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Legalization and Long-Term Outcomes of Immigrant Workers

Abstract

This paper establishes a new fact about immigration policies: legalization has long-term effects on formal employment of undocumented immigrants and their assimilation. We exploit the broad amnesty enacted in Italy in 2002 together with rich survey data collected in 2011 on a representative sample of immigrant households to estimate the effect of regularization in the long run. Immigrants who were not eligible for the amnesty have a 14% lower probability of working in the formal sector a decade later, are subject to more severe ethnic segregation on the job and display less linguistic assimilation than their regularized counterparts.

JEL-Codes: J150, J610, K370.

Keywords: undocumented immigrants, amnesty program, formal employment, discrimination, segregation.

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1 Introduction

In recent decades globalization, climate change and political instability in various parts of the world have led millions of people to migrate in search of better employment opportunities and lifestyles. According to the Pew Research Centre, in 2017 the EU-EFTA countries counted some 24 million foreigners among their half-billion residents.[1] Among them, nearly one fifth were unauthorized immigrants (Connor and Passel, 2019). In that year, the United States hosted nearly 38 million immigrants who arrived post-1980, with approximately one-third (11.4 million) being undocumented (Baker, 2021).

Within the next few years many OECD countries, particularly in Europe, will host growing numbers of undocumented or semi-documented immigrant workers (OECD, 2018). These people often constitute one of the most vulnerable sub-groups in the labor force: readily exploitable, lacking fundamental rights, and with little or no access to basic welfare. Apart from any ethical considerations, their condition may impede assimilation, nurture the shadow economy and, ultimately, diminish the overall welfare of the host country (Kossoudji and Cobb-Clark, 2002; Carter, 2005; Rozo and Winkler, 2021). This effect is likely to persist in the long run.

This paper addresses the issue directly by studying how eligibility for regularization affects the formal employment of immigrants and their assimilation in the long run. The empirical evidence on the long-run effects of regularization programs is scant, mainly due to the lack of suitable data. We aim to fill this gap in the literature by exploiting a large-scale amnesty enacted in Italy in 2002 and extremely rich survey data on immigrant households collected in 2011. We use retrospective information provided by the survey to study how eligibility for legalization affects the likelihood of being employed in the formal sector as well as segregation and discrimination on the job a decade later.

Italy offers a uniquely convenient setting for assessing the impact of legalization policies on immigrants' labor market outcomes and social inclusion for three reasons. First, in recent decades Italy has been on the front line of migration to the European continent and is expected to be heavily exposed to an unprecedented increase in immigration pressure in the future (Hanson and McIntosh, 2016). Second, with the issue of work permits to over 640,000 immigrants, the 2002 amnesty is one of the largest enacted in decades.[2] Third, the

[1] The EU-EFTA countries are the 27 EU Member States plus Iceland, Liechtenstein, Norway, Switzerland and the United Kingdom.

[2] By comparison, the 1986 Immigration Reform and Control Act (IRCA) in the USA granted amnesty to nearly 2.7 million undocumented immigrants, the Zapatero Reform in Spain in 2014 to nearly 600,000

program made eligibility conditional on a predetermined minimum residence requirement and on being employed in the informal sector at the time of application. These peculiar features provide a natural experiment to estimate the causal effect of regularization on long-term outcomes.

Our identification strategy relies on a Difference-in-Differences estimator which isolates the effect of *ineligibility* for regularization on immigrants' participation and assimilation into the formal labor market in the long run. We select the relevant pool of immigrants residing in Italy before the 2002 policy change. Using retrospective information on immigrants' work experiences, we exploit the variability of the year their first job began and the type of work relationship (i.e., "written contract" or "oral agreement") to define two groups of immigrants: those potentially eligible for the amnesty, who started their first job in the *informal* sector before 2002, and those who were ineligible because their first informal job started only after the program was enacted. Then, we take the difference in their outcomes at the time of the interview (i.e., 2011) and compare it with the analogous difference in the outcomes of immigrants who started their first job in the *formal* sector (i.e., the control group) before and after the amnesty.

Our estimates indicate that immigrants who started working in the informal sector after 2002, and for this reason were not exposed to the amnesty, are 13 percentage points less likely to be in formal employment in the long run. This translates into a 14% lower probability of having a formal job in 2011 in comparison to the control group average over the pre-amnesty period. As we cannot observe the actual residence status of individuals in the sample, we expect that our estimate is biased downward, because some of the immigrants initially employed in the informal sector (our treatment group) could have already been legally resident and hence not eligible.

We provide evidence of heterogeneous effects on formal employment. While we find no differences in terms of gender or age, we show that immigrants from Africa suffer additional penalties from their status, consistent with employer discrimination on the basis of ethnic origin (Bansak and Raphael, 2001; Edo et al., 2019; Duguet et al., 2010). Moreover, the effects are especially salient in labor-intensive sectors (i.e., agriculture, construction and manufacturing), possibly owing to the larger proportion of informal employment there.

Next, we discover additional long-run effects, relating to assimilation and segregation

non-EU citizens, and the Colombian Permiso Especial de Permanencia of 2018 legalized 440,000 Venezuelan immigrants. In terms of resident population at the time of the amnesty, the Italian program targeted around 1.1 immigrants per 100 residents, as against 1.1% in the USA, 1.4% in Spain and 0.9% in Colombia.

at work. The literature has largely overlooked these long-run consequences, despite their crucial policy relevance. Overall, our evidence supports the thesis that off-the-books work traps immigrants in jobs with worse conditions and poorer long-run prospects. We show that ineligibility for legalization produces less job mobility, which is also a possible factor in lower wages, and more severe ethnic segregation in the workplace (Simón et al., 2014; Kossoudji and Cobb-Clark, 2002). Our estimates indicate that ineligible immigrants are 18% less likely to interact with native colleagues, and have 40% less competence in speaking Italian. This is in line with Fasani et al. (2021), who also find diminished language proficiency when refugees are subject to employment restrictions.

However, we do not find indications of an effect on perceived dissatisfaction, either in terms of experience of discrimination or in a desire to change jobs. Immigrants not exposed to the amnesty are significantly more likely to assess that their current working conditions are less adverse than their experience before arrival in Italy. This could be due to the lowered expectations of undocumented workers resulting from irregular labor market status (Ong and Shah, 2012) or to their inclination to prioritize avoiding detection by authorities over maximizing labor market returns (Kossoudji and Cobb-Clark, 2002).

Amnesties 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, an amnesty might attract new arrivals, especially if further legalization programs are expected in the foreseeable future. On the other hand, inflows might be discouraged and immigrants might choose to go elsewhere if they think they have missed their only real chance to gain legal status. As for the population of immigrants already in the country, they might be redirected into sectors or industries experiencing disruptions in the incoming labor supply.

In our case, the amnesty was accompanied by a tightening of the requirements for residence permits to new entrants, who now must have a regular employment contract before arrival. Thus, our estimates might suffer from bias due to the positive selection of post-2002 immigration cohorts and a consequent shock to the labor supply. Accordingly, we focus on immigrants who arrived in Italy up to 2002 only. This selection resolves issues related to the potential change in the type of newcomers, by referring only to the population of immigrants already present. 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 that there was no disproportionate disruption in the immigrant inflow.

We conduct a series of tests, falsification exercises and robustness checks to rule out bias deriving from additional unobserved characteristics across groups or stemming from the time it takes workers to enter employment for the first time. Moreover, we address potential recall bias and sample attrition and exclude that our results are driven by selective out-migration of different sub-populations of immigrants between the early 2000s and the year of the interview.

This paper contributes to the vast literature on the labor market and assimilation outcomes of immigrants (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; Carillo et al., 2023) and especially to the strand focusing on the impacts of regularization programs for undocumented immigrants. Previous works typically study labor market prospects and wage differentials of legalized workers (Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Orrenius and Zavodny, 2003; Kaushal, 2006; Pan, 2012; Chassamboulli and Peri, 2015; Amuedo-Dorantes and Bansak, 2011; Pope, 2016; Ruhs and Wadsworth, 2018; Devillanova et al., 2018; Di Porto et al., 2018; Amuedo-Dorantes et al., 2020; Bahar et al., 2021; Elias et al., 2018; Ortega and Hsin, 2022). Other studies also examine the impact of legalization policies on crime (Baker, 2015; Mastrobuoni and Pinotti, 2015; Pinotti, 2017; Fasani, 2018), consumption (Dustmann et al., 2017), welfare (Machado, 2017) and gender differences (Amuedo-Dorantes et al., 2007).

Our analysis closely relates to Di Porto et al. (2018) and Devillanova et al. (2018), who also study the effects of the 2002 Italian amnesty. Di Porto et al. (2018) focus on firm employment and firm-level wages using administrative data from the Italian Social Security Institute (INPS). They find a short-term but not persistent (i.e., up to December 2002 only) positive effect on employment and no effect on wages. Devillanova et al. (2018) use data collected by a NGO operating in the city of Milan and exploit the plausibly exogenous discontinuity in eligibility based on date of arrival to show that the prospects of acquiring legal status substantially increase the employment outcomes of undocumented immigrants.

Nonetheless, our results are largely in line with works analyzing similar policies in other countries. For instance, Kossoudji and Cobb-Clark (2002) also find substantial penalties for non-legalized workers after the IRCA program in the USA, while Elias et al. (2018) look at the 2004 legalization in Spain and show that a large share of formerly undocumented housekeeping service immigrant workers transition to different sectors, often joining larger and higher-paying firms. They also find evidence of some spillovers onto native workers.

We differ from these studies in research question, identification, data and outcome variables. We do not consider labor market spillovers on natives and focus on a less investigated area of research: the long-run effects on immigrants' employment and assimilation. While the special features of our setting allow us to look at the sub-group of immigrants who were not eligible for the amnesty, our analysis also extends to the beneficial effects associated with regularization programs on immigrants' outcomes. In particular, our findings suggest that the probability of being formally employed in the long run increases significantly among immigrant workers who are given a regular contract. Up to now, the literature had largely neglected the durability of such effects.

Moreover, our analysis suggests that eligibility for regularization is associated with greater job mobility and lower risk of being discriminated against or segregated in the workplace. In turn, this has implications for linguistic assimilation. By studying these outcomes we provide novel and more comprehensive evidence on the effects of legalizing undocumented workers.

Last, where previous studies typically build on different comparison groups (e.g., native workers or immigrants from different countries), our approach compares immigrants in the same employment period cohort with and without formal employment contracts, while holding constant many crucial individual characteristics, such as year of arrival, country of origin, education and reason for migration.

The remainder of the paper is as follows. In the next section we describe the institutional context, the data and the empirical strategy. Section 3 reports our results and a discussion on potential identification issues (namely, selection, attrition and recall bias). Section 4 concludes.

2 Setting and Data

In this section, we illustrate the institutional context. Then we describe the data source, the selection of the sample and the identification strategy, which is based on a Difference-in-Differences (DiD) set-up.

2.1 Institutional Context

According to Italian law, all foreign citizens living in Italy are subject to immigration law. Citizens of the European Union are essentially equal to Italian citizens. They do not need a permit to live in Italy but only need to register to the civil registry.[3] Non-EU citizens must apply for a permit to live and work in Italy.

In 1981 the Italian Census registered 211,000 resident foreign citizens (0.4% of the population). The first regularization program, enacted in 1986 (Law 943/1986), aimed to extend the same rights as native Italians to around 100,000 undocumented foreign workers. Subsequent laws have regulated immigration by narrowing inflows and setting pre-determined numbers of accesses (quotas) based on labor market needs. Regular immigrants who stay beyond the expiration of their permit and those exceeding the quota are considered to be irregular, when detected (Law 39/1990). Since 1998 these persons are mandated to be detained in temporary centers (Law 40/1998). By 2001, the Census reported 1.3 million registered foreigners (2.3% of the population), with an estimated half a million irregular immigrants (ISMU, 2021).

Law 189/2002, known as *Bossi-Fini* law, with its accompanying Decree-Law 195/2002, represents a sharp discontinuity with respect to previous actions. The core of the reform consists in limitations on the ways non-EU immigrant workers can obtain a permit.[4] Differently from the past, when entry permits were based on the sponsorship of individuals or NGOs, providing *de facto* a pathway for legal residence that did not necessarily coincide with immediate employment, the *Bossi-Fini* law requires immigrants to secure a regular employment contract ensuring self-sustainment before their arrival, making the regularization procedure more stringent and restrictive. Therefore, residence permits are directly tied to the immigrant's employment status, and the cessation of the employment contract determines the loss of the residence permit.[5]

[3] This is since the Schengen Agreement and the ratification of the European Directive 2004/38/CE. The EU underwent significant enlargement in 2004 (with 10 new Member States) and 2007 (with two additional Member States). Citizens of these new Member States faced restrictions, requiring permits to work in certain industries during a transitional period.

[4] In addition, it tightens the norms against aiding and abetting irregular immigration. Moreover, it mandates forced detention (and no longer just possible detention) and subsequent deportation of all immigrants found in Italy without the necessary documentation.

[5] Apart from humanitarian reasons, permits are still granted for tourism or business (up to three months), family reunification (limited to spouses, children and parents over 65 of regular immigrants for a maximum of two years), and to students successfully enrolled in full time education. Work permits have specific durations: nine months for seasonal workers, one year for temporary contracts, and two years for permanent contracts and self-employed individuals. Renewals can be requested at the local police headquarters (Questura). Application and renewal fees range up to approximately EUR 130. After 10 years (or two years, if married to an Italian citizen) of continuous legal residence, foreign citizens can apply for Italian citizenship. Those with a valid permit can travel within Italy and the Schengen area. Access to national health services is granted regardless of nationality or permit validity.

With the new law, the government aimed to limit inflows and heavily sanction undocumented immigrants living in Italy. Yet, a significant portion of undocumented immigrants had been employed in the informal sector, and excluding them from the labour market would have generated substantial shortages. Thus, the *Bossi-Fini* law also incorporated a large-scale amnesty. At first, the regularization targeted households employing extra-EU citizens working as caregivers or housekeepers informally so that they would declare taxes and insurance contributions. After a few weeks, the scope of regularization expanded to include employers across all industries, as the informal sector in Italy is not limited to household workers. According to 2002 and 2014 data from the National Institute of Statistics (ISTAT), around 16% of workers in Italy are employed in the informal sector. Among them, 17.3% are foreign workers (2010 data, De Gregorio and Giordano, 2015). Di Porto et al. (2018) report that in both manufacturing and construction 21% of the firms inspected in 2001 had at least some workforce irregularities, whereas in other sectors this share was always below 5%, except for hotels and restaurants (24%) and retail trade (18%).

Applications could be submitted by employers declaring that they had informally employed an irregular immigrant continuously for at least three months before the enactment of the law. Employers had to pay an amnesty fee and express commitment to legally hire the worker under a renewable contract lasting at least one year at a minimum salary of EUR 439 per month. From the first draft of the bill (end of February 2002) to the final approval of the regulation (9 September 2002) only a few months passed. To some extent this limits the chances of anticipation effects. This is even more important considering that the amnesty was originally intended to target domestic workers alone, and other workers were included in the regularization only later.[6]

Eventually, 92.5% of applicants were granted legal status. This resulted in the largest regularization in Italian history, with 641,636 undocumented immigrants receiving a permit. According to the Ministry of Interior, around half of the total permits were released

[6] Applications were based on a self-declaration form, given the impossibility of effectively demonstrating when the informal employment began. Employers and employees were no longer liable for any irregularity prior to the date corresponding to the minimum 3-month period of irregular employment. The fee was equivalent to roughly three months' worth of social security contributions. Immigrants with a criminal record and those against whom an expulsion order had already been issued for reasons other than failure to renew a previous permit were ineligible. Applications could be submitted up to 11 November 2003. By 12 January 2004, only 5,525 applications out of 705,403 had yet to be processed. See the Parliament reports at: `https://leg15.camera.it/cartellecomuni/leg14/RapportoAttivitaCommissioni/commissioni/allegati/01/01_all_CorteC_2003.pdf` and `https://leg15.camera.it/cartellecomuni/leg14/RapportoAttivitaCommissioni/testi/01/01_cap09_sch05.htm`, last accessed 20 January 2024.

to domestic workers. INPS estimates that the remaining permits were granted to immigrants working within the industries with the highest concentration of informal workers: construction (37%), manufacturing (25%), hotels and restaurants (10%). Overall, around 100,000 firms benefited from the amnesty (INPS, 2017).[7]

2.2 Data

We use a dedicated survey on the conditions and social integration of foreign citizens conducted by the ISTAT in 2011. The survey provides information on a sample of around 12,000 resident households where at least one member is a foreigner.[8] The questionnaire covers a rich list of variables, which encompass family composition, education, migration and work history, current working conditions and other aspects of social participation, including experiences of discrimination and victimization. This makes it a valuable, unique tool for studying the integration and assimilation process in Italy.

We take all foreign-born respondents aged 28 to 75 at the time of the interview in 2011, meaning that we consider only immigrants who are likely to have been in the labor force around the time of the amnesty (i.e., aged 18-65 in 2001). To obtain a homogeneous sample, we keep only individuals who were born outside of Italy, are not Italian citizens, arrived after compulsory schooling age, and have worked in Italy at least once but found their first job after arrival.[9] We drop the few immigrants with multiple arrivals and those that have changed foreign nationality to rule out differences in preference for staying abroad. We also account for the provisions of the *Bossi-Fini* law and retain only individuals who migrated to Italy up to 2002. This is because the new prerequisite for immigrants to secure employment contracts prior to arrival might induce selection in the type of the incoming foreign population.

Finally, as we rely on a DiD approach and analyze the dynamics of the effect of the 2002 policy change, we select workers based on the year they started their first job. We

[7] The agriculture industry is not covered by INPS data. However, ISTAT estimates 19 irregular workers per 100 in 2001.

[8] The survey is representative of the population of resident immigrants in 2011. It was 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.

[9] All foreign-born individuals with Italian citizenship in the data are also flagged as having Italian citizenship since birth, suggesting they are likely children of Italian citizens. We exclude them as they are expected to be essentially similar to natives. We restrict the sample to those who were at least 18 in 2001 to avoid confounders deriving from participation to the Italian schooling system. This does not exclude, of course, that adult immigrants arriving to Italy might have gained no education at all in their home country.

take the year immediately preceding the policy change (i.e., 2001) as baseline, as it is standard practice in DiD settings, and consider a 4-year interval on either side of the baseline, i.e., we keep workers who have started their first job within the interval 1997-2005. With this last restriction we aim to maximize comparability across workers in the sample. This involves focusing on individuals entering the labor market within a relatively narrow timeframe, while concurrently mitigating the impact of potential confounders, such as the financial crisis in the late 2000s. The final sample is thus reduced to 3,927 observations, as summarized in Table A.1.

2.3 Empirical Strategy

We disentangle the effect of stepping into the labor market of the host country irregularly from that of other factors influencing the probability of working on a regular contract in 2011, exploiting the quasi-experimental setting offered by the 2002 amnesty in a DiD design. We build on the fact that the amnesty allowed the regularization of informal workers who had been employed for at least three months. Thus, we combine information on the year when the first job in Italy started and on the type of employment relationship, namely, "written contract" or "oral agreement".

We define the variable F_i as equal to one if the first job in Italy was under oral agreement, because this type of job arrangement is most likely to occur in the informal sector, which implies eligibility 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 regular and irregular jobs might not be perfect substitutes, as they might differ in some unobservables, such as aspirations or talent, that could be correlated with a specific preference for a type of contract or with different employment rates. The DiD 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:

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

where the coefficient of interest δ is associated with the interaction between the variable F_i and a binary indicator for starting the first job after the 2002 amnesty. This identifies the effect of starting off as an undocumented worker in the informal labor market. We do not observe residence status at the time of first employment or at the interview. If anything, this implies an underestimation of δ. While workers with a contract (i.e., the control group) are presumably known to the authorities, the treatment group may be composed of both undocumented and documented workers, the latter being registered, with a permit for family reunification, say, but still employed in the informal sector. Thus, δ identifies an "exposure-to-the-amnesty" effect.[10]

As outcomes Y_i, we consider employment-related variables and indicators capturing the degree of workplace discrimination and segregation experienced by the respondents, 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.[11]

In its full specification, the model includes a large number of control variables and fixed effects, which are encompassed in the set X_i. These refer to gender (also interacted with household type, to account for differentials in propensity to work between men and women in the presence of children), age, education, marital status, number of children, area of residence, type of municipality, country of origin, year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, indicators for who helped the person in migrating to Italy and who hosted them upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival. These variables are described in Table A.2.[12]

Under the parallel trends assumption, changes in the outcomes of immigrants who had their first job in Italy on a contract (i.e., in the formal sector) can be used as the

[10] We cannot exclude that some of the workers who started with a formal first employment contract overstay their visa and, thus, become undocumented later. In this case we would be estimating a lower bound. Additionally, as noted, the survey is run on a sample of households where *at least one* but not necessarily every member is a resident immigrant. Indeed, 21% of the respondents in our final sample reside in a household with one or more other respondents. This means that some proportion of them might be potentially undocumented in 2011. At the same time, in order to be interviewed they must be members of the same household as a registered immigrant. Thus, they could be positively selected compared to the overall population of undocumented immigrants in 2011.

[11] Country of origin, defined as citizenship at birth, envisages 26 countries or groups of countries: 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. Area of residence is classified into North-West, North-East, Centre, and South/Island. Results are identical if standard errors are clustered based on country of origin, country-of-origin-specific arrival cohorts or employment cohorts.

[12] Age and year of arrival enter our model linearly, while all the other variables are either binary or categorical. All categories are listed in Table A.2.

counterfactual for the performance of immigrant workers who were exposed to the amnesty (i.e., those employed in the informal sector at the time). To put it simply, in the absence of the 2002 amnesty the two groups would have maintained the same differences in outcomes as in the baseline period (prior to 2002).

We provide support for the causal interpretation of our results in several ways. First, we include a very rich set of covariates and fixed effects that could be correlated with differential trends in unobservable factors. Second, we test for the existence of differentials between treatment and control groups in the pre-implementation period to ensure that self-reported past job status is not endogenously related to pre-treatment differentials in the outcomes measured at the time of the survey. We consider the following event-study specification:

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

where, taking 2001 as baseline, if the leads are not statistically different from zero, this implies that treated individuals were trending similarly to their controls prior to the amnesty, so that this constant heterogeneity vanishes in differences. Although not a formal proof, this is typically interpreted as offering support for the parallel trends assumption.

Third, we test the identifying assumption as suggested by Pei et al. (2019), using our covariates on the left-hand side of the main regression. If this test has null effects, meaning that the observables are not affected by the coefficient of interest, the design is presumed to be reliable. Finally, we estimate the same model taking the probability of being employed at the time of the interview as a sort of falsification exercise: if immigrant workers who started off with regular and irregular jobs differ in their probability of being 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 other, hypothetical treatment dates.

3 Results

This section presents the results on the long-run effects of ineligibility for regularization. We first consider the probability of being formally employed in 2011 and then discuss indirect effects in terms of workplace segregation and discrimination.

3.1 The Long-Run Effect on Formal Employment

The amnesty called for the regularization of undocumented foreign citizens who were working in Italy without a regular employment contract. Accordingly, we expect immigrants not exposed to the program (i.e., those who had their first job under a regular contract) not to change behavior around the change in policy. Thus, we define as treated those workers who started their first job via oral arrangement. Our control group is made of a similar pool of workers (holding constant age, country of origin, education, reason of moving, marital status, cohort of entrance in the job market, etc.) that started their first job on a written contract. The timing is given by the years of entrance in the labour market (1997-2005). Then, we look at the changes in the likelihood of being formally employed in 2011 across cohorts and types of contract.

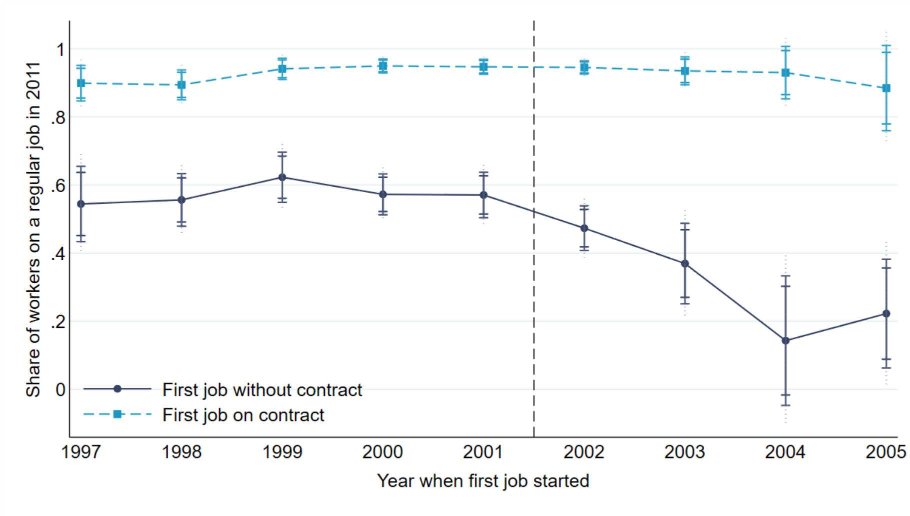
Figure 1 displays the variability across cohorts of entrance in the formal and informal employment that we exploit in the empirical strategy. The plot shows that 94% of the workers who had started off on a contract (blue squares) were employed formally in 2011, and this share is constant across all cohorts of first-job workers, i.e before and after the introduction of the amnesty program. It follows that the remaining 6% were not employed formally in 2011. By contrast, the immigrant workers targeted by the amnesty are those who had their first job regulated informally (under an oral agreement), and had started their first period of employment at least three months before the amnesty. Hence, those whose first job started in the second half of 2002 or later did not benefit from the legalization program.

Figure 1 reports that, on average, 57% of the eligible immigrants (navy circles) were employed in the formal market in 2011, while the remaining 43% were not formally employed. For those who missed the opportunity offered by the amnesty this proportion decreases substantially, from 47% in 2002 to 22% in 2005. In other words, we compare immigrants that are exposed to the amnesty and those that are not exposed (based on the type of contract in their first job), before and after the policy change.[13]

We estimate our model as from Equation 1 on the probability of being regularly employed on a contract in 2011. Table 1 reports the estimates for the main variables: a dummy identifying immigrants who had their first job without a contract (F_i), a dummy for starting the first job in 2002 or later, which identifies those who began only post-

[13] This pattern also rules out the possibility that the decrease was generated mechanically not by the policy change but by irregular workers needing time to find a job. If that were the case, one should observe a downward sloping pattern since the beginning of the period (i.e., since 1997).

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



Note: Raw data, immigrants employed in 2011 only (n=3,306). Markers show 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%. The dashed vertical line splits the period into before and after the amnesty.

amnesty, and the interaction between the two, which defines the differential effect of being ineligible for employment regularization, net out of the time-invariant differences between treated and controls. Column 1 reports the unconditional estimates, while in columns 2 and 3 we successively add two sets of demographic and migration-related control variables. We consider the model in column 3 to be our preferred specification to ensure the most conservative estimates. Nonetheless, the coefficients are remarkably stable across columns.

The dummy for having a first job without a contract, which picks up the difference between the treated and control groups in the pre-2002 years, suggests 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 p.p., i.e., 37% compared to the baseline.[14] The dummy for the post-amnesty period is never statistically significant or economically relevant, which further suggests that the restriction on entries from outside the EU enacted together with the amnesty did not affect the rate of employment in the

[14] The baseline is the average outcome of the control group in the pre-amnesty period: the probability of being employed in the formal sector in 2011 for individuals who started working on a contract before 2002, i.e., 94%. This is also the mean value of the blue squares to the left of the dashed line in Figure 1.

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 comprise 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 are year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in migrating to Italy, accommodation upon arrival, whether the individual could speak Italian, and their desire to settle in Italy upon arrival.

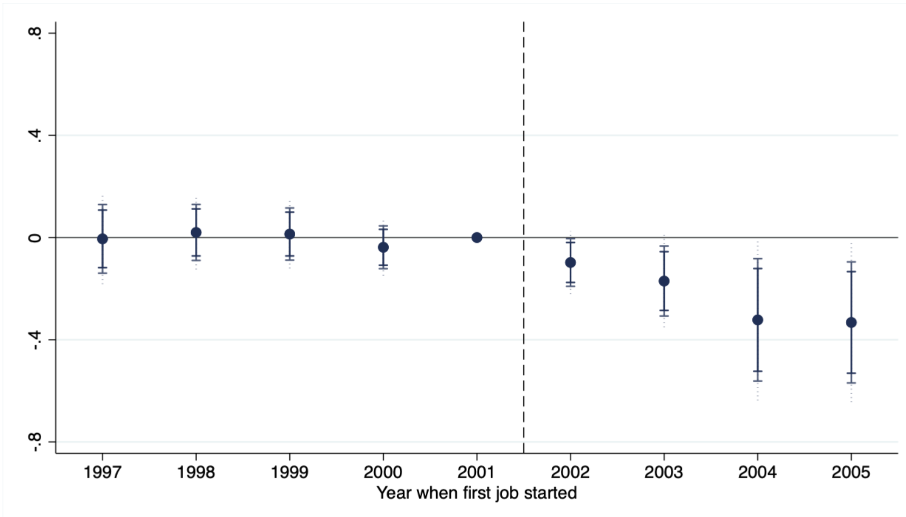
formal sector for immigrants who had already arrived before the change in the law.

The interaction term is negative and strongly significant. This indicates that the likelihood of being employed in the formal job market in the long run decreases for immigrants who were ineligible for recognition of a regular contract (i.e., were not exposed to the amnesty).[15] The negative effect amounts to 13 p.p., which is equivalent to a drop of about 14% with respect to the pre-amnesty average value of those working on contract. Thus, the total effect of starting off in the informal sector for those beginning to work after 2002 is greater than 50%.

The data support the assumptions of the DiD. Figure 2 reports the coefficient estimated in the event-study analysis as from Equation 2. This confirms the absence of differentials between treatment and control units in the pre-amnesty period, thus discarding the possibility that the policy is endogenously related to pre-treatment differentials in the outcome. That is, it suggests that the parallel trends assumption on which our identification rests is likely to hold. As shown earlier in Figure 1, the two groups follow a parallel pattern over the period 1997-2001, with a constant 35 p.p. difference. After 2002, it is the treatment

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

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



Note: Event-study analysis as from Equation 2 on the probability of employment in the formal sector in 2011. Includes demographic controls (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, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in 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%. The dashed vertical line splits the period into before and after the amnesty.

group of non-eligible informal workers that diverges from the previous path and decreases substantially, while the trend for those in the control group is unchanged. This alleviates concerns over a potential selection into the treatment for those who did not find a job by 2002, following the approval of tougher entry requirements in 2002, which might have prompted greater competition in the formal sector.[16]

According to INPS (2017), the amnesty led to a substantial increase in regularly employed immigrant workers by 411,000 units (from 730,000 in 2001 to 1,141,000 in 2002). Figure 1 indicates that around 57% of workers eligible for the amnesty were formally employed in 2011 (blue squares in the period 1997-2001). This implies 243,000 regularized

[16] This also suggests the absence of spillovers from the regularization on the control group, which would otherwise imply a violation of the Stable Unit Treatment Value Assumption (SUTVA). First, individuals in the control group are already known to the authorities, as they work in the formal sector. Second, as shown later in Figure A.5, we do not find evidence of contamination in terms of employability of control units with respect to treated workers.

workers still formally employed in 2011. Our estimated causal effect reveals that not being exposed to the amnesty bears a 14% penalty in the probability of formal employment in 2011. This suggests that without regularization only 176,730 workers would be formally employed in 2011, leaving 57,540 workers in the informal sector. Accordingly, we estimate that by 2011 the regularization generated approximately an extra 3.8 billion euros in tax revenues from labor income and social security contributions alone (i.e., 0.002% of GDP).[17]

Robustness checks

Our results are highly robust to a variety of empirical exercises, reported in Table A.3. In the first column, we include additional employment-related control variables: whether the individual ever worked in the country of origin, whether they found their 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 possible residual heterogeneity 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 trends by type of first contract. We then restrict the main sample in order to enhance the comparability of the individuals. We select immigrants who arrived in Italy in 2001-2002 only (i.e., less than two years before the amnesty, column 5), and individuals who migrated up to 2001 only (i.e., those fully exposed to the amnesty, column 6). In all cases, the coefficients are in line with the main result.[18]

In the last two columns we consider the special conditions applying to EU citizens. At the time of the policy change, only citizens of the EU15 countries had rights equivalent to Italian nationals (i.e., they did not need any permit to live and work in Italy), while the citizens of the countries that entered the EU in 2004 and in 2007 were subject to a series of restrictions for a transitional period during which a permit was required to work in some industries.[19] However, our country-of-origin indicator distinguishes only between

[17] This is likely to be a lower-bound estimate, as INPS counts do not include workers regularized in the agriculture industry and our calculations assume no salary increases over a 9-year period. Upon regularization, 97% of regularized workers are low-skilled employees and earn an average gross monthly salary of 1,174 euros (INPS, 2017), so we consider a 13th-month yearly gross salary with 23% labor tax rate on the quota exceeding a 5,261 euros tax allowance (computed based on $7,500 \times [26,000 + 7,500 - \text{gross income}/26,000]$), and 33% social security tax rate.

[18] Also, controlling for whether the immigrant still resides in the same province as that of arrival, to account for individual propensity to move, yields identical results.

[19] The EU15 were Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Lux-

Romania, Poland and other EU countries in 2011. Accordingly, we restrict the sample either to immigrants from non-EU countries (column 7) or to those from EU countries only (column 8). As expected, the overall results hold in both columns, while somewhat attenuated in the case of the sample with immigrants from EU countries only.

Threats to identification: selection, attrition and recall bias

Selection. Next, we address the possibility that immigrants who start working long after their arrival might be negatively selected. Although our main estimates already factor in year of arrival, we run the model again on different sub-samples, successively excluding individuals based on the number of years between migration and first employment. Figure A.1 shows that our main estimate is not sensitive to considering only individuals who got their first job upon arrival or after intervals of between one and ten years afterward.[20]

Then, we consider the two canonical tests suggested by Pei et al. (2019). First, we obtain an indication that the identifying assumptions are supported, because the estimated effect 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.3. The same conclusions can be drawn from Figure A.2, which demonstrates that the event-study analysis is remarkably robust to the inclusion of different sets of covariates. Importantly, the stability of the coefficients with and without individual controls rules out the possibility that the effect may be confounded by potentially endogenous control variables.

A second test consists in placing such variables on the left-hand side of the regression. Here, one should expect the treatment of interest, namely, $\mathbb{1}(Year \geq 2002) * F_i$, not to yield a coefficient different from zero, as in the balancing tests typically carried out on baseline characteristics or pre-treatment outcomes in randomized control trials and regression discontinuity designs. We perform this test and find encouraging evidence that covariates are balanced, as reported in Figure A.3.

We also show that observable individual characteristics do not differ between immi-

embourg, the Netherlands, Portugal, Spain, Sweden and the United Kingdom. Those joining the EU in 2004 were the Czech Republic, Cyprus, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia and Slovenia. Bulgaria and Romania joined in 2007. 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`.

[20] Here, the first coefficient on the left is for the sub-sample of individuals who got their job in the year they arrived in Italy. Given that we only consider workers who migrated to Italy up to 2002 and that 2002 is partially treated, this might explain why this coefficient is slightly smaller and less precisely estimated.

grants who started working before and after the amnesty, separately for those who had their first job on contract and not (Figure A.4).[21] This indicates that the composition of the two groups is fairly stable across employment cohorts and that the disruption induced by a change in the type of incoming workforce is negligible.

To further corroborate our identification, we perform two important falsification exercises. The first focuses on employment probability. The amnesty was intended only to enable undocumented workers to move from the shadow economy to the formal sector, without affecting the total supply of immigrant workers. Thus, we should expect to find no evidence of a differential change in employability between immigrants in the treatment and control groups. Figure A.5 confirms the absence of any divergence in probability of being employed in 2011. The average coefficient is also reported in column 1 of Table A.4. This result implies that, *ceteris paribus*, the individuals in our sample are comparable also in their long-run employment probability, alleviating concerns over the potential negative selection of those who did not benefit from the amnesty.[22] Second, columns 2 to 4 of Table A.4 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.

Finally, given that we reconstruct the individuals' histories using retrospective information on their first employment but observe them only in 2011, one may suspect that our analysis is affected by disproportionate sample attrition or by recall bias.

Attrition. As for the danger of sample attrition, if positively-selected immigrants who started their first job without a contract had a greater propensity to out-migrate after 2002 and so were less likely to be interviewed in 2011, our effect might be overestimated (Dustmann and Görlach, 2016b).[23] In other words, we might be mis-measuring the effect of interest if abler or more ambitious immigrants whose first job in Italy was under an oral agreement and who were not exposed to the amnesty decided to leave the country. This would result in 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 began working in the post-amnesty years were negatively selected.

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

[22] Nor do we find differences in the probability of working part-time or having a second job in 2011.

[23] 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.

There could be different reasons why immigrants arriving before 2002 might have left Italy by 2011. First, emigration from the country of origin or the choice of Italy as destination may have been for reasons correlated with the likelihood of remaining in Italy.[24] Similarly, they might not have wanted to settle in Italy in the first place, or might have formed this preference in the meantime. Then, they might not have ties (e.g., presence of family members) to the host country. Finally, they might have worse prospects in the labor market given their educational attainment or current position. Reassuringly, Figure A.6 shows that, for all the plausible reasons why a specific sub-population of immigrants might have left the country by the time of the interview, the composition of the two groups is homogeneous over time (i.e., by type of working arrangement and first year of employment).

Recall bias. Data referred to the first job are collected retrospectively, thus respondents have to recall past events. Then, estimates may be biased if recall errors are systematically related to the explanatory variables of interest (Berman et al., 2003; Orrenius and Zavodny, 2005). If the recall bias is constant across groups, however, the DiD design nets out estimates from this bias. In order for recall bias to confound our results, respondents should systematically shift self-reporting of their first job arrangement (i.e., treatment) and this should be correlated with the outcome of interest. A priori, the impact is ambiguous. In our context, it possible that immigrants consider only their first formal job as first employment spell. This might happen due to reasons such as stigma, lack of understanding, etc. In this case, immigrants who started in the informal sector would be classified as formally employed and this would yield a downward bias in our estimates. On the other hand, it is possible that workers who had an unfavorable assimilation process in between arrival and interview report adverse memories with respect to their first job arrangements. Vice versa, workers who successfully assimilated to the host country might recall memories that are on average more positive.

However, as shown by the evidence proposed in this section, none of the retrospective answers pertaining to respondents' conditions, intentions and expectations upon arrival seem to systematically vary across groups and periods (Figure A.6). Furthermore, observable characteristics that are potentially correlated to unobserved individual heterogeneity appear to be orthogonal to the treatment assignment (see Figures A.2, A.3 and A.4). Over-

[24] For instance, if they left their home country for economic or family reasons, or if they migrated to Italy because they perceived it as a more suitable environment to live in. These features, in turn, might be correlated with the individual's risk aversion, which, as Dustmann et al. (2020) show, is an important determinant of migration decisions.

all, this suggests that the potential bias arising from respondents misstating their first-job arrangements may play a minor role.

3.2 Who is penalized the most by missing the amnesty?

We then examine whether there are relevant heterogeneities across groups of workers. That is, we seek to identify sub-populations of immigrants for which the response in terms of a scarring effect of informal employment is more or less pronounced. 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, channel for finding job and industry. While there seem to be no differences in terms of gender or age (columns 1 and 5), the results by continent of origin are interesting. Although the coefficient associated with the European continent is not statistically significant, its sign is consistent with an attenuated negative effect (column 2).[25] Conversely, columns 3 and 4 suggest that the effect may be exacerbated for the sub-groups of workers from Africa and Asia. This is especially true for African immigrants, for whom the coefficient is negative, large and statistically significant. This supports the thesis that employers tend to discriminate on pay and working 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 workers who found their first job thanks to relatives or friends display no difference in the probability of being on a regular contract in 2011 from those who resorted to formal channels (job posts, agencies, etc.). This finding is in keeping with the broad absence of a pay-off generated by personal networks among immigrants in the UK and Canada estimated by Battu et al. (2011) and Goel and Lang (2019), respectively.[26]

[25] Here, the dummy takes value one for immigrants from any country within the European continent, EU and non-EU alike. The attenuated effect is consistent both with EU immigrants not being subject to any work permit conditions and with Europeans in general being less likely to be discriminated against 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, and there are none at all from Oceania.

[26] Additionally, there are no differences between individuals with more and less than secondary education. However, women and workers that resorted to informal channels and were exposed to the amnesty (i.e., had their first job without a contract, starting before 2002) have a higher probability of working regularly in 2011 by 12 and 28 percentage points, respectively. This is unsurprising, because the amnesty was initially intended to regularize especially family caregivers, a sector in which 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)
$\mathbb{1}(Year \geq 2002)$ * **H**	-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)
$\mathbb{1}(Year \geq 2002)$ * **H**	-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 comprise 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, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival.

Last, columns 7 and 8 indicate that immigrants who work in labor-intensive industries (namely agriculture, construction and manufacturing) at the beginning of their career are penalized more severely than those employed in less labor-intensive ones (i.e., trade and services), where the long-run effects of irregular status appear to be attenuated. This might be because irregular workers in general are more heavily represented in labor-intensive industries and in predominantly low-skilled tasks (Kossoudji and Cobb-Clark, 2002).

3.3 Lasting Effects on Segregation and Discrimination at Work

Our results to this point indicate that ineligibility for legalization in 2002 leads to a substantially lower probability of having a regular employment contract in 2011, which raises concerns about other unintended indirect effects. Apart from their extended period in the shadow labor market, in fact, non-regularized immigrant workers might suffer other undesirable side effects, such as workplace segregation and discrimination. We address this question by considering a set of outcome variables relating to interactions at work, reported experiences of discrimination, and perceived quality of the job.

Column 1 of Table 3 shows lower job mobility for the ineligible workers, who are significantly more likely (21% above baseline) to have the same job as their first employment. 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, and with Elias et al. (2018), who show that a large share of formerly undocumented housekeeping service workers move into other sectors of the economy and to larger and higher paying firms.

Moreover, immigrants who were not exposed to the legalization process tend to be more ethnically segregated at work. This aligns with the notion that undocumented workers tend to be disproportionately concentrated in specific low-wage occupations, which are commonly recognized as traditionally associated with immigrant or illegal workers (Kossoudji and Cobb-Clark, 2002). They are considerably less likely (18% below baseline) to interact with native colleagues (column 2). A similar inference can be drawn from the coefficients reported in columns 3 and 4, which albeit not statistically significant, suggest that they may be less inclined to speak Italian at work. Column 5 shows that these immigrants are more likely (40% over baseline) to report having difficulties in communicating in Italian over the phone. This is also consistent with refugees exposed to employment bans having lower language proficiency in the long run (Fasani et al., 2021). On the whole, our findings suggest that missing the chance for a regular work permit under the amnesty results in greater subsequent workplace segregation, which in turn implies less assimilation and poorer language skills.

In spite of the above evidence, there does not appear to be much difference in perceived discrimination at work (column 6).[27] In addition, column 7 indicates that the irregular workers unaffected by the amnesty are no more prone to change jobs – if anything, we

[27] In our sample, 96% of those who experienced discrimination report that it was on an ethnic basis.

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 comprise 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, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival.

estimate a negative coefficient. This may be due, however, to the fact that they perceive their working conditions in Italy (both pay and tasks) as less adverse than their experiences prior to arrival (column 8). As discussed by Kossoudji and Cobb-Clark (2002), undocumented immigrants might experience less bargaining power on the labor market, either due to limited access to information or opportunities to exercise their rights. At the same time, their status might induce them to prioritize avoiding detection by authorities over maximizing their earnings in the labor market. Thus, the lack of legal status may trap irregular workers in a situation of hardship and poor prospects in the long run. In other words, regularized workers, together with a work permit and better employment conditions, might gain a heightened awareness of the (potential) positive returns from immigration.

4 Conclusions

International migration has been growing in recent years and all indications are that it will continue to do so (OECD, 2018). Thus the receiving countries will necessarily have to monitor the effectiveness of their immigration policies to cope with the current and future migratory pressure (Fasani, 2015; Hanson and McIntosh, 2016).

This paper exploits the large-scale amnesty enacted 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 distinguish immigrant workers in the formal and the informal labor markets and evaluate the likelihood of exiting from the shadow economy. Our analysis shows that immigrants who were not eligible for the amnesty are 14% less likely to be employed on a regular contract a decade later than those who already had formal employment contracts pre-amnesty. This was not due to changes in employability, since ineligible undocumented immigrants were just as likely as their eligible counterparts to be employed in the long run.

We also provide novel evidence bearing on generally under-explored matters relating to workplace discrimination and segregation. We find that ineligibility for the amnesty determines lower job mobility and more severe segregation. In fact, non-regularized immigrants report greater difficulty speaking Italian and are less likely to work with natives. Nonetheless, this does not produce job dissatisfaction. Indeed, these workers consider their working conditions to be less adverse than their experiences prior to migration.

Our results carry important policy implications. Restrictions on the regularization of undocumented workers increase their probability of being confined to the informal sector in the long run. From a macro perspective, this is unquestionably detrimental to the overall welfare of the host country, given the serious consequences in terms of market competition and public finances of a large-scale shadow economy. Indeed, Elias et al. (2018) show evidence of increased payroll-tax revenues associated to immigrant regularization. Similarly, we estimate that by 2011 the amnesty increased tax revenues from labor income and social security contributions alone by roughly 3.8 billion euros (i.e., 0.002% of GDP).

This analysis also highlights the negative impact of undocumented status on the working conditions and linguistic assimilation of a sub-population of workers with little or no bargaining power. These workers may be disproportionally allocated to low-paying and low-skilled jobs and renounce to maximizing their returns on the labor market to avoid detection by authorities (Kossoudji and Cobb-Clark, 2002). We show that missing out on the opportunity to gain legal status via the amnesty yields poorer working conditions, especially in terms of workplace segregation, and less linguistic assimilation. Coupled with the evidence on immigrants' job contentment, this suggests that in the long run non-regularized workers may develop lower expectations and ambitions.

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A Appendix A: Additional tables and figures

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
Area of residence: North West	0.203	0.402	0	1
Area of residence: North East	0.204	0.403	0	1
Area of residence: Centre	0.213	0.410	0	1
Area of 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, 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				✓

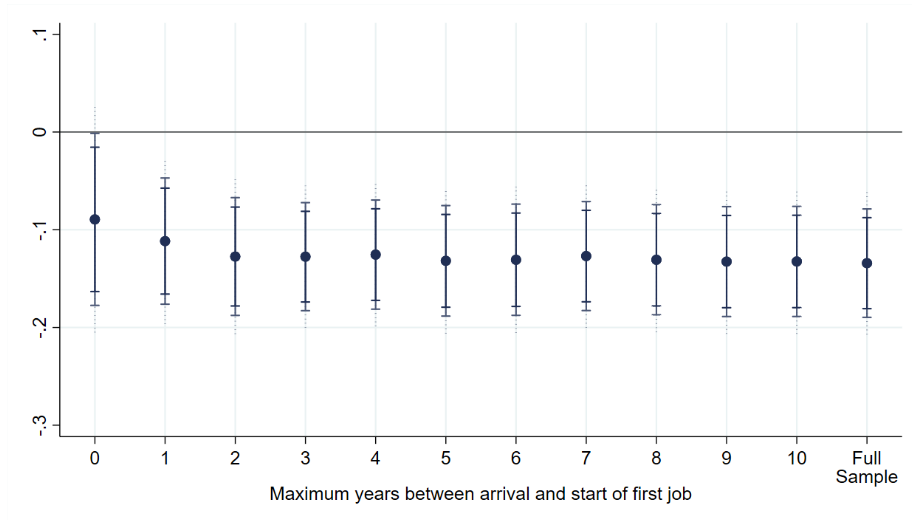
Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls comprise gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in migrating to Italy, accommodation upon arrival, whether the individual could speak Italian and their desire to settle in Italy upon arrival. Employment-related controls comprise 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.4: 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	✓	✓	✓	✓

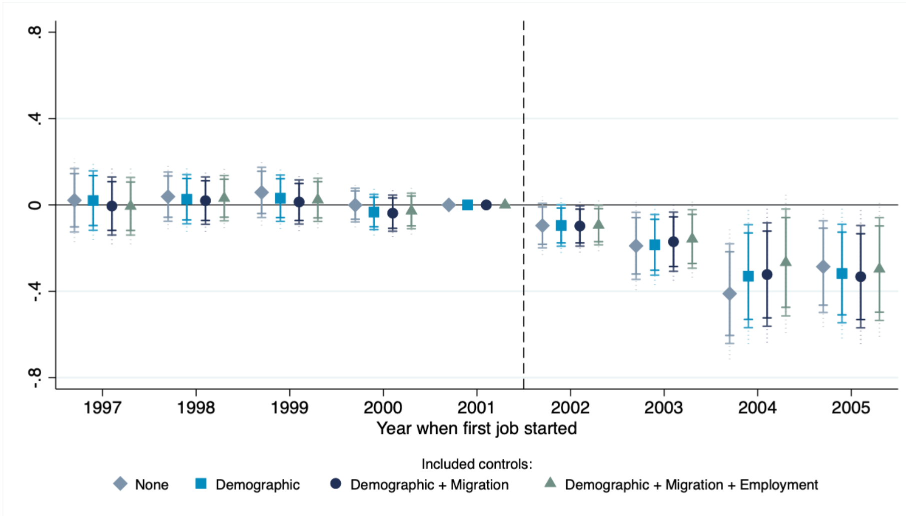
Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls comprise gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in 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 their 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 started working prior to 2002 only.

Figure A.1: Long-run effect on formal employment, by years between arrival and first job



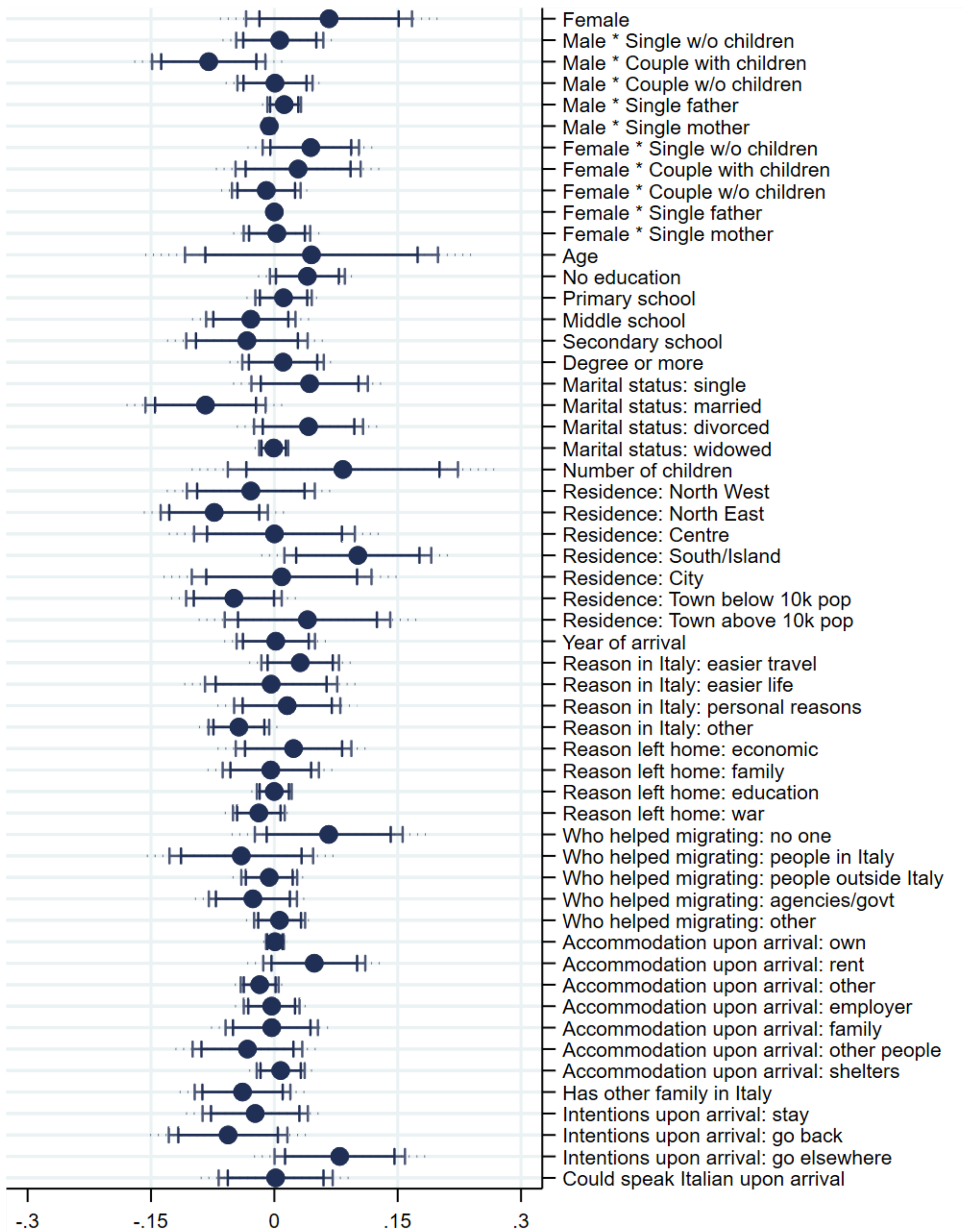
Note: Estimates as from Equation 1 on the probability of having a job in the formal sector in 2011, where each coefficient refers to the sub-sample of immigrants with up to the specified number of years between arrival and first job, except for the rightmost one, which refers to the main estimate as in column 3 of Table 1. Includes demographic controls (gender - also interacted with household type – and age, education, marital status, number of children, area of residence, type of municipality and country of origin) and migration-related controls (year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in 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%.

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



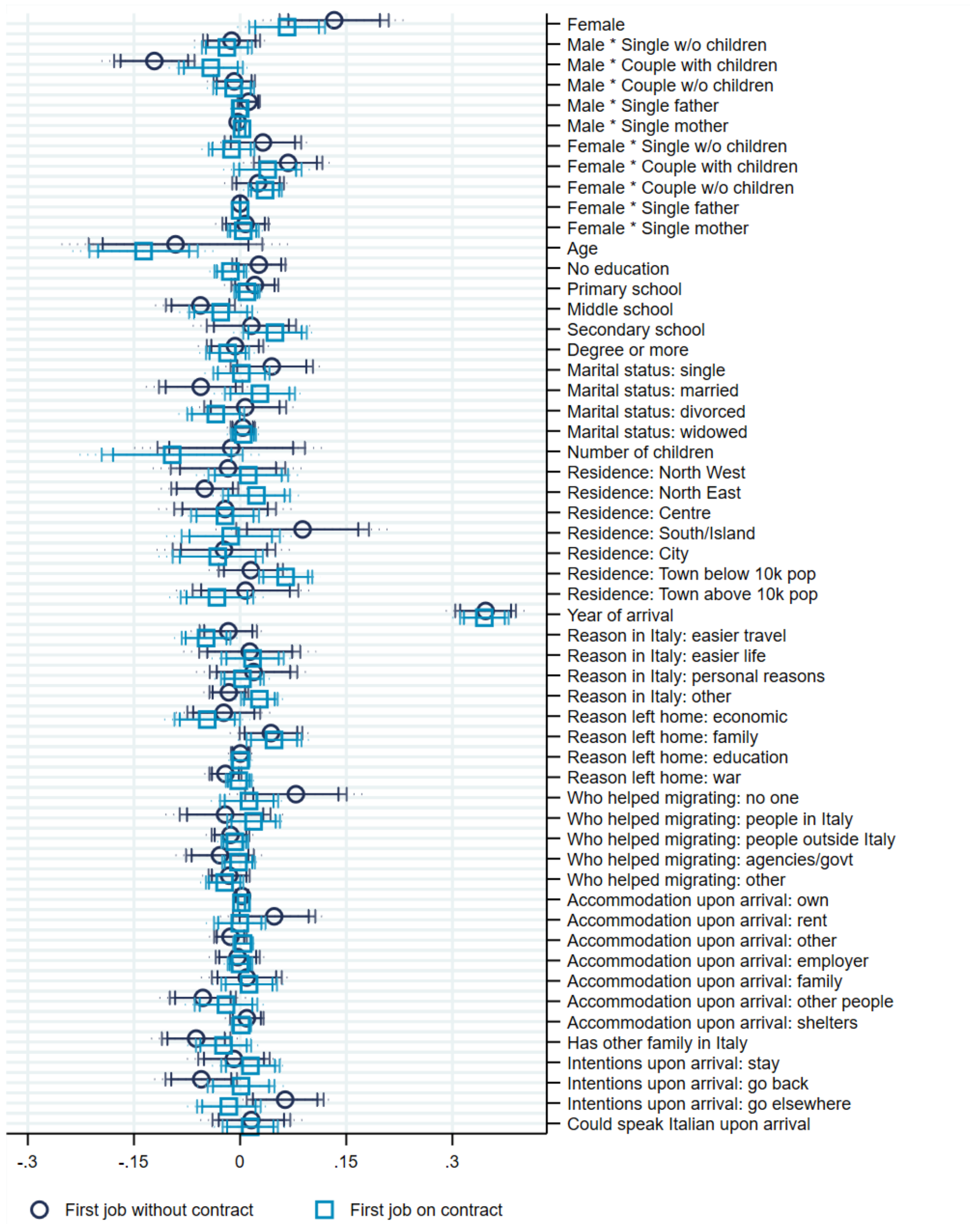
Note: The plot presents the event-study analysis as from Equation 2 on the probability of having a job in the formal sector in 2011. Demographic controls comprise gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. Migration-related controls encompass year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in 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 their 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%. The dashed vertical line splits the period into before and after the amnesty.

Figure A.3: Balancing test on covariates



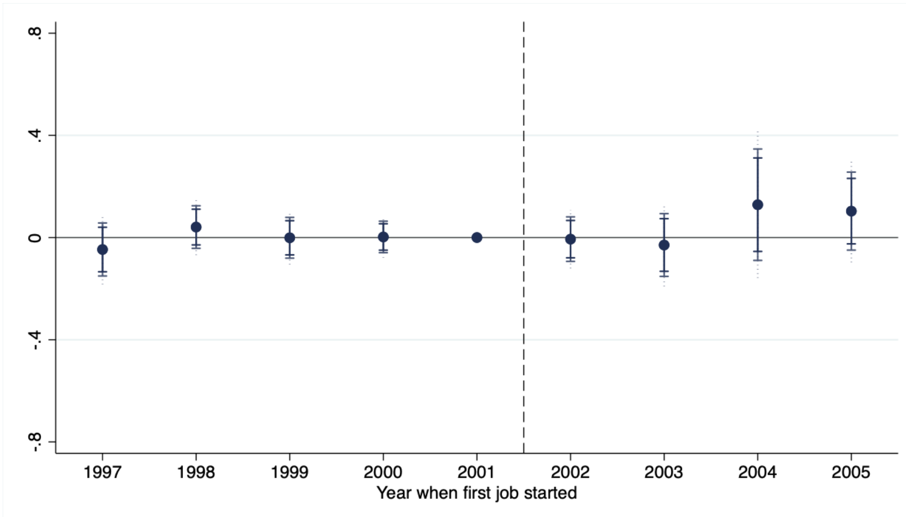
Note: The plot presents the estimated coefficient associated with the treatment where each covariate included in the analysis is used as outcome, in the spirit of Pei et al. (2019). Robust standard errors are clustered at the country-of-origin×area-of-residence level. The horizontal bars indicate confidence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

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



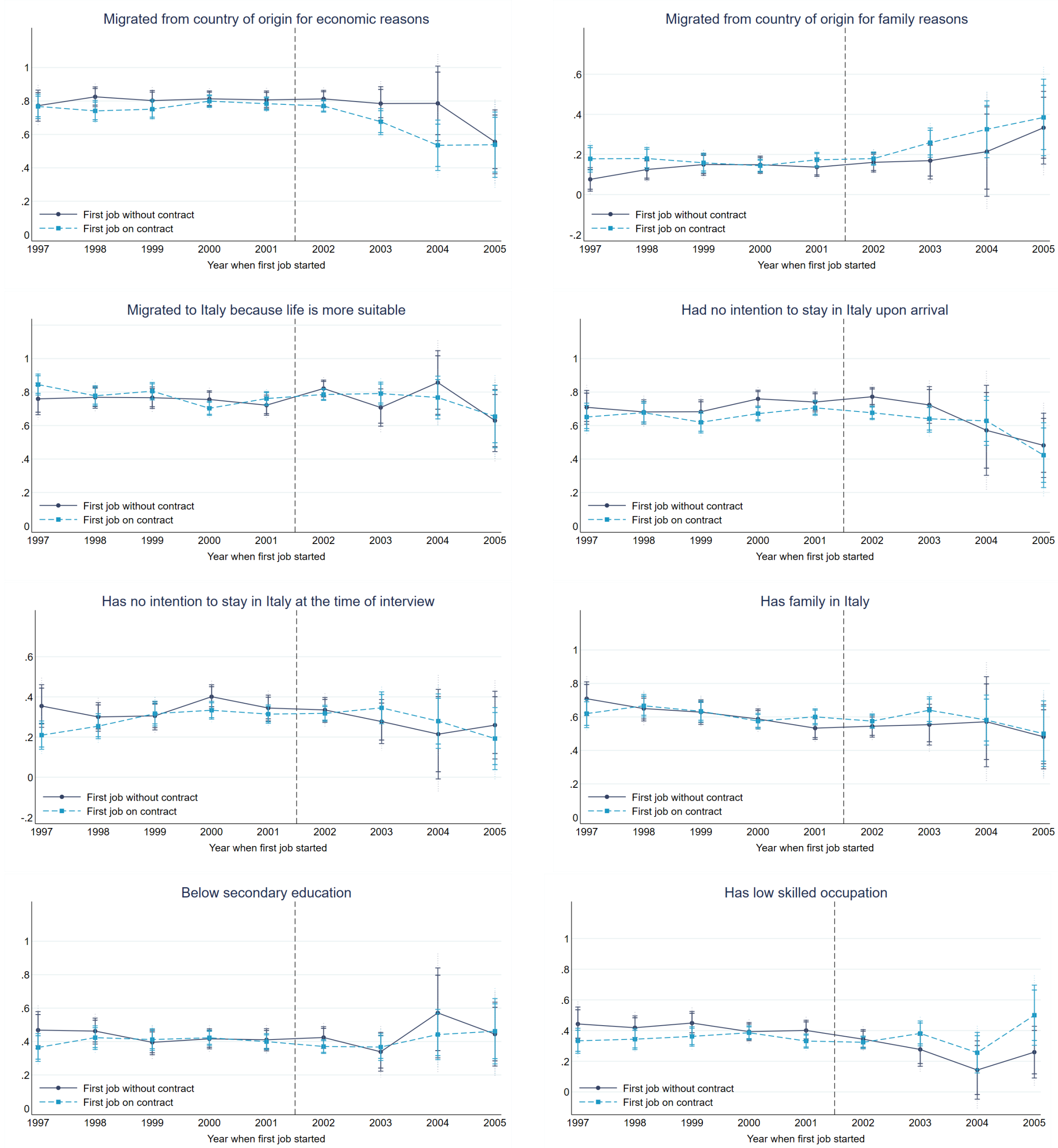
Note: The plot presents the estimated coefficient associated with the post-2002 dummy where each covariate included in the analysis is used as outcome, separately on the sub-samples of individuals whose first job was on a contract (blue squares) or not (navy circles). Robust standard errors are clustered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

Figure A.5: Long-run effect on employment, event study



Note: Event-study analysis as from Equation 2 on the probability of being employed in 2011. Comprises demographic controls (gender - also interacted with household type – and age, education, marital status, number of children, area of residence, type of municipality and country of origin) and migration-related controls (year of arrival, reason for emigration, reason for immigration to Italy, whether other family members live in Italy, who helped in 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%. The dashed vertical line splits the period into before and after the amnesty.

Figure A.6: Characteristics by first-job arrangement and year



Note: Raw data, immigrants employed in 2011 only (n=3,306). Markers display the share of immigrant workers in regular employment 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%. The dashed vertical line splits the period into before and after the amnesty.