Migration Restrictions and Criminal Behavior: Evidence from a Natural Experiment*

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Abstract

We estimate the causal effect of immigrants' legal status on criminal behavior exploiting exogenous variation in migration restrictions across nationalities driven by the last round of the European Union (EU) enlargement. Unique individual-level data on a collective clemency bill enacted in Italy five months before the enlargement allow us to compare the post-release criminal record of inmates from new EU member countries with a control group of pardoned inmates from candidate EU member countries. Differences in differences in the probability of rearrest between the two groups before and after the enlargement show that obtaining legal status lowers the recidivism of economically motivated offenders, but only in areas that provide relatively better labor market opportunities to legal immigrants. We provide a search-theoretic model of criminal behavior that is consistent with these results.

Keywords: immigration, crime, legal status
JEL codes: F22, K42, C41

1 Introduction

Concerns about the effects of immigration on crime are widespread. As a matter of fact, foreigners are heavily overrepresented among the prison population of all developed countries. In recent years the share of foreigners among official residents barely reached 10% in the United States (and it was significantly lower in most other countries), while their incidence among incarcerated individuals was many times larger (Figure 1). Such numbers increase support for migration restrictions, which prevent part of the prospective immigrant population from (legally) residing in the host countries. At the same time, if enforcement is loose, migration restrictions may also create a pool of unauthorized immigrants who are able to (illegally) cross the frontier but, once in the host country, do not enjoy legal status and therefore cannot work in the official sector.

The implications of migration barriers for crime are then ambiguous. On the one hand, restrictive policies prevent a number of immigrants (who would be potentially at risk of committing a crime) from entering the country, or expel them after entry; on the other hand, those who manage to enter anyway face worse income opportunities in official markets, which raises their propensity to engage in criminal activity. Empirically identifying the overall effect is difficult, since immigrants determine, at least in part, their legal status. The decision about whether to reside legally or illegally in the destination country may, in fact, respond to several individual characteristics (e.g., working ability) that are also likely to enter the decision on whether to commit a crime. In addition to this problem, the size of the illegal immigrant population is usually not reported in official statistics, so their crime rate also remains unobserved.

This paper exploits exogenous variations in legal status, provided by the last round of the European Union (EU) enlargement, and detailed criminal records on a sample of pardoned immigrants in Italy to address these issues. After August 1, 2006, more than 9,000 foreigners were released from Italian prisons upon approval of a collective clemency bill passed by the Italian Parliament; five months later, on January 1, 2007, about 800 of them acquired the right to legally stay in Italy as their origin countries, namely, Romania and Bulgaria, entered the EU. We thus exploit the asymmetric effect of the EU enlargement across nationalities to estimate the effect of legal status on criminal behavior, as measured by the post-release criminal record of pardoned foreign individuals.

The empirical strategy is grounded on a search-theoretic model of crime that relates legal status to the probability of committing a crime. In the tradition of economic models of crime, agents choose between legitimate and illegitimate activities by comparing the economic costs and benefits of the two. Granting access to the official sector, legal status raises the returns to legitimate activities (or equivalently the opportunity cost of illegitimate ones), which in turn lowers the probability of engaging in crime. However, there is also another effect that works in the opposite direction: Immigrants without legal status may be deported back to their own country with some positive probability, which mechanically reduces the pool of individuals in this group who are at risk of committing

2

an offense.

The model also allows for self-selection into legal status. In particular, migration policy imposes some fixed cost on official entrants, so that only immigrants with higher (legitimate) income opportunities in the host country will decide to comply with it; the other ones will prefer to enter unofficially and face the risk of being expelled in the future. Therefore, self-selection at the frontier implies that the distribution of individual characteristics potentially correlated with criminal activity differs systematically between the groups of legal and illegal immigrants; this is also the main threat to the identification of the causal effect of legal status.

To address this issue, we focus on the difference in differences in the probability of rearrest between pardoned inmates from new EU member countries and a control group of inmates from candidate member countries before and after the EU enlargement. While there are significant differences between the average characteristics of the two groups, weighting observations by the (inverse) propensity score of belonging to each group eliminates such differences, as well as differences in pre-enlargement outcomes. Baseline estimates suggest that the average probability of rearrest over a six-month period declines from 5.8% to 2.3% for Romanians and Bulgarians after obtaining legal status (as a consequence of the EU enlargement), relative to no change in the control group. When we distinguish between different categories of potential offenders, the effect is significant only for pardoned inmates who were previously incarcerated for economically motivated crimes, and the reduction is stronger in areas characterized by better income opportunities for legal (relative to illegal) immigrants. These results are robust to alternative estimation techniques and to several robustness checks.

We contribute to the literature on the social and economic effects of immigration. Until very recently, this research area has traditionally emphasized the labor market competition between immigrants and natives (surveys include Borjas, 1994; Friedberg and Hunt, 1995; Bauer et al., 2000; Card, 2005), as well as the effects of immigration on fiscal balances (Storesletten, 2000; Lee and Miller, 2000; Chojnicki et al., 2005) and prices (Lach, 2007; Cortes, 2008). Drawing on survey evidence from 21 European countries in 2002, Card et al. (2009) show that, besides these ("economic") issues, natives' support for migration restrictions is shaped also (and, indeed, mostly) by other "compositional amenities," among which crime plays a major role (see also Bauer et al., 2000)

Partly as a consequence of this increasing awareness, a few previous papers have examined the empirical relation between immigration and crime. At the aggregate level, Butcher and Piehl (1998a), Bianchi et al. (2008), and Bell et al. (2010) document some correlation between and within local areas in the United States, Italy, and the United Kingdom, respectively, but conclude that the causal effect is not different from zero (with the exception, maybe, of asylum seekers in the United Kingdom).[1] At the micro level,

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[1] Partly in contrast with these findings, Borjas et al. (2010) argue that migration indeed has an effect, but a very indirect one: By displacing black males from the labor market, immigration increases their criminal activity.

Butcher and Piehl (1998b, 2007) use census data to show that, keeping other individual characteristics constant, current immigrants have lower incarceration rates than natives, while the opposite was true for former immigrants at the beginning of the 20th century (Moehling and Piehl, 2007). Yet, no previous study has investigated the role of legal status, which is precisely the contribution of the present paper.

We also add to a huge empirical literature on the relation between legitimate income opportunities and a criminal career. Indeed, the fact that the propensity to engage in crime should decrease with outside options in official markets is one of the key results of the economic model of crime (Becker, 1968). Over the years, several papers have examined the empirical validity of this prediction, finding, in general, a good deal of consistent supporting evidence: A non-exhaustive list includes Witte (1980), Meyers (1983), Grogger (1998), Gould et al. (2002), and Machin and Meghir (2004). We contribute to this strand of the literature by showing that in our sample of pardoned foreigners as well, access to better legitimate income opportunities (through the acquisition of legal status) lowers the individual propensity to engage in crime.

The next section summarizes the main features of immigration in Italy, paying particular attention to the gap between legal and illegal immigrants in terms of labor market outcomes and crime. Section 3 provides a theoretical framework that captures these elements in a very simple way, studies the channels through which legal status can impact crime, and clarifies the main threats to identification. Section 4 describes in detail the natural experiment, while Section 5 derives the estimating equations. Finally, Section 6 discusses the empirical results and Section 7 concludes.

2 Legal and illegal immigrants: Preliminary evidence from Italy

After centuries of massive emigration, in the late 1980s Italy became a recipient of positive net inflows. As a consequence, the legislative framework in this respect is also very recent, the first migration law being enacted in 1990 and later amended in 1995 and 2002. Throughout these changes, Italian migration policy remained firmly grounded on the institution of the *residence permit*, which establishes a direct link between working conditions and legal residence: The main condition for eligibility in Italy is receiving a job offer. However, the total number of residence permits issued each year is fixed on the basis of migration quotas decided by the government.

2.1 Official migration

Over the last two decades, the number of valid residence permits rose from less than 1 million at the beginning of the 1990s to more than 2 million in 2005, slightly declining thereafter. The number of foreign (official) residents increased even more steeply, from less than 600,000 to almost 4 million, in the face of an otherwise declining population

(see Figure 2). Official residents include immigrants holding a valid residence permit (and possibly also their close relatives), as well as foreigners enjoying legal status in Italy for other reasons, such as being a citizen of an EU member country. The divergence between the two measures (permits and residents) toward the end of the period is indeed explained by the EU enlargement, which, starting in 2004, relieved an increasing number of Eastern European citizens from needing a residence permit to legally reside in Italy.

Notwithstanding the spectacular growth of the official immigrant population, the number of newly issued residence permits fell systematically short of total demand over the years, often by a large extent. For instance, 170,000 permits were issued in 2007 in the face of more than 740,000 demands; the following year, the number of new residence permits decreased to 150,000, to be primarily assigned to applications left pending from the year before (thus increasing the gap between current demand and the supply of permits). In addition to that, the 2002 reform of migration policy requires prospective immigrants to find a job contract before entering the country, thus further hampering the match of foreign workers with Italian employers. Stringent requirements on permit eligibility and the tight rationing of migration quotas, coupled with weak border enforcement (also due to the geographic configuration and location of the Italian peninsula), resulted in an increasing number of undocumented immigrants illegally crossing the frontier or overstaying tourist visas.

2.2 Unofficial migration

While the very nature of unofficial migration prevents accurate estimates of its size, amnesties of formerly undocumented immigrants provide some information in this respect. During these episodes, immigrants illegally present in Italy can apply for a valid residence permit under very mild conditions, with clear incentives to report their illegal status.[2] General amnesties have been enacted every four to five years since 1986, growing in size from 100,000 to 200,000-250,000 individuals during the 1990s, and reaching a peak of 700,000 in 2002. The acceleration in official migration observed during the last few years was thus accompanied by an analogous one in unofficial inflows (see Figure 3).

Figure 3 also reports the number of illegal immigrants expelled from the country. In Italy illegal immigrants apprehended by the police are *not* incarcerated; rather, they are accompanied to the frontier and deported back to their origin country. In some cases, deportation is not enforced and the individual just receives an injunction to leave the country.[3] While the fraction of immigrants who are deported is not large, it is not negligible. Reviewing the years in which there was an amnesty, one finds the ratio of expulsions

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[2]Bianchi et al. (2008) and Fasani (2009) also use applications for amnesty to estimate the size of the illegal population in Italy, while several studies adopt the same methodology to count the number of undocumented immigrants in the United States after the amnesty passed with the 1986 Immigration Reform and Control Act (see, e.g., Winegarden and Khor, 1991).

[3]In 2002 the last reform of migration policy (Law 189/2002) introduced the possibility of incarceration for those who did not comply with a previous injunction to leave and were later re-apprehended by the police. However, such a norm was never enforced because it was deemed anticonstitutional by the Constitutional Court.

to demands for amnesty by unofficial immigrants was 17% in 1986, went up to 28% in 1998, and then decreased again to 15% during the last amnesty program of 2002.

2.3   Criminal and labor market outcomes

Even though foreigners cannot be incarcerated for breaking migration laws, they are nevertheless overly represented in the Italian prison population. The number of foreign inmates more than doubled since the early 1990s, from less than 10,000 to more than 20,000 in 2008, in the face of just a slight increase of the total prison population. As a result, the share of foreigners in the prison population has reached one-third (Figure 4), an order of magnitude greater than the share of immigrants in the whole population. Of course, such an imbalance is not necessarily driven by differences in criminal behavior, since it can also depend on statistical discrimination in law enforcement against foreigners (Becker, 1957). Yet, it is hard to believe that such a huge difference in incarceration rates between natives and foreigners is solely the product of discrimination.

However, an important distinction needs to be made between official and unofficial immigrants. While detailed statistics on convicted foreigners disaggregated by legal status are not publicly available, the Italian Ministry of Internal Affairs (2007) claims that in 2006 legal immigrants represented about 6% of all individuals reported by the police to the judiciary authority, which is in line with the share of immigrants among the total population. The disproportionate incidence of foreigners in prison population is entirely due to undocumented immigrants, who account for 70% and 80% of foreigners reported for violent and property crimes, respectively.

Legal status thus seems to have profound implications for immigrants' criminal careers. There are several reasons why this may be the case. In particular, legal and illegal immigrants can face very different (legitimate) earning profiles, which in turn affects the opportunity cost of crime. Most likely, the administrative and penal sanctions (including the possibility of incarceration) faced by employers who hire undocumented immigrants, in addition to the risk of job destruction due to the worker's expulsion, adversely impact the demand for illegal immigrants (relative to those holding a valid residence permit). This effect may be amplified by selection into legal status. Far from being randomly distributed, legal status is strongly correlated with other individual characteristics that are known to be important determinants of criminal activity.

While information in this respect is not available for a representative sample of (legal and illegal) immigrants in Italy, survey evidence from a region in the northwest is consistent with this conjecture. Starting in 2001, the non-governmental organization *Iniziative e Studi sulla Multietnicitá* (ISMU) has conducted yearly interviews on a sample of about 9,000 immigrants in the Lombardy region. The data contain information on labor market outcomes, along with several individual characteristics, including legal status. A sampling of illegal immigrants is attained through the "center-sampling technique" devised by the ISMU statistical team in the early 1990s. The main idea is to exploit social networks in

the foreign population and base sampling on a number of "aggregation centers" that are attended by both legal and unauthorized immigrants (care centers, meeting points, shops, telephone centers, etc.), as opposed to administrative sources that cover only official residents. The methodology is described at length in Blangiardo et al. (2004) and Blangiardo (2008).[4]

Table 1 compares the characteristics of legal and illegal immigrants in the 2006 round of the survey (i.e., immediately before the EU enlargement of 2007). It turns out that unauthorized immigrants are, on average, younger and less educated, are disproportionately single males, and have fewer children. Most importantly, they tend to be employed in lower-skill occupations and earn significantly lower wages. In addition, the wage gap is relatively more severe among the highly skilled.[5]

While restrictions imposed by migration laws on the employment of illegal immigrants certainly explain part of the wage gap, the striking differences in other observable characteristics point to the importance of selection in legal status. In particular, immigrants can voluntarily self-select when deciding whether to comply or not with migration policy. For instance, individuals with better income prospects in the host country can exert greater effort in dealing with the bureaucratic difficulties imposed by (legal) entry procedures. On the other hand, selection can also be involuntary (on the part of the immigrant), as when less-skilled individuals have lower chances of receiving a job offer eventually entitling them to apply for a permit. Whatever the reasons, the differences reported in Table 1 suggest that legal status is not randomly distributed across immigrants, which considerably hampers the empirical identification of its effect on criminal behavior.

We next present a model of crime that is consistent with the stylized facts described above and which clarifies the main threats to identifying the causal effect of legal status on crime.

3 Theoretical framework

Consider a population of infinitely lived, risk-neutral prospective immigrants. If they decide to actually move to the destination country, they incur a travel cost T. In addition, official entry in the host country imposes an upfront cost B on legal immigrants (L); such a cost can include, for instance, the time and money spent to deal with paperwork, acquire health certifications, and pay head taxes. Alternatively, immigrants may decide to cross the frontier illegally. In this way, illegal immigrants (I) avoid the burden imposed by migration policy, but face the risk of being apprehended and deported back to their home

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[4]Using the ISMU data to estimate the determinants of immigrants' earnings in Italy, Accetturo and Infante (2010) examine the reliability of the information on legal status by comparing the results of the 2002 survey with the demands for amnesty presented the same year, concluding that the extent of undersampling of unauthorized immigrants and/or misreporting of legal status is modest.

[5]Drawing on several rounds of the ISMU survey, Accetturo and Infante (2010) confirm these findings in a multivariate regression analysis. Extensive empirical evidence on the wage gap suffered by illegal immigrants is available also for the United States (see e.g. Bratsberg et al., 2002; Kossoudji and Cobb-Clark, 2002; Kaushal, 2006).

country at the beginning of any subsequent period.

Once in the host country, both legal and illegal immigrants may engage in crime. Criminal activities deliver an immediate payoff z, which is randomly distributed across agents in each period according to the cumulative density $F(z)$; however, those committing a crime are arrested and sent to jail in the following period with probability π. If we assume a constant discount factor $\rho < 1$, the expected utility of seizing a crime opportunity z for immigrants of type $k = I, L$ is then

$$C_k(z) = z + \rho\left[\pi P + (1-\pi)EV_k\right], \tag{1}$$

where P is the utility associated with incarceration, V_k is the utility when not in prison, and E denotes expectations over (future) values of z; without loss of generality, we can impose P equal to zero.

Apart from criminal proceeds, immigrants have access to labor earnings that vary with individual skills and the returns to skills for different groups of immigrants in the labor market. In particular, letting h denote the (heterogeneous) level of human capital, the wage of each immigrant is $w_L h$ if he or she holds a residence permit, and $w_I h$ otherwise, with $\Delta w = w_L - w_I \geq 0$.[6] While the strict inequality would be consistent with the empirical evidence discussed in the previous section, the conservative assumption that w_L is no lower than w_I is sufficient for all the results that follow.

The utility of legal immigrants is

$$V_L = \max\left\{C_L(z); \rho EV_L\right\} + w_L h, \tag{2}$$

which depends both on the decision about criminal activity (the first term on the right-hand side of equation 2) and on legitimate income (the second term). The utility of illegal immigrants is similar, except for the fact that, with probability $\delta > 0$, they are apprehended and deported back to their origin country:

$$V_I = \delta V_H + (1-\delta)\left[\max\left\{C_I(z); \rho EV_I\right\} + w_I h\right], \tag{3}$$

where V_H is the utility in the home country, which depends, positively, on the labor market income $w_H h$, and we posit that $w_H < w_I \leq w_L$.[7] Henceforth, we assume for simplicity that those expelled from the country do not try to migrate anymore.

Individuals face three decisions: whether to migrate or not; in case they do migrate,

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[6]From now on the notation Δ will always refer to the difference between the (potential) legal and illegal outcomes of an individual.

[7]Since in most cases human migration is an economic phenomenon, it seems natural to assume that wages in the destination country are higher than in the origin country. In our formulation, this occurs through higher returns to human capital, which may be at odds with standard factor proportion explanations of migration. On the other hand, this assumption is consistent with more recent models stressing the effect of skill-biased technical change on labor market outcomes in destination countries, which, in turn, affects the relative returns of more- and less-educated migrants (Acemoglu, 2002, provides a survey). In addition, the assumption is consistent with extensive empirical evidence on the positive selection of immigrants (see Grogger and Hanson, 2010, for a recent study).

whether to cross the frontier legally or illegally; and, finally, once in the host country, whether to accept or reject the crime opportunities available in each period. The latter decision is at the core of the economic model of crime first proposed by Becker (1968), in which individuals choose whether or not to engage in crime, depending on the relative return of legitimate and illegitimate activities.[8] Admittedly, our framework is very stylized in this respect, reducing such a problem to a discrete choice between crime and lawfulness. In this way, we prevent continuous time allocation choices between legitimate and illegitimate activities (Grogger, 1998), as well as ex ante investments in human capital (Lochner, 2004). However, these simplifications are inconsequential for the empirical analysis, given that our data do not contain such information. The present model captures the institutional features of Italian migration policy that are common to most other countries, namely, that legal aliens face a substantial bureaucratic and economic burden upon entry and that illegal aliens can be deported back to their origin country. We next describe in greater detail the trade-offs involved in each decision and solve the problem backward, starting with the choice about criminal activity.

3.1 Criminal behavior

In deciding whether or not to engage in criminal activity, each individual of type $k = I, L$ compares the expected returns from criminal activity, $C_k(z)$, with its opportunity cost, ρEV_k. Since $C_k(z)$ depends, positively, on the value z of illicit income opportunities available in each period (while ρEV_k does not), there must exist a reservation value z_k^* such that each individual of type k commits a crime if and only if $z \geq z_k^*$.[9] Imposing crime's expected payoffs equal to its opportunity cost for both legal and illegal immigrants, $C_k(z_k^*) = \rho EV_k$ $(k = I, L)$, and substituting into (1) delivers the reservation values

$$z_k^* = \rho \pi EV_k. \tag{4}$$

Conditional on legal status, the reservation value completely characterizes criminal behavior. In particular, the probability of committing a crime for legal immigrants simply equals the probability of receiving a crime opportunity worth more than z_L^*,

$$c_L = 1 - F(z_L^*). \tag{5}$$

For illegal immigrants, we must first condition the probability of committing a crime on the risk of deportation,

$$c_I = (1 - \delta) \left[1 - F(z_I^*) \right]. \tag{6}$$

The log probability of committing a crime for each individual, conditional on legal

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[8]See also Ehrlich (1973), Grogger (1998), and Machin and Meghir (2004) for later developments; Burdett et al. (2003), Lochner (2004), and Lee and McCrary (2009) provide extensions in dynamic settings that are most similar to ours.

[9]Note the analogy with the notion of "reservation wage" commonly adopted in equilibrium search models of labor (see Rogerson et al., 2005, for a survey).

status, can be written compactly as

$$\ln c(h) = \ln c_I(h) + \beta(h)L,$$

where $L = 1$ if the immigrant is legal and $L = 0$ otherwise, and $\beta(h) \equiv \Delta \ln c(h)$ is the causal effect of legal status conditional on h. Using equations (5) and (6), we obtain

$$\beta(h) \approx \delta - [F(z_L^*) - F(z_I^*)]. \tag{7}$$

The sign of (7) depends on two effects. On the one hand, holding constant the propensity to engage in criminal activity, the deportation of illegal aliens (at rate δ) lowers the number of crimes they commit relative to legal immigrants. This is the *incapacitation* effect of migration restrictions, which is apparent from the first term on the right-hand side, moving the crime rate upward after the removal of migration restrictions. On the other hand, the propensity to criminal behavior also changes with legal status, because different labor market opportunities for legal and illegal immigrants entail a difference in terms of the opportunity cost of crime. This second effect, which is apparent from the last term of the equation (i.e., the change in the probability of accepting a crime opportunity for formerly unofficial immigrants), increases with the wage premium to legal status, $\Delta w \geq 0$. However, its sign and strength, as well as the direction of the overall effect in (7), depend crucially on the equilibrium distribution of h across legal and illegal immigrants, which we examine next.

3.2 Equilibrium

The remainder of this section provides a graphical representation of the equilibrium and the intuition behind all results; formal proofs are presented in the Appendix. First note that $w_H < w_I$ implies that there exists a threshold for h above which individuals decide to leave the origin country. In particular, Figure 5a shows that all individuals characterized by $h \geq h_I$ prefer to unofficially cross the frontier rather than stay home.

What about the option of entering the destination country by complying with migration policy? Note that, once in the host country, all immigrants prefer to be legal rather than illegal, that is, $E\Delta V = E(V_L - V_I) > 0$. By a simple revealed preference argument all those willing to (illegally) migrate prefer to live in the destination country than in the origin country; therefore, these same individuals are better off avoiding the risk of being deported. Moreover, the differential $E\Delta V$ increases with h; intuitively, better labor market opportunities in the destination country mean a greater utility loss in case of expulsion.[10]

After arrival, legal immigrants are thus better off holding a valid residence permit; however, upon arrival, they must bear the entry cost B, so they apply for legal status if and only if $E\Delta V \geq B$. Since $E\Delta V(h)$ is upward sloping, the latter condition must hold

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[10]The Appendix presents the formal proof that $E\Delta V \geq 0$ and $\frac{\partial E\Delta V}{\partial h} \geq 0$ for $h \geq h_I$.

beyond some threshold h_L (see Figure 5b). Figure 5b clearly illustrates the self-selection of immigrants at the frontier: Individuals in the upper tail of the distribution of skills comply with migration policy, while those in the intermediate range $[h_I, h_L]$ enter the country unofficially.

Therefore, labor skills differentiate between immigrants and non-immigrants, and, among the former, between legal and illegal entrants. Figure 5c then shows how the equilibrium distribution of h determines within- and between-group differences in criminal behavior. The probability of committing a crime for legal immigrants, c_L, is inversely related to the reservation value z_L^*, which, in turn, is proportional to the expected utility EV_L. The same is true for illegal immigrants, *conditional on not being deported.* Therefore, $EV_L(h) > EV_I(h)$ implies that, conditional on h, unauthorized immigrants who are not deported commit more crimes than legal immigrants: $\tilde{c}_I = 1 - F(z_I^*) \geq 1 - F(z_L^*) = c_L$. However, the unofficial population includes also those who are deported at the beginning of each period (before committing a crime), so deportation shifts the crime rate for this group down from $\tilde{c}_I$ to c_I.

The causal effect of legal status on crime is then the average difference between the curves $\ln c_L$ and $\ln c_I$ over the interval $[h_I, h_L]$,

$$\beta \equiv E\left[\beta(h)|h_I \leq h \leq h_L\right] = E\left[\ln c_L(h)|h_I \leq h \leq h_L\right] - E\left[\ln c_I(h)|h_I \leq h \leq h_L\right]. \quad (8)$$

The sign of (8) is a priori ambiguous, depending upon whether the average (relative) reduction in criminal activity by formerly unofficial immigrants after the concession of legal status, $E\left[(F(z_L^*) - F(z_I^*))|h_I \leq h \leq h_L\right]$, is strong enough to counterbalance the increase in crime brought about by the end of deportations. Therefore, determining the effect of legal status on criminal activity is ultimately an empirical issue.

3.3   Identification

Given data on criminal activity for a sample of legal and illegal immigrants, the main threat to empirically identifying β is that each individual is commonly observed in only one state (either with or without legal status), so the first term on the right-hand side of equation (8), namely, the (counterfactual) log probability of committing a crime for illegal immigrants conditional on obtaining legal status, is not observed (in the terminology of Rubin, 1974, it is the "potential outcome"). Omitting this element, one can alternatively conduct a naive comparison between legal and illegal immigrants,

$$\tilde{\beta} = E\left[\ln c_L(h)|h_L \leq h\right] - E\left[\ln c_I(h)|h_I \leq h \leq h_L\right];$$

however, $E\left[\ln c_L(h)|h_L \leq h\right] \leq E\left[\ln c_L(h)|h_I \leq h \leq h_L\right]$ (because crime decreases with h), so $\tilde{\beta}$ would provide a downward-biased estimate of β,

$$\tilde{\beta} = \beta + \underbrace{E\left[\ln c_L(h)|h_L \leq h\right] - E\left[\ln c_L(h)|h_I \leq h \leq h_L\right]}_{SELECTION\ BIAS} < \beta$$

11

(see the last diagram in Figure 5).

The last round of the EU enlargement provides an exogenous source of variation in legal status that allows us to remove the selection bias. The next section describes in detail this quasi-experimental setting.

4   The natural experiment

4.1   The EU enlargement

With the fall of the Eastern Bloc and the EU enlargement toward the east, immigrants from Central and Eastern Europe became a large and growing share of total inflows in Italy (see Figure 2). A first round of the enlargement took place in 2004, with the admission of the Czech Republic, Estonia, Cyprus, Latvia, Lithuania, Hungary, Malta, Poland, Slovenia, and Slovakia. Then, on January 1, 2007, Bulgaria and Romania also joined the EU. The process of enlargement is far from over, since several countries are now "candidate members" of the EU. In particular, Croatia, Turkey, and the former Yugoslavian Republic of Macedonia are already negotiating admission conditions, while such negotiations should start soon for Albania, Bosnia and Herzegovina, Kosovo, Montenegro, and Serbia. New member and candidate member countries are shown in Figure 6.

Article 39 of the European Commission Treaty allows, in principle, citizens of new member countries to i) look for a job in any other country within the EU, ii) work there without needing any permit, iii) live there for that purpose, iv) stay there until the end of the employment relationship, and v) enjoy equal treatment with the natives in access to employment, working conditions, and all other social and tax advantages that can help integration in the host country. In practice, however, several countries in Europe maintained significant restrictions to the free movement of immigrants from new member countries.

The application of the EU directives was at the center of a heated public debate in Italy until the very last weeks before the enlargement, mostly because of the alleged impacts on crime. However, in the end (on December 28, 2006) the center-left government led by Romano Prodi guaranteed free movement to all new EU citizens and completely liberalized access to the labor market in the following sectors: agriculture, hotel and tourism, managerial and highly skilled work, domestic work, care services, construction, engineering, and seasonal work. These sectors account for the bulk of foreign employment, both before and after the enlargement. In the rest of the official economy (basically the manufacturing sector), migration quotas were also eased to accommodate the larger number of workers from Romania and Bulgaria.

The removal of migration restrictions led to sharp changes in the composition of the foreign population in Italy. The left-hand graph of Figure 7 compares the number of (official) residents from new member and candidate member countries before and after the EU enlargement. Until 2006, the combined size of the Romanian and Bulgarian

communities was about half of the other group, with the difference between the two remaining constant over the period. Then, in the wake of admission to the EU, the number of Romanians and Bulgarians officially residing in Italy nearly doubled, while the size of the other group continued to grow at approximately the same rate as in previous years.

A similar pattern arises among individuals arrested by the Italian police between 2006 and 2007. However, the (differential) increase was much less pronounced in this case, so the ratio of arrested to total official residents actually declined for Romanians and Bulgarians, while no significant change is observed for the control group (see the right-hand graph in Figure 7). At first sight, one might be tempted to conclude that the removal of migration restrictions favored a decline in criminal activity. Yet, the increase in Romanians and Bulgarians between 2006 and 2007 includes both inflows from abroad and acquisitions of legal status by (formerly unofficial) immigrants already in Italy, the two components being undistinguishable from each other. Figure 7 would indeed be consistent with a decline in the crime rate if the sharp increase in the left-hand graph were driven largely by inflows of new immigrants after the EU enlargement. On the other hand, if it were caused by changes in the legal status of foreigners already residing in Italy before 2007, the decline in the incidence of arrests would be due to the fact that formerly unofficial immigrants impact, positively, on the total official population only after 2007, but affect the number of crimes both before and after that period.

One way to circumvent this problem is to focus on a sample of immigrants who were already present in Italy before the enlargement.

4.2   The July 2006 Collective Pardon

Italian collective pardons eliminate a part of the sentences, typically two or three years, of all prison inmates; then, all those whose residual sentence is less than that length of time are immediately released. In this way, pardons generate sudden releases of large numbers of inmates. The only ones excluded are Mafia members, terrorists, kidnappers, and sexual offenders, but even violent criminals such as murderers and robbers can be pardoned. Whenever a pardoned prisoner recommits a crime within five years, the commuted prison term gets added to the new term.

Collective clemency bills are deeply rooted in Italian history; over the last 40 years there has been on average a pardon every five years (Barbarino and Mastrobuoni, 2010). The last one was voted by the Italian Parliament in July 2006 and enacted shortly thereafter (on August 1). About 22,000 individuals, corresponding to more than one-third of the total prison population, were freed within a few days. More than 8,000 of them were foreigners, reaching 9,642 by the end of 2006.

We were granted access to the criminal records from August 2006 through December 2007, of all prison inmates released with the 2006 collective clemency bill.[11] The most

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[11]These data are similar to those used by Drago et al. (2009) to study the deterrent effect of residual

13

important information for our purposes is the nationality and date of rearrest (if any). Figure 8 shows that, among foreigners, a large number of pardoned inmates were rearrested over the following year and a half. In particular, 795 individuals were back in jail by the end of 2006, before the EU enlargement, growing to 1,654 one year later, after the EU enlargement. The main idea behind our empirical strategy is then to exploit differences in the probabilities of rearrest before and after the EU enlargement across different nationalities in our sample.[12]

5 Empirical strategy

In this section we devise a difference-in-differences estimator that exploits the unique characteristics of our sample to estimate the effect of legal status on the probability of committing a crime. If legal status does indeed affect the criminal behavior of immigrants, we should observe a change in such a probability for Romanians and Bulgarians after the EU enlargement. In terms of our theoretical model, the average log probability for this group in 2006 (before the policy change) is

$$E\left[\ln c(h)\right] = E\left[\ln c_I(h)|h_I \leq h < h_L\right]G(h_L) + E\left[\ln c_L(h)|h_L \leq h\right]\left[1 - G(h_L)\right], \quad (9)$$

where $G(.)$ is the cumulative density of h among the immigrant population in the host country.[13] Then, in 2007 (after the enlargement), everybody in this group obtains legal status, so

$$E\left[\ln c'(h)\right] = E\left[\ln c'_L(h)\right]. \quad (10)$$

Subtracting (9) from (10) yields the change after the extension of legal status to all (formerly illegal) immigrants from new EU member countries,

$$E\left[\ln c'(h) - \ln c(h)\right] = \beta G(h_L) + \Psi, \quad (11)$$

where Ψ is the (unobserved) counterfactual change between the two periods, absent the policy shock,

$$\begin{aligned}\Psi = \quad & E\left[\ln c'_I(h) - \ln c_I(h)|h_I \leq h < h_L\right]G(h_L) \\ & + E\left[\ln c'_L(h) - \ln c_L(h)|h_L \leq h\right]\left[1 - G(h_L)\right].\end{aligned} \quad (12)$$

Equation (11) clarifies which are the main estimating issues, examined next.

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sentences.

[12]While examining only former prisoners may, at a first sight, seem to limit the external validity of our results, it allows one to focus on the group of individuals who are at the margin between a criminal career and legitimate activity; at the opposite end, the great majority of the population at large never engages in any type of (serious) crime. For this and other reasons, previous offenders have been widely studied in the empirical literature on crime (see, e.g., Witte, 1980; Lee and McCrary, 2009).

[13]Letting $\Gamma(h)$ denote the cumulative distribution of h over the entire population in the origin country (including migrants and non-migrants), $G(h) = 0$ for $h \leq h_I$ and $G(h) = \frac{\Gamma(h) - \Gamma(h_I)}{1 - \Gamma(h_I)}$ for $h > h_I$.

5.1 The control group

To isolate any causal effect β from common trends and time-specific shocks in Ψ, we rely on a control group that is as close as possible to Romanians and Bulgarians, except for the change in legal status between 2006 and 2007. Pardoned inmates from candidate EU member countries naturally provide such a control group.

As described in the previous section candidate members are countries that have either started access negotiations or that are going to do so in the near future. Therefore, this group of countries should be most comparable to new EU members along the economic and political criteria required for admission to the EU. As a matter of fact, they all belong to the same geographical area, and most of them (with possibly the exception of Turkey) share a great deal of linguistic, cultural, and historical heritage.

Our sample includes about 800 Romanians and Bulgarians, as well as 1,800 immigrants from candidate member countries; restricting it to the subsample of males (to reduce heterogeneity), we are left with 725 treated and 1,622 control individuals. Using the change in crime for the control group between 2006 and 2007 to estimate the right-hand side of equation (12) and substituting into (11), we obtain

$$\beta G(h_L) = E\left[\ln c'(h) - \ln c(h)\right] - E\left[\ln c'_0(h) - \ln c_0(h)\right], \tag{13}$$

where the subscript 0 denotes the control group.

Equation (13) constitutes the basis for our empirical analysis. Moving to its sample analog requires addressing some important measurement issues.

5.2 Measurement

The first issue concerns the measurement of criminal activity, which remains partly unobserved. This happens either because of the under-reporting of criminal offenses or because, even for recorded offenders, identities cannot always be determined. One observes incarceration, though, which is often used as a proxy for unobserved criminal activity (see, e.g. Ehrlich, 1996; Levitt, 1996). Consistent with this approach, our model maintains that the probability of being arrested after committing a crime is constant and equal to π, which implies that $\frac{\pi c_L(h)}{\pi c_I(h)} = \frac{c_L(h)}{c_I(h)}$. It follows that the relative log probability of incarceration for legal and illegal immigrants is exactly equal to the relative log probability of committing a crime, which remains unobserved. Since the former is observable, we use it as our main dependent variable. The confounding effect of departures from this assumption will be discussed as we present the empirical results.

The second measurement issue concerns the term $G(h_L)$, namely, the fraction of illegal immigrants in the (prison) population. Since illegal status is not by itself a valid reason for incarceration, administrative criminal records do not report the legal status of the individuals in our sample. Nevertheless, a valid estimator for the right-hand side of equation (13) would still be informative about the sign of β and would bound its magnitude from below.

Moreover, as already pointed out in Section 2, the fraction of illegals is very high (between 70% and 80%) among the foreign prison population, so the attenuation bias induced by $G(h) < 1$ is going to be small.

5.3 Propensity score weighting

Longitudinal data on the (re)arrests of former inmates from new EU member countries and candidate member countries over the period 2006-2007 allow us to estimate equation (13) using the difference in differences between the log probability of arrest for the two groups before and after the EU enlargement. The identifying assumption is that in the absence of the policy shock, the hazard rates for the two groups would have followed parallel paths. Such a condition may be implausible in the presence of significant differences in individual characteristics that are possibly correlated with criminal activity (Abadie, 2005).

The left-hand columns of Table 2 compare the two groups in terms of the observable characteristics reported in our data, namely, age, gender, marital status, and education, the latter being available only for a very restricted subsample; the type of crime for which the individual was first incarcerated before the pardon (possibly more than one, such that the group means of economic and violent crimes do not add up to one); and, finally, the length of sentence and amount of time commuted with the pardon.[14] While marital status is not significantly different, Romanians and Bulgarians appear to be on average younger (31 versus 33 years of age) and more educated than individuals in the control group; they are also more (less) likely to commit violent (economic) crimes but, despite this, they are given lighter sentences. One reason may be that migration waves from such countries are more recent, whereas immigrants from some countries in the control group (primarily Albanians) arrived earlier and are more likely to have recidivated.

If we assume that deviations from the "parallel paths" depend solely on differences in observable characteristics, conditioning on such differences removes all biases. This assumption, which is alternatively referred to as unconfoundedness, conditional independence, selection on observables, or ignorability of treatment, constitutes an important special case in the econometrics of program evaluation (Imbens and Wooldridge, 2009). While unconfoundedness may be a strict requirement, note that we are imposing it on *changes* over time in the crime rate (as opposed to *levels*); that is, we allow for (time-invariant) differences between groups to persist even after conditioning on observable characteristics. Moreover, the availability of longitudinal data over repeated periods before the policy change provides us with the opportunity to investigate the plausibility of unconfoundedness.

One simple way of adjusting for differences between the two groups is to weight observations based on the *propensity score* of assignment, that is, the conditional probability of belonging to each group, conditional on observed covariates.[15] Specifically, we weight

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[14]The data also report the prison from which the individual was released (167 institutions in total), which will be used later in the analysis.

[15]Rosenbaum and Rubin (1983) show that, under unconfoundedness, conditioning on the propensity score

each unit by

$$new\ EU_i \frac{p}{P(X_i)} + (1 - new\ EU_i) \frac{1-p}{1-P(X_i)}, \tag{14}$$

where $new\ EU_i$ is a dummy equal to 1 if the i-th individual is a citizen of a new EU member country, and 0 otherwise; p is the unconditional probability of belonging to the $new\ EU$ group; and $P(X_i)$ is the same probability conditional on the vector of individual characteristics X_i.

The weighting scheme (14) enhances the comparability between the two groups by attaching greater (lower) weight to units that are less (more) different from the other group relative to the average individual in the sample.[16] As is generally the case in non-experimental settings, the propensity score is unknown, so we estimate it by exploiting all the information available in our data set. In practice, we compute the predicted propensity score based on a logit regression of an indicator variable for being Romanian or Bulgarian on the following vector of covariates: a quadratic polynomial in age, marriage status, education (indicator variables for illiteracy and primary and secondary school, as well as missing information on education), the type of crime committed when first incarcerated (seven categories), a quadratic polynomial in the sentence and the commuted sentence, and, finally, a full set of fixed effects for the region of the prison from which the individual was released.[17] Figure 9 shows the distribution of the estimated propensity score by group. As expected, there is a large tail of individuals in the control group whose estimated propensity score is close to zero, meaning they are very different (in terms of observable characteristics) from Romanians and Bulgarians. Then, the weighting scheme will discount these observations, while attaching greater importance to the observations of both groups that lie in the middle range of the distribution. The right-hand columns of Table 2 show that weighting observations according to (14) actually eliminates differences in average group characteristics. We next examine the extent to which this adds to the credibility of unconfoundedness by investigating pre-enlargement differences in outcomes, which depend on both observable and unobservable characteristics.

5.4   Preliminary evidence

Figure 10 plots nonparametric estimates of the daily log hazard rates of rearrest of inmates from new EU member (solid line) and candidate member countries (dashed line).[18] Since we are particularly interested in the effect of legal status through legitimate earn-

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and conditioning on the full set of covariates are equivalent methods of guaranteeing the independence of potential outcomes across groups. One advantage of using the propensity score is that it reduces the dimensionality of the conditioning problem (from a multidimensional vector to a scalar).

[16]The idea of using the propensity score to weight observations was pioneered by Hirano et al. (2003); Abadie (2005) extends the same approach to difference-in-differences estimators.

[17]We dropped a few observations for which certain covariates other than schooling were missing.

[18]The hazard rate is the probability of being rearrested at each period at risk τ, conditional on not having been rearrested in the $\tau - 1$ periods before. For the sake of graphical illustration, we restrict the sample to inmates released during the first week after the pardon (about 72% of the total sample), so that the duration at risk on the horizontal axis is the same for all individuals. The results presented in subsequent tables will be based instead on the total sample.

ing opportunities, we focus on individuals who were first arrested (before the pardon) for economically motivated crimes (mainly property- and drug-related offenses).

The left-hand graph of Figure 10 shows the results obtained before applying the weighting scheme. While Romanians and Bulgarians display greater recidivism during the first months after the pardon, the opposite is true after they obtain legal status. Turning to the plausibility of identifying assumptions, the evidence from the pre-enlargement period seems broadly consistent with the hypothesis of parallel outcomes (absent the policy change). Since we weight observations by the propensity score (right-hand graph of Figure 10), not only the dynamics but also the levels of the hazard rates are very similar between the two groups, which provides strong empirical support for the unconfoundedness of changes over time across groups. With regard to the effect of legal status, a few weeks before the EU enlargement the probability of incarceration started to decrease for Romanians and Bulgarians, continuing to do so through the first months of 2007.

To quantify such an effect, Table 3 cross-tabulates the hazard rate of rearrest for each group over the last two trimesters of 2006 and the first two trimesters of 2007, as well as the difference between the two groups in each period and the difference in differences across different periods. The left panel of Table 3 shows the results for economically motivated offenders. The second row confirms that, after weighting by the propensity score, the two groups exhibit an identical probability of incarceration before the policy change. In 2007 the hazard rate does not change significantly for the control group, but it decreases from 5.8% to 2.3% for Romanians and Bulgarians; as a result, the difference in differences is also negative and very similar in absolute value (3.2%). Instead, the effect is positive and smaller in magnitude for violent offenders.[19]

Table 3 also report both robust and bootstrapped standard errors. While bootstrapping will generally lead to valid standard errors and confidence intervals for propensity score weighting estimators (Abadie and Imbens, 2008), Busso et al. (2009) suggest that robust regression standard errors provide a good approximation as the number of observations grows large. This is confirmed in our case, since the two estimates of the standard errors are always extremely similar. For the sake of computational efficiency, inference on the multivariate regression models presented next will be based on robust (clustered) standard errors.

With regard to inference, the effect of the EU enlargement on the recidivism of economically motivated offenders from new EU member countries is statistically significant at conventional confidence levels, while the differences for violent crimes are not significantly different from zero.

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[19]Note that when we focus on violent offenders, we are restricted to individuals who were previously in prison for having committed *only* violent crimes. Instead, individuals who were reported for both economic and violent crimes are included among economically motivated offenders, because the latter type of crime was probably instrumental to the first one (e.g., an assault during a robbery).

5.5 Parametric and semiparametric models

To probe these results further, in the next section we fit parametric and semiparametric models for the probability of rearrest at each point in time. Specifically, the parametric model takes the logistic form

$$\ln\left(\frac{c_{it}}{1-c_{it}}\right) = new\ EU_i + post_t + \beta new\ EU_i \times post_t + x_i'\gamma + \theta_t, \qquad (15)$$

where c_{it} is the probability of observing the arrest of individual i at time t; $new\ EU_i$ and $post_t$ are fixed effects for the group of new EU member countries' citizens and the period after the EU enlargement, respectively; x_i is a vector of individual characteristics reported in our data set; and θ_t is a polynomial in (calendar) time. Therefore, in this setting $post_t$ captures any discontinuous change in the log odds of rearrest between 2006 and 2007 (besides the smooth trend θ_t), while β is the average differential effect for Romanians and Bulgarians after the EU enlargement.

To condition the probability of rearrest on not having being rearrested before, we follow Efron (1988) and estimate equation (15) on the weekly (unbalanced) panel of inmates who are at risk of rearrest during each period. Therefore, the cross section of observations for the first week includes all individuals released immediately after the pardon; the second week includes all those released until then and not rearrested in the first week, and so on. In this way we end up with 124,019 person-week observations for the subsample of economically motivated offenders.[20]

An alternative approach is to model the hazard rate (as opposed to the probability) of rearrest:

$$\ln \lambda_{it\tau} = new\ EU_i + post_t + \beta new\ EU_i \times post_t + x_i'\gamma + \lambda_\tau^0, \qquad (16)$$

where $\lambda_{it\tau}$ is the probability of being rearrested in period t, given that the individual has not been rearrested during the τ periods at risk before. Therefore, this class of models allows us to disentangle the effect of calendar time shocks (i.e., the EU enlargement) from that of time at risk (which varies across individuals, depending on the exact moment at which they were released). Equation (16) is the general form of proportional hazard models, which differ from each other with respect to the specification of the baseline hazard function λ_τ^0. The most flexible approach is to leave it unrestricted and estimate the coefficients of interest by partial maximum likelihood (Cox, 1972). Therefore, the semiparametric model trades off efficiency for flexibility relative to the fully parametric approach.

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[20]Lee and McCrary (2009) also adopt this strategy to estimate the probability of reincarceration, while Ashenfelter and Card (2002) apply the same methodology to study retirement choices.

6 Results

This section presents our estimates of equations (15) and (16), along with several robustness exercises and specification tests. Observations are weighted by the (inverse) propensity score, as described in the previous section, and standard errors are clustered by Italian region and country of origin to allow for within-network correlations in criminal activity (immigrant networks being defined on the basis of nationality and geographic proximity in Italy).

6.1 Baseline estimates

Table 4 reports the estimated coefficients and standard errors for the subsample of economically motivated offenders. The first columns show the results for the fully parametric model (15). Column (1) of 4 includes only group- and year-specific fixed effects besides the interaction between the two; in columns (2) and (3) we add a quadratic time trend and the vector of individual controls reported in our data, respectively; and in column (4) we adopt the most flexible parametric specification by controlling for a full set of weekly dummies. In line with the preliminary evidence in the previous section, the estimated coefficient of the interaction variable is negative and significantly different from zero (at the 95% confidence level). As to its magnitude, a decrease in the log odds of 73-74% (very stable through all specifications) is also in line with the (unconditional) change in the probability of rearrest reported in Table 3 (from 0.058 to 0.023). The coefficient changes only slightly when we move to the semiparametric Cox model in the last two columns of Table 4. Note also that the group fixed effect *new EU* is always close to zero and non-statistically significant, further strengthening the credibility of the main identifying assumption.[21]

Table 5 reports the results for violent offenders. Inference is much less precise in this case, because the sample size is much smaller.[22] Still, the point estimates are again consistent with the preliminary evidence in Table 3, namely, that violent crime *increases* after the policy change. Therefore, the lack of expulsions after the EU enlargement may have determined an increase in the number of crimes committed by this category of offenders, who may be less (or not at all) responsive to economic motives when engaging in criminal activity. At the opposite, access to legitimate income opportunities seems strong enough to move the crime rate in the opposite direction for the subsample of economically motivated offenders. We explore this mechanism in detail next.

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[21]As to the other explanatory variables, the residual sentence plays a deterrence role, in line with the results of Drago et al. (2009); marriage status also has a significant deterrence effect.

[22]For the same reason, we do not estimate the unrestricted specification, including the full set of week-specific fixed effects, for the subsample of violent offenders.

6.2 The role of labor market opportunities

In our theoretical framework, the overall effect of legal status depends on two opposite forces: on the one hand, the end of deportations of formerly unofficial immigrants (and some potential criminals among them) increases the number of crimes they commit in the host country; on the other hand, access to legitimate income opportunities raises the opportunity cost of crime. We next confront these predictions with the data, exploiting variations in the relative importance of these factors across different regions in Italy.

Specifically, the Italian Statistical Office (ISTAT) assigns eight regions each to northern and southern Italy, and four to central Italy. If we aggregate the Center region with the South, we obtain an area that is comparable to the North in terms of number of observations in our sample, but profoundly different in terms of economic and social development. As shown in Table 6, the North is in fact characterized by higher income and better labor market opportunities in the official sector. Six out of 10 regions in the Center-South fall into the "Objective 1" areas according to the EU classification (meaning, e.g., that their gross domestic product (GDP) per capita falls below 75% of the EU average). At the same time, a large share of the labor force in the Center-South resorts to employment in the unofficial sector; the relative size of the shadow economy is twice as large as in the North (last column of Table 6).[23]

The relative importance of the official and unofficial sectors determines in turn the income opportunities of legal and illegal immigrants, respectively. Therefore, we expect the increase in the opportunity cost of crime after obtaining legal status to be greater in the North.

On the other hand, the North and Center-South exhibit similar levels of enforcement of migration restrictions. In particular, if we estimate the number of illegal immigrants based on the applications presented during the last amnesty program (as previously explained in Section 2.2), we obtain similar ratios of unofficial over official immigrants in the two areas (see the bottom panel of Table 6). Therefore, the change in the probability of deportation for Romanians and Bulgarians after the EU enlargement should have been of similar magnitude for both the North and the Center-South.

In the end, if we compare the policy effect in the North relative to that in the Center-South, we keep approximately constant the change in incapacitation while increasing the strength of the economic channel (see also Figure 11). If the effect of legal status goes through the mechanism proposed in our theoretical framework, the magnitude of the (negative) change in the criminal activity of Romanians and Bulgarians should then be greater in the North.

This is exactly the evidence emerging from Tables 7 and 8. The unconditional change in the fraction of Romanians and Bulgarians rearrested in the North between 2006 and 2007 as well as the difference in differences relative to the control group are almost twice

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[23]Indeed, the differential between northern and southern Italy has been widely studied over the years, see for instance Eckaus (1961) and Helliwell and Putnam (1995).

as much as the average effect in the whole country. On the other hand, no significant (differential) effect is detected in the Center-South. These results are qualitatively unaffected by conditioning on common time trends and individual characteristics in the parametric and semiparametric models.

6.3   Structural breakpoint

The estimates presented so far detect a statistically significant decrease in the criminal activity of immigrants from new EU member countries relative to the control group after the EU enlargement. Moreover, variations across areas characterized by different relative income opportunities for legal and illegal immigrants are consistent with the channel proposed in our theoretical framework.

One question is whether such estimates really capture the effect of this policy change, as opposed to other events affecting the relative hazard rate of the two groups before and/or after that moment. To rule out this possibility, we run a placebo experiment at any possible date in our sample period to identify the most likely breakpoint in the behavior of the two groups, much in the spirit of the tests of Andrews (1993) for identifying structural changes with unknown breakpoints. If the most likely period is close to (distant from) the date of the EU enlargement, this would be evidence against (in favor of) spurious effects driving the results.

In practice, for each placebo date t^*, we estimate the following linear probability models on the sample of individuals at risk of incarceration in each period $t \leq t^*$ and $t > t^*$:

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\begin{aligned}
H_0 : d_{it}^* &= new\ EU_i + post_t^* \\
H_1 : d_{it}^* &= new\ EU_i + post_t^* + \beta new\ EU_i \times post_t^*,
\end{aligned}
\tag{17}
$$

where $post_t^* = 1$ after t^*, and $post_t^* = 0$ otherwise, and $d_{it}^* = 1$ if individual i was rearrested in period t, and $d_{it}^* = 0$ otherwise. Note that estimating β by ordinary least squares is an alternative way of calculating the differences in differences reported in Table 3. Then, for any possible breakpoint t^*, we compute the R-squared ratio between models H_1 and H_0 as a measure of the importance (in terms of explanatory power) of any differential change in criminal behavior between the two groups at date t^*.

The placebo estimated coefficients and R-squared ratio for Italy, as well as for the North and Center-South areas, are presented in the left-hand plots in Figure 12. The most likely breakpoint for Italy as a whole is December 12, which is indeed quite close to the date of the enlargement and consistent with immigrants rationally anticipating the policy change and modifying their behavior as uncertainty about the policy change gradually unravels (see Section 4.1).[24] When the same test is run separately for the North and Center-South, the estimated breakpoints are very similar (December 1 and December 7), but while for the North the additional explanatory power is considerable, for the Center-South the R-

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[24] A likelihood ratio test statistic based on Cox's proportional hazard model gives very similar results. See Ichino and Riphahn (2005) for a similar exercise.

22

squared increases by just a little. Moreover, in the latter case the difference-in-differences effect is positive. This may be explained by the absence of significant improvements of the income opportunities of formerly unofficial immigrants after obtaining legal status, so that in Center-South regions the incapacitation effect prevails for economically motivated offenders as well. It is also worth noting that the magnitude of the effect (gray line) is always higher when we let the data "choose" the breakpoint rather than fix it at January 1, 2007.

The right-hand plots in Figure 12 provide additional visual evidence in this respect, showing the discontinuity in criminal behavior at the estimated breakpoint. The plots are the predicted (daily) hazard rates of rearrest as a function of a third-order polynomial in time for Romanians and Bulgarians (solid line) and for the control group (dashed line), before and after the breakpoint. In line with the results in Tables 7 and 8, the discontinuity for the first group is particularly relevant in northern regions, reaching 8%, as opposed to no discontinuity at all for the control group.

One interpretation of the general increase in the magnitudes is that estimating the breakpoint addresses the measurement error induced by the fact that individuals will not, in general, stick to the official date of the policy change when adjusting their behavior. On the other hand, when we choose the breakpoint by maximizing the explanatory power of the difference in differences, specification search bias implies that the coefficient estimated using the same data will have a nonstandard distribution (Leamer, 1978). In particular, conventional test statistics too often reject the null hypothesis that the coefficient is equal to zero. For this reason, we stick to the (more conservative) estimates reported in the regression tables.[25]

6.4 Threats to identification

This section discusses some additional identification issues. One concern is that legal status affects the probability of being arrested and/or incarcerated, conditional on having committed an offense. For instance, immigrants found without documents may be carefully scrutinized and additional evidence may become available upon closer inspection. Also, conditional on the severity of charges, official immigrants may have easier access to sanctions other than institutionalization (such as, e.g., home detention). If this is the case, the reduction in incarceration that we observe may be explained by changes in the probability of ending up in jail, conditional on criminal behavior, as opposed to changes in criminal behavior itself.

While we cannot directly address this issue (because the conditional probability of

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[25]In a study on the dynamics of segregation, Card et al. (2008) address this source of bias by randomly splitting the sample and using different subsamples to estimate the breakpoint and the size of the change. However, their data set includes about 40,000 census tract observations from 114 metropolitan areas, so that even after splitting the sample into two-thirds and one-third subsamples for estimating the breakpoint and the coefficient, respectively, the authors end up with a reasonably large number of observations in both steps. On the other hand, with just over 2,000 individuals, we run into serious trouble in terms of statistical power.

incarceration remains unobserved), there are several reasons why we believe that it is possible to exclude these alternative scenarios on the basis of the available empirical evidence. First of all, changes in the probability of arrest and incarceration should matter *after* the EU enlargement, while all the tests identify the breakpoint *before* that date, which is more consistent with expectation-induced changes in (criminal) behavior. In addition, there is little or no reason for these alternative explanations to impact differentially in the North and Center-South regions, or among economic and violent offenders. Finally, as to the possibility of alternative sanctions to incarceration, the pardon status precludes all individuals in our sample from accessing this opportunity.

Another issue is that legal and illegal immigrants may be characterized by a different willingness to travel back to their origin country, since they retain the right to return to Italy at any subsequent moment. While we consider at risk all individuals released with the pardon, spending less time in Italy would decrease the probability of committing a crime there for Romanians and Bulgarians after the acquisition of legal status. While this would also imply a reduction in the crime rate of this group, the mechanism would be totally different from the one proposed in this paper. However, this alternative channel (like the ones discussed before) should also work mostly after the policy change, and, again, it should impact all of our sample similarly (regardless of the type of crime previously committed and the region of residence in Italy).

A related concern is that, after obtaining the right to free movement, immigrants may consider moving to other European countries that offer relatively better labor market opportunities to legal immigrants. In addition to the usual counterarguments in terms of breakpoint and heterogeneity of effects (which also hold true in this case), we note that migration to other EU states would not occur instantaneously; indeed, if this were really driving the change in reincarceration, the effect should be *increasing* over time, as more and more Romanians and Bulgarians exit from the pool at risk. Therefore, we can investigate the empirical relevance of this alternative explanation by examining the evolution of the differential effect over time. For this reason, we re-estimate the model (15), truncating the longitudinal dimension at each week after the EU enlargement. The results, shown in Figure 13, are remarkably stable over time, suggesting, if anything, that most of the action takes place in the first weeks after the enlargement, when migration to other countries probably plays little or no role.

One final issue concerns the possibility of interactions in crime between different communities of immigrants. In particular, the change in the criminal behavior of Romanians and Bulgarians after the EU enlargement could have affected the activities of the other individuals in our sample, thus "contaminating" the control group. In particular, substitution between the criminal activity of the two groups would bias our estimates upward, in that other immigrants would increase their criminal activity in response to the decrease by Romanians and Bulgarians. The opposite is true in the case of complementarity.

While interactions in crime raise formidable estimating issues (which we do not address in this paper), descriptive evidence seems supportive of the complementarity hypothesis.

24

Figure 14 plots the change (between 2006 and 2007) in the number of crimes committed by all Romanians in Italy against the same changes for (some of) the nationalities included in the control group, for different types of crime.[26] A positive correlation between the two clearly emerges. While we can hardly attach any causal interpretation to this finding, this correlation seems more consistent with the existence of complementarity in the criminal activities of the two groups (as opposed to substitution), in which case our estimates would be biased downward.

7 Conclusions

We use a natural experiment, namely, the last round of the EU enlargement, to identify the effect of legal status on immigrant crime. We provide a theoretical framework that illustrates the two main effects of legal status: On the one hand, it increases crime by precluding the deportation of potential foreign criminals; on the other hand, it lowers the propensity to engage in crime by providing immigrants with better income opportunities. Evidence from a sample of former prison inmates released in Italy a few months before the enlargement suggests that the second effect prevails. In particular, the probability of rearrest decreases by more than half after obtaining legal status as a consequence of the EU enlargement.

Besides the effect on the pool of undocumented immigrants already in the host country, the concession of legal status (either through subsequent rounds of the EU enlargement or through amnesties) is also likely to attract new immigrants: Indeed, thousands of Romanians and Bulgarians were standing along the European frontiers on New Year's Eve of 2007. It is then likely that changes in migration policy also affect the quantitative dimension of incoming flows, as well as their composition. Policy makers also need an estimate of the cost and benefits of these additional consequences, but this goes beyond the scope of this paper.

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[26]These data are not available for Bulgarians and some other small foreign communities in the control group.

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Appendix

This Appendix characterizes the expected utility functions EV_L and EV_I defined in Section 3, as well as the difference between the two, $E\Delta V = E(V_L - V_I)$.

Starting with EV_L, we subtract ρEV_L from both sides of equation 2 to obtain

$$V_L - \rho EV_L = \max\{C_L(z) - \rho EV_L; 0\} + w_L h.$$

Taking expectations with respect to z and recalling that $z_k^* = \rho\pi EV_k$ (from equation 4) yields

$$EV_L(1-\rho) = \int_{z_L^*}^{+\infty} (z - z_L^*)\, dF(z) + w_L h;$$

after integrating by parts,

$$EV_L(1-\rho) = \int_{z_L^*}^{+\infty} [1 - F(z)]\, dz + w_L h. \qquad (18)$$

Similarly, one obtains the expected utility of illegal immigrants as

$$EV_I(1-\rho) = \delta[V_H - \rho EV_I] + (1-\delta)\left[\int_{z_I^*}^{+\infty} [1 - F(z)]\, dz + w_I h\right]. \qquad (19)$$

Proof that $E\Delta V(h) \geq 0 \ \forall h \geq h_I$. By contradiction: Assume that $\exists h' \geq h_I$ such that $E\Delta V(h') < 0$, which implies (after combining 18 and 19)

$$\int_{z_L^*}^{+\infty} [1 - F(z)]\, dz + w_L h < \delta[V_H - \rho EV_I] + (1-\delta)\left[\int_{z_I^*}^{+\infty} [1 - F(z)]\, dz + w_I h\right]. \qquad (20)$$

Since $[V_H - \rho EV_I] < 0$ over $h \geq h_I$ and $\Delta w \geq 0$, a necessary condition for (20) to hold is that $z_L^* - z_I^* = \Delta z^* > 0 > E\Delta V$; but Δz^* and $E\Delta V$ having a different sign contradicts condition (4).

Proof that $\frac{\partial E\Delta V}{\partial h} \geq 0$. Again using equation (4), $\frac{\partial E\Delta V}{\partial h} \geq 0 \Leftrightarrow \frac{\partial z_L^*}{\partial h} \geq \frac{\partial z_I^*}{\partial h}$; also, equations (18) and (19) become, respectively,
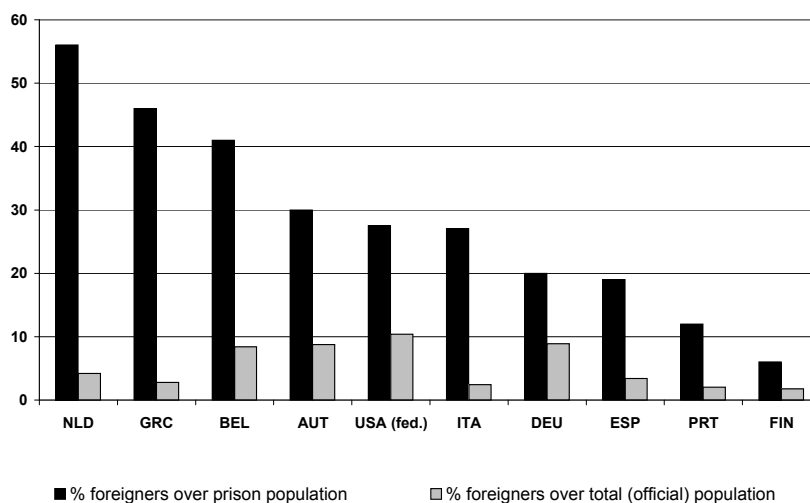
$$z_L^*\left[\frac{1 - \rho(1-\delta)}{\pi\rho}\right] = \int_{z_L^*}^{+\infty} [1 - F(z)]\, dz + w_L h$$

$$z_I^*\left(\frac{1-\rho}{\pi\rho}\right) = \delta V_H + (1-\delta)\left[\int_{z_I^*}^{+\infty} [1 - F(z)]\, dz + w_I h\right].$$

Applying the implicit function theorem and the Leibniz rule, we obtain

$$
\begin{aligned}
\frac{\partial z_L^*}{\partial h} &= \frac{\pi \rho w_L}{1 - \beta + 2\pi \rho \left[1 - F\left(z_L^*\right)\right]} \\
\frac{\partial z_I^*}{\partial h} &= \frac{(1 - \delta)\,\pi \rho w_I}{1 - \beta\left(1 - \delta\right) + 2\pi \rho\left(1 - \delta\right)\left[1 - F\left(z_I^*\right)\right]}.
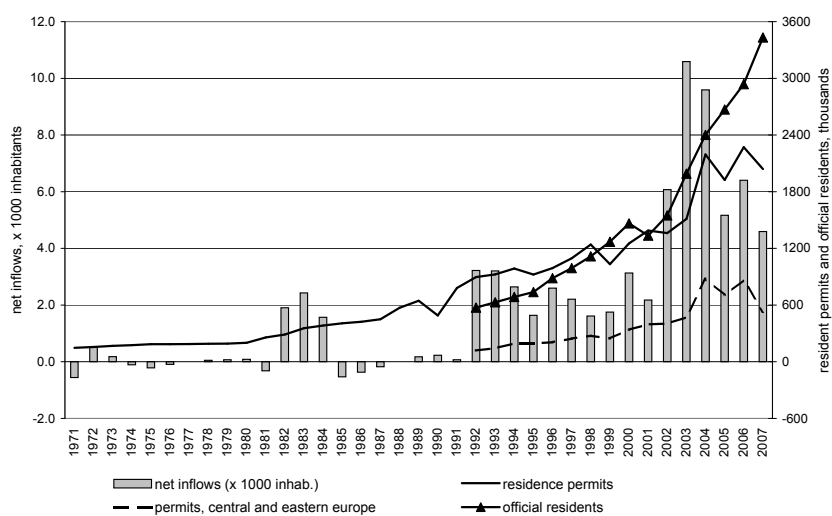\end{aligned}
$$

Then, a sufficient condition for having $\frac{\partial E \Delta V}{\partial h} = \frac{1}{\pi \rho} \frac{\partial \Delta z^*}{\partial h} \geq 0$ is that $\Delta z^* = \pi \rho E \Delta V \geq 0$, which is always true over the interval $h \geq h_I$.

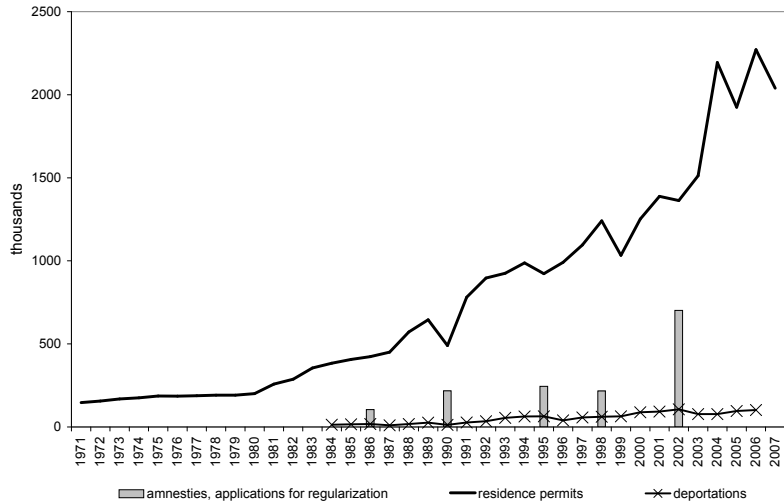Figure 1: Share of foreigners among the total and prison populations.

Figure 2: Legal immigrants.

Note: This figure shows net migration inflows in Italy, as well as foreign official residents and valid residence permits during the period 1971-2007. Source: ISTAT and the Ministry of Interior.

Figure 3: Illegal immigrants.

Note: This figure shows the number of valid residence permits (since 1971), applications for the regularization of formerly unofficial immigrants during the amnesty programs (1986, 1990, 1995, 1998, 2002), and the number of deportations of undocumented immigrants over the period 1984-2006. Source: Ministry of Interior.

Figure 4: Prison inmates.
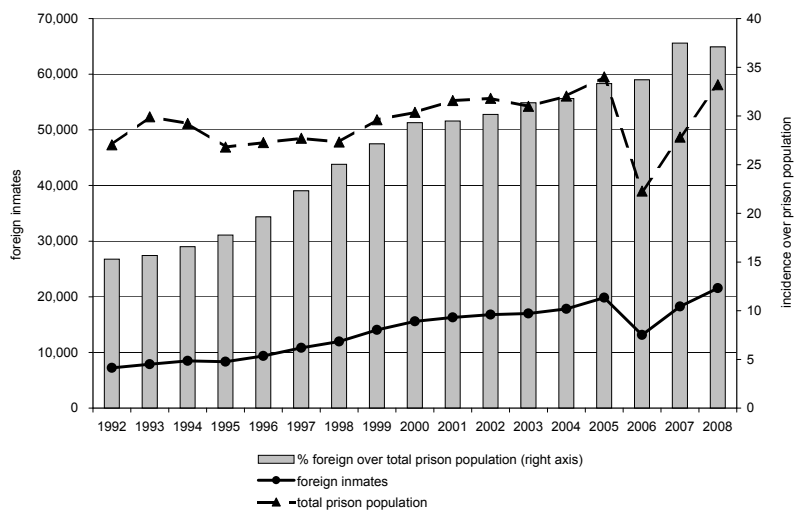
Note: This figure plots the number of native and foreign prison inmates in Italy during the period 1992-2008, as well as the incidence of foreigners in the total prison population. Source: Ministry of Justice.

Figure 5: Theoretical model.
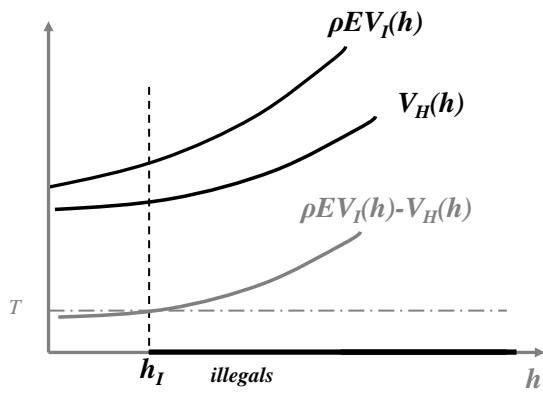

Figure 5a: illegal immigrants
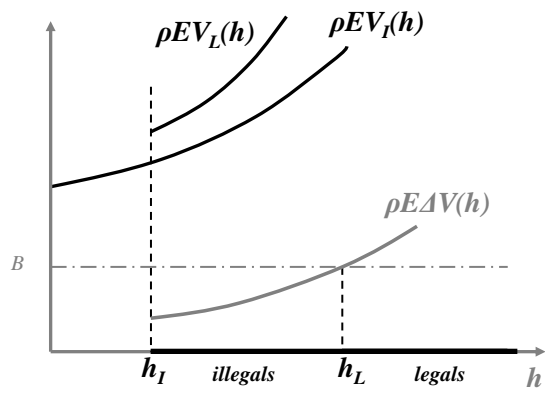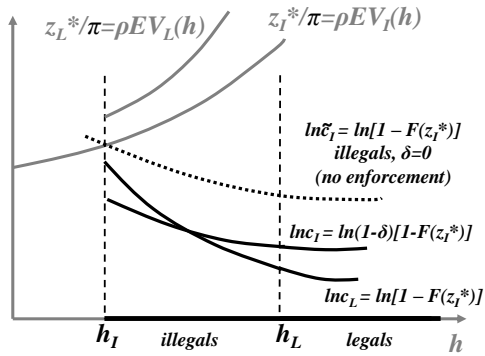
Figure 5b: self selection into legal status
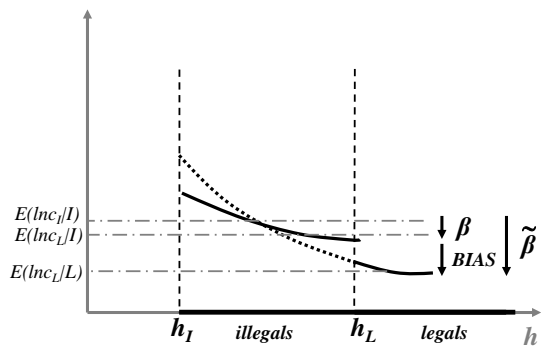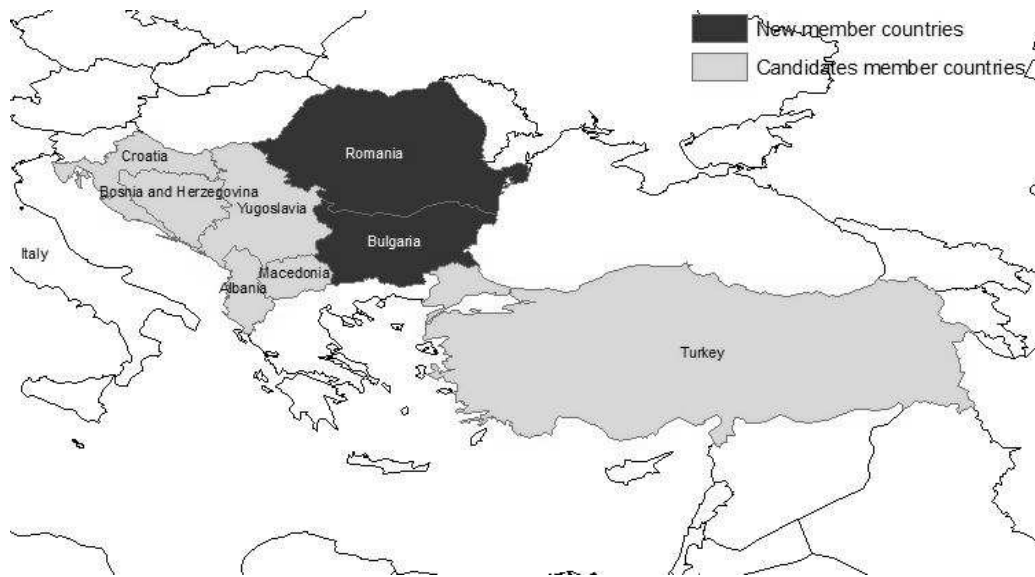
Figure 5c: criminal behavior

Figure 5d: selection bias

34

Figure 6: New EU member and candidate member countries.

Note: The map shows the countries admitted to the EU during the last round of the enlargement (in black), as well as the group of candidate member countries (in gray). Source: European Commission.

Figure 7: Immigrants in Italy and new EU member and candidate member countries.

official residents in Italy (thousands) — people reported by the police, x 100k residents

Romanians & Bulgarians — — — EU candidate countries — Romanians & Bulgarians — — — EU candidates (Albania & Serbia-Mont)
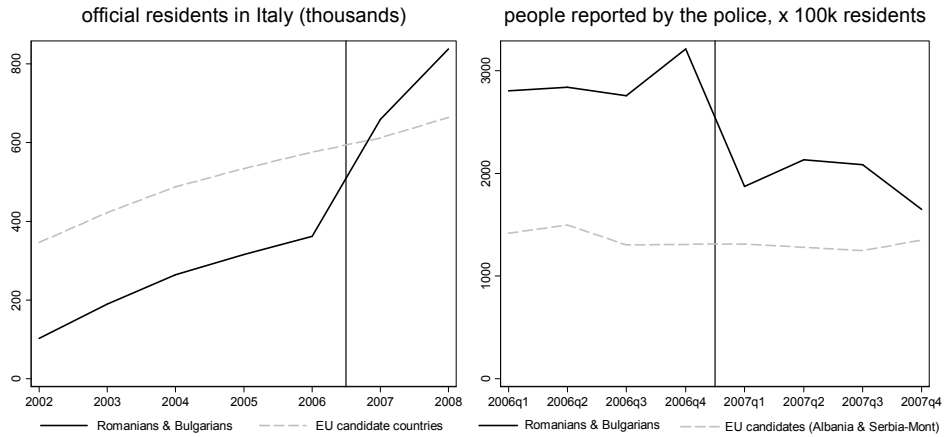
Note: The left-hand graph plots the number of citizens of new EU member and candidate member countries officially residing in Italy during the period 2002-2008. The right-hand graph shows the ratio of those arrested by police to official residents in each quarter during the period 2006-2007. In both graphs the vertical line refers to the date of the last EU enlargement. Source: ISTAT and Ministry of Interior.

Figure 8: The Collective Clemency Bill.

released — rearrested
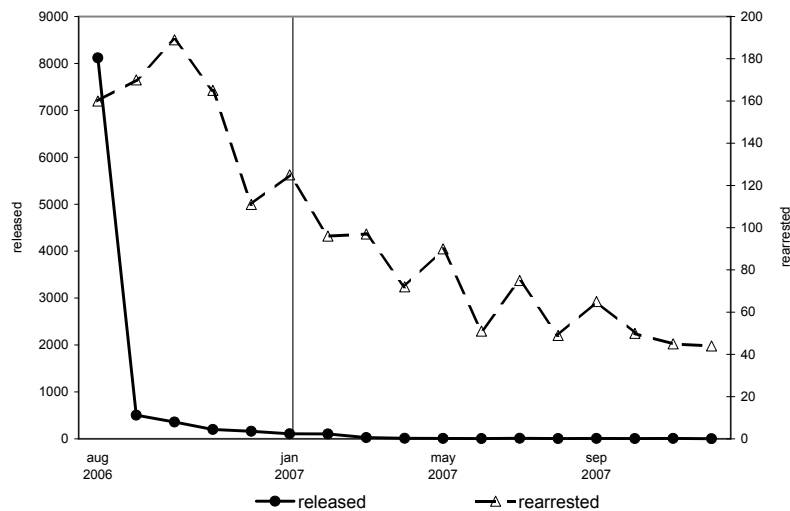
Note: This figure plots the number of foreign prison inmates released after the collective clemency bill in August 2006 (on the left axis), as well as well as the number of those rearrested up through December 2007 (right axis). The vertical line indicates the moment of the EU enlargement. Source: Ministry of Justice.

36

Figure 9: Propensity score weighting.
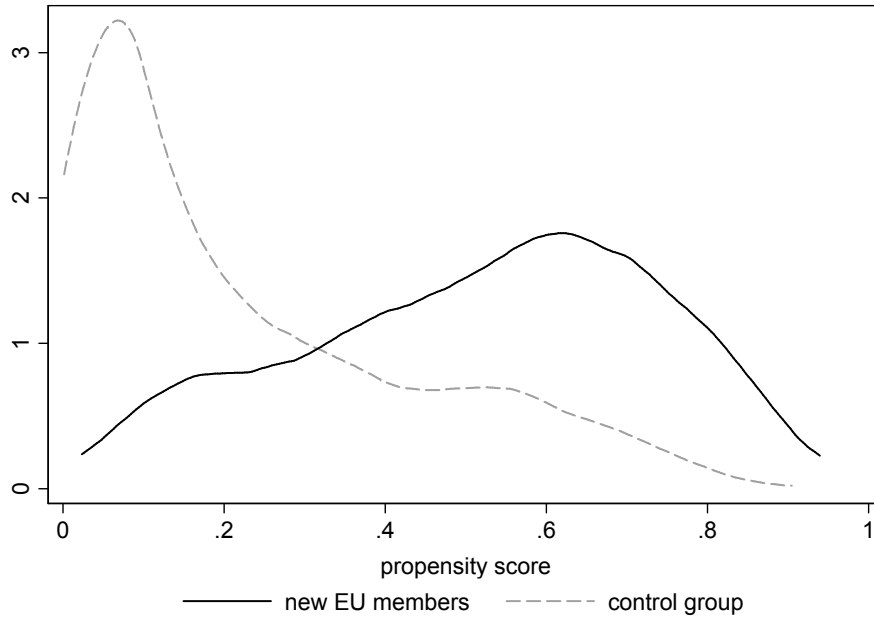
Note: This figure shows the kernel density of the estimated propensity score across groups. The propensity score is the probability of belonging to the groups of citizens of new EU member countries, conditional on observable characteristics. The estimate is based on a logit regression of a dummy for being Romanian or Bulgarian on a flexible specification of the individual information included in our sample.

Figure 10: Hazard rates of rearrest.
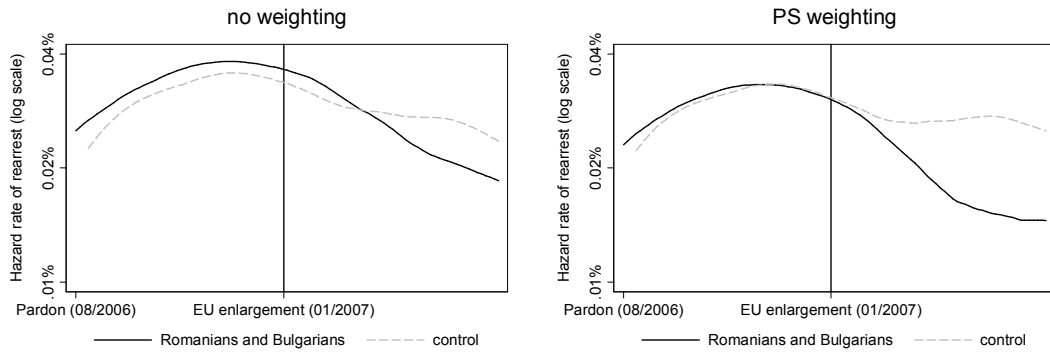
Note: This figure plots the nonparametric (Nelson-Aalen) estimates of daily log hazard rates of rearrest between August 2006 and May 2007 for Romanians and Bulgarians (solid line) and for the control group (dashed line). The scale on the vertical axis reports the (estimated) hazard rate of rearrest each day. In the graph to the right, observations are weighted by the (estimated) propensity score according to formula (14).

Figure 11: Differences between the North and South.

Enforcement of migration restrictions
(incapacitation effect)

.87

.48

Unofficial economy
(opportunity cost effect)

.31

.073
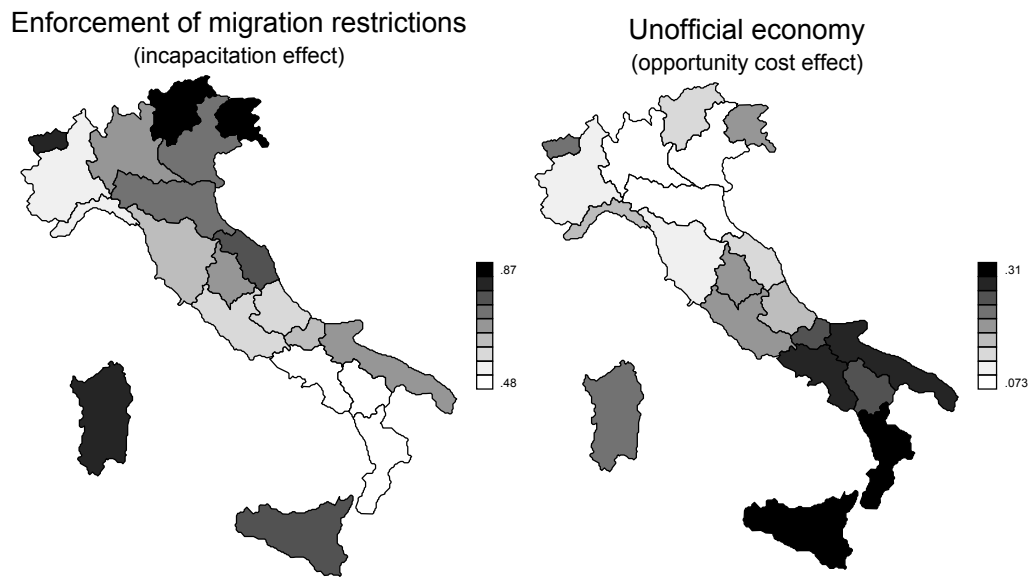
Note: The map on the left shows the ratio of legal to all (legal and illegal) immigrants across Italian regions; the map to the right shows the relative size of the unofficial economy. In both cases darker colors refer to higher values. Source: ISTAT and the Ministry of Interior.

Figure 12: Structural break test.
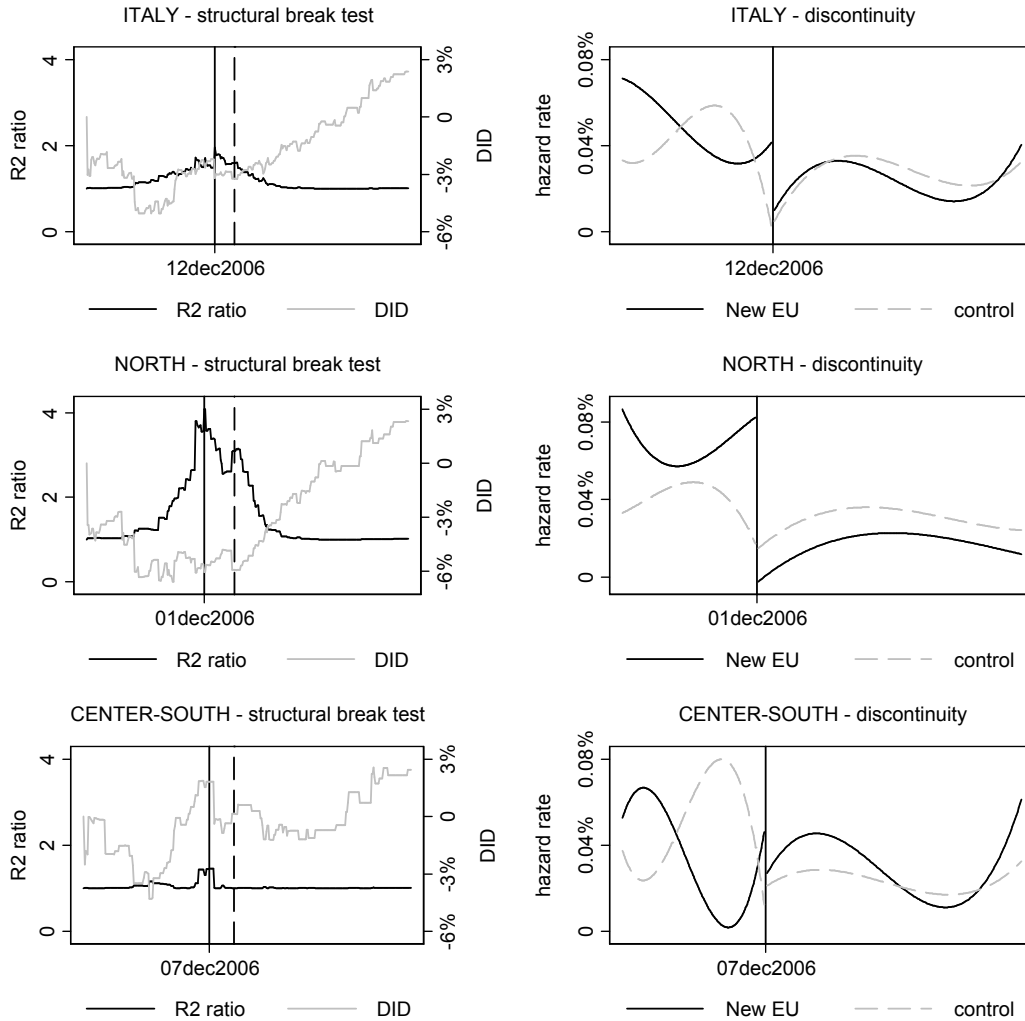
Note: The left-hand graphs plot (black line and left axis) the ratio of the R^2 of the difference-in-differences model H_1 in (17), estimated at each possible date in the sample period, over the R^2 of the restricted model H_0. The estimated coefficient of the interaction term in H_1 is also shown (gray line and right axis). The vertical solid line corresponds to the day that maximizes the R^2 ratio (i.e., the most likely breakpoint), while the vertical dashed line is the official date of the EU enlargement. The right-hand graphs plot the predicted (daily) hazard rates of rearrest as a function of a third-order polynomial in time for Romanians and Bulgarians (solid line) and for the control group (dashed line), before and after the breakpoint.

Figure 13: Effect over time.
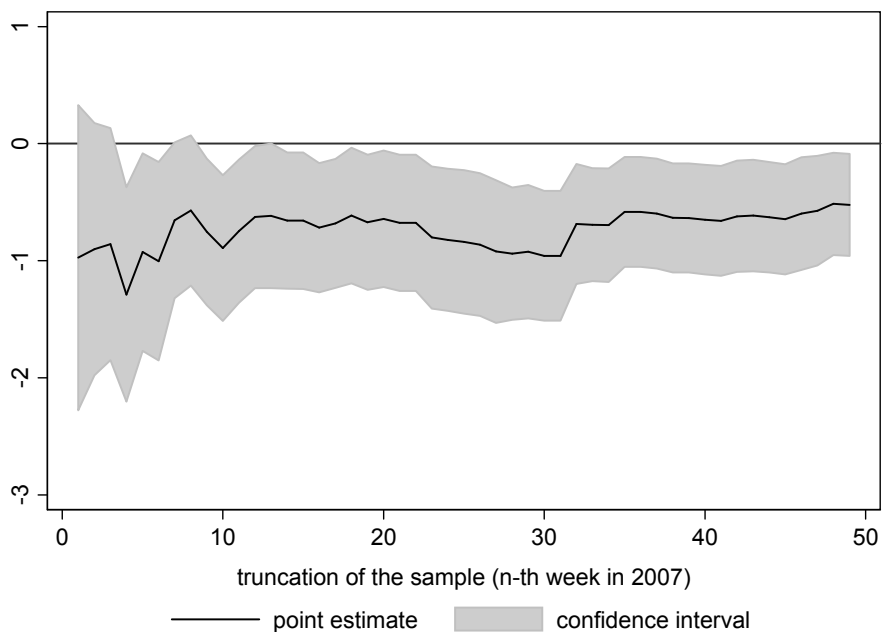
Note: This graph plots the estimated β in model (15), namely, the difference in differences between the log odds of rearrest for Romanians and Bulgarians relative to the control group, before and after the EU enlargement, when the longitudinal dimension of the sample is truncated at each week during 2007.

Figure 14: Substitution of criminal activity.
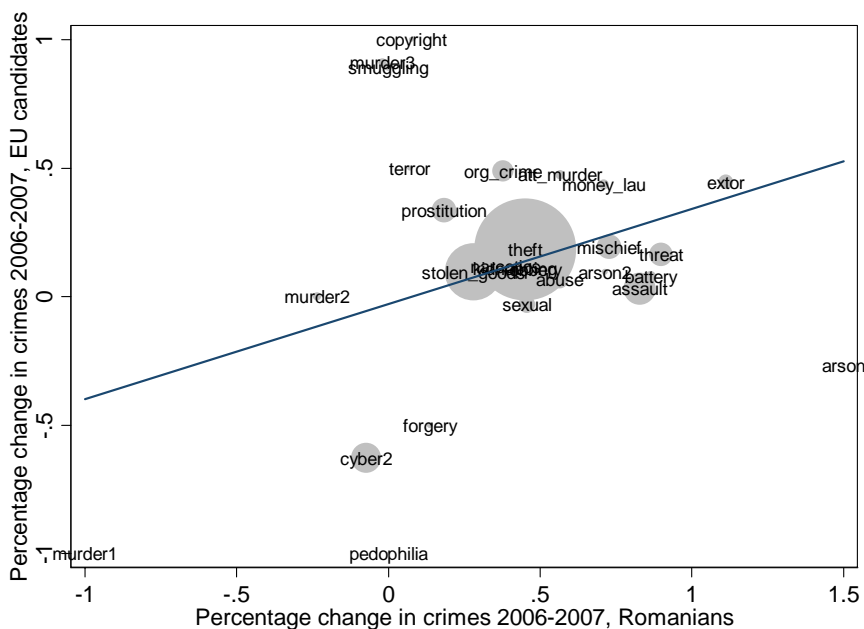
Note: This figure plots the (percentage) change between 2006 and 2007 in the number of Romanians arrested in Italy for different types of crime against the same change for citizens of candidate member countries. The area of markers is proportional to the total number of offenses committed in each category. Source: Ministry of Interior.

Table 1: Legal and illegal immigrants, individual characteristics, and labor market outcomes.

Variable	Illegals		Legals		Diff.
	Obs	*Mean*	*Obs*	*Mean*	
age	1280	31.29	7343	34.63	-3.34***
		(8.94)		(9.36)	(0.28)
female	1281	0.39	7353	0.44	-0.05***
		(0.49)		(0.50)	(0.01)
married	1281	0.34	7353	0.59	-0.26***
		(0.47)		(0.49)	(0.01)
number of kids	1279	0.76	7339	1.18	-0.41***
		(1.19)		(1.28)	(0.04)
college	1281	0.14	7353	0.16	-0.02***
		(0.34)		(0.37)	(0.01)
low skilled	1281	0.12	7353	0.09	0.04**
		(0.33)		(0.28)	(0.01)
income (euros per month)	949	824	5339	1130	-306***
		(371)		(652)	(22)
college premium	949	9	5339	112	-103*
		(35)		(25)	(62)

Note: This table reports the average characteristics of legal and illegal immigrants, as well as the between-group differences in each variable. The source is the 2006 round of the ISMU survey, and the sample is representative of the entire immigrant population of the Italian region of Lombardy. Robust standard errors are reported in parentheses. Here *, **, and *** denote between-group differences that are statistically significant at the 90%, 95%, and 99% confidence levels, respectively.

Table 2: Sample statistics by group.

	Non-weighted sample					Propensity score weighting				
	New EU		Control		Diff	New EU		Control		Diff
	Obs	*Mean*	*Obs*	*Mean*	*Mean*	*Obs*	*Mean*	*Obs*	*Mean*	*Mean*
age	725	31.083	1622	33.269	-2.187***	700	33.335	1493	32.716	0.619
		(7.597)		(8.088)	(0.355)		(8.528)		(7.914)	(0.38)
low education	725	0.339	1622	0.461	-0.122***	700	0.437	1495	0.422	0.015
		(0.474)		(0.499)	(0.022)		(0.496)		(0.494)	(0.023)
no education	725	0.017	1622	0.017	-0.0001	700	0.015	1493	0.018	-0.003
		(0.128)		(0.128)	(0.006)		(0.122)		(0.133)	(0.006)
education missing	725	0.539	1622	0.404	0.135***	700	0.437	1493	0.450	-0.013
		(0.499)		(0.491)	(0.022)		(0.496)		(0.498)	(0.024)
married	725	0.257	1622	0.288	-0.031	700	0.266	1493	0.277	-0.011
		(0.437)		(0.453)	(0.02)		(0.442)		(0.448)	(0.021)
economic crimes	725	0.84	1622	0.894	-0.054***	700	0.857	1493	0.877	-0.02
		(0.367)		(0.308)	(0.015)		(0.35)		(0.328)	(0.016)
violent crimes	725	0.295	1622	0.242	0.053***	700	0.284	1493	0.262	0.022
		(0.456)		(0.428)	(0.02)		(0.451)		(0.44)	(0.021)
sentence (months)	725	20.31	1622	39.183	-18.873***	700	32.115	1493	33.269	-1.154
		(20.706)		(32.33)	(1.306)		(30.63)		(30.593)	(1.435)
residual sentence	725	9.305	1622	15.727	-6.423***	700	13.349	1493	13.83	-0.481
		(10.615)		(14.784)	(0.609)		(12.917)		(14.13)	(0.646)

Note: This table compares the characteristics of Romanians and Bulgarians in our sample with a group of citizens from candidate EU member countries. The first three columns report non-weighted averages for each group, as well as the between-group difference for each variable. In the last three columns, observations are weighted by the inverse propensity score, according to (14). Robust standard errors are reported in parentheses. Here *, **, and *** denote between-group differences that are statistically significant at the 90%, 95%, and 99% confidence levels, respectively.

Table 3: Differences in differences.

	ECONOMIC CRIMES			NON-ECONOMIC CRIMES		
	New EU	**Control**	**Diff.**	**New EU**	**Control**	**Diff.**
Post	0.023	0.054	-0.031**	0.047	0.034	0.013
	(0.005)	(0.008)	(0.010)	(0.020)	(0.014)	(0.025)
	[0.006]	[0.008]	[0.010]	[0.021]	[0.014]	[0.025]
Pre	0.058	0.057	0.001	0.033	0.043	-0.009
	(0.013)	(0.007)	(0.015)	(0.028)	(0.021)	(0.035)
	[0.014]	[0.008]	[0.015]	[0.019]	[0.022]	[0.029]
Diff.	-0.035**	-0.003	-0.032*	0.014	-0.009	0.023
	(0.014)	(0.011)	(0.017)	(0.034)	(0.025)	([0.043])
	[0.014]	[0.011]	[0.018]	[0.028]	[0.027]	[0.039]

Note: This table reports the fraction of individuals in our sample who are reincarcerated, distinguishing between citizens of new EU member countries and candidate member countries, as well as between before ("pre") and after ("post") the EU enlargement. The difference in differences is reported in the lower right-hand corner of the table. The left and right panels show the cross-tabulation for the subsamples of former inmates who were previously incarcerated (before the pardon) for economic and violent crimes, respectively. Observations are weighted by the inverse propensity score according to (14). Robust standard errors are reported in parentheses. Here *, **, and *** denote between-group differences that are statistically significant at the 90%, 95%, and 99% confidence levels, respectively. Bootstrapped standard errors, based on 400 replications, are also reported in square brackets.

Table 4: Economic crimes and parametric and semiparametric estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
	Logistic regression				*Cox model*	
new EU	0.091	0.091	0.071	0.073	0.022	0.002
	(0.219)	(0.220)	(0.222)	(0.222)	(0.218)	(0.219)
post	-0.404**	-0.162	-0.162		-0.249	-0.277
	(0.167)	(0.354)	(0.354)		(0.363)	(0.364)
new EU × post	-0.744**	-0.740**	-0.733**	-0.736**	-0.679**	-0.668**
	(0.289)	(0.288)	(0.289)	(0.289)	(0.308)	(0.308)
time		-0.004	-0.004			
		(0.011)	(0.011)			
time^2		-0.000	-0.000			
		(0.000)	(0.000)			
age			0.090	0.090		0.088
			(0.058)	(0.058)		(0.059)
age^2			-0.001	-0.001		-0.001
			(0.001)	(0.001)		(0.001)
married			-0.381**	-0.380**		-0.283*
			(0.176)	(0.176)		(0.158)
residual sentence			-0.020***	-0.020***		-0.021***
			(0.007)	(0.007)		(0.006)
Observations	124019	124019	124019	115428	3547	3547
Subjects	1918	1918	1918	1918	1918	1918
Week dummies	NO	NO	NO	YES	.	.
Pseudo R^2	0.009	0.010	0.016	0.042	0.003	0.009
Log likelihood	-1704	-1702	-1691	-1631	-1797	-1786

Note: This table shows the results of parametric and semiparametric estimates of the probability of incarceration for immigrants from new EU member and candidate member countries before and after the EU enlargement. The parametric model in columns (1) through (4) is a logit equation for the log odds or incarceration estimated on a panel of individual-weeks observations. The panel is unbalanced because we include only those individuals who are at risk of rearrest in any given week. The semiparametric model in columns (5) and (6) is a Cox proportional hazard model for the log hazard rate of incarceration. The sample includes all inmates from new EU member and candidate member countries released after the July 2006 collective pardon who were previously incarcerated for having committed an economically motivated offense. The dummy $post_t$ is equal to one in the weeks after the EU enlargement (January 1, 2007), and *time* and *time^2* are linear and quadratic trends in time, respectively. Regressions are weighted by the inverse propensity score according to (14). Robust standard errors clustered by Italian region and country of origin are reported in parentheses. Here *, **, and *** denote coefficients significantly different from zero at the 90%, 95%, and 99% confidence levels, respectively.

Table 5: Non-economic crimes and parametric and semiparametric estimates

	(1)	(2)	(3)	(4)	(5)
	Logistic regression			*Cox model*	
new EU	-0.281	-0.283	-0.222	-0.276	-0.215
	(1.030)	(1.030)	(0.998)	(0.691)	(0.691)
post	-0.546	0.044	0.051	0.490	0.493
	(0.585)	(0.999)	(1.005)	(0.838)	(0.845)
new EU × post	0.251	0.251	0.245	0.256	0.243
	(1.144)	(1.143)	(1.138)	(0.864)	(0.865)
time		-0.015	-0.015		
		(0.027)	(0.027)		
$time^2$		-0.000	-0.000		
		(0.001)	(0.001)		
age			0.223		0.229
			(0.193)		(0.212)
age^2			-0.003		-0.003
			(0.003)		(0.003)
married			0.111		0.108
			(0.588)		(0.420)
residual sentence			-0.008		-0.007
			(0.023)		(0.020)
Observations	18105	18105	18105	531	531
Subjects	272	272	272	272	272
Pseudo R^2	0.004	0.008	0.015	0.002	0.012
Log likelihood	-202.9	-202.1	-200.6	-150.2	-148.6

Note: This table shows the results of parametric and semiparametric estimates of the probability of incarceration for immigrants from new EU member and candidate member countries before and after the EU enlargement. The parametric model in columns (1) through (3) is a logit equation for the log odds of incarceration estimated on a panel of individual-week observations. The panel is unbalanced because we include only those individuals who are at risk of rearrest in any given week. The semiparametric model in columns (4) and (5) is a Cox proportional hazard model for the log hazard rate of incarceration. The sample includes all inmates from new EU member and candidate member countries released after the July 2006 collective pardon who were previously incarcerated for having committed (only) violent offenses. The dummy $post_t$ is equal to one in the weeks after the EU enlargement (January 1, 2007), and *time* and $time^2$ are linear and quadratic trends in time, respectively. Regressions are weighted by the inverse propensity score according to (14). Robust standard errors clustered by Italian region and country of origin are reported in parentheses. Here *, **, and *** denote coefficients significantly different from zero at the 90%, 95%, and 99% confidence levels, respectively.

Table 6: Differences between northern and southern Italy.

	North	Center-South	North/CSouth
Total sample	1244	1103	1.1
New EU	348	377	0.9
Candidate countries	896	726	1.2
Economic structure (labor market opportunities)			
GDP per capita	30066	20947	1.4
Shadow economy (%GDP)	9%	18%	0.5
Employment rate	48%	37%	1.3
Illegal condition in 2002 (incapacitation)			
Residence permits, ths.	832	616	1.4
Illegals (applications for amnesty), ths.	366	336	1.1
Illegals/permits	31%	35%	0.9

Note: This table displays the average characteristics of the North and Center-Southern regions, as well as the ratio between the two (in the third column). Source: ISTAT and the Ministry of Interior.

Table 7: North versus South, differences in differences.

	Northern regions			Southern regions		
	New EU	**Control**	**Diff.**	**New EU**	**Control**	**Diff.**
Post	0.014	0.061	-0.046**	0.034	0.046	-0.013
	(0.006)	(0.010)	(0.012)	(0.009)	(0.013)	(0.016)
Pre	0.066	0.053	0.013	0.049	0.063	-0.014
	(0.020)	(0.009)	(0.022)	(0.017)	(0.012)	(0.021)
Diff.	-0.052**	0.007	-0.059**	-0.015	-0.017	0.001
	(0.021)	(0.014)	(0.025)	(0.020)	(0.017)	(0.026)

Note: This table reports the fraction of individuals in our sample who are reincarcerated, distinguishing between citizens of new EU member countries and candidate member countries, as well as between the period before ("pre") and after ("post") the EU enlargement. The difference in differences is reported in the lower right-hand corner of the table. The left and right panels show the cross-tabulation for the subsamples of former inmates who were released from a prison in the North or Center-South of Italy, respectively. Observations are weighted by the inverse propensity score according to (14). Robust standard errors are reported in parentheses. Here *, ** and *** denote between-group differences that are statistically significant at the 90%, 95%, and 99% confidence levels, respectively.

Table 8: North versus South, parametric and semiparametric estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NORTHERN REGIONS				CENTER-SOUTH REGIONS			
	Logistic		Cox model		Logistic		Cox model	
new EU	0.226	0.246	0.214	0.234	-0.085	-0.123	-0.224	-0.256
	(0.254)	(0.246)	(0.287)	(0.287)	(0.355)	(0.351)	(0.339)	(0.340)
post	0.168	0.170	-0.277	-0.343	-0.612	-0.612	-0.142	-0.612
	(0.432)	(0.432)	(0.477)	(0.478)	(0.524)	(0.524)	(0.560)	(0.524)
new EU $\times$ post	-0.948**	-0.933**	-0.940**	-0.923**	-0.459	-0.465	-0.323	-0.331
	(0.413)	(0.422)	(0.407)	(0.408)	(0.348)	(0.347)	(0.472)	(0.472)
age		0.151*		0.151*		0.028		0.022
		(0.083)		(0.085)		(0.086)		(0.081)
age^2		-0.002*		-0.002		-0.000		-0.000
		(0.001)		(0.001)		(0.001)		(0.001)
married		-0.605***		-0.599***		-0.087		0.117
		(0.195)		(0.217)		(0.315)		(0.231)
residual sentence		-0.022**		-0.022***		-0.019*		-0.022**
		(0.009)		(0.007)		(0.010)		(0.009)
Observations	68151	68151	1982	1982	55868	55868	1566	1566
Time trend	quad.	quad.	flex.	flex.	quad.	quad.	flex.	flex.
Pseudo R^2	0.011	0.021	0.005	0.015	0.011	0.015	0.003	0.009
Log likelihood	-989.9	-979.9	-950.9	-941.0	-710.4	-706.9	-679.6	-675.7

Note: This table shows the results of parametric and semiparametric estimates of the probability of incarceration for immigrants from new EU member and candidate member countries before and after the EU enlargement, distinguishing between the North and Center-South regions. The parametric model - columns (1) and (2) and (5) and (6) - is a logit equation for the log odds for incarceration estimated on a panel of individual-weeks observations. The panel is unbalanced because we include only those individuals who are at risk of rearrest in any given week. The semiparametric model - columns (3) and (4) and (7) and (8) - is a Cox proportional hazard model for the log hazard rate of incarceration. The sample includes all inmates from new EU member and candidate member countries released after the July 2006 collective pardon from a prison in the North - columns (1) through (4) - and in the Center-South - columns (5) through (8). The dummy $post_t$ is equal to one in the weeks after the EU enlargement (January 1, 2007), and $time$ and $time^2$ are linear and quadratic trends in time, respectively. Regressions are weighted by the inverse propensity score according to (14). Robust standard errors clustered by Italian region and country of origin are reported in parentheses. Here *, ** and *** denote coefficients significantly different from zero at the 90%, 95%, and 99% confidence, respectively.