EMPLOYMENT OF UNDOCUMENTED IMMIGRANTS AND THE PROSPECT OF LEGAL STATUS: EVIDENCE FROM AN AMNESTY PROGRAM

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This article estimates the causal effect of the prospect of legal status on the employment outcomes of undocumented immigrants. The identification strategy exploits a natural experiment provided by an Italian amnesty program that introduced an exogenous discontinuity in eligibility based on date of arrival. The authors find that immigrants who are potentially eligible for legal status under the amnesty program have a significantly higher probability of being employed relative to undocumented immigrants who are not eligible. The size of the estimated effect is equivalent to about half the increase in employment that undocumented immigrants in our sample normally experience during their first year in Italy. These findings are robust to several checks and falsification exercises.

The substantial presence of undocumented immigrants, which is a common feature in most developed countries, has generated debate in

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both Europe and the United States over the types of immigration policies that should be adopted. In the United States, for example, with an estimated 11.4 million unauthorized immigrants (U.S. Department of Homeland Security 2012), the immigration policy reforms most often proposed include complementary strategies aimed at curbing both future flows of undocumented migrants (e.g., by intensifying controls or increasing sanctions) and existing populations (through some form of legalization path). The programs subject to the most heated discussion are those that involve amnesty. Whereas one side stresses the need to recognize immigrants' contribution to the US economy, making it impractical to deport undocumented immigrants living within the nation's borders, opponents argue that amnesty would unfairly reward law-breaking behavior and reveal the time inconsistency of the US migration policy. In Europe (the EU-27), with a recent estimate of between 1.9 and 3.8 million undocumented immigrants but large inter-country variability in incidence over total population (Vogel, Kovacheva, and Prescott 2011), policies affecting immigrants' legal status are often at the very core of the migration policy debate. In recent years, nations looking to reduce the number of undocumented residents have often resorted to legalization programs (Casarico, Facchini, and Frattini 2012), which in the EU have granted legal status to more than five million individuals since 1996 (Brick 2011).

In this article, we address amnesty programs' labor market effects on their target population of undocumented immigrants. More specifically, we study the effects that the *prospect of legal status* has on undocumented migrants' employment rate, whereas the received literature focuses on the labor market effects of *gaining legal status* for legalized immigrants. Indeed, amnesty programs generally impose some eligibility conditions, which immediately differentiate potential applicants from ineligible undocumented immigrants. We propose a stylized conceptual framework to improve understanding of how the prospect of legal status may shift labor demand and supply of undocumented immigrants even *before* legal status is actually granted.[1] We show that the possibility of applying for amnesty per se has significant labor market consequences. For the first time, we empirically sign and quantify the effect of the *prospect* of becoming legal on undocumented workers' employment outcomes. In doing so, we explore labor market effects that, although essential for a complete analysis of amnesty program outcomes, have so far been overlooked. Remarkably, pre- and post-legalization employment effects may have opposite signs. An accurate assessment of amnesty programs' overall impact, therefore, requires consideration of their effects both *during* the application period (when undocumented immigrants become eligible and apply for amnesty) and *after* legalization of successful applicants. Our data allow us to focus on the former effect, thus complementing the results from previous studies.

[1] The mechanisms we analyze may also be in place with visa sponsorship schemes that condition the issuance and/or renewal of residence permits on having an employer willing to support the application. These policies are commonly adopted in major immigration countries and our results can shed some light on their labor market effects.

To identify the causal effect of the prospect of legal status on undocumented immigrants' employment probability, we innovatively exploit a natural experiment provided by the 2002 legalization program in Italy. The program conditioned eligibility on both a predetermined minimum residence requirement and on being employed at the time of application. As we discuss in our Conceptual Framework section, such an amnesty design produces ambiguous employment effects. Furthermore, the retrospective and unpredictable threshold based on date of arrival in Italy exogenously assigns undocumented immigrants into one of two groups: those who arrived in Italy before the threshold date (treatment group) and those who arrived after (control group). We exploit this quasi-experimental setting, together with a unique data set of undocumented immigrants, to construct an almost "ideal comparison group: . . . a randomly selected group of undocumented immigrants similar to the target group, but ineligible for, and unaffected by, the amnesty" (Kaushal 2006: 635). This design improves on extant research, which generally had to rely on arbitrary control groups of documented migrants or natives.

Theoretical and Empirical Background

Previous Findings

Several articles investigate whether amnesty is an appropriate policy tool to address undocumented migration (e.g., Chau 2001).[2] Whereas some examine amnesty's possible effects on future undocumented migrant flows (Orrenius and Zavodny 2003) or on the labor market outcomes of natives (Cobb-Clark, Shiells, and Lowell 1995; Chassamboulli and Peri 2015), others assess amnesty programs' general effect on their target population of undocumented immigrants with a particular focus on changes in labor market outcomes experienced by legalized immigrants.[3]

According to all the theoretical channels highlighted in the literature, *gaining legal status* unambiguously increases wages, wage growth, and returns to skills for employed immigrants, while the effect on employment is theoretically undetermined.[4] On the demand side, matches with documented

[2]For the theoretical and empirical debate on alternative migration control policies to address undocumented immigration (border controls, domestic enforcement, and so forth) see, among others, Ethier (1986), Hanson and Spilimbergo (1999), Hanson (2006), Facchini and Testa (2011), and Bohn, Lofstrom, and Raphael (2014).

[3]A few other articles examine the impact of legal status on outcomes outside the labor market, such as remittances (Amuedo-Dorantes and Mazzolari 2010), consumption (Dustmann, Fasani, and Speciale 2017), and crime (Mastrobuoni and Pinotti 2015); a related strand of literature addresses the labor market effects of naturalization (Bratsberg, Ragan, and Nasir 2002; Mazzolari 2009). See Fasani (2015) for a survey of empirical papers on immigrants' outcomes and legal status.

[4]The main theoretical channels identified in the literature are better employer–employee matching (because of such factors as increased geographical and occupational mobility, reduced risk in job search activity, and access to formal recruiting channels), higher bargaining power, and eligibility for social programs (e.g., Rivera-Batiz 1999; Amuedo-Dorantes and Bansak 2011).

immigrants may be more valuable for employers (as they cannot be exogenously interrupted by a worker's deportation) but may also imply higher costs. On the supply side, the overall effect depends on the relative size of income and substitution effects. Indeed, the empirical literature consistently observes that newly legalized immigrants have higher wages after legalization than before (see, e.g., Borjas and Tienda 1993; Kossoudji and Cobb-Clark 2002; Kaushal 2006; Amuedo-Dorantes, Bansak, and Raphael 2007), although the employment effect remains empirically unclear.[5] Most of these empirical studies exploit the variation in legal status induced by the Legally Authorized Workers (LAW) program—one of the legalization programs introduced in the United States by the 1986 Immigration Reform and Control Act (IRCA)—and use data from the Legalized Population Survey (LPS), a longitudinal survey of immigrants who obtained legal status through that particular program.[6] The LAW-IRCA amnesty, which granted legal status to more than 1.6 million immigrants, was open to aliens with a minimum length of residence in the United States of about four years. Two other nationality-specific amnesty programs examined in the US context are the 1992 Chinese Student Protection Act (CSPA; Orrenius, Zavodny, and Kerr 2012) and the 1997 Nicaraguan Adjustment and Central American Relief Act (NACARA; Kaushal 2006), which imposed a minimum residence requirement for legal status eligibility.[7] Comparison groups used in the literature include legal foreign-born population (Borjas and Tienda 1993), legal Latino immigrants (Kossoudji and Cobb-Clark 2002), legal immigrants from a selected group of Latin American countries (Kaushal 2006), and a subsample of Hispanic natives (Amuedo-Dorantes and Bansak 2011). Three recent papers (Barcellos 2010; Lozano and Sorensen 2011; and Pan 2012) exploited the discontinuity in eligibility for legal status created by the cutoff date (January 1, 1982) of the LAW-IRCA program.[8]

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[5]For instance, Amuedo-Dorantes et al. (2007) and Amuedo-Dorantes and Bansak (2011) found that both male and female newly legalized workers experienced lower employment, which results in higher unemployment for men and lower participation for women. Kaushal (2006), however, identified only a statistically insignificant effect on employment, whereas Pan (2012) found a positive relation but only for female immigrants.

[6]The LPS contains information about a sample of 6,193 undocumented migrants living in the United States in 1986/87 who sought legal permanent residence through LAW-IRCA. The survey data were collected from the entire group in 1989, and again (from 4,012 of these respondents) in 1992 (see, e.g., Borjas and Tienda 1993; Rivera-Batiz 1999; Kossoudji and Cobb-Clark 2000; Kossoudji and Cobb-Clark 2002; Amuedo-Dorantes et al. 2007; Amuedo-Dorantes and Bansak 2011; Pan 2012).

[7]The CSPA, designed to prevent political persecution of Chinese students in the aftermath of the Tiananmen protests of 1989, granted permanent residency to all Chinese nationals who arrived in the United States on or before April 11, 1990. The NACARA, enacted in November 1997, granted legal status to about 450,000 immigrants from Nicaragua, Guatemala, Cuba, and El Salvador (if in the United States since 1990), together with their spouses and children (if continuously in the United States since December 1995).

[8]All these papers face severe data limitations (legal status and year of arrival in the United States are, respectively, not observed and only partially observed), which makes it hard to isolate the true effects of legalization.

Conceptual Framework

Our conceptual framework centers on our primary research question: What effect does the prospect of legal status have on undocumented migrants' employment rate? As already emphasized, the focus of this question differs from that in previous research, which addresses the labor market effect of gaining legal status. Because these potential pre-legalization effects may depend on amnesty program design, they should definitely be considered when assessing a program's overall effects.[9] Substantial heterogeneity is seen in the eligibility requirements that amnesty programs set for granting legal status. Most amnesty programs base eligibility on some predetermined individual condition (e.g., minimum residence condition, past employment), aimed at preventing new inflows of undocumented immigrants. Any predetermined requirement affects employers' relative demand for eligible versus ineligible immigrants prior to legalization. The direction of the demand shift is ambiguous: On the one hand, the prospect of legalization increases the value of the matches because they become more stable; on the other, these matches are more expensive because of payroll taxes and/or regularization fees. Amnesty can also require undocumented immigrants to be employed at the moment of application, as has been the case for most amnesty programs launched in Southern European countries (Kraler 2009; Chauvin, Garcés-Mascareñas, and Kraler 2013). In addition to these demand effects, employment-conditional amnesty that requires immigrants to be employed at the time of application also shifts the labor supply of undocumented immigrants (before legalization). The value of being employed is increased by the prospect of obtaining legal status, inducing a reduction in potential applicants' reservation wages and, therefore, increasing their labor supply. The net change in the surplus of potential matches remains ambiguous because of the indeterminacy of labor demand shifts.[10] Note that the pre-amnesty supply-side effect we describe has the opposite sign with respect to the generally expected effect from legalization (i.e., a negative labor supply shift because of improved outside options and higher reservation wages). Ignoring the positive shift in labor supply occurring before legalization may thus lead to misleading conclusions on the overall employment effects of an amnesty.

[9]In online Appendix A, we throw some light on this as-yet unexplored issue using a novel conceptual framework. We develop a simple Nash bargaining model in which we capture the prospect of legalization in three complementary ways: a lower apprehension probability for potentially eligible undocumented workers, a positive payroll tax/legalization fee on firms, and a premium that immigrants associate with being legalized. This framework implies that the possibility of future legal status modifies the job match surplus—defined as the difference between the maximum wage a firm is willing to pay to employ an undocumented worker and the immigrant's reservation wage—for undocumented immigrants who can be legalized compared to those who cannot, and thus their relative employment rate.

[10]We identify the conditions under which the prospect of legal status unambiguously increases the job match surplus in online Appendix A.

The amnesty program we study in this article entails both a predetermined condition and a current employment requirement. This type of legalization splits undocumented immigrants into one group that satisfies the first requirement and another that does not. Throughout the article, we define these two groups as *qualified* and *unqualified*, respectively. Conditional on having or finding a job, only the former group becomes fully eligible for legal status, meaning that amnesty with such a design shifts both labor demand and supply—but only for qualified immigrants. Those who do not satisfy the predetermined condition (the unqualified) are left out of the legalization process and experience no change in job match surplus. This surplus differential can in turn be expected to affect both job retention and job finding rates and, ultimately, relative employment rates. For instance, if the surplus associated with qualified immigrants is higher than that linked to unqualified immigrants, we expect that the former will have higher job retention and higher job finding rates, leading in turn to a progressively higher employment rate among the qualified immigrants after the announcement of amnesty. If being qualified reduces the net job match surplus, the reverse will be true.

In sum, under the plausible assumption that the job match surplus for qualified immigrants is greater than that for unqualified immigrants, we expect a higher employment rate for the former group. Although in principle this implication could be tested by regressing undocumented immigrants' employment status on an indicator for being qualified (i.e., satisfying the predetermined eligibility condition), retrieving a causal parameter from such a regression requires random assignment of the qualified status to the immigrant population. The design of the 2002 Italian regularization program and the uniqueness of our data permit us for the first time to address this empirical question in a quasi-experimental setting.

A Natural Experiment

The 2002 Italian Amnesty

The natural experiment analyzed here is an amnesty for undocumented workers deliberated by the Italian government on September 9, 2002, and made effective the next day (Decree-Law no. 195/2002). This amnesty, Italy's largest legalization process ever with more than 700,000 applications, offered a renewable two-year work and residence permit to all undocumented immigrants who could find an employer willing to legally hire them under a minimum one year contract at a minimum monthly salary (439 euros) and to pay an amnesty fee (330 euros for domestic workers and 800 euros for all other workers).[11] Unlike all previous amnesties granted in Italy, the applications had to be filed directly by the employers rather than by the immigrants. Employers were also asked to declare that they had

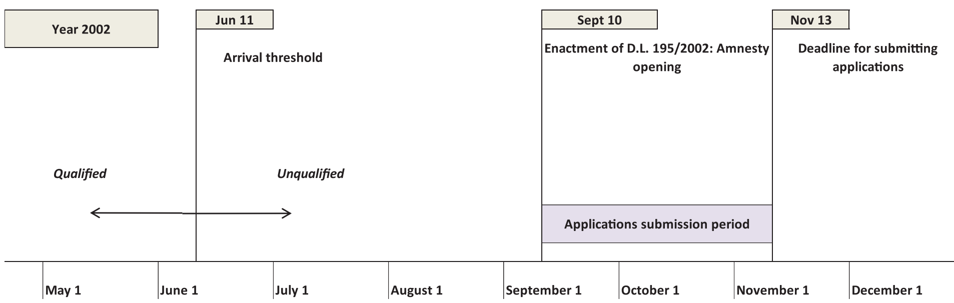
[11]Legalization of immigrant workers did not extend to family members.

continuously employed the immigrant for the three months before the legalization law was passed, that is, since June 11, 2002. It is crucial to note that this last condition was only *formally* a predetermined employment requirement, but it was *effectively* a predetermined residence requirement. Indeed, all employment relationships of undocumented immigrants are by definition informal and unknown to the authorities. As such, their exact duration is hardly measurable and clearly not verifiable, making the past employment requirement not enforceable. Coherently, the amnesty application procedure did not require employers to prove in any way the duration of immigrants' past employment, and it simply requested them to pay a fee roughly equivalent to three months of overdue social security contributions. Nevertheless, a necessary condition for immigrants to have been employed since June 11, 2002, was that they had arrived in Italy before that date. This condition was actually verifiable. The amnesty application form, indeed, required stating the exact date of arrival in Italy and attaching copies of all passport pages to the application form. It is worth noting that the vast majority of undocumented immigrants in Italy are visa overstayers (up to 70%, according to data from the Italian Ministry of Internal Affairs for the 2000–2006 period; Fasani 2010), whose presence in Italy before June 11, 2002, could be established by the visa stamp on the passport and the Italian police records. In addition, in the case of amnesty applications being checked, immigrants who arrived before the threshold date were more able to provide documentation supporting their eligibility (e.g., money transfer receipts, medical records, mobile phone contracts). Making false statements in the amnesty application was a punishable criminal offense, and therefore providing information that could be easily falsified could not only lead to the rejection of the application but, potentially, also to a criminal charge against the employer.[12] Filing an amnesty application for an undocumented immigrant who had truly arrived before June 11, 2002, would have a higher chance of success and would not imply any risk for the employer.

Applications could be submitted during a two-month period—from September 10 to November 13, 2002—beginning on the day the amnesty decree was approved. After the submission deadline, Italian police authorities began screening the applications and summoning successful employers and immigrants to sign their employment contracts. Only when this last stage had been successfully completed was the residence permit granted. The amnesty simultaneously legalized both the residence status and the employment contract of successful applicants, and it implied that the Italian authorities could not prosecute employers and employees for any of the past law infringements reported in the application (e.g., undeclared

[12]The submission of false statements or documents to the Italian authorities in the application for amnesty was punishable with up to nine months of detention (and possibly more, if the false declarations were recognized as a more serious offense, such as fraud or corruption). Approximately 20% of unsuccessful applicants were sentenced to expulsion from the country (Ministry of Interior, *Direzione centrale dell'immigrazione e della polizia di frontiera*, November 11, 2004).

Figure 1. Amnesty Timeline



employment, tax evasion, unauthorized entry and residence). Protection from deportation of the undocumented applicants was also granted during the screening process. The screening process took almost two years to be completed, and approximately 95% of applicants eventually received legal status.

The time frame of the amnesty program is presented in Figure 1, in which qualified and unqualified immigrants are those who arrived in Italy before and after June 11, 2002, respectively.

Because the 2002 Italian amnesty program entails both a predetermined condition and a current employment requirement, we expect it to modify the *job retention rate* of qualified immigrants, thereby creating a difference in their employment rate compared to unqualified immigrants (see next section). We cannot rule out the possibility, however, that immigrants who arrived before that date but were not employed when amnesty was announced might also experience a change in their *job finding rate*. In fact, as long as the migrant had been in Italy at least since June 11, 2002, employers willing to hire this worker and to apply for amnesty could easily make a false declaration that the employment relationship had begun before the threshold date.[13]

Identification Strategy

In our empirical analysis, we exploit the discontinuity created by the retrospective condition of arrival date in Italy to identify the causal effect of the prospect of legalization on the employment status of undocumented immigrants. The unexpected and unpredictable nature of this discontinuity generates a quasi-random assignment of undocumented immigrants around

[13]Note that the possibility for immigrants and employers to provide false statements is not specific to this particular amnesty or to the Italian context. Serious limitations in authorities' ability to verify statements contained in applications arise with any amnesty attempting to introduce eligibility rules for legal status. For instance, the US 1986 IRCA-SAW program legalized more than 1.2 million unauthorized immigrants conditional on their having been employed in the agricultural sector. The U.S. Immigration and Naturalization Service concluded that it was nearly impossible to distinguish a legitimate from a fraudulent SAW application (see Gonzalez Baker 1990).

the threshold date. Even though the granting of amnesty was intensely debated within the government coalition, received wide coverage in the Italian media, and might have been foreseeable based on the frequency and regularity of earlier general amnesties (in 1986, 1990, 1995, and 1998; see Fasani 2010), two crucial and intertwined aspects could not have been predicted even by very well-informed immigrants. First, it was impossible to forecast *if* and *when* the Italian government would reach a consensus and actually pass an amnesty law; second, it was equally difficult to predict the exact criteria for eligibility, in particular, the length of the minimum residence in Italy.[14] The uncertainty about these two aspects makes the retrospective arrival threshold completely ex ante unpredictable for immigrants, thus preventing endogenous sorting around it. This unpredictable discontinuity creates a local randomized experiment (Lee 2008; Lee and Lemieux 2010); that is, there is no reason to expect significant differences in (observable and unobservable) characteristics between immigrants who arrived immediately before and immediately after June 11, 2002.

The experiment is local because outside the neighborhood of the threshold we can expect a substantial selection into eligibility as potential immigrants keen on becoming legal residents intensified and accelerated their attempts to arrive in Italy in time for amnesty. If the unobserved characteristics determining these individuals' migration behavior (e.g., networks, credit constraints) are correlated with their employment outcomes in Italy, this selection would introduce a bias into our estimates. We therefore remove this bias by comparing only individuals who arrived in Italy in a neighborhood of the threshold date.

Data and Estimation

In this article, we use a unique data set collected by Naga, a large Italian nongovernmental organization (NGO) founded in 1987 that offers free basic health care exclusively to undocumented immigrants.[15] Providing health care for a daily average of more than 60 visits, five days a week, this association does not discriminate against immigrants in any way according to nationality or religion. Naga has only one branch, located in a fairly central and well-connected area of Milan, the second largest Italian city, whose province was home to 3.7 million inhabitants in 2002 (6.5% of the Italian population). About 150,000 of the city's inhabitants were legally resident immigrants (9.7% of the foreign population in the country). The province

[14]The length of this minimum residence period could not be inferred from previous amnesties. Indeed, the amnesties in 1998 and in 1990 required seven months and two months, respectively, of minimum residence in Italy, whereas the amnesties approved in 1986 and 1995 made no such stipulation—undocumented immigrants simply had to prove they had been in Italy at least since the day before the law was passed. None of the previous amnesties included an employment requirement.

[15]Documented immigrants are completely integrated into the Italian National Health Service, so if they seek medical assistance at Naga, the staff redirects them to public hospitals.

received 87,000 applications for the 2002 amnesty, which amounts to approximately 12% of total amnesty applications. Volunteers collected data on each immigrant's first visit to Naga using a brief questionnaire that profiled immigrants' social and economic situation at the time of interview (gender, age, education, country of origin, month of arrival in Italy, current employment status). Unfortunately, this information is not updated after the first visit. These data, available in electronic format since 2000, constitute a cross-sectional data set of daily observations on undocumented immigrants.[16]

This data set offers two major advantages. First, when used in conjunction with the quasi-experimental setting created by the 2002 amnesty, it allows us to create an almost ideal comparison group of undocumented immigrants randomly excluded from applying for amnesty (Kaushal 2006). Second, the availability of daily observations allows us to analyze the employment status of undocumented immigrants at various points in time.

The main shortcoming of the data set is that it includes only individuals who visited the Naga premises for medical care. The vast majority of them attend Naga for basic and temporary medical needs; treatment for emergency and chronic disease is offered by the Italian National Health Service. The sample selection does not threaten our identification strategy because the exogeneity of the cutoff arrival day ensures that the selection into Naga should not systematically differ between qualified and unqualified immigrants.[17] To investigate the extent of this selection, in Appendix Table A.1, we compare the Naga sample with the Iniziative e Studi Sulla Multietnicità (ISMU) sample,[18] a random sample of undocumented immigrants living in Milan. We find that the two data sets are very similar, although Naga tends to oversample women, which is consistent with the well-established fact that women have higher levels of health care utilization than do men (Bertakis et al. 2000; Redondo-Sendino, Guallar-Castillón, Banegas, and Rodríguez-Artalejo 2006).

[16]An earlier version of this data set was used in Devillanova (2008), to which we refer for an accurate description of the data and individual variables.

[17]These data limitations should be assessed bearing in mind the intrinsic difficulties of researching undocumented migration: Given that one ignores both the size and characteristics of such a population, extracting a truly representative sample is simply not possible. Such is even more the case when the object of analysis, as in our article, is the population of recently arrived undocumented immigrants, whose elusiveness is magnified. Our data set shares this limitation with any other sample used in the literature on undocumented immigrants (e.g., the LPS data set is a random sample of the self-selected subpopulation of applicants for the LAW-IRCA amnesty).

[18]ISMU is an independent research foundation that promotes studies on immigration. The ISMU data are sampled using an intercept point survey methodology based on the tendency of immigrants to cluster at certain locations (McKenzie and Mistiaen 2009). The ISMU survey provides a representative sample of the total migrant population residing in the Lombardy region. The interview questionnaire contains a variety of questions on individual characteristics (e.g., demographics, educational level, labor market outcomes, legal status) and household characteristics (e.g., number of household members in Italy, family members abroad, housing). Unfortunately, ISMU data are not suitable to perform our main DiD analysis as they have no information on the month of arrival in Italy. See Dustmann et al. (2017) for a description of these data.

To estimate the causal effect of the prospect of obtaining legal status on employment probability, we look at migrants arriving in Italy around the amnesty threshold date (June 11, 2002) and compare the employment rate of those who entered before this threshold (qualified) with those who entered after (unqualified). Although ideally the treatment and comparison groups should include only those immigrants who arrived in Italy on the day before or after the arrival threshold, this procedure is infeasible because our data set precisely records only the month and year of entry into Italy. We therefore assign individuals to the treatment and comparison group according to month of arrival, excluding all those who arrived in June 2002 because we cannot determine whether they arrived before or after June 11. We then define as qualified (the treatment group) all immigrants who arrived in April and May 2002 and as unqualified (the control group) all those who arrived in July and August 2002.[19] Individuals who arrived outside of these months were excluded from the analysis.

For both groups, we measure the employment rate at the same point in time in order to keep constant the overall labor market conditions to which the immigrants were exposed. The availability of daily observations in our data set allows for a high degree of flexibility in choosing when to measure migrant employment. It would, of course, be preferable to examine employment status the day after amnesty closed (November 14, 2002), when all applications had been submitted but no one had yet been legalized. To increase the sample size, however, we need to extend our observation window. We face a trade-off between having a larger sample size and introducing an amnesty-induced sample selection: the further away from the amnesty deadline, the more likely that amnesty applicants have gained legal status and disappeared from our sample.[20] We use a two-month observation window (November 14–January 13), which also coincides with the screening period initially envisaged by the amnesty bill.[21] Figure 2 summarizes the time structure of our analysis.

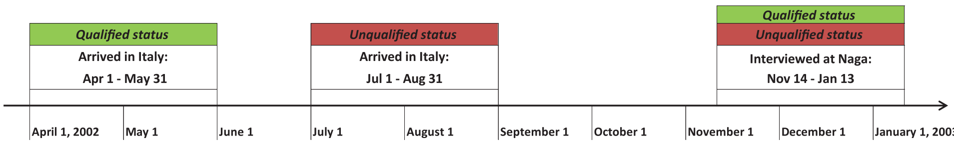
By construction, individuals in the treatment group have spent more time in Italy than have those in the control group. Because time spent in the host country is a key determinant of immigrants' labor market integration, a finding that qualified immigrants have a higher employment rate than unqualified immigrants might simply reflect different average residence

[19]To check the robustness of our results, we further restrict the neighborhood around the eligibility threshold by comparing those who arrived in May 2002 with those who arrived in July 2002. The results are qualitatively similar, although the sample size shrinks.

[20]In fact, not only those actually legalized but also those who had applied for amnesty but were still waiting were entitled to receive free medical care from the National Health Service and so were no longer admitted to Naga. This process, however, involved some administrative delay and some learning on all sides—migrants, public hospitals, and Naga volunteers—so in the weeks immediately after the amnesty deadline, applicants in need of medical assistance still had to turn to Naga. As time passed, however, applicants tended to disappear from the sample.

[21]Decree-Law no. 195/2002, article 4. Our results hold when using different observation windows after the amnesty deadline (one, two, and three months). Results are available upon request.

Figure 2. Estimation Timeline



spells. We address this potential threat to our identification strategy using a difference-in-differences (DiD) setting. Specifically, using data from two years before and two years after 2002, we check whether significantly different employment rates between April–May immigrant arrivals and July–August immigrant arrivals were also in place during non-amnesty years. We construct consistent samples for amnesty and non-amnesty years: For each year t in the 2000–2004 interval, our main sample contains undocumented immigrants observed at Naga between November 14 t and January 13 $t+1$ who had arrived in Italy in April, May, July, or August of the same year t.

We then estimate the following linear probability model:

$$EMPL_{it} = \alpha APMAY_i + \beta APMAY_i \times Y2002_t + X_{it}\gamma + \tau_t + u_{it} \quad (1)$$

where $EMPL_{it}$ is a dummy variable that equals 1 if individual i who arrived in Italy in year t is employed, and 0 otherwise. Similarly, $APMAY_i$ is a dummy variable equal to 1 for immigrants who arrived in April or May and equal to 0 for those who arrived in July or August of every year t, which captures any systematic difference in employment probability between the two groups. The interaction term $APMAY_i \times Y2002_t$ identifies qualified immigrants; that is, those who arrived in April or May in the amnesty year 2002. The vector X_{it} includes individual control variables; τ_t is a full set of year dummies for the 2000–2004 period that captures all year-specific labor market features equally affecting all individuals in the sample, and u_{it} is an idiosyncratic shock. Our main coefficient of interest is β, which measures the difference in employment probability between qualified and unqualified undocumented immigrants. The sign of this coefficient is theoretically ambiguous: Whereas supply should unambiguously increase in response to the prospect of legal status, the direction of shifts in labor demand is unclear (see Conceptual Framework section). Hence, a positive and significant coefficient would suggest that the prospect of legal status (i.e., being qualified) significantly increases the surplus of job matches with immigrants who can be legalized, leading to a higher probability of being employed.

Descriptive Statistics

Panel A in Table 1 reports summary statistics for our main sample, and in the next two panels we differentiate between immigrants who arrived in April to May and immigrants who arrived in July to August in year 2002

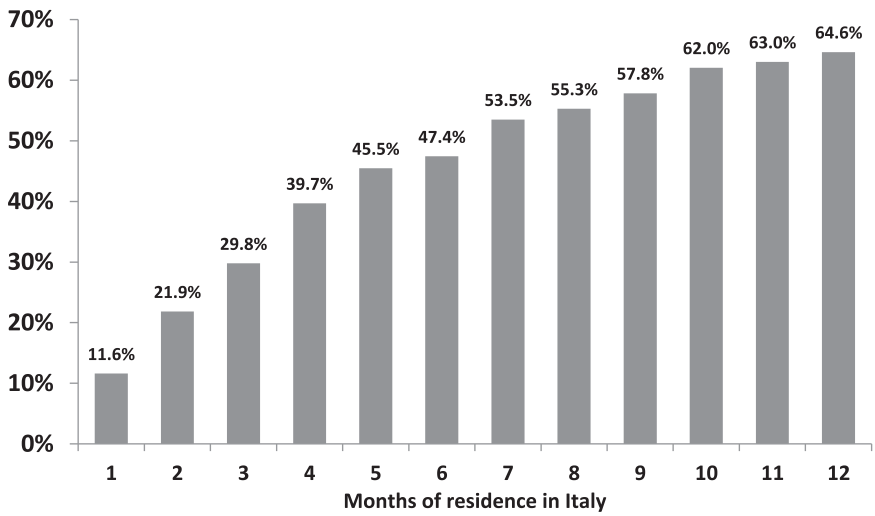
Table 1. Descriptive Statistics

Variable		Panel A: Whole sample	Panel B: 2002 (amnesty year): Arrived in April–May	Panel B: 2002 (amnesty year): Arrived in July–August	Panel C: 2000, 2001, 2003, 2004: Arrived in April–May	Panel C: 2000, 2001, 2003, 2004: Arrived in July–August
Employment	mean	0.511	0.625	0.403 †	0.502	0.519
	sd	*0.500*	*0.489*	*0.495*	*0.501*	*0.501*
Men	mean	0.518	0.571	0.532	0.478	0.535
	sd	*0.500*	*0.499*	*0.503*	*0.501*	*0.500*
Age	mean	30.822	30.338	29.896	31.611	30.530
	sd	*8.678*	*7.581*	*9.061*	*9.011*	*8.540*
Education						
Primary	mean	0.127	0.179	0.145	0.089	0.142
	sd	*0.334*	*0.386*	*0.355*	*0.285*	*0.350*
Secondary	mean	0.360	0.321	0.258	0.360	0.392
	sd	*0.480*	*0.471*	*0.441*	*0.481*	*0.489*
High school	mean	0.420	0.411	0.516	0.429	0.392
	sd	*0.494*	*0.496*	*0.504*	*0.496*	*0.489*
University	mean	0.093	0.089	0.081	0.123	0.073
	sd	*0.291*	*0.288*	*0.275*	*0.329*	*0.261*
Origin						
Europe	mean	0.196	0.054	0.323 †	0.163	0.223 †
	sd	*0.397*	*0.227*	*0.471*	*0.370*	*0.417*
Asia	mean	0.090	0.054	0.065	0.089	0.104
	sd	*0.286*	*0.227*	*0.248*	*0.285*	*0.306*
North Africa	mean	0.203	0.250	0.274	0.192	0.185
	sd	*0.403*	*0.437*	*0.450*	*0.395*	*0.389*
Sub-Saharan Africa	mean	0.072	0.089	0.097	0.030	0.096 †
	sd	*0.259*	*0.288*	*0.298*	*0.170*	*0.295*
Latin America	mean	0.439	0.554	0.242 †	0.527	0.392 †
	sd	*0.497*	*0.502*	*0.432*	*0.500*	*0.489*

Notes: Panel A reports means and standard deviations of selected characteristics of our main sample. The next two panels differentiate between immigrants who arrived in Italy in April to May (*qualified*) and July to August (*unqualified*) in the amnesty year 2002 (Panel B) and in control years 2000, 2001, 2003, and 2004 (Panel C). Data for the individuals in all groups were collected on their first visit to Naga between November 14 and January 13 in each year. The sample is composed of 581 individuals, 45% of whom have "qualified" status. "sd," standard deviation.
†Denotes a difference between the treatment and the control group that is significant at least at 5%.

(panel B) and in the non-amnesty years 2000, 2001, 2003, and 2004 (panel C). The average age of the sample is almost 31 years, with 52% being male. The education level is relatively high: approximately 42% have attended high school and about 9% have some university education. In panel B, we show that the differences between the qualified and the unqualified group in these variables are never statistically significant at 5%. We find a similar pattern in non-amnesty years (panel C). The distribution of areas of origin is slightly different between the two groups in both amnesty and non-

Figure 3. Average Employment Rate of Undocumented Immigrants (2000–2004)



Notes: The figure is based on individuals in the 2000–2004 Naga data set with at most 12 months of residence in Italy. Sample size: 13,732 observations.

amnesty years, suggesting a seasonality in undocumented flows from different source countries that is completely unrelated to the 2002 amnesty. In our empirical analysis, we always report both conditional and unconditional estimates. About half of the regression sample is employed. Our data identify as employed all immigrants who reported having a paid job at the time of interview at Naga. We have no information on number of hours worked per week or on wages. Remarkably, the employment probability is not statistically different at 5% for immigrants who arrived in April to May and immigrants who arrived in July to August, but in the amnesty year 2002, the employment probability of qualified immigrants is 23 percentage points higher than the probability of unqualified immigrants. This difference between the two groups is attributable to both a higher share of employed workers in the group of qualified immigrants and to a lower share of employed workers among unqualified immigrants in 2002 relative to other years. Although this descriptive evidence may suggest that our control group was somehow affected by the 2002 amnesty, in the robustness checks we use alternative control groups to show that this was not the case.

Figure 3, based on the almost 14,000 individuals with at most 12 months of residence in Italy who are in the Naga data set in the 2000 to 2004 period, illustrates the evolution of these undocumented immigrants' employment probability over their first year of residence in Italy. Immediately apparent is that the employment rate of recently arrived undocumented immigrants changes considerably with time spent in the host country. Only 12% of immigrants with one month of residence in Italy report having a job, but the share of employed immigrants increases by roughly 10 percentage points for each additional month, reaching 40%

after four months. The profile then tends to become somewhat flatter, stabilizing around 60% for immigrants with a residence duration of 10 months or longer. In general, therefore, the employment probability of undocumented immigrants increases by 50 percentage points during the first year after arrival in Italy.

Descriptive evidence in Table 1 shows that observable individual characteristics are evenly distributed in our treatment and control groups, which also serves as a test of treatment status randomness. Although the ex ante unpredictability of the retrospective arrival threshold prevented immigrants to endogenously sort around it, two types of potential concerns remain regarding the observed composition of our sample. First, although one important advantage of our data set is that it is based on the information immigrants reported to an independent NGO—and thus they had no clear motivation to falsely report their date of arrival to Naga volunteers—we cannot completely rule out the possibility of misreporting.[22] Specifically, for example, employed undocumented immigrants in the unqualified group might have falsely reported an earlier arrival date to appear eligible for the amnesty. A second concern is that patterns of return migration might have differed among qualified and unqualified immigrants. In particular, the former may have had more incentives to remain in Italy to enjoy the future reward of legal status than did the latter. Selective outmigration, however, is unlikely to be a major issue in our analysis, given the very short time window we consider.

We can empirically test for evidence of either source of bias by comparing the reported distribution of months of arrival of immigrants going to Naga in 2002 and in non-amnesty years. In the presence of selective outmigration and/or misreporting, we should observe that those who went to Naga during fall 2002 were systematically more likely to report arriving in Italy before June of the same year than would immigrants who went to Naga during the fall of non-amnesty years. In online Appendix B, we empirically test for this implication, finding no evidence in this direction. Our empirical exercise is analogous to the McCrary (2008) test.

Estimation Results

Main Results

We start by estimating our main DiD regression (1). We report results from linear probability models, and we account for the heteroskedasticity this choice implies by using robust standard errors.[23] Table 2 reports the

[22]Whether the Italian authorities would judge immigrants' applications as eligible for amnesty was completely independent of their answers to Naga volunteers. In addition, Naga is an independent NGO that does not exchange information with the Italian authorities, an independence of which undocumented immigrants are aware and that is the precise reason they go to Naga without fearing arrest.

[23]In unreported regressions, we have checked the robustness of our findings to using probit or logit regression models. Results are available upon request.

Table 2. Difference-in-Differences (DiD) Estimates: Main Results

Variable	*1*	*2*	*3*	*4*	*No. of observations*
		Panel A. Amnesty effect			
Qualified status (β)	0.240**	0.236**	0.252**	0.262***	581
	[0.102]	[0.102]	[0.099]	[0.100]	
		Panel B. Initial difference			
Qualified status (β)	0.034	0.033	0.054	—	419
	[0.140]	[0.138]	[0.140]		
Gender, age, education	No	Yes	Yes	Yes	
Area of origin	No	No	Yes	Yes	
Month dummies	No	No	No	Yes	

Notes: Each cell reports the estimated coefficient on the interaction between a dummy for arrival in April to May and a dummy for the amnesty year 2002 from linear regressions of a dummy for employment status on a dummy for arrival in Italy in April or May (versus July or August), dummies for years 2000 to 2004, and the interaction of the arrival dummy with the 2002 dummy. In panel A, which reports the amnesty effect, immigrants are observed between November 14 of each year and January 13 of the following year. In panel B, which checks for initial differences in the two arrival groups, immigrants are observed between the 1st and the 31st of September of each year. Columns (2) to (4) gradually add in additional control variables. Gender, age, and education controls include a male dummy, dummies for five-year age groups, and dummies for four education levels (primary, secondary, high school, university). Area of origin is denoted by dummies for five macro-areas of origin: Europe, Asia, North Africa, Sub-Saharan Africa, and Latin America. Month dummies are variables indicating the month in which an individual was observed. Panel B is based on a single month sample, thus column (4) is not reported. The last column displays the number of observations used in each regression. Robust standard errors are in brackets.
***$p < 0.01$; **$p < 0.05$; and *$p < 0.1$.

estimates of the main coefficient of interest in our DiD exercise: the interaction between the dummy for April to May (versus July to August) arrival in each year and the dummy for the amnesty year 2002. Each cell in the table reports the estimated coefficient from a separate regression. Column (1) reports the unconditional estimates, and the following three columns gradually add further groups of control variables (gender, age, and education; area of origin dummies; month dummies). We maintain this structure throughout the rest of the article.

Panel A of Table 2 shows that the impact of amnesty on employment probability is positive, strongly significant, and remarkably stable across different specifications. If we focus on the fully specified model (column (4)), we find that the prospect of obtaining legal status increases undocumented immigrants' employment probability by 26.2 percentage points, with a coefficient that is significant at the 1% level.[24] Based on our theoretical discussion, this result suggests that the prospect of legal status increases the net surplus of job matches with qualified immigrants, leading to a higher

[24]In unreported regressions, we test for heterogeneity in the eligibility effect on employment, by including additional interactions with gender and education level. Point estimates suggest a slightly stronger effect for women, although the difference is far from being statistically significant at any conventional level. We find no evidence of heterogeneity by education.

employment rate among this group of immigrant workers. This larger surplus is the result of theoretically ambiguous shifts in labor demand and of an unambiguously positive shift in labor supply.

Yet how large is the estimated effect? Recently arrived undocumented immigrants have a very low probability of being employed but tend to experience sharp increases in their employment rates in the first few months after arrival, specifically, about a 50 percentage point increase within the first 12 months. Hence, the prospect of obtaining legal status accelerates the labor market integration of newly arrived undocumented immigrants by approximately half the increase in employment they normally experience in their first year after arrival.

Using the DiD setup of Equation (1), we can check whether the employment status differs between the two groups before the amnesty. This method provides a compelling test of treatment status randomness. Given that before the deliberation on the amnesty bill, qualified and unqualified immigrants were indistinguishable, their employment probability should not have systematically differed. Finding evidence against this conjecture would imply an immediate loss of credibility for our entire empirical exercise. Indeed, the common trend assumption would be immediately falsified if the employment rates of the two groups were already diverging before the amnesty. Bearing in mind that the amnesty was announced on September 10, 2002, and that our control group is all those who arrived in July and August, we are left with only the first nine days of September and a few observations to perform this empirical exercise. To have a reasonable sample size, we extend the observation window to the whole month of September. This choice is conservative for our purpose, meaning that it makes it more likely to find a statistically significant difference in the employment probability between the two groups because it includes 20 days (September 11 to September 30) during which qualified immigrants (and employers) could potentially react to the amnesty announcement.

Results for our coefficient of interest estimated in September are reported in panel B of Table 2 ("Initial difference"). Note that column (4) is not reported because we cannot identify month dummies using one single month of observations. As Table 2 shows, the point difference between the two groups' employment rates is close to zero and not statistically significant in any specification. Reassuringly, the substantial difference in employment rate we observe after the application period ended (panel A) did not pre-exist the amnesty announcement.

Robustness Checks

To check the robustness of our results, we first run a falsification test using placebo arrival thresholds. If our estimations truly capture the effect of the prospect of legal status, we should find no systematic differences in

Table 3. Placebo Tests: Qualified and Unqualified vs. Unqualified

Variable	*1*	*2*	*3*	*4*	*No. of observations*
Qualified (February-March) vs. Qualified (April-May)	0.061 [0.105]	0.061 [0.104]	0.037 [0.101]	0.035 [0.102]	503
Qualified (April) vs. Qualified (May)	–0.043 [0.148]	–0.082 [0.143]	–0.094 [0.142]	–0.087 [0.144]	259
Unqualified (July-August) Vs Unqualified (September-October)	–0.024 [0.083]	–0.020 [0.084]	0.001 [0.082]	–0.002 [0.081]	793
Unqualified (July) Vs Unqualified (August)	–0.106 [0.156]	–0.128 [0.162]	–0.138 [0.155]	–0.115 [0.148]	322
Gender, age, education	No	Yes	Yes	Yes	
Area of origin	No	No	Yes	Yes	
Month dummies	No	No	No	Yes	

Notes: The first row reports the estimated coefficient on the interaction between a dummy for arrival in February to March vs. April to May and a dummy for the amnesty year 2002 from linear regressions of a dummy for employment status on a dummy for arrival in Italy in February or March (versus April or May), dummies for years 2000 to 2004, and the interaction of the arrival dummy with the 2002 dummy. Rows 2 to 4 have the same structure, but the arrival dummy is modified as described in each row's heading. Immigrants are observed between November 14 of each year and January 13 of the following year. Columns (2) to (4) gradually add in additional control variables (controls are identical to those described in the note to Table 2). The last column displays the number of observations used in each regression. Robust standard errors are in brackets.
***$p < 0.01$; **$p < 0.05$; and *$p < 0.1$.

employment within the groups of qualified or unqualified immigrants. Indeed, all qualified immigrants should be as intensely affected by the policy, whereas all unqualified immigrants should remain totally unaffected. To verify that placebo thresholds have no significant effects, we first estimate our DiD regressions with the actual threshold (June 11) replaced by a placebo threshold of April 1 and compare qualified immigrants who arrived in February to March with those who arrived in April to May. As an alternative, we also split the group of qualified immigrants used in the main analysis (those who arrived in April–May) into two subgroups: those who arrived in April compared to those who arrived in May, implying a threshold date of May 1 (see online Appendix Figure A.1). The first and second rows of Table 3 report the results for the April 1 and May 1 thresholds, respectively. As before, column (1) reports the unconditional estimates, and columns (2) to (4) gradually include additional controls.

The next two rows of Table 3 display the results from similar placebo tests performed only on the population of unqualified immigrants. First, in row three, we compare the group of unqualified immigrants used in our main analysis (i.e., those who arrived in July–August) with those who arrived in the following two months (September–October), and then, in the fourth row, we split the July to August group into two subgroups (July versus August). Again, this division is equivalent to setting two alternative placebo

Table 4. DiD Robustness Checks: Alternative Control Years

Variable	*1*	*2*	*3*	*4*	*No. of observations*
			Panel A. Amnesty effect		
2002 vs. (2003 & 2004)	0.280**	0.280**	0.321***	0.323***	295
	[0.118]	[0.118]	[0.116]	[0.116]	
2002 vs. (2001 & 2003)	0.244**	0.253**	0.265**	0.266**	343
	[0.113]	[0.113]	[0.111]	[0.111]	
2002 vs. (2000 & 2001)	0.216**	0.202*	0.197*	0.217**	404
	[0.108]	[0.109]	[0.107]	[0.108]	
			Panel B. Initial difference		
2002 vs. (2003 & 2004)	–0.020	–0.003	–0.019	–	189
	[0.154]	[0.146]	[0.156]		
2002 vs. (2001 & 2003)	0.026	0.028	0.039	–	225
	[0.150]	[0.159]	[0.157]		
2002 vs. (2000 & 2001)	0.065	0.069	0.094	–	284
	[0.146]	[0.149]	[0.150]		
Gender, age, education	No	Yes	Yes	Yes	
Area of origin	No	No	Yes	Yes	
Month dummies	No	No	No	Yes	

Notes: In both panels, each cell reports the estimated coefficient on the interaction between a dummy for arrival in April to May and a dummy for the amnesty year 2002 from linear regressions of a dummy for employment status on a dummy for arrival in Italy in April or May (versus July or August), year dummies, and the interaction of the arrival dummy with the 2002 dummy. Rows differ in the control years used in the analysis, as described in each row's heading. In panel A, which reports the amnesty effect, immigrants are observed between November 14 of each year and January 13 of the following year. In panel B, which checks for initial differences in the two arrival groups, immigrants are observed between the 1st and the 31st of September of each year. Columns (2) to (4) gradually add in additional control variables (controls are identical to those described in the note to Table 2). Panel B is based on a single month sample, thus column (4) is not reported. The last column displays the number of observations used in each regression. Robust standard errors are in brackets.
***$p < 0.01$; **$p < 0.05$; and *$p < 0.1$.

thresholds on September 1 and August 1, respectively. The results in Table 3, far from falsifying our findings, strongly support their validity. Regardless of whether the threshold is moved forward or back by one month or two, we find no effect of placebo qualified status on the employment status of undocumented immigrants. In fact, none of the coefficients of interest obtained from these 16 placebo regressions is even marginally statistically significant.

Our second set of robustness checks is designed to verify that the results are not driven by the inclusion of specific non-amnesty years in the estimating sample. For this set, we replicate our main results using the two years after amnesty (2003 and 2004), the year before and after amnesty (2001 and 2003), and the two years before amnesty (2000 and 2001), reported in panel A of Table 4. All results are fully robust to changes in the set of control years. Panel B of Table 4 shows that our estimates of the initial differences between the two groups also are unaffected.

In our third falsification exercise, based on placebo amnesty years (Table 5), we run DiD regressions in which 2002 is dropped from the sample and

Table 5. Placebo Amnesty

Years	*1*	*2*	*3*	*4*	*No. of observations*
			Panel A. Amnesty effect		
2000	0.110	0.128	0.137	0.139	463
	[0.099]	[0.098]	[0.099]	[0.099]	
2001	–0.041	–0.042	–0.016	–0.021	463
	[0.102]	[0.102]	[0.102]	[0.102]	
2003	0.043	0.033	0.017	0.011	463
	[0.119]	[0.117]	[0.118]	[0.119]	
2004	–0.141	–0.153	–0.182	–0.173	463
	[0.116]	[0.115]	[0.115]	[0.115]	
			Panel B. Initial difference		
2000	–0.021	–0.034	–0.026	—	365
	[0.102]	[0.103]	[0.104]		
2001	–0.074	–0.071	–0.060	—	365
	[0.112]	[0.112]	[0.113]		
2003	0.118	0.139	0.130	—	365
	[0.118]	[0.121]	[0.123]		
2004	0.007	0.001	–0.018	—	365
	[0.128]	[0.125]	[0.124]		
Gender, age, education	No	Yes	Yes	Yes	
Area of origin	No	No	Yes	Yes	
Month dummies	No	No	No	Yes	

Notes: In both panels, each cell reports the estimated coefficient on the interaction between a dummy for arrival in April to May and a dummy for the placebo amnesty year indicated in each row's heading, from linear regressions of a dummy for employment status on a dummy for arrival in Italy in April or May (versus July or August), year dummies, and the interaction of the arrival dummy with the placebo amnesty year dummy. In panel A, which reports the amnesty effect, immigrants are observed between November 14 of each year and January 13 of the following year. In panel B, which checks for initial differences in the two arrival groups, immigrants are observed between the 1st and the 31st of September of each year. Columns (2) to (4) gradually add in additional control variables (controls are identical to those described in the note to Table 2). Panel B is based on a single month sample, thus column (4) is not reported. The last column displays the number of observations used in each regression. Robust standard errors are in brackets.
$^{***}p < 0.01$; $^{**}p < 0.05$; and $^{*}p < 0.1$.

each of the remaining non-amnesty years is alternatively given placebo amnesty status. Reassuringly, the resulting estimates of both the amnesty effect (panel A) and the "initial difference" (panel B) are generally very close to zero and never statistically significant.

Further, to ensure that the earlier estimated employment differential between qualified and unqualified immigrants originates exclusively from events in year 2002 and not from (unexplained) changes in other non-amnesty years, we estimate the following equation separately for each year in our sample:

$$EMPL_i = a + bAPMAY_i + X_i c + \varepsilon_i \tag{2}$$

where the employment status of undocumented migrants is regressed on a dummy for arrival in April to May and other individual controls. This

Table 6. Year-by-Year Estimates

Variable	*1*	*2*	*3*	*4*	*No. of observations*
Amnesty year					
2002	0.222**	0.217**	0.198**	0.212**	118
	[0.091]	[0.095]	[0.095]	[0.099]	
Placebo years					
2000	0.057	0.037	0.014	0.013	147
	[0.081]	[0.086]	[0.091]	[0.093]	
2001	–0.046	–0.055	–0.049	–0.049	139
	[0.085]	[0.091]	[0.091]	[0.091]	
2003	0.017	0.034	0.038	0.042	86
	[0.108]	[0.109]	[0.117]	[0.120]	
2004	–0.132	–0.113	–0.149	–0.138	91
	[0.104]	[0.107]	[0.117]	[0.120]	
Gender, age, education	No	Yes	Yes	Yes	
Area of origin	No	No	Yes	Yes	
Month dummies	No	No	No	Yes	

Notes: Each cell reports the estimated coefficient on a dummy for arrival in April to May from linear regressions of a dummy for employment status on a constant and a dummy for arrival in Italy in April or May (versus July or August). Results for the amnesty year 2002 and for all other non-amnesty years are reported in separate rows. Immigrants are observed between November 14 of each year and January 13 of the following year. Columns (2) to (4) gradually add in additional control variables (controls are identical to those described in the note to Table 2). The last column displays the number of observations used in each regression. Robust standard errors are in brackets.
***$p < 0.01$; **$p < 0.05$; and *$p < 0.1$.

specification, unlike our previous DiD estimates, fails to control for the dissimilar average permanence in Italy of individuals in the treatment and the control groups. Table 6 reports year-by-year estimates for Equation (2), with each cell in the table corresponding to the estimated coefficient on the April to May dummy. We first perform this exercise in the year of amnesty (2002) and then in each of the four non-amnesty years (2000, 2001, 2003, and 2004). Our findings fully corroborate our previous results: As expected, we find a positive and significant effect of having arrived in April to May (rather than in July to August) only in year 2002.

Finally, we check the robustness of our results to the choice of alternative control groups. As argued above, our comparison group is very close to the ideal one: "a randomly selected group of undocumented immigrants similar to the target group, but ineligible for, and unaffected by, the amnesty" (Kaushal 2006: 635). The only aspect in which our control group may depart from this ideal definition regards the possibility that its members may have been directly affected by the policy, given that qualified and unqualified immigrants compete in the same local labor market.

We address this concern by using alternative control groups of legal immigrants and natives that, although less comparable to our treatment group, are unlikely to be affected by the amnesty. Table 7 reports results from estimating DiD regressions in which qualified and, separately,

Table 7. Alternative Control Groups

Variable	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*
	Unskilled						*All*					
	Qualified			*Unqualified*			*Qualified*			*Unqualified*		
Panel A. ISMU sample (2001–2004)												
i) Milan: legal immigrants, ysm ≤ 4	0.384***	0.334***	0.332***	0.013	–0.031	–0.030	0.346***	0.288***	0.291***	–0.029	–0.074	–0.072
	[0.109]	[0.112]	[0.111]	[0.115]	[0.115]	[0.113]	[0.100]	[0.103]	[0.103]	[0.106]	[0.107]	[0.106]
No. of observations	728	728	728	747	747	747	1,755	1,755	1,755	1,774	1,774	1,774
ii) Lombardy: legal immigrants, ysm ≤ 4	0.350***	0.297***	0.299***	–0.024	–0.048	–0.047	0.334***	0.283***	0.294***	–0.041	–0.065	–0.065
	[0.097]	[0.105]	[0.103]	[0.104]	[0.108]	[0.106]	[0.096]	[0.101]	[0.101]	[0.103]	[0.105]	[0.105]
No. of observations	3,435	3,431	3,431	3,454	3,450	3,450	6,964	6,956	6,956	6,983	6,975	6,975
iii) Milan: legal immigrants, ysm ≤ 2	0.305**	0.260**	0.260**	–0.058	–0.103	–0.101	0.328***	0.282**	0.282**	–0.047	–0.083	–0.084
	[0.129]	[0.131]	[0.129]	[0.133]	[0.132]	[0.131]	[0.107]	[0.109]	[0.110]	[0.112]	[0.113]	[0.113]
No. of observations	348	348	348	367	367	367	789	789	789	808	808	808
iv) Lombardy: legal immigrants, ysm ≤ 2	0.279***	0.252**	0.257**	–0.099	–0.104	–0.101	0.285***	0.257**	0.271***	–0.093	–0.098	–0.099
	[0.101]	[0.109]	[0.107]	[0.107]	[0.111]	[0.109]	[0.098]	[0.104]	[0.104]	[0.104]	[0.107]	[0.107]
No. of observations	1,352	1,351	1,351	1,371	1,370	1,370	2,856	2,854	2,854	2,875	2,873	2,873
Panel B. EULFS (2000–2004)												
v) Lombardy: all residents	0.218**	0.166*	0.197**	–0.109	–0.110	–0.102	0.216**	0.176**	0.206**	–0.112	–0.116	–0.121
	[0.088]	[0.091]	[0.091]	[0.097]	[0.100]	[0.099]	[0.088]	[0.087]	[0.089]	[0.097]	[0.098]	[0.097]
No. of observations	11,417	11,417	11,417	11,450	11,450	11,450	27,315	27,315	27,315	27,348	27,348	27,348
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender and age	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Education	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table reports results of DiD regressions where *qualified* (arrived in April–May; columns (1)–(3) and (7)–(9)) and, separately, *unqualified* (arrived in July–August; columns (4)–(6) and (10)–(12)) immigrants in the Naga sample are compared to alternative control groups of legal immigrants and natives. In Panel A, the four control groups are extracted from the ISMU sample and are defined as follows: i) legal immigrants who have spent less than four years in Italy and live in Milan; ii) legal immigrants who have spent less than four years in Italy and live in Lombardy; iii) legal immigrants who have spent less than two years in Italy and live in Milan; iv) legal immigrants who have spent less than two years in Italy and live in Lombardy. In Panel B, the control group is extracted from the EULFS sample and is composed of all legal residents (immigrants and natives) of Lombardy. In all cases, the control group is restricted to individuals age 15 to 40. We further restrict the control groups to unskilled individuals in columns (1) to (6), whereas we consider all levels of education in columns (7) to (12). Each cell reports the estimated coefficient on a dummy for being qualified in Naga sample in linear regressions of a dummy for employment status on a constant, the variable of interest and year dummies. Gender, age, and education controls include a male dummy, dummies for five-year age groups, and dummies for four education levels (primary, secondary, high school, university). Robust standard errors are in brackets.
***p <0.01; **$p < 0.05$; and *$p < 0.1$.

unqualified immigrants in the Naga sample are compared to five different control groups. These groups are defined as 1) legal immigrants who have spent less than four years in Italy and live in Milan; 2) legal immigrants who have spent less than four years in Italy and live in Lombardy; 3) legal immigrants who have spent less than two years in Italy and live in Milan; 4) legal immigrants who have spent less than two years in Italy and live in Lombardy; and 5) all legal residents (immigrants and natives) of Lombardy. In all cases, the control group is restricted to individuals age 15 to 40. For each control group, we first consider only unskilled (at most secondary education) individuals (columns (1)–(6)), and we then include all levels of education (columns (7)–(12)). Data on legal migrants are taken from an annual survey administered by the ISMU foundation to approximately 8,000 documented and undocumented immigrants in Lombardy (the region in which Milan is situated).[25] Data on the entire resident population of Lombardy instead come from the EU Labor Force Survey (EULFS; fourth quarter).

Irrespective of the control group considered, estimation results for qualified immigrants (columns (1)–(3) and (7)–(9)) are remarkably similar to our baseline estimates: We find a positive, sizeable, and statistically significant increase in employment rate in the amnesty year compared to a non-amnesty year. By contrast, results for unqualified immigrants (columns (4)–(6) and (10)–(12)) generally have a negative sign but are substantially smaller in absolute value and are never statistically significant at any conventional level. These additional results rule out any major concern that general equilibrium effects from the treatment to the control group in the Naga sample are driving the main findings of our empirical analysis.

Additional Results: Persistence of the Employment Effect

Our results so far indicate that the prospect of legal status under the 2002 Italian amnesty caused a substantial increase in the employment rate of qualified undocumented immigrants, which raises the policy-relevant question of this effect's persistence. Unfortunately, because the Naga sample is not longitudinal and does not include legalized immigrants, it cannot be used to address this issue. Instead, we again use the ISMU survey to derive descriptive evidence on the persistence of the employment effect. The 2003

[25]ISMU is an independent research foundation that promotes studies on immigration. The ISMU data are sampled using an intercept point survey methodology based on the tendency of immigrants to cluster at certain locations (McKenzie and Mistiaen 2009). The ISMU survey provides a representative sample of the total migrant population residing in the Lombardy region. The interview questionnaire contains a variety of questions on individual characteristics (e.g., demographics, educational level, labor market outcomes, legal status) and household characteristics (e.g., number of household members in Italy, family members abroad, housing). Unfortunately, ISMU data are not suitable to perform our main DiD analysis as they have no information on the month of arrival in Italy. See Dustmann et al. (2017) for a description of these data.

Table 8. Persistence of the Eligibility Effect on Undocumented Immigrants' Employment Status

Variable	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*
	Year(s) of arrival in Italy							
	1997–2001		*1999–2001*		*2001*		*2002*	
2002 Amnesty applicant	0.163***	0.169***	0.227***	0.234***	0.255***	0.261***	0.166***	0.162***
	[0.031]	[0.031]	[0.042]	[0.041]	[0.049]	[0.048]	[0.035]	[0.035]
Year and province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes	No	Yes
No. of observations	1,172	1,172	793	793	457	457	615	615
Share of applicants	0.79		0.81		0.75		0.47	

Notes: Each cell reports the estimated coefficient of an indicator for amnesty applicants in regressions of a dummy for employment status on a dummy that equals 1 if the respondent applied for the 2002 amnesty (and 0 otherwise), on year and province dummies, and on individual controls (age, age squared, gender, years since migration and its square, and dummies for education, and for geographic area of origin). Regressions are estimated on the sample of all undocumented immigrants who have arrived in Italy in 1997 to 2001 (columns (1)–(2)), 1999 to 2001 (columns (3)–(4)), 2001 (columns (5)–(6)), or 2002 (columns (7)–(8)). Robust standard errors are in brackets.
***$p < 0.01$; **$p < 0.05$; and *$p < 0.1$.

and 2004 waves of this survey contain information on whether the undocumented respondents had applied for the 2002 amnesty. Given that it took almost two years for the Italian authorities to process all applications, a significant share of applicants in both 2003 and 2004 were still waiting for a response.

After pooling the observations from the 2003 and 2004 waves, we compare the employment probability of undocumented immigrant applicants who were not yet legalized with that of undocumented immigrants who had not applied. We first consider immigrants who arrived in Italy in 2001 at the latest (i.e., all qualified for amnesty), and we then focus exclusively on those who arrived in 2002. Consistent with the eligibility rules of the 2002 amnesty, the share of applicants among undocumented immigrants who arrived in 2001 and earlier is 75 to 81%, whereas it drops to 47% among those who arrived in year 2002 (see last row of Table 8). Although dissimilarities in outcomes between applicants and non-applicants may result primarily from selection into amnesty application, a statistically significant difference in employment between the two groups could still suggest that the effect of the amnesty may have been persistent.

We run linear regressions of the probability of being employed on a dummy for amnesty application (equal to 1 if the respondent applied, 0 otherwise), on interview year and province of residence dummies, and on individual controls (age, age squared, gender, years since migration and its square, and dummies for education and geographic area of origin). We run separate regressions for immigrants who arrived in Italy in 1997 to 2001, 1999 to 2001, 2001, and 2002.

Estimation results in Table 8 show that one to two years after the amnesty application period, the undocumented amnesty applicants have an employment rate that is 16 to 26 percentage points higher than that of the non-applicant undocumented immigrants. This coefficient is strongly significant and robust to gradual reduction of the sample size. This finding is in line with the size of the effect estimated from the Naga data and suggests that the effect was persistent. Further evidence in this direction is provided by the Italian National Office of Statistics: An estimated 85% of the immigrants legalized under the 2002 amnesty managed to maintain legal employment in Italy and to renew the residence permit two years after legalization (Istat 2008).

Persistence effects are possibly reinforced in the Italian legal framework, as the renewal of the two-year work and residence permit granted by the amnesty was also subject to applicants being still employed (although changes in employers were allowed). This requirement likely generated strong incentives for the immigrants to remain in employment.

Discussion and Concluding Remarks

In this article, we take advantage of a natural experiment provided by a 2002 legalization program in Italy that conditioned eligibility both on a predetermined minimum residence requirement and on being employed at the time of application. Specifically, we exploit the exogenous discontinuity in eligibility based on date of arrival in the country, together with a unique data set, to estimate the causal effect of the prospect of legalization on undocumented immigrants' employment outcomes.

Our empirical findings indicate that the prospect of legal status significantly improves the employment outcomes of immigrants who meet the arrival requirement relative to other undocumented immigrants. In particular, we estimate a statistically significant increase in employment probability of about 26 percentage points, a substantial effect roughly equivalent to half the increase in employment probability that undocumented immigrants normally experience during their first year in Italy. These findings are fully robust to several sensitivity and placebo tests, as well as to the choice of alternative control groups. In addition, using a supplementary set of microdata, we suggest that these effects may persist even for some years after the amnesty.

Overall, we make three main contributions to the literature on the effects of amnesty programs. First, unlike previous studies that have focused exclusively on the effect of *gaining* legal status for recently legalized immigrants, our study is the first to consider the effect of the *prospect of becoming legal* on undocumented workers' employment outcomes. In particular, we show that important changes may take place even *before* legalization actually occurs. Accordingly, our findings suggest that focusing solely on the changes that eligible immigrants experience when they obtain legal status may underestimate the overall increase in employment probability induced by amnesty. Second, we study the labor market effect of a legalization program that conditions eligibility on being employed at the time of application, a type of amnesty design that, although common, has not as yet been studied. Finally, our novel and innovative research design has enabled us to study the effect of amnesty in a quasi-experimental setting using a clean identification strategy and an almost ideal comparison group.

Given the frequent claim that one of amnesty's main objectives is to safeguard the civil rights of undocumented migrants and to prevent their exploitation in the labor market,[26] the assessment of amnesty's economic consequences on undocumented immigrants is crucial from a policy perspective. Our theoretical model suggests that amnesty programs that impose a requirement of employment at the moment of application generate important increases in immigrant labor supply that are likely to reinforce the employment effect. A similar effect can be expected in the context of temporary workers' programs or other migration schemes that condition the issuance and/or renewal of a visa on having an employer willing to support the application. The shift in immigrants' labor supply, however, although perhaps desirable in terms of the amnesty program's efficacy in accelerating their labor market incorporation, may impose considerable costs on the immigrants themselves. Indeed, immigrants with limited bargaining power in the labor market—as is likely for recently arrived undocumented immigrants (Borjas 2016)—may be willing to accept drastic wage reductions to achieve legal status. Unfortunately, this issue is one our data prevent us from empirically addressing.

By granting amnesty, governments may generate an economic surplus, mainly from the positive value that immigrants and prospective employers attach to the prospect of legalization. The distribution of this surplus among the agents involved (i.e., undocumented immigrants, employers, and government) may depend on the type of amnesty program implemented. In particular, we suggest that employment-based legalization initiatives may increase the scope for employers to appropriate the surplus. Hence, whatever the political stance on the best allocation of this surplus, this aspect should always be taken into account when designing regularization programs.

[26]See, for example, *The White House Fact Sheet on New Temporary Worker Program for Undocumented Immigrants*, January 7, 2004; *The White House Fact Sheet on Fixing our Broken Immigration System so Everyone Plays by the Rules*, January 29, 2013; and Council of Europe, *Parliamentary Assembly, Recommendation 1807/2007*.

Appendix

Table A.1. Comparison of Naga and ISMU Samples

		Panel A		*Panel B*	
		2002 (amnesty year)		*Full sample*	
Variable		*Naga*	*ISMU*	*Naga*	*ISMU*
Men	mean	0.551	0.664	0.518	0.655 †
	sd	*0.500*	*0.475*	*0.500*	*0.476*
Age	mean	30.106	30.858	30.822	29.879
	sd	*8.359*	*9.724*	*8.678*	*8.649*
University education	mean	0.085	0.088	0.093	0.114
	sd	*0.280*	*0.285*	*0.291*	*0.318*
Origin					
Europe	mean	0.195	0.301	0.196	0.228
	sd	*0.398*	*0.461*	*0.397*	*0.420*
Asia	mean	0.059	0.062	0.090	0.060
	sd	*0.237*	*0.242*	*0.286*	*0.238*
North Africa	mean	0.263	0.115 †	0.203	0.125 †
	sd	*0.442*	*0.320*	*0.403*	*0.331*
Sub-Saharan Africa	mean	0.093	0.062	0.072	0.123 †
	sd	*0.292*	*0.242*	*0.259*	*0.329*
Latin America	mean	0.390	0.460	0.439	0.463
	sd	*0.490*	*0.501*	*0.497*	*0.499*
No. of observations		118	113	581	447

Notes: The table reports means and standard deviations of selected characteristics for the Naga and ISMU samples. The Naga sample includes all immigrants in our main estimating sample observed, respectively, in year 2002 (panel A) and in the entire period 2000–2004 (panel B). The ISMU sample includes all immigrants who reported to lack legal status and to have at most one year of residence in Italy and who were interviewed in the Milan province in, respectively, year 2002 (panel A) and the period 2001 to 2004 (panel B). "sd," standard deviation. Naga, Italian nongovernmental organization; ISMU, Iniziative e Studi Sulla Multietnicità.
†Denotes a difference between the two samples that is significant at least at 5%.

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