

Employment of Undocumented Immigrants and the Prospect of Legal Status: Evidence from an Amnesty Program

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Abstract:

This paper estimates the causal effect of the prospect of legal status on the employment outcomes of undocumented immigrants. Our identification strategy exploits a natural experiment provided by an Italian amnesty program that introduced an exogenous discontinuity in eligibility based on date of arrival. We find that the prospect of legal status significantly increases the employment probability of immigrants that are potentially eligible for the amnesty relative to other undocumented immigrants. The size of the estimated effect is equivalent to about half the increase in employment that undocumented immigrants in our sample normally experience in their first year after arrival in Italy. These findings are robust to several checks and falsification exercises.

Keywords: Illegal immigration, Natural experiment, Legalization

JEL codes: F22, J61, K37

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Introduction

The substantial presence of undocumented immigrants, which is a common feature in most developed countries, has generated debate in both Europe and America over the types of immigration policies that should be adopted. In the U.S, for example, with an estimated stock of about 11.4 million unauthorized immigrants (U.S. Department of Homeland Security, 2012), the immigration policy reforms most often proposed include a mix of complementary strategies aimed at curbing both future flows of undocumented migrants (e.g., by intensifying controls or increasing sanctions) and existing stocks (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 U.S. 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 U.S. 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 et al., 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 et al., 2012), which in the EU have granted legal status to over 5 million individuals since 1996 (Brick 2011).

In this paper, 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, while 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 help understanding 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 sign. 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.

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 dataset 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, p. 635). This design improves on extant research, which had generally to rely on arbitrary control groups of documented migrants or natives.

¹ The mechanisms we analyze may also be in place with visa sponsorship schemes that condition the issuance and/or renewal of residence permit 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.

Theoretical and empirical background

Previous findings

Several papers 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 et al., 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 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, instead, 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 et al., 2007) although the employment effect remains empirically unclear.⁵ Most of these empirical studies exploit the variation in legal status induced by the Legally

² For the theoretical and empirical debate on alternative migration control policies to deal with undocumented immigration (border controls, domestic enforcement, etc.) see, among others, Ethier (1986), Hanson and Spilimbergo (1999), Hanson (2006), Facchini and Testa (2011) and Bohn et al. (2014).

³ A few other papers examine the impact of legal status on outcomes outside the labor market, such as remittances (Amuedo-Dorantes and Mazzolari, 2010), consumption (Dustmann et al., forthcoming) and crime (Mastrobuoni and Pinotti, 2015), while a related strand of literature addresses the labor market effects of naturalization (Bratsberg et al., 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).

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

Authorized Workers (LAW) program—one of the legalization programs introduced in the U.S. 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 U.S. of about four years. Two other nationality-specific amnesty programs examined in the U.S. context are the 1992 Chinese Student Protection Act (CSPA; Orrenius et al., 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)) exploit the discontinuity in eligibility for legal status created by the cut-off date (January 1, 1982) of the LAW-IRCA program).⁸

Conceptual framework

Our conceptual framework is centered 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

⁶The LPS contains information about a sample of 6,193 undocumented migrants living in the U.S. 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).

⁷The 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 U.S. 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 U.S. since 1990), together with their spouses and children (if continuously in the U.S. since December 1995).

⁸ All these papers face severe data limitations (legal status and year of arrival in the U.S. are, respectively, not observed and only partially observed) that make it hard to isolate the true effects of legalization.

amnesty program design, they should definitely be considered when assessing a program's overall effects.⁹ There is substantial heterogeneity 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 pay-roll taxes/regularization fees. Amnesty can also require undocumented immigrants to be employed at the moment of application, as it has been the case for most amnesty programs launched in Southern European countries (Chauvin et al. 2013; Kraler 2009). 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). In fact, 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 opposite sign with respect to the effect that is generally expected from legalization (i.e. a negative labor supply shift, due to 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.

⁹In the online appendix A1, we throw some light on this as yet unexplored issue using a novel conceptual framework. We develop a simple Nash bargaining model where we capture the prospect of legalization in three complementary ways: a lower apprehension probability for potentially eligible undocumented workers, a positive pay-roll 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 and 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 the online appendix A1.

The amnesty program we study in this paper 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 paper, we define these two groups as “qualified” and “unqualified”, respectively. Conditional on having/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, on the other hand, 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 over 700 thousand 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 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 the immigrants. Importantly, employers were also asked to declare that they had 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 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

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

immigrants in Italy are visa overstayers (up to 70 percent, 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 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 offence, 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 truly arrived before June 11, 2002, would instead 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 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 employment, tax evasion, unauthorized entry and residence). Protection from deportation of the undocumented applicants was also granted during the screening process. It took almost two years for screening process to be completed and approximately 95 percent of applicants eventually received legal status.

¹²The 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 offence, such as fraud or corruption). About 20 per cent of unsuccessful applicants was sentenced to expulsion from the country (Ministry of Interior, *Direzione centrale dell'immigrazione e della polizia di frontiera*, November 11, 2004).

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

[Figure 1 approximately here]

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). Nor, however, can we rule out the possibility 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 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 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

¹³It is worth noting 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 U.S. the 1986 IRCA-SAW program legalized over 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).

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 paper, we use a unique dataset collected by Naga, a large Italian NGO founded in 1987 that offers free basic health care exclusively to undocumented immigrants.¹⁵ Providing a daily average of over 60 health care visits 5 days a week, this association does not discriminate against immigrants in any way according to nationality and/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

¹⁴The length of this minimum residence period could not be inferred from previous amnesties. Indeed, the amnesties in 1998 and in 1990 required seven and two months of minimum residence in Italy, respectively, while 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.

¹⁵Documented 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.

was home to 3.7 million inhabitants in 2002 (6.5 percent of the Italian population), about 150 thousand of them legally resident immigrants (9.7 percent of the foreign population in the country). The province received 87 thousand applications for the 2002 amnesty, which amounts to about 12 percent of total amnesty applications. Data were collected by volunteers 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 dataset of daily observations on undocumented immigrants.¹⁶

This dataset 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 different points in time.

The main shortcoming of the dataset, however, 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 while 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 cut-off arrival day ensures that the selection into Naga should not systematically differ between *qualified* and *unqualified* immigrants.¹⁷ In order to investigate the extent of this selection, in the online appendix Table A 1 we compare the Naga sample with the ISMU sample, a random sample of undocumented immigrants living in Milan (ISMU data are described in footnote

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

¹⁷These 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 paper, is the population of recently arrived undocumented immigrants, whose elusiveness is magnified. Our dataset shares this limitation with any other sample used in the literature on undocumented immigrants (e.g., the LPS dataset is a random sample of the self-selected subpopulation of applicants for the LAW-IRCA amnesty).

25). We find that the two datasets 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 men (Bertakis et al. 2000; Redondo-Sendino et al. 2006).

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 dataset 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 2002.¹⁸ Individuals who arrived outside of these months are 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 dataset 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. However, to increase the sample size, 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

¹⁸To 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.

applicants have gained legal status and disappeared from our sample.¹⁹ We use a two months observation window (14 November - 13 January), which also coincides with the screening period initially envisaged by the amnesty bill.²⁰ Figure 2 summarizes the time structure of our analysis.

[Figure 2 approximately here]

By construction, individuals in the treatment group have spent more time in Italy than 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 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:

$$EMPL_{it} = \alpha AP_{MAY}_i + \beta AP_{MAY}_i \times Y_{2002,t} + X_{it}\gamma + \tau_t + u_{it} \quad (1)$$

where $EMPL_{it}$ is a dummy variable that equals one if individual i who arrived in Italy in year t is employed and zero otherwise. Similarly, AP_{MAY}_i is a dummy variable equal to one for immigrants who arrived in April or May and equal to zero for those who arrived in July or August of every year

¹⁹In 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.

²⁰ 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.

t , which captures any systematic difference in employment probability between the two groups. 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, X_{it} is a vector of individual control variables, and u_{it} is an idiosyncratic shock. 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. Our main coefficient of interest is β , which measures the difference in employment probability between *qualified* and *unqualified* undocumented immigrants. Following on from our discussion, 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. 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, while in the next two panels we differentiate between immigrants arrived in April-May and immigrants arrived in July-August in year 2002 (Panel B) and in the non-amnesty years 2000, 2001, 2003 and 2004 (Panel C). The average age of the sample is almost 31, with 52 percent being male. The education level is high: about 42 percent has attended high school, while about 9 percent has 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 percent. 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-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 percent for immigrants arrived in April-May and immigrants arrived in July-August, but in the amnesty year: in 2002, the employment probability of qualified immigrants is 23 percentage points higher than the one 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.

[Table 1 approximately here]

Figure 3, based on the almost 14 thousand individuals with at most 12 months of residence in Italy who are in the Naga dataset in the 2000–2004 period, illustrates the evolution of these undocumented immigrants' employment probability over their first year of residence in Italy. It is immediately apparent that the employment rate of recently arrived undocumented immigrants changes considerably with time spent in the host country. Only 12 percent 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 percent after four months. The profile then tends to become somewhat flatter, stabilizing around 60 percent for immigrants with a residence duration of 10 months or more. In general, therefore, the employment probability of undocumented immigrants increases 50 percentage points during the first year after arrival in Italy.

[Figure 3 approximately here]

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, there are still two types of potential concerns regarding the observed composition of our sample. First, although one important advantage of our dataset 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.²¹ Specifically, we might fear that employed undocumented immigrants in the unqualified group would falsely report an earlier arrival date in order to appear as eligible for the amnesty. A second concern is that patterns of return migration might have differed among *qualified* and *unqualified* immigrants. In particular, one may worry that the former had more incentives to remain in Italy to enjoy the future reward of legal status than 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 in the Fall of 2002 were systematically more likely to report arriving in Italy before June of the same year than immigrants who went to Naga in the Fall of non-amnesty years. In the online Appendix 2, we empirically test for this implication, finding no evidence in this direction. Our empirical exercise is analogous to the McCrary (2008) test.

²¹ Whether the Italian authorities would judge immigrants' applications as eligible or not 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 the precise reason they go to Naga without fearing arrest.

Estimation Results

Main Results

We start by estimating our main difference-in-differences regression (1). We report results from linear probability models and we account for the heteroskedasticity this choice implies by using robust standard errors.²² Table 2 reports the estimates of the main coefficient of interest in our DiD exercise: the interaction between the dummy for April–May (versus July–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, while 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 paper.

[Table 2 approximately here]

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 percent level.²³ 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 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.

²² In unreported regressions, we have checked the robustness of our findings to using probit or logit regression models. Results are available upon request.

²³ 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.

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 about half the increase in employment they normally experience in their first year after arrival.

Using the difference-in-differences setup of equation (1) we can check whether before the amnesty the employment status differs between the two groups. This is 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 the 10th of September 2002 and that our control group are all those arrived in July and August, we are left only with the first nine days of September and a few observations to perform this empirical exercise. In order 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 statistical significant difference in the employment probability between the two groups because it includes twenty days (September 11-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 using one single month of observations we cannot identify month dummies. 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 employment within the groups of *qualified* or *unqualified* immigrants. Indeed, all *qualified* immigrants should be as intensely affected by the policy, while 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–March with those arrived in April–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 versus those who arrived in May, implying a threshold date of May 1 (see 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 2–4 gradually include additional controls.

[Table 3 approximately here]

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–August group into two subgroups (July versus August). Again, this division is equivalent to setting two alternative placebo 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 also our estimates of the initial differences between the two groups are unaffected.

[Table 4 approximately here]

In our third falsification exercise, based on *placebo amnesty years*, we run DiD regressions in which 2002 is dropped from the sample and 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 of the “initial difference” (Panel B) are generally very close to zero and never statistically significant.

[Table 5 approximately here]

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:

$$EMPL_i = a + bAPMAY_i + X_i c + \varepsilon_i \quad (2)$$

where the employment status of undocumented migrants is regressed on a dummy for arrival in April–May and other individual controls. This specification, unlike our previous DiD estimates, fails to control for the different average permanence in Italy of individuals in the treatment and 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–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–May (rather than in July–August) only in year 2002.

[Table 6 approximately here]

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, p.635). The only aspect where 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 where *qualified* and, separately, *unqualified* immigrants in the Naga sample are compared to five different control groups. These groups are defined as: 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; v) all legal residents (immigrants and natives) of Lombardy. In all cases, the control group is restricted to individuals aged 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 around 8,000 documented and undocumented immigrants in Lombardy (the region in which Milan is situated).²⁴ Data on the whole resident population of Lombardy instead come from the EU Labor Force Survey (EULFS; fourth quarter).

[Table 7 approximately here]

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 non-amnesty year. Results for *unqualified* immigrants (columns 4-6 and 10-12), instead, have generally a negative sign but are substantially smaller in absolute value and 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

²⁴ISMU 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. (forthcoming) for a description of these data.

not longitudinal and does not include legalized immigrants, it cannot be used to address this issue. Instead, we use again the ISMU survey to derive descriptive evidence on the persistence of the employment effect. The 2003 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 are 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 arrived in Italy in 2001 at the latest (i.e., all *qualified* for amnesty) and we then focus exclusively on those arrived in 2002. Consistently with the eligibility rules of the 2002 amnesty, the share of applicants among undocumented immigrants arrived in 2001 and earlier is around 75-81 percent, while it drops to 47 percent among those 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 one if the respondent applied, zero 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 arrived in Italy in 1997-2001, 1999-2001, 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 16–26 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 percent 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).

[Table 8 approximately here]

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 being still employed (although changes in employers were allowed). This likely generated strong incentives for the immigrants to remain in employment.

Discussion and Concluding Remarks

In this paper, 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 dataset, 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 that 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 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 paper 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 just on the changes that eligible immigrants experience when they get 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 prevent their exploitation in the labor market,²⁵ 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

²⁵ 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*.

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 in order 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 different agents involved (i.e., undocumented immigrants, employers, and government) may depend on the type of amnesty program implemented. In particular, our paper suggests 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.

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Figures

Figure 1. Amnesty timeline

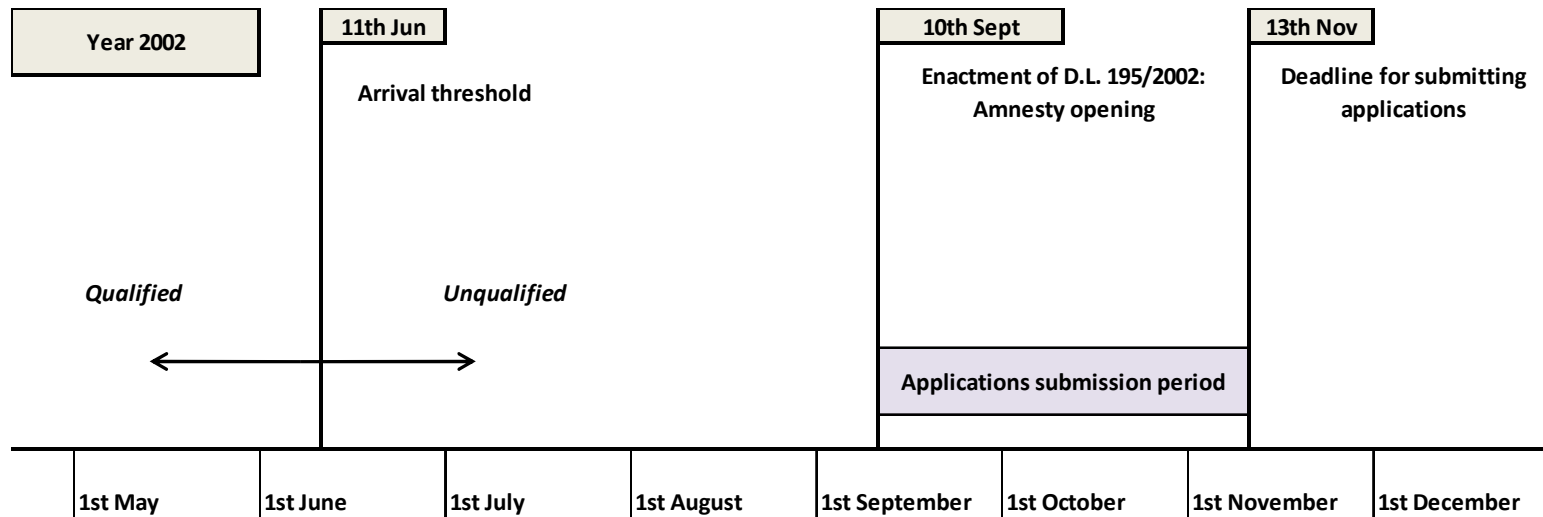


Figure 2. Estimation timeline

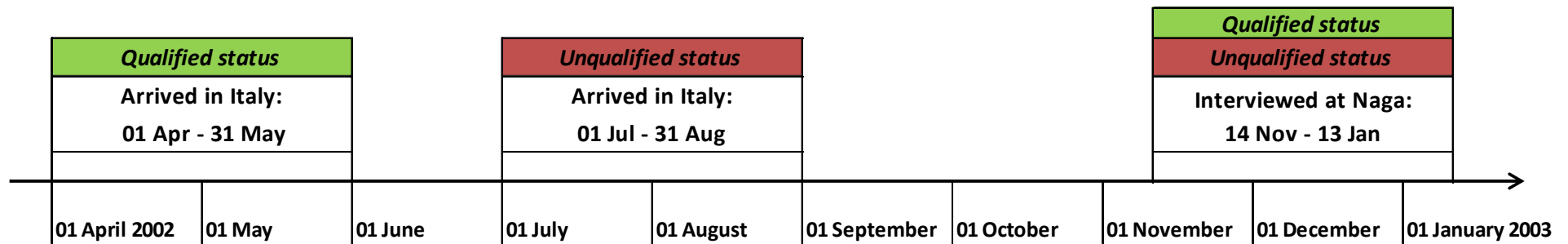
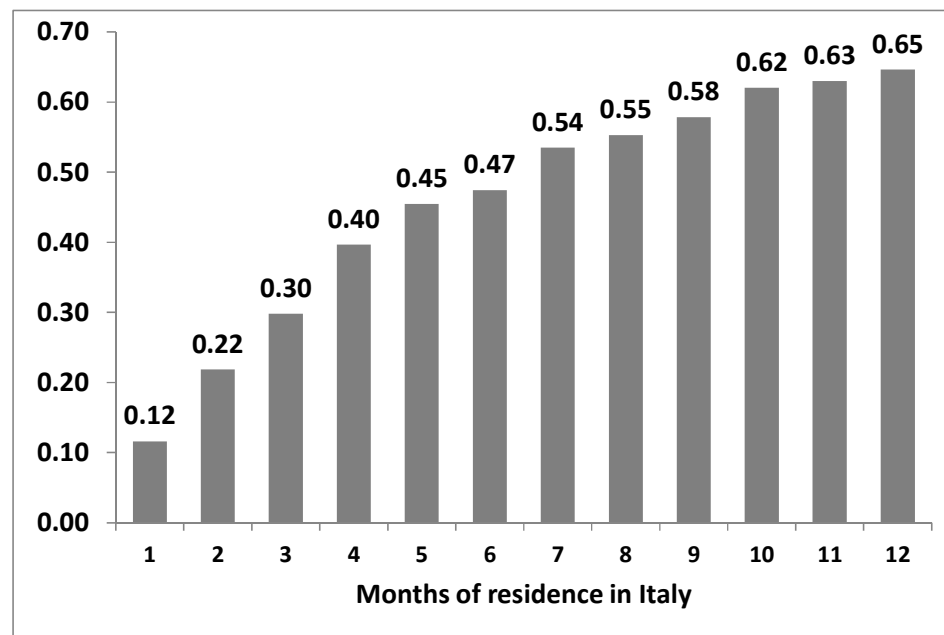


Figure 3. Average employment rate of undocumented immigrants (2000–2004)



Note: The figure is based on individuals in the 2000–2004 Naga dataset with at most 12 months of residence in Italy. Sample size: 13732 observations.

Tables

Table 1. Descriptive statistics

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

Note: Panel A reports means and standard deviations of selected characteristics of our main sample. The next two panels differentiate between immigrants arrived in Italy in April-May (*qualified*) and July-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 was collected on their first visit to Naga between November 14 and January 13 in each year. The sample is composed of 581 individuals, 45 percent of which have “qualified” status. † denotes a difference between the treatment and control group that is significant at least at 5%.

Table 2. DiD estimates: Main results

	1	2	3	4	obs.
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	

Note: Each cell reports the estimated coefficient on the interaction between a dummy for arrival in April–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 2000–2004, and the interaction of the arrival dummy with the 2002 dummy. In panel A, which reports the amnesty effect, immigrants are observed between the 14th of November of each year and the 13th of January 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 2–4 gradually add in additional control variables. Gender, age, and education controls include a male dummy, dummies for 5-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 dummy 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 parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 3. Placebo tests: *Qualified vs. Qualified* and *Unqualified vs. Unqualified*

	1	2	3	4	obs.
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	

Note: The first row reports the estimated coefficient on the interaction between a dummy for arrival in February–March vs. April–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 2000–2004, and the interaction of the arrival dummy with the 2002 dummy. Rows 2–4 have the same structure, but the arrival dummy is modified as described in each row’s heading. Immigrants are observed between the 14th of November of each year and the 13th of January of the following year. Columns 2–4 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 parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 4. DiD robustness checks: Alternative control years

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

Note: In both panels, each cell reports the estimated coefficient on the interaction between a dummy for arrival in April-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 the 14th of November of each year and the 13th of January 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 2–4 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 parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 5. Placebo amnesty years

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

Note. In both panels, each cell reports the estimated coefficient on the interaction between a dummy for arrival in April-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 the 14th of November of each year and the 13th of January 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 2–4 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 parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 6. Year-by-year estimates

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

Note: Each cell reports the estimated coefficient on a dummy for arrival in April–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 the 14th of November of each year and the 13th of January of the following year. Columns 2–4 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 parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 7. Alternative control groups

	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]
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]
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]
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]
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]
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

Note: 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 aged 15 to 40. We further restrict the control groups to unskilled individuals in columns 1-6, whereas we consider all levels of education in columns 7-12. 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 5-year age groups, and dummies for four education levels (primary, secondary, high school, university). Robust standard errors are in parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 8. Persistence of the eligibility effect on undocumented immigrants' employment status

	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*** [0.031]	0.169*** [0.031]	0.227*** [0.042]	0.234*** [0.041]	0.255*** [0.049]	0.261*** [0.048]	0.166*** [0.035]	0.162*** [0.035]
Year and province dummies	yes	yes	yes	yes	yes	yes	yes	yes
Individual controls	no	yes	no	yes	no	yes	no	yes
Observations	1,172	1,172	793	793	457	457	615	615
Share of applicants	0.79		0.81		0.75		0.47	

Note: 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 one if the respondent applied for the 2002 amnesty (and zero 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 1997-2001 (cols. 1-2), 1999-2001 (cols. 3-4), 2001 (cols. 5-6), or 2002 (cols. 7-8). Robust standard errors are in parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Online Appendix

Appendix 1 - The Labor Market Effects of the Prospect of Legal Status: A Theoretical Framework

This appendix outlines a stylized model to elucidate the labor market effects of immigration amnesty on potentially eligible undocumented migrants. We first sketch a Nash-bargaining model of the labor market and then study how different amnesty designs affect immigrants' outcomes.

The Labor Market

Consider the problem of firm f , which must decide whether to employ an undocumented immigrant. The marginal productivity of the immigrant is constant ($A > 0$) and with probability $p \geq 0$ s/he will be apprehended by the police, the match expires, and the firm incur a sanction $c^f (\geq 0)$ for having unlawfully employed the undocumented worker. The firm finds it profitable to employ the undocumented immigrant as long as the expected gain exceeds the wage. The solution to the firm's problem thus defines labor demand in terms of the maximum wage $w^f(p)$ that the firm is willing to pay to employ an undocumented worker for any given level of p :

$$w^f(p) = (1-p) \cdot A - p \cdot c^f \quad (\text{A1})$$

Here, $w^f(p)$ is linearly decreasing in p , and for $p=0$, the salary equals the worker's marginal productivity ($w^f = A$).

We next consider the choice of an undocumented immigrant m who must decide whether to accept or reject a job offer. This worker will accept the offer if the wage is larger than the opportunity cost of not working $b(\geq 0)$, where both terms are weighted by one minus the probability of apprehension. If found out, s/he will incur a penalty $c^m(\geq 0)$, which can be interpreted in terms of detention time and/or the economic and psychological cost of deportation. The undocumented immigrant finds it profitable to accept the job offer if the expected gain from working is larger than

or equal to the expected gain from not working; i.e., $(1-p) \cdot w - p \cdot c^m \geq (1-p) \cdot b - p \cdot c^m$.²⁶ This condition defines a flat labor supply:

$$w^m(p) = b \quad (\text{A2})$$

Where $w^m(p)$ denotes the immigrant's reservation wage. If the marginal productivity of the match is higher than the individual's utility of not working (i.e. if $A > b$), equations (A1) and (A2) identify an apprehension probability \bar{p} such that $w^f(\bar{p}) = w^m(\bar{p})$.

Define the job match surplus $S(p) = w^f(p) - w^m(p)$. When the apprehension probability is sufficiently low (i.e., $p \leq \bar{p}$), the surplus is positive, $S(p) \geq 0$, but when $p > \bar{p}$, it is $S(p) < 0$, so there is no possibility of a mutually profitable match between the firm and worker. We therefore focus on cases in which $p \leq \bar{p}$.

To close the model, we assume that the firm and the worker negotiate the wage according to standard Nash bargaining:

$$w(p) = \arg \max \left\{ [S^f(p)]^\beta [S^m(p)]^{1-\beta} \right\} \quad (\text{A3})$$

where $w(p)$ is the equilibrium wage of a successful match; $S^f(p) = w^f(p) - w(p)$ and $S^m(p) = w(p) - w^m(p)$ are the surpluses of the match for the firm and worker, respectively, and $\beta \in (0,1)$ and $(1-\beta)$ their respective bargaining power. Problem (A3) yields to the equilibrium wage $w(p) = w^f(p) - \beta S(p)$, and the total surplus of the match is shared proportionally based on the bargaining strength of the firm and worker: $S^f(p) = \beta S(p)$ and $S^m(p) = (1-\beta) S(p)$.

Amnesty

This model can be used to illustrate the labor market effects of amnesty eligibility. We capture the prospect of legalization in three complementary ways: First, the probability of apprehension is

²⁶ Here, for analytical convenience, the probability of apprehension is independent on the employment status. The apprehension probability of workers is likely to vary across occupations (e.g. relatively low for domestic occupations and much higher in construction and services) and we have no a priori on the ranking between workers, in general, and unemployed immigrants. Below, we introduce a twist in the apprehension probability for employed and unemployed individuals in order to capture one important feature of work-related regularization programs.

lower for *eligible* than for *ineligible* immigrants ($0 \leq p^e < p^i < 1$). This condition is appropriate if the time span between eligibility and application is not too long. Second, immigrants attach a positive value (B) to the prospect of legal status, because they anticipate all the advantages of residing lawfully in the host country (e.g., access to the financial and legal systems, travel home, and so forth). Third, we introduce a positive cost T of amnesty application, which is borne by the firm and comprises payroll taxes and fines. Whether T is formally levied on the firm or the worker is immaterial for the results. Here, we apply these constructs to assess the effects of two different amnesty designs.

Predetermined conditions only

Consider first an amnesty program that conditions eligibility on some *predetermined individual conditions* (residence, employment, or both). Those individuals that satisfy (do not satisfy) the predetermined condition are *eligible* (*ineligible*) for amnesty. We denote these two groups with the superscripts $m=(e, i)$. It is then easy to verify that, because under this amnesty design both the potential reward B and the probability of apprehension are independent of being employed or not, labor supply (A2) remains unchanged for both *eligible* and *ineligible* immigrants. The prospect of legalization will, however, shift the labor demand for *eligible* immigrants, which now becomes

$$w^{f,e}(p^e) = (1 - p^e) \cdot (A - T) - p^e \cdot c^f \quad (\text{A4})$$

Eligibility for legal status thus has an ambiguous labor demand effect: a lower probability of apprehension $p^e < p^i$ drives $w^{f,e}(p^e)$ up, while the application fee T shifts the $w^{f,e}(p^e)$ curve downward. If the former effect dominates, the value of a match with an eligible undocumented immigrant increases, implying

$$S^e(p^e) > S^i(p^i). \quad (\text{A5})$$

Hence, the maximum wage that the firm is willing to pay for an eligible worker is higher than that for an ineligible worker: ($w^{f,e}(p^e) > w^{f,i}(p^i)$).

Predetermined conditions and current employment requirement

Consider next an amnesty program that entails both a *predetermined condition* and a current *employment requirement*. This design inherently divides undocumented immigrants into one group that satisfies the first requirement and another that does not. Following the terminology adopted in the main text, we define these two groups of immigrants as “qualified” and “unqualified,” respectively. Conditional on being employed, the former group becomes fully eligible for legal status. Hence, we must now distinguish four different groups of immigrants, $m = (e, i, q, u)$, with e and i still denoting eligible and ineligible immigrants but q and u denoting the group of *qualified* and *unqualified* immigrants, respectively.

In terms of our modeling assumptions, this amnesty design has two main consequences. First, *employed qualified* immigrants become fully *eligible* and thus face an apprehension probability $p^q|_{employed} = p^e < p^i$. If they do not become employed, however (i.e., if they fail to become fully eligible for amnesty), their probability of being detected is equal to that of *unqualified* immigrants, and both are simply equal to the probability of apprehension of an ineligible immigrant: $p^q|_{unemployed} = p^u = p^i$. The above observation allows us to simplify the notation by using $m=e$ ($m=i$) to denote employed (unemployed) *qualified* immigrants. Second, the reward B is now conditional on being employed.

It now follows that both labor demand and supply may be affected by the amnesty, although only for *qualified* immigrants. In particular, labor demand for *qualified* immigrants is still described by equation (A4) since this group of immigrants becomes eligible if employed and faces an apprehension probability p^e . Hence, the labor supply of *qualified* immigrants is now determined by the following problem:

$$(1 - p^e) \cdot (w + B) - p^e \cdot c^m \geq (1 - p^i) \cdot b - p^i \cdot c^m \quad (\text{A6})$$

and their reservation wage becomes

$$w^e(p^e) = \frac{p^e \cdot c^m + \tilde{b}}{(1 - p^e)} - B \quad (\text{A7})$$

where $\tilde{b} = (1 - p^i) \cdot b - p^i \cdot c^m$. The reservation wage $w^e(p^e)$ is increasing in the probability of being detected, with $w^e(0) = \tilde{b} - B$ and $\lim_{p^e \rightarrow 1} w^e(p^e) = \infty$. Comparing (A7) with (A2) then shows that the prospect of legalization for *qualified* immigrants unambiguously reduces their reservation wage as a consequence of both the lower risk of apprehension and the reward B associated with employment. Given that unqualified/ineligible immigrants do not change their labor supply, then $w^i(p^i) > w^e(p^e)$. It should also be noted that when B is high enough, a negative reservation wage for eligible immigrants cannot be ruled out. Moreover, if the bargaining power of undocumented workers is low ($\beta \approx 1$), the equilibrium wage $w(p)$ is close to the reservation wage $w^m(p)$ for both groups, $m = (e, i)$, and the regularization program unambiguously reduces the wage of *qualified* immigrants. This downward pressure on wages is absent in amnesty programs that condition eligibility on predetermined individual characteristics only.

Figure A 2 graphically illustrates the labor market effects of an amnesty program that conditions eligibility on current employment and some predetermined condition. The dotted lines $w^{f,i}(p)$ and $w^i(p)$ represent the labor demand and labor supply, respectively, of *unqualified*, and hence ineligible, undocumented immigrants. The intersection of the two curves identifies a region of the apprehension probability in which a profitable match is possible $p \in [0, \bar{p}]$. p^i denotes the probability of apprehension for *unqualified/ineligible* immigrants, and $S^i(p^i)$ is the total surplus, which is split between employer and employee according to parameter β . On the demand side, the prospect of legalization does two things: (a) shifts the labor demand curve downward to $w^{f,e}(p)$ (so the intercept is now $A-T$) and (b) reduces the apprehension probability to $p^e < p^i$ for *qualified* immigrants only. At p^e the demand for *qualified* workers becomes D' . If the labor supply were to remain unchanged, the associated surplus $S^e(p^e)$ would be either greater or lower than the initial

surplus $S^i(p^i)$ depending on model parameterization. The prospect of legalization, however, completely changes the supply of *qualified* immigrants, which becomes $w^e(p)$. For $p = p^i$, $w^e(p^i) = b - B$. To the left of $p = p^i$, the reservation wage is monotonically decreasing in p . $S^e(p^e)$ is the total surplus of a successful job match with a qualified immigrant. In this specific graphical representation, $S^e(p^e) > S^i(p^i)$.

In general, the net change in surplus of potential matches remains ambiguous because of the indeterminacy of the shift in labor demand. It is readily apparent, however, that if the value the immigrant attaches to the prospect of legalization is high enough (if $B \geq T$), then condition (A5) holds (i.e., the value of a match with an eligible immigrant is larger than one with an ineligible immigrant).

Concluding remarks

As explained in the main text, whenever (A5) is satisfied, the firm will increase both the retention rate of already employed *qualified/eligible* workers and the hiring rate of unemployed *qualified* workers (who then become fully eligible for amnesty). Those who do not satisfy the predetermined condition (the *unqualified* ones), in contrast, are simply left out of the legalization process and experience no change in surplus. It thus follows that, *ceteris paribus*, the employment rate will be higher among *qualified* than among *unqualified* undocumented migrants:

$$\frac{\text{employment rate}^q}{\text{employment rate}^u} > 1 \quad (\text{A8})$$

The opposite would hold if the net effect of the shifts in labor demand and labor supply led to a larger job surplus for ineligible than for eligible immigrants.

Although this model could be enriched in many directions—for instance, by introducing additional channels that might shape the predicted effect of an amnesty on the labor market outcomes of undocumented immigrants—its main conclusion that important changes in the labor

market may take place even *before* the actual legalization occurs would still hold. The direction of these effects on the relative employment of qualified versus unqualified immigrants, however, remains theoretically ambiguous and needs to be addressed empirically.

Appendix 2 - Possible misreporting of arrival date and selective return migration

As discussed in the main text, although misreporting of the arrival date and selective return migration are unlikely to drive our results – due to the nature of the dataset and the very short time window used in the analysis – we can empirically test for any evidence of the two sources of bias. Our test is based on the fact that, in the presence of selective outmigration and/or misreporting, we should observe that immigrants who went to Naga in the Fall of 2002 were systematically more likely to report arriving in Italy before June of the same year than immigrants who went to Naga in the Fall of non-amnesty years. Thus, we should observe a change in the distribution of the arrival date around the 2002 threshold date relative to non-amnesty years.

To test this possibility, we perform the following empirical check. We use the *APMAY* dummy as the dependent variable. In each year, this dummy is equal to one if the individual reported having arrived in April or May and zero if s/he arrived in July or August (of the same year). As in the remainder of our analysis, individuals who arrived in other months are dropped from the estimation sample. Pooling the observations for years 2000–2004 (which is exactly our regression sample), we run linear probability models of the probability of having arrived in April-May over a constant and dummies for non-amnesty years (2000, 2001 2003, and 2004; 2002 is omitted as the benchmark year). The constant term measures the share of individuals who arrived in Italy in April and May 2002, while the year dummies measure the percentage point differences in this share between 2002 and each of the four non-amnesty years.

The results are reported in Table A2. Column 1 reports the unconditional estimates, while the following three columns gradually add in groups of controls (gender, age and education, and

dummies for area of origin, and month). Looking specifically at the unconditional estimates in column 1 of Table A2, the estimated coefficient on the constant term indicates that the immigrants who arrived in Italy in 2002 are almost evenly distributed between the two arrival groups. No systematic differences in this share are observed in any of the remaining four years: the estimated coefficients on the year dummies for 2000, 2001, 2003, and 2004 are very small and not significantly different from zero. The inclusion of further controls in columns 2–4 does not alter this conclusion. These results provide truly reassuring evidence against the existence of selective outmigration or systematic arrival date misreporting in our data. The estimation results using probit or logit regressions (available from the authors upon request) are very similar.

Appendix figures

Figure A 1. Placebo tests: *Qualified* vs. *Qualified*

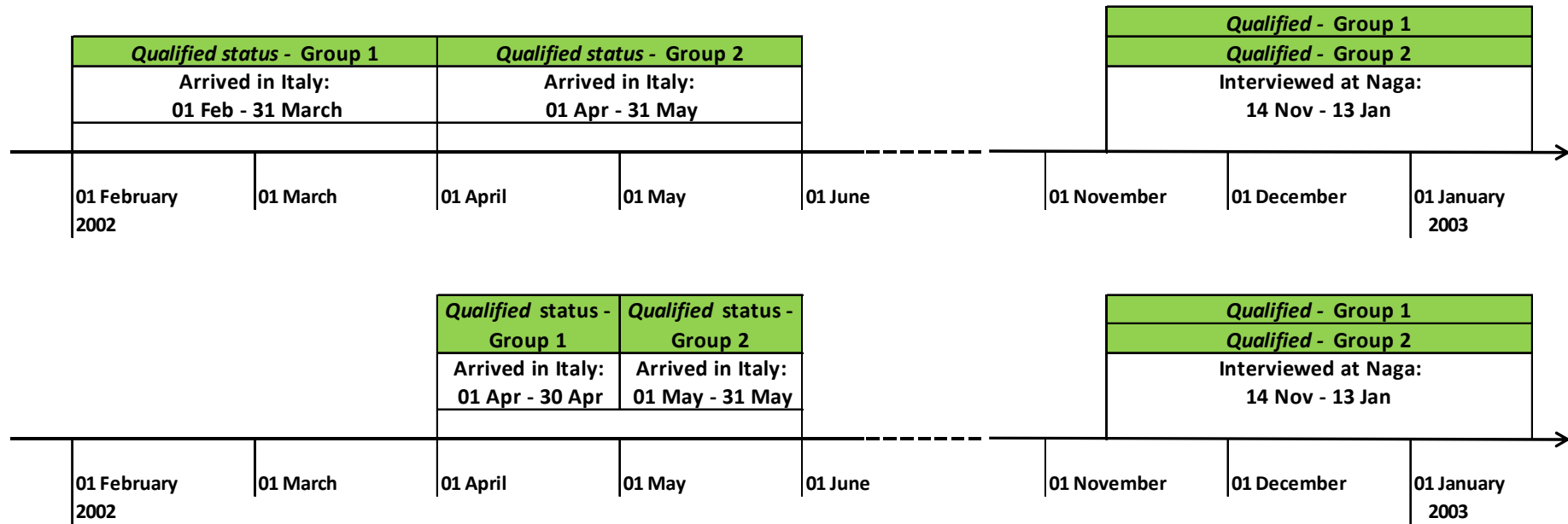
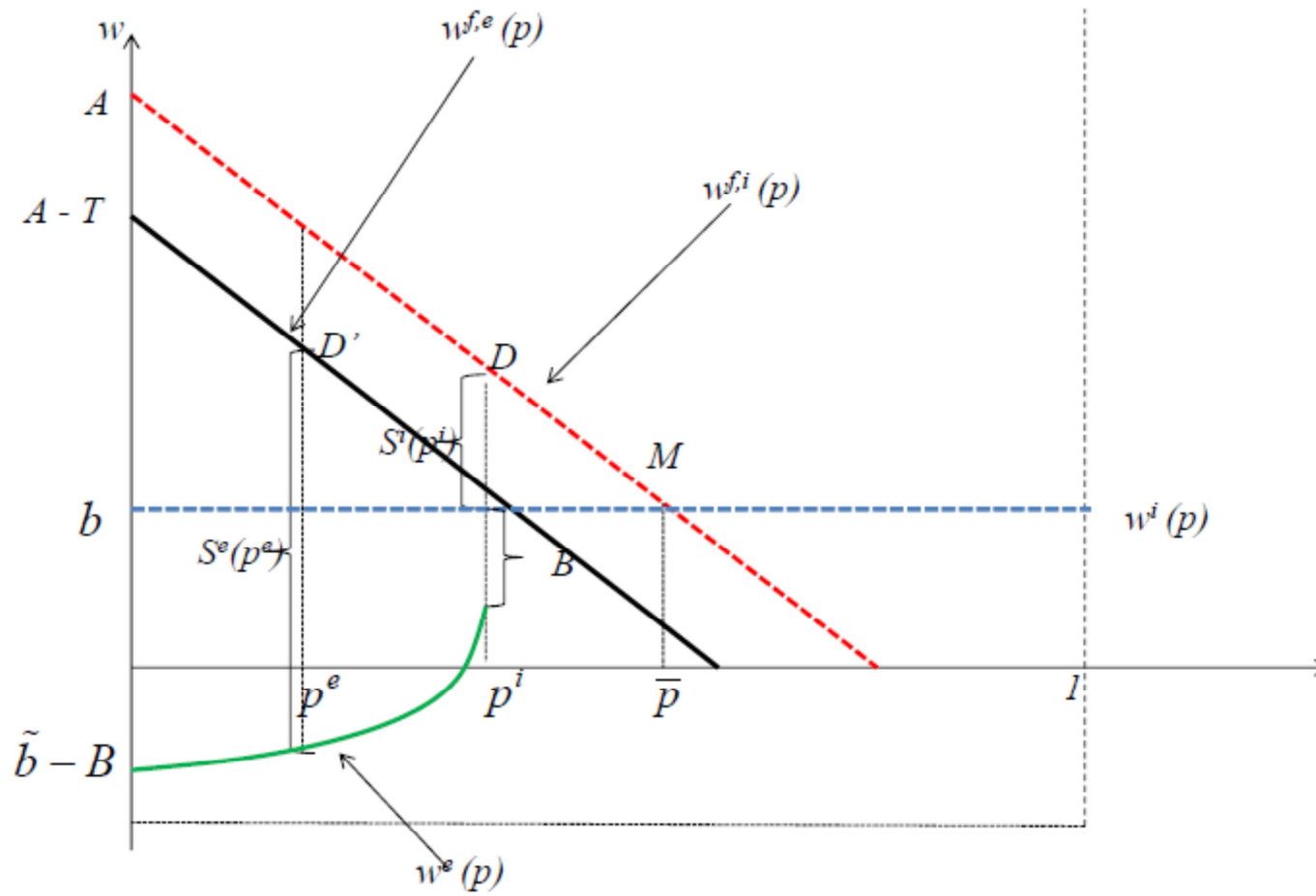


Figure A 2. Theoretical framework



Appendix tables

Table A 1. Comparison of NAGA and ISMU samples

		Panel A 2002 (<i>amnesty year</i>)		Panel B Full sample			
		<i>NAGA</i>	<i>ISMU</i>	<i>NAGA</i>	<i>ISMU</i>		
Men	mean	0.551	0.664	0.518	0.655	†	
	<i>sd</i>	<i>0.500</i>	<i>0.475</i>	<i>0.500</i>	<i>0.476</i>		
Age	mean	30.106	30.858	30.822	29.879		
	<i>sd</i>	<i>8.359</i>	<i>9.724</i>	<i>8.678</i>	<i>8.649</i>		
University education	mean	<i>0.085</i>	<i>0.088</i>	<i>0.093</i>	<i>0.114</i>		
	<i>sd</i>	0.280	0.285	0.291	0.318		
Origin	<i>Europe</i>	mean	0.195	0.301	0.196	0.228	
		<i>sd</i>	<i>0.398</i>	<i>0.461</i>	<i>0.397</i>	<i>0.420</i>	
	<i>Asia</i>	mean	0.059	0.062	0.090	0.060	
		<i>sd</i>	<i>0.237</i>	<i>0.242</i>	<i>0.286</i>	<i>0.238</i>	
	<i>North Africa</i>	mean	0.263	0.115	† 0.203	0.125	†
		<i>sd</i>	<i>0.442</i>	<i>0.320</i>	<i>0.403</i>	<i>0.331</i>	
	<i>Sub-Saharan Africa</i>	mean	0.093	0.062	0.072	0.123	†
		<i>sd</i>	<i>0.292</i>	<i>0.242</i>	<i>0.259</i>	<i>0.329</i>	
	<i>Latin America</i>	mean	0.390	0.460	0.439	0.463	
		<i>sd</i>	<i>0.490</i>	<i>0.501</i>	<i>0.497</i>	<i>0.499</i>	

Note: 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 include 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 2001-2004 (panel B). † denotes a difference between the two samples that is significant at least at 5%.

Table A 2. Probability of having arrived in Italy in April-May (versus July-August)

	1	2	3	4
Arrival year 2000	-0.053 [0.062]	-0.075 [0.062]	-0.093 [0.060]	-0.090 [0.060]
Arrival year 2001	-0.007 [0.063]	-0.013 [0.065]	-0.03 [0.065]	-0.026 [0.065]
Arrival year 2003	0.002 [0.071]	0.005 [0.072]	0.000 [0.071]	-0.001 [0.071]
Arrival year 2004	-0.09 [0.069]	-0.092 [0.069]	-0.065 [0.068]	-0.057 [0.068]
Constant	0.475*** [0.046]	0.328 [0.206]	0.324 [0.215]	0.342 [0.219]
Observations	581	581	581	581
Gender, age, education	no	yes	yes	yes
Area of origin	no	no	yes	yes
Month dummies	no	no	no	yes

Note: The table reports results from linear regressions of a dummy for arrival in April-May (versus July or August) on a constant and year dummies (excluding 2002). Columns 2–4 gradually add in additional control variables (controls are identical to those described in the note to Table 2). Robust standard errors are in parentheses; ***p<0.01, **p<0.05, and *p<0.1.