fondazione RODOLFO DEBENEDETTI

XV European Conference

Immigration Policy and Crime

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Report prepared for the XV European Conference of the
Fondazione Rodolfo Debenedetti

"*Legal and Illegal Careers*"

Caserta
June 22, 2013

* Josh Angrist provided invaluable mentoring, critical insight and enthusiastic support. We thank Alessandra Camporota, Angelo Caruso, Massimo Fasciani, Gemma De Filippo, Vincenzo Gattagrisi, Emanuele Maugeri, Antonella Misiani and Salvatore Rametta of the Italian Ministry of Interior for disclosing the data on residence permits and crime, and Giancarlo Blangiardo and Ilaria Cingottini for providing part of the data used in Chapters 3 and 4. We also thank Dimitrios Bermperoglou, Giammarco Cezza and Giulia Tagliaferri for skilful research assistance. The views expressed in this work by Marco Tonello are those of the author and do not reflect those of the Bank of Italy.

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Introduction

International flows of people are a distinctive trait of our contemporary globalized world, inasmuch as international flows of goods, services and ideas. Differently from the latter, however, migration faces a fierce opposition in most destination countries (Hatton & Williamson, 2005; Mayda, 2008). One reason for this is that natives and immigrants may compete for the same jobs or welfare programs. Indeed, foreign immigrants are sometimes blamed for hurting the labor market prospects of native workers – particularly the low-skill ones – and for imposing an additional burden on welfare expenditures (Scheve and Slaughter, 2001; Mayda, 2006; Hanson, Scheve and Slaughter, 2007; Facchini and Mayda, 2009; Ortega and Polavieja, 2012). In most countries, however, natives are far more concerned that immigrants increase crime, rather than unemployment or taxes. Indeed, fears of immigrants' involvement in crime are at the center of the public and political debate about immigration, and they have been a major reason for the rise of anti-immigrant parties in several European countries (Dinas and van Spanje, 2011).

Partly in response to such concerns, a few recent papers have examined the empirical relationship between immigration and crime. Butcher and Piehl (1998) provide the first systematic analysis in this respect, showing that immigration did not lead to a significant increase in crime across U.S. cities over the period 1980-1990; Reid et al. (2005) and Wadsworth (2010) find analogous results for more recent periods. Moving to European countries, Bianchi et al. (2012) exclude that immigration increased crime across Italian provinces, while Alonso-Borrego et al. (2012) estimate a positive relationship between immigration and crime in Spain. Finally, Bell et al. (2012) focus on two large waves of recent UK immigration, namely the late 1990s/early 2000s asylum seekers and the post-2004 inflow from EU accession countries, and find that only the former brought a significant increase in (property) crime.

This last result suggests that average estimates across geographical areas may mask significant heterogeneity in the effect of different groups of immigrants. Indeed, it is reasonable to expect that immigrants' propensity to engage in criminal activity would vary strongly with individual characteristics, such as age, gender, education, etc., in a similar way as it does for natives. Indeed, the choice-theoretic model of crime (Becker, 1968) posits that individuals choose between legitimate and illegitimate activities by comparing the expected costs and benefits of these two types of activities, which in turn depend on several individual characteristics.

Compared to natives, however, immigrants differ among themselves along an additional, important dimension: legal status.

Legal status may profoundly affect criminal behavior by changing the relative payoffs of legitimate and illegitimate activities. In most destination countries, legal status is indeed a prerequisite for working in the official economy. Therefore, unauthorized aliens may be completely excluded from legitimate economic activities or, depending on the actual enforcement of restrictions, they may be able to work just in the shadow economy. In either case, they would face worse (legitimate) income opportunities compared to legal immigrant or native workers, and thus a lower opportunity cost of crime.

The aim of the present report is to investigate, both from a theoretical and an empirical perspective, the effect of migration policy on immigrant crime. Chapter 1 motivates the analysis by presenting recent survey evidence on natives' attitudes toward immigrants in a number of countries in Northern America and Europe, as well as some stylized facts about immigrants' involvement in crime. It turns out that, in the majority of countries, natives are mostly concerned about the effects of immigration on crime and, as a matter of fact, foreigners are overly represented in the prison population.

Chapter 2 discusses the effect of legal status on the probability of committing crimes within a simple theoretical framework in which illegal immigrants suffer a wage discount and can be deported back to their home country; to obtain legal status, they can participate to a costly lottery for residence permits, or they can just hope into general amnesty for illegal immigrants. The main prediction of the model is that legal status has an ambiguous effect on the number of crimes committed by immigrants in destination countries: on the one hand, legal status raises the opportunity cost of crime for immigrants that are not deported; on the other hand, part of the illegal immigrants are actually deported, which mechanically reduces the number of crimes committed by this group, so the sign of the overall effect is ultimately an empirical issue.

The theoretical model also clarifies which is the main threat to estimating the (causal) effect of legal status on crime, namely that heterogeneous returns from legal status – for instance, by educational attainment – would induce a non-random distribution of legal status across individuals. In particular, the probability of obtaining legal status may be higher among individuals that have a lower probability of engaging in crime to start with. Therefore, voluntarily and/or involuntarily selection into legal status may bias the estimates toward finding a negative effect of legal status on crime.

An attempt to overcome these identification difficulties is proposed by Mastrobuoni and Pinotti (2012), who exploit a large-scale amnesty of prison inmates in Italy (in August 2006) and the last round of the EU enlargement (five months later, in January 2007) as a natural experiment to separately identify causality from selection. They show that the recidivism of released inmates that are citizens of newly admitted EU countries, who obtained legal status in all EU-member countries (including Italy), decreased after the EU accession from 5.8 to 2.3 percentage points over a six-months period, as compared to no change in a control group of inmates from EU-candidate member countries. This result suggests that access to legal status may significantly reduce the propensity to engage in criminal behavior, and that such effect prevails over the increase in crime driven by the stop of deportations.

The event-study approach, while allowing for internally consistent estimates of the effect of legal status, poses serious limits to the external validity of the results. In particular, the estimates capture the effect of a one-and-for-all change in legal status (as driven by the EU enlargement process) on the recidivism of a sample of former prison inmates. It is therefore unclear how they can be generalized to the rest of the immigrant population, and to differences in legal status that are induced by different migration policies.

In order to answer these questions, the present report investigates the effect of changes in legal status that are routinely induced by the current migration policy in Italy, which are extensively described in Chapter 3. In recent years, Italian migration policy has been based upon a set of migration quotas by country of origin, type of permit and province of destination. Such institutional framework is by no means specific to the Italian context, as migration policy in many destination countries (e.g. Austria, Canada and Spain) is currently based, implicitly or explicitly, on analogous quota systems. One peculiarity of the Italian system lies in the tight rationing of permits, as total quotas are always substantially lower than the number of applications for residence permits. Moreover, it is well understood that in the Italian context, the quota system is mainly used to legalize undocumented workers already resident in the country rather than to regulate entries of new workers. These facts, in addition to poor border enforcement (also due to the geographical position of Italy), has led to the formation of large pools of unauthorized entrants, and to the recurrent need for generalized amnesties of illegal immigrants.

In chapter 4 we exploit these features of Italian migration policy to estimate the effect of legal status on crime across Italian regions and provinces. Specifically, we estimate the relationship between changes in crime and the share of applicants obtaining legal status with the last four general amnesties (1991, 1995, 1998 and 2002), and with the so-called "click day" in 2007; the latter expression refers to the first year in which applications for residence permits within the quota system framework had to be sent electronically in a pre-determined period of the year. The analysis conducted on repeated amnesty programs suggests that, in the year following an amnesty, regions in which a higher share of immigrants obtained legal status experience a greater decline in immigrant crime rates, relative to the other regions. Additional evidence for the period 2006-2008 (i.e. before and after the 2007 "click day"), confirms that larger shares of legalized immigrants imply a decline in criminal activities of foreign offenders.

In chapter 5, we take advantage of the fact that the electronic applications received for the 2007 "click day" were processed on a first-come, first-served basis until exhaustion of the available quotas. Such a mechanism provides an ideal "regression discontinuity" design to estimate the effect of legal status at the individual-level. We thus matched the administrative records on applicants – including the timing of the application (at the millisecond) – with individual police files, and compared the probability of being subsequently reported for a felony between individuals that applied just before and just after the exhaustion of quotas. Our baseline estimates suggest that being denied legal status – for a matter of seconds in submitting the application – more than doublesthe probability of committing a felony for immigrants that applied for domestic-work permits, while the effect is not significantly different from zero for immigrants whose application was sponsored by a firm. One explanation for the (different) results obtained for the two groups is that immigrants that are sponsored by a firm are, in many cases, already employed there (although not officially), so they may face a substantial opportunity cost of crime even in case their application is not accepted. By contrast, applications for domestic-work permits mask, in many cases, immigrants that have no employment relationship in Italy – not even in the shadow economy – so their opportunity cost of crime may be very low in case they do not obtain legal status.

Interestingly, the relative decline in the probability of committing crimes observed for the applicants for domestic-work permits is comparable to that estimated by Mastrobuoni and Pinotti (2012). The results of the two analyses dovetail nicely also along other dimensions. In particular, both studies find that the relationship between legal status and crime is steeper in Northern Italian regions, which are characterized by better economic opportunities for legal immigrants (relative to the illegal ones).

Overall, both the aggregate and the individual-level results in chapter 4 and 5, respectively, suggest that access to legal status reduces the number of crimes committed by (formerly illegal) immigrants in Italy. Nevertheless, policies which grant legal status to undocumented workers who are already residing in the country can only be considered second-best policy interventions. Indeed, their short-term beneficial effect on crime may be completely offset by the creation of rational expectations among the undocumented immigrants that similar policies will be implemented again in the future. This clearly creates incentives for persistent unauthorized inflows of immigrants, with the negative consequences on crime which can be easily pointed out by noting that immigrants are largely overrepresented among the population of offenders and inmates in Italy and that about 70-80 percent of the foreign born offenders lack legal status (see chapter 3). Rather than ex-post legalizing unauthorized immigrants, Italy should probably steer its migration policy toward creating better possibilities and incentives for legal entry and legal participation into its labour market.

Italy's weaknesses in shaping its migration policy can be possibly justified on the grounds of its relatively limited experience in being an immigration country. A comparison with the Unites States, arguably the country with the longest experience in dealing with immigrant inflows, can be useful to think about potential paths of improvement for Italy. Indeed, although the United States share with Italy a high presence of unofficial immigrants – around 11 million, one-third of the total foreign-born population in the United States – the two countries have adopted very different institutions to regulate and deal with immigration. For instance, the United States also employ a quota system, but one that favors family-sponsored and high-skill immigration. Indeed, the most recent immigration reform proposals go as far as to hypothesize a faster path to citizenship for STEM (Science, Technology, Engineering and Mathematics) Ph.Ds. In contrast, the quota for temporary low-skill immigrants is set at 66,000 per year, and employers applying for these visas have to establish that "there are not enough U.S. workers who are able, willing, qualified, and available to do the temporary work." As shown in Chapter 1, the U.S. public is much more concerned about the effects of immigration on natives' labor market outcomes than about the possible increase in crime rates due to immigration.

In Chapter 6, we discuss how immigration enforcement in the US likely plays a role in decreasing immigrants' inflows and preventing immigration from becoming a law and order problem. For instance, removals of criminal aliens work as a deterrent for crime for those who decide to immigrate anyways. Most importantly, the U.S. government only rarely concedes amnesties: the last one was granted under the Immigration Reform and Control Act (IRCA) in 1986.

Exploiting the IRCA, Baker (2012) examines the effects of obtaining legal status on crime rates in the United States, finding that IRCA applicants are associated with higher crime rates prior to legalization, but not after legalization. In particular, much of the drop in crime can be attributed to greater job market opportunities among those legalized by IRCA. On the other hand, Freedman et al. (2013) find that immigrants to San Antonio who had arrived too late to benefit from IRCA are hurt by it, and the neighbourhoods where they reside experience increased crime rates.

We exploit a different source of exogenous variation in immigration, drawing from the Mariel Boatlift case study, and we reach similar conclusions, albeit with some caveats. The sudden arrival of 125,000 Cubans in Miami in the spring of 1980 was first analyzed in Card (1990) who, surprisingly, found no impact of the Boatlift on wages or unemployment rates of blacks in Miami. However, Angrist and Kruger (1999) dispute this result by showing that the comparison of labor market outcomes in Miami and Card's pool of control cities is likely to be flawed by city-specific labor market trends. Hence, for our analysis we take advantage of the synthetic control methodology and randomization inference developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010). We find that homicide rates increase by around 66%, and the effect persists for more than two years. Similarly, robberies increase by around 75% and motor vehicle thefts increase by around 20%. In contrast, we do not find any impact on non-violent crime.

To make sense of these numbers it is worth to consider that the Boatlift consisted in a 4% increase in Miami population, and a 7% increase in the city's workforce. Hence, if we assume that immigration effects are constant, we can extrapolate that a 1% increase in the population of a city, as negatively selected as the immigrants from the Boatlift, increases homicide rates by 10%, robberies by 18.75% and motor vehicle thefts by 5%. However, no U.S. city sees a 1% annual increase in its population due to illegal immigration, and these numbers represent a non-binding upper bound. In other words, the Mariel Boatlift represents an exception to the norm: many, negatively selected immigrants arrived all at once in a single city, whereas the U.S. immigration system focuses on attracting a few, positively selected immigrant.

The report follows the following structure. In chapter 1 we report some evidence on perceptions about immigration and the involvement of immigrants in criminal activities. Chapter 2 presents a stylized theoretical model which analysis the link between immigrant legal status and criminal choices. Chapter 3 describes the Italian institutional setting and its migration policy and sets the ground for the empirical analysis which follows. In chapter 4 and

5, we develop, respectively, an aggregate and an individual-level analysis of the effect of policies granting legal status to immigrants on their criminal behaviour. The US experience on migration experience is discussed in chapter 6. Finally, we have some concluding remarks.

Chapter 1 - Migration Policy, Legal status and Crime: Perceptions and Some Stylized Facts

Immigration is a contentious issue in most destination countries. From a purely economic perspective, the removal of barriers to labor mobility would allow for the efficient allocation of productive factors at the global level. At the same time, the distributional consequences of such re-allocation may undermine the political support for the free movement of people across countries. Most importantly, natives in destination countries may oppose immigration on other grounds than just labor market competition. In fact, recent public opinion surveys suggest that natives in destination countries are mostly concerned that immigration may bring an increase in crime.

In this chapter we first qualify such concerns: in particular, we show that natives operate a clear distinction between legal and illegal immigrants, blaming mostly the latter group for the alleged increase in crime. We then move from perceptions to facts, discussing the available evidence on the size and composition by legal status of the immigrant population in a sample of 13 destination countries, and looking at some measures of the actual involvement in crime by legal and illegal immigrants. On the one hand, it seems that natives tend to over-state the size of both legal and illegal migration, and that opposition to migration may be driven, at least in part, by such mis-perceptions; on the other hand, we show that foreigners are actually over-represented, often to a large extent, in criminal statistics. Crucially, whenever the data allow to distinguish between legal and illegal immigrants, the latter group seems mostly responsible for the high presence of foreigners among the people reported by the police.

Finally, we describe the empirical relationship between restrictions to (legal) migration and the illegality rate across destination countries. We conclude the chapter by discussing some hypotheses – which will be formalized and tested in the next chapters – about the interplay between migration policy, the size and composition of the immigrant population and criminal activity.

1.1. Attitudes toward immigrants

Transatlantic Trends is a comprehensive annual survey conducted in a number of North American and European countries on a wide range of economic and political issues. In year 2008 an ad-hoc module on immigration was introduced, which covers multiple aspects of the current debate about issues such as the integration of immigrants, their impacts on the labor market of the host countries, preferences about migration policy and so on.[1]

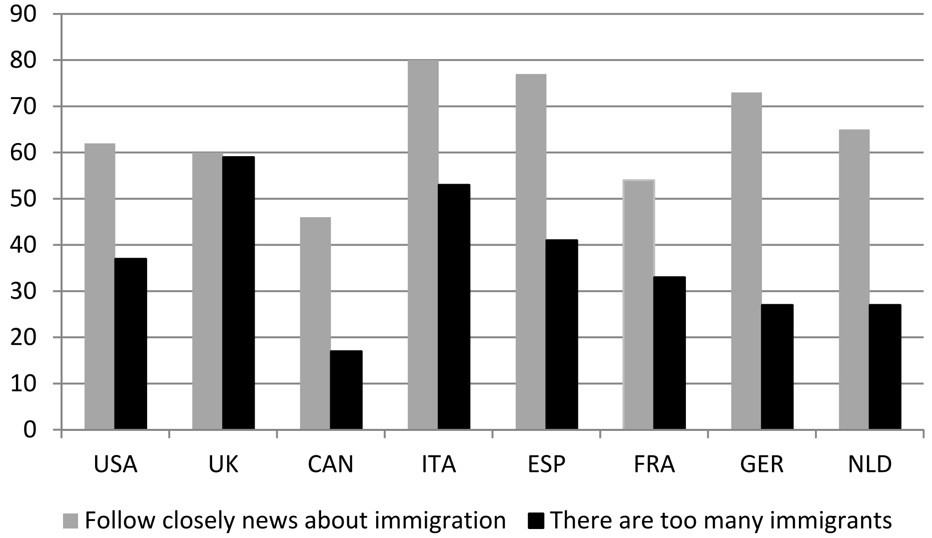
The United States, the United Kingdom, Italy, Germany, the Netherlands and France were included in all waves of the Transatlantic Trends Survey (TTS henceforth) since year 2008, while Canada and Spain were included after 2010. While such sample is smaller than in other multi-country surveys (like the World Values Survey, the International Social Survey Programs or the European Social Survey), the explicit focus of the TTS on migration issues allows for a much deeper understanding of attitudes toward this phenomenon in the main destination countries.

The first important element that emerges from the last round of the survey (2011) is the salience of immigration issues in the public debate; indeed, the majority of respondents in each country declared to follow closely news about immigration. Most importantly, a very large share of people in all countries believes that the number of immigrants is excessively high, see Figure 1. This is particularly true in Anglo-Saxon countries (with the notable exception of Canada) and in Southern Europe. In particular, those calling for a reduction in the presence of foreigners represent the majority of respondents in the UK and Italy. Anti-immigrant sentiments are less widespread in the rest of continental Europe (France, Germany and the Netherlands), where opponents to migration account for one fourth to one third of the respondents.

Another measure of opposition to migration, and one that is very relevant for the political economy of migration restrictions, is the electoral support for parties that have an explicit anti-immigrant stance. Figure 2 plots the vote share obtained by some of these parties over the last few years. Such share reaches almost 25% of total votes in the case of the Progress Party, which is currently the second largest party of Norway. Other anti-immigrant parties, such as the Freedom Party in Austria and the Northern League in Italy, enjoy strong bargaining power as key players for the formation of coalitional governments. Finally, a few other parties may be firmly rooted in some segments of the society in spite of even modest vote shares; this is the case of the British National Party or Golden Dawn in Greece.

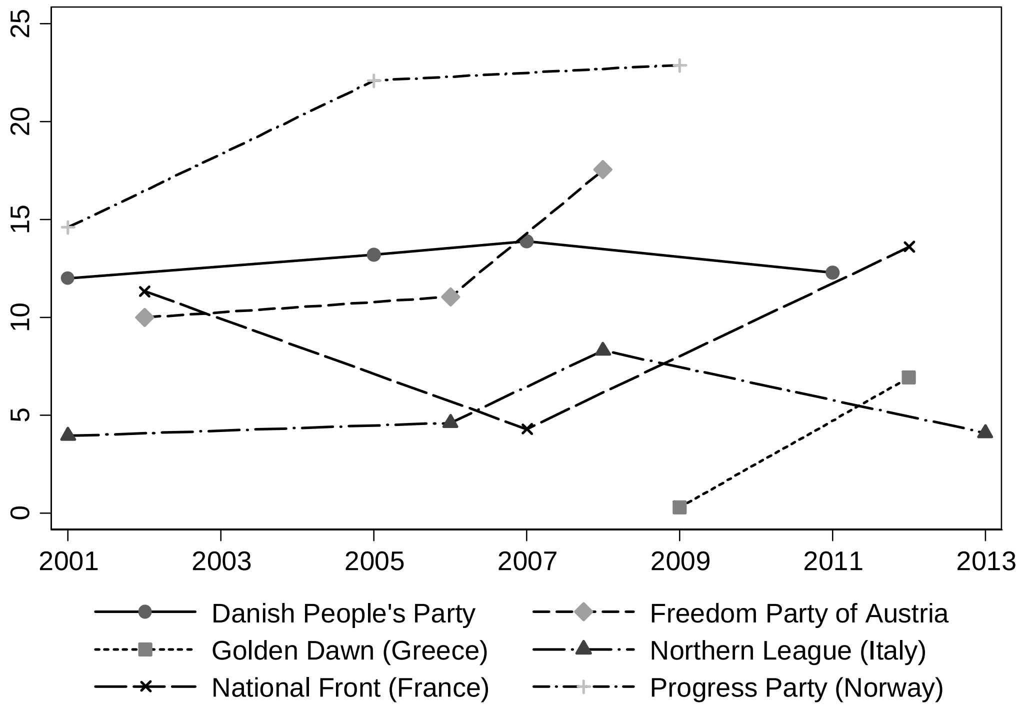
[1] The survey is a joint project of the German Marshall Fund of the United States, the Compagnia di San Paolo, and the Barrow Cadbury Trust, with additional support from the Fundación BBVA. The number of people interviewed in each country is around 1,000. Additional information is available through the website http://trends.gmfus.org/immigration/about/

Figure 1: Natives' concerns about immigration



Note: the graph shows measures of attitudes toward migration in a number of North American and European countries, based on the results of the Transatlantic Trends Survey (http://trends.gmfus.org/immigration/about/)

Figure 2: Percentage vote share of some anti-immigrant parties in Europe



Note: the graph shows the share of votes obtained by some anti-immigrant parties at national elections in Europe since year 2001.

But which are the main reasons for opposition to migration? Until few years ago, the economics literature has focused primarily on the role of competition between immigrants and natives in the labor market and/or in the use of social services. Starting with the former dimension, a downward sloping demand for labor implies that immigration could have in principle a negative effect on wages in the destination country. However, the effect depends on the elasticity of substitution between immigrants and (different categories of) native workers.

In practice, extensive empirical work on the US labor market has reached no consensus about the existence of any negative effect of immigration on natives' wages and employment, with the range of estimates varying widely across samples, time periods and methodologies.[2]

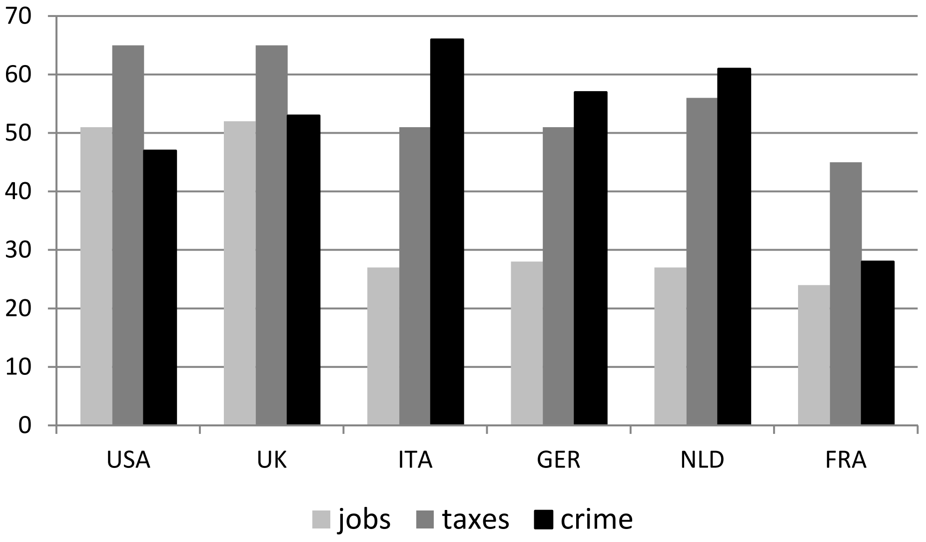
Outside the labor market, immigrants may compete with natives also for social services. In this case too, however, the effect is a priori unclear: on the one hand, immigrants have generally a more widespread skill distribution and higher fertility rates than natives, which make for a high dependence on public assistance; on the other hand, they are also younger and have high participation rates in the labor market, which means that they contribute more to social security and other forms of public expenditure. Storesletten (2000) shows indeed that the net government revenue of admitting one additional immigrant depends crucially on their age and skill distribution relative to natives.

Although the evidence on the effects of immigration on natives' labor market outcomes and on demand for social services remains mixed, the TTS 2008 confirms that both issues are important determinants of opposition to migration. In particular, the survey asks whether the respondent believes that "immigrants take jobs away from the native born", and that "immigration in general will cause taxes to be raised because of immigrants' demand for social services". Figure 3 shows that the latter concern is prevalent in all countries, while fears over the labor market impacts of immigration are much more widespread in the UK and the US.

[2] Borjas (2003) estimates a 0.3-0.4 elasticity of natives' wages to labor supply shifts across education-experience cells, while cross-areas studies a-la-Card (2001) generally conclude that immigration had no negative effect on natives' labor market outcomes, with Ottaviano and Peri (2012) going as far as suggesting the existence of complementarity between immigrants and natives. Dustmann et al. (2013) go beyond average effects and show that immigration depresses wages below the 20th percentile of the wage distribution, while it benefits natives at the top of the wage distribution. Friedberg and Hunt (1995) provide a survey of this literature.

Interestingly, such countries are characterized by flexible labor markets, in which natives may be more exposed to competition from immigrants.[3]

Figure 3: Concerns about the impact of immigrants on jobs, taxes and crime



Note: the graph shows the fraction of people worried that immigrants increase unemployment, taxes and/or crime rates, based on the results of the Transatlantic Trends Survey (http://trends.gmfus.org/immigration/about/).

However, the most striking result that emerges from Figure 3 is that a very large share of people in all countries is afraid about immigration for a totally different reason, namely that immigrants increase crime rates. Specifically, the TTS 2008 asks whether "Immigration in general will increase crime in our society". The majority of people in the UK, Germany, Italy and the Netherlands believes that this actually the case, and in the latter three countries there are more people worried about the impact of immigration on crime than on anything else. In the US, the share of people concerned about crime effects is just below 50 percent.

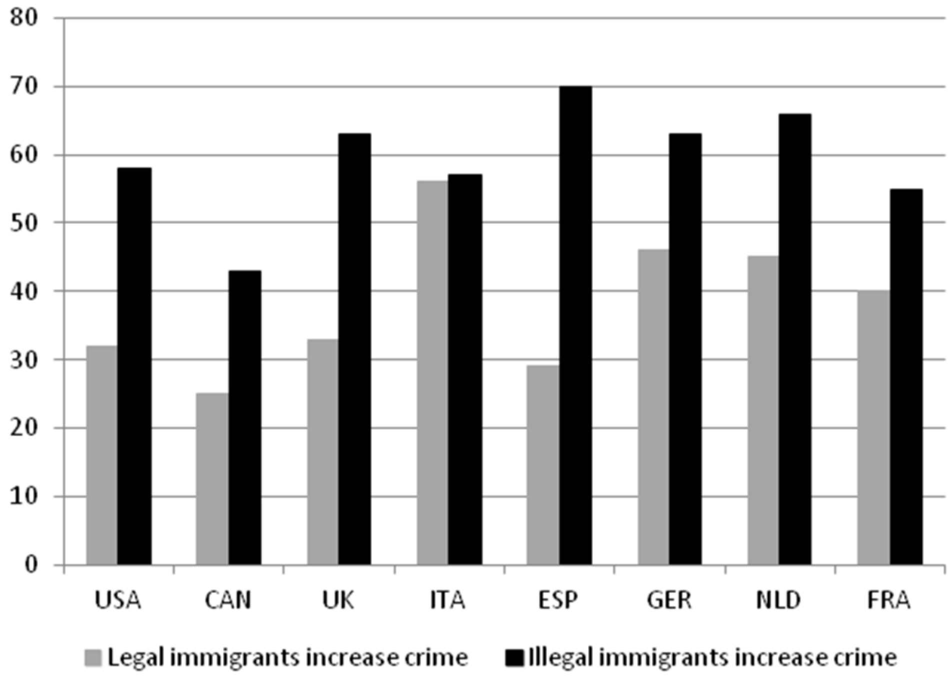
Therefore, political demand for migration restrictions in many countries, particularly in continental Europe, may be driven to a large extent by fears that immigration increases crime. This simple descriptive evidence is confirmed in a multivariate regression framework by Card et al. (2012). Using data on attitudes toward migration from the European Social Survey, they distinguish between purely economic concerns (i.e., effects on wages and taxes) and changes in "compositional amenities", which capture other externalities of immigration (including

[3] For an extensive analysis of the relationship between exposure to labor market competition and natives' attitudes toward migrants, both at the individual-level and across countries, see Facchini and Mayda (2008,2009).

crime). They conclude that the latter type of concerns are substantially more important to understand opposition to immigration.

Going back to the TTS, crime concerns are not directed indiscriminately toward all immigrants. The 2010 wave of the survey allows the respondents to distinguish between legal and illegal immigrants. It turns out that the respondents operate a clear distinction between the two groups: while in all countries the illegals are generally blamed for increasing crime, legal immigrants cause much less concern in all countries, with the notable exception of Italy; see Figure 4.

Figure 4: Concerns over legal and illegal immigrants



<u>Note</u>: the graph shows the fraction of people worried that legal and illegal immigrants, respectively, increase crime rates, based on the results of the Transatlantic Trends Survey (http://trends.gmfus.org/immigration/about/).

Indeed, illegal immigrants may have a higher propensity to engage in criminal activities, for at least two reasons. First, although illegal residence is arguably very different from outright criminal activities, the very act of remaining in the destination country may already signal a greater propensity to break the law. More in general, illegal immigrants may be *negatively selected*: other things equal, the characteristics of individuals in this group make them more prone to engage in criminal activity relative to legal immigrants. The second reason why illegal immigrants may be more at risk of committing crime is that, in most countries, they are prevented from working in the official sector, so they could be more willing to resort to illegitimate activities. At the same time, illegal immigrants may be apprehended by the police and expelled from the destination country, which mechanically reduce the number of crimes that they commit there.

Therefore, migration policy may affect the criminal behavior of immigrants, and ultimately the crime rate in the host countries, in a non-trivial way. In spite of their importance for the correct evaluation of migration policies, there is currently very little empirical evidence on these issues. One important reason is that, from a statistical point of view, it is generally very difficult to observe illegal immigrants, not to mention their involvement in criminal activity.

In the rest of this chapter we provide data on the presence of legal and illegal immigrants, and on their involvement in criminal activity, across several destination countries.

1.2. Legal and illegal immigration: perceptions vs. Reality

Against the backdrop of an increasing integration of the world economy, international migration has also been growing at a fast pace over the last decades. During the fifty-year period between 1960 and 2010, net migration flows toward OECD countries increased from 0.3% to 1.6% of the population in destination countries, bringing the share of foreigners at about one tenth of total residents (up from less than half of it in 1960).

Yet, the size of international migration remains small compared to other dimensions of globalization; for instance, during the same period, total trade in OECD countries (imports plus exports) increased from 20 to 50 percent of GDP. Indeed, the movement of people across different countries still faces considerably more restrictions than trade or international capital flows.

Barriers to international migration may take several forms: fixed limits to the number of foreigners that can be admitted in a given year (*quotas*), admission and residence requirements (for instance in terms of income and/or education), taxes or other bureaucratic costs, and so on. Such policy measures are aimed at preventing the *legal* entry by part of the prospective immigrants; yet, some of these people may decide to migrate illegally, unofficially crossing the border or over-staying tourist visas.

Of course, the very nature of illegal migration makes it hard to obtain accurate data on this phenomenon. Still, estimates of the stock of illegal immigrants are produced for many countries using a variety of methods. For instance, amnesties of illegal immigrants allow for a count of the applications sent by foreigners that are unofficially present in the country. This method has been often employed for Italy, where several amnesties have been conducted since the mid-1980s. Alternatively, one may compare the (cumulative) number of residence and/or working permits issued during a given period, with the total number of foreigners measured by sources that cover both official and unofficial migration; the difference between the two numbers can then be attributed to illegal migration. This second method is currently employed in the US, where the national census covers both legal and illegal immigrants, see Hoefer et al. 2009.

Data on legal and illegal immigration in Italy and the US will be discussed in great detail in chapters 3 and 5, respectively. Instead, the remaining of this chapter will describe a few cross-country patterns using comparable data on the size and composition of immigration, incarceration rates and migration policy that are available for 12 European countries: Austria, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, Spain and the United Kingdom. Although the measure of migration policy is not available for the US, we include data on migration and incarceration rates for such country.

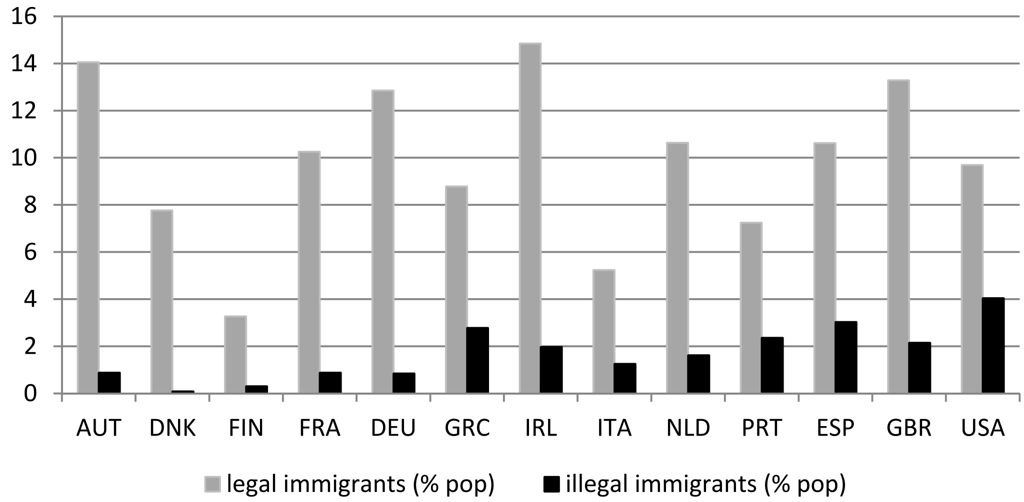
Figure 5 shows the stock of legal and illegal immigrants in year 2005, as a percentage of the total resident population in the receiving country; the sources of these data are the World Bank's World Development Indicators and the Clandestino Project, respectively.[4] The total of legal and illegal immigrants ranges from a minimum of 3.6% of the total population in Finland to a maximum of 16.8% in Ireland; the share of immigrants is above 10% also in Austria, France, Germany, Netherlands, Spain, the United States and the United Kingdom. The simple average across these countries is 11.6%, which is in line with the rest of OECD countries.

The graph also shows that, in the greatest majority of countries, the presence of illegal immigrants remains at extremely low levels, between 1% of all foreigners in Denmark and 13% in Netherlands and the UK. Some notable exceptions in this respect are the Mediterranean countries in our sample (Greece, Italy, Portugal and Spain), in which the illegals account for

[4] The Clandestino Project was financed within the 6th EU framework and provides upper and lower bound estimates for the size of the illegal immigrant population in several European countries in three benchmark years (2002, 2005 and 2008). The documentation and the data are publicly available through the website http://research.icmpd.org/1244.html. Since the project does not cover countries outside Europe, we took the estimate of the illegal population in the US from the Department of Homeland Security (Hoefer et al. 2009).

one fifth to one fourth of total immigrants, and the United States, in which the share of illegals is just below 30%. In any case, such numbers represent a minimal share of the total resident population (natives plus foreigners), below 2% for most countries and up to a maximum of 4% in the United States.

Figure 5: Share of immigrants over total resident population



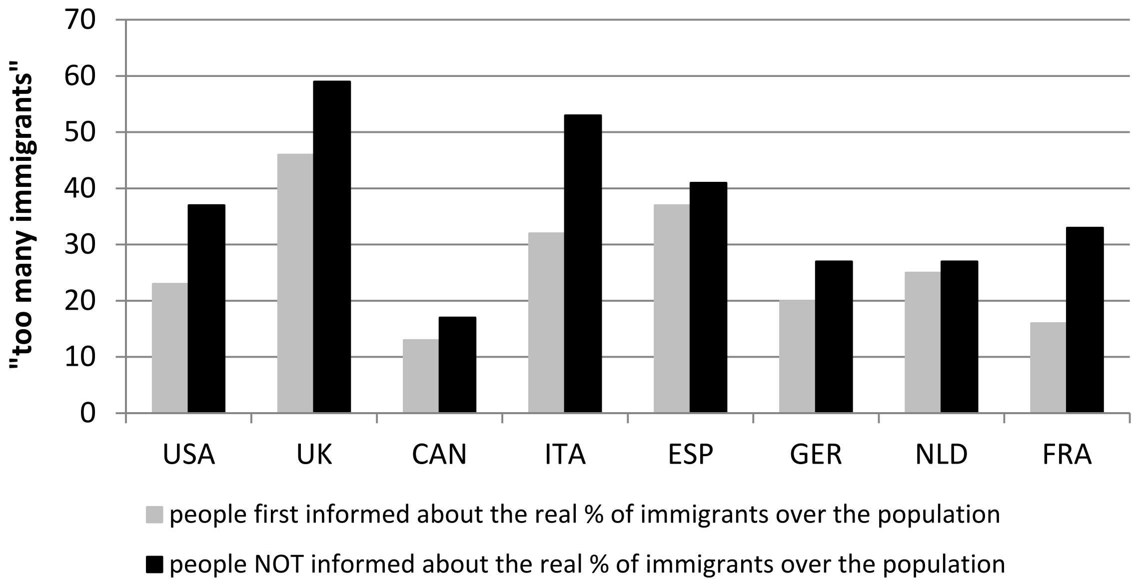
Note: the graph shows the fraction of legal and illegal immigrants over the total resident population in a number of countries during the period 2002-2008. The data on legal immigrants comes from the OECD Population Statistics, while the sources of data for illegal immigrants in Europe and the US are the Clandestino Project (http://research.icmpd.org/1244.html) and the US Department of Homeland Security (Hoefer et al. 2009), respectively..

Are those numbers about the size of the legal and illegal population correctly factored in by the respondents of the TTS? Evidence from two questions included in the 2010 wave suggests that this may *not* be the case, and that anti-immigrant sentiments may depend, at least in part, on inaccurate beliefs about the size and composition the immigrant population.

In the first question, the sample of people interviewed in each country was randomly split in two groups: people in the first group were simply asked whether they believed that there are "too many immigrants"; people in the second group were asked the same question, but they were first informed about the actual percentage of immigrants over the total population (according to official estimates). Access to such information lowered dramatically concerns that there is an excessive migration, see Figure 6. This simple test suggests that opposition to migration may reflect, at least in part, a general tendency to over-estimate the number of immigrants that are actually present in the country.

Such tendency seems even more pronounced when it comes to illegal migration. Another question of the TTS 2010 asks whether, in the opinion of the respondents, most of the immigrants resided legally or illegally in the country. The majority of respondents in Italy, the United States and Spain (65%, 58% and 50%, respectively), and a sizeable fraction of the respondents in other countries (from 11% in Germany to 38% in the UK), believed that most immigrants were illegally present in the country. However, in all such countries the estimates of the share of illegal immigrants is well below 50% (see again Figure 5). In this case too, there is thus a profound divergence between perceptions of migration and the reality of the phenomenon.

Figure 6: Information and concerns about excessive immigration



<u>Note</u>: the graph shows the fraction of people worried about excessive migration among equally-sized sub-samples of the Transatlantic Trends Survey in each country. The grey bars refer to sub-samples of individuals that were previously informed about the true fraction of foreigners in the population, while the black bars refers to sub-samples of people that did not receive such information.

Summarizing, it seems that people tend to over-state the number of immigrants that are present in the country, and that such biases may partly drive concerns about immigration. In the same way, the perception that immigrants increase crime rate could depend on mis-perceptions about these two phenomena. For this reason, we next move from perceptions to actual data on immigrants' involvement in criminal activity.

1.3. Immigrants' involvement in criminal activity: preliminary evidence

The empirical analysis of crime poses serious challenges. The main reason is that data on reported crimes under-estimate the true number of (unobserved) crimes that are actually committed; if the extent of under-reporting varies in a way that is correlated with other determinants of criminal activity, heterogeneity in reporting rates may hamper the identification of the relationships of interest.

Crime statistics are also problematic because there may be a considerable lag between the time at which the offense was committed and the moment of the arrest, conviction and incarceration (if any). Related to this, the probability of "type I" or "type II" errors (respectively, the probability of arresting/convicting/incarcerating an innocent, and not doing so with a criminal) may vary depending on the specific event which is used to construct the statistics. Finally, and most importantly for our analysis, such probabilities may vary between different categories of individuals (particularly, immigrants and natives).

Such problems have long been recognized in the crime literature (see MacDonald, 2002, for a throughout analysis of these issues) and are exacerbated in cross countries studies, due to the presence of extreme heterogeneity in terms of police enforcement, penal codes, judiciary system, etc. For this reason, we do not even attempt to provide a rigorous analysis of immigrants' involvement in crime across countries; such analysis will instead be conducted in the next chapters looking at the specific experiences of two countries, Italy and the US. In this chapter, we rather provide a descriptive analysis of the pattern of immigrants' incarceration at the international-level.

Figure 7 reports the share of foreigners in the prison population across countries. Although this is a very crude measure of involvement in criminal activity, for the reasons explained above, it is the only one for which we could find comparable data for the 13 countries in our sample. Such data come from the International Centre for Prison Studies (http://www.prisonstudies.org/) and refer to the last year available for each country, which falls in most cases within the period 2010-2012. In the same graph, we also report the share of foreigners over the total population, including both legal and illegal immigrants (i.e. the sum of the grey and black bars in Figure 5).

Based on this information, we can compute the probability of being incarcerated for immigrants relative to natives. The ratio of the probability of being incarcerated for the two groups is in fact

$$\frac{foreign\ prisoners}{foreign\ residents}\bigg/\frac{native\ prisoners}{native\ residents}$$

or, equivalently,

$$\frac{foreign\ prisoners}{native\ prisoners}\bigg/\frac{foreign\ residents}{native\ residents}.$$

Multiplying and dividing the first ratio by the total resident population, and the second ratio by the total prison population, we obtain that the relative probability of incarceration equals

$$\frac{p}{1-p}\bigg/\frac{r}{1-r},$$

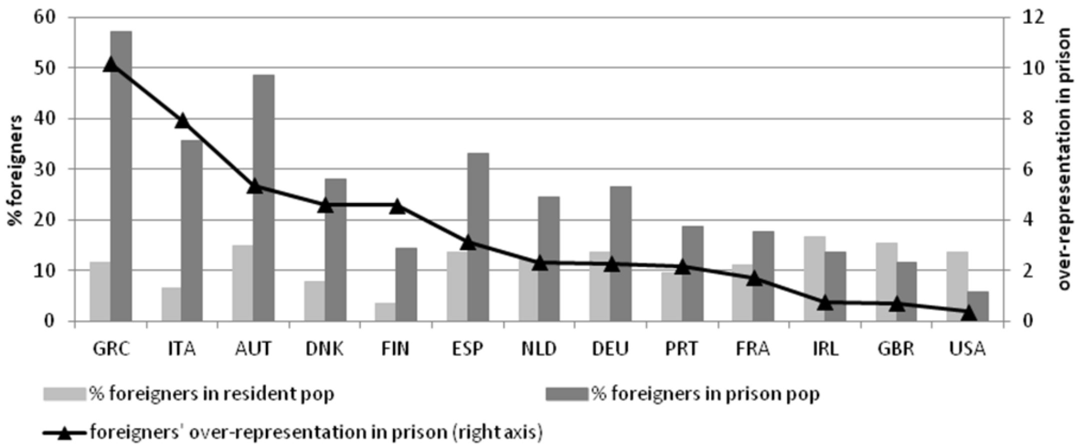
where *p* and *r* denote the share of immigrants in prison and in the total resident population, respectively. To the extent that a higher probability of incarceration reflects a higher probability of having committed a crime, the relative probability of incarceration provides a first measure of immigrants' involvement in crime, relative to natives. Such measure is also plotted in Figure 7.

It turns out that immigrants are overly represented in the prison population of all countries in continental Europe. In particular, the probability of incarceration is 10 and 8 times larger than that of natives in Greece and Italy, respectively, and 2 to 6 times larger in the other countries in continental Europe.

By contrast, in all the Anglo-Saxon countries immigrants are under-represented in prison. Their probability of incarceration is about 75% of that of natives in Ireland and the United Kingdom, and 40% in the US. Taken at face value, such evidence lines up with natives' perceptions across countries, as depicted in Figure 3. People are in fact more concerned about the impact of immigration on crime in Europe, where immigrants are actually over-represented in prison, than in Anglo-Saxon countries, where they are under-represented.

However, using immigrants' presence in jail as a measure of their propensity to commit crime is problematic, because the probability of being incarcerated conditional on having committed a crime may differ substantially between immigrants and natives. For instance, foreigners may be subject to additional restrictions that prevent them from taking advantage of alternatives to prison, such as home detention. Chapter 3 will provide an extensive discussion of these issues for the case of Italy. In particular we will show that, during the last few years, the share of foreigners among all people reported by the police, as well as the share of those convicted by the judicial authority, is much lower (between one and two thirds) than their share in the prison population.

Figure 7: Immigrants' incarceration rate across countries



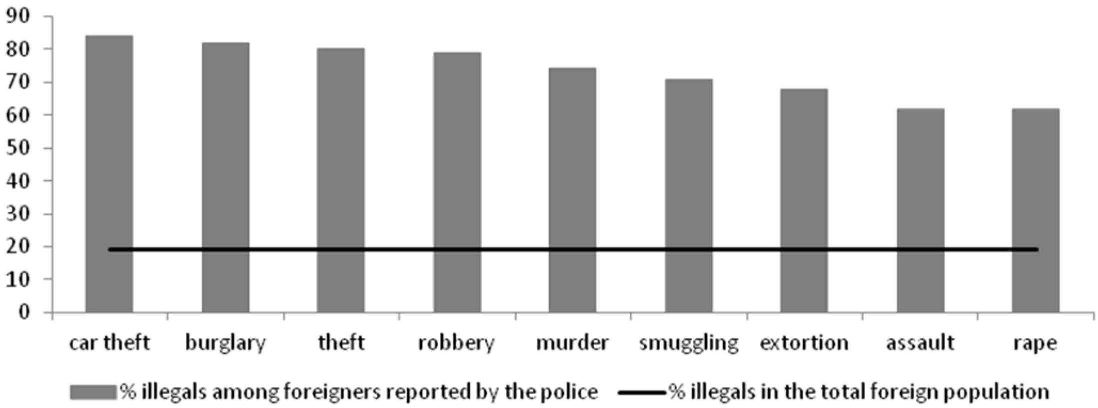
Note: the graph shows the fraction of foreigners in the total resident population (light bars) and in the prison population (darker bar); foreigners' over-representation in prison (black line) is computed as the ratio of the latter to the former measure. Source: Centre for Prison Studies (http://www.prisonstudies.org/).

For this reason, incarceration rates can hardly be interpreted as an accurate measure of the degree of immigrants' involvement in criminal activity. On the other hand, it is not clear that different treatment of immigrants and natives, on the part of the judicial system, can possibly explain 8- or 10-fold differences in the probability of incarceration. For this reason, it is worth asking whether there are some factors that increase the risk of immigrants' involvement in criminal activity, at least in some destination countries.

In this book, we focus on the role of legal status. From an empirical perspective, our interest is primarily motivated by the striking differences in the probability of being reported by the police (for reasons other than illegal residence) between legal and illegal immigrants in Italy, the only country for which we could find a breakdown by legal status in criminal statistics. Specifically, an official report by the Italian Ministry of Interior (2007) provides very detailed information in this respect for year 2006.

The main findings of the report are summarized in Figure 8. The share of illegals ranges from about 80% among the foreigners reported for (different types of) property crime, to about 60% of those reported for assaults and rapes. The figure also reports the share of illegals in the total immigrant population, which we can use to compute the relative probability of being reported by the police for illegal and legal immigrants (in the same way as we did before for the relative probability of incarceration between immigrants and natives). For property crimes, the probability of being reported is about 20 times larger for the illegal than for the legal immigrants, and ‘only’ 7 times larger in the case of assaults and rapes.

Figure 8: Percentage of illegals among foreigners reported by the police, by type of crime



Note: the graph shows the fraction of illegals among the foreigners reported by the police in Italy, for several types of crime. Source: Italian Ministry of Interior.

To the extent that a higher probability of being reported by the police is a symptom of greater involvement in criminal activity, illegal immigrants seem substantially more at risk of committing crimes. This is particularly true for property crimes, consistently with the idea that the difference with legal immigrants may be due to worse economic opportunities in official markets, which imply a lower opportunity cost of criminal activity.

The next chapter will provide a simple theoretical framework that relates criminal behavior to legal status, distinguishing between selection and causal effects. The model will also clarify the role of restrictions to legal migration for the emergence of a pool of unauthorized immigrants. As a preliminary step to the theoretical analysis, we conclude this chapter by looking at empirical relationship between migration restrictions and the illegality rate of the immigrant population across destination countries.

1.4. Migration policy and illegal migration

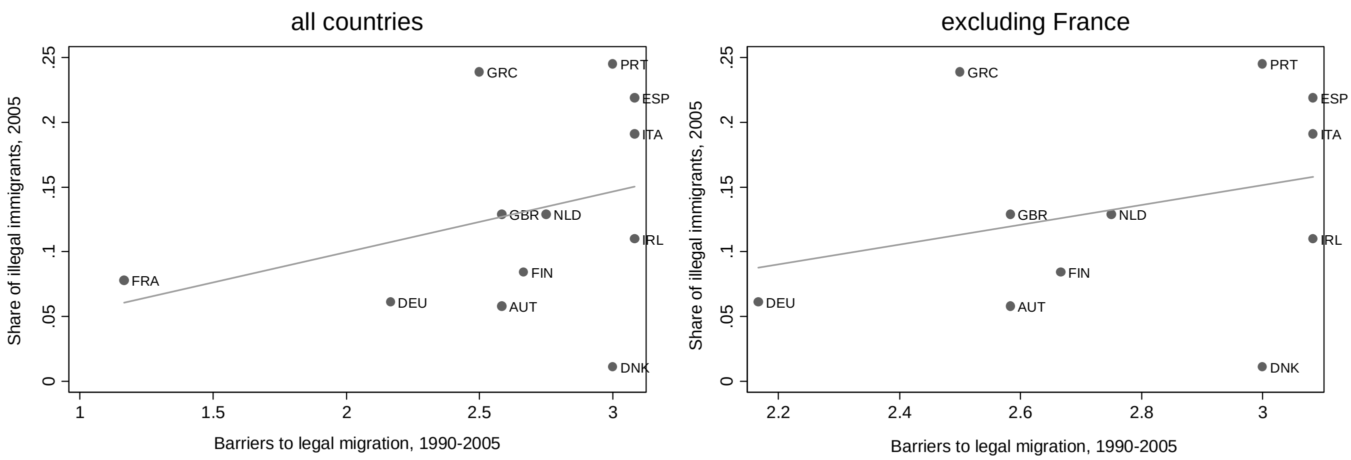
Restrictions to migration – in the form of admission requirements, quotas and so on – prevent a number of prospective immigrants from entering the country, at least legally. At the same time, some of these people may try to enter unofficially, illegally crossing the frontier or overstaying temporary permits (e.g. tourist visas). Therefore, migration policy may affect not only the size, but also the composition of migration flows.

Once again, the paucity of comparable data on migration policies and illegal migration prevents us from conducting an exhaustive empirical analysis of these effects across countries. Still, we can use the classification of countries' policies over the period 1990-2005 in Boeri and Fumagalli (2009) to compare migration outcomes across different policy regimes. Such classification covers several dimensions of migration policy: the existence of quotas to legal migration, the need of obtaining a residence permit before entering the country, the number of other requirements (in addition to the residence permit) imposed upon entrants, and so on. Based on this information, Boeri and Fumagalli (2009) construct a synthetic index, between 0 and 6, of the restrictiveness of migration policy: it takes a value of 0 for countries that are more open to migration flows, while it takes a value of 6 for those that impose the highest barriers to (official) migration. The index is available for all the countries in our sample with the exception of the US.[5]

Figure 9 plots the index against the composition of the immigrant population by legal status, across the countries in our sample. On average, higher barriers to legal migration are associated with a higher share of illegals among the immigrant population. This remains true even after excluding France (right graph), which is a clear outlier in terms of having very open policies.

[5] The dataset is publicly available through the website of the Fondazione Debenedetti at http://www.frdb.org/language/eng/topic/data-sources/dataset/international-data/doc_pk/11028

Figure 9: Restrictions to legal migration and share of illegal immigrants across countries



Note: the graph shows the relationship between restrictions to legal migration, as coded by Boeri and Fumagalli (2009), and the fraction of illegals over total immigrants, as estimated by the Clandestino Project. The left graph includes all countries, while the right graph excludes France, which is a clear outlier in the left graph.

1.1. Conclusions

This chapter discussed the centrality of legal status for the current debate on the relationship between immigration and crime, and presented some preliminary evidence on the cross country relationships between migration restrictions, the size and composition of the immigrant population, and its involvement in criminal activity. The analysis of all these phenomena faces formidable difficulties in terms of measurement; most importantly, the empirical patterns observed across countries likely reflect complex equilibrium relationships.

For this reason, none of the findings presented so far can be given a univocal (causal) interpretation; taken together, however, they indicate the avenue for a deeper understanding of the relationship between immigration and crime. In this view, legal status play a crucial role, as illegal immigrants seem characterized by a much greater risk of involvement in criminal activity relative to legal immigrants. Is this mainly the result of a different composition of the two groups (for instance in terms of age, gender, education, etc.), or is it due, at least in part, to the causal effect of legal status on the propensity to commit crime?

The answer to this last question is crucial for the correct evaluation of migration restrictions, which prevent a number of prospective immigrants from entering – and thus from committing crimes in – the destination country, but also create a pool of unauthorized immigrants, who can have a higher propensity to commit crimes. To the extent that legal status has indeed a causal effect on criminal behavior, the second effect may prevail, bringing an increase in criminal activity in the destination country, while the opposite would be true if differences in criminal behavior between legal and illegal immigrants depend just on the different composition of the two groups.

The next chapter clarifies these issues in the context of a simple model of immigration and crime.

Chapter 2 - Theoretical Framework

In this chapter we provide a simple theoretical model that allows us to study the effect of migration policy to the criminal behavior of immigrants in the destination country. In spite of its simplicity, the model allows for a rich characterization of policies in terms of entry barriers, quotas, expulsions and amnesties. We will first study the effects of these different policy instruments on the size and composition of the immigrant population, in terms of labor market ability and legal status. Then, we will extend the model to incorporate decisions about criminal activity by different types of immigrants, in order to examine the implications of alternative policies for the number of crimes committed by immigrants in the host country.

2.1. The individual problem

Consider a unit-mass population of individuals, living for two periods, who must choose whether to remain in their (home) country or migrate into a different (destination) country. Lifetime utility in the home country equals $V_H = w_H a$, where w_H is the labor market price for skills and a are heterogeneous individual skills, which are distributed according to the cumulative $G(a)$.[6]

Alternatively, individuals incur a travel cost τ and move to another country characterized by better labor market opportunities. One possibility is to just cross the border unofficially, possibly hoping for a later amnesty. A generalized amnesty occurs in fact between the first and second period with probability σ, in which case all unofficial immigrants obtain legal status and can work, in the second period, for a wage $w_L > w_H$. If the amnesty does not occur, however, a fraction δ of all unofficial immigrants is apprehended and expelled from the country at the beginning of the second period. In any case, even if they are not expelled, they can work just in the shadow economy for a wage w_I, which is lower than the one earned by legal immigrants, but still higher than the one in the home country, i.e. $w_L > w_I > w_H$. Summarizing, the expected utility from migrating illegally is

$$V_I = \sigma w_L a + (1-\sigma)[\delta w_H a + (1-\delta) w_I a] - \tau.$$

Rather than entering illegally, prospective immigrants may instead apply for a valid residence permit before moving to the host country. By paying an additional cost *b*, they participate into

[6] Without loss of generality, we assume that the discount rate is equal to zero.

a lottery that awards residence permits with probability *q*; you can think of *b* as the time and money spent to deal with paperwork, and *q* as the ratio of awarded permits relative to the total number of applicants. In the second period, those that obtain the residence permit work in the host country, while the others may remain in the home country or they can still try to migrate illegally. The expected utility of participating into the lottery for residence permits is thus

$$V_L = q(w_L a - T) + (1 - q)\,Max\{V_I; V_H\} - b.$$

Individuals decide between remaining in the home country and migrating, either legally or illegally, by comparing the expected utilities V_H, V_L and V_I. Consider first the choice between remaining in the home country and migrating unofficially. Letting Δ denote differences with respect to outcomes when remaining in the home country, i.e. $\Delta V_i = V_i - V_H$ and $\Delta w_i = w_i - w_H$ for $i = I, L$, we have that illegal migration is preferred whenever $\Delta V_I > 0$, where

$$\Delta V_I = [\sigma \Delta w_L + (1 - \sigma)(1 - \delta)\Delta w_I]a - \tau.$$

Since $\Delta w_L > \Delta w_I > 0$, ΔV_I is monotonically increasing in a, and there exists a threshold a_I such that ΔV_I is positive for all and only the individuals with $a > a_I$, see Figure 10.

Turning to legal migration, the difference in utility between applying for the lottery and remaining in the home country is

$$\Delta V_L = q(\Delta w_L a - \tau) + (1 - q)\,Max\{\Delta V_I; 0\} - b,$$

which is also increasing in a. Moreover, plugging the expression for ΔV_I into the last equation it is immediate to show that ΔV_L is steeper than $\Delta V_I > 0$ when $a > a_I$. Therefore, there exists a second threshold a_L above which $\Delta V_L > \Delta V_I > 0$, i.e. the individual prefers legal over illegal migration.

In Figure 10 we depict the case in which $a_L > a_I$, so that some migrants (those with a between a_I and a_L) do not even apply for a residence permit but just enter unofficially. If instead $a_L < a_I$, which occurs for a sufficiently low *b* and/or a sufficiently high *q*, all prospective migrants first apply for a valid residence permit and they enter unofficially only if the application is rejected. The comparative statics that follow consider primarily the former case, which seems more relevant from an empirical point of view.

2.2. Migration policy and the selection of immigrants

The model parameters characterize migration policy in terms of upfront costs (b), migration quotas (q), amnesties for unofficial immigrants (σ) and enforcement of deportations (δ). We next discuss some simple comparative statics results for the total size of the immigrant population and its composition in terms of legal status.

2.2.1. Restrictions to legal migration

An increase in the entry costs, b, and/or a decrease in the entry quotas, q, lowers the utility associated with legal migration, thus shifting the curve ΔV_L downward; see the left graph in Figure 2. As a result, prospective immigrants with ability between a_L and a'_L find it more convenient to just migrate illegally (as opposed to apply first for a residence permit), so that the ratio of illegals increases. At the same time, more of these people are expelled from the host country (a fraction $1-\delta$, rather than $1-\delta$ times $1-q$), so the total size of the immigrant population decreases. Summarizing, changes in barriers to legal migration have opposite effects on the size of the foreign population and the ratio of illegals: in particular, restrictive policies reduce total migration but increase the ratio of illegals, the opposite is true for a lifting of migration restrictions.

2.2.2. Enforcement of migration restrictions

Entry costs and quotas limit migration only to the extent that immigrants that are not entitled to enter the country are effectively prevented from doing so. Enforcement depends on the expected probability of benefiting from a future amnesty, σ, and the fraction of illegals that are actually expelled in case of no amnesty, δ. A higher probability of amnesty and/or a lower expulsion rate increase the expected utility of illegal migration, ΔV_I, so the minimum ability above which individuals are willing to migrate, a_I, decreases. Since ΔV_I enters the utility of applying for a valid residence permit (because immigrants that have their application rejected can still try to migrate unofficially) ΔV_L does also increase. However, the (direct) effect on ΔV_I is greater than the one on ΔV_L, so the difference $\Delta V_I - \Delta V_L$ increase and a_L shifts to the right, see the right graph in Figure 2. As a consequence, illegal migration increases, both in absolute terms and relative to total migration; since, in addition, illegal immigrants are less likely to be expelled, the total size of the immigrant population also increases. Therefore, changes in enforcement affect the size of the foreign population and the ratio of illegals in the same direction: lax enforcement increases both total migration and the ratio of illegals, the opposite is true when enforcement is stricter.

These simple comparative statics highlight the crucial role of migration policy for the size and composition of the immigrant population: we next examine the importance of the latter for the crime effects of immigration in the destination country.

2.3. Legal status and criminal activity

We now introduce the possibility for both the legal and illegal immigrants to engage in crime during the second period, conditional on not having been deported at the beginning of the period. Criminal opportunities deliver a payoff z, which is randomly distributed across individuals according to the cumulative density F(z), but criminal are arrested and sent to jail with probability π, in which case they obtain a utility equal to zero.

In line with the basic predictions of the economic model of crime, the propensity to engage in criminal activity decreases with legitimate income opportunities, which in turn depend on ability and wages. Formally, individuals commit a crime whenever its expected payoffs exceed those of legitimate activities, $(1-\pi)z > w_i a$, which occurs with probability $c(w_i, a) = 1 - F\left(\frac{w_i a}{1-\pi}\right)$, for i=I,L. Therefore, $c(w_i, a) < c(w_i, a')\ \forall a > a'$, and $w_L > w_I$ implies that $c(w_L, a) < c(w_I, a)\ \forall a$; see Figure 3.

The individual probability of committing a crime may be written compactly as

$$p(a) = sc(w_L, a) + (1-s)(1-\delta)c(w_I, a)\ ,$$

where s equals 1 and 0 for legal and illegal immigrants, respectively. Notice that the probability of committing a crime for legal immigrants is simply the probability of receiving a crime opportunity that is more profitable (in expected value) than legitimate activities, while for illegal immigrants one must first condition on the probability of not having been expelled before, $1-\delta$.

The coefficient of s in $p(a)$ is the *causal*, or *treatment effect* of legal status on the individual probability of committing a crime in the host country, conditional on ability a:

$t(a) = c(w_L, a) - (1 - \delta)c(w_I, a).$

The sign of $t(a)$ depends on two opposite effects: on the one hand, $c(w_L, a) < c(w_I, a)\ \forall a$ implies that legal status always decreases the probability of committing crime *for immigrants that are not expelled*; on the other hand, illegal immigrants are expelled with probability δ, which reduces the probability that they commit a crime in the host country.

The direction of the overall effect remains thus theoretically ambiguous. A high enough wage premium for legal status and/or a low expulsion rate makes for a reduction in crime after the acquisition of legal status, while the opposite is true for a low wage premium and/or a high expulsion rate. Therefore, the sign of $t(a)$ is ultimately an empirical issue.

2.4. Identification

In general, the empirical identification of the causal effect of legal status is hampered by the fact that legal and illegal immigrants are drawn from different part of the ability distribution. In terms of our simple model, immigrants who prefer illegal over legal migration (i.e. those with ability between a_I and a_L in Figure 10 to Figure 12) have a lower ability relative to immigrants that apply for legal status; this is also consistent with the simple means' comparisons presented in the previous section. Due to their average individual characteristics, illegal immigrants would have then a higher probability of committing crimes to start with, regardless of the (causal) effect of legal status. This is a *selection effect*, which confounds the identification of the causal effect $t(a)$.

The observed difference in average crime rates of legal and illegal immigrants reflects both causal and selection effects. Empirically, it is generally very difficult to identify the former effect separately from selection on (possibly unobserved) individual characteristics, as this would require to observe the same individual in both states. This is of course impossible, as each individual at any given moment is either legal or illegal.

However, in chapter 3 we will approximate this ideal experiment in two ways. At the individual level, we will compare criminal activity across sub-samples of legal and illegal immigrants that exhibit quasi-random variation in legal status induced by the rationing of residence permits. At the aggregate level, we will exploit the recurrent amnesty episodes that characterized Italy over the last few decades. As a preliminary step to such analysis, in the next chapter we provide an accurate description of Italian migration policy over the last two decades.

Figure 10: Individual choice about migration

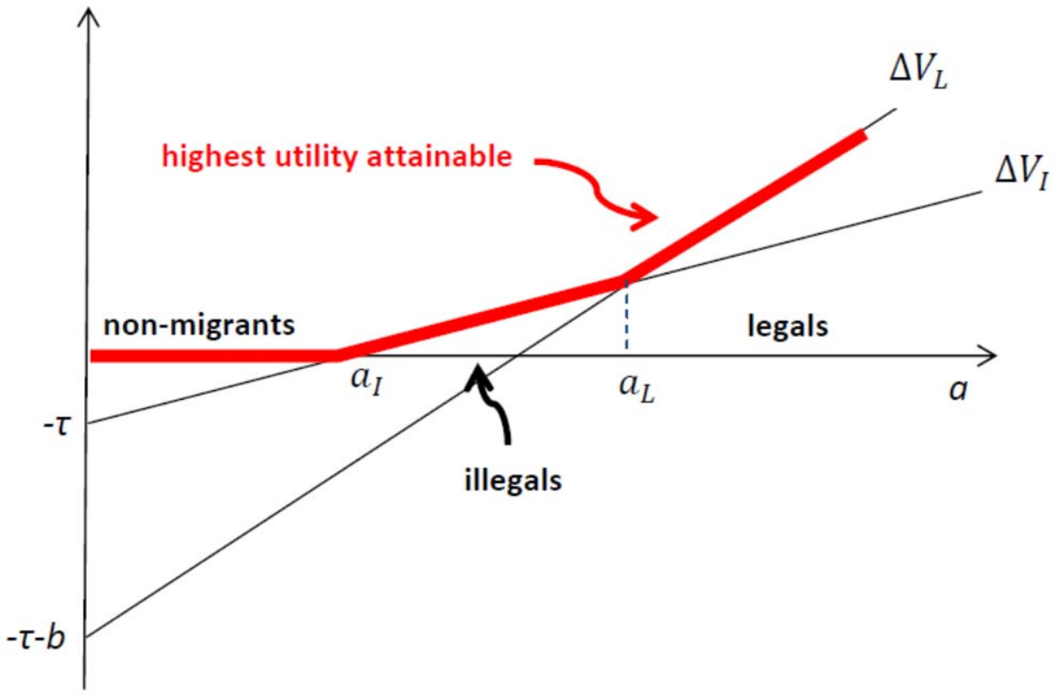


Figure 11: Comparative statics

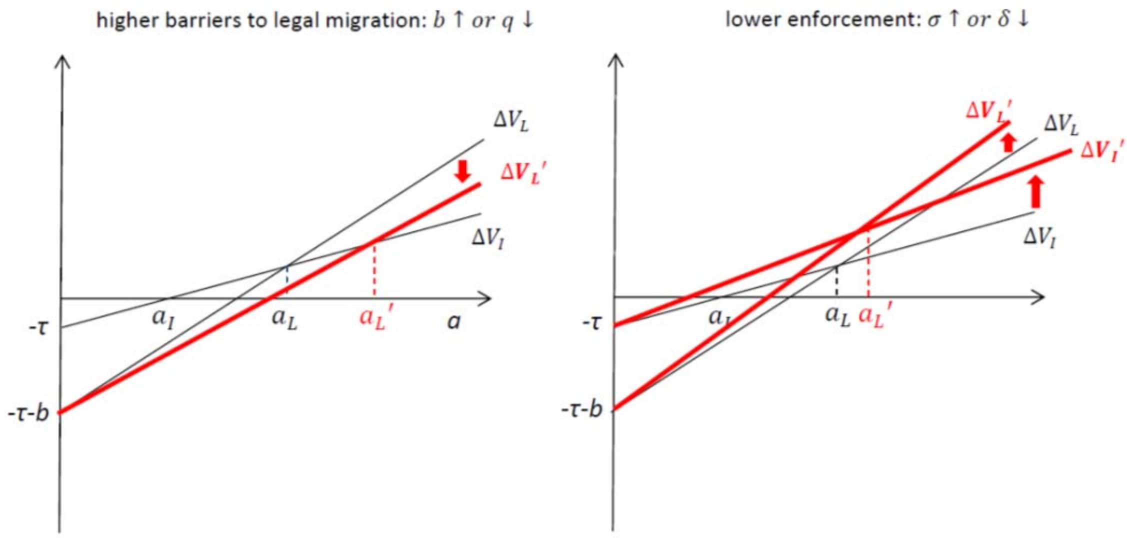
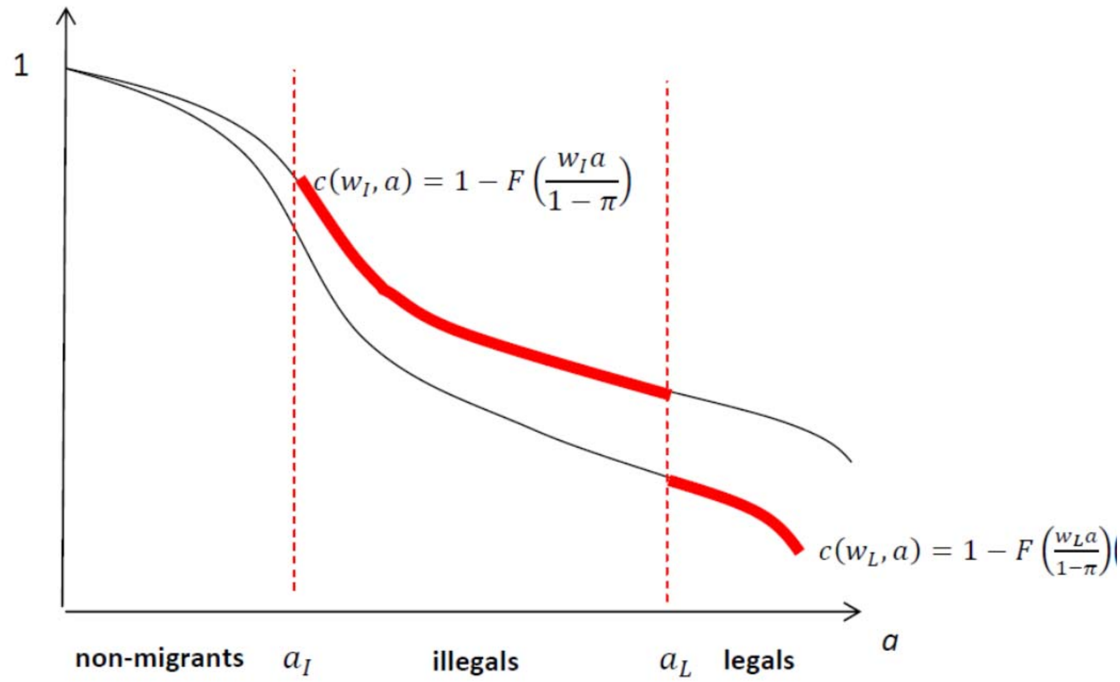


Figure 12 : Probability of committing a crime



Chapter 3 – Migration Policy and Crime in Italy

3.1. Introduction

In this chapter, we introduce the Italian setting and the policies we analyze in our empirical analysis in the following chapters. In section 3.2, we briefly describe the evolution and some characteristics of the immigrant population in Italy. The Italian migration policy, with a specific focus on the two main policy tools used in this context to manage immigrant flows (namely, the quota system and the amnesties), is explained in section 3.3. Finally, section 3.4 discusses evidence on the involvement of immigrants in criminal activities in Italy.

3.2. Migration in Italy

In 2011, there were more than 4.5 million documented immigrants in Italy; this corresponds to about 7.5 per cent of the population.[7] More than 50 per cent of them are citizens of a European country: 26 per cent from a New Member State (NMS) in the EU27, 24 per cent from a Central-Eastern European country outside the EU27 and only 4 per cent from the EU15. The other continents follow with 22 per cent arriving from Africa (15 per cent from Northern Africa), 17 per cent from Asia and 8 per cent from America (95 per cent of which from Latin America).

In 2011, there were almost 2.2 million foreign-born workers employed in Italy, which account for almost a tenth of the employed workers in the country. Their employment rate was 63 per cent – 66 per cent for EU27 citizens and 60 per cent for non-EU27 citizens – while the employment rate of natives was 56 per cent.[8] Immigrant workers mainly work in services (59

[7] According to estimates from ISMU (see Appendix A2) there were about 450 thousand undocumented immigrants in Italy in 2011, roughly 8 per cent of the total immigrant population residing in Italy (Fondazione ISMU, 2011).

[8] As shown in Dustmann & Frattini (forthcoming), Italy, Greece and Portugal are the only three EU15 countries where the employment rate is higher for immigrants than for natives. In the case of Italy this can be explained by the relatively lower employment rate among Italian women (due to their low participation rate) and by compositional differences, between natives and immigrants, in age, education and region of residence. Indeed, Dustmann & Frattini (forthcoming) use linear regression models to show that if one compares immigrants to natives with the same observable characteristics (gender, age, education) and living in the same geographical areas, the probability of being employed is actually lower for immigrants than for natives.

per cent), followed by manufacture (20 per cent), construction (17 per cent) and agriculture (4 per cent). About a third of them reside in the North Western regions of the country, 26 per cent in the North East area, 27 per cent in Central Italy and the remaining 13 per cent in the South and in the Islands. With regards to the medium-level of qualifications the distribution of immigrant population does not differ substantially from that of Italian workers: the share of workers with secondary education is fairly similar in the two groups. Nevertheless, immigrants are more likely to have just primary education (13 versus 6 per cent of natives) and less likely to have college education (8 versus 16 per cent of natives). As far as occupations are concerned, about 82 per cent of foreign-born workers have a low-medium skilled job: 59 per cent are blue-collar workers or artisans and 23 per cent are in low-skilled jobs. This figure is in sharp contrast with the corresponding share for the native population which is about half of it (43 per cent). The skill-downgrading and segregation of immigrant workers in the lowest ranks of the occupational ladder is a key – and worrying – feature of the labour market integration of foreign workers in Italy (Fullin and Reyneri, 2011; Dell´Aringa and Pagani, 2011).

In 2011, there were about 300 thousand unemployed immigrants in Italy, which implies an unemployment rate around 12 per cent (in comparison to 8 per cent for native workers). The participation rate, instead, was higher for immigrants, being 75 per cent for EU27 citizens, 69 per cent for non-EU27 citizens and 61 per cent for Italian citizen in working age.

The number of foreign citizens who are owner of firms in Italy reached almost 360 thousand in 2011, about 10 per cent of the total. Of this 10 per cent, about 2 per cent are EU27 citizens and the remaining 8 per cent are non-EU27 citizens.

3.3. Migration policy in Italy: Legislative framework and actual practices

Since 1998, Italy has managed the access of immigrant workers to its labor market through a quota system which sets yearly caps to the number of new entrants. As we discuss in section 3.3.1, regulating the legal access to the labor market through the quota system does not necessarily imply regulating the entry of immigrants. Indeed, substantial inflows of unauthorized immigrants have been a constant feature of the Italian experience, and general amnesties to grant legal status to the undocumented population have been a frequent policy option in this country (section 3.3.2). Still, undocumented flows have persisted in spite of an increasing investment in migration policy enforcement (section 3.3.3).

3.3.1. The quota system

3.3.1.1. The theory…

Since the early 1990s, Italy has repeatedly attempted to set up a quota system to manage the legal inflows of migrant workers. This system has been finally put in place in 1998 by the "Turco-Napolitano" law and it has been confirmed in 2002 by the following "Bossi-Fini" law (see Appendix A1). According to the design of the system, the government establishes every year – through the so called "Flows decree" (in Italian: "*Decreto Flussi*") – the number of immigrants which will be allowed to enter the country in the following year for working purposes (both seasonal and non-seasonal workers). Each region is attributed region-specific quotas and special quotas are reserved for specific countries of origin (mainly those who have signed bilateral agreements with Italy).[9] The government is also allowed to set an entry quota equal to zero in any given year, or to allow the access only of seasonal workers.

Moreover, according to the law, the annual "Flow decree" should be produced within a framework of medium-run planning which should be specified every three years by the Government with the so-called "3-year Planning Document" (in Italian: "*Documento Programmatico triennale*"). Other than defining the Government's plan of action regarding immigration in the next three years, the document also establishes the general criteria for entry flows which should constitute the basis for defining the annual "Flows Decrees". It is worth noting that the last "3-year Planning Document" approved concerned the period 2004-2006.[10]

[9] In order to produce the yearly estimates of the number of new foreign workers needed from abroad – by region and by type (seasonal / non-seasonal) – the Italian Government usually collects information from three main sources: a) the National Institute of Statistics (ISTAT) regarding demographic trends of the Italian population and the decrease of the working population; b) the Union of Chambers of Commerce, which annually provides an estimate of the additional immigrant workforce to be included in the labour market ; c) different studies on the state of the Italian productive system (EMN Italy, 2010).
[10] The "2007-2009 Planning Document" was elaborated by the centre-left Prodi government and never approved as final because of the early termination of that legislature. The following Berlusconi government (elected in 2009), instead, produced, in 2010, a "Plan for secure integration: Identity and Meeting" (in Italian: "*Piano per l'integrazione nella sicurezza: Identità e Incontro*") (EMN Italy, 2010).

When the "Flows Decree" is approved and becomes effective, potential employers can start applying for hiring immigrant workers until the number established by the quota is reached. According to the law, these immigrant workers should be recruited from abroad and should not be already residing in the country. When applying for an immigrant employee within the quota system, the employer can either request a specific individual (in Italian: "*chiamata nominativa*") or hire the first person in the lists of job-seekers that are compiled by Italian embassies and consulates in origin countries.

In 2007 the functioning of the application process has been digitalized, inaugurating the so-called "click-days". Since then, applicants can download, fill in and submit their forms on-line. While the direct contact with the public offices Italian happens only when the procedure is finalized and the candidate for the working permit – and her potential employer – are summoned to sign the employment contract. If the innovation of the click-days saved the immigrants from having to queue for hours outside the offices devoted to the collection of the application forms, it made patently evident the randomness of the system. As we will discuss in the next chapter, a few minutes difference in filing the application on-line may imply a drastic reduction in the probability of obtaining legal status.

3.3.1.2. … and the practice

In spite of the fact that politicians and members of government have always kept referring to the quota system as a mechanism to allow the entry of new workers – as it was in the intentions of the legislator – it is hard to deny that its real functioning is a different one (Ambrosini, 2011). Indeed, similarly to an amnesty, it mainly serves the scope of *ex-post* legalizing existing (but undeclared) employment of undocumented immigrants who are already residing in Italy. In general, foreign workers first enter the Italian labour market as undocumented immigrants (or with a tourist visa) and then, if they find a job and an employer who wants to legalize their employment relation, they wait for the "Flows Decree" and apply for a place. If their application is accepted, they move back to their origin countries and then return to Italy, entering officially this time and pretending not to have been in the country before (Fasani, 2010).[11] The bottom line of this procedure is that, in the Italian context, the

[11] Quantitative evidence of this malfunctioning, for instance, is provided by the 2006 ISMU survey (see Appendix A2): more than one third of the undocumented migrants interviewed – and therefore already resident in Italy – had applied for the 2006 "Flows decree", pretending to be still residing in their origin countries (Sciortino, 2007).

main difference between an amnesty and the "Flows decree" is that the latter procedure establishes a cap to the number of legalized individuals while the first does not.

The system as it is currently designed has two major limitations. The first one relies in the fact that it only allows employers to hire foreign workers who (in theory) are still residing abroad: This would imply that the match between employers and employees should occur from a distance.[12] The second shortcoming is the uncertainty about the size of the quotas, which inevitably hinder firms' strategies of foreign recruitment. This is especially true if the quotas are binding, as it seems to have often occurred in the Italian context (Fasani, 2010).
Moreover, one can identify at least three specific features of the Italian context which – interacting with those weaknesses in the quota system – led to the actual functioning of the quota system (Fasani, 2013). First, the potential employers of foreign workers are mainly households or small firms, both of which give substantial value to the personal knowledge of their potential employees and have limited capability to engage in hiring from abroad.[13] Second, immigrant workers in Italy are mostly demanded for low-skilled, manual and domestic care occupations: these are all skills which are, in general, hardly certifiable and verifiable at a distance. Third, the size of the underground economy – well above the European average – together with a large presence of undocumented immigrants (Reyneri, 2003). Indeed, these latter features provide a viable alternative to the difficulties and uncertainty implied by hiring workers from abroad through the quota system.

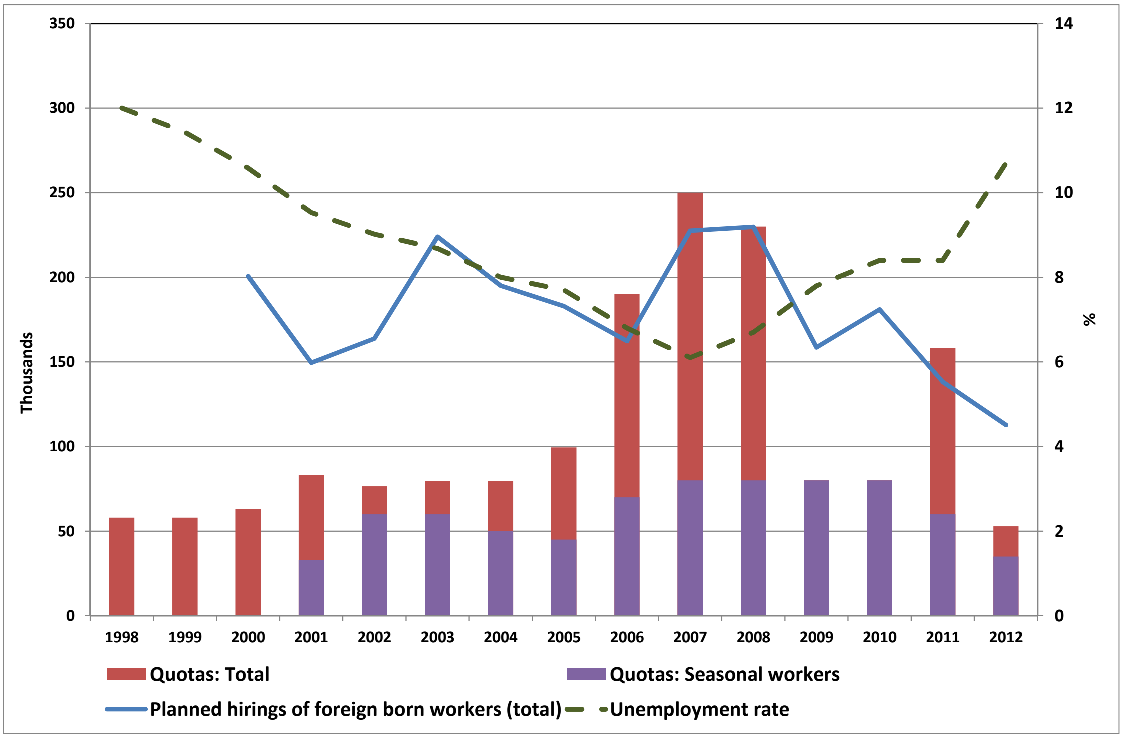
3.3.1.3. Quotas and labor demand

During the fifteen years elapsed since its introduction in 1998, the quota system has allowed legal "entry" to the Italian labour market to more than 1.6 million workers. As Figure 13 shows, the size of the quotas has experienced substantial fluctuations over time. One can see an initial phase where the quotas were kept below the 100 thousand units per year, with a gradual increase from the 58 thousand places of 1998 to the 99.5 thousand in 2005. Then, there were

[12] As a matter fo fact, the 1998 Turco-Napolitano law which introduced the quota system in Italy also envisaged the possibility of legally accessing the Italian labour market as a job-searcher (the so-called sponsor institute). This possibility has been abolished by the 2002 Bossi-Fini law.
[13] Italy has a peculiar firms' structure: 95 per cent of the 4.5 million Italian firms have less than 10 employees and account for almost 50 per cent of the employed workers. Moreover, Italy has one of the lowest average numbers of employees per firm in Europe. In 2010, the figure was at 4 employees per firm with respect to a EU27 average of 6.2. Below Italy are only Portugal (around 4) and Greece (3.3). Italy has a peculiar firms' structure: 95 per cent of the 4.5 million Italian firms have less than 10 employees and account for almost 50 per cent of the employed workers.

three years (2006-2008) where the size of the quotas was more than doubled, peaking in 250 thousand in 2007.[14] This increase was followed by a drastic reduction in 2008 and 2009, when the quota for non-seasonal workers was set to zero, while the quota for seasonal workers was maintained at 80 thousand as in the previous years. The quota jumped up again in 2011, reaching almost 160 thousand units and dropped again in 2012, when only 53 thousand workers (17.8 thousand of which for non-seasonal work) were allowed to legally access the Italian labour market.

Figure 13 : Flow decrees and demand for foreign born workers (1998-2012)



Note: In the period 1998-2000, the Flows Decree did not distinguish between seasonal and non-seasonal workers. The measure of planned hirings of foreign born workers is obtained from the Excelsior survey and it includes both seasonal and non-seasonal workers. Quotas and planned hirings are measured in thousand (vertical axis on the left hand side) while the unemployment rate is measured in percentage points (vertical axis on the right hand side). Source: elaboration from ISTAT, Ministry of Labour and Excelsior-Unioncamere data.

It is quite difficult to identify a clear rationale in these fluctuations. Part of the jumps may be explained by changes in the government. For instance, the restrictive turn occurred between 2008 and 2009 coincides with a transition from a left-wing government to a right-wing one.

[14] As discussed in section 3.3.2, in 2005 there was also an extraordinary quota of 350 thousand units which was set to accommodate all the applications in excess of the 190 thousand limit established by the 2005 Flows Decree.

But a similar political transition took place between 2000 and 2001 without any substantial change: if anything, the quotas were increased under the right-wing government elected in 2001.

Given that the quotas should be set accordingly to labor market needs, in Figure 13 we look at two indicators of labour demand: the unemployment rate and a measure of the planned hirings of foreign workers by Italian firms. The dotted line plots the trend in unemployment rate in the period 1998-2012. One can immediately appreciate a fairly long period of substantial improvement in the labor market prospect of Italy, with unemployment rate dropping from 12 percent in 1998 down to about 6 percent in 2007, followed by the negative impact of the economic crisis, which rapidly brought the unemployment back to the levels of the late ´90s (10.7 percent in 2012). The quotas do not seem to strictly follow the trend in unemployment, with a substantial increase in size only at the end of the positive phase of the business cycle (in 2006), followed by an abrupt drop when unemployment starts increasing (in 2009 and 2010), a new increase in 2011 and a further drop in 2012.

Further, we can look at the trend of a specific measure of labor demand for foreign-born workers. Since 2000, indeed, the statistical office of the Italian Chambers of Commerce produces estimates of the number of immigrant workers that Italian firms plan to hire in the following year.[15] As discussed in section 3.3.1.1, these forecasts are (should be) used by the government in establishing the size of the quotas and in deciding how to distribute them across regions. Two clarifications are needed here. First, this measure only considers labour demand coming from firms, and not from households, and, therefore, it underestimates the total demand for immigrant workers. Second, it measures the demand for immigrant workers overall and not just for new-coming (additional) immigrant worker arriving. Indeed, at least part of that demand could be satisfied by the pool of unemployed immigrant workers already residing in Italy. This implies that it would overestimate the demand for new entries of immigrant workers. Figure 13 report the planned hirings from 2000 to 2012 (continuous line). The values oscillates around 200 thousand units per year until 2010 (the average value over this period is almost 190 thousand hirings per year), and then it drops to 112 thousand in 2012. Keeping in mind the two caveats explained above, we can attempt to compare the

[15] The “Sistema Informativo Excelsior”, which is the statistical office of Unioncamere (the Italian Chambers of Commerce), together with the Italian Ministry of Labour, runs this annual survey since 1997. A sample of about 100 thousands firms with at least on employee are interviewed each year and asked about their hirings in the past year and their planned hirings for the current year. The Excelsior data contain detailed information on the industry, qualifications, occupations, etc. the immigrant workers are sought for. For the most recent data, see (Unioncamere, 2011). The data from this survey are available at: http://excelsior.unioncamere.net.

relative size of quotas and (this measure of) labor demand for immigrant workers. From a quick glance at the graph, one can see that, until 2006, planned hirings were larger than quotas by a factor of 2-3 times and that this gap was filled in the period 2006-2008, when quotas almost precisely reflected labor demand. Similarly to what observed for the unemployment rate, the evolution during the crisis years (2009-2012) is much more erratic: while labor demand is going down, quotas look quite jumpy.

3.3.2. Amnesties

During the first years of its immigration experience,[16] Italy has often used general amnesties to ex-post regulate the presence of immigrants in its territory and compensate for the absence of an adequate legal framework to manage inflows of immigrants. Indeed, three amnesties were granted in 1986, 1990 and 1995 which granted legal status to, respectively, 105, 210 and 240 thousand immigrants. As discussed in the previous section (section 3.3.1), in 1998 Italy reformed its migration policy and adopted a quota system to manage inflows of foreign-born workers. Nevertheless, this change in policy regime did not imply that amnesties were abandoned as a policy tool. On the contrary, four more general amnesties - in 1998, 2002, 2009 and 2012 – followed and involved about 1.1 million immigrants (see Table 1).[17]

Italy is not the only European country which has extensively used amnesties in its migration policy. Spain, for instance, granted six general amnesties in roughly the same span of time. And regularization processes took place between 1980 and 2008 also in Austria, France, Greece and Portugal (Casarico, Facchini, & Frattini, 2012). This is quite different from the United States, where the only general amnesty was granted in 1986 (see chapter 6 on the US).

Interestingly enough, the decisions to grant an amnesty have been taken by governments of any political orientation, including the so-called "technical governments". The first two amnesties (1986 and 1990) were granted by centrist governments, the third one by the "technical government" led by Dini in 1995, then was the turn of a left-wing government (in

[16] After almost a century of emigration history towards the US, Northern Europe and South America, Italy has become an immigration country relatively recently. The net emigration balance became negative for the first time in 1973 (Einaudi, 2007), but substantial immigrant flows started arriving only in the '80s. See (Del Boca & Venturini, 2003) for an analysis of both emigration and immigration patterns in Italy.

[17] The 2009 amnesty was limited to domestic and care workers employed by families, while all the other allowed both firms and households to legalize their foreign-born employees.

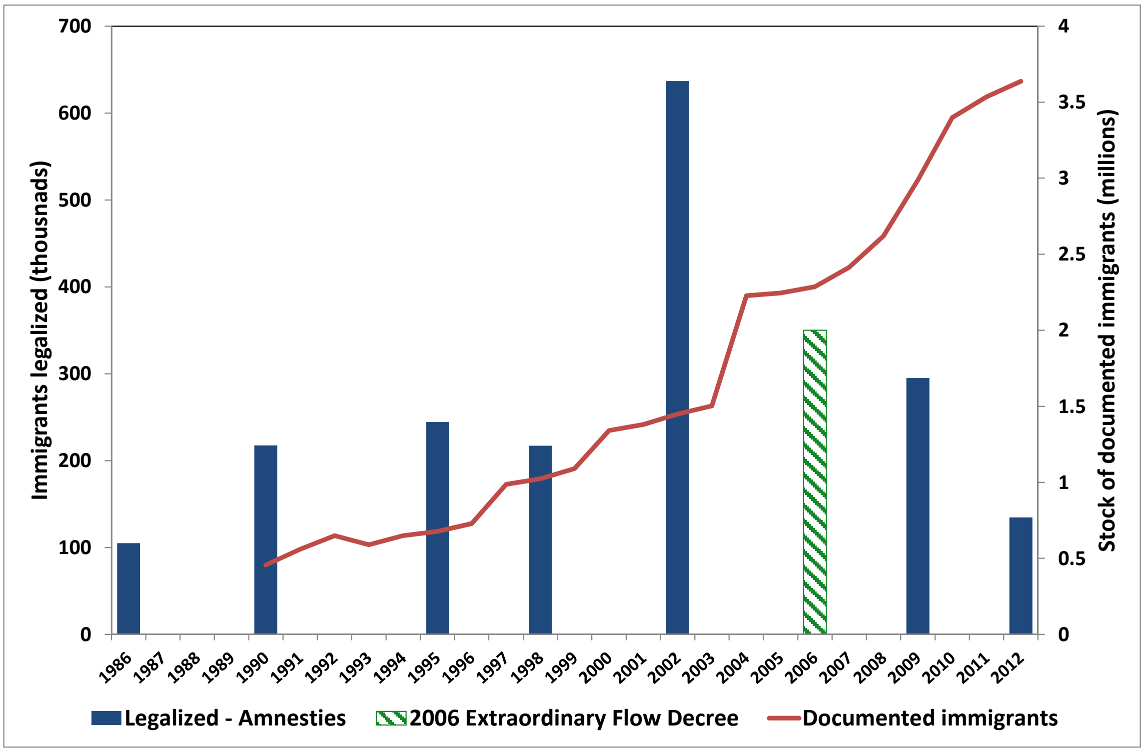
1998), two more procedure were implemented by right-wing governments (in 2002 and 2009) and the last one by the Monti “technical government” in 2012.

Overall, about 1.7 million immigrants have received legal status through one of these procedure: this is a very large number for a country which has a stock of about 4.5 million immigrants in 2011. Even more so, given that – as we have seen in section 3.3.1 – amnesties have not been the only possibility for undocumented migrants to obtain legal residence status. Indeed, the very subtle difference between the actual functioning of the quota system and the amnesties became evident in 2006. The Flows Decree for this year envisaged places for 170 thousand new immigrant workers, but, after receiving more than 500 thousand applications, the government decided to expand the quota to 550 thousand workers: the new quota basically created enough space to potentially accept all the applications and it was substantially equivalent to an amnesty.

The contribution of amnesties (and the pseudo-amnesty of 2006) to the increase in the stock of documented immigrants in Italy is depicted in Figure 14. The bars represent the number of migrants who obtained legal status in each procedure, while the continuous line shows the trend in the legal migrant population as measured respectively by the number of residence permits (continuous line). The immigrant population increased from less than 0.5 million units in the late ‘80s to about 3.5 million individuals in 2012. Especially from the time series of the residence permits, one can clearly recognize substantial jumps in the stock in the year/ two years after each amnesty.

Although hard to measure with precision, one can expect the frequency of the amnesties to have produced a roller-coaster trend in the undocumented population residing in Italy in the last twenty years. Indeed, general amnesties should substantially reduce the stock in the immediate aftermath of the process. Nevertheless, in the absence of a drastic change in migration policy, we can expect this stock to start growing again immediately after the amnesty, and possibly at an even stronger pace than it would have done otherwise due to the recall effect of the amnesty. The ISMU foundation (see Appendix A2) has produced estimates of the undocumented population since 1990: we report these estimates as black round dots in Table 3 . Indeed, the estimates show substantial fluctuations in correspondence of the regularization programs. The dotted line, instead, reports the ratio between the (estimated) undocumented population and the legal population. While the fluctuations reflect those in the stock of unauthorized immigrants, the overall downward trend is the result of the gradual increase in the stock of legal migrants that we have seen in Figure 15. Indeed, if in 1990 an estimated 470 thousand undocumented immigrants accounted for about 50 per cent of the stock of legal immigrants, in 2010 an estimate of 560 undocumented immigrants was just 13 percent of the stock of documented immigrants. Therefore, even if the presence of immigrants without legal status seems to have increased over time – the average estimate was 290 thousand in the period 1990-2000 and it increased to 530 thousand in the period 2001-2010 – it has become less relevant relative to the increase of the legally resident population. As we will see in chapter 6, the picture of the US is quite different: with just one amnesty in 1986, the constant inflow of unauthorized immigrants kept piling up over time, reaching the current 11-12 million stock.[18]

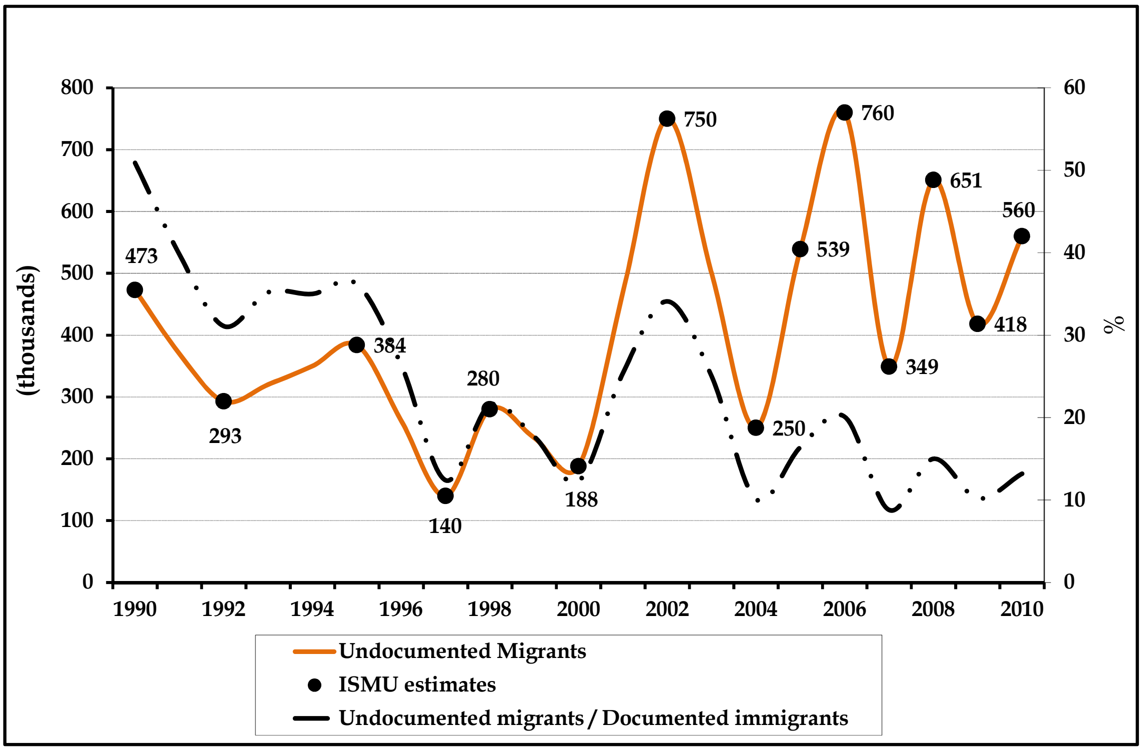
Figure 14 : Stock of documented immigrants and amnesties (Years 1986-2012)



Note: The full (blue) bars report the number of immigrants legalized in each amnesty program, while the striped (green) bar reports the number of immigrants legalized with the 2006 extraordinary Flow Decree (vertical axis on the left). The continuous (red) line shows the stock of documented immigrants (measured as number of residence permits). Source: elaborations from ISTAT data.

[18] (Passel & Cohn, 2008) estimate that in 2008 alone, the U.S. received about 500,000 new unauthorized immigrants.

Figure 15: Estimates of undocumented immigrants (Years: 1990-2010)



Note: The dots correspond to estimates of the stock of undocumented immigrants residing in Italy produced by ISMU (see appendix A2). The dotted line report the ratio of undocumented immigrants over documented immigrants residing in Italy in each year (vertical axis on the left). Source: elaborations from ISTAT and ISMU data.

3.3.3. Migration policy enforcement

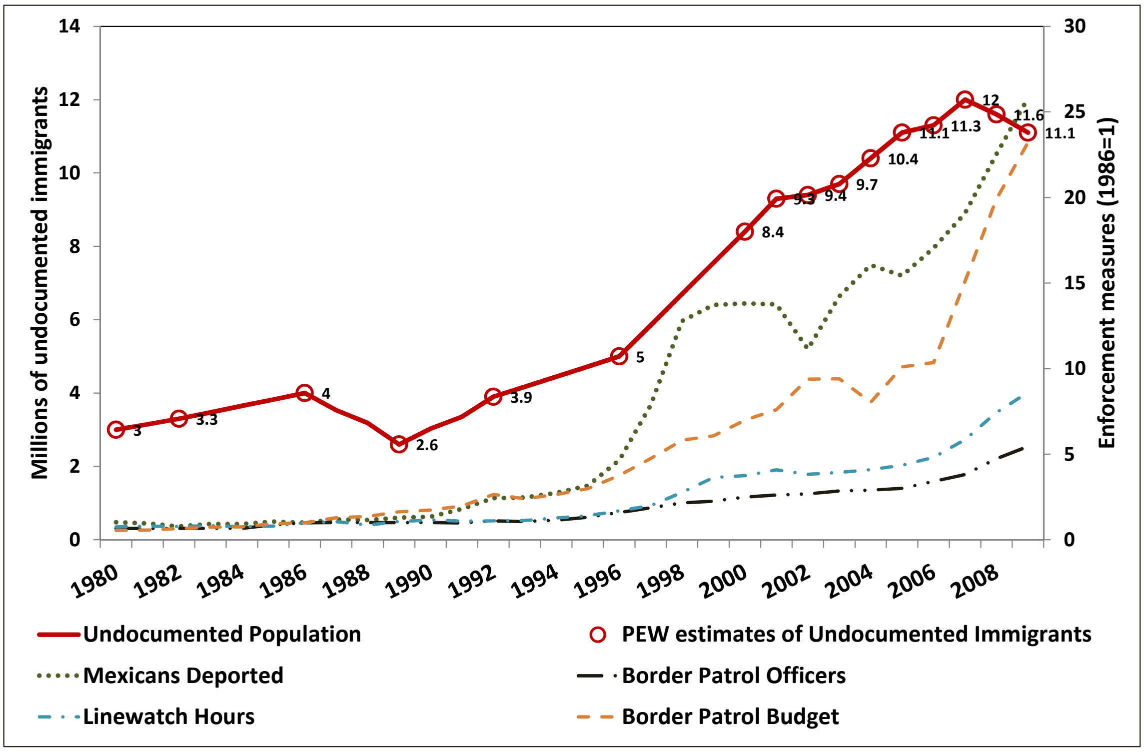
In the political and media discourse about the failures of migration policy in Italy a frequent argument is that Italy basically left its borders open for decades and never really enforced its migration policy. If it had enforced its rule with more decision – the argument goes – Italy would not have experienced such a large and persistent presence of undocumented immigrants in its territory.

This argument may too simplistic. First of all, increasing enforcement is a necessary (maybe) but not a sufficient condition for reducing the stock of undocumented immigrants.[19] Second, when one looks at actual figures – rather than at individual perceptions and anecdotal evidence – of migration policy enforcement in Italy it is far from clear that Italy has been softer on migration relatively to other countries.

[19] See Hanson (2006) for a discussion of the political economy of migration policy enforcement in the US.

We can start from the first point, by looking at a country such as the US which, in the last couple of decades, has uncontroversially invested substantial resources in the enforcement of its migration policy.

Figure 16: US: Border Patrol Enforcement and estimates of undocumented immigrants stock (Year: 1980-2009)



Note: Estimates for the undocumented population (circles) are produced by the PEW Research Center. Enforcement measures are normalized to 1 in 1986 (vertical axis on the right) and are obtained from data provided by the US Department of Homeland Security.

As Figure 16 shows, four different measures of enforcement experienced dramatic increases in the last twenty years. All measures have been normalized to 1 in 1986 when the US completely reformed their migration legislation and passed the first – and last – general amnesty for unauthorized immigrants. Indeed, since 1986, the budget for Border enforcement increased by 23 times: this implied a 5-fold increase in the number of border Patrol Officers, an almost 9-fold increase in the number of hours of Linewatch patrolling and a 25-fold increase in the number of Mexican immigrants deported from the US. Apparently unaffected by this sharp tightening of the migration policy, the stock of undocumented immigrants kept increasing steadily - until the economic crisis kicked in in 2007 - reaching a peak of an estimated 12 million people in 2007 (Figure 16). Actually, it remained fairly constant (oscillating between 2 and 4 million units) during the ´80s and started its dramatic expansion in the early 90s, exactly at the same time with the enforcement effort. Moreover, similarly to what we have seen for the Italian case, the only clear reduction in the stock occurred after the general IRCA amnesty in 1986.

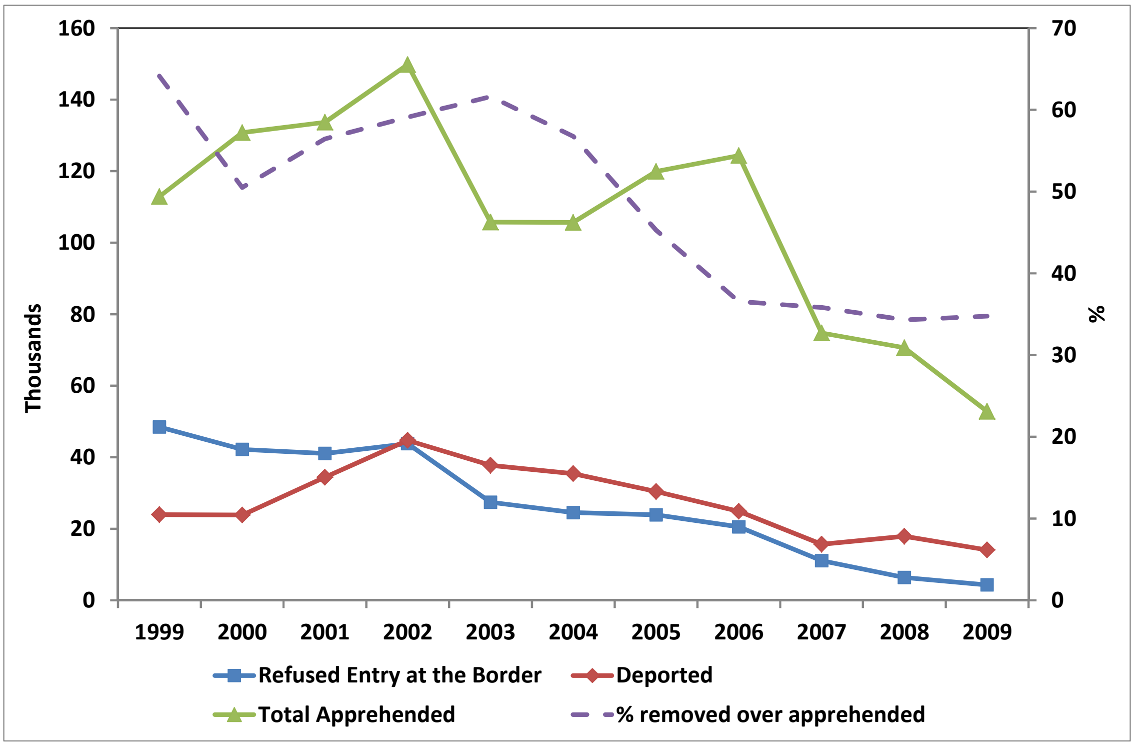
This graph shows that increasing effort may not be enough. The complicate issue here is clearly the absence of a counterfactual: how many undocumented immigrants would be residing in the US now in the absence of that investment in enforcing their migration policy?
We can now look at the performance of Italy, as reported in Figure 17.[20] In the period 1999-2009 – basically, since the major reform of migration policy in Italy in 1998 (see section 3.3) – the Italian police have apprehended a yearly average of more than 100 thousand undocumented immigrants and have managed to remove from Italy, in average, about 50 percent of them. Of the immigrants removed, about half has been stopped and refused entry at the border, while the others have been apprehended within the Italian territory and sent back to their origin countries. Over the period considered (1999-2009), Italy has removed a total number of almost 600 thousand undocumented immigrants. This is not a negligible number, especially if one considers the size of migration and unauthorized migration in Italy over the same period. Still, an equally large number (about 580 thousand individuals) of unauthorized immigrants were apprehended by the Italian police, but they were not forced to move back to their origin countries.[21]

Moreover, Figure 17 clearly shows a marked reduction in the number of immigrants apprehended (in 2007-2009, for instance, there were less than half the apprehensions than in the period 1999-2001). This may be due to a reduction in migration pressure on Italy, as the time series of border entry refusal (which approaches zero in 2009) would suggest. But, one can clearly see a substantial reduction in the effectiveness of the enforcement as measured by the share of total removals over total apprehension. If this ratio was fluctuating between 50

[20] In Italy, the responsibility for the enforcement of the migration policy is spread on different government and police bodies. This factor – together with an a certain endemic lack of transparency of public administration – prevents us from having time series of the investment in enforcement (budget, number of officers, number of officer-hours, etc.). Therefore, we can only look at outcomes of enforcement (undocumented immigrants apprehended, deported, etc.).
[21] These are typically immigrants who received an official order to leave the country but whose removal was not enforced for different reasons. In some cases, the immigrant receives the formal removal injunction but she is then released. In other cases, the order cannot be enforced because the Italian authorities do not manage to correctly identify the immigrant, or because the country of origin does not recognize the immigrant as one of its citizens and refuse to accept her. Finally, there may be conditions of the immigrant (e.g. pregnancy, risk of persecution, etc.) and/or of the origin country (e.g. civil conflicts, wars, etc.) which forbid her removal.

and 60 percent until 2004, it seems to have moved down to the 30-40 percent range in more recent years.

Figure 17: Italy: Border and internal enforcement (Years 1999-2009)



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

3.4. Migration and crime in Italy

3.4.1. Italian and immigrants: from criminal charges to prison

As we have seen throughout this chapter, Italy is a country where a substantial fraction of the current stock of documented migrants has lacked legal residence status at the initial stage of its presence in Italy. If being undocumented increases the incentives to commit crime (by lowering the value of outside options; see Chapter 2), we can expect a substantial involvement of immigrants in crime.

A first (approximate) answer to the question "do immigrants commit crime?" can be provided by looking at whether immigrants are over- rather than under- represented among the population of "criminals". We do this in Figure 18, where we analyze all the different stages – charges, convictions and imprisonments – of the interaction of immigrants with the Italian judicial system for the last two decades (1991-2011). In each year, the graph reports the share of migrants over the total population of: 1) individual charged (for some criminal offence); 2) individuals convicted; 3) individuals who entered a prison after a conviction; 4) individuals currently detained in a prison. In order to benchmark these shares, we report also the fraction of migrants over the total population in Italy. We comment the figures for the most recent years, but, as one can appreciate from a quick glance at the graph, the patterns were already there in the early '90s.

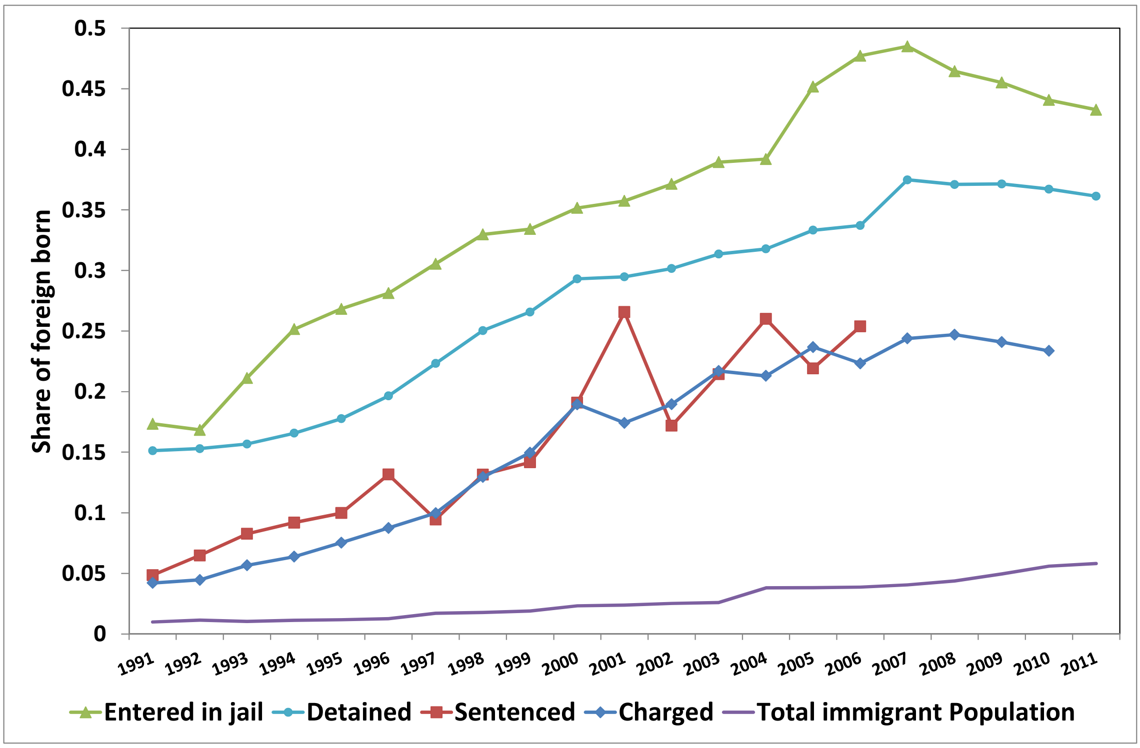
Immigrants are clearly and substantially overrepresented among the population of citizens who are charged for some criminal offence. In 2010, indeed, immigrants accounted for almost 23 percent of the criminal charges although they represented only 6-7 percent of the resident population. Even if we add the estimated 560 thousand undocumented immigrants who were residing in Italy in 2010 according to ISMU (see Figure 15), the picture does not change in any relevant way: total immigrant population would account for 7-8 percent of the resident population and the substantial overrepresentation among the subject who receive a criminal charge would still be there.

If we then move to the next step, i.e. the convictions, the picture remains quite similar. In 2006 (the last year for which conviction data for immigrants are available), 22 percent of the charges and 25 percent of the conviction regarded immigrants. As one can see from the graph, conviction rate for immigrants has not always been above the charge rate: at least since the late ´90s, the two figures have closely followed each other and in some years the latter has even been above the former. This would seem to suggest that, conditional on having been charged, immigrants are not disproportionately more likely to receive a conviction. This could happen if the judicial system – voluntary or not – is biased against immigrants (e.g. police forces are more accurate in gathering evidence against immigrants; immigrants can afford lower quality lawyers and their limited knowledge of language and legislation make them more vulnerable; judges discriminate against immigrants, etc.).[22] On the other hand, convictions arrive with a lag with respect to charges. If we consider an average lag of two-three years between charges and convictions, we would conclude that the 25 percent conviction rate of

[22] We are not aware of any systematic study of discrimination of immigrants in the Italian judicial system. For the US, recent empirical evidence shows that minorities tend to be disadvantaged in court, being either more likely to receive a sentence, or to receive longer sentences, or both (see, among others, Mustard, 2001 and Abrams et al. 2012).

2006 refers to the 20-21 percent charge rate of 2003-2004. The gap between the two rates becomes more sizeable, but it still remains far from substantial.

Figure 18: Foreign born immigrants and the Italian judicial system (Years: 1991-2011)



Note. Elaborations from data of the Italian Minister of Internal Affairs.

An impressive jump, instead, takes place when we move from convictions to jail. In 2011, immigrants accounted for a stunning 43 percent of individuals entering jail and for 36 percent of the stock of inmates. These are figures often used to support the argument of a disproportionate large participation of immigrants in crime in Italy. But they need to be carefully analyzed. How comes that 25 percent of conviction rate for immigrants in 2006 implies that they account for 48 percent of entries in jail in the same year? There is a limit to what we can say about this aspect using aggregate data – more research would be badly needed in this area – but we can make a few points.

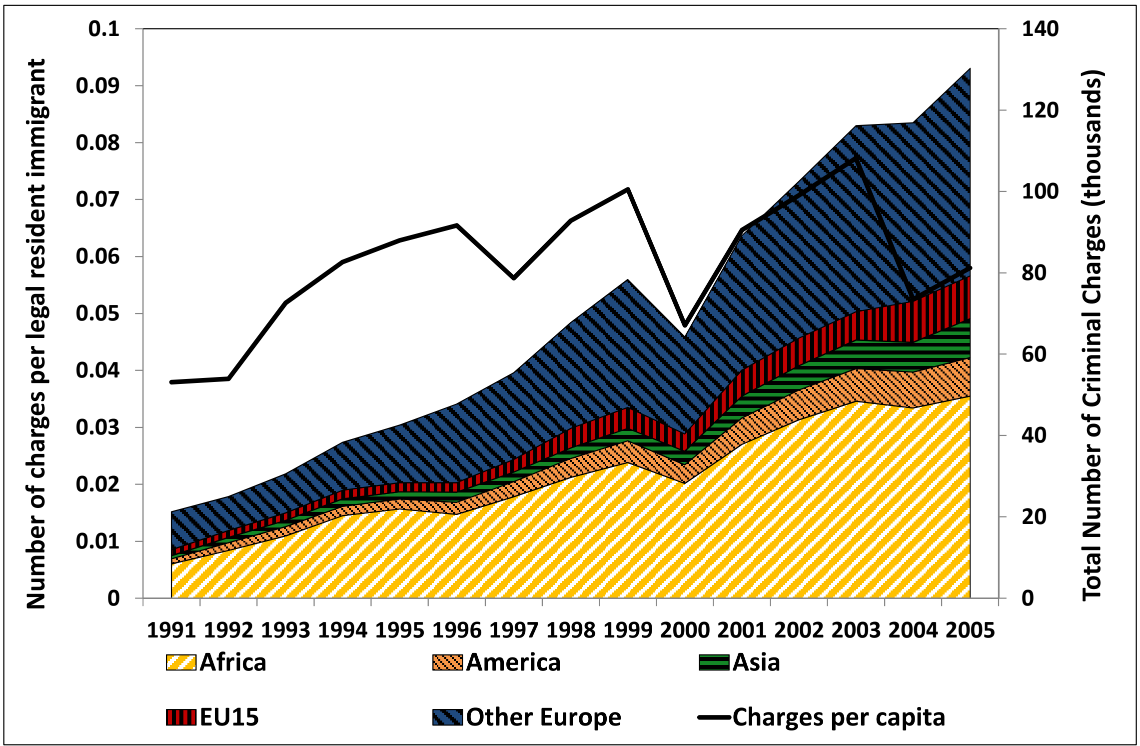
First, immigrants are more likely to enter jail before receiving a final conviction than Italians. About 47 percent of the foreign born citizens who were detained in 2011 were still waiting for their final conviction (in any), while this share was 10 percentage points lower for Italian citizens (Table 2). Second, immigrants who are in jail with a definitive conviction are substantially more likely to have received shorter sentences than Italians (Table 3). In 2011, indeed, immigrants accounted for almost 50 percent of those with final sentences between 0-1 years and for only 10 percent of those who received sentences longer than 20 years. Further, 65 percent of the immigrants had a final sentence shorter than 5 years, while the corresponding figure for Italian citizens is 44 percent. This sizeable difference in the distribution of sentence duration may simply reflect a relative specialization of immigrants in petty crime with respect to natives. But it is also due to the fact that convicted immigrants are less likely to be given house arrest or to be assigned to alternative measures (i.e. outside prison) than Italians (Istat, 2012).[23] This is the third element we consider. In 2011, 30.7 percent of the Italian citizens convicted to detention were assigned to alternative measures, while for immigrants this figure was down to 12.7 percent (Table 4). Alternatively, immigrants account for 31 percent of convictions to detention and, within this set, for 36 percent of those who are jailed and for just 16 percent of those who receive alternative measures. This higher probability of being assigned to prison upon a conviction to detention is primarily due to the fact that immigrants often do not fulfill the conditions - having a regular job, having a domicile, having a family able to host the individual, etc. – which are required in order to apply for measures alternative to detention in jail.

3.4.2. Areas of origin

Using data on criminal charges for the period 1991-2005, we can look at the area of origin of immigrants involved in criminal activities. As Figure 19 shows, Africa and Other European countries (non EU-15) account for the vast majority of the charges. Indeed, in average during the period considered, they accounted for, respectively, 44 and 37 percent of the charges. Each of the remaining three areas (America, Asia and EU15), instead, accounted for roughly 5-6 percent of the total number of charges. This composition directly reflects the composition of the immigrant population residing in Italy, which is predominantly composed by Eastern Europeans and Africans (see section 3.2).

[23] Italy has a detention rate sizably lower than other European countries but the share of individuals who are convicted to detention and are then assigned to alternative measures is also substantially lower. For instance, both France and the UK in 2010 had a number of convicted assigned to alternative measures which was almost 3 times larger than the number of those being imprisoned. On the contrary, this figure was down to 25 percent in Italy (**Table 4**) (Istat, 2012).

Figure 19: Criminal charges of immigrants, by macro area of origin (1991-2005)



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

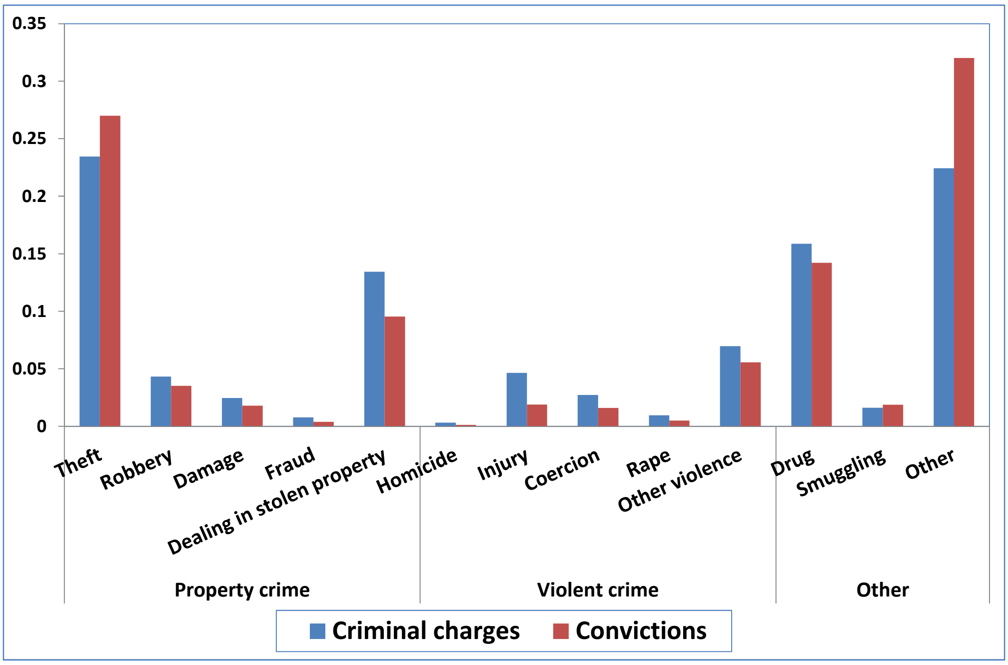
Similarly, the sharp increase in the number of charges – from about 21 thousand in 1991 to 130 thousand in 2005 – follows the trend in the stock of resident immigrants, which quadrupled in the same period. When we look at the number of criminal charges per legal resident immigrant (the dark continuous line in Figure 19), we can appreciate an increase from less than 0.04 charges per capita in 1991 to almost 0.06 in 2005. Still, this measure experiences wide fluctuations in the period considered. Similarly, one can notice that the upward trend in total charges shows two clear breaks in 2000-2001 and in 2003-2004.

3.4.3. Criminal offences

We can now briefly look at the type of criminal offences committed by immigrants in Italy.

Figure 20 reports the distribution by criminal offence of both charges and convictions of foreign born (values are averaged over the period 2000-2005). Immigrants are mainly involved in property crime, which accounts for about 45 percent of the charges and 42 percent of the convictions. Within this category, the two main offences are theft (23 percent of charges and 27 percent of convictions) and dealing in stolen property (13 percent of charges and 9 percent of convictions). A much smaller fraction of immigrants, instead, commits violent crime: overall, 15 percent of the charges and 10 percent of the convictions. Finally, the residual category of “other crime” is responsible of 39 percent of the charges and 45 percent for the convictions. In this category, we find offences such as drug crime (which, alone, accounts for about 15 percent of both charges and convictions), smuggling, dealing with counterfeit goods, resistance to public officers, infringements of migration laws, etc.

Figure 20 :Criminal charges and convictions of foreign born, by criminal offence (average 2000-2005)



Notes: Elaborations from data of the Italian Minister of Internal Affairs.

3.4.4. The role of legal status

As anticipated in the first chapter, undocumented immigrants are responsible for the vast majority of crimes committed in Italy by immigrants. Table 5 reports the share of immigrants without residence permit among those charged by the Italian police of some criminal offence during the six most recent years for which data are available (2004-2009) and for ten years before (1994-1999). One can immediately note that the share of undocumented immigrants varies between 60 and 70 percent for violent crimes, and it increases to 70-85 for property crime. In 2009, the highest shares are in burglary (85), car theft (78), theft (76), robbery (75), assaulting public officer / resisting arrest (75) , handling stolen goods (73). Interestingly, this overrepresentation of unauthorized immigrants among those charged of crime seems to be a stable feature in the Italian context: not only these shares were already high in the mid `90s, but one can observe little variation over time (Table 5). These figures provide a strong motivation for the empirical analysis we perform in the next two chapters: in the Italian context, there is a strong and persistent link between lacking legal status and propensity to engage in crime.

3.4.5. Immigrants as victims of crime

The substantial overrepresentation of immigrants among the authors of crime that we have documented so far is present also when we look at victimization. The share of immigrants is particularly high among the victims of violent crime. As Table 6 shows, in 2009 about 33 (29) percent of attempted murders against women (men) in Italy regarded immigrant women (men), while about 24 (26) of women (men) actually murdered were immigrant women (men). Similarly, immigrants were the victims of 31 (21) percent of sexual violence against women (men) and of 22 (19) percent of unlawful wounding against women (men). Immigrants are also substantially overrepresented among the victims in criminal offences against property such as extortion, robbery and theft.

Together with the data on crime, these figures on victimization suggest that immigrants in Italy live in truly uncertain conditions which increase both their incentives to engage in and their exposure to crime.

3.5. Conclusions

In this chapter, we have introduced the Italian setting and described the policies we will use in our empirical analysis in the following chapters of the report. In section 3.2, we have briefly described the evolution and some characteristics of the immigrant population in Italy. In section 3.3, we have summarized the Italian migration policy, with a specific focus on the two main policy tools – the quota system and the amnesties – used in this context to manage immigrant flows. Finally, in section 3.4 we have reported evidence of the involvement of immigrants in criminal activities and discusses their overrepresentation at the different stage of the judicial system (charges, convictions, imprisonment).

3.6. Tables

Table 1: Number of applications for the amnesties and geographical distribution by macro-area

Amnesty	Area				
	North-West	North-East	Central	South	Italy
1986	24,296	11,678	33,056	35,970	105,000
	23.1	*11.1*	*31.5*	*34.3*	
1990	54,969	31,337	72,116	59,204	217,626
	25.3	*14.4*	*33.1*	*27.2*	
1995	74,651	39,959	73,165	56,717	244,492
	30.5	*16.3*	*29.9*	*23.2*	
1998	80,740	29,608	68,281	38,495	217,124
	37.2	*13.6*	*31.4*	*17.7*	
2002	233,943	132,291	203,852	132,070	702,156
	33.3	*18.8*	*29.0*	*18.8*	
2009	105529	58289	61144	70150	295112
	35.8	*19.8*	*20.7*	*23.8*	
2012	45761	26440	29243	33132	134576
	34.0	*19.6*	*21.7*	*24.6*	

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

Table 2: Inmates by status: Italian and Foreign Born (Year 2011)

		Italian		Foreign Born		Total		% Foreign Born
			%		%		%	
Total untried	Waitig for first sentence	8169	*19.12*	5530	*22.88*	13699	*20.48*	*40.37*
	Appeal	6445	*15.09*	5612	*23.22*	12057	*18.02*	*46.55*
	Other	1245	*2.91*	324	*1.34*	1569	*2.35*	*20.65*
		15859	*37.12*	11466	*47.43*	27325	*40.85*	*41.96*
Total condemned	Final conviction	21911	*51.29*	11490	*47.53*	33401	*49.93*	*34.40*
	Other	3568	*8.35*	1054	*4.36*	4622	*6.91*	*22.80*
		25479	*59.64*	12544	*51.89*	38023	*56.84*	*32.99*
Other	Subject to “security measures”	1385	*3.24*	164	*0.68*	1549	*2.32*	*10.59*
	Total	42723	*1*	24174	*1*	66897	*1*	*36.14*

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

Table 3: Inmates with a definitive conviction, by sentence duration: Italian and Foreign Born (Year 2011)

	Italian			Foreign Born			Total			% foreign born
		%	cumulative %		%	cumulative %		%	cumulative %	
0-1 years	1383	*5.43*	*5.43*	1350	*10.76*	*10.76*	2733	*7.19*	*7.19*	*49.40*
1-2 years	2119	*8.32*	*13.74*	1603	*12.78*	*23.54*	3722	*9.79*	*16.98*	*43.07*
2-3 years	2448	*9.61*	*23.35*	1852	*14.76*	*38.31*	4300	*11.31*	*28.29*	*43.07*
3-5 years	5305	*20.82*	*44.17*	3384	*26.98*	*65.28*	8689	*22.85*	*51.14*	*38.95*
5-10 years	7516	*29.50*	*73.67*	3062	*24.41*	*89.69*	10578	*27.82*	*78.96*	*28.95*
10-20 years	3583	*14.06*	*87.73*	1043	*8.31*	*98.01*	4626	*12.17*	*91.12*	*22.55*
20 years +	1659	*6.51*	*94.25*	188	*1.50*	*99.51*	1847	*4.86*	*95.98*	*10.18*
Life sentence	1466	*5.75*	*100*	62	*0.49*	*100*	1528	*4.02*	*100*	*4.06*
Total	25479	*100*		12544	*100*		38023	*1*		*0.33*

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

Table 4: Convicted to detention assigned to prison and to alternative measures (Years 2008-2011)

	Italian convicted				Foreign Born convicted				Total convicted				% Foreign Born		
	Prison	Alternative measures	Total	*% alternative measures*	Prison	Alternative measures	Total	*% alternative measures*	Prison	Alternative measures	Total	*% alternative measures*	Prison	Alternative measures	Total
2008	36565	9151	45716	*20.02*	21562	1069	22631	*4.72*	58127	10220	68347	*14.95*	*37.09*	*10.46*	*33.11*
2009	40724	11895	52619	*22.61*	24067	1521	25588	*5.94*	64791	13416	78207	*17.15*	*37.15*	*11.34*	*32.72*
2010	43007	16315	59322	*27.50*	24954	2120	27074	*7.83*	67961	18435	86396	*21.34*	*36.72*	*11.50*	*31.34*
2011	42723	18900	61623	*30.67*	24174	3523	27697	*12.72*	66897	22423	89320	*25.10*	*36.14*	*15.71*	*31.01*

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

Table 5: Share of undocumented immigrants among total criminal charges against foreign born citizens, by criminal offence (Period: 1994-2009)

		1994	1995	1996	1997	1998	1999	2004	2005	2006	2007	2008	2009
Violent crime	Affray	*80*	*83*	*74*	*73*	*75*	*69*	*59*	*58*	*61*	*62*	*69*	*64*
	Attempted murder	*79*	*82*	*79*	*76*	*79*	*71*	*67*	*67*	*72*	*71*	*71*	*70*
	Exploiting prostitution	*73*	*76*	*76*	*71*	*74*	*70*	*60*	*58*	*63*	*64*	*67*	*65*
	Murder	*75*	*82*	*69*	*83*	*79*	*83*	*72*	*69*	*74*	*70*	*70*	*69*
	Rape	*70*	*78*	*74*	*70*	*65*		*60*	*63*	*62*	*58*	*64*	*60*
	Unlawful wounding	*78*	*80*	*74*	*69*	*72*	*68*	*62*	*61*	*62*	*60*	*64*	*62*
Property crime	Burglary							*80*	*82*	*82*	*84*	*87*	*85*
	Car theft	*90*	*92*	*91*	*88*	*88*	*85*	*80*	*83*	*84*	*84*	*84*	*78*
	Criminal damage	*80*	*83*	*77*	*77*	*78*	*74*	*70*	*71*	*71*	*69*	*73*	*71*
	Extortion	*73*	*79*	*71*	*72*	*74*	*71*	*63*	*64*	*68*	*64*	*69*	*68*

	Handling stolen goods	*77*	*83*	*79*	*80*	*83*	*78*	*68*	*68*	*70*	*72*	*77*	*73*
	Robbery	*86*	*87*	*85*	*81*	*83*	*81*	*74*	*75*	*79*	*79*	*79*	*75*
	Theft	*90*	*92*	*89*	*88*	*90*	*85*	*78*	*79*	*80*	*80*	*80*	*76*
Other crime	Assaulting public officer; resisting arrest	*81*	*85*	*79*	*75*	*77*		*69*	*70*	*74*	*77*	*79*	*75*
	Smuggling	*83*	*90*	*83*	*86*	*89*		*58*	*64*	*71*	*53*	*53*	*56*
	Unlawful possession of weapon	*85*	*85*	*84*	*81*	*82*	*78*	*75*	*76*	*75*	*71*	*73*	*71*

Note: Elaborations from Barbagli and Colombo (2011)

Table 6: Share of immigrants among victims of crime, by gender and major criminal offences (Years: 2004-2009)

		Women						Men					
		2004	2005	2006	2007	2008	2009	2004	2005	2006	2007	2008	2009
Violent crime	Attempted murder	*22.3*	*26.4*	*20.3*	*26.5*	*29.9*	*33.2*	*22.9*	*26*	*23.3*	*26.6*	*27.4*	*29.4*
	Murder	*23.5*	*23.3*	*23.2*	*23.3*	*27.5*	*23.6*	*19.1*	*17.3*	*21.8*	*21*	*26*	*25.9*
	Sexual violence	*27.8*	*28.6*	*29.3*	*31.8*	*32.4*	*31.3*	*17.3*	*20.7*	*18.6*	*21.5*	*21.9*	*21.2*
	Unlawful wounding	*16.5*	*17.5*	*18.7*	*20.8*	*20.8*	*21.8*	*15.8*	*15.9*	*16.4*	*18.1*	*18.7*	*18.8*
Property crime	Burglary	*5.3*	*5.5*	*5.7*	*6.2*	*6.3*	*6.4*	*4.3*	*4.5*	*4.6*	*5*	*4.9*	*4.7*
	Car theft	*3*	*3.7*	*4.3*	*4.6*	*4.7*	*4.9*	*4.1*	*4.6*	*5.2*	*5.3*	*5.3*	*5*
	Extortion	*19.3*	*19.1*	*18.8*	*22.9*	*19.6*	*24.7*	*9.5*	*9.9*	*10.4*	*12.9*	*12.9*	*13.5*
	Fraud	*3.6*	*3.5*	*3.3*	*3.8*	*5.6*	*5.1*	*3.7*	*3.2*	*3.1*	*3.7*	*5.4*	*5.3*
	Robbery	*15.4*	*15.6*	*17.5*	*20.4*	*22*	*23*	*13.8*	*12.7*	*12.4*	*13.7*	*13.7*	*14.1*
	Theft	*12*	*12*	*13.7*	*14.8*	*14.7*	*13.8*	*19.3*	*18.5*	*20*	*20.6*	*19.1*	*17.1*

Note: Elaborations from Barbagli and Colombo (2011)

3.7. Appendix

A1. Italian legislation on migration

The first attempt of defining an extensive framework to regulate migration and to shape Italian migration policy is made only in 1990, after more than a decade of migrants' inflows. A marginal intervention, and the first legalisation process, were decided in 1986, applying an ILO convention (no. 143, 1975) aimed at establishing the principle of equality of treatment between foreign and native workers. The legislation of entrance and residence permits for foreigners, instead, was regulated by the so called "Codice Rocco", a royal decree dated 1931, until the "Law Martelli" (39/1990) established new rules and tried to introduce a "planned number" of new entrants each year. As a matter of fact, this number was never fixed and it remained equal to zero, while the immigrants kept entering the country from the "back door", either by passing the borders irregularly or by overstaying their tourist visas. In 1990 the second amnesty was held.

The following years were dominated by the political crisis of Albania (1990 and 1997) and former Yugoslavia (1995, Bosnian war, and 1997, Kosovo war) that produced huge flows of refugees reaching the near Italian coasts. The government response was that of emergency legislation and "ad hoc" interventions, with a new amnesty process opened in 1995.

In 1998, under the pressure of the commitment to the Schengen Convention, a left-wing government passed the "Turco-Napolitano Law" no. 40/1998 (later confirmed by the Single Act no. 286 of July 25, 1998), in which the Italian migration policy was extensively defined in every single aspect, from the discipline of entry, residence and working conditions, to that of deportations and control of the illegal phenomenon. Apart from a new emphasis on the need to curb undocumented immigration, the main innovation was an effective introduction of a "programmed entries" system of foreign workers via quotas to be established yearly. At the same time the fourth amnesty was approved.

In 2002 the previous legislation was modified by the "Bossi-Fini Law" (no.189/2002). Passed by the current right-wing government coalition, its main declared target was increasing the effectiveness of the irregular immigration contrast. The same intervention opened the fifth, and last, legalisation process.

A2. The ISMU survey

The ISMU survey is an annual survey started in 2001 which interviews a representative sample of about 8 thousand documented and undocumented migrants residing in the Lombardy region, one of Italy's largest (8% of the Italian territory), most populated (9.6 million of inhabitants in 2008, about 16% of the Italian population), and wealthiest regions. It also has the largest migrant population of both documented (23 per cent of the entire migrant population legally residing in Italy in 2005) and undocumented migrants (22 per cent of the amnesty applications in the last regularization process in 2002). The ISMU survey is conducted by the Fondazione ISMU (Iniziative e Studi sulla Multietnicità), an autonomous and independent organization promoting studies, research and projects on multi-ethnic and multi-cultural society, and focusing in particular on the phenomenon of international migrations (www.ismu.org).

The interview questionnaire contains a variety of questions on individual characteristics (e.g., demographics, educational level, labour market outcomes, legal status) and household characteristics (e.g., number of household members in Italy, family members abroad, housing).

To elicit truthful reporting of legal status, the interviews are anonymous, ask for no sensitive information (e.g., addresses), and are carried out in public spaces by foreign-born interviewers (when possible, from the same country as the interviewees) who emphasize the independence of the ISMU Foundation from any Italian government body. The information on legal status is obtained by asking the immigrants about the type of legal documents they have, starting with the most permanent (being an Italian citizen) and moving down to the option of "no documents".

The ISMU data are sampled using an intercept point survey methodology based on the tendency of immigrants to cluster at certain locations (Blangiardo G., 2008; McKenzie & Mistiaen, 2009). The first step is to create a list of popular intercept points (e.g., ethnic shops and gatherings, churches, health care facilities) and then randomly select the meeting points and the migrants who visit them for interview. At each location, interviewees are asked how often they visit any of the other meeting points, which allows ex-post selection probabilities to be computed into the sample. The Italian government officially recognized the reliability of this technique in 2005, when it commissioned and financed survey implementation at the national level, with over 30 thousand immigrants interviewed. See (Strozza, 2004) for a survey of the different methodologies used to estimate undocumented migrants in the Italian context.

Chapter 4 – Legal Status and Crime in Italian Cities: Evidence from Policy Experiments

4.1. Introduction

In this chapter, we exploit policies which exogenously granted legal status to large fractions of the undocumented population in order to empirically investigate whether similar changes in legal status induced significant reductions in immigrants' crime rates in Italy. In particular, we will present aggregate evidence from different amnesty programs and from the 2007 *click-day* (see the previous chapter for a detailed discussion of these policies). Empirical evidence from individual data for the 2007 *click-day* will be presented in the following chapter.

4.2. Does immigrants' crime fall after an amnesty?

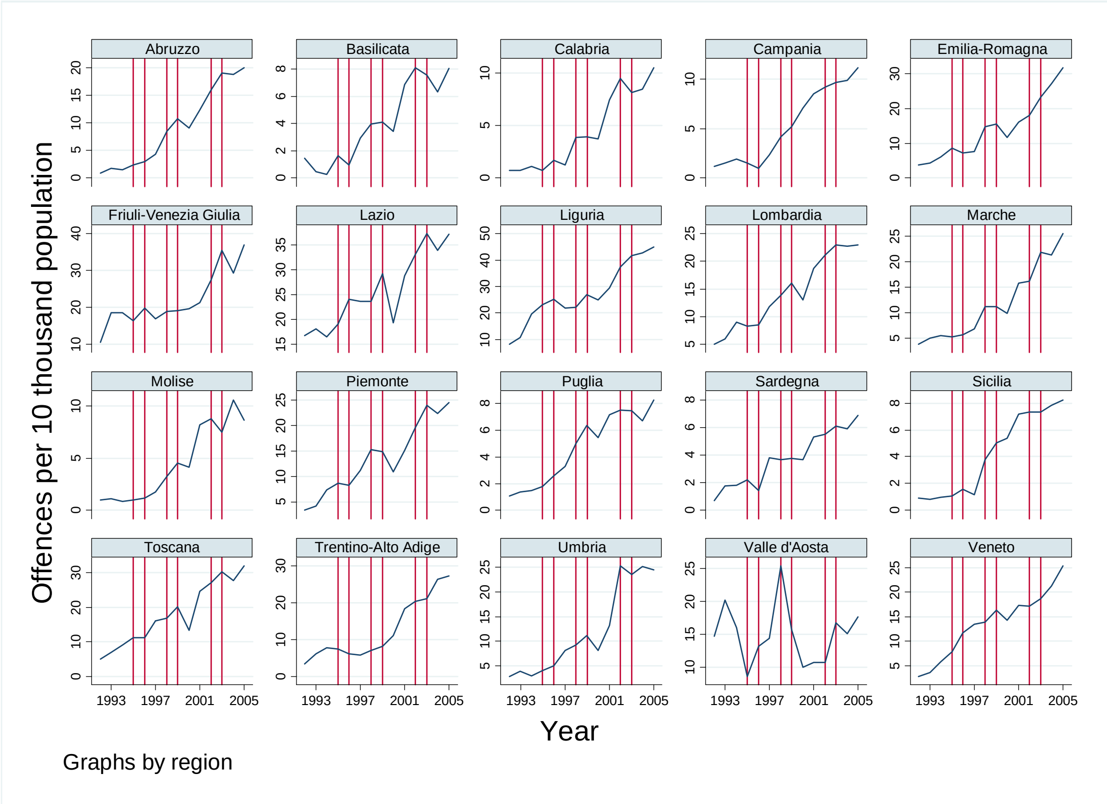
Amnesty programs provide a potentially ideal setting for analyzing the relationship between legal status and crime. Indeed, if lacking legal status implies a higher propensity to commit crime – and as long as an amnesty manages to reduce the stock of undocumented immigrants residing in a country – one would expect to observe a drop in crime after the legalization takes place. On the other hand, crime could increase after an amnesty if the process attracts more undocumented immigrants than it manages to legalize. A positive effect of crime (in the sense of increasing it) could also be observed if the undocumented immigrants who do not manage to obtain legal residence in the country through the current amnesty are discouraged in their economic integration by the prospect of waiting several years before the next legalization program, and decide to engage in more criminal activities.

Figure 21 suggests that amnesties in Italy may have produced short-term reductions in immigrants' crime rate. We have plotted the total number of criminal charges of immigrants (per 10 thousand population) for the period 1993-2005 for each of the 20 Italian regions.[24] One can immediately notice the steep increasing trend in immigrants' crime rates in all regions, which mirrors the expansion of the foreign born population which we have documented in the previous chapter. The vertical lines on the graphs sign the three general amnesties (1995-96; 1998-99; 2002-03) granted in this period. Although with substantial heterogeneity across regions, one can notice that in amnesty years – or in the years immediately following an

[24] The data used in these graphs are described in appendix 110B.a.

amnesty – regional crime rates of immigrants experienced a marked slow-down of the increasing trend or, even, a reduction.

Figure 21: Immigrants' crime and amnesties (1992-2005)



Notes: The graph plots the total number of criminal charges of immigrants (per 10 thousand population) for the period 1992-2005 for each Italian region. The vertical lines sign the three general amnesties granted in this period (1995-96; 1998-99; 2002-03).

This graph provides suggestive evidence of a causal relationship between policies which grant legal status to the undocumented population and the criminal behavior of immigrants. In this section, we will investigate this relationship with a fully-fledged econometric analysis. In particular, we will first exploit the series of repeated amnesties granted in Italy in the last two decades (see previous chapter) and we will then focus on the largest amnesty which has so far been held in Italy (the 2002 amnesty).

4.2.1. Evidence from repeated amnesty programs

The frequency of amnesties of undocumented immigrants in Italy suggests looking for evidence of drops in immigrants' crime in the immediate aftermath of these legalization processes. Although each of the amnesties took place in the entire country at the same point in time, the intensity of the *legalization treatment* may have varied across different areas depending on the number of immigrants legalized during each programs. Indeed, if legal status matters for immigrants' decisions to engage in crime, one could expect to observe immigrants' crime to experience larger drops in areas where a larger number of immigrants was granted legal status.

Exploiting repeated amnesties in Italy, we can answer two empirical questions in this section. First, which is the elasticity of immigrants' crime to changes in the number of undocumented immigrants who received legal status through an amnesty program? Second, what is the timing of the effect?

4.2.1.1. Empirical strategy

For the empirical analysis discussed in this section, we regress the yearly change in immigrants' crime rate in each region on the number of immigrants legalized in that region by an amnesty in the same year. Clearly, this latter variable will be equal to zero if there was no amnesty in that particular year. Hence, we estimate the following regression:

$$\Delta\ln\left(\frac{CR_{rt}^{F}}{Pop_{rt}}\right)=\beta_1\ln\left(\frac{L_{rt}}{Pop_{rt}}\right)+\ \Delta X_{rt}'\gamma+\Delta\mu_t+\Delta\varepsilon_{rt} \qquad [1]$$

where:

- Δ : first difference operator (difference in the variable between *t* and *t-1*);
- $\ln\left(\frac{CR_{rt}^{F}}{Pop_{rt}}\right)$: log of the ratio of total number of criminal charges of foreign born individuals over total resident population in region r;
- $ln\left(\frac{L_{rt}}{Pop_{rt}}\right)=\begin{cases}ln\left(\frac{L_{rt}}{Pop_{rt}}\right) & if \quad amnesty_t=1\\ 0 & otherwise\end{cases}$

  where:
  - L_{rt} is the number of immigrant legalized with the amnesty in time *t* in region r;
  - $amnesty_t$ is a dummy which is equal to one if there was an amnesty in time *t* and to zero otherwise;
- X_{rt} is a set of time-varying regional controls;
- μ_t are year dummies
- ε_{rt} is an error term.

The coefficient of interest (β_1) identifies the elasticity of immigrants' crime rate to the intensity of the *legalization treatment.* Finding a negative (and statistically significant) coefficient, would suggest that regions which legalized a larger number of undocumented immigrants in amnesty years, experienced an immediate drop in immigrants' crime with respect to the previous year. Clearly, the effect of the legalization does not have to be immediate. Indeed, given that the process of assessment of the applications usually takes several months, one can expect to observe some lag for the effect on crime to become visible. The timing of the effect clearly depends on the timing of the procedure itself. But it also depends on whether the crime-reducing effect of legalization is produced only once the legal status is actually granted, or if the simple prospect of becoming legal already has some effect on criminal decisions. In other words, do immigrants reduce their engagement in crime only when they become legal or do they anticipate (at least part of) this change while they are still applicants for legal status? In order to investigate the timing of this effect, we have gradually included the 1st and 2nd lag of the *legalization treatment* $(ln\left(\frac{L_{rt}}{Pop_{rt}}\right))$. Further, we have included the 1st lead of this variable: insofar as we do not expect to observe a reduction in crime before the amnesty is announced, this can be considered as a falsification exercise.

In order to perform this exercise, we use data on the total number of criminal charges of foreign born citizens in each of the 20 Italian regions for the period 1991-2005 (see appendix B.a).[25] During this span of time, three general amnesties – in 1995, 1998 and 2002 – were granted (see chapter 3). In addition, an amnesty program had been opened in 1990, and its effects should potentially be observable at the beginning of the period we consider.

In the amnesties, no exogenous cap to legalization was imposed in any of the regions.[26] This implies that the implementation of the policy does not necessarily guarantee the exogeneity of the *treatment intensity* (i.e. the number of immigrants legalized) in different areas. In all the three amnesties considered, the share of applicants who were granted legal status was about 90-95 percent. The number of legalized immigrant, therefore, closely follows the number of

[25] Data on committed crime cannot be used for this analysis because the nationality of the offender is not known (given that the offender is unknown). In order to work with data on immigrants' crime, one can only use criminal charges, convictions and data on inmates. Clearly, data on convictions and inmates have a substantial lag with respect to when the crime was actually committed. The lag is present also with criminal charges but it is substantially smaller. The presence of this lag is an additional reason to expect to observe a lagged effect of the legalization program on criminal charges against immigrants.

[26] As discussed in the previous chapter, the presence of exogenously determined regional quotas is one of the main differences in the *de-facto* implementation of the Italian quota system with respect to the general amnesties.

applications filed in by the undocumented immigrants and, therefore, is determined by their endogenous decisions to apply for legal status. Clearly, one could expect undocumented immigrants to have more incentives to apply in areas where the return to having legal status is higher. Indeed, in regions which offer more opportunities of legal employment, undocumented immigrants should be more willing to obtain legal residence in order to have access to those legal jobs. On the contrary, in areas where employment opportunities for immigrants are primarily in the shadow or in the illegal economy, having legal status may imply little advantage for the immigrants. Therefore, we could expect to see more (less) applications in regions with higher (lower) employment rates. Insofar as regions with higher employment rate have lower crime, in a cross-sectional analysis we would observe that relatively higher numbers of amnesty applications are associated with lower crime rates. Our fixed effect analysis, though, by looking at the *within area* variation of crime rate, is robust to this identification threat. Nevertheless, in deciding whether to apply or not for the amnesty, immigrants may be – at least partially – able to anticipate future shocks to the local economy. If undocumented immigrants who expect to observe an increase in employment in the next period are more likely to apply for legal status, and if a positive shock to employment induces a reduction in crime, we could still observe that larger numbers of applications are associated to larger reduction in crime rates. We address this further concern in two ways. First, we condition on regional GDP per capita and regional employment rate: in this way we control for time-varying provincial variables which may contemporaneously influence both the outcome (crime) and the treatment (i.e. the number of applications). Second, we instrument the actual number of legalizations with a number predicted on the basis of a previous legalization process held in 1986. 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.[27] In this context, we will predict the number of immigrants legalized in each region *r* in each amnesty year t ($\hat{L}_{rt}$) using the total number of legalizations in each amnesty (L_t) and "distributing" them across regions according to the distribution recorded in the 1986 amnesty. Indeed, our instrument is:

$$\hat{L}_{rt} = \overline{sh}_{86} * L_t \qquad [2]$$

where: $\overline{sh}_{86} = {L_{r86}}/{L_{86}}$ and $t = (1991, 1995, 1998, 2002)$.

This instrument is valid under the reasonable identifying assumption that the economic shocks which may have increased/decreased the number of application in a specific region which

[27] See, among others, Card (2001), Ottaviano and Peri (2006), Bianchi et al (2008), Dustmann et al. (2008), Bell et al. (2013).

occurred in the different provinces in 1986 are not systematically correlated with those occurred in 1991, 1995, 1998 or 2002. The relevance of the instrument will be tested in the data.

4.2.1.2. Data and descriptive statistics

Table 7 reports some descriptive statistics for the data used for the empirical analysis in this section. In the upper part of the table, we have reported the number of immigrants who were granted legal status (per 10 thousand population) in each of the four amnesties which were held between 1990 and 2005. The number of legalizations is fairly constant in the first three amnesty programs, with an average of 32-35 immigrants legalized every ten thousand residents, while it tripled (up to 98) in occasion of the 2002 amnesty.[28]

The following rows in Table 7 report summary statistics for regional data for charges of foreign born citizens for different criminal offences, which are available for the period 1991-2005 (see appendix B.a). The average number of criminal charges against immigrants during this period is 11.4 charges per 10 thousand population, with a standard deviation of 9.4. Following the steep increase in the foreign born population in Italy, immigrant' crime rate increased from 3.6 in 1991 to 21.6 in 2005. This corresponds to a 500 percent increase. In the same period, the stock of legally resident immigrants in Italy increased by 300 percent.

In addition to the variation over time, immigrant's crime rate show substantial heterogeneity across regions. The rate of criminal charges against immigrant is higher in Central Italy (19.4), relatively similar in the North-West and North-East areas (respectively, 14.8 and 13.8) and substantially lower in the South of Italy (4.6). Nevertheless, most of this regional variation is driven by heterogeneity in residential location choices of immigrants, who tend to cluster in the Northern and Central regions of Italy. Indeed, the number of charges per 100 legally resident immigrants shows relatively little variation. In the period considered, there were 6.4 charges per 100 legally resident immigrants in North-West Italy, 6.2 in Central Italy, 5.5 in North-East Italy and 4.9 in Southern Italy.[29]

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[28] The exceptional size of the 2002 amnesty - together with the availability of provincial data for both legalizations and immigrants' crime in that period – motivated a specific analysis on this program (see section 4.2.2).

[29] In the empirical analysis of this section, we will always normalize criminal charges and number of immigrants legalized by the total resident population rather than by the resident immigrant population. Indeed, while total population is relatively constant over time, the stock of legally resident migrants experiences substantial jumps after each legalization process (see chapter 3). If we were to normalize criminal offences by the number of documented immigrants, the crime rate would mechanically fall

4.2.1.3. Results

In presenting our empirical results, we start by discussing the timing of the effect of legalizations on immigrants' crime rate and present OLS estimates of equation [1] (see section 4.2.1.1). Our dependent variable is the yearly change in the log of the ratio of total criminal charges against immigrants over total resident population: $\Delta\ln\left(\frac{CR_{rt}^{F}}{Pop_{rt}}\right)$. The main explanatory variables is log of the number of immigrants granted legal status in year *t* (if any) normalized by the total resident population. We will include different lags and leads of this variable in the regressions. Finding negative – and statistically significant – coefficients on these variables would suggest that regions where more immigrants received legal status experienced relatively larger reduction in immigrants' crime. All regressions include year dummies in order to capture any national trend. Time-varying regional controls (GDP per capita, employment rate and immigrant share) are gradually added in the specification. Standard errors are clustered by region. Estimation results are reported in Table 8.

In the first column, the change in immigrants' crime rate between year *t* and *t-1* is regressed on the number of immigrants legalized in year *t* (and year dummies). We find a negative – but not statistically significant – coefficient. In column 2, we have included both the current value of the *legalization treatment* and its first lag. Now, the coefficient on the current value becomes marginally significant, but the first lag is clearly more relevant. The estimated coefficient on this later variable, indeed, is negative, strongly significant and about three times larger in magnitude (-0.025). The inclusion of the second lag of the *legalization treatment* delivers a coefficient not statistically different from zero, without altering our findings on the role of the treatment in *t* and *t-1* (column 3). These results suggest that in regions where larger shares of immigrants (with respect to the resident population) were legalized, relatively larger reductions in criminal charges against immigrants were observed. Consistently with the fact that amnesty programs require some time to grant legal status to the applicants, the negative effect on crime is not contemporaneous, but it becomes visible only one year after the legalization. Moreover, the effect does not seem to be persistent: two years after the legalization, we fail to find any significant effect on crime.

To check the robustness of our results, we have performed several tests.

First, we have conducted a simple falsification exercise. Establishing the precise timing – as we did - of the legalization treatment once the amnesty has been granted is clearly a purely

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after a legalization, due to the sudden and substantial increase of the denominator. Normalizing by total population allows to identify actual reductions in the number of criminal offences.

empirical question. On the contrary, finding a significant effect (of any sign) on current crime of future legalizations of immigrants would cast some serious doubt on our empirical findings. Indeed, it would suggest that regions which experienced stronger drops/increases in immigrants' crime would then see a larger number of immigrants legalized. Reassuringly, when we include the first lead of the *legalization treatment* (column 4) we find a coefficient very close to zero and not statistically significant, while the estimated coefficient on the treatment in *t*, *t-1* and *t-2* are not affected.

So far we have treated the *legalization treatment* as exogenous in our model, but we have discussed in section 4.2.1.1 potential issues of endogeneity and our approach in dealing with it. First, we include controls for the regional GDP per capita and regional employment rate in the regression. These controls should deal with the potential endogeneity of the *legalization treatment,* which may still be left even after using a fixed effects estimation (as we do). Indeed, our estimation strategy exploit the within-region variation and, therefore, removes any permanent difference in average immigrant' crime rate and in average number of amnesty applications which may exist across regions. Nevertheless, if undocumented immigrants who expect to observe an increase in employment in the next period are more likely to apply for legal status, and if a positive shock to employment induces a reduction in crime, we could still observe that larger numbers of applications are associated to larger reduction in crime rates. As column 5 shows, conditioning on controls for the regional business cycle do not alter our finding. The coefficients of interest – on the *legalization treatment* variable in different periods – are basically unaffected, suggesting the absence of correlation between local economic cycle and number of amnesty applications.

Further, we implement an IV strategy, where the actual number of individual who obtain legal status in different amnesties in each region is instrumented with a predicted number of legalized immigrants. This predicted measure is based on the distribution of applications for the 1986 amnesty and should, therefore, be exogenous to local shocks (see section 4.2.1.1). After having established the precise timing of the effect of amnesty on immigrants' crime (Table 8), we maintain in our specification only the first lag of the *legalization treatment* variable. In Table 9, we report OLS and IV estimates, where the number of immigrants legalized is instrumented with the predicted number of legalizations. As for the previous table, we present estimates with and without regional controls. OLS estimates (columns 1 and 2) are basically as in Table 8, with an estimated coefficient on the firs lag of the *legalization treatment* equal to minus 0.024-0.026. When this potentially endogenous variable is instrumented (columns 3 and 4), we obtain coefficients only marginally larger in magnitude (minus 0.027-0.029) and equally significant. As the F-statistics on the excluded instrument show (lower part of Table 9), the instrument is a strong predictor of the actual number of legalized immigrants. The IV estimates suggest that using OLS we were potentially underestimating the true effect of amnesties on crime, although the difference is far from substantial.

The estimated coefficient is an elasticity: a ten percent increase in the share of immigrants legalized would imply a 0.3 percent reduction in immigrants' criminal charges in the following year. This is a fairly small effect. After presenting findings from the other empirical exercises in this chapter, we will discuss how to interpret the magnitude of the effect.

4.2.2. Evidence from the largest Italian amnesty

As discussed in the previous chapter, with more than 650 thousand immigrants legalized, the 2002 amnesty has been the largest legalization program granted so far in Italy. The size of the process can be better appreciated by recalling that the documented population residing in Italy in that year was about 1.5 million. The amnesty, therefore, implied almost a 70 percent increase in the size of the documented foreign born population. One can, therefore, expect to observe a substantial impact on crime rates of such a radical policy event.

The exceptional size of this amnesty - together with the availability of more detailed data for both legalizations and immigrants' crime in the period around year 2002 – motivated a specific analysis on this program. Indeed, we can extend the analysis presented in the previous section by using provincial rather than regional data, and by looking at different type of offences.

4.2.2.1. Empirical strategy and identification issues

In this analysis, we will use a differences-in-differences approach to estimate the impact of the amnesty on crime. Indeed, we expect regions with larger shares of legalized immigrants in 2002 to experience a stronger reduction (or a weaker growth) in crime committed by foreign born citizens. Similarly to our previous analysis, the number of immigrants legalized in each province (divided by total resident population in the province) is our *treatment* while the outcome is the total number of immigrants charged for criminal offences in each province (divided by total resident population in the province). We use data for the period 2000-2005, when we observe the total number of foreign born citizens charged of criminal offences by Italian province (see data appendix B.a).

We estimate the following regression:

$$\ln\left(\frac{CR_{rt}^{F}}{Pop_{rt}}\right) = \alpha + \beta_1 \ln\left(\frac{L_{r2002}}{Pop_{r2002}}\right) + \beta_2 after2002_t + \beta_3 \left(\ln\left(\frac{L_{r2002}}{Pop_{r2002}}\right) \times after2002_t\right) +$$
$$+ X_{rt}'\gamma + \varphi_r + \varepsilon_{rt} \quad [3]$$

where: $\left(\frac{CR_{rt}^{F}}{Pop_{rt}}\right)$ is the ratio of total number of criminal charges of foreign born individuals recorded in province r in year t over total resident population; $\frac{L_{r2002}}{Pop_{r2002}}$ is the share of immigrants legalized in 2002 in the province r over total resident population in 2002; $after2002_t$ is a dummy equal to 1 for years after 2002 and zero otherwise; X_{rt} is a set of tyme-varying regional controls; φ_r are province fixed effects; ε_{rt} is an error term.

The main coefficient of interest in the estimating equation above is β_3, which identifies the elasticity of immigrants' crime to changes in the number of immigrants legalized in the 2002 amnesty. We can interpret this coefficient as a causal parameter if we can reasonably assume that the intensity of the treatment $\left(\frac{L_{r2002}}{Pop_{r2002}}\right)$ is exogenous in the regression.

In the 2002 amnesty, about 93 percent of the applications were accepted and the applicants were granted legal status. The number of legalized immigrant, therefore, closely follows the number of applications filed in by the undocumented immigrants and, therefore, is determined by their endogenous decisions to apply for legal status. In section 4.2.1.1, we have already discussed how this potential endogeneity may affect our results and how we deal with this identification threat. In the analysis focused on the 2002 amnesty that we present in this section, we follow the same identification strategy: fixed effects estimation, inclusion of time-varying provincial controls, and, finally, an IV strategy based on the distribution of applications from previous amnesties. In this context, we will predict the number of immigrants legalized in each province ($\hat{L}_{r02}$) using the total number of legalizations in 2002 (L_{02}) and "distributing" them across provinces according to the distribution recorded in the 1995 amnesty. Indeed, our instrument is:

$$\hat{L}_{r02} = \overline{sh}_{95} * L_{02} \quad [4]$$

where: $\overline{sh}_{95} = {}^{L_{r95}}/_{L_{95}}$.

This instrument is valid under the reasonable identifying assumption that the economic shocks which occurred in the different provinces in 1995 are not systematically correlated with those occurred in 2002.

4.2.2.2. Descriptive statistics

Table 10 reports some descriptive statistics for the data used in this section.

From the first two rows we can see that the 2002 amnesty granted legal status to an average value of about 6.3 thousand immigrant in each province, varying from a minimum of 144 legalizations to a maximum of 97 thousand. As a ratio over the resident population, these figures imply that, in average, there 91 legalization per 10 thousand resident population.

The following rows report summary statistics for provincial data for charges of foreign born citizens for different criminal offences, which are available for the period 2000-2005 (see appendix B.a). In the period considered, there was an average of 13.6 total charges of immigrants per 10 thousand population per year, with a minimum value of 2.9 and a maximum of 45.6 charges. Offences can be disaggregated into four macro-categories. As the last column of Table 10 shows, about 44 percent of the charges were for property crime, 19 percent were for violent crime, 26 percent for “Drug and crimes against public trust” and the remaining 11 percent for “Crimes against the State and public order”. A further disaggregation by crime type, allows us to identify four main types of offences which account for 56 percent of all charges: theft (20 percent), dealing in stolen property (11 percent), drug offences (12 percent), false statements or identity (11 percent).

4.2.2.3. Results

In Table 11, we report empirical results from estimating the DID regression described in section 4.2.2.1. The 2002 amnesty was opened in September 2002, the immigrants had a two-month time window to apply for legal status, and it took more than one year for the Italian authorities to assess all the applications received (Devillanova et al., 2013). This implies that, during the entire year 2003, the process was still in the making. In our empirical analysis, therefore, we leave out year 2003 and we compare crime rates of foreign born citizens before and after the legalization. In particular, we will first define *before* as the year 2002 and *after* as year 2004 and we will then take the average over 2001-2002 as *before* and the average over 2004-2005 as *after*.

We pool the data for the four broad categories of crime described in the previous section (property; violent; drug and crimes against public trust; crimes against the State and public order) and we obtain a panel of 760 observations (95 province x 4 crime types x 2 periods). All regressions include both province and crime fixed effects, so that we exploit the *within* *province-crime* variation in the number of offences. Given that the main explanatory variable – i.e. the number of immigrants legalized with the 2002 amnesty – varies at the provincial level, we have clustered the standard errors by province.

As Table 11 shows, with OLS regressions we find a negative and significant relationship between the legalization of immigrants and the number of foreign born immigrants charged with a criminal offence. Provinces where a larger number of immigrants obtained legal status (relative to the total resident population) experienced a significant drop in immigrants' offences rate after the legalization process was concluded. This is true both when we only look at the year before and the year after the legalization (2002 VS 2004; column 1) and when we look at two years before and two years after (2001-2002 Vs 2004-2005; column 5). This negative relationship is robust to the inclusion of provincial controls for GDP per capita and employment rate (columns 2 and 6).

The IV regressions (columns 3-4 and 7-8) confirm our findings. As explained in section 4.2.2.1, we have instrumented the actual number of immigrants legalized in each province with a predicted number which we have obtained using the distribution of applications in the 1995 amnesty. The F-statistics reported in the bottom part of Table 11 confirm the relevance of our instrument. In all cases, the IV estimate is only marginally different with respect to the OLS ones and the level of significance is basically unaffected.

The coefficient we estimate is an elasticity. According to the IV coefficient in the last column of Table 11, a one percent increase in the number of immigrant legalized (divided by the resident population) would imply a 0.04 percent reduction in the number of criminal charges against immigrants (divided by the resident population). The average number of immigrants legalized in each province in 2002 is 91.2 per ten thousand population. Therefore, legalizing 100 more immigrants (per ten thousand population) in one province would approximately lead to a 4.5 percent reduction of immigrants' criminal charges.

Further, we have looked at heterogeneous effect of the legalization treatment by type of offence. Indeed, we have interacted the treatment $\ln\left(\frac{L_{r2002}}{Pop_{r2002}}\right)$ with the dummy $after2002_t$ *and* a set of dummies for each of the four main categories of criminal offences (see section 4.2.2.2). In Table 12 we report the estimated coefficient on this triple interaction. As for the previous table, we report OLS and IV estimates for 2002 VS 2004 and for 2001-2002 Vs 2004-2005. We find negative and statistically significant effects of immigrants' legalization on all categories of crime, although with some heterogeneity in size and level of significance of the estimated coefficients. If we consider the last column of Table 12 (IV estimates, with provincial controls of the sample 2001-2002 Vs 2004-2005), we find the strongest effects on violent crime and crime against the state (- 0.05), followed by property crime (- 0.04) and, finally, by “Drug and Other crime“ (- 035).

4.3. The 2007 “click day” and immigrants’ crime

After having presented evidence from amnesty programs, we consider the quota system which, in the Italian context, offers a similar opportunity of analyzing the impact of higher *legalization rates* of immigrants on foreign born crime rates. In particular, in this section we exploit the “as-good-as-random” variation in the legalization rate induced by the 2007 “click day”.

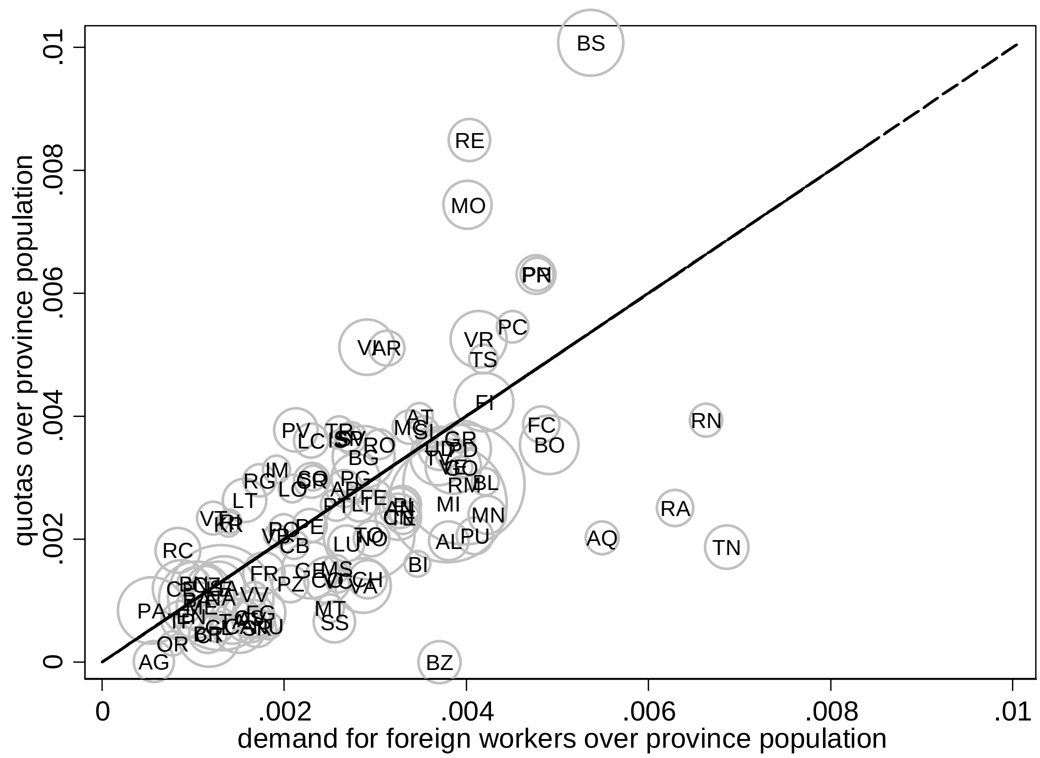
4.3.1. Institutional setting: the 2007 “click-day”

Throughout the years since the “Turco-Napolitano” Law 40/1998, Italian migration policy has remained firmly anchored to a quota system that sets a cap the number of residence permits available each year for different categories of immigrants (see section 3.3). More specifically, fixed quotas are reserved to immigrants coming from 14 countries that subscribed bilateral agreements with Italy for the contrast of illegal migration: Albania, Algeria, Bangladesh, Egypt, Ghana, Morocco, Moldova, Nigeria, Pakistan, Philippines, Senegal, Somalia, Sri Lanka and Tunisia. The rest of residence permits are awarded to immigrants from other countries, with no cap by nationality but, instead, fixed quotas by type of permit: “A” for domestic workers, and “B” for non-domestic workers, which include mostly firm-employees in the manufacturing and construction sectors.

Column (1) of Table 13 shows the quotas fixed by the Flow Decree 2007. Out of 170,000 permits, just above one fourth were reserved for privileged nationalities; at the same time, this group accounts for the greatest majority of applications, about 350,000 out of 610,000, see column (4). Contrary to the intentions of the policy-maker, the so-called “privileged” nationalities face thus a tighter rationing of residence permits, relative to non-privileged nationalities. The average, unconditional probability (at the national level) of obtaining legal status equals in fact the ratio of the quota of permits available for each group of immigrants over the total number of applications received from such group, which is reported in the last column of Table 13. Such ratio is as low as 13 percent for immigrants of privileged nationalities, and it rises to 48 and 50 percent for immigrants of other nationalities that apply for type A and type B permits, respectively. This means that, say, Albanians or Moroccans, who in principle should be favored through quotas specifically reserved for their own nationality, have on average a much lower probability of obtaining a residence permit compared to, say, Chinese or Latin American immigrants that apply for type A or type B permits outside the reserved quotas.[30]

National quotas are then partitioned across the 103 Italian provinces. In principle, such partition should match the expected demand for foreign workers, as communicated in each previous year by the Union of Italian Chambers of Commerce, Industry, Craft and Agriculture (Unioncamere), through the databank Excelsior (http://excelsior.unioncamere.net). In practice, however, many administrative bodies concur, at different stages, to the final determination of quotas, so that the final allocation of quotas may reflect several factors.

Figure 22: Demand for foreign workers planned for year 2008 and quotas fixed by the Flows Decree 2007, across Italian provinces

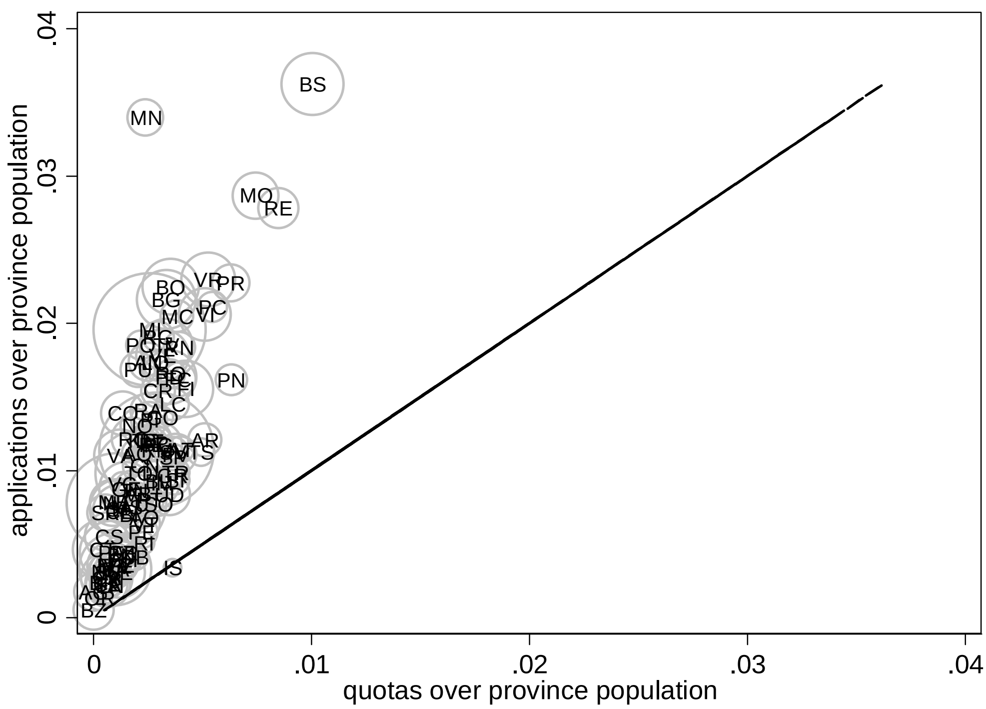


30 In all Flows Decrees after 2009 privileged nationalities were actually favored over non-privileged nationalities, as the quotas for the latter group were always kept very low – in some cases at zero.

Figure 22 plots the total provincial quotas (for all nationalities and all types of permits) fixed by the Flows Decree 2007 against the job vacancies for immigrant workers planned by Italian firms for year 2008; the size of the markers is proportional to total province population, and both quotas and vacancies are taken as a ratio on the latter variable. The graph also shows the 45-degree line; if quotas matched exactly the (expected) demand for foreign workers, the scatter plot would coincide with the 45-degree line. While this is not the case, we see however that most provinces remain very close to the line, meaning that the partition of quotas tries, to some extent, to match the demand and supply of foreign workers.

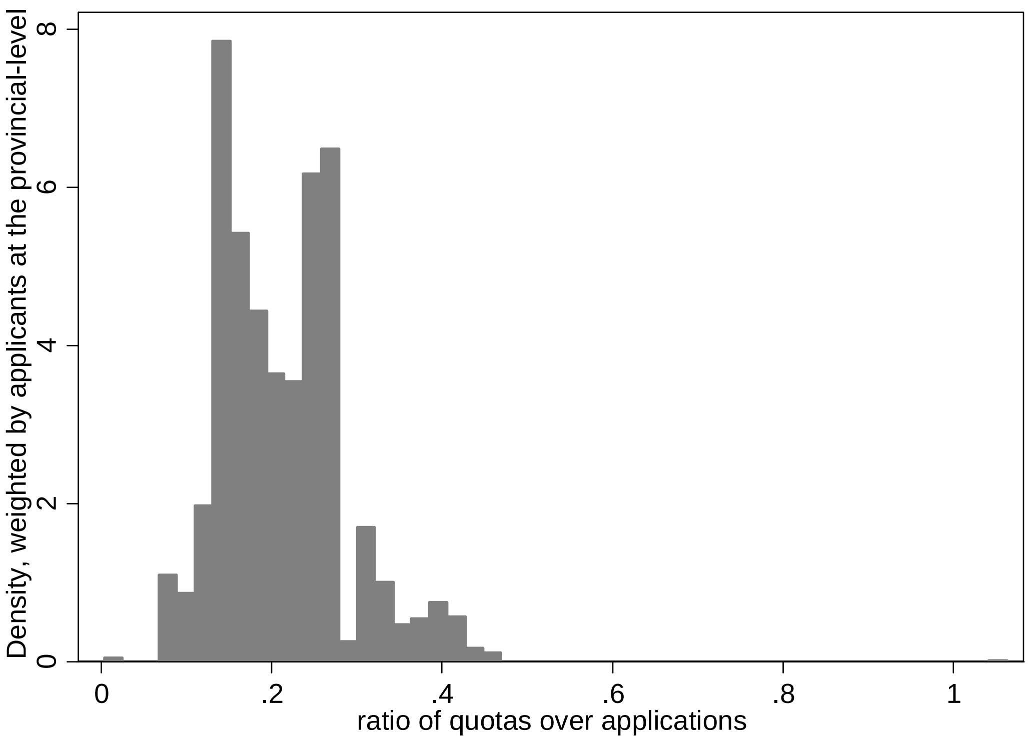
Figure 23 plots an analogous graph for the number of applications received against the available quotas. In this case too, the markers for all provinces – with the exception of a small city in the South, Isernia – lie above the 45-degree line, meaning that the demand for permits greatly exceeds the available supply. This is particularly true in Northern provinces, characterized by better labor market opportunities for foreign immigrants. In Milan, the economic capital of the country, the number of applicants reaches 2 percent of the province population, while the available quotas are 10 times lower; other northern provinces such as Brescia, Mantua, Modena and Reggio Emilia exhibit an even larger imbalance between applications and quotas.

Figure 23: Quotas fixed by the Flows Decree 2007 and applications received across Italian provinces



The histogram in Figure 24 shows the distribution of the ratio of quotas over applicants across provinces, weighted by the number of applicants in each province. Most of the distribution is concentrated between 0.1 and 0.3, meaning that most applicants face a very low probability of obtaining a residence permit. [31]

Figure 24: Distribution of the ratio of quotas over applications across Italian provinces, weighted by the number of applicants in each province.



Interestingly, the ratio of quotas over applications – and, thus, the average probability of obtaining a permit – does not seem to vary significantly with socio-economic characteristics at the provincial level. The first two graphs in Figure 25 show that, although the expected demand for foreign workers was an important predictor for provincial quotas in Figure 22, it is not systematically related to the ratio of quotas over applications; the same is true for other economic indicators such as the (log) value added per capita and the employment, unemployment and activity rate, see the other graphs in the first two rows of Figure 25. Finally, the graphs in the last row show that the tightness of quotas (relative to the number of applicants) is also unrelated to the presence of immigrants over total province population –

[31] The extreme right tail of the graph contains the small province of Isernia, in which the ratio of quotas over applicants in above 1. As this is a clear outlier – in addition to being a very small province – it will be excluded from the scatter plots we present next.

both the stock in 2006 and the change between 2002 and 2006 – as well as to the extent of anti-immigrant sentiments – as proxied by the electoral support for the anti-immigrant Northern League party at the 2006 elections.

Most importantly for the purpose of our analysis, the graphs in Figure 26 seem to exclude any systematic correlation with province crime rates – for different types of crime and for the total crime rate – both as levels in 2006 (first two rows), and as changes between 2002 and 2006 (last two rows).

Summarizing, the partition of national quotas across provinces responds to economic conditions, as reflected in the local demand for foreign workers; however, the ratio of quotas over applications does not. More in general, the latter exhibits some variation across provinces, but in a way that is not systematically correlated with other province characteristics, including crime rates. This will be particularly important for the analysis that follows.

Figure 25: quotas over applications and province socio-economic characteristics

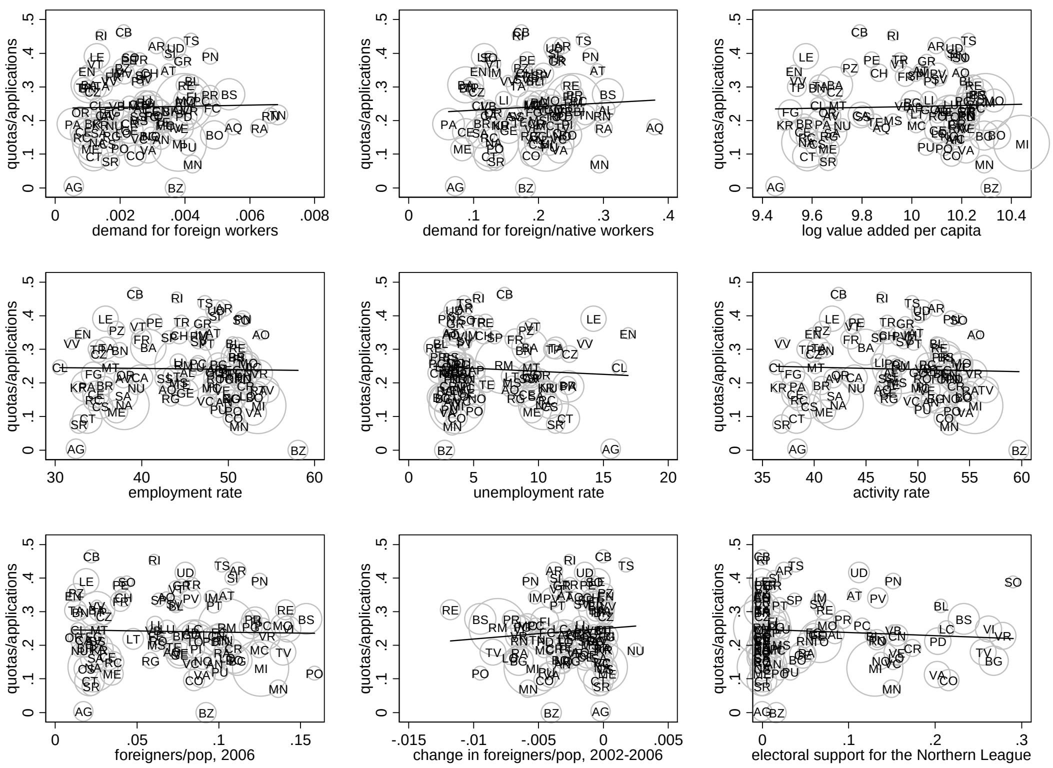
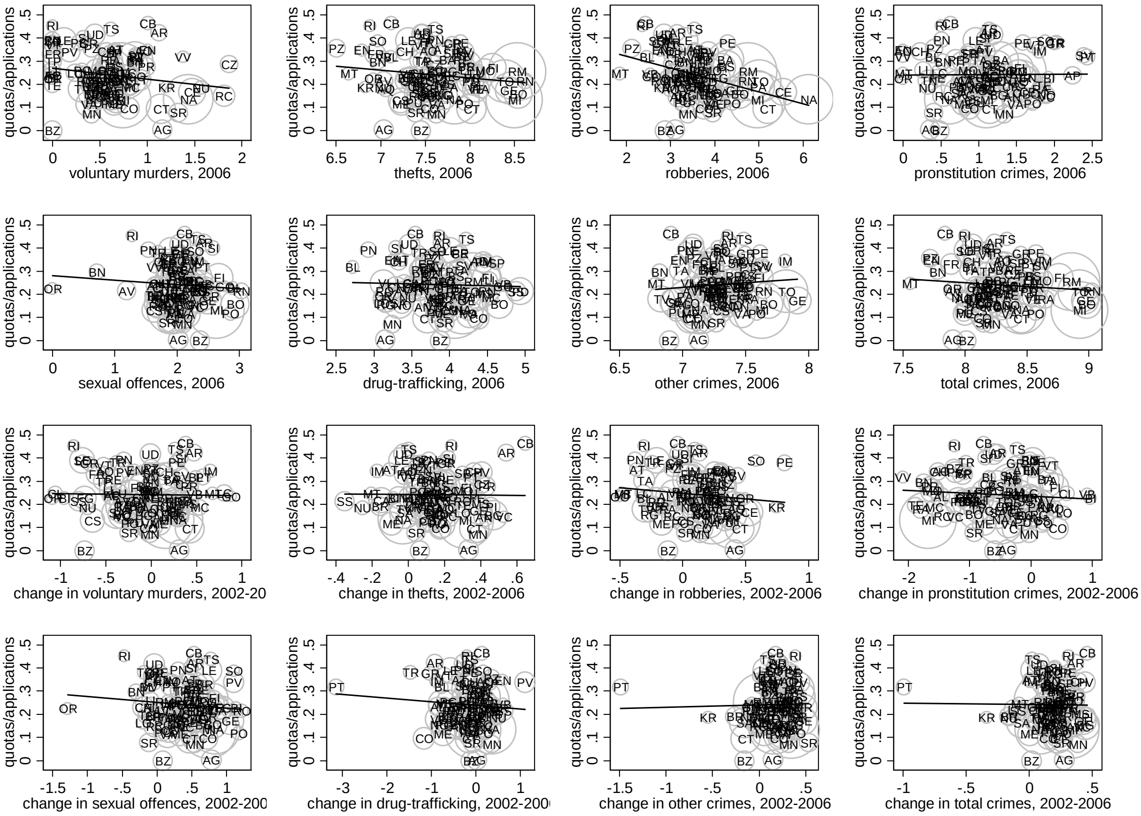


Figure 26: Quotas over applications and province crime rates (levels and changes), for different types of crime



4.3.2. Empirical strategy

Insofar as the lack of legal status makes immigrants more likely to commit crime, we expect regions with larger shares of legalized immigrants to experience a stronger reduction (or a weaker growth) in crime committed by foreign born citizens. As argued in the previous section, we can be confident in considering the share of undocumented immigrants who have been legalized of the total number of applications for legal status which were submitted for the 2007 "click-day" as completely exogenous in our regressions. Therefore, we apply a DID strategy and estimate the following baseline regression:

$$\log(OR_{rt}^{F}) = \alpha + \beta_1 \frac{L_{rt}}{I_{rt}} + \beta_2 after2007 + \beta_3 \left(\frac{L_{rt}}{I_{rt}} \times after2007\right) + X_{rt}'\gamma + \varphi_r + \varepsilon_{rt} \qquad [5]$$

where the (continuous) treatment variable is given by L_{rt}/I_{rt} , i.e. the share of legalized (L_{rt}) immigrants over total applications (I_{rt}) submitted for the 2007 click-day and the dependent variable is the log of the rate of immigrant offenders, i.e. the ratio between the number foreign-born offenders (for all types of offenses) and resident population; $after2007$ is a dummy equal to 1 for years after 2007 and zero otherwise. The coefficient of interest is β_3: it identifies the causal effect on immigrants' crime of having a higher *legalization rate* in the 2007 "click-day". Finally, X_{rt} is a vector of provincial controls which are aimed to capture time-variant heterogeneity in some observable characteristics. Following the literature on the determinant of criminal activity, we include demographic and socio-economic controls (see, among others, Levitt, 1998; Oster and Agell, 2007; Dills et al. 2010). Demographic variables include the log of resident population and the log of foreign-born (legal) residents; the share of males aged between 15 and 39 and the share of foreign-born males aged between 15 and 39; the share of population living in cities with more than 100,000 inhabitants. Socio-economic variables include the log of per capita Value Added (as a proxy of regional GDP) and the unemployment rate. To consider the expected cost of crime, we include the *clear-up rate*, defined as the ratio of the crimes for which the Police identified a "suspect" offender and the total number of reported crimes and the log of the total number of (suspected) offenders (Ehrlich, 1996; Bianchi et al. 2012).[32]

The baseline treatment variable (i.e. the legalization rate) is constructed as the share of undocumented immigrants who have been legalized over the total number of applications for legal status which were submitted for the 2007 "click-day" (L_{rt}/I_{rt}). Moreover, we also exploit the information on the 15 nationalities more represented in the "click day" natural-experiment to construct a "weighted treatment variable" in the following way:[33]

$$\frac{L_{rt}}{I_{rt}} = \sum_{i=1}^{N} \overline{sh}_{ir} \frac{L_{irt}}{I_{irt}} \qquad [2]$$

where: $\frac{L_{irt}}{I_{irt}}$ is the share of legalized (L_{rt}) immigrants over total applications (I_{rt}) submitted for the 2007 click-day for nationality *i*; $\overline{sh}_{ir}$ is the share of immigrants from nationality *i* residing in region *r* before 2007.[34] Basically, this second measure gives more weight to a higher legalization rate of the nationality *i* if there are more immigrants of that nationality residing in region *r*.

[32] Notice that similar results can be obtained using as dependent variable the share of immigrant offenders over the total number of offenders.
[33] These include demands from Morocco, China, Bangladesh, India, Ukraine, Moldavia, Albania, Pakistan, Sri Lanka, Philippines, Egypt, Peru, Tunisia, Senegal and Ghana.
[34] 2005 is the reference year, while the share is calculated over the total legal migrant population of the same 15 main nationalities residing in region *r*.

We can also expect legalization to have different effects with respect to different crime categories. This is because, for example, immigrants who are at the margin of committing or not committing a crime may respond differently to the policy, depending on the type of crime they are willing to commit or depending on the type of criminal behavior they are used to. Thus, to test for the existence of heterogeneous effects with respect to different categories of criminal offences, we construct a specific rate of immigrant offenders for the main types of criminal offenses, pool the observations for each crime category (*c*) and province (*r*) together and estimate the following model:[35]

$$log(OR_{rt}^{F,c}) = \alpha + \beta_1 \frac{L_{rt}}{I_{rt}} + \beta_2 after2007 + \beta_3^c \left(\frac{L_{rt}}{I_{rt}} \times after2007 \times crime\_type_c\right) +$$

$$+X_{rt}'\gamma + \delta_c crime\_type_c + \varphi_r + \varepsilon_{crt} \qquad [3]$$

The parameters of interest are now the coefficients of the interaction terms between the treatment variable and the offence types dummies ($crime\_type_c$), so that we estimate a β_3^cβ coefficient for each crime category (*c*). In this case, the regression also includes dummies for each crime type category ($crime\_type_c$).

4.3.3. Descriptive statistics

We mainly exploit two data sources: the Italian National Institute for Statistics (ISTAT) and the Investigation System Database (ISD). The Italian National Institute for Statistics (ISTAT) provides general socio-economic statistics at the provincial level (103 regions corresponding to NUTS 5 aggregation level). From various ISTAT sources we collect demographic and socio-economic information, while we exploit the Investigation System Database (ISD) to construct aggregate crime measures aimed at sizing the involvement of immigrants (both in legal and illegal status) in criminal activities before and after the 2007 "click day" (see appendix B.b).

We focus on the years 2006 and 2008 (the years immediately before and after the 2007 "click day") and we construct two main crime measures: the "rate of immigrant offenders" (over the resident population) and the "share of immigrant offenders" (over the total number of offenders). We calculate overall measures including all types of offenses and separate measures for the relevant categories of criminal offenses (see Appendix B.b for a detailed description).

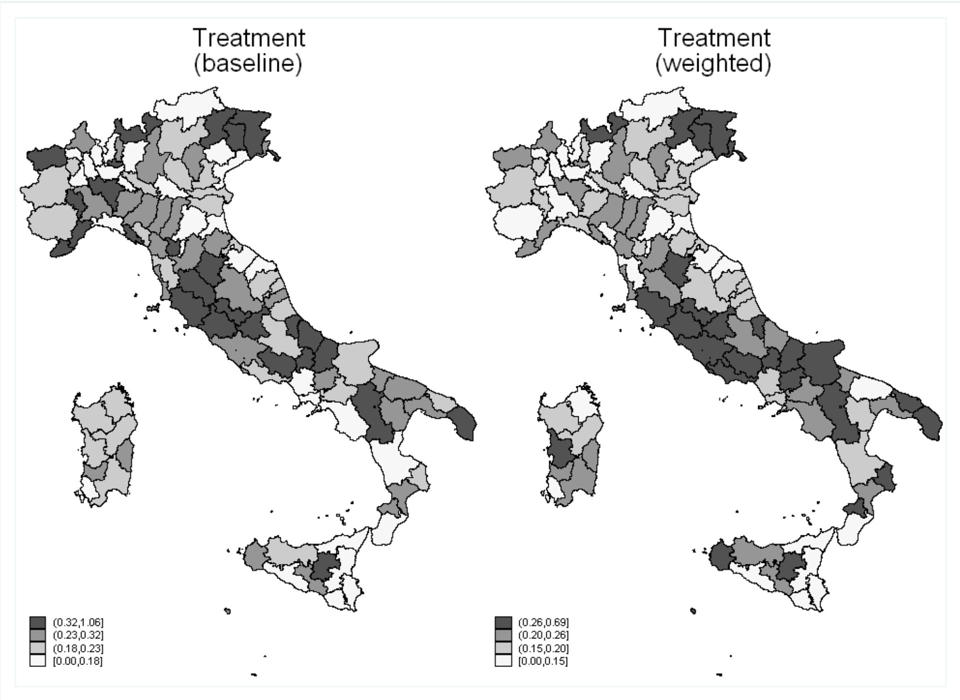
[35] See Appendix A for data description and categories construction.

Table 14 contains general descriptive evidence concerning the involvement of immigrants in criminal activities in the observed period. In general, we observe lower offender rates for immigrants compared to natives (0.46 immigrant offenders every 10,000 inhabitants compared to 1.31 natives), but about one third of the offenders (31.6%) were immigrant. Immigrants are more involved in sexual related crimes (forcible rapes and prostitution, 50%), property crimes (theft, car theft, and arson, 35%), drug and economic related crimes (smuggling, patent and brand infringements). However, the simple descriptive evidence does not highlight relevant differences in the crime patterns across the two years, which indeed seem to show an increasing involvement of immigrants from 2006 to 2008.

Table 15 contains descriptive statistics on different specifications of the treatment variable (i.e. the legalization share) and on the dependent variable of our aggregate analysis (which is constructed as the log of the rate of immigrant offenders, see appendix B.b). We obtain three specifications for the treatment variable. In specification (i) – our baseline - the legalization share is obtained as the ratio between the accepted and the overall applications; in specification (ii) it is obtained as the ratio between the accepted applications and the applications demands from the main 15 nationalities, while specification (iii) corresponds to the weighted treatment constructed as we explained in the previous Sections. Focusing on the baseline treatment variable (i), we observe that on average 24.95% of applications were accepted. Restricting the denominator of the legalization share on the applications made by the main 15 nationalities determines a slight increase in the baseline legalization share, while the weighted treatments, which gives more weights according to the distribution of the 15 most represented nationalities in each province, is slightly lower compared to the baseline.

Finally, Figure 27 depicts the territorial variability of the legalization share. It is worth noticing that the legalization share varies across Italian provinces without specific territorial patterns. For instance, we do not observe the North-South divide either in the baseline or in the weighted version of the treatment variable.

Figure 27. Territorial variability of the legalization share (treatment variable) determined by the 2007 “click day”.



Note: The legalization share is obtained as the ratio between the accepted and the overall applications; the figure on the left depicts the baseline treatment variable (specification (i) in Table 2), the figure on the right the weighted treatment variable (specification (iii) in Table 2). Darker areas correspond to higher legalization share. Source: 2007 “click day” data.

4.3.4. Results

We estimate our DID regressions using OLS with robust standard errors. In the baseline regressions we consider as outcome variables the log of the rate of immigrant offenders for all types of offenses. Thus, we obtain our baseline estimations exploiting a panel of 103 observations with time dimension (t) equal to 2, where 2006 is the pre-policy and 2008 the post-policy period. This specification corresponds to the “robust DID specification” suggested by Bertand et al. (2004). Table A 1 shows descriptive statistics for the (time-variant) control variables included in the vector X_{rt} and described above. These include: log of resident population and the log of foreign-born (legal) residents, which implicitly control for population and immigrants’ density on the territory, given the inclusion of provincial FE in all the specifications; the share of males aged between 15 and 39 (*Male1539*), and the share of foreign-born males aged between 15 and 39 (*foreign born male1539*); the share of population living in cities with more than 100,000 inhabitants (*Urban*); log of per capita Value Added (as a proxy of provincial GDP) and the unemployment rate; the clear-up rate and the log of the total number of (suspected) offenders.

In Table 16, we show the results for the three types of treatment variables described in the previous Section: treatments (i) and (ii) refer to the simple legalization rates (treatment (i) is the baseline), treatment (iii) to the weighted one. We observe that the policy has statistical significant effects on the rate of immigrant offenders: the coefficient of interest (did, corresponding to β_3) is negative and statistically significant for (almost) all specifications of the treatment variable. Considering specification in column (b) - with the baseline treatment variable and the inclusion of the complete set of the control variables (X_{rt}) - we obtain that an increase of 10 percentage points in the legalization share implies a 2,2% reduction in the rate of immigrant offenders. Results are robust to the inclusion of the complete set of time-variant provincial controls (X_{rt}) and to the different definitions of the treatment. The effects of the legalization policy are slightly less precisely estimated and greater in magnitude when we use the weighted treatment, columns (e) and (f): in this case, an increase of 10 percentage points in the legalization share implies a 3,5% reduction in the rate of immigrant offenders.

To test for heterogeneous effects with respect to different categories of criminal offenses (violent, property, organized crime, economic related, sexual related and drug related criminal offenses), we implement our second specification where we pool the category-specific rates of immigrant offenders for each province and year and interact the DID variable with crime-type dummies (the coefficients of interests are β_3^c, where c is each crime-category). Results are shown in Table 17 and include specifications with the three treatment variables, and with/without the complete vector of control variables (X_{rt}). Focusing on our baseline specification (treatment (i)) with the inclusion of the complete set of controls - Table 17, column (b) – we find that increasing by 10 percentage points the legalization share has statistically significant effects in reducing violent crimes (-0.8%), property crimes (-1.4%), economic (-1.7%), sexual related (-2.2%) and drug related crimes (-1.7%). Notice that effects on property crimes, economic crimes and drug related crimes are statistically significant in all specifications. Economic related offenses include smuggling and brand infringements which constitute common illegal activities of (often undocumented) immigrants, while the drug related and property crimes categories include a variety of minor offenses such as thefts, drug trafficking and consumption which are plausibly more sensitive to the change of the legal status.

Some robustness checks of our findings are reported in appendix A.

4.4. Concluding remarks

In this final section, we briefly summarize the empirical findings discussed throughout this chapter and we discuss their magnitude and the implications they bear for public policy.

4.4.1. Empirical findings from an aggregate analysis

In this chapter, we have reported different pieces of evidence all pointing at a decline in crime rate of immigrants occurring after the adoption of policies which exogenously grant legal status to large fractions of the undocumented population. In particular, we have presented aggregate evidence from different amnesty programs and from the 2007 *click-day*.

In section 4.2, we have first exploited the occurrence of four repeated amnesties in Italy (in 1990, 1995, 1998 and 2002) to show a statistically significant reduction in immigrants' crime in the year after the legalization took place. The negative effect is increasing in the *intensity* of the *legalization treatment*, i.e. in the number of immigrants who are granted legal status. More specifically, we find that a ten percent increase in the share of immigrants legalized in one region would imply a 0.3 percent reduction in immigrants' criminal charges in the following year in that same region. Our findings are based on within-region variation and appear robust to the inclusion of time varying regional controls. In order to address the potential endogeneity of the legalization treatment across regions we have adopted an instrumental variable strategy that uses predicted legalizations (based on a past amnesty) as instrument for current ones: IV estimates closely confirm the OLS ones. Further, we have focused on the 2002 amnesty – the largest amnesty ever granted in Italy – and shown that the same significant relation between *legalization rate* and immigrants' crime is found using a DID setup and more geographically disaggregated data (i.e. provinces rather than regions). Moreover, we have reported evidence of heterogeneous effects of the *legalization treatment* across different crime types. Indeed, we find the strongest effects on violent crime and crime against the state, followed by property crime and, finally, by "Drug and Other crime".

In section 4.3, instead, we have focused on the 2007 "click day" as a source of large exogenous shock to the legal status of the undocumented population. As explained in the text, the specific characteristics of the policy and of its implementation created "as-good-as-random" variation in the legalization rate, that is, in the share of applicants who obtained legal status in different areas. Similarly to our analysis for the 2002 amnesty, we use a DID setup and show that provinces where a large fraction of applicants was legalized experienced larger – and statistically significant – reductions in immigrants' crime rates. Indeed, an increase of 10 percentage points in the legalization share would imply a 3,5% reduction in the rate of immigrant offenders. Moreover, when looking at different crime categories, we find that increasing by 10 percentage points the legalization share has statistically significant effects in reducing violent crimes (-0.8%), property crimes (-1.4%), economic (-1.7%), sexual related (-2.2%) and drug related crimes (-1.7%).

4.4.2. Discussion

All this empirical evidence clearly shows that we can expect to observe statistically significant reductions in immigrants' crime following policies which grant legal status to substantial shares of the undocumented immigrant population. This is an important finding for policy-makers and it confirms our theoretical framework: legal status does matter in affecting migrants criminal decisions. Nevertheless, the effects we find are not particularly large. On the basis of our estimates, one could hardly argue that using policies, such as amnesties and the *click-days*, that *ex-post* grant legal status to undocumented immigrants already residing in the country will substantially reduce total immigrants' crime rate. Still, this does not imply that granting legal status is irrelevant for individual decisions to participate in criminal activities. In order to clarify this important point, we need to discuss in more details how to interpret the parameters we identify in our empirical analysis.

Throughout our aggregate analysis, our dependent variable is always total immigrant crime in different areas. This measure does not allow us to distinguish between crime committed by documented rather than by undocumented immigrants. Moreover, among the offenders (and within each category of legal and illegal immigrants) we will have "marginal criminals" and "non-marginal criminals", where the former are immigrants at the margin between committing or not a crime, while the latter are immigrants whose expected return from crime is substantially higher than the expected return from legal employment. Note that we expect the policies under study (amnesties/ *click-days*) to produce an effect only on "marginal criminals" who do not already have legal status. Indeed, documented immigrants – *marginal* or *non-marginal* – are simply not affected by those policies, while the undocumented ones will see their criminal choices affected only if obtaining legal status implies that legal employment – rather than crime – becomes the most rewarding activity. Clearly, for undocumented *non-*

marginal criminals obtaining a residence permit may simply not be enough to induce a change in criminal behavior.

We need to get an idea of the size of this subpopulation of immigrants for whom we expect policies granting legal status to impact on criminal behavior (the *undocumented marginal criminals*) in order to interpret the estimates we obtain when we look at an aggregate measure of total immigrant crime as dependent variable (as we do in this chapter). A numerical example is helpful here.

Suppose we have 100 offences committed by immigrants in one region. Given the evidence presented in the initial chapters of this report, we can make the reasonable assumption that, in the Italian context, 20 of those crimes are committed by documented immigrants and the remaining 80 by unauthorized immigrants. Within this latter group, suppose that *non-marginal criminals* are responsible for 60 offences while *marginal-criminals* for the remaining 20.[36] As discussed above, we can expect an amnesty or a click-day to have a negative effect only on this latter group of 20 offences. Now, even if the policy has a very strong effect – assume a 50 percent reduction in propensity to commit crime for the affected immigrants (the *undocumented marginal criminals*) – the effect we would observe on total immigrant crime is a fall to 90 offences, that is, a 10 percent reduction with respect to the previous level of crime. Therefore, the fact that a substantial fraction of immigrant offenders are not responsive to policies which grant legal status implies that, by looking at aggregate measures of total migrant crime, we will mechanically tend to underestimate (in the example, by a factor of 5) the effect of granting legal status on individual behavior. This is clearly happening with the aggregate estimates presented in this chapter.

Using an aggregate outcome such as total immigrant crime has other potentially important consequences for the interpretation of the results.

Going back to our simple numerical example above, if the undocumented marginal criminals reduces the number of offences from 20 to 10 as a consequence of the legalization policy, we

[36] The international evidence shows that in different source of criminal data (self-report survey data, police arrest data, administrative data from prison facilities) a small number of individuals tend to appear frequently, suggesting that a small share of all offenders commit a disproportionate part of all crime, particularly serious crime (see, among others: Hales et al. 2009). Indeed, detailed studies of offending behavior confirm the hypothesis that the distribution of crimes over offenders is highly skewed: while most offenders commit only a few offences, a small group of offenders turn into prolific offenders (Piquero, Farrington and Blumstein 2007). In our discussion, these prolific offenders are clearly *non-marginal criminals* (they systematically engage in crime), while all the others are *marginal criminals* (depending on the conditions, they may seize some criminal opportunities or not).

can think of those 10 less offences as 10 profitable criminal opportunities not seized by anyone in that particular regional market for crime. Shouldn´t we expect the other actors (documented immigrants and undocumented *non-marginal criminals*) in the market to seize at least some of them? Given that the demand for crime is not affected by the legalization policies, a reduction in supply by some group in the criminal market will imply an increase in return from offending, possibly inducing members of other groups increase their supply. If there is some substitution in criminal activities, the effect on total crime we will be able to uncover will be further diluted. Sticking to our numerical example, even if just half of the 10 criminal opportunities are seized by other groups of migrants, we will observe total offences falling from 100 to 95. Hence, we would observe only a 5 percent reduction in total migrant crime, even if the policy induces a 10 times larger effect on the affected immigrants.

4.5. Insights from an individual-level analysis

As we have just discussed, an aggregate point of view may not be the most effective in uncovering the relation between legal status and immigrant criminal behavior. How would our estimate improve by having access to individual data of applicants for one of these policies and compare criminal behavior for those who obtained and those who did not obtain legal status (assuming, for the moment, that there is randomness in granting legal status to applicants)? First of all, by looking directly at individual behavior rather than adding up individual choice in an aggregate measure, the possible presence of substitution in the criminal labor market does not induce an underestimation of the policy effect. Second, we would have no documented immigrants in the sample to dilute the effect. Third, we will probably see *non-marginal criminals* relatively underrepresented among the applicants population. Indeed, if the main effect (as in our model in chapter 2) of legal status is improving the wage for individuals in employment[37], *non-marginal criminals* may have little incentive to apply. For them, criminal opportunities strictly dominate employment opportunities: trying to improve the return from the latter ones has little sense if the utility maximizing strategy still remains engaging in crime.[38] The opposite is true for (undocumented) *marginal criminals:* by definition, these

[37] The empirical literature finds consistently that illegal immigrants have lower earnings (see, e.g., Borjas and Tienda 1993; Kossoudji and Cobb-Clark, 2002; Kaushal 2006; Amuedo-Dorantes et al. 2007). In the Italian context, Dustmann et al. (2013) find that undocumented immigrants earns 25-30 percent less than similar documented workers.

[38] Additional reasons may explain a lower propensity to apply for legal status by undocumented immigrants who have specialized in crime (those that we have defined as *non-marginal criminals*). First, they may prefer to maintain their illegal status in order to complicate their identification by the police

individuals have returns from crime and from employment as undocumented immigrant which are very close to each other. Obtaining legal status would, therefore, allow them to improve their utility, hence creating strong incentives to apply. In the limit situation where only the *marginal-criminals* apply for legal status, we would then be able to recover the full reduction in incentives to engage in crime caused by the policy (the hypothetical 50 percent reduction discussed above). Nevertheless, among the applicants we will also have a third group of immigrants which may still contribute to water down the effect. Indeed, there will be undocumented immigrants whose return from employment is already substantially larger than that from acting as criminals.[39] These immigrants would see their utility increase if they obtain legal residence, but they would not alter their choices between employment and crime (they would keep preferring employment).

In conclusion, analysing individual data and comparing criminal choices of immigrants who obtained legal status with those of comparable immigrants who did not is probably the ideal setting to address the empirical question concerning the relation between legal status and immigrant criminal behaviour. We take exactly this further step in the next chapter.

From a policy point of view, we see the two approaches (aggregate and individual-level) as strong complements. As clarified in our numerical example, one can envisage many reasonable scenarios where legalization policies have strong effects on individual decisions for undocumented immigrants who are at the margin between opting for employment or for crime, but they produce limited effects on total immigrant crime. We will further discuss these aspect in the final discussion in this report.

(this is documented, in the Italian context, by Barbagli, 2008). Second, Italian amnesties and click-days have often explicitly discouraged applications from criminals by making the concession of legal status conditional on having no previous criminal records in Italy.

[39] Given that the return in the Italian labor market from employment as undocumented immigrant is close to the subsistence level, these would be individuals for whom their age, gender, risk aversion, moral values, etc. make criminal options particularly unattractive.

4.6. Tables

Table 7: Immigrants legalized through different amnesty programs and total number of criminal charges against foreign born citizens (per 10 thousand population), by region; years 1991-2005

Immigrants legalized (per 10 thousand population)	Mean	Std. Dev.	Min	Max	
1990 Amnesty	31.42	20.57	8.59	102.18	
1995 Amnesty	34.58	20.96	7.86	100.96	
1998 Amnesty	32.82	21.05	9.54	88.40	
2002 Amnesty	97.70	57.56	19.26	238.60	
Total offences of immigrants (per 10 thousad population)	**Mean (1991-2005)**	**Std. Dev. (1991-2005)**	**1991**	**2005**	**% change 1991-2005**
Italy	11.42	9.45	3.61	21.62	498.9
North-West	14.84	7.14	6.45	25.65	297.4
Liguria	26.08	11.56	11.71	45.00	284.2
Lombardia	13.75	6.61	6.39	23.01	260.1
Piemonte	12.92	7.17	4.45	24.48	450.2
Valle d'Aosta	14.59	4.42	10.00	17.58	75.8
North-East	13.80	7.72	2.85	29.19	924.8
Emilia-Romagna	13.13	9.00	1.07	31.76	2861.6
Friuli-Venezia Giulia	21.01	8.27	6.49	36.99	470.3
Trentino-Alto Adige	11.96	8.36	2.87	27.25	848.0
Veneto	12.88	6.89	3.43	25.34	637.9
Central	19.42	8.45	6.20	32.95	431.7
Lazio	24.67	8.31	9.66	37.15	284.6
Marche	11.10	7.44	1.58	25.51	1512.5
Toscana	17.05	9.44	3.77	31.98	748.8
Umbria	11.35	8.84	2.94	24.42	729.2
South & Islands	4.63	3.39	0.92	9.92	972.8
Abruzzo	8.62	7.18	0.93	20.04	2058.2
Basilicata	3.84	2.86	1.51	8.06	434.6
Calabria	4.17	3.66	0.52	10.54	1939.0
Campania	5.02	3.85	0.89	11.16	1154.0
Molise	4.17	3.61	0.12	8.66	7064.4
Puglia	4.43	2.72	1.01	8.28	718.2
Sardegna	3.65	1.93	2.23	6.89	208.4
Sicilia	3.94	3.05	0.61	8.25	1251.4

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

Table 8: Repeated amnesties and crime rate: FD regressions (Years: 1990-2005)

ln (Total Offences / Tot Population)	1 OLS	2 OLS	3 OLS	4 OLS	5 OLS
ln (Immigrants Legalized / Tot Population)	-0.003	-0.007*	-0.004	-0.005	-0.004
	[0.003]	[0.003]	[0.004]	[0.005]	[0.005]
1st lag of ln (Immigrants Legalized / Tot Population)		-0.025***	-0.021***	-0.022***	-0.025***
		[0.004]	[0.005]	[0.006]	[0.006]
2nd lag of ln (Immigrants Legalized / Tot Population)			0.008	0.006	0.007
			[0.006]	[0.007]	[0.009]
1st lead of ln (Immigrants Legalized / Tot Population)				-0.003	-0.003
				[0.007]	[0.006]
Year dummies	X	X	X	X	X
GDP per capita & Employment rate					X
Observations	280	280	280	280	280
R-squared	0.079	0.109	0.112	0.112	0.130

Note**:** Table reports First Differences regressions of total criminal charges against immigrants (per 10 thousand population) on the number of immigrants legalized (per 10 thousand population) in each region in each amnesty program (1990, 1995, 1998 and 2002). In columns 2 to 4 we gradually include the 1st lag of the number of legalized immigrants, the 2nd lag and the 1st lead. In all regressions we control for year dummies and in column 5 we include regional GDP per capita and regional employment rate. Standard errors are clustered by 20 regions: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Repeated amnesties and crime rate: FD and IV regressions (Years: 1990-2005)

ln (Total Offences / Tot Population)	1 OLS	2 OLS	3 IV	4 IV
1st lag of ln (Immigrants Legalized / Tot Population)	-0.024***	-0.026***	-0.027***	-0.029***
	[0.004]	[0.005]	[0.004]	[0.006]
Year dummies	X	X	X	X
GDP per capita & Employment rate		X		X
Observations	280	280	280	280
R-squared	0.106	0.125	0.106	0.125
IV: F-stat			802.9	582.8
IV: p-value			0.00	0.00

Note: Table reports First Difference regressions of total criminal charges against immigrants (per 10 thousand population) on the 1st lag of the number of immigrants legalized (per 10 thousand population) in each region in each amnesty program (1990, 1995, 1998 and 2002). In IV regressions, the number of immigrants legalized in each region is instrumented with a predicted number of legalised immigrants. In all regressions we control for year dummies and in columns 2 and 4 we include regional GDP per capita and regional employment rate. Standard errors are clustered by 20 regions: *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Immigrants legalized in 2002 amnesty and number of offences per 10 thousand population, by crime type (Years: 2000-2005)

2002 Amnesty	Mean	Std. Dev.	Min	Max	
Immigrants legalized	6279.89	12791.47	144	96857	
Immigrants legalized (per 10 thousand population)	91.21	53.37	8.92	317.68	
Years: 2000-2005	Mean	Std. Dev.	Min	Max	% of total crime
Total crime	13.64	8.04	2.91	45.57	*1.00*
Property crime	5.98	4.08	1.34	26.74	*0.44*
Theft	2.82	1.89	0.39	11.42	*0.20*
Robbery	0.42	0.37	0.02	2.15	*0.03*
Criminal damage	0.35	0.21	0.05	1.34	*0.03*
Fraud	0.13	0.12	0.02	0.93	*0.01*
Dealing in stolen property	1.96	2.12	0.28	18.91	*0.14*
Other	0.30	0.24	0.02	2.04	*0.02*
Violent crime	2.47	1.22	0.45	5.22	*0.19*
Homicide	0.04	0.08	0.00	0.67	*0.00*
Injury	0.77	0.42	0.10	1.61	*0.06*
Violence	0.45	0.24	0.06	1.07	*0.04*
Sexual offences	0.12	0.08	0.00	0.50	*0.01*
Other violence	1.08	0.55	0.14	2.61	*0.09*
Drug and crimes against public trust	3.82	2.78	0.37	13.91	*0.26*
Drug Offences	1.83	1.78	0.06	7.22	*0.12*
False statements or identity	1.58	1.42	0.17	10.57	*0.11*
Counterfeiting	0.15	0.21	0.00	1.55	*0.01*
Other	0.25	0.34	0.04	3.38	*0.02*
Crimes against the State and public order	1.37	0.88	0.27	5.00	*0.11*
Smuggling	0.14	0.40	0.00	3.51	*0.01*
Resisting an officer	0.69	0.46	0.07	2.68	*0.05*
Criminal association	0.06	0.06	0.00	0.32	*0.00*
Other	0.48	0.25	0.15	1.55	*0.04*

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

Table 11: 2002 Amnesty: Immigrants' crime and legalizations

	1	2	3	4	5	6	7	8
ln (Total Offences / Tot Population)	2002 Vs 2004				2001-2002 Vs 2004-2005			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
ln (Immigrants Legalized / Tot Population)	-0.014**	-0.018*	-0.014***	-0.022**	-0.033***	-0.032***	-0.035***	-0.043***
	[0.006]	[0.010]	[0.005]	[0.010]	[0.005]	[0.012]	[0.005]	[0.014]
Year dummies	X	X	X	X	X	X	X	X
GDP per capita & Employment rate		X		X		X		X
Observations	760	760	760	760	760	760	760	760
R-squared	0.031	0.058			0.178	0.186		
IV: F-stat			1284	499.1			1285	258.0
IV: p-value			0.00	0.00			0.00	0.00

Note: Table reports DID regressions of total criminal charges against immigrants (per 10 thousand population) on the number of immigrants legalized (per 10 thousand population) in each province during the 2002 amnesty and other controls. In IV regressions, the number of immigrants legalized in each province is instrumented with a predicted number of legalised immigrants. In all regressions we control for year dummies and in even columns we include regional GDP per capita and regional employment rate. Standard errors are clustered by 95 province: *** p<0.01, ** p<0.05, * p<0.1.

Table 12: 2002 Amnesty: Immigrants' crime and legalizations, by crime type

ln (Total Offences / Tot Population)	1	2	3	4	5	6	7	8
	2002 Vs 2004				2001-2002 Vs 2004-2005			
	OLS	OLS	IV	IV	OLS	OLS	IV	IV
ln (Immigrants Legalized / Tot Population)*(Property crime)	-0.010	-0.014	-0.012**	-0.020*	-0.027***	-0.027**	-0.030***	-0.039***
	[0.007]	[0.011]	[0.006]	[0.010]	[0.006]	[0.012]	[0.006]	[0.014]
ln (Immigrants Legalized / Tot Population)*(Violent crime)	-0.016**	-0.020*	-0.019**	-0.027**	-0.036***	-0.036***	-0.040***	-0.049***
	[0.008]	[0.011]	[0.008]	[0.011]	[0.007]	[0.013]	[0.007]	[0.015]
ln (Immigrants Legalized / Tot Population)*(Drug and Other crime)	-0.005	-0.009	-0.002	-0.010	-0.027***	-0.026*	-0.026***	-0.035**
	[0.009]	[0.012]	[0.008]	[0.011]	[0.009]	[0.014]	[0.008]	[0.015]
ln (Immigrants Legalized / Tot Population)*(Crime against the state)	-0.023**	-0.027**	-0.024***	-0.031**	-0.040***	-0.040***	-0.041***	-0.050***
	[0.009]	[0.013]	[0.009]	[0.012]	[0.008]	[0.014]	[0.007]	[0.015]
GDP per capita & Employment rate		X		X		X		X
Observations	760	760	760	760	760	760	760	760
R-squared	0.039	0.066			0.183	0.191		
IV: F-stat			1274	648.4			1275	585.3
IV: p-value			0	0			0	0

Note: Table reports OLS and IV regressions of total criminal charges against immigrants (per 10 thousand population) on the number of immigrants legalized (per 10 thousand population) in each province with the 2002 amnesty and other controls. In order to investigate potential heterogeneity in the effect, the coefficient on the main regressor is interacted with dummies for each of the four macro-groups of criminal offences (property; violent; drug and crimes against public trust; crimes against the State and public order). In IV regressions, the number of immigrants legalized in each province is instrumented with a predicted number of legalised immigrants. In all regressions we control for year dummies and in even columns we include regional GDP per capita and regional employment rate. Standard errors are clustered by 95 province: *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Flows Decree 2007, quotas and applications at the national level

	(1) total quotas	(2) total applications type A	(3) type B	(4) A + B	(5) ratio quotas/appl.
<u>**First click day: December 15, 2007**</u>					
Privileged nationalities	44,600	206,938	146,049	352,987	0.13
Albania	*4,500*	*5,794*	*22,770*	*28,564*	*0.16*
Algeria	*1,000*	*1,057*	*847*	*1,904*	*0.53*
Bangladesh	*3,000*	*30,193*	*24,877*	*55,070*	*0.05*
Egypt	*8,000*	*3,431*	*15,402*	*18,833*	*0.42*
Ghana	*1,000*	*11,035*	*1,022*	*12,057*	*0.08*
Morocco	*4,500*	*56,243*	*40,836*	*97,079*	*0.05*
Moldova	*6,500*	*23,152*	*8,134*	*31,286*	*0.21*
Nigeria	*1,500*	*4,717*	*1,172*	*5,889*	*0.25*
Pakistan	*1,000*	*15,889*	*11,641*	*27,530*	*0.04*
Philippines	*5,000*	*20,177*	*1,628*	*21,805*	*0.23*
Senegal	*1,000*	*11,743*	*3,092*	*14,835*	*0.07*
Somalia	*100*	*133*	*26*	*159*	*0.63*
Sri Lanka	*3,500*	*17,913*	*4,053*	*21,966*	*0.16*
Tunisia	*4,000*	*5,461*	*10,549*	*16,010*	*0.25*
<u>**Second click day: December 18, 2007**</u>					
Domestic work (type A permits)	65,000	136,576	0	136,576	0.48
<u>**Third click day: December 21, 2007**</u>					
Firm-employed (type B permits)	60,400	0	120,676	120,676	0.50
construction	*14,200*				
transportation and fishing	*700*				
all other sectors	*30,000*				
self-employed	*3,000*				
managers	*1,000*				
study abroad	*7,000*				
training abroad	*1,500*				
other special categories	*3,000*				
Total	**170,000**	**343,514**	**266,725**	**610,239**	**0.28**

<u>Note:</u> Elaborations from data of the Italian Minister of Internal Affairs.

Table 14: Descriptive statistics: aggregate measures for the involvement of immigrants in criminal activities

	2006		2008		Total		
	mean	sd	mean	sd	mean	sd	N
Immigrants offender rate per 10,000 inhabitants (total):	0.4260	0.1966	0.5017	0.2361	0.4638	0.2200	206
Natives offender rate per 10,000 inhabitants (total):	1.3273	1.0220	1.3019	0.9758	1.3146	0.9968	206
Share of immigrant offenders (total)	0.3001	0.1463	0.3320	0.1508	0.3161	0.1491	206
Immigrants offender rate per 10,000 inhabitants (by category):							
Violent	0.0623	0.0363	0.0863	0.0459	0.0743	0.0430	206
Property	0.0723	0.0531	0.0797	0.0478	0.0760	0.0505	206
Org.Crime	0.0339	0.1682	0.0351	0.1680	0.0345	0.1677	206
Economic	0.0509	0.0253	0.0505	0.0303	0.0507	0.0278	206
Drug related	0.0257	0.0227	0.0298	0.0228	0.0277	0.0228	206
Sex	0.0909	0.2827	0.0532	0.2172	0.0722	0.2525	206
Other	0.2044	0.1101	0.2427	0.1305	0.2235	0.1220	206
Share of immigrant offenders (by category):							
Violent	0.1973	0.1196	0.2341	0.1207	0.2157	0.1213	206
Property	0.3357	0.1739	0.3647	0.1676	0.3502	0.1709	206
Org.Crime	0.2386	0.1692	0.3121	0.1883	0.2754	0.1824	206
Economic	0.3176	0.1507	0.3265	0.1361	0.3221	0.1433	206
Drug related	0.3193	0.2143	0.3456	0.2166	0.3324	0.2153	206
Sex	0.4742	0.2832	0.5239	0.2335	0.4988	0.2603	206
Other	0.3301	0.1533	0.3706	0.1690	0.3504	0.1622	206

Note: see Appendix A for the definition of the variables. Source: ISD data, 2006 and 2008.

Table 15. Descriptive statistics: specifications of the treatment variable and dependent variables

	Descriptive statistics				
	mean	sd	max	min	N
Treatment var.: legalization share					
(i) accepted/applications (tot) (*baseline*)	0.2495	0.1254	1.0627	0	103
(ii) accepted/applications15 (*15 most represented nationalities*)	0.2876	0.1544	1.2727	0	103
(iii) accepted/applications15 (*weighted*)	0.2176	0.1032	0.6891	0	103
Dependent var.:					
Log immigrant offender rate (total):	8.3177	0.5270	9.3239	6.5424	206
Log immigrant offender rate (by category):					
Violent	6.4227	0.6632	7.7962	4.4653	206
Property	6.4167	0.7009	8.2622	4.1858	206
Org.Crime	3.8945	1.2090	9.2103	0.9367	206
Economic	6.0947	0.5247	7.7645	4.5040	206
Drug related	5.3011	0.8442	7.0985	2.8826	206
Sexual related	3.7032	1.7213	9.2103	-0.3530	206
Other	7.5632	0.5626	8.6959	5.8412	206

Note: Baseline legalization share: (i) is obtained as the ratio between the accepted and the overall applications; (ii) is obtained as the ratio between the accepted applications and the applications demands from the main 15 nationalities; weighted legalization share (iii), weights are calculated using the share of main nationalities over the immigrant population of the same 15 main nationalities. **Source:** ISD data (2006 and 2008) for the dependent variable and “2007 click day data” for treatment variables.

Table 16. The effects of legalization on the rate of immigrant offenders: baseline OLS regressions

	Dep. Var.: log(rate of immigrant offenders)					
	(a)	(b)	(c)	(d)	(e)	(f)
did	-0.3184**	-0.2268**	-0.2685**	-0.1957**	-0.3527	-0.3491**
	(0.1370)	(0.1078)	(0.1109)	(0.0924)	(0.2137)	(0.1543)
after2007	0.2350***	0.1087	0.2328***	0.1127	0.2323***	0.1187
	(0.0383)	(0.1178)	(0.0369)	(0.1190)	(0.0487)	(0.1147)
log(resident pop.)		-1.0012***		-1.0059***		-1.0497***
		(0.2057)		(0.2052)		(0.2009)
log(total No. of offenders)		0.9508***		0.9504***		0.9596***
		(0.1691)		(0.1689)		(0.1658)
clear up		-0.0913*		-0.0929*		-0.0879*
		(0.0469)		(0.0469)		(0.0463)
urban		-0.1266		-0.1192		-0.1371
		(0.0824)		(0.0845)		(0.0858)
male1539		-0.9768		-1.0155		-1.1023
		(5.0013)		(4.9828)		(5.0430)
unemployment rate		-0.0070		-0.0068		-0.0073
		(0.0155)		(0.0154)		(0.0156)
log(pc VA)		-0.3988		-0.3653		-0.4635
		(0.5871)		(0.6031)		(0.5467)
log(foreign born pop.)		0.2517		0.2318		0.3050
		(0.2812)		(0.2812)		(0.2832)
foreign born male1539		-0.3547		-0.2852		-0.1573
		(2.0555)		(2.0624)		(1.9301)
R2	0.96	0.98	0.96	0.98	0.96	0.98
Adj. R2	0.92	0.95	0.92	0.95	0.92	0.95
N	206	206	206	206	206	206

Note: specifications in columns (a) and (b) use the *baseline treatment* (i) constructed as the ratio between the accepted and the overall applications; specifications in columns (c) and (d) use specification treatment (ii) obtained as the ratio between the accepted applications and the application demands from the main 15 nationalities; specifications in columns (e) and (f) use weighted legalization share (iii), weights are calculated using the share of the main 15 nationalities over the total immigrant population of the same 15 main nationalities (see also Table 2 for the definition of the treatment variables). Robust standard errors in parenthesis; *, ** and *** denote statistical significance at 10%, 5% and 1% level.

Table 17. The effects of legalization on the rate of immigrant offenders: heterogeneous effects with respect to relevant categories of criminal offenses.

	Dep. Var.: Log(rate of immigrant offenders)					
	(a)	(b)	(c)	(d)	(e)	(f)
did*violent	-0.1129	-0.0874**	-0.0383	-0.0553	-0.1346	-0.1304**
	(0.3001)	(0.0384)	(0.2627)	(0.0365)	(0.4095)	(0.0587)
did*property	-0.6708*	-0.1418***	-0.4764	-0.0986**	-0.9999*	-0.2076***
	(0.3576)	(0.0423)	(0.3136)	(0.0397)	(0.5101)	(0.0646)
did*org_crime	-0.1893	-0.1223	-0.1822	-0.0910	0.2632	-0.1225
	(0.6284)	(0.0935)	(0.5390)	(0.0816)	(0.8572)	(0.1326)
did*economic	-1.0584***	-0.1694***	-0.8454***	-0.1250***	-1.0327**	-0.2087***
	(0.3040)	(0.0379)	(0.2652)	(0.0340)	(0.4570)	(0.0597)
did*sexual	-1.2067	-0.2161*	-0.9448	-0.1631	-1.0919	-0.2422
	(0.8485)	(0.1147)	(0.7208)	(0.0983)	(1.1897)	(0.1593)
did*drug	-0.7150*	-0.1656***	-0.5005	-0.1201***	-1.2010**	-0.2180***
	(0.3842)	(0.0393)	(0.3374)	(0.0367)	(0.5652)	(0.0628)
did*other	-0.9422***	-0.1229**	-0.7292**	-0.0808	-0.9408**	-0.1530*
	(0.3184)	(0.0537)	(0.2836)	(0.0533)	(0.4696)	(0.0784)
R2	0.75	0.33	0.75	0.32	0.75	0.33
Adj. R2	0.73	0.26	0.73	0.26	0.73	0.26
Clusters	103	103	103	103	103	103
N	1440	1440	1440	1440	1440	1440
Province FE	yes	yes	yes	yes	yes	yes
Provincial controls (X_{rt})	no	yes	no	yes	no	yes
Crime category dummies	yes	yes	yes	yes	yes	yes

Note: specifications in columns (a) and (b) use the *baseline treatment* (i) constructed as the ratio between the accepted and the overall applications; specifications in columns (c) and (d) use specification treatment (ii) obtained as the ratio between the accepted applications and the application demands from the main 15 nationalities; specifications in columns (e) and (f) use weighted legalization share (iii), weights are calculated using the share of the main 15 nationalities over the total immigrant population of the same 15 main nationalities (see also Table 2 for the definition of the treatment variables). Provincial control variables include all variables included in Tables 4 and 5, and the dummy *after2007*. Robust standard errors in parenthesis clustered at province level; *, ** and *** denote statistical significance at 10%, 5% and 1% level.

4.7. Appendix

A. Appendix - The 2007 “click-day”: Further robustness checks

A possible threat to our DID identification strategy is constituted by the EU enlargement to Bulgaria and Romania in January 2007 which determined substantial changes in the immigration waves across European countries and exogenously changed the legal status of immigrants from these two countries (Mastrobuoni and Pinotti, 2012; Geay et al. 2012). As a matter of fact, immigrants from Romania and Bulgaria to Italy were subject to a transitory period until January 2012. Immediately after the EU enlargement, Bulgarians and Romanians were allowed to legally reside in Italy (as well as in all other EU member states). However, during this transitory period a specific “quota” program was implemented for the regulation of their access to the Italian labor market. ISD data do not allow identifying the nationality of foreign-born citizens, thus we cannot exclude from the estimation (suspected) offenders from Bulgaria and Romania. Although this fact does invalidate our identification strategy, it is possible to introduce some bias in the results.

As a robustness check, we rerun our analysis including in the set of control variables the share of foreign-born citizens from Romania and Bulgaria residing in each Italian province in 2006 interacted with the time-trend dummy after2007. As a preliminary check, we also verified that this share does not show a statistically significant correlation with the treatment variable. Table A 2 shows the results of the robustness check. The variable is never statically significant and its inclusion does not alter the pattern of the results, although the estimated coefficients for β_3 (i.e. the did variable) are slightly higher compared to the corresponding estimations in Table 16.

A second threat to our identification strategy is constituted by the pardon occurred in Italy in August 2006. Several works document that this specific pardon and, in general, collective clemency bills in Italy, could have had short-run effects on crime rates which could have affected crime trends in 2006, and not in 2008. As a robustness check we run the analysis using as pre-policy reference year 2004. On the one hand, crime measures in 2004 are free from potential confounding effects due to the 2006 collective pardon. On the other hand, the differences in crime measures between 2004 and 2008 plausibly embed other confounding factors so that is difficult to attribute these differences only to the effects “2007 click day” policy.

Results in Table A 3 show negative and statistically significant effects for all the specifications of the treatment variable, so that the general pattern of the results is confirmed. Concerning the magnitude of the did parameter, it is now about twice as big as the baseline estimation in Table 16 (0.8 standard deviations of the dependent variable). If we assume - consistently with part of the empirical literature (Barbarino and Mastrobuoni, 2012) - that collective pardons have prevalent deterrent effects on criminal behavior of the released inmates in the short run, we can consider our baseline estimates (Table 16) as a lower bound of the "true" effect. If this is the case, the "short run deterrent effects" of the 2006 pardon would cause a downward bias in the baseline estimations, which is consistent to the greater magnitude of the effects found using 2004 as pre-policy period (Table A 3).]

B. Data appendix

a. Criminal charges of foreign born citizens

The Italian National Institute of Statistics (ISTAT; www.istat.it) provides aggregate data on criminal charges of foreign born citizens for the period 1991-2005. For the period 1991-1999 data are at the regional level (for the 20 Italian regions) and allow distinguishing immigrants by country of origin (only main countries of origin are specified). For the period 2000-2005, instead, data are at the provincial level (for the 103 Italian provinces) and are disaggregated also by type of crime and by main countries of origin.

Using these data we have built a dataset of total offences by region for the entire period 1991-2005 which we have used to analyse the impact on crime of repeated amnesties (see section 4.2.1). The empirical analysis of the 2002 amnesty (see section 4.2.2), instead, is conducted on the 2002-2005 dataset on total offences by province.

b. Investigation System Database (ISD)

The *Investigation System Database* (ISD) contains information on a detailed list of criminal offenses committed in all Italian provinces on yearly basis.[40] The number of criminal offenses in each province includes those directly discovered by the Police in day-by-day investigation activities, as well as those reported to the Police by citizens, and by the Judiciary Authority starting an investigation.[41] The dataset includes offenses committed by known and unknown authors, as well as offenses for which a following judiciary process will start or not. These characteristics make the ISD data source less spoiled by underreporting issues and more precise in terms of recording the place and time in which the offense is actually happened (Calabria, 2008; Barone and Narciso, 2012).[42] In the empirical analysis we exploit an extract of the ISD data for 2006 and 2008 (t) in which we have limited information on the citizenship of the persons who committed (or are suspected to have committed) the criminal offense reported to the Police.

In particular, for each category of crime (c) in each region (r) and year (t) we know: the total number of reported crimes (c_{crt}) and the fraction of reported crimes for which the authors are known (kc_{crt}); the total

[40] The ISD draws directly from the IT system used for investigation activities by the Police Force, it is collected and managed by the Italian Ministry of Interior (IMI), compiled and updated daily. We exploit a consolidated and restricted version of the archive released for research purposes.
[41] In the ISD terminology, the data record any event (i.e. ISD fact) which is involving any discovered or suspected crime (Calabria, 2008). ISD are similar to the Uniform Crime Reports provided in the U.S. by the Federal Bureau of Investigation which collects data from reports compiled by enforcement law officials.
[42] E.g. it includes crime reported by Forest Rangers and IT and Postal Police which were not included in the ISTAT official statistics before 2004.

number of (suspected) offenders, distinguished between those with Italian citizenship ($a^N{}_{crt}$) and those without Italian citizenship ($a^F{}_{crt}$). Immigrant (i.e. foreign-born individuals without Italian citizenship) can be both in a legal or illegal status. Notice that ISD data do not allow recovering the actual number of reported *crimes* committed by native and immigrants.

We exploit the richness of the ISD list of crime types to generate seven main categories (*c*). First, we reproduce the violent and property crime categories usually exploited in the literature. In detail, *violent crimes* include murder (all types), attack, robbery, aggravated assault, and kidnap, while *property crimes* include burglary, theft (cars and other), damage and arson. Then, we create separate categories for *drug*, *sexual related* and *economic related* crimes: drug related offenses include drug trafficking and consumption; forcible rape and prostitution are included in the sexual related offenses category; economic related offenses include smuggling, ITC related crimes, patent and brand infringements, money laundering and usury. We also consider a separate category for organized crime offenses, which include "organized crime association" (Art. 416 Italian Penal Code), "mafia-type association" (Art. 416-bis Italian Penal Code), and extortions. All remaining minor offenses are included in a residual category (i.e. *other crimes*) which also includes white collar crimes.

Defining pop_{rt} as the overall resident population in region *r* and year *t*, from the ISD we obtain the "offender rates" (i.e. the number of native or immigrant offenders relative to the resident population) and the "share of immigrant offenders". These two constitute our measures to describe the involvement of immigrants in reported offenses in the descriptive statistics, while for the dependent variable of the regressions of the aggregate analysis we only focus on the log of the "rate of immigrant offenders". The rate of (suspected) offenders is obtained as the ratio between the total number of suspected offenders over the resident population. We then multiply this measure by a scaling factor so to express the number of offenders every 10,000 inhabitants:

$$OR_{rt}^{c} = \frac{\left(a_{rt}^{N,c} + a_{rt}^{F,c}\right)}{pop_{rt}} 10{,}000 \qquad \text{[A.1]}$$

We distinguish between offender rates for native and immigrants specifying two separate measures:

$$OR_{rt}^{F,c} = \frac{a_{rt}^{F,c}}{pop_{rt}} 10{,}000 \qquad \text{[A.2]}$$

and:

$$OR_{rt}^{N,c} = \frac{a_{rt}^{N,c}}{pop_{rt}} 10{,}000 \qquad \text{[A.3]}$$

The dependent variable for the regressions of the aggregate analysis is obtained as the (natural) logarithm of the "rate of immigrant offenders":

$$\log(OR_{rt}^{F,c}) = \log\left(\frac{a_{rt}^{F,c}}{pop_{rt}}\right) \qquad \text{[A.4]}$$

We also construct a direct measure of the involvement of immigrants with respect to the total number of offenders (i.e. the share of immigrant offenders):

$$S_{rt}^{F,c} = \frac{a_{rt}^{F,c}}{\left(a_{rt}^{N,c}+a_{rt}^{F,c}\right)} \qquad \text{[A.5]}$$

In the aggregate analysis, we include the (log) of total number of offenders and the clear-up rate as control variables. The (log) of the total number of offenders is the log of (a^N_{crt}+ a^F_{crt}); the clear-up rates for each category of offense are obtained as the ratio between the total number of *crimes* for which a (suspected) author has been identified by the Police and the total number of reported crimes (i.e. kc_{crt} /c_{crt}). Finally, notice that all crime measures can be calculated for each category of offense, as well as aggregating all types of offenses so to obtain the total rate of immigrant offenders (OR_{rt}^{F}) and the total share of immigrant offenders (S_{rt}^{F}).

C. Appendix tables

Table A 1: Descriptive statistics: control variables.

	Descriptive statistics				
	mean	sd	max	min	N
Resident pop. (thousands inhabitants)	83318.93	241403.4	2090000	3914.13	206
Urban (*)	0.2955	0.3709	0.9830	0	206
Male1539 (*)	0.3147	0.0245	0.3659	0.2465	206
Unemployement rate (**)	6.7743	3.7588	17.5787	1.9015	206
Provincial VA pc (***)	21985.73	5319.59	34081.90	12193.00	206
Immigrant resident pop. (*)	29719.91	45374.45	344367.00	1016.00	206
Immigrant male1539 (*)	0.2561	0.0285	0.3677	0.1581	206
Share of citizens from Romania&Bulgaria in 2006 (*)	0.00007	0.0000	0.0004	0	103
Clear- up (by category): (****)					206
Violent	0.59	0.15	0.86	0.09	206
Property	0.07	0.06	0.46	0.00	206
Org.Crime	0.82	0.19	1.00	0.20	206
Economic	0.53	0.21	0.94	0.11	206
Drug related	0.94	0.14	1.00	0.00	206
Sexual related	0.79	0.25	1.00	0.00	206
Other	0.68	0.19	1.00	0.00	206
Clear-up (total) (****)	0.40	0.27	1.00	0.02	206
*No. of offenders (by category): (*****)					206
Violent	1432.93	1197.80	7435.00	157.00	206
Property	912.48	1003.55	7554.00	107.00	206
Org.Crime	112.70	193.07	1965.00	4.00	206
Economic	735.94	970.51	8010.00	62.00	206
Drug related	419.49	546.35	3594.00	25.00	206
Sexual related	34.72	52.44	466.00	0.00	206
Other	2874.93	3369.13	22557.00	274.00	206
No. of offenders (total) (****)	6559.01	7021.70	44753.00	827.00	206

Source: (*) ISTAT-Demographics data on population census; (**) ISTAT-Labor Force Surveys; (***) ISTAT-Regional Accounts data; (****) ISD data, 2006 and 2008;

Table A 2: The effects of legalization on the share of immigrant offenders: robustness to the inclusion of the share of foreign-born citizens from Romania and Bulgaria after the EU enlargement (2007).

	Dep. Var.: log(immigrant offender rate)		
	(a)	(b)	(c)
did	-0.2554**	-0.2231**	-0.3639**
	(0.1153)	(0.1003)	(0.1553)
after2007	0.1046	0.1090	0.1136
	(0.1179)	(0.1190)	(0.1150)
log(resident pop.)	-1.0361***	-1.0444***	-1.0791***
	(0.2104)	(0.2100)	(0.2064)
log(total No. of offenders)	0.9390***	0.9375***	0.9524***
	(0.1693)	(0.1691)	(0.1659)
clear up	-0.0878*	-0.0893*	-0.0845*
	(0.0469)	(0.0468)	(0.0462)
urban	-0.1129	-0.1034	-0.1250
	(0.0865)	(0.0894)	(0.0880)
male1539	-1.7538	-1.8629	-1.7255
	(5.0756)	(5.0554)	(5.0960)
unemployment rate	-0.0089	-0.0089	-0.0086
	(0.0158)	(0.0157)	(0.0158)
log(pc VA)	-0.3999	-0.3607	-0.4875
	(0.5829)	(0.5997)	(0.5368)
log(foreign born pop.)	0.2296	0.2052	0.2883
	(0.2834)	(0.2839)	(0.2852)
foreign born male1539	-0.1266	-0.0284	0.0331
	(2.0820)	(2.0936)	(1.9568)
share foreign born Bulgaria and Romania*after2007	211.0394	228.5417	170.5724
	(204.5298)	(208.2872)	(196.9052)
_cons	20.7154***	20.7231***	21.6035***
	(6.1526)	(6.1690)	(6.1782)
R2	0.98	0.98	0.98
Adj. R2	0.95	0.95	0.95
N	206	206	206

Note: *share Bulgaria and Romania*after2007* is the share (over the total population) of foreign-born citizens from Romania and Bulgaria living in each Italian province in 2006 interacted with *after2007* dummy. Specification in column (a) uses the *baseline treatment* (i) constructed as the ratio between the accepted and the overall applications; specification in column (b) uses specification treatment (ii) obtained as the ratio between the accepted applications and the application demands from the main 15 nationalities; specification in columns (c) the weighted legalization share (iii), weights are calculated using the share of the main 15 nationalities over the total immigrant population of the same 15 main nationalities (see also Table 2 for the definition of the treatment variables). Robust standard errors in parenthesis; *, ** and *** denote statistical significance at 10%, 5% and 1% level.

Table A 3: The effects of legalization on the share of immigrant offenders: robustness using 2004 year as pre-policy period.

	Dep. Var.: log(immigrant offender rate)		
	(a)	(b)	(c)
did	-0.4939***	-0.4289***	-0.5139*
	(0.1715)	(0.1439)	(0.2625)
after2007	0.0775	0.0930	0.0486
	(0.1465)	(0.1467)	(0.1461)
log(resident pop.)	-1.3456***	-1.3573***	-1.3737***
	(0.2233)	(0.2220)	(0.2468)
log(total No. of offenders)	0.6830***	0.6828***	0.6960***
	(0.1488)	(0.1476)	(0.1529)
clear up	-0.0756*	-0.0792*	-0.0600
	(0.0448)	(0.0448)	(0.0446)
urban	-0.1777	-0.1601	-0.1854*
	(0.1220)	(0.1270)	(0.1088)
male1539	-1.9649	-2.0168	-1.9642
	(3.3256)	(3.3143)	(3.3428)
unemployment rate	-0.0128	-0.0119	-0.0156
	(0.0122)	(0.0121)	(0.0126)
log(pc VA)	0.2401	0.2570	0.2293
	(0.4549)	(0.4537)	(0.4730)
log(foreign born pop.)	0.4529**	0.4256**	0.4886**
	(0.1975)	(0.1926)	(0.2165)
foreign born male1539	0.5280	0.6690	0.4008
	(1.2221)	(1.2173)	(1.2604)
share foreign born Bulgaria and Romania*after2007	181.5036	208.7210	90.2305
	(252.8940)	(253.8499)	(259.7169)
_cons	19.6866***	19.9462***	19.8913***
	(4.8545)	(4.8664)	(4.6101)
R2	0.97	0.98	0.97
Adj. R2	0.94	0.94	0.94
N	206	206	206

Note: estimates obtained using 2004 as pre-policy period and 2008 as post-policy period; *share Bulgaria and Romania*after2007* is the share (over the total population) of foreign-born citizens from Romania and Bulgaria living in each Italian province in 2006 interacted with *after2007* dummy. Specification in column (a) uses the *baseline treatment* (i) constructed as the ratio between the accepted and the overall applications; specification in column (b) uses specification treatment (ii) obtained as the ratio between the accepted applications and the application demands from the main 15 nationalities; specification in columns (c) the weighted legalization share (iii), weights are calculated using the share of the main 15 nationalities over the total immigrant population of the same 15 main nationalities (see also Table 2 for the definition of the treatment variables). Robust standard errors in parenthesis; *, ** and *** denote statistical significance at 10%, 5% and 1% level.

Chapter 5 - Legal status and individual criminal behavior: evidence from a regression-discontinuity design

In this chapter we examine the relationship between legal status and crime at the individual-level. As it was the case in the previous chapter, we are going to exploit variation in legal status that is driven by the rationing of permits – i.e. the fact that quotas fall short of applications (often by a large amount). In particular, the probability of obtaining a residence permit, for each individual applicant, depends on the group of immigrants that compete for the same residence permits; for the sake of exposition, and to stress the main source of quasi-random variation at the hearth of our identification strategy, in what follows we will refer to each such group as a "lottery".

Therefore, the relevant lottery for an immigrant of a privileged nationality is made of all immigrants of the same nationality that apply in the same province. For an immigrant of a non-privileged nationality, it is made instead of all other immigrants of non-privileged nationalities that apply in the same province, and for the same type of permit: domestic (type A), non-domestic (type B1) and non-domestic in the construction sector (type B2).

The peculiar mechanism introduced 2007 to allocate the residence permits when the number of applicants exceeds the quota – which is the case for the greatest majority of lotteries – and the availability of data at the individual-level, allows for a clean identification of the causal effect of legal status under extremely weak assumptions.

5.1. The click days

Although the quota system has been in place since 1998, in 2007 the application procedure was completely digitalized. This meant primarily two things: first, the Flow Decree must also indicate a few days – the so-called *click days* – in which different categories of immigrants can apply through the internet; second, such applications are processed on a first-come, first-served basis, according to the exact moment in which they were received by the electronic system.

In 2007, the first year under the new policy regime, privileged nationalities could start applying on December 15, at 8:00am; the "click days" for immigrants of other nationalities were December 18 and December 21, respectively (also at 8:00am), depending on whether they were applying for type A or type B permits.[43] Applications are then processed in a strict chronological order of arrival within each lottery. The scrutiny of each application by immigration officers involves a cross-checking with the police records and other administrative archives. If the application is accurate and complete, and the applicant has no criminal record, (s)he receives the *nulla osta* for a residence permit; if instead part of the information is missing, inaccurate or false, or the applicant has a criminal record, the application gets rejected. The process continues until the number of permits awarded exhausts the quota available for a the lottery.

Such a procedure implies that, for immigrants applying at about the same time as the last successful applicant, a few seconds of delay when "clicking" can make all the difference for obtaining a residence permit; this setting provides an ideal *Regression Discontinuity* (RD) design to estimate the causal effect of legal status on immigrants' behavior.

The RD approach, pioneered by Thistlethwaite and Campbell (1960), allows one to estimate the effects of a "treatment" that is assigned across units according to whether a "running" (or "forcing") variable exceeds a known cutoff point. The main idea is to compare units just below and just above the cutoff, on the presumption that these two groups are similar in terms of other characteristics; therefore, any difference in the outcomes of interest can be attributed to the treatment effect – as opposed to variation in other omitted factors.

As stressed by Lee and Lemieux (2010), and shown formally by Lee (2008), the RD framework allows for a clean identification of treatment effects under the relatively mild condition that the agents are unable to *precisely* control the assignment near the cutoff. In other words, it is not necessary to assume away voluntarily selection into treatment; it is instead enough to assume that, no matter how hard they try, agents that are arbitrarily close to the cutoff will not be able to determine whether they would end up just below or just above it.

This is exactly the case in the present setting: the treatment of interest is the immigrant's legal status, the running variable is the timing of the application, and the cutoff is the moment in which the last application entering the quota was received; immigrants would then obtain a residence permit if and only if their application was received before the cutoff time.

In general, it seems very plausible that, for immigrants applying at about the same time, it would be just a matter of luck whether they end up being on one side or the other of cutoff. Moreover, in this specific case, the cutoff could not be known in advance, as it would ultimately depend on the timing of *all* the applications, as well as on the fraction of those that were eventually rejected. These complexities provide a

[43] Notice that the click day is the same for all immigrants in each given lottery, so the timing of the three click days is irrelevant for the individual probability of getting a residence permit. The only thing that matters, in this respect, is the timing of one application relative to the other ones in the same lottery.

compelling argument for the assumption that treatment is as good as randomly assigned near the cutoff. We can thus be sure that two immigrants applying, respectively, just before and just after the expiration of quotas, would does not differ for any other reason than the few seconds expired between their applications; yet, the former possibly obtained a residence permit (provided that the application was not incomplete or incorrect), while the latter did not. Therefore, any difference in criminal behavior during the following period can be attributed to the (causal) effect of legal status.

5.2. Data

In principle, the current quota system in Italy generates 1751 potential lotteries: 1442 for immigrants of privileged nationalities (103 provinces times 14 nationalities) and 309 for non-privileged nationalities (103 provinces times 3 types of permit: A, B1 and B2). Among the non-privileged nationalities, we dropped the 103 lotteries for Sri Lankan immigrants, because a computer bug corrupted the allocation mechanism for such nationality; another 68 lotteries in total received no applications, which leaves us with 1580 lotteries.

Our data come from two administrative sources maintained by the Italian Ministry of Interiors. From the Immigration Department, we were disclosed the administrative records of the applications sent in all three days; more precisely, we were provided with the first 400,000 applications (out of 700,000) that were actually processed by immigration officers. Since the total quotas of available permits totaled 170,000, the last 300,000 applications were not even considered. Each record in our data includes the timing of the application, at the millisecond; the type of permit for which the immigrant applied, and the outcome of the application; finally, it includes a few individual characteristics, namely gender, age and nationality.

These data were matched with detailed information on all the offenders reported by the police for having committed any serious felony: robbery, theft, drug-trafficking, smuggling, extortion, kidnappings, murders, violent assaults and rapes. The use of police reporting rates, as opposed to other measures such as incarceration rates, should attenuate differences in the treatment of legal and illegal immigrants by the judicial system.[44] Notice also that violations of the immigration law do not constitute a serious felony, so that any difference in observed crime rates between legal and illegal immigrants should *not* reflect the fact that individuals in the latter group are reported just for being illegally present in the country.

Importantly, we limit ourselves to crimes committed in year 2008, because applicants that did not make it into the quotas for year 2007 were re-considered, one year later, with the Flows Decree 2008. In order to clear the backlog accumulated in the previous year, no new application was in fact allowed. Instead, the

[44] As it was discussed in Chapter 3, other things equal, immigrants are less likely to be given house arrest or to be assigned to alternative measures than Italians (Istat, 2012). This is particularly true for illegal immigrants, who are not entitled to a legal residence in the country.

first 150,000 left out in December 2007 would be processed, again based on the initial timing of arrival, and subject to a renewal of the application (to be sent between December 15th, 2008, and January 3rd, 2009). Therefore, applicants to the right of the cutoff, who constitute our control group, also had the possibility of obtaining legal status in year 2009.[45]

The first two columns of Table **18** show the percentage of individuals in our sample – males and females, respectively – that were reported by the police, for each type of crime. As it is usually the case, men commit more crimes than women; close to one in 100 males in our sample was reported by the police in year 2008, while the figure is very close to zero for females. Since males represent also the majority of individuals in our sample, they are responsible for 96% of all reported crimes. We will thus focus on this group in our empirical analysis. As to the type of crimes, most individuals were reported for property crimes (thefts and robberies), drug-trafficking and violent assaults.

Table **18** also reports the sample averages of the few individual characteristics available for the (male) individuals in our sample, namely age and country of origin. The latter was classified into four economic groups – low, lower middle, upper middle and high income – according to the classification provided by the World Bank. Two thirds of all applicants come from low and lower middle income countries, almost another third from upper middle income countries, and less than 1% from high income countries. The last three rows distinguish applicants by the region of residence in Italy. Consistently with the fact that migration to Italy (as in many other countries) is mostly an economic phenomenon, slightly less than two-third of all immigrants in Italy reside in the Northern regions, which are characterized by better economic opportunities.

5.3. Cutoff points

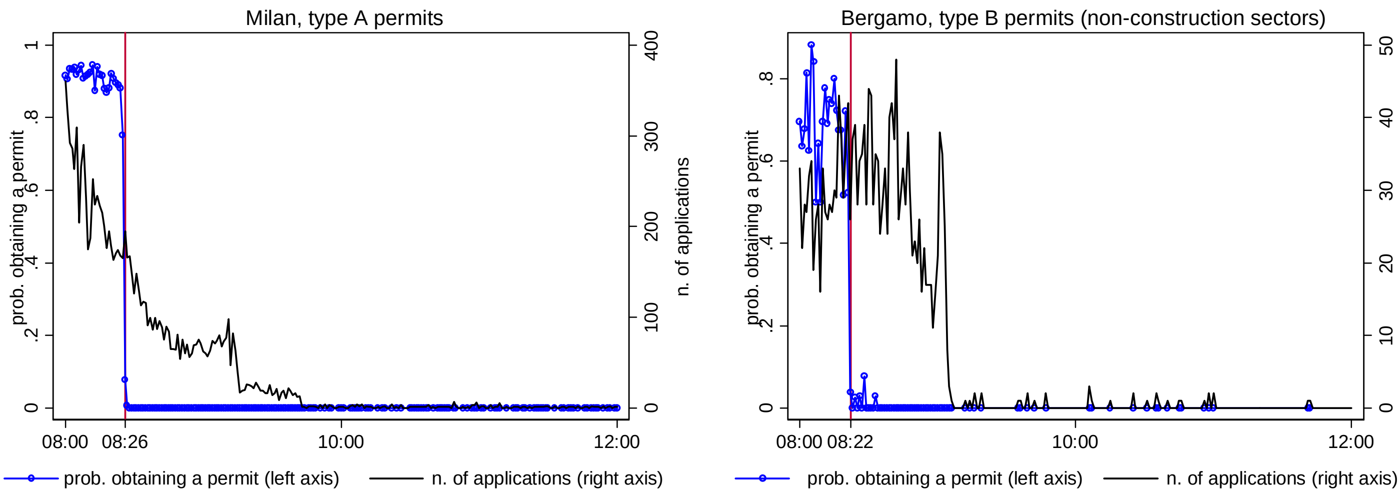
Figure 28 shows two examples of lotteries for residence permits. The left graph refers to the lottery for type A permits, among immigrants of non-privileged nationalities, in the province of Milan. The latter is the biggest city in northern Italy, and one with a very high presence of immigrants (about 13% of the resident population in 2011). The black line shows the total number of applications received, at each minute in time, between 8:00 am (when the lottery starts) and 12:00 pm. Most of the applications were received in the first moments of the day, at the rate of hundreds per minute, and by 9:40 the flow had already slowed

[45] In principle, anticipation effects could affect the behavior of immigrants in the control group already before January 2009. In practice, however, the Flows Decree 2008 was approved by the government on December 4th, 2008, and officially announced one week later. The prioritization of previous applications was an absolute novelty, hard to foresee during the previous months, and the whole procedure was surrounded by considerable uncertainty. In any case, anticipation effects would make the control and treatment groups more similar during the last part of the sample period, thus biasing the estimates of treatment effects toward zero.

down to just a very few ones per minute. Although we limit the time window to 12:00, the trickle of applications, at the rate of 20-30 per hour, continued well beyond that point, in some cases also during the following days.

However, the entire quota got exhausted just half an hour since the start of the lottery; to be precise, the last application that made it into the quota was sent at 8:27:04 and 547 milliseconds. After that moment, the probability of obtaining a residence permit drops from about 90 percent to zero, see the blue line in Figure 28. Notice that such probability is always lower than unity (even before the cutoff) because some applications that were sent on time were eventually rejected. In this respect, the RD design is *fuzzy*, meaning that the probability of treatment assignment changes discontinuously at the cutoff from a 'high' to a 'low' value, which are not necessarily equal to 1 and 0, respectively (as it is instead the case in *sharp* RD designs).

Figure 28: two examples of lotteries for residence permits



Note: the graphs show the total number of applications received (black line, left axis) and the fraction of those that eventually obtained a residence permit (blue line, left axis) at each second in time between 8:00 and 12:00, for the case of two lotteries: the lottery for type A permits in Milan and the lottery for type B permits in Bergamo. The vertical line shows the estimated timing of the cutoff, based on Andrews' (1993) structural break test.

Importantly, the fact that an application was rejected means that all or part of the information provided by the applicant was missing, incomplete or false. Therefore, whether the immigrant eventually obtained a residence permit does not depend solely on the timing of the application, but also on other factors potentially correlated with subsequent behavior. For instance, an applicant that provided false information, and had the application rejected for that reason, may have a greater propensity to break the rules; if this is the case, any difference in illegal behavior, relative to individuals that eventually obtained a residence permit, may hardly be attributed to the sole effect of legal status.

Still, the fuzzy RD design allows us to exploit only variation in legal status that is entirely due to the timing of the application. In particular, we will define an indicator variable *Z* equal to 1 for all immigrants applying before the cutoff. For all the reasons explained above, Z is as good as randomly assigned across immigrants that applied near the cutoff; at the same time, it is a powerful predictor of legal status, as only immigrants for whom Z=1 would make it into the quota. These two conditions allow to use *Z* as an instrument for legal status in a Two-Stage-Least-Square (TSLS) framework.

In some lotteries, the RD design was fuzzy also on the right-hand side of the cutoff, as the acceptance rate did not fall immediately to zero, but remained instead at a low – but still positive – value for a few minutes. The lottery for type B1 permits in the province of Bergamo (a city near Milan), provides an example in this respect. The right graph in Figure 28 shows that the probability of acceptance declined markedly, down from 70 percent to less than 10 percent, at about 8:20, but it reached zero only 10 minutes later.

This happened because, when an application was rejected (because of missing, inaccurate or false information) or the applicant did not collect the permit that had been eventually authorized, the ordering in which the next applicants were re-contacted sometimes subverted the initial ordering of applications. To the extent that this subversion was due to idiosyncratic implementation errors, this would not cause any systematic bias in the comparison of immigrants obtaining or not a residence permit. If anything, it would make treatment assignment even more similar to an actual lottery, further reinforcing our main identification assumption.

Moreover, even the case in which the subversion was systematically correlated with immigrant characteristics (for instance, because of strategic manipulation by the immigration officials) can be easily accommodated for in our fuzzy RD design. TSLS estimate would in fact exploit only variation in treatment assignment that depends only on the ordering of applications near the cutoff, as opposed to later subversions of such ordering.

One practical issue, when the discontinuity becomes fuzzy on both sides, concerns the definition of the cutoff. The timing of the last accepted application may be no longer a precise measure, as the last accepted application could have been sent a few minutes later than the actual discontinuity in treatment assignment (like in right graph of Figure 28). Faced with the same problem – i.e. estimating the unknown cutoff point in a fuzzy RD design – Chay et al. (2005) and Bertrand et al. (2010) run a battery of regressions of treatment assignment on a dummy that equals 1 after each possible cutoff point, and choose the one that maximizes the R^2 of the regression.

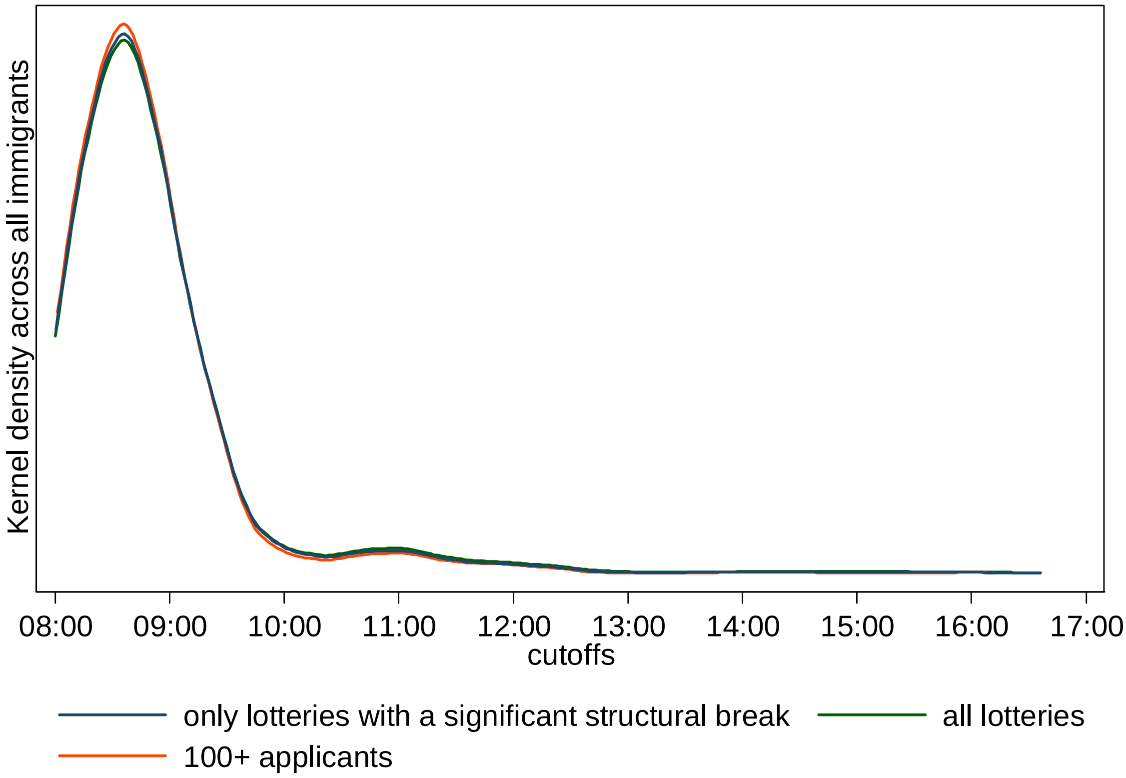
Our approach follows the same idea, but it allows in addition to test for the existence of a discontinuity in treatment assignment. This is important in our context, since in some lotteries there was no rationing of permits, so the only applicants that did not receive a residence permit were those that had the application rejected. A structural break test with unknown breakpoint allows us to distinguish such cases from those in which there is actually a rationing of permits.

Specifically, we conduct an Andrews' (1993) test on the series of acceptance rates at each second in time during the first day of each lottery. The test identifies the "most likely breakpoint" as the second in time at which the F-statistics of the coefficient of a change in the acceptance rate is maximized. The test rejects (at the 99% confidence level) the null hypothesis that there is no structural break in 966 out of 1580 lotteries, for a total of 340,450 applicants out of 403,741. This subsample accounts thus for 61% of lotteries, but for 84% of applicants; intuitively, rationing of permits is more likely to occur when the number of applicants increases.

The vertical lines in Figure 28 indicate the most likely breakpoints in the two examples considered so far, while Figure 29 shows the distribution of cutoffs across immigrants in all lotteries. When considering only lotteries with a statistically significant structural break, the median cutoff is at 08:39:06, and the majority of quotas were exhausted well before 9:00. We will mostly consider this sub-sample of lotteries in our empirical analysis, although the results are virtually identical under alternative definitions of the sample, namely including all lotteries (even those in which the structural break was not statistically significant), or considering only lotteries with more than 100 applicants (to avoid small sample bias in the structural break test, see Makram and Giesen, 2013). Figure 29 and Table **18** show that the distribution of cutoffs and the average individual characteristics are largely unaffected under these alternative definitions of the sample.

Once we have identified the cutoff point (if any) in each lottery, we define a common running variable, T, as the time elapsed between each application and the cutoff of the lottery in which it is competing. Therefore, $T>0$ for applications received after the cutoff and $T<0$ for applications received on time.

Figure 29: distribution of the cutoffs across immigrants



Note: the graphs show the density of cutoff points across all lotteries, lotteries with a significant structural break in the probability of treatment assignment, and lotteries with more than 100 applicants.

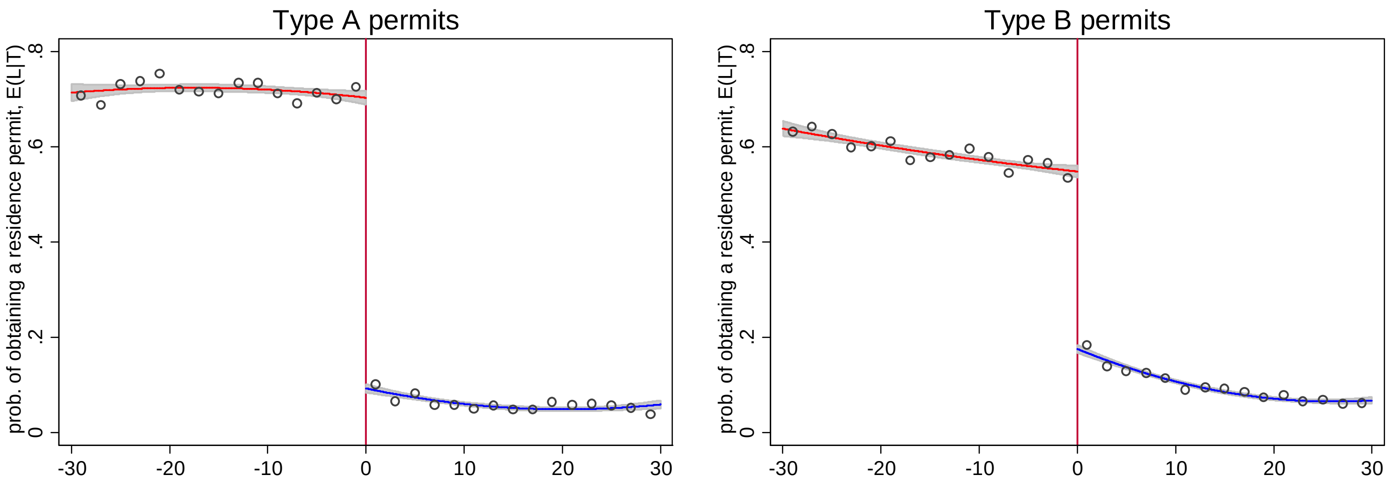
5.4. Graphical evidence

Let L be an indicator variable for individuals that are assigned into legal status – i.e. those that obtained a residence permit at the click day 2007 – and Y be an indicator variable for individuals committing a crime in Italy during year 2008. Therefore, E(L|T) and E(Y|T) are, respectively, the expected probability of assignment and the expected probability of committing a crime, conditional on the timing of application.

Figure 30 plots E(L|T) across equally-sized bins for T, within a symmetric interval of one hour around the cutoff, distinguishing between applicants to type A and type B permits. The width of the bins coincides with the optimal bandwidth under a squared error loss criterion, as derived by Imbens and Kalyanaraman (2012) (IK2012 henceforth). Depending on the outcome variable that is considered, the optimal bandwidth will vary between 1 and 2 minutes. The graphs in Figure 30 also show the estimated relationship and associated confidence intervals, based on separate regressions to the left and right of T=0, of a dummy for treated individuals on a quadratic polynomial in T.

The average probability of treatment assignment, E(L|T), exhibits a discontinuous change at T=0, greater in the case of lotteries for type A permits (from 0.7 to 0.1) than in lotteries for type B permits (from .55 to 0.2); in both cases, the standard errors and associated confidence intervals are very small.

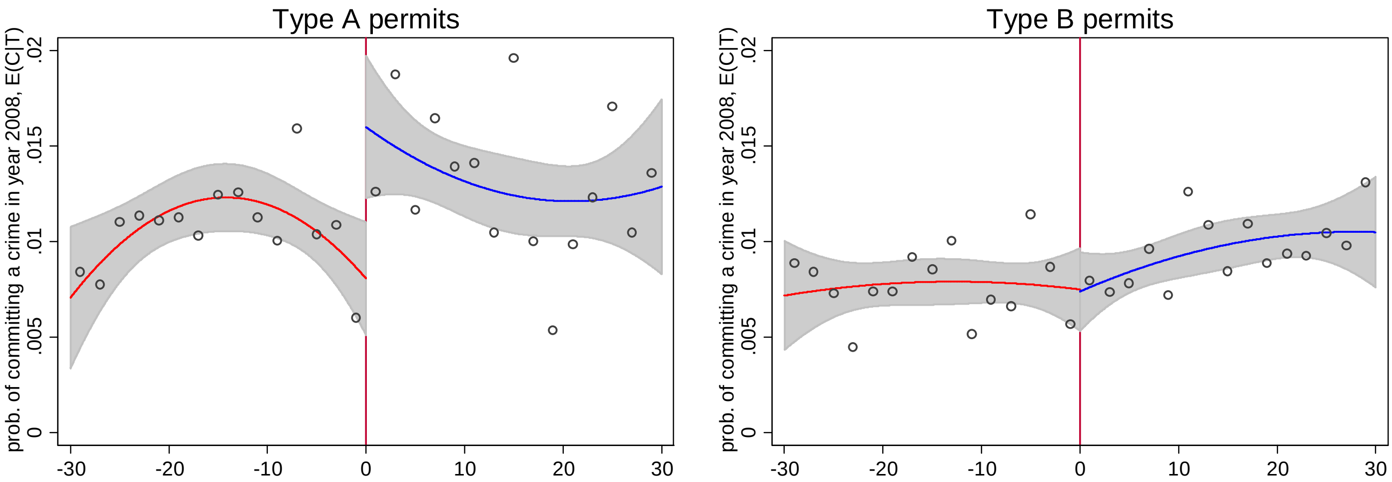
Figure 30: timing of the application and probability of treatment assignment near the cutoff, by type of application



Note: the graphs show the average probability of obtaining a residence permit for applications received within an interval of 30 minutes before and after the cutoff. The scatterplot shows the average probability within equally-sized bins; the size is computed according to the optimal bandwidth criterion by Imbens and Kalyanaraman (2012). The estimated relationship and confidence intervals, based on quadratic regressions on the two sides of the cutoff, are also shown in the graph. The left and right graphs refer to lotteries for type A and type B permits, respectively.

For applicants to type A permits, the discontinuous decrease in the probability of obtaining a residence permit at T=0 coincides with an increase in the probability of committing a crime in the year following the click day, see Figure 31. The difference is statistically significant and very high in terms of magnitude. It implies that obtaining a residence permit – as opposed to being refused it, just for a matter of seconds in the timing of the application – would cut almost by half the probability of committing a crime over the following year. Such difference is even more relevant if one considers that not all individuals to the left of the cutoff obtain a permit, and some of those to the right also do; put differently, it represents just the "intention-to-treat" effect of legal status, while the actual treatment effect would be even higher.

Figure 31: timing of the application and probability of committing a crime near the cutoff, by type of application



Note: the graphs show the average probability of committing a crime for applications received within an interval of 30 minutes before and after the cutoff. The scatterplot shows the average probability within equally-sized bins; the size is computed according to the optimal bandwidth criterion by Imbens and Kalyanaraman (2012). The estimated relationship and confidence intervals, based on quadratic regressions on the two sides of the cutoff, are also shown in the graph. The left and right graphs refer to lotteries for type A and type B permits, respectively.

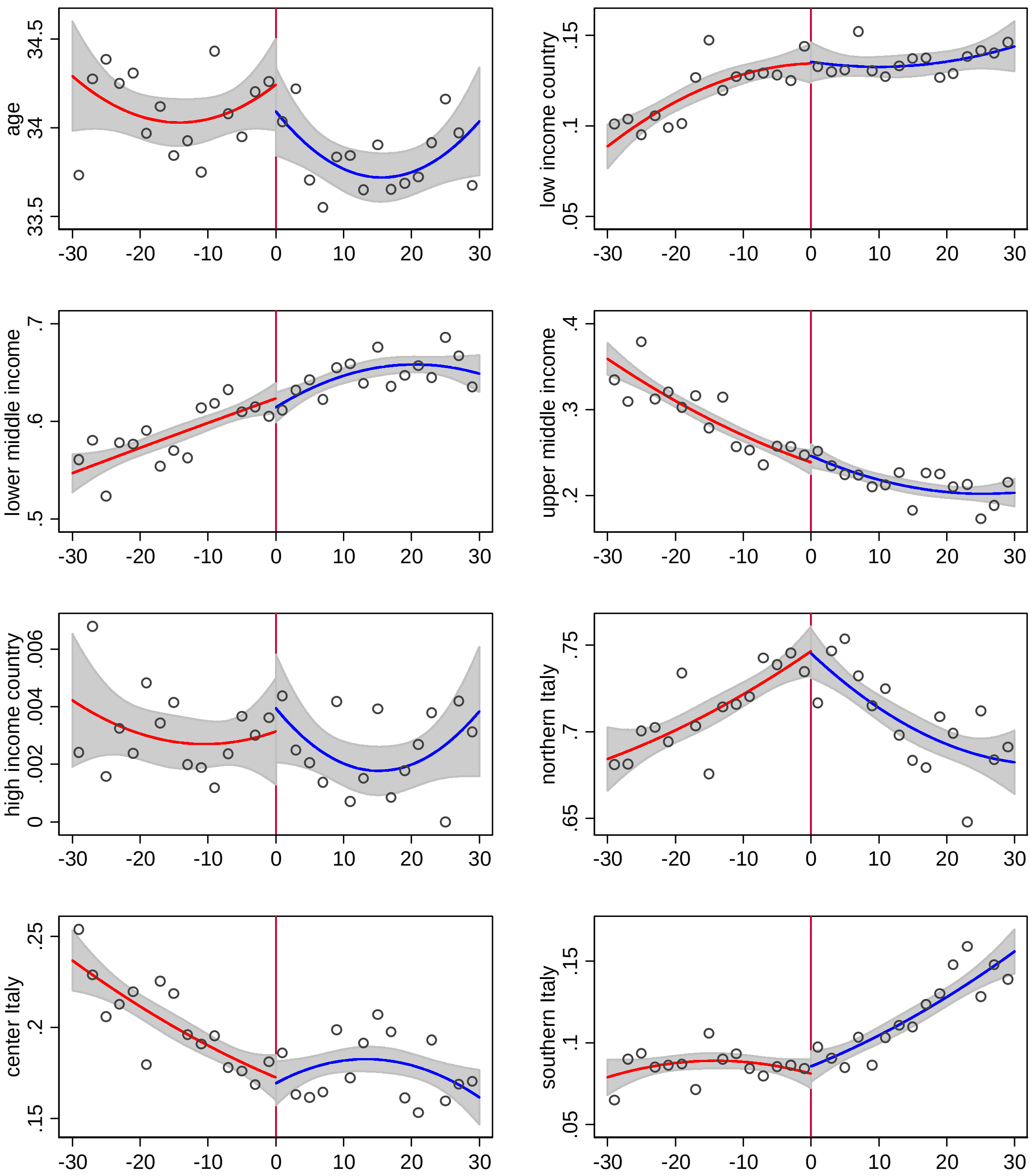
By contrast, the probability of committing a crime for applicants to type B permits, in the right graph of Figure 31, does not exhibit any significant change near the cutoff. The different effects observed for type A and type B applicants may reflect a different composition of the two groups, as well as the different incentives and constraints that they face.

In particular, one explanation for the (different) results obtained for the two groups is that immigrants that are sponsored by a firm are, in many cases, already employed there (although not officially), so they may face a substantial opportunity cost of crime even in case their application is not accepted. By contrast, applications for domestic-work permits mask, in many cases, immigrants that have no employment relationship in Italy – not even in the shadow economy – so their opportunity cost of crime may be very low in case they do not obtain legal status.

We next check, graphically, for the presence of discontinuities in other individual characteristics that may potentially explain the change in the probability of committing crime for applicants to type A permits. We thus replicate the same graphical analysis in Figure 31 for all the other individual characteristics that are reported in our data, namely age, country of origin and Italian province of destination.

The graphs in Figure show that, although average individual characteristics vary with the timing of the applications (for instance, younger individuals and immigrants from lower income countries apply later in time), there is no significant discontinuity at the cutoff. Therefore, immigrants applying right before the cutoff are not significantly different, in terms of observable characteristics, from immigrants applying right.

Figure 32: differences in observable characteristics near the cutoff (balance test)

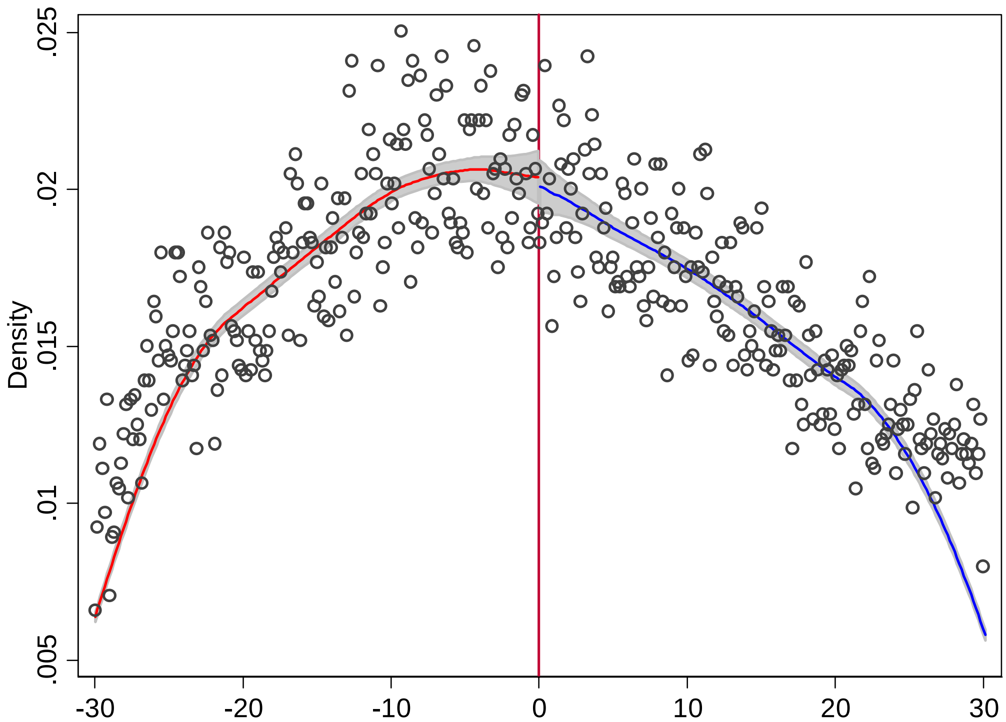


Note: the graphs show the average individual characteristics of individuals that applied for a residence permit within an interval of 30 minutes before and after the cutoff. The scatterplot shows the average probability within equally-sized bins; the size is computed according to the optimal bandwidth criterion by Imbens and Kalyanaraman (2012). The estimated relationship and confidence intervals, based on quadratic regressions on the two sides of the cutoff, are also shown in the graph. The left and right graphs refer to lotteries for type A and type B permits, respectively.

Of course, this does not allow us to exclude that the two groups may differ along other dimensions. Unfortunately, our administrative data do not report any information on important determinants of criminal activity such as income and educational levels; even if this was the case, they would leave out other factors that are very hard-to-measure, such as attitudes toward illegal activities and risk aversion. Nevertheless, the fact that observable characteristics are balanced is consistent with the assumption that treatment assignment is as good as random near the cutoff.

Evidence from the distribution of the running variable allows to exclude another case in which the assumptions of the RD design would be violated, namely if immigrants near the cutoff were able to select on one side or the other of the threshold (i.e. the running variable is *manipulated*). To the extent that this happens in a monotonic way, meaning that all individuals manipulate the variable in one direction only, the density of applications near the cutoff would differ from the case of random assignment. Building on this idea, McCrary (2008) provides a formal test for the presence of a discontinuity in the (log) height of the distribution at the cutoff. The test rejects the existence of any such discontinuity; the test statistics (namely, the log-difference in the height of the distribution just on the right and on the left of the distribution) equals -0.014 with a standard error of 0.036. The result is presented graphically in Figure 33.

Figure 33: Mccrary test for the presence of discontinuity in the density of the timing of applications



Note: the graph shows the density of applications received within an interval of 30 minutes before and after the cutoff. The scatterplot shows the average density within equally-sized bins;

the size is computed according to the optimal bandwidth criterion used in the Mccrary (2008) test. The estimated relationship and confidence intervals, based on quadratic regressions on the two sides of the cutoff, are also shown in the graph.

Taken together, the evidence in Figure and Figure 33 provide strong arguments for attributing any difference in criminal behavior between individuals to the left and to the right of the cutoff, to the treatment effect of legal status – as opposed to other omitted factors.

5.5. Baseline results

5.5.1. Non-parametric estimates

Treatment effects in a RD design equal the difference between the limits of E(Y|T) when T tends to the cutoff point (T=0, in this case) from the right and from the left. Letting Z be an indicator variable for applications received before the cutoff (i.e. Z=1 for T<0 and Z=0 for T>0, respectively), non-parametric estimates can easily be obtained by taking the difference between the average of Y, conditional on Z=1 and Z=0, in a bandwidth of the cutoff point. When the design is fuzzy – as in the present case – we can recover the local average treatment effect (LATE) for the subsample of compliers near the cutoff as the ratio of the difference in outcomes over the difference in the probability of treatment assignment,

$$\frac{E(Y|Z=1)-E(Y|Z=0)}{E(L|Z=1)-E(L|Z=0)}. \tag{1}$$

The estimation of (1) requires to make choices about the size of the bandwidth and the estimator of the conditional mean. Starting with the latter issue, Imbens and Lemieux (2008) recommend using local linear regression in order to control for any local relationship between Y and T that may confound the identification of the discontinuity at T=0. In practice, we will be estimating the system of equations

$$Y = \alpha + \beta L + \gamma T + \delta Z \cdot T + \varepsilon \tag{2}$$

and

$$L = \alpha_0 + \beta_0 Z + \gamma_0 T + \delta_0 Z \cdot T + \varepsilon_0, \tag{3}$$

across observations within the interval [-BW,+BW], where BW is the bandwidth chosen according to the criterion of IK2012. It is then immediate to estimate the coefficient of main interest, β, by using Z as an instrument for L in a two stage least squares (2SLS) framework. Following Hahn et al. 2001, we will also use a triangular kernel centered at the cutoff point to account for the fact that we are interested in the regression function at one single point, T=0. We will then check the consistency of the results against different choices about the optimal bandwidth, the specification of the local regression function and the kernel.

Table 19 presents the results for the baseline specification, distinguishing between applicants to type A and type B permits (columns 1-3 and 4-6, respectively). Starting with the former group, the criterion of IK2012 selects an optimal bandwidth of 1:20 minutes, with almost 2,400 individuals applying within the narrow time window [-1:20,+1:20]. The first coefficient in the top row of the table reports the kernel local linear regression of Y on Z (controlling for T and Z·T). The probability of being reported for a crime is one percentage point lower for individuals that applied just before the cutoff (Z=1), relative to individuals that applied just afterwards (Z=0). The estimate is robust to extending considering bandwidths that are two and three times larger than the optimal one (columns 2 and 3, respectively).

However, not all the immigrants applying before the cutoff obtain a residence permit, and not all those applying after the cutoff get refused it, so the coefficients in the top row represents just the intention-to-treat effect of legal status. To obtain the average treatment effect, one must divide this reduced form estimate by the first stage estimated effect of Z on L (controlling for T and Z·T), which is reported in the middle column of Table 19. Such estimates are always strongly statistically significant – the F-statistics for the excluded instrument ranges between 170 and 900 across specifications (1) to (6) – and higher for applicants to type A permits, in line with the visual evidence in Figure 30.

The resulting 2SLS coefficients, reported in the bottom row of Table 19, range between -1.7 and -1.4 percentage points. To get a sense of the magnitude of the coefficient, the probability of committing a crime for type A applicants that obtained legal status is just below 1 percent. In line with the graphical analysis in the previous section, there is instead no effect on the criminal activity of type B applicants. From now on, we thus focus on the sample of type A applicants.

In Table 20 we distinguish between different types of offences, namely economically-motivated vs. violent crimes. The first category includes property crimes (thefts and robberies), drug-trafficking, smuggling and extortions, while murders, violent assaults and rapes are classified as violent crimes.[46]

Interestingly, the negative effect of legal status on criminal activity is mainly driven by a decrease in the probability of committing property and other economically-motivated crimes. This is consistent with the idea that legal status affects criminal activity by changing the relative, economic returns of legitimate and illegitimate activities, which in turn are a key determinant of the decision to commit economic crimes. On

[46] We excluded illegal carrying of firearms and kidnappings, as they can not be clearly classified into either of the two categories.

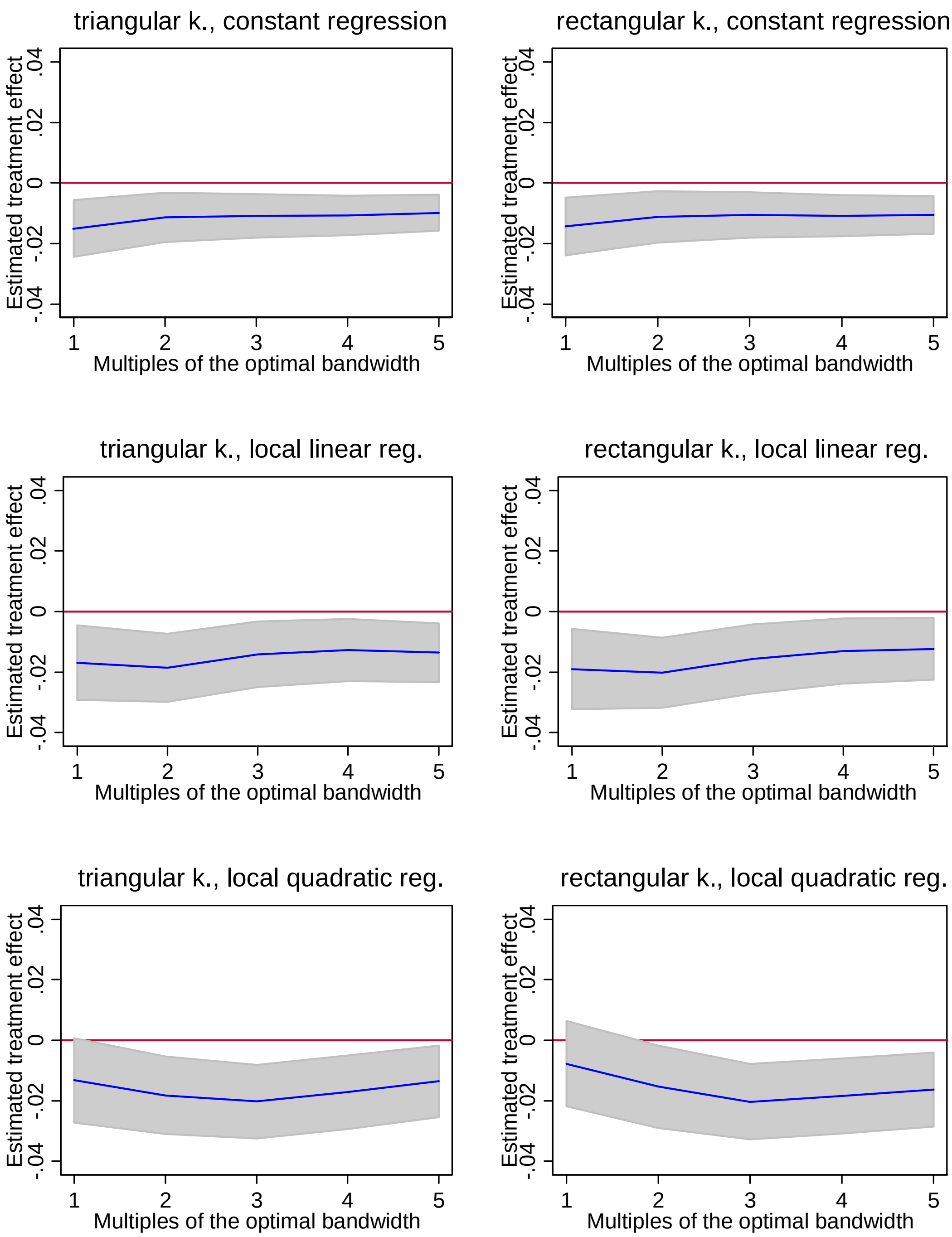
the other hand, the coefficients are smaller and not significantly different from zero for violent crimes, which should depend to a lesser extent on economic motives (Machin and Meghir, 2004).[47]

Summarizing the results in Table 19 and Table 20, it seems that being refused a residence permit – just for a matter of seconds in sending the application – would increase by about 1.5 percentage points the probability of committing economic crimes, for applicants to type A permits (but not for applicants for type B permits). In Table 21 we estimate E(X|Z) for the other individual characteristics available in our data, in order to exclude that such effects are driven by differences in other omitted factors. In line with the visual evidence in Figure , this does not seem to be the case. Although a couple of coefficients are significantly different from zero (first row), such differences disappear when we consider wider bandwidths (second and third column).

By contrast, the estimated effect of legal status, for individuals near the cutoff, is extremely robust to different specifications of the non-parametric regression. Each graph in Figure 34 shows the point estimate, and the associated confidence intervals, of the treatment effect of legal status on the probability of committing crimes for different bandwidths (horizontal axis), and different specifications of the kernel local regression function. In particular, the left and right graphs use triangular and rectangular kernels, respectively; the top, middle and bottom graphs include in the regression only the constant, a linear term, and a quadratic polynomial in the running variable, respectively. The results are generally very stable; the same is true for the effect on the probability of committing economic crimes, reported in Figure 35.

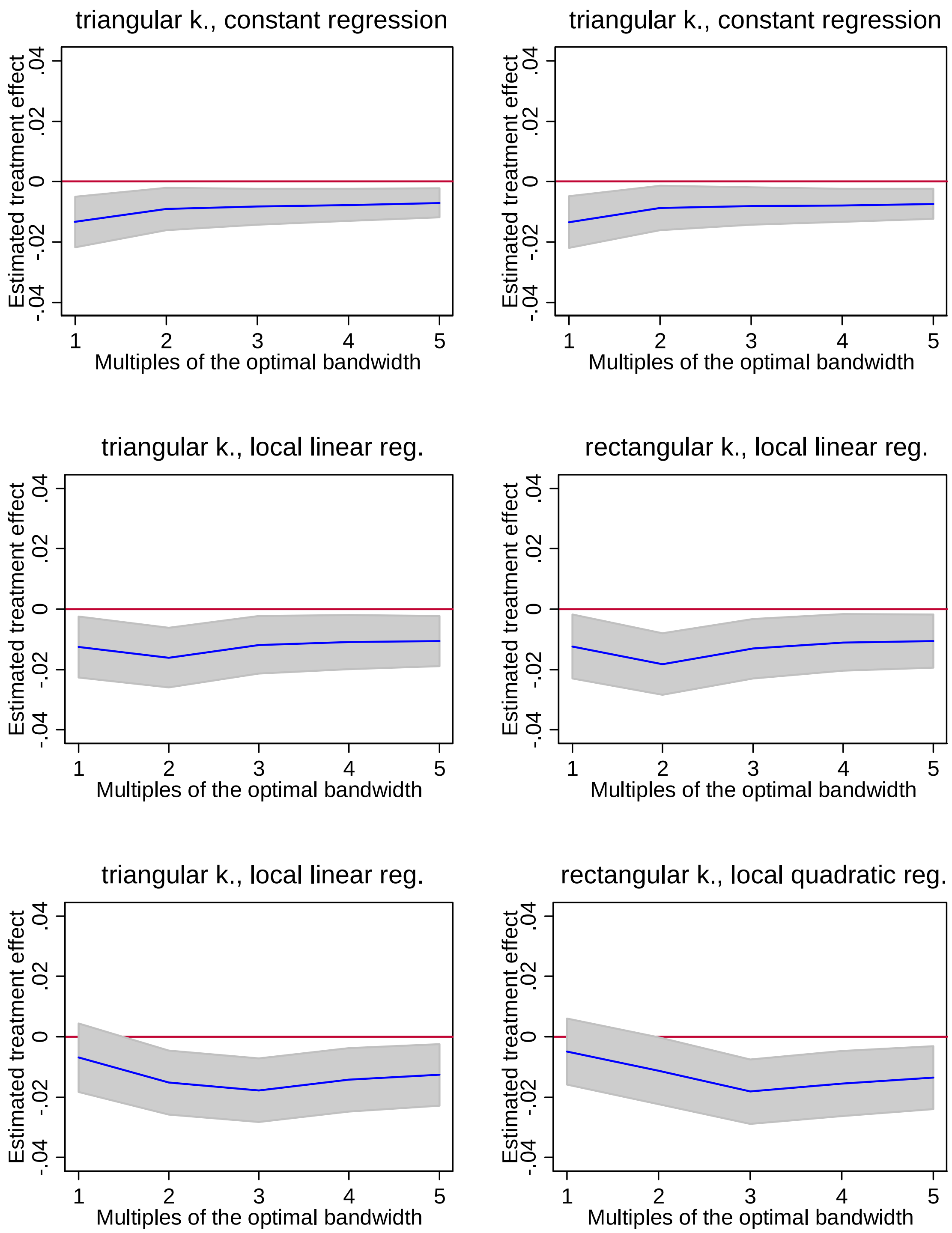
[47] Of course, violent crimes could be instrumental to economic crimes (e.g. an assault during a robbery), in which case the police archive would record both types of offences. The fact that the estimated coefficient on violent crimes is lower than that on economic crimes means thus that legal status has less of an effect on violent crimes that are not associated with – and, presumably, are not instrumental to – other economic crimes.

Figure 34: treatment effect of legal status on the probability of committing any felony, non-parametric estimates, sensitivity analysis



Note: the graphs show the sensitivity of the 2SLS coefficient in columns (1)-(3) of to different specifications of the kernel local regressions. In particular, we consider different time windows, up to 5 times the optimal bandwidth (on the horizontal axis of each graph), and different specification of the regression function (rows) and of the kernel (columns).

Figure 35: treatment effect of legal status on the probability of committing economic crimes, non-parametric estimates, sensitivity analysis



Note: the graphs show the sensitivity of the 2SLS coefficient in columns (1)-(3) of to different specifications of the kernel local regressions. In particular, we consider different time windows, up to 5 times the optimal bandwidth (on the horizontal axis of each graph), and different specification of the regression function (rows) and of the kernel (columns).

5.5.2. Parametric estimates

In order to examine the sensitivity of the results across individuals that applied in a larger window around the cutoff point, we progressively expand the sample to include all individuals that applied within 10, 20, and 30 minutes away from the cutoff (about 44, 81 and 110 thousand observations, respectively). When doing so, we control for confounding factors by including on the right hand side of the equation a quartic polynomial in T, as well as individual age, age square, and a full set of lottery fixed effects; heteroskedasticity-robust standard errors are also clustered at the lottery-level.[48]

Table **22** and Table 23 present the main results, distinguishing respectively between applicants to type A and type B permits, and – within the former sample – between the effect on economic and violent crimes. The coefficients are very similar to those estimated by non-parametric methods, both in terms of statistical significance and magnitude.

As in the case of non-parametric estimates, they are also extremely stable for different specifications of the regression. The graphs in Figure 36 show how the point estimate, and the associated confidence intervals, of the treatment effect of legal status varies with the degree of the polynomial in T (on the horizontal axis) and with the time bandwidths. Both the effect on all types of crimes and on economic crimes (left and right column, respectively) remains remarkably stable.

We next examine the sensitivity of the results along another dimension, namely the timing of the cutoff. For the reasons explained in the previous sections, the RD estimates are (internally) consistent in a neighborhood of the cutoff, but their (external) validity may be limited as we move away from there. To some extent, such limit is intimately related to the very nature of the RD approach. In the present case, however, we are provided with a full distribution of cutoffs – one for each lottery – which in turn delivers a distribution of estimated effects for individuals that applied at different moments of the day.

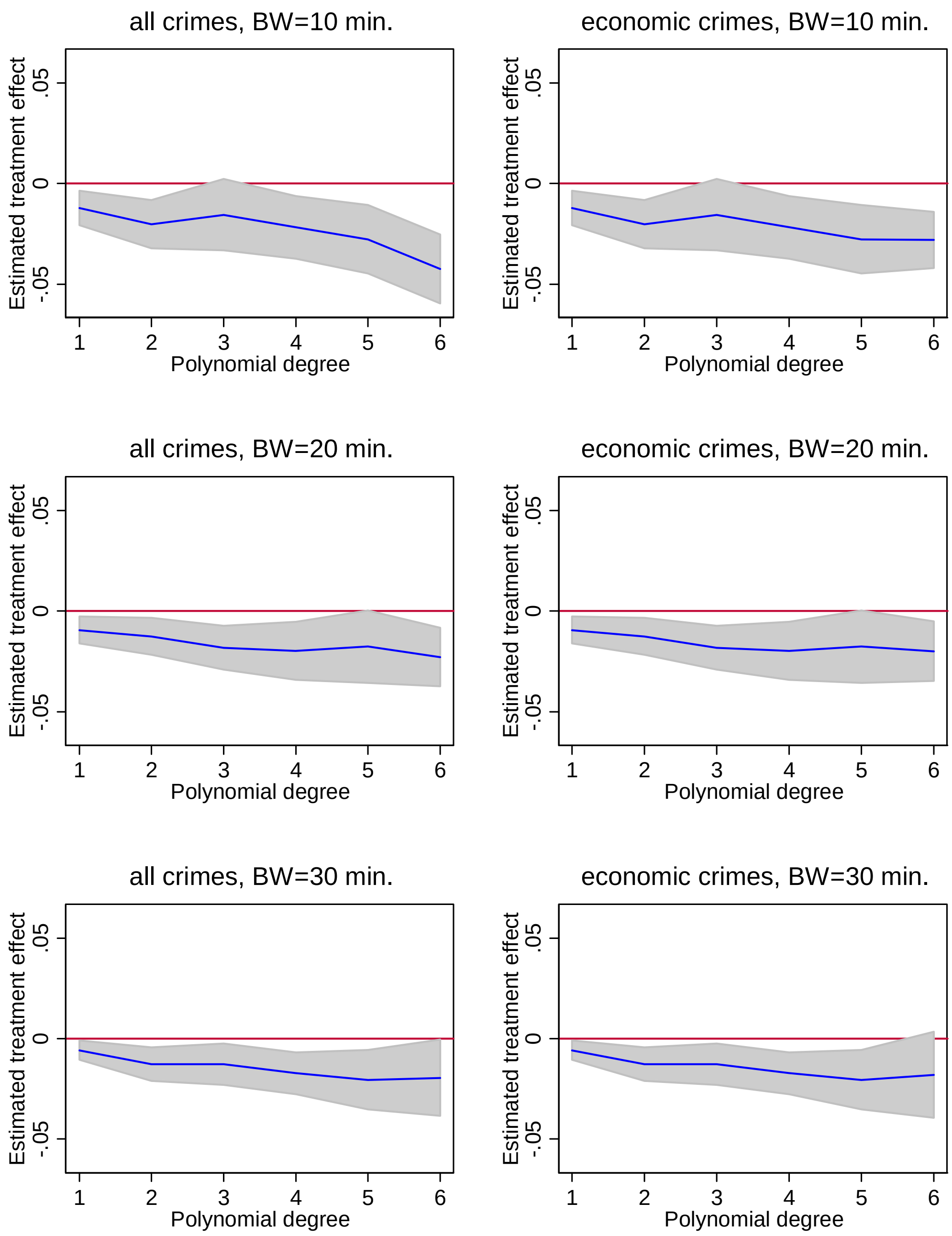
In practice, we include on the right-hand side of the equation a full set of indicator variables for the subsamples defined by the deciles of the distribution of cutoffs' timing, and interact them both with the second stage regressor of main interest, L·T, and with its first stage instrument, Z·T. Figure 37 shows that the interaction coefficients, and associated confidence intervals, are extremely stable. While each of these estimates still captures the local effect for the "compliers" with treatment assignment, it is nevertheless re-

[48] Card and Lee (2008) recommend to cluster the standard errors across groups of individuals reporting the same (discrete) values of the running variable. In this case, however, the running variable continuous – both conceptually and empirically – thanks to the availability of application times at the millisecond; indeed, no two applications were sent exactly at the same millisecond. Instead, we cluster the errors at the lottery-level to account for the possibility of interactions in crime among groups of immigrants of the same nationality and/or residing in the same Italian province.

assuring to see that the negative RD coefficient estimated on the whole sample remains stable across different subsamples.

Overall, the 2SLS-estimated coefficients confirm that the refusal of residence permit raises significantly, by between 1 and 2 percentage points (depending on the specification), the probability of committing a crime for applicants to type A permits, and provides additional evidence about the robustness of such result.

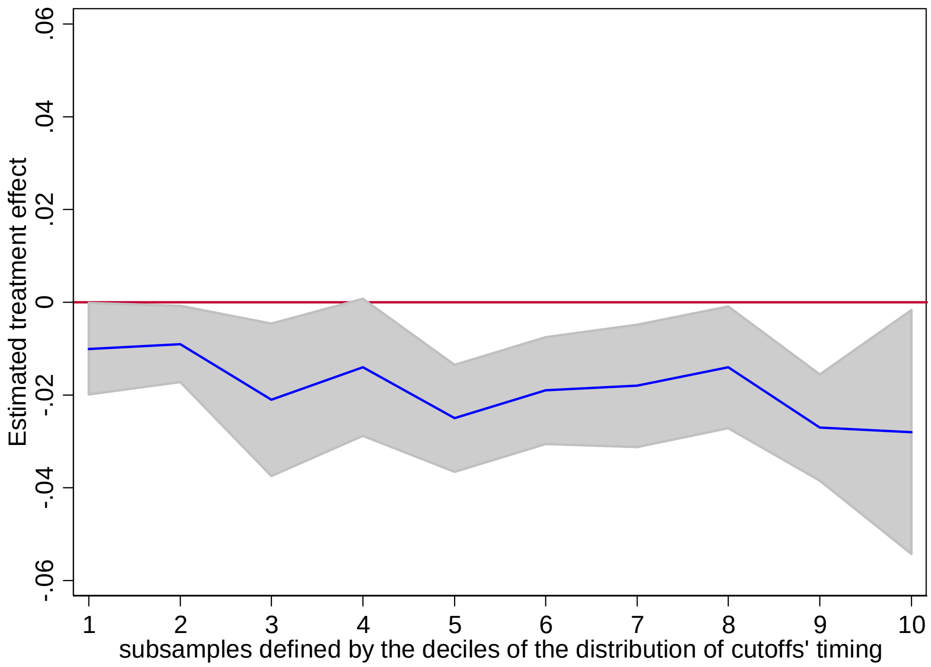
Figure 36: treatment effect of legal status on the probability of committing crimes, 2SLS estimates, sensitivity analysis



Note: the graphs show the sensitivity of the 2SLS coefficient in columns (1)-(3) of

Table **22** and Table 23 to different specifications of the time windows (rows) and different specifications of the polynomial in the regression (on the horizontal axis of each graphs).

Figure 37: treatment effect of legal status at different cutoff points



5.6. Concluding remarks

The results in this chapter confirm that legal status has important consequences for immigrant crime. Comparing immigrants that obtained and did not obtain legal status – for just a matter of seconds in sending the application – we show that the latter group is characterized by a higher risk of committing crimes over the following year. This means that the increase in the individual propensity to commit crimes may counter the incapacitation of potential criminals that occurs through expulsions.

We also showed that there is a substantial heterogeneity in the effect of legal status across different types of applicants. In particular, applicants that are sponsored by a firm (and that probably work there already) always exhibit a very low propensity to commit crimes, regardless of whether they obtained legal status or not. Immigrants applying just for domestic work, which may mask an absence of real employment opportunities in Italy, are instead at a much greater risk of engaging in crime in case they are refused legal status. Such heterogeneity confirms that the effect of legal status works through the different labor market opportunities of legal and illegal immigrants.

5.7. Tables

Table 18: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	all lotteries		**significant break**		**100+ applicants**	
	males	Females	males	females	males	females
	REPORTING RATES BY THE POLICE (PERCENTAGES)					
any felony	0.852	0.065	0.883	0.066	0.914	0.067
	(0.018)	(0.007)	(0.020)	(0.007)	(0.021)	(0.007)
thefts	0.197	0.034	0.201	0.035	0.209	0.035
	(0.009)	(0.005)	(0.010)	(0.005)	(0.010)	(0.005)
robberies	0.148	0.004	0.155	0.005	0.158	0.005
	(0.008)	(0.002)	(0.009)	(0.002)	(0.009)	(0.002)
drug-trafficking	0.141	0.007	0.141	0.007	0.142	0.007
	(0.007)	(0.002)	(0.008)	(0.002)	(0.008)	(0.002)
smuggling	0.016	0.004	0.017	0.005	0.017	0.005
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
kidnappings	0.022	0.000	0.023	0.000	0.024	0.000
	(0.003)		(0.003)		(0.003)	
extortions	0.043	0.007	0.045	0.008	0.048	0.008
	(0.004)	(0.002)	(0.005)	(0.002)	(0.005)	(0.003)
illegal carrying of firearms	0.116	0.001	0.117	0.002	0.123	0.001
	(0.007)	(0.001)	(0.007)	(0.001)	(0.008)	(0.001)
murder	0.032	0.002	0.033	0.002	0.034	0.002
	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)
violent assault	0.238	0.006	0.252	0.005	0.264	0.006
	(0.010)	(0.002)	(0.011)	(0.002)	(0.012)	(0.002)
rape	0.088	0.000	0.092	0.000	0.098	0.000
	(0.006)		(0.007)		(0.007)	
	OTHER INDIVIDUAL CHARACTERISTICS					
age	33.92	39.09	33.86	39.14	33.84	39.20
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
from low income country	8.46	1.35	8.71	1.27	8.41	1.28
	(0.05)	(0.03)	(0.06)	(0.03)	(0.06)	(0.03)
from lower-middle income	58.78	57.19	56.49	55.72	55.99	54.84
	(0.10)	(0.13)	(0.11)	(0.14)	(0.11)	(0.14)
from upper-middle income	31.98	40.58	34.01	42.17	34.76	43.03
	(0.09)	(0.13)	(0.10)	(0.14)	(0.11)	(0.14)
from high income country	0.75	0.86	0.77	0.82	0.82	0.83
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Northern Italy	62.62	60.47	64.75	61.64	65.89	62.04
	(0.10)	(0.13)	(0.10)	(0.14)	(0.11)	(0.14)
Centre Italy	20.48	22.75	20.52	22.58	20.55	22.79
	(0.08)	(0.11)	(0.09)	(0.12)	(0.09)	(0.12)
Southern Italy	16.90	16.78	14.73	15.78	13.57	15.17
	(0.07)	(0.10)	(0.08)	(0.10)	(0.08)	(0.10)

N. of observations	256,703	147,038	212,039	128,411	198,939	125,092

Note: The table shows the average probability of being reported by the police for different types of crime (top panel) and the average characteristics (bottom panel) of the individuals in our sample. The first two columns refer to the total sample; the second and third column refer to the subsample of lotteries with a significant structural break in the probability of treatment assignment; finally, the last two columns refer to lotteries with more than 100 applicants.

Table 19: treatment effect of legal status, kernel local linear regression, by category of applicant

	Dependent variable: Y=1 if committed a felony in year 2008					
	Type A applicants			Type B applicants		
	(1)	(2)	(3)	(4)	(5)	(6)
Bandwith:						
multiples of optimal b.	*1*	*2*	*3*	*1*	*2*	*3*
value	*1:20 min.*	*2:40 min.*	*4:00 min.*	*1:07 min.*	*2:14 min.*	*3:21 min.*
Estimated coefficients:						
reduced form	-0.010**	-0.011***	-0.009**	0.008	0.003	0.000
	(0.005)	(0.004)	(0.004)	(0.008)	(0.005)	(0.004)
first stage	0.610***	0.603***	0.607***	0.411***	0.367***	0.343***
	(0.032)	(0.024)	(0.020)	(0.031)	(0.023)	(0.019)
2SLS estimate	-0.017**	-0.019***	-0.014**	0.019	0.008	0.001
	(0.008)	(0.007)	(0.007)	(0.019)	(0.013)	(0.011)
Obs. inside the window	2,393	4,557	6,779	3,572	6,638	9,850

Note: the tables shows the results of non-parametric estimates of the effect of obtaining a residence permit at the click day 2007 on the probability of committing a felony in year 2008. The dependent variable is a dummy Y=1 for individuals committing a felony in year 2008. The left and right columns refer to applicants for type A and type B permits, respectively. The estimates are based on (triangular) kernel local linear regressions in a symmetric time window around cutoff. The half-width of the time window is reported on top of each column, and corresponds to a given multiple of the optimal bandwidth, computed according to the criterion of Imbens and Kalyanaraman (2012). The number of observations inside the time window is reported on the bottom of each column. The estimated coefficient in the first row is the reduced form regression of Y on a dummy Z=1 for individuals that applied before the cutoff, the coefficient in the second row is the first stage regression of a dummy L=1 for obtaining a residence permit on Z, and the 2SLS coefficient in the third row is the ratio of the second and the first stage coefficients. Heteroskedasticity-robust standard errors are reported in parenthesis.

Table 20: treatment effect of legal status, kernel local linear regression, by type of crime

	Dependent variable: Y=1 if committed a felony in year 2008					
	economic crimes			violent crimes		
	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth:						
multiples of the optimal b.	*1*	*2*	*3*	*1*	*2*	*3*
value	*1:12 min.*	*2:24 min.*	*3:36 min.*	*1:13 min.*	*2:26 min.*	*3:39 min.*
Estimated coefficients:						
reduced form	-0.007*	-0.010***	-0.008**	-0.004	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)
first stage	0.608***	0.603***	0.606***	0.608***	0.603***	0.606***
	(0.034)	(0.026)	(0.021)	(0.034)	(0.025)	(0.021)
2SLS estimate	-0.012*	-0.017***	-0.013**	-0.006	-0.005	-0.004
	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Obs. inside the window	2,159	4,144	6,098	2,205	4,226	6,222

Note: the tables shows the results of non-parametric estimates of the effect of obtaining a residence permit at the click day 2007 on the probability of committing a felony in year 2008, distinguishing between economic (columns 1-3) and violent crimes (columns 4-5). The dependent variable is a dummy Y=1 for individuals committing a felony in year 2008. The estimates are based on (triangular) kernel local linear regressions in a symmetric time window around cutoff. The half-width of the time window is reported on top of each column, and corresponds to a given multiple of the optimal bandwidth, computed according to the criterion of Imbens and Kalyanaraman (2012). The number of observations inside the time window is reported on the bottom of each column. The estimated coefficient in the first row is the reduced form regression of Y on a dummy Z=1 for individuals that applied before the cutoff, the coefficient in the second row is the first stage regression of a dummy L=1 for obtaining a residence permit on Z, and the 2SLS coefficient in the third row is the ratio of the second and the first stage coefficients. Heteroskedasticity-robust standard errors are reported in parenthesis.

Table 21: differences in observable characteristics near the cutoff (balance test), kernel local linear regression

bandwidth	**Covariates**							
multiples of the optimal bandwidth	age	low income	lower-middle	upper-middle	high income	Northern Italy	Centre Italy	Southern Italy
1	0.266	-0.031	-0.040	0.075**	0.006	0.047	-0.012	-0.055**
	(0.378)	(0.024)	(0.035)	(0.032)	(0.009)	(0.032)	(0.028)	(0.025)
2	0.140	-0.005	-0.018	0.030	0.001	0.032	-0.011	-0.028
	(0.278)	(0.018)	(0.026)	(0.024)	(0.006)	(0.023)	(0.020)	(0.018)
3	0.127	-0.001	-0.004	0.008	-0.002	0.028	-0.012	-0.021
	(0.231)	(0.015)	(0.022)	(0.020)	(0.004)	(0.019)	(0.017)	(0.015)

Note: the tables shows the results of (triangular) kernel local linear regressions, in a symmetric window of the cutoff, of individual characteristics on a dummy Z=1 for applying before the cutoff. The half-width of the time window corresponds to a given multiple (reported in the first column) of the optimal bandwidth, based on the criterion of Imbens and Kalyanaraman (2012). Heteroskedasticity-robust standard errors are reported in parenthesis.

Table 22: legal status and probability of committing crimes, by category of applicant, 2SLS estimates

	type A applicants			type B applicants		
Second stage. Dependent variable: Y=1 if committed a felony in year 2008						
	10 min.	*20 min.*	*30 min.*	*10 min.*	*20 min.*	*30 min.*
Legal status	-0.022**	-0.020**	-0.017***	0.004	-0.011	-0.003
	(0.009)	(0.009)	(0.006)	(0.014)	(0.010)	(0.008)
First stage. Dependent variable: L=1 if obtained a residence permit at the click day 2007						
Z	0.657***	0.650***	0.631***	0.366***	0.356***	0.353***
	(0.038)	(0.033)	(0.032)	(0.044)	(0.044)	(0.039)
Observations	16,131	29,737	40,451	27,995	51,212	69,886
F-stat excluded instruments						

Note: the tables shows the results of parametric estimates of the effect of obtaining a residence permit at the click day 2007 on the probability of committing a felony in year 2008, distinguishing between applicants to type A permits (columns 1-3) and applicants to type B permits (columns 4-5). The dependent variable is a dummy Y=1 for individuals committing a felony in year 2008, and the main explanatory variable is a dummy L=1 for obtaining a residence permit. The effect is estimated by 2SLS regression, in a symmetric time window around cutoff; the half-width of the time window is reported on top of each column. The first-stage instrument is a dummy Z=1 for individuals that applied before the cutoff. The coefficients of main interest in the second and first stage are reported in the top and bottom panel, respectively. Both the first and second stage regression include in addition a quartic polynomial of the lag between the application and the cutoff, as well as a full set of lottery fixed effects, individual age and age squared. Heteroskedasticity-robust standard errors clustered by lottery are reported in parenthesis.

Table 23: legal status and the probability of committing crimes, by type of crime, 2SLS estimates

	economic crimes			violent crimes		
Second stage. Dependent variable: Y=1 if committed a felony in year 2008						
	10 min.	*20 min.*	*30 min.*	*10 min.*	*20 min.*	*30 min.*
Legal status	-0.019**	-0.014*	-0.012**	-0.007	-0.008*	-0.006*
	(0.008)	(0.008)	(0.005)	(0.006)	(0.005)	(0.004)
Observations	16,131	29,737	40,451	27,995	51,212	69,886

Note: the tables shows the results of parametric estimates of the effect of obtaining a residence permit at the click day 2007 on the probability of committing a felony in year 2008, distinguishing between economic (columns 1-3) and violent crimes (columns 4-6). The dependent variable is a dummy Y=1 for individuals committing a felony in year 2008, and the main explanatory variable is a dummy L=1 for obtaining a residence permit. The effect is estimated by 2SLS regression, in a symmetric time window around cutoff; the half-width of the time window is reported on top of each column. The first-stage instrument is a dummy Z=1 for individuals that applied before the cutoff. Both the first and second stage regression include in addition a quartic polynomial of the lag between the application and the cutoff, as well as a full set of lottery fixed effects, individual age and age squared. Only the coefficient of main interest in the second stage is reported, as the first stage is identical to that in columns (1-3) of Table 18. Heteroskedasticity-robust standard errors clustered by lottery are reported in parenthesis.

Chapter 6 – The U.S. experience

6.1. Introduction

"America's immigration system is broken. Too many employers game the system by hiring undocumented workers and there are 11 million people living in the shadows. Neither is good for the economy or the country."[49]

Although the figure of 11 million illegal immigrants residing in the United States is at the center of the current political debate, this number represents roughly one third of the foreign-born population in the United States, while total immigrants amount to about one tenth of the U.S. population.[50] Is this much ado about nothing?

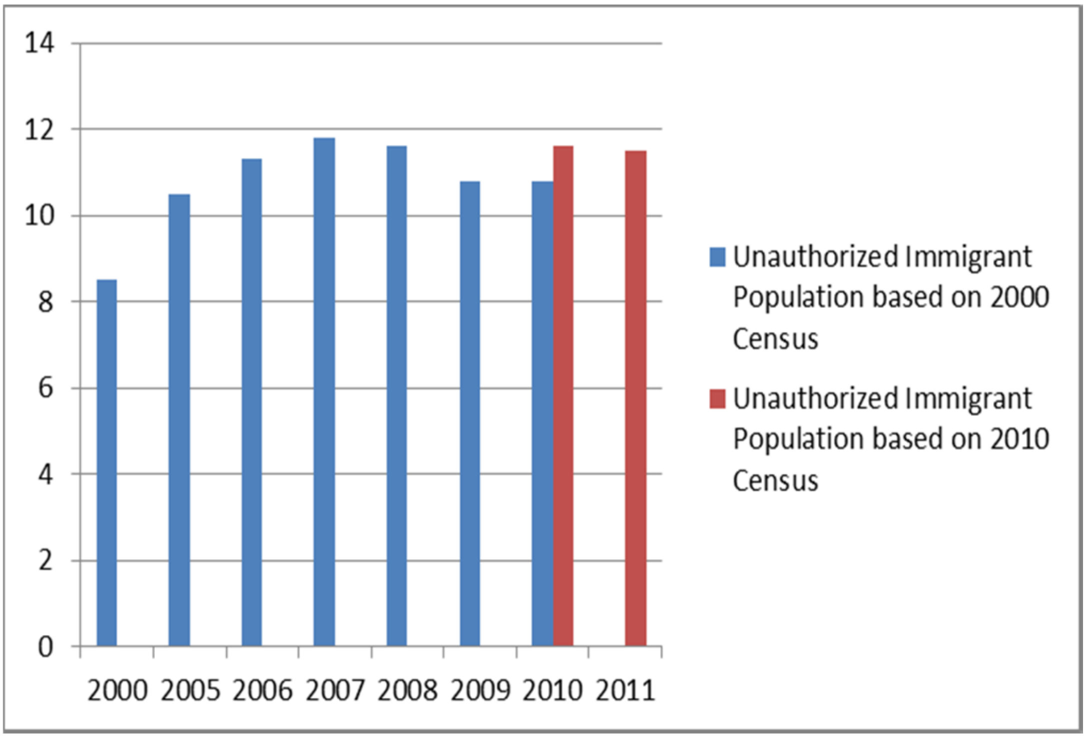
In what follows, we assess how the current immigration policy in the United States has historically shaped immigration inflows, and we investigate causal links between immigration, legal status, and crime. A first comparison of the trends in illegal immigration and crime (Figure 38 to Figure 41) shows no evidence of co-movements in these series; we will argue that, with some exceptions, immigration to the United States does not increase crime. There are plausible reasons for this, including a well-functioning deterrence system and the pro-cyclicality of illegal immigration.

In Section 1 we review the U.S. institutional framework concerning immigration, analyzing how it affects immigration decisions. We compare the predictions from Chapter 2 with data on different cohorts of immigrants by citizenship status, focusing on labor force participation, selection into industries, earnings and poverty. Our results show evidence of labor market discrimination for non-citizens, despite similarities in occupations for immigrants coming from the same continent. Furthermore, lower earnings and higher poverty rates among non-citizens suggest that the path to citizenship is relevant for immigrants' wellbeing. In Section 2 we discuss these qualitative results in light of the existing literature, while Section 3 presents novel estimates of the effect of immigration on crime, exploiting the Mariel Boatlift as a case study. Our estimates represent an upper bound on the impact of unregulated immigration on crime rates, with the Boatlift being an exception to the norm because many, negatively selected immigrants were concentrated in a single area and were not granted a path to citizenship for years. The magnitudes that we estimate suggest that, under more typical circumstances, immigration to the United States does not increase crime rates, despite immigrants facing persistently worse labor market opportunities than U.S. citizens.

[49] Source: White House website, accessed on April 7, 2013. http://www.whitehouse.gov/issues/immigration

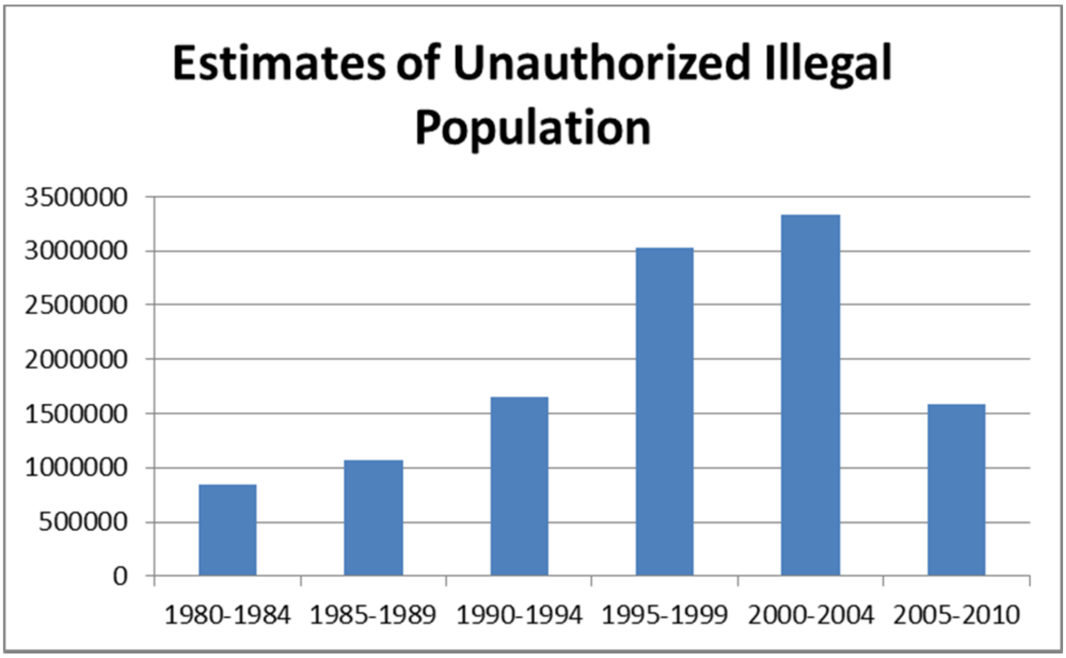
[50] Hoefer et al. (2012) estimate the illegal immigrant population to be around 11.5 million and the legal immigrant population to be around 22.1 million people, as of January 2012.

Figure 38: Estimates of the Unauthorized Immigrant Population, Millions of Individuals



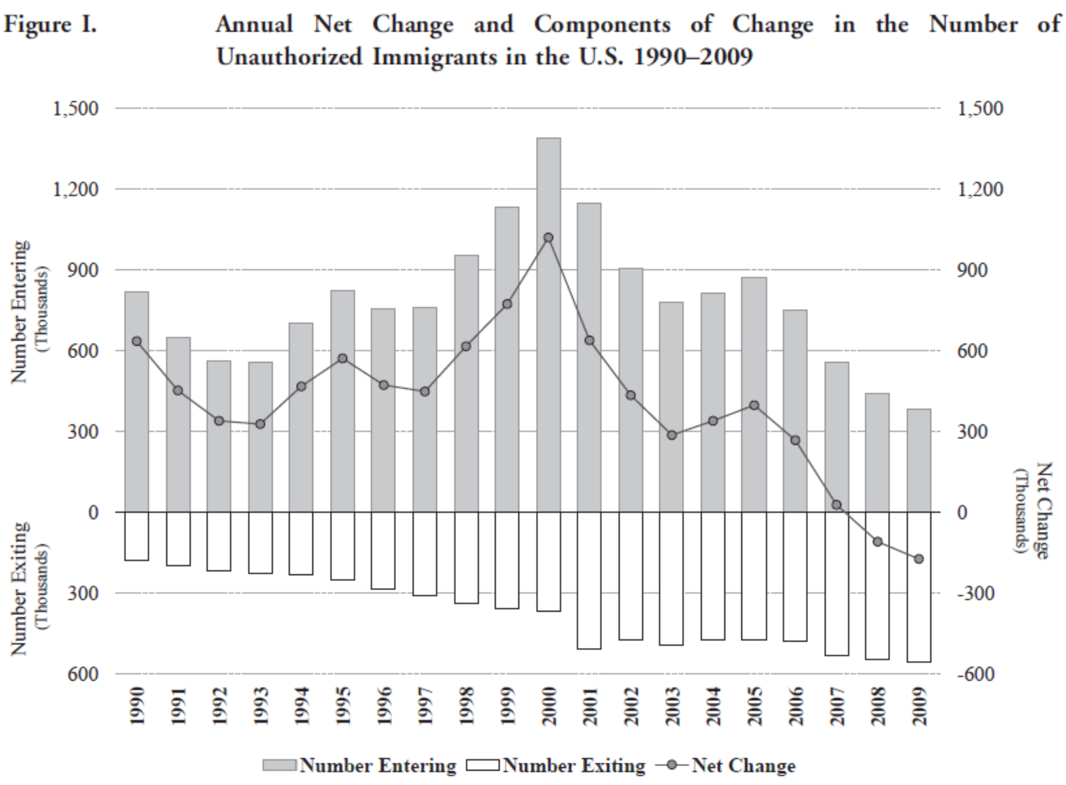
Source: Hoefer et al. (2012)

Figure 39: Estimates of the Unauthorized Immigrant Population by wave of entry



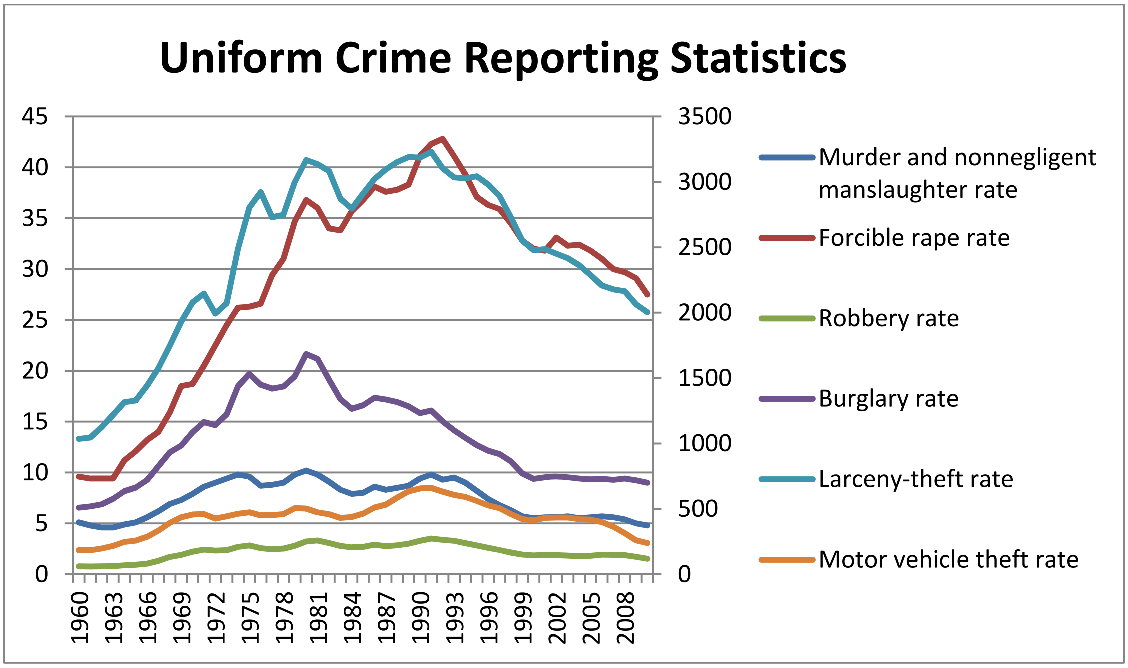
Source: Hoefer et al. (2012).

Figure 40: Annual Net Change and Components of Change in the Number of Unauthorized Immigrants



Source: Warren and Warren (2013).

Figure 41: Trends in Crime Rates



Source: FBI-UCR. Murder and rape rates to be read on the left axis.

6.2. Immigration to the US: Facts and Policy

This chapter describes the current legal framework for immigration to the United States, and provides an account of the two main immigration reform proposals circulating in the press. Then, we analyze how this institutional framework shapes both the stock of immigrants now in the United States and future inflows of immigrants.

6.2.1. Institutional Framework

6.2.1.1. The current Policy: Legislation and Enforcement

Legal immigration to the United States can be either permanent, through a permanent resident card (i.e. green card) or temporary, through a non-immigrant visa. Permanent residents can apply for citizenship after five years (shortened to three if they are married to a U.S. citizen). Temporary immigration, in contrast, does not provide a path to citizenship.

The Immigration Act of 1990, amending the Immigration and Nationality Act (INA), sets overall annual caps on the number of green cards for certain categories. An overwhelming majority of cards are set aside for family-sponsored immigration (226,000 in the fiscal year 2012, the legal minimum).[51] The remaining slots are divided between job-related green cards (140,000) and lottery greencards, awarded randomly to citizens of "low-admission" states (55,000). The employment-related green cards are further regulated: 57.2% (80,080) are to be allocated to individuals with extraordinary ability, outstanding researchers, managers of multinationals, or professionals holding advanced degrees. This leaves only 40,040 slots, of which only up to 20,020 are for unskilled workers, resulting in waiting times for these workers of more than five years. Moreover, the INA sets the per-country preference visa limit to 7% of the total. Mexico, China and the Philippines currently exceed this limit, with their citizens having to wait even longer for their visas.

The temporary work visas are mainly the H-2A visa (seasonal agricultural workers) or the H-2B visa (temporary non-agricultural workers). To request these visas, employers have to establish that the need for the worker is indeed temporary (or seasonal in nature), that *"there are not enough U.S. workers who are able, willing, qualified, and available to do the temporary work and that the employment of H-2A/H-2B workers will not adversely affect the wages and working conditions of similarly employed U.S. workers."*[52] H-2B visas are capped at 66,000 per fiscal year, to be evenly allocated in the two halves of the fiscal year.

[51] See the Visa Bulletin for April 2013, accessed at http://www.travel.state.gov/visa/bulletin/bulletin_5900.html
[52] USCIS website, accessed on April 7, 2013.

In light of these numbers, Hanson (2009) notes that the slots legally available to low-skilled immigrants are *"an inconsequential component of domestic low-skilled employment."* Furthermore, the current system favors family-sponsored immigration at the cost of employment-based immigration.[53] Thus, many workers who would benefit from immigrating legally into the United States might turn to illegal immigration: hence, immigration enforcement is crucial in this system. Here we discuss two policies that are peculiar to the United States – the Secure Fence Act and the E-verify procedure – and we present trends in different measures of enforcement.

6.2.1.2. Border Enforcement

The 2006 Secure Fence Act mandated the construction of a physical barrier between Mexico and the United States. As of now, however, only parts of that fence have been erected, and President Obama has frozen the expansion of the barrier. An interrupted barrier might simply divert migration flows to the open parts of the border; hence, the policy effects of this measure are hard to evaluate.

Border enforcement seems to have improved in the last decade. Indeed, apprehensions declined, although we cannot disentangle the deterrence effect of better patrolling from the demand effect caused by the financial crisis.[54]

Figure 42 shows that apprehensions start declining steadily in 2005, prior to the fence construction, and reached the quota 600,000 in 2010. Interpretation of these numbers is further complicated by the lack of data on attempts to immigrate, as we discuss more thoroughly in Section 2. Nonetheless, Table 24 and Table 25 show some suggestive evidence of an effect of the fence: the biggest decline in apprehensions is among Mexican immigrants and has happened in the Southwest sector, i.e. on the Mexican border. If we believe that all immigrants, not only Mexicans, respond to changes in labor market conditions, then the demand-side effect should be more symmetric across continents of origins and ports of entry.

Illegals' presence in the United States is also affected by removal of existing illegal immigrants, and Enforcement and Removal Operations (ERO) have increased over time.[55] The Immigration and Customs Enforcement agency (ICE) was established in 2003 in the context of the Homeland Security Act, as a

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[53] This has been true since the INA amendment of 1965 which discontinued the National Origins Formula in place since 1924. Clearly, to the extent that family-sponsored immigrants work, this provision will change the immigrants' pool of skills.

[54] Hanson (2009) argues that illegal immigration is pro-cyclical.

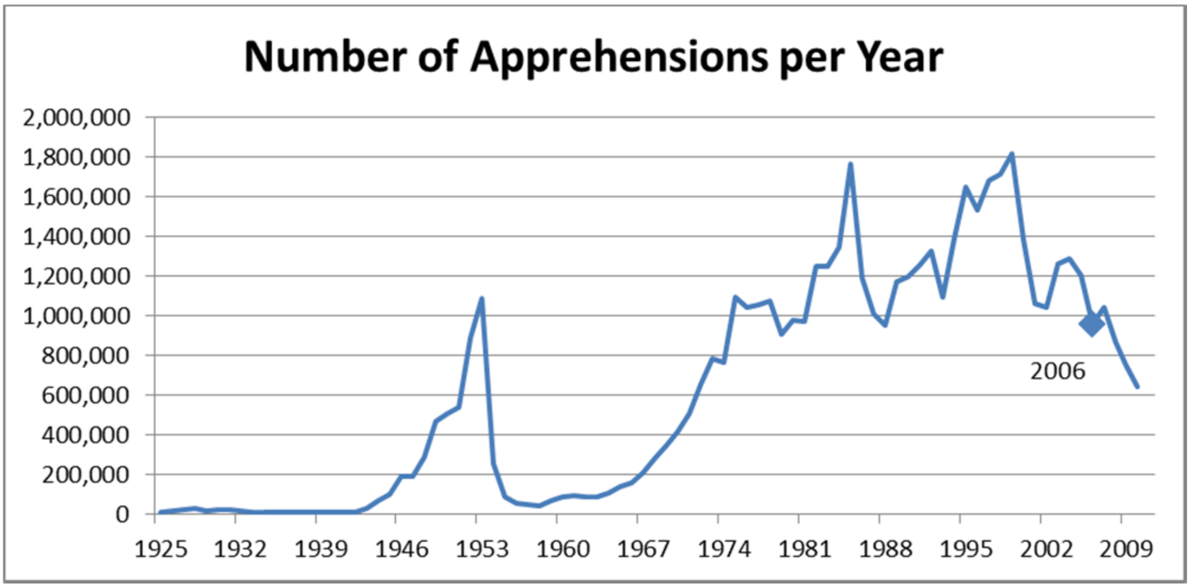
[55] Removals are the compulsory and confirmed movement of an inadmissible or deportable alien out of the United States based on an order of removal. An alien who is removed has administrative or criminal consequences placed on subsequent reentry owing to the fact of the removal (Yearbook of Immigration and Statistics, 2011).

response to the events of 9/11/2001. Remarkably, the number of operations under this program remained high during the years 2008-2011, while border apprehensions were declining steadily.

Figure 43 shows a surge in the number of removals, which have risen from around 45,000 in 1994 to 400,000 in 2010, and a “crackdown” on existing illegal immigrants. In particular, Figure 44 and Figure 45 show that removal of criminal aliens (i.e. aliens with prior criminal convictions) has increased, especially for immigrants from Central America and Mexico. In the context of the theoretical model in Chapter 2, removals constitute a very strong deterrent for crime: increasing the probability of removal decreases the stream of profits from criminal activities and illegal immigration. As we will see, both reform proposals currently on the table emphasize support for such operations.

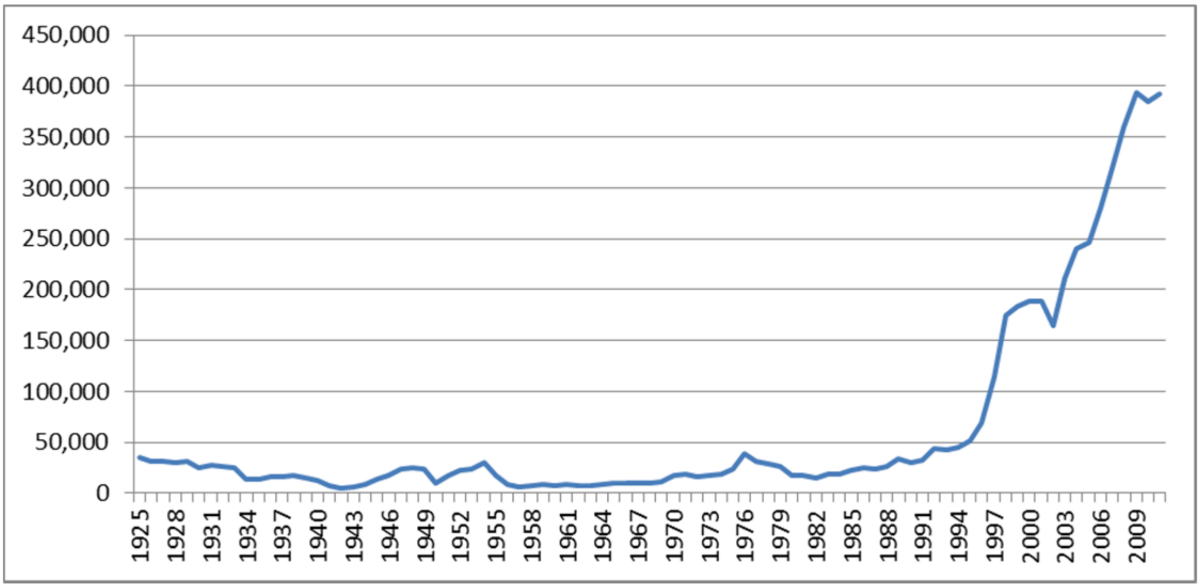
There is no evidence of substitution between border patrol surveillance and removal operations. On the contrary, Figure 46 shows a trend-breaking increase in total spending for immigration enforcement following the creation of ICE, separately from the Customs and Border Protection agency (CBP), in the context of the Homeland Security Act. In 2012, total spending on immigration was around $18 billion, two-thirds of which went to CBP and one-third to ICE. Other measures of enforcement, such as the number of border patrol agents and the number of linewatch hours, show similar increases over time. Figure 47 displays trends in border patrol persons and linewatch hours for the Mexican border; since the mid-1990s, linewatch hours have increased by 400% and border patrol agents have more than doubled.

Figure 42: Number of Apprehensions per Year



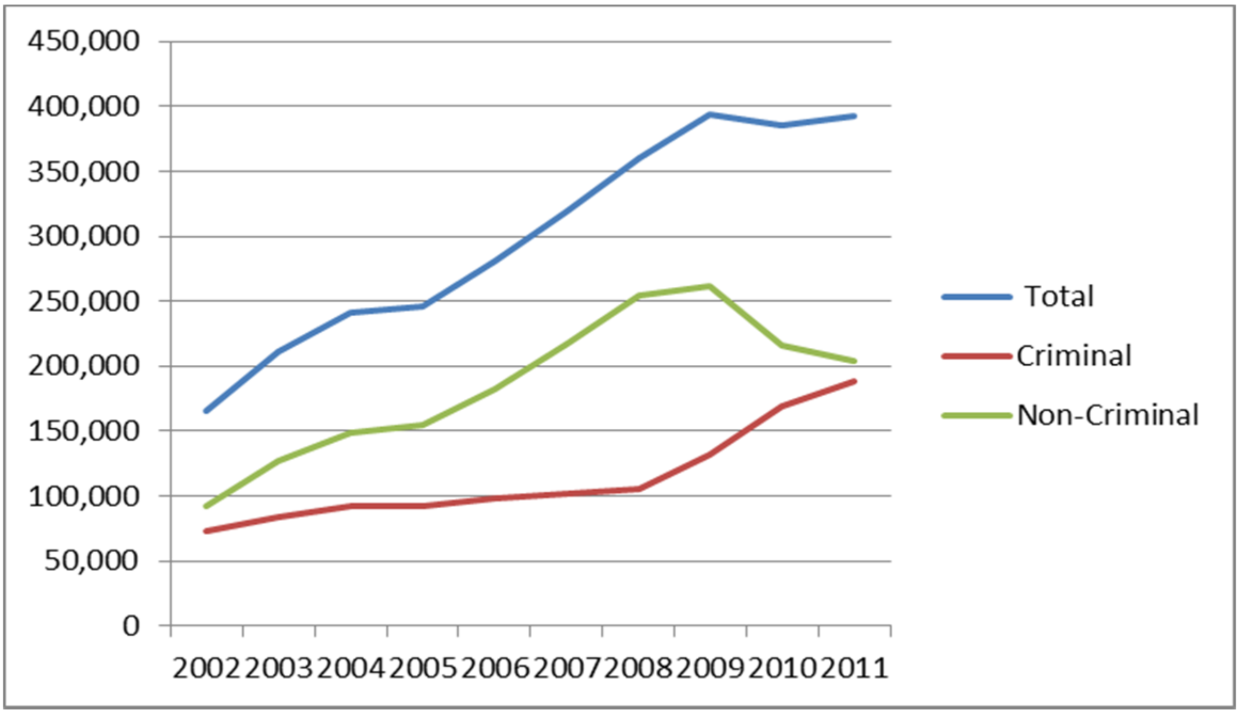
Source: Yearbook of Immigration Statistics, 2011

Figure 43: Number of Removals per Year



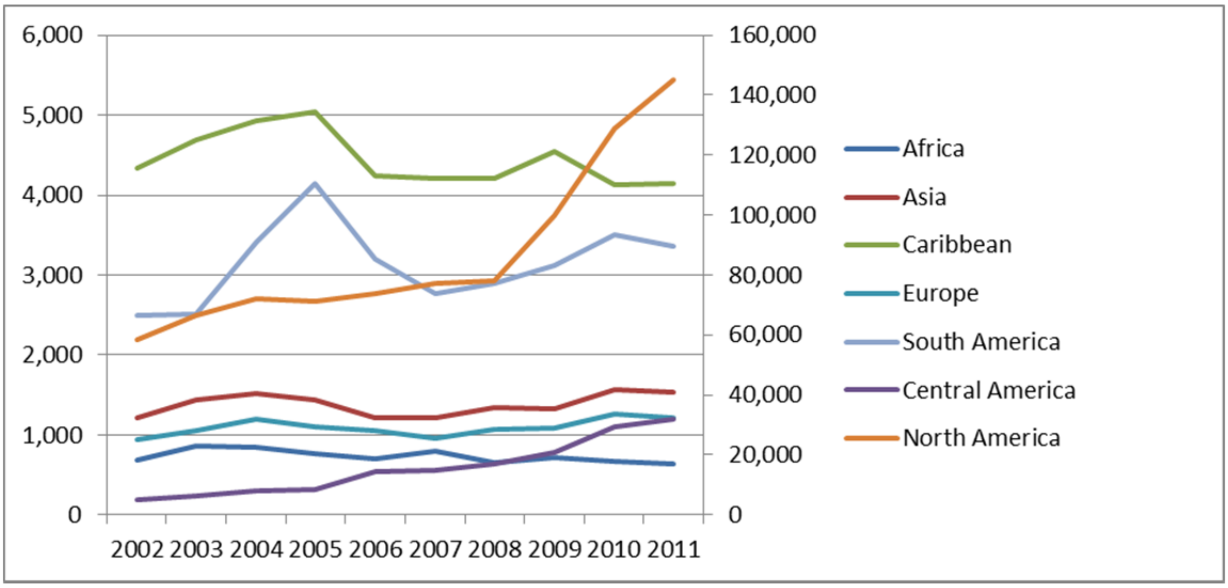
Source: Yearbook of Immigration Statistics, 2011.

Figure 44: Total Aliens Removed by Criminal Status



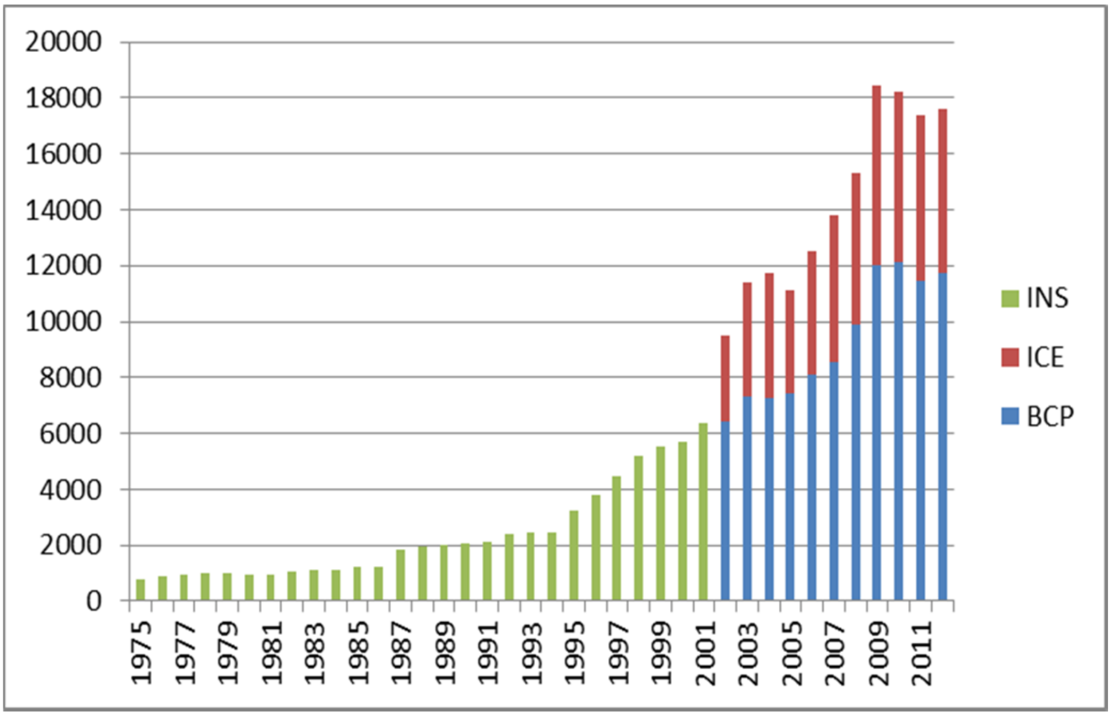
Source: Yearbook of Immigration Statics, 2011

Figure 45: Criminal Aliens Removed by Region of Nationality



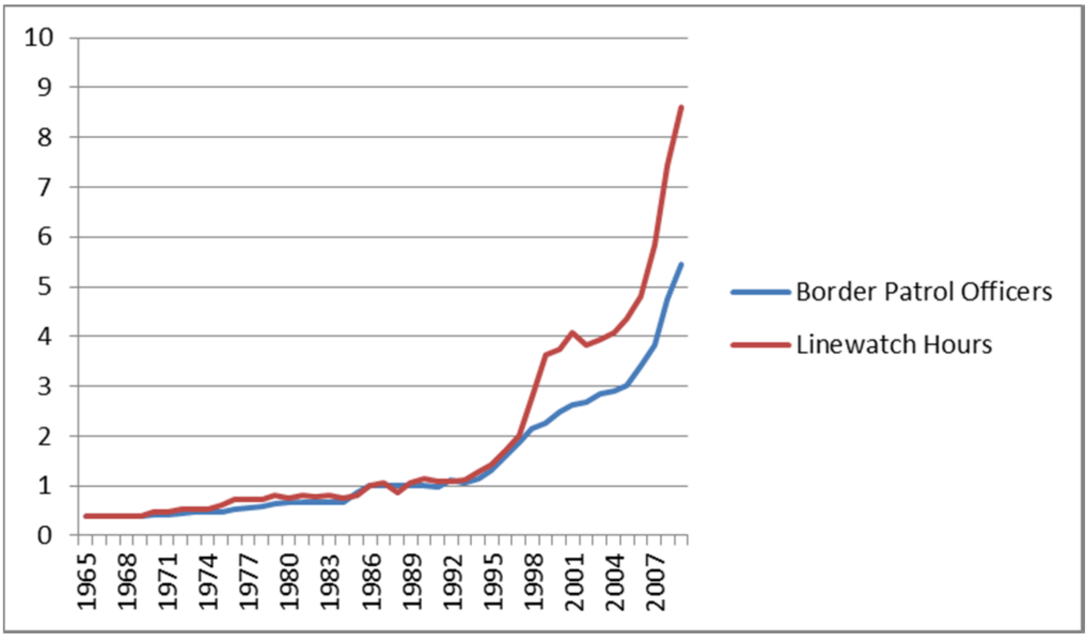
Note: Central and North America refer to the right axis. Source: Yearbok of Immigration Statistics, 2011.

Figure 46: Spending for Immigration Enforcement



Source: DHS Budget in brief. FY 2004-onwards. US Department of Justice: Immigration and Naturalization Service Budget. http://www.justice.gov/archive/jmd/1975_2002/2002/html/page104-08.htm. 2012 and 2013 are subject to revisions. Millions of 2012 Dollars.

Figure 47: Immigration Enforcement



Note: All figures have been normalized to be 1 in 1986.

6.2.1.3. Employer Verification

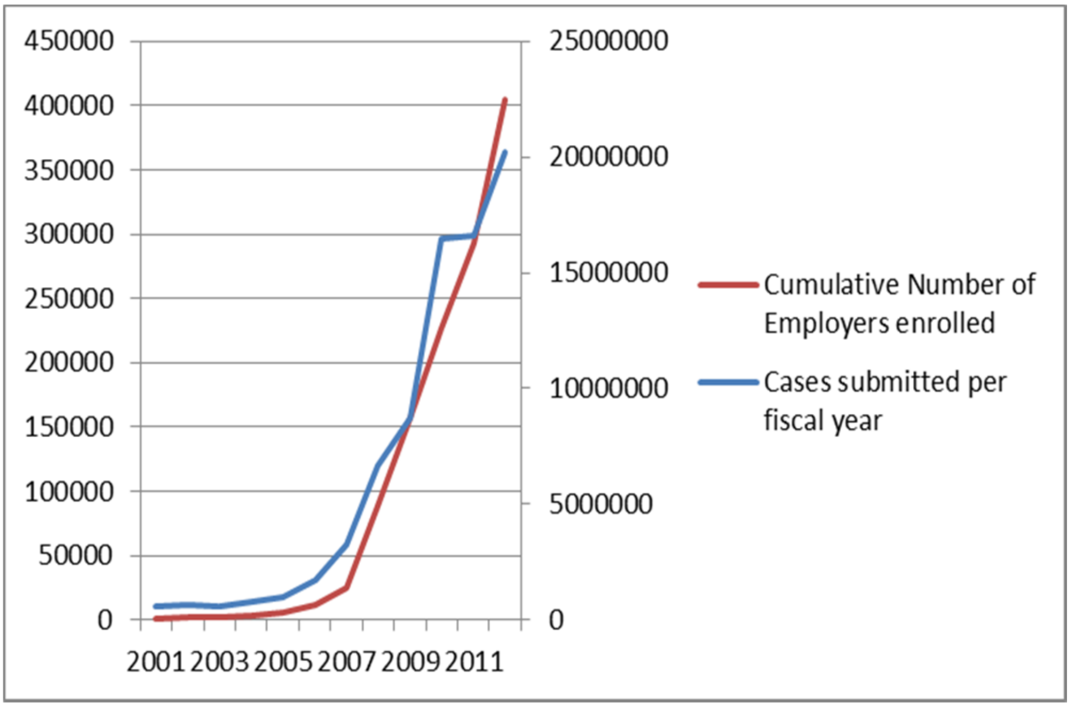
The E-verify program, a policy instrument that is peculiar to the United States, is designed for employers to verify the legal status of their employees, thereby lowering the value of illegal immigration. Differently from other receiving countries (such as Italy, for instance), undocumented immigrants in the United States often provide their employers with fake social security numbers (Brown et al., 2009; Hotchkiss & Quispe-Agnoli, 2009): in the absence of other official documents establishing the legal status of their employees, their ability to produce a social security number (although fake) may induce the employer to believe that they are lawfully residing in the US.

Begun as a voluntary pilot program in 1997 in California, Florida, Illinois, Nebraska, New York and Texas, the original version of the E-verification program allowed employers to compare information in INS and Social Security Administration (SSA) records in order to discover fake social security numbers. This basic pilot program was then re-authorized in various successive waves, and was extended to all 50 states by December 2004, when a new version of the program exploited the internet, making verification easier and quicker. In 2007 the program was re-named E-verify and acquired new features, such as photo matching and biometrics. At the same time, an employer-education campaign was launched, and its use was encouraged for state agencies. Employers responded to improvements in the software by enrolling in higher numbers, although the absolute number of employers enrolled was still low in relative terms. Figure

48 shows the evolution of the cumulative number of employers enrolled in the program in any given year, together with the number of cases verified per fiscal year. The 50,000 employers enrolled in 2007 now have become 350,000.

Starting in 2009, the program acquired a new mandatory flavor. First, the system began to keep track of evidence of noncompliant behavior by employers.[56] Second, federal agencies required federal contractors to verify all new employees and existing ones who work on covered federal contracts (i.e. contracts of a certain size). Finally, between 2011 and 2012 a new program that allows immigrants to self-check their work eligibility was launched. On the one hand, these self-checks should reduce the number of mismatches discovered by employers, because any illegal alien can verify that his fake credentials will be deemed invalid. On the other hand, immigrants, when they are aware of its existence, might perceive this process to be risky and hence avoid using it. In total, over 20 million employees have been verified in fiscal year 2012, but only 1.1% of them – 221,155 – were deemed not authorized to work (Figure 49). Unfortunately, we do not have figures for earlier years, and one might think that the program had its biggest impact as soon as employers enrolled. Another plausible explanation is that, upon enrollment in E-verify, employers fire those workers they know are illegal; hence, E-verify works more as a deterrent to hiring illegal aliens than as an investigative device, especially after the introduction of self-checks.

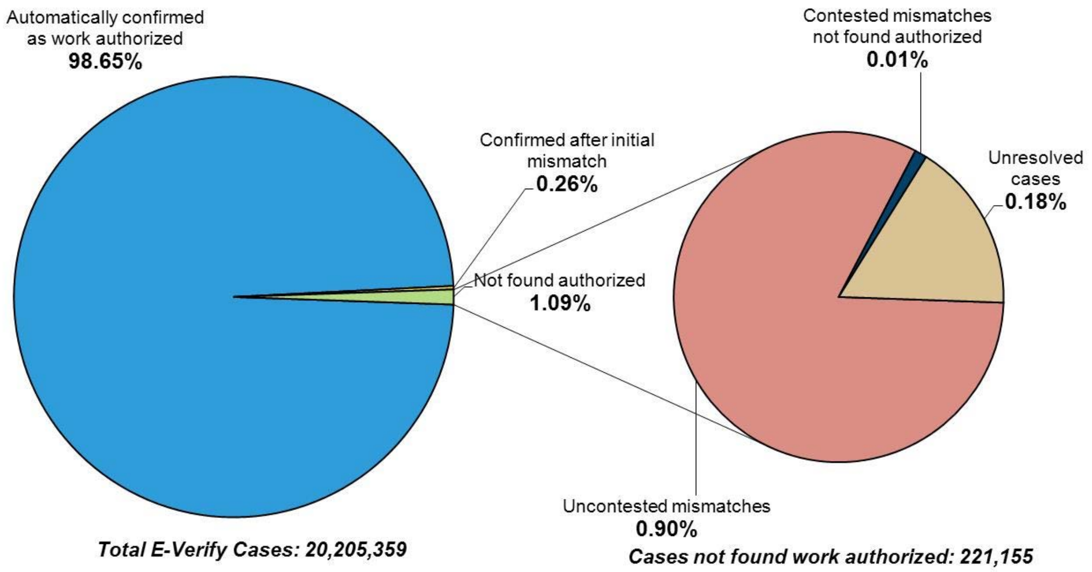
Figure 48: E-verify trends



[56] Noncompliance ranges from not respecting the deadlines for submission of information regarding new hires, to not allowing employees under Tentative Nonconfirmation (TNC), i.e. employees whose first verification failed and resulted in a mismatch, to defend themselves.

Source: USCIS, E-verify website. Employers enrolled levels are to be read on the left axis, number of cases on the right axis.

Figure 49: E-verify outcomes for fiscal year 2012



Source: USCIS, E-verify website. Accessed on April 8 2013.

E-verify is one example of how immigration law and enforcement differ across U.S. states. Some states, listed in Table 26, mandate the use of E-verify for certain categories of workers; others allow employers to use E-verify registration as a defense when charged with hiring an illegal alien (Tennessee and Pennsylvania). Finally, Arkansas forbids hiring illegal workers, but does not mandate E-verify. Availability of funding and personnel to enforce these state mandates constitutes another source of variation in immigration enforcement.[57]

Finally, some states have gone in the opposite direction. In October 2011, California prohibited its municipalities from mandating that businesses use E-verify; in January 2011, Rhode Island rescinded an earlier order from 2008 that required the use of E-verify.

E-verify is not the only case of state variation in immigration policy. Another well-known piece of state legislation on immigration is the 2010 Arizona SB 1070, which requires police officers to check immigration status whenever police activity is initiated. Similar heterogeneity can be found at the city level: some cities,

[57] See Feere (2012) for a report on funding.

called sanctuary cities, have adopted ordinances restraining their officials from questioning individuals for the sole purpose of determining their immigration status.[58]

Local variation in legislation and enforcement should be taken into account in any analysis of immigration and crime. Nonetheless, this variation is likely to be endogenous, and there is a need for good identification strategies in future research. The model in Chapter 2 highlights the salience of employer verification for criminal activity by illegal immigrants. By shutting down labor market opportunities for them, E-verify pushes illegal aliens towards crime; in the long run, however, lack of legal labor market opportunities will deter illegal immigrants, reducing crime rates. Which of the two effects prevails is an empirical question that cries out for an answer, which will depend upon data availability.

6.2.2. Immigration Reform Proposals

Discussions of imminent immigration reform have become increasingly serious over the last years. Here we analyze the two proposals currently on the table in the light of the theory sketched in Chapter 2. This section would not be complete, however, without mentioning another program recently implemented, and the likelihood of its being incorporated into the reform.

In June 2012 Secretary of Homeland Security Janet Napolitano put in place the Deferred Action for Childhood Arrivals (DACA) program. Under it, individuals below the age of thirty who satisfy certain requirements can be considered for relief from removal on a case by case basis: the directive is very clear in emphasizing that this does not provide a path to citizenship.

To be considered under DACA, immigrants have to prove that they arrived in the United States before they were sixteen. Furthermore, they need to currently be in school, or to have attained some higher education (high school graduation or a general education development certificate), and that they *"have not been convicted of a felony offense, a significant misdemeanor offense, multiple misdemeanor offenses, or otherwise pose a threat to national security or public safety."*[59] In other words, the measure aims at increasing education among young immigrants who might otherwise be deterred from continuing their studies. Hence, it is interesting to look at the number of individuals potentially covered by this provision.

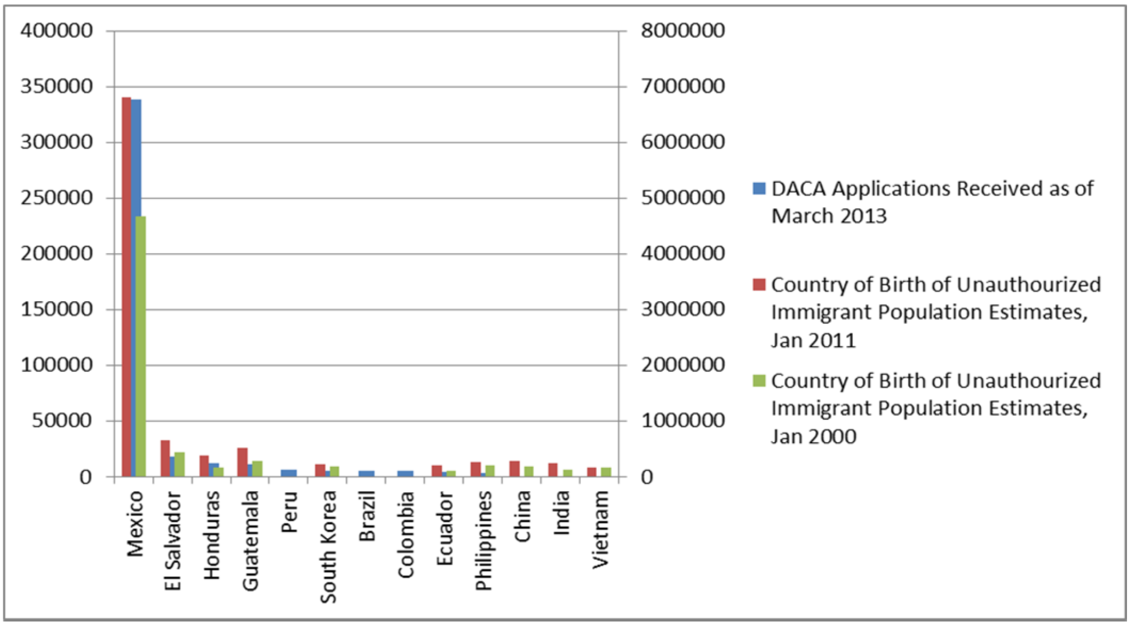
Table 27 shows trends in applications submitted and accepted up to March 14 2013: over half of the applications were submitted at the beginning of the program, between August and October 2012. As of now, around 4% of the estimated unauthorized population (469,530 individuals) has applied for DACA, but

[58] Cambridge, San Francisco, Los Angeles and New York City are among these.
[59] Source: DHS website. http://www.dhs.gov/news/2012/06/15/secretary-napolitano-announces-deferred-action-process-young-people-who-are-low

it is unclear what fraction of the eligible population this is. DACA application patterns mimic the estimates for the illegal population, with the exceptions of China and India which do not feature among the top ten countries of origin of DACA applicants (Figure 50 and Figure 51). The majority of the applicants come from Central America, and currently live in California and New York. In other words, immigrants' children do not seem to select differently into schooling based on their origins. Whether this is informative of selection patterns of their parents is open to further research.

The DACA program was arguably implemented in response to the stagnation in Congress of the DREAM Act initiative. This Act would give a 6-year path to citizenship to young illegal immigrants, conditional on either completion of college or two years of military service. The proponents of the Act estimate that this measure would affect some 65,000 youth, a number that does not suggest its likely effects on crime rates; nonetheless, an empirical test of DACA would shed some light on the relationship between education of young immigrants and crime rates.[60] Some states have their own version of the DREAM Act already in place, with provisions that especially deal with in-state tuition fees and financial aid, a matter of great controversy in the political debate around the DREAM Act.

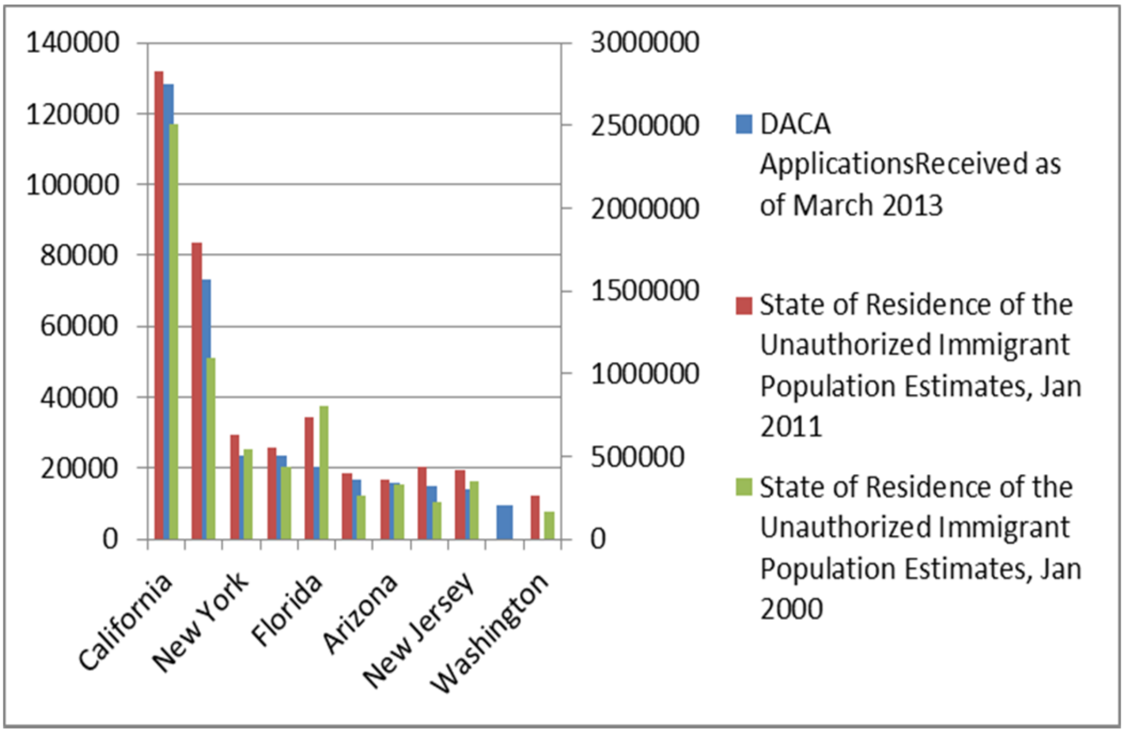
Figure 50: DACA Applications and Unauthorized Illegal Population by Top 10 Country of Origin



Sources: USCIS website and Hoefer et al. (2012). Estimates of the Unauthorized Illegal Population levels are on the right axis

[60] http://dreamact.info/

Figure 51: DACA Applications and Unauthorized Illegal Population by Top 10 State of Residence



Sources: USCIS website and Hoefer et al. (2012). Estimates of the Unauthorized Illegal Population levels are on the right axis.

As the history of the DREAM Act and DACA program shows, immigration reform is likely to be put on hold because of lack of consensus. The Obama administration recently proposed a comprehensive immigration reform that aims at balancing different vested interests by focusing on four main aspects of immigration.

First, it proposes to strengthen border security and infrastructure also through agreements with border countries, and to crack down on organizations involved in the smuggling of illegal immigrants. This should deter inflows of new immigrants attracted by the prospects of the path to citizenship granted under the reform. As seen in Chapter 2, both the probability of apprehension and the probability of an amnesty matter for the immigration decision. Under the reform currently discussed, the path to citizenship is analogous to an increase in the probability of an amnesty; by increasing enforcement, the administration hopes to keep illegal immigration down in the future.

In line with the former point, the reform proposes a crackdown on the hiring of illegal immigrants through an expansion of E-verify. This further increases the probability of apprehending illegal immigrants and decreases the expected returns from illegal immigration. What's more, by granting more protection against illegal immigrants taking natives' jobs, this measure may gain broader support for reform. Tougher enforcement, along the lines followed in recent years, includes: the immediate deportation of criminally convicted immigrants at the end of their sentences; more generally, prioritization of deportation for violent offenders and for those who have overstayed their visa and represent a threat to national security. On the other hand, the reform will establish a path to citizenship for those individuals approved under the DACA program.

Under the immigration reform, the more general path to citizenship would start with registration of the illegal immigrants currently residing in the United States. This process will involve criminal background and national security checks, and payment of fees and taxes, in some proportion to the taxes evaded during the illegal stay.[61] This first step would allow an illegal immigrant to apply for provisional legal status, but not grant eligibility for welfare or other federal benefits, consistent with current law. To obtain permanent legal status, the immigrant will need to satisfy additional requirements, such as learning English and U.S. civics.[62] One caveat about the registration process is the extent to which illegal immigrants are able to provide proof of residency, as well as to pay the taxes that are needed to initiate this process.

Finally, the administration proposes changes in the quota system. In particular, it aims at raising the caps for family-sponsored immigration, eliminating country-specific caps to work-related immigration, and granting green cards to STEM (science, technology, engineering and mathematics) Ph.Ds.

A parallel proposal is being drafted by a bipartisan group of eight senators who seem to be more focused on economic outcomes for immigrants and natives than the administration is.[63] In particular, they are drafting a pro-cyclical temporary visa quota system, i.e. one that allows more temporary immigrants when the economy expands and protects natives during bad times by restricting the inflows of cheap substitutable labor. This goes in the direction indicated by Hanson (2009), who argues that illegal immigrants are very attractive to employers precisely because of their pro-cyclical nature. Indeed, illegal immigration does respond to recessions in the receiving countries, making illegal immigrants a much more flexible buffer stock for employers than the regulated temporary legal immigrants. The remaining provisions of the draft bill are very similar in spirit to the administration's proposal, and concentrate on encouraging high skill immigration, ensuring a path to citizenship to the existing stock of illegal immigrants, and securing border protection.

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[61] It is worth noting that even if illegal immigrants are not subject to payroll taxes due to their forced participation in the shadow economy, they do pay other fees and taxes already, for instance consumption and value added taxes.

[62] See http://www.whitehouse.gov/issues/immigration/earned-citizenship. It is worth noting that most of these provisions replicate the 1986 Immigration Reform and Control Act (IRCA) exactly, as we will see in Section 2.

[63] See for example, an article on The Economist of April 6th 2013, "Getting There", available at http://www.economist.com/news/united-states/21575764-progress-last-making-things-easier-immigrants-america-getting-there?fsrc=nlw|hig|4-4-2013|5458027|37757587|

Summing up, the times seem ripe for an immigration reform that has bipartisan support and provides conditional legal status to the existing stock of illegal immigrants while tightening enforcement on future immigrants. We have discussed these provisions through the lens of Chapter 2, and the reform seems to be well suited to providing better outcomes to immigrants currently in the United States without encouraging further immigration. In Section 6.2.1, we discussed how spending on immigration enforcement has increased steadily in the past decade, and how apprehensions under the Enforcement and Removal Operations by ICE have increased, especially for aliens with criminal convictions. Hence, it seems that the U.S. government can credibly commit to a high probability of apprehension of future immigrants, despite giving signals of a higher likelihood of future amnesties. Moreover, amnesties are not frequent in the United States: the last one was granted under the Immigration Reform and Control Act of 1986, which we discuss in detail in Section 6.3. Hence, it is not certain that by committing to grant a path to citizenship to the current stock of illegal immigrants in the United States, the U.S. government and Congress are giving any signal of a higher probability of amnesty in the future or, at least, in the near future. Thus, the reform's most likely effect seems to be a deterrence of future immigration.

6.2.3. Immigration to the United States: Some Facts

6.2.3.1. Characteristics of Immigrants

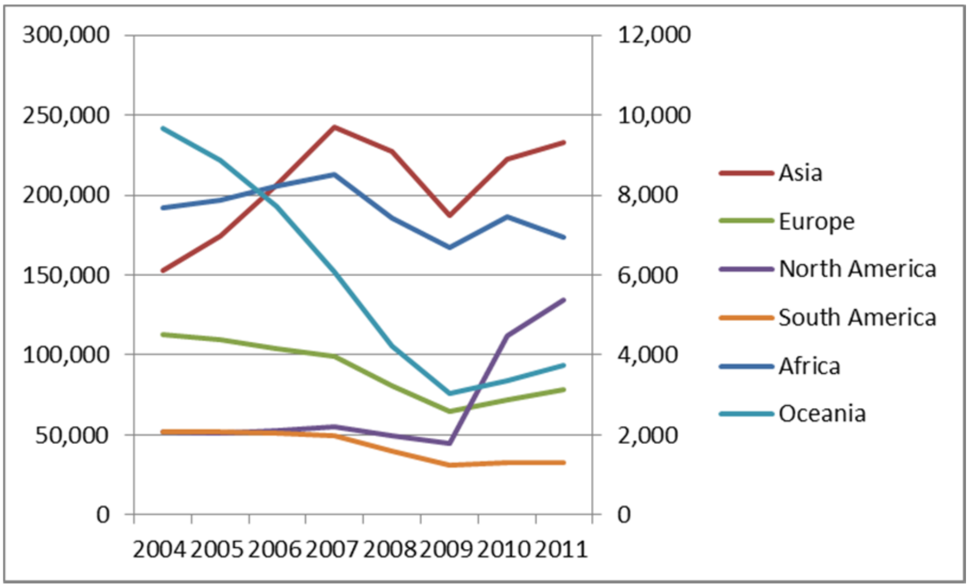
The stock of illegal immigrants present in the United States has been stable, between 10 and 12 million since 2005, as shown in Figure 38. This is due both to an increase in exits of illegal immigrants during the Great Recession, shown in Figure 40 from Warren and Warren (2013), and to a decrease in entries (between 2005 and 2010, only 1.5 million illegal immigrants entered the country as opposed to over 3 million between 2000 and 2004, as shown in Figure 39).

In this subsection we look more closely at the characteristics of immigrants to the United States, exploring differences by legal status. Unfortunately, we do not have data on illegal immigrants; hence, we compare immigrants who have been granted citizenship to other immigrants, focusing on their origins and their labor market outcomes (Section 1.2.2).

In contrast to illegal immigration, which mostly comes from Central and South America, the biggest fraction of temporary workers on H-1B Visa in 2011 came from Asia. Figure 52 shows that North America (Canada, Mexico and Central America) now sends the second biggest fraction, over 100,000 temporary workers, exceeding Europe during the economic crisis.

In terms of the industries they work in, immigrants do not look like the typical U.S.-born male. In particular, Figure 53 to Figure 55 show that there is selection into sectors by continent of origin more than by citizenship status, even though construction and manufacturing are more prevalent among non-citizens. For instance, U.S.-born individuals split evenly across most sectors, with a predominance of jobs in professional services, but only European immigrants have similar rates of jobs to natives in professional services (around 30%).[64] Africans and South and Central Americans, especially non-citizens, mostly work in other services, i.e. accommodation and food services, and construction and manufacturing. Asian immigrants specialize in health services, instead. No clear pattern over immigration period emerges, hinting that cohort effects have been fairly stable over the past 30 years, but we will return to that issue at the end of this section. A final caveat concerns the industry definition we use: broad industry definitions do not take into account the skill content of the job executed.

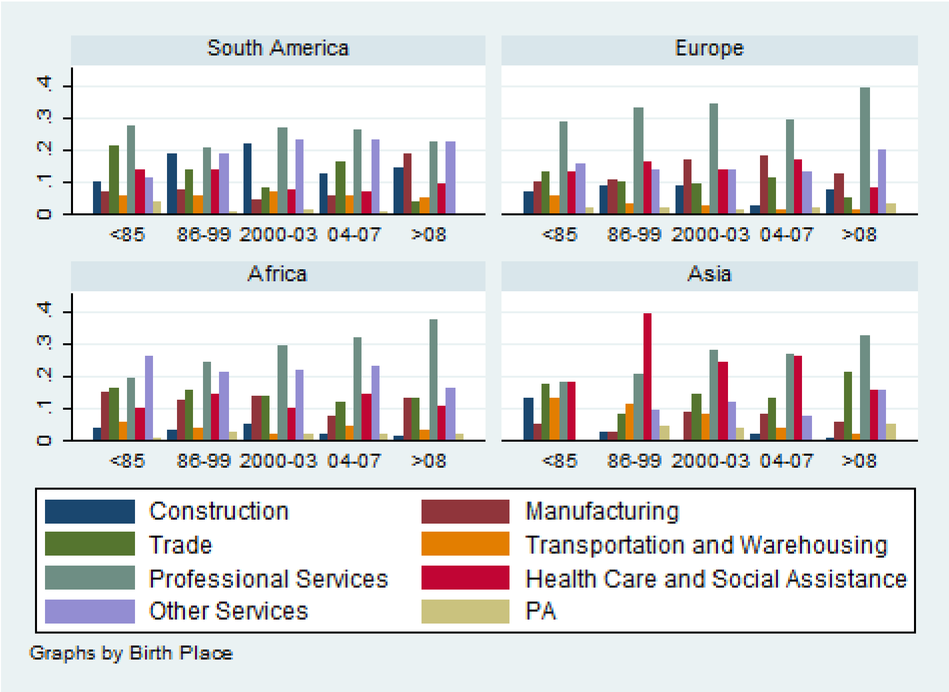
Figure 52: Non-Immigrant Temporary Visa (H1B) by Continent of Origin



Note: Africa and Oceania are to be read on the right-axis. North America includes Central America. Source: Yearbook of Immigration Statistic

[64] Professional Services include: Information, Finance, Insurance, Real Estate, and Rental and Leasing; Professional, Scientific, Management, Administrative, and Waste management services; Educational Services.

Figure 53: Industry Distribution of Non-Citizens by Continent of Origin and Immigration Period

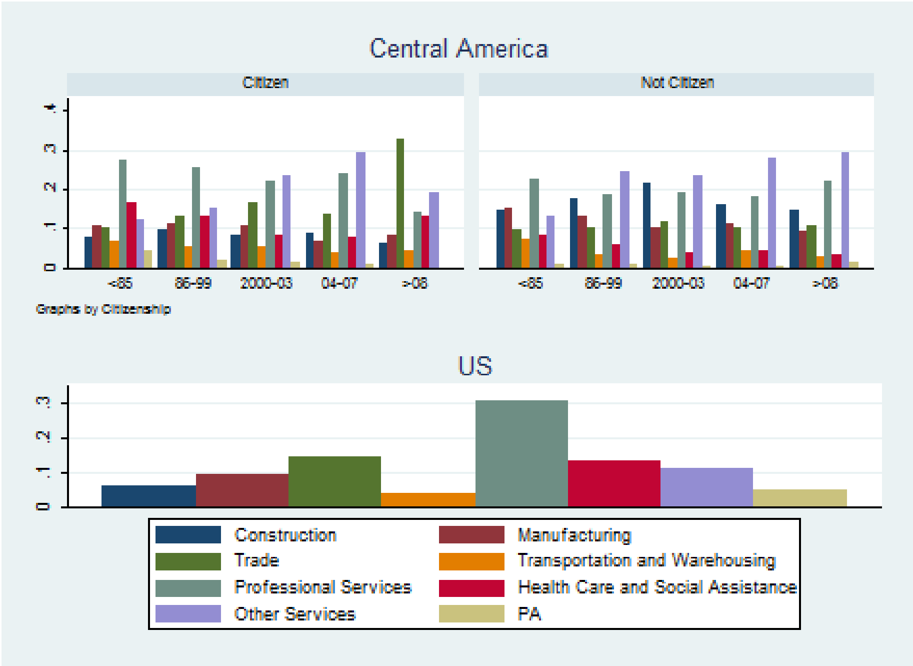


Source: Authors' calculations from 2012 CPS data

Figure 54: Industry Distribution of Citizens by Continent of Origin and Immigration Period

Source: Authors' calculations from 2012 CPS data

Figure 55: Industry Distribution of Individuals born in Central America and the US



Source: Authors' calculations from 2012 CPS data

6.2.3.2. Immigrants' Outcomes

In this section we analyze labor market outcomes and welfare of immigrants by their citizenship status.

As shown in Figure 58, the fraction of unemployed among U.S.-born respondents of the Current Population Survey (CPS) in 2012 was below 5% and the fraction out of the labor force was around 35%. **Figure 56** and Figure 57 portray a very similar picture for immigrants, with some nuances. Immigrants from Asia have higher unemployment rates than natives, whereas non-citizens from Africa and Europe participate less in the labor force than citizens, hinting at family-sponsored migration for women. In general, however, we do not see significant differences between citizens and non-citizens.

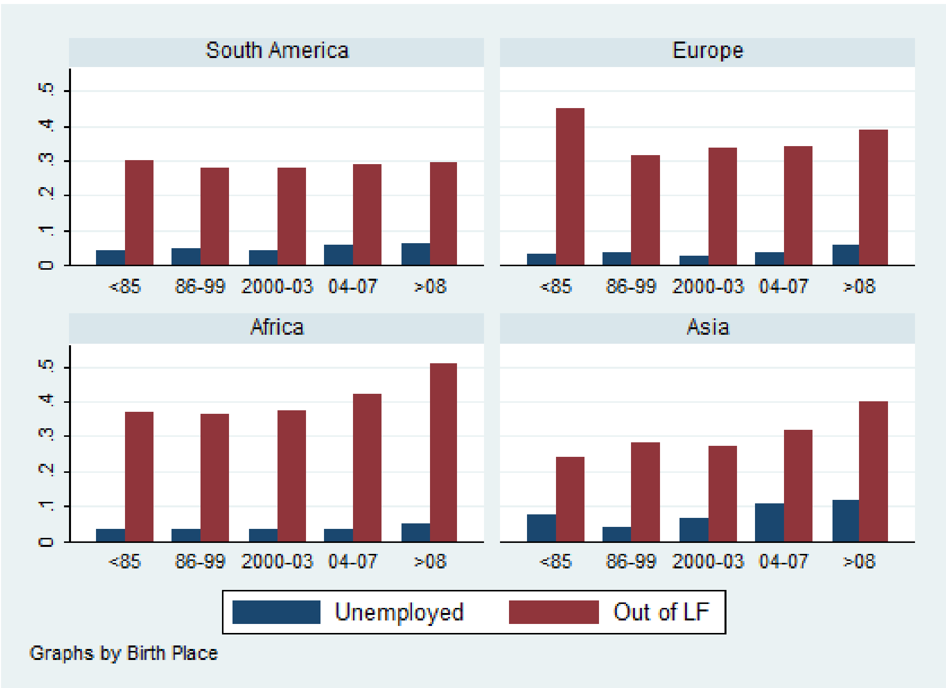
A very different picture emerges in Figure 59, which plots kernel density estimates of the distribution of log wages for citizens and non-citizens by birth place. For all continents of origin, the distribution for non-citizens is to the left of the one for citizens, implying that non-citizens earn lower wages than citizens. Furthermore, the distributions for foreign-born citizens look quite similar to the one for U.S.-born citizens, despite displaying a higher variance.

Figure 60 plots the distribution of weekly earnings by period of immigration for each continent, pooling citizens and non-citizens: younger cohorts from all continents earn lower wages than older cohorts. Assimilation and job tenure effects might be at play, or immigrant cohorts' quality may have deteriorated over time. Nonetheless, we do not find any cohort trends in the distribution of immigrants across sectors, or in labor force status. Hence, we believe that the assimilation story or the job tenure effect might be more plausible explanations. Figure 61 shows that the differential between citizens and non-citizens mostly disappears for European and Asian immigrants when we look at total income. Currently, we do not have a good explanation for this, and we believe that further work is needed to better understand how citizenship status affects labor market opportunities and outcomes in the United States.

Differential earnings do translate to different poverty rates. In 2012, 15% of the U.S.-born population living in the United States was below the poverty line, with another 10% being between 100% and 150% of the poverty line, as shown in Figure 64. Poverty rates are generally higher for immigrants, but not for European or African immigrants, which may suggest different selection rules from different places (Figure 62 and Figure 63). Poverty rates are also higher for non-citizens, who are excluded from certain types of benefits. Consistent with cohort effects in earnings, we find an upward trend in poverty rates for earlier cohorts.

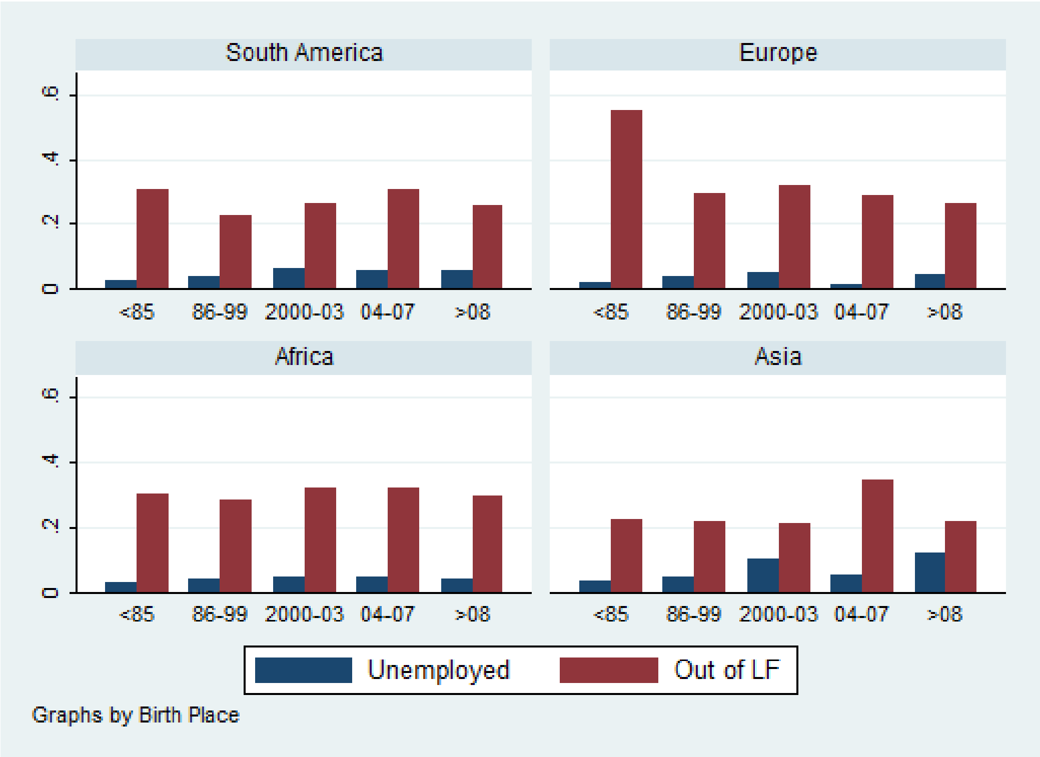
Summing up, we do see differentials in earnings and in overall welfare between those immigrants who have been granted citizenship status and those who have not. Whether this translates into a fiscal burden of immigration is left for further research. Moreover, we cannot disentangle discrimination from selection of immigrants into permanent or temporary immigration, based for instance on ability. In the remainder of the chapter, our analysis takes a first step in this direction, but further research is needed to analyze labor market outcomes for immigrants of different types, and how these relate to crime rates.

Figure 56: Labor Force Status of Non-Citizens by Continent of Origin and Immigration Period



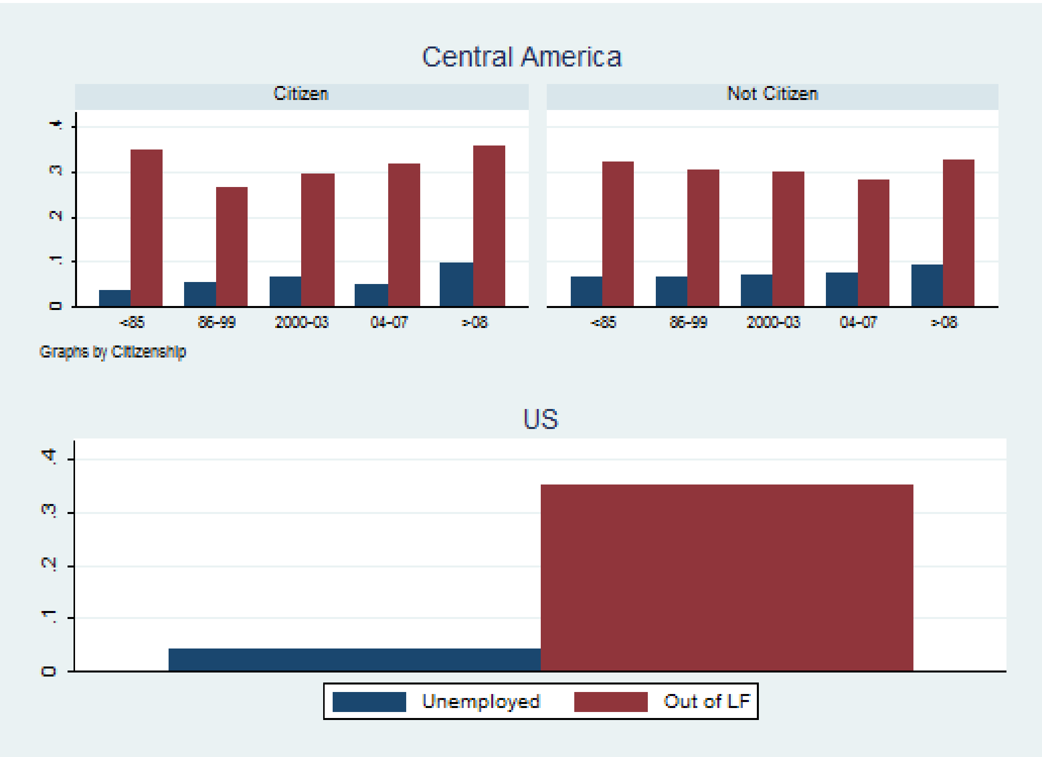
Source: Authors' calculations from 2012 CPS data

Figure 57: Labor Force Status of Citizens by Continent of Origin and Immigration Period



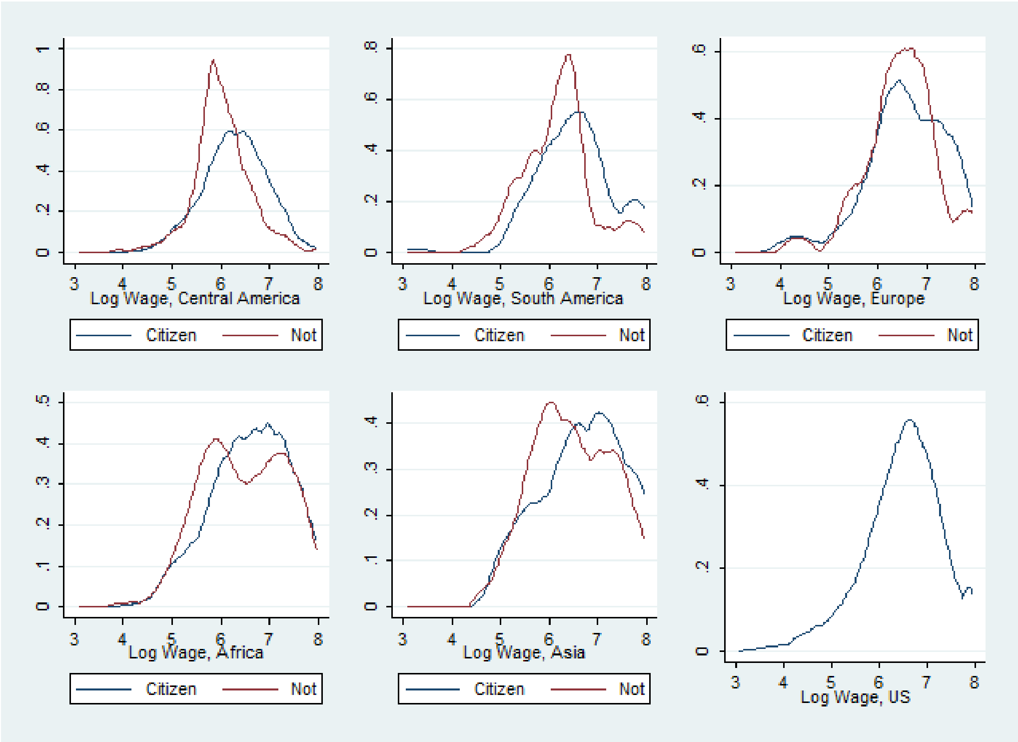
<u>Source:</u> Authors' calculations from 2012 CPS data

Figure 58: Labor Force Status of Individuals born in Central America and the US



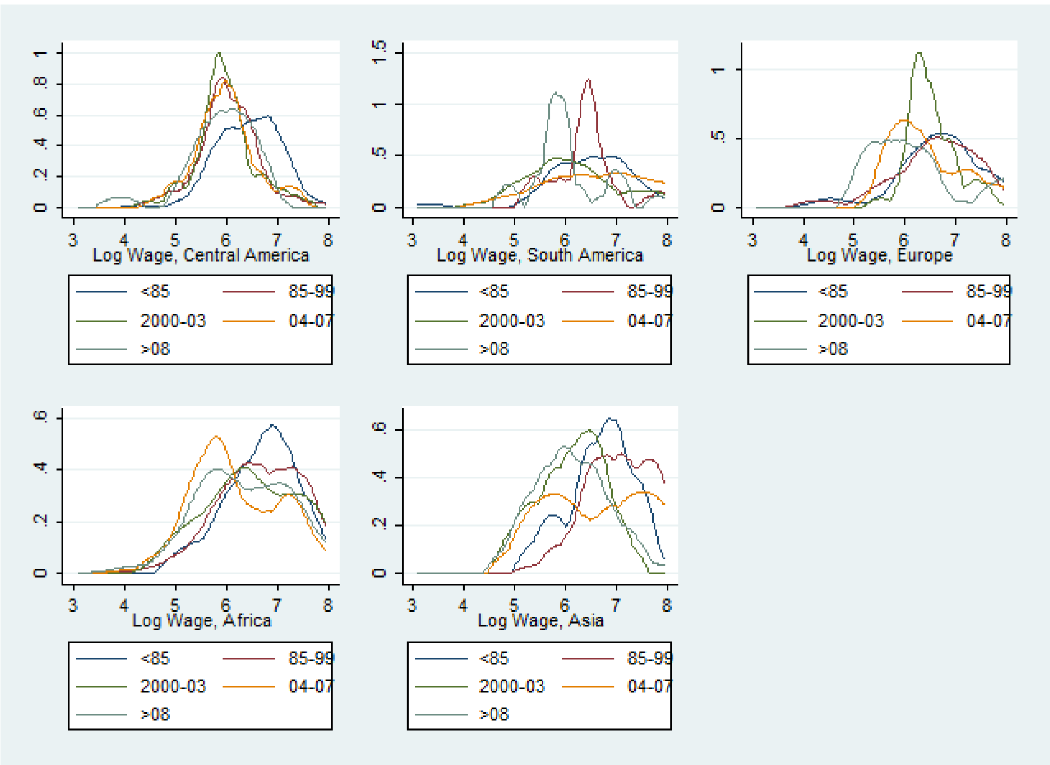
<u>Source:</u> Authors' calculations from 2012 CPS data

Figure 59: Log Weekly Earnings by Citizenship Status and Continent of Origin



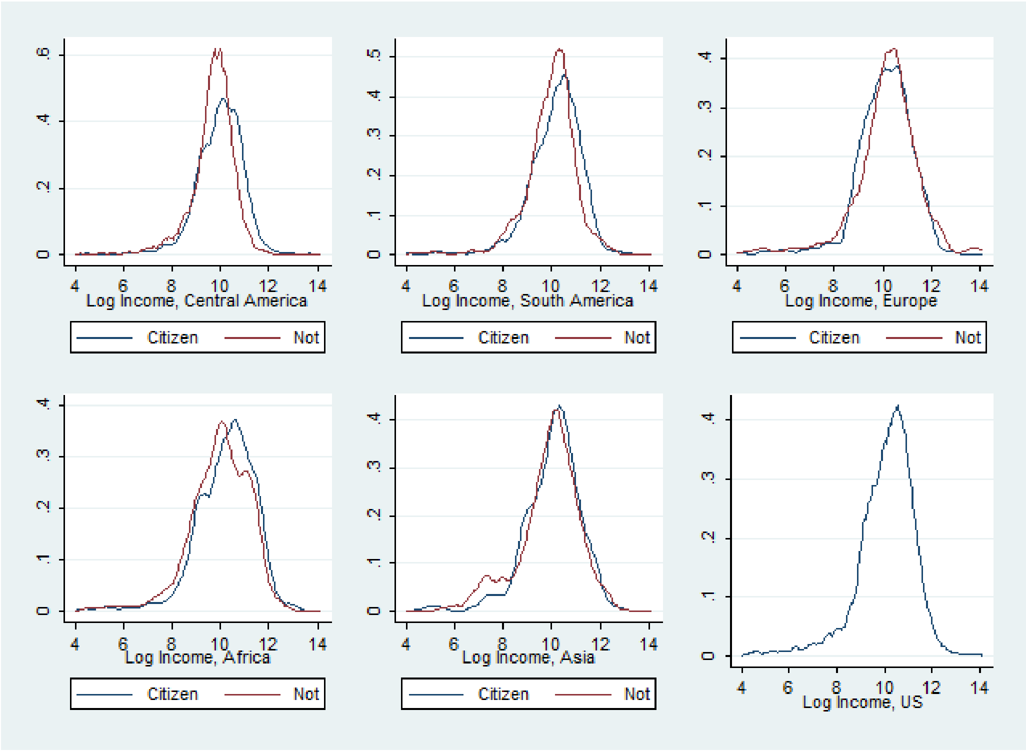
Source: Authors' calculations from 2012 CPS data

Figure 60: Log Weekly Earnings by Period of Immigration and Continent of Origin



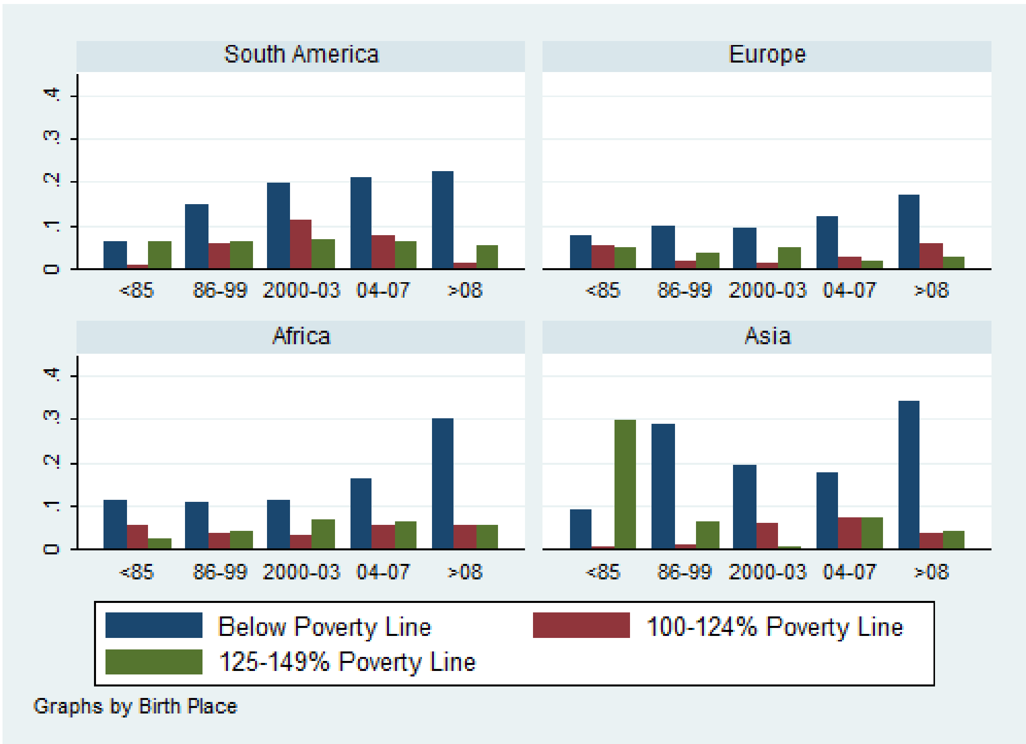
Source: Authors' calculations from 2012 CPS data

Figure 61: Log Income by Citizenship Status and Continent of Origin



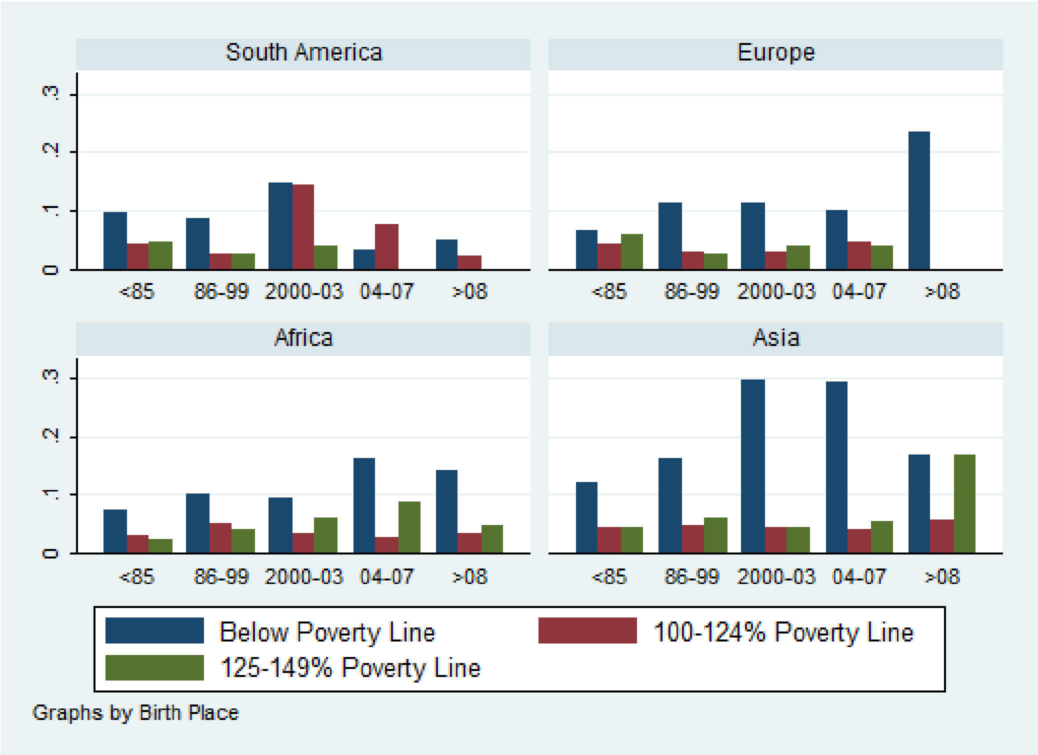
Source: Authors' calculations from 2012 CPS data.

Figure 62: Share of Non-Citizens in Poverty by Continent of Origin and Immigration Period



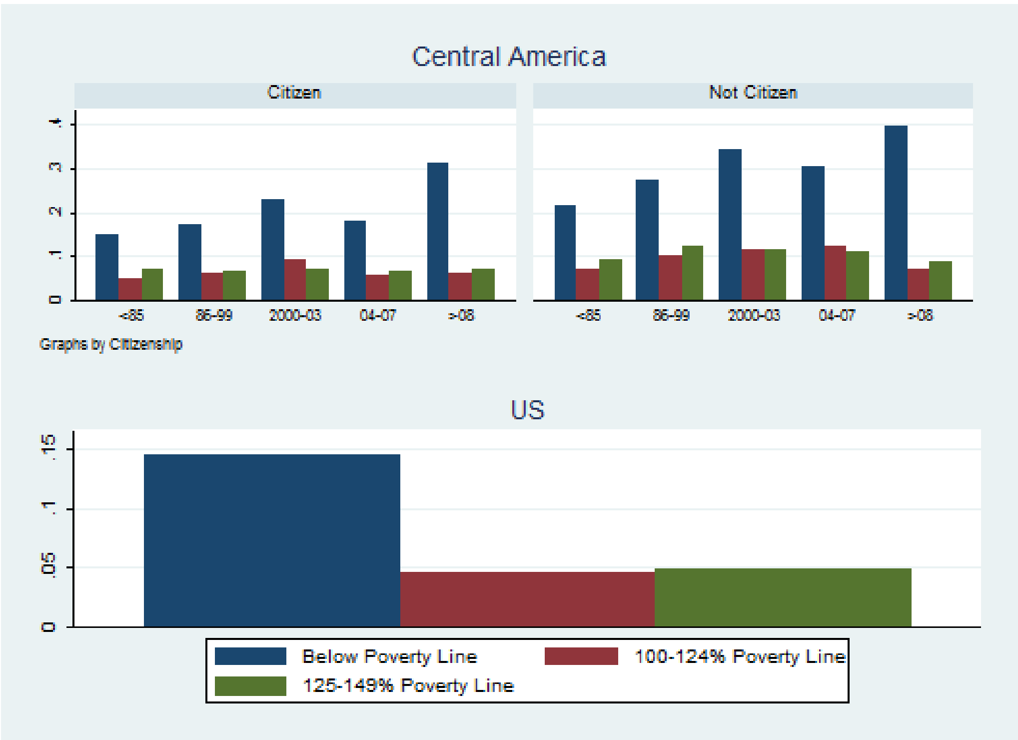
Source: Authors' calculations from 2012 CPS data.

Figure 63: Share of Citizens in Poverty by Continent of Origin and Immigration Period



<u>Source</u>: Authors' calculations from 2012 CPS data.

Figure 64: Share of Individuals born in Central America and the US who live in Poverty



<u>Source</u>: Authors' calculations from 2012 CPS data.

6.3. Literature Review

6.3.1. Immigration, Legal Status and Crime in the United States

Spenkuch (2011) analyzes the effect of immigration on crime rates. Using panel data on U.S. counties, and instrumenting current immigration with past immigration patterns, he finds a strong effect of immigration on crimes motivated by financial gain, such as motor vehicle theft and robbery. Moreover, the effect is present only for those immigrants most likely to have poor labor market outcomes, consistent with a standard economic model of crime *a la* Becker (1968) or Ehrlich (1973).

To study the effects of legal status on crime rates of immigrants in the United States one has to look back at the 1986 Immigration Reform and Control Act (IRCA), which legalized over 2.5 million illegal aliens of the 3.2 million who were estimated to be in the United States at that time. This is the most recent amnesty approved in the United States. IRCA provided paths to citizenship for two groups of immigrants, conditional on not having committed felonies or misdemeanors. The first were immigrants who had resided in the United States for a relatively uninterrupted period since 1982 and applied between May 1987 and May 1988. The second were Special Agricultural Workers (SAW). As in the case for the current reform proposal, immigrants would receive permanent resident status after 18 months, conditional on completing an English test and a U.S. civics test. Prior to receiving the permanent resident status, immigrants were not entitled to most government benefit programs and could not sponsor family members for entry to the United States.

Baker (2012) examines the effects of IRCA on crime in the United States, using distance from the main ports of entry and 1960 immigrants' enclaves as instruments. He finds that IRCA applicants are associated with higher crime rates prior to legalization, but not after legalization. He also calibrates a labor market model of crime, using empirical wage and employment data, and finds that much of the drop in crime can be attributed to greater job market opportunities among those legalized by IRCA. Baker discusses changes in the family structure of IRCA applicants following legalization as one channel that potentially affects crime rates. Men may become eligible to sponsor family members as legal residents, and research generally has shown declines in criminal activity upon marriage. Changes in the relationship with police also could affect the amount of crime committed by, and against, IRCA applicants. Greater trust of police after legalization may cause IRCA applicants to report more of the crimes committed against them. However, this mechanism would go in the opposite direction of what Baker (2012) finds, i.e. it would result in higher reported crime rates after IRCA. On the other hand, greater cooperation with police by IRCA applicants following legalization may increase police effectiveness, and thereby decrease crime. In general, immigrants may feel more at home upon legalization, and hence less likely to engage in anti-social activity.

Cobb-Clark and Kossoudji (2002) also find that legalized immigrants enjoy better labor market opportunities, even after controlling for individual fixed effects.

IRCA enhanced the controls on the hiring of illegal aliens as well, in order to decrease illegal immigration into the United States: the bill made it illegal to knowingly hire or recruit illegal aliens and required employers to at least make a cursory investigation into the immigration status of their employees. Some states already had similar provisions in place, but nothing similar existed at the federal level. Freedman et al. (2013) analyze the implications of these provisions of IRCA on immigrants who arrived too recently to be eligible for legalization. These authors apply a differences-in-differences (DiD) methodology on administrative data on the criminal justice involvement of individuals in San Antonio, Texas. They show evidence of an increase in felony charges filed against Hispanic residents of San Antonio after the expiration of the IRCA amnesty deadline. Furthermore, they estimate in which neighborhoods recent immigrants were most likely to live, and show that crime rates increase precisely in those neighborhoods, suggesting a strong relationship between access to legal jobs and criminal behavior.

In total, there are very few papers that focus on immigration and crime rates in the United States, and even fewer that look at legal status. As discussed above, the identification is limited to geographic variation by the infrequent amnesties and the lack of exogenous variation in legal status or enforcement. As argued in Chapter 1, the U.S. public seems to be more concerned with changes in the labor market caused by immigration than with increases in crime rates. Nonetheless, we believe that advances in econometric techniques will spur further research on these issues, and will shed light on the differentials created, for instance, by path to citizenship.

6.3.2. Enforcement and Incarcerations

The model in Chapter 2 predicts that illegal immigration will respond to immigration enforcement, as well as to criminal opportunities in the receiving country. Intrinsically, data on illegal immigration are hard to retrieve; in what follows, we discuss the few papers that have tried to overcome this issue.

Hanson and Spilimbergo (1999) study the relationship between illegal immigration and border enforcement. They use apprehensions as a proxy for illegal immigration, which is not ideal if we think that increases in linewatch hours and the number of border patrol agents raise the percentage of illegals apprehended and decrease the number of illegals who attempt to cross the border. They instrument for enforcement using defense spending and elections, but this does not solve the issue raised here. Similar to what we illustrated in section 6.2.1, they find that apprehensions increase with enforcement and respond to changes in wages, both in Mexico and in the United States.

As shown in Chapter 1, immigrants are underrepresented in U.S. institutionalization facilities. Several papers by Butcher, Piehl and coauthors investigate this peculiarity. In particular, Butcher and Piehl (2007) argue that the low incarceration rates among immigrants are not due to deportation of criminal aliens. Using a logit model on Census data from 1980 to 2000, they show that the process of migration, including background checks performed by immigration officials, selects individuals who either have lower criminal propensities or are more responsive to deterrent effects than the average native. In particular, the newly arrived immigrants in the 1980s and 1990s are particularly unlikely to be involved in criminal activity. Furthermore, immigrants who are already in the country reduce their relative institutionalization probability over time. Relatedly, Butcher and Piehl (2008) find a negative correlation between the share of immigrants in California cities and crime rates in those cities. Finally, Bodenhorn et al. (2010) show that during the 19th century, immigrants were underrepresented in Pennsylvania's prisons in urban areas and overrepresented in Pennsylvania's prisons in rural areas. Although they do not provide an explanation for this, they suggest that their use of prisons' data might cause them to undercount petty crimes, especially in urban areas. Hence, they do not reject the economic model of crime. These findings are in some contrast with the results in Moehling and Piehl (2009) who write that the foreign-born have higher incarceration rates for non-violent crimes, possibly due to their higher concentration in urban settings. Moreover, they compute counterfactual incarceration rates based on the age distribution of natives and find that the different age distribution of immigrants leads to underestimate their propensity to commit crime, in sharp contrast with what is usually believed. Finally, the gap in incarceration rates is reduced over time, and closed by 1930, due to changes in the immigration legislation that influenced the selection of immigrants.

It is worth making a final caveat on natives' incarceration rates. Borjas et al. (2010) model a two-tier labor market, where African-Americans are the most vulnerable to competition from immigrants, and hence respond to immigration by increasing their crime rates. Indeed, these authors find a negative correlation between immigration and African-Americans' employment, and a positive relation between immigration and African-Americans' incarceration rates. This could explain at least part of the underrepresentation of immigrants in U.S. prisons.

To summarize, the empirical literature on crime rates, enforcement, and incarceration of immigrants in the United States is far from complete. A lack of data has hindered a causal analysis of the links between deterrence and correction and crime rates. However, we believe that this could be an avenue for further research now that more administrative data are becoming available.

6.4. The Mariel Boatlift: a Case Study

The literature discussed in the previous section provides no conclusive evidence of an adverse effect of immigration to the United States on crime rates. Section 6.2 discusses how this is attributable to both a well-functioning enforcement system ensuring deterrence and a relatively good labor market for illegal immigrants providing employment opportunities when the economy expands.

In this section, we present some novel evidence on the effect of immigration on crime rates in the United States in the special case of a system "breakdown." Indeed, we analyze a case-study that represents an exception to the norm of U.S. immigration policy, and we discuss the setting in more detail below. We find that immigration significantly increased homicide rates, robberies, and motor vehicle thefts, but it had no effects on other types of crime. This is consistent with a model of economically-driven crime, but we cannot disentangle whether the increase in violent crimes is due to negative selection or to the fact that, if caught committing crimes, immigrants without permanent legal status might face removal. Nonetheless, we discuss how, under more typical circumstances, immigration is not likely to increase crime rates.

6.4.1. The Setting

In April 1980, after years of Cuba's closed borders, Fidel Castro unexpectedly allowed some 125,000 Cubans to leave the country from the port of Mariel, catching the U.S. government by surprise. Hence, no attempt to stop the flotilla was made, even though the so-called Marielitos landed in Miami in different waves. All of the immigrants who arrived prior to October 20, 1980 were granted an *ad-hoc* immigration status as "Cuban-Haitian-Special Entrants".[65] This made it legal for them to be on U.S. soil, but prevented them from reaching permanent resident alien status without special legislation. The path to citizenship (or to permanent alien resident status, we will use citizenship for brevity in the rest of the discussion) enters into someone's utility through future income streams. If we think that asylum seekers likely prefer to stay in the United States, then this will be relevant in our case. It was only in 1984 when the Cuban special entrants were provided a route to permanent resident status through an administrative interpretation of a 1966 statute. The Haitian special entrants were omitted from this decision, and were legalized in 1986, under IRCA.

Some features of this immigration inflow make it extraordinary, and thus might threaten the policy relevance of this exercise. First, the Boatlift consisted of a large number of immigrants arriving in a

[65] This includes the Haitians that fled the Duvalier regime at the same time. See Baker (1990) for an interesting discussion of the political context that led to the creation of this new immigration status. In particular, the immigrants had petitioned for political asylum under the auspices of the brand new 1980 Refugee Act, but were nonetheless treated differently.

relatively short period of time to a small area. This is worth keeping in mind when extrapolating the results from this exercise to policy implications. Nonetheless, we think that the results are of interest when dealing with emergency situations that involve the concentration of large numbers of evacuees in an area, such as those that arise in the aftermath of natural disasters. It is important to note, however, that in this setting we cannot disentangle the scale effect from the "denied-path-to-citizenship" effect. Finally, in this case some immigrants were artificially negatively selected by the Castro regime, which was accused of having deliberately sent convicted criminals and individuals with mental issues. Hence we might overstate the effects on crime rates, unless the individual self-selection into emigration is already negative to begin with.

The sudden decision by Fidel Castro, who had stopped all previous emigration attempts out of Cuba, provides a neat argument for the exogeneity of the immigrants' inflow into Miami. Card (1990) is the first to exploit this natural experiment to study labor market effects of immigration in a differences-in-differences context. In particular, he compares labor market outcomes in the Metropolitan Statistical Area (MSA) of Miami to outcomes in Atlanta, Houston, Los Angeles and Tampa-St. Petersburg. Surprisingly, he finds no effect of immigration on wages or employment of African-Americans, who should be the most affected by the immigrants' inflow. Subsequently, Angrist and Kruger (1999) perform a falsification test on a second Mariel Boatlift in 1994 to show that the earlier analysis is flawed. Indeed, they find significant effects on labor market outcomes, despite the fact that in 1994 no Cubans set foot in Miami, because they were sent back by the U.S. government. To avoid issues of MSA-specific trends, we present both a classic DiD analysis and a synthetic control analysis.

The synthetic control approach was used first in Abadie and Gardeazabal (2003) to study the effects of terrorism on economic growth, and it generalizes the DiD methodology. To assess the effects of terrorism on the Basque region, the authors construct a synthetic region by weighting the control units so that the synthetic control satisfies the common trends assumption. Abadie et al. (2010) improve this methodology and apply it to the California Tobacco Control Program. Moreover, they develop a series of tests based on randomization inference that will prove useful in our analysis.

In this study, we are interested in the treatment effect of the Boatlift on crime rates in Miami. In the context of Rubin's (1974) potential outcome model, this is defined as the difference between the crime rates we observe in the presence of the treatment, Y_i^1 , and the crime rates that would have arisen in the absence of the Boatlift, Y_i^0: $\beta = Y_i^1 - Y_i^0$.

The synthetic control approach estimates β with:

$$\hat{\beta} = Y_i^1 - \sum_{j \neq i} w_j \, Y_j^0.$$

This can be done naturally by minimizing the difference between treated and control regions over the pre-treatment period. As long as the weights reflect structural parameters that would not vary in the absence of the treatment — at least over the medium period – the synthetic control approximates the unobserved counterfactual evolution of the potential outcomeY_i^0. Abadie et al. (2010) show that the synthetic control method generalizes DiD by allowing the effect of unobserved confounders to vary over time, according to a flexible factor representation of the potential outcomes of the *i*-th region, instead of assuming that unobserved differences between treated and non-treated units are time-invariant

The synthetic control method involves a two-step procedure that selects the optimal weights for the control units as well as the optimal weights for the predictors. Specifically, let X_i and X_j be the $K \times 1$ vectors of predictors for the treated region and for each *j*-th region in the control group, respectively; also, let V be a $K \times K$ diagonal matrix with non-negative entries measuring the relative importance of each predictor. Conditional on V, the optimal vector of weights, $W^*(V)$, must solve

$$\min \left(X_i - \sum_{j \neq i} w_j X_j \right)' V \left(X_i - \sum_{j \neq i} w_j X_j \right)$$

$$sub\ w_j \geq 0, \sum_{j \neq i} w_j = 1.$$

Then, the optimal V is chosen to minimize the mean squared error of pre-treatment outcomes,

$$\frac{1}{t} \sum_{s<t} (Y_{is} - \sum_{j \neq i} w_j^* Y_{js})^2$$

where *t* is the treatment period.

For our analysis, we use data from the Uniform Crime Report (UCR) Supplementary Homicide Report (SHR) and the UCR Offenses Known (OK) datasets. The SHR dataset has the advantage of reporting information on race and ethnicity of the offenders, as well as the circumstances of the murder and the weapon used. The OK dataset includes crime rates for larcenies, motor vehicle thefts, burglaries, robberies and rapes.[66]

[66]The OK dataset reports homicide rates as well, enabling us to compare the two homicides' measures as a robustness check. The two measures appear to be homogeneous across datasets. Hence, we feel comfortable comparing changes in offenses' rates across datasets.

Unfortunately, we cannot perform an Angrist and Kruger (1999) falsification test in our sample, because Florida reported only intermittently after year 1987.[67]

We also construct the predictors for the synthetic control method, such as minorities' and dropouts' shares in the population, from the Public Use Microdata Sample available at the iPUMS-CPS website. Other controls are real GDP at 1980 prices, downloaded from the St. Louis FED website, and population density.[68] All controls are annual.

Our crime data are monthly; nonetheless, we take quarterly averages in order to smooth out some of the noise. We prefer not to aggregate the data further, say to the annual level, to maintain an adequate number of pre-treatment periods for the synthetic control analysis. Our sample includes the years 1975-1986.

6.4.2. Results

In our analysis of crime rates in Miami around the Mariel Boatlift, we first present the results by type of offense, and then look at race and ethnicity of offenders.

6.4.2.1. Results by Offense

Figure 65 and Figure 66 show trends in crime rates in Miami and other MSAs.[69] Our control sample is comprised of 33 relatively large MSAs for which we have crime data from 1975 to 1986.[70] Homicides, robberies and motor vehicle thefts increase in Miami around the Mariel Boatlift, whereas rapes, larcenies and burglaries do not seem to be affected. Within homicides, we see an increase in those homicides perpetuated with a firearm, possibly related to robberies. Homicides committed by strangers already show an increasing trend prior to 1980, as do drug-related homicides. In what follows, we will establish the

[67]However, in an analysis not reported here, we perform a replication of both the original Card (1990) exercise and of the falsification test. Although we still find no effect of the Boatlift on employment, we obtain a p-value of 0.2 on the falsification test, which is comforting for our results.

[68] The variable is constructed using MSA land area in 1970, from the US Census and population estimates from the Real Estate Center at Texas A&M University (decennial population values are from the US Census), accessed at http://recenter.tamu.edu/data/pop/.

A recent debate concerning the relevance of MSA average population density has moved the US Census to measure weighted density, where the weights reflect the population of a Census tract in the MSA. However, for the purpose of this study we think that average density is a good proxy for factors related to employment and crime.

[69] In the whole analysis criminal rates are per 100,000 inhabitants.

[70] We have so few MSAs in our sample for two reasons. First, we were unable to compute some of the control variables for the smallest sample, since we computed population shares from the CPS. Secondly, we are using 1980 MSAs which had a broader definition.

causal relation between the arrival of the Marielitos and the increase in certain types of crime. In Section 3.3 we will discuss plausible interpretations of these results.

Table 28 and Table 29 report the basic DiD results for various crimes. In particular, Panel A estimates regressions of the form:

$$Crime\ Rate_{it} = \alpha + \beta * Miami_i + \gamma * Post\ 1980q2_t + \delta * Miami_i * Post\ 1980q2_t \quad (1)$$

where the variable $Post\ 1980q2$ equals 1 from the second quarter of 1980. Most of the δ coefficients are positive and significant, which is surprising given what we saw in Figure 65 and Figure 66. Indeed, we do not believe that clustered standard errors are correct, since we have only one treated unit. Hence, we compute Monte Carlo rejection rates by assigning the treatment at random: rejection rates are in the order of 70% in the OK dataset and 50% in the SHR dataset, much higher than the nominal 5%, indicating that inference based on clustered standard errors is likely to be flawed.[71] It is worth noting that Monte Carlo simulations are analogous to the randomization inference we implement to validate our synthetic control results.

Although we believe the DiD results should be taken with a grain of salt, we report selected estimates in Table 28 and Table 29 Panel B from regressions of the form:[72]

$$Crime\ Rate_{it} = \alpha + \beta * Miami_i + \sum_{t=1975q1}^{1986q4} \gamma_t Quarter_t + \sum_{t=1975q1}^{1987q4} \delta_t Quarter_t * Miami_i \quad (2)$$

Figure 67 and Figure 68 display the $\hat{\delta}_t$ series estimated from regression (2) for the OK and the SHR dataset, together with their 95% confidence band. Most of the coefficients fluctuate around zero prior to the Boatlift and become more positive around the event. Next, we validate these results by turning to synthetic control.

Figure 69 and Figure 70 show the crime rates in Miami and in the synthetic control unit for the OK and SHR dataset. Our preferred specification includes the following predictors: lagged dependent variable, density, real GDP, shares of black and Hispanic people in the MSA, unemployment rate, and shares of dropouts and high-school graduates in the MSA. Figure 71 and Figure 72 display very similar results generated from an alternative specification that drops the shares of dropouts and high-school graduates. In fact, the optimization procedure assigns weights in the order of 0.9 to the lagged dependent variable, and hence the results are not sensitive to dropping most of the controls, as shown in Table 30. The shares of black and

[71] We follow Bertrand et al. (2004). We replicate the exercise 400 times.
[72] In both Specifications (1) and (2) we have performed robustness checks adding controls, and the results do not change.

Hispanic people in the MSA and unemployment rates are more important predictors, but their weights are still very small.[73]

Figure 65: Trends in offenses rates at the quarter level, Miami vs Other MSAs

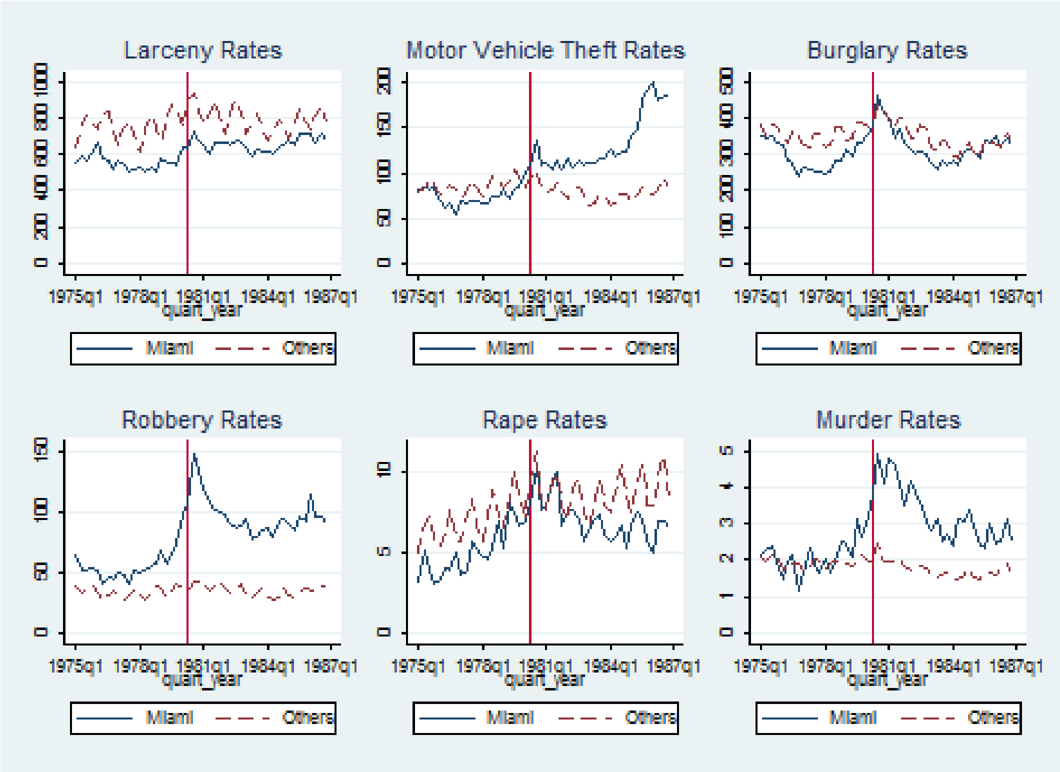
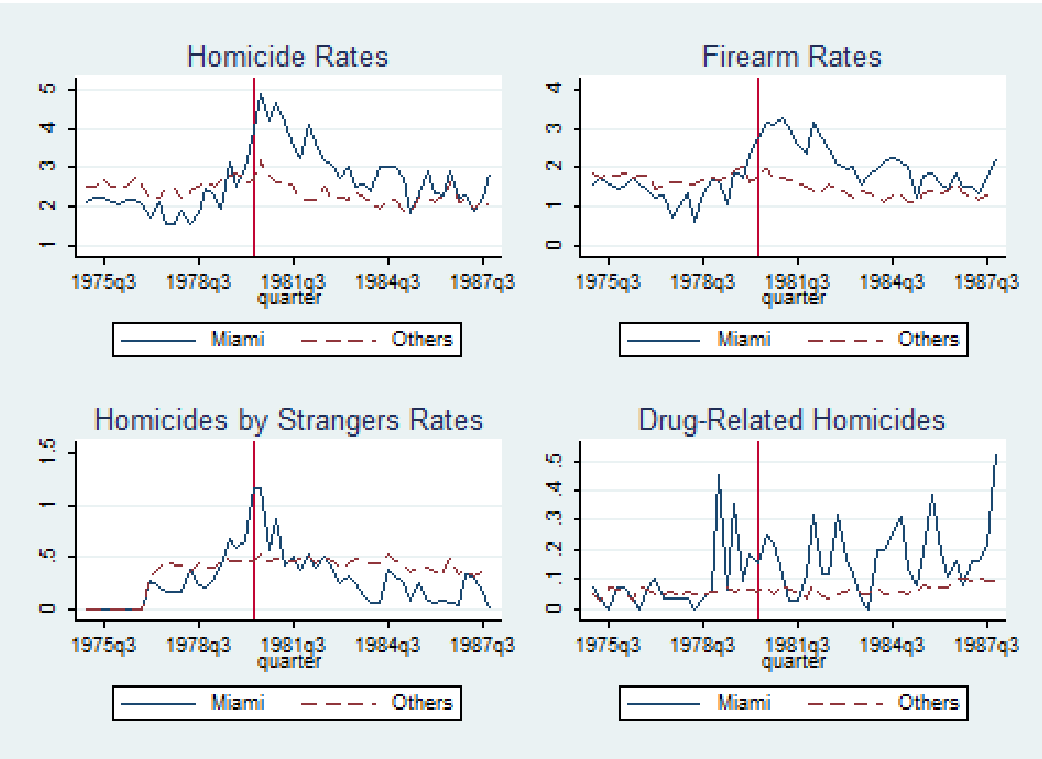


Figure 66: Trends in homicide rates by type, Miami vs Other MSAs, SHR data



[73] Table 4 shows the weights assigned to the control MSAs.

Figure 67: Cofficients on Quarter-Miami interactions, Offenses Known Dataset.

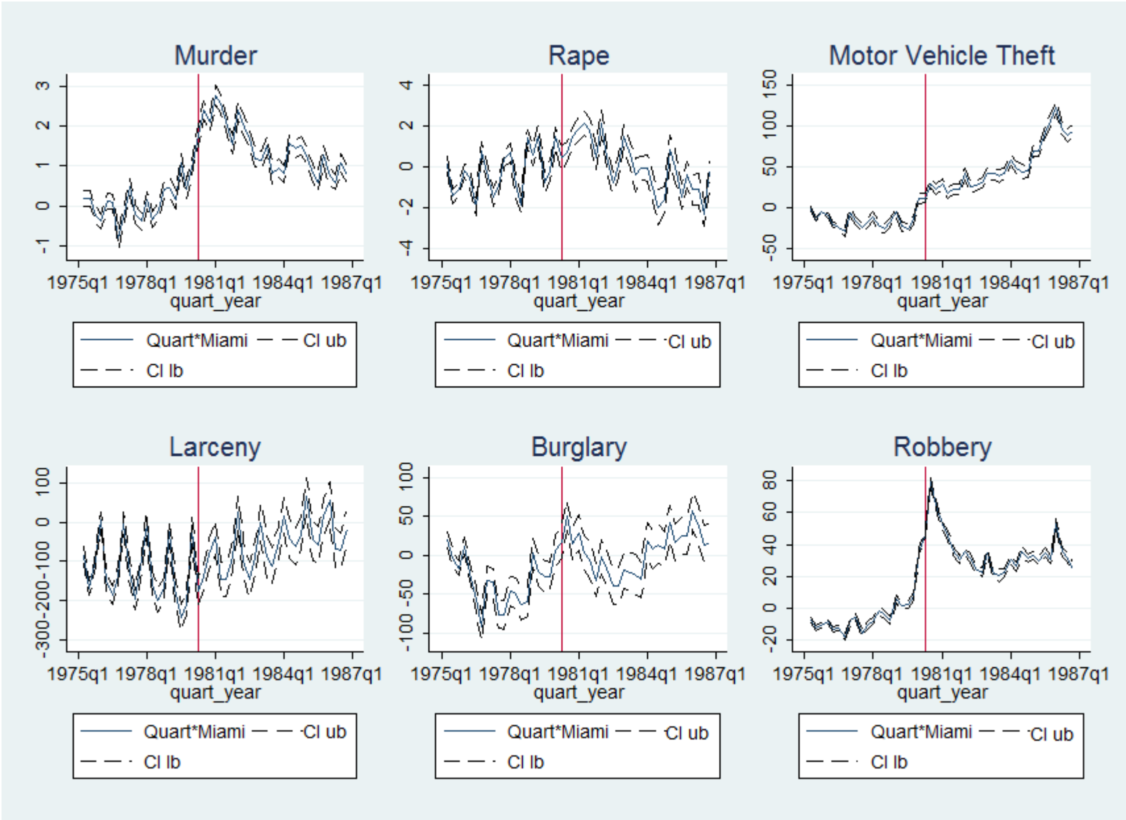


Figure 68: Cofficients on Quarter-Miami interactions, SHR dataset.

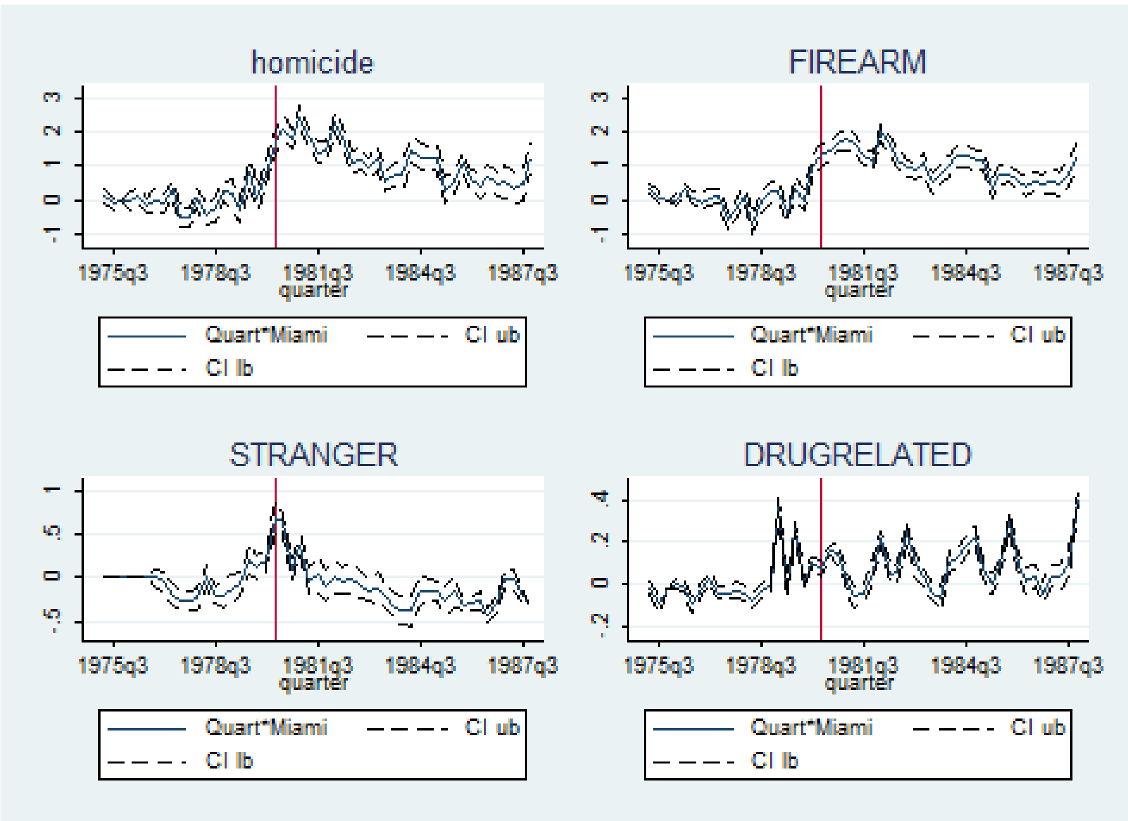


Figure 69: Synthetic Control on Offenses Known Dataset, Preferred Specification

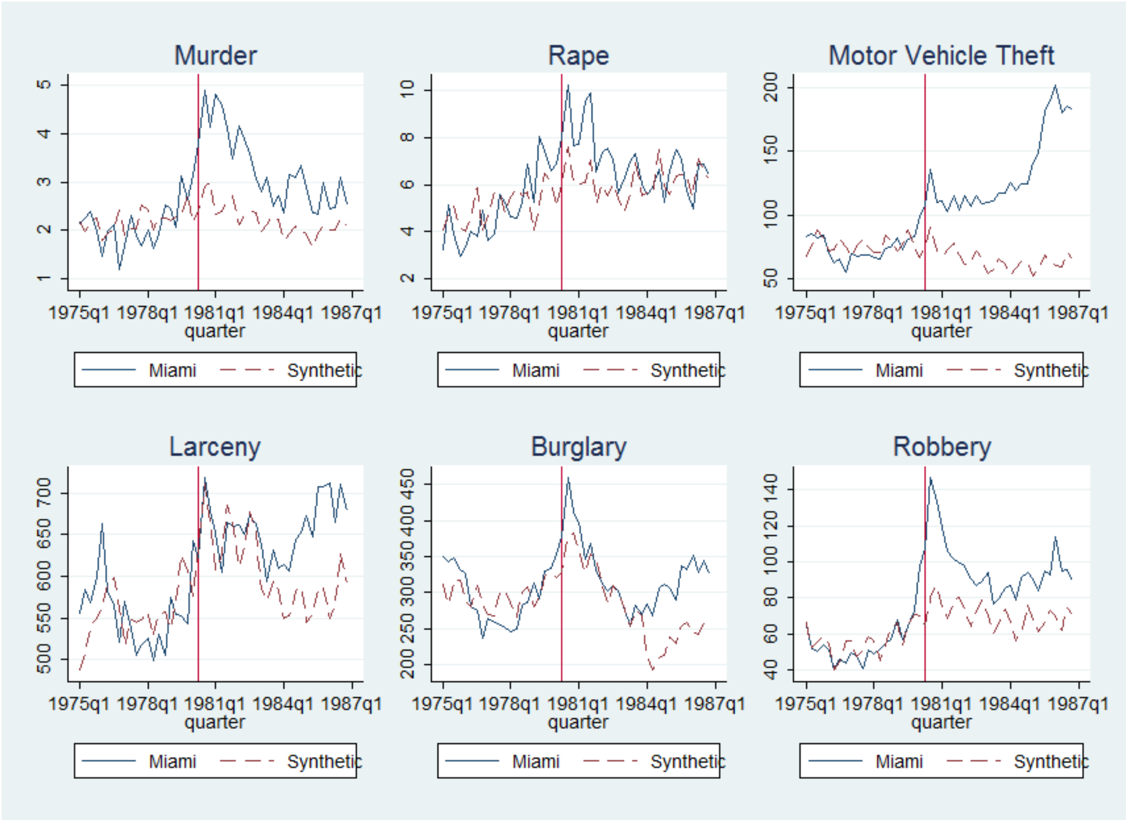


Figure 70: Synthetic Control on Supplementary Homicide Report Dataset, Preferred Specification

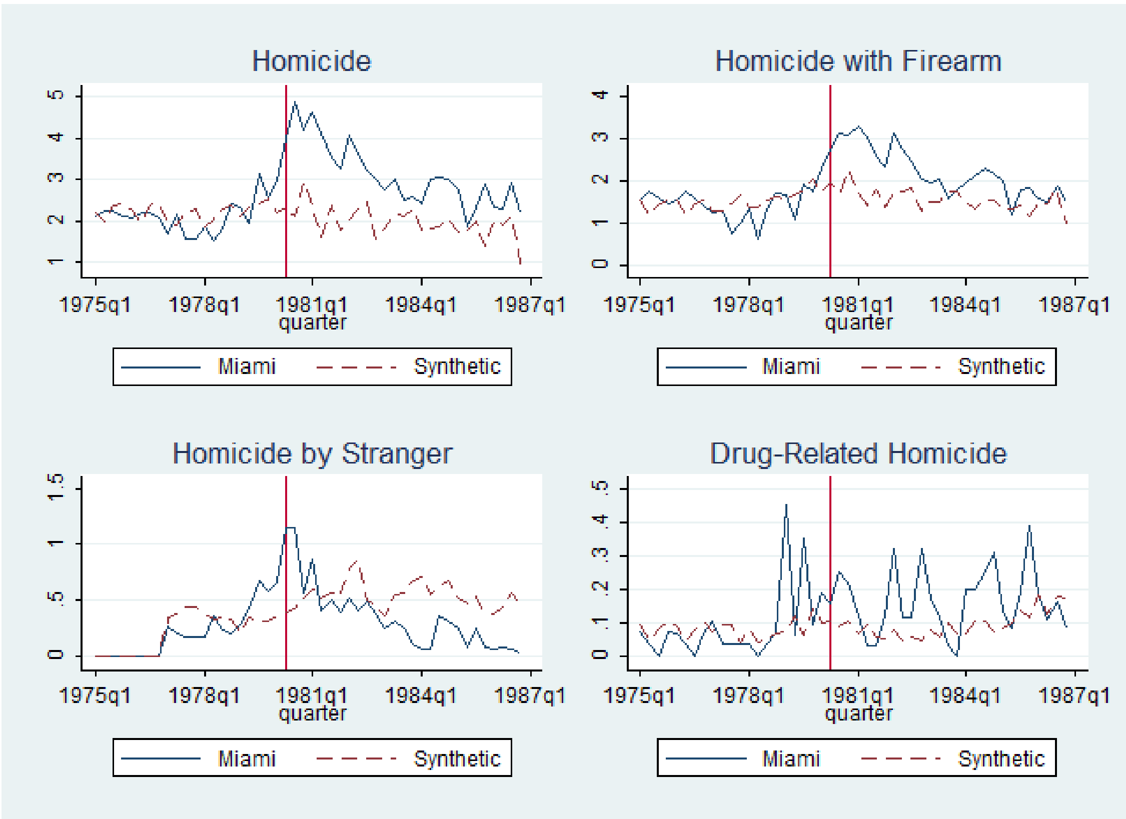


Figure 71: Synthetic Control on Offenses Known Dataset, Alternative Specification

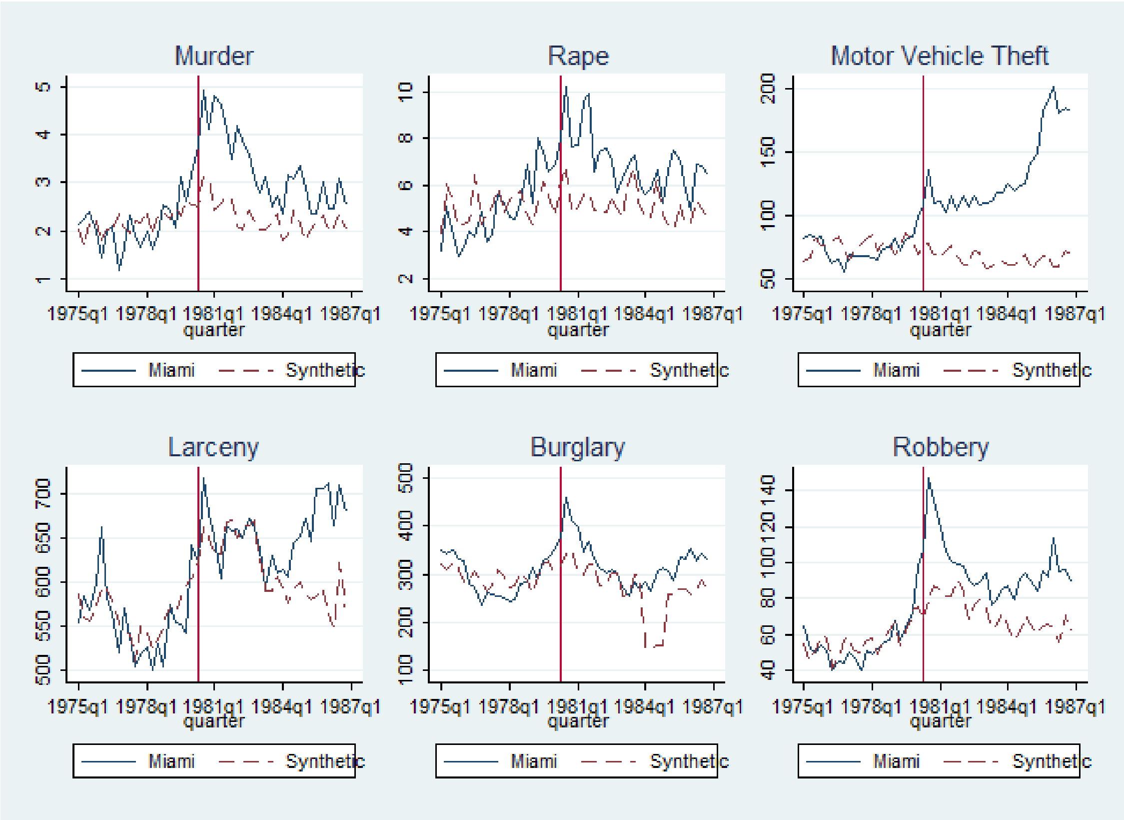
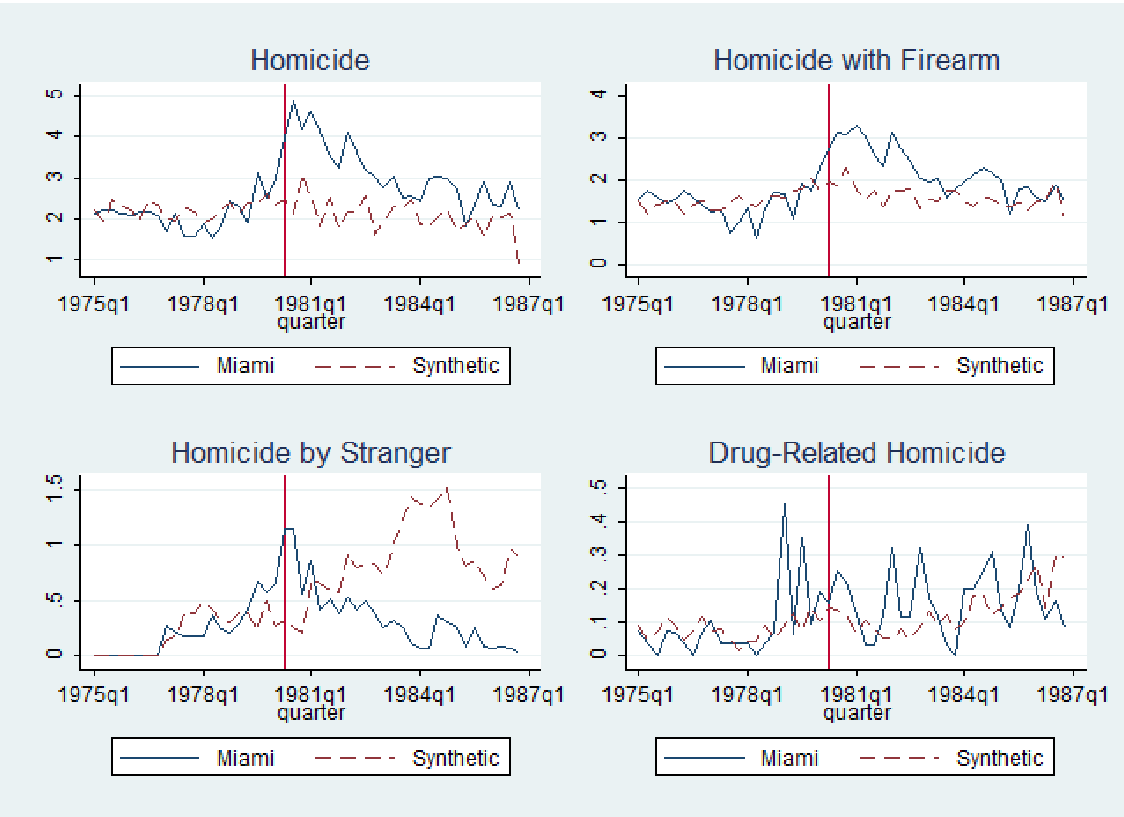


Figure 72: Synthetic Control on Supplementary Homicide Report Dataset, Alternative Specification



The synthetic control analysis shows an effect of the Boatlift on most of the crimes. In particular, homicide rates increase from below three per 100,000 people to five (those by firearm increase from two per 100,000 people to three), and the effects persist until 1983. Robberies display a similar trend, increasing from 80 per 100,000 people to 140. Motor vehicle thefts present the most persistent increase, peaking from 100 to 120 per 100,000 people, and remaining constantly around 110 per 100,000 people from 1982 onwards. Rapes and burglaries peak around the Boatlift, too, but only briefly, whereas larceny rates do not change around the Boatlift. As mentioned above, for homicides by strangers and drug-related homicides, the synthetic control is not a good match before the Boatlift.

To assess the significance of our estimates we perform placebo analyses treating each control unit in turn as the treatment unit and computing the difference between the "treated" MSA's crime rates and its synthetic control's crime rates.[74] Figure 73 and Figure 74 plot these gaps for each dataset, and one can evaluate the relative position of Miami in the distribution to assess whether the effects we find indeed can be attributed to the Boatlift. To get a precise p-value on these effects, we then compute the ratio of post-treatment Mean Squared Predicted Error (MSPE) for each of these placebo gaps, i.e. the norm of the distance between the treated unit and its synthetic control in the post-treatment period, over pre-treatment MSPE.[75] The empirical distribution of these ratios is plotted in Figure 75 and Figure 76. For homicides in the SHR dataset, Miami has the highest ratio, with a value of 10.74. Because there are 33 MSAs in our sample, the probability that a random draw from the empirical distribution of the MSPE ratios is as high as the value for Miami is 1/33, i.e. 0.03. Hence we can reject the null of no effect at the 5% level, and the same is true for motor vehicle thefts. We can compute the p-values for the effects on other crime rates in a similar way: the effects on robberies and homicides by firearm are marginally significant (p-values of 0.9 and 0.68 respectively).

[74] Abadie et al. (2010) include California, the treatment unit in their experiment, in the controls for these placebo tests. We decided to leave Miami out instead, which yields more conservative estimates. Indeed, if Miami's outcomes are high in the post-treatment period, when they are included in the synthetic control they will raise the synthetic outcomes, potentially lowering the gap between the placebo treatment unit and its synthetic control.

[75] We limit the post-treatment period to end with the second quarter of 1982 since that seems to be when even the most persistent effects start to fade out.

Figure 73: Placebo treatments, Offences Known Dataset

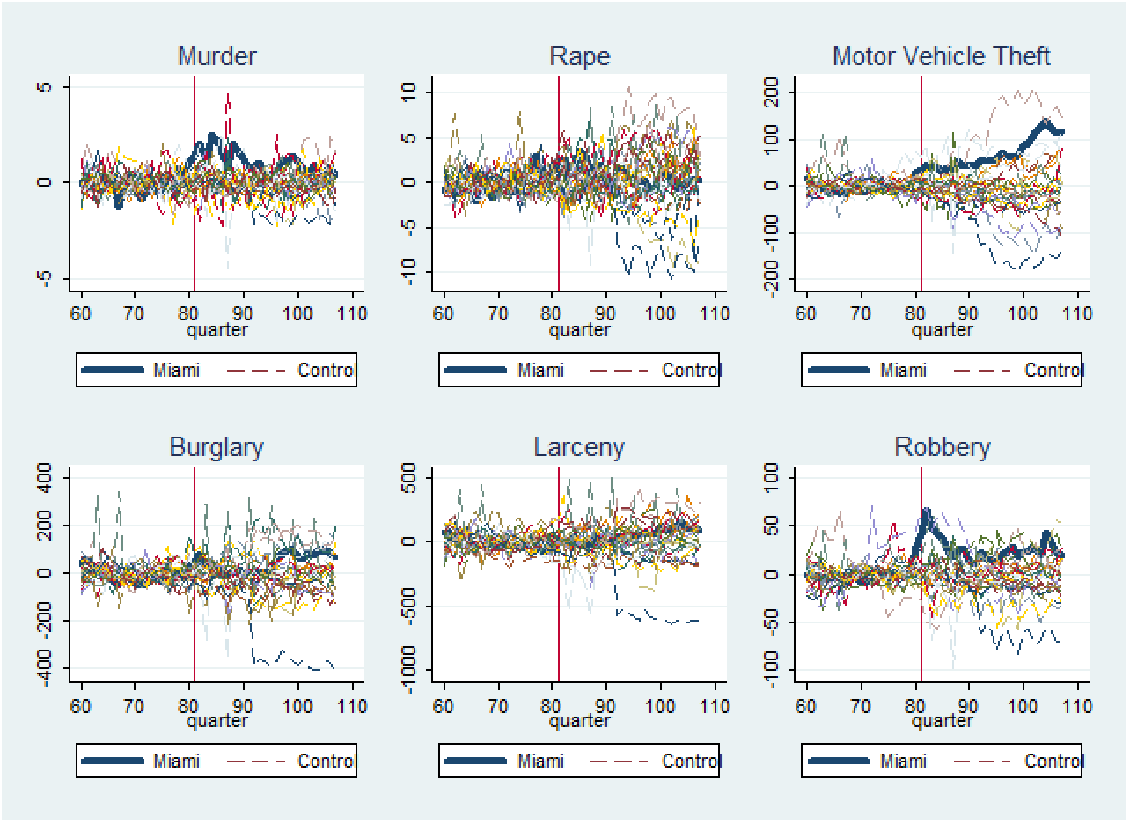


Figure 74: Placebo treatments, Supplementary Homicide Report Dataset

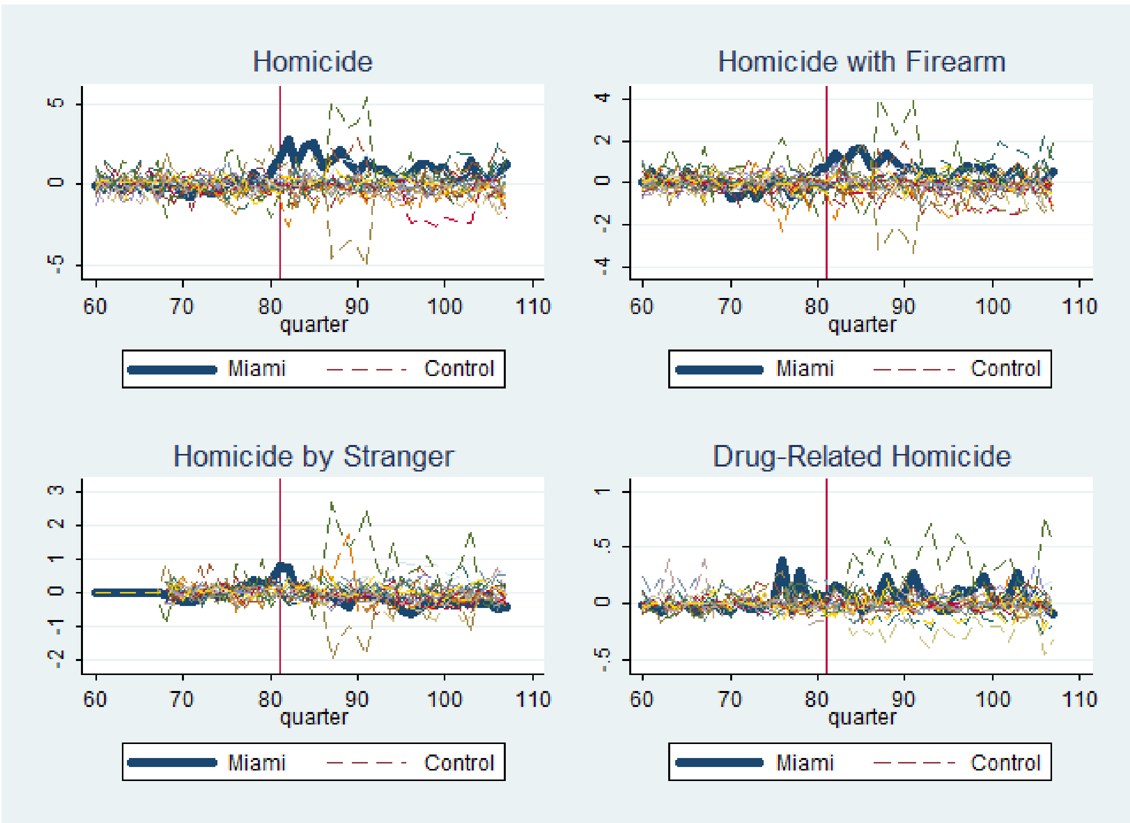


Figure 75: Post/Pre MSPE Ratios, Offenses Known Dataset

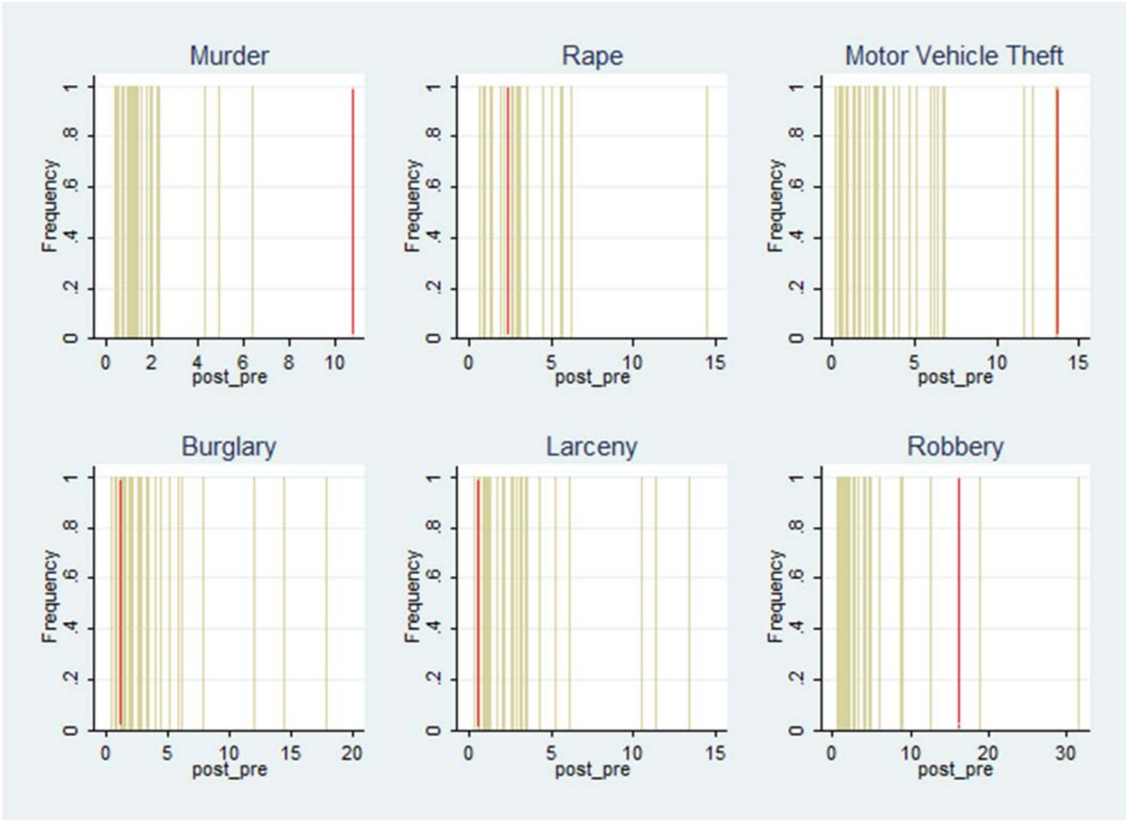
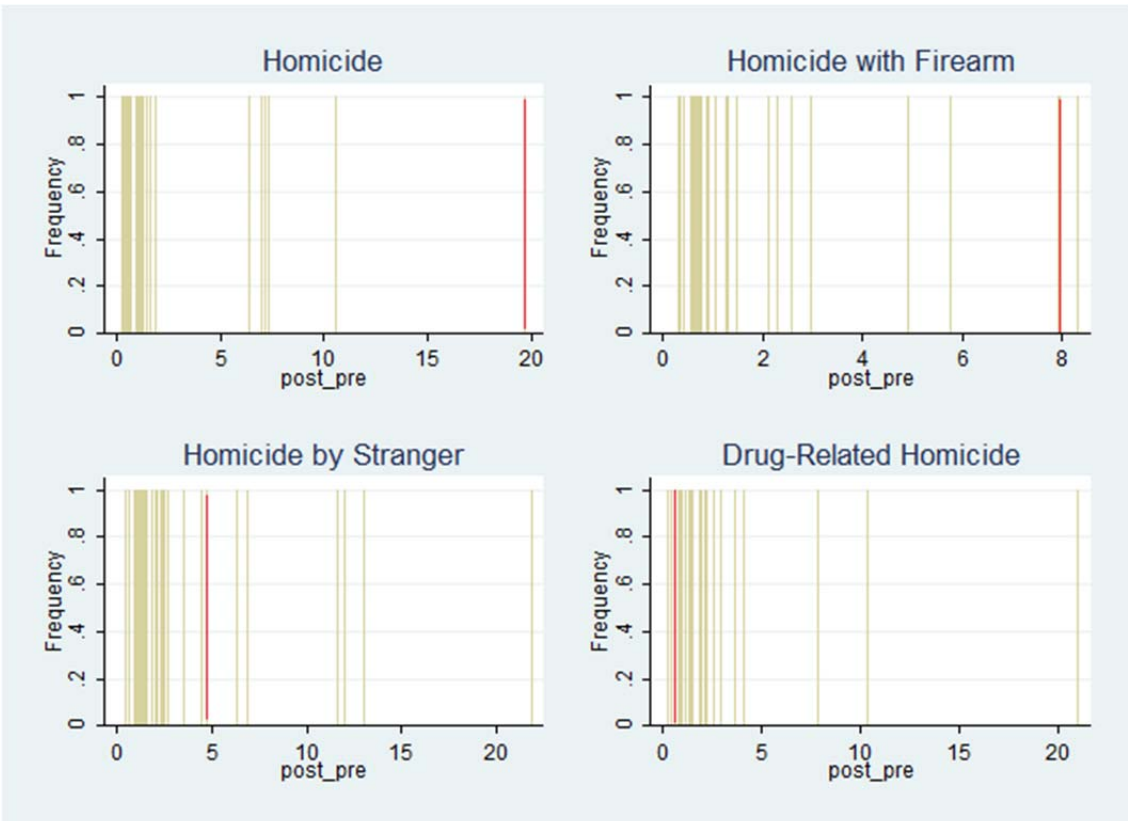


Figure 76: Post/Pre MSPE Ratios, Supplementary Homicide Report Dataset



6.4.2.2. Results by Offenders

A deeper understanding of the relationship between immigration and crime can be achieved by decomposing crime rates by characteristics of the offenders. In particular, we are interested in understanding whether the increase in crimes is due mostly to immigrants committing more offenses, or whether natives respond to immigration by increasing crime rates. Unfortunately, we do not have data on citizenship status of the offenders, but we do observe their race and ethnicity. Hence we construct rates of homicides committed by black as well as Hispanic offenders and compare these rates in Miami with those in other parts of the country. The SHR dataset reports race of offenders only from 1977, whereas ethnicity was collected only beginning in 1980. Therefore, our discussion of crime rates of Hispanics will be purely descriptive. Furthermore, in the interest of space, we focus on the synthetic control analysis for homicide rates among blacks, reporting results for our preferred specification only.

Homicides among Hispanics surge between the third and the fourth quarter of 1980, going from around 0.2 per 100,000 people to 1, and stay high thereafter. A simple DiD analysis looking at pre- and post-treatment would clearly yield large and significant effects, but we want to be careful in evaluating those. Homicides among black people increase from below 1 per 100,000 individuals to 1.5, and bounce back in 1982. Figure 77 only plots row trends, but Figure 78 confirms the results for blacks by plotting the synthetic control results.[76] Randomization inference in Figure 79 warns us that the p-value on this effect is 0.1379. This is probably due to the measure of homicides by offender's race being noisier. It is worth noting that our results are in line with the finding in Borjas et al. (2010) of a negative relation between immigration and African-Americans' employment, and a positive one between immigration and African-Americans' incarceration rates.

[76] Note that to do this analysis we perform the optimization again. In other words, we match the pre-trend for homicide rates among blacks instead of using the optimal weights found in the previous section in the analysis of homicide rates. Interestingly, the optimization procedure still assigns the most weight to Cincinnati and New York (see Table 5) as in the homicide analysis, but the combination of cities changes.

Figure 77: Trends in Homicide Rates by Blacks and Hispanics

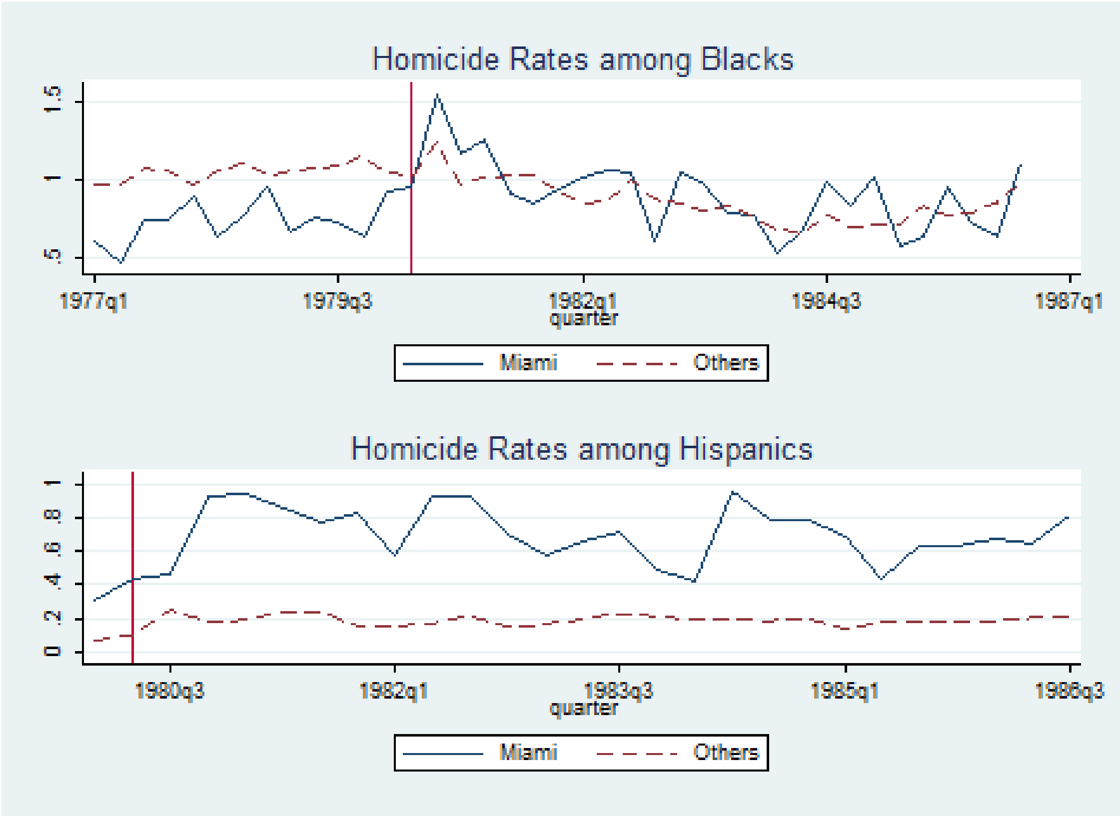


Figure 78: Synthetic Control Results for Homicide Rates among Blacks.

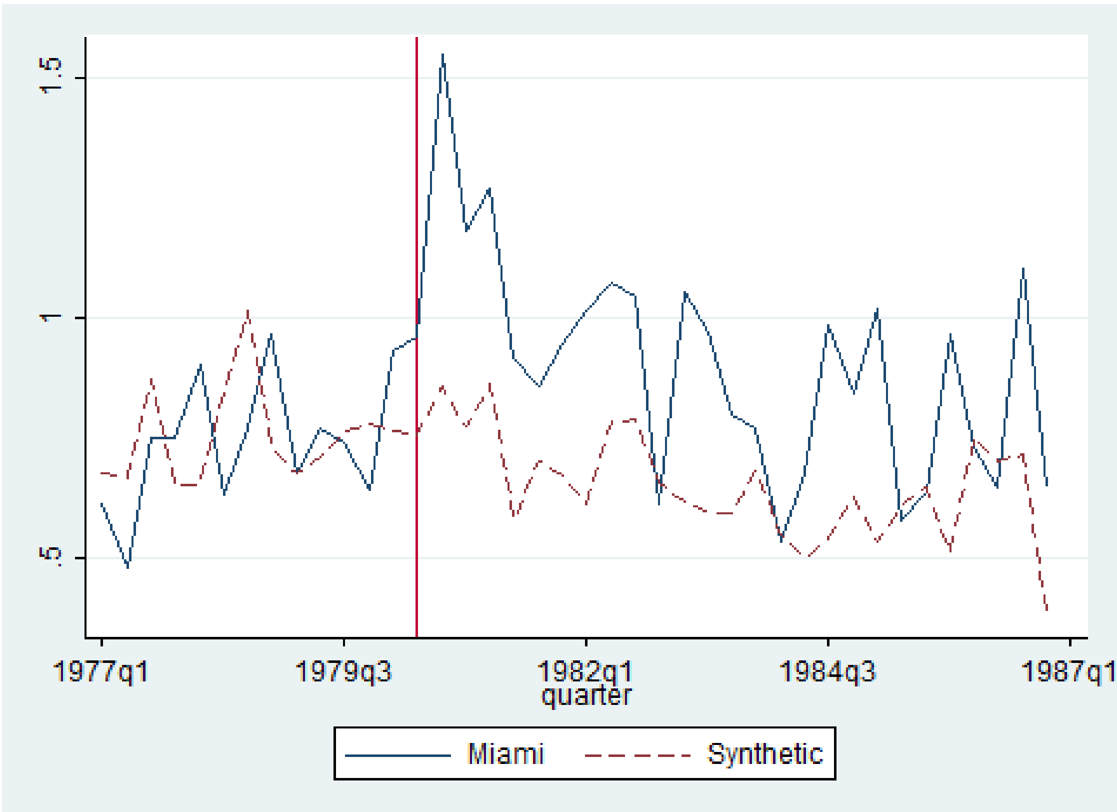
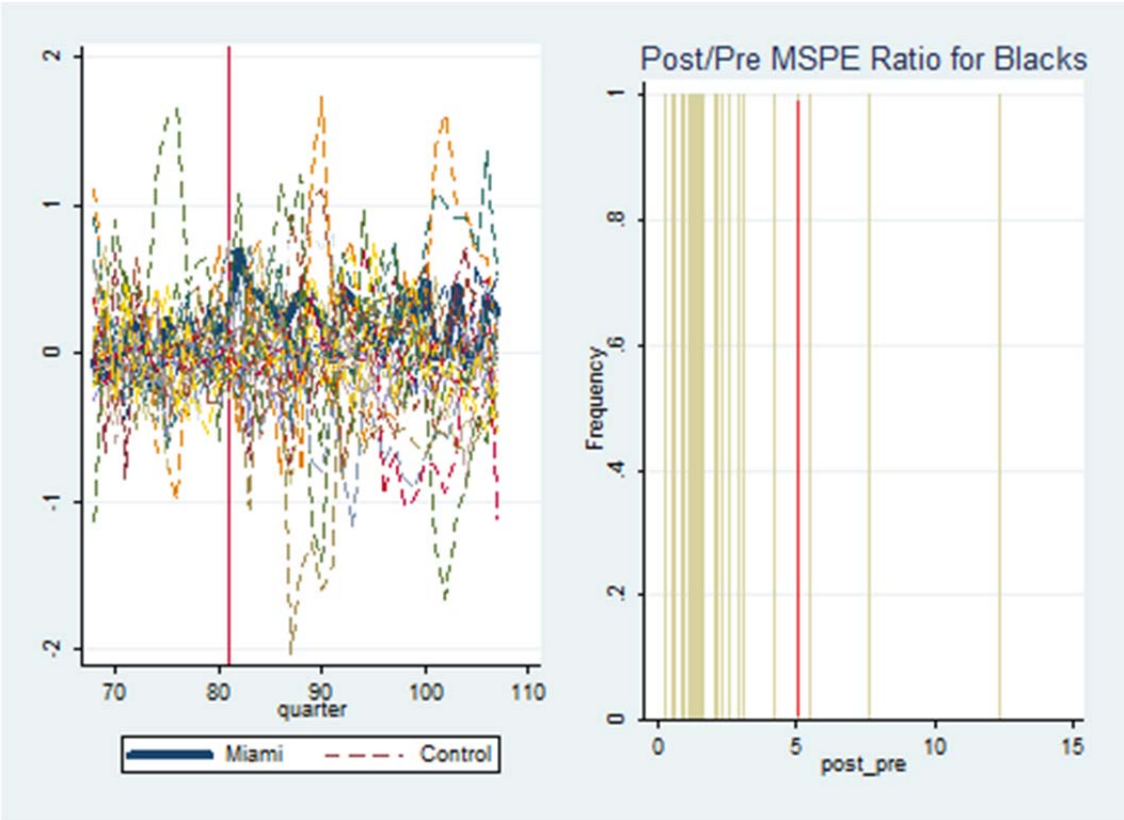


Figure 79: Randomization Inference for Results on Homicide Rates among Blacks



6.4.2.3. Discussion

Summing up, we find that the Mariel Boatlift had significant effects on certain types of crime rates and not others. Here we discuss possible explanations for these differences.

According to our synthetic control analysis, homicide rates increase by around 66% (significant at the 5% level) after the Boatlift, and the effect persists for more than two years. Similarly, robberies increase by around 75% (significant at the 10% level) and motor vehicle thefts increase by around 20% (significant at the 5% level). We find no effects on larcenies, burglaries or rapes. The effects on rapes are comforting, if we believe a model in which immigrants respond to the economic opportunities of crime but fear law enforcement. Interestingly, the increase in homicide rates does not correspond to an increase in homicides where the victim does not know the offender, which we interpreted as a proxy for homicides related to robberies.[77]

[77] However, it might well be the case that thieves rob victims they know, since they can assess how profitable the robbery will be.

Furthermore, we do not find any evidence of an increase in crime rates among African-Americans in Miami, consistent with the lack of natives' labor market effects of the Boatlift.[78] Most likely, the temporary legal status of the Marielitos made them less substitutable for African-Americans in the labor market. To assess substitution patterns, it would be important to analyze crime rates of Hispanics, especially natives or previous immigrants. We cannot assess causality in this case, but homicide rates among Hispanics seem to be responsible for most of the surge in homicide rates in Miami, although we do not observe any increase in drug-related homicides, traditionally a sector that is attributed to Hispanics in Miami.

In conclusion, the lack of significant results on larcenies and burglaries contrasts with the increase in robberies and homicides. This is in part consistent with the theory of economically-driven crime exposed in Chapter 2. Nonetheless, we do not have an explanation for the differential effect on violent crimes as opposed to other types of crime. Perhaps, violent crimes are "safer" for the offender, meaning they provide higher expected revenues at the same risk, if immigrants are removed upon committing any type of crime. Unfortunately, we cannot disentangle the effect of legal status from the fact that (some of) the Marielitos had been already convicted in Cuba.

Finally, we emphasize that the results on homicides are novel in the literature. Bianchi et al. (2012) use a supply-push approach to directly analyze the effects of immigration on crime in Italy and exploit bilateral migration with other European countries to completely isolate demand-pull factors that could be at play in the receiving Italian provinces. Similar to Spenkuch (2011), they find that the effects of immigration on crime rates are concentrated in economic crimes, thefts in the case of Spenkuch (2011) and robberies in the case of Bianchi et al. (2012).

It is useful to perform some back of the envelope calculations to get at an upper bound on the effects of illegal immigration on crime. Indeed, the negative selection of the Marielitos and their great number constitute unique circumstances, and under more general conditions the effects of immigration on crime will be lower than the ones estimated here. These 125,000 immigrants represent around 4% of the population in the Miami area at the end of 1979 (which was 3,121,900 individuals) and 7% of Miami's labor force (Card, 1990). Hence, if we assume that immigration effects are constant, we can extrapolate that a 1% increase in the population of a city (around 31,000 immigrants in this case), with an immigrant composition similar to the one of the Boatlift, increases homicide rates by 10%, robberies by 18.75% and motor vehicle thefts by 5%. Nonetheless, immigration effects are likely to be increasing with the number of immigrants arriving, meaning that these are upper bounds. Moreover, Hoefer et al. (2012) estimate that between 2005 and 2010 around 1,500,000 illegal immigrants entered the United States. If we assume a constant inflow of immigrants between 2005 and 2010, this translates into a flow of about 250,000 illegal

[78] Performing a synthetic control replication of Card (1990) still finds no impact of the Boatlift on natives' labor market outcomes.

immigrants entering every year, who then distribute across many U.S. cities. Hence, we do not believe that any city sees a 1% annual increase in its population due to illegal immigration, and these numbers represent a not-binding upper bound.

6.5. Conclusion

In this chapter we have discussed the current immigration policy in the United States and how it affects the composition of the pool of immigrants who arrive in the United States. In particular, by capping the number of temporary workers admitted each year at very low levels, the United States likely encourages illegal immigration. There is evidence that a buffer stock of temporary migrants is beneficial for U.S. firms, and that illegal immigration responds to demand fluctuations in a much more flexible way than does legal immigration. Consequently, attempts are currently being made to incorporate business cycle dynamics into regulated flows of immigrants.

The U.S. government always has been careful in balancing immigration reforms (and amnesties) with increased border security and enforcement, in order to counteract the increase in the probability of future amnesties as perceived by immigrants and to keep illegal inflows at a minimum. This also seems to be the case in the current reform. In particular, immigration enforcement has increased in the past decade, especially due to a surge in removals of criminal aliens.

Finally, we present a unique case study: an increase of 4% in Miami's population due to the arrival of 125,000 Cubans which increased homicide and robbery rates significantly. Nonetheless, we argue that due to the negative selection of the immigrants, and to the unique circumstances that led to such a high concentration of immigrants in a single area, this is likely to constitute an upper bound on the effects of immigration on crime.

Our exercise raises new questions that will need to be answered in future research. In particular, we believe that U.S. immigration policy places a lot of emphasis on granting the path to citizenship only to certain categories of immigrants, and on refusing it to others (for instance, the Marielitos in our case study). This might imply that some immigrants are excluded from welfare benefits, or face different criminal enforcement policies, because citizens cannot be removed when convicted. We have shown that immigrants who are now citizens earn more than non-citizens, and are less likely to be poor, and this does not seem to be related to selection. Now it would be useful to understand how immigrants respond to these incentives, and whether (and why) legal permanent residents who are not citizens face discrimination in the labor market.

Furthermore, whoever is born on U.S. soil is a U.S. citizen. Yet, we do not know how immigrants incorporate this into their immigration or criminal decisions, for instance by considering future income streams coming from their offspring. Indeed, this could be another channel that leads illegal immigrants to value staying in the United States and deters them from committing a crime that could result in their deportation.

6.6. Tables

Table 24: Apprehensions by Nationality

Region of nationality	2002	2003	2004	2005	2006	2007	2008 [1]	2009	2010	2011
Total	1,062,270	1,046,422	1,264,232	1,291,065	1,206,417	960,772	1,043,863	869,857	752,329	641,633
Africa	2,606	4,707	2,092	2,804	3,509	3,039	5,231	5,572	5,418	4,867
Asia	9,615	17,304	7,229	9,273	10,326	7,348	13,206	13,200	14,455	15,476
Europe	3,485	3,220	2,826	2,926	2,905	2,498	5,151	4,999	5,364	5,339
North America	1,038,073	1,010,371	1,214,322	1,237,532	1,179,554	938,934	1,003,223	830,161	711,926	602,206
Oceania	349	332	284	175	218	170	497	456	528	499
South America	8,119	10,479	14,093	38,128	9,001	8,671	15,529	14,970	14,039	12,755
Unknown	23	9	23,386	227	904	112	1,026	499	599	491

[1] Beginning in 2008, includes all administrative arrests conducted by ICE ERO.

Source: Yearbook of Immigration Statistics, 2011.

Table 25: Apprehensions by program

Program and sector/jurisdiction	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Total	1,062,270	1,046,422	1,264,232	1,291,065	1,206,417	960,772	1,748,885	1,410,708	1,200,060	969,210
CBP Border Patrol	955,310	931,557	1,160,395	1,189,031	1,089,096	876,803	723,840	556,032	463,382	340,252
Of which: Southwest sectors	929,809	905,065	1,139,282	1,171,462	1,071,979	858,737	705,022	540,851	447,731	327,577
ICE Homeland Security Investigations [1]	106,960	114,865	103,837	102,034	101,854	53,562	31,212	21,280	18,312	16,296
ICE Enforcement and Removal Operations [2]	-	-	-	-	15,467	30,407	288,811	292,545	270,635	285,085

[1] By 2008, no longer includes arrests under the 287(g) program.

[2] In 2006 and 2007 includes only arrests of fugitive and nonfugitive aliens under the National Fugitive Operations Program of ICE ERO; beginning in 2008, includes all administrative arrests conducted by ICE ERO.

Source: Yearbook of Immigration Statistics, 2011.

Table 26: States, Enforcement Dates and Categories

State	Enforcement Date	Categories covered
Arizona	January 2008	All Employers
Mississippi	July 2008	All Employers
Alabama	April 2012	All Employers
Georgia	July 2007	Public employers
Georgia	January 2012	Private employers
North Carolina	October 2012	All Employers
Indiana	July 2011	Public employers and public contractors
Nebraska	October 2009	Public employers and public contractors
Oklahoma	July 2008	Public employers and public contractors
Virginia	June 2011	Public employers and public contractors
Missouri	January 2009	Public employers and public contractors
Louisiana	January 2012	Public contractors
Minnesota	January 2008	Some public contractors
Pennsylvania	January 2012	Some public contractors
Idaho	July 2009	Public employers
Florida	May 2011	Some state agencies

Table 27: DACA applications

Month	Received	Accepted	Average requests/day	Under Review	Approved
Aug-12	37864	36601	2913	0	0
Sep-12	108586	104910	5715	29552	1707
Oct-12	117213	113494	5328	105648	26908
Nov-12	79757	77280	3988	147577	47954
Dec-12	47331	45705	2367	152155	49358
Jan-13	32454	31173	1545	131744	50218
Feb-13	29547	28278	1555	108214	45631
Mar-13	16778	16148	1678	100060	23717
Cumulative Total	469530	453589	3261		245493

August data from August 15 - August 30, 2012

March data from March 1 - March 14, 2013

Source: USCIS website.

Table 28: Differences-in-Differences Estimates on Offenses Known (OK) Dataset.

Variable	(1) Homicides, OK	(2) Rapes	(3) Motor Vehicle Thefts	(4) Larcenies	(5) Burglaries	(6) Robberies
Panel A						
Miami	0.204**	-2.003***	-11.77***	-196.2***	-59.70***	22.11***
	(0.081)	(0.24)	(3.14)	(19.3)	(9.80)	(1.61)
Post Mariel	-0.239***	1.392***	-9.920***	14.44	-19.92***	0.398
	(0.038)	(0.18)	(2.01)	(14.9)	(6.70)	(0.72)
Miami Post Mariel	1.288***	0.367**	63.99***	62.23***	38.25***	37.75***
	(0.038)	(0.18)	(2.01)	(14.9)	(6.70)	(0.72)
	[0.7675]	[0.825]	[0.705]	[0.7]	[0.7425]	[0.7625]
Constant	1.918***	7.006***	86.00***	752.9***	355.1***	33.32***
	(0.081)	(0.24)	(3.14)	(19.3)	(9.80)	(1.61)
Panel B						
Miami 1979q1	1.294***	-0.242	24.45***	114.2***	95.16***	23.71***
	(0.10)	(0.25)	(3.33)	(14.6)	(10.0)	(1.22)
Miami 1979q2	0.988***	0.702**	6.645**	-32.38**	71.34***	19.31***
	(0.10)	(0.28)	(3.25)	(14.7)	(9.86)	(1.13)
Miami 1979q3	1.915***	-1.556***	3.855	-106.1***	65.10***	20.53***
	(0.10)	(0.31)	(3.44)	(15.6)	(10.4)	(1.19)
Miami 1979q4	1.255***	-0.968***	14.52***	-71.24***	64.53***	25.18***
	(0.097)	(0.31)	(3.39)	(16.1)	(10.7)	(1.26)
Miami 1980q1	1.959***	0.659**	40.99***	116.6***	98.56***	53.79***
	(0.10)	(0.26)	(3.41)	(15.0)	(10.7)	(1.26)
Miami 1980q2	2.608***	-0.374	41.77***	-40.01**	107.8***	64.17***
	(0.10)	(0.30)	(3.38)	(16.6)	(10.7)	(1.24)
Miami 1980q3	3.228***	-0.0754	59.72***	-2.999	142.6***	96.96***
	(0.12)	(0.35)	(3.60)	(17.4)	(10.9)	(1.35)
Miami 1980q4	2.919***	0.665**	51.79***	55.40***	107.6***	82.52***
	(0.096)	(0.30)	(3.44)	(16.0)	(10.7)	(1.37)
Miami 1981q1	3.597***	1.071***	58.33***	98.18***	121.2***	70.90***
	(0.099)	(0.27)	(3.76)	(20.0)	(12.4)	(1.41)
Miami 1981q2	3.386***	1.344***	48.15***	-7.620	94.91***	63.60***
	(0.11)	(0.30)	(3.72)	(20.6)	(12.2)	(1.32)
Miami 1981q3	2.824***	1.001***	52.26***	-7.457	87.07***	54.06***
	(0.10)	(0.32)	(3.93)	(21.3)	(12.2)	(1.40)
Miami 1981q4	2.372***	-0.362	52.11***	50.01**	60.21***	48.29***
	(0.095)	(0.31)	(3.73)	(21.6)	(12.6)	(1.58)
Observations	10435	10435	10435	10435	10435	10435
R-squared	0.01	0.02	0.01	0.00	0.00	0.01

*** p<0.01, ** p<0.05, * p<0.1

Standard Errors Clustered at the MSA level in parenthesis. Monte Carlo Rejection Rates for the Miami*Post-Mariel interaction in Panel A are in square brackets.

Table 29: Differences-in-Differences Estimates on Offenses Known (SHR) Dataset.

Variable	(1) Homicides, SHR	(2) Firearm Homicides	(3) Homicides by Strangers	(4) Drug-Related Homicides
Panel A				
Miami	-0.395*	-0.264	-0.0497	0.0324***
	(0.21)	(0.17)	(0.033)	(0.0094)
Post Mariel	-0.242***	-0.308***	0.169***	0.0115
	(0.076)	(0.067)	(0.032)	(0.015)
Miami Post Mariel	1.135***	1.006***	-0.0485	0.0768***
	(0.076)	(0.067)	(0.032)	(0.015)
	[0.5075]	[0.555]	[0.4775]	[0.385]
Constant	2.524***	1.715***	0.259***	0.0549***
	(0.21)	(0.17)	(0.033)	(0.0094)
Panel B				
Miami 1979q1	0.190	0.258	-0.114	0.371***
	(0.16)	(0.16)	(0.074)	(0.017)
Miami 1979q2	-0.328	-0.388**	-0.0667	-0.0276
	(0.21)	(0.16)	(0.083)	(0.017)
Miami 1979q3	0.789***	0.334**	0.203***	0.275***
	(0.17)	(0.14)	(0.073)	(0.016)
Miami 1979q4	0.0767	0.0133	0.116	0.00706
	(0.19)	(0.17)	(0.087)	(0.012)
Miami 1980q1	0.771***	0.957***	0.206***	0.101***
	(0.17)	(0.15)	(0.065)	(0.014)
Miami 1980q2	1.687***	1.324***	0.693***	0.0745***
	(0.17)	(0.15)	(0.061)	(0.013)
Miami 1980q3	2.081***	1.447***	0.629***	0.159***
	(0.21)	(0.17)	(0.080)	(0.021)
Miami 1980q4	1.821***	1.687***	0.101*	0.147***
	(0.19)	(0.14)	(0.057)	(0.011)
Miami 1981q1	2.406***	1.810***	0.367***	0.0157
	(0.14)	(0.13)	(0.062)	(0.027)
Miami 1981q2	1.886***	1.652***	-0.0340	-0.0588**
	(0.19)	(0.16)	(0.058)	(0.022)
Miami 1981q3	1.415***	1.289***	0.0330	-0.0441*
	(0.15)	(0.13)	(0.081)	(0.025)
Miami 1981q4	1.457***	1.117***	-0.0922	0.0586***
	(0.16)	(0.13)	(0.11)	(0.019)
Observations	1508	1508	1508	1508
R-squared	0.01	0.03	0.05	0.03

*** p<0.01, ** p<0.05, * p<0.1

Standard Errors Clustered at the MSA level in parenthesis. Monte Carlo Rejection Rates for the Miami*Post-Mariel interaction in Panel A are in square brackets.

Table 30: Weights on Predictors in Synthetic Control and RMSPE

Specification	Weight								RMSPE
	Lagged Dep. Var.	Density	Real GDP	Black Share	Hispanic Share	Unempl. Rate	Dropout Share	High-school Share	
Figure 3.4									
Homicide, OK	0.907	0.004	0.006	0.029	0.013	0.012	0.015	0.015	0.487
Rape	0.909	0.009	0.016	0.006	0.011	0.009	0.020	0.019	1.249
Motor Vehicle Theft	0.930	0.011	0.012	0.009	0.009	0.008	0.015	0.007	11.489
Larceny	0.920	0.002	0.014	0.010	0.009	0.002	0.022	0.022	50.005
Burglary	0.942	0.005	0.006	0.017	0.003	0.006	0.011	0.010	31.420
Robbery	0.929	0.009	0.013	0.013	0.012	0.004	0.015	0.004	8.114
Figure 3.5									
Homicide, OK	0.943	0.003	0.004	0.028	0.014	0.008	-	-	0.455
Rape	0.949	0.008	0.014	0.008	0.012	0.010	-	-	1.356
Motor Vehicle Theft	0.957	0.010	0.006	0.007	0.011	0.009	-	-	13.057
Larceny	0.964	0.002	0.008	0.010	0.012	0.004	-	-	30.139
Burglary	0.967	0.004	0.004	0.015	0.004	0.007	-	-	31.265
Robbery	0.952	0.008	0.007	0.011	0.017	0.006	-	-	7.613
Figure 3.6									
Homicide, SHR	0.866	0.003	0.009	0.061	0.016	0.010	0.018	0.017	0.397
Homicide with Firearm	0.891	0.004	0.008	0.057	0.015	0.008	0.010	0.009	0.386
Homicide by Stranger	0.855	0.013	0.016	0.008	0.010	0.018	0.033	0.047	0.162
Drug-Related Homicide	0.526	0.027	0.040	0.081	0.068	0.045	0.126	0.087	0.113
Figure 3.7									
Homicide, SHR	0.905	0.002	0.006	0.061	0.017	0.008	-	-	0.379
Homicide with Firearm	0.916	0.003	0.005	0.055	0.015	0.006	-	-	0.387
Homicide by Stranger	0.939	0.013	0.013	0.007	0.011	0.017	-	-	0.169
Drug-Related Homicide	0.697	0.032	0.078	0.067	0.098	0.029	-	-	0.108

Table 31: Weights on MSAs in Synthetic Control

Specification	Weight														
	Albany	Birmingham	Chicago	Cincinnati	Houston	Los Angeles	New York	Philadelphia	Pittsburgh	Riverside	Rochester	Saint Louis	San Diego	San Jose	DC
Figure 3.4															
Homicide, OK	0.167	0	0	0	0	0.214	0.481	0	0.138	0	0	0	0	0	0
Rape	0.146	0	0	0	0	0.062	0.343	0	0.003	0	0.446	0	0	0	0
Motor Vehicle Theft	0.135	0	0	0.619	0	0	0.083	0	0	0	0	0	0	0	0.164
Larceny	0	0.087	0	0	0	0.328	0.484	0	0	0	0	0	0	0	0
Burglary	0	0.168	0.222	0	0	0.072	0.135	0.39	0.014	0	0	0	0	0	0
Robbery	0	0.313	0	0.033	0	0.268	0	0	0	0.305	0	0	0	0.081	0
Figure 3.5															
Homicide, OK	0	0	0	0	0	0.173	0.366	0	0	0	0	0	0.157	0.304	0
Rape	0.464	0	0.152	0	0	0.05	0.313	0	0	0	0.021	0	0	0	0
Motor Vehicle Theft	0.754	0	0	0	0	0.046	0.131	0	0	0	0	0	0	0	0.067
Larceny	0	0	0	0	0	0.838	0.162	0	0	0	0	0	0	0	0
Burglary	0	0.078	0.705	0	0	0.147	0.07	0	0	0	0	0	0	0	0
Robbery	0	0.04	0	0	0	0.352	0	0	0	0.07	0	0	0	0.538	0
Figure 3.6															
Homicide, SHR	0	0	0	0.213	0	0.017	0.681	0	0	0	0	0	0	0.088	0
Homicide with Firearm	0	0	0	0	0	0.243	0.668	0	0	0	0.01	0	0.078	0	0
Homicide by Stranger	0	0.13	0	0.007	0	0.291	0.2	0.229	0	0	0.144	0	0	0	0
Drug-Related Homicide	0	0	0	0	0.13	0.366	0.298	0	0	0	0	0.149	0	0	0
Figure 3.7															
Homicide, SHR	0	0	0	0	0	0	0.69	0	0	0	0	0	0.035	0.275	0
Homicide with Firearm	0	0	0	0	0	0.305	0.554	0	0	0	0	0	0.141	0	0
Homicide by Stranger	0	0	0	0	0	0.939	0	0	0	0	0	0	0	0	0
Drug-Related Homicide	0	0	0	0	0	0.924	0	0	0	0	0	0.076	0	0	0

Note: Weigths might not sum to 1 because we left out some marginal MSAs

Table 32: Optimal Weights for Black Homicide Rates

Variable	Weight	MSA	Weight
Lagged Dep. Var.	0.915329		
Density	0.003668		
Real GDP	0.002841	Birmingham	0.019
Black Share	0.024967	Cincinnati	0.35
Hispanic Share	0.004049	New York	0.457
Unempl. Rate	0.008281	DC	0.173
Dropout Share	0.022448		
High-school Share	0.018418		
RMSPE	0.1615328		

Conclusions: policy matters

When thinking about the link between migration and crime, policy does matter. This is probably one of the main findings of this report. Although the media and political debate often suggest it, there does not seem to be anything intrinsically criminal in immigrants. Indeed, the fact that their behavior respond to policy changes (such as legalizations) suggest that it is the policy, rather than some persistent characteristics of the immigrants, which matters in shaping their criminal decisions. This is hardly surprising for economists, who are used to think that individual respond to incentives. As a matter of fact, migration policies produce two main effect on immigrants: first, determining their initial selection and, second, shaping their incentives and behavior. Clearly, both aspects have important consequences for immigrants' criminal rates. In the theoretical framework presented in chapter 2 we have discussed precisely how, given a certain migration policy, we can expect potential migrants to select into legal and illegal migration and how their choices between employment and crime will, then, be affected by their legal status.

A relevant role of migration policy in determining criminal behavior of immigrants is consistent with empirical findings in Mastrobuoni and Pinotti (2012), Baker (2012), Bell et al. (2013) and Freedman et al. (2013). Interestingly, when policy-makers design migration policies, they usually have some specific target in mind, but they may fail to take into account unintended consequences and side-effects of their policies. Bell et al. (2013), for instance, suggest that a change in the asylum policy introduced by the British government in order to limit the number of asylum seekers and to reduce abuse and unfounded applications may have induced asylum seekers to engage in more property crime. Similarly, restrictive migration policies which aim at maintaining "under control" the number of legal migrants admitted in the country, may lose control on the undocumented immigrants and create the conditions for this latter group to be particularly prone to criminal behavior.

In the short term, the empirical findings of this report suggest that granting legal status to unauthorized immigrants is effective in reducing immigrant crime rates. In the medium and long run, instead, such a policy would be ineffective if the legalized immigrants are quickly replaced by newly arrived undocumented immigrants who face the same initial selection and the same incentives to engage in crime. In order to obtain a permanent reduction in immigrant crime, one certainly needs to design a more general intervention on the migration policy.

International comparisons can be extremely useful in thinking about ways of improving policies. In this report, we have looked at Italy and at the United States. These are clearly very different countries in many respects (size, institutions, economic structure, etc.), nevertheless in both countries the presence of large populations of undocumented immigrants is a distinctive and persistent feature. As discussed in chapter 3 (Italy) and chapter 6 (US), there are important differences in these unauthorized flows and in the migration policies implemented to prevent them and to deal with them. Regarding criminal behavior of immigrants, the two countries stand quite apart from each other: if Italy is a country where immigrants are strongly overrepresented among criminals, the evidence for the US points in the opposite direction. Having substantial population of unauthorized immigrants, therefore, does not seem to be a sufficient condition for seeing large fractions of criminal involved in criminal activities. Nevertheless, both theory (chapter 2) and existing evidence (Mastrobuoni and Pinotti, 2012; Baker, 2012; Freedman et al., 2013) suggests that the lack of legal status increase criminal behavior. Different elements may contribute to explain the relatively lower participation of undocumented immigrants in crime in the US than in Italy.

As we have seen, thanks to the frequent amnesties and to the malfunctioning of the quota system (chapter 3), undocumented immigrants in Italy have a reasonable expectation to obtain legal status within a few years from arrival. In the US, instead, the last amnesty was granted in 1986 and undocumented immigrants had to wait 25 years before a credible debate about a possible amnesty in the near future started again (chapter 6). What implications the difference in expectations may have for immigrants? First, the value of being illegal is lower if the probability of becoming legal is close to zero. Second, in the absence of legalization chances, there is no return from just staying in the country. In periods of weak labor demand, unauthorized immigrants in the US may be better off by returning back home and wait for a better economic cycle: this is precisely the pro-cyclical nature of undocumented migration in the US discussed by Hanson (2009). In Italy, instead, the possibility of becoming legal (and, hence, gaining right to family reunification, gaining access to other European labor markets, etc.) may induce immigrants not to return to the home country even if they face harsh economic conditions in the host country. Indeed, the “legalization cycle” (i.e. the fluctuations in the probability of obtaining legal status) may make unauthorized immigrants quite unresponsive to the economic cycle: immigrants who do not move away when labor market opportunities are poor are obviously more likely to engage in crime; even more so if, being undocumented, they cannot rely on support from the welfare state.

Related to this point, given that the vast majority of unauthorized immigrants living in the US are Mexican citizens, one can speculate that the cost of travelling back and forth from Mexico to the US is lower for them than for unauthorized immigrants in Italy who come from a very diverse set of countries, some of which quite remote from Italy (see Dustmann et al., 2013). This difference in migration costs would strengthen the point made in the previous paragraph, allowing unauthorized migrants in the US to engage more in circular migration and reducing their exposure to downturns in labor demand.

A second important difference between Italy and the US relies in the degree of uncertainty and in the extent of the detrimental effect on labor market prospect that the lack of legal status implies for immigrants in the two countries. If unauthorized immigrants in the US are in a relatively better situation than unauthorized immigrants in Italy, one would expect the former to engage less in crime than the latter. Being an unauthorized immigrant in Italy implies a daily exposure to substantial uncertainty. By law, all citizens are required to carry the official Italian ID with them at all times while immigrants must always carry their passport and the documents proving the legitimacy of their residence in the country. Italy is an ethnically homogenous country where immigrants can be easily spotted by police forces who inspect a large number of individuals every year and routinely apply racial profiling in doing so.[79] The lack of residence permit prevents immigrants from having a legal working contract, from having access to the welfare state (basically, only emergency care is guaranteed), from signing a house rental contract, etc. Basically, it confines them into a fully informal – and fairly unpredictable – world. In the US, instead, there are no compulsory ID, police forces do not racially profile potential unauthorized immigrants and are not authorized to ask them to prove the legitimacy of their residence in the US (see the debate on the 2010 Arizona SB 1070), undocumented immigrants can live in the US for years[80], have pseudo-legal jobs (where they pay social contributions), rent houses and drive cars, re-constitute their households, send kids to school (and see them becoming American citizens if they were born in the US, thanks to the *ius soli* principle which determines citizenship acquisition). Hence, if being illegal in Italy implies substantially harsher conditions than in the US, this would influence the initial

[79] During the period considered in this study (2004-2007), the Italian police reports having checked and identified about 7-9 million individuals in each year. This is a very large number for a country which has about 59 million residents.

[80] According to the Pew Research Center (2013), about two thirds of the estimated 11 million undocumented immigrants in the US have been there for more than ten years. This is in striking contrast with data from Dustmann et al. (2013) for Italy, where undocumented immigrants have an average duration of residence of about 2.6 years.

selection and the incentives to crime, both in the direction of increasing the likelihood that unauthorized immigrants will be more likely to engage in crime.[81]

Finally, differences in law enforcement are likely to matter. The judicial system in the US is clearly more effective and tougher than the Italian one: just by looking at one indicator, the US has one of the largest share of prison population per capita, while Italy is a country of recurrent general amnesties of inmates (Barbarino and Mastrobuoni, 2012). If the probability of being caught upon committing a crime and then expelled for lacking legal status is substantially higher in the US than in Italy, we should clearly expect undocumented immigrants to engage less in crime.

More research is badly needed in this area. We need to fully understand the role of migration policy in determining the participation of immigrants in criminal activities. As a matter of fact, immigrant crime is a top concern among the electorates in receiving countries and it gives ground to politicians that support sub-optimally restrictive migration policies. On the contrary, we want to design policies that flexibly allow welfare-enhancing migration flows but that also prevent immigrants from disproportionately engage in crime. This is clearly a crucial step to address this widespread concern among voters.

[81] Dustmann et al. (2013) suggest that exposure to uncertainty will induce selection on risk aversion: undocumented immigrants will be relatively less risk averse than legal immigrants. If less risk averse individuals are more likely to engage in crime, this would be an additional channel which explain the higher propensity of undocumented immigrants to engage in crime.

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