DISCUSSION PAPER SERIES

IZA

IZA DP No. 10235

Immigrant Crime and Legal Status: Evidence from Repeated Amnesty Programs

Francesco Fasani

September 2016

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Immigrant Crime and Legal Status: Evidence from Repeated Amnesty Programs

Francesco Fasani
Queen Mary University of London, CReAM, CEPR and IZA

Discussion Paper No. 10235
September 2016

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 10235
September 2016

ABSTRACT

Immigrant Crime and Legal Status: Evidence from Repeated Amnesty Programs*

Do general amnesty programs lead to reductions in the crime rate among immigrants? We answer this question by exploiting both cross-sectional and time variation in the number of immigrants legalized generated by the enactment of repeated amnesty programs between 1990 and 2005 in Italy. We address the potential endogeneity of the "legalization treatment"" by instrumenting the actual number of legalized immigrants with alternative predicted measures based on past amnesty applications patterns and residential choices of documented and undocumented immigrants. We find that, in the year following an amnesty, regions in which a higher share of immigrants obtained legal status experienced a greater decline in non-EU immigrant crime rates, relative to other regions. The effect is statistically significant but relatively small and not persistent. In further results, we fail to find any evidence of substitution in the criminal market from other population groups - namely, EU immigrants and Italian citizens - and we observe a small and not persistent reduction in total offenses.

JEL Classification: F22, J61, K37

Keywords: illegal migration, legalization, migration policy

Corresponding author:

Francesco Fasani
School of Economics and Finance
Queen Mary University of London
327 Mile End Road
E1 4 NS London
United Kingdom
E-mail: f.fasani@qmul.ac.uk

* I am especially grateful to Tommaso Frattini, Marco Tonello and Mathis Wagner for their comments and suggestions. The paper also benefitted from feedback received from participants at: 9th International Conference on Migration and Development, EUI Fiesole; 13th Annual Migration Meeting (AM), Bonn; Workshop on Immigration, Science Po. I would like to thank Paolo Buonanno for sharing his migration data with me. Financial assistance from the Fondazione Debenedetti (www.frdb.org) is gratefully acknowledged. The usual disclaimer applies.

1 Introduction

According to the 2014 Transatlantic Trends Survey (TTS) on Migration, 32 percent of respondents in Europe and 38 percent in the US believe that there are too many immigrants in their countries (TTS, 2014). Interestingly, these shares drop to approximately 20 percent in both areas when respondents are informed of the number of immigrants actually residing in their countries. The fact that natives often tend to largely overestimate the size of the immigrant population may be explained by the belief that a substantial share of that population is undocumented and thus not recorded by official statistics. Native residents in receiving countries often perceive undocumented immigrants as a particularly unsettling presence. Data from recent waves of the Transatlantic Trends Survey on Immigration reported in Figure 1 show how attitudes of respondents in selected OECD countries vary when they consider undocumented (on the vertical axis) rather than documented immigrants (on the horizontal axis). The graph in panel A shows that the share of respondents concerned about undocumented immigrants is well above 50 percent in all countries and substantially larger than the share concerned about documented immigrants. Italy, the country we study in this paper, has the largest percentage of interviewees that are concerned about unauthorized immigrants (86 percent) while only 27 percent of Italian respondents report concern about legal foreign residents. The graph in panel B summarizes the responses given to the question "do you think that most of the immigrants in your country are here legally or illegally?". Unsurprisingly, in countries such as Italy, Spain and the US - where a large presence of unauthorized immigrants is a well documented fact - the vast majority of respondents believe that their foreign born population is predominantly undocumented. Italy has the largest share of respondents saying that undocumented immigrants prevail (64 percent), while Germany has the lowest share (13 percent). Further, Panel C shows that in all countries, the fraction of respondents reporting that undocumented immigrants are a burden on social services is larger than those casting such blame on documented immigrants. Similarly, panel D shows that the concern about immigrants increasing crime rates in host societies is stronger for unauthorized residents than for legal ones. Remarkably, in both of these last two graphs, Italian respondents seem to be nearly equally concerned about documented and undocumented immigrants.

Undocumented immigrants are met with stronger opposition among receiving societies because their presence tends to be immediately associated with law-breaking behavior. Individuals who circumvented migration legislation rules, so the argument goes, may be more prone to ignore legislation in general, including criminal law. Moreover, the fact that police and judicial authorities in host countries generally lack any record on undocumented immigrants generates the concern that it may be harder to arrest and convict them, increasing their incentives to offend.

The theoretical predictions concerning the relationship between legal status and criminal behavior are ambiguous. If lacking legal status is associated with poorer economic prospects, undocumented immigrants may have stronger incentives to engage in economically motivated crime than documented immigrants. However, they may face harsher punishment than documented immigrants if forced removals are implemented as additional sanctions on unauthorized immigrants and if the fact of having a criminal record prevents the migrant from obtaining legal status in the future.

Establishing whether receiving legal status generates a reduction in the individual criminal behavior of newly legalized immigrants - as convincingly shown by Mastrobuoni and Pinotti (2015) and Pinotti (2015) - is important for advancing our understanding of the mechanisms that induce individuals to engage in crime or to refrain from doing so. From the perspective of voters, however, it is the overall level of immigrant crime that enters (negatively) their utility function and not the criminal behavior of one particular immigrant sub-population (such as the newly legalized ones). Voters are likely to support a policy that sizably reduces total immigrant crime, while they will be indifferent (if not hostile) to interventions that either produce negligible effects or that differentially affect the behavior of distinct immigrant sub-populations while leaving overall immigrant crime unaffected. As general amnesties for unauthorized residents are often met with widespread opposition among the electorate, policy-makers who intend to propose such an intervention would need strong empirical evidence demonstrating that mass legalizations are effective in reducing immigrant crime.[1] Our paper addresses this policy-relevant question. Legalized immigrants only account for a fraction - generally a minority fraction - of the total migrant population. Hence, the conclusions one can reach on the behavior of this specific sub-population may differ from those obtained when analyzing aggregate outcomes of the entire foreign born population, which includes those who were already legal residents before the amnesty, those who became legal residents thanks to the amnesty, those who remained undocumented and those who arrived (lawfully or unlawfully) after the amnesty was closed.

In the US context, Baker (2015) estimates a sizable negative effect on property crime of the legalizations granted under the amnesty program in the Immigration Reform and Control Act (IRCA) of 1986. We adopt a similar approach but we analyze a context where amnesty programs have been repeatedly and frequently enacted. In our paper, we exploit four general amnesties, which exogenously granted legal status to large fractions of the undocumented immigrant population in Italy, and we empirically investigate whether legalizations were followed by significant reductions in the crime rate among immigrants. Our identification strategy relies on both the geographical variation in the number of immigrants legalized in different Italian regions and the time variation generated by the repeated programs. We address the potential endogeneity of the "legalization treatment" by instrumenting the actual number of legalized immigrants with alternative predicted measures based on past residential choices of documented and undocumented immigrants and on applications patterns in previous amnesties. We find that, in the year following an amnesty, regions in which a higher share of immigrants obtained legal status experienced a greater decline in immigrant crime rates, relative to other regions. The effect is statistically significant but relatively small in magnitude and not persistent. In

[1] Evidence on the effect of alternative migration policy interventions on immigrant crime is also badly needed. To the best of our knowledge, only Miles and Cox (2014) study whether increased enforcement against undocumented immigrants leads to lower crime rates. They study the effect of "Secure Communities" - a policy that dramatically increased the likelihood of being deported for unauthorized immigrants who are arrested - and fail to find any crime-reducing effect of this program.

further results, we fail to find evidence of substitution in the criminal market from other population groups - such as EU immigrants and Italian citizens - whose residence status was not directly affected by amnesties. Moreover, we find that the total number of offenses decreased more in areas that legalized a higher number of undocumented immigrants, although the effect is small and not persistent. Our findings suggest that although many other good arguments can be advanced to support amnesties for unauthorized residents, their crime-reducing impact does not seem to be a compelling one.

The paper is organized as follows. In section 2, we briefly summarize related literature on immigration, legal status and crime. Section 3 describes the Italian institutional setting, focusing on amnesties and on immigrants' involvement in criminal activities. Section 4 offers a discussion of our data and some descriptive statistics. Identification issues and our empirical strategy are explained in section 5. Estimation results are presented in section 6. Finally, some concluding remarks are made in section 7.

2 Legal Status and Crime: Theory and Evidence

If obtaining legal status has positive consequences for immigrants' integration in host countries labor markets - as the existing evidence suggests[2] - we should also expect to observe a reduction in their propensity to engage in crime. Indeed, a standard economic model of criminal decisions *à* la Becker (Becker, 1968) would predict that individuals with poorer labor market

[2]When "coming out of the shadows", immigrants may gain access to a wider spectrum of employment opportunities, possibly accessing occupations and industries that offer higher earnings and better working conditions. Further, legal residence status is generally associated with eligibility for unemployment benefit schemes, social assistance and other welfare provisions. Improving their outside options should allow legalized immigrants to increase their reservation wages, to look for better matches and to gain bargaining power with their current and prospective employers. Moreover, legalized residents are no longer subject to the hazard of being arrested and deported for lacking legal status, adding value and stability to their job matches. Finally, employers of legalized immigrants stop facing the uncertainty of fines and of a sudden and undesired termination of their employment relationships, although they may face higher costs (having to pay payroll taxes and social contributions). All of these channels should lead to an unambiguous increase in wages and returns to skills for newly legalized immigrants who are employed. The effect on employment, however, is theoretically unclear. Indeed, the empirical evidence on legal status and labor market outcomes generally finds a clear increase in wages and an ambiguous effect on employment after legalization. See Fasani (2015) for a recent review of this literature.

opportunities should be more likely to turn to crime. However, if a criminal conviction for an illegal resident implies additional sanctions such as being deported from the host country or being permanently barred from applying for legal residence status in the future, undocumented immigrants would face harsher punishment than their documented counterparts and may thus have lower incentives to offend.[3] The effect of legal status on the crime rate is therefore theoretically ambiguous.

Empirically establishing whether becoming a legal resident reduces the propensity to engage in crime is challenging. The legal status of immigrants is generally not observed in major surveys. Data on undocumented immigrants are rare and often collected in non-systematic way. In addition, researchers need to convincingly address the endogenous sorting of immigrants into legal status. A direct comparison of immigrants with and without legal status is hardly informative of causal relationships. Undocumented immigrants often have demographic characteristics - e.g., being younger, less educated, more likely to be male than their documented counterparts - that strongly increase their likelihood of being potential offenders. Unobservable characteristics may reinforce this gap if, for instance, undocumented immigrants are more impatient or less risk averse than documented immigrants.[4] Unless changes in legal status were exogenously determined, comparing immigrants' outcomes before and after legalization would not be conclusive either: individuals who applied for legalization and who were eventually successful differ from those who did not apply or who were rejected.

In the last few years, some studies have attempted to estimate the causal impact of changes in legal status on immigrants' criminal decisions by exploiting policies that created arguably exogenous variation in legal status among the immigrant population. This recent literature adds to a growing body of evidence that focuses on the more general relationship between the presence of immigrants and crime rates in receiving societies.[5] In the US context, Baker

[3]For instance, applicants for IRCA legalization were not admissible if they had previously been convicted of a felony or of three or more misdemeanors (Kerwin, 2010).

[4]Dustmann et al. (n.d.) study the impact of legal status on the consumption behavior of immigrants and find evidence compatible with less risk averse individuals sorting into illegal residence status.

[5]This literature has examined both the US (Butcher and Piehl, 1998*a*,*b*; Borjas et al., 2010; Chalfin, 2014;

(2015) and Freedman et al. (2014) study the effect on immigrant crime of the 1986 Immigration Reform and Control Act (IRCA) amnesty. Baker (2015) estimates the response of aggregate county crime rates to the number of immigrants legalized in the area through IRCA exploiting cross-county variation in the number of legalizations and quasi-random variation in the timing of processing the applications. According to his estimates, the amnesty had a sizable crime-reducing effect, primarily driven by a decline in property crime. Evidence pointing in the opposite direction is provided by Freedman et al. (2014). They use individual-level data on felony charges in one Texas county and compare involvement in crime of Hispanic and non-Hispanic individuals - Hispanic ethnicity is used as a proxy for illegal residence status, which is not observed in their data - before and after the 1986 IRCA reform, in a difference-in-differences setting. They find that Hispanic citizens offended significantly more after the IRCA amnesty expired. They rationalize their results with the IRCA reform having introduced employment restrictions that made it more difficult for newly arrived undocumented immigrants to find a job in the US. Empirical work on legal status and immigrant crime in Europe has so far exclusively focused on Italy, exploiting policies that changed immigrants' residence status but that were not general amnesties. Mastrobuoni and Pinotti (2015) use the 2007 European Union Enlargement and compare recidivism rates of released inmates from new EU member countries (Romania and Bulgaria) and from candidate member countries in a difference-in-differences approach. On the 1st of January 2007, citizens from the former group switched from being (mostly) undocumented to having full European citizenship status, while no change was experienced by the latter group. According to their estimates, obtaining legal status is associated with a 50 percent reduction in the re-arrest probability. Pinotti (2015) exploits the Italian quota system to analyze the relationship between immigrant crime and legal status using a regression discontinuity design.[6] After having matched applicants' records with police data on criminal

Spenkuch, 2014) and European countries (Bianchi et al., 2012; Bell et al., 2013; Piopiunik and Ruhose, 2015). The evidence is quite mixed, although most of these studies fail to find major increases in host countries ' crime rates attributable to the arrival of immigrants. Interestingly, Nunziata (2015) finds that immigration increases the fear of crime - but not actual crime rates - in European countries.

[6]Although devised by the legislator to regulate the entry of foreign-born workers, the Italian quota system is

charges, Pinotti (2015) compares criminal behavior of individuals that applied just before and just after the exhaustion of the quota. He finds that obtaining legal status more than halves the number of serious crimes committed by applicants in the following year. The effect is large but admittedly driven by approximately 20 percent of his sample of male applicants.

If individuals who become legal residents have lower incentives to engage in crime, general amnesties may represent an extremely effective policy instrument to reduce immigrant crime. By taking a large fraction of the undocumented population "out of the shadows", they should permanently reduce these individuals' incentives to commit crime. If this effect is sizable and economically relevant, it should be taken into account when debating the pros and cons of enacting an amnesty program. The overall effect of an amnesty on immigrant crime, however, will depend on the impact produced on all groups of migrants, not merely on the effect on those who have gained legal status. If this latter group will experience a reduction in the propensity to commit crime, one could expect the incentives to engage in crime to increase for those who failed to obtain legal status and remained undocumented. Indeed, massive legalizations generate general equilibrium effects and likely lead to a deterioration of the labour market outcomes of immigrants who remain undocumented in the immediate aftermath of an amnesty. Moreover, these immigrants consider their prospect of being legalized to be increasingly uncertain and delayed until the next legalization (if any). Both factors may induce higher criminal behavior among this population, potentially offsetting the reduction in the propensity of the newly legalized immigrants to commit crime. In addition, if amnesties reduce the crime supply of a segment of the immigrant population but leave the demand for crime unaffected, the crime opportunities that are not taken by the newly legalized immigrants may be seized by other groups of the migrant population and/or by native offenders. The size and the duration of the crime-reducing effect of amnesties will also depend on how effective legalizations are in reduc-

widely used to *ex post* legalize the residence status of undocumented immigrants who are already residing and working in the country (see Fasani, 2010). The quota system sets binding regional quotas before the submission process begins, and electronically submitted applications are then processed on a first-come, first-served basis until quotas are filled.

ing the presence of illegal residents in the country. While legalizing undocumented residents, amnesties can attract new unauthorized arrivals, who may arrive in the host country with the expectation of benefitting from the current or a future amnesty. This effect may be particularly strong if the enactment of the amnesty generates the expectation that other amnesties will be granted in the future. As we will see in section 3.1, this should be a major concern in a context, such as the Italian one, in which repeated amnesties are routinely enacted. If undocumented inflows increase with respect to the pre-amnesty period and newly legalized immigrants are immediately replaced by unauthorized newcomers, the crime-reducing effect of the amnesty may be zero even in the very short run. Even if the amnesty does not encourage higher unauthorized inflows but there is no policy intervention to curb future unauthorized flows, the amnesty effect will last only as long as new inflows do not reconstitute the initial stock of unauthorized immigrants in the host country.[7]

3 Institutional Setting

3.1 Migration Policy and Amnesties in Italy

General amnesties have been a constant feature of the Italian migration policy. Between 1986 and 2012, seven legalization programs were enacted to grant legal status to the growing undocumented foreign-born population residing in Italy. In particular, mass legalization programs took place in 1986, 1990, 1995, 1998, 2002, 2009 and 2012. Overall, approximately 1.85 million immigrants were legalized through one of these programs, a very large number for a country that hosted about 3.6 million documented immigrants in 2012. Italy is not alone among European countries in having adopted legalization programs: Casarico et al. (2012) report that between 1980 and 2008 several amnesties were granted in Austria, France, Greece, Portugal and

[7] In the US context, Orrenius and Zavodny (2003) show that the IRCA did not change long-term patterns of undocumented immigration. The inflow of unauthorized immigrants (as measured by border apprehensions) slightly declined when the IRCA was implemented but quickly resumed its long-term upward trend. Although the IRCA's legalization does not seem to have encouraged higher inflows in the short run, its sanctions and tougher enforcement seem to have failed to discourage inflows in the long run.

Spain. However, the extent and frequency of these policy interventions make Italy comparable only to Spain, where six general amnesties were offered over approximately the same period.

Amnesties in Italy are decided by the central government and simultaneously implemented in all regions using nationally uniform procedures. Remarkably, the decision to enact an amnesty has been made by governments of all political orientations.[8] Amnesties offered a temporary and renewable residence permit to all undocumented immigrants who applied before a certain date and satisfied specific criteria. Although the requirements changed over time, eligibility was generally based on a predetermined residence condition.[9] A valid residence permit would then allow immigrants to have a regular employment contract and work in the formal sector. The process of screening the applications and releasing the residence permits was generally concluded within one or two years from the closing date of the submission window.[10] The acceptance rate of applications has been extremely high, being above 90 percent for all four amnesties that we analyze in this paper (1990, 1995, 1998, 2002). Such a high success rate creates strong incentives for undocumented immigrants to apply. Together with the frequency of the amnesties, it also likely generates the expectation among potential unauthorized migrants that, if they succeed in entering Italy, becoming a legal resident in the country is just a matter of waiting a few years.

Figure 2 summarizes the evolution of the foreign-born population in Italy between 1986 and 2012. The continuous line shows the stock of legal residents - as measured by the number of valid residence permits in each year - that grew from fewer than 0.5 million in 1990 to approximately 3.6 million individuals in 2012, that is from less than 1 percent to approximately

[8]The first two amnesties (in 1986 and 1990) were granted by centrist governments, the third by the "government of experts" led by Lamberto Dini in 1995, a left-wing government voted the fourth amnesty in 1998, two more legalizations were enacted by right-wing governments (in 2002 and 2009), and the last one by Mario Monti and his "government of experts" in 2012.

[9]The amnesties in 1986 and 1995 simply asked the applicant to prove they had been in Italy at least since the day before the amnesty law was passed. The amnesties in 1990 and 1998 required two and seven months of minimum residence in Italy, respectively. The subsequent amnesties (2002, 2009 and 2012) conditioned eligibility on both a residence and an employment condition. See Devillanova et al. (2014) for a discussion of the effects of alternative criteria.

[10]In contrast to the 1986 IRCA legalization program (see Baker, 2015), data on legalization dates are not available for Italian amnesties.

6 percent of the total resident population in Italy. The vertical bars report the number of immigrants legalized in each amnesty (in thousands): 105 in 1986, 218 in 1990, 244 in 1995, 217 in 1998, 637 in 2002, 295 in 2009 and 135 in 2012. Finally, the dots are estimates of the stock of unauthorized residents produced by an independent research foundation called ISMU. [11] According to these estimates, the undocumented population in Italy shows a distinctive roller-coaster trend: amnesties substantially reduce the stock of unauthorized residents in the short run, but fail to stem new inflows of undocumented newcomers that rapidly re-create this population. Although data on actual unauthorized inflows are not available (by definition), police records on enforcement at the Italian border suggest that amnesties may have generated larger inflows of undocumented immigrants. The continuous line in Figure 3 reports the total annual number of foreign citizens refused entry at the Italian border for being undocumented between 1989 and 2006 (vertical axis on the right). Although changes in this time series may be also driven by changes in enforcement, entry refusals are an arguably good proxy of unauthorized migratory pressure on the borders. They shows a downward trend over time - from around 60 thousand per year in the early '90s to 20-30 thousand in the early '00s - but also distinctive spikes in correspondence of the four amnesties (identified by the vertical lines). Remarkably, yearly growth rates in people refused entry at the border - reported by the bars in Figure 3 (vertical axis on the left) - are generally positive precisely in the years the amnesties were taking place.

3.2 Immigrant Crime in Italy

The 2010 Transatlantic Trends Survey on Immigration shows that the majority of Italian citizens are concerned about immigrants increasing crime rates (see Figure 1, panel D). In contrast to other OECD countries, where this concern tends to focus primarily on undocumented immi-

[11] The ISMU (Institute of Multiethnic Studies) Foundation (www.ismu.org) conducts an annual survey of a representative sample of approximately 8 thousand documented and undocumented migrants residing in the Lombardy region (see Dustmann et al., n.d. for a description of these data). ISMU is the the only Italian organization producing estimates of the undocumented population in a systematic way and using a methodologically coherent approach (Fasani, 2009).

grants, Italian citizens appear to worry about migrants in general, irrespective of their residence status. Do these widespread negative attitudes reflect a substantial engagement of immigrants in criminal activities in Italy?

Figure 4 reports the time series of four indicators of immigrants' criminal involvement in Italy over the period 1991-2005. In particular, it shows the share of migrants among the population of those who: 1) received a criminal charge (diamond marker); 2) were convicted (square marker); 3) entered prison after a conviction (circle marker); and 4) were detained in prison (triangle marker). The distance between these lines and the dotted line reporting the share of documented immigrants over the total resident population highlights a striking over-representation of immigrants among criminal statistics at all stages of the Italian criminal justice system. For instance, in 2005 (the last year in the time series) immigrants accounted for approximately 23 percent of individuals receiving a criminal charge, a share nearly six times larger than the documented immigrant share in the total population (approximately four percent in the same year).[12] The pattern is similar when we move to the next step of the criminal justice process: 22 percent of individuals convicted in Italian tribunals in 2005 were foreign-born citizens. The share of immigrants among individuals receiving criminal charges and among those being convicted closely tracked one another over the entire period (although the latter time series is more volatile). An impressive jump, instead, is observed when looking at prison population records: in 2005, 45 percent of the individuals entering prison were immigrants, while they accounted for 33 percent of the total stock of inmates.

Overall, Figure 4 suggests that a troubling large fraction of the immigrant population commits crime in Italy. A higher propensity to engage in crime among immigrants, however, is just one of the potential explanations for their overrepresentation in criminal statistics. A competing explanation is that immigrants are often overrepresented among those groups - i.e., male, young, low-educated and poor individuals - who are more likely to commit crime. Moreover,

[12] Even if we account for the 540 thousand undocumented immigrants who were residing in Italy in 2005 according to ISMU estimates, the immigrant share would increase to 4.7 percent and immigrants would still be over-represented among individuals receiving a criminal charge by a factor of nearly 5.

the criminal justice process may be biased against immigrants: this bias may originate from voluntary discrimination by police forces and judges and from the inherent hurdles immigrants face in interacting with the judicial system (e.g., language barriers, limited knowledge of host country legislation, poor legal assistance in trials).[13]

What share of the immigrants committing crime in Italy lacks legal status?[14] This information is not collected in a systematic way and it is only available for criminal charges, at the national level and for some years (see Barbagli and A., 2011). In the period 2004-2009, the average share of illegal residents varies between 60 and 80 percent, depending on the type of offense: the highest average shares are in burglary (0.83), car theft (0.82), theft (0.78) and robbery (0.77). Very similar figures were recorded in the mid-1990s: the Italian setting appears characterized by a strong and persistent link between lacking legal status and immigrants' propensity to engage in crime.

A country where immigrants largely contribute to criminal statistics and where undocumented immigrants are responsible for the majority of these offenses provides an ideal setting for analyzing whether mass legalizations are an effective policy instrument for reducing immigrants' crime rates. Remarkably, the only rigorous study on immigration and crime in Italy (Bianchi et al., 2012) finds no causal impact of a higher share of foreign-born citizens on the incidence of any criminal offense (except for robberies, which account for a minuscule share of total crime). Although the authors attempt to control for the presence of undocumented migrants in several ways, the inherent mismeasurement of the distribution of this population across areas and of its movements over time may explain why their estimates suggest that

[13]A closer examination of Italian prison statistics (ISTAT, 2006, 2012), for instance, allows one to at least partially explain the dramatic overrepresentation of foreign-born citizens in the Italian prison population. First, immigrants are more likely to enter jail before receiving a final conviction than are Italian citizens. Second, immigrants enter prison with shorter average sentences than Italian citizens and, therefore, enter more frequently and for less serious crimes. Third, immigrants are less likely than natives to be sentenced to house arrest or to be assigned to alternative measures outside prison because they generally fail to fulfill the conditions - e.g., having a legal domicile, having a family able to host the individual - that are required to let the defendant free during the trial or to apply for measures alternative to detention while in prison.

[14]Note that during the period we study, being an illegal resident was not a criminal offense in Italy but rather an administrative infraction. Apprehensions of immigrants lacking legal status, therefore, are not counted in criminal statistics.

immigrants do not cause crime to rise.

4 Data and Descriptive Statistics

In our analysis, we use a panel of the twenty Italian regions over fifteen years (1991-2005) and analyze the empirical relationship between regional crime rates of foreign-born citizens and the number of immigrants legalized in each region by one of the four amnesties that were enacted over this period.

Immigrant crime. We measure immigrant crime using yearly records from the Italian Ministry of Justice on the number of individuals who received a criminal charge. Data are consistently available for the period 1991-2005 and are disaggregated by the region where the crime was committed and by the nationality of the (potential) offender.[15] We use these records to construct the main outcome variable for our analysis, namely the number of non-EU immigrants receiving a criminal charge. These data allow also to measure the number of EU immigrants and of natives charged with criminal offenses, two additional outcomes analyzed in the paper. Unfortunately, the criminal charges data cannot be further disaggregated by type of crime, preventing us from analyzing the effect of amnesties on different criminal offenses.

Criminal charges account only for a subset of the crimes that are reported to the police by victims, those corresponding to "cleared cases" for which a potential offender has been identified. Nevertheless, criminal charges are arguably the best indicator for studying the criminal behavior of immigrants because they record the nationality of the offenders. Recorded crime, instead, generally lacks any demographic information about the offenders, who are often unknown at the time the crime is reported to the police. Information on the nationality of the subjects involved is also often available for convicted and detained individuals, but these latter data capture a further selected subset of the population of criminals and generally have a substantial lag with respect to the moment the offense was committed (a few years in contexts,

[15] Unfortunately, the series was discontinued after year 2005.

such as Italy, where the judicial process is xtremely lengthy).

In order to develop an alternative measure of immigrant crime, we gained access to records from the Ministry of Internal Affairs on the number of immigrants arrested by the Italian police forces in each year. These data are disaggregated by region but are available only for ten years (1992-2001) and do not distinguish between EU and non-EU immigrants. Note that individuals arrested and individuals receiving a criminal charge are both subsets of the total pool of offenders, but they only partially overlap: not all individuals being arrested are subsequently charged, and not all those receiving a criminal charge are also arrested.[16]

Apart from information on the number of individuals involved, the Italian Ministry of Justice data on criminal charges contain information on the total number of offenses for which the Italian criminal justice system has initiated prosecution proceedings, including cases in which the offender is still unknown (which are the majority of cases). These data are available with regional disaggregation and over the same period (1991-2005) as the dataset on individuals charged. The offenses data are not linked to specific offenders and cannot thus be attributed to immigrants rather than to natives. The information on the type of offense is also not available. We use these records to construct a measure of total regional crime.

Panel A in Table 1 reports some descriptive statistics for these variables. Over the period we consider (1991-2005), there were 10.7 non-EU immigrants charged per ten thousand population each year, with overall and within-region standard deviations of 9.2 and 6.3, respectively. Non-EU immigrants accounted for 12 percent of the individuals receiving a criminal charge, EU immigrants for one percent (with approximately one individual charged every year per ten thousand population), and the remaining 87 percent were Italian citizens. This latter population group had an annual average of approximately 83 individuals charged per ten thousand

[16] A possible disadvantage of using data on criminal charges or arrests is that observed changes in crime may be driven by changes in the criminal justice systems treatment of immigrants rather than by actual changes in their underlying criminal activity. Bohn et al. (2015) report some evidence in this direction for misdemeanors (but not felonies) in one American county in the aftermath of the IRCA. We do not see particular reasons for expecting changes in policing practices after the amnesties in Italy. Moreover, similar changes would bias our results only if policing behavior changed differently across regions and in a way that is systematically correlated with the number of immigrants legalized in each region.

population. The average number of offenses for which the criminal justice system initiated prosecution was nearly 455 per ten thousand population, with overall and within-region standard deviations of 173 and 84, respectively. Further, approximately 8.4 immigrants per ten thousand population were arrested over the period 1992-2001.

Legalizations. Amnesty data are used to construct our regional measure of "legalization treatment". Over the period we study (1991-2005), three general amnesties were granted in Italy: in 1995, 1998 and 2002 (see section 3.1). In addition, an amnesty program was implemented in 1990, and its effects on crime may still be present at the beginning of the period we consider. Aggregate records on the total number of undocumented immigrants legalized in each region by each of these four amnesties were obtained from the Italian Ministry of Internal Affairs and used to construct our main dependent variable. Panel B in Table 1 shows that the average number of immigrants legalized by amnesties over the whole period analyzed is approximately 12 individuals (per ten thousand population) with the overall and within-region standard deviation both being close to 30. This variable, however, includes many zero values in non-amnesty years. To obtain a better sense of the actual size of the "legalizations treatment", the table also reports descriptive statistics for the average number of legalizations computed exclusively in amnesty years. The average figure increases to approximately 45 immigrants legalized per ten thousand population, with a minimum value of 7.8 and a maximum of nearly 230.

Other regional controls. The other regional controls used in our analysis - resident population, employment rate, GDP per capita and documented immigrant population - are provided by the Italian Office of Statistics (ISTAT; www.istat.it). Over the period 1991-2005, the average region in our sample had a native population of 2.8 million people, a GDP per capita of 12.7 thousand euros (at constant 1990 euro-equivalent prices), an unemployment rate of 10.7 percent and approximately 185 documented immigrants per ten thousand native population (see panel C of Table 1).

5 Empirical Strategy

5.1 Estimating Equation

The following regression equation could be estimated to analyze the contribution of undocumented immigrants to immigrant crime:

$$FB\_crime_{rt} = a + bI_{rt} + X'_{rt}c + d_t + e_r + u_{rt} \tag{1}$$

where: FB_crime_{rt} is a measure of immigrant crime committed in region r in period t; α is a constant; I_{rt} is the number of undocumented immigrants living in region r in period t; X_{rt} is a set of time-varying regional controls; d_t are year dummies; e_r are regional fixed effects; and u_{rt} is an error term. By taking first-differences, we remove any regional fixed effect that may be correlated with both dependent and explanatory variables:

$$\Delta FB\_crime_{rt} = b\Delta I_{rt} + \Delta X'_{rt}c + \Delta d_t + \Delta u_{rt} \tag{2}$$

If one could observe the stock of undocumented immigrants, exogenous variation in I_{rt} would allow to consistently estimate the parameter b and to identify the marginal effect of changes in the size of the undocumented population on immigrant crime. In general, neither the size of the undocumented stock nor its changes over time are observed, preventing us from directly estimating equation (2). Amnesty programs, however, induce changes in the population of undocumented immigrants that are both measurable and (arguably) exogenous. Defining A_{rt} as the number of immigrants legalized in region r in year t, we can write the change in the stock of undocumented immigrants ΔI_{rt} as:

$$\Delta I_{rt} = -A_{rt} + I^{NET}_{rt} \tag{3}$$

where A_{rt} is positive in amnesty years and zero otherwise while I^{NET}_{rt} is the net inflow of undocumented immigrants to the region. Replacing identity (3) into equation (2) leads to the following equation:

$$\Delta FB\_crime_{rt} = -b(A_{rt}) + \Delta X'_{rt}c + \Delta d_t + \Delta v_{rt} \tag{4}$$

where $\Delta v_{rt} = \Delta u_{rt} + bI_{rt}^{NET}$. Now the parameter b can be consistently estimated as long as $cov(A_{rt}, \Delta v_{rt}) = 0$.[17]

Our main measure of immigrant crime is the number of non-EU immigrants receiving a criminal charge (FB_ch), while the legalizations variable A_{rt} is the number of individuals legalized in each region by one of the four amnesties enacted between 1990 and 2005. We normalize both variables using the total native population residing in the region (in tens of thousands), and we take logs.[18] Our main estimating equation is:

$$\Delta ln\left(\frac{FB\_ch_{rt}}{Pop_{rt}}\right) = \beta ln\left(\frac{A_{rt}}{Pop_{rt}}\right) + \Delta X'_{rt}\gamma + \Delta d_t + \Delta\varepsilon_{rt} \tag{5}$$

The coefficient of interest β identifies the elasticity of immigrant crime rate with respect to the "legalization treatment" (i.e. the number of immigrants legalized). A negative coefficient would imply that regions where a higher number of immigrants gained legal status experienced a larger decline in immigrant crime. The elasticity of immigrant crime with respect to legalizations, however, can be consistently estimated only if the number of immigrants legalized in each region is exogenous in the regression equation. In section 5.2, we discuss potential endogeneity issues affecting the "legalization treatment" and how we address them in our empirical analysis.

[17]Our empirical strategy partially resemble the approach adopted by Barbarino and Mastrobuoni (2014). They estimate the impact of changes in the prison population on crime and exploit repeated collective pardons that generate exogenous variation in the number of detained criminals. While they can observe both the prison population and the number of pardoned inmates (and can thus instrument the former variable with the latter), we only observe the number of legalized immigrants and not the population of undocumented immigrants.

[18]Because natives may react to inflows of immigrants by moving to a different region, throughout the analysis we always use the first lag of the native population. Results do not significantly change if further lags or contemporaneous population are used.

Equation (5) specifies a contemporaneous relation between immigrant crime and legalization of undocumented immigrants. However, the timing and duration of this effect (if any) is primarily an empirical question. The effect does not have to be immediate. As discussed in section 3.1, the time required to screen the applications, decide on each case and release the residence permits to successful applicants implied a sizeable lag between the application submission and the obtainment of legal status. If the migrants' incentives to engage in crime drop only once they become legal residents, the crime-reducing impact of amnesties may be observed only after some time. The simple prospect of becoming legal, however, may already produce some effect on the criminal decisions: applicants may anticipate the benefits of getting a residence permit and not want to harm their chances of legalization by committing crime while their application is still being assessed. In addition, the effect may be more or less persistent. As we argued in section 2, its duration depends on how the amnesty affects the criminal incentives of the different groups in the migrant population (newly legalized immigrants, undocumented immigrants, documented immigrants), their relative size and their growth rates.

5.2 Econometric Issues

Amnesties are enacted by the central government and simultaneously implemented in all regions. While the decision to grant amnesties and the timing of these political decisions are arguably orthogonal to immigrants' endogenous residential choices and to local shocks, the number of legalized individuals in each region may be correlated with both types of variables. None of the amnesties we consider had a predetermined exogenous cap on the total number of immigrants who could be legalized, and the share of applicants who were granted legal status was generally above 90 percent. These features imply that the number of immigrants legalized in each region closely followed the number of applications submitted. Amnesty applications are determined by the residential choices of undocumented immigrants and by their decisions to participate in the program. Both types of choice are potentially endogenous in our regression. For instance, undocumented immigrants will have greater incentive to settle in areas that offer them better labor market opportunities and to apply in regions where returns to having legal status are higher. Therefore, we could expect to observe more (less) applications in regions with higher (lower) employment rates. Insofar as higher employment translates into lower crime rates, a cross-sectional analysis would suggest that regions that receive higher numbers of amnesty applications tend to experience lower levels of crime. Our empirical analysis is robust to this identification threat because we remove any persistent regional difference by first-differencing our data and exploiting within-region variation over time in legalizations and crime. One may nevertheless be concerned that immigrants are (at least partially) able to anticipate future shocks to the local economy and to modify their residential choices and/or their participation in amnesties according to their predictions. For instance, if undocumented immigrants who expect to observe an increase in employment in one region in the next period are more likely to move there and to apply for legal status, and if a positive shock to employment induces a reduction in crime, we could observe that a higher numbers of applications is filed in regions experiencing larger reductions in crime rates.[19] In this case, removing regional fixed effects would not be sufficient to identify a causal parameter. In addition, one cannot rule out the existence of other time-varying unobservable variables that correlate with both the outcome and our main dependent variable and that could bias our estimate of the parameter β. Unobservable changes in the strictness of regional police enforcement against undocumented immigrants, for instance, may influence both their presence and their crime involvement in a region.

We address these concerns in two ways. First, in our main specification, we include time-varying regional controls - namely, the unemployment rate and GDP per capita - to capture the local economic cycle that is potentially correlated with both crime rates and the number of applications. Second, we instrument the actual number of legalized immigrants (the "legalization

[19]Equation (3) suggests that the net inflow of undocumented immigrants in a region - which is not observed and is likely correlated with the number of amnesty applications submitted - is a potential candidate for introducing omitted variable bias into our estimates.

treatment") with alternative predicted measure based on past location choices of immigrants and past amnesty application decisions of undocumented immigrants. Specifically, we develop and employ three different instruments. We consider the first instrument as our benchmark identification strategy while the other two allow us to test the robustness of our approach.

For our main instrument, we predict the number of legalizations in each region r in each amnesty year t ($\widehat{A}^{81}_{rt}$) taking the total number of immigrants from each source country c legalized during each amnesty (A_{ct}) and allocating them across regions according to the distribution of immigrants recorded in the 1981 census ($\overline{sh}^{81}_{cr}$). Note that the census survey captures all immigrants who are residing in the country, including undocumented immigrants, although the data do not allow to distinguish immigrants by their legal status. Our proposed instrument is:

$$\widehat{A}^{81}_{rt} = \sum_{c} \overline{sh}^{81}_{cr} * A_{ct} \qquad \text{for } t = (1990, 1995, 1998, 2002) \tag{6}$$

where $\overline{sh}^{81}_{cr}$ is defined as the ratio between the number of immigrants from country c residing in region r in year 1981 (M^{81}_{cr}) and the number of immigrants from country c residing in Italy in year 1981 (M^{81}_{c}):

$$\overline{sh}^{81}_{cr} = \frac{M^{81}_{cr}}{M^{81}_{c}} \tag{7}$$

This instrument is conceptually similar to the supply-push component instrument proposed by Altonji and Card (1991) and, since then, widely used in the migration literature.[20] The rationale for this instrument is motivated by a large body of evidence showing that settlement patterns of previous waves of immigrants are strong predictors of residential choices of following waves (see, among others, Bartel, 1989 and Munshi, 2003). The instrument has two components: a set of constant shares $\overline{sh}^{81}_{cr}$, which generate cross-sectional variation, and a national flow variable A_{ct}, which varies over time. The exogeneity of the instrument with respect to regional shocks is ensured by the fact that the first component is predetermined with respect to the

[20] In studying the impact of immigrants on local crime rates, for instance, this instrumental variable approach has been adopted by both Bianchi et al. (2012) for Italy and Bell et al. (2013) Bianchi for the UK.

period analyzed (the census took place ten years before the beginning of the period under study, namely 1991-2005) while the second component is measured at the national level and should thus not reflect shocks in any particular Italian region. The instrument is valid under the reasonable identifying assumption that - conditional on regional fixed effects - the shocks that occurred in year 1981 (and before) and determined the distribution of immigrants of different nationalities across regions in that year are not systematically correlated with those that determined the distribution of applications in the amnesties granted in 1990, 1995, 1998 or 2002. Records on the first twenty nationalities of immigrants legalized in each amnesty are available for all four programs in the period we study. Data are reported in Appendix Table A 1. Countries of origin like Morocco, Tunisia, Philippines, China and Senegal are consistently placed among the top national groups in all amnesties. Other nationalities, especially Eastern European ones, appear in the mid-90s and quickly reach the top of the ranking. These data are used to measure the time varying component A_{ct} of our instrument. The distribution of immigrants in 1981 ($\overline{sh}_{cr}^{81}$) is measured with 1981 census data that are available at the regional level and for national groups of immigrants that were predominant at the time.[21]

More detailed data on the geographical distribution of different national group of immigrants in Italy are available in more recent years. In particular, there are Ministry of Internal Affairs's records on residence permits for all Italian provinces and all foreign nationalities that start in 1990. Employing these data, we can improve on the 1981 regional shares by using information on a wider set of nationalities. This improvement is especially important for predicting legalizations in more recent amnesties. Indeed, the overlap between predominant nationalities in 1981 and top 20 countries of origin of legalized immigrants gradually shrinks over time, because new nationalities started migrating to Italy while flows from some of the first group of nationalities declined over time. This implies that our main instrument $\widehat{A}_{rt}^{81}$ is based on a progressively

[21]Beyond EU-15 nationals, the main national groups residing in Italy in 1981 were (in decreasing order): Tunisia, Jugoslavia, Iran, Libya, Venezuela, Argentina, Egypt, Ethiopia, Philippines, Brazil, Chile, Morocco, Cape Verde, Somalia and Algeria.

smaller number of nationalities.[22] The drawback of using data from 1990, however, is that we should now worry about persistent shocks that may have influenced the distribution of both the immigrants in 1990 and the legalizations in later years. Obviously, the concern is particularly salient for the first two amnesties we analyze. Here, we face a trade-off between having better data and potentially threatening the validity of our IV strategy. With respect to the firs instrument we discussed ($\widehat{A}_{rt}^{81}$), we construct our alternative instrument replacing the immigrants shares measured in 1981 with those of 1990. We define this second instrument as $\widehat{A}_{rt}^{90}$.

Finally, we develop a third instrumental variable strategy that is exclusively based on the regional distribution of amnesty applications in a legalization program that took place well before the period we study starts. The first general amnesty in Italy was enacted in 1986 and legalized approximately 105 thousand undocumented immigrants. As for the previous instruments, settlement patterns of previous immigrant cohorts can serve as instrument for patterns of later cohorts. We therefore use the number of immigrants legalized in each Italian region with the 1986 amnesty as instrument for the legalizations granted in following amnesties. From one amnesty to the other, local economic shocks should change the relative attractiveness of regional labor markets for undocumented immigrants arriving in Italy and their incentives to apply for legal status. While this latter variation is potentially endogenous in our regressions, the instrument we propose should isolate the component of exogenous variation that can be predicted based on past residential and amnesty application choices of undocumented immigrants. Our instrument is time invariant and we therefore interact it with amnesty year dummies. These interaction terms allow legalizations in 1986 to differentially predict the "legalization treatment" in each of the following amnesties. As before, the instrument is valid under the assumption that regional shocks are not too persistent. This latter approach is analogous to

[22] For instance, nine of the first twenty nationalities that were legalized with the 1990 amnesty were also among the major national communities residing in Italy in 1981. The number drops to four countries in occasion of the 2002 amnesty, when Eastern Europeans - who could hardly emigrate due to the Iron Curtain in the early 1980s - were very high in the ranking of legalized immigrants.

the main instrumental variable strategy adopted in Dustmann et al. (2013). In the rest of the paper, this third instrument is denoted with $\widehat{A}_{rt}^{86}$.

Overall, we develop a set of three alternative instruments that can be used to address the potential endogeneity of the actual number of legalizations $\widehat{A}_{rt}^{81}$, $\widehat{A}_{rt}^{90}$ and $\widehat{A}_{rt}^{86}$. In the empirical analysis, we consider the predicted number of legalizations based on the 1981 census as our main instrument and we show that our 2SLS estimates are robust to the choice of any of the alternative instruments.

In our view, the other regional controls included in our regressions - i.e., resident population, unemployment rate, GDP per capita, share of documented immigrants - do not pose particular empirical issues. Nevertheless, throughout our empirical analysis, we will test the robustness of our main results by first presenting unconditional estimates of the impact of legalizations on immigrant crime and then by gradually adding regional controls. When conditioning on the resident population, we exclusively use the native population and lag it once, as natives may react to immigrant inflows by moving to other areas (although inter-regional migration is very low in Italy). The first lag of the native population is also used to normalize both immigrant crime and the "legalization" treatment. Local economic controls (unemployment rate and GDP per capita) are considered exogenous in our regression: as the number of individuals legalized is small relative to the native population (its average value in amnesty years is approximately 45 individuals for every ten thousand native residents; see Table 1), we exclude an effect of the "legalization treatment" on regional economic outcomes. [23] The inclusion of the share of documented immigrants, however, is potentially more problematic. Although the documented stock shows independent variation with respect to the legalization programs - driven, for instance, by new legal entries and by return migration - the number of immigrants legalized and the changes in the documented stock are highly and positively correlated in the years immediately

[23] All our findings are robust to using the first lag of regional unemployment rate and GDP per capita. Results can be provided upon request.

following an amnesty. Obviously, regions where larger numbers of immigrants obtain legal status experience larger increases in the stock of documented immigrants. These amnesty-driven mechanical increases in the documented immigrant population make us wary of conditioning on this variable in our regressions, and we therefore include this control only in some of the specifications. If both immigrant crime and the number of legalizations are positively correlated with the stock of legal migrants, leaving the latter variable in the error term will potentially generate an upward bias in our estimates of the impact of legalization on immigrant crime. As this latter parameter is expected to be negative, we would therefore estimate a lower bound of the effect of interest.

6 Results

6.1 Main Results

In Table 2, we investigate whether legalizations are associated with lower immigrant crime and the exact timing and duration of this empirical relationship. As discussed in section 5.1, the effect does not have to be contemporaneous and may be more or less persistent. Following our estimating equation (5), we regress the yearly change in the log of non-EU immigrant charged with a criminal offense (per ten thousand population) on the log of the number of individuals legalized (per ten thousand population) in each amnesty.[24] In all regressions, regional fixed effects are removed by taking first-differences, a full set of year dummies is included to capture national trends, and standard errors are clustered at the regional level to allow for within-region serial correlation in local shocks. In columns 1-4 of Table 2, we alternatively include the contemporaneous value of the "legalization treatment", its first and second lags and its first lead. The estimates reported in these columns show that while contemporaneous legalizations do not result in lower immigrant crime (column 1), the first lag of the "legalization treatment" produces

[24] The number of immigrants legalized (A_{rt}) takes positive values in amnesty years and is equal to zero otherwise. In order to have defined values when taking logs, we have added one to the number of applicants in all years when constructing our main dependent variable: $ln[(A_{rt}+1)/Pop_{rt}]$.

a significant negative effect: the estimated coefficient on this latter variable is minus 0.03 and significant at the five percent level (column 2). The estimated coefficient on the second lag of legalization is close to zero and not significant (column 3). Similarly, legalizations in period *(t+1)* do not produce any significant effect on current crime rates. In the following columns (column 5-9), we gradually include all of the "legalization treatment" variables plus time-varying regional controls (log of total native population, log of GDP per capita, unemployment rate, log of documented immigrant share). In all specifications, the estimated coefficient on legalizations in (t-1) remains negative and statistically significant, while the coefficients on the other legalization variables are closer to zero and not statistically different from it.

The estimates in Table 2 strongly suggest that regions where larger shares of immigrants were legalized experienced relatively larger reductions in the number of immigrants receiving a criminal charge. This crime-reducing effect is not contemporaneous, becoming significant only one year after the amnesty took place. This finding is fully consistent both with the delays of amnesty programs in actually granting legal status to the applicants and with criminal charges measuring changes in committed offenses with some lag. The estimates in Table 2 further suggest that the effect is not persistent: two years after legalization, we fail to find any significant effect on immigrant crime. A non-significant coefficient is also found on the first lead of the "legalization treatment". Note that while establishing the precise timing of the legalization treatment *after* the amnesty is a purely empirical question, finding a significant effect (of any sign) of legalizations on current crime *before* the amnesty would be difficult to reconcile with a causal interpretation of the relationship between legalizations and immigrant crime (unless strong anticipation effects were in place). Based on the estimates presented in Table 2, in the following empirical analysis, we focus on legalizations in period $(t-1)$ and perform several tests to check the robustness of our findings.

In panel A of Table 3, we test the sensitivity of the estimated coefficient on legalizations in period $(t-1)$ to the gradual inclusion of controls and of different national and local trends.

Beginning with a specification in which we condition only on year dummies (column 1), we add regional controls in column 2 (log of total native population, log of GDP per capita, unemployment rate) and the (log of the) documented immigrants share in column 3. We follow the same pattern in columns 4-6, but we now include in the first-differenced equation dummies for the four Italian macro-areas (North-West, North-East, Central, South) to allow for macro-area linear trends. Finally, in columns 7-9, we allow for any trend at the macro-area level by conditioning on a full set of interactions between year dummies and macro-area dummies. In all regressions, standard errors are clustered at the regional level. The estimated coefficient on the "legalization treatment" in *(t-1)* oscillates around minus 0.03 and is significant across all specifications.[25] In Appendix Table A 2, we show OLS estimates for the other regional controls that we included in this set of regressions. Controls and trends are added to the specification following the same pattern as in Table 3. We find positive and significant coefficients on both the regional unemployment rate and documented immigrant share. The first relationship suggests that worse economic conditions increase immigrants' propensity to engage in crime, and this is consistent with a standard criminal choice model *à-la-Becker* and with the fact that immigrants are generally particularly exposed to economic downturns (Dustmann et al., 2010). The second coefficient mechanically captures the fact that having more foreign-born residents leads to having more immigrants involved in crime. Both coefficients identify economically sizeable effects.[26]

Throughout our empirical analysis, we cluster the standard errors at the regional level.

[25]Note that the estimated coefficient increases in magnitude when the documented share is included in the regressions (columns 3, 6 and 9), suggesting that the OLS estimate is upward biased when the legal migrant population is left in the error term. As discussed in section 5.2, we should expect a positive bias if both immigrant crime and the number of legalizations were positively correlated with the stock of documented immigrants.

[26]The estimated coefficient on unemployment suggests that a one-percentage-point increase in this regional control (corresponding to a nearly 10 percent increase with respect to its mean value, see Table 1) would imply a contemporaneous 5-6 percent increase in the number of non-EU immigrants receiving a criminal charge (per ten thousand population). According to the estimated coefficient on the documented immigrant share, instead, a 10 percent increase in this population would lead to a 6-8 increase in the number of non-EU immigrants receiving a criminal charge: taking these variables at their mean values in the sample would entail that approximately 18 more documented foreign-born residents in one region (per ten thousand population) would imply 0.6-0.8 more non-EU immigrants charged with a criminal offense.

While allowing for intra-region serial correlation in shocks seems the most sensible approach to get correct inference in our setting, one may worry that the small number of clusters (there are twenty regions in Italy) may lead to a downward bias in estimating the standard errors (Bertrand et al., 2004). As a matter of fact, the asymptotic justification for inference with cluster-robust standard errors assumes that the number of clusters goes to infinity. Although there is no recognized rule-of-thumb to establish when the clusters are "too few", twenty clusters may be close to that worrying threshold (although the issue is less problematic when the panel is balanced, as it is in our case; see Cameron and Miller, 2015). Cameron et al. (2008) recommend using the cluster-robust (Huber-White) variance estimator but prescribe using bootstrap when there are few clusters. In particular, they suggest using wild cluster bootstrap to improve finite-sample inference (see Cameron and Miller, 2015, for a detailed discussion of this method). In Appendix Table A 3, we report again the OLS estimates of the "legalization treatment" shown in Panel A of Table 3 (excluding those conditional on share of documented immigrants). We compare the p-values obtained by clustering the standard errors with those produced by implementing wild cluster bootstrap (using an increasing number of bootstrapping repetitions: 500, 1000, 2000 and 5000).[27] Remarkably, bootstrapped p-values are not systematically larger than those obtained from "simple" clustering (actually, they tend to be smaller in four columns out of six) and the level of significance of the estimates is identical for the two procedures in all cases. Overall, estimates in Appendix Table A 3 suggest that in our setting having twenty clusters does not affect the correctness of our inference.

In Panel B of Table 3, we use alternative measures of non-EU immigrant crime. In panel B1, the outcome is the yearly change in the number of non-EU immigrants charged with a criminal offense normalized by the total number of individuals receiving a criminal charge (rather than by the resident native population). In panel B2, we use data on the number of immigrants ar-

[27] Wild cluster bootstrap p-values are obtained using the STATA command *cgmwildboot*, written by Judson Caskey.

rested in each region by the Italian police, normalized by the total native population.[28] Regional controls and dummies for national and area trends are included following the same pattern as in Panel A of Table 3. The estimation results in panels B1 and B2 of Table 3 show that a negative and significant effect of legalizations on immigrant crime (in the following period) is identified irrespective of the measure of immigrant crime used.

In Table 4, we report IV estimates whereby we address the potential endogeneity of the "legalization treatment" by instrumenting it with a predicted number of legalizations. As explained in section 5.2, our main instrument ($\widehat{A}_{rt}^{81}$) takes immigrants legalized in each amnesty from each country of origin and allocates them across regions according to the distribution of immigrants recorded in the 1981 census. Estimates obtained by using this instrument are presented in Panel A of Table 4. Panel B, instead, reports IV estimates obtained with the two alternative instruments that we discussed in section 5.2. In panel B1, the instrument is predicted legalizations based on the distribution of documented immigrants in year 1990 ($\widehat{A}_{rt}^{90}$), while in panel B2 we use regional legalizations in the 1986 amnesty interacted with amnesty year dummies ($\widehat{A}_{rt}^{86}$). Controls and trends are gradually included in the regressions following exactly the same pattern as in Table 3. Standard errors are always clustered at the regional level.

In all specifications in panel A, our main instrument is a strong predictor of the actual number of immigrants legalized, with an F-statistic (*IV: F-stat*) that is well above the rule-of-thumb value of ten for weak instruments. The IV estimates of the coefficient on the number of legalizations at time $t-1$ are all negative and significantly different from zero. They are also very similar in magnitude to the corresponding OLS estimates in panel A. Indeed, we use a cluster-robust version of the Hausman test and find little evidence of endogeneity.[29] The

[28]Note that arrest data are only available for the period 1992-2002 (leaving us with 200 observations) and do not allow to distinguish between EU and non-EU immigrants (see section 4).

[29]The standard form of the Hausman test assumes homoscedasticity and no within-group serial correlation. As we always cluster the standard errors by region, we implement a modified Hausman test that is robust to clustering, as proposed in Wooldridge (2002). We include the predicted residuals from the first-stage regression

estimates reported in Panel B of Table 4 show that the two alternative instruments we propose are equally strong predictors of the number if immigrants legalized. Irrespective of the instrument used, however, the IV coefficients reported in this panel are generally significant and very similar to the IV estimates reported in panel A of the same table. In all cases, the Hausman test suggests no clear evidence of endogeneity of the "legalization treatment".[30]

The estimates presented in this section unambiguously confirm that legalizing undocumented immigrants produces a crime-reducing effect in the year following the amnesty. Before proposing amnesties as an effective response to immigrant crime, however, we need to discuss the magnitude of the effect that we have identified. The estimated coefficient on the "legalization treatment" implies that a one-percent increase in the number of legalizations (per ten thousand population) leads to an approximately 0.03 percent reduction in the ratio of immigrants charged in year t over those charged in year $t-1$.[31] A more intuitive interpretation of our results is obtained by taking our variables at their mean values and computing the impact of legalizing, for instance, ten more immigrants per ten thousand population. According to Table 1, increasing A_{rt}/Pop_{rt} by ten units corresponds to an approximately 80 percent increase with respect to its mean value (12.1) and to a change equal to roughly one third of its within-region standard deviation (28.8). In a region where immigrant crime is at its mean value (10.7 immigrants charged per ten thousand population; see Table 1), legalizing ten more immigrants (per ten thousand population) would lead to having nearly 0.24 fewer immigrants charged with criminal offenses (per ten thousand population). The average yearly change in the number of

as an additional control in the main OLS equation and test whether the estimated coefficient on this variable is significantly different from zero. See Cameron and Miller (2015) for details. In all columns, the p-values reported in Panel A of Table 4 (*IV: Hausman test: p-value*) imply that we fail to reject the null hypothesis of the exogeneity of the "legalization treatment".

[30] IV estimates for the two alternative measures of immigrant crime - the share of non-EU immigrants receiving a criminal charge over the total number of individuals charged and the the number of immigrants arrested (per ten thousand native population) - are reported in Appendix Table A 4. We use our main instrument, $\widehat{A}^{81}_{rt}$). These IV estimates are very similar to the OLS estimates reported in panel B of Table 3. The Hausman test points at the presence of endogeneity for immigrants arrested (Panel B).

[31] Note that we are using a log-log specification, but the dependent variable is taken in differences while the main outcome is in levels (see equation 5).

immigrants charged in our sample is 1.2 (per ten thousand population): a region that legalized ten more immigrants (per ten thousand population) would therefore experience a yearly change in immigrant crime that is 20 percent smaller than the average change in our sample. The effect is economically small although not completely negligible.

6.2 Further Results and Robustness Checks

Heterogeneity across areas. The fact that more legalizations in one region decrease immigrant crime suggests that obtaining legal status may reduce the incentives to engage in crime by opening up better employment opportunities for immigrants in the formal sector. This effect should thus be stronger in areas where formal labor markets are more developed and more capable of absorbing new workers. Italy is characterized by a deep and persistent divide between its northern and southern regions, with the latter having higher levels of unemployment, lower growth rates and a relatively larger shadow economy.[32] In Appendix Table A 5, we investigate whether the effect of legalizations we identify is heterogenous across different areas of Italy. We report OLS estimates conditional on year and area dummies (columns 1 and 4) and on the full set of interactions between year dummies and area dummies (columns 2-3 and 5-6). Regional controls are included in columns 3 and 6. In columns 1-3, we interact the "legalization treatment" with a dummy for the northern region and with a dummy for the central and southern regions: the estimated coefficients are both negative and similar in magnitude, but the effect is only significant in the North of Italy. In columns 4-6, we include interactions of legalizations with a dummy for each of the four Italian macro-areas. Estimated coefficients are negative and significant in the North-Western and North-Eastern regions. In the Central and South regions, there is no significant impact of legalizations on immigrant crime, although the point estimates are quite large in the South. Mastrobuoni and Pinotti (2015) report similar

[32]Over the period we analyze in this paper, for instance, the average unemployment rate was 4.5 percent in the North-East, 6.3 in the North-West, 7.7 percent in Central Italy and 17.5 in the South and Islands. Average GDP per capita was 16, 15.7, 13.7 and 9.2 thousand euros, respectively. The estimated shares of shadow employment were 9, 10, 13 and 22 percent, respectively.

results, finding an effect of legal status on re-incarceration rates only in the North of Italy.

Natives and EU immigrants. Table 5 reports estimation results of the impact of amnesties on criminal charges against Italian citizens (Panel A) and EU immigrants (Panel B). Over the period we study, natives accounted for the largest share of individuals charged (87 percent) while EU immigrants accounted for only one percent of them (see section 4). OLS estimates are reported in columns 1-3 while IV estimates (using our main instrument $\widehat{A}_{rt}^{81}$) are reported in columns 4-6. All regressions include regional controls. As in the previous tables, we control for national and regional trends by including year dummies (columns 1 and 4), year and area dummies (columns 2 and 5) and a full set of interactions of year and area dummies (columns 3 and 6). As the residence status of Italian and EU citizens is not affected by amnesties, these two population groups should not be directly affected by the legalization programs. This set of estimates can thus be interpreted as a falsification exercise: it would be quite puzzling to find that regions where a larger number of non-EU immigrants obtained legal status experienced a reduction in crime committed by these other two population groups. Indirect effects, however, may take place. If amnesties reduce the crime supply but leave crime demand unaffected, (at least some of) the criminal opportunities that are no longer taken by the legalized immigrants could be seized by other groups of potential offenders. The estimation results reported in Table 5 show no significant effect of legalizations on the number of individuals who received a criminal charge among these two populations. The coefficients are all close to zero, and none of them is significant. In summary, we do not find any crime-reducing effect of the legalization treatment on these two population groups that were not directly affected by the amnesties, nor do we find any empirical evidence of an indirect positive effect on their involvement in crime.

Total offenses. The findings presented in the previous paragraph imply that there was no substitution in the criminal market across different population groups. If the reduction in non-EU immigrant crime was not matched by a change of the opposite sign in the crime rate of EU immigrants and natives, we should expect to observe a decline in overall crime rates. In Table 6, we test this conjecture by estimating the impact of legalizations on total offenses rather than on the number of individuals receiving a criminal charge. From the same Ministry of Justice dataset, we can observe the total number of crimes committed in the region in which the criminal prosecution was initiated, including cases in which the offender is still unknown (see section 4). As with the previous outcomes, we normalize the number of offenses by total native population (in tens of thousands) and we take logs. OLS (Panel A) and IV estimates (Panel B) are reported in Table 6. Each sub-panel B reports a different set of IV estimates obtained using an alternative instrument for legalizations: $\widehat{A}_{rt}^{81}$ in Panel B1, $\widehat{A}_{rt}^{90}$ in Panel B2 and $\widehat{A}_{rt}^{86}$ in Panel B3. Controls and trends are gradually included in the regressions following exactly the same pattern as in Table 3. All OLS coefficients are negative, although not significant. The IV estimates are slightly larger in size and some of them - especially when year times macro-area dummies are included - are significant or marginally significant. This is true independently of the particular instrument used. Although the coefficients are imprecisely estimated, these estimates suggest an overall crime-reducing effect of amnesties.[33] The IV coefficient on the "legalization treatment" is approximately minus 0.01. To understand the magnitude of this effect, we can perform the same calculations as we did when assessing the impact on immigrant crime. A region that legalized ten more immigrants (per ten thousand population) - an increase roughly corresponding to 80 percent of the mean value of the legalization variable - would experience a 1 percent reduction in total offenses or 4.5 fewer offenses with respect to the mean value (that is 454.8; see Table 1). As expected from the fact that the effect on immigrant crime is economically small, the resulting effect on total offenses is also quite small. In unreported regressions, we find that the effect is also not persistent.

7 Conclusions

In this paper, we test whether amnesty programs - that repeatedly granted legal status to large shares of the undocumented immigrant population in Italy - led to significant reductions in

[33]Note that in column 6, the Hausman test rejects the null hypothesis of exogeneity of the legalization treatment.

the immigrant crime rate. Our empirical analysis identifies a small and not persistent crime-reducing effect of legalizations. When interpreting the magnitude of the effect we identify, one must bear in mind that our main dependent variable is total (non-EU) immigrant crime: if undocumented and/or documented immigrants increase their propensity to commit crime in response to the reduction in criminal activities of newly legalized immigrants, we would find a small effect of amnesties on aggregate immigrant crime even if the effect on those who obtained legal status were substantial. Moreover, the arrival of new inflows of undocumented immigrants may reduce the persistence and even nullify the potential crime-reducing impact of amnesty. In the Italian context, where amnesties have been frequently and repeatedly adopted and where the existing estimates of the undocumented immigrant stock in Italy suggest that this population rapidly regenerated after each amnesty, this is undoubtedly an important explanation for our findings.

Our results suggest that, although there may be many other good reasons to grant legal residence status to unauthorized residents, policy-makers can hardly use the argument that mass legalizations produce an economically important crime-reducing effect. Rather than enacting one-off national programs, however, offering permanent opportunities for legalization for individuals satisfying certain criteria may be more effective in achieving the desired reduction in immigrant crime. Legalizations decided on an individual basis, indeed, may reduce the propensity to engage in crime of legalized individuals without generating sudden inflows of new unauthorized entrants. Moreover, individual legalizations do not generate the general equilibrium effects that a massive program implies and that may damage the economic prospects of immigrants who fail to obtain legal status (inducing them to engage in more crime). The disadvantage of these permanent schemes, however, is creating the expectation among immigrants that becoming a legal resident is possible for undocumented immigrants, potentially generating more unauthorized entries and longer residence duration among undocumented immigrants. Repeated and frequent amnesties have similar - and likely stronger - disadvantages and should therefore be considered the least desirable policy option in this regard.

References

Altonji, J. J. and Card, D. (1991), The effects of immigration on the labor market outcomes of less-skilled natives, *in* J. M. Abowd and R. B. Freeman, eds, 'Immigration, Trade and Labor', University of Chicago Press.

Baker, S. (2015), 'Effects of immigrant legalization on crime', *American Economic Review - Papers and Proceedings* **105**(5), 210–13.

Barbagli, M. and A., C. (2011), Rapporto sulla criminalit e la sicurezza in italia 2010, Technical report, Italian Ministry of Internal Affairs.

Barbarino, A. and Mastrobuoni, G. (2014), 'The Incapacitation Effect of Incarceration: Evidence from Several Italian Collective Pardons', *American Economic Journal: Economic Policy* **6**(1), 1–37.

Bartel, A. P. (1989), 'Where do the new u.s. immigrants live?', *Journal of Labor Economics* **7**(4), 371–91.

Becker, G. S. (1968), 'Crime and punishment: An economic approach', *The Journal of Political Economy* **76**(2), 169–217.

Bell, B., Fasani, F. and Machin, S. (2013), 'Crime and immigration: Evidence from large immigrant waves', *The Review of Economics and Statistics* **95**(4), 1278–1290.

Bertrand, M., Duflo, E. and Mullainathan, S. (2004), 'How Much Should We Trust Differences-In-Differences Estimates?', *The Quarterly Journal of Economics* **119**(1), 249–275.

Bianchi, M., Buonanno, P. and Pinotti, P. (2012), 'Do immigrants cause crime?', *Journal of the European Economic Association* **10**(6), 1318–1347.

Bohn, S., Freedman, M. and Owens, E. (2015), 'The criminal justice response to policy interventions: Evidence from immigration reform', *American Economic Review - Papers and Proceedings* **105**(5), 214219.

Borjas, G. J., Grogger, J. and Hanson, G. H. (2010), 'Immigration and the Economic Status of African-American Men', *Economica* **77**(306), 255–282.

Butcher, K. F. and Piehl, A. M. (1998*a*), 'Cross-city evidence on the relationship between immigration and crime', *Journal of Policy Analysis and Management* **17**(3), 457–493.

Butcher, K. F. and Piehl, A. M. (1998*b*), 'Recent immigrants: Unexpected implications for crime and incarceration', *Industrial and Labor Relations Review* **51**(4), 654–679.

Cameron, A. C., Gelbach, J. B. and Miller, D. L. (2008), 'Bootstrap-Based Improvements for Inference with Clustered Errors', *The Review of Economics and Statistics* **90**(3), 414–427.

Cameron, A. C. and Miller, D. L. (2015), 'A practitioner's guide to cluster-robust inference', *Journal of Human Resources* **50**(2), 317–373.

Casarico, A., Facchini, G. and Frattini, T. (2012), Spending More is Spending Less: Policy Dilemmas on Irregular Migration, Development Working Papers 330, Centro Studi Luca dÁgliano, University of Milano.

Chalfin, A. (2014), 'What is the contribution of mexican immigration to u.s. crime rates? evidence from rainfall shocks in mexico', *American Law and Economics Review* **16**(1), 220–268.

Devillanova, C., Fasani, F. and Frattini, T. (2014), Employment of Undocumented Immigrants and the Prospect of Legal Status: Evidence from an Amnesty Program, CReAM Discussion Paper Series 1415, Centre for Research and Analysis of Migration (CReAM), Department of Economics, University College London.

Dustmann, C., Fasani, F. and Speciale, B. (n.d.), 'Illegal migration and consumption behavior of immigrant households', *Journal of European Economic Association* .

Dustmann, C., Frattini, T. and Preston, I. P. (2013), 'The effect of immigration along the distribution of wages', *The Review of Economic Studies* **80**(1), 145–173.

Dustmann, C., Glitz, A. and Vogel, T. (2010), 'Employment, wages, and the economic cycle: Differences between immigrants and natives', *European Economic Review* **54**(1), 1–17.

Fasani, F. (2009), A country report on undocumented migration in italy, Technical report, CLANDESTINO - Undocumented Migration: Counting the Uncountable - Data and Trends Across Europe.

Fasani, F. (2010), The quest for "la dolce vita"? undocumented migration in italy, *in* A. Triandafyllidou, ed., 'Irregular Migration in Europe: Myths and Realities', Ashgate, chapter 9, pp. 167–186.

Fasani, F. (2015), 'Understanding the role of immigrants legal status: Evidence from policy experiments', *CESifo Economic Studies* **61**(3-4), 722–763.

Freedman, M., Owens, E. and Bohn, S. (2014), Immigration, employment opportunities, and criminal behavior.

ISTAT (2006), Gli stranieri e il carcere: aspetti della detenzione, Technical report, ISTAT.

ISTAT (2012), I detenuti nelle carceri italiane - anno 2011, Technical report, ISTAT.

Kerwin, D. M. (2010), More than irca: Us legalization programs and the current policy debate, Policy brief, Migration Policy Institute.

Mastrobuoni, G. and Pinotti, P. (2015), 'Legal status and the criminal activity of immigrants', *American Economic Journal: Applied Economics* **7**(2), 175–206.

Miles, T. J. and Cox, A. B. (2014), 'Does immigration enforcement reduce crime? evidence from secure communities', *Journal of Law and Economics* **57**(4), 937 – 973.

Munshi, K. (2003), 'Networks in the modern economy: Mexican migrants in the u. s. labor market', *The Quarterly Journal of Economics* **118**(2), 549–599.

Nunziata, L. (2015), 'Immigration and crime: evidence from victimization data', *Journal of Population Economics* **28**(3), 697–736.

Orrenius, P. and Zavodny, M. (2003), 'Do amnesty programs reduce undocumented immigration? Evidence from Irca', *Demography* **40**(3), 437–450.

Pinotti, P. (2015), Clicking on heaven's door: The effect of immigrant legalization on crime.

Piopiunik, M. and Ruhose, J. (2015), Immigration, regional conditions, and crime: Evidence from an allocation policy in germany, IZA Discussion Papers 8962, Institute for the Study of Labor (IZA).

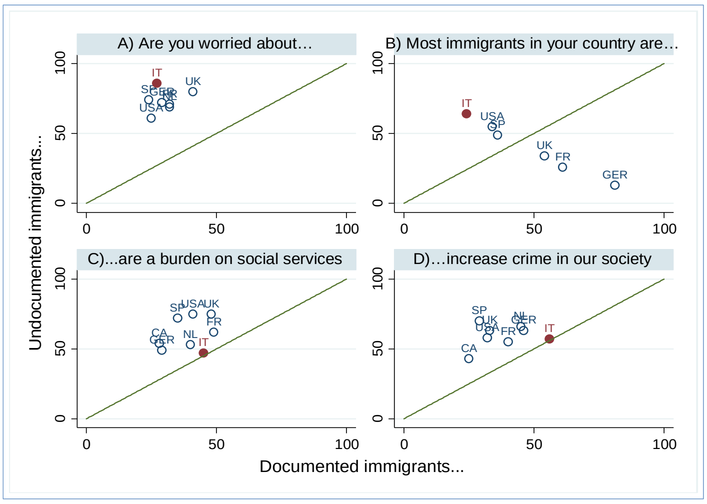
Spenkuch, J. L. (2014), 'Understanding the impact of immigration on crime', *American Law and Economics Review* **16**(1), 177–219.

TTS (2014), Transatlantic trends: mobility, migration and integration, Technical report, German Marshall Fund.

Wooldridge, J. (2002), *Econometric analysis of cross s ection and panel data*, The MIT press.

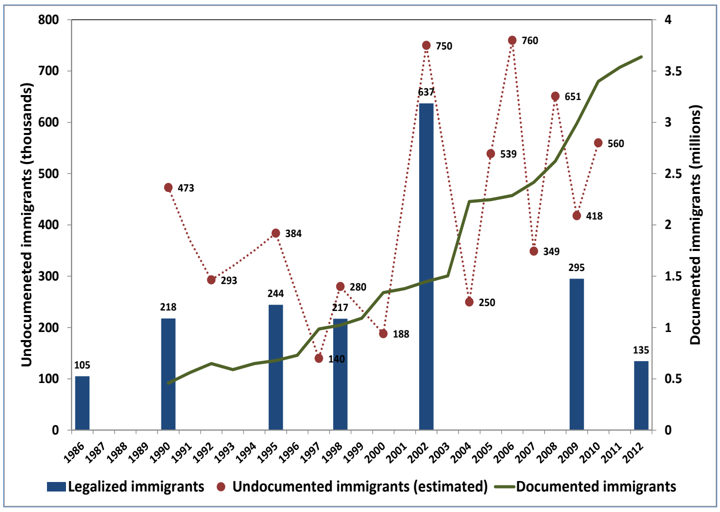
Figures

Figure 1: **Concern About Documented and Undocumented Immigrants in Selected OECD Countries (Share of Respondents Agreeing with Each Statement)**



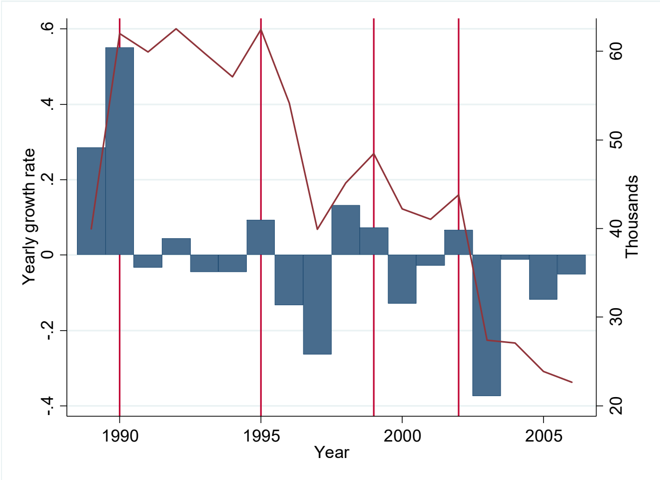
Source: Transatlantic Trends - Immigration 2010, 2011 and 2013.

Figure 2: **Documented Immigrants, Undocumented Immigrants and Amnesties in Italy (Years 1986-2012)**



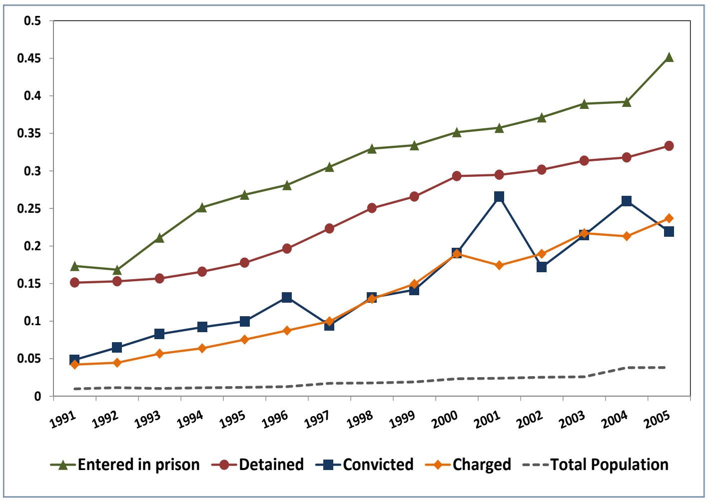
Note: the continuous line shows the stock of documented immigrants, as measured with the number of residence permits (right vertical axis); the dots are estimates of the stock of undocumented immigrants produced by ISMU (left vertical axis); the bars report the number of immigrants legalized in each amnesty program (left vertical axis). Source: elaborations from ISTAT and ISMU data.

Figure 3: **Foreign Citizens Refused Entry at the Border (Years 1986-2006)**



Note: the continuous line reports the total annual number of foreign citizens refused entry at the Italian border for being undocumented (right vertical axis); the bars report yearly growth rates in people refused entry at the border (left vertical axis); the vertical lines identify the four amnesties (1990, 1995, 1998, 2002) that took place in this period of time. Source: elaborations from data of the Italian Minister of Internal Affairs.

Figure 4: **Share of Foreign Born Population at Different Stages of the Italian Criminal Justice Process (Years: 1991-2005)**



Note: Elaborations from data of the Italian Minister of Internal Affairs and Minister of Justice.

Tables

Table 1: **Descriptive Statistics**

	Mean	Std. Dev. Overall	Std. Dev. Within	Min	Max	Obs	Regions	Years
Panel A: Crime								
Non-EU Immigrants Charged (per 10 thousand pop)	10.72	9.23	6.34	0.16	43.88	300	20	15
Share of Non-EU Immigrants Charged (over tot individuals charged)	0.12	0.10	0.07	0.00	0.36	300	20	15
EU Immigrants Charged (per 10 thousand pop)	1.02	0.90	0.75	0.00	5.08	300	20	15
Share of EU Immigrants Charged (over tot individuals charged)	0.01	0.01	0.01	0.00	0.05	300	20	15
Italian Citizens Charged (per 10 thousand pop)	83.31	35.69	24.18	27.15	290.73	300	20	15
Share of Italian Citizens Charged (over tot individuals charged)	0.87	0.10	0.07	0.62	0.99	300	20	15
Total Offences (leading to prosecution - per 10 thousand pop)	454.87	173.46	84.58	196.10	1165.42	300	20	15
Immigrants Arrested (per 10 thousand pop)	8.45	18.33	17.55	0.16	119.24	200	20	10
Panel B: Amnesties								
Immigrants Legalized (per 10 thousand pop)	12.10	29.66	28.80	0.00	229.56	300	20	15
Immigrants Legalized (per 10 thousand pop) - only amnesty years	45.39	42.42	32.91	7.08	229.56	80	20	4
Panel C: Regional controls								
Resident Native Population (millions)	2.82	2.22	0.03	0.11	8.96	300	20	15
GDP pc (constant 1990 euro-equivalent prices - thousands)	12.74	3.18	0.81	7.00	18.08	300	20	15
Unemployment rate	10.77	6.72	1.95	2.50	28.30	300	20	15
Documented Immigrants (per 10 thousand pop)	184.98	142.65	99.25	22.67	671.58	300	20	15

Notes: *elaborations on data from the Italian Ministry of Justice, the Ministry of Internal Affairs and the National Institute of Statistics (Istat). Period: 1991-2005.*

Table 2: **Non-EU Immigrants' Crime and Legalizations: Timing of the Effect (OLS Estimates)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
ln (Imm Legalized/Nat Pop)(t)	0.005				0.022	0.024	0.024	0.028	0.028
	[0.024]				[0.026]	[0.027]	[0.023]	[0.028]	[0.027]
ln (Imm Legalized/Nat Pop)(t-1)		-0.032**			-0.042*	-0.046**	-0.045*	-0.042*	-0.050**
		[0.014]			[0.020]	[0.021]	[0.023]	[0.024]	[0.022]
ln (Imm Legalized/Nat Pop)(t-2)			-0.003				-0.002	0.006	0.012
			[0.023]				[0.018]	[0.024]	[0.022]
ln (Imm Legalized/Nat Pop)(t+1)				-0.016				-0.017	-0.016
				[0.023]				[0.037]	[0.037]
Δ Regional controls						X	X	X	X
Δ ln (Doc Imm / Nat Pop)									X
Year dummies	X	X	X	X	X	X	X	X	X
Observations	300	300	300	300	300	300	300	300	300
R-squared	0.27	0.27	0.27	0.27	0.28	0.30	0.30	0.30	0.319

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges against non-EU immigrants (per ten thousand population) on the number of immigrants legalized (per ten thousand population) in different amnesty program (1990, 1995, 1998 and 2002) and other controls. We gradually include the current number of legalizations, its first and second lag and its first lead. All regressions include a full set of year dummies (1991-2005). Regional controls are: log of total native population (in t-1), unemployment rate and GDP per capita (columns 6-9). The share of documented immigrants (per ten thousand native population) is included in column 9. Standard errors are clustered at the regional level (20 regions): *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Table 3: **Non-EU Immigrants' Crime: OLS estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Panel A: Δ ln (Non-EU Imm Charged/Nat Pop)						
ln (Imm Legalized/Nat Pop)(t-1)	-0.032**	-0.035**	-0.041***	-0.029*	-0.032**	-0.037**	-0.026*	-0.026*	-0.038**
	[0.014]	[0.014]	[0.014]	[0.014]	[0.015]	[0.014]	[0.014]	[0.015]	[0.016]
Observations	300	300	300	300	300	300	300	300	300
			Panel B: Alternative Outcomes						
			Panel B1: Δ ln (Non-EU Imm Charged / Tot Charged)						
ln (Imm Legalized/Nat Pop)(t-1)	-0.031*	-0.035**	-0.040**	-0.028*	-0.032**	-0.037**	-0.031**	-0.031**	-0.041**
	[0.015]	[0.016]	[0.015]	[0.014]	[0.015]	[0.015]	[0.014]	[0.015]	[0.016]
Observations	300	300	300	300	300	300	300	300	300
			Panel B2: Δ ln (Imm Arrested / Nat Pop)						
ln (Imm Legalized/Nat Pop)(t-1)	-0.039**	-0.038**	-0.039**	-0.037*	-0.038**	-0.038**	-0.035*	-0.037**	-0.037*
	[0.017]	[0.016]	[0.017]	[0.019]	[0.018]	[0.018]	[0.018]	[0.017]	[0.019]
Observations	200	200	200	200	200	200	200	200	200
Δ Regional controls		X	X		X	X		X	X
Δ ln (Doc Imm / Nat Pop)			X			X			X
Year dummies	X	X	X	X	X	X			
Macro-Area dummies				X	X	X			
Year x Macro-Area dummies							X	X	X

Notes: *the table reports First Differences estimates of the change in (the log of) alternative measures of non-EU immigrants' crime on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. Amnesty program were enacted in 1990, 1995, 1998 and 2002. Panel A reports OLS estimates for (the log of) criminal charges against non-EU immigrants (per ten thousand population). Panel B reports OLS estimates for two alternative measures of non-EU immigrants' crime: the share of non-EU immigrants receiving a criminal charge over the total number of individuals charged (panel B1) and the number of immigrants arrested (per ten thousand native population; panel B2). Regional controls are: log of total native population (in t-1), unemployment rate and GDP per capita (columns 2-3, 5-6 and 8-9). The share of documented immigrants (per ten thousand native population) is included in columns 3, 6 and 9. National and/or regional trends are captured by: year dummies (columns 1-3), year and macro-area dummies (columns 4-6), interactions between year and macro-area dummies (columns 7-9). Standard errors are clustered at the regional level (20 regions):* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: **Non-EU Immigrants' Crime - IV Estimates**

	1	2	3	4	5	6	7	8	9
				Panel A - Instrument: $\widehat{A}_{rt}^{81}$					
ln (Imm Legalized/Nat Pop)(t-1)	-0.022**	-0.025**	-0.030**	-0.020***	-0.023***	-0.030***	-0.020**	-0.021*	-0.031**
	[0.010]	[0.012]	[0.014]	[0.007]	[0.009]	[0.010]	[0.009]	[0.011]	[0.013]
IV: F-stat	1409	1460	1354	1112	1163	1032	2011	2146	1529
IV: Hausman test: p-value	0.419	0.438	0.414	0.435	0.486	0.505	0.610	0.705	0.568
				Panel B: alternative instruments					
				Panel B1 - Instrument: $\widehat{A}_{rt}^{90}$					
ln (Imm Legalized/Nat Pop)(t-1)	-0.032***	-0.036***	-0.037***	-0.030**	-0.033***	-0.034***	-0.027**	-0.028**	-0.035***
	[0.012]	[0.013]	[0.012]	[0.012]	[0.013]	[0.012]	[0.011]	[0.012]	[0.013]
IV: F-stat	3037	3124	2659	3171	3616	3225	6601	6498	4958
IV: Hausman test: p-value	0.991	0.859	0.286	0.761	0.647	0.265	0.715	0.587	0.352
				Panel B2 - Instrument: $\widehat{A}_{rt}^{86}$					
ln (Imm Legalized/Nat Pop)(t-1)	-0.028**	-0.029**	-0.033***	-0.026***	-0.027**	-0.033***	-0.026***	-0.026**	-0.034***
	[0.011]	[0.013]	[0.013]	[0.010]	[0.011]	[0.010]	[0.010]	[0.011]	[0.012]
IV: F-stat	2154	2503	2125	671.9	859.9	833.2	2063	2191	1487
IV: Hausman test: p-value	0.637	0.458	0.413	0.714	0.538	0.532	0.966	0.977	0.567
Δ Regional controls		X	X		X	X		X	X
Δ ln (Doc Imm / Nat Pop)			X			X			X
Year dummies (1991-2005)	X	X	X	X	X	X			
Macro-Area dummies				X	X	X			
Year x Macro-Area dummies							X	X	X
Observations	300	300	300	300	300	300	300	300	300

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges against non-EU immigrants (per ten thousand population) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. Amnesty program were enacted in 1990, 1995, 1998 and 2002. Each panel reports a different set of IV estimates where the legalization variable is instrumented with an alternative measure of predicted legalizations. In panel A, predicted legalizations are based on the distribution of immigrants in 1981 (by nationality) and the total number of immigrants legalized in each of the following amnesties ($\widehat{A}_{rt}^{81}$). In panel B1, we use the distribution of documented immigrants in year 1990 (by nationality) and the total number of immigrants legalized ($\widehat{A}_{rt}^{90}$). In panel B2, we use regional legalizations in the 1986 amnesty interacted with amnesty year dummies ($\widehat{A}_{rt}^{86}$). In each panel, the F-statistic for the excluded instruments ($IV : F - stat$) and the p-value for a cluster-robust version of the Hausman test ($IV : Hausman\ test : p - value$) are reported below the corresponding IV estimates. Regional controls and dummies for national and/or regional trends are as in Table 3. Standard errors are clustered at the regional level (20 regions):* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: **Criminal Charges Against Natives and EU Immigrants**

	1 **OLS**	**2** **OLS**	**3** **OLS**	**4** **IV**	**5** **IV**	**6** **IV**
	Panel A: Δ ln (Nat Charged/Nat Pop)					
ln (Imm Legalized/Nat Pop)(t-1)	0.002	0.003	0.007	-0.001	-0.002	-0.002
	[0.007]	[0.007]	[0.005]	[0.008]	[0.008]	[0.007]
Observations	300	300	300	300	300	300
IV: F-statistics				1460	1163	2146
IV: Hausman test: p-value				0.373	0.174	0.00126
	Panel B: Δ ln (EU Imm Charged/Nat Pop)					
ln (Imm Legalized/Nat Pop)(t-1)	-0.011	-0.004	-0.005	-0.005	0.001	-0.003
	[0.021]	[0.021]	[0.021]	[0.021]	[0.020]	[0.019]
Observations	294	294	294	294	294	294
IV: F-statistics				1422	1229	1970
IV: Hausman test: p-value				0.501	0.657	0.817
Δ Regional controls	X	X	X	X	X	X
Year dummies (1991-2005)	X	X		X	X	
Macro-Area dummies		X			X	
Year x Macro-Area dummies			X			X

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges (per ten thousand native population) against natives (panel A) and EU immigrants (panel B) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. OLS and IV estimates are reported in columns 1-3 and in columns 4-6, respectively. In columns 4-6, the legalization variable is instrumented with a predicted number of legalizations based on the distribution of immigrants in 1981 (by nationality) and the total number of immigrants legalized in each of the following amnesties* ($\widehat{A}^{81}_{rt}$). *In each panel, the F-statistic for the excluded instrument* ($IV : F-stat$) *and the p-value for a cluster-robust version of the Hausman test* ($IV : Hausman\ test : p-value$) *are reported below the number of observations. Regional controls (log of total native population in t-1, unemployment rate and GDP per capita) are included in all regressions. National and/or regional trends are captured by: year dummies (columns 1 and 4), year and macro-area dummies (columns 2 and 5), interactions between year and macro-area dummies (columns 3 and 6). Standard errors are clustered at the regional level (20 regions): **** $p < 0.01$, **** $p < 0.05$, *** $p < 0.1$.

Table 6: **Total Offences and Legalizations**

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
					Panel A: OLS				
ln (Imm Legalized/Nat Pop)(t-1)	-0.006	-0.009	-0.010	-0.006	-0.008	-0.010	-0.007	-0.008	-0.010
	[0.006]	[0.006]	[0.006]	[0.006]	[0.007]	[0.007]	[0.006]	[0.006]	[0.006]
					Panel B: IV				
					Panel B1 - Instrument: $\widehat{A}_{rt}^{81}$				
ln (Imm Legalized/Nat Pop)(t-1)	-0.006	-0.008*	-0.010**	-0.006	-0.008	-0.010*	-0.007	-0.009*	-0.011*
	[0.004]	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]	[0.006]
					Panel B2 - Instrument: $\widehat{A}_{rt}^{90}$				
ln (Imm Legalized/Nat Pop)(t-1)	-0.007	-0.009	-0.009	-0.007	-0.009	-0.009	-0.008*	-0.010**	-0.011**
	[0.005]	[0.006]	[0.006]	[0.005]	[0.006]	[0.006]	[0.005]	[0.005]	[0.005]
					Panel B3 - Instrument: $\widehat{A}_{rt}^{86}$				
ln (Imm Legalized/Nat Pop)(t-1)	-0.009	-0.011*	-0.012**	-0.008	-0.010*	-0.012*	-0.009*	-0.010**	-0.012**
	[0.005]	[0.006]	[0.005]	[0.006]	[0.006]	[0.006]	[0.005]	[0.005]	[0.005]
D. Regional controls		X	X		X	X		X	X
D. ln (Doc Imm / Nat Pop)			X			X			X
Year dummies (1991-2005)	X	X	X	X	X	X			
Macro-Area dummies				X	X	X			
Year x Macro-Area dummies							X	X	X
Observations	300	300	300	300	300	300	300	300	300

Notes: *the table reports First Differences estimates of the change in (the log of) total number of offences (per ten thousand native population) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. OLS estimates are reported in Panel A and IV estimates in Panel B. Each sub-panel B reports a different set of IV estimates obtained using an alternative instrument for legalizations:* $\widehat{A}_{rt}^{81}$ *in Panel B1,* $\widehat{A}_{rt}^{90}$ *in Panel B2 and* $\widehat{A}_{rt}^{86}$ *in Panel B3. Regional controls and dummies for national and/or regional trends are as in Table 3. Standard errors are clustered at the regional level (20 regions):* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Tables

Table A 1: **Number of Immigrants Legalized in Each Amnesty Program: First 20 Nationalities (1990, 1995, 1998 and 2002)**

	Amnesty Program							
	1990		1995		1998		2002	
Ranking:	**country**	**#**	**country**	**#**	**country**	**#**	**country**	**#**
1	Morocco	48670	Morocco	34258	Albania	38996	Romania	134909
2	Tunisia	26318	Albania	29724	Romania	24098	Ukraine	101651
3	Senegal	15966	Philippines	21406	Morocco	23850	Albania	47763
4	Philippines	13684	China	14437	China	16778	Poland	30021
5	Jugoslavia	8924	Peru	12753	Senegal	10727	Moldova	29471
6	China	8580	Romania	11099	Egypt	9467	Bulgaria	8305
7	Egypt	7632	Tunisia	10362	Nigeria	7354	Russia	5868
8	Ghana	6517	Senegal	9889	Philippines	6696	China	33950
9	Poland	5366	Jugoslavia	9173	Bangladesh	6689	India	13399
10	Sri Lanka	5258	Egypt	8174	Pakistan	6592	Bangladesh	10687
11	Somalia	4912	Nigeria	7993	Jugoslavia	5908	Philippines	9821
12	Pakistan	4510	Poland	7926	Tunisia	5565	Pakistan	9649
13	Bangladesh	3861	Algeria	7505	Ecuador	5178	Sri Lanka	7030
14	Mauritius	3314	Sri Lanka	6993	Poland	5077	Morocco	48174
15	Nigeria	3308	Bangladesh	6162	Peru	4960	Egypt	15470
16	India	2819	Ghana	5936	India	4697	Senegal	12372
17	Brazil	2809	India	5623	Ghana	4531	Tunisia	8843
18	Albania	2471	Pakistan	4499	Sri Lanka	4090	Nigeria	5884
19	Argentina	2459	Ivory Coast	3068	Algeria	3286	Ecuador	34292
20	Iran	2327	Brazil	2520	Ukraine	2005	Peru	16213
First 20 countries		179705		219500		196544		583772
Total		217626		244492		217124		646829

Notes: *the table reports the number of immigrants legalized in each amnesty program (1990, 1995, 1998 and 2002) for the first 20 nationalities. Source: Ministry of Internal Affairs records.*

Table A 2: **Non-EU Immigrants' Crime - Full Specification - OLS Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln (Imm Legalized/Nat Pop)(t-1)	-0.032**	-0.035**	-0.041***	-0.029*	-0.032**	-0.037**	-0.026*	-0.026*	-0.038**
	[0.014]	[0.014]	[0.014]	[0.014]	[0.015]	[0.014]	[0.014]	[0.015]	[0.016]
Δ ln (Nat Pop)		1.510	3.235		1.429	3.838		2.098	6.619
		[3.951]	[4.494]		[3.673]	[4.326]		[3.691]	[5.492]
Δ ln (GDP per capita)		2.565	2.916		2.368	2.816		0.984	1.994
		[1.795]	[1.886]		[1.804]	[1.913]		[2.257]	[2.613]
Δ Unempl rate		0.053*	0.056*		0.055*	0.059*		0.046	0.056
		[0.028]	[0.028]		[0.029]	[0.029]		[0.034]	[0.036]
Δ ln (Doc Imm / Nat Pop)			0.581*			0.635*			0.830**
			[0.328]			[0.335]			[0.390]
Year dummies	X	X	X	X	X	X			
Macro-Area dummies				X	X	X			
Year x Macro-Area dummies							X	X	X
Observations	300	300	300	300	300	300	300	300	300

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges against non-EU immigrants (per ten thousand population) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. Amnesty program were enacted in 1990, 1995, 1998 and 2002. The specifications are identical to those reported in Panel A of Table 3. Standard errors are clustered at the regional level (20 regions):* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A 3: **Non-EU Immigrants' Crime: OLS estimates - Clustering Standard Errors and Wild Cluster Bootstrapping**

		(1)	(2)	(3)	(4)	(5)	(6)
ln (Imm Legalized/Nat Pop)(t-1)		-0.032	-0.035	-0.029	-0.032	-0.026	-0.026
		[0.014]	[0.014]	[0.014]	[0.015]	[0.014]	[0.015]
Δ Regional controls			X		X		X
Year dummies		X	X	X	X		
Macro-Area dummies				X	X		
Year x Macro-Area dummies						X	X
p-values clustered standard errors		0.028**	0.026**	0.059*	0.048**	0.088*	0.098*
p-values wild cluster bootstrap	**# replications:**						
	500	0.048**	0.048**	0.052*	0.04**	0.052*	0.076*
	1000	0.038**	0.042**	0.046**	0.034**	0.062*	0.094*
	2000	0.04**	0.043**	0.043**	0.033**	0.065*	0.096*
	5000	0.041**	0.043**	0.045**	0.038**	0.06*	0.095*
Observations		300	300	300	300	300	300

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges against non-EU immigrants (per ten thousand population) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. The specifications are identical to those reported in columns 1, 2, 4, 5, 7 and 8 in Panel A of Table 3.* $p-values$ *clustered standard errors are obtained by clustering the standard errors at the regional level (20 regions).* $p-values$ *wild cluster bootstrap are obtained by implementing wild cluster bootstrap with an increasing number of bootstrapping repetitions: 500, 1000, 2000 and 5000 (the STATA command* cgmwildboot *is used). Level of significance:* *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A 4: **Alternative Measures of non-EU Immigrants' Crime - IV estimates**

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
			Panel A: Δ ln (Non-EU Imm Charged / Tot Charged)						
ln (Imm Legalized/Nat Pop)(t-1)	-0.017	-0.021	-0.025	-0.015	-0.019	-0.024**	-0.015	-0.017	-0.025*
	[0.015]	[0.016]	[0.016]	[0.012]	[0.012]	[0.012]	[0.012]	[0.013]	[0.014]
Observations	300	300	300	300	300	300	300	300	300
IV: F-statistics	1409	1460	1354	1112	1163	1032	2011	2146	1529
IV: Hausman test: p-value	0.223	0.228	0.217	0.218	0.242	0.251	0.204	0.257	0.184
				Panel B: Δ ln (Imm Arrested / Nat Pop)					
ln (Imm Legalized/Nat Pop)(t-1)	-0.028**	-0.029**	-0.029**	-0.027**	-0.029**	-0.030**	-0.025**	-0.028**	-0.028**
	[0.012]	[0.013]	[0.013]	[0.014]	[0.014]	[0.015]	[0.012]	[0.013]	[0.014]
Observations	200	200	200	200	200	200	200	200	200
IV: F-statistics	3340	3231	3169	2502	2432	2305	3769	3399	2672
IV: Hausman test: p-value	0.0553	0.0550	0.0526	0.0691	0.0919	0.0929	0.0314	0.0382	0.0314
Δ Regional controls		X	X		X	X		X	X
Δ ln (Doc Imm / Nat Pop)			X			X			X
Year dummies	X	X	X	X	X	X			
Macro-Area dummies				X	X	X			
Year x Macro-Area dummies							X	X	X

Notes: *the table reports First Differences estimates of the change in (log of) alternative measures of non-EU immigrants' crime on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. In panel A, the dependent variable is the change in (the log of) the share of non-EU immigrants receiving a criminal charge over the total number of individuals charged. In Panel B, the dependent variable is the change in (the log of) the number of immigrants arrested (per ten thousand native population). In all regressions, the legalization variable is instrumented with a predicted number of legalizations based on the distribution of immigrants in 1981 (by nationality) and the total number of immigrants legalized in each of the following amnesties ($\widehat{A}_{rt}^{81}$). In each panel, the F-statistic for the excluded instrument ($IV : F - stat$) and the p-value for a cluster-robust version of the Hausman test ($IV : Hausman\ test : p - value$) are reported below the number of observations. Regional controls and dummies for national and/or regional trends are as in Table 3. Standard errors are clustered at the regional level (20 regions): *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Table A 5: **Non-EU Immigrants' Crime - Heterogenous Effects Across Areas**

		(1)	(2)	(3)	(4)	(5)	(6)
ln (Imm Legalized/Nat Pop)(t-1)	*** North**	-0.031**	-0.024**	-0.030**			
		[0.015]	[0.010]	[0.014]			
ln (Imm Legalized/Nat Pop)(t-1)	*** Central-South**	-0.025	-0.028	-0.022			
		[0.014]	[0.030]	[0.030]			
ln (Imm Legalized/Nat Pop)(t-1)	*** North West**				-0.034**	-0.024*	-0.029*
					[0.015]	[0.012]	[0.016]
ln (Imm Legalized/Nat Pop)(t-1)	*** North East**				-0.021	-0.025*	-0.031**
					[0.016]	[0.014]	[0.015]
ln (Imm Legalized/Nat Pop)(t-1)	*** Central**				-0.024	-0.001	0.007
					[0.014]	[0.022]	[0.022]
ln (Imm Legalized/Nat Pop)(t-1)	*** South**				-0.020	-0.037	-0.032
					[0.018]	[0.038]	[0.037]
Δ Regional controls				X			X
Year dummies		X			X		
Macro-Area dummies		X			X		
Year x Macro-Area dummies			X	X		X	X
Observations		300	300	300	300	300	300

Notes: *the table reports First Differences estimates of the change in (the log of) criminal charges against non-EU immigrants (per ten thousand population) on the number of immigrants legalized (per ten thousand population) in the previous year and other controls. Amnesty program were enacted in 1990, 1995, 1998 and 2002. In column 1-3, we interact the "legalization treatment" with a dummy for northern regions and with a dummy for central-southern regions. In columns 4-6, we interact the "legalization treatment" with a dummy for each of the four Italian macro-areas. Regional controls (included in columns 3 and 6) are: log of total native population (in t-1), unemployment rate and GDP per capita. National and/or regional trends are captured by: year and macro-area dummies (columns 1 and 4) and interactions between year and macro-area dummies (columns 2-3 and 5-6). Standard errors are clustered at the regional level (20 regions): *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*