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ABSTRACT

Legalization and Long-Term Outcomes of Immigrant Workers*

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 Classification: J15, J61, K37

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 the same year, the United States hosted nearly 38 million immigrants, a third of whom were undocumented (Baker, 2021).

Within the next few years many OECD countries, particularly in Europe, will be host to 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.[2]

This paper addresses the issue directly by studying how eligibility for regularization affects the formal employment of immigrants in Italy and their assimilation in the long run. We exploit the large-scale amnesty enacted in 2002 and use extremely rich survey data on immigrant households, collected in 2011, to study how eligibility or ineligibility for the 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 fruitful setting for assessing the impact of legalization policies on immigrants' labor market outcomes and social inclusion for three main 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 700,000 immigrants, the 2002 amnesty is one of the largest enacted in decades.[3] Third, the

[1] The 28 EU Member States (including the United Kingdom) plus Iceland, Liechtenstein, Norway and Switzerland.

[2] The political debate in many countries has often fuelled populism and extremism, especially during periods of large immigrant inflows (Steinmayr, 2021). See also Magris and Russo (2016) and Casarico et al. (2018), who study the trade-offs faced by politicians in the case of immigration amnesties. A recent review by Fasani (2015) discusses the need for causal evidence on the consequences of amnesties.

[3] By comparison, the Zapatero Reform in Spain in 2014 granted amnesty to nearly 600,000 non-EU

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.

The 2011 survey data provide detailed retrospective information on the immigrants' arrival and on their first spell of employment in Italy. We use year of arrival to select the relevant pool of immigrants residing in Italy before the 2002 policy change. The year their first job began and the type of work relationship (i.e., "written contract" or "oral agreement") allow us to classify immigrant workers into those who could potentially apply for the amnesty and those who could not, depending on the timing of their first period of employment and on whether they were employed in the *formal* or *informal* sector.

Our identification strategy relies on a difference-in-differences estimator which, in light of the characteristics of the policy and the data structure, isolates the effect of *ineligibility* for regularization on immigrants' participation and assimilation into the formal labor market in the long run. That is, we compare the outcomes of two groups of immigrants, all of whom had arrived before 2002: those eligible for the amnesty (i.e., having started their first job in the *informal* sector before 2002) and those who were unaffected because they did not get their first informal job until after the program was enacted. We then compare this difference 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 estimate indicates that immigrants who started working in the informal sector after 2002 and so 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 by comparison with 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

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 1.2 immigrants per 100 residents, as against 1.4% in Spain and 0.9% in Colombia.

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 at work. The literature has largely overlooked these long-run consequences, despite their crucial policy relevance. Overall, our evidence is consistent with the thesis that off-the-books work traps immigrants in jobs with worse conditions and poorer long-run prospects. In particular, ineligibility for legalization produces less job mobility, which is also a possible factor in lower wages (Simón et al., 2014; Kossoudji and Cobb-Clark, 2002), and more severe ethnic segregation in the workplace. Our estimates indicate that ineligible immigrants are 18% less likely to interact with native colleagues, and have 40% less competence in speaking Italian. However, we do not find any indication of an effect on perceived dissatisfaction, either in terms of experience of discrimination or in a desire to change jobs. As a matter of fact, immigrants not exposed to the amnesty are significantly more likely to report that their current working conditions are less adverse than their experience before arrival in Italy. This could depend on the fact that having suffered irregular labor market status might have lowered the expectations of undocumented workers (Ong and Shah, 2012).

Amnesties may well 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 generate an attraction effect for 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 where the incoming labor supply has been disrupted.

In our case, the amnesty was accompanied by a tightening of the requirements for residence permits to new entrants, who were now required to 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.[4] We also address potential sample attrition and exclude the possibility 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. Overall, the evidence is that the treatment and control immigrants in our sample do not differ except in entering the Italian labor market with either formal or informal employment arrangements.

This paper contributes to the vast literature on the labor market and assimilation outcomes of immigrants (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; Fasani 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; Pope, 2016; Ruhs and Wadsworth, 2018; 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 examined 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).[5]

Our study closely relates to Di Porto et al. (2018) and Devillanova et al. (2018), who also analyze the effects of the 2002 Italian amnesty. Di Porto et al. (2018) focus on its impact on firm employment and firm-level wages using administrative data from the Italian Social Security Administration (INPS). Devillanova et al. (2018) use data collected by a non-governmental organization operating in the city of Milan and exploit the plausibly exogenous discontinuity in eligibility based on date of arrival to study how the prospects of acquiring legal status affect the employment outcomes of undocumented immigrants. We

[4] In particular, our estimates are invariant to selecting the sample on different arrival dates and to including only workers who found their first job within the arrival year, or up to ten years after migrating to Italy.

[5] Similarly, others investigate the impact of acquiring citizenship (e.g., Bratsberg et al., 2002; Gathmann and Keller, 2018; Hainmueller et al., 2019; Govind, 2021). Recently, Felfe et al. (2021) show that birthright citizenship can significantly improve the propensity of immigrant youths to cooperate with their native peers.

depart from these authors in research question, identification, data and outcome variables.

While the special features of our setting allow us to focus on 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 have been 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 less comparable control groups (e.g., native workers or immigrants from different countries), our difference-in-differences approach can count on an almost ideal control group, in that we compare immigrants in the same employment period cohort with and without 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 identification strategy. Section 3 reports our results, and 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 set-up.

2.1 Institutional Context

In the course of the 1970s Italy changed from being a country of mass emigration to one of mass immigration. In 1981 the Census registered 321,000 foreign citizens. The first regularization program, for some 100,000 undocumented immigrants, was enacted in 1986 (Law 943/1986), with the aim of guaranteeing foreign workers the same rights as native Italians. Since then, various laws have regulated immigration by narrowing inflows and

setting pre-determined numbers of accesses (quotas) on the basis of labor market needs. Regular immigrants who stay beyond the expiration of their permit and those exceeding the quota are considered to be irregular if detected in the territory (Law 39/1990). Since 1998 these persons are mandated to be detained in temporary centers (Law 40/1998). By 2001, the population of registered foreigners in the Census was 1.3 million, while the number of irregular immigrants was estimated at half a million (ISMU, 2021).

Law 189/2002, also known as the *Bossi-Fini* law, with its accompanying Decree-Law 195/2002, represents a sharp discontinuity with respect to previous actions, especially in terms of regularization of undocumented immigrants already present in Italian territory. The core of the reform consisted in limitations on the ways non-EU immigrants could obtain a residence permit.[6] Differently from the past, when entry permits could be issued to job seekers, the law requires immigrants, before arrival, to already have a regular employment contract that allows self-sustainment, making the regularization procedure more stringent and restrictive.[7]

The *Bossi-Fini* law also addressed the issue of irregular immigrants already present with a large-scale amnesty. Applications could be submitted by firms 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 manifest their willingness to legally hire the worker under a renewable contract lasting at least one year at a minimum salary of EUR 439 a month.[8]

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 that might have influenced the behavior of the economic agents involved. This is even more important considering that the amnesty was originally intended to target family caregivers alone, and other workers were included in the regu-

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

[7] Entry for family reunification was still permitted to spouses, children and parents over 65 of regular immigrants, subject to their inability to provide for themselves otherwise.

[8] Applications, which were based on a self-declaration form (given the impossibility of effectively demonstrating when the informal employment began), could be submitted over a period of two months, from September to November 2002. 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 residence permit were ineligible. See Devillanova et al. (2018) and Di Porto et al. (2018) for further details.

larization 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

We use a dedicated survey on the conditions and social integration of foreign citizens conducted by the Italian National Institute of Statistics (ISTAT) in 2011. The survey provides information on a sample of around 12,000 resident households where at least one member is a foreigner.[9] The survey covers a rich list of variables, which makes it a valuable, unique tool for studying the integration and assimilation process in Italy. They encompass family composition, education, migration and work history, current working conditions and other aspects of social participation, including experiences of discrimination and victimization.

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 and have not obtained Italian nationality, who arrived with at least compulsory schooling, and who have worked in Italy at least once but found their first job after arrival. We also drop the few immigrants with multiple arrivals and those that have changed foreign nationality, in order to rule out differences in preference for staying abroad.

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

[9] 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.

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 of the *Bossi-Fini* law in a difference-in-differences 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" (i.e., no contract).

We define the variable F_i as equal to one if the first job in Italy was under oral agreement, considering that 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 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:

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

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]

[10] 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

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×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 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, it is posited that 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

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. If standard errors are clustered based on country of origin, country-of-origin-specific arrival cohorts or employment cohorts, the results are identical.

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

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 test 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

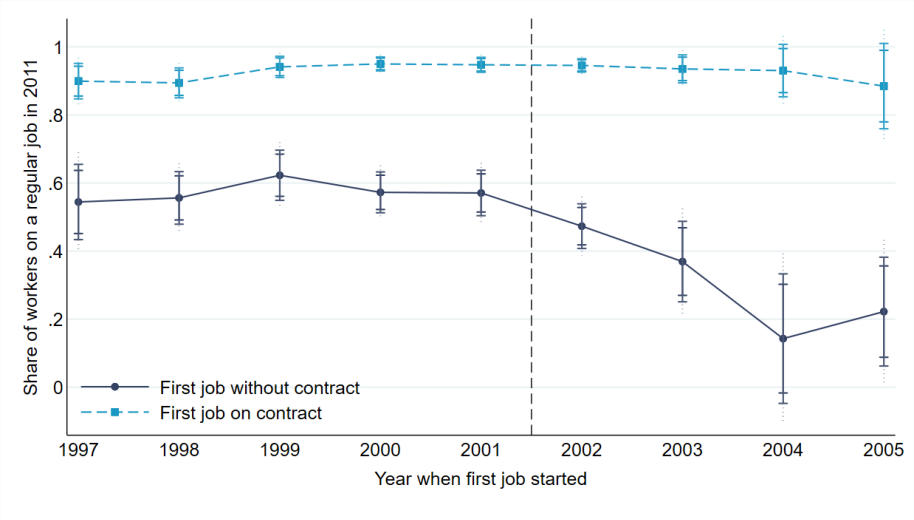
This section presents the results on the long-run effects of ineligibility for regularization. We first consider the probability of being formally employed in 2011 and then discuss indirect effects in terms of workplace segregation and discrimination.

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.

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* were regularly employed in 2011, and this share

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.

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 regularly employed in 2011. By contrast, the immigrant workers targeted by the regularization are those who had their first job *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 were employed in the formal market (while the remaining 43% were not formally employed) in 2011. For those who missed the opportunity offered by the amnesty this proportion decreases substantially, from 47% in 2002 to 22% in 2005.[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 start-

[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).

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.

ing the first job in 2002 or later, which identifies those who began only post-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 columns 2 and 3 we successively add two sets of demographic and migration-related control variables. The coefficients are remarkably stable across columns. The fully-specified model in column 3 is our preferred one.[14]

The coefficients suggest 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.[15] The dummy for the post-amnesty period is never statistically significant or economically relevant, which further suggests that the more restrictive policy on entries from outside the EU enacted together with the amnesty did not affect the rate of employment in the formal sector for immigrants who had already arrived before the change in the law.

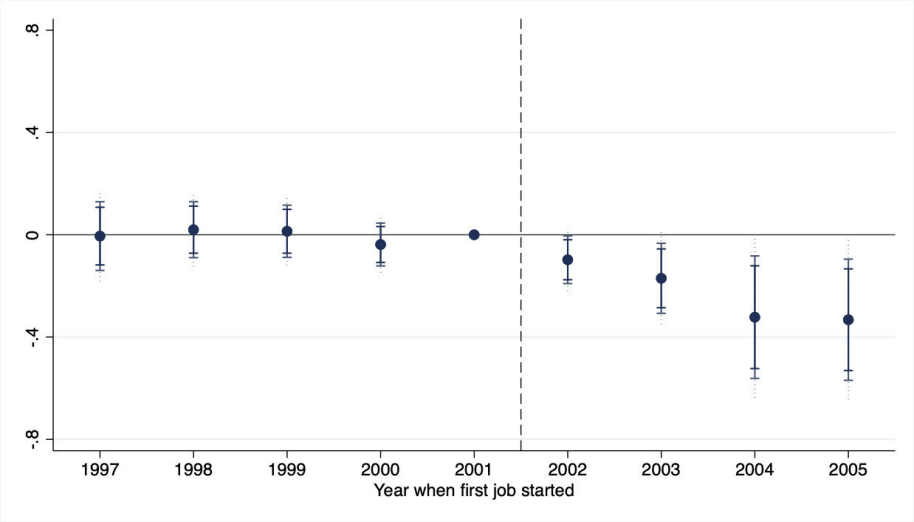
[14] Controlling for whether the immigrant still resides in the same province as that of arrival, to account for individual propensity to move, yields identical results.

[15] The baseline is the average outcome of the control group in the pre-amnesty period. This is 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 average value of the blue squares to the left of the dashed line in Figure 1.

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).[16] 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 contract. Thus, the total effect of starting off in the informal sector for those beginning to work after 2002 is greater than 50%.

The value of the differences-in-differences design is enhanced by the fact that the data corroborate the assumptions. Figure 2 reports the coefficient estimated in the event-study analysis as from Equation 2. This confirms the absence of differentials between treatment

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.

[16] 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.

and control units in the pre-amnesty period, precluding 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 difference of 35 pp. After the 2002 amnesty, it is the treatment 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.

Our results are highly robust to a variety of different specifications and robustness tests, described in Appendix B. These include adding covariates related to past employment experience and a range of fixed effects and linear trends. Importantly, the estimates are invariant to selecting the sample on different arrival dates (all prior to the amnesty) and to considering only workers who started their first job within specific time ranges after arrival (from within the year of arrival to ten years after). This suggests that our identification strategy plausibly attenuates the relevance of any residual unobserved heterogeneity relating to the speed at which workers manage to enter employment for the first time.

Moreover, in the spirit of Pei et al. (2019), we show that the estimated effects do not depend on specific sub-groups, since targeting is largely homogeneous in covariates (namely, demographic, migration-related and employment characteristics). Additionally, the treatment and control units do not display differential trends in probability of employment. Given that the amnesty was not intended to increase employability *per se* but only to facilitate workers in getting out of labor market informality, this exercise supports the validity of our design by showing that the two groups do not differ in probability of employment.

Last, in Appendix B we rule out bias stemming from sample attrition. We discuss a number of plausible causes of selective out-migration of different sub-populations of immigrants between the early 2000s and 2011, the year of the interview.[17] These features are typically unobservable and may all be correlated with the probability of a specific sub-group of immigrants' having left Italy by the time of the survey. We exploit the rich retrospective and current information on the immigrants' attitude towards the host

[17] These are: (*i*) current and past intention to stay in Italy, (*ii*) the existence of ties (e.g., family) with the host country, (*iii*) the choice of migrating out of the home country or to Italy for specific reasons (e.g., economic or due to personal preferences), and (*iv*) employment prospects in the host country.

country and show that, for each of these possible causes, the composition of the sample is homogeneous over time and across groups.

3.2 Who is penalized the most by missing the amnesty?

We also 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).[18] 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 furnishes support to 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.[19]

[18] 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.

[19] 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).[20]

[20] In our data, industries are classified into five groups: agriculture, construction, manufacturing, trade, and services. Di Porto et al. (2018) report that in both manufacturing and construction 21% of the firms

3.3 Lasting Effects on Segregation and Discrimination at Work

Our results to this point indicate that ineligibility for legalization under the amnesty in 2002 is associated with 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.

Moreover, immigrants who were not exposed to the legalization process tend to be more ethnically segregated at work. They are considerably less likely (17% 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 while 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 consistent with Fasani et al. (2021), who estimate that refugees exposed to employment bans tend to have less language proficiency in the long run.

On the whole, these 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).[21] In addition, column 7 indicates that the irregular workers unaffected by the amnesty are no more prone to change jobs – if anything, we

inspected in 2001 had at least some workforce irregularities, whereas in other sectors this share was always below 5%, with the exception of hotels and restaurants (24%) and retail trade (18%). As for agriculture, which is excluded from the analysis of Di Porto et al. (2018), ISTAT estimates 19 irregular workers per 100 in 2001.

[21] In our sample, 96% of the individuals 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 these individuals perceive their working conditions in Italy (both pay and tasks) as less adverse than their experiences prior to arrival (column 8). That is, 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 migratory pressure.

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. This analysis also points up the risks associated with a sub-population of workers with little or no bargaining power. Missing out on the opportunity to gain legal status via the amnesty, in fact, resulted in poorer working conditions, especially in terms of workplace segregation, together with 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

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

B Appendix B: Robustness checks and identification issues

In this section we present a series of tests in support of our identification strategy.

First, in Table B.1 we compare workers whose first period of employment began before and after 2002, in the informal sector only. We find that immigrants who were not eligible for the amnesty are 17 percentage points less likely to have a formal employment contract in the long run than those who were eligible. The point estimate is remarkably close to the interaction term reported in Table 1.

Table B.2 reports a battery of robustness checks. 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.

Table B.1: 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 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.

In the last two columns of Table B.2 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

Table B.2: Long-run effect on formal employment, robustness checks

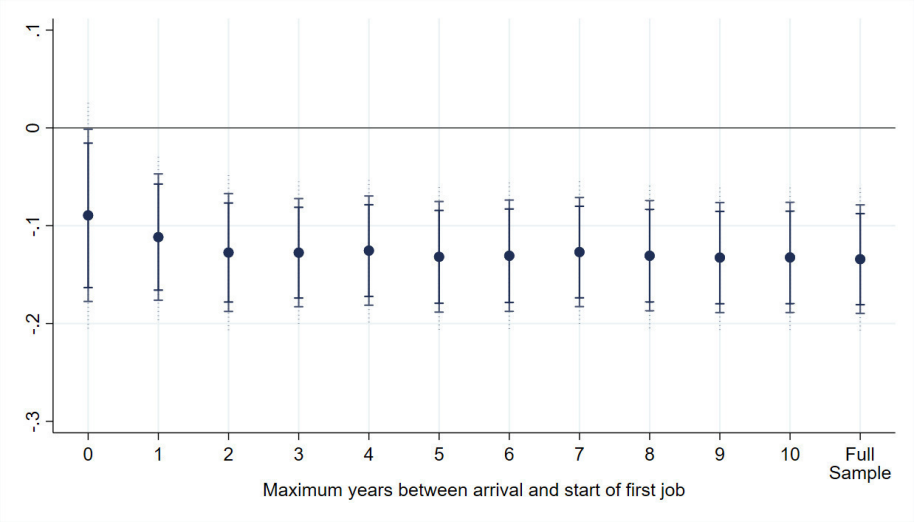
	(1)	(2)	(3)	(4)
	Probability of having a job in the formal sector in 2011			
First job w/o contract	-0.346***	-0.342***	-0.346***	-0.823***
	(0.028)	(0.029)	(0.028)	(0.111)
$\mathbb{1}(Year \geq 2002)$	-0.010	-0.015	0.012	0.008
	(0.019)	(0.019)	(0.026)	(0.027)
First job w/o contract * $\mathbb{1}(Year \geq 2002)$	-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.

in some industries.[22] However, our country-of-origin indicator distinguishes only between 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.

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

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



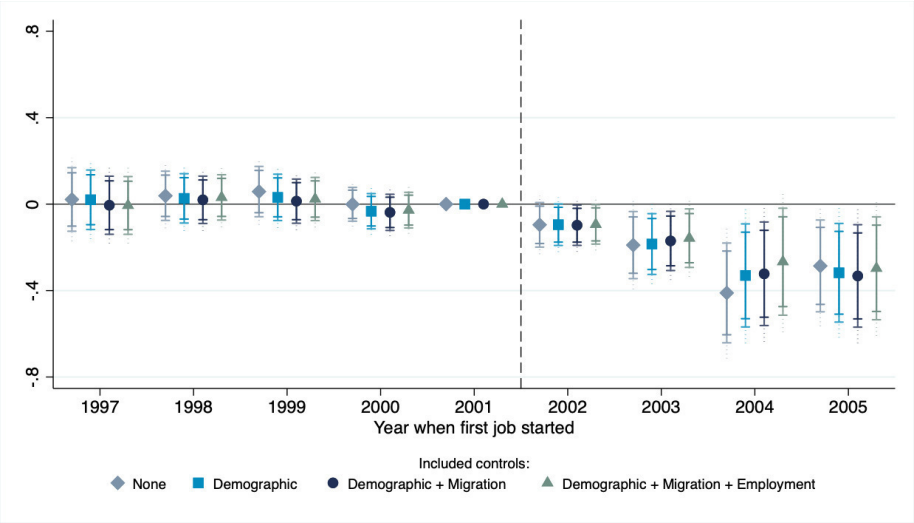
Note: Estimates as from Equation 1 on the probability of having a job in the formal sector in 2011, where each coefficient refers to the sub-sample of immigrants with up to the specified number of years between arrival and first job, except for the rightmost one, which refers to the main estimate as 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%.

[22] The EU15 were Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, 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.

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 B.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.[23]

Next, 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 relevant 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 B.2. The same conclusions can be drawn from Figure B.2, which demonstrates that the event-study analysis is remarkably

Figure B.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.

[23] 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.

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 B.3.

We also show that observable individual characteristics do not differ between immigrants who started working before and after the amnesty, separately for those who had their first job on contract and not (Figure B.4).[24] 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 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 B.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 B.3. 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.[25]

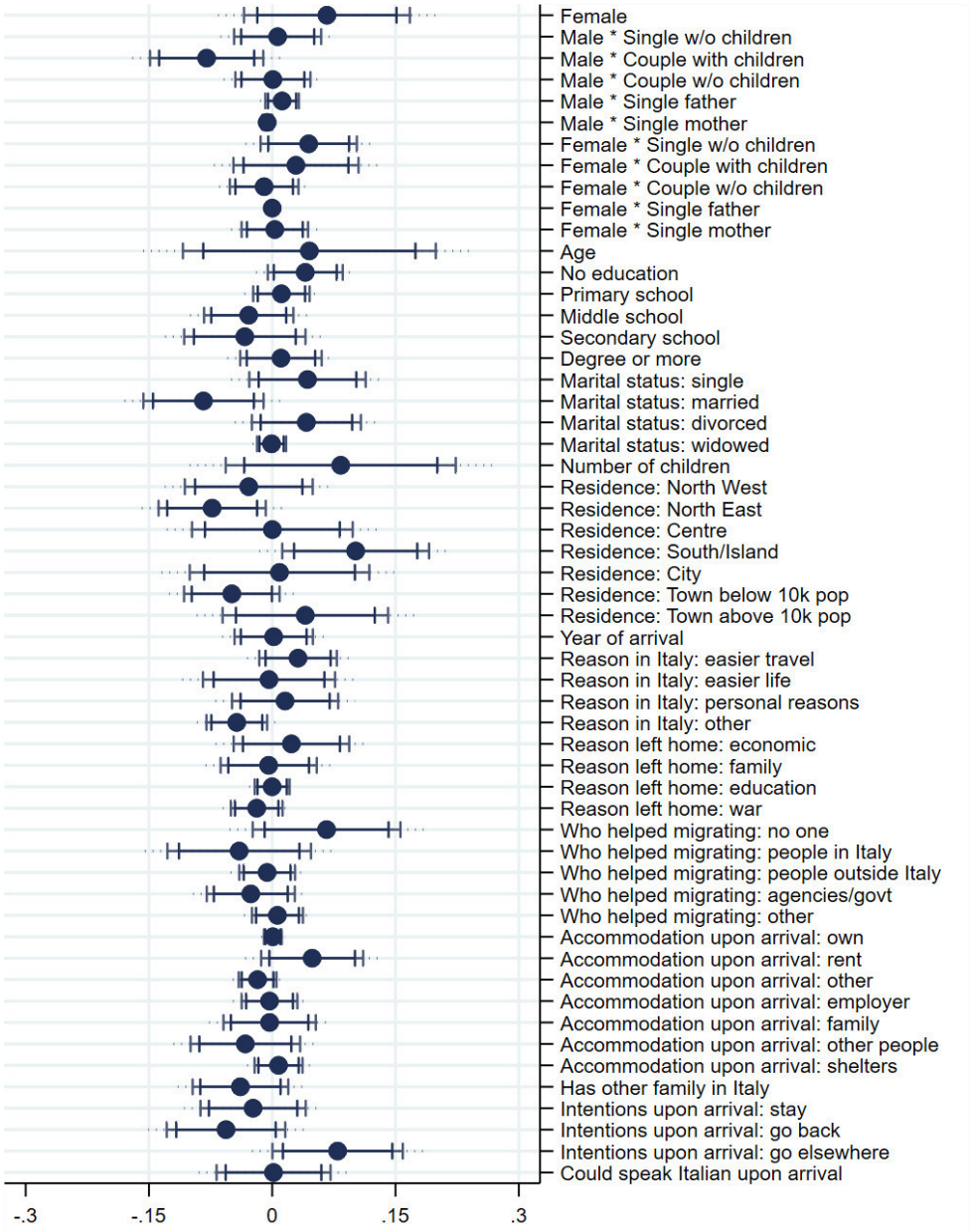
Second, columns 2 to 4 of Table B.3 show that restricting the sample to the pre-amnesty years only and assigning false cut-offs in 1998, 1999 and 2000 does not produce any statistically or economically relevant effect.

Finally, given that we reconstruct the individuals' histories using retrospective information on their first employment but observe them only in 2011, one may suspect the danger of disproportionate sample attrition. If positively-selected immigrants who started

[24] 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.

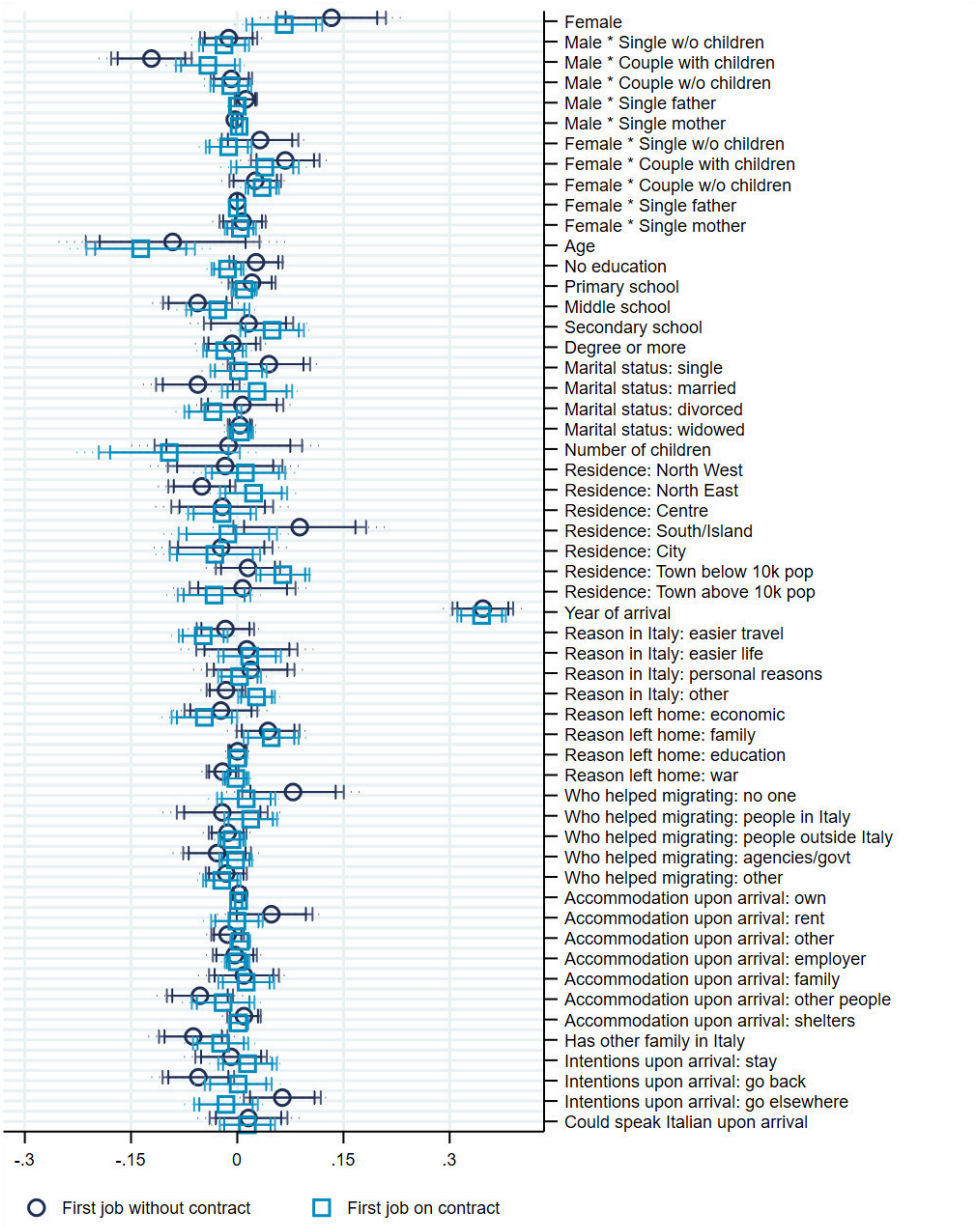
[25] Nor do we find any differences in the probability of being in part-time work or having a second job in 2011.

Figure B.3: Balancing test on covariates



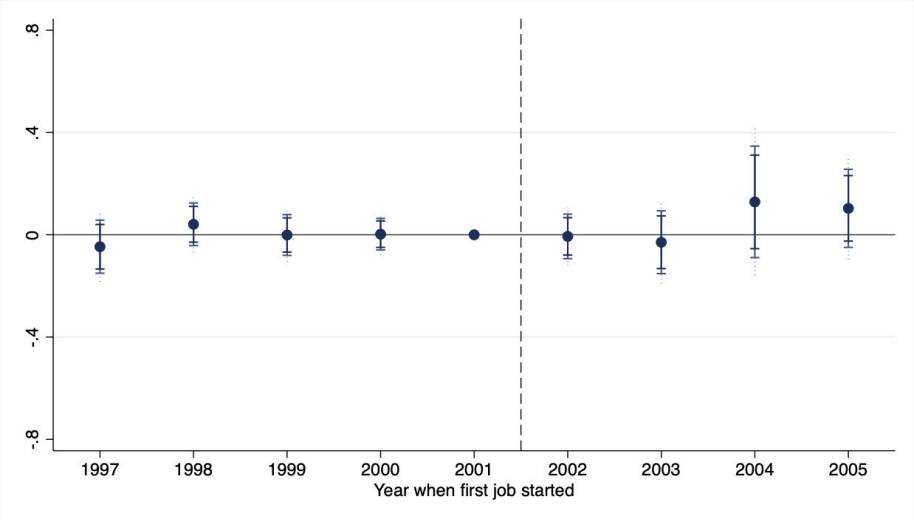
Note: The plot presents the estimated coefficient associated with the treatment where each covariate included in the analysis is used as outcome, in the spirit of Pei et al. (2019). Robust standard errors are clustered at the country-of-origin×area-of-residence level. The horizontal bars indicate confidence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

Figure B.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 B.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.

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).[26] 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.

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.[27]

[26] 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.

[27] For instance, if they left their home country for economic or family reasons, or if they migrated to

Table B.3: 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	✓	✓	✓	✓

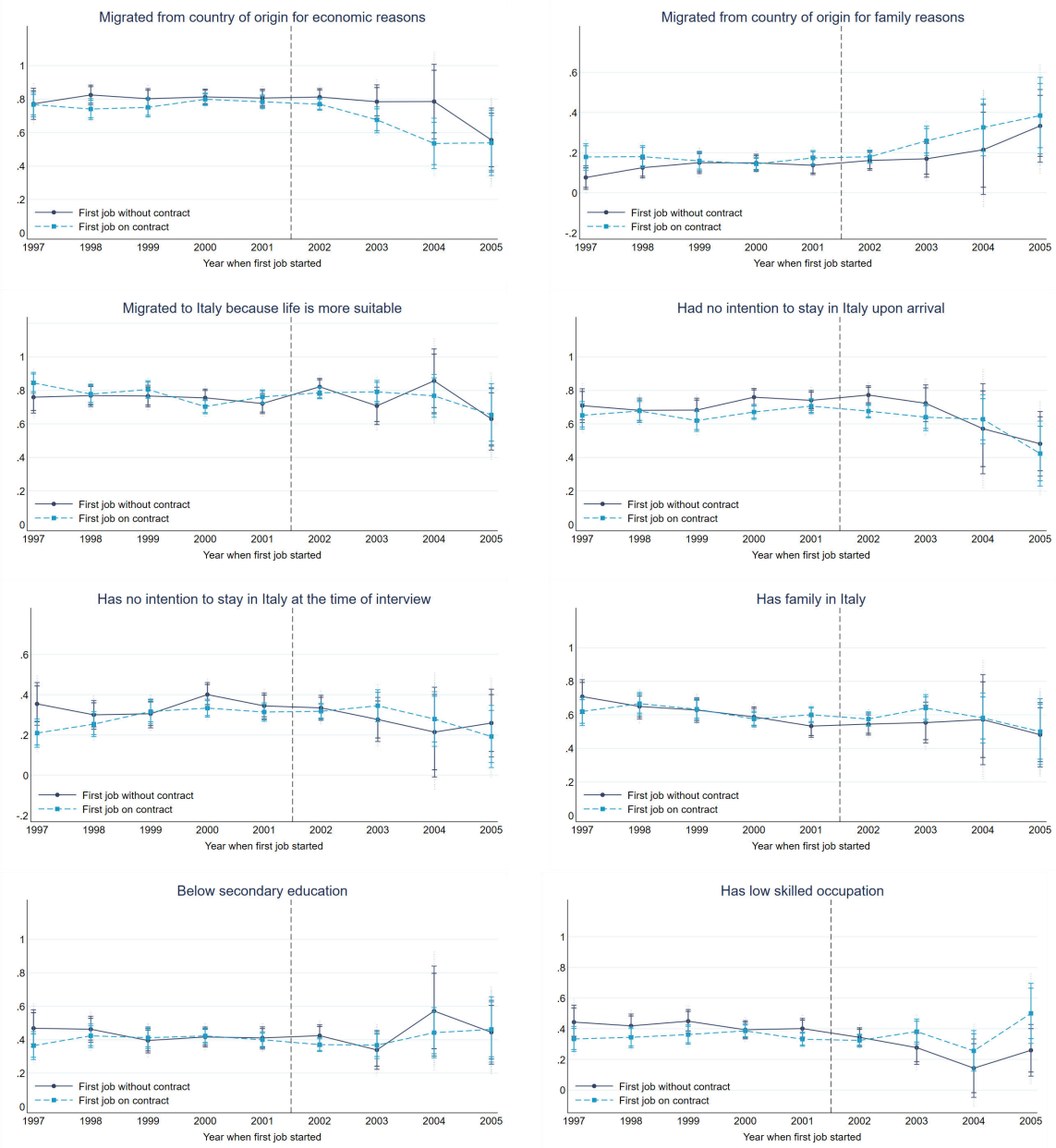
Note: * p<.10 ** p<.05 *** p<.01. Robust standard errors clustered at the country-of-origin×area-of-residence level. Demographic controls comprise gender (also interacted with household type), age, education, marital status, number of children, area of residence, type of municipality and country of origin. 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.

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 B.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).

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.

Figure B.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.