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Abstract

We estimate the long-run effects of ineligibility for legalization on immigrants' formal employment and assimilation at work. Our empirical approach exploits the exogenous change in probability of obtaining legal status induced by a 2002 Italian amnesty program targeting irregular foreign workers. We show that immigrants unexposed to the amnesty have a 15% lower probability of being regularly employed a decade later than their counterparts. They also experience a deterioration in their working conditions in the long run, with increases in job immobility and segregation, and a decline in linguistic assimilation.

JEL classification: J15, J61, K37.

Keywords: Undocumented immigrants; Amnesty program; Formal employment; Discrimination; Segregation.

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1 Introduction

Immigration policy often reflects governments' perception that incoming flows might hurt labor market outcomes of the native population or alter existing social equilibria. In recent years, many countries have adopted both plans aimed at curbing future inflows (e.g., by imposing stricter regulations for obtaining a work permit, intensifying border controls or increasing sanctions) and large-scale amnesty programs which, under given conditions, mandate the legalization of existing sub-populations of undocumented immigrants.

Amnesties are especially controversial. While some believe that they would unfairly reward people that violate the law and potentially attract additional inflows, others claim that it is fair to recognize immigrants' contribution to the host economy (Chau, 2001; Carter, 2005; Bohn et al., 2014).[1] Another argument upheld in support of amnesties is that, given that a large fraction of undocumented immigrants is typically placed within the informal labor market, their legalization not only can help decreasing the size of the shadow economy, but also foster their assimilation, thus raising the welfare of the host country (Kossoudji and Cobb-Clark, 2002; Dustmann et al., 2017; Monras et al., 2018).

In this paper, we address how missing out on eligibility for amnesty programs affects formal employment of unauthorised immigrants and their assimilation in the workplace in the long run. We consider a large-scale amnesty implemented in Italy in 2002 and, using extremely rich survey data on immigrant households collected in 2011, we study how (the lack of) eligibility for legalization affects their likelihood of being employed in the formal market as well as their segregation and discrimination at work 10 years later.

The case of Italy offers a unique setting to evaluate the impact of legalization policies on immigrants' labor market outcomes and social inclusion for three main reasons. First, while the number of unauthorized migrants living in Europe in 2016 peaked at an estimated range between 4.1 and 5.3 million (Connor and Passel, 2019), Italy has been on the front-line of migration to the European continent during the past years and is expected to be heavily exposed to an unprecedented growth in immigration pressure in the future (Hanson and McIntosh, 2016).

Second, with the release of around 700,000 work permits, the 2002 Italian amnesty program, commonly referred to as the *Bossi-Fini* law, is one of the largest ever implemented in recent years. As a means of comparison, the 2014 Zapatero Reform granted

[1] A recent review by Fasani (2015) debates on the importance to show causal evidence on the consequences of granting amnesties.

amnesty to nearly 600,000 non-EU citizens, while the 2018 Colombian Permiso Especial de Permanencia to 440,000 Venezuelan immigrants.[2]

Third, the peculiar features of the 2002 Italian legalization program, which conditioned eligibility on a *de facto* predetermined minimum residence requirement and on being employed in the informal sector at the time of application, provide a natural experiment to estimate the causal effect of ineligibility for regularization on immigrants' participation and assimilation into the formal labor market of the host country.

The data we employ provide detailed retrospective information on the immigrants' arrival to Italy and on their first job spell in the country. We exploit the year of arrival to select the relevant pool of individuals, while the year their first employment started and the type of working relationship (i.e., "with contract" or "orally") allow us to identify immigrant workers employed in the formal and informal sectors who were exposed to the amnesty depending on the timing of their first job spell.

Thus, we adopt a difference-in-differences strategy and compare outcomes of immigrants who were potentially exposed to the amnesty (i.e., who started their first job in the informal sector before 2002) and those who were unaffected because they undertook their first job in the informal sector after the program. Then, we compare their difference with the analogous difference in the outcomes of immigrants who started their first job in the formal sector before and after the amnesty.

Using an event-study approach, we provide direct evidence on the absence of pre-treatment differentials across immigrant workers who entered the market with a regular contract and those who did not. Furthermore, this approach allows us to rule out potential anticipation effects. In the spirit of Pei et al. (2019), we show that such differences do not depend on specific "treated" sub-groups of the population, since targeting is largely homogeneous in covariates (namely, demographics, migration-related information and employment characteristics).

In principle, granting legal status may alter the composition of the foreign workforce and induce immigrants to self-select into specific types of jobs (Epstein and Weiss, 2001; Karlson and Katz, 2003; Gang and Yun, 2007). For instance, the amnesty might generate a magnet effect for new arrivals, especially if further legalization programs are expected in the near future. Vice versa, new inflows might be discouraged and immigrants might choose

[2] In terms of resident population at the time of the amnesty enactment, the Italian legalization program targeted around 1.2 immigrants per 100 residents, while those implemented in Spain and Colombia affected 1.4% and 0.9% of the resident population, respectively.

to go elsewhere if they think they have missed the opportunity of receiving legal status and this will not be repeated soon. As for the existing stock of unemployed immigrants, these might be redirected into sectors or industries where the incoming labor supply has been disrupted.

In our case, the 2002 amnesty was accompanied by the tightening of the prerequisites to obtain a residence permit for new entrants, who were now required to have a standing job contract before arrival. Thus, the effect we estimate might suffer from bias due to the potential positive selection of post-2002 immigration cohorts and the consequent shock to the labor supply. We address this issue by focusing on immigrants who arrived in Italy up to 2002 only. This selection resolves the issues related to the potential change in the type of newcomers after the amnesty, by inferring only on the population of immigrants already present in the country. We also show that the characteristics of workers in the formal and informal sectors do not differ before and after the change in rules, suggesting the absence of a disproportionate disruption in the supply of immigrant workforce as well as the absence of selective out-migration. This is crucial to the identification of our estimates because it rules out the potential compositional bias arising from attrition in the sample.

Our estimate of interest indicates that immigrants working in the informal sector who were not exposed to the amnesty are 15% less likely to be in formal employment in the long run compared to immigrants workers with a regular job who started before the program. In addition, while there are no differences across immigrants who start working in the formal sector before or after the amnesty, those starting off in the informal market are 37% less likely to switch to a regular job in the long run. Our results are robust to the selection of the sample on different arrival dates as well as to the inclusion of covariates related to past employment experience. Importantly, treated and control units do not display differential trends in their propensity to work, which might potentially hide unobservable confounding characteristics, meaning that the two groups do not differ in this respect. This latter exercise can also be interpreted as a placebo test for the validity of the design, since the amnesty is not meant to increase the employability *per se* but only to facilitate the exit from informality.

Then, we uncover the existence of additional side effects of not being eligible for legal status, which relate to immigrant discrimination and segregation at work. The policy relevance of this evidence is high and the literature mostly overlooked these long run consequences. First, while we document no differences in terms of gender or age, our results show that immigrants from African countries suffer additional penalties from their status, which would be consistent with employers discriminating on the basis of appearance or ethnic origin (Bansak and Raphael, 2001; Edo et al., 2019; Duguet et al., 2010). Moreover, we find that the adverse effects of not being exposed to the regularization program are especially salient for workers in labor-intensive industries (i.e., agriculture, construction and manufacturing), possibly due to the higher share of informality in these sectors, whereas they are attenuated for those employed in less labor-intensive industries (i.e., trade and services).

Second, we provide evidence that is coherent with irregular work trapping immigrants in jobs of worse conditions and with poorer prospects in the long run. Our results suggest, in fact, that not benefiting from the amnesty yields lower job mobility - which is also potentially related to lower wages (Simón et al., 2014; Kossoudji and Cobb-Clark, 2002) - and higher levels of ethnic segregation in the workplace. Our estimates indicate that ineligible immigrants are less likely to interact with native colleagues by 18% and this is accompanied by a lower ability to speak the language of the host country by 40%.

Yet, we do not find indication of an effect on perceived dissatisfaction, neither in terms of experience of discrimination nor desire to change job. As a matter of fact, immigrants unexposed to the amnesty are significantly more likely to report that their current working conditions are less unfortunate than their previous experiences before arrival. This could be linked to the fact that having to endure irregularity in the market might reduce expectations of undocumented workers (Ong and Shah, 2012).

Our work contributes to the vast literature that examines the labor market and assimilation outcomes of immigrants in the host country (see, e.g., Borjas, 1994, 2003; Dustmann, 1996; Dustmann et al., 2005; Amuedo-Dorantes and De la Rica, 2007; Barrett and McCarthy, 2008; Manacorda et al., 2012; Beerli et al., 2021) and especially to the strand focusing on the impacts of regularization programs for undocumented immigrants. Previous analyses have mainly studied the labor market prospects and wage differentials of legalized workers (Kossoudji and Cobb-Clark, 2002; Orrenius and Zavodny, 2003; Kaushal, 2006; Chassamboulli and Peri, 2015; Amuedo-Dorantes and Bansak, 2011; Devillanova et al., 2018; Di Porto et al., 2018; Monras et al., 2018; Amuedo-Dorantes et al., 2020; Bahar et al., 2021). Other studies have also looked at the impact that legalization policies have on crime (Baker, 2015; Mastrobuoni and Pinotti, 2015; Pinotti, 2017; Fasani, 2018), consumption (Dustmann et al., 2017), welfare (Machado, 2017), fertility (Lanari et al., 2020) and gender differences (Amuedo-Dorantes et al., 2007).[3]

Our paper closely relates to Di Porto et al. (2018) and Devillanova et al. (2018), who also analyze the effects of the 2002 Italian amnesty on immigrants' labor market outcomes. Di Porto et al. (2018) focus on the short-term impact on firm employment and firm-level wages using administrative data from the Italian Social Security Agency. Devillanova et al. (2018) exploit the plausibly exogenous discontinuity in eligibility based on the date of arrival to study how the prospects of legal status affect the employment outcomes of undocumented immigrants using data collected by a non-governmental organization operating in the city of Milan. We depart from them in terms of research question, identification, data and outcome variables.

In particular, we are able to combine retrospective information on the start of the first job spell and whether this was in the formal (i.e., on a contract) or informal (i.e., agreed orally) sector, which is something previous research could hardly attain, especially when using data representative at the national level. Our difference-in-differences approach relies on an almost ideal control group, as we can compare immigrants in the same job spell cohort working on a contract or not, while holding constant many important individual characteristics. This is an improvement with respect to the existing literature, which typically builds on less comparable control groups in similar contexts. Moreover, while both Di Porto et al. (2018) and Devillanova et al. (2018) examine the short-run impact of the program, our analysis refers to the long term effects on outcomes that are measured up to 14 years later. Finally, we do not only concentrate on employment measures, but also look at relevant effects in terms of discrimination and segregation in the workplace, which entrusts this work a more comprehensive perspective.

The remainder of the paper is as follows. In the next section we describe the institutional context, the data and the identification strategy. Section 3 reports our results, while section 4 concludes.

2 Setting and Data

In this section, we describe the institutional context in which the analysis is framed. Then, we illustrate the data source, the selection of the sample and the identification strategy, which is based on a difference-in-differences set-up.

[3] Similarly, others investigate the impact of acquiring citizenship, (e.g., Bratsberg et al., 2002; Gathmann and Keller, 2018; Hainmueller et al., 2019; Govind, 2021).

2.1 Institutional Context

Italy changed from being a country of mass emigration to one of mass immigration during the 1970s. In 1981 the Italian Census registered 321,000 foreign citizens. The first regularization program of around 100,000 undocumented immigrants was implemented in 1986 (Law 943/1986), with the aim of guaranteeing foreign workers the same rights as natives. Since then, different laws have regulated immigration by narrowing incoming flows and setting pre-determined number of accesses (quota) on the basis of labor market needs. Regular immigrants that stay after the expiration of their permit of stay or those exceeding the quota, are considered to be irregular if detected in the territory (Law 39/1990) and since 1998 they are mandated to be detained into temporary centers (40/1998). By 2001, the population of registered foreigners in the Census stood at around 1.3 million, while the number of irregular immigrants was estimated to be around half a million.[4]

The Law 189/2002, also known as the *Bossi-Fini* law, and its accompanying Decree-Law 195/2002 represent a strong discontinuity with respect to previous actions, especially in terms of regularization of undocumented immigrants already present in the Italian territory. The core of the reform consists of limitations in the ways non-EU immigrants could obtain a residence permit.[5] Differently from the past, when entry permits to job seekers were admitted, the law requires immigrants to have a standing work contract that allows self-sustainment before their arrival, making the regularization procedure more stringent and restrictive.[6]

The *Bossi-Fini* law also addressed the issue of irregular immigrants that were already present in Italy by allowing a large-scale amnesty for undocumented workers. Applications could be submitted by firms declaring that they had informally employed an irregular immigrant continuously for at least three months before the day of the approval of the law. Employers had to pay an amnesty fee and manifest their willingness to legally hire the worker under a renewable contract lasting at least one year at a minimum salary of 439 euros per month.[7]

[4] https://www.ismu.org/wp-content/uploads/2021/05/Stime-stranieri-irregolari_ISMU_Anni-1991_2020.xls

[5] In addition, it tightens the norms against the aiding and abetting of irregular immigrants. Moreover, it mandates forced detention (and no longer intimation of detention) and subsequent deportation of all immigrants found on Italian ground and lacking the necessary documentation.

[6] Entry for family reunification is still permitted to spouses, children and parents aged above 65 of a regular immigrant, subject to their inability to provide for themselves otherwise.

[7] Applications, which were based on a self-declaration form (given the impossibility to factually prove the start of the informal employment spell), could be submitted over a period of two months, from September

From the first draft (end of February 2002) to the final approval of the regulation (9 September 2002) only a few months passed. This limits to some extent the chances of anticipation effects that might have influenced the behavior of the economic agents involved in the regularization. This is even more important if one considers that the amnesty was originally intended to target family caregivers only, while other workers in the private sector were included in the regularization only at a later stage. Eventually, around 95% of applicants were granted legal status. This resulted in the largest regularization in Italian history, with more than 700,000 undocumented immigrants receiving a permit.

2.2 Data

In this study, we exploit the natural experiment provided by the 2002 amnesty for undocumented workers to study the long term effects of starting off without a regular job on labor market and assimilation outcomes. To do so, we use a special survey on the conditions and social integration of foreign citizens conducted by the Italian National Institute of Statistics (ISTAT) in 2011.

The data contain a rich list of variables which make the survey a valuable and unique tool to study the integration and assimilation process of foreign citizens in Italy. These encompass family composition, education, migration and work history, current working conditions and other aspects concerning social participation, including experiences of discrimination and victimization. The data provide information on a sample of around 12,000 resident households where at least one member is a resident foreign citizen.[8]

We take all foreign-born individuals aged 28 to 75 at the time of the interview in 2011, meaning that we consider only immigrants who are likely to be in the labor force around the time of the policy change (i.e. aged 18-65 in 2001). To obtain a homogeneous sample, we keep only individuals who are born outside of Italy but have not obtained the Italian nationality, have arrived after compulsory schooling only, and have worked in Italy at least once but have found their first job after their arrival. We also drop the few immigrants

to November 2002. Employers and employees became no longer prosecutable for any irregularity occurred prior to the date corresponding to the minimum 3-month period of irregular work. The fee was equivalent to roughly three months of overdue social security contributions. Immigrants with a criminal record and those against whom an expulsion order had already been issued for reasons other than the failure to renew a previous residence permit were ineligible to the regularization. See Devillanova et al. (2018) and Di Porto et al. (2018) for further details.

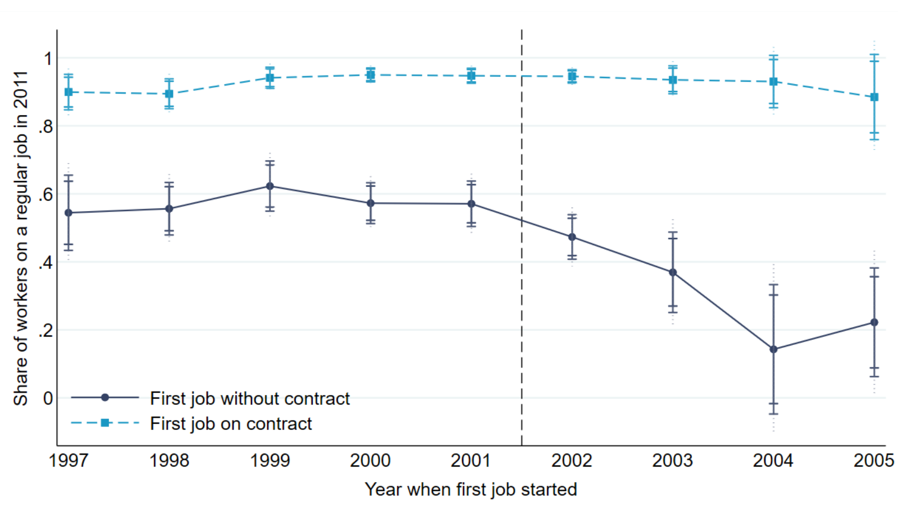
[8] The survey is representative of the 2011 foreign population. It is conducted in full on all foreign or Italian naturalized citizens, while for household members having Italian citizenship since birth only basic socio-demographic characteristics are provided.

with multiple arrivals to Italy and those that have changed foreign nationality at least once to rule out differences in preference for staying abroad.

Finally, we keep only individuals that have migrated to Italy up to 2002 to account for the provisions of the *Bossi-Fini* law. In fact, the changes in immigration regulation, and especially the fact that immigrants were now required to have a job contract before their arrival, might induce selection in the type of incoming foreign population. As the new law is enforced at the end of 2002, we consider immigrants who have started their first job within a 4-year time interval around the policy change (i.e. between 1997 and 2005), while already present in the territory before 2003. The final sample is reduced to 3,927 observations, as summarised in Table A.1.

The amnesty determined the regularization of undocumented foreign citizens that were working in Italy without a regular contract. Thus, we expect immigrants that are not exposed to the program, i.e., those who had their first job on a contract, not to change behaviour from before to after the change in policy. Indeed, in Figure 1 we show that around 94% of the workers who had started off on a contract are regularly employed in

Figure 1: Share of workers on formal employment in 2011 by first-job contract



Note: Raw data, immigrants employed in 2011 only (n=3,306). Markers display the share of immigrant workers regularly employed in 2011 by year of first employment in Italy and whether the first job was on a contract (blue squares) or not (navy circles). Vertical bars indicate confidence intervals at 90, 95 and 99% level of significance. The dashed vertical line splits the period into before and after the amnesty.

2011, and this share is constant across all cohorts of first-job workers. On the contrary, immigrant workers targeted by the regularization are those who had their first job agreed orally (i.e., informally) and started their first spell by three months before the approval of the amnesty. Hence, those whose first job started in the second half of 2002 or later did not benefit from the amnesty. The trend reported in Figure 1 shows that 57% of immigrants who were eligible for regularization are employed in the formal market in 2011, while for those who missed the opportunities offered by the amnesty the share of workers on a contract by 2011 drops significantly, from 47% in 2002 to 22% in 2005.[9]

2.3 Empirical Strategy

We disentangle the effect of stepping into the labor market of the host country irregularly from the impact of other factors influencing the probability of working on a regular contract in 2011, exploiting the quasi-experimental setting offered by the 2002 amnesty of the *Bossi-Fini* law in a difference-in-differences design. We build on the fact that the 2002 amnesty allowed the regularization of informal workers that had been employed for at least three months by 2002. Thus, we combine information on the year when the first job in Italy started and the type of working relationship, namely, "with contract" or "orally" (i.e., without a contract).

We define our intention-to-treat variable F_i as equal to one if the first job in Italy was regulated orally, with the idea that these jobs are most likely in the informal sector, and hence, eligible for regularization under the amnesty. Conversely, immigrants who started off with a contract can be considered as formally employed and are used as control group.

Immigrant workers with a regular or irregular job might not be perfect substitutes, as they might present differences in unobservables such as aspirations or talent that could be correlated with a specific preference for a type of contract or different employment rates. The difference-in-differences approach specifically accounts for variations in the ability of the different types of immigrants to find a regular job as well as to start their first job in a given year. Thus, we estimate the following model:

$$Y_i = \alpha + \beta \mathbb{1}(Year \geq 2002) + \gamma F_i + \delta \mathbb{1}(Year \geq 2002) * F_i + \rho X_i + \epsilon_i, \qquad (1)$$

[9] Such pattern also excludes that this decrease is mechanically generated by irregular workers needing some time to find a job in the regular sector, rather than by the change in policy. If that was the case, one should observe a downward sloping pattern since the beginning of the period considered (i.e., also for those with an informal first employment in the years prior to 2002).

where the coefficient of interest δ is associated with the interaction between the intention-to-treat variable F_i and a binary indicator for starting the first job after the 2002 amnesty. This identifies the effect of starting off as undocumented worker in the informal labor market of the host country.

As outcomes Y_i, we consider employment-related variables and indicators capturing the degree of discrimination and segregation experienced by the respondents in the workplace, measured at the time of the interview in 2011. We account for potential heteroskedasticity by clustering standard errors at country-of-origin times area-of-residence level.[10]

In its full specification, the model includes a large number of control variables and fixed effects which are enclosed in the set X_i. These encompass gender (also interacted with household type, to account for differentials in working propensity across genders when children are present), age, education, marital status, number of children, area of residence, type of municipality, country of origin, year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, indicators for who helped the person migrating to Italy and who hosted them upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival.[11] These variables are described in Table A.2.

Under the parallel trends assumption, the behavior of immigrants who had their first job in Italy on a contract (i.e., in the formal sector) can be used as the counterfactual for the performance of immigrant workers that where exposed to the change in regulation (i.e., those employed in the informal sector). In simple words, in the absence of the 2002 amnesty, the two groups would have maintained differences in outcomes similar to the ones observed in baseline period (namely, the years prior to 2002). We provide support to the causal interpretation of our results in several ways.

First, we include a very rich set of covariates and fixed effects that could potentially be correlated with differential trends in unobservable factors.

Second, the critical assumption for our identification strategy is that differences in the outcomes between those who started on a regular or irregular job are not associated with

[10] Results are identical if standard errors are clustered based on country of origin, country-of-origin-specific arrival cohorts or employment cohorts.

[11] Country of origin is defined on the basis of citizenship at birth and accounts for 26 (groups of) regions of the world: Romania, Poland, Other EU countries, Albania, Ukraine, Moldova, Macedonia, Other European countries, Morocco, Tunisia, Egypt, Other Northern African countries, Central and Southern African countries, Eastern African countries, Western African countries, China, Philippines, Other Eastern Asian countries, India, Bangladesh, Other Southern Asian countries, Western Asian countries, Ecuador, Peru, Other Latin American countries, North American countries.

differential trends in the absence of the amnesty. We test for the existence of differentials between treated and controls in the pre-implementation period to ensure that self-reporting of the past job status is not endogenously related to pre-treatment differentials in the outcomes measured at the time of the survey interview. We consider the following event-study specification:

$$Y_i = \alpha + \sum_{j=1997}^{2006} \beta_j F_i \times \mathbb{1}[Year = j] + \sum_{j=1997}^{2006} \mathbb{1}[Year = j] + \gamma F_i + \rho X_i + \epsilon_i \tag{2}$$

where, taken 2001 as baseline, if the leads are not statistically different from zero, this implies that treated individuals are trending similarly to their controls prior to the amnesty, and this constant heterogeneity vanishes in differences. Although not a formal proof, this test is typically interpreted as supportive of the parallel trends assumption.

Third, we implement the test of the identifying assumption suggested by Pei et al. (2019), using our covariates on the left-hand side of the main regression. If such test provides null effects, meaning that the observables are not affected by the coefficient of interest, the design is presumed to be reliable. We use a similar reasoning to test for the existence of attrition in our sample, which might lead to biased estimates. We run this test separately for workers whose first job was in the formal and informal sectors. If the pool of workers assigned to the post-treatment period have similar characteristics to those who started working before 2002, it is unlikely that the composition of either group changed from before to after the amnesty, e.g., due to selective out-migration during the years 2002 to 2011.

Finally, we estimate the same model by considering the probability of being employed at the time of the interview as a sort of falsification exercise. In fact, if immigrant workers who started off on a regular or irregular job differ in terms of their propensity to be employed a decade later, this might signal the existence of differences in unobservable characteristics across the two groups. We also check for the absence of systematic treatment effects by considering additional fake treatment dates.

3 Results

In this section we present the results on the long-run effects of ineligibility to regularization. First, we focus on the probability of being formally employed in 2011. Then, we discuss indirect effects in terms of segregation and discrimination in the workplace.

3.1 Long-Run Effect on Formal Employment

We start by estimating our main model as from Equation 1 on the probability of being regularly employed on a contract in 2011. Table 1 displays estimates associated with the main variables of interest: a dummy identifying immigrants who had their first job without a contract (i.e., our intention-to-treat variable F_i), a dummy for starting the first job on 2002 or later, which identifies immigrants who started working after the amnesty, and the interaction between the two, which defines the differential effect of being ineligible for employment regularization. Column 1 reports the unconditional estimates, while in the following two columns we gradually add two sets of demographic and migration-related control variables. Coefficients are remarkably stable across columns, and we consider the fully-specified model in column 3 to be our preferred specification.[12]

Table 1: Long-run effect on formal employment

	(1)	(2)	(3)
	Probability of having a job in the formal sector in 2011		
First job w/o contract	-0.359***	-0.337***	-0.346***
	(0.037)	(0.029)	(0.028)
$\mathbb{1}(Year \geq 2002)$	0.005	0.004	-0.015
	(0.012)	(0.012)	(0.019)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-0.163***	-0.143***	-0.134***
	(0.033)	(0.029)	(0.028)
Observations	3,306	3,306	3,306
R-squared	0.239	0.332	0.347
Included controls:			
Demographic		✓	✓
Migration-related			✓

Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors are clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality, and country of origin. Migration-related controls encompass year of arrival, the reason for migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian, and their desire to settle in Italy upon arrival.

[12] Also, controlling for whether the immigrant resides in the same province of arrival, to account for the individual's propensity to move, yields identical results. The same applies if we only include predetermined characteristics and exclude potentially endogenous controls: education, household type, marital status,

The estimated coefficients imply that immigrants starting off their employment career in Italy without a contract (i.e., in the informal sector) are less likely to have a regular job in the long-run by around 35 percentage points, i.e. 37% compared to the baseline.[13] The dummy identifying the post-amnesty period is never statistically nor economically relevant, which also suggests that the more stringent policy on entries from outside the EU enacted in parallel with the amnesty did not affect the job finding rate in the formal sector for immigrants who had already arrived in the country before the change in law.

The interaction term is negative and strongly significant. This indicates that the likelihood of being employed in the formal market in the long run decreases for immigrant workers ineligible to the recognition of a regular contract (i.e., were not exposed to the amnesty).[14] The negative effect amounts to 13 pp, which is equivalent to a drop of about 14% with respect to the pre-amnesty average value of those working on a contract. Thus, the total effect of starting off in the informal sector for those beginning to work after 2002 is larger than 50%.

Finally, in Table A.3 we compare workers whose first employment spell started before or after 2002, in the informal sector only. We find that immigrants who were not exposed to the amnesty are 17 percentage points less likely to be formally employed in the long run with respect to those that were potentially eligible to the amnesty.

Robustness checks and identification issues

We verify the robustness of our result with a battery of checks, presented in Table A.4. In the first column, we include further employment-related control variables, which account for whether the individual ever worked in the country of origin, whether they found the first job via informal channels (e.g., via family or friends), and the industry and skill level of their first job in Italy. In columns 2 to 4 we absorb any residual heterogeneity that may be related to differences in ethnic geographical clusters and immigration waves over time by including, respectively, the interaction of country-of-origin and area-of-residence fixed effects, country-of-origin-specific linear trends and country-of-origin-specific linear

number of children, area of residence, type of municipality and whether other family members live in Italy.

[13] The baseline is the share of individuals who started working in on a contract before 2002 and that are employed in the formal sector in 2011, i.e., 94%. This is also the average value of the blue squares to the left of the dashed line in Figure 1.

[14] This result is also consistent with Govind (2021), who finds that gaining citizenship via marriage in France leads to an increase by 29% in annual earnings and that this is explained by an increase in declared work following naturalization.

trends by type of first contract. Then, we restrict our main sample to further increase the comparability of the individuals in our sample. We select immigrants who arrived in Italy between 2001-2002 only (i.e., within two years from the amnesty, column 5), and individuals who have migrated up to 2001 only (i.e., those fully exposed to the amnesty, column 6). In all cases, estimated coefficients are in line with the main result.

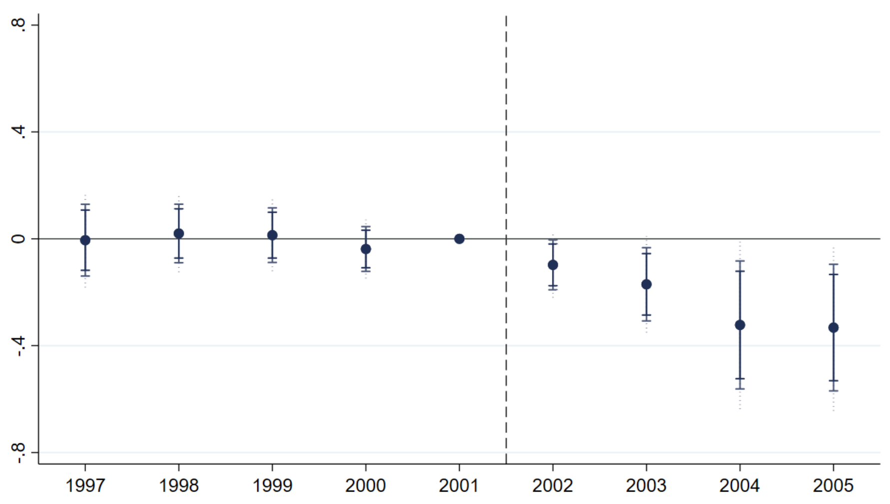
In the last two columns of Table A.4 we consider the special conditions applying to EU citizens. At the time of the policy change, only citizens of the EU15 group had similar rights as Italian nationals (i.e., they did not need any permit to live and work in Italy), while for the citizens of the countries that entered the EU in 2004 and in 2007 Italy applied a period of transitional restrictions, during which a permit was required to work in some industries.[15] However, our country of origin indicator only allows to distinguish between Romania, Poland and other countries belonging to the EU in 2011. Thus, we restrict the sample to immigrants coming from non-EU countries (column 7) and from EU countries only (column 8). As expected, the overall results hold in both columns, while somewhat attenuated in the case of the sample including also EU15 immigrants.

Then, we address the possibility that immigrants who start working long after their arrival might be negatively selected. Although our main estimates already account for the year of arrival, we re-run our model on different sub-samples where we gradually exclude individuals based on the number of years elapsed between their migration and their first employment spell. In Figure A.1 we show that our main estimate is not sensitive to considering only individuals who have found their first job within one to ten years since their arrival.

The credibility of the differences-in-differences design can be enhanced explicitly by showing that the data support the assumptions of the model. Figure 2 reports the coefficient estimated in the event-study as from Equation 2. This confirms the absence of differentials between treated and controls units in the pre-amnesty period, which allows us to rule out that the policy is endogenously related to pre-treatment differentials in the outcome. Hence, it suggests that the parallel trends assumption on which our identification rests is likely to hold. In fact, as shown earlier in Figure 1, the two groups follow

[15] EU15 countries include Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Sweden, Spain and the UK. Countries that accessed the EU in 2004 are Czech Republic, Cyprus, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia and Slovenia. In 2007, Bulgaria and Romania also joined the EU. The transitional restrictions did not apply to Malta and Cyprus. See https://ec.europa.eu/commission/presscorner/detail/en/IP_11_506 and https://ec.europa.eu/commission/presscorner/detail/en/MEMO_11_259.

Figure 2: Long-run effect on formal employment, event study



Note: Event-study analysis as from Equation 2 on the probability of having a job in the formal sector in 2011. Includes demographic (gender - also interacted with household type, age, education, marital status, number of children, area of residence, type of municipality and country of origin) and migration-related controls (year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival). Robust standard errors clustered at the country-of-origin×area-of-residence level. Vertical bars indicate confidence intervals at 90, 95 and 99% level of significance. The dashed vertical line splits the period into before and after the amnesty.

a parallel pattern over the period 1997-2001, with a 35-pp constant difference. After the 2002 amnesty is executed, it is the treated group of non-eligible informal workers which diverges from the previous path and decreases substantially, while the behavior of those in the control group is unchanged. This also alleviates concerns over a potential selection into the treatment for those who did not find a job by 2002, following the approval of tougher pre-requisites at entry in 2002, which might have induced increased competition in the regular sector.

Next, we consider the two canonical tests suggested by Pei et al. (2019). First, we obtain indication that the identifying assumptions are supported because the estimated effect of interest is not sensitive to adding covariates on the right-hand side of the regression, as we do in columns 2-3 of Table 1 and column 1 of Table A.4. The same conclusions can be drawn from Figure A.2, where we demonstrate that the estimated coefficients in the event-study analysis are remarkably robust to the inclusion of different sets of covariates. A second test consists of placing such variables on the left-hand side of the regression, instead of the outcome variable. Here, one should expect that the treatment of interest, namely, $\mathbb{1}(Year \geq 2002) * F_i$, does not yield a coefficient different from zero, following the rationale of the balancing tests that are typically carried out on baseline characteristics or pre-treatment outcomes in randomized control trials and regression discontinuity designs. We perform such test and find encouraging evidence on covariates being balanced, as shown in Figure A.3.

We also show that observable individual characteristics do not differ across immigrants who started working before or after the amnesty, separately for those who had their first job on a contract or not (Figure A.4).[16] This indicates that the composition of the two groups is fairly stable across employment cohorts and that the potential disruption induced by a change in the type of incoming workforce is negligible. This exercise also reassures us on another issue that might undermine the reliability of our analysis and that is related to a potential disproportionate attrition in our sample. If positively-selected immigrants who started their first job without a contract where more inclined to out-migrate after 2002 and, therefore, less likely to be interviewed in 2011, our effect might be overestimated (Dustmann and Görlach, 2016b).[17] Reassuringly, the coefficients associated with the post-2002 treatment are aligned around zero, especially for what concerns variables that might be proxying ability (i.e., education) and migration-related aspects (e.g., reason of migration, presence of network in Italy, intentions upon arrival).

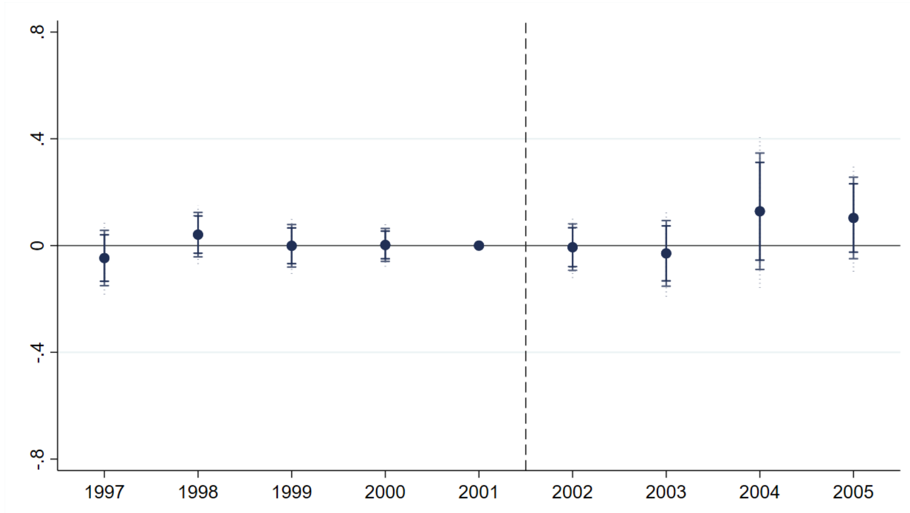
As last piece of evidence in support of our identification, we perform two types of falsification exercises. First, we focus on the employment probability. In fact, the amnesty program was meant to only induce undocumented workers to move from the shadow economy to the formal sector, without affecting the total supply of immigrant workers. Thus,

[16] We do observe, however, a larger probability of being female in the post-amnesty years. Moreover, immigrants who start working after the amnesty tend to be younger and of more recent arrival cohorts. These differences, however, are likely to be mechanically determined by the selection of the sample.

[17] In other words, we might be mis-measuring the effect of interest if abler or more ambitious immigrants who had their first job regulated orally and were not exposed to the amnesty decided to move away from the country. This would result in an under-sampling of these individuals in 2011. As a consequence, we would not be able to disentangle the effect of the amnesty from the fact that immigrants who begin working in the post-amnesty years were negatively-selected. See also Borjas and Bratsberg (1996), Dustmann and Görlach (2016a) and Borjas et al. (2019) for a discussion on selection issues related to temporary or return migration.

we should expect to find no evidence of a differential change in employability across the two groups of treated and control immigrants. Figure 3 confirms the absence of any diverging pattern on the probability of being employed in 2011. The average coefficient is also reported in column 1 of Table A.5. This result implies that, *ceteris paribus*, the individuals in our sample are comparable also in their long-run propensity to work, attenuating concerns as regards the potential negative selection of those who did not benefit from the amnesty.[18] Finally, in columns 2 to 4 of Table A.5 we show that restricting the sample to the pre-amnesty years only and assigning false cut-offs in 1998, 1999 and 2000 does not produce any statistically or economically relevant effect.

Figure 3: Long-run effect on employment, event study



Note: Event-study analysis as from Equation 2 on the probability of being employed in 2011. Includes demographic (gender - also interacted with household type, age, education, marital status, number of children, area of residence, type of municipality and country of origin) and migration-related controls (year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival). Robust standard errors clustered at the country-of-origin×area-of-residence level. The vertical bars indicate confidence intervals at 90, 95 and 99% level of significance. The dashed vertical line splits the period into before and after the amnesty.

[18] We also do not detect any differences in the probability of being in part-time work or having a second job in 2011.

Who is penalized the most by missing the amnesty?

We also examine whether there are relevant heterogeneities across groups of workers. With this, we seek to identify possible sub-populations of immigrants for which the response in terms of scarring effect of informal employment is more or less marked. Thus, we augment our model with interactions of the baseline coefficients with a dummy indicating the corresponding sub-group of interest. In Table 2 we display the results by gender, continent of origin, age, job finding mode and industry.

While there seem to be no differences in terms of gender or age (columns 1 and 5), the evidence by continent of origin reveals interesting results. Although the coefficient associated to the European continent is not statistically significant, its sign is consistent with an attenuated negative effect (column 2).[19] Conversely, those associated to the sub-groups of workers from Africa and Asia suggest that the effect may be exacerbated (columns 3-4). This is especially true for African immigrants, for whom the coefficient is negative, large and statistically significant. This would be supportive of the hypothesis that employers tend to discriminate on pay and job conditions primarily on the basis of personal appearance and ethnic origin (Bansak and Raphael, 2001; Edo et al., 2019; Duguet et al., 2010).

At the same time, we find no evidence of a role of networks (column 6), as those who found their first job thanks to relatives or friends are not differentially likely to be on a regular contract in 2011 with respect to workers who have resorted to formal channels (e.g., job posts, agencies, etc.). This would be in line with the overall absence of a pay-off generated by a greater use of personal networks amongst non-white immigrants in the UK estimated by Battu et al. (2011).[20]

Last, columns 7 and 8 indicate that immigrants working in labor-intensive industries (i.e., agriculture, construction and manufacturing) at the beginning of their career suffer

[19] Here, the dummy takes value one for immigrants originating from any country within the European continent, i.e., EU and non-EU countries. The attenuated effect is consistent with both EU immigrants not being subject to any work permit conditions and with Europeans in general being less likely to be discriminated on the basis of their physical appearance. We also find no differences for the small sample of immigrants from Latin America. Immigrants from North American countries are only 34, while there are none from Oceania.

[20] Additionally, there are no differences across individuals having above and below secondary education. Yet, females and workers that resorted to informal channels and who were exposed to the amnesty (i.e., had their first job without a contract which started before 2002) have a higher probability of working regularly in 2011 by 12 and 28 percentage points, respectively. This is expected because the amnesty was initially meant to regularize especially family caregivers, where workers are typically women and word of mouth is important.

Table 2: Long-run effect on formal employment, heterogeneities

	(1)	(2)	(3)	(4)
	Probability of having a job in the formal sector in 2011			
First job w/o contract	-0.400***	-0.334***	-0.334***	-0.340***
	(0.037)	(0.043)	(0.031)	(0.032)
$\mathbb{1}(Year \geq 2002)$	-0.010	0.003	-0.021	-0.016
	(0.025)	(0.024)	(0.019)	(0.020)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-0.167***	-0.180***	-0.117***	-0.129***
	(0.036)	(0.043)	(0.030)	(0.033)
First job w/o contract * **H**	0.121***	-0.021	-0.070	-0.042
	(0.045)	(0.057)	(0.073)	(0.056)
$\mathbb{1}(Year \geq 2002)$	-0.004	-0.029	0.038	0.012
	(0.023)	(0.024)	(0.033)	(0.035)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$ * **H**	0.035	0.075	-0.122*	-0.030
	(0.063)	(0.059)	(0.071)	(0.067)
Observations	3,306	3,306	3,306	3,306
R-squared	0.353	0.348	0.350	0.348
Heterogeneity (**H**)	Female	Europe	Africa	Asia
	(5)	(6)	(7)	(8)
	Probability of having a job in the formal sector in 2011			
First job w/o contract	-0.312***	-0.570***	-0.334***	-0.381***
	(0.035)	(0.041)	(0.028)	(0.048)
$\mathbb{1}(Year \geq 2002)$	-0.011	0.036	-0.027	0.011
	(0.025)	(0.023)	(0.020)	(0.026)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-0.179***	-0.114**	-0.103***	-0.214***
	(0.043)	(0.045)	(0.035)	(0.044)
First job w/o contract * **H**	-0.067*	0.275***	-0.036	0.054
	(0.037)	(0.042)	(0.047)	(0.048)
$\mathbb{1}(Year \geq 2002)$	-0.008	-0.064***	0.033	-0.041*
	(0.027)	(0.021)	(0.023)	(0.023)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$ * **H**	0.089	-0.009	-0.099*	0.120**
	(0.063)	(0.057)	(0.056)	(0.057)
Observations	3,306	3,306	3,306	3,306
R-squared	0.349	0.370	0.349	0.351
Heterogeneity (**H**)	Aged 40+	Informal	Labor intens.	Not labor int.

Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival.

greater penalties as opposed to those employed in less labor-intensive ones (i.e., trade and services), where the long-run effects of not being regularized seems to be attenuated. This might be due to irregular workers being largely represented in labor-intensive industries and in predominantly low-skilled tasks (Kossoudji and Cobb-Clark, 2002).[21]

[21] Indeed, Di Porto et al. (2018) report that within both the manufacturing and the construction industries around 21% of firms inspected in 2001 have been found with some irregularities concerning the

3.2 Effects on Segregation and Discrimination at Work

Our results so far indicate that not being exposed to legalization under the amnesty is associated with a substantial decrease in the probability of having a job on a contract in 2011, which raises concerns on other potential unintended indirect effects. Apart from being kept in the informal labor market for long, in fact, non-regularized immigrant workers might experience other undesirable side effects in terms of segregation and discrimination in the workplace. We address these questions by considering a set of outcome variables that concern the interactions occurring at work, the reported experience of discrimination and the perceived quality of the job.

In column 1 of Table 3 we find evidence of a lower job mobility for workers not exposed to the amnesty, who are significantly more likely to be on the same job as their first employment spell by 21% compared to the baseline value. This is in line with Di Porto et al. (2018), who argue that the regularized immigrants appearing in the administrative registries tend to be more mobile across industries in the short-run.

Moreover, we observe that immigrants who have not been exposed to the legalization procedure tend to be more ethnically segregated in the workplace. In fact, they are remarkably less likely to interact with native colleagues by 17% with respect to the baseline (column 2). A similar clue is provided by the coefficients reported in columns 3 and 4, which, although not statistically significant, suggest that they may be less inclined to speak Italian at work. As a matter of fact, column 5 shows that these immigrants are more likely to report having difficulties in communicating in Italian over the phone by almost 40%. Overall, these results suggest that missing the chance to receive a work permit under the amnesty yields a higher degree of segregation in the workplace and this implies also a lower assimilation in terms of ability to speak the language of the host country.

In spite of the above evidence, differences in the perceived discrimination in the workplace do not seem to arise (column 6).[22] In addition, column 7 gives indication that the irregular workers unaffected by the amnesty are not more prone to change job, and, if anything, we estimate a negative coefficient. This, however, could be explained by the fact that these individuals perceive their working conditions in the host country (both in terms

workforce, whereas within other industries this share is always below 5%, with the exception of hotels and restaurants (24%) and sales (18%). As for agriculture, which is excluded from the analysis of Di Porto et al. (2018), ISTAT estimates 19 irregular workers per 100 employees in 2001.

[22] In our sample, 96% of the individuals who have experienced discrimination, report that this was ethnic-related.

Table 3: Long-run effects on segregation and discrimination at work

	(1) Same as first job	(2) Work peers mostly Italian	(3) Usually speaks Italian at work	(4) Difficulties with Italian at work
First job w/o contract	-0.364***	-0.208***	0.009	-0.005
	(0.030)	(0.033)	(0.014)	(0.010)
$\mathbb{1}(Year \geq 2002)$	0.010	0.075**	0.011	0.005
	(0.032)	(0.031)	(0.014)	(0.012)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	0.137***	-0.105**	-0.027	0.015
	(0.037)	(0.042)	(0.020)	(0.026)
Observations	3,306	2,354	3,306	3,306
R-squared	0.265	0.169	0.113	0.074
Average outcome	0.655	0.622	0.926	0.031
	(5) Difficulties with Italian on the phone	(6) Discriminated at work	(7) Wish to change job	(8) Worse job condit's here
First job w/o contract	-0.015	0.052***	0.071***	0.063***
	(0.014)	(0.015)	(0.018)	(0.017)
$\mathbb{1}(Year \geq 2002)$	-0.052***	0.026	0.027	-0.005
	(0.017)	(0.023)	(0.029)	(0.020)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	0.049*	0.003	-0.041	-0.065**
	(0.028)	(0.031)	(0.037)	(0.025)
Observations	3,306	3,306	3,271	3,286
R-squared	0.176	0.077	0.074	0.070
Average outcome	0.126	0.156	0.198	0.129

Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival.

of pay and tasks) as less unfortunate than their previous experiences before the arrival in Italy (column 8). That is, not undergoing legalization might keep irregular workers in a trap of hardship and lack of better prospects in the long run. In other words, regularized workers, together with a work permit and better employment conditions, might gain awareness on their (potential) positive returns to immigration.

4 Conclusions

This paper investigates a large-scale amnesty program implemented in Italy in 2002 to study how ineligibility for regularization affects immigrants' employment and assimilation outcomes in the long run. We use a rich survey to identify immigrant workers that are employed in the formal and informal labor markets and evaluate the likelihood to exit from the shadow economy. We also provide novel results on several measures of segregation and discrimination in the workplace.

Our analysis shows that immigrants who were not exposed to the amnesty face a 15% lower probability of being employed on a regular contract a decade later with respect to those who were already employed in the formal sector in the pre-amnesty years. We argue that this is not due to changes in employability, as undocumented immigrants unexposed to the legalization are as likely as their counterparts to be employed in the long run.

We also provide evidence on aspects which are typically under-explored and related to discrimination and segregation on the workplace. On the one hand, we find that not being exposed to the amnesty determines lower job mobility and higher segregation, as non-regularized immigrants report difficulties with speaking the language and are less likely to work with natives. Nonetheless, not only this does not yield job dissatisfaction, but working conditions are deemed to be less unfortunate compared to other experiences prior to migration.

Our results have important policy implications. Imposing restrictions on the regularization of undocumented workers induces an increase in their propensity to remain in the informal sector in the long run. From a macro perspective, this is undoubtedly detrimental for the overall welfare of the host country, given the serious consequences in terms of market competition and public finances determined by the presence of a conspicuous shadow economy. This analysis also informs on the risks associated to the existence of a sub-population of workers with little bargaining power. Missing out the opportunity to receive legal status via the amnesty, in fact, yields a deterioration of the working conditions, especially in terms of observed segregation in the workplace, as well as a decrease in the level of linguistic assimilation. Coupled with the evidence pointing towards immigrants' contentment about their job, this evidence suggests that non-regularized workers might develop lower expectations and ambitions in the long run.

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Appendix

Table A.1: Sample selection

Initial Sample	25,326
Individuals aged 28-75 only	-12,393
Foreign-born only	-14
Individuals with foreign nationality only	-373
Individuals who have arrived aged 16+ only	-194
Individuals who have worked in Italy at least once only	-2,069
Individuals who have found first job after arrival only	-147
Individuals who have migrated to Italy only once	-61
Individuals who have changed foreign nationality	-63
Individuals who have arrived before 2003	-3,956
Individuals who have started their first job in 1997-2005 only	-2,124
Missing info	-5
Final Sample	**3,927**

Table A.2: Descriptive statistics

Variable	Mean	Std. Dev.	Min	Max
First job in the formal sector	0.634	0.482	0	1
Start of first job	2000	1.793	1997	2005
Demographics				
Female	0.481	0.500	0	1
Household type: Single w/o children	0.286	0.452	0	1
Household type: Couple with children	0.489	0.500	0	1
Household type: Couple w/o children	0.140	0.347	0	1
Household type: Single father	0.012	0.111	0	1
Household type: Single mother	0.073	0.260	0	1
Age	41.169	8.523	28	71
Education: No education	0.080	0.271	0	1
Education: Primary school	0.054	0.227	0	1
Education: Middle school	0.272	0.445	0	1
Education: Secondary school	0.456	0.498	0	1
Education: Degree or more	0.137	0.344	0	1
Marital status: single	0.223	0.416	0	1
Marital status: married	0.532	0.499	0	1
Marital status: divorced	0.211	0.408	0	1
Marital status: widowed	0.035	0.183	0	1
Number of children	1.385	1.124	0	5
Residence: North West	0.203	0.402	0	1
Residence: North East	0.204	0.403	0	1
Residence: Centre	0.213	0.410	0	1
Residence: South/Island	0.379	0.485	0	1
Type of municipality: City	0.304	0.460	0	1
Type of municipality: Town below 10k pop	0.204	0.403	0	1
Type of municipality: Town above 10k pop	0.492	0.500	0	1
Migration-related characteristics				
Year of arrival	2000	2.237	1980	2002
Reason in Italy: easier travel	0.166	0.372	0	1
Reason in Italy: easier life	0.621	0.485	0	1
Reason in Italy: personal reasons	0.144	0.351	0	1
Reason in Italy: other	0.069	0.253	0	1
Reason left home: economic	0.775	0.417	0	1
Reason left home: family	0.168	0.374	0	1
Reason left home: education	0.018	0.131	0	1
Reason left home: war	0.040	0.195	0	1
Has other family in Italy	0.596	0.491	0	1
Who helped migrating: no one	0.611	0.488	0	1
Who helped migrating: people in Italy	0.234	0.423	0	1
Who helped migrating: people outside Italy	0.038	0.191	0	1
Who helped migrating: agencies/govt	0.075	0.263	0	1
Who helped migrating: other	0.043	0.202	0	1
Accommodation upon arrival: own	0.004	0.065	0	1
Accommodation upon arrival: rent	0.182	0.386	0	1
Accommodation upon arrival: other	0.033	0.179	0	1
Accommodation upon arrival: employer	0.057	0.232	0	1
Accommodation upon arrival: family	0.392	0.488	0	1
Accommodation upon arrival: other people	0.307	0.461	0	1
Accommodation upon arrival: shelters	0.024	0.153	0	1
Could speak Italian upon arrival	0.294	0.456	0	1
Intentions upon arrival: stay	0.312	0.463	0	1
Intentions upon arrival: go back	0.400	0.490	0	1
Intentions upon arrival: go elsewhere	0.288	0.453	0	1

Table A.3: Long-run effect on formal employment, treated group only

	(1)	(2)	(3)
	Probability of having a job in the formal sector in 2011		
$\mathbb{1}(Year \geq 2002)$	-0.158***	-0.132***	-0.173***
	(0.032)	(0.030)	(0.049)
Observations	1,210	1,210	1,206
R-squared	0.020	0.282	0.323
Included controls:			
Demographic		✓	✓
Migration-related			✓

Note: * p<.10 ** p<.05 *** p<.01. Workers who started in the informal sector only. Robust standard errors are clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality, and country of origin. Migration-related controls encompass year of arrival, the reason for migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian, and their desire to settle in Italy upon arrival.

Table A.4: Long-run effect on formal employment, robustness checks

	(1)	(2)	(3)	(4)
	Probability of having a job in the formal sector in 2011			
First job w/o contract	-0.346***	-0.342***	-0.346***	-0.823***
	(0.028)	(0.029)	(0.028)	(0.111)
$\mathbb{1}(Year \geq 2002)$	-0.010	-0.015	0.012	0.008
	(0.019)	(0.019)	(0.026)	(0.027)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-0.130***	-0.128***	-0.135***	-0.127***
	(0.029)	(0.028)	(0.029)	(0.032)
Observations	3,306	3,304	3,306	3,306
R-squared	0.358	0.368	0.354	0.374
Included controls:				
Demographic	✓	✓	✓	✓
Migration-related	✓	✓	✓	✓
Job-related	✓			
Country * Area of residence FE		✓		
Country linear trends			✓	
Country * Group linear trends				✓
	(5)	(6)	(7)	(8)
	Probability of having a job in the formal sector in 2011			
First job w/o contract	-0.300***	-0.346***	-0.321***	-0.401***
	(0.046)	(0.027)	(0.023)	(0.033)
$\mathbb{1}(Year \geq 2002)$	-0.006	0.002	-0.014	-0.034
	(0.029)	(0.019)	(0.023)	(0.043)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-0.166***	-0.195***	-0.148***	-0.111**
	(0.048)	(0.055)	(0.045)	(0.047)
Observations	1,294	2,613	2,358	939
R-squared	0.377	0.347	0.364	0.362
Included controls:				
Demographic	✓	✓	✓	✓
Migration-related	✓	✓	✓	✓
Sub-sample:				
Arrival cohorts 2001-2002 only	✓			
Arrival cohorts $\leq$ 2001 only		✓		
No EU countries			✓	
EU countries only				✓

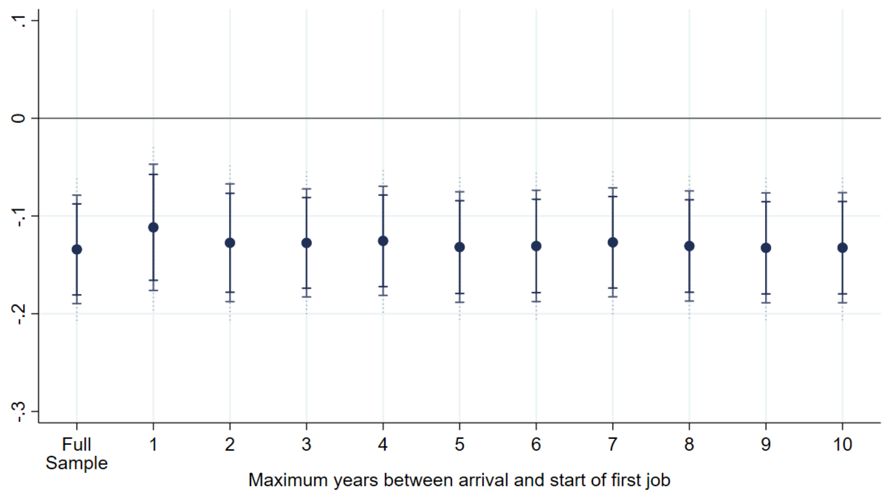
Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival. Employment-related controls include whether the individual ever worked in the country of origin, whether they found the first job via formal channels and the industry and skill level of their first job in Italy.

Table A.5: Placebo tests

	(1) Probability of having a job in 2011	(2) Probability of having a job in the formal sector in 2011	(3)	(4)
First job w/o contract	-0.042**	-0.353***	-0.326***	-0.330***
	(0.017)	(0.053)	(0.038)	(0.034)
$\mathbb{1}(Year \geq 2002)$	-0.033			
	(0.022)			
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	0.002			
	(0.031)			
$\mathbb{1}(Year \geq 1998)$		-0.008		
		(0.043)		
First job w/o contract * $\mathbb{1}(Year \geq 1998)$		0.009		
		(0.051)		
$\mathbb{1}(Year \geq 1999)$			0.033	
			(0.027)	
First job w/o contract * $\mathbb{1}(Year \geq 1999)$			-0.024	
			(0.038)	
$\mathbb{1}(Year \geq 2000)$				-0.014
				(0.025)
First job w/o contract * $\mathbb{1}(Year \geq 2000)$				-0.028
				(0.037)
Observations	3,927	2,251	2,251	2,251
R-squared	0.112	0.330	0.331	0.331
Included controls:				
Demographic	✓	✓	✓	✓
Migration-related	✓	✓	✓	✓

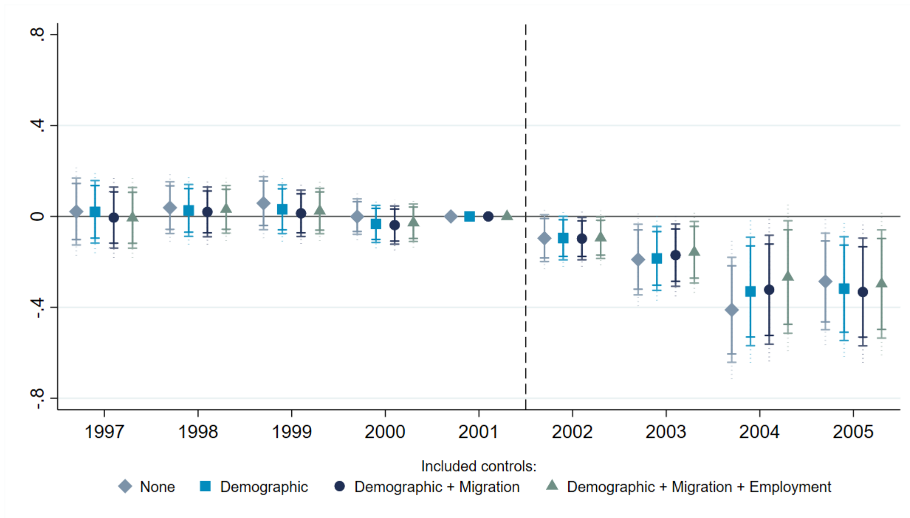
Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival. Employment-related controls include whether the individual ever worked in the country of origin, whether they found the first job via formal channels and the industry and skill level of their first job in Italy. Columns 2-4 refer to the sample of immigrants who have started working prior to 2002 only.

Figure A.1: Long-run effect on formal employment, by years since arrival



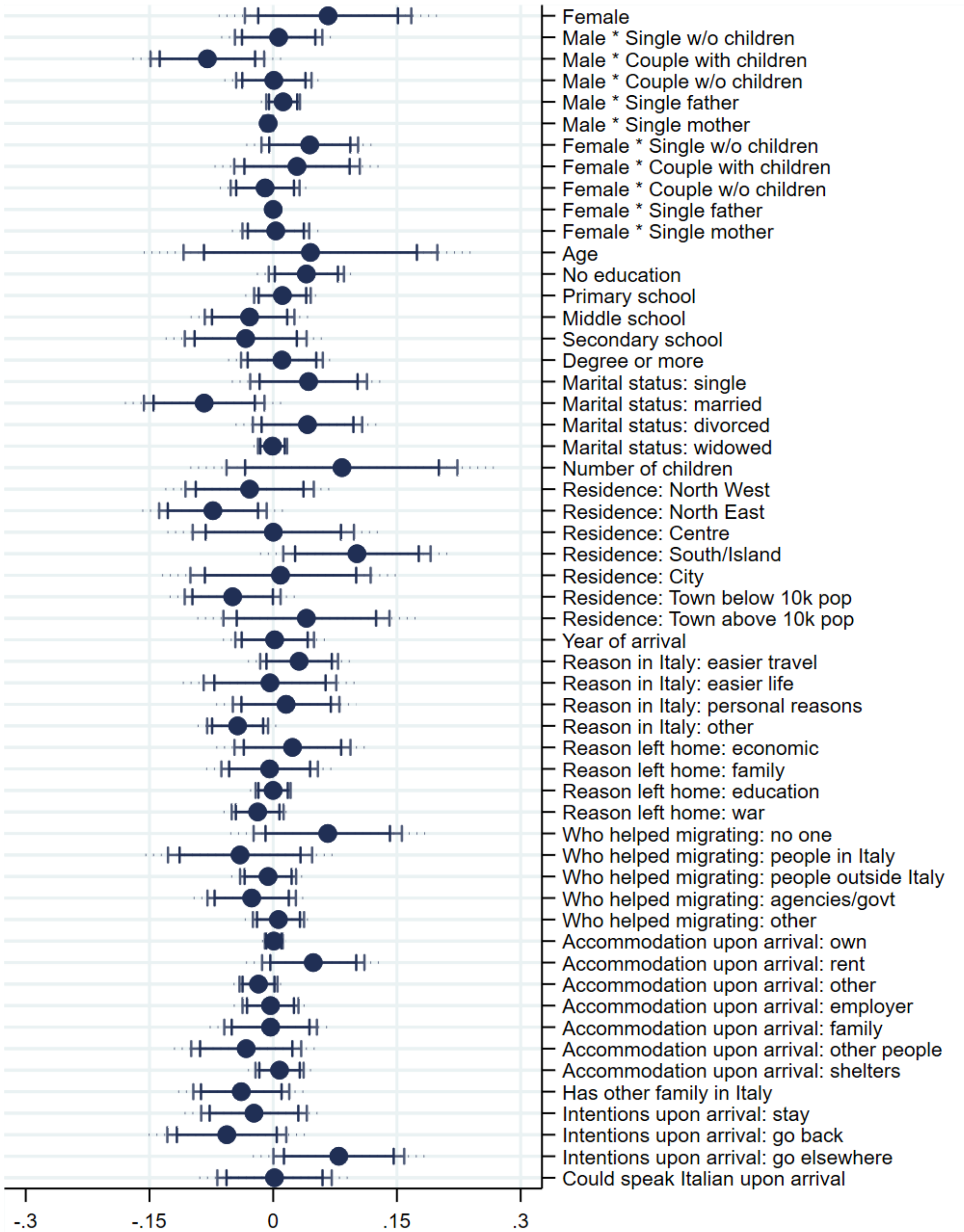
Note: The plot presents estimates as from Equation 1 on the probability of having a job in the formal sector in 2011, where each coefficient refers to the sub-sample of immigrants with maximum a given number of years between arrival and start of the first job, except for the first one on the left, which refers to the main estimate as in column 3 of Table 1. Includes demographic (gender - also interacted with household type, age, education, marital status, number of children, area of residence, type of municipality and country of origin) and migration-related controls (year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival). Robust standard errors are clustered at the country-of-origin×area-of-residence level. The vertical bars indicate confidence intervals at 90, 95 and 99% level of significance.

Figure A.2: Long-run effect on formal employment, sensitivity of event study



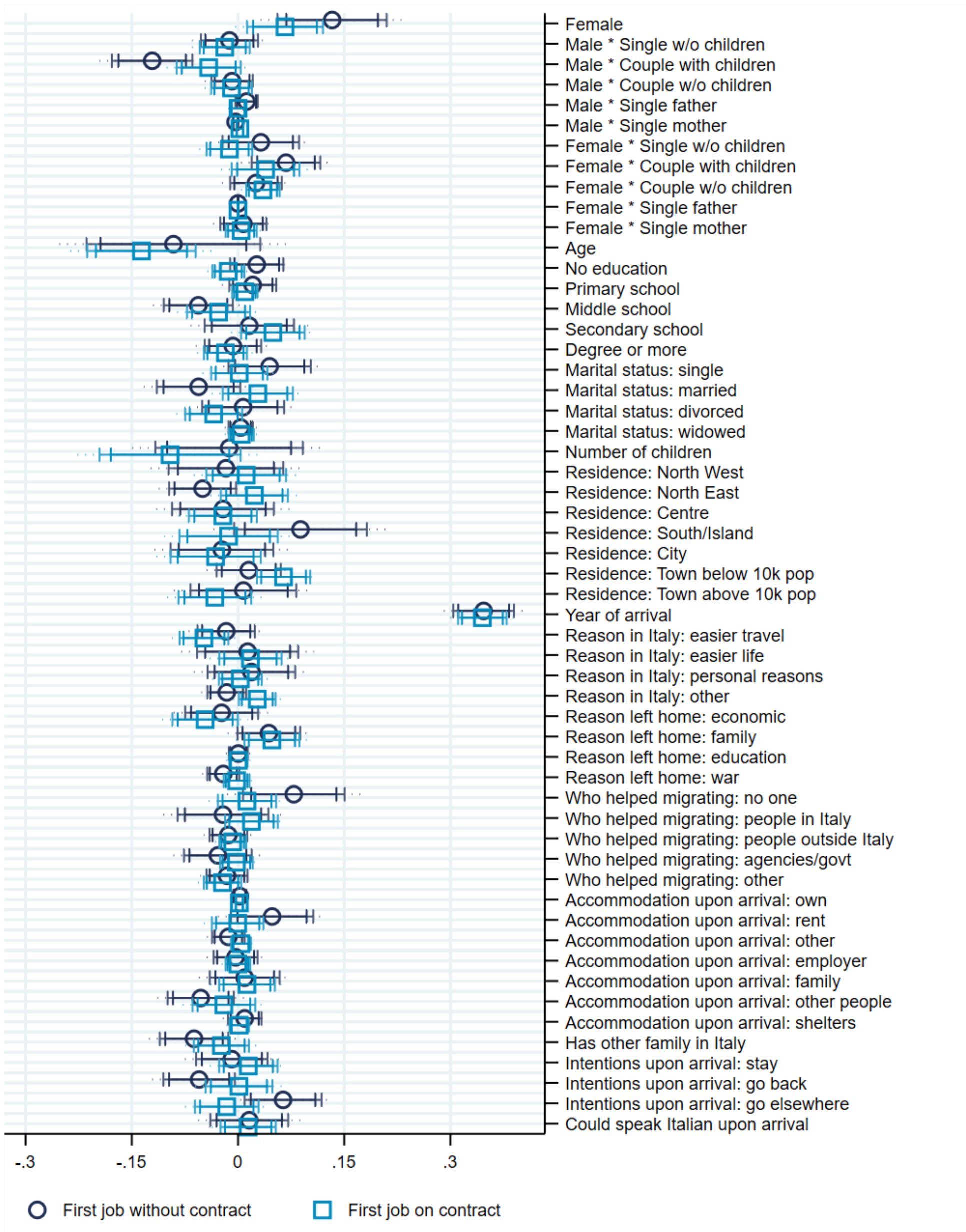
Note: The plot presents the event-study analysis as from Equation 2 on the probability of having a job in the formal sector in 2011. Demographic controls include gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, the reason of migration from the home country, the reason of migration to Italy, whether other family members live in Italy, who helped migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival. Employment-related controls include whether the individual ever worked in the country of origin, whether they found the first job via formal channels and the industry and skill level of their first job in Italy. Robust standard errors are clustered at the country-of-origin×area-of-residence level. The vertical bars indicate confidence intervals at 90, 95 and 99% level of significance. The dashed vertical line splits the period into before and after the amnesty.

Figure A.3: Balancing test on covariates



Note: The plot presents the estimated coefficient associated to the treatment where each covariate included in the analysis is used as outcome, in the spirit of Pei et al. (2019). Robust standard errors are clustered at the country-of-origin×area-of-residence level. The horizontal bars indicate confidence intervals at 90, 95 and 99% level of significance.

Figure A.4: Balancing test on covariates by first-job contract



Note: The plot presents the estimated coefficient associated to the post-2002 dummy where each covariate included in the analysis is used as outcome, separately on the sub-samples of individuals whose first job was on a contract (blue squares) or not (navy circles). Robust standard errors are clustered at the country-of-origin×area-of-residence level. The horizontal bars indicate confidence intervals at 90, 95 and 99% level of significance.