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Worker Substitution Patterns Across Ages, Language, and Nationalities

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The Labor Market Effects of an Unexpected Amnesty for Undocumented Workers *

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Abstract

This paper investigates the labor market effects of the 2002 Italian amnesty for undocumented workers which allowed employers to declare their undocumented employees. The amnesty granted a residence permit to around 700,000 foreign workers. Exploiting the variation in the share of amnesty workers within each labor market, I find a negative effect on the probability of being formally employed in a formal occupation. Furthermore, I find that the amnesty tends to crowd out the lowest native workers leading to a positive composition effect on wages within each market. Indeed, using individual data, the employment effect persists while the wage effect fades away. This effect is higher within regions with a high fraction of low-skilled workers. To explain such mechanism, I develop a model showing firms' hiring decision in the formal market.

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

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1 Introduction

The recent migration crises increased the inflow of undocumented migrants in both European Union and the US. In 2009, United Nations estimated around 50 millions 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 millions in 2008 (Kovacheva et al., 2011). Numbers are very likely to be larger after the 2011 and 2015 migration crises.

The large number of undocumented immigrants heats the debate on granting an amnesty. To support the amnesty, democratic parties argue that undocumented foreigners are mainly employed in hard labor jobs where both primary and secondary sectors of Western countries have been complaining about labor shortages (Orrenius and Zavodny (2009)). Yet, even if a great deal of articles show positive effects of amnesties on several outcomes of legalized migrants (Amuedo-Dorantes and Bansak (2011); Amuedo-Dorantes et al. (2007); Bahar et al. 2021; Baker (2015); Cobb-Clark et al. (1995); Devillanova et al. (2017); Di Porto et al. (2018); Kaushal (2006); Mastrobuoni and Pinotti (2015); Monras et al. (2020); Pinotti (2017)), few papers have investigated on the side effects of an amnesty on the labor market outcomes of natives.

This paper investigates the effects of the 2002 Italian amnesty on the labor market outcomes of native workers. The two main characteristics of the amnesty were: it was granted suddenly in September 2002 after an increase in penalties for hiring undocumented workers; and, employers were in charge of applying for the regularization of their undocumented employees. The post-amnesty increase in the penalties led to a greater labor cost that could lower the labor demand of legal natives workers and/or increase the demand of undeclared native workers. Penalties' increase was a common policy also in other countries (e.g. the 1986 U.S. amnesty (IRCA) and the 2004 Spanish amnesty). While, the proof of the existence of a labor relationship was a novelty among Western countries. Putting in charge employers of the application gave firms an increase in the monopsony power since it was worth applying for the amnesty only if the cost of keeping on employing undocumented workers was greater than the cost of declaring them. The focus is to understand whether firms substitute legal native workers with amnestied workers, a demand effect.

The main threat arising from estimating a pure demand effect of an amnesty program on the labor market outcomes of native workers is to deal with supply factors that might bias the estimates. In particular, estimates of the demand effect are biased if amnestied migrants decide to change labor market after getting the residence permit. Indeed, some amnesty programs (e.g. IRCA 1986) do not allow to isolate such effect since migrants are free to move across markets before and after the regularization. While, the 2002 Italian amnesty requires that undocumented migrants can apply for the work visa only if they have been working for the same employers for, at least, the last three months before the amnesty[1]. The requirements of the Italian amnesty lower the likelihood of estimating a mixed effect between supply and demand factors since the labor supply of undocumented migrants is very likely to be fixed in a narrowed window around the policy implementation. Furthermore, Italian Government implemented the policy just few days after the announcement, while other countries opened the amnesty some months after approving it (e.g. the 2004 Spanish amnesty). Therefore, the Italian case provides a natural experiment to estimate the pure demand effects of an amnesty for undocumented foreign workers on native workers.

Before empirically assessing the labor demand effects of the amnesty, I introduce a model showing firms' hiring decision in both formal and informal positions. The model assumes a production function with a continuum of labor inputs with different productivity levels as in De Paula and Scheinkman (2011) and Ulyssea (2018). The solution of a representative firm's maximization problem is a productivity level (threshold) such that workers above this productivity level are legally employed, while the ones below work without a formal contract. Foreigners' and natives' thresholds depend on the cost of hiring them without a formal contract. Since the penalties for hiring undocumented workers are grater than the ones for hiring documented workers without a formal contract, the two threshold are not equal. Finally, the

[1] Di Porto et al. (2018) show that the amnestied migrants do not change occupation in the very short run.

comparative statics shows that an increase in the fine for hiring undocumented workers leads to an increase in natives' threshold and a decrease in foreigners' threshold. In other words, the model predicts a lower probability of being declared for natives and a higher probability of being declared for foreigners after the reform.

To test empirically the predictions of the theoretical model, I use a monthly sample of social security data on both employers and employees between 2001 and 2002. The data provides information on individual characteristics, labor contracts and firm characteristics. In particular, I predict the amnestied foreign workers by matching the application rules and the labor contract characteristics. Then, I use the market-specific share of regularized immigrants to evaluate the impact on the individual labor market outcomes of natives. To define the market, I assume that firms sharing the same probability of being inspected belong to the same market. Since I do not observe the likelihoods, I infer that firms belong to the same market if they are within the same province-sector-size cell.

I use an event-study framework to estimate the causal effect of the amnesty on labor market outcomes of native workers. Using monthly observations, I can control for a full set of month-cell, year-cell, year-month fixed effects. Conditional on the fixed effects, the share of amnestied migrants captures the causal effect of the amnesty. Results show a negative effect on the legal employment of native workers both at cell and at individual level. Moreover, I find a null effect on individual wages and a positive effect on the cell wage which implies that the low-skilled natives are more likely to be crowded out. Finally, I show that the effect is bigger within locations with a high concentration of low-skilled native workers.

My contribution is threefold. First, the theoretical framework shows how the labor demands for both declared and undeclared native workers change when employers are in charge of applying for the amnesty and experience an increase in the penalties for hiring undocumented workers. The main result of the model is that an increase in the penalties for hiring undocumented workers might lead to an increase in the share of undeclared natives. The novelty is the introduction of a detection probability that depends on the productivity of each worker. Previous models assume that the probability of being fined is exogenous and does not depend on the workers' skills. I assume that the detection probability is an increasing function of the productivity level since hours worked are an increasing function of the productivity of each worker. Therefore, the expected fine is both a function of an exogenous probability of being inspected and an endogenous probability of detecting an informal worker.

Second, I study the short-run labor market effects of the amnesty by using monthly employer-employee data. The use of monthly observations allows me to lower the bias stemming from general equilibrium effects. Indeed, native workers are less likely to change occupation and/or location immediately to offset the negative effects of the amnesty in the very short run. Furthermore, it allows me to control for any seasonal and yearly effect within each market. This is the first paper using monthly observations to evaluate the impact of an amnesty program on the labor market outcomes of native workers in a developed country.

Third, this is the first paper to isolate the pure labor demand effects of an amnesty on legal employment of native workers. Estimating labor demand effect of an amnesty helps to understand whether a change in the labor cost of some specific group of workers, amnestied migrants, leads to adverse effects on the employment of another group of workers, native workers. In particular, demand effects play a key role when the two groups are very likely to supply the same skills in the labor market. Italy is a good example since the fraction of native workers with a lower than secondary educational attainment was around 60% in 2002 and amnestied migrants were employed in low-skilled occupations.

This paper contributes to the discussion about side effects of amnesty programs. The research has mainly focused on evaluating the impact on the post-amnesty outcomes of regularized workers (Amuedo-Dorantes and Bansak (2011), Devillanova et al. (2017), 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, even if their results are in line with mine, the amnesty rules were different as well as the skill composition of Venezuelan workers. In particular, undocumented workers could apply for the amnesty without having any job. Second, Di Porto et al. (2018) use also the 2002 Italian amnesty and social security data to show the effects on labor market outcomes. They do not find any effect on the labor market outcomes of native workers. However, their estimates might be biased by general equilibrium effects since they use yearly observations. After one year, the negative effect of the amnesty on the legal employment of native workers might be offset by the decision of affected workers to move to another market. Finally, Monras et al. (2021) show no effect on the employment and on wage of native workers, either. As for Di Porto et al. (2018), they use yearly observations to evaluate the impact of the amnesty on native workers.

Finally, the theoretical framework also contributes to understand the dynamics within the informal markets. Recent papers investigate the general equilibrium effects of an increase in sanctions for hiring undocumented workers and/or an enforcement of the border patrols (Albert (2019), Chassamboulli and Peri (2015), Machado (2017), Ulyssea (2018)). However, partial effects have not been deeply studied. Monras et al. (2021) is the only paper to discuss the micro effect of an amnesty or/and an increase in the sanctions. They develop the model by assuming different degree of substitutability among labor inputs and different labor supply curves between legal and illegal foreign workers. However, in the very short run prices are more likely to be fixed when the labor supply is fixed, e.g. after an amnesty. Therefore, contrary to them, I assume that both prices and production are fixed.

The reminder of the paper is organized as follows. Section 2 presents the background of the amnesty. Section 3 presents the model. Section 4 describes the data and show some descriptive evidences. Section 5 discusses the empirical strategy. Section 6 shows the results. Section 7 provides some robustness checks. Finally, Section 8 concludes.

2 Background

During the ’70s, 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 have increased from 600,000 in 1991 to 1.5 millions in 2002. Up to the late ’80s, a 1930 law and governmental instructions ruled the status and the relative rights of migrants in Italy. In 1986, 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. However, the new migration rules did not prevent inflows of undocumented migrants.

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 after a training period. Yet, quotas were too low to provide residence permits to all migrants working in the country [2]. Therefore, Italian Governments implemented four amnesties in 1990, 1995, 1998, and 2002 to provide a legal status to undocumented immigrants [3]. 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 sector.

Initially, the 2002 amnesty was targeted only at care-givers and domestic workers (Law 189/02). Yet, Government was concerned that foreign employees would have applied for this amnesty by cheating

[2] 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.

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

on the actual occupation [4]. Therefore, after the approval of the new migration law in July, government started to discuss about an amnesty for undocumented foreigners already employed. The most amnesty-skeptic parties of the majority government, *Lega Nord* and *Alleanza Nazionale*, approved the amnesty, Law 222/02, only in September just few days before the entry into force of the new penalties for hiring undocumented migrants (Law 189/02).

The next subsections describe the rules of the amnesty and the employers' incentives to apply.

2.1 The Amnesty

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[5]. To apply 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[6]. 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.

The discussion about an amnesty for undocumented foreign workers started after the approval of the Law 189/02. Even if entrepreneurs' associations were pushing for an amnesty, the parties of the government majority reached an agreement on the characteristics of the amnesty just one week before regularization window opened. The start of regularization overlapped with the start of the new migration law which increased the penalties for hiring undocumented migrants. Therefore, firms could not anticipate the effect of the regularization by changing the labor composition within the firm.

Table 1 shows a summary of the regularization. The total number of applications was around 700,000

[4] This practice was quite common in Italy. 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.

[5] Since most of undocumented worker never had a formal job in any register, *Prefetture*, provincial offices of the Minstry of Interior, check whether migrants were in Italy just before the regularization and did not have another regular occupation.

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

and number of residence permits issued by the government was around 640,000. Less than 50,000 applications were rejected around 7% of the total applications. 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.

2.2 The Demand for the Amnesty

The high number of applications stems from both the new migration law and a low supply of quotas provided by the previous government. After the enactment of the Law 189/02, both undocumented foreign workers and their employers had more incentives to demand for an amnesty since the government increased the penalties for both of them [7]. The new law increased the time of detection in the repatriation centers, the border patrols and the detection in the jails. While, the penalties for hiring undocumented migrants increased from a 3,000 euro to a 5,000 euro fine per each undocumented migrant employed in the firm plus a criminal trial with the possibility of being jailed from three months to one year. Furthermore, the 2001 quota supply did not match the demand of firms for foreign workers. In particular, the government excluded firms in the South of Italy from immigration quotas. For such reason, entrepreneurs said publicly they would had hired undocumented migrants[8].

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

[7] On the employer side, Di Porto et al. (2018) report also an increase in the number of inspections across the country set by Law 383/01. However, the results of that policy were quite poor since the share of regularized natives was quite low

[8] https://ricerca.repubblica.it/repubblica/archivio/repubblica/2001/05/18/niente-stagionali-al-sud-usate-vostri-disoccupati.html?ref=search

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[9]. The top-left figure shows the largest industry to apply for the amnesty within a region. While, the top-right and the 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.

3 Theoretical Framework

This section offers a framework for understanding a firm's decision of declaring a worker when firms are free to hire foreigner workers and experience higher penalties for hiring undocumented migrants than for hiring undeclared native workers. In this paper, I use the following definition: undocumented workers are migrants working without a work visa and, therefore, are also undeclared; undeclared native workers are all documented workers employed informally. Given the higher labor cost of employing illegal migrants and the high labor demand of foreign workers, it is foreseeable that amnesty will end up to increase 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 the 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

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

framework summarizes both intuitions showing how the substitution occurs.

I assume that a specific-market representative firm produces a homogeneous good sold both in the formal and in the informal market. 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 substitute 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[10].

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

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

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

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 of all foreigners with a productivity level below $\overline{\theta^M}$ and all natives with a productivity level below $\overline{\theta^N}$. While, a firm employs all the workers with a productivity level above those thresholds in the formal production. 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:

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

$$H(\theta)^j = h(\theta)^j L(\theta)^j \quad (3)$$

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[11]. 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 grater than hiring them with an informal contract. Assume that equally productive employees earn the same, heterogeneous expected fines for hiring undeclared workers lead to different probabilities of being declared. The cost of declaring a worker is equal to $w_I(1+t)H_F$, while the cost of hiring an undeclared worker is $w_I H_I + \alpha E[f^j]$. w_I is the unique wage rate in the market, t represents the contribution rate, α is is the probability of being inspected, and, $E[f^j]$ is the expected fine when employing undeclared workers of group j.

I assume that expected fines depend on the probability of detecting an undeclared worker during an inspection. Since the most productive workers are more likely of being employed with a full-time permanent contract, the probability of being detected is increasing in productivity[12]. I assume that detection probability is linear in the productivity:

$$p(\theta^j) = \theta^j \quad (4)$$

The function of the expected fine is the following:

[11] In the empirical specification, the time span is a month.

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

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

where j is either M or N. $L(\theta)$ is the sum of all workers with a specific productivity level. Finally, f^j is the fine for hiring an undeclared worker.

Finally, the firm maximizes the following profit function:

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

p_1 and p_2 are the prices of the informal and formal goods, respectively. H_F and H_I 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 solution is to find 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 $\overline{\theta^N}$. I get the following solution for each productivity threshold:

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

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

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

All pairs $(\overline{\theta_M}, \overline{\theta_N})$ that solve equation (8) maximize 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 [13].

[13] 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.

3.1 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 $\overline{Y^I}$[14]. 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:

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

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

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 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[15].

[14] 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.

[15] The result does not consider the cost of regularizing a migrant since I am considering the increase in the fine and the

4 Data and Descriptive Statistics

I use the Work Histories Italian Panel (WHIP) dataset, a 1% sample of social security records. 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. The sample includes only the employees working in the private sector. Therefore, the analysis excludes domestic workers, care-givers, and self-employed.

I drop women 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.

Following the rules of the amnesty, I select the migrants who are more likely to be regularized. 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. (2018) and Zucchetti (2004). Table 2 shows the share of

amnesty jointly. In the case of no-zero regularization cost, the effect is smaller.

amnestied employees by the main nationalities. The share in my sample, last column, are very similar to shares in the two papers. 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. (2018).

Table 4 shows the descriptive statistics for natives, 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 3.2, 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 employment 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 less-skilled native workers.

5 Empirical Strategy

The identification strategy of the causal effect of the amnesty on the labor market outcomes of natives 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 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 employing informal workers within provinces with a high fraction of small firms. To provide an 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 amnestied migrants on the employment probability of natives. In the main analysis, I consider only the months between May and November and the years 2001 and 2002. The following equation describes the estimation model:

$$y_{i(c)\tau} = \beta_0 + \sum_{t=-3}^{3} \beta_t \frac{Amn_{cy}}{Imm_{cy}} 1\{m = t+8\} + \gamma_{cm} + \gamma_{cy} + \gamma_\tau + \varepsilon_{i\tau} \tag{12}$$

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. β_t measures the effect of the amnesty 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.

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. 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.

The event-study method has two main advantages to estimate the amnesty effects. First, the monthly estimates show whether the effect varies right after the regularization and persists over time. Second, the estimates of the pre-amnesty $\beta_t s$ show whether the assumption of non-anticipatory effect holds. 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.

I include 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.

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 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 wage 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, 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 show that the negative effect of the policy on the probability of being legally employed persists also in December.

Table 5 shows the effect of the amnesty on the extensive margins of native workers. Yet, employer 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, elderlies 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 elderlies 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 elderlies, 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 elderlies to retire and lower the legal hours spent at work of middle-aged workers.

The Tables described above show an affect 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 crowing-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 [16].

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

[16] Appendix 8.3 shows the effects on other labor market outcomes that proxy labor productivity

points. To check whether it is a composition effect, the second and the third columns show the estimates adjusted for age and age squared and age, age squared and a dummy for region of birth, respectively. Estimates become smaller and less significant. The pre-amnesty estimates are not different from zero. Table 9 shows the estimates by age group. The estimates are grater and positive for the youngsters and the elderlies than for the middle-aged.

6.3 Individual Effects of the Amnesty

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 individual fixed effects.

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 it, I select only the individuals who are in the data for 24 months between 2001 and 2002. Then, I compute the first differences within each month and run the regression only for 2002. The estimator is the following:

$$\Delta y_{i(c)m} = \beta_0 + \sum_{t=-3}^{3} \beta_t \frac{Amn_c}{Imm_c} 1\{m = t+8\} + \gamma_i + \gamma_i * m + \gamma_m + \varepsilon_{im} \tag{13}$$

$\Delta y_{i(c)m}$ is the first difference of either the employment dummy or the log wage, β_t is the parameter of interest which measures the effect of the amnesty on the individual outcomes, $\frac{Amn_c}{Imm_c}$ is the amnesty ratio, $1\{m = t+8\}$ is a set of monthly dummies. $\gamma_i, \gamma_i * m, \gamma_m$ are individual fixed effects, individual fixed effects times a monthly trend, and the month fixed effects, respectively. ε_{im} is the an 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[17]. In particular, the employment likelihood declines over time.

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 [18]. 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 grater effect.

Table 11 shows the effect of the amnesty on the probability of being declared and on wages within cells. The odd and the 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 in the Northern regions. While the crowding-out effect on the legal employment is more likely within Southern regions.

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 model for the years 2001

[17]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

[18]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.

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, even subtracting the positive effect in 2001 to 2002 estimates, the negative effect still holds[19].

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

[19] 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 16 in Appendix 9.3. The results still hold.

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

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 14 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. While, the composition effect on wages is larger because not crowded-out foreign workers are more likely to be more productive and, so, weight more in upper part of the wage distribution.

8 Conclusion

This article investigates the effects of a policy that increases the penalties for hiring undocumented workers and allows to employers to regularize them on the demand of legal native workers. To study this relationship, I exploit the 2002 Italian amnesty which granted a work visa to around 600,000 undocumented migrants. I find that the policy lowers the legal employment of the native workers. Old and young workers experience a decrease of the legal employment on extensive margins since they are more likely to be as productive as undocumented workers in hard labor jobs. While middle-aged workers experience only an increase in part-time jobs. To check whether the crowded-out workers are less productive than the unaffected workers, I show that wages increase within markets but not at individual level. Therefore, the policy changes the composition of legal workers by crowding out the less skilled ones. Finally, I show that the employment effect is greater in areas with a large share of low-skilled workers.

The results show that employers prefer to regularize undocumented workers than hiring legal native workers when they experience an increase in the cost of employing undocumented workers. Evaluating the demand effects of an amnesty policy bundled with an increase in the penalties for hiring undocumented migrant on the legal employment of native workers is not trivial. The 2002 Italian amnesty is suited for showing this mechanism since it required a formal proof showing that foreign employee have been working for the same firm before applying for the regularization and was unexpected. Other amnesties, like 1986 IRCA in the U.S. and the 2004 Spanish amnesty, do not ask for this requirement to apply for the regularization and/or are anticipated. For such reason, the effect of the amnesty is very likely to be a mix between supply and demand factors when government anticipates the amnesty and/or does not require an employment proof. While, the former employment relationship and the sudden decision of allowing an amnesty allow me to isolate the labor demand effects. However, even if the effect of the bundled treatment, amnesty and penalties' increase, on the labor demand of native legal workers is negative, I cannot observe whether there is an increase in the labor demand of undeclared workers. Therefore, a future research project would like to answer to the question of whether amnesty increases the informal employment of native workers.

Finally, even though amnesties have positive effects on the well-being of former undocumented migrants, a policy maker should also consider the side effects on other groups of workers. In particular, when the share of low-skilled workers is large, the amnesty might have adverse effects on the legal employment of native workers. A worsening of the labor market outcomes might lead to anti-immigration sentiments and an increase in the vote for far-right parties. Therefore, the enactment of an amnesty policy should also consider the country-specific labor market characteristics to avoid negative side effects on other groups of workers.

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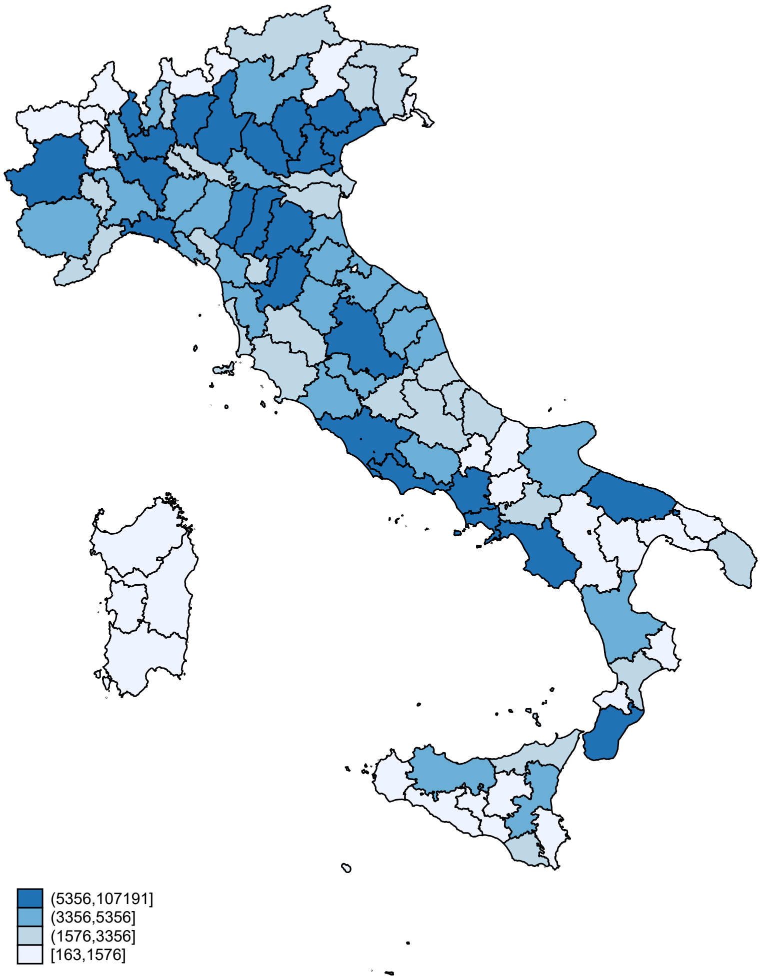
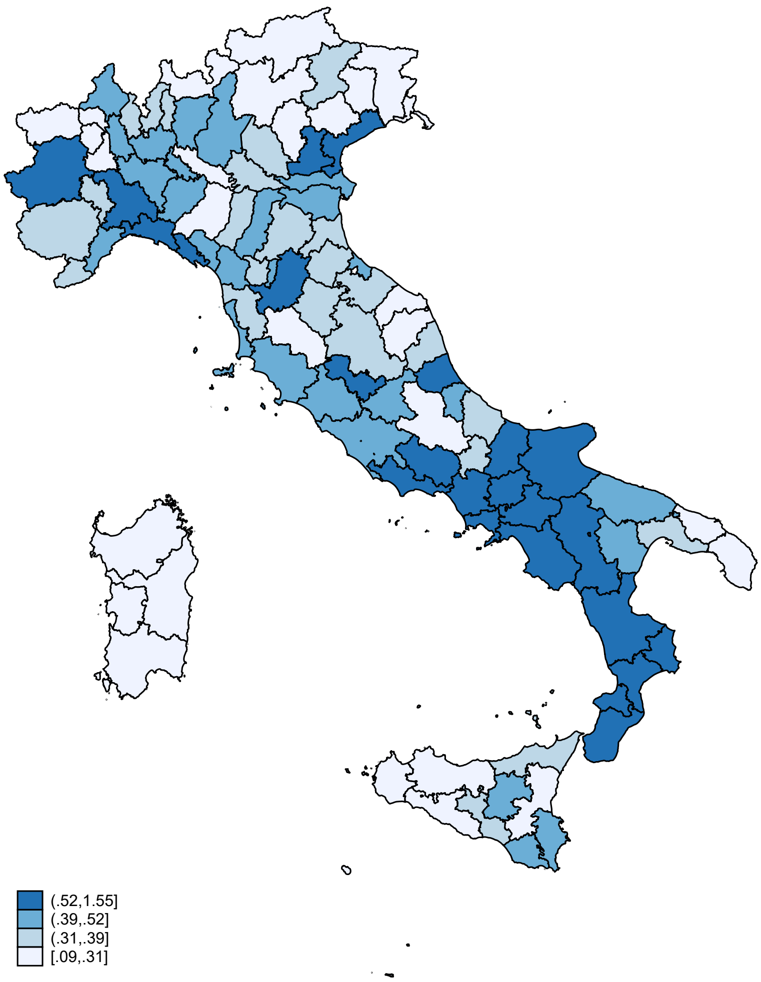
Figure 1: Total number of Applications by Province



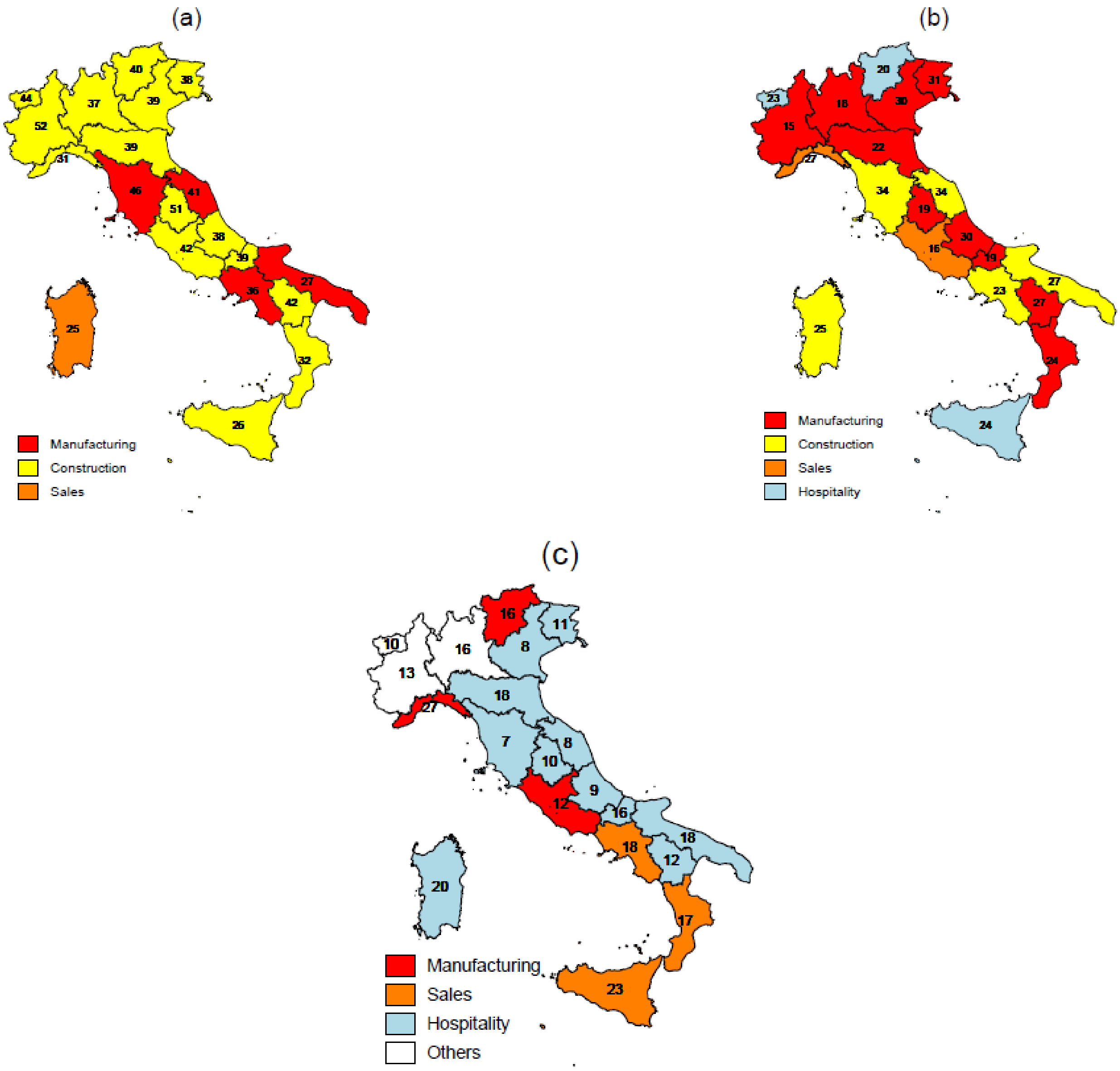
Notes: Figure shows the number of applications for the amnesty within each province.
Source: Zucchetti (2004)

Figure 2: Ratio of Accepted Applications to the 2001 Legal Foreigners within Province



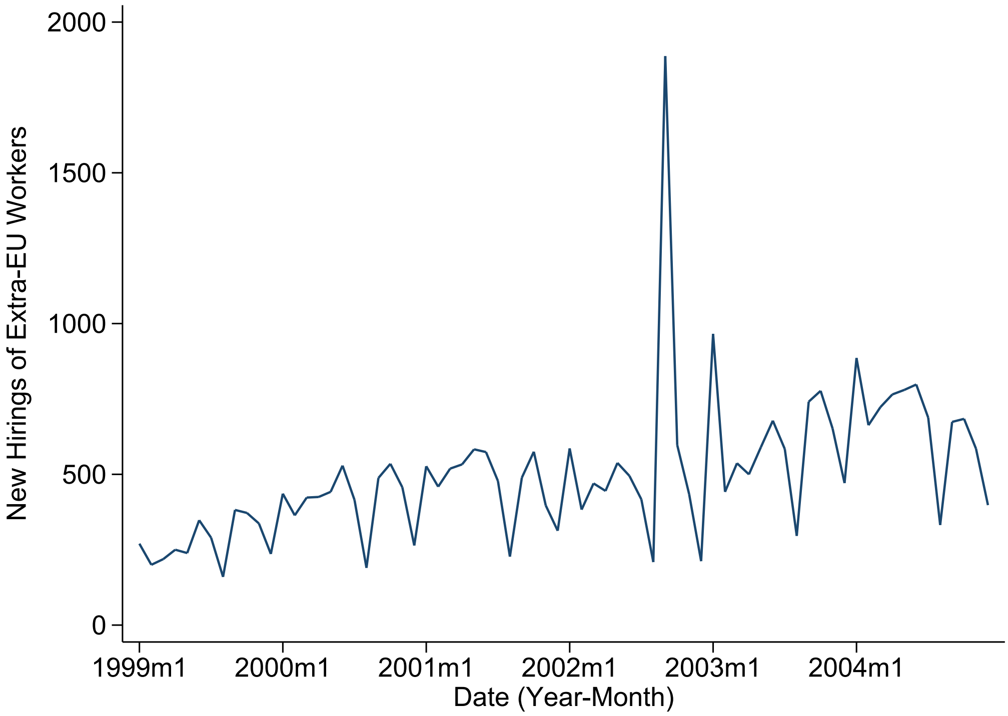
Notes: Figure shows the ratio of amnestied workers to the former legal foreigners within each province.
Source: Zucchetti (2004) and ISTAT

Figure 3: Demand for the Amnesty by Region and Sector



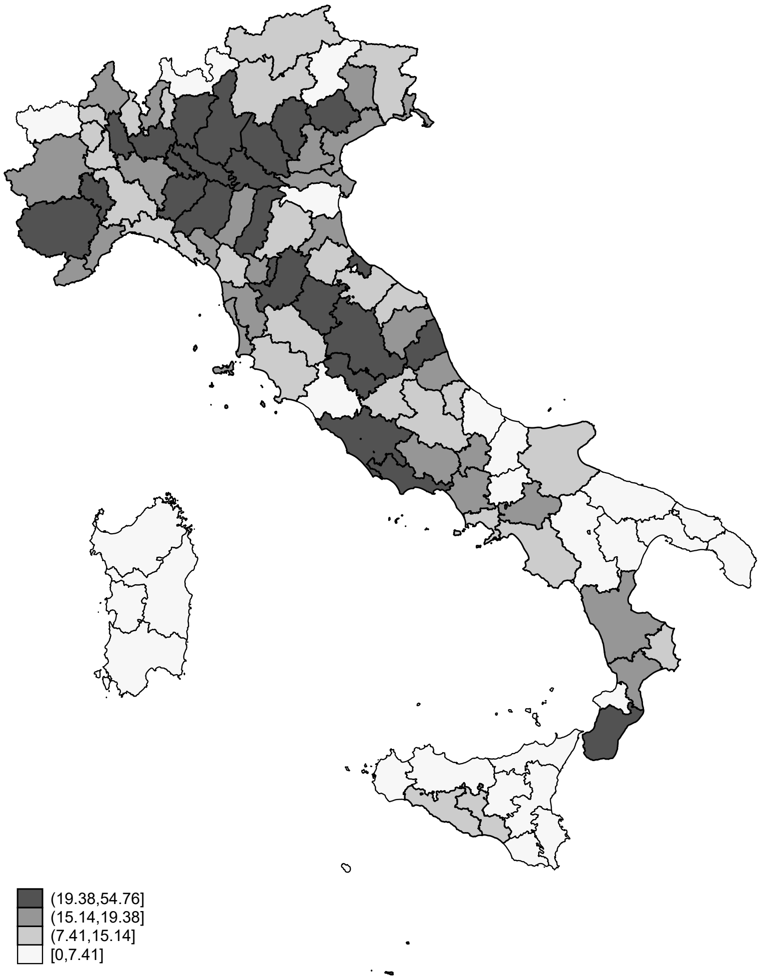
Notes: Figures show the share of applications by industry within regions. (a), (b), and (c) show the share of the first, the second and the third industry, respectively.
Source: Congia (2005).

Figure 4: New Contracts of Extra-EU Workers Between 2000 and 2004



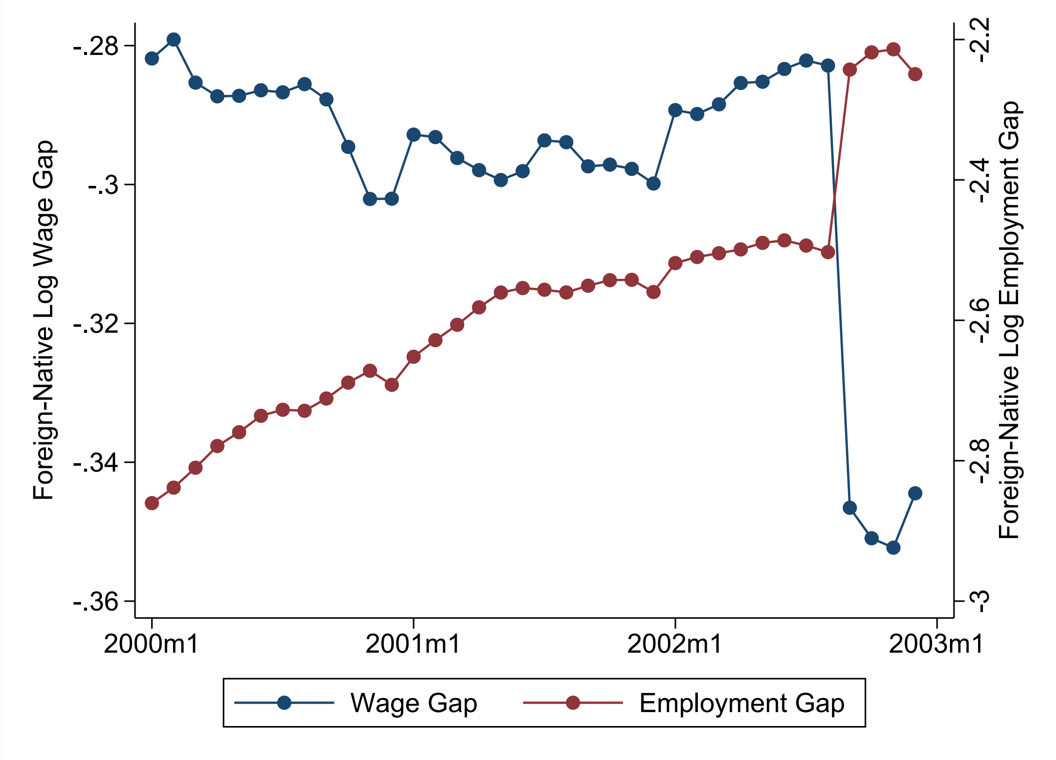
Notes: Figure shows the new hirings of non-EU workers from January 1999 to December 2004.
Source: WHIP.

Figure 5: Ratio between non-EU worker, regularized, workers and employees per thousands of contracts (‰)



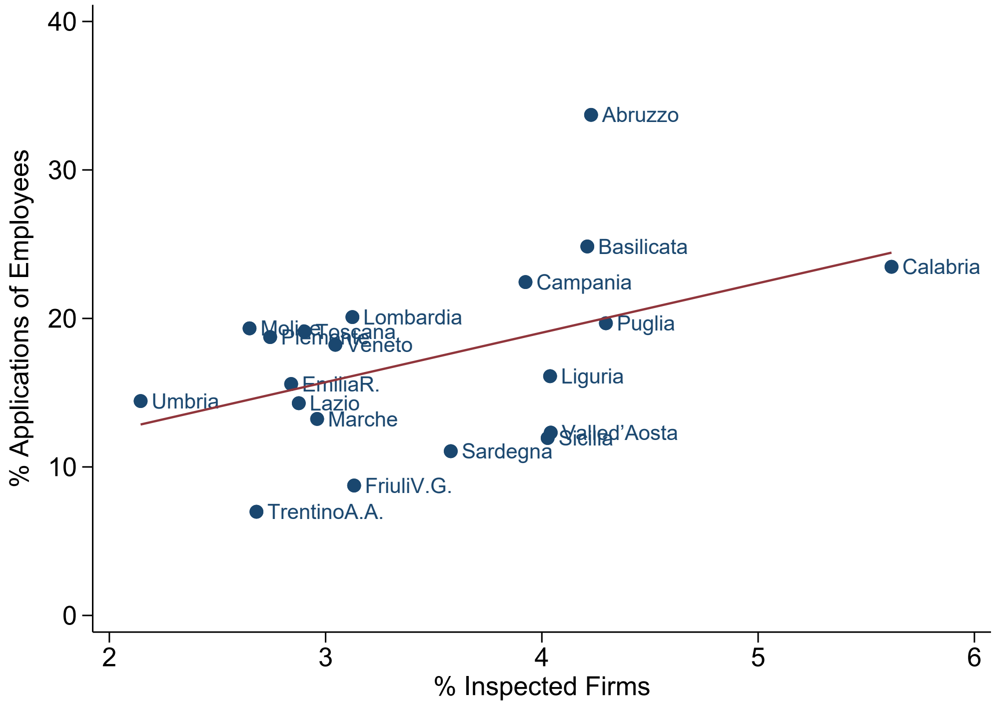
Notes: Figure shows ratio of amnestied workers to the total employment within each province.
Source: WHIP.

Figure 6: Foreign-Native Log Employment Gap and Log Weekly Wage Gap by Year and Month



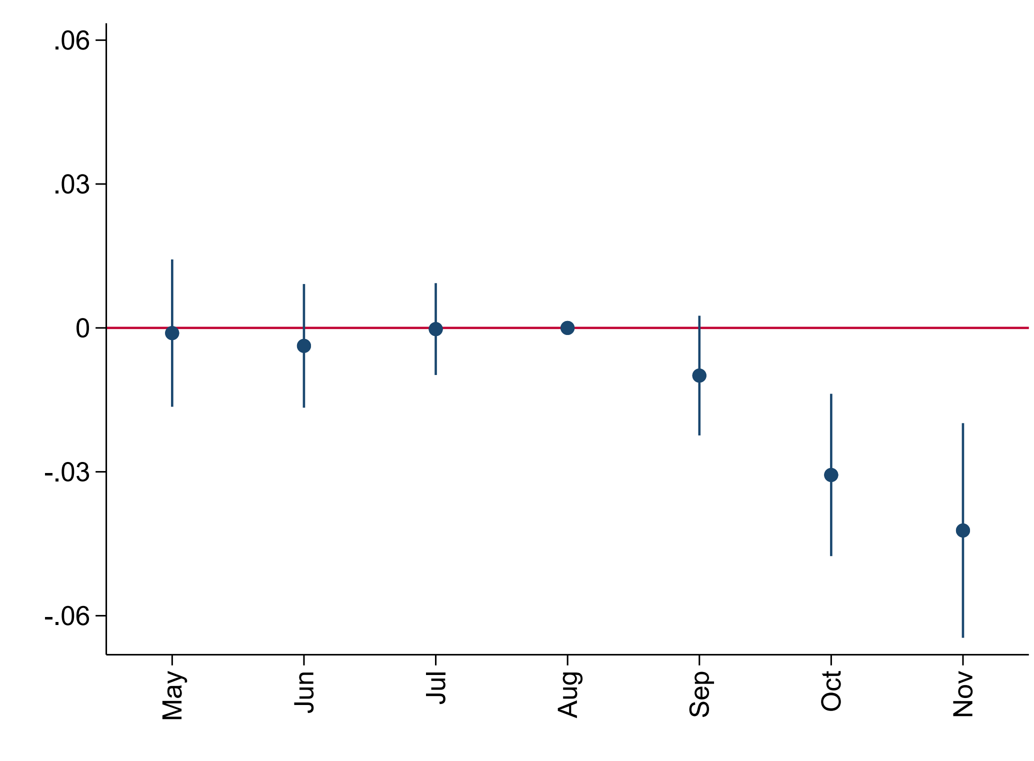
Notes: Figure shows the log employment gap and the log weekly wage gap between foreigners and natives from January 2000 to December 2002.
Source: WHIP.

Figure 7: Share of Inspected Firms and Applications of Employees by Region in 2002



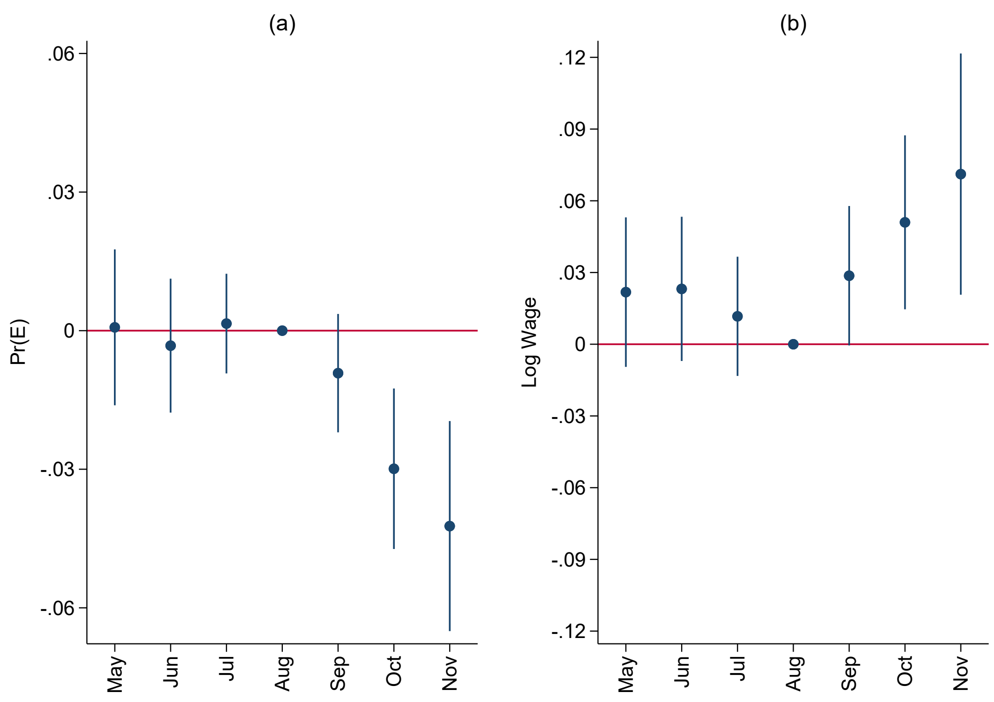
Notes: Figure shows the relationship between the share of applications of employees and the share of inspected regions by regions in 2002.
Source: INPS and ISTAT.

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. (2019)**	**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 Americas

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

	Firm Size					
Industry	**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 Statics 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	$28\leq$Age≤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≤49	>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	-0.002
	(0.005)	(0.001)
Jul	-0.007*	-0.001
	(0.004)	(0.001)
Aug	0.000	0.000
	(.)	(.)
Sep	-0.004	0.001
	(0.005)	(0.001)
Oct	-0.019**	0.002
	(0.008)	(0.001)
Nov	-0.023**	0.002
	(0.010)	(0.003)
Dec	-0.028**	0.004
	(0.011)	(0.004)
N	422,004	339,912

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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

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: Standard errors are clustered at cell level. * p<0.10, ** p<0.05, *** p<0.01

9 Appendix

9.1 The Measurement Issue

The share of declared native workers and the fraction of amnestied workers are unobservables 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:

$$\frac{E^{DN}_{c\tau}}{L^{N}_{cy}} = \frac{E^{DN}_{c\tau}}{L^{DN}_{cy} + L^{UN}_{cy}} = \frac{E^{DN}_{c\tau}}{L^{DN}_{cy}} \frac{L^{DN}_{cy}}{L^{DN}_{cy} + L^{UN}_{cy}} \tag{14}$$

$$\frac{AMN_{cy}}{L^{M}_{cy}} = \frac{AMN_{cy}}{L^{DM}_{cy}+L^{UM}_{cy}} = \frac{AMN_{cy}}{L^{DM}_{cy}}\frac{L^{DM}_{cy}}{L^{DM}_{cy}+L^{UM}_{cy}} \tag{15}$$

where E^{DN}_{cmy} 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^{j}_{cy} is the total labor supply of each group j. L^{Uj}_{cy} and L^{Dj}_{cy} 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 the data, I observe only $\frac{E^{DN}_{c\tau}}{L^{DN}_{cy}}$ and $\frac{AMN_{cy}}{L^{DM}_{cy}}$. Rewriting both equations as a function the true variables, $\frac{E^{DN}_{c\tau}}{L^{N}_{cy}}$ and $\frac{AMN_{cy}}{L^{M}_{cy}}$:

$$\frac{E^{DN}_{c\tau}}{L^{DN}_{cy}} = \frac{E^{DN}_{c\tau}}{L^{N}_{cy}}(1+\frac{L^{UN}_{cy}}{L^{DN}_{cy}}) \tag{16}$$

$$\frac{AMN_{cy}}{L^{DM}_{cy}} = \frac{AMN_{cy}}{L^{M}_{cy}}(1+\frac{L^{UM}_{cy}}{L^{DM}_{cy}}) \tag{17}$$

The estimates of the relationship between the true proxies is:

$$\widehat{\beta} = \frac{Cov(\frac{E^{DN}_{c\tau}}{L^{DN}_{cy}},\frac{AMN_{cy}}{L^{DM}_{cy}})}{Var(\frac{AMN_{cy}}{L^{DM}_{cy}})} \tag{18}$$

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

$$\widehat{\beta} = \frac{Cov(\frac{E^{DN}_{c\tau}}{L^{N}_{cy}}(1+\frac{L^{UN}_{cy}}{L^{DN}_{cy}}),\frac{AMN_{cy}}{L^{M}_{cy}}(1+\frac{L^{UM}_{cy}}{L^{DM}_{cy}}))}{Var(\frac{AMN_{cy}}{L^{M}_{cy}}(1+\frac{L^{UM}_{cy}}{L^{DM}_{cy}}))} \tag{19}$$

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

$$\widehat{\beta}=\frac{1}{\sigma_{\widehat{x}}^{2}}\left(Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}},\frac{AMN_{cy}}{L_{cy}^{M}})+Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}\alpha_{cy}^{N},\frac{AMN_{cy}}{L_{cy}^{M}})+Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}}\alpha_{cy}^{N},\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M})+Cov(\frac{E_{c\tau}^{DN}}{L_{cy}^{N}},\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M})\right) \tag{20}$$

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:

$$E(\widehat{\beta})=\frac{1}{\sigma_{\widehat{x}}^{2}}E\left(\beta Var(\frac{AMN_{cy}}{L_{cy}^{M}})+\beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N},\frac{AMN_{cy}}{L_{cy}^{M}})\right)+ \tag{21}$$

$$+\frac{1}{\sigma_{\widehat{x}}^{2}}E\left(\beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}},\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M})+\beta Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N},\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M})\right)$$

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:

$$E(\widehat{\beta})=\frac{\beta}{\sigma_{\widehat{x}}^{2}}\left(\sigma_x^2+\sigma_x^2-\sigma_x^2+Cov(\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{N},\frac{AMN_{cy}}{L_{cy}^{M}}\alpha_{cy}^{M})\right) \tag{22}$$

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 $\frac{AMN_{cy}}{L_{cy}^{M}}\leq\frac{AMN_{cy}}{L_{cy}^{DM}}$, I can rewrite eq. 22 as:

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

where $\gamma \geq 1$. Rewriting the $Cov(\frac{AMN_{cy}}{L^M_{cy}}\alpha^N_{cy}, \frac{AMN_{cy}}{L^M_{cy}}\alpha^M_{cy})$ as $\sigma_{\tilde{x},\bar{x}}$. Eq. 23 is:

$$E(\widehat{\beta}) = \beta \left(\frac{1}{\gamma} + \frac{\sigma_{\tilde{x},\bar{x}}}{\gamma \sigma^2_x}) \right) \tag{24}$$

Since $\sigma^2_x \geq \sigma_{\tilde{x},\bar{x}}$ and $\sigma_{\tilde{x},\bar{x}}$ is negative, the bias in the parenthesis is positive and lower than one. The estimate of the true parameter is a lower bound.

9.2 Employment Spells and Occupation

Two possible side composition effects of crowding out the less productive workers are an increase in the average employment spell and a decline in the probability of being occupied in a blue-collar occupation since the less productive workers are more likely to experience large unemployment spells and are more likely to fill a blue-collar vacancy. Table 14 shows the effect of the amnesty on the probability of being employed in a blue-collar occupation by age. The first four columns starting from the left side show the effect on the whole sample, the youngsters, the middle-age workers and the elders, respectively. While, the last four columns show the estimates when I add the individual controls. The overall effect is negative and different from zero after the amnesty but September and December. However, the effect fades away adding individual controls. Youngsters experience an increase in the probability of being blue collar even adding individual controls. The explanation is that employers hire more youngsters with blue collar contracts even when they should be hired as white collars. The middle-age workers experience a small decline but the effects does not hold adding controls. Occupation of older workers is not affected by the amnesty. As predicted, the negative effect on the distribution of blue collar workers is only a composition effect.

Table 15 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. Therefore, the selected workers are the most productive one who spend much time into employment.

Table 14: Effects of the Amnesty on Probability of Working as Blue Collar by Age

	Without Controls				With Controls			
	Overall	<29	28≤Age≤49	>48	Overall	<29	28≤Age≤49	>48
May	-0.002	0.016	-0.004	-0.007	-0.002	0.017	-0.004	-0.006
	(0.004)	(0.011)	(0.004)	(0.008)	(0.004)	(0.011)	(0.004)	(0.008)
Jun	-0.003	0.006	-0.004	-0.009	-0.003	0.007	-0.004	-0.008
	(0.003)	(0.010)	(0.003)	(0.007)	(0.003)	(0.010)	(0.003)	(0.007)
Jul	0.002	-0.001	0.001	0.000	0.002	0.000	0.000	-0.000
	(0.002)	(0.008)	(0.002)	(0.004)	(0.002)	(0.008)	(0.002)	(0.004)
Aug	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Sep	-0.001	0.013**	-0.004**	-0.001	-0.000	0.013**	-0.003	-0.001
	(0.002)	(0.006)	(0.002)	(0.003)	(0.002)	(0.006)	(0.002)	(0.003)
Oct	-0.007**	0.009	-0.009***	0.001	-0.004	0.013	-0.006**	0.002
	(0.003)	(0.008)	(0.003)	(0.007)	(0.003)	(0.008)	(0.003)	(0.007)
Nov	-0.008**	0.010	-0.008*	-0.002	-0.004	0.012	-0.005	0.000
	(0.004)	(0.011)	(0.004)	(0.008)	(0.004)	(0.011)	(0.004)	(0.008)
Dec	-0.004	0.022	-0.007	-0.002	-0.000	0.025*	-0.005	0.002
	(0.004)	(0.013)	(0.005)	(0.010)	(0.004)	(0.013)	(0.005)	(0.010)
Ind. contr.	No	No	No	No	Yes	Yes	Yes	Yes
N	553,875	146,792	507,676	150,228	553,875	146,792	507,676	150,228

Notes: The dependent variable is a dummy equal to one if the individual is employed as blue collar 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 15: Effects of the Amnesty on Months Worked within a Year by Age

	Without Controls				With Controls			
	Overall	<29	28≤Age≤49	>48	Overall	<29	28≤Age≤49	>48
May	0.075	0.177	0.034	0.095	0.072	0.189	0.033	0.097
	(0.057)	(0.183)	(0.062)	(0.149)	(0.054)	(0.162)	(0.061)	(0.147)
Jun	0.082	0.136	0.044	0.242*	0.080	0.134	0.042	0.240*
	(0.051)	(0.161)	(0.054)	(0.140)	(0.049)	(0.142)	(0.054)	(0.139)
Jul	0.056	0.084	0.054	0.159*	0.055	0.078	0.053	0.157*
	(0.040)	(0.114)	(0.037)	(0.093)	(0.038)	(0.103)	(0.037)	(0.092)
Aug	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Sep	0.081	0.016	0.044	0.217**	0.079	0.031	0.042	0.216**
	(0.055)	(0.136)	(0.049)	(0.108)	(0.053)	(0.128)	(0.049)	(0.108)
Oct	0.241***	0.447**	0.055	0.334**	0.226***	0.421**	0.052	0.330**
	(0.068)	(0.180)	(0.069)	(0.134)	(0.066)	(0.169)	(0.068)	(0.133)
Nov	0.320***	0.596**	0.084	0.412***	0.302***	0.573**	0.081	0.409***
	(0.090)	(0.238)	(0.087)	(0.154)	(0.087)	(0.229)	(0.087)	(0.153)
Dec	0.469***	0.782***	0.191**	0.610***	0.450***	0.744***	0.188**	0.606***
	(0.101)	(0.278)	(0.094)	(0.180)	(0.096)	(0.264)	(0.093)	(0.180)
Ind. contr.	No	No	No	No	Yes	Yes	Yes	Yes
N	553,875	146,792	507,676	150,228	553,875	146,792	507,676	150,228

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.

9.3 Results Using 2000 As Control Year

Table 16: 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Economic Assimilation of Immigrants:

Adverse Effects of an Integration Program

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Abstract

Integration programs play a key role in increasing the economic assimilation of immigrants. However, these programs might affect also labor market outcomes of people who are not directly involved in the program. The aim of this paper is to study whether integration programs have adverse effects on labor market outcomes within skill groups. I use the post-treatment discontinuity in the labor force participation of individuals around the threshold to identify the spillover effects on labor market outcomes. The causal identification is possible for two reasons: people have the same characteristics, and, the treatment assignment is as good as random at the threshold. In order to do this, I take advantage of a natural experiment in France where immigrants are more likely to attend a language training course if they score below a given threshold in a French language test. Results show that an increase in labor force participation of eligible migrants has a negative spillover effects on the probability of participating in the labor force and also of being employed.

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1 Introduction

A faster economic assimilation of immigrants is one of the main challenges faced by the governments of developed countries. Over the last decade, immigrants have been experiencing a downturn in the labor market outcomes at arrival. Dustmann and Frattini (2012) show that economic integration of immigrants, measured as the native-foreigner wage gap, is far from being reached in Europe. In the U.S., Albert et al. (2020) and Borjas (2015) show that economic integration of new arrivals has lowered throughout the years. To tackle this decline, most developed countries have enacted integration programs to foster a faster economic assimilation. However, there is still a debate on what kind of integration policy might be enacted to have both short-run and long-run positive effects on the labor market outcomes of foreigners. Furthermore, the literature has not highlighted on possible adverse effects of the programs when they do not achieve the expected goals.

Integration programs are divided into two types: “job first” and “skill first”. The “job first” programs aim at increasing the employment opportunities for immigrants. The “skill first” program aims to improve the quality of the skills to be supplied in the labor market. Card et al. (2017) and Holtz et al. (2006) summarize the effects of some active labor market policy in Europe and in the U.S. between 1980 and 2010. They find an increase in the labor market outcomes 2-3 years after the program. Further, they find a stronger positive effect of the “skill first” programs in the long-run. However, both papers study active labor programs for natives, while it is possible that immigrants might not experience the same effects.

Recent literature shows that integration policies have a positive effect on the labor market outcomes of immigrants. Sarvimäki and Hämäläinen (2016) show the positive effect of a new integration program on earnings and employment of unemployed immigrants in Finland between 2000 and 2009. The program was a mix between “job first” and “skill first” programs since immigrants were assigned to a tailored set of active labor market programs. Arendt et al. (2020) and Arendt (2019) show a positive effect of an integration program on the labor outcome of refugees in Denmark between 2000 and 2010. Arendt et al. (2020) show that refugees experience a lowering in the welfare benefits and an improvement in the language training. The program leads to a long-lasting effect on both employment and earnings. Arendt (2019) studies the effect of a “job first” policy enacted in Denmark in 2016. The policy was aimed at improving the labor force participation of refugees early after their arrival. Arendt shows a positive effect of the policy in the very short run. Battisti et al. (2019) show that refugees experience an improvement in labor market integration when they receive job search assistance at arrival.

All the above-mentioned papers study integration programs in Scandinavian countries and Germany where the demand for low-skilled workers is high. An excess demand of low-skilled workers might lead to high returns of integration programs for immigrants. To the best of my knowledge, Lochmann et al. (2019) is the only paper evaluating the impact of an integration program for immigrants in France where the demand for low-skilled workers is not as high as in the Scandinavian countries. They study the effect of a language training on the labor market outcomes of immigrants. Lochman et al. (2019) show that language training has affected the labor force participation only three years after the beginning of the program. Increasing the labor force participation of immigrants might have a positive effect on their economic assimilation. However, the new foreign labor force might probably compete with the old one leading to adverse effects on labor market outcomes. For instance, higher labor market competition might lower employment probability or wages. As a result, an increase in labor supply might offset the gains of the integration program lowering the economic assimilation of immigrants.

I show that adverse effects are possible when an integration program increases only the competition in the labor market. In the literature on labor economics competition among migrants has received scant attention. Borjas (2003) started a body of research on the effect of immigration on the labor market outcomes of natives using a skill-cell approach. And, he showed that natives have experienced a negative effect of immigration on wages in U.S. between 1960 and 2001. Later, D’Amuri et al. (2011), Manacorda et al. (2012), Ottaviano and Peri (2012) find that immigrants are imperfect substitute with natives,and that gains from inflows of immigrants are larger than the losses of the labor market outcomes arisen from the increase in labor competition. Finally, they show that immigrants are more likely to compete with each other than with natives within the same education-specific labor market.

I set up a spillover effects model to study whether an increase in labor supply of eligible migrants affects the individual labor market outcomes. The two main problems of spillover effects estimation are: the *reflection problem* and the bias of correlated unobservables. *Reflection problem* arises when the mean of the dependent variable is equal to the independent variable. And, the correlated effects bias is due to a correlation between omitted unobservable group variables and the independent variable. I solve these two problems by exploiting the quasi-random variation of the language training program around the cut-off . The share of migrants just below the threshold captures this variation within skill groups. I use this measure as an instrument to identify the main equation.

To show how the model works, I study the effect of a foreign labor supply increase on the labor market outcomes of immigrants within skill groups. I exploit a 2006 reform on the integration policy of immigrants in France. Since 2006, immigrants have to sign the *Contract d'accueil et d'intgration* (CAI) when they get a residence visa. Signatories of *CAI* might attend a language course to achieve a basic knowledge of the French language if they show low host language skills. The Government provides an oral and written French exam to test the language skills of immigrants. Immigrants are more likely to attend the language course if they have a result below 50/100. Assuming that immigrants share the same characteristics around this threshold, labor force participation should differ at the cut-off if the language training is effective. Using a 2010-2013 longitudinal survey of the signatories, Lochmann, et al. (2019) find that people just below the threshold show a higher labor force participation than those just above. I take advantage of this finding to study the effects of the language training on labor market outcomes within skill groups.

Results show an adverse effect of the labor force participation on the individual labor market outcomes. A 1% increase in the labor force participation of eligible migrants lowers the overall probability to participate in the labor market by around .3% and the employment probability by around .5%. These results are in line with previous findings (D'Amuri et al, 2010; Dustmann et al., 2017) on the substitutability among immigrants within a skill-specific labor market.

As a first contribution, this paper complements the analysis on the integration policy for immigrants in the host country. So far, economic literature has focused on the partial equilibrium effects of the policy by paying less attention to spillover effects (Angrist, 2014; Manski, 1993; Moffitt, 2001). I show that positive partial equilibrium effects might lead to adverse effects in a general equilibrium framework. As a second contribution, I expand the analysis on labor substitutability in the labor market between immigrants. Finally, I use an instrumental variable setting to study spillover effects by lowering the bias from both reverse causality and correlated group effects.

This paper shows a statistical tool to evaluate whether integration programs lead to adverse effects on labor market competitors. The narrowed number of observations in the ELIPA data does not allow me to infer a strong causal evidence. Yet, the model might be helpful to assess the impact of both "job first" and "skill first" programs on all the workers when a large data is available. A "language skill" integration program fosters the integration of immigrants in both social and economic terms (Chiswick (1991), Chiswick and Miller (1995), Dustmann and van Soest (2001), Dustmann and Fabbri (2003) among others). However, a "language skill first" approach might have an adverse effect on the labor market outcomes of other immigrants when a new labor supply increases the competition in the labor market. In particular, the new foreign labor force might supply the same skills of the old labor force. An adverse effect on the labor market outcome of immigrants could slow down their economic assimilation at least in the short run.

2 Identification Strategy

2.1 Natural Experiment within Skill Groups

The aim of this paper is to study whether an integration program has indirect effects on labor market outcomes. If labor market outcomes of individuals depend on the labor market outcomes of people within the same skill group, the integration program might affect labor market outcomes of both program takers and non-takers. Exploiting quasi-random variation in the labor market outcomes of treated individuals within a group, I investigate whether labor market competitors of treated individuals also experience a change in their labor market outcomes.

To clarify the system of simultaneous equations that I am going to solve, I built a model similar to Dahl et al. (2014). Suppose that there are only two individuals within each skill group. The outcome of each individual depends on individual characteristics, fixed and time-variant group characteristics, and, labor market outcomes of people within the same skill group. Moreover, suppose that only the individual 1 has the opportunity to attend the integration program. In this setting, the system of simultaneous equations within a skill group g is:

$$y_{1g} = \alpha_1 + \beta_1 y_{2g} + \gamma_1 x_{1g} + \tau_1 x_{2g} + \theta_1 w_g + \lambda p_{1g} + e_{1g} \tag{1}$$

$$y_{2g} = \alpha_2 + \beta y_{1g} + \gamma_2 x_{2g} + \tau_2 x_{1g} + \theta_2 w_g + e_{2g} \tag{2}$$

where y_{ig} is the outcome of individual i in a group g, x_{ig} are observable characteristics of individual i in group g, w_g is a set of group characteristics, and e_{ig} is an error term. Further, p_{1g} represents the group-specific "price" of individual 1 to attend the language training. This model shows that the integration program might have an indirect effect on individuals 2's outcome.

The effect of the price on individual 1's outcome, λ, is identified in equation (1) since p_{1g} is random. Identification holds as long as the effect of the integration program on individual 2's outcome occurs after the effect on individual 1. Since the price variation is uncorrelated with all other individual and group characteristics, the sequential effect is a reasonable assumption.

The exogenous variation of p_{1g} solves the reflection problem of simultaneity and the omitted variable bias in the equation (1). Indeed, the exclusion of a variable from equation (2) breaks the simultaneity (reflection problem) of the two equations. Moreover, the bias arising from unobservable group variables does not affect the estimates since p_{1g} is exogenous to both observable and unobservable characteristics.

3 Background and Data

3.1 Background

3.1.1 *Contract d'Accueil et d'Intégration* (CAI)

From 2007 to 2016, new legal immigrants to France who were older than 16 and were coming from an non-EU country had to sign a contract[1]. This contract imposes: a civic training, a language training if needed, an information session about France, a social support if needed, and an evaluation of personal job skills. The language training was mandatory for immigrants who showed low proficiency in the French language.

In order to test the host language proficiency, a pool of instructors, holding the "FLE" (French as Foreign Language) certification to teach, carries out the French test during an interview at the OFII (*Office Français de l'Immigration et de l'Intégration* or French Immigration and Integration Office). The pass score is set to a sufficient French level, which is equal to an A1.1 level. People who do not pass the French entrance test are assigned a number of hours up to 400. The aim of the language course was

[1] Also immigrants with a long residence permit who arrived in France between 16 and 18 years old are eligible to sign the contract. However, they are only the 6% of immigrants who signed the CAI

to bring all immigrants to the same level in the French language. At the end of the course, they got a diploma which is key to extend the residence permit in the following years.

The assignment to the language training and the number of hours depended on both the entry language test and on socio-demographic characteristics. Immigrants who are from certain specific countries of origin (e.g. Sri Lanka) or hold a non-labor residence permit are more likely to attend the language training. However, Table 1 shows that only 4% of immigrants who passed the test are assigned a language training, against 85% among those who failed. Therefore, the test result is a key variable in predicting the language course assignment.

3.1.2 The French Language Test

Immigrants were more likely to attend the French language training if they failed to pass the French entrance test. The test is divided into an oral and a written examination. In the first part, immigrants have a talk to an instructor. The possible grades of the oral examination are: 0, 35, 70. In the second part, instructors set up four written tests in ascending order to evaluate the reading and writing skills in the French language. The written tests use two values only: a positive score, if the answer is right, and 0, otherwise. The first and the last written tests have a positive score of 5, while the remaining two have a positive score of 10. Hence, the maximum score is 100 since the highest score in the oral examination is 70 and the highest score in the written examination is 30. The first pass grade is 50 being the sum of the oral and the written exams.

Table 2 shows the distribution of the grades in the written exam by the oral grades and the number of right answers in a row. Panel A shows that the majority of immigrants is proficient in the French language. Furthermore, 71% of the top scorers in the oral exam get the maximum score in the written test. Unlike them however, 89% of the bottom scorers in the oral exam have a result of 0 points in the written tests. Table 2 shows also that the last two questions were the hardest, which explains the low mass of immigrants who get a result of 10 or 20 at the written examination. The increase in the level of difficulty is the main explanation for the low mass at the first fail grade, 45. Panel B shows immigrants are more likely to pass the successive written test when they have already passed all the previous written tests.

3.2 Data and Sample

3.2.1 Enquête Longitudinale sur l'Intégration des Primo-Arrivants (ELIPA)

I use *Enquête Longitudinale sur l'Intégration des Primo-Arrivants* (ELIPA) dataset, which is a longitudinal survey carried out by the *Département des statistiques, des études et de la documentation* (DSED) of the French Ministry of the Interior. The longitudinal survey took place in 2010, 2011 and 2013. The aim of the survey was to collect information on socio-demographic characteristics, bureaucratic itinerary, employment, language skills, living conditions and social integration of immigrants. In 2010, 6,107 immigrants were surveyed from a population of 97,736 immigrants who signed the CAI in 2009. The immigrants surveyed were at least 18 years old and wanted to settle permanently in France. Immigrants who responded to the second and third wave were 4,756, and 3,573, respectively.

The longitudinal survey is representative of 97,736 immigrants arrived in Metropolitan France in 2009. DSED used a stratification sampling to show a representative sample for different groups of interest. Since the goal of the survey is to study the integration process, the stratification is based on the following three variables: country of origin, residence in France, and years since migration. The sample is representative of each class among these variables[2]. In particular, the survey focuses on immigrants who have entered in France with a family reunification permit or with an asylum seeker permit.

[2]The survey ensures a minimum representativeness of all strata

3.2.2 Skill groups

I use the following three characteristics to create skill groups: country of origin, region of residence, and years since migration. The cross product of these three characteristics creates different cells which define the skill groups. Individuals are considered job competitors if they belong to the same cell. The total number of skill groups is 28. The number is very small since many skill groups do not have people within the bandwidth around the cut-off. The group average size is 16 (SD=8).

The decision of the reference group stems from the higher probability that individuals are more likely to be substitute within the same region if they supply the same labor skills and migrate for the same reason. I use the time spent in France as a proxy of the experience, and the region of residence as proxy for the local labor market. Furthermore, immigrants could experience different labor market outcomes even if they lived in the same place and supplied the same labor skills. Indeed, Adser and Chiswick (2007) show that, in 15 European countries, labor market outcomes of immigrants vary by country of origin. Therefore, I also add the characteristics of the country of origin to define skill groups [3].

3.2.3 Sample

The final sample includes 1,257 observations out of 3,573 sampled in the third wave. I exclude groups with one person since studying spillover effects is impossible when there are no people in the group. Furthermore, I consider only groups showing heterogeneity in the results of the test score among eligible migrants.[4] Finally, I consider only observations without missing values in the variables of interest (outcome, endogenous variable, instrument and controls).

[3] Literature on skill-cell approach uses also education to set the skill groups. Yet, including education variable might lead to bias estimates of the average variable since education is not a stratification variable. Moreover, education is not so heterogeneous within skill groups. Therefore, adding education in the skill-group setting process might be much more detrimental than beneficial for estimation.

[4] An invariant test score does not provide any information to understand the relationship between labor force participation and share of less proficient migrants within a skill group.

As already discussed in Section 2.1, I use the observations falling in an interval around the cut-off, 50, to compute the peer variables. I follow the procedure implemented by Calonico et al. (2014) to choose the bandwidth around the cut-off. The bandwidth includes all the individuals who have a result of 35 at the oral examination in the sample, and, I use the observations of those individuals to compute the group variables.

Table 3 shows the descriptive statistics for immigrants around the threshold and for the whole sample. Labor force participation is very similar for both groups in 2013. However, the entire sample shows a higher labor force participation in 2010. The most important entry channel is the family reunification, around 80% of immigrants in both samples. Instead, refugees represent the second largest group. Women form the majority of immigrants in both groups, 64% and 58% respectively. And, the majority of immigrants in the whole sample had moved earlier to France. Indeed, they perform better at the French language in 2010. Finally, the distribution of immigrants by country of origin is heterogeneous even if Maghreb immigrants are the most representative in both groups.

4 Identification

4.1 Empirical Strategy

I use an IV approach to estimate the spillover effects of the labor force participation. To create the instrument, I exploit the discontinuity stemming from the decision rule to assign immigrants to the language training: individuals who have a result below 50 are eligible to be assigned to the language training (Lee et al. 2010). Since the eligible rule leads to a quasi-random variation at the threshold, I can exploit the group-specific share of eligible migrants as an instrument.

In a many-to-one model, the following two equations show the first stage and the second stage, respectively:

$$E_{-i}[LF_{-ig}|g] = E_{-i}[\alpha_{-i}|g] + \lambda E_{-i}[D_{-ig}|g] + E_{-i}[f(TS_{-ig})|g] + E_{-i}[\varepsilon_{-ig}|g] \tag{3}$$

$$LF_{ig} = \alpha_i + \beta E_{-i}[LF_{-ig}|g] + \delta D_{ig} + f(TS_{ig}) + E_{-i}[f(TS_{-ig})|g] + \varepsilon_{ig} \tag{4}$$

where LF_{ig} is a dummy equal to one if the individual i is employed or looking for a job within the group g, $E_{-i}[LF_{-ig}|g]$ is the labor force participation of individual i' job competitors within the group g, D_{ig} is a dummy equal to one if the test score of individual i was below the threshold, $E_{-i}[D_{-ig}|g]$ is individual i' share of eligible migrants, $f(TS_{ig})$ is a function of the test score, $E_{-i}[f(TS_{-ig})|g]$ is the average test score of individual i' job competitors, and, $E_{-i}[\varepsilon_{-ig}|g]$ and ε_{ig} are the error terms of the job competitors and of the individual, respectively. β measures the indirect effects of the language training program. The group-specific share of eligible migrants is a good instrument as long as the individuals within each group cannot manipulate their test score near the threshold, otherwise the shares are not more randomly distributed.

The identifications strategy follows Dahl et al. (2014) where they use the discontinuity on the timing of a new paternity leave policy to study whether peers who get the benefit affect the probability of other individuals of the group to take up leave. Unlike the latter who narrow the analysis to groups with only one peer in the reform window, I extend the analysis to skill groups which have at least one person having a test score close to the cut-off. The choice of "many-to-one" spillover effects raises some doubts on the true functional form of the first stage. I decide to use the mean function to compute the group variables since it is not affected by the number of people and allows me to exploit the heterogeneity of the shares across groups.

The choice of a functional form for the assignment variable, the test score, is the most challenging part of the identification strategy since the functional form might vary by groups and by sides of the cut-off. To overcome this issue, I use only observations within a smaller symmetrical interval around the cut-off where a linear functional form is more likely to fit the data (Gelman and Imbens, 2019). As a result, I compute all the group variables by using observations falling in this interval. However, I have to assume that the spillover effects are the same whatever subset in a group of people is chosen. In my case, this assumption is reasonable since skill groups define different labor markets in which individuals are more likely to be stronger substitutes. Finally, I add a set of controls to control for the labor supply of people further from threshold.

4.2 Threats To Identification

One possible threat to identification is the self-selection in the language training. Immigrant might cheat at the written tests and get a low grade on purpose to attend the language training. Therefore, cheating might affect the quasi-random variation of the eligibility criterion. However, immigrants do not have any incentive to fail the test on purpose since the consequence is to spend up to 400 hours in a language training. In particular, they must spend at least 20 hours per week to complete the assigned hours. This constraint might affect the probability to work since the language training might bind the number of working hours to supply. Furthermore, immigrants get a certificate of French language if they pass the initial test. The certificate released by the OFII is key to get the *visa long séjour valant titre de séjour* (VLS-TS) which allows immigrants to stay in France for a longer period and travel within the Schengen Area. If they fail the exam during the OFII meeting, immigrants should attend the training and pass the *Diplôme Initial de Langue Française* (DILF) to get the VLS-TS. Hence, they should opt to get the French certification as soon as they can.

I test the manipulation around the threshold by plotting the distribution of immigrants around the cut-off and testing the difference on pre-determined characteristics on both sides of the threshold. Figure 1 shows the distribution of the results in the 2010 French test around the cut-off. The distribution does not show any jump at the cut-off validating the assumption of no-manipulation at the threshold. Furthermore, Table 4 shows the balancing tests on the characteristics of immigrants in 2010. Estimates confirm that characteristics do not show any statistical difference for immigrants just below and just above the threshold [5].

Another threat to the identification strategy is the difference in the labor market competition at the threshold. As long as the quasi-random variation of the language test identifies the spillover effects, immigrants must experience the same level of competition just below and just above the threshold. Indeed, the stable unit treatment value assumption (SUTVA) is violated if the effect of labor market competition is heterogeneous at the threshold. Using the leave-one-out labor force participation, I perform a balancing test to check whether migrants experience different competition level at the threshold. Table 6 shows that this is not the case.

5 Results

5.1 Spillover effects estimates

Table 7 shows the 2SLS estimates of the spillover effects on labor force participation for different specifications. The first two columns show the unweighted and weighted estimates of an unconditional regression and the last two columns show the unweighted and weighted estimates of a conditional regression. Panel A shows the first stage of the model. The instrument is positive and significant in each specification. Also, the magnitude of the first-stage coefficients do not change across specifications. Panel B shows that the second-stage estimates are negative but they are significant only in the conditional regressions. However, the magnitude is pretty similar in all specifications. In the second-stage, I find that a one percent increase in the share of labor force participation of eligible migrants lowers the probability of

[5] As a further check, I replicate the analysis of Lochmann et al. (2019) in Table 5 since I use a different sample. The analysis is consistent with the findings of the previous paper.

participating in the labor market by around 0.3%.

Panel C shows the estimates of spillover effects on the employment. The estimates are still negative but significant only in the conditional regressions. Nor do they change across specifications. The impact is larger than the labor force participation one. There are several explanations on why the impact is wider but the most credible one is that the effect of the labor force participation of eligible migrants on the employment is a reduced-form effect. Hence, the true labor force participation parameter is multiplied by some positive 'social multiplier'. Estimates show that one percentage point increase in the labor force participation decreases the employment rate by around 5%. This finding is pretty much similar to D'Amuri et al. (2010) who find of an increase in foreign labor supply on native employment rate.

A brief discussion on the standard errors and, as consequence, on the F-stat is due since they vary a lot across specifications. The standard errors in the unconditional regressions are quite large since the variation of the instrument does not explain a large part of the variation of both the endogenous regressor and the labor market outcomes. As a result, estimates experience higher standard errors and the F-stat goes down. To overcome this issue, I add a set of controls to reduce the variance of the residuals. Indeed, Panel B and Panel C show more efficient estimates in the last columns without affecting the estimates of the spillover effects.

Another important result comes up comparing the weighted and unweighted estimates in both Panel B and Panel C. Unweighted estimates are always larger in absolute value than the weighted one. This is due mainly to the negative selection of the immigrants into the sample. Some reference groups which were more likely to be sampled in the survey were more likely to experience worse labor market outcomes. As a result, the unweighted estimates show a larger negative effect of the labor force participation of eligible migrants on both labor force participation and employment. Hence, the weighted estimates show more reliable estimates and more efficient standard errors.

5.2 Spillover effects on Non-Eligible Immigrants

In this subsection, I show the effect of an increase in the labor force participation on the labor outcomes of ineligibles for the language training. In the previous subsection, I show the effect of an increase in the labor force participation on the whole sample. I exploit the information on the test score and number of assigned hours to divide immigrants in two groups: eligible and ineligible. Immigrants are in the group of the eligibles if they do not pass the French test or/and are assigned to the language training. Therefore, ineligible immigrants are those who do pass the French test and are not assigned to the language training.

Table 8 shows the estimates of spillover effects on labor force participation and employment rate of not eligible. Estimates are still negative and significant even if little bit smaller than the ones for the overall sample. This evidence shows that even immigrants holding higher host-language skills might experience adverse effects triggered by language training program.

6 Conclusion

Economic integration of immigrants is in the agenda of developed countries given the surge of economic migrants over the last 10 years. Characteristics of migrants are quite heterogeneous across destination areas. In particular, the share of low skilled migrants is more likely in Central and Southern Europa. An excess of supply of low skilled foreign workers might affect the both economic integration and economic assimilation of workers when the demand of such workers is fixed, at least in the short run. Therefore, integration programs become fundamental to lower the job competition within skill groups. However, integration programs might push people in the labor market even if they do no provide any job-specific course. In this case, the effect might be negative since migrants could compete in already saturated labor markets.

So far, the discussion about spillover effects of integration programs is very poor. The main reason is that spillover effects invalidate the first order effects since many evaluation policy analyses assume SUTVA. This paper provides a statistical tool to evaluate integration programs without affecting the validity of the SUTVA. The characteristics of the model allow to isolate the exogenous variation needed to study the spillover effects without invalidating the direct effects of an integration program.

In order to provide an empirical justification of such mechanism, this paper investigates the spillover effects of an integration program based on a language training course. Using the random nature of the language training assignment and higher probability to participate in the labor market after the course, I show the spillover effects within the skill groups. I find that immigrants experience adverse effects on their labor market outcomes when the labor market competition increases. In particular, a 1% increase in the labor force participation of eligible migrants lowers the probability of participating in the labor market and of being employed by .3% and .5%, respectively. The magnitude of the effect is stable regardless I add controls.

These results have strong policy implications on the evaluation of integration programs . Even if the bias stemming from small sample size might affect somehow the estimates, the results show that the "skill first" approach might have positive effects on the labor market outcomes of the program takers but they might also have adverse effects on the labor market outcomes of non-takers. The explanation of such mechanism for some "skill first" integration programs could stem from the fact that course attendees learn faster than non-attendees. In the language training example, the training course might have a positive effect on the learning curve of program attendees by leading both groups to have the same level of language proficiency. However, language training courses do not provide any job specialization leading migrants to probably compete within already saturated labor markets.

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Table 1: Share of immigrants assigned to a language training by test result

	Test Result	
	Passed %	**Failed** %
Language Training		
Not Assigned	96	15
Assigned	4	85

Table 2: Test Score Distribution

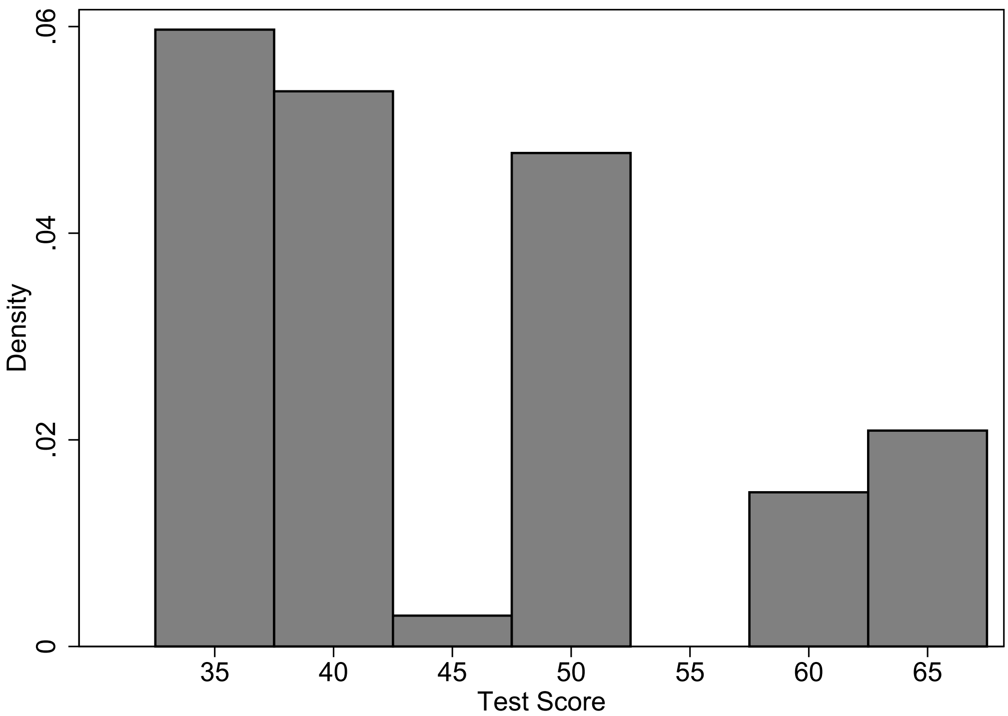
	Oral Grade			
Panel A: Written Grade	**0 points**	**35 points**	**70 points**	**Total**
0	91	30	3	10
5	6	27	7	8
10	0	1	1	1
15	3	24	8	8
20	0	0	2	2
25	0	8	9	8
30	0	10	70	63
Panel B: Right Answers				
0	94	36	6	13
1	5	24	8	8
2	1	24	7	8
3	0	6	9	8
4	0	10	70	63

Table 3: Summary statics

	Oral Test Score			
	35		Any	
	mean	sd	mean	sd
Employment Level 2013	0.61	0.49	0.59	0.49
Employment Level 2010	0.30	0.46	0.43	0.50
Labor Force Participation in 2013	0.80	0.40	0.78	0.41
Labor Force Participation in 2010	0.58	0.50	0.68	0.47
Total Test Score	42.04	8.05	75.98	35.85
Age	32.19	9.62	32.50	8.42
Age Squared	1127.57	748.00	1127.27	621.53
Education Level in 2010	9.21	5.93	10.06	5.54
Household Members	0.08	0.35	0.53	1.34
Married	0.92	0.27	0.82	0.39
Number of Children	0.54	0.96	0.79	1.03
Male	0.36	0.48	0.42	0.49
Resident in Ile-de-France	0.26	0.44	0.54	0.50
Years since Migration	1.99	3.26	3.14	4.22
Labor Migrants	0.03	0.17	0.06	0.24
Refugees	0.13	0.34	0.10	0.30
Other Channel	0.03	0.18	0.04	0.19
Family Reunification	0.81	0.40	0.80	0.40
Birth reg.: America and Oceania	0.00	0.00	0.03	0.18
Birth reg.: Asia	0.23	0.42	0.24	0.43
Birth reg.: Europe	0.22	0.42	0.12	0.32
Birth reg.: Maghreb	0.47	0.50	0.31	0.46
Birth reg.: Other Africa	0.05	0.22	0.12	0.33
Birth reg.: Sub-Saharan Africa	0.03	0.18	0.17	0.38
N	67		1,257	

Notes: All the statics are weighted by the sample weights.

Figure 1: Distribution of the Test Score



Notes: Distribution of the French language test over the sample around the cut-off (50). The x-axis shows the grades of the test score. The y-axis shows the density of each test score class.

Table 4: Balancing tests of 2010 characteristics

	Schooling	Age	Years since Migration	Resident in Ile-de-France	Married	Male
Eligible immigrants	-5.248	-15.21*	-5.775	-0.591	0.113	0.303
	(4.130)	(8.443)	(3.793)	(0.461)	(0.146)	(0.250)
	Number of Children	N. of HH inhabitants	Employment Level 2010	Family Reunification	Labor Migrants	Refugees
Eligible immigrants	-0.570	-0.467	-0.0878	0.267	-0.119*	0.0659
	(0.674)	(0.714)	(0.390)	(0.342)	(0.0654)	(0.276)
Observations	67	67	67	67	67	67

Notes: *Eligible immigrants* is a dummy equal to one if the French test score was below 50. The regression includes the normalized test score and its interaction with *Eligible immigrants* variable. The reported standard errors are clustered by country of origin times entry French test score. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: Labor force participation and language training

Panel A: First Stage		
Eligible Immigrants	2.992^{***}	2.912^{***}
	(0.499)	(0.358)
Panel B: Second Stage		
Hours of language training	0.192^{***}	0.185^{***}
	(0.0726)	(0.0441)
N	67	67
Controls	X	√
M.O. F-stats	64.74	67.11

Notes: The outcome variable is the labor force participation of eligible migrants within a group *g* in 2010. The independent variable is the assigned hours of language training. The instrument is a dummy equal to one if the entry score was below 50. Columns (2) includes the following controls: education level, age, age squared, gender, flat mates, years since migration, country of origin, channel of entrance, region of residence at arrival, standardized French entry test score, entry French test score dummy interacted with the standardized French entry test score. The reported standard errors are clustered by country of origin times entry French test score. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Competition Effects at Threshold

	(1) [35,65]	(2) [35,65]
Eligible Immigrants	-0.260 (0.241)	-0.0610 (0.0718)
Observations	67	67
Controls	X	√

Notes: The outcome variable is the labor force participation of eligible migrants within a group *g* in 2010. The independent variable is a dummy equal to one if the entry score was below 50. Columns (2) includes the following controls: education level, age, age squared, gender, flat mates, years since migration, country of origin, channel of entrance, region of residence at arrival, standardized French entry test score, entry French test score dummy interacted with the standardized French entry test score. The reported standard errors are clustered by country of origin times entry French test score. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Spillover Effects on Labor Market Outcomes

	2SLS (1)	2SLS-W (2)	2SLS (3)	2SLS-W (4)
Panel A: First Stage (endogenous regressor: LFP of eligible migrants)				
Share of eligible immigrants	0.947**	1.058***	0.784***	0.843***
	(0.414)	(0.406)	(0.137)	(0.134)
	[0.13,1.76]	[0.26,1.85]	[0.52,1.05]	[0.58,1.11]
Panel B: Second Stage (dependent variable: Labor force Participation)				
LFP of eligible migrants	-0.381	-0.305	-0.371***	-0.283**
	(0.313)	(0.238)	(0.133)	(0.131)
Panel C: Second Stage (dependent variable: Employment probability)				
LFP of eligible migrants	-0.499	-0.341	-0.597***	-0.532***
	(0.421)	(0.316)	(0.183)	(0.163)
N	1,257	1,257	1,257	1,257
M.O. F-stats	5.406	6.547	33.85	34.21
Controls	X	X	√	√

Notes: Columns (1)-(2) and (3)-(4) show without and with controls, respectively. The odd (even) columns show unweighted (weighted) estimates. The independent variable is the labor force participation of eligible migrants within the 35-65 window. The instrument is the share of immigrants who have a result lower than 50 in the entry French test within each skill group. All specifications include the following controls: a dummy equal to one if the test score was below 50 and the normalized test score both at individual and at group level. Specifications (3)-(4) include: education level, age, age squared, male dummy, number of inhabitants in HH, number of children, married dummy, employment in 2010, group-average employment in 2010, share of people with a labor visa in each group,group average age. Further specification (3) and (4) include also a set of further-from-threshold migrants' characteristics: share of eligible migrants, normalized test score, interaction of share of eligible immigrants with normalized test score, age, age squared, share of males, share of labor migrants. Standard errors are clustered at skill-group level. Regressions are weighted by sample weights. Confidence intervals of the first stage estimates in the square brackets. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Spillover Effects on Labor Market Outcomes of Non-Eligible Immigrants

	2SLS	2SLS-W	2SLS	2SLS-W
	(1)	(2)	(3)	(4)
Panel A: First Stage (endogenous regressor: LFP of eligible migrants)				
Share of eligible immigrants	0.995**	1.054***	0.777***	0.787***
	(0.395)	(0.377)	(0.128)	(0.109)
	[0.22,1.77]	[0.31,1.79]	[0.53,1.03]	[0.57,1.00]
Panel B: Second Stage (dependent variable: Labor force Participation)				
LFP of eligible immigrants	-0.355	-0.255	-0.369***	-0.254***
	(0.272)	(0.225)	(0.113)	(0.0982)
Panel C: Second Stage (dependent variable: Employment)				
LFP of eligible immigrants	-0.483	-0.350	-0.588***	-0.480***
	(0.377)	(0.309)	(0.172)	(0.143)
N	1,091	1,091	1,091	1,091
M.O. F-stats	6.572	7.486	38.29	42.05
Controls	X	X	√	√

Notes: Columns (1)-(2) and (3)-(4) show without and with controls, respectively. The odd (even) columns show unweighted (weighted) estimates. The independent variable is the labor force participation of eligible migrants within the 35-65 window. The instrument is the share of immigrants who have a result lower than 50 in the entry French test within each skill group. All specifications include the following controls: a dummy equal to one if the test score was below 50 and the normalized test score both at individual and at group level. Specifications (3)-(4) include: education level, age, age squared, male dummy, number of inhabitants in HH, number of children, married dummy, employment in 2010, group-average employment in 2010, share of people with a labor visa in each group,group average age. Further specification (3) and (4) include also a set of further-from-threshold migrants' characteristics: share of eligible migrants, normalized test score, interaction of share of eligible immigrants with normalized test score, age, age squared, share of males, share of labor migrants. Standard errors are clustered at skill-group level. Regressions are weighted by sample weights. Confidence intervals of the first stage estimates in the square brackets. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Imperfect Substitutability Between Old And Young Workers.

(Preliminary draft. Please do not circulate.)

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Abstract

Employment rate of older workers in Italy has increased over the last decade, meanwhile youth employment rate have experienced a big decline. These divergent employment paths raise a question about the substitutability between old and young workers. In order to answer that question, we propose a novel identification strategy to estimate the elasticity of substitution in production between old and young workers. We start setting the labor demand functions for both groups within the same region-occupation-time group to estimate such elasticity. Then, we develop a theoretical model that shows towards-zero estimation bias induced by time correlations within each region-occupation-time group. To overcome this estimation problem, we use a set of instruments based on yearly employment changes by age and citizenship. Using yearly Italian administrative data for the period 1995-2004, we exploit a number of pension and labor migration reforms to create a set of exogenous instruments to time correlations within a region-occupation-time group. Finally, we find that old and young employees within the same region-occupation-time cell experience imperfect substitutability in production.

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1 Introduction

May increases in both life expectancy and retirement age affect youth employment opportunities? Most developed countries experience a decline in either youth employment rate or youth participation rate together with an increase in older participation rate. In 2018, France's and Italy's youth unemployment rates were still larger than pre 2008-crises, 20.08% and 32.2% (OECD database) respectively. While, the U.K.'s and the U.S.'s youth participation rates were 5% and 4% (OECD database) lower than pre 2008-crises, respectively. At the same time, all developed countries experience an increase in participation and employment rate for workers age over 55. These divergent patterns raise a question on the existence of a large degree of substitutability between old and young workers.

The substitutability between old and young workers is an outstanding question in labor literature. On the one hand, the *lump of labor* concept claims that old and young workers compete for a scarce good: a job. Boeri et al. (2017) show that the 2011 sudden increase in the retirement age of *baby-boomers*, the generation born between the end of the World War II and the late '50, due to a pension reform has negatively affected youth employment in Italy. Mohnen (2019), using 1980-2017 U.S. data, finds that the effect of an increase in the retirement age on the youth employment is wider the larger older worker share in low skilled jobs. Bertoni and Brunello (2017) find the same results in Italy between 2004 and 2015. Further, Bovini and Paradisi (2018) find that the effect of an increase in Italian retirement age on youth labor outcomes is wider the larger share of manufacturing workers over the period 2009-2015. On the other hand, the existence of imperfect substitutability between old and young workers should lower the competition for the same job. Brugiavini and Peracchi (2010) find that delaying retirement has a positive on the youth employment rate in Italy between 1997 and 2004. Gruber and Wise (2010) find a positive effect of an increase in older participation rate on youth employment rate by studying labor markets of several developed economies from late '70 to the beginning of the new century. Munnell and Wu (2012), using 1977-2011 U.S. data, show that an older employment increase leads to better labor outcomes for young workers, raising both wages and employment rate. Our paper fills in by offering a novel identification strategy to estimate the old-young elasticity of substitution in production.

In our paper, the elasticity of substitution between old and young workers is the ratio of the percentage change in old-young employment ratio (labor gap) to the percentage change in the old-young wage ratio (wage gap) within the same region-occupation-year cell. We estimate the inverse of such elasticity to identify the causal relation of an increase in labor gap on wage gap. The greater is the identified effect, the smaller is the substitutability between old and young workers. To put it in another way, imperfect substitutability leads to a smaller effect on the age-group wage not affected by the employment increase.

We estimate a structural model to find the elasticity of substitution between old and young workers. We choice a nested constant elasticity of substitution (CES) production function to derive the relation between labor gap and wage gap for two reasons. First, the nested CES dimensions, in our case region-occupation-age-year, allow to control for different demand shifts. Second, the linearity of log first order conditions enables to study the old-young elasticity of substitution by using linear estimators. To estimate the model, we use 1995-2004 Work Histories Italian Panel (WHIP) employee data to estimate such elasticity. We restrict the sample to 712,514 full-time male workers in the private sector as they experience larger employment spells and, hence, accumulate on-the-job human capital at a constant pace. We aggregate data on employees to build total employment and average wage per region-occupation-age-year cell.

Estimating the effect of a labor gap change on the wage gap is not trivial as long-run dynamics might bias the estimates. In the short run, age-specific labor supply shocks might lower the wage gap through a change in the labor gap, but the occurrence of general equilibrium adjustments restores the previous wage gap equilibrium in the long run. Hence, labor supply shocks might have a negative effect on wages in the short run, but a positive one through general equilibrium adjustments in the long run. The net effect might be null, showing inverse-elasticity estimates biased towards zero, since the two effects offset each other. We call the general equilibrium adjustment mechanism *offsetting mechanism*, because it offsets any wage disequilibrium in the long run. Since the *offsetting mechanism* is unobservable and positively correlated with the labor gap and wage gap, elasticity estimates are upward biased.

Our paper contributes to solve this puzzle with two main innovations. First, we develop a theoretical model to understand how the *offsetting mechanism* biases the estimates. The idea is that the *offsetting mechanism* begins to adjust the disequilibrium a year after the labor supply shock applies. Hence, we model *offsetting mechanism* as a function of past labor supply shocks. In order to reduce the bias of the past shocks, a good instrument is a shock at current year. To the best of our knowledge, this way to study the *offsetting mechanism* bias is a novelty in the elasticity of substitution estimation literature.

Second, we provide a novel set of instruments to estimate the elasticity of substitution. As mentioned above, we have to find an instrument uncorrelated with past labor supply shocks to identify such elasticity. One candidate is the labor gap in first differences, because first differences sweep away past trends. However, labor gap in first differences might be correlated with region-occupation-year unobservable heterogeneity. In order to avoid this endogeneity issue, we combine the age dimension, old and young, with the citizenship dimension, native and foreigners, to create four instruments. Each instrument is the ratio of yearly region-occupation-age-citizenship employment change to the total yearly region-occupation-age employment. We exploit a deeper dimension aiming at lowering the region-occupation-year bias. To strengthen our identification strategy, we exploit a set of age-citizenship specific reforms enacted in Italy between 1995-2004. Reforms were enacted to save the Italian social security system from default by increasing both the retirement age and the workers per retiree. The timing of the reforms is suitable to identify the elasticity of substitution since not-serial correlated employment changes lowers the bias with past region-occupation-year labor gap changes.

We find that an increase in the labor gap lowers the wage gap by around 16% within the same region-occupation-year labor market. Further, the effect corresponds to an elasticity of substitution around 6, because in our theoretical model the effect reflects the negative inverse of such elasticity. The elasticity of substitution value range starts from 0, perfect complementarity, to infinity, perfect substitutability, our findings are closer to zero than infinity showing an imperfect degree of substitutability between old and young workers. Our findings are in line with the existing literature on old-young elasticity of substitution (Borjas, 2003; Card and Lemieux, 2001; D'Amuri et al., 2010; Manarcorda et al., 2012; and Ottaviano and Peri, 2012) as scholars find very similar results for different countries in different periods.

We provide a set of robustness checks and sensitivity analyses to test our results. First, we change the weights used to estimate the elasticity, because different weights may lead to different point estimates (Borjas, et al. (2012)). We use wage gap variance as a weight in our baseline estimates, while we weight for total employment in every region-occupation-year cell to test our results. Results do not change. Second, we test our identification with the foreign employment instrument. This instrument is very common in the literature and it is widely used to estimate the old-young elasticity of substitution. We show that the instrument is weak in our specification. Third, we assume that labor supply shocks identify the effect, through our instruments, excluding region-occupation-year demand shocks. We use temporary laid-off workers as a labor demand shock instrument to check our assumption. The instrument is weak and estimates are not significant. Fourth, we evaluate whether our parameters of interest are time-varying. We interact with our instruments with a linear trend increasing the set of instruments from four to eight. The results do not change. Hence, our baseline specification is robust to time dimension.

Related literature.— The literature on old-young substitutability in production exploits demographic changes to understand the degree of complementary among several age groups. Freeman (1979) is one of the first to study the degree of substitution among workers belonging to different cohorts. He estimates the elasticity of substitution between *baby-boomers* and previous cohorts in the US. He finds that an increase in the young labor supply has a larger effect on younger workers' wage than on older workers' one. This result shows workers belonging to different cohorts are imperfect substitutes in production. Katz and Murphy (1992) extend the analysis by exploiting the industry level variability. They identify demand shocks with technological shifts and labor shocks with demographic cohort characteristics. They show that both have a role in setting out the degree of imperfect substitutability across different age groups. A further extension of Katz and Murphy (1992) is Card and Lemieux (2001), where they improve the accuracy of elasticity of substitution estimates by taking into account both time effects and cohort effects. The underlying intuition relies on different salary paths among cohorts over time. By exploiting "baby-boomer" shock in the U.S., Canada, and the UK in a nested constant elasticity of substitution, they are able to make cross-country comparisons of the results. They estimate an elasticity of substitution among different age groups in the range of 4 to 6 by proving that additional fixed effects play an important role to exclude any possible bias due to supply or demand shifts. Borjas (2003), D'Amuri et al. (2010), Manarcorda et al. (2012) and Ottaviano and Peri (2012) extend Card and Lemieux (2001) exploit foreign labor force as instrument to estimate old-young elasticity of substitution, but thier estimates do not show any significant difference.

All the mentioned scholars do not pay much attention to long-run effects, while Llull (2018) points out that demand for different labor inputs depend on past labor supply shocks[1]. He shows that human capital accumulation is one of the main drivers to adjust wage disequilibrium in the long run. Also Jaeger et al. (2018) show that firms anticipate the labor supply shifts by adjusting the capital level. These mechanisms happen when labor force increases are stable across years. However, they only focus on foreign labor supply shocks, while we address the long-run bias issue by taking into account native labor supply shocks as well. We provide an estimation strategy that complements the literature on old-young elasticity of substitution estimate and adds an other piece to general equilibrium adjustment puzzle. Our estimation method relies on Arellano and Bover (1995) who exploit the first differences to identify the long run parameter in a dynamic panel framework. The underlying intuition is the same, but we apply that in a static framework.

The article proceeds as follows. Section 2 describes the institutional background over the considered time span. Section 3 presents the theoretical framework. Section 4 shows the data and the descriptive statistics. Section 5 discusses the empirical strategy. Section 6 shows the results. Section 7 presents robustness checks. Section 8 concludes.

[1]People reshape their human capital accumulation after experienced labor supply shocks.

2 Institutional Background

At the turn of the 20th century, Italy has experienced a number of labor market reforms mostly tackling the supply side. Among others there were pension reforms and migration flows regulations. Depending on the type of reform, different cohorts of workers were involved.‘ What follows is a brief review of all these different policies, grouped by theme.

2.1 Pension Reforms

The age threshold defining the active population of a country clearly affects the size of labor force, and pension reforms play an important role in setting such a threshold. The idea behind reforms in the '90s was keeping older workers in the labor market as longer as possible. This is due to an increase in life expectancy and experts were casting doubts on the sustainability of a pay as you go pension system. Two main pension reforms characterize the end of the century, Dini reform in 1995 and Prodi reform in 1997. As mentioned, the main aims were containment of public spending and curbing early retirement. The very first attempt to postpone retirement age (gradually) occurs with Amato reform, in 1992. In 1995, Dini reform, (L.335/1995), raised the age and contribution requirements for seniority pension. The change was gradual and finished in 2008. Prodi reform in 1997 further increases age and contribution requirements for seniority pensions.

2.2 Migration Reforms

Italy has long been a country of emigration. First regulations on immigration flows date back to the 80s. Up to that moment legalization of immigrant workers mainly happened through amnesties. In the '90s, a pool of migration laws enacted and included an amnesty to legalize migrant workers who had been working (or living) in the country for a year before. In 1990, Martelli law, L. 39/1990, was the first to regulate economic immigration in the country and legalize 215,000 foreign workers.

This law imposed restrictions to incoming flows, and set a maximum number of workers to be accepted each year, based on foreseen Italian labor market needs. Dini decree, in 1995, allowed for the legalization of 244,500 immigrant workers. In 1998, the Turco-Napolitano law (L 40/1998) implemented major changes. This law represented the milestone for migration regulation in Italy. It involved inclusion of migrant workers within the labor force and made procedures and rules smoother and clearer. It allowed immigrants a temporary visa through the sponsorship channel to look for a job. Together with this reform, other 217,000 workers were regularized. Political debates spurred by increased migration flows conveyed into the Bossi Fini law (L. 189/2002). That stopped the sponsorship system and introduced stricter limitations to immigration. Few months after the enactment, the situation in black market was dramatic and, therefore, the two Ministers,Bossi and Fini, promoted the largest amnesty in Europe (634,700 immigrant workers were legalized). As such, some scholars used it to better understand the impact of amnesties on labor market outcomes. Devillanova et al. (2014) exploit it as a natural experiment and show that an increase in employment probability follows the prospect of legal status. Size of this increase is two third of the increase in employment rate illegal immigrant experience in the five years after entering the country. Di Porto et al. (2019) show the short term impact of 2002's regularization, with most of the legalized workers staying in the legal labor market for long. Amnesty regularized 62% of regular immigrants in the country in 2002 (Barbagli et al., 2004).

3 Model

3.1 Theoretical Framework

In order to study the elasticity of substitution between old and young workers we use a nested constant elasticity of substitution (CES) approach. Most of the prominent studies (e.g., Card and

Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2012) have used an aggregate production function to estimate the elasticity of substitution between old and young workers. An aggregate model provides an overview of national labor market but loses information about differences among local labor markets. We prefer to add the regional dimension to take into account local differences in labor force. We assume an identical Cobb-Douglas production function in each region r at time t:

$$Y_{rt} = A_{rt} K_{rt}^{\alpha} L_{rt}^{1-\alpha} \tag{1}$$

where Y is the output, A is exogenous total factor productivity, K is the physical capital, L is a CES aggregate of different types of labor, and α is the income share of capital. L_{rt} includes workers who differ by occupation and age, respectively. Let

$$L_{rt} = [\theta_{rBCt} L_{rBCt}^{\frac{\sigma-1}{\sigma}} + \theta_{rWCt} L_{rWCt}^{\frac{\sigma-1}{\sigma}}]^{\frac{\sigma}{\sigma-1}} \tag{2}$$

where BC (WC) indicates blue collar workers (white collar workers) and σ is the elasticity of substitution between blue collar and white collar workers ($0 \leq \sigma < \infty$). The θs are the region-occupation-time specific productivity parameters, with $\theta_{rBCt} + \theta_{rWCt}$=1. Finally, every occupation-specific labor input is a CES aggregate of imperfect substitute age-specific labor inputs. In particular,

$$L_{rst} = [\gamma_{rsOt} L_{rsOt}^{\frac{\lambda-1}{\lambda}} + \gamma_{rsYt} L_{rsYt}^{\frac{\lambda-1}{\lambda}}]^{\frac{\lambda}{\lambda-1}} \quad s = BC, WC \tag{3}$$

where O (Y) indicates old worker (young worker) labor input and λ, our parameter of interest, is the elasticity of substitution between old and young workers, with $\lambda \geq 0$. γs are the region-occupation-age-time specific productivity parameters, with $\gamma_{rsOt}+\gamma_{rsYt}$=1. We get the old (young) labor demand within each region-occupation-year cell by assuming that marginal product of old (young) labor is equal to the old worker (young worker) wage. Using logs, the age specific labor demand within each region-occupation-year cell is equal to:

$$ln(w_{rsat}) = ln(A_{rt}K_{rt}^{\alpha}L_{rt}^{-\alpha}(1-\alpha)) + \frac{1}{\sigma}ln(L_{rt}) + ln(\theta_{rst}) - (\frac{1}{\sigma} - \frac{1}{\lambda})ln(L_{rst}) + ln(\theta_{rsat}) - \frac{1}{\lambda}ln(L_{rsat}) \quad (4)$$

Taking the difference side by side of labor demands for old and young workers, we get rid of all terms on the right-hand side, but the difference between productivity parameters and the difference between old and young labor demands:

$$ln(\frac{w_{rsOt}}{w_{rsYt}}) = ln(\frac{\theta_{rsOt}}{\theta_{rsYt}}) - \frac{1}{\lambda}ln(\frac{L_{rsOt}}{L_{rsYt}}) \quad (5)$$

Hereafter, we define the wage difference between the old and young worker as "wage gap" and the employment difference between old and young workers as "labor gap".

3.2 Labor gap between old and young by citizenship

In this subsection, we show how local and foreign labor supply shocks affect the labor gap between old and young workers. We assume that the employment level for old and young workers within the same region-occupation-year cell is a function of native and foreign employment[2] :

$$L_{rsat} = f(L_{rsaNt}, L_{rsaFt})) \quad (6)$$

where N (F) indicates native (foreigner) characteristics. We assume that each age-specific labor input is continuously differentiable and the first derivative with respect to citizenship dimension

[2]A wide range of literature (e.g. D'Amuri et al., 2010, Manarcorda et al., 2012 and Ottaviano and Peri, 2012) use a CES aggregate of native and foreign labor inputs in every age-specific group. We do not assume any functional form to avoid any constraint on the age-citizenship elasticity of substitution.

is greater than zero. Looking at differential[3] in discrete form we obtain:

$$\Delta(ln(L_{rsOt}) - ln(L_{rsYt})) = \Sigma_c(\beta_{Oc}\frac{\Delta L_{rsOct}}{L_{rsOt}} - \beta_{Yc}\frac{\Delta L_{rsYct}}{L_{rsYt}}) \qquad c = N, F \tag{7}$$

where

$$\beta_{Oc} = \frac{\partial L_{rsOt}}{\partial L_{rsOct}} \qquad \beta_{Yc} = \frac{\partial L_{rsYt}}{\partial L_{rsYct}} \tag{8}$$

with c indicating if they are either natives or foreigners[4]. Each β is assumed to be fixed[5]and measures the elasticity of labor gap differential change with respect to a specific subgroup differential change, where a subgroup is the total number of workers with age a and citizenship c.

This decomposition allows us to understand how labor supply shocks affect the labor gap. By imposing β_{OF}=β_{YF}=β_F and rearranging the terms we get:

$$\frac{\Delta(ln(L_{rsOt}) - ln(L_{rsYt}))}{\frac{\Delta L_{rsOFt}}{L_{rsOFt}} - \frac{\Delta L_{rsYFt}}{L_{rsYFt}}} = \beta_F + \beta_{ON}\frac{\frac{\Delta L_{rsONt}}{L_{rsOt}}}{\frac{\Delta L_{rsOFt}}{L_{rsOFt}} - \frac{\Delta L_{rsYFt}}{L_{rsYFt}}} - \beta_{YN}\frac{\frac{\Delta L_{rsYNt}}{L_{rsYt}}}{\frac{\Delta L_{rsOFt}}{L_{rsOFt}} - \frac{\Delta L_{rsYFt}}{L_{rsYFt}}} \tag{9}$$

The left-hand side is the elasticity between a change in old-young labor gap and a change in old-young foreigner labor gap, where the denominator represents a change in foreign employment. We show that the effect of a foreign labor supply shock is not constant, but it depends on old and young native labor supply change.

This is an important result, in the literature a very common assumption is the exogeneity of foreign labor supply shock with respect to native employment. In our model, instead, the effect of a foreign labor supply shock on old-young labor gap depends also on native labor supply shocks (if any).

[3]In Appendix for the mathematical derivation.

[4]This methodology is very similar to one developed by Amiti et al. (2019) that provide a decomposition of a firm price differential.

[5]In the empirical section we allow parameters to vary over time.

In the Empirical Strategy Section we take into account this finding to estimate the elasticity of substitution.

4 Data

The empirical analysis is based on the information on the wages and employment of old and young workers drawn from the 1995-2004 Work Histories Italian Panel (WHIP) dataset. The WHIP also contains information on citizenship that we use to create our set of instruments.

The WHIP database includes information on social securities records of 2,164,829 employees from 1995 to 2004, around 140,000 observations per year. Since our aim is to provide with a new identification strategy using a standard theoretical framework, we follow the literature to create wage and employment variables.

The main sample is restricted to men aged 18-64 working in the private sector. We narrow the analysis only to male employees as old and young females do not accumulate constantly experience in their working life showing larger substitutability (Freeman, 1979). Further, foreign females in Italy are usually employed as either caregivers or domestic helper, while native females are often employed in the public sector (Venturini and Villosio, 2008). We narrow the analysis only to private sector as public sector has more rigid labor dynamics. EU15 foreign workers are excluded from the analysis to avoid confounding effects due to similarities between EU15-foreign and Italian workers. Including EU15 workers in native (foreign) group might overestimate (underestimate) the elasticity of substitution. Further, the EU15 worker exclusion allows us to focus our attention on foreigners with different skills with respect to natives. We take the gross average log daily wage as wage measure[6]. Unfortunately, we have only information on total contribution days, where one contribution day is equal to 8 hours spent at work. The lack of information on hours spent at work

[6]We get rid of the first and last percentile of the distribution in order to avoid any confounding effect due to outliers.

does not allow us to include part time jobs as they differ by hours spent at work per day. Due to this missing information we are not able to homogenize the daily wage of part-time workers. Hence, we prefer to consider only full time workers. To measure the cell-specific employment we, first, measure the days spent in each region-occupation-age-year cell to the total worked days in a year per every worker. Then, we sum these shares to obtain the total employment in each region-occupation-age-year cell. We follow this procedure to overcome the assignment of a single worker to different region-occupation-age-year cells since there are some workers that change either working region or job within a year. Following the literature (i.e. Card, 1999), we assign the 'old' label for workers age over 38[7].

Table 1 and Table 2 show the shares in each macro-region-age-citizenship[8] cell by occupation. Table shows that for every 10 young blue (white) collar workers, there are 5 old blue collar workers (7 white collar workers). This evidence shows that old employees are more likely to hold a white collar occupation than a blue collar one. Further, the age-specific ratio of the blue collar total employment to the white collar total employment is equal to 2.96 and 2.08 for the young and old employees, respectively. These ratios show that old employees compete much more for white collar occupations since there are three young blue collar workers for each young white collar worker, and only two old blue collar workers for each old white collar worker. Hence, an increase in retirement age has a larger effect on young white collar workers than young blue collar workers. Further, Table 1 and Table 2 show that foreigners are more likely to be young and blue collar workers. Hence, an increase in foreign workers affects mostly the native young blue-collar workers. Table 3 and 4 show log daily wages for blue and white collars, respectively. In each table, we have information on log daily wages in each macro region-age cell over 1995-2004 period. Wages are

[7]The explanation is that workers accumulate on-the-job human capital with a lower pace when they are age over 38.

[8]I show macro regions instead of regions for the sake of table clarity. The regional summary statistics lead to the same descriptive evidences. North includes: Emilia Romagna, Friuli Venezia Giulia ,Lombardia, Liguria, Piemonte and Valle d'Aosta, Trentino Alto Adige, Veneto. Centre includes: Lazio, Marche, Toscana and Umbria. South and Islands include: Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sardegna and Sicilia

deflated to 2001 euro by using OECD's Italian CPI series. We observe an overall sharp increase of real wages until 1999, followed by a decline and a renewal until 2004. Looking at the difference between the old and young average log daily wages, we see a declining path for blue collar wage differences, while there is no evidence of such trend for white collars. This finding suggests that old and young white collar workers are more substitute than old and young blue collar workers. Young blue collar wage does not respond to migration and pension reforms, while old blue collar wages lower over time by decreasing the gap with young blue collar workers. Instead, young white collar workers do not fill the wage gap with old white collar workers over time, showing a larger substitutability. Although these evidences are only descriptive, we might consider them as a first evidence on the degree of substitutability between old and young workers.

Finally, Figure 1 shows the trend for each instrument from 1985-2004. Each instrument is the ratio of yearly region-occupation-age-citizenship employment change to the total yearly region-occupation-age employment. We have four subgroups: old natives, young natives, old foreigners and young foreigners. Yearly changes are very noisy from 1995 to 2004, they do not follow a common path as observed for young and old natives in the previous periods. Hence, we exploit this variability to estimate old-young elasticity of substitution.

5 Empirical strategy

In this section we explain how to implement our theoretical findings in an empirical setting to estimate the elasticity of substitution between old and young workers. We estimate the equation (5) in Section 3. As shown in Section 4, we use as dependent variable the difference between average log daily wage of old workers and average log daily wage of young workers for each region-occupation-year cell (wage gap). Independent variable is the difference between the total employment of old and young workers for each region-occupation-year cell (labor gap). The old-young elasticity of substitution, $1/\lambda$, may be biased towards zero since labor gap might be positively correlated with long run *offsetting mechanism*. In the next subsection, we explain how long run adjustment may bias our estimates.

5.1 The *offsetting mechanism* function

We start estimating the equation (5) by substituting the log of old-young productivity ratio with a broad set of fixed effects and an error term. The new estimating equation is:

$$ln(\frac{w_{rsOt}}{w_{rsYt}})) = \phi_{rt} + \phi_{rs} + \phi_{st} - \frac{1}{\lambda} ln(\frac{L_{rsOt}}{L_{rsYt}}) + \varepsilon_{rst} \quad (10)$$

where ϕ_{rt} and ϕ_{st} capture every time productivity shift in each regional and occupation labor market, respectively. ϕ_{rs} captures any time invariant characteristic in each region-occupation labor market. ε_{rst} stands for whatever time variant residual components in every specific region-occupation labor market. However, we are not able to fully control for productivity shifts by using pairwise region-occupation-year fixed effects.

The unobservable region-occupation-year productivity shifts nested in the error term are very likely to be correlated with the labor gap. Indeed, the current labor gap and the current productivity shifts are both an increasing function of past age-group specific labor supply shocks. However, they have different effects on wage gap. An increase in the labor gap should have a negative effect on the wage gap, since an age-group specific labor supply has a larger negative effect on their own wages than on other age-group wages. On the other hand, an increase in productivity should have a positive effect on wage gap, since an increase in productivity might increase the wages. Hence, the positive correlation between productivity and both labor and wage gap might biases the estimates of the elasticity of substitution towards zero. We call this mechanism *offsetting mechanism* because it offsets the effect of the labor gap increase on the wage gap triggered by a labor supply shocks.

We want to shed light on this mechanism and on the correlation between labor gap and the *offsetting mechanism*. We assume that labor gap has a stable AR(1) process, we can see this process as MA(∞) process:

$$ln(\frac{L_{rsOt}}{L_{rsYt}}) = \sum_{k=1}^{t} \beta^t \epsilon_{rsk} + ln(\frac{L_{rsO0}}{L_{rsY0}}) \tag{11}$$

where $\epsilon_{rsk} = \Delta ln(\frac{L_{rsOk}}{L_{rsYk}})$, the last term is the yearly variation in the labor gap. Hence, on the right-hand side of Eq. (11) we have the sum of all labor gap yearly variations until t plus the initial condition. When $\Delta ln(\frac{L_{rsOk}}{L_{rsYk}}) \neq 0$, there is a change in the wage gap and the *offsetting mechanism* starts affecting later in time. Assuming that *offsetting mechanism* fully offsets wage shift in the following period[9], we nest the *offsetting mechanism* process in the error term:

$$\varepsilon_{rst} = \xi_{rst} + f(\epsilon_{rst-1}) \tag{12}$$

where ξrst is a random effect uncorrelated with the labor gap and $f(\epsilon_{rst-1})$ is the *offsetting mechanism* that depends on lag of yearly labor gap variation, ϵ_{rst-1}. As a result, this *offsetting mechanism* has a positive correlation with the labor gap biasing old-young elasticity of substitution estimate[10].

Offsetting mechanism function takes into account only the previous period labor gap change and not the current one. This structure allows us to exploit the current change as exogenous to the *offsetting mechanism* function.

The described mechanism is in line with papers that use new wave of migrants as an instrument to estimate the old-young elasticity of substitution (e.g. Borjas, 2003; Ottaviano and Peri, 2012), since yearly labor supply shocks are uncorrelated with previous ones[11].

[9] Adding more lags results hold.

[10] Because the $Cov(ln(\frac{L_{rsOt}}{L_{rsYt}}), f(\epsilon_{rst-1})) > 0$

[11] Jaeger et al. (2018) point out that the exogeneity of migration inflows depends on previous wave of migrants.

5.2 A short run instrument

In the previous subsection, we discussed the structure of the *offsetting mechanism* process nested in the error term and the features that an instrument must have in order to identify the elasticity parameter. In our setting, we exploit variations based on current foreign and native labor supply shocks. We exploit the elements on the right-hand side of Eq. (7) as instruments, where each of them takes into account yearly change in each region-occupation-age-citizenship cell[12]. This methodology is very common in the macro literature, especially in dynamic panel data models[13].

In order to identify the true parameter and rule out all the time correlations between the instruments and the error term, we assume that employment time series in every subgroup is a random walk:

$$\Delta L_{rsact} = \varepsilon_{rsact} \tag{13}$$

where, the first differences are equal to the error at current period. Testing this assumption we cannot reject the presence of unit root for every region-occupation-age-citizenship group[14].

The main concern is that the instruments might still depend on unobservable characteristics within region-occupation-year cell. To overcome this problem, we exploit Italian reforms enacted over the period 1995-2004. Between 1995 and 2014, Italy has enacted a series of national policies

If the flow of new migrants is stable across years, the labor demand can foresee the new inflow and adjust itself before it comes up.

[12]In the Appendix B, we compute parameter distortion when we use the first order differences as instrument and we assume that the *offsetting mechanism* function is linear. The estimated distortion is very small and equals to $-\frac{T}{(T-2)(T-1)}$, in particular it is smaller than the Nickell's one (Nickell, 1981).

[13]Arellano and Bover (1995) were the pioneers of this identification strategy that exploits the short run changes (i.e. first differences) to instrument levels. They use the lagged first differences as an instrument for the lagged value of the dependent variable that is their explanatory variable.

[14]P-values of Harris-Tzavalis unit-root test are: 1.00 for old foreign labor supply, .798 for young foreign labor supply, .1406 for old native labor supply and .9995 for young native labor supply

that have affected the labor supply of all considered categories across years. They provide us with exogenous variation to identify the effect as they are not labor market specific. This continuous treatment has allowed us to exploit short run effects as instrument.

6 Results

Table 5 shows the old-young elasticity parameter, $-\frac{1}{\lambda}$, by using different estimators. All regressions are weighted by the inverse of the wage gap variance to reduce the bias of the cells with small sample size[15]. In the first two columns we show the results by using ordinary least squares, OLS. As discussed in previous sections, the estimates are biased towards zero both without and with the time-occupation fixed effects. This finding is in line with the literature, that highlights the positive correlation between labor gap and *offsetting mechanism* in every region-occupation-year cell.

In the following six columns we use IV methodology by exploiting different estimators. From the third to the sixth column, we show the results by using two stage least squares, 2SLS, and the limited information maximum likelihood, LIML. As pointed out by Angrist and Krueger (1991), 2SLS and LIML estimates have to be very close in an overidentified framework because asymptotically they have the same distribution. The point estimates are -0.250 and -0.259 for 2SLS and LIML without occupation-time fixed effects and -0.168 and -0.171 for 2SLS and LIML including them. The quite similar results do not show any problem in the specification. Furthermore, the specifications pass the F-statistic and the overidentification tests. The former is 45.46 and 14.69, respectively, without and with occupation-time fixed effects and the other one cannot reject the null hypothesis of good specification at a significance level of 5%. The last two columns show

[15]There is a wide debate about the weight to be used. OP (2012) use the sample size in every specific cell as weight, while Borjas et.al (2012) in a comment to their paper say that is better to use the inverse of the wage variance.

the estimates with a continuously-updated GMM estimator, that allows for heteroskedastic and autocorrelation disturbances, we add this estimation in order to take into account of a possible correlation among different shocks. Estimates confirm the previous results.

In Table 7 we show the estimates with employment cell weight to be sure that wrong weights drive our estimates. The estimates are quite similar, not showing any difference to use different weights. Results in Table 5 and 7 prove that there is not difference between the wage variance weight and employment-cell weight when the model is well specified.

In Table 8 we use a control function approach[16]. This approach used by Wooldridge (2015) is suitable for our aim, because residuals contain the endogeneity source that we cannot control in our model. In this way by adding this part in the main regression we control directly for the bias. Residuals' parameter captures the *offsetting mechanism* of shocks. Indeed, as showed in Table 8, the estimate has the opposite sign of our old-young elasticity parameter and more or less the same magnitude.

Hence, by using the control function approach we have not only the elasticity parameter but also the relative bias. In particular when we add the occupation-time fixed effects the parameter is even closer in absolute value to the elasticity estimate.

Old-young elastacity of substitution, λ, is between 4 and 6. The results are in line with Ottaviano and Peri (2012), Borjas (2003) and Card and Lemieux (2001) estimates when they use 8 level of experience and 4 level of education. Our result differ from Ottaviano and Peri (2012), when they use old and young as a proxy for experience. They find an elasticity of substitution around 3[17]. This result shed lights on time correlation bias.

[16]In the control function approach you have to run an IV first stage, then get the residuals and put them in the main regression.

[17]Their estimate is equal to -0.31

7 Robustness checks

7.1 The comparison with migration instrument

Ottaviano and Peri (2012) and Borjas (2003) exploit foreign workers as instrument to identify age-group elasticity of substitution[18]. They assume that foreign labor supply shocks in the foreign labor force in each region-occupation-year cell, once added fixed effects, identify the elasticity of substitution between old and young workers.

In subsection 4.2 we find that the effect of a change in foreign labor supply on the labor gap is not constant since it depends on native labor supply changes. In order to take that into account, we consider both native and foreign labor supply shocks in each region-occupation-age-citizenship cell aiming at having an unbiased estimate.

In this subsection, we compare our specification with the one used by Borjas (2003) and Ottaviano and Peri (2012) by using their instrument to estimate the elasticity of substitution between old and young workers.

Table 9 shows estimates close to Ottaviano and Peri (2012)[19] when we do not control occupation-time fixed effects. The results change when we add them. The parameter is not more significant with standard error quite large. Our explanation is that large standard errors are due to the correlation between instrument and error term. The missing information on native labor supply changes in other skilled categories might create a correlation between the foreign instrument and the *offsetting mechanism* process.

In our setting we add both native and foreign labor supply changes, which help us to rule out

[18] An other instrument widely used by researchers is the shift-share instrument (Altonij and Card, 1991) that exploits migrant enclaves in the previous decades to create an instrument exogenous with respect to current economic conditions. Unfortunately we cannot compare our methodology with that, because our dataset does not have information later than 1985 and because the level of inflows in decades before 1995 is almost null or is selected among high skilled workers (before 1990 in Italy there was not a labor migration policy, so for migrant was almost impossible to come in Italy with a labor VISA.)

[19] Our estimates are little bit larger because we use a regional approach as opposed to a national one. For this reason the bias is smaller than Ottaviano and Peri (2012).

any possible correlation with the long run adjustments.

7.2 Labor demand shocks

In our paper we exploit short run changes in every region-occupation-age-citizenship cell. We state that the short run changes are supply driven assuming that the possible labor demand shocks are captured by fixed effects. In this section we test this assumption by using an instrument that is based on labor demand shock.

We exploit the information on unemployment support mechanisms provided in our dataset. The Italian government helps firms to face crisis periods by paying part of employee salaries for a given period[20], when employers suspend temporary employment relationships. During the suspension firms cannot hire other workers in order to hold the benefit and, at the same time, employees cannot engage in another job. This tool has been created mainly to help the manufacturing sector, where most of the workers are engaged. We, thus, use the information whether a worker is in this program to capture a labor demand shock due to a firm's temporary crisis.

Our first stage is the labor gap on the log of the number of temporary suspended workers. The sample is reduced to 322 observations from 1996-2004 since we have some cells that do not experience any temporary suspension. Table 10 shows the results. The estimates are not significant and F-stat of the instrument is very low. The F stat is very low showing how a labor demand-driven instrument is not suitable to provide an exogenous variation to identify labor gap.

7.3 Relaxing time invariant assumption on the first-stage parameters

In subsection 4.2 we have assumed that the derivatives of each specific region-occupation-age labor input with respect to a change in both native and foreign employment is constant over time.

[20]The so called "Cassa Integrazione"

In this subsection we show the results when derivatives vary over time.

In order to add a time dimension to parameters, we multiply the instruments by a linear time trend[21]. Rearranging Eq. (7) we get:

$$\Delta(ln(L_{rsOt}) - ln(L_{rsYt})) = \Sigma_c(\beta_{Oc}(1 + trend)\frac{\Delta L_{rsOct}}{L_{rsOt}} - \beta_{Yc}(1 + trend)\frac{\Delta L_{rsYct}}{L_{rsYt}}) \qquad c = N, F \tag{14}$$

Table 11 shows the results using this broader set of instruments that takes into account of time dimension. The results are quite similar to the time constant ones. Hence, without loss of generality, we can assume time invariant parameters.

8 Conclusions

How much old and young workers are substitutes in production is of great interest worldwide. Demographic changes have affected the composition of the labor force. This paper tries to shed light on the degree of substitutability between old and young workers in production.

In line with the tradition on this topic, we use a nested-CES framework at regional level to derive the elasticity of substitution between old and young workers. The choice of a local approach allows us to rule out any possible misleading effect due to huge differences across Italian regions. We can get a more precise estimate of the overall elasticity of substitution controlling for different local growth patterns.

In the literature, adding specific skill fixed effects is generally used to control for different demand shifts. Still, possible biases might remain due to log-run factors. In particular, skill-specific unobservable long-run adjustments offset any variation suited to study the effect of a skill-specific labor force change on the skill-specific wages biasing the elasticity estimates towards zero. Studying the dynamics of the *offsetting mechanism*, we identify the bias and create an instrument

[21] We try also to interact time dummies with every instrument. The results are not different from adding a linear trend.

to overcome the omitted variable problem. We exploit the variation triggered by Italian reforms to create a set of instruments exogenous to skill-specific . The estimated elasticity of substitution is between 4 and 6, in line with previous findings.

In a global scenario, young workers concern about the longer working life of some old workers. Our results show old and young workers are imperfect substitution in production. Hence, policies aiming at reducing the youth unemployment should not consider the early retirement as a solution. Instead, they should look at specific characteristics of young workers to increase the match between firms needs and young worker skills.

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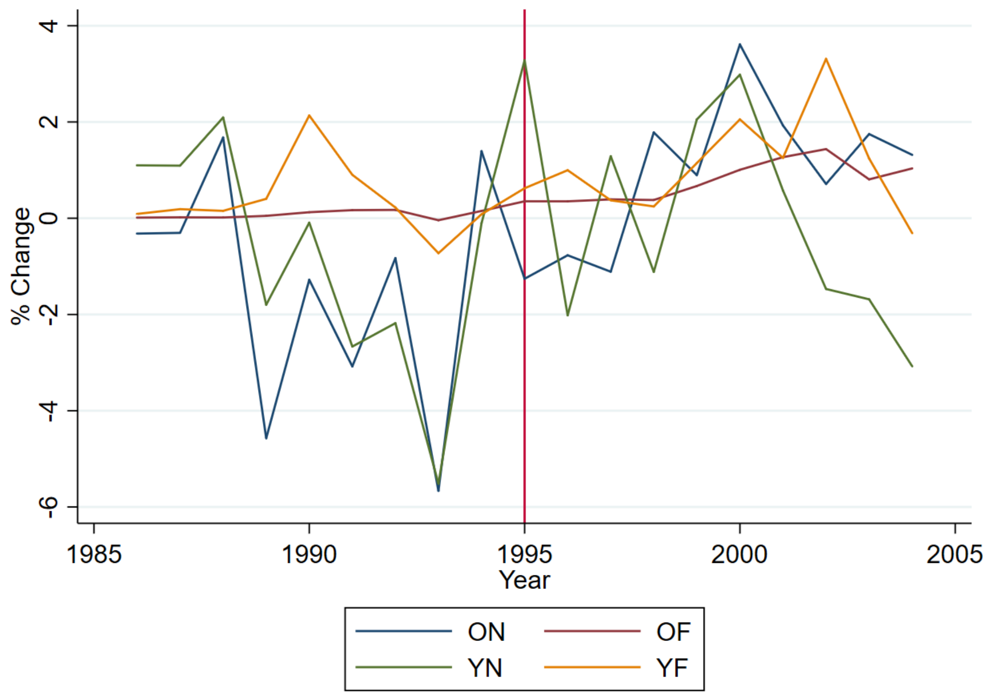
Tables and Figures

Figure 1: Ratios of age-citizenship yearly employment changes to the relative yearly age employment between 1985 and 2004



ON: old natives. OF: old foreigners. YN:young native. YF: young foreigners.

Table 1: Blue collar subgroup shares within all sample, within the subgroup sample and within the sector sample

		Old			Young		
Macro Region	Place of birth	(1)	(2)	(3)	(1)	(2)	(3)
North	Foreign-born	1.695	5.122	19.616	6.947	10.384	80.384
	Native-born	15.880	47.982	32.917	32.362	48.371	67.083
Centre	Foreign-born	0.434	1.312	21.110	1.623	2.426	78.890
	Native-born	5.674	17.144	36.439	9.897	14.793	63.561
South and Islands	Foreign-born	0.177	0.534	22.971	0.593	0.886	77.029
	Native-born	9.235	27.905	37.365	15.481	23.140	62.635
Subgroup Obs			168,968			341,571	

Notes: (1) subgroup share in the row sector with respect to all sample;(2) subgroup share in the row sector with respect to subgroup sample; (3) subgroup share in the row sector with respect to row-sector sample. Sample from 1995 to 2004.

Table 2: White collar subgroup shares within all sample, within the subgroup sample and within the sector sample

		Old			Young		
Macro Region	Broad Experience	(1)	(2)	(3)	(1)	(2)	(3)
North	Foreign-born	0.289	0.709	35.850	0.517	0.873	64.150
	Native-born	23.499	57.635	38.952	36.830	62.185	61.048
Centre	Foreign-born	0.172	0.422	46.964	0.195	0.328	53.036
	Native-born	8.667	21.256	42.406	11.771	19.874	57.594
South and Islands	Foreign-born	0.088	0.215	39.955	0.132	0.222	60.045
	Native-born	8.058	19.762	45.164	9.783	16.518	54.836
Subgroup Obs			82,375			119,660	

Notes: (1) subgroup share in the row sector with respect to all sample;(2) subgroup share in the row sector with respect to subgroup sample; (3) subgroup share in the row sector with respect to row-sector sample. Sample from 1995 to 2004.

Table 3: Average log daily wage for male blue collar workers, 1995-2004

Macro Region	Broad Experience	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
North	Old	4.4465	4.4394	4.4551	4.4504	4.4455	4.4258	4.4259	4.4154	4.4040	4.4255
	Young	4.2900	4.2879	4.3038	4.3111	4.3045	4.2988	4.2974	4.2924	4.2915	4.3116
Centre	Old	4.4341	4.4239	4.4318	4.4214	4.4174	4.4127	4.3780	4.3712	4.3611	4.3762
	Young	4.2591	4.2570	4.2639	4.2703	4.2615	4.2619	4.2587	4.2425	4.2473	4.2639
South and Islands	Old	4.4278	4.4087	4.4175	4.3898	4.3941	4.3781	4.3700	4.3526	4.3514	4.3596
	Young	4.2612	4.2430	4.2471	4.2269	4.2184	4.2410	4.2381	4.2430	4.2397	4.2609

Notes: The table reports the mean of the log daily wage of workers in each region-age group. All wages are deflated to 2001 euro using the Italian CPI index from OECD database.

Table 4: Average log daily wage for male white collar workers, 1995-2004

Macro Region	Broad Experience	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
North	Old	4.9591	4.9633	4.9865	4.9917	5.0035	5.0045	4.9935	4.9927	4.9949	5.0029
	Young	4.6578	4.6478	4.6752	4.6835	4.6943	4.6969	4.6991	4.7036	4.6916	4.7041
Centre	Old	4.9576	4.9545	4.9712	4.9756	4.9598	4.9522	4.9507	4.9392	4.9479	4.9503
	Young	4.6336	4.6288	4.6339	4.6441	4.6497	4.6499	4.6426	4.6455	4.6350	4.6320
South and Islands	Old	4.8804	4.8644	4.8806	4.8623	4.8648	4.8614	4.8413	4.8333	4.8241	4.8364
	Young	4.5260	4.5136	4.5329	4.5257	4.5026	4.4920	4.4916	4.4737	4.4747	4.4880

Notes: The table reports the mean of the log daily wage of workers in each region-age group. All wages are deflated to 2001 euro using the Italian CPI index from OECD database.

Table 5: Estimated old-young elasticity of substitution, $-\frac{1}{\lambda}$, weighted by the inverse of dependent variable variance

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	OLS (1)	OLS (2)	LIML (3)	LIML (4)	2SLS (5)	2SLS (6)	GMM-CUE (7)	GMM-CUE (8)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.0893***	-0.0157	-0.259***	-0.171***	-0.250***	-0.168***	-0.215***	-0.164***
	(0.0322)	(0.0366)	(0.0518)	(0.0510)	(0.0494)	(0.0499)	(0.0461)	(0.0484)
Skill x Time FE	NO	YES	NO	YES	NO	YES	NO	YES
P-value Lm-stat			0.000298	0.000745	0.000298	0.000745	0.000298	0.000745
F-stat			45.46	14.69	45.46	14.69	45.46	14.69
P-value J-stat			0.0773	0.389	0.0742	0.386	0.0742	0.386
Observations	342	342	342	342	342	342	342	342

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The regressions are weighted by the inverse of the wage ratio variance. The dependent variable is the log ratio of average old and young worker wages in a region-occupation-age-time-specific cell and the explanatory variable is the log of the ratio between old and young employment inside the same cell. LM test is for underidentification test, F statistic is for weak identification test (Cragg-Donald or Kleibergen-Paap) and J test is for overidentification test. Time span covers from 1996-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 6: First stage and Reduced Form weighted by the inverse of dependent variable variance

	First stage		Reduced Form	
$\frac{\Delta L_{rsOFt}}{L_{rsOt-1}}$	1.399	-0.0254	-0.659***	-0.138
	(0.907)	(0.716)	(0.213)	(0.253)
$\frac{\Delta L_{rsYFt}}{L_{rsYt-1}}$	-0.448*	-0.160	0.102	0.0300
	(0.246)	(0.521)	(0.105)	(0.183)
$\frac{\Delta L_{rsONt}}{L_{rsOt-1}}$	0.433***	0.508***	-0.0850**	-0.0658*
	(0.119)	(0.0918)	(0.0391)	(0.0393)
$\frac{\Delta L_{rsYNt}}{L_{rsYt-1}}$	-0.743***	-0.569***	0.166***	0.115**
	(0.107)	(0.105)	(0.0520)	(0.0538)
Skill x Time FE	NO	YES	NO	YES
Observations	342	342	342	342

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The regressions are weighted by the inverse of wage ratio variance. The explanatory variables are measured as the yearly change in the old (young) native (foreign) group over the total old (young) employment. Time span covers from 1996-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Estimated old-young elasticity of substitution, $-\frac{1}{\lambda}$, weighted by the size of independent variable

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	OLS (1)	OLS (2)	LIML (3)	LIML (4)	2SLS (5)	2SLS (6)	GMM-CUE (7)	GMM-CUE (8)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.0897***	-0.0168	-0.258***	-0.167***	-0.249***	-0.164***	-0.210***	-0.158***
	(0.0306)	(0.0343)	(0.0499)	(0.0492)	(0.0472)	(0.0480)	(0.0436)	(0.0470)
Skill x Time FE	NO	YES	NO	YES	NO	YES	NO	YES
P-value Lm-stat			0.000108	0.000190	0.000108	0.000190	0.000108	0.000190
F-stat			51.22	18.00	51.22	18.00	51.22	18.00
P-value J-stat			0.0525	0.402	0.0490	0.400	0.0490	0.400
Observations	342	342	342	342	342	342	342	342

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The regressions are weighted by the size of every region-occupation-time cell. The dependent variable is the log ratio of average old and young worker wages in a region-occupation-age-time-specific cell and the explanatory variable is the log of the ratio between old and young employment inside the same cell. LM test is for underidentification test, F statistic is for weak identification test (Cragg-Donald or Kleibergen-Paap) and J test is for overidentification test. Time span covers from 1996-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Control function estimates of elasticity of substitution $\frac{1}{\lambda}$

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	Control Function (1)	(2)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.250***	-0.168***
	(0.0435)	(0.0509)
Residuals	0.209***	0.180***
	(0.0559)	(0.0568)
Skill x Time FE	NO	YES
Observations	342	342

Notes: All regressions include region-year fixed effects and region-skill fixed effects. The reported standard errors are clustered by region-skill. The regressions are weighted by the inverse of the wage ratio variance. Time span covers from 1996-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 9: Estimated old-young elasticity of substitution, $-\frac{1}{\lambda}$ by exploiting foreign employment as instrument

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.0893***	-0.0157	-0.298***	-0.0167
	(0.0322)	(0.0366)	(0.104)	(0.338)
Skill x Time FE	NO	YES	NO	YES
P-value Lm-stat			0.00981	0.415
F-stat			5.285	0.336
Observations	342	342	334	334

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The dependent variable is the log ratio of average old and young worker wages in a region-occupation-age-time-specific cell and the explanatory variable is the log of the ratio between old and young employment inside the same cell. The instrument is the log oh the total region-occupation-time specific foreign employment. LM test is for underidentification test and F statistic is for weak identification test (Cragg-Donald or Kleibergen-Paap). Time span covers from 1996-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 10: Estimated old-young elasticity of substitution, $-\frac{1}{\lambda}$, by exploiting temporary laid-off workers as instrument

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.0915***	-0.0264	-0.268	0.0263
	(0.0310)	(0.0535)	(0.220)	(0.796)
Skill x Time FE	NO	YES	NO	YES
P-value Lm-stat			0.100	0.619
F-stat			1.055	0.106
Observations	342	342	322	322

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The regressions are weighted by the inverse of the wage ratio variance. The regression are run only on the manufacturing sector. The dependent variable is the log ratio of average old and young worker wages in a region-occupation-age-time-specific cell and the explanatory variable is the log of the ratio between old and young employment inside the same cell. The instrument is the log of the total number of temporary laid-off workers. LM test is for underidentification test and F statistic is for weak identification test (Cragg-Donald or Kleibergen-Paap). Time span covers from 1995-2004. * p<0.10, ** p<0.05, *** p<0.01

Table 11: Estimated old-young elasticity of substitution, $-\frac{1}{\lambda}$, by adding a linear trend to instruments

$ln(\frac{w_{rsOt}}{w_{rsYt}})$	OLS (1)	OLS (2)	LIML (3)	LIML (4)	2SLS (5)	2SLS (6)	GMM-CUE (7)	GMM-CUE (8)
$ln(\frac{L_{rsOt}}{L_{rsYt}})$	-0.0893***	-0.0157	-0.268***	-0.168***	-0.249***	-0.164***	-0.189***	-0.144***
	(0.0322)	(0.0366)	(0.0532)	(0.0556)	(0.0476)	(0.0541)	(0.0353)	(0.0406)
Skill x Time FE	NO	YES	NO	YES	NO	YES	NO	YES
P-value Lm-stat			0.00321	0.00808	0.00321	0.00808	0.00321	0.00808
F-stat			46.73	12.09	46.73	12.09	46.73	12.09
P-value J-stat			0.208	0.802	0.192	0.798	0.192	0.798
Observations	342	342	342	342	342	342	342	342

Notes: All regressions include region-year fixed effects and region-occupation fixed effects. The reported standard errors are clustered by region-occupation. The dependent variable is the log ratio of average old and young worker wages in a region-occupation-age-time-specific cell and the explanatory variable is the log of the ratio between old and young employment inside the same cell. LM test is for underidentification test, F statistic is for weak identification test (Cragg-Donald or Kleibergen-Paap) and J test is for overidentification test. Time span covers from 1995-2004. * p<0.10, ** p<0.05, *** p<0.01

Appendix A

By defining labor gap for old and young workers as function of native and foreigners, we get

$$ln(L_{rsOt}) - ln(L_{rsYt}) = ln(f(L_{rsONt}, L_{rsOFt})) - ln(f(L_{rsYNt}, L_{rsYFt})) \tag{15}$$

Computing the differential, we get:

$$d(ln(L_{rsOt}) - ln(L_{rsYt})) = d(ln(f(L_{rsONt}, L_{rsOFt})) - ln(f(L_{rsYNt}, L_{rsYFt}))) =$$

$$= \frac{1}{L_{rsOt}} \frac{\partial L_{rsOt}}{\partial L_{rsONt}} dL_{rsONt} + \frac{1}{L_{rsOt}} \frac{\partial L_{rsOt}}{\partial L_{rsOFt}} dL_{rsOFt} - (\frac{1}{L_{rsYt}} \frac{\partial L_{rsYt}}{\partial L_{rsYNt}} dL_{rsYNt} +$$

$$+ \frac{1}{L_{rsYt}} \frac{\partial L_{rsYt}}{\partial L_{rsYFt}} dL_{rsYFt}) \tag{16}$$

Labor ratio differential in discrete is:

$$\Delta(ln(L_{rsOt}) - ln(L_{rsYt})) = \Sigma_c(\beta_{Oc} \frac{\Delta L_{rsOct}}{L_{rsOt}} - \beta_{Yc} \frac{\Delta L_{rsYct}}{L_{rsYt}}) \qquad c = N, F \tag{17}$$

where

$$\beta_{Oc} = \frac{\partial L_{rsOt}}{\partial L_{rsOct}} \qquad \beta_{Yc} = \frac{\partial L_{rsYt}}{\partial L_{rsYct}} \tag{18}$$

with c indicating if they are natives or foreigners.

Appendix B

By assuming that the residuals, x, obtained by applying Frisch–Waugh–Lovell theorem to labor gap first difference, follow an AR(1) process:

$$x_{rst} = \rho x_{rst-1} + \varepsilon_{rst} \tag{19}$$

By subtracting x_{rst-1} on both sides we obtain:

$$\Delta x_{rst} = (\rho - 1)x_{rst-1} + \varepsilon_{rst} \tag{20}$$

By substituting the first difference in the labor demand we have:

$$ln(\frac{w_{rsOt}}{w_{rsYt}}) = \beta \Delta x_{rst} + u_{rst} \tag{21}$$

Where y_{rst} is the residuals from wages by using the Frisch–Waugh–Lovell theorem and t goes from 2 to T. We omit to multiply the parameter from the first stage regression.

$$\operatorname*{plim}_{N\to\infty} \widehat{\beta} = \beta + \frac{\text{plim}_{N\to\infty} \frac{1}{N}\Sigma_{i=1}^{N}(x_{rst} - x_{rs.})(u_{rst} - u_{rs.})}{\text{plim}_{N\to\infty} \frac{1}{N}\Sigma_{i=1}^{N}(x_{rst} - x_{rs.})^2} \tag{22}$$

$$\operatorname*{plim}_{N\to\infty} \widehat{\beta} - \beta = \frac{\text{plim}_{N\to\infty} \frac{1}{N}\Sigma_{i=1}^{N}(x_{rst} - x_{rs.})(u_{rst} - u_{rs.})}{\text{plim}_{N\to\infty} \frac{1}{N}\Sigma_{i=1}^{N}(x_{rst} - x_{rs.})^2} = \frac{A}{B} \tag{23}$$

$$A = \underbrace{E[x_{rst}u_{rst}]}_{\text{endogeneity bias}} \underbrace{-E[x_{rst}u_{rs.}] - E[x_{rs.}u_{rst}] + E[x_{rs.}u_{rs.}]}_{\text{Nickell bias}} \tag{24}$$

$$B = E[x_{rst}^2] - 2E[x_{rst}x_{rs.}] + E[x_{rs.}^2] \tag{25}$$

Let u_{rst} equals to the sum of a random effect and a *offsetting mechanism*:

$$u_{rst} = \xi_{rst} + f(\varepsilon_{rst-1}) \tag{26}$$

Let $f(\epsilon_{rst-1})$ linear and function of labor supply shock at t-1:

$$f(\epsilon_{rst-1}) = \varepsilon_{rst-1} \tag{27}$$

Let ρ is equal to one:

$$\Delta x_{rst} = \varepsilon_{rst} \tag{28}$$

Then A turns into:

$$A = E[\varepsilon_{rst}\varepsilon_{rst-1}] - E[\varepsilon_{rst}\varepsilon_{rs.-1}] - E[\varepsilon_{rs.}\varepsilon_{rst-1}] + E[\varepsilon_{rs.}\varepsilon_{rs.-1}] \tag{29}$$

Showing the time means of A:

$$A = E[\varepsilon_{rst}\varepsilon_{rst-1}] - \tfrac{1}{T-1}E[\varepsilon_{rst}\textstyle\sum_{j=2}^{T}\varepsilon_{rsj-1}] - \tfrac{1}{T-1}E[\textstyle\sum_{j=2}^{T}\varepsilon_{rsj}\varepsilon_{rst-1}] + \frac{1}{(T-1)^2}E[\sum_{j=2}^{T}\varepsilon_{rsj}\sum_{j=2}^{T}\varepsilon_{rsj-1}] \tag{30}$$

Solving covariates:

$$A = 0 - \frac{1}{T-1}\sigma_\varepsilon^2 - \frac{1}{T-1}\sigma_\varepsilon^2 + \frac{1}{(T-1)^2}(T-2)\sigma_\varepsilon^2 \tag{31}$$

$$A = -\frac{T}{(T-1)^2}\sigma_\varepsilon^2 \tag{32}$$

Doing the same for B:

$$B = E[\varepsilon_{rst}^2] - 2E[\varepsilon_{rst}\varepsilon_{rs.}] + E[\varepsilon_{rs.}^2] \tag{33}$$

$$B = E[\varepsilon_{rst}^2] - \frac{2}{(T-1)}E[\varepsilon_{rst}\sum_{j=2}^{T}\varepsilon_{rsj}] + \frac{1}{(T-1)^2}E[\sum_{j=2}^{T}\varepsilon_{rsj}^2] \tag{34}$$

$$B = \sigma_\varepsilon^2 - \frac{2}{T-1}\sigma_\varepsilon^2 + \frac{1}{(T-1)^2}(T-1)\sigma_\varepsilon^2 \tag{35}$$

$$B = \sigma_\varepsilon^2\frac{T-2}{T-1} \tag{36}$$

Computing the ratio between A and B:

$$\frac{A}{B} = -\frac{T}{(T-2)(T-1)} \tag{37}$$