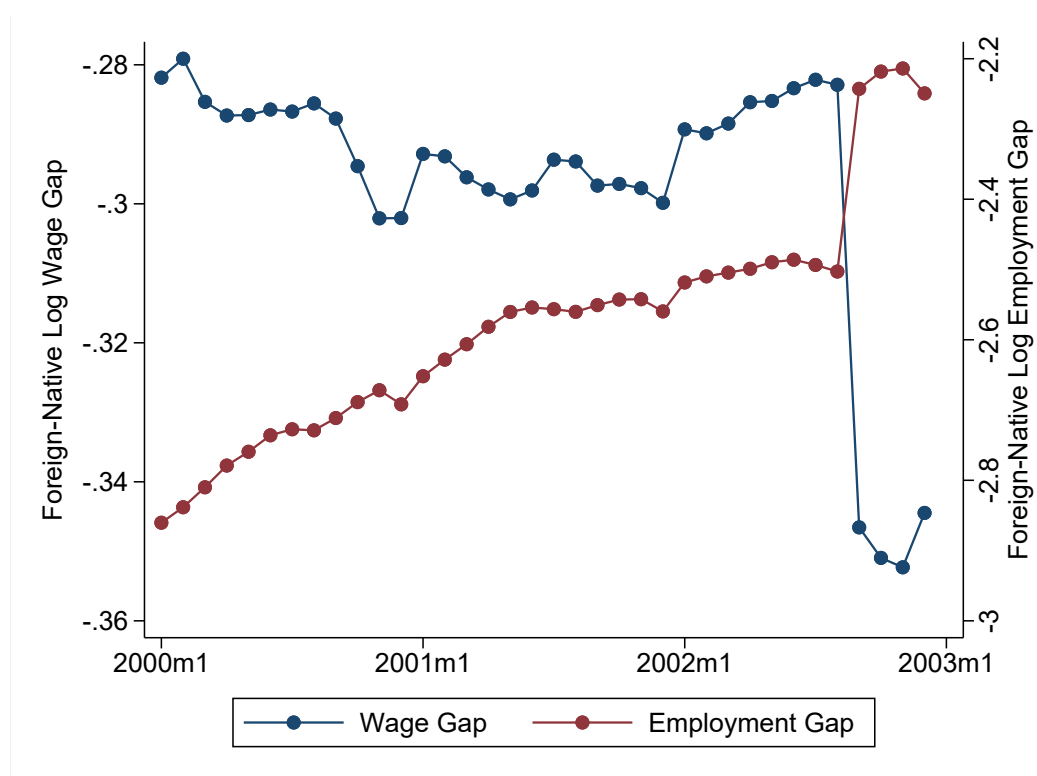
Notes: Figure shows the log employment gap and the log weekly wage gap between foreigners and natives from January 2000 to December 2002.
Source: WHIP.

Figure 7: Share of Inspected Firms and Applications of Employees by Region in 2002
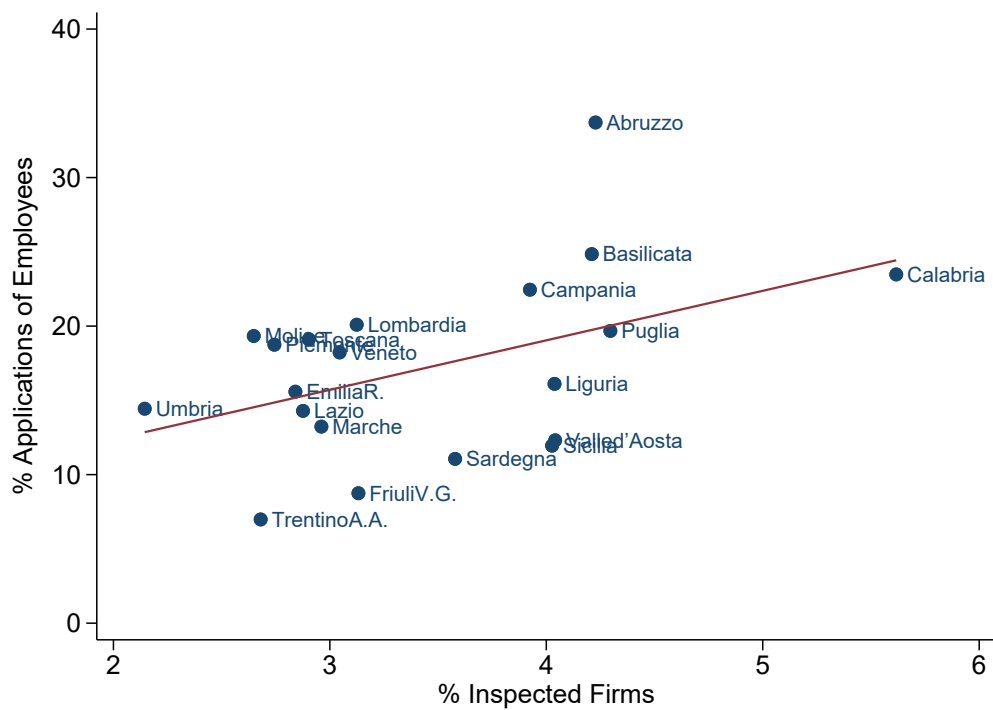
Notes: Figure shows the relationship between the share of applications of employees and the share of inspected regions by regions in 2002.
Source: INPS and ISTAT.

Figure 8: Effects of the Amnesty on the Employment Probability of Native Workers
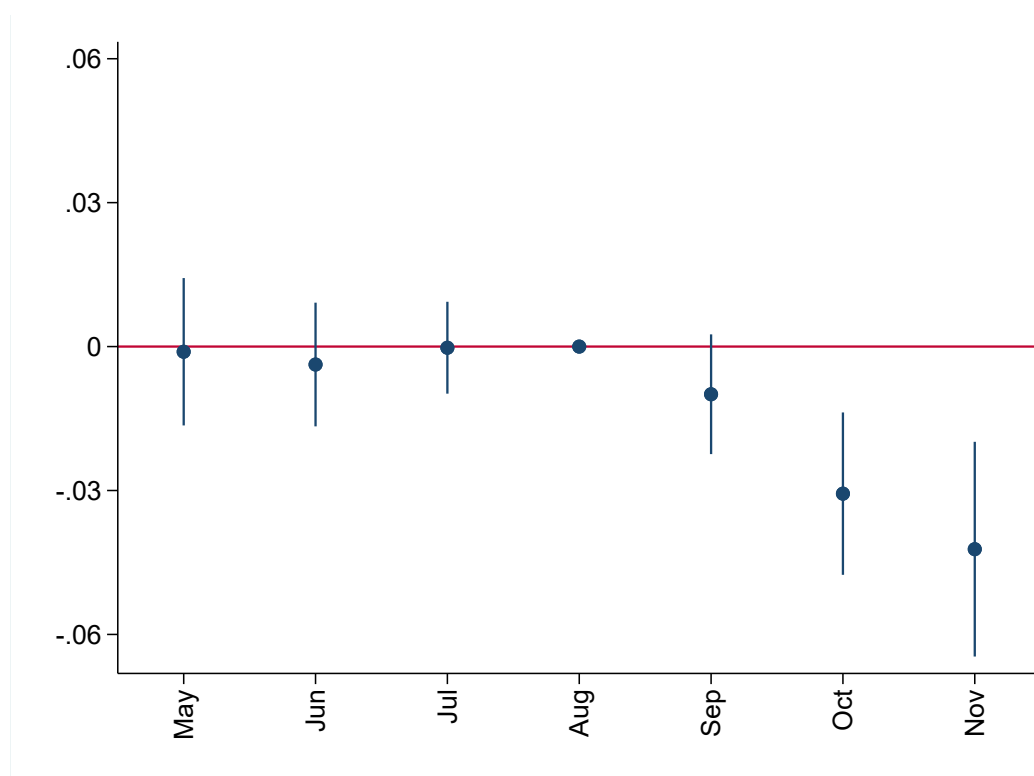
Notes: Figures show the coefficients of the interactions between share of amnestied migrants and monthly fixed effects. The regression includes cell-year fixed effects, cell-month fixed effects, month-year fixed effects. Standard errors are clustered at cell level. Estimates show a 95% confidence interval.

Figure 9: Effects of the Amnesty on the Employment and on Wages considering the entire sample
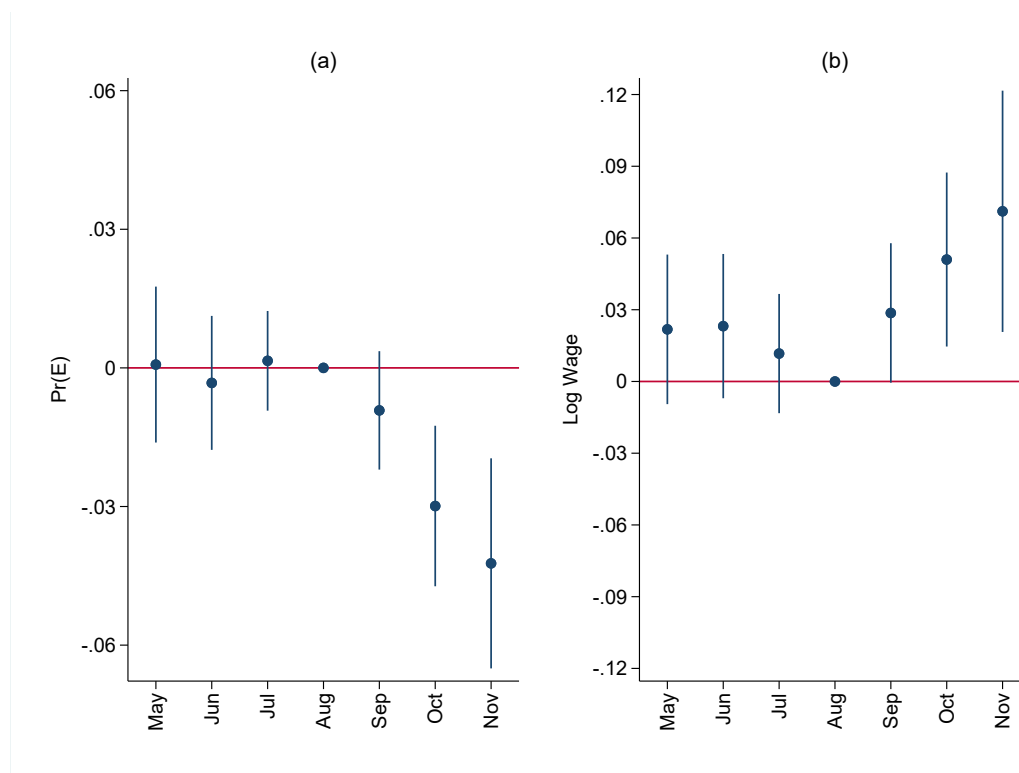
Notes: Figures show the effect of the amnesty on the probability of being employed (Panel (a)) and on the log wage (Panel (b)). Total observations in (a) and in (b) are 632,677 and 554,111, respectively. The regression includes cell-year fixed effects, cell-month fixed effects, month-year fixed effects. Standard errors are clustered at cell level. Estimates show a 95% confidence interval.

Table 1: Summary of the Amnesty

	Obs
Released Permits	641,638
Not Defined	3,079
Rejected Applications	49,220
Cases of Rejection:	
Archived	21,056
Repatriations	3,518
Future Repatriations	6,227
Litigation	18,419

Source: Ministry of Interior

Table 2: Cross-Validation

Nationality	Di Porto et al. (2021)	Zucchetti (2004)	Sample
Albania	12.57	11.5	12.45
Cina	11.31	8.5	8.7
Ecuador	.54*	3.1	2.8
Ex Jugoslavia	5.03	4.6	5.2
Marocco	11.94	11.9	10.61
Romania	26.64	22.4	18.61

* Di Porto et al. show only the total amount of regularized from the Americas

Table 3: Distribution of Regularized Foreign Workers by Industry and Firm Size

Industry	Firm Size					Total
	1-9	**10-19**	**20-49**	**50-249**	**over 250**	**Total**
Agricolture and forestry	0.12	0.00	0.00	0.00	0.00	0.12
Fishery	0.06	0.00	0.00	0.00	0.00	0.06
Mining	0.12	0.06	0.00	0.00	0.00	0.19
Manufacturing	17.33	6.72	3.33	1.60	0.37	29.36
Water, electricity and gas suppliers	0.06	0.00	0.00	0.00	0.00	0.06
Constructions	29.73	3.52	2.59	0.56	0.12	36.52
Retailers and wholesale	7.28	0.74	0.19	0.31	0.06	8.57
Hotels and restaurants	7.53	1.36	0.43	0.00	0.06	9.38
Transports, storage and communications	2.16	0.62	1.11	1.73	0.31	5.92
Financial	2.04	1.36	2.04	2.53	0.68	8.64
Real estate, rentals and R&D	0.99	0.19	0.00	0.00	0.00	1.17
Total	67.43	14.56	9.69	6.72	1.60	100.00

Notes:The total number of observations is 1,621.

Table 4: Summary Statistics on September 2002

	Natives	**Legal Foreigners**	**Amnestied Foreigners**
Age	38.13	35.43	30.38
	(10.41)	(8.33)	(7.77)
Fr. of Employed Workers	.88	.8	-
	(.32)	(.4)	-
Log Weekly Wage	6.22	5.94	5.67
	(.42)	(.36)	(.33)
Fr. of manufacturing workers	.47	.53	.3
	(.5)	(.5)	(.46)
Fr. of construction workers	.09	.13	.39
	(.29)	(.33)	(.49)
Fr. of hospitality workers	.03	.1	.09
	(.18)	(.3)	(.29)
Fr. of workers in the Center-North	.74	.92	.91
	(.44)	(.27)	(.28)
Firm Size	2109.61	740.51	78.61
	(7781.62)	(4591.26)	(1488.47)
N. of observations	56,787	5,083	1,610

Notes: Standard deviations in parenthesis

Table 5: The Effect of the Amnesty on Employment Probability of Native Workers

	(1)	(2)	(3)	(4)
May	-0.001	-0.001	-0.001	-0.001
	(0.008)	(0.008)	(0.008)	(0.008)
Jun	-0.004	-0.004	-0.004	-0.004
	(0.007)	(0.007)	(0.007)	(0.007)
Jul	-0.000	-0.000	-0.000	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)
Aug	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Sep	-0.010	-0.010	-0.010	-0.010
	(0.006)	(0.006)	(0.006)	(0.006)
Oct	-0.031***	-0.031***	-0.031***	-0.031***
	(0.009)	(0.009)	(0.009)	(0.009)
Nov	-0.042***	-0.042***	-0.042***	-0.042***
	(0.011)	(0.011)	(0.011)	(0.011)
Dec			-0.054***	-0.054***
			(0.012)	(0.012)
Mean dep. var	.897	.897	.896	.896
Mean indep. var	.075	.075	.075	.075
Ind. contr.	No	Yes	No	Yes
N	544,901	544,901	622,744	622,744

Notes: The dependent variable is a dummy equal to one if the individual is employed in the month m at year y within the cell c. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Individual controls are: age, age squared and a dummy for region of birth. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6: Amnesty Effect by Age Groups

	<29	$28 \leq \text{Age} \leq 48$	>48
May	-0.022	0.001	0.004
	(0.023)	(0.010)	(0.021)
Jun	-0.006	-0.001	-0.021
	(0.018)	(0.007)	(0.018)
Jul	-0.019	0.002	0.005
	(0.015)	(0.005)	(0.011)
Aug	0.000	0.000	0.000
	(.)	(.)	(.)
Sep	-0.017	-0.002	-0.016
	(0.017)	(0.008)	(0.013)
Oct	-0.064***	-0.010	-0.036**
	(0.023)	(0.010)	(0.017)
Nov	-0.078**	-0.013	-0.072***
	(0.031)	(0.013)	(0.022)
N	115,605	322,364	98,994

Notes: The dependent variable is a dummy equal to one if the individual is employed in the month m at year y within the cell c. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 7: Effect of the Amnesty on the Probability of Having a Part-Time Job

	Overall	<29	28≤Age≤48	>48
May	0.000	0.006	-0.002	-0.004
	(0.003)	(0.006)	(0.003)	(0.006)
Jun	0.000	0.002	-0.001	-0.004
	(0.003)	(0.005)	(0.002)	(0.005)
Jul	0.004*	0.003	0.001	-0.001
	(0.002)	(0.005)	(0.002)	(0.003)
Aug	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Sep	0.002	0.001	0.003*	-0.007
	(0.002)	(0.004)	(0.002)	(0.004)
Oct	0.003	-0.006	0.008***	-0.003
	(0.003)	(0.007)	(0.003)	(0.005)
Nov	0.001	-0.017**	0.009***	-0.003
	(0.003)	(0.008)	(0.003)	(0.006)
N	483,908	85,988	296,482	87,059

Notes: The dependent variable is a dummy equal to one if the individual is a part-time worker in the month *m* at year *y* within the cell *c*. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell *c*. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * p<0.10, ** p<0.05, *** p<0.01

Table 8: The Effect of the Amnesty on Log Wage

	(1)	(2)	(3)
May	0.021	0.018	0.018
	(0.014)	(0.013)	(0.013)
Jun	0.019	0.017	0.017
	(0.013)	(0.012)	(0.012)
Jul	0.009	0.005	0.005
	(0.012)	(0.011)	(0.011)
Aug	0.000	0.000	0.000
	(.)	(.)	(.)
Sep	0.025*	0.022*	0.021*
	(0.014)	(0.013)	(0.013)
Oct	0.045***	0.038**	0.035**
	(0.017)	(0.016)	(0.016)
Nov	0.071***	0.064***	0.061***
	(0.022)	(0.021)	(0.021)
Mean of dep. var.	10.11	10.11	10.11
Mean of indep. var.	.074	.074	.074
Age & Age Squared	No	Yes	Yes
Region of birth	No	No	Yes
N	483,908	483,908	483,908

Notes: The dependent variable is the individual log wage in the month m at year y within the cell c. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 9: Wage Effect by Age Groups

	<29	28≤Age≤48	>48
May	0.056	0.011	-0.001
	(0.036)	(0.014)	(0.030)
Jun	0.036	0.007	0.039
	(0.031)	(0.012)	(0.026)
Jul	0.035	-0.000	-0.006
	(0.030)	(0.009)	(0.019)
Aug	0.000	0.000	0.000
	(0.000)	(.)	(.)
Sep	0.042	0.005	0.042*
	(0.029)	(0.012)	(0.023)
Oct	0.066*	0.007	0.056**
	(0.037)	(0.015)	(0.026)
Nov	0.114**	0.020	0.065*
	(0.056)	(0.020)	(0.035)
N	85,988	296,482	87,059

Notes: The dependent variable is the individual log wage in the month m at year y within the cell c. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 10: Individual effects of the Amnesty

	Pr(E)	wage
Jun	-0.004	0.000
	(0.006)	(0.000)
Jul	-0.005	0.001
	(0.005)	(0.001)
Aug	0.000	0.000
	(.)	(.)
Sep	-0.005	-0.000
	(0.009)	(0.001)
Oct	-0.025*	-0.000
	(0.014)	(0.002)
Nov	-0.031*	-0.001
	(0.018)	(0.004)
N	441,518	375,312

Notes: The dependent variables are a dummy equal to one if the individual is employed and the log wage in the first and second columns, respectively. Regressions include individual-year fixed effects, individual-month fixed effects, individual-year time monthly specific time trends, and month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 11: The Effects of the Amnesty across Areas

	Pr(E)		wage	
	North	South	North	South
May	0.000	-0.009	0.019	0.030
	(0.009)	(0.015)	(0.017)	(0.027)
Jun	-0.001	-0.015	0.011	0.040
	(0.008)	(0.013)	(0.015)	(0.026)
Jul	-0.001	-0.006	0.013	0.001
	(0.005)	(0.010)	(0.014)	(0.021)
Aug	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Sep	-0.009	-0.012	0.022	0.026
	(0.007)	(0.013)	(0.015)	(0.029)
Oct	-0.031***	-0.024	0.046**	0.026
	(0.010)	(0.016)	(0.019)	(0.032)
Nov	-0.038***	-0.041*	0.070***	0.047
	(0.014)	(0.022)	(0.026)	(0.043)
N	456,617	88,284	411,072	72,836

Notes: The dependent variables are a dummy equal to one if the individual is employed and the log wage in the first two columns and in the second two columns. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * p<0.10, ** p<0.05, *** p<0.01

Table 12: Falsification test

	Pr(E)		wage	
	(1)	(2)	(3)	(4)
May	0.002	0.002	-0.000	0.001
	(0.005)	(0.005)	(0.006)	(0.006)
Jun	0.000	0.000	0.001	0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Jul	-0.001	-0.001	0.001	0.002
	(0.002)	(0.002)	(0.005)	(0.004)
Aug	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Sep	0.002	0.002	-0.004	-0.004
	(0.003)	(0.003)	(0.006)	(0.005)
Oct	0.007**	0.007**	-0.011*	-0.008
	(0.003)	(0.003)	(0.006)	(0.006)
Nov	0.009**	0.009**	-0.015**	-0.012
	(0.004)	(0.004)	(0.008)	(0.007)
Ind. contr.	No	Yes	No	Yes
N	574,679	574,679	508,414	508,414

Notes: The dependent variable is a dummy equal to one if the individual is employed in the month m at year y within the cell c in columns (1) and (2), while the dependent variable is the log wage in columns(3) and (4). The independent variables are the interaction between monthly dummies and the fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 13: Effects of Amnesty on the Employment and Wages including Former Legal Foreigners in the sample

	Pr(E)	wage
May	-0.001	0.015
	(0.008)	(0.013)
Jun	-0.005	0.021
	(0.007)	(0.013)
Jul	0.000	0.004
	(0.005)	(0.011)
Aug	0.000	0.000
	(.)	(.)
Sep	-0.010	0.024*
	(0.006)	(0.013)
Oct	-0.037***	0.054***
	(0.009)	(0.016)
Nov	-0.051***	0.085***
	(0.011)	(0.020)
N	613,417	538,897

Notes: The dependent variables are a dummy equal to one if the individual is employed and the log wage in the first and second columns, respectively. Regressions include cell-year fixed effects, cell-month fixed effects, and month-year fixed effects. Standard errors are clustered at cell level. * p<0.10, ** p<0.05, *** p<0.01

Table 14: Effects of the regularization on the labor market outcomes using a different definition in 2002

	Pr(E)	wage	Months worked
May	-0.014	0.021	0.082
	(0.010)	(0.017)	(0.071)
Jun	-0.002	-0.003	-0.000
	(0.009)	(0.017)	(0.062)
Jul	0.001	0.002	-0.001
	(0.006)	(0.016)	(0.047)
Aug	0.000	0.000	0.000
	(.)	(.)	(.)
Sep	-0.024***	0.040**	0.189***
	(0.009)	(0.017)	(0.067)
Oct	-0.032***	0.049**	0.228***
	(0.012)	(0.019)	(0.082)
Nov	-0.034**	0.046*	0.203**
	(0.014)	(0.025)	(0.103)
Age & Age Squared	Yes	Yes	Yes
Region of birth	Yes	Yes	Yes
N	710,962	630,111	630,111

Notes: The dependent variables are the employment dummy, log wage and months worked in columns(1), (2) and (3), respectively. The independent variables are the interaction between monthly dummies and the fraction of amnestied workers within each cell c, defined here as the interaction between provinces and sectors. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 15: Effects of the regularization on the labor market outcomes using a different definition in 2003

	Pr(E)	wage	Months worked
Jan	0.000	0.000	0.000
	(.)	(.)	(.)
Feb	-0.015**	0.027*	0.056
	(0.007)	(0.016)	(0.054)
Mar	-0.017*	0.010	-0.024
	(0.010)	(0.020)	(0.072)
Apr	-0.036***	0.045**	0.103
	(0.011)	(0.023)	(0.082)
May	-0.041***	0.033	0.120
	(0.014)	(0.024)	(0.096)
Jun	-0.052***	0.024	0.173
	(0.017)	(0.026)	(0.110)
Jul	-0.069***	0.058**	0.282**
	(0.019)	(0.028)	(0.126)
Aug	-0.077***	0.046*	0.303**
	(0.019)	(0.027)	(0.123)
Sep	-0.102***	0.102***	0.572***
	(0.023)	(0.032)	(0.151)
Oct	-0.106***	0.108***	0.618***
	(0.024)	(0.033)	(0.154)
Nov	-0.123***	0.135***	0.735***
	(0.028)	(0.038)	(0.178)
Dec	-0.128***	0.152***	0.798***
	(0.028)	(0.042)	(0.193)
Age & Age Squared	Yes	Yes	Yes
Region of birth	Yes	Yes	Yes
N	1,172,448	1,038,321	1,038,321

Notes: The dependent variables are the employment dummy, log wage and months worked in columns(1), (2) and (3), respectively. The independent variables are the interaction between monthly dummies and the fraction of amnestied workers within each cell c, defined here as the interaction between provinces and sectors. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

**Table 16:** Effects of the regularization on the labor market outcomes using a different definition in 2004

	Pr(E)	wage	Months worked
Jan	0.000	0.000	0.000
	(.)	(.)	(.)
Feb	-0.002	-0.003	-0.032
	(0.007)	(0.019)	(0.060)
Mar	-0.000	-0.036	-0.132
	(0.011)	(0.024)	(0.082)
Apr	-0.012	-0.025	-0.100
	(0.013)	(0.028)	(0.102)
May	-0.010	-0.040	-0.138
	(0.017)	(0.031)	(0.121)
Jun	-0.009	-0.056*	-0.154
	(0.017)	(0.031)	(0.118)
Jul	-0.017	-0.025	-0.076
	(0.019)	(0.032)	(0.131)
Aug	-0.011	-0.046	-0.145
	(0.021)	(0.034)	(0.141)
Sep	-0.016	-0.045	-0.088
	(0.022)	(0.034)	(0.147)
Oct	-0.035	-0.031	0.017
	(0.024)	(0.037)	(0.159)
Nov	-0.046*	-0.009	0.095
	(0.025)	(0.042)	(0.182)
Dec	-0.053*	-0.001	0.157
	(0.032)	(0.050)	(0.235)
Age & Age Squared	Yes	Yes	Yes
Region of birth	Yes	Yes	Yes
N	1,117,320	987,849	987,849

Notes:The dependent variables are the employment dummy, log wage and months worked in columns(1), (2) and (3), respectively. The independent variables are the interaction between monthly dummies and the fraction of amnestied workers within each cell c, defined here as the interaction between provinces and sectors. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

9 Appendix A

9.1 Employment effects using the share of amnestied workers in the entire sample of workers

Figure 10: Effects of the Amnesty on the Employment Probability of Native Workers using the share of amnestied workers in the entire sample
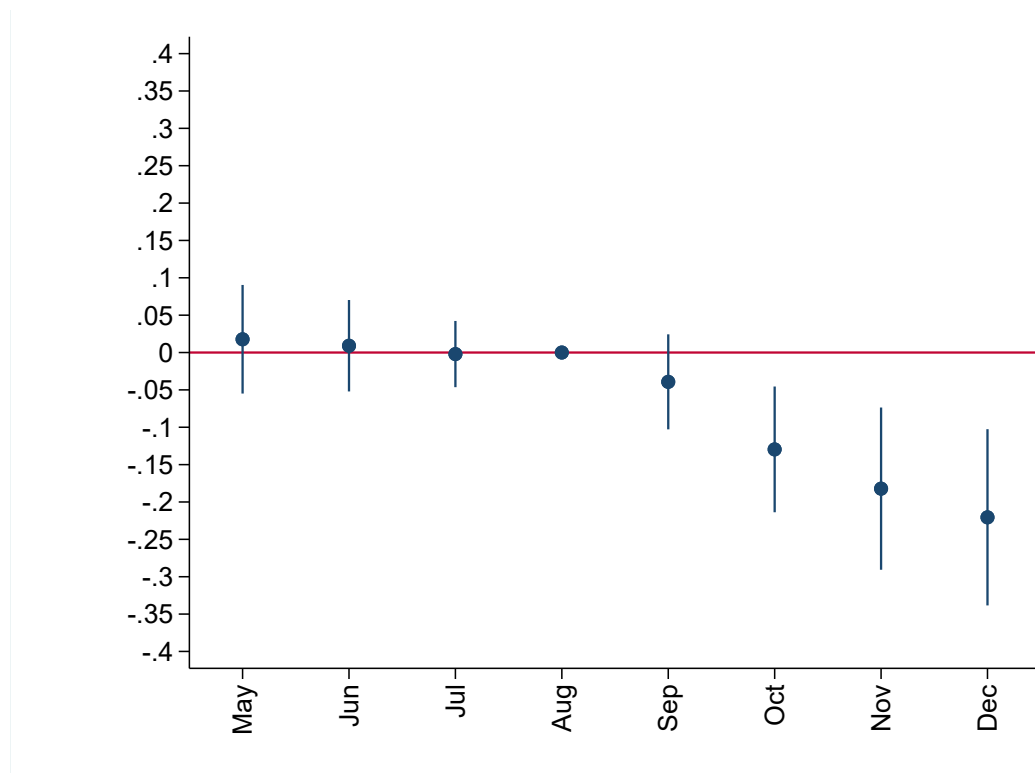
Notes: Figures show the coefficients of the interactions between share of amnestied migrants in the entire sample of workers, natives and foreigners, and monthly fixed effects. The regression includes cell-year fixed effects, cell-month fixed effects, month-year fixed effects. Standard errors are clustered at cell level. Estimates show a 95% confidence interval.

9.2 The Measurement Issue

The share of declared native workers and the fraction of amnestied workers are unobservable since informal workers are not observed in the social security records. Therefore, I use two proxies for both variables: the ratio of declared native employees to the total number of native workers who work at least for one month in a given year and the ratio of amnestied workers to the total number of foreign workers who work at least for one month in a given year. The

relationships between the true variables and the proxies are the following:

$$\frac{E_{c\tau}^{DN}}{L_{cy}^{N}} = \frac{E_{c\tau}^{DN}}{L_{cy}^{DN} + L_{cy}^{UN}} = \frac{E_{c\tau}^{DN}}{L_{cy}^{DN}} \frac{L_{cy}^{DN}}{L_{cy}^{DN} + L_{cy}^{UN}} \tag{15}$$

$$\frac{AMN_{cy}}{L_{cy}^{M}} = \frac{AMN_{cy}}{L_{cy}^{DM} + L_{cy}^{UM}} = \frac{AMN_{cy}}{L_{cy}^{DM}} \frac{L_{cy}^{DM}}{L_{cy}^{DM} + L_{cy}^{UM}} \tag{16}$$

where E_{cmy}^{DN} is the total number of declared native employees within the cell c at time *tau* (year-month). AMN_{cy} is total number of amnestied migrants within a cell c in a given year. Let $j = M, N$, L_{cy}^{j} is the total labor supply of each group j. L_{cy}^{Uj} and L_{cy}^{Dj} are the total number of undeclared workers who did not ever have a formal job in a given year and the total number of declared workers who have spent at least one month in formal job, respectively. From data, I observe only $\frac{E_{c\tau}^{DN}}{L_{cy}^{DN}}$ and $\frac{AMN_{cy}}{L_{cy}^{DM}}$. Rewriting both equations as a function the true variables, $\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}$ and $\frac{AMN_{cy}}{L_{cy}^{M}}$:

$$\frac{E_{c\tau}^{DN}}{L_{cy}^{DN}} = \frac{E_{c\tau}^{DN}}{L_{cy}^{N}} (1 + \frac{L_{cy}^{UN}}{L_{cy}^{DN}}) \tag{17}$$

$$\frac{AMN_{cy}}{L_{cy}^{DM}} = \frac{AMN_{cy}}{L_{cy}^{M}} (1 + \frac{L_{cy}^{UM}}{L_{cy}^{DM}}) \tag{18}$$

The estimates of the relationship between the true proxies is:

$$\widehat{\beta} = \frac{Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{DN}}, \frac{AMN_{cy}}{L_{cy}^{DM}})}{Var(\frac{AMN_{cy}}{L_{cy}^{DM}})} \tag{19}$$

Substituting to both proxies with the equations (16) and (17), I obtain:

$$\widehat{\beta} = \frac{Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}(1 + \frac{L_{cy}^{UN}}{L_{cy}^{DN}}), \frac{AMN_{cy}}{L_{cy}^{M}}(1 + \frac{L_{cy}^{UM}}{L_{cy}^{DM}}))}{Var(\frac{AMN_{cy}}{L_{cy}^{M}}(1 + \frac{L_{cy}^{UM}}{L_{cy}^{DM}}))} \tag{20}$$

Let α_{cy}^{j} be the ratio $\frac{L_{cy}^{Uj}}{L_{cy}^{Dj}}$ and $Var(\frac{AMN_{cy}}{L_{cy}^{M}}(1 + \frac{L_{cy}^{UM}}{L_{cy}^{DM}}))$ be $\sigma_{\widehat{x}}^{2}$.

$$
\begin{aligned}
\widehat{\beta} = \frac{1}{\sigma_{\widehat{x}}^{2}} &\left(Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}, \frac{AMN_{cy}}{L_{cy}^{M}}) + Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}\alpha_{cy}^{N}, \frac{AMN_{cy}}{L_{cy}^{M}}) \right) \\
+ \frac{1}{\sigma_{\widehat{x}}^{2}} &\left(Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}\alpha_{cy}^{N}, \frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M}) + Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}, \frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M}) \right)
\end{aligned} \tag{21}
$$

Substituting the true relationship, $\frac{E_{c\tau}^{DN}}{L_{cy}^{N}} = \beta\frac{AMN_{cy}}{L_{cy}^{M}} + \varepsilon_{c\tau}$, and assuming that all the covariances including the error term are zero.

The expected value of the estimates is:

$$
\begin{aligned}
E(\widehat{\beta}) = \frac{1}{\sigma_{\widehat{x}}^{2}}E &\left(\beta Var(\frac{AMN_{cy}}{L_{cy}^{M}}) + \beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N}, \frac{AMN_{cy}}{L_{cy}^{M}}) \right) \\
+ \frac{1}{\sigma_{\widehat{x}}^{2}}E &\left(\beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}}, \frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M}) + \beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N}, \frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M}) \right)
\end{aligned} \tag{22}
$$

I assume that the total number of both native and foreign undeclared workers is smaller than the total number of both native and foreign declared workers, respectively. Therefore, α_{cy}^{j} is smaller than one and the variance of $\frac{AMN_{cy}}{L_{cy}^{M}}$ is larger of $\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{j}$. Since $|Cov(x,y)| \leq max\{\sigma_{x}^{2}, \sigma_{y}^{2}\}$, I can rewrite the numerator:

$$E(\widehat{\beta}) = \frac{\beta}{\sigma_{\widehat{x}}^{2}}\left(\sigma_{x}^{2} + \sigma_{x}^{2} - \sigma_{x}^{2} + Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N}, \frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M}) \right) \tag{23}$$

The second σ_{x}^{2} has a negative sign since the covariance between the share of amnestied and share of amnestied times the ratio of "invisible" foreigners to "visible" foreigners is negative.

Following the theoretical framework, an increase in the share of amnestied lowers the share of "invisible" migrants. While, an increase in the amnestied workers might increase the number of "invisible" native workers. Since the variance of the denominator is larger than σ_x^2 since $\frac{AMN_{cy}}{L_{cy}^M} \le \frac{AMN_{cy}}{L_{cy}^{DM}}$, I can rewrite eq. 22 as:

$$E(\widehat{\beta}) = \frac{\beta}{\gamma \sigma_x^2} \left(\sigma_x^2 + Cov(\frac{AMN_{cy}}{L_{cy}^M}\alpha_{cy}^N, \frac{AMN_{cy}}{L_{cy}^M}\alpha_{cy}^M) \right) \tag{24}$$

where $\gamma \ge 1$. Rewriting the $Cov(\frac{AMN_{cy}}{L_{cy}^M}\alpha_{cy}^N, \frac{AMN_{cy}}{L_{cy}^M}\alpha_{cy}^M)$ as $\sigma_{\widetilde{x},\overline{x}}$. Eq. 23 is:

$$E(\widehat{\beta}) = \beta \left(\frac{1}{\gamma} + \frac{\sigma_{\widetilde{x},\overline{x}}}{\gamma \sigma_x^2} \right) \tag{25}$$

Since $\sigma_x^2 \ge \sigma_{\widetilde{x},\overline{x}}$ and $\sigma_{\widetilde{x},\overline{x}}$ is negative, the bias in the parenthesis is positive and lower than one. The estimate of the true parameter is a lower bound.

9.3   Employment Spells

The main assumption of the theoretical model is that most productive workers work more hours in large enough time span, for instance a month, than the least productive workers. Therefore, a lower probability of being employed for the least productive group should reflect in an increase in the average months spent at work at aggregate level.

Table A.1 shows the effect of the amnesty on the months worked by age. The average employment spell increases after August. The effect is meaningful for youngsters and elders. In particular, youngsters experience the largest increase in the employment spells. However, the effect declines when I add controls. The estimates mirrors the employment effect for the age groups since the probability of being employed decreases only for youngsters and elders.

Table A.2 shows the effect when adding individual fixed effects. As for the wage effect,

the effect of the amnesty on months worked fades away controlling for fixed unobservable characteristics.

Table A.1: Effects of the Amnesty on Months Worked within a Year by Age

	Without Controls				With Controls			
	Overall	<29	28≤Age≤48	>48	Overall	<29	28≤Age≤48	>48
May	0.039	0.273	0.000	0.029	0.033	0.251	0.000	0.032
	(0.056)	(0.177)	(0.062)	(0.145)	(0.054)	(0.158)	(0.061)	(0.144)
Jun	0.046	0.200	-0.002	0.169	0.041	0.160	-0.003	0.168
	(0.049)	(0.147)	(0.047)	(0.126)	(0.047)	(0.129)	(0.047)	(0.125)
Jul	0.029	0.157	0.002	-0.002	0.022	0.114	0.002	-0.001
	(0.039)	(0.115)	(0.036)	(0.079)	(0.037)	(0.105)	(0.036)	(0.079)
Aug	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Sep	0.096*	0.221*	0.017	0.192**	0.090*	0.185	0.017	0.190**
	(0.053)	(0.128)	(0.054)	(0.097)	(0.051)	(0.117)	(0.053)	(0.096)
Oct	0.248***	0.558***	0.075	0.278**	0.230***	0.506***	0.072	0.277**
	(0.067)	(0.176)	(0.069)	(0.116)	(0.065)	(0.164)	(0.068)	(0.115)
Nov	0.321***	0.627**	0.106	0.391**	0.301***	0.568**	0.103	0.388**
	(0.092)	(0.250)	(0.094)	(0.155)	(0.089)	(0.241)	(0.094)	(0.156)
Ind. contr.	No	No	No	No	Yes	Yes	Yes	Yes
N	483,908	85,988	296,482	87,059	483,908	85,988	296,482	87,059

Notes: The dependent variable is the individual number of months worked within a year in the month m at year y within the cell c. The independent variables are the interaction between monthly dummies and fraction of amnestied workers within each cell c. Estimates include the year-cell fixed effects, month-cell fixed effects and the month-year fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.2: Effects of the Amnesty on Months Worked within a Year by Age Controlling for Individual Fixed Effects

	Months Worked
Jun	-0.002
	(0.003)
Jul	-0.001
	(0.003)
Aug	0.000
	(.)
Sep	-0.001
	(0.005)
Oct	-0.004
	(0.009)
Nov	-0.010
	(0.019)
N	375,312

Notes: The dependent variable is the number of months worked in a year. The independent variables are the interaction between monthly dummies and the fraction of amnestied foreigners within the cell c. Regression includes individual-year fixed effects, individual-month fixed effects, individual-year time monthly specific time trends, and month-year fixed effects. Standard error are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

9.4 Results Using 2000 As Control Year

Table A.3: Effects of the Amnesty with 2000 as base year

	Pr(E)	wage
May	-0.007	0.031**
	(0.010)	(0.014)
Jun	-0.003	0.020*
	(0.007)	(0.011)
Jul	-0.007	0.018*
	(0.005)	(0.010)
Aug	0.000	0.000
	(.)	(.)
Sep	-0.017***	0.032**
	(0.006)	(0.012)
Oct	-0.026***	0.038***
	(0.008)	(0.013)
Nov	-0.028***	0.043**
	(0.010)	(0.017)
N	526,701	468,557

Notes: The dependent variables are a dummy equal to one if the individual is employed and the log wage in the first and second columns, respectively. Regressions include individual fixed effects, individual specific time trends, and month fixed effects. Standard errors are clustered at cell level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

10   Appendix B

The main model assumes that firms fully observes the productivity of each worker. Here, I relax this assumption by assuming that a representative firm employs a worker, either foreigner or native, with a productivity level below the threshold in an informal occupation with probability ω_1, while employs a worker with a productivity threshold above the threshold in an informal occupation with probability ω_2. The complement is the probability of hiring the group of workers in a formal occupation. Therefore, the production functions and the cost functions becomes

the following:

$$Y_I = (\omega_1 (\int_0^{\overline{\theta^M}} (H(\theta)^M)^\beta \, d\theta + \int_0^{\overline{\theta^N}} (H(\theta)^N)^\beta \, d\theta$$
$$+ \omega_2 (\int_{\underline{\theta^M}}^1 (H(\theta^M)^\beta \, d\theta + \int_{\underline{\theta^N}}^1 (H(\theta)^N)^\beta \, d\theta))^{\frac{1}{\beta}} \tag{26}$$

$$Y_F = ((1 - \omega_1)(\int_0^{\overline{\theta^M}} (H(\theta)^M)^\beta \, d\theta + \int_0^{\overline{\theta^N}} (H(\theta)^N)^\beta \, d\theta$$
$$+ (1 - \omega_2)(\int_{\underline{\theta^M}}^1 (H(\theta^M)^\beta \, d\theta + \int_{\underline{\theta^N}}^1 (H(\theta)^N)^\beta \, d\theta))^{\frac{1}{\beta}} \tag{27}$$

$$TC_I = \omega_1 (w_I (\int_0^{\overline{\theta^M}} (H(\theta)^M) \, d\theta + \int_0^{\overline{\theta^N}} (H(\theta)^N) \, d\theta)$$
$$+ \int_0^{\overline{\theta^M}} f^M p(\theta^M) L(\theta)^M \, d\theta + \int_0^{\overline{\theta^N}} f^N p(\theta^N) L(\theta)^N \, d\theta)$$
$$+ \omega_2 (w_I (\int_{\underline{\theta^M}}^1 (H(\theta^M) \, d\theta + \int_{\underline{\theta^N}}^1 (H(\theta)^N) \, d\theta)$$
$$+ \int_{\underline{\theta^N}}^1 f^N p(\theta^N) L(\theta)^N \, d\theta + \int_{\underline{\theta^M}}^1 f^M p(\theta^M) L(\theta)^M \, d\theta) \tag{28}$$

$$TC_F = w_I (1 + t)((1 - \omega_1)(\int_0^{\overline{\theta^M}} (H(\theta)^M) \, d\theta + \int_0^{\overline{\theta^N}} (H(\theta)^N) \, d\theta)$$
$$+ (1 - \omega_2)(\int_{\underline{\theta^M}}^1 (H(\theta^M) \, d\theta + \int_{\underline{\theta^N}}^1 (H(\theta)^N) \, d\theta)) \tag{29}$$

The first order condition for $\overline{\theta}^M$ is the following:

$$\frac{1}{\beta}(c\overline{\theta}^M L^M)^\beta [p_1 Y_I^{1-\beta} - p_2 Y_F^{1-\beta}](\omega_1 - \omega_2) - \overline{\theta}^M L^M [f_M - cwt](\omega_1 - \omega_2) = 0 \tag{30}$$

The term $(\omega_1 - \omega_2)$ simplifies in equation (30). Therefore, the solution of each threshold does not change from the main model.