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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 the 2002 amnesty program in Italy 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 two thirds of 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 falsification exercises.

Keywords: Illegal immigration, Natural experiment, Legalization

JEL codes: F22, J61, K37

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1. 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 11.5 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,[1] opponents argue that amnesty unfairly rewards law-breaking behavior and reveals 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.

Nations looking to reduce the number of undocumented residents have often resorted in recent years to legalization programs (Casarico et al., 2012). 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, 2014), others assess amnesty programs' general effect on their target population of undocumented

[1] *The White House Fact Sheet on Fixing our Broken Immigration System so Everyone Plays by the Rules*, January 29, 2013.

[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).

immigrants with a particular focus on labor market outcomes.[3] Most of these empirical studies exploit the variation in legal status induced by the Legally Authorized Workers (LAW) program—one of the legalization programs introduced in the 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.[4] 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.[5]

Our paper is related to this literature on the effect of gaining legal status for successful amnesty applicants but departs from it in three major ways: First, we argue that important changes in immigrant outcomes and behavior should be expected even before legal status is actually granted. Indeed amnesty programs impose some eligibility conditions, which immediately differentiate potential applicants from ineligible undocumented immigrants. We show that eligibility status per se has significant labor market consequences. For the first time, we 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

[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. 2014) and crime (Mastrobuoni and Pinotti, 2011), while a related strand of literature addresses the labor market effects of naturalization (Bratsberg et al., 2002; Mazzolari, 2009).

[4] The 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;).

[5] The CSPA, designed to prevent political persecution of Chinese students in the aftermath of the Tiananmen protests of 1989, granted permanent residency to all Chinese nationals who arrived in the 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).

outcomes, have so far been overlooked. In particular, an accurate assessment of these programs' overall impact 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.

Second, we take into account that the effects of amnesty depend greatly on the specific program design; that is, there is substantial heterogeneity in the eligibility requirements that amnesty programs set for legal status. For instance, as the LAW–IRCA, CSPA, and NACARA programs, amnesty often requires a minimum residence condition, aimed at preventing new inflows of undocumented immigrants (the "recall effect"). Eligibility can also be linked to a predetermined employment requirement. For instance, the IRCA provided for a second legalization program, the Special Agricultural Worker (SAW), which conditioned eligibility on having been employed in the agricultural sector for a certain minimum time. Although almost ignored in the literature on the IRCA amnesty,[6] the SAW–IRCA program was similar in magnitude to the LAW–IRCA program, legalizing over 1.2 million unauthorized immigrants. Amnesty can also require undocumented immigrants to be employed at the moment of application, as has been the case for most amnesty programs launched in Spain (1985, 1991, 2001, and 2004) and Italy (2002 and 2006; Casarico et al. 2012). Assessing the labor market impact of different types of regularization programs is thus crucial for designing future policies. Yet, as the 2007 Parliamentary Assembly of the Council of Europe acknowledged, despite the range of different types of regularization programs tried in European countries since the 1980s, "much more research on the impact of these programs is needed" (Resolution 1568–2007). Hence, we not only set up a theoretical framework that enables us to discuss the effects of different amnesty designs, but, unlike the prior research focus on the impact of amnesty programs with predetermined requirements (like the IRCA), we study an amnesty that imposes a current employment requirement on potential applicants.

[6]The Legalized Population Survey does not include illegal immigrants legalized under the SAW-IRCA and, to the best of our knowledge, its labor market effects for former undocumented immigrants have not so far been analyzed.

Third, 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, which conditioned eligibility on both a predetermined minimum residence requirement and being employed at the time of application. This retrospective and unpredictable threshold based on date of arrival in Italy generates a local randomized experiment (Lee and Lemieux, 2010) that 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 to rely on arbitrary control groups of documented migrants or natives.[7]

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 30 percentage points, a substantial effect roughly equivalent to two thirds of 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. In addition, using a supplementary set of microdata, we derive descriptive evidence for the persistence of these effects following amnesty.

The structure of the paper is as follows. Section 2 briefly describes the mechanisms linking the prospect of legal status to undocumented immigrants' employment outcomes. Section 3 discusses

[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). Barcellos (2010) implements a research design similar to ours in analysing the impact of the LAW-IRCA legalization program on the economic status of legalized immigrants. She exploits a discontinuity in eligibility for legal status based on date of arrival (the cut-off date for the LAW-IRCA program was the 1st of January, 1982) but she faces severe data limitations (legal status and year of arrival in the U.S. are, respectively, not observed and only partially observed) that, admittedly, make it hard to identify the true effects of legalization.

the 2002 Italian amnesty and related identification issues. Section 4 introduces the data and our estimation strategy, after which section 5 presents our descriptive statistics. Section 6 then reports the results of our main estimations, robustness checks, and placebo tests. Section 7 summarizes our conclusions and suggests relevant policy implications.

2. 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*. 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,[8] while the effect on employment is theoretically undetermined. 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, 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.[9] Remarkably, the literature to date completely ignores the possibility that the mere announcement of amnesty could generate changes in undocumented immigrants' labor market outcomes *before actual legalization* takes place. Because these potential effects may depend on

[8]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).

[9]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.

amnesty program design, they should definitely be considered when assessing a program's overall effects.

We throw light on this as yet unexplored issue using a novel conceptual framework; namely, a simple Nash bargaining model that captures 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 theoretical framework implies that the possibility of future legal status modifies the job match surplus—and thus the relative employment rate—for undocumented immigrants who can be legalized compared to those who cannot. The subsequent discussion highlights the major insights provided by the formal model, which is fully explained in Appendix 1.

Any amnesty program that bases eligibility on some *predetermined individual condition* (residence, employment, or both) 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. 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. 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.

An amnesty program that entails both a predetermined condition and a current employment requirement (i.e., the type studied here) automatically divides undocumented immigrants into one group that satisfies the first requirement and another that does not. Throughout the paper, we define these two groups as, respectively, "qualified" and "unqualified". Conditional on having/finding a job, only the former 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 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.[10]

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.

3. A Natural Experiment

3.1. 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. 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 whose employers were willing

[10]In the appendix, we identify the conditions under which the prospect of legal status unambiguously increases the job match surplus.

to: (a) declare that they had continuously employed the immigrant for the three months before the legalization law was passed (i.e., since June 11, 2002), (b) legally hire the immigrant under a minimum one year contract at a minimum monthly salary (439 euros), and (c) pay an amnesty fee (330 euros for domestic workers and 800 euros for all other workers). Hence, unlike all previous amnesties granted in Italy, the applications had to be filed directly by the employers rather than the immigrants during a two-month period beginning the day of amnesty approval (i.e., September 10–November 13). 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: Therefore the amnesty simultaneously legalized both the residence status and the employment contract of successful applicants.[11]

Interestingly, the predetermined employment requirement created an additional *implicit* predetermined eligibility condition, the date of arrival in Italy, the only criterion that *de facto* mattered in the legalization process. That is, because the application procedure did not require employers to prove the duration of the immigrants' past employment, relying merely on self-declarations implicitly endorsed by amnesty fee payment, the predetermined employment requirement was virtually immeasurable and unverifiable. On the other hand, one necessary condition for fulfilling the employment requirement was that the immigrant had arrived in Italy before June 11, 2002. This condition was verifiable. The amnesty application form required stating the exact date of arrival in Italy and attaching copies of all passport pages to the application form. It is worth noting that the vast majority of undocumented immigrants in Italy are visa overstayers (up to 70 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

[11]Amnesty also implied that the Italian state could not prosecute either the employers or the employees for all past law infringements reported in the application (e.g., undeclared employment, tax evasion, unauthorized entry and residence).

applications being checked, immigrants arriving *before* the threshold date were more able to provide documentation supporting their eligibility (e.g. money transfer receipts, medical records, mobile phone contracts).

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 section 2). 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.[12] Attempting to legalize an immigrant who arrived in Italy *after* that date, on the other hand, would involve a substantially high risk of being charged with making a false statement.[13]

3.2. 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

[12]It 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. Immigration and Naturalization Service concluded that it was nearly impossible to distinguish a legitimate from a fraudulent SAW application (see Gonzalez Baker, 1990).

[13]The submission of false statements or documents to the Italian authorities in the application for amnesty was punishable with up to nine months of detention (and possibly more, if the false declarations were recognized as a more serious offence, such as fraud or corruption).

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

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

[14]The length of this minimum residence period could not be inferred from previous amnesties. Indeed, the amnesties in 1998 and in 1990 required seven 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.

4. Data and Estimation

In this paper, we use a unique dataset collected by Naga, a large Italian NGO founded in 1987 that offers free 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 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).[16] 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, profession in the home country, current employment status). These data, available in electronic format since 2000, constitute a cross-sectional dataset of daily observations on undocumented immigrants.[17]

This dataset offers three 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 any point in time (i.e., during the amnesty, immediately after the amnesty). Third, although immigrants had strong incentives to make false statements about arrival

[15] Documented immigrants are completely integrated into the Italian National Health Service, so if they seek medical assistance at Naga, the staff redirect them to public hospitals.
[16] Source: ISTAT Demo-Geodemo.
[17] An earlier version of this dataset was used in Devillanova (2008), to which we refer for an accurate description of the data and individual variables.

dates on the amnesty application, there was no clear motivation to misreport information when interviewed at Naga.[18]

The main shortcoming of the dataset is that it includes only individuals who visited the Naga premises for medical care. Nevertheless, 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. Moreover, given that individuals with lower health and socioeconomic status are probably overrepresented in the sample (see Devillanova, 2008) and possibly less reactive in the labor market (relative to the overall population of undocumented immigrants), amnesty is – if anything – less likely to have an effect on their employment outcomes.[19]

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 gives precisely 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

[18]In Appendix 2, we discuss the issue of potential misreporting in the information collected at Naga. In particular, we empirically test for manipulation of the reported date of arrival in Italy, finding no evidence in this direction. Our empirical exercise is analogous to the McCrary (2008) test.

[19]These data limitations should be assessed bearing in mind the intrinsic difficulties of researching undocumented migration: given that one ignores both the size and characteristics of such a population, extracting a truly representative sample is simply not possible. Such is even more the case when the object of analysis, as in our 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).

July and August 2002.[20] 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 13, 2002) when all applications had been submitted but no one had yet been legalized. However, to increase the sample size, we extend our observation window for up to three months after the amnesty deadline. It should also be noted that we face a trade-off between having a larger sample size and introducing an amnesty-induced sample selection: the further away from the amnesty deadline, the more likely that amnesty applicants have gained legal status and disappeared from our sample.[21] We thus use three observation windows of decreasing lengths: three months (November 14, 2002–February 13, 2003), two months (November 14, 2002–January 13, 2003) and one month (November 14, 2002–December 13, 2002) after the amnesty deadline. 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

[20]To check the robustness of our results, we further restrict the neighborhood around the eligibility threshold by comparing those who arrived in May 2002 with those who arrived in July 2002. The results are qualitatively similar, although the sample size shrinks.
[21]In fact, not only those actually legalized but also those who had applied for amnesty but were still waiting were entitled to receive free medical care from the National Health Service and so were no longer admitted to Naga. This process, however, involved some administrative delay and some learning on all sides—migrants, public hospitals, and Naga volunteers—so in the weeks immediately after the amnesty deadline, applicants in need of medical assistance still had to turn to Naga. As time passed, however, applicants tended to disappear from the sample.

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 February 13 $t+1$ who had arrived in Italy in April, May, July, or August of the same year t.

We then estimate the following linear probability model:

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

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, $APMAY_i$ 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 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. Thus, our main coefficient of interest is β, which measures the difference in employment probability between *qualified* and *unqualified* undocumented immigrants. Following on from our section 2 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.

5. Descriptive Statistics

Panel A in Table 1 reports the major descriptive statistics for our sample in the amnesty year 2002, by *qualified* and *unqualified* status, which also serves as a test of treatment status randomness. As the table shows, the average age of the sample is around 31, with 50 percent being male. The average education is high: about 50 percent has attended high school, while about 6 percent has some university education. The differences between the *qualified* and the *unqualified* group in these variables are never statistically significant at 5 percent. For area of origin, the composition is slightly different: Latin America and East Europe are the largest single origin area for the first and second groups, respectively. It is worth stressing, however, that a similar distribution pattern for area of origin is also evident for our sample in the non-amnesty years (panel B), which suggests a seasonality in undocumented flows from different source countries that is completely unrelated to the 2002 amnesty. In our empirical analysis, therefore, we report both conditional and unconditional estimates.

[Table 1 approximately here]

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. 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]

6. Estimation Results

6.1. 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 four columns gradually add further groups of control variables (gender, age, and education; area of origin dummies; month dummies; profession in the home country dummies). We maintain this structure throughout the rest of the paper.

[Table 2 approximately here]

In panel A of Table 2, which refers to the post amnesty period, we use three observation windows for undocumented immigrants' employment status: three, two, and one month(s) after the amnesty deadline. As pointed out in section 4, we face a trade-off between having a larger sample size and introducing an amnesty-induced sample selection. In particular, because legalized immigrants progressively leave the sample, we are left with an increasingly negatively selected

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

group of *qualified* immigrants who arrived before June 2002 but had no job entitling them to amnesty. This selection mechanism is not in place for *unqualified* immigrants, none of whom could be legalized. Clearly, this selection would bias our results against finding a difference in employment between the two groups. We thus expect the estimated coefficient on the *qualified* status to decrease as the observation window grows longer.

In fact, the estimated coefficient on the DiD interaction is positive, strongly significant, and remarkably stable across different specifications, and as expected, its size increases with restriction of the observation period (from row 1 to row 3). If we focus on the fully specified model looking only at one month after the conclusion of the amnesty application period (row 3 and column 5), we find that the prospect of obtaining legal status increases undocumented immigrants' employment probability by 34.5 percentage points, with a coefficient that is significant at the 1 percent level.[23] Based on our theoretical discussion (section 2), 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.

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 (see section 5). Hence, the prospect of obtaining legal status accelerates the labor market integration of newly arrived undocumented immigrants by about two thirds of the increase in employment they normally experience in their first year after arrival.

Having identified the effect of amnesty after the deadline for application submission we now analyze the dynamics of this effect by looking at how the employment probability differential evolves over time. Before the deliberation on the amnesty bill, *qualified* and *unqualified* immigrants

[23]In unreported regressions, we test for heterogeneity in the eligibility effect on employment, by including additional interactions with gender, education level, or age group. We find no evidence of heterogeneous effects.

were indistinguishable, so their employment probability should not have systematically differed. Once the amnesty was approved, however, both the immigrants and their (potential) employers became aware of the difference created between the two groups. In particular, as discussed in section 2 (and Appendix 1), *qualified* immigrants should have had a higher probability of retaining or finding a job, meaning that from the first day of the amnesty application period, their employment rates should begin gradually diverging over the two-month period between September 11 and November 13, 2002. In other words, the employment differential between immigrants who arrived before and after June should not be significantly different from zero at the opening of amnesty but should monotonically increase over time during the application period. Because finding evidence against this conjecture would imply an immediate loss of credibility for our entire empirical exercise, the dynamic analysis also provides a powerful falsification exercise.

Results for our coefficient of interest estimated *during* the amnesty application period are reported in panel B of Table 2. In particular, we split the application period into the first month (September 10 to October 10, upper part) and the second (and last) month (October 11 to November 13, lower part).[24] Figure 4 clarifies our empirical exercise.

[Figure 4 approximately here]

As Table 2 shows, the results are fully coherent with our theoretical expectations: In the first month after amnesty opens, the difference between the two groups' employment rates is about 10 percentage points, but not statistically significant in any specification.[25] In the second month of amnesty, the gap in the employment rate of the two groups increases: *qualified* immigrants have an

[24]It should be noted that, unfortunately, the timing of the amnesty does not allow us to look at the pre-amnesty employment rate of immigrants who do (not) satisfy the arrival condition. In fact, we define as *unqualified* all those who arrived in July and August, while amnesty began on September 11, giving us only the first 10 days of September and too few observations with which to perform this empirical exercise

[25]The fact that we find point estimates that are positive and substantially far from zero (i.e., that vary between 0.09 and 0.11), although not significantly different from zero, is not particularly surprising: Here, we are considering a month-long span from the start of amnesty, whereas at the time of interview, *qualified* immigrants (and employers) had already had an average of two weeks to react to the amnesty announcement.

employment probability that is about 16–19 percentage points higher than *unqualified* immigrants, although the difference is still barely significant. In the following month, however, after the application period has ended, this gap not only increases to more than 30 percentage points but becomes highly significant. Hence, consistently with the considerations outlined above, the difference in employment between the treatment and control group does indeed increase monotonically as agents have time to react to the amnesty design.

6.2. Robustness Checks

To test 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 across different 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, 2002) 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). Panels A and B of Table 3 report the results for the April 1 and May 1 thresholds, respectively, including estimates based on the three-, two-, and one-month observation periods for each placebo test. As before, column 1 reports the unconditional estimates, and columns 2–5 gradually add in additional controls.

[Table 3 approximately here]

Panels C and D of Table 3 display the results from similar placebo tests performed only on the population of *unqualified* immigrants. First, in panel C, 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 panel D, 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, further 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 90 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), the two years before amnesty (2000 and 2001), the year after amnesty (2003), and the year before amnesty (2001), reported in rows 1-5 of Table 4, respectively. All results, both during and after the amnesty, are fully robust to changes in the set of control years.

[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 both for after the amnesty (Panel A) and during it (Panel B) are generally very close to zero and never statistically significant.

[Table 5 approximately here]

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

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

where the employment status of undocumented migrants is regressed on a dummy for arrival in April–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. For each of the three observation windows (three, two, and one month[s] after amnesty), 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]

6.3. 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 immigrants in our sample were only observed once, our dataset cannot be used to address this issue. Instead, we use microdata from an annual survey by the ISMU foundation to derive descriptive evidence on the persistence of the employment effect.[26] The 2003 and 2004 waves of this survey, administered to around 8,000 documented and undocumented immigrants in Lombardy (the region in which Milan is situated) in each year, 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 7). 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 7 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

[26]ISMU is an independent research foundation that promotes studies on immigration. The ISMU data are sampled using an *intercept point survey methodology* based on the tendency of immigrants to cluster at certain locations (McKenzie and Mistiaen, 2009). The ISMU survey provides a representative sample of the total migrant population residing in the Lombardy region. The interview questionnaire contains a variety of questions on individual characteristics (e.g., demographics, educational level, labor market outcomes, legal status) and household characteristics (e.g., number of household members in Italy, family members abroad, housing). See Dustmann et al. (2014) for a description of these data.

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 7 approximately here]

7. 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 results provide strong evidence that fulfilling the exogenous residence condition causes a significant increase in employment probability, a finding robust to several falsification exercises. In fact, the effect we estimate is equivalent to about two thirds of the increase in employment probability that undocumented immigrants experience during their first year after arrival in Italy. We also report descriptive evidence that this increase in employment rate is persistent.

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,[27] the assessment of amnesty's economic consequences on undocumented immigrants is crucial from a policy perspective. Our theoretical model suggests that the peculiar design of the 2002 Italian amnesty—specifically, its requirement of employment at the moment of application—is likely to have reinforced the employment effect by generating important increases in immigrant labor supply. This feature, however, although perhaps desirable in terms of the amnesty program's efficacy in accelerating immigrants' labor market integration, may impose considerable costs on the immigrants themselves. Indeed, immigrants with limited bargaining power in the labor market—as is likely for recently arrived undocumented immigrants—may be willing to accept drastic wage reductions in order to achieve legal status. A similar concern may arise 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. 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. Our paper,

[27] 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*.

however, indicates that the distribution of this surplus among the different agents (i.e., undocumented immigrants, employers, and government) may depend on the type of amnesty program implemented. In particular, our results suggest that much of the surplus generated by an amnesty may accrue to the employers. 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

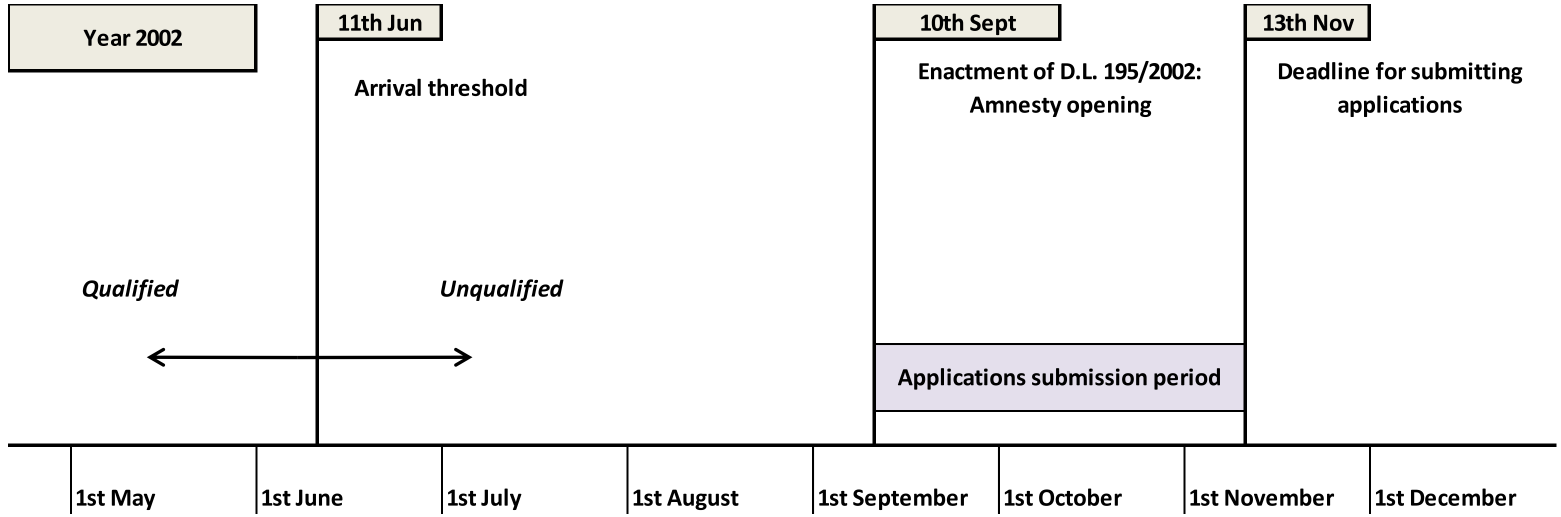


Figure 2. Estimation timeline

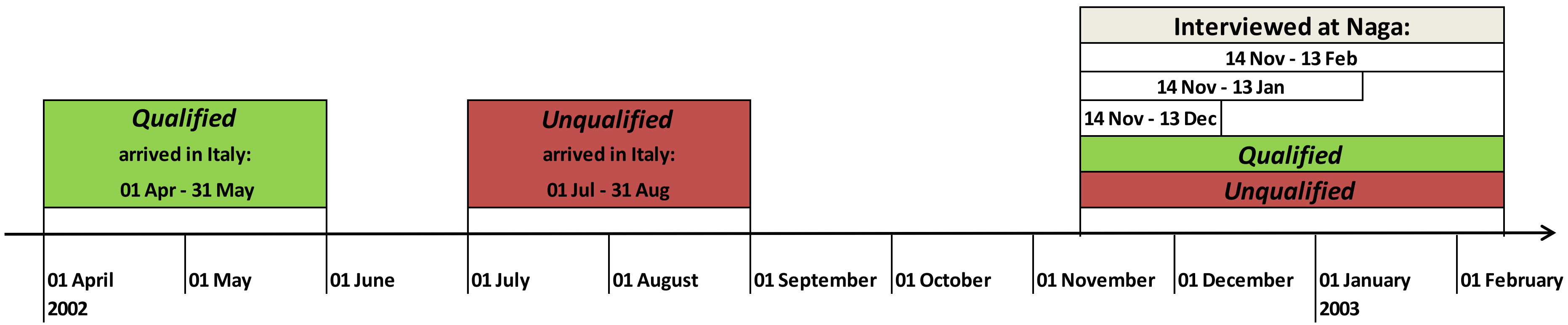
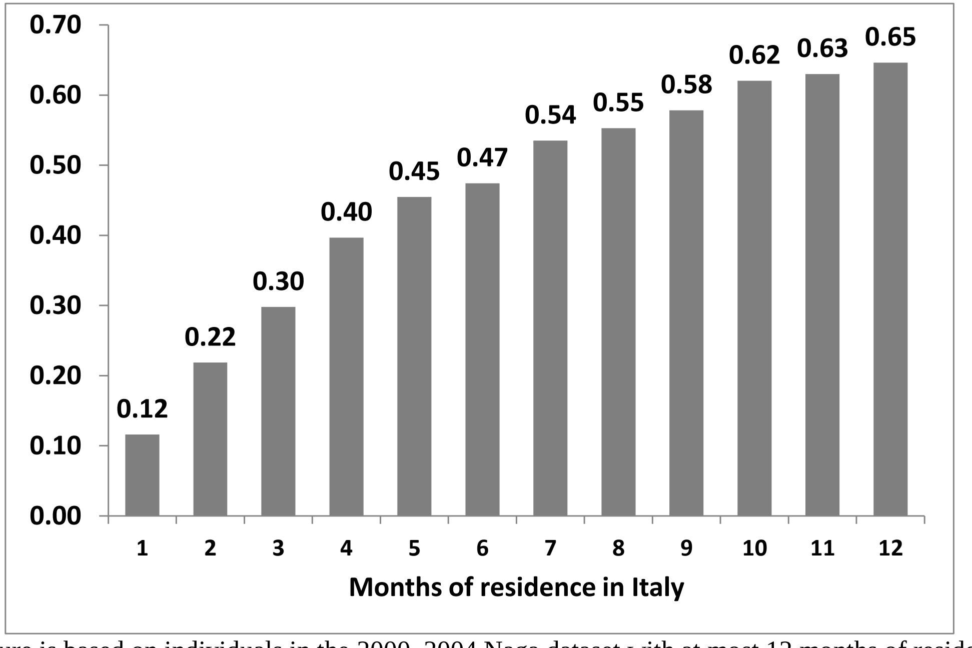
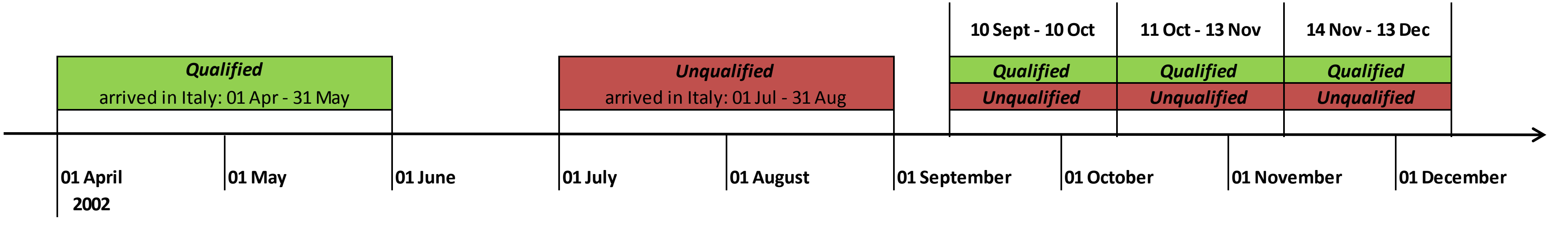


Figure 3. Average employment rate of undocumented immigrants (2000–2004)



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

Figure 4. Dynamic effects of amnesty



Tables

Table 1. Descriptive statistics

		Panel A *2002 (amnesty year)* *April-May*	 *July-August*		Panel B *2000, 2001, 2003, 2004* *April-May*	 *July-August*	
Men	mean	0,513	0,505		0,476	0,541	†
	sd	*0.501*	*0.501*		*0.500*	*0.499*	
Age	mean	30,644	31,132		31,125	31,003	
	sd	*8.233*	*9.603*		*8.907*	*9.077*	
Education							
Primary	mean	0,156	0,16		0,102	0,148	†
	sd	*0.364*	*0.367*		*0.303*	*0.355*	
Secondary	mean	0,292	0,268		0,361	0,382	
	sd	*0.456*	*0.444*		*0.481*	*0.486*	
High school	mean	0,494	0,51		0,437	0,383	†
	sd	*0.502*	*0.501*		*0.496*	*0.486*	
University	mean	0,058	0,062		0.100	0,087	
	sd	*0.235*	*0.242*		*0.300*	*0.282*	
Origin							
Europe	mean	0,143	0,289	†	0.170	0,219	†
	sd	*0.351*	*0.454*		*0.376*	*0.414*	
Asia	mean	0,071	0,067		0,109	0,108	
	sd	*0.258*	*0.251*		*0.311*	*0.310*	
North Africa	mean	0,182	0,222		0,147	0,196	†
	sd	*0.387*	*0.416*		*0.354*	*0.397*	
Sub-Saharan Africa	mean	0,097	0,082		0,058	0,084	
	sd	*0.297*	*0.276*		*0.235*	*0.278*	
Latin America	mean	0,506	0,34	†	0,516	0,393	†
	sd	*0.502*	*0.475*		*0.500*	*0.489*	
Observations		154	194		599	817	

Note: The table reports means and standard deviations of selected characteristics for immigrants arrived in Italy in April-May and July-August in the amnesty year 2002 and in control years 2000, 2001, 2003 and 2004. Data for the individuals in all groups was collected on their first visit to Naga between September 10 and February 13 in each year.
† 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	5	obs.
Panel A - After amnesty						
	3 months (14 Nov - 13 Feb)					
	0.112	0.151*	0.170**	0.193**	0.197**	877
	[0.083]	[0.082]	[0.080]	[0.081]	[0.081]	
	2 months (14 Nov - 13 Jan)					
Effect of *qualified* status (β)	0.240**	0.236**	0.252**	0.262***	0.278***	581
	[0.102]	[0.102]	[0.099]	[0.100]	[0.100]	
	1 month (14 Nov - 13 Dec)					
	0.335**	0.342***	0.342***	0.346***	0.345***	315
	[0.132]	[0.129]	[0.125]	[0.124]	[0.126]	
Panel B - During amnesty						
	1st month (10 Sept - 10 Oct)					
	0.092	0.101	0.101	0.102	0.108	448
Effect of *qualified* status (β)	[0.127]	[0.127]	[0.126]	[0.127]	[0.130]	
	2nd month (11 Oct - 13 Nov)					
	0.191*	0.17	0.157	0.166	0.165	439
	[0.111]	[0.111]	[0.112]	[0.113]	[0.116]	
Gender, age, education	no	yes	yes	yes	yes	
Area of origin	no	no	yes	yes	yes	
Month dummies	no	no	no	yes	yes	
Profession in home country	no	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. Columns 2–5 gradually add in additional control variables. Each line corresponds to a different observation window. The last column displays the number of observations used in each regression. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11 categories of occupation and labor market status in the country of origin, including a dummy for missing values.
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	5	obs.
Panel A: *Qualified* (February-March) Vs *Qualified* (April-May)						
			3 months (14 Nov - 13 Feb)			
	0.037	0.046	0.046	0.054	0.042	771
	[0.085]	[0.084]	[0.081]	[0.083]	[0.083]	
			2 months (14 Nov - 13 Jan)			
Effect of placebo *qualified* status	0.061	0.061	0.037	0.035	0.02	503
	[0.105]	[0.104]	[0.101]	[0.102]	[0.103]	
			1 month (14 Nov - 13 Dec)			
	0.021	0.038	-0.004	-0.016	-0.033	287
	[0.125]	[0.126]	[0.124]	[0.123]	[0.124]	
Panel B: *Qualified* (April) Vs *Qualified* (May)						
			3 months (14 Nov - 13 Feb)			
	-0.056	-0.055	-0.073	-0.068	-0.067	373
	[0.126]	[0.124]	[0.121]	[0.123]	[0.126]	
			2 months (14 Nov - 13 Jan)			
Effect of placebo *qualified* status	-0.043	-0.082	-0.094	-0.087	-0.083	259
	[0.148]	[0.143]	[0.142]	[0.144]	[0.146]	
			1 month (14 Nov - 13 Dec)			
	-0.027	-0.043	-0.045	-0.039	-0.064	141
	[0.179]	[0.169]	[0.175]	[0.176]	[0.190]	
Panel C: *Unqualified* (July-August) Vs *Unqualified* (September-October)						
			3 months (14 Nov - 13 Feb)			
	-0.068	-0.05	-0.034	-0.028	-0.022	1,218
	[0.067]	[0.067]	[0.067]	[0.066]	[0.066]	
			2 months (14 Nov - 13 Jan)			
Effect of placebo *qualified* status	-0.024	-0.02	0.001	-0.002	0.013	793
	[0.083]	[0.084]	[0.082]	[0.081]	[0.082]	
			1 month (14 Nov - 13 Dec)			
	0.041	0.046	0.066	0.068	0.07	455
	[0.115]	[0.113]	[0.111]	[0.110]	[0.111]	
Panel D: *Unqualified* (July) Vs *Unqualified* (August)						
			3 months (14 Nov - 13 Feb)			
	-0.142	-0.178	-0.18	-0.134	-0.136	504
	[0.110]	[0.111]	[0.110]	[0.110]	[0.108]	
			2 months (14 Nov - 13 Jan)			
Effect of placebo *qualified* status	-0.106	-0.128	-0.138	-0.115	-0.151	322
	[0.156]	[0.162]	[0.155]	[0.148]	[0.147]	
			1 month (14 Nov - 13 Dec)			
	0.162	0.079	0.098	0.097	0.049	174
	[0.198]	[0.208]	[0.203]	[0.204]	[0.208]	
Gender, age, education	no	yes	yes	yes	yes	
Area of origin	no	no	yes	yes	yes	
Month dummies	no	no	no	yes	yes	
Profession in home country	no	no	no	no	yes	

Note: Each cell in panel A 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. Columns 2–5 gradually add in additional control variables. Each line corresponds to a different observation window. The last column displays the number of observations used in each regression. Panels B-D have the same structure, but the arrival dummy is modified as described in the title of each panel. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11 categories of occupation and labor market status in the country of origin, including a dummy for missing values. 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	5	obs.	6	7	8	9	10	obs.	11	12	13	14	15	obs.
	After amnesty						**During amnesty**											
	1 month (14 Nov - 13 Dec)						*1st month (10 Sept - 10 Oct)*						*2nd month (11 October - 13 November)*					
2002 Vs (2003 & 2004)	0.356**	0.405***	0.456***	0.452***	0.482***	161	0.044	0.065	0.054	0.037	0.008	196	0.160	0.169	0.145	0.163	0.181	234
	[0.158]	[0.148]	[0.146]	[0.147]	[0.156]		[0.141]	[0.144]	[0.145]	[0.146]	[0.156]		[0.128]	[0.128]	[0.129]	[0.132]	[0.136]	
2002 Vs (2001 & 2003)	0.405***	0.398***	0.382***	0.381***	0.391***	188	0.146	0.197	0.201	0.190	0.169	243	0.258**	0.232*	0.220*	0.230*	0.247*	269
	[0.148]	[0.145]	[0.143]	[0.143]	[0.148]		[0.138]	[0.145]	[0.144]	[0.144]	[0.151]		[0.123]	[0.125]	[0.126]	[0.128]	[0.130]	
2002 Vs (2000 & 2001)	0.322**	0.305**	0.292**	0.313**	0.302**	225	0.118	0.116	0.125	0.126	0.120	318	0.212*	0.173	0.168	0.171	0.161	301
	[0.141]	[0.142]	[0.139]	[0.138]	[0.138]		[0.133]	[0.137]	[0.135]	[0.136]	[0.140]		[0.120]	[0.122]	[0.123]	[0.124]	[0.127]	
2002 Vs 2003	0.346*	0.330*	0.367**	0.327*	0.326	118	0.127	0.167	0.178	0.161	0.067	133	0.215	0.220	0.206	0.232	0.274*	174
	[0.188]	[0.180]	[0.181]	[0.183]	[0.198]		[0.164]	[0.177]	[0.182]	[0.185]	[0.215]		[0.145]	[0.153]	[0.157]	[0.164]	[0.164]	
2002 Vs 2001	0.444***	0.427**	0.391**	0.399**	0.435**	141	0.158	0.199	0.201	0.191	0.170	176	0.293**	0.224	0.196	0.196	0.190	191
	[0.167]	[0.171]	[0.171]	[0.171]	[0.174]		[0.152]	[0.163]	[0.163]	[0.162]	[0.169]		[0.142]	[0.145]	[0.144]	[0.145]	[0.151]	
Gender, age, education	no	yes	yes	yes	yes		no	yes	yes	yes	yes		no	yes	yes	yes	yes	
Area of origin	no	no	yes	yes	yes		no	no	yes	yes	yes		no	no	yes	yes	yes	
Month dummies	no	no	no	yes	yes		no	no	no	yes	yes		no	no	no	yes	yes	
Profession in home country	no	no	no	no	yes		no	no	no	no	yes		no	no	no	no	yes	

Note: In all 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. Columns 2–5 gradually add in additional control variables. The last column displays the number of observations used in each regression. Each panel corresponds to a different observation window. Rows differ in the control years used in the analysis. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11 categories of occupation and labor market status in the country of origin, including a dummy for missing values. 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	5	obs.
Panel A: After amnesty (14 November - 14 February)						
Placebo amnesty: 2000	0.102	0.086	0.085	0.075	0.083	691
	[0.079]	[0.078]	[0.078]	[0.076]	[0.077]	
Placebo amnesty: 2001	0.027	0.047	0.066	0.055	0.042	691
	[0.083]	[0.083]	[0.083]	[0.083]	[0.084]	
Placebo amnesty: 2003	-0.051	-0.037	-0.043	-0.034	-0.037	691
	[0.093]	[0.089]	[0.089]	[0.089]	[0.090]	
Placebo amnesty: 2004	-0.119	-0.138	-0.154	-0.137	-0.128	691
	[0.095]	[0.095]	[0.095]	[0.095]	[0.095]	
Panel B: During amnesty (10 September - 13 November)						
Placebo amnesty: 2000	0.037	0.051	0.047	0.059	0.057	725
	[0.077]	[0.077]	[0.076]	[0.076]	[0.076]	
Placebo amnesty: 2001	-0.099	-0.093	-0.082	-0.081	-0.089	725
	[0.081]	[0.081]	[0.080]	[0.080]	[0.080]	
Placebo amnesty: 2003	-0.043	-0.035	-0.055	-0.079	-0.058	725
	[0.087]	[0.089]	[0.089]	[0.089]	[0.090]	
Placebo amnesty: 2004	0.135	0.095	0.109	0.114	0.106	725
	[0.095]	[0.096]	[0.096]	[0.095]	[0.093]	
Gender, age, education	no	yes	yes	yes	yes	
Area of origin	no	no	yes	yes	yes	
Month dummies	no	no	no	yes	yes	
Profession in home country	no	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. Columns 2–5 gradually add in additional control variables. The last column displays the number of observations used in each regression. Each panel corresponds to a different observation window. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11categories of occupation and labor market status in the country of origin, including a dummy for missing values. 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	5	obs.
Panel A: 3 months (14 November - 13 Feb 2003)						
Amnesty year						
2002	0.127*	0.130*	0.127*	0.149**	0.165**	186
	[0.074]	[0.074]	[0.073]	[0.075]	[0.072]	
Placebo years						
2000	0.086	0.054	0.036	0.026	0.050	203
	[0.065]	[0.067]	[0.071]	[0.068]	[0.067]	
2001	0.034	0.036	0.036	0.039	0.004	199
	[0.070]	[0.074]	[0.076]	[0.075]	[0.080]	
2003	-0.025	-0.053	-0.070	-0.053	-0.030	149
	[0.083]	[0.078]	[0.083]	[0.084]	[0.085]	
2004	-0.082	-0.134	-0.160*	-0.148	-0.138	140
	[0.086]	[0.088]	[0.092]	[0.095]	[0.100]	
Panel B: 2 months (14 November - 13 Jan)						
Amnesty year						
2002	0.222**	0.217**	0.198**	0.212**	0.250**	118
	[0.091]	[0.095]	[0.095]	[0.099]	[0.099]	
Placebo years						
2000	0.057	0.037	0.014	0.013	0.036	147
	[0.081]	[0.086]	[0.091]	[0.093]	[0.099]	
2001	-0.046	-0.055	-0.049	-0.049	-0.078	139
	[0.085]	[0.091]	[0.091]	[0.091]	[0.094]	
2003	0.017	0.034	0.038	0.042	0.042	86
	[0.108]	[0.109]	[0.117]	[0.120]	[0.133]	
2004	-0.132	-0.113	-0.149	-0.138	-0.175	91
	[0.104]	[0.107]	[0.117]	[0.120]	[0.115]	
Panel C: 1 month (14 November - 13 Dec)						
Amnesty year						
2002	0.313***	0.293**	0.260**	0.276**	0.284**	71
	[0.115]	[0.113]	[0.112]	[0.105]	[0.114]	
Placebo years						
2000	0.094	0.091	0.002	-0.017	0.025	84
	[0.112]	[0.125]	[0.146]	[0.153]	[0.190]	
2001	-0.132	-0.142	-0.137	-0.136	-0.193	70
	[0.121]	[0.148]	[0.147]	[0.146]	[0.166]	
2003	-0.033	-0.005	0.005	0.042	0.209	47
	[0.149]	[0.152]	[0.160]	[0.160]	[0.211]	
2004	-0.057	-0.129	-0.108	-0.131	-0.281*	43
	[0.161]	[0.164]	[0.160]	[0.142]	[0.143]	
Gender, age, education	no	yes	yes	yes	yes	
Area of origin	no	no	yes	yes	yes	
Month dummies	no	no	no	yes	yes	
Profession in home country	no	no	no	no	yes	

Note: In all panels, 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. Columns 2–5 gradually add in additional control variables. The last column displays the number of observations used in each regression. Each panel corresponds to a different observation window. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11 categories of occupation and labor market status in the country of origin, including a dummy for missing values. Robust standard errors are in parentheses; ***p<0.01, **p<0.05, and *p<0.1.

Table 7. 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***	0.169***	0.227***	0.234***	0.255***	0.261***	0.166***	0.162***
	[0.031]	[0.031]	[0.042]	[0.041]	[0.049]	[0.048]	[0.035]	[0.035]
Year and province dummies	yes	yes	yes	yes	yes	yes	yes	yes
Individual controls	no	yes	no	yes	no	yes	no	yes
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 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.

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.

a. 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 expire, 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*:

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

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., $(1-p)\cdot w - p\cdot c^m \geq (1-p)\cdot b - p\cdot c^m$. This condition defines a flat labor supply:

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

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 $\overline{p}$ such that $w^f(\overline{p}) = w^m(\overline{p})$.

Define the match $S(p) = w^f(p) - w^m(p)$. When the apprehension probability is sufficiently low (i.e., $p \leq \overline{p}$), the surplus is positive, $S(p) \geq 0$, but when $p > \overline{p}$, 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 $p \leq \overline{p}$.

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

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

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 $S^m(p) = (1-\beta)S(p)$.

b. 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 lower for *eligible* than for *ineligible* immigrants $(0 \leq p^e < p^i < 1)$. 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.

i. 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

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

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

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

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

ii. 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 $p^q\big|_{employed}=p^e<p^i$. 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: $p^q\big|_{unmployed}=p^u=p^i$. 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:

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

and their reservation wage becomes

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

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 $w^e(0) = \tilde{b}$ and $\lim_{p^e \to 1} w^e\left(p^e\right) = \infty$. 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 $p \in \left[0, \overline{p}\right]$. 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 ether 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).

c. 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:

$$\frac{employment\ rate^q}{employment\ rate^u} > 1 \tag{A8}$$

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

As discussed in section 4, one important advantage of our dataset is that it is based on the information immigrants reported to Naga volunteers and not on what they declared to the Italian authorities during the amnesty process. That is, whereas immigrants may have had incentives to falsely report their date of arrival when filing their application for legal status, there was no clear motivation to misreport information when interviewed at Naga: Whether the Italian authorities would judge them 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. In any case, misreporting would threaten the validity of our analysis *only* if the incentives for doing so were associated with unobservable individual characteristics correlated with the probability of being employed.

Nevertheless, we empirically test for systematic misreporting of arrival date to Naga but find no evidence for it. Our test is based on the fact that, in the presence of misreporting, we should observe a change in the distribution of the arrival date around the 2002 threshold date relative to non-amnesty years. In other words, 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 a non-amnesty year.

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 the sample described in section 4), 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 A 1. As before, column 1 reports the unconditional estimates, while the following four columns gradually add in groups of controls (gender, age and education, and dummies for area of origin, month, and profession in the home country). In panel A, we consider immigrants interviewed at Naga during the application period (September 10–November 13), while in panel B, we use the sample of immigrants interviewed in the three months after the end of the application period (November 14–February 14). Looking specifically at the unconditional estimates in column 1 of Table A1, the estimated coefficient on the constant term indicates that of the immigrants who arrived in Italy in April–May 2002, 47 percent arrived during amnesty and 42 percent arrived after amnesty. 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 2–5 does not alter this conclusion. These results provide truly reassuring evidence against the existence of 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*

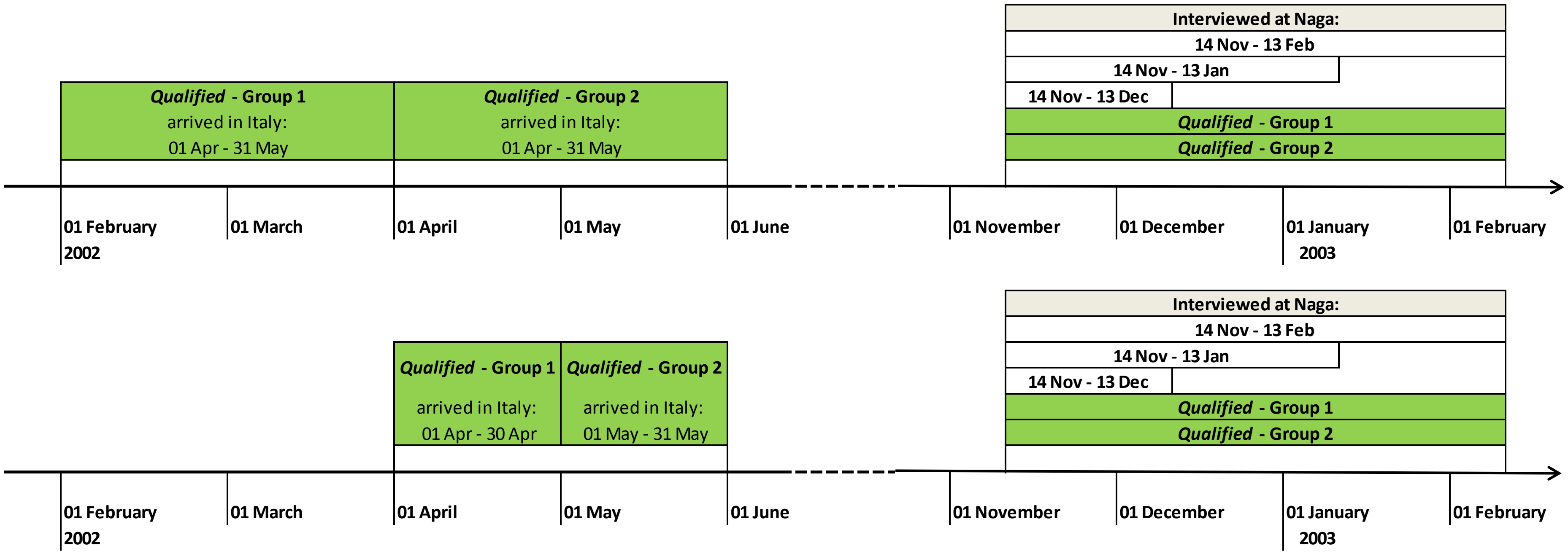
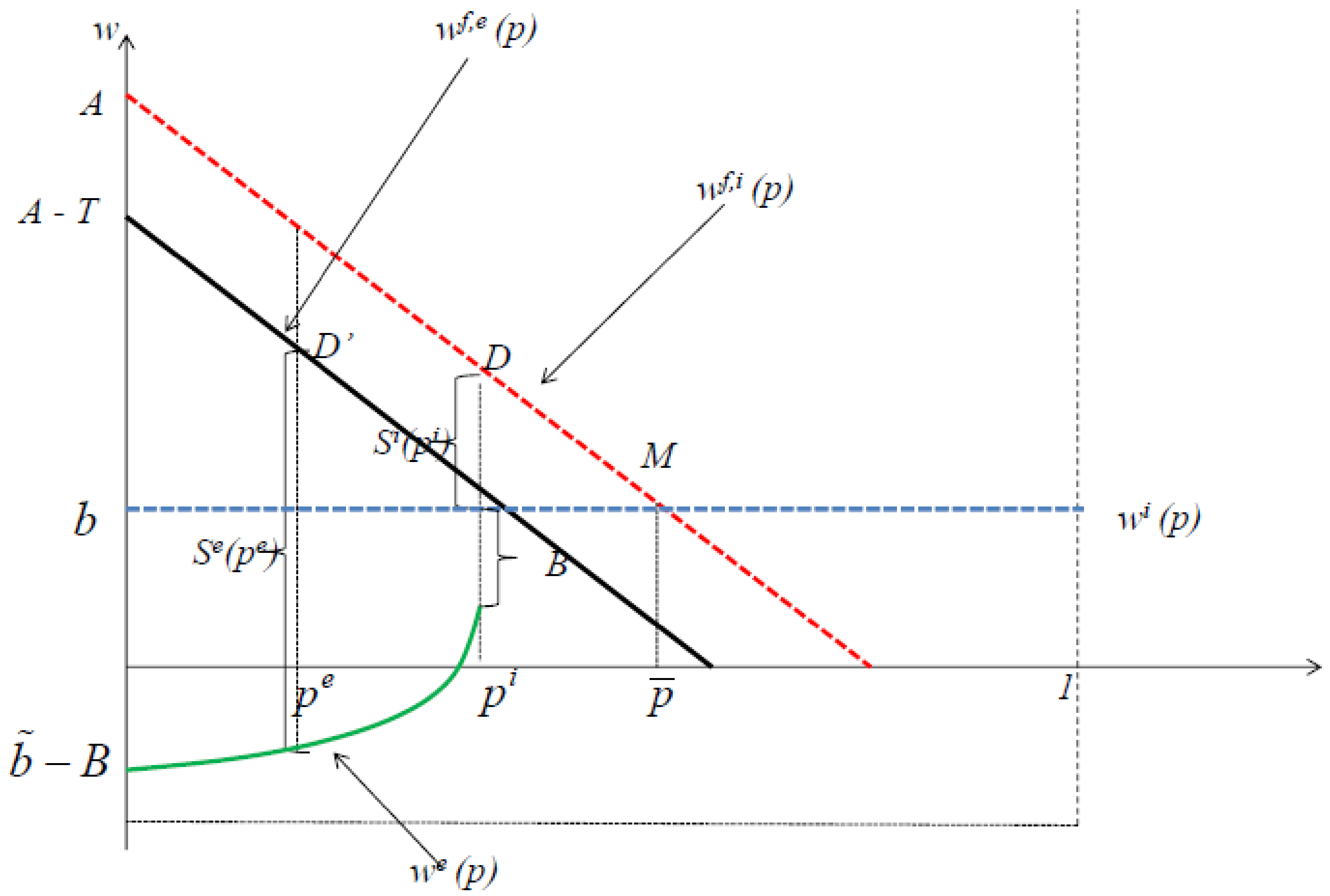


Figure A 2. Theoretical framework



Appendix tables

Table A 1. Probability of having arrived in Italy in April-May (versus July-August)

	1	2	3	4	5
Panel A: During amnesty (10 September - 13 November)					
Arrival year 2000	-0.056	-0.059	-0.069	-0.067	-0.073
	[0.050]	[0.051]	[0.051]	[0.051]	[0.052]
Arrival year 2001	-0.045	-0.034	-0.040	-0.042	-0.003
	[0.052]	[0.054]	[0.054]	[0.054]	[0.058]
Arrival year 2003	-0.028	-0.027	-0.031	-0.035	-0.030
	[0.057]	[0.057]	[0.057]	[0.057]	[0.057]
Arrival year 2004	-0.071	-0.065	-0.060	-0.062	-0.055
	[0.059]	[0.059]	[0.059]	[0.059]	[0.060]
Constant	0.469***	0.571**	0.602**	0.599**	0.658**
	[0.039]	[0.229]	[0.235]	[0.235]	[0.293]
Observations	887	887	887	887	887
Panel B: After amnesty (14 November - 14 February)					
Arrival year 2000	0.014	0.004	-0.016	-0.019	-0.030
	[0.050]	[0.051]	[0.050]	[0.050]	[0.051]
Arrival year 2001	0.053	0.063	0.039	0.039	0.036
	[0.051]	[0.053]	[0.053]	[0.053]	[0.056]
Arrival year 2003	0.010	0.018	0.009	0.012	-0.001
	[0.055]	[0.056]	[0.055]	[0.054]	[0.054]
Arrival year 2004	-0.069	-0.066	-0.048	-0.044	-0.074
	[0.054]	[0.055]	[0.054]	[0.053]	[0.053]
Constant	0.419***	0.206	0.118	0.057	0.247
	[0.036]	[0.160]	[0.168]	[0.168]	[0.205]
Observations	877	877	877	877	877
Gender, age, education	no	yes	yes	yes	yes
Area of origin	no	no	yes	yes	yes
Month dummies	no	no	no	yes	yes
Profession in home country	no	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 2–5 gradually add in additional controls. Gender, age, and education controls include a gender 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. Profession in home country is denoted by dummies for 11 categories of occupation and labor market status in the country of origin, including a dummy for missing values. Robust standard errors are in parentheses; ***p<0.01, **p<0.05, and *p<0.1.