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Back to Black?

**The Impact of Regularizing
Migrant Workers**

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Back to Black?

The Impact of Regularizing Migrant Workers

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Back to Black?

The Impact of Regularizing Migrant Workers*

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Abstract

This paper provides a firm and individual level analysis of the impact on labor market outcomes of regularizing undocumented migrant workers. Using unique administrative data released by the Italian Social Security Institute, we evaluate Italy's largest ever regularization process. We employ an unexpected quasi-random auditing program to deal with firms' self-selection into treatment. Our results show that regularization has only a short-run positive impact on firm employment and no effect on firm-level wages. Nonetheless, 73.5% of regularized migrants remains within the formal Italian labor market, and we find also that legalized migrant coworkers were not affected (negatively) by the reform. Our findings highlight that high mobility of migrants to other firms, provinces and industries is an important driver of our results.

Keywords: Migration, Legalization, Shadow Economy, Tax Compliance, Policy Evaluation

JEL codes: J6, H26, O17

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1 Introduction

While most economics research on migration focuses on the impact of new inflows of migrants on natives' labor market outcomes (Borjas, 2014; Card and Peri, 2016), regularization policies apply to migrants who are already present and active in the native labor market, and who due to legislative constraints have been forced into the informal labor market, increasing the size of the shadow economy. Undeclared work is an immediate employment opportunity for irregular migrants, and especially during periods of mass migrations such as the refugee crisis which has been affecting Europe in recent years.¹

In recent decades, many Western countries have implemented legalization policies such as the Border Security, Economic Opportunity, and Immigration Modernization Act of 2013 (S.744) in the US, the Zapatero Reform of 2006 in Spain, and the Greek Law no. 2910/2001. Mass migrant legalization increases the labor supply for the formal labor market. There are some recent studies which suggest that labor supply shocks can have (ex ante) ambiguous effects on native labor market outcomes due for instance to complementarity/substitutability among heterogeneous labor inputs, endogenous technological change and differences in task specialization (see among others Clemens et al., 2018; Ottaviano and Peri, 2012; Lewis, 2011; Peri and Sparber, 2009). Therefore, it is crucial to understand the impact of legalization policies on labor market outcomes. However, the literature includes only a few empirical papers. These few include a paper by Elias et al. (2018) which investigates the effects of Spanish regularization in 2006 making use of province level data, and a study conducted by Devillanova et al. (2014) which investigates in the city of Milan in 2002 the impact of prospective legal status on employment. In addition, there are a few theoretical papers that examine the effect of migrant legalization (Chassamboulli and Peri, 2015; Casarico et al., 2018, among others), and few papers on the impact of legalization on other outcomes such as crime (Mastrobuoni and Pinotti, 2015; Pinotti, 2017) or consumption (Dustmann et al., 2017).

¹Most legislations do not allow migrants to reside in their countries without regular resident permits, which are available only to immigrants who are legally employed. However, the decision to migrate is rarely influenced by the legislation in the destination country (Orrenius and Zavodny, 2003).

We contribute to the literature making use of very refined longitudinal microdata at the firm and individual levels which allow us to investigate the impact of mass legalization on the employment and wage dynamics in regularizing firms, and on the legalized migrants' career paths. We also investigate the effects on coworkers' careers. Our contribution is related also to the identification strategy, using a quasi-random variation related to an unexpected auditing program to deal with firms' self-selection.

We focus on the largest regularization policy in Italy, implemented in 2002 (D.l. 195/2002), and approved as part of a wider reform, L. 189/2002, also known as the *Bossi-Fini* Law). The objective of this policy was to provide incentives for firms and undocumented migrants to move from the informal to the formal labor market through the provision of work-residence permits. Law 189/2002 was approved in July 2002 but the regularization for private employees was not introduced until September 9 2002 with D.l. 195/2002, which became effective the following day.² The regularization procedure resulted in around 700,000 applications, and induced a sizeable drop (17%) in the rate of undeclared work between 2001 and 2003, in a country characterized historically by a large shadow economy (Figure 1).

The Italian labor market is an excellent case for studying this type of policy. The increase in immigration is quite recent, and has been rapid and characterized by high ethnic heterogeneity and low levels of education (Barone et al., 2016; De Arcangelis et al., 2015). According to Eurostat, the Italian immigrant population increased from 1.7% in 1998 to 8% in 2012. This increment, associated to an ineffective assimilation policy for illegal migrants, has led to an increase in differential labor market dynamics between regular and irregular migrants.

For our empirical analysis, we make use of recently available data issued by the Italian Social Security Institute (*INPS*). We merge the universe of firms in the private sector (excluding agriculture) with the universe of individual career histories over the last 30 years (roughly 15 million jobs per year). Moreover, we add to this dataset the universe

²As discussed in Section 3, the timing of the parliamentary approval of the Law helps us to rule out the possibility of an anticipation effect.

of firm level audit programs (more than 30,000 inspections per year).

We exploit the richness of our data and the specific features of the regularization to identify a causal impact. We believe that to design a reliable empirical strategy in this setting requires an analysis of the employers' incentives to undertake the regularization, an issue that is usually undervalued in legalization studies. This setting has some features in common with a tax amnesty: under-declaring workers can be considered a way to evade taxes. The choice of an employer to declare illegal migrant workers depends directly on her propensity to bear the risks of evading social security payments and being discovered by the tax enforcement authorities. Subscribing to the regularization allows the employer to pay a lump sum fee which is lower than the regular social security costs.³ Furthermore, regularization might lower the risk of the employer being inspected (and fined) in the future for employing illegal migrants.⁴

Firms might self-select into regularization based on firm unobservable characteristics. To deal with self-selection we exploit an unexpected auditing program (program *ex lege 383*), enforced around the time of implementation of the regularization policy, as an instrument for the firm's selection into treatment. We provide evidence that this program constitutes a quasi-random variation in the probability of inspection, exogenously increasing the expected fine related to the employer's labor tax evasion, and thus changing the firms' willingness to take up the amnesty.

We regress employment and wage dynamics on being a treated firm, i.e. undertaking the amnesty, using an IV approach. As instrument we employ the exposure to the additional auditing program in the same province and industry. We include firm fixed effects and standard inspections in $t-1$ to control respectively for unobserved heterogeneity and firms' expectations about the auditing probability.

³Employers who had employed irregular migrant worker for at least 3 months were asked to pay an amnesty fee of 700 Euro per worker. Based on the average wage for a blue collar worker in Italy, three months of social security payments (around 1600 Euros) would greatly exceed 700 Euros.

⁴As in other legislations, in Italy a tax payer that is amnestied for a specific tax period can be re-inspected but the inspection excludes tax periods subject to previous amnesties. Therefore, a tax authority that wants to maximize expected fines will have little incentive to inspect a firm audited in the past.

Results at the firm level show that the regularization has a positive causal impact on firm employment in the short run (May 2002 to December 2002) but that the effect disappears in the medium (May 2002 to May 2003) and long run (May 2002 to September 2003). We detect no significant causal impact on average wages at the firm level. We conduct a set of robustness checks confirming our main results, and we additionally point out that, at firm level, employment of native workers is not affected by the regularization.

Based on these firm level findings, it could be concluded that the policy was ineffective in permanently increasing regular employment, and that regularized migrants go *back to black*, i.e. return to the shadow economy. However, the individual level analysis provides contrasting results. We find evidence that on average, 73.5% of regularized migrants remained attached to the Italian formal labor market (more than) four years after regularization although not necessarily in the original regularizing firm: after four years, only 17.6% of regularized workers were employed in the same firm. We find also that this attachment to the labor market is not at the expenses of the migrants' coworkers, who do not experience neither higher job-separation rates nor longer spells of non-employment.

A possible explanation for our findings might be related to the higher levels of spatial and industrial mobility of migrants with respect to natives, which allow regularized migrants to fill vacancies in different industries and provinces left unfilled by less mobile native workers. We provide convincing evidence that regularized migrants are far more mobile than native workers along many dimensions (firm, province, industry). This evidence is in line with the findings in Cadena and Kovak (2016) for the US labor market, and to our knowledge is the first evidence for a European labor market.⁵

Our main findings are confirmed in a second causal analysis. Specifically, we derive similar findings using a differences-in-differences methodology comparing regularizing firms with a group of firms that applied for regularization but ultimately did not hire any new migrant workers. We discuss why these firms represent an appropriate control group; they display a comparable pre-trend in terms of employment with respect to regularizing

⁵A recent NBER working paper by Basso et al. (2018) carry out a macro analysis in the same vein of Cadena and Kovak (2016) for Europe at the regional level, with consistent results.

firms, and expressed a willingness to adopt the policy.

The paper is organized as follows: in Section 2, we provide a short review of the related literature, in Section 3 we describe the Italian institutional background and the regularization program, in Section 4 we describe the data and in Section 5 we introduce our identification strategy. Sections 6 and 7 report the results of the firm and individual level (migrants' careers and impact on coworkers) analyses. Section 8 discusses the results of our second causal analysis and Section 9 concludes.

2 Related literature

The economic literature says little about the impact of a mass regularization in the domestic economy. Elias et al. (2018) investigate the consequences of legalizing some 600,000 immigrants in Spain implemented by the Zapatero government in 2004. Using data on payroll-tax revenues at the province level, they estimate that each newly legalized immigrant increased local social-security revenues by an average 3,504 Euros. They estimated also that the policy change reduced the labor-market outcomes of low-skilled natives and immigrants but improved the outcomes of high-skilled workers. Further, after correcting for internal migration and selection, for each newly legalized immigrant they identified an increase in payroll-tax revenues of 4,368 Euros, i.e. 25% higher than the raw payroll-tax revenue data estimates.

Devillanova et al. (2014) explore the impact of the prospect of legal status on employment opportunities: they exploit the introduction of decree 195/2002, which is the same studied in this paper, as an exogenous variation in eligibility to apply for a residence permit, based on the arrival date in Italy of irregular migrants.⁶ They use survey data for the city of Milan and find that eligibility for the amnesty significantly increased immigrants' employment probability.⁷

⁶Only immigrants who had been in irregular employment for at least 3 months were eligible.

⁷Other work on the US shows that the employment prospects of newly legalized immigrants improve, e.g. Amuedo-Dorantes and Bansak (2011), Amuedo-Dorantes et al. (2007), and Cobb-Clark et al. (1995) for the impact of the IRCA amnesty program, and Kaushal (2006) for the effects of the NACARA act.

While few empirical evaluations of legalization policies are available, there are some theoretical papers on this issue. Among others, Chassamboulli and Peri (2015) make use of a model where illegal immigrants are associated to the worse outside option, followed by legal immigrants and then natives. Hence, firms in the receiving country can cut labor costs by hiring illegal migrants at lower wages, and as a consequence they might be willing to post more job openings even for unskilled native workers. Chassamboulli and Peri (2015) also address the impact of different policies to reduce illegal migrants, such as increased deportation, stricter border control, and legalization. According to the model calibration, legalization is the only policy that produces a positive effect on wages and employment of skilled natives and a positive effect also on unskilled native employment. Another recent theoretical paper is by Casarico et al. (2018), who investigate the trade-offs faced by politicians in the decision to support the introduction of an immigration amnesty. They show that an amnesty is more desirable the more restricted are the occupational opportunities of illegal immigrants and the smaller is the fiscal leakage via the welfare state.

Other papers have also investigated the impact of legalization on other outcomes, such as crime. Exploiting the administrative procedure to apply for a residence permit in place in Italy in 2012, Pinotti (2017) finds that receiving a residence permit significantly decreases the probability of committing crimes. The author argues that the higher employment opportunities of regular migrants are the driver of this results, increasing the crime opportunity cost. Mastrobuoni and Pinotti (2015) point out that obtaining legal status reduces the recidivism rate, mainly in local labor market characterized by better labor market opportunities. As for the relation between migrant legalization and consumption, Dustmann et al. (2017) point out that undocumented immigrants consume about 40% less than documented immigrants, and only one quarter of this decrease is explained by lower incomes.

Our paper is related also to other strands of literature, and in particular those on tax evasion and tax enforcement. Our identification strategy exploits an exogenous variation

in the firm's audit probability which increases the firm's propensity to undertake regularization. It draws on work by Almeida and Carneiro (2012): using Brazilian data, they find that stronger enforcement decreases the size of the informal labor market. We show that stronger enforcement does increase the probability that an employer participates in the regularization program.

In our framework, legalization of a migrant worker passes through an amnesty on labor tax evasion. The behavioral mechanism underlying our identification strategy relies on Snow and Warren (2007), where the firm's decision about workers' informality is the result of the firm's expected-utility maximizing behavior given its expectations about the auditing probability, which the employer updates in each period based on past audit experience.

3 Institutional background: the *Bossi-Fini* Law (L. 189/2002)

In 2002, the Italian parliament passed a law (Law 189/2002, known also as *Bossi-Fini* after the ministers who drafted it) which changed the definition of non-EU migrants' legal status in Italy. After the law was approved, new migrants could enter Italy only if they had a regular job contract. The duration of migrant residence permits was reduced with respect to previous rules. Alongside these reforms, other restrictive procedures were strengthened and some new measures related to security were introduced. This law made Italy one of the first European countries to require immigrants applying for residence permits to provide their fingerprints. Overall, public opinion and media perceived the new law as tightening the legislation regulating the legal status of migrants, and restricting the possibilities for new entries in Italy.

In addition to the set of rules for new arrivals, law 189/2002 also addressed the issue of irregular migrants already present in Italy in 2002. The political message was to achieve a massive regularization in 2002 and a strong reduction over time of (regular and irregular)

new migrant inflows. In this framework, the law announced an amnesty (D.l. 195/2002) that allowed all migrant workers informally employed for at least three months to formalize their status.

To subscribe to the regularization, employers were required to (i) declare irregular employment of a worker for at least three months before the date of the regularization law (September 2002), (ii) pay a lump-sum (700 euros) to cover social security contributions evaded before the regularization, and (iii) hire the irregular migrant on a minimum one-year contract. The amnesty also covered domestic workers and resulted in the largest regularization in Italian history, with more than 600,000 migrants being regularized and a sharp reduction in the amount of undeclared work (see Figure 1).

It could be argued that there was an anticipation effect influencing the strategies of economic agents involved in the regularization. However, the timing of the parliamentary process for L. 189/2002 allows us to claim that an anticipation effect could not be at work. On February 28, 2002, the first draft of the law was approved in Senate. This first version included an amnesty limited strictly to family caregivers, excluding private sector employment. On June 4, 2002, the Lower Chamber began discussing the possible extension to the private sector. However, even when the law was finally approved on July 11, government restricted regularization to family caregivers only. It was not until a supplementary decree, D.l. 195/2002 approved on September 9, 2002, came into force on September 10, that government decided to introduce regularization for dependent workers in the private sector. Hence, up to September 2002 the regularization was limited to family caregivers, and public opinion and the media focused mainly on the core of the *Bossi-Fini* Law, i.e. tightening the conditions for obtaining a residence permit in Italy, and strengthening the security measures related to illegal immigration. As we show later, since we consider May 2002 as the pre-treatment period, we can be confident that at that time the regularization applied only to domestic workers and could not have affected the decisions of entrepreneurs, workers and migrants in the private sector.

Figure 2 shows the number of non-EU dependent workers in the private sector: the

2002 regularization resulted in the largest increase (more than 50%) in the time span investigated.⁸

4 Data

We build a unique dataset combining different sets of data provided by INPS and recently available via the VisitINPS program. The main information comes from the registries of firms and workers, and the O1M archives. The first two archives contain firm demographics (industry, date of constitution, location, etc.) and worker demographics (dates of birth, nationality, gender, etc.); the O1M archives collect firm communications with INPS at the worker level and are available from 1983 to 2016, including monthly information on employment dynamics. These communications report information on job contracts (full time/part time, permanent/fixed term, wages including bonuses and premia, occupation -i.e. blue collar, white collar, etc.) and labor supply information (i.e. weeks worked). By exploiting the longitudinal dimension of these data, we can reconstruct the work histories of each individual in the private sector. Also, since it is an employer-employee data-set, we can derive firm level longitudinal outcomes in terms of employment and wage dynamics, workforce composition (blue vs. white collar), hirings and dismissals, earnings structure and survival rates.

Firms willing to subscribe to the 2002 amnesty were required to request INPS for preliminary authorization, and to declare their potential interest in the regularization process. These firms were identified by a specific authorization code (“0U”) reported in the Social Security registry. When the amnesty became operational, all authorized firms were required to declare to INPS how many workers they hired under the regularization; this information is available in another archive (DM10) which we linked to our data. Not all authorized firms reported regularized workers. In our empirical analysis, we identify a treatment group as all firms that obtained authorization (those identified by the code

⁸We observe another jump in 2007, linked to Romania and Bulgaria entering the EU but the size of that increase was half the rise in 2002.

“0U”) and in 2002 reported at least one regularized worker.⁹

We also exploit data on the universe of auditing activities performed by INPS in the private industry (excluding agriculture) between 2000 and 2004 (VG00 archives from INPS); these data are collected at the audit level and include some information on the inspected firm, the characteristics of the audit, and its outcome (length of inspection, amount of under-reported taxes, number of undeclared workers identified). We can thus link the auditing data to the firm level data to identify all firms audited between 2000 and 2004. This information allows us to build measures of the local/industry level auditing probability.

Our firm level sample includes all firms that were active in 2002. We construct a short panel, using information from 2001 to 2004. The data allow us to derive measures for monthly level employment, hirings, dismissals and total wages paid by the firm, distinguishing among workers’ nationality, occupation and contract type (part time, temporary, etc.). To avoid possible selection problems in both the treated and control groups, we focus only on firms established before 2002. In particular, Law 195/2002 provided the possibility to regularize workers employed in economic activities not legally constituted as firms; in this case, the opening of a position for the firm at INPS and the worker regularization process were contemporaneous, and the firm was assigned a default date of constitution. The behavior and rate of mortality of these “black” firms are peculiar and hard to compare with other firms; so we prefer to exclude them from the main analysis. This leaves a sample of around 1,200,000 firms active in 2002, among which we can identify 60,472 treated firms.¹⁰

To identify regularized workers, we isolate all non-EU migrant workers in the treated firms who were hired between September and December 2002. We define them as regular-

⁹In Section 8, we report some additional results based on a control group of all authorized firms that did not declare any regularized workers.

¹⁰From the 98,000 firms that undertook the amnesty, around 20,000 were undeclared firms that have been regularized entirely (and which are not considered in this paper). After selecting those firms that were active in 2002 and 2001 and after dropping some outliers, we are left with 60,472 firms for which we have longitudinal data. Specifically, we exclude firms in the 99th percentile for number of employees in May 2002, and firms in the 1st and 99th percentiles for changes in employment between May and December 2002.

ized if we do not observe them working in the regularizing firm during the previous three months (as required by Law 195/2002).¹¹ We define coworkers/colleagues as all the other workers employed in the regularizing firms between September and December 2002.

5 Identification strategy

Let us consider an economy where T_i is a binary treatment variable at firm level, and takes the value 1 if the firm undertakes the regularization, so that firm i has only two potential outcomes: $Y_{i,1}$ and $Y_{i,0}$. The matrix X_i includes observable firm characteristics. In this scenario, the relation between regularization and firm outcome can be expressed through a linear estimation equation:

$$Y_i = \beta_0 T_i + \beta_1 X_i + \varepsilon_i$$

where $cov(T_i, \varepsilon_i)$ might be different from zero for several reasons, eventually implying a biased estimate of the coefficient β_0 . First, firms have different unobserved endowments of illegal migrants before the policy, so that only some firms are eligible for the regularization. Second, employers have different taste for evasion which is relevant in our setting since, as already noted, employing a certain number of irregular workers can be considered similar to evading social security payments and labor taxes, i.e. the number of illegal workers employed in a firms maps directly on to the intensive margin of the evasion choice. Third, employers have different taste for risk which is an important variable since the decision to evade is always anticipated by an evaluation of the risks related to this choice, i.e. the probability of being audited by the tax authorities. Lastly, firms have heterogeneous characteristics, some of which cannot be observed by the econometrician.

Given this setting, employers choose to evade payment of social security contributions, i.e. employing a specific number of illegal migrants, evaluating their beliefs about the level of the expected audit fine which, as in the standard Allingham-Sandmo approach,

¹¹We can compare these numbers derived using this indirect identification with the numbers of regularized workers declared by the firms in the DM10 archive: reassuringly, they are highly consistent.

depends on the probability of being audited and the amount of the fine: $E[\textit{fine}] = f[P(\textit{audit}), \textit{fine}]$. To obtain an unbiased estimate of β_0 would require the regularization choice of (at least) some firms to depend on exogenous factors Z_i such that $\textit{cov}(Z_i, \varepsilon_i) = 0$. Indeed, a policy that exogenously shocks the employer’s beliefs about tax enforcement, $E[\textit{fine}]$, will affect the employer’s decision to undertake the amnesty.¹² The best candidate for Z would be an unexpected auditing program with quasi randomly assigned inspections.

In 2001, more than one year before the *Bossi-Fini* law, the Italian government enacted a program of fiscal incentives for firms to exit from the shadow economy aimed at reducing undeclared work (L. 383/2001). The program targeted all irregular workers, irrespective of citizenship, provided they were legally residing in Italy: irregular migrants were not eligible. Overall, L. 383/2001 was unsuccessful since it attracted the participation of only a very small number of firms.¹³ More important for our analysis is that L. 383/2001 established an additional inspection plan, with respect to the standard national inspection plan carried out by INPS. This represented a unique exception in the design of national auditing activities over the last decades. While these additional inspections should have started in January 2002, the criteria were not defined until May 2002 (CIPE 36/2002), and the audits began in July. Figure 3 summarizes the timing of the policy interventions. In July 2002, the *Bossi-Fini* Law was approved by the Italian Parliament. In September, D.l. 195/2002 introduced regularization for dependent workers but the rules for application were not announced by INPS until October (C. 161/2002), further reducing the regularization process time window.

It is important to underline that while the normal inspection activity is managed only by INPS at the national level via its *Direzione Centrale Vigilanza* department, the government decided that the additional inspection program related to L. 383/2001 should

¹²A similar setting for the decision to undertake tax evasion is proposed by Snow and Warren (2007), using a Bayesian updating model.

¹³From our administrative data, we can identify 1,073 firms involved in the program. Section 6.4 includes a robustness check to corroborate this descriptive evidence. The main reason for this lack of success was the uncertainty over implementation of the Law (the deadline and procedures were redefined several times).

be organized according to different criteria. The decision making process was decentralized at the regional level and several new institutions were involved in designing the additional plan (regions, the Fiscal Authority, the Inter-ministerial Committee, relevant ministries, etc.¹⁴). Both the decentralization process and the involvement of new actors rendered the additional inspection program completely different from the standard inspection plan. This resulted in a strikingly different distribution of the inspection program *ex lege 383* with respect to the standard INPS auditing activity.

In this framework, we contend that there are two main reasons why this additional inspection program can be considered an exogenous shock to the firm's expected fine in the identification strategy. First, the inspections *ex lege 383* determined an increase in the overall number of inspections, as shown in Figure 4: in most regions, while the number of standard inspections remained rather stable, the total number of inspections increased sharply as a result of the additional inspections. Second, since the criteria for the standard auditing plan were set by a different decision maker (the aforementioned local committees), the distribution and characteristics of the inspections changed. Figure 5 reports the incidence of inspections in the region over total national inspections: while the distribution of standard inspections is the same for the years 2001 and 2002, the regional distribution of inspections *ex lege 383* differs substantially. This applies also to the distribution at the industry level, as shown in Figure 6: in most regions, standard inspections were distributed fairly evenly across industries in 2001 and 2002 but the relative distribution of inspections *ex lege 383* was different.

Differences emerge also in relation to the characteristics of the audits (see Table 1). First, as already noticed, distribution of the program across industries *ex lege 383* varies dramatically from the distribution of the standard audit program, with a higher incidence of the *ex lege 383* program in manufacturing and sales and lower incidence in construction and hotels and restaurants. Second, the incidence of audits with no irregularities in the *ex lege 383* program is much higher than in the standard program (60% vs. 40-43%),

¹⁴Regional INPS departments were also involved in the decision process but there was no explicit coordination among the different regional committees

and the proportion of audits resulting in the employer being fined is much lower (28% vs. 43%). Also, even if a fine is imposed, it is less severe than in standard inspections (669 euros vs. 6,000-7,000 euros).

The validity of our instrument relies on the incidence of these unexpected audits. We assume that the number of inspections *ex lege 383* at the local level in a given industry provides an exogenous shock to the perceived audit probability, and through this channel, to the firm's decision to subscribe to the regularization. In our baseline specification, we define our instrument for a given firm i by summing the *ex lege 383* inspections for firm j , with $j \neq i$, in cells c identified by the interaction between (110) provinces and (88) 2-digit NACE industries:¹⁵

$$insp383_{i,t|i \in c} = \sum_{j \in c, j \neq i} insp383_{j,t}$$

This definition helps to eliminate any possible impact of the characteristics of firm i at time t on the aggregate (at cell level) value, since whether firm i is inspected is not considered in the construction of the instrument.

Figure 7 shows that the proportion of inspections *ex lege 383* implemented in a cell is uncorrelated to the proportion of standard inspections in the same cell in 2001 (observations are weighted by cell size). Figure 8 provides supporting evidence for the relevance of our instrument: the unexpected auditing program *ex lege 383* resulted in an increase in the number of inspections in most of our cells, and where the increment is larger we observe a higher proportion of regularizing firms (larger bubbles in the figure).

To achieve a credible identification using this type of instrument we need also to control for the firm's beliefs about the expected fine, to take account of the heterogeneous propensity of a specific firm to evade in this specific auditing setting. Also, including firm fixed effects allows us to control for all time invariant firm characteristics.

After defining all the elements in our identification strategy, we can summarize our econometric model as follows:

¹⁵A robustness check on the definition of cells is provided in Section 6.4.

$$y_{i,c,t} = \beta_0 \widehat{T}_{i,c,t} + \beta_1 X_{i,c,t} + \beta_2 \text{belief}_{c,t-1} + \eta_i + \sigma_S \times \delta_t + \sigma_{PROV} \times \delta_t + \varepsilon_{i,c,t} \quad (1)$$

$$T_{i,t} = \gamma_0 Z_{i,t} + \gamma_1 X_{i,c,t} + \gamma_2 \text{belief}_{c,t-1} + \eta_i + \sigma_S \times \delta_t + \sigma_{PROV} \times \delta_t + v_{i,c,t}$$

where c is the cell, i.e the group of firms in the same province and industry, $T_{i,c,t} = 1$ for employers that undertake the regularization in 2002, $X_{i,c,t}$ is a set of controls (age of the firm, size of the reference cell in terms of active firms), and η_i are firm fixed effects. We also include industry by year fixed effects and province by year fixed effects to capture time varying common shocks; $\text{belief}_{c,t-1}$ is the number of audits in the reference cell in $t-1$ and is a measure of the expected audit probability of the firms in the cell. Since both the treatment $T_{i,c,t}$ and the instrument $Z_{i,t}$ varies at firm level, we cluster the errors accordingly. Furthermore, any common shock affecting our observations at the industry or province level is captured by fixed effects interacted with the year dummies ($\sigma_S \times \delta_t$ and $\sigma_{PROV} \times \delta_t$).

6 Firm level analysis

6.1 Descriptive statistics: which are the regularizing firms?

Table 2 shows the distribution of treated firms across industries in 2002 (column 2), and compares it with all firms active in the Italian labor market in 2002 - both regularizing and non-regularizing (column 1), with firms that employed at least one migrant worker in May 2002 (column 3) and firms that were inspected and found to have at least one irregular worker (column 4).¹⁶ Column 5 reports the share of treated firms in the industry. We find that the industry share of treated firms does not map closely to the firm industry share: regularizing firms are concentrated more in industries such as construction (37.8 vs. 14.58), hotel/restaurants (14.95 vs. 8.43), and manufacturing (25.89 vs. 22.05). The

¹⁶The first four columns are computed as shares across industries, and hence the sum of all the shares is equal to 100.

higher incidence of treated firms in these industries can be explained, in some cases, by the higher incidence of firms employing migrants: in the construction sector, the share of firms that use migrants is 20% and the share of firms with irregular migrants is 21.27%, compared to the incidence of firms in this industry which is 14.58%. This evidence confirms that the regularization is associated to the presence of firms employing migrant workers and firms using undeclared workers.

Table 3 reports the same type of distribution for regions. It seems first that there is a clear regional divide related to the incidence of regularizing firms, with migrants concentrated in the Center-North of the country. However, the share of firms that use irregular workers is relatively higher in the South, confirming the higher incidence of undeclared work in the Southern regions (column 4). In addition to this regional divide, there is a higher incidence of treated firms in Lazio, Lombardia, Piemonte, and Veneto.

Table 4 refers to the same distribution of firms over municipality size classes, and suggests that the distribution of regularizing firms resembles the distribution of firms at the municipality size level: urban patterns seem not to play a major role, as shown by the last column for within city size incidence. Also, the incidence of firms with irregular migrants is under represented in big cities.

6.2 Descriptive statistics of the outcome variables

Table 5 reports the descriptive statistics for the main outcomes of interest, namely monthly employment and average wages. The upper part of the table shows the evolution of employment from May 2002 to December 2002, May 2003, and September 2003, for regularizing and non-regularizing firms. We consider May 2002 as the pre-treatment situation.¹⁷ We use December 2002 as the timing for the short term outcomes of employment and wages, since this was the first month after the end of the regularization window. The first two columns in Table 5 show that in May 2002, before the policy was implemented,

¹⁷We prefer to exclude the summer months which are peculiar in the Italian economy in terms of employment and wage levels. Further, as already mentioned, we are confident that using May 2002 rules out any possible anticipation effect of the policy.

regularizing firms were slightly larger in terms of employment than non-regularizing companies. In December 2002 treated firms experienced a non-negligible increase in numbers of employees (more than 1 worker on average, 30% in percentage terms), while in the control group, firm size remained stable. To evaluate the medium and long term effects we consider May 2003, one year after the pre-treatment situation, and September 2003, one year after the start of the regularization.¹⁸ In the last four columns of Table 5 we show that the employment levels observed for regularizing firms in December 2002 decreases after that date while it increases slightly in the case of the control firms.

In relation to wages, the lower part of Table 5 shows that in the pre-treatment situation wages were slightly higher in the control group, and this holds for subsequent periods, while the wages of regularizing firms declined over time, suggesting a less clear pattern between regularizing and non-regularizing firms in terms of wages. This descriptive evidence suggests a short run relation between the policy and employment but no relevant changes to wages.

Figure 9 shows monthly employment trends for the sample of regularizing and control firms. The graphical evidence confirms also that, despite a (minor) difference in levels prior to the regularization, the trend in employment is the same for both groups. Actually, the trends are parallel until June 2002, while in July and August 2002 there is little evidence of a possible anticipation effect in the treatment group, with a slight fall in firm size. When the regularization occurred, in September 2002, regularizing firms experienced an initial significant increase in employment followed by a decline, while employment was stable among the control group firms over the period.

6.3 Results

This section presents the econometric results from the OLS and the 2SLS identification strategies, and focuses on the impact of regularization on firm outcomes in the short (May-

¹⁸We cannot go beyond September 2003 in our regression analysis since we use panel estimation with all firms present in the database in 2001 and 2002. Hence, in our estimates we make a within comparison of a treated firm in 2001 and 2002. Going beyond September 2003 would imply that the treated firms observed in the pre-treatment period would enter in the treatment period in September 2002.

December 2002), medium (May 2002-May 2003) and long runs (May 2002 - September 2003), as in Equation 1. Table 6 reports the results for employment changes. The OLS estimates show a significant increase for regularizing firms in the short and medium terms, although at a decreasing rate, and a reduction in the long run; this would suggest that, as soon as the one-year contract restriction expires, regularized workers left the initial hiring firms, with an overall negative impact on firm employment one year after the policy. However, the IV results show that only the short run impact is significant. Participating in the regularization process increases firm employment by more than 2.5 units but only in the short run: the effect is not significant in the medium and long run. The relevance of our instrumenting strategy is confirmed by a KP test which is around 108.¹⁹

The impact on wages is coherent with the results for employment and the hypothesis that regularized migrants may have entered at a lower wage than that paid to incumbent workers (Table 7): the OLS estimates show a negative impact in the short and medium runs, and a positive effect over the long run, although the magnitude of the coefficients is small. However, the 2SLS estimation coefficients are largely non-statistically significant, suggesting that there is no causal impact on wages. Also in this case, the KP test is quite high, around 85.²⁰

We also examine the impacts on yearly hirings and firings in 2002 for migrants and natives separately (Table 8): while there are no significant effects on the flow of natives, we detect a positive increase in the probability of hiring a migrant for the treated firms but no effects for separations of migrants. This confirms that the channel affecting firm employment increases concerns hirings of migrants, consistent with the goal of the policy.

¹⁹The first stage results imply an increase in treatment of about 10%. More specifically, the estimated coefficient of γ_0 in Equation 1 is 0.00008 (t-statistics 10.53); the sample standard deviation of $Z_{i,t}$ (the number of 383 inspections in the reference cell of the firm, excluding its own inspection) is 29.987. The average value of $T_{i,t}$ is 0.025. Thus, a change of one standard deviation in the instrument implies a 9.2% change in the probability of treatment.

²⁰Note that the number of observations for the wage regressions is lower than in the employment case: if in one of the two years employment is equal to zero, the associated firm average wage is missing.

6.4 Firm level analysis: robustness check

To validate our findings we run a set of robustness tests presented in Tables 9 and 10. We report only the results for the 2SLS estimations which are our preferred specification.²¹

Table 9 refers to the robustness checks for firm employment dynamics. First, it could be argued that our results may reflect an increase in employment due to either the *Bossi-Fini* law or the delayed application of the previous program (Law 383/2001), which would violate the exclusion restriction that our instrument is affecting outcomes only via the 195/2002 regularization. If this were the case, the auditing program *ex lege 383* could have increased firm employment levels through the regularization also of native workers, leading to upward biased estimates. We deal with this issue in two ways. First, we replicate the analysis using firm employment of native workers as the outcome variable: column (1) of Table 9 shows that being a regularizing firm within the Bossi-Fini program has no impact on the number of the firm's native employees, consistent with the fact that the Bossi-Fini was related only to migrant regularization. The rationale for this is that the 383 amnesty resulted in very few worker regularizations, and applied only to Italian workers and migrants with residence permits. Moreover, this represents an interesting preliminary evidence that subscribing to the *Bossi-Fini* regularization has no (negative) impact on the firm's employment of native workers. Second, column (2) reports the results excluding firms that were inspected under the *ex lege 383* program; the results remain mostly unchanged.

Column (3) checks our identification strategy in relation to definition of the cell used to compute our IV. In the baseline estimates, cells are defined at the province-industry level: the results do not change if we define the cells at a finer geographical unit, i.e. the interaction between local labor markets (LLM) and industry at 2-dgt level.

Column (4) controls for the number of migrant workers employed in the firm in June to verify whether heterogeneity in exposure to migrants matters; column (5) introduces share of standard inspections in the cell in 2002 in the baseline estimates, under the

²¹Results of the robustness checks using the OLS estimates are available on request.

assumption that beliefs had to be computed at both time $t-1$ and t . In both cases, the main results are mostly confirmed. Finally, in column (6) we repeat our analyses using cross-sectional regressions, and including in the econometric specification all the firms present in our data in 2002, without firm fixed effects (all other features being equal to the specification in 1); we find that the short run impact remains positive, and slightly greater than the baseline.

The same robustness checks are performed for the wages regressions reported in Table 10; the results generally confirm that regularization had no effect on the monthly wages paid by the firm, as in the baseline estimates.

Since the cross-sectional and the panel results are similar, we can use the cross-sectional setting to move to longer run outcomes after September 2003. Table 11 reports the results for employment and wage dynamics up to May 2004, two years after the pre-treatment period. Columns (1) and (2) confirm the panel estimation effects: no impact on employment and wage dynamics for regularizing firms. Column (3) investigates an interesting related issue, i.e. whether being a regularizing firm has an impact on the probability of firm closure at time $t+1$. We find no significant impact of regularization on firm survival. Column (4) shows that regularizing firms also have a lower risk of being inspected in 2004: this confirms that for firms one of the incentives of participation in the regularization is reducing expected auditing probability in the future.

7 Individual level analysis

So far we have investigated firm features related to the regularization. The results from a policy perspective are disappointing; we identified only a very short run causal impact of regularization which dissipates after just one year of the policy. It could be argued that regularized workers returned to the informal labor market or left the country rendering the regularization policy ineffective for creating long term regular employment. In this section we move on analyzing the effect of the regularization on workers exploiting data

from INPS archives for the universe of workers. First, we analyze careers and survival rates of regularized migrants in the formal labor market. Second, we investigate the impact of regularization on the careers of regularized migrants' coworkers. To our knowledge, this is the first regulation analysis to exploit individual level data on the universe of workers.

7.1 Descriptive statistics: who are the regularized workers?

Table 12 compares regularized migrants who entered the formal labor market in 2002 with various groups of comparable workers such as migrants who appeared in INPS archive data for the first time as private employees in 2000 or 2001 and the stock of all migrants observed in the INPS archives as private employees in May 2002. The last column of Table 12 reports descriptive evidence for natives that entered INPS archive data for the first time in 2002.

If we compare the first two columns in the table we observe that the distribution by citizenship of regularized migrants is different from the distribution of migrants recorded in INPS archive data for the first time in 2000-2001 and the stock of migrants in 2002. The incidence of Romanians and Chinese workers in the regularized group is much higher (respectively 26% vs. around 11% and 11% vs. 5%). However, regularized Albanian, other Asian, Australian, and African workers are under-represented. This suggests a role of ethnic networks in affecting the success of a legalization policy. In relation to demographic characteristics, the incidence of men is much higher among regularized workers (85% vs. around 62% for the other groups of migrants), while the distribution by age is similar to the distribution of migrants hired in 2000-2001 and little different from the migrant distribution in 2002.

In relation to work characteristics, regularized migrants with respect to migrants hired in 2000-2001 include more blue collar workers (97% vs. 82%) and permanent contracted workers (91% vs. 71%), and a lower incidence of part-time workers (70% vs. 80%). In 2002, the yearly earnings of regularized migrants were much lower due mostly to the fact that because regularization started in September, on average they had worked for fewer

months compared to migrants hired in 2000 and 2001 (4 vs. 5.4). The monthly wages are instead similar but much lower than the average wages of the stock of migrants in 2002.

7.2 Survival rates of regularized workers: *Back to black?*

Interestingly, moving to the individual level analysis, the scenario derived in the firm level analysis completely changes. Figure 10 shows that the attachment of regularized workers to the Italian formal labor market is very high: 73.5% of them were still in regular employment in the Italian private sector four years after the policy. Also, this high survival rate can represent a lower bound since some regularized workers may have moved to self-employment, become entrepreneurs or entered agriculture. However, the differences are impressive in terms of survival rates in the same firm, province, region, and industry. The probability that a regularized worker leaves the regularizing firm is around 20% in the first year, and increases between 2003 and 2004 (after expiry of the one-year minimum contract imposed by the regularization): after four years, only 20% of regularized workers were still employed in their original firms. There is evidence also of high geographical mobility. Four years after the regularization, less than 50% (60%) of regularized workers are working in the province (region) of regularization. The figures are similar for industry mobility: less than 40% of regularized workers are working in the same industry after four years. To sum up, regularized workers' survival rates are very high, and they show high levels of geographical and industry mobility.

These high survival rates have important policy implications: regularizing workers do not return to the black economy; they remain attached to the formal labor market. Thus, regularization increases payment of social security contributions and the taxes paid by workers who in the absence of the regularization would have continued to work in the undeclared labor market. This shows that regularization can be considered similar to an active labor market policy for a specific group of workers, irregular migrants, and pushes them to remain in the legal labor market which if not at the expenses of migrants' coworkers, shows that regularization "greases the wheels" of the economy.

7.3 Regularization and the effects on coworkers

A hot topic in policy debate is the impact of migrants on destination country labor markets. The results of the firm level analysis provide some evidence by showing that undertaking regularization does not affect native employment (see column (1) Table 9). In this section, we exploit individual data on worker careers. In particular, in this section we focus on the impact of regularized migrants on (co)workers within the firm, which is the group that potentially is more exposed to the effects of regularization. We define coworkers as all individuals who were employed in a regularizing firm between September and December 2002. Exploiting INPS archive data on coworkers allows us to build career histories before and after the regularization window in 2002. The control group consists of workers employed in non-regularizing firms in the same period. We obtained a sample of around 270,000 coworkers (treated workers) and three million controls.

Our outcomes of interest are job separation (employment in another firm, or unemployment/exiting from the labor market) in the following year, the probability of exit from the sample in all subsequent years (to proxy for becoming long term unemployed), and length of non-employment spell (number of months when the worker does not appear in the INPS data on employees in the private sector).

We employ the same 2SLS econometric specification used in the firm level analysis, and consider the universe of workers in 2002 that were already present in the INPS data in 2001, to exploit the longitudinal dimension of our data. The intuition is the same as in the case of the firm level analysis: individuals working in firms that are more (less) exposed to higher incidence of unexpected audits related to the *ex lege 383* are more (less) likely to be exposed to the regularization. The specification can be written as follows:

$$y_{w,i,c,t} = \beta_0 \widehat{T}_{i,t} + \beta X_{w,i,c,t} + \eta_w + \delta_t + \varepsilon_{w,i,c,t} \quad (2)$$

$$T_{i,t} = \gamma_0 Z_{i,t} + \gamma X_{w,i,c,t} + \eta_w + \delta_t + v_{w,i,c,t}$$

where c is the employer cell, namely the group of firms active in the same province and industry, w is for the individual, i is the firm and t is the year. $T_{i,t}$ is equal to 1

if the employer regularizes some workers in 2002, $x_{i,w,c,t}$ is a set of controls (worker age, occupation - blue collar or white collar, firm size), η_w is individual fixed effect and δ_t is year fixed effect. As in the firm level analysis, we use as instrument Z inspections *ex lege* 383 at the province and industry (2 digit) levels in 2002, excluding the firm's own.

In the individual level analysis, we restrict our attention to firms with less than 15 employees which includes around 90% of Italian firms.²² We excluded from the analysis the tails of the monthly earnings distribution, i.e. the top and bottom 1%, and all individuals who worked less than two months between January and September 2002.

Table 13 reports the main descriptive statistics for our analytical sample. It can be seen that the rate of exit from the labor market is similar for coworkers and controls (0.08 vs. 0.1) but that the rate of job separation is higher for coworkers (26% vs. 12%), suggesting that working in a regularizing firm increases the probability of experiencing a separation.

Table 14 reports the results of the panel estimation on the probability of job separation and the duration of a non-employment spell between January and August 2003.²³ Note also that the weak instrument KP test is largely above 10. Interestingly, and consistent with the descriptive statistics, in the IV estimates coworkers are associated to a higher job-separation rate but the coefficient is non-statistically significant. The impact on the duration of non-employment spells is also not statistically significant. This is clear evidence that being the coworker of a regularized worker does not (negatively) affect the coworker's career.

We next conduct the cross-section analysis. The trade-off from moving to a cross-section specification is that although we cannot control for time invariant unobserved

²²This choice is based on the fact that the instrument is relatively stronger for firms with less than 15 employees, suggesting that the group of compliers is characterized mainly by small and medium size firms. Also, workers in this group of firms are more likely to be homogeneous and comparable. As a robustness check, we replicated the firm level analysis with the same sample restriction and the results did not change. Estimates are available upon request.

²³In the panel estimation, we do not include the September-December 2003 period since observations relative to $t - 1$ (2001) would be included in t in the treatment period. Also, we cannot use permanent exit from the INPS data in $t + 1$ as an outcome variable because this is always equal to zero in 2001. See the cross-section estimates for an analysis of these outcomes.

heterogeneity we have the relative advantage of studying both the probability to exit from the INPS archive data and the long term outcomes variables after September 2003. We consider job separation in 2003, exit from the INPS archive data in 2003 and 2004, and cumulated spells of non-employment (expressed in months) in 2003 and 2003-2004. Table 15 shows that coworkers show higher (by 21%) job-separation rates in 2003. Since in the panel estimates this impact was not statistically significant, it could be argued that it is strongly correlated to unobserved heterogeneity which we do not control for in the cross-section estimates. Moreover, in the cross-section estimates the coefficient of exiting the labor market in the years after regularization is not statistically significant. Similarly, the coefficients of cumulated non-employment spells in 2003 and 2003-2004 are not significant. This suggests that although having a regularized coworker might increase job-separation over time, it is not associated to longer spells of non-employment or a higher incidence of exiting permanently from the labor market. In fact, working in a regularized firm implies only higher mobility across firms.

Table 16 reports the same analysis for the sample of blue collar workers. There are at least two reasons why the impact of regularization on coworkers might be different in the case of blue collars. First, the literature suggests that in developed countries such as Italy the impact of (largely low-skilled) immigration tends to be stronger for workers in low-skill jobs. Second, the incidence of blue collars among regularized migrants is much higher than the incidence among migrants and among the new migrants hired in 2000-2001 (97% vs. 84% and 82% respectively) as shown in Table 12. This suggests increased competition with blue collar coworkers. The findings presented in Table 16 confirm the results derived for the whole sample.

7.4 Higher migrant mobility as a driving force

Evidence at the individual level suggests that regularized migrants have remarkably high survival rates, around 75% after four years, and also that this increase in legal migrant labor does not affect the careers of coworkers. What is the explanation for these findings?

It might be that the higher mobility of migrants with respect to natives allows regularized migrants to fill vacancies in different industries and provinces which would not be filled by natives. In this section we provide suggestive evidence to support this explanation.²⁴

We compare regularized workers (*Bossi-Fini* law) with two control groups: regular migrant workers who appeared in the INPS archive data for the first time in 2000 or 2001 and native workers that entered INPS archives for the first time in 2002.²⁵

Figure 11 compares their raw survival rates in the labor market: regularized migrants show the strongest attachment to the labor market, while native workers are the most likely to leave the private sector in subsequent years. These findings may be due to some heterogeneity in the observable characteristics of the first job for each type of worker, as detected in Table 12.

We investigate this issue in more depth by estimating the differences in terms of mobility between the three groups, i.e. regularized migrants, migrants and native. We implement cross section regressions considering the sample of workers in the three groups in their first year in the data. As dependent variables we use the cumulated changes of firm, or industry, or province, from the first year after the hiring to the sixth year.²⁶ We include as control variables individual characteristics (age, age square, gender, years spent in the data in the period) and industry and province fixed effects related to the firm in the first year.

From Table 17 emerges a straightforward descriptive evidence: native workers are much less mobile than both legalized and migrant workers. In particular, in the first three columns we show that being in the group of native and migrant workers, with respect to legalized, is associated to a reduction of 0.357 in the number of cumulated firm changes, equal to a -21.1% fall with respect to the average cumulated firm changes

²⁴Rigorous proof of this explanation is beyond the scope of our research.

²⁵We do not consider the group of migrants hired outside the legalization program in 2002, since it could represent an unreliable control group, in terms of composition this group might display peculiar observable characteristics.

²⁶Results do not change when modifying the length of this interval. Further, similar findings emerge when using a panel estimations, focusing on the yearly probability of changing firm, or industry, or province. We choose to use a cross section regression since differences in mobility across groups are constant over time. Panel estimates are available on request.

(equal to 1.69). Similar findings are derived for the industry changes (-8.6%) and for the province changes (-16.4%). The last three columns provide the same exercise considering the group of native workers with respect to all migrants (legalized and not legalized), results are widely confirmed: migrant workers are much more mobile than native ones.

The results for the US labor market in Cadena and Kovak (2016) are similar. Specifically, this study provides evidence of higher mobility among low-skilled migrant workers with respect to low skilled native workers. Immigrants' location choices respond strongly to changes in local labor demand, and their mobility reduces the incidence of local demand shocks on natives. Cadena and Kovak (2016) conclude that *“as (US) policy makers seek ways to normalize the status of unauthorized workers (...), they should consider the geographic flexibility immigrants provide labor markets when they are free to change locations and employers in response to changing demand conditions?”*. Our results complement these findings by showing that when migrants achieve legal status they are able to change their location since their moving costs reduce²⁷ and they are able to fill vacancies in labor markets that where there is excess demand. To our knowledge, this is the first evidence on differential geographical mobility of native and migrant workers in the case of a European labor market.²⁸

8 Additional evidence from eligible firms

As mentioned in Section 4, our data identify a group of firms which applied for the regularization process but did not complete it, i.e. they did not hire any new migrant employee in the relevant time window. We define this group as “eligible” firms, and infer that they are very similar to regularizing firms since both groups expressed interest in the policy to regularize undocumented migrant workers. However, the eligible group of firms did not finalize the regularization for several unobservable reasons (errors in the application, unanticipated departure of irregular workers, inability to prove the minimum-

²⁷Undocumented migrants might be less likely to move because the probability of their being caught by the authorities increases with their mobility.

²⁸Basso et al. (2018) document high migrants mobility focusing on the Euro area.

three month requirement, etc.), thus constituting an appropriate control group for our analysis. We compare our outcomes of interest for these two groups in a differences-in-differences framework.

Table 18 shows the distribution of regularizing and eligible firms across industries: these two distributions appear very similar since in both groups most firms are concentrated in manufacturing, constructions, sales and hotels/restaurants. Table 19 shows that the main outcomes pre-regularization are very similar for both groups: the median value of employment is 3 and the median wage differs only by 12 Euros.

In this analysis, our identifying assumption is that conditional on the control variables, the trends among the outcomes of interest in the two groups of firms are parallel and would have been the same in the absence of the policy. With regularization, the firms able to complete the process experience a shock to their employment (wage) levels, while eligible firms that did not complete it do not.

Figure 12 depicts the monthly employment trends for regularizing and eligible firms:²⁹ prior to September 2002, the two groups show a similar trend but after regularization is launched the group of regularizing firms show a sharp increase in number of employees. Although this level of employment stabilizes for regularizing firms it continues to increase in eligible firms after the amnesty.

To test the significance of our descriptive results, we estimate a difference-in-differences model with multiple pre- and post-periods, controlling for time varying characteristics and firm fixed effects. Specifically, we estimate

$$y_{i,t} = \sum_{k=Jan02}^{Dec03} \alpha_k I(t = k) \times T_i + \beta X_{i,t} + \eta_i + \varepsilon_{i,t} \quad (3)$$

where $X_{i,t}$ includes the dimension of the reference firm's cell and number of inspections in t-1, and $y_{i,t}$ is the firm's monthly employment and total monthly wages. We exclude May 2002 as the reference month.³⁰ Figures 13 and 14 report the results of our estimates:

²⁹ Figure 12 refers to the sample of all firms constituted before 2002 excluding the highest percentile for employment in May 2002.

³⁰We chose May to be the reference month in order to have a month not affected by anticipation effects

participating in the regularization increases firm employment in the short run but the differences between the two groups of firms quickly decrease and becomes non-significant after two years. The level of the monthly wages paid by the firm follows a similar pattern: it increases by around 1,000 Euro after regularization (reflecting an increase in employment of around 1 worker) and tends to zero in the medium run.³¹ It is reassuring that the findings from two different identification strategies are comparable, which supports the robustness of our results.

9 Conclusions

In this paper, we explored the effects of an amnesty which allowed undocumented migrant workers to obtain residence permits and to move from the informal to the formal labor market. Availability of data on the universe of employers and employees, released by INPS, allowed us to precisely identify the firms and workers affected by the policy. We make use of an unexpected auditing program to identify a causal impact of the regularization, since it provides an exogenous variation in expected fines for firms undertaking the regularization. The first stage of our identification strategy shows that the additional unexpected inspection plan *ex lege 383* has a direct impact on the probability of participating in the tax amnesty: legalization is more effective within an enforcement program (the additional auditing plan) introduced immediately before implementation of the regularizing process.

We find out that the impact on employment at the firm level is transitory (and it is negligible and not statistically significant on firm wages). However, the individual level analysis shows a very high (around 73.5%) survival rate of regularized workers in the formal Italian labor market. Thus, the policy was effective for increasing regular employment in a permanent way. Our results show that the regularization can be considered

and by seasonality issues: labor market conditions for blue collar worker can vary widely during the summer.

³¹We performed a standard diff-in-diff analysis at the individual level, comparing non regularized workers in regularizing firms with workers in eligible firms. The results are in line with our main findings and are available upon request.

an active labor market policy applying to a specific group of workers, irregular migrants, which pushed them into remaining attached to the formal labor market. We also investigate the impact of the regularization on regularized migrants coworkers. We find no significant impact on the probability that coworkers leave the labor market or experience longer spells of unemployment. This is consistent with theoretical papers showing that an increase in the share legalized workers do not necessarily imply a negative impact on native employment (Chassamboulli and Peri, 2015).

We suggest that relatively higher migrants' mobility with respect to natives explains our findings (Cadena and Kovak, 2016; Basso et al., 2018): higher levels of mobility might explain the lack of a negative effect on employment dynamics for either firms or coworkers after the regularization: more mobile regularized migrants find jobs in local labor markets with excess demand, and fill vacancies that are less attractive to less mobile natives.

In this scenario, the high survival rates for legalized workers have important policy implications: regularized workers do not return to the black economy, i.e. do not go *back to black*, and remain strongly attached to the labor market. Thus, in periods of persistent mass migration resulting in increased incidence of illegal migrants in the population, as experienced by Italy and Europe in recent years, regularizing undeclared migrant workers would appear to be a credible tool to increase social security payments and tax payments from workers who if not regularized would have continued to work in the shadow economy.

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Tables

Table 1: Descriptive statistics of inspections

By industry (above 1% in 2001)	2001	2002	Ex 383
Manufacturing	22.11	19.49	28.65
Construction	21.44	21.68	10.68
Sales	20.65	20.80	30.79
Hotels and Restaurants	20.18	21.53	12.12
Transport	1.93	1.85	1.15
Services	2.30	2.28	2.40
Characteristics of inspections	2001	2002	Ex 383
No irregularity found	0.43	0.40	0.60
Irregular but not fined	0.22	0.26	0.28
Irregular and fined	0.45	0.43	0.17
Irregular migrant found	0.15	0.18	0.07
Mean fine	6,999	6,078	664
Median fine	1,716	1,341	691
No. inspections	42,056	44,967	10,997

The Table reports relevant descriptive statistics across types of inspections. Columns “2001” and “2002” refer to the standard auditing activity carried out by INPS in 2001 and 2002. Column “Ex 383” refers to the additional auditing plan defined in Law 383/2001.

Table 2: Descriptive evidence on the distribution of regularizing firms, by industry

	All firms	Regularizing firms	Firms with migrant	Irregular firms	% regularizing within industry
Agriculture	0.83	0.38	0.30	0.51	2.29
Mining	0.21	0.19	0.30	0.15	4.41
Manufacturing	22.05	25.89	35.23	20.55	5.83
Energy	0.03	0.01	0.01	0.01	1.23
Water	0.25	0.37	0.38	0.15	7.35
Construction	14.58	37.80	20.00	21.27	12.88
Sales	22.45	10.64	11.89	18.44	2.35
Transport	3.20	3.02	3.44	1.93	4.68
Hotel/Restaurants	8.43	14.95	14.30	24.26	8.81
Communication	1.83	0.23	0.70	0.47	0.62
Finance	1.64	0.08	0.38	0.39	0.23
Real estate	0.77	0.23	0.37	0.48	1.46
Professionals	6.61	0.49	1.79	1.06	0.37
Services	3.28	3.22	3.15	2.08	4.87
Public Administration	0.10	0.00	0.05	0.02	0.16
Education	0.89	0.15	0.50	0.39	0.83
Health	3.64	0.33	1.62	0.93	0.44
Art&sport	0.76	0.62	0.63	1.21	4.04
Other	5.41	1.37	2.31	5.38	1.26
ONG	3.02	0.05	2.62	0.32	0.08
	100	100	100	100	-
No. Firms	1,217,112	60.464	161.437	21.071	60.464

Data refer to year 2002. “Firms with migrants” are defined as those employing at least one non-EU worker in May 2002. “Irregular” refers to firm inspected in 2001 that have been found with some irregularities concerning the workforce. The column “% regularizing within industry” refers to the share of firms regularizing workers within each the industry (the sum of the values in the column does not have to sum up to 100).

Table 3: Descriptive evidence on the distribution of regularizing firms, by regions

	All firms	Regularizing firms	Firms with migrant	Irregular firms	% regularizing within region
Abruzzo	2.19	1.71	1.91	3.29	3.87
Basilicata	0.79	0.24	0.23	1.87	1.49
Calabria	2.24	1.47	0.67	2.75	3.26
Campania	6.72	5.29	2.27	10.06	3.90
Emilia Romagna	8.61	9.08	11.16	9.51	5.23
Friuli	2.25	1.46	3.25	2.21	3.21
Lazio	8.20	12.31	9.65	4.66	7.44
Liguria	3.06	2.34	2.54	2.69	3.80
Lombardia	18.88	23.31	23.50	15.44	6.12
Marche	3.06	3.03	4.03	3.77	4.91
Molise	0.43	0.13	0.16	0.46	1.51
Piemonte	7.72	11.45	8.42	6.88	7.35
Puglia	5.34	1.44	1.90	6.37	1.34
Sardegna	2.56	0.23	0.40	1.33	0.44
Sicilia	6.45	0.73	2.36	7.18	0.56
Toscana	7.78	8.57	9.26	9.22	5.46
Trentino Alto Adige	2.29	1.30	2.72	1.69	2.81
Umbria	1.63	2.53	2.30	1.56	7.67
Valle d'Aosta	0.31	0.22	0.25	0.32	3.56
Veneto	9.48	13.16	13.03	8.73	6.88
	100	100	100	100	-
No. Firms	1,220,189	60.472	161.629	21.095	60.472

Data refer to year 2002. “Firms with migrants” are defined as those employing at least one non-EU worker in May 2002. “Irregular” refers to firm inspected in 2001 that have been found with some irregularities concerning the workforce. The column “% regularizing within region” refers to the share of firms regularizing workers within the region (the sum of the values in the column does not have to sum up to 100).

Table 4: Descriptive evidence on the distribution of regularizing firms, by municipality size

Population	All	Regularizing firms	Firms with migrants	Irregular firms	% regularizing within class
<1,000	1.43	1.87	1.75	1.76	6.51
1,000-4,999	13.56	14.95	15.14	15.75	5.46
5,000-14,999	22.35	25.63	25.66	25.28	5.68
15,000-49,999	21.93	21.28	19.49	24.47	4.81
50,000-249,999	19.66	14.46	15.85	17.52	3.65
>=250000	21.07	21.81	22.10	15.23	5.13
	100	100	100	100	-
Total	1,220,197	60.472	161.631	21.095	60.472

Data refer to year 2002. “Firms with migrants” are defined as those employing at least one non-EU worker in May 2002. “Irregular” refers to firm inspected in 2001 that have been found with some irregularities concerning the workforce. The column “% regularizing within class” refers to the share of firms regularizing workers within the class of municipality size (the sum of the values in the column does not have to sum up to 100).

Table 5: Descriptive statistics for main outcomes: number of employees and monthly wages

	May 2002		December 2002		May 2003		September 2003	
	Regularizing	Controls	Regularizing	Controls	Regularizing	Controls	Regularizing	Controls
Number of employees								
Mean	4.38	3.99	5.69	3.93	5.58	4.28	5.30	4.28
Median	2	2	4	2	4	2	3	2
Monthly wage								
Mean	1,396	1,487	1,318	1,467	1,333	1,501	1,343	1,492
Median	1,422	1,466	1,333	1,447	1,364	1,480	1,374	1,469

The Table reports descriptive statistics for the main outcomes of interest. Number of employees is measured at monthly level. To estimate monthly wages, we divide the yearly wage observed for each worker-firm by the number of months worked in that firm. Excluding firms constituted after 2001, 99th percentile in terms of employment in May 2002 and 1st and 99th percentiles of change in employment between May and December 2002.

Table 6: Impact on employment

	May-Dec '02	May '02-May '03	May '02-Sep '03
OLS estimates			
Regularizing	1.327*** (0.011)	1.003*** (0.017)	-0.448*** (0.017)
Obs.	2,023,626	1,848,664	1,848,664
R-sq.	0.555	0.473	0.645
IV estimates			
Regularizing	2.626*** (0.648)	1.280 (0.987)	0.092 (1.002)
Obs.	2,023,626	1,848,664	1,848,664
R-sq.	0.543	0.473	0.645
KP	108.996	108.212	108.212

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Errors clustered at firm level. Excluding firms constituted after 2001, 99th percentile in terms of employment in May 2002 and 1st and 99th percentiles of change in employment between May and December 2002. Controls included: number of inspections in t-1 in the cell, cell dimension, firm's fixed effects, province-year fixed effects, industry (NACE 2 digits)-year fixed effects. IV: inspections ex lege 383/2001 in the cell.

Table 7: Impact on wages

	May-Dec '02	May '02-May '03	May '02-Sep '03
OLS estimates			
Regularizing	-31.898*** (1.501)	-36.765*** (2.112)	14.792*** (1.792)
Obs.	1,737,954	1,684,454	1,639,094
R-sq.	0.496	0.387	0.486
IV estimates			
Regularizing	-128.862 (105.334)	-40.623 (167.884)	-137.522 (156.958)
Obs.	1,737,954	1,684,454	1,639,094
R-sq.	0.493	0.387	0.483
KP	83.69	85.69	86.00

*** p<0.01, ** p<0.05, * p<0.1

Errors clustered at firm level. Excluding firms constituted after 2001, 99th percentile in terms of employment in May 2002 and 1st and 99th percentiles of change in employment between May and December 2002. Controls included: number of inspections in t-1 in the cell, cell dimension, firm's fixed effects, province-year fixed effects, industry (NACE 2 digits)-year fixed effects. IV: inspections ex lege 383/2001 in the cell.

Table 8: Impact on hirings and separations - 2002

	Hirings		Separations	
	Natives	Migrants	Natives	Migrants
IV estimates				
Regularizing	0.791 (0.784)	0.909** (0.377)	0.617 (0.905)	0.367 (0.317)
Obs.	2,023,626	2,023,626	2,023,626	2,023,626
KP	110.82	110.82	110.82	110.82

*** p<0.01, ** p<0.05, * p<0.1

Errors clustered at firm level. The variables are computed at yearly level. Excluding firms constituted after 2001, 99th percentile in terms of employment in May 2002 and 1st and 99th percentiles of change in employment between May and December 2002. Controls included: number of inspections in t-1 in the cell, cell dimension, firm's fixed effects, province-year fixed effects, industry (NACE 2 digits)-year fixed effects. IV: inspections ex lege 383/2001 in the cell.

Table 9: Robustness checks: Employment

	(1)	(2)	(3)	(4)	(5)	(6)
May-Dec '02						
Regularizing	0.890 (0.582)	2.264*** (0.585)	3.038*** (0.615)	2.457*** (0.635)	2.397*** (0.551)	3.360*** (0.543)
Obs.	2,023,626	2,010,346	2,019,468	2,023,626	2,023,626	1,169,739
R-sq.	0.533	0.549	0.536	0.551	0.547	-0.050
May '02-May'03						
Regularizing	-0.294 (0.888)	1.206 (0.882)	1.878** (0.916)	1.104 (0.938)	1.257 (0.840)	0.741 (0.764)
Obs.	1,848,664	1,835,952	1,844,792	1,848,664	1,848,664	1,056,699
R-sq.	0.470	0.473	0.472	0.489	0.473	0.022
May '02-Sep '03						
Regularizing	0.413 (0.891)	-0.180 (0.893)	0.420 (0.910)	-0.089 (0.947)	-0.026 (0.851)	-2.410*** (0.915)
Obs.	1,848,664	1,835,952	1,844,792	1,848,664	1,848,664	1,056,699
R-sq.	0.642	0.645	0.645	0.657	0.645	-0.073

*** p<0.01, ** p<0.05, * p<0.1

Errors clustered at firm level. Standard controls included. IV: inspections *ex lege* 383/2001 in the cell. Standard sample restrictions apply. (1) Employment of Italian workers, (2) Excluding firms who received inspection 383, (3) Cell: LLM x industry (NACE 2 digits), (4) Controlling for number of migrant workers employed in the firm in June, (5) Controlling for standard (non *ex lege* 383) inspections in 2002, (6) Cross-section specification.

Table 10: Robustness checks: Wages

	(1)	(2)	(3)	(4)	(5)
May-Dec '02					
Regularizing	-100.290 (91.876)	-107.228 (97.982)	-125.426 (104.797)	-107.592 (88.645)	-102.546 (83.937)
Obs.	1,725,892	1,734,328	1,737,954	1,737,954	1,017,503
R-sq.	0.495	0.495	0.494	0.494	-0.003
May '02-May '03					
Regularizing	-57.872 (147.690)	74.946 (165.903)	-33.612 (167.035)	-88.566 (140.057)	184.195 (123.269)
Obs.	1,672,774	1,680,916	1,684,454	1,684,454	971,727
R-sq.	0.387	0.386	0.388	0.387	-0.029
May '02-Sep '03					
Regularizing	-111.300 (138.437)	37.326 (149.828)	-130.283 (155.966)	-160.678 (131.509)	117.673 (122.272)
Obs.	1,627,716	1,635,660	1,639,094	1,639,094	951,178
R-sq.	0.484	0.487	0.484	0.481	-0.009

*** p<0.01, ** p<0.05, * p<0.1

Errors clustered at firm level. Standard controls included. IV: inspections *ex lege* 383/2001 in the cell. Standard sample restrictions apply. (1) Excluding firms who received *isp.* 383, (2) Cell: LLM x industry (NACE 2 digits), (3) Controlling for number of migrant workers employed in the firm in June, (4) Controlling for standard (non *ex lege* 383) inspections in 2002, (5) Cross-section specification.

Table 11: Long run outcomes

	Δ employment May '02-May '04	Δ wages May '02-May '04	Firm's exit in 2003	Prob. being inspected in 2004
IV estimates				
Regularizing	-0.007 (1.027)	-178.055 (150.924)	0.114 (0.111)	-0.315*** (0.076)
Obs.	976,978	896,602	1,169,739	976,978

*** p<0.01, ** p<0.05, * p<0.1

Errors clustered at firm level. Cross-section specification. Controls included: number of inspections in t-1 in the cell, cell dimension, firm's age, province fixed effects, industry (NACE 2 digits) fixed effects. IV: inspections *ex lege* 383/2001 in the cell. Standard sample restrictions apply.

Table 12: Comparison between regularized, migrants and natives

	Regularized	Migrants hired in 2000/2001	Migrants Stock in 2002	Natives hired in 2002
Nationality				
Albania	12.57	16.13	14.36	-
Romania	26.64	11.65	10.45	-
Ex Yugoslavia	5.03	5.74	6.93	-
China	11.31	5.58	4.73	-
Morocco	11.94	12.65	13.51	-
Other Europe	12.45	13.92	17.53	-
Asia/Australia	8.19	14.36	12.50	-
Africa	11.32	17.64	17.75	-
America	0.54	2.32	2.25	-
Socio-Demographic				
Male	0.85	0.64	0.63	0.52
≤24	0.27	0.28	0.32	0.56
25-34	0.47	0.46	0.41	0.29
35-49	0.22	0.23	0.24	0.11
≥50	0.04	0.02	0.34	0.04
Labor Market characteristics				
Full time	0.70	0.80	0.82	0.78
Permanent	0.91	0.71	0.79	0.63
White collars	0.02	0.08	0.09	0.3
Blue collars	0.97	0.82	0.84	0.46
Yearly earnings	4648	6780	13115	5071
Monthly earnings	1185	1155	1425	906
Months worked	3.95	5.4	8.66	4.97
Observations	209,570	284,188	669,907	654,967

The Table reports a comparison in terms of socio-demographic characteristics and labor market characteristics of different groups of workers. We compare migrants who were regularized in 2002 with migrants who were regularly hired in 2000 or 2001, the stock of migrants in 2002 (excluding the regularized workers) and all Italian workers regularly hired in 2002.

Table 13: Descriptive statistics between treated (coworkers) and controls

	2001		2002	
	Treated	Controls	Treated	Controls
Demographics				
Age	34.1	34.6	34.3	34.9
Female	0.26	0.42	0.26	0.42
Migrant	0.23	0.05	0.27	0.05
Labor market				
Firm size	6.3	5.8	6.5	5.8
Blue collar	0.77	0.57	0.79	0.57
Monthly wage	1472	1251	1423	1264
Outcomes				
Exit	-	-	0.13	0.10
Job separation	0.33	0.16	0.27	0.14
Unemployment t+1 (Jan-Aug)	1.04	0.73	1.05	0.69
Unemployment t+1	1.69	1.22	1.75	1.18
Unemployment t+1 and t+2	2.68	1.81	2.61	1.77
Observations	213,715	3,234,551	268,093	3,752,441

The Table compares the coworkers of regularized migrants with the universe of employees active in 2002. We exclude all workers who were active less than two months between January and September 2002, workers in firms with 15 employees or more and the 1st and the 99th percentiles of the monthly wage distribution.

Table 14: Individual level analysis: IV Fixed Effects Estimates

	Job separation	Unemp. spell Jan-Aug '03	Job separation	Unemp. spell Jan-Aug '03
Treated	0.120 (.452)	-.227 (1.059)	0.225 (.679)	-0.707 (1.707)
Year#province FE	NO	NO	YES	YES
Year#industry FE	NO	NO	YES	YES
Obs.	6,077,242	5,471,664	5,862,138	5,278,986
KP	20.14	17.80	9.83	7.87

Errors clustered at firm level. We exclude all workers who were active less than two months between January and September 2002, workers in firms with 15 employees or more and the 1st and the 99th percentiles of the monthly wage distribution. Controls included: firm size, occupation and year fixed effects. IV: inspections ex lege 383/2001 in the cell.

Table 15: Individual level analysis: IV Cross-sectional estimates

	Job separation	Exit	Unempl. spell	
			2003	2003-04
Treated	0.204** (.083)	-0.087 (.071)	-0.341 (.539)	-0.149 (.833)
Obs.	3,878,808	3,878,808	3,410,248	2,981,291
KP	41.54	41.54	40.16	40.01

Errors clustered at firm level. We exclude all workers who were active less than two months between January and September 2002, workers in firms with 15 employees or more and the 1st and the 99th percentiles of the monthly wage distribution. Controls included: age, gender, migrant status, occupation, firm size, industry (NACE 2 digits) fixed effects, province fixed effects. IV: inspections ex lege 383/2001 in the cell.

Table 16: Individual level analysis: IV estimates on the sample of Blue collar workers

	Panel		Cross section			
	Job separation	Unemp. Jan-Aug '03	Job separation	Exit	Unemp. 2003	Unemp. 03-04
Treated	-0.500 (.427)	-0.414 (1.091)	0.227*** (.086)	-0.074 (.071)	-0.774 (.629)	-1.547 (.983)
Obs.	3,490,456	3,120,470	2,296,503	2,296,503	1,996,742	1,722,249
KP	14.44	12.10	33.25	33.25	32.15	34.56

Errors clustered at firm level. We exclude all workers who were active less than two months between January and September 2002, workers in firms with 15 employees or more and the 1st and the 99th percentiles of the monthly wage distribution. Standard controls included. IV: inspections ex lege 383/2001 in the cell.

Table 17: Higher mobility of regularized migrants *vs* natives, at the firm, industry and province level

	(1)	(2)	(3)	(4)	(5)	(6)
	firm	industry	prov	firm	industry	prov
Native+migrants (wrt to legalized)	-0.357*** (0.003)	-0.106*** (0.003)	-0.182*** (0.003)			
Native (wrt legalized/migrants)				-0.331*** (0.003)	-0.141*** (0.002)	-0.164*** (0.002)
Mean of the Y	1.69	1.23	1.11	1.69	1.23	1.12
Impact as %	-21.1%	-8.6%	-16.4%	-19.6%	-11.5%	-14.8%
Observations	1,142,411	1,142,411	1,142,411	1,142,411	1,142,411	1,142,411
R-squared	0.344	0.368	0.405	0.345	0.369	0.406

*** p<0.01, ** p<0.05, * p<0.1.

Robust standard errors in parentheses. Controls included: male, quadratic in age, year fixed effects, province fixed effects, industry (NACE 2 digits) fixed effects.

Table 18: Descriptive evidence on the distribution of regularizing and eligible firms, by industry

	Eligible	Regularizing
Agriculture	1.31	0.37
Mining	0.24	0.23
Manufacturing	24.50	27.99
Energy	0.04	0.01
Water	0.39	0.39
Construction	31.30	35.43
Sales	13.74	10.86
Transport	3.23	3.15
Hotel/restaurants	15.73	14.78
Communication	0.51	0.24
Finance	0.30	0.08
Real estate	0.21	0.22
Professionals	1.11	0.47
Services	3.36	3.15
Public administration	0.00	0.00
Education	0.32	0.18
Health	0.62	0.38
Art/sport	1.05	0.65
Other	1.60	1.34
NGO	0.45	0.06
	100	100
No. Firms	4,674	43,186

Data refer to 2002. Excluding firms constituted after 2001, 99th percentile in terms of employment in May and 1st and 99th in terms of change in employment between May and December.

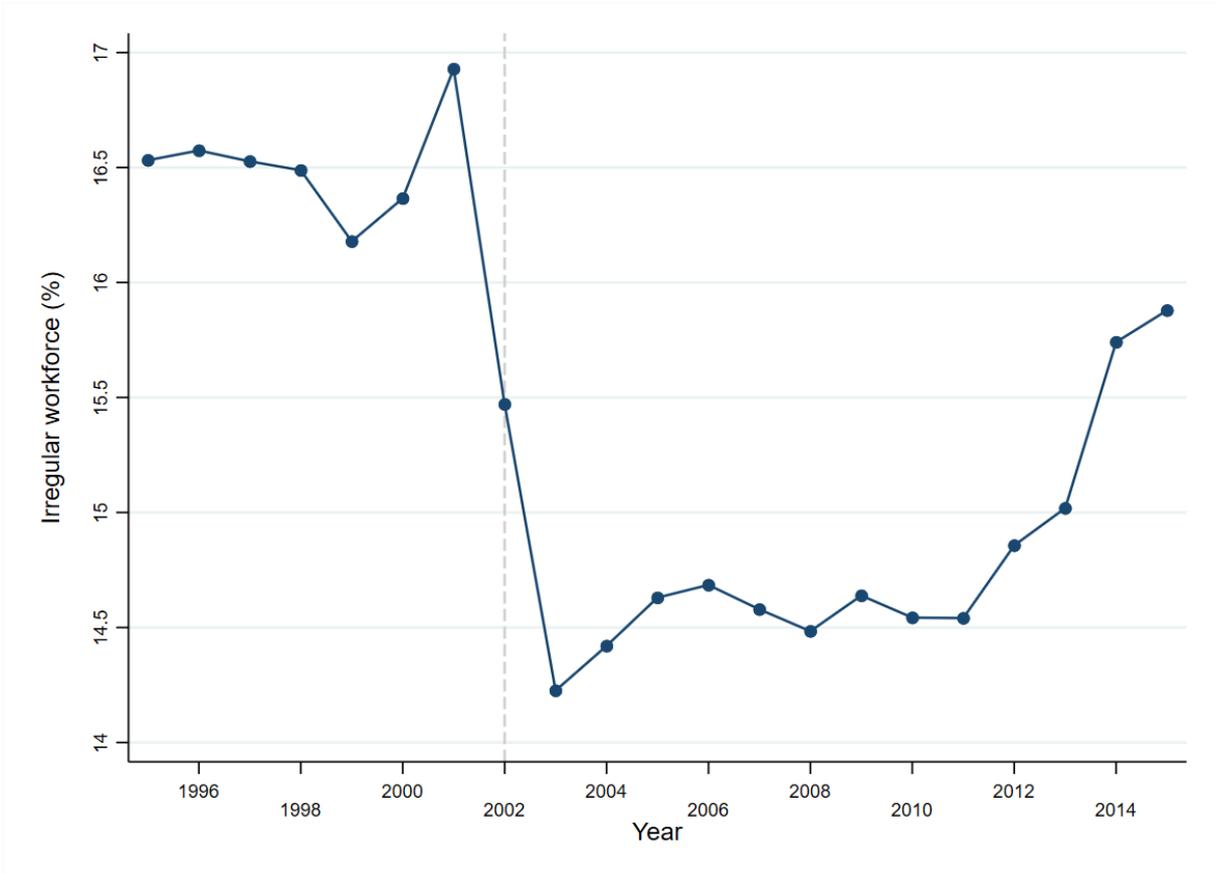
Table 19: Descriptive statistics: outcome differences between regularizing and eligible firms

	May 2002		December 2002		May 2003		September 2003	
	Regularizing	Eligible	Regularizing	Eligible	Regularizing	Eligible	Regularizing	Eligible
Number of employees								
Mean	5.47	5.02	6.80	5.70	6.74	6.07	6.55	5.97
Median	3	3	4	3	4	4	4	4
Monthly wage								
Mean	1,443	1,447	1,358	1,376	1,379	1,396	1,386	1,400
Median	1,459	1,447	1,370	1,370	1,404	1,391	1,410	1,391

The Table reports descriptive statistics for the main outcomes of interest. Number of employees is measured at monthly level. To estimate monthly wages, we divide the yearly wage observed yearly for each worker-firm by the number of months worked in that firm. Excluding firms constituted after 2001, 99th percentile in terms of employment in May and 1st and 99th percentiles in terms of change in employment between May and December.

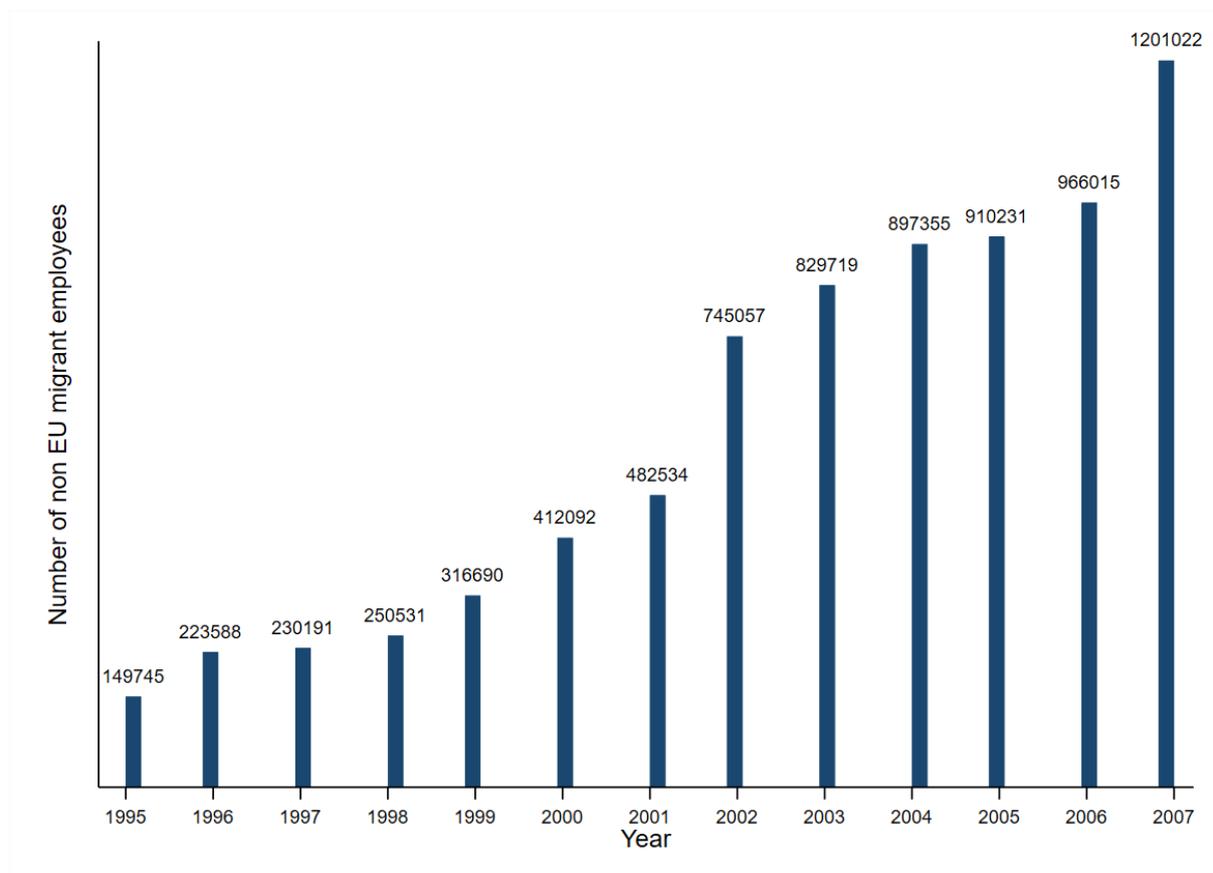
Figures

Figure 1: Irregular workforce in Italian labor market (%)



ISTAT data. The rate of undeclared work is computed as the percentage of units of undeclared work over total units of work.

Figure 2: Non EU migrant employees in the private sector



Data from INPS archives on dependent workers in the private sector. Non-EU workers are defined according to the definition of EU in 2002.

Figure 3: Policy time frame

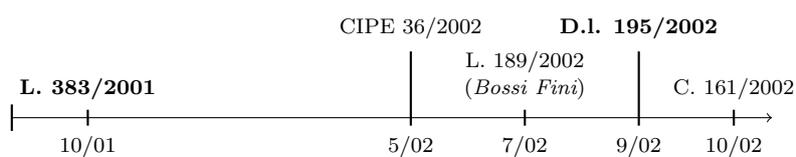
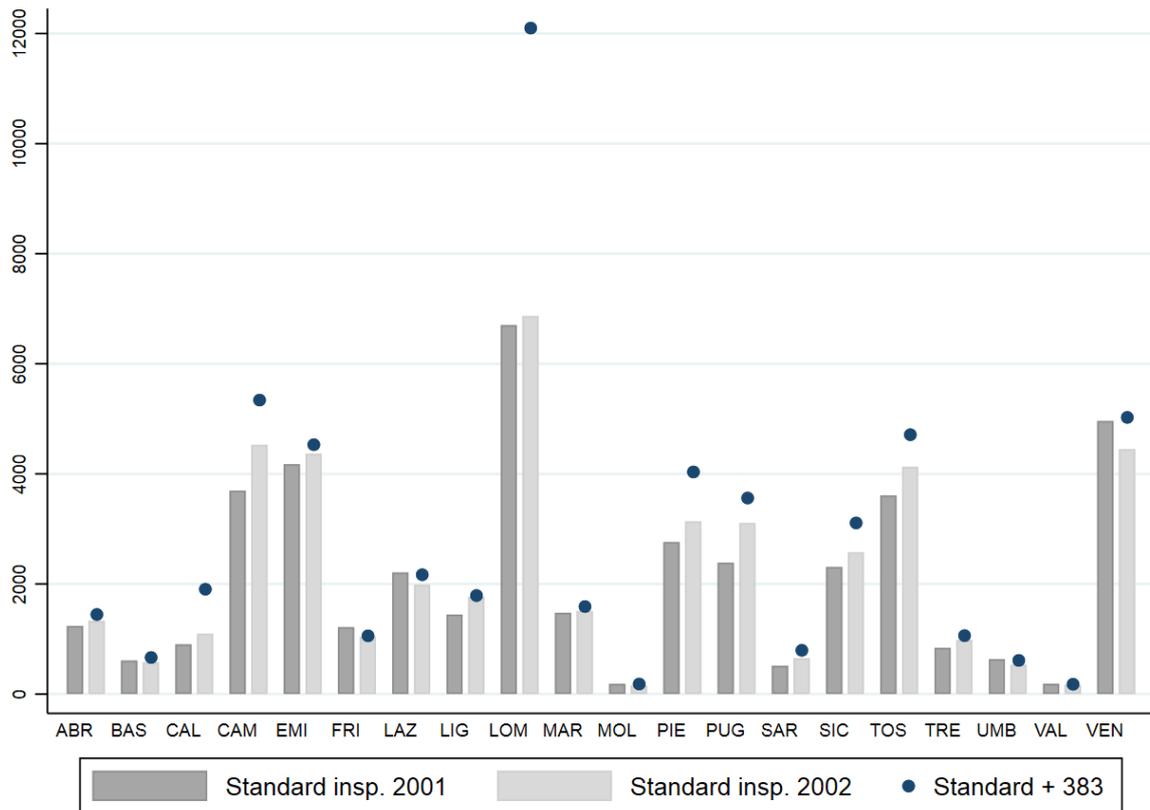
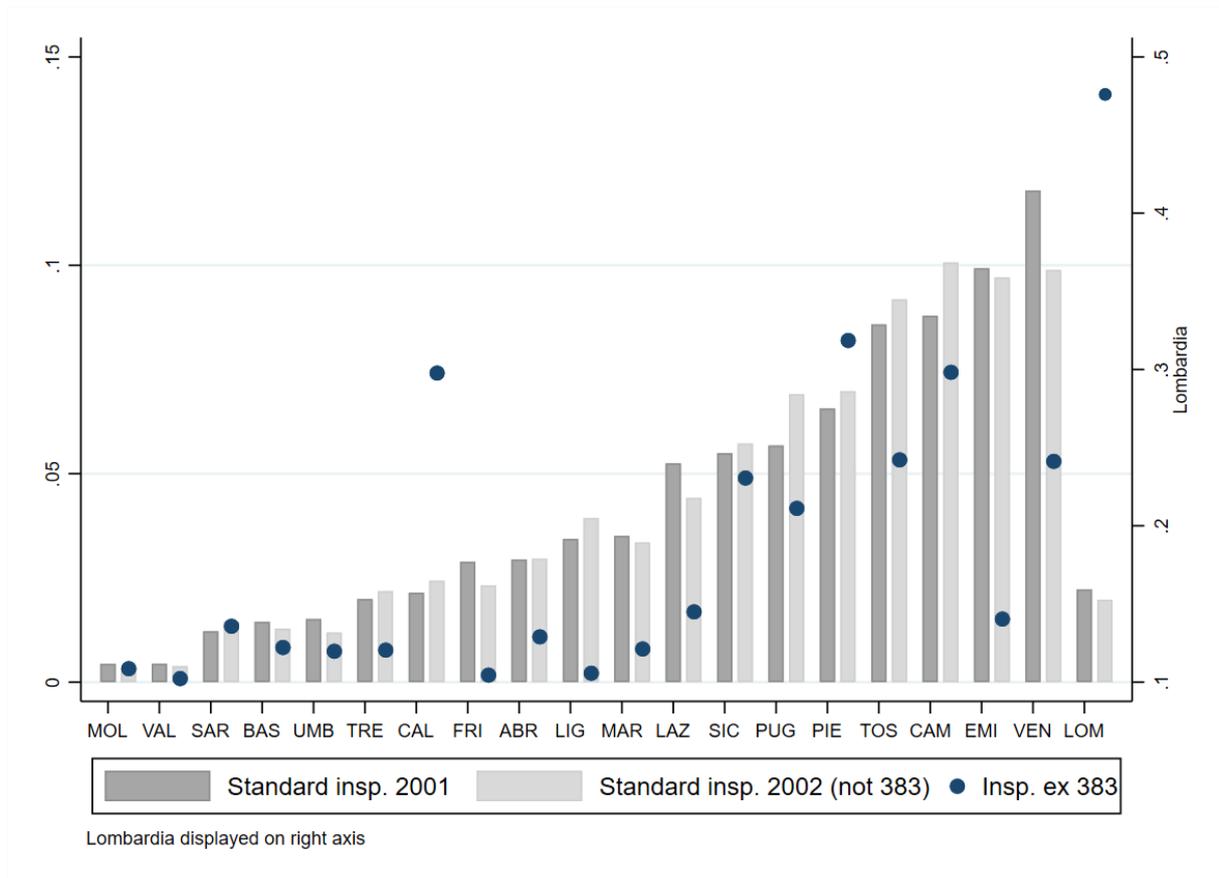


Figure 4: Number of inspections by region



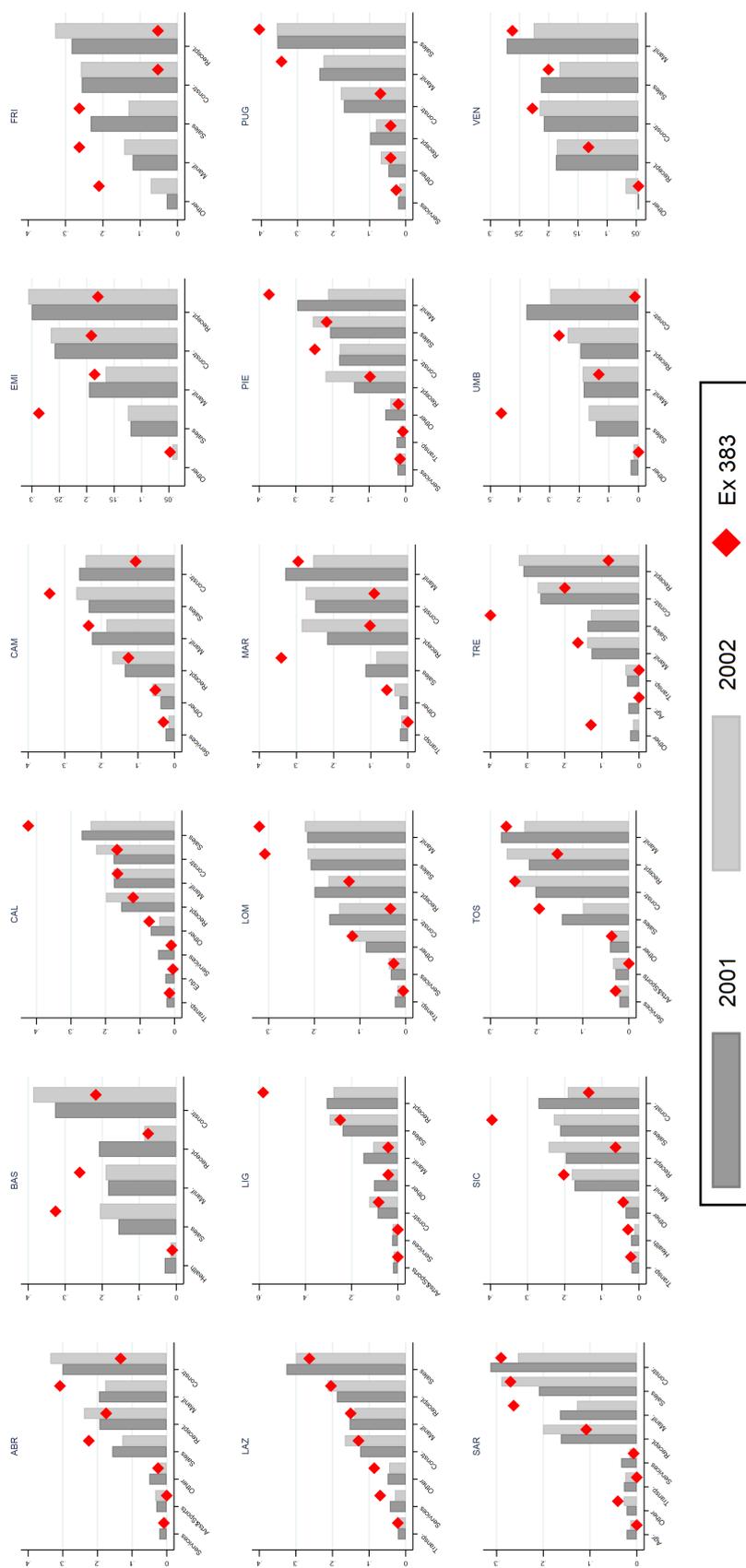
Each bar represents the number of standard inspections in each region in 2001-2002. The dots represents the total number of inspections in each region in 2002, including inspections ex lege 383/2001.

Figure 5: Distribution of inspections by region



The Figure represents the percentage of different types of inspections (standard in 2001, standard in 2002, and ex lege 383 in 2002) by region. Regions are ranked by the proportion of standard inspections in 2001. The right vertical axis concerns the Lombardia Region, which displays a rather different scale with respect to the other regions (reported in the left y-axis).

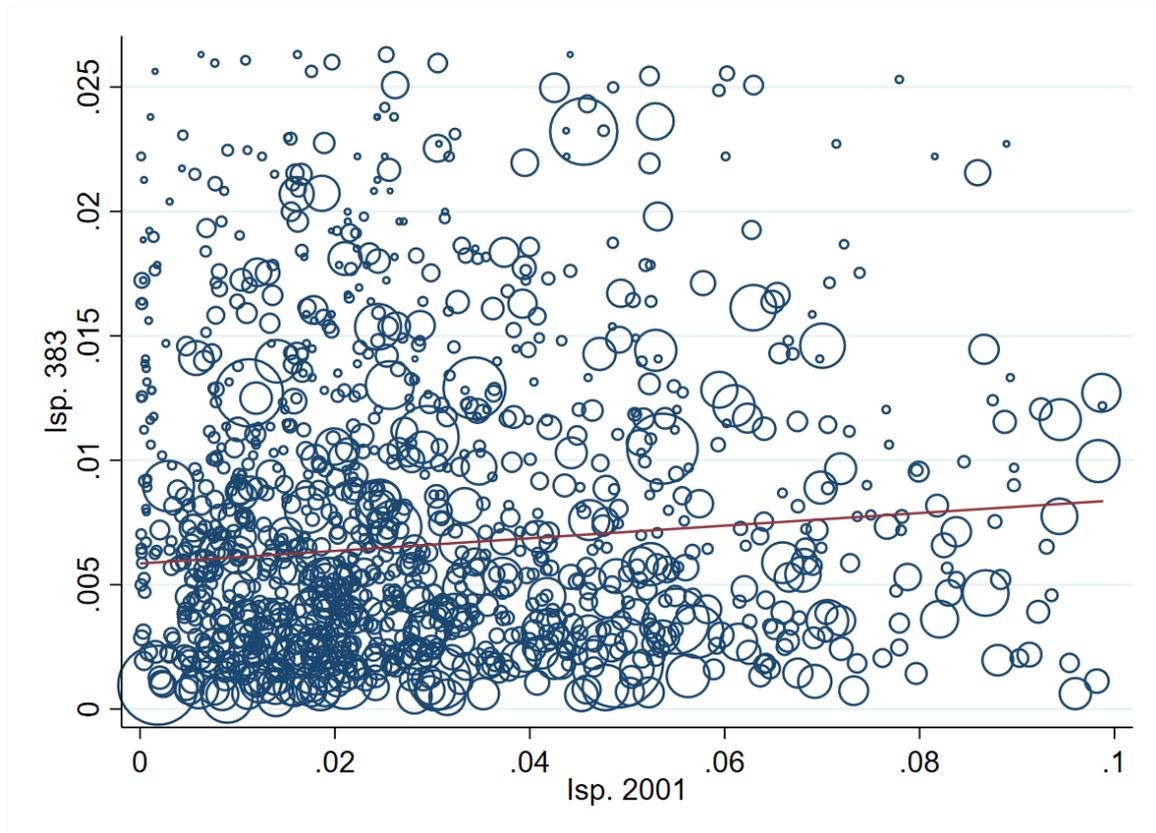
Figure 6: Distribution of inspections by industry



Molise and Val d'Aosta excluded

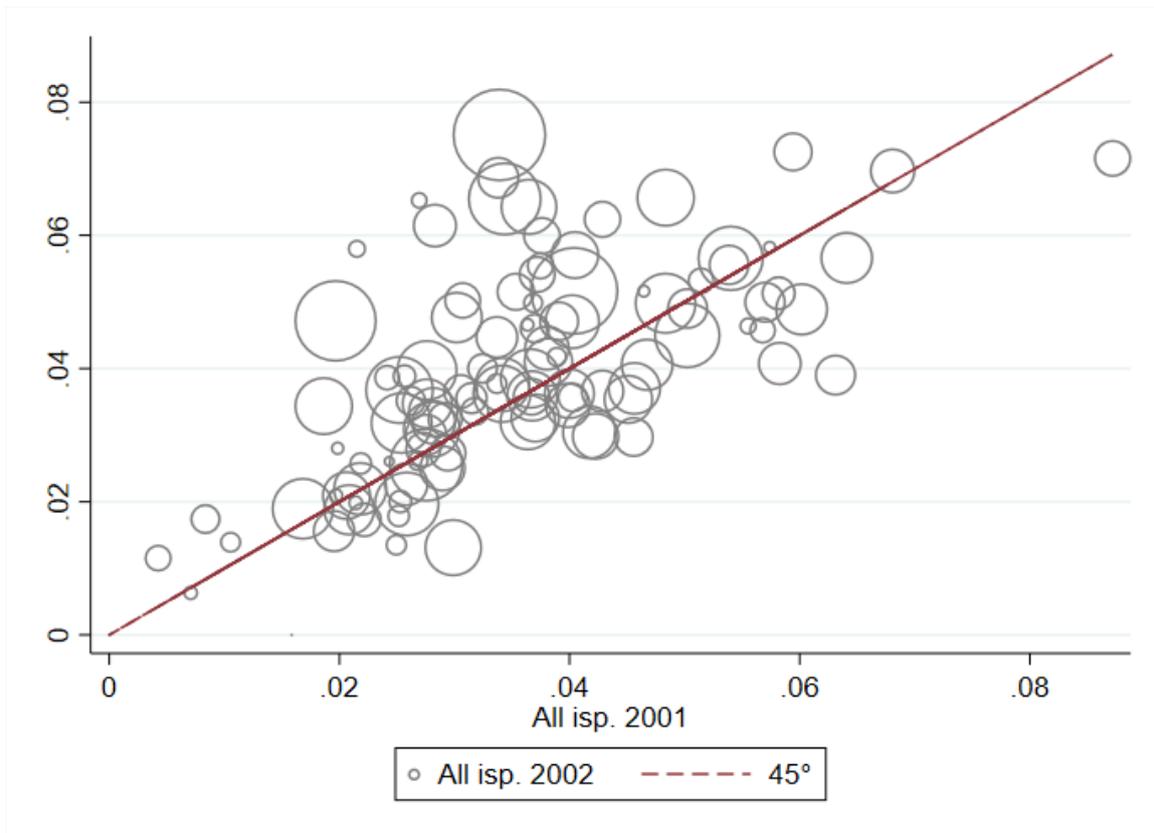
Observations are ranked by the proportion of standard inspections occurred in the industry in 2001 over total inspections in 2001.

Figure 7: Distribution of inspections in 2001 and inspections 383



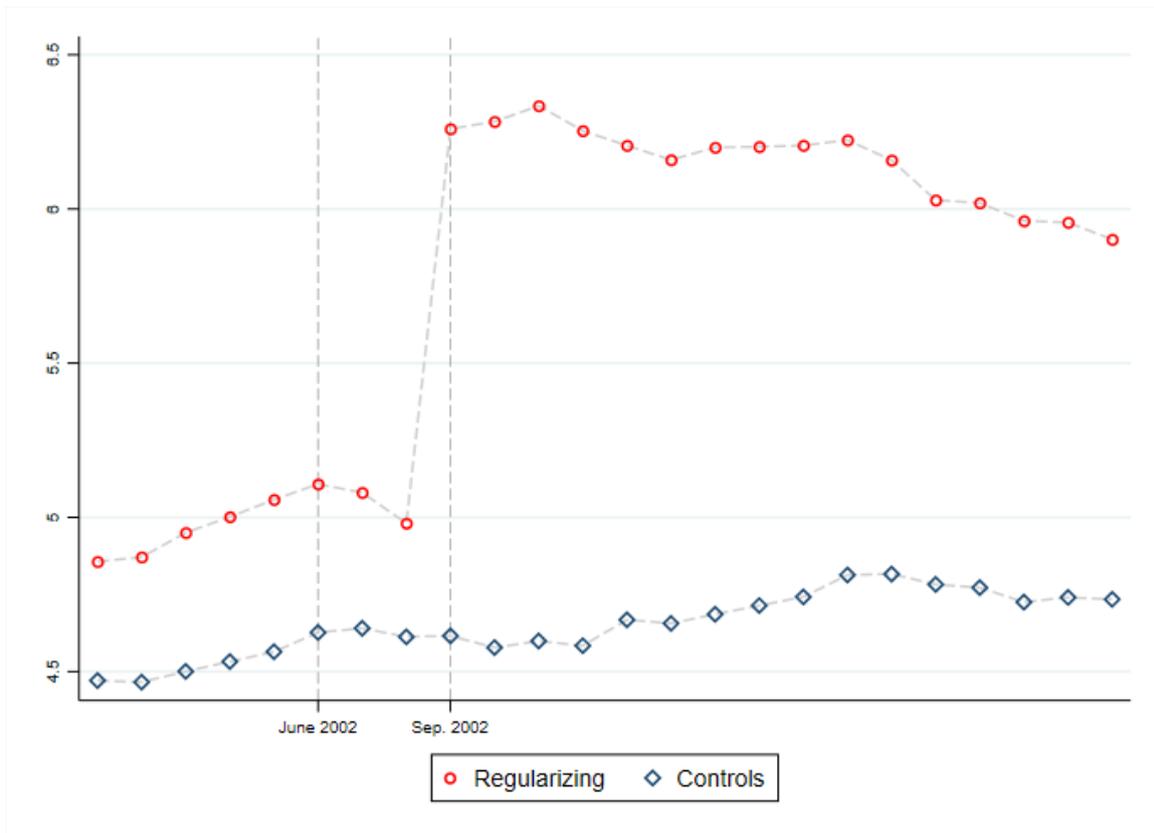
The Figure reports the correlation between standardized proportion of inspections in 2001 and the proportion of 383 inspections, at the cell level. The 5th and 95th percentiles are excluded. Observations are weighted by the dimension of the cell.

Figure 8: Distribution of inspections in 2001 and inspections in 2002



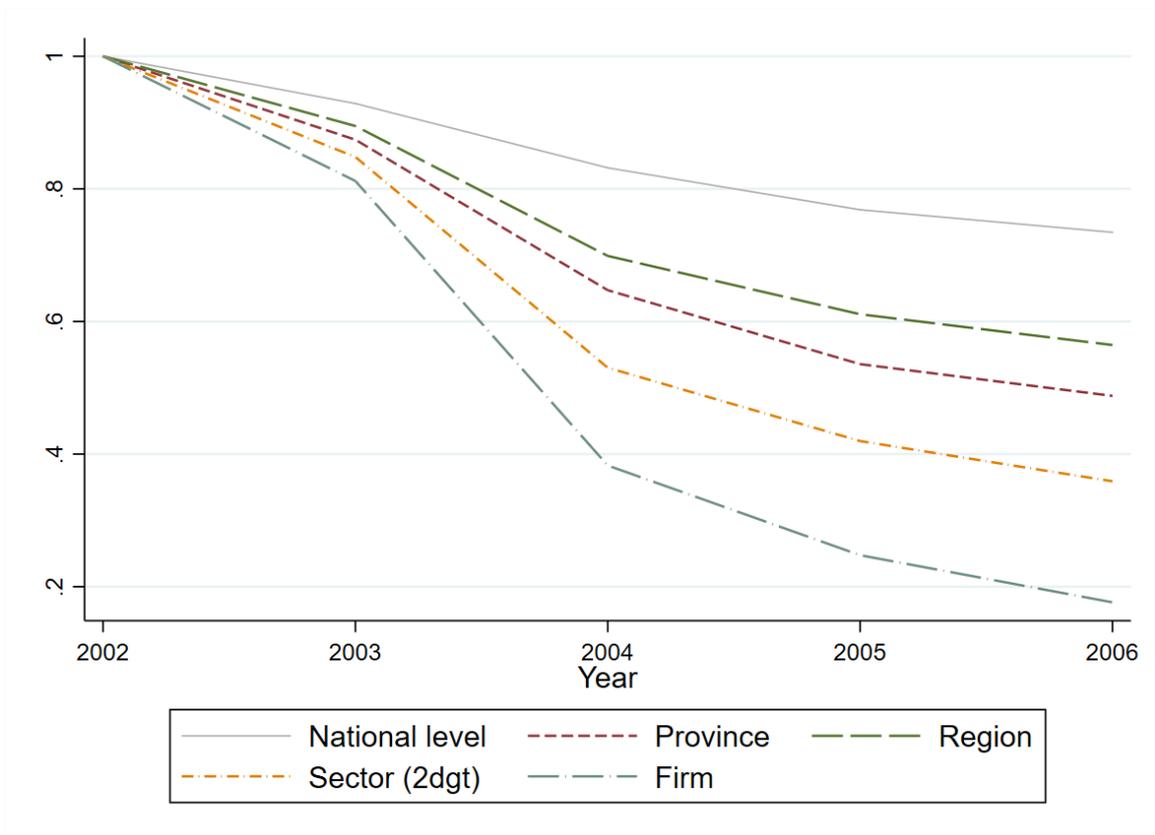
The Figure reports the correlation between the proportion of inspections in 2001 and the proportion of total inspections in 2002, at the cell level. Observations are weighted by the percentage of regularized firms in the cell.

Figure 9: Firm level employment, by month



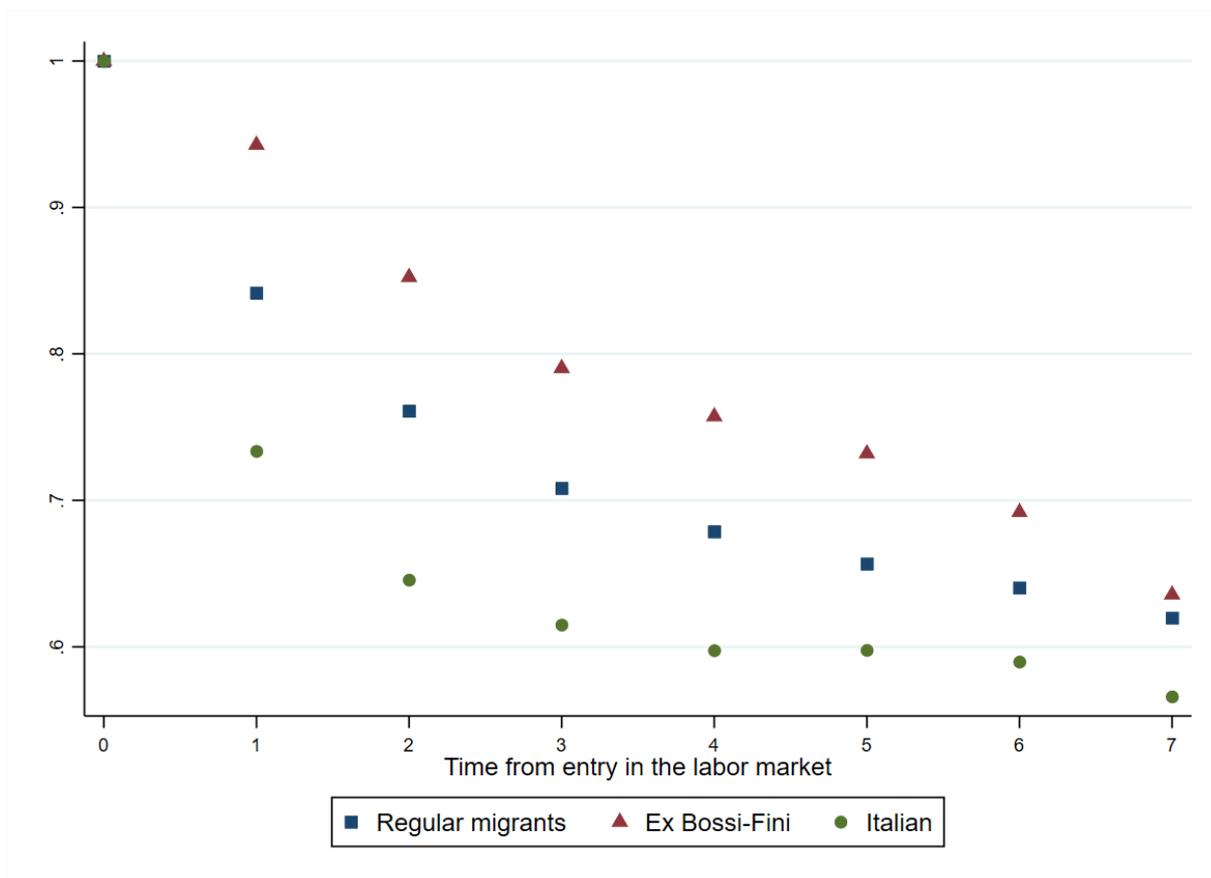
The Figure reports monthly employment for regularizing and non regularizing firms. We include all firms active in 2002 and constituted before 2002 and exclude the 99th percentile of firms in terms of employment in May 2002 and the firms in the 1st and 99th percentiles in the change in employment between May and December 2002.

Figure 10: Probability of staying in employment for regularized migrant workers



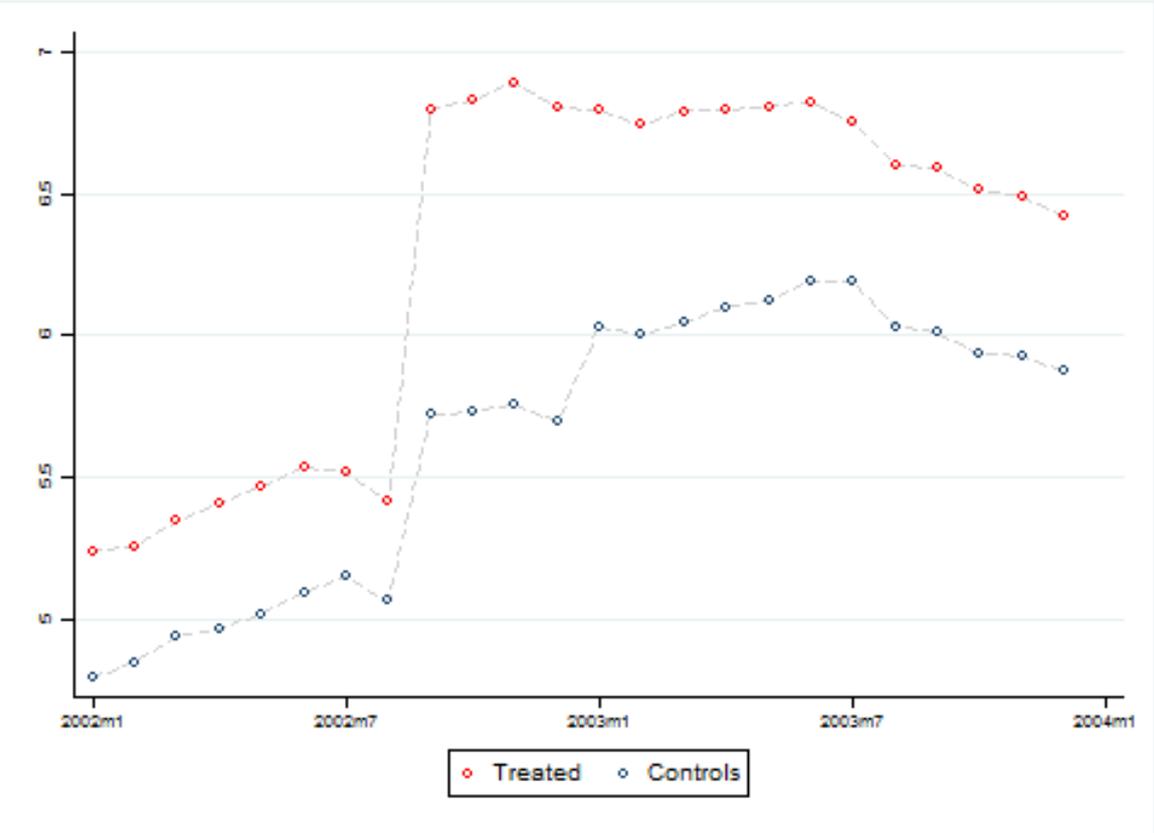
The Figure shows the probability that the regularized worker remains employed in the private sector, in the same province, region, 2 digits industry or firm where she was first hired, for each year after the regularization.

Figure 11: Probability of staying in employment



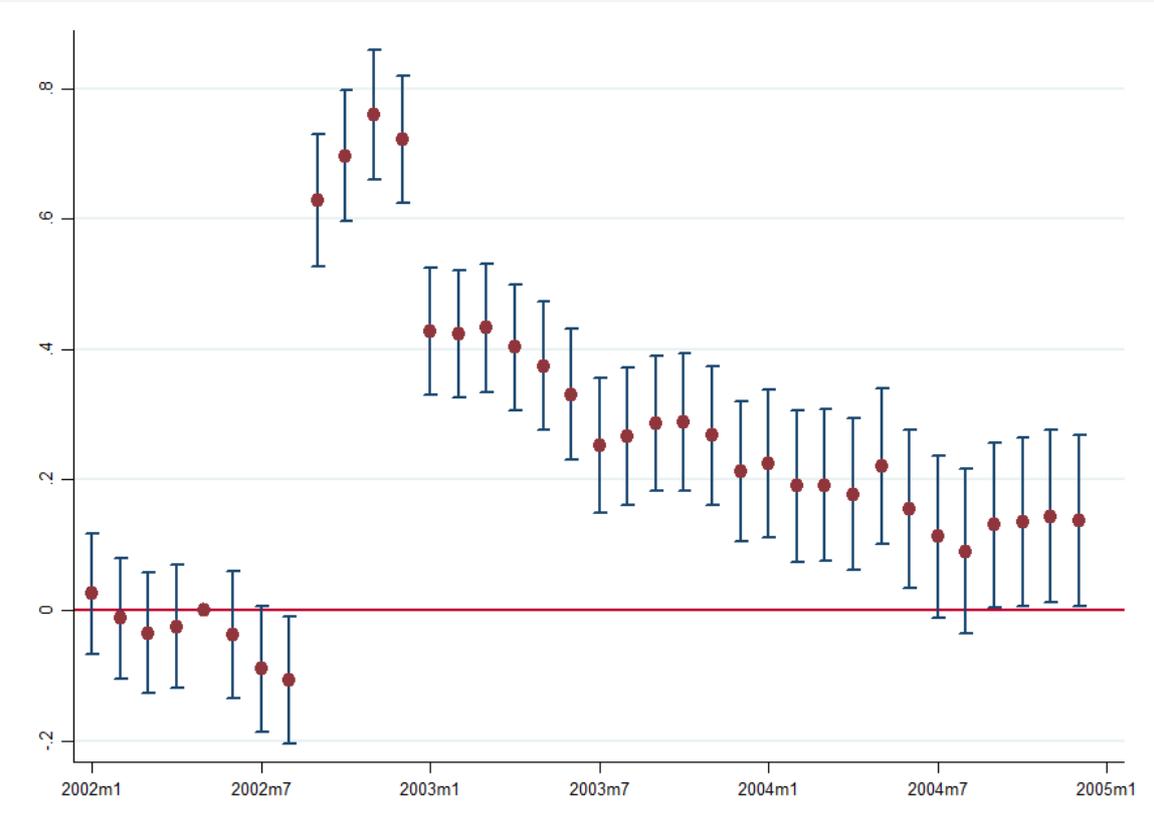
The Figure shows the probability of staying in employment for three groups of workers. “Regular migrants” refers to all migrants regularly hired for the first time in 2000 or 2001. “ Ex Bossi-Fini” refers to all migrants regularized under Law 195/2002. “Italian” refers to all native workers regularly hired for the first time in 2002.

Figure 12: Firm level employment, by month. Descriptive evidence from regularizing and “eligible” firms



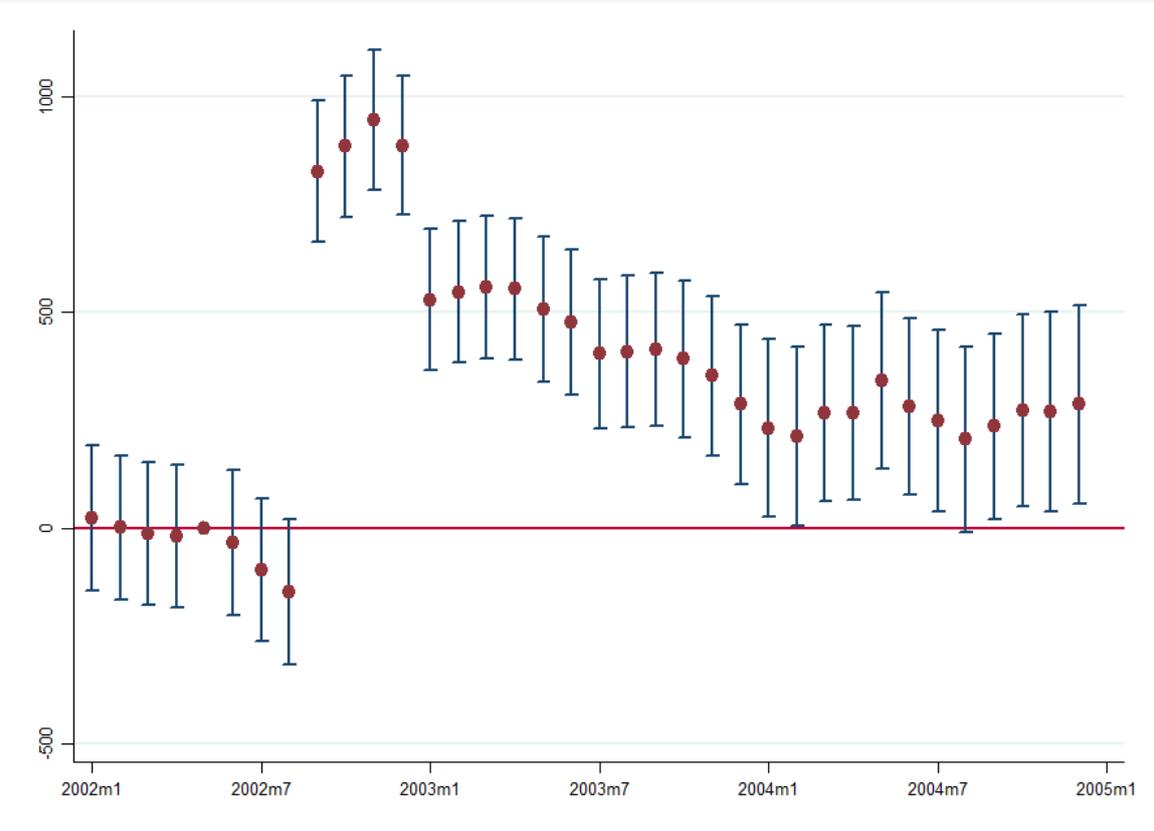
The Figure reports monthly employment for regularizing and eligible firms. We include all firms active in 2002 and constituted before 2002 and exclude the 99th percentile of firms in terms of employment in May 2002 and the firms in the 1st and 99th percentiles in the change in employment between May and December 2002.

Figure 13: DID Firm level employment, by month. Evidence from regularizing and “eligible” firms



The Figure reports results from a DID on monthly employment for regularizing and non regularizing firms. Errors clustered at firm level. Reference period: May 2002. We include all firms active in 2002 and constituted before 2002 and exclude the 99th percentile of firms in terms of employment in May 2002 and the firms in the 1st and 99th percentiles in the change in employment between May and December 2002. Controls included: number of inspections in t-1 in the cell, cell dimension, firm’s fixed effects.

Figure 14: DID Firm level monthly wage, by month. Evidence from regularizing and “eligible” firms



The Figure reports results from a DID on monthly wage for regularizing and non regularizing firms. Errors clustered at firm level. Reference period: May 2002. We include all firms active in 2002 and constituted before 2002 and exclude the 99th percentile of firms in terms of employment in May 2002 and the firms in the 1st and 99th percentiles in the change in employment between May and December 2002. Controls included: number of inspections in t-1 in the cell, cell dimension, firm’s fixed effects.