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Giovanni Mastrobuoni and Paolo Pinotti

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Carlo F. Dondena Centre for Research on Social Dynamics
Università Bocconi, via Guglielmo Röntgen 1, 20136 Milan, Italy
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Legal status and the criminal activity of immigrants*

Giovanni Mastrobuoni
Collegio Carlo Alberto

Paolo Pinotti
Dondena Centre, Università Bocconi

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Abstract

We present a theoretical model of immigration and crime in which legal status raises the opportunity cost of crime, illegal immigrants may be deported, and there is endogenous selection into legal status. We estimate the model exploiting administrative records on the universe of prison inmates pardoned with a clemency bill in Italy on August 2006, and exogenous variation in legal status after the European Union enlargement of January 2007. The causal effect of legal status amounts to a 50% reduction in recidivism, and explains 1/2 to 2/3 of the observed differences in crime rates between legal and illegal immigrants.
Keywords: immigration, crime, legal status
JEL codes: F22, K42, C41

1 Introduction

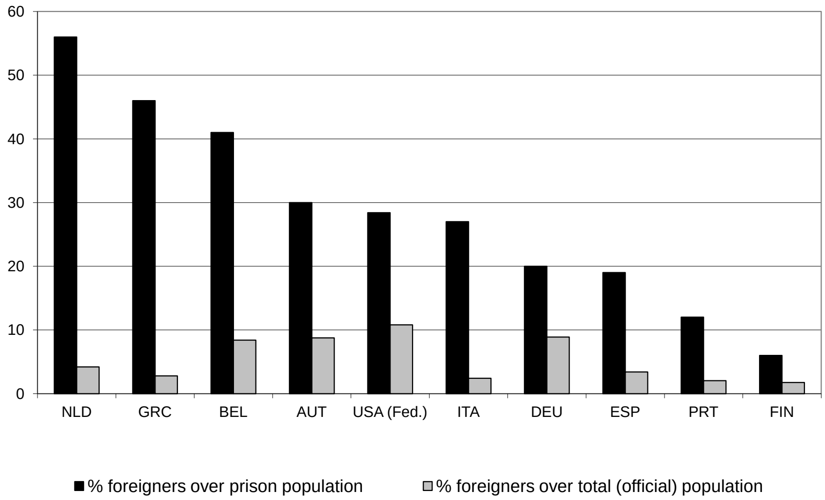
Immigrants are overly represented in prison. In most countries the share of foreign prisoners is indeed much larger than the share of foreign residents, see Figure 1. Such numbers raise widespread concerns among the native population and increase support for migration restrictions that prevent potential immigrants from legally residing in the destination country (Card et al., 2009).[1]

However, migration restrictions often produce a pool of unauthorized immigrants, those who manage to enter the country despite the restrictions (by crossing unofficially the frontier or over-staying tourist visas). In Italy, at the time of the 2002 amnesty for undocumented migrants, the share of illegals reached 1/3 of the total foreign population; in the US, where the last amnesty dates back to 1986,

*Contacts: giovanni.mastrobuoni@carloalberto.org, Via Real Collegio 30, Moncalieri, Italy, and paolo.pinotti@unibocconi.it, Via Roentgen 1, Milan, Italy. This is a heavily revised version of a previous paper circulated with the title *Migration restrictions and criminal behavior: evidence from a natural experiment*. We would like to thank Josh Angrist, Federico Cingano, David Card, Giacomo De Giorgi, Raquel Fernandez, Andrea Ichino, Justin McCrary, Enrico Moretti, Alfonso Rosolia, Adriaan Soetevent, Giordano Zevi and seminar participants at the FEEM-CEPR Conference on Economics of Culture, Institutions and Crime, Center for Studies in Economics and Finance in Naples, University of Padua, University of Paris X, INSIDE Workshop in Barcelona, NBER Summer Institute 2010 (Labor Studies and Crime groups), Universitat van Amsterdam, Cornell University, Brown University, the Royal Economic Society Conference 2011 (special session on Immigration and Crime), and the LIEPP-Chicago Crime Conference for very useful comments. Financial support from the Collegio Carlo Alberto, the W.E. Upjohn Institute for Employment Research, and Fondazione Antonveneta is gratefully acknowledged. Giovanni Mastrobuoni thanks INSIDE at Universitat Autnoma de Barcelona University for their hospitality. Giancarlo Blangiardo kindly provided the ISMU data.

[1] Based on Census data, Butcher and Piehl (2007) argue that immigrants are under-represented among the population of "institutionalized" individuals in the US. But this group pools together prison inmates and other individuals reported in "mental institutions, hospitals, drug treatment centers, and long-term care facilities," among which recent immigrants are likely under-represented.

Figure 1: Share of foreigners over total and prison population in some OECD countries



Note: This figure shows the incidence of foreigners among the prison and total populations, respectively, in some OECD countries around the year 2000. The source is the OECD for all countries other than the United States. The data for the United States are from Stana (2005) and refer exclusively to federal prisons; representative data on state and local jails are not publicly available.

the 11.5 million illegal immigrants are close to outnumber the 13.1 million legal ones (Rytina, 2011; Hoefer et al., 2011). These individuals are excluded from the official labor market, as an increasing number of audits and work-site roundups force businesses to fire suspected illegal immigrants on the pay roll, which in turn lowers their opportunity cost of crime.

Therefore, the implications of migration barriers for the level of criminal activity remain ambiguous. On the one hand, restrictive policies prevent a number of perspective immigrants, who would be at risk of committing crime, from entering the country (or expel those who entered); on the other hand, those who manage to enter anyway have a higher propensity to engage in criminal activity.

In the next section we show that the crime rate of legal immigrants in Italy is 8 times lower than that of illegal immigrants. Three factors are likely to influence this difference: i) the causal effect of legal status on criminal behavior, ii) the deportation of illegal immigrants, and iii) selection into legal status. We formalize this decomposition in the context of a search-theoretic model of immigration and crime, in which agents decide whether to engage in crime or not depending on the relative returns of legitimate and criminal activities. Legal status affects criminal behavior by changing the opportunity cost of crime and the probability of expulsion from the country. The model allows for selection into legal status, since only the immigrants with higher (legitimate) income opportunities afford the costs imposed by official entry; the others prefer to enter unofficially, and face the risk of being expelled in the future. Therefore, the distribution of individual characteristics that are correlated with criminal activity differs systematically between legal and illegal immigrants. This represents the main threat to the empirical identification of the model. Another challenge is that official statistics do not report precise information about illegal immigrants.

To address these two issues, we exploit i) detailed information on a sample of pardoned immigrants in Italy, and ii) exogenous variation in their legal status after the last round of the European Union (EU) enlargement. Within a few days after August 1, 2006, more than 8,000 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 reside and work in Italy as their origin countries, namely Romania and Bulgaria, entered the EU. We estimate the causal effect of legal status on recidivism by comparing, before and after the EU enlargement, the recidivism of pardoned Romanians and Bulgarians with a control group of inmates from candidate member countries.

We find that over a six-month period recidivism declines from 5.8 to 2.3 percentage points for Romanians and Bulgarians after the EU enlargement, as compared to no change in the control group. Maximum likelihood estimates confirm that, controlling for other individual characteristics and a baseline hazard function of time-at-risk, the hazard rate of re-incarceration falls by almost 50% after the acquisition of legal status. The change affects only economically-motivated offenders, and is larger in areas that grant relatively better legitimate income opportunities to legal immigrants. Since immigrants in Italy are not sentenced to prison just for being illegally present in the country, and pardoned inmates can never benefit from alternative measures to institutionalization (regardless of their legal status), changes in re-incarceration rates must reflect changes in criminal activity after the acquisition of legal status.

Overall, our results attribute 1/2 to 2/3 of the difference in reporting rates between legal and illegal immigrants in Italy to the causal effect of legal status on the probability of committing crimes. The remaining part is likely explained by average between-group differences in individual characteristics (because of selection into legal status) and/or different probabilities of imprisonment conditional on having committed a crime (which remains constant for the pardoned inmates in our sample, but could vary across legal and illegal immigrants at large).

A battery of falsification tests suggest that these results are neither driven by the specific characteristics of the control group, nor by other events occurring during the same period. Estimates of peer effects across nationalities refute that the estimates are biased by substitution or complementarity in crime between treated and control groups. Finally, we also discuss the possibility of differential attrition due to mobility across the border of the newly legalized immigrants, explaining why mobility (and attrition) among Romanians and Bulgarians should have been greater before the acquisition of legal status, thus biasing our estimates toward zero.

We contribute to the literature on the social and economic effects of immigration. Until very recently, this research area has mainly focused on 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 on the effects of immigration on public finance (Storesletten, 2000; Lee and Miller, 2000; Chojnicki et al., 2005) and prices (Lach, 2007; Cortes, 2008). However, Card et al. (2009) show that besides these more “economic” issues natives’ support for migration restrictions is also (and indeed mostly) shaped by other “compositional amenities,” among which crime plays a major role (see also Bauer et al., 2000).

In response to these concerns, a few recent papers have examined the empirical relationship between immigration and crime. At the aggregate level, Butcher and Piehl (1998a), Bianchi et al. (2010) and Bell et al. (2010) document some correlation between- and within-local areas in the US, Italy and the UK, respectively, but conclude that the causal effect of immigration is not different from zero (possibly with the exception of asylum-seekers in the UK).[2] At the micro level, Butcher and Piehl (1998b, 2007) use Census data to show that, controlling for other individual characteristics, current immigrants

[2]More recently, Spenkuch (2011) challenges the findings of Butcher and Piehl (1998a) by estimating a positive relationship in the US during the last decades. Borjas et al. (2010) also argue that migration has indeed an effect, though an indirect one: by displacing black males from the labor market, immigrants increases the criminal activity of this latter group.

have lower incarceration rates than natives, while the opposite was true for former immigrants at the beginning of the 20^{th} century (Moehling and Piehl, 2007).

Yet, none of these previous studies investigates the role of legal status; this is precisely our contribution to this literature. In this respect, our paper is close to Baker (2011), who examines the effect of the 1986 Immigration Reform and Control Act on (changes in) crime rates across US counties. Relative to his work, we take advantage of a clean quasi-experimental design and individual-level data on criminal activity. Bratsberg et al. (2002), Kossoudji and Cobb-Clark (2002), Kaushal (2006), Amuedo-Dorantes et al. (2007) and Lozano and Sorensen (2011) look instead at the effects of legal status on legitimate income, finding that legal status improves significantly the labor market opportunities of foreign immigrants. We incorporate these stylized facts into our search model of crime and draw their implications for the criminal activity of immigrants, both from a theoretical and an empirical perspective.

Finally, we also contribute to a large empirical literature on the relationship between legitimate income opportunities and criminal careers. The negative effect of legitimate income opportunities on the propensity to engage in crime is indeed one of the key results of the economic model of crime (Becker, 1968). Over the years, several papers have examined the empirical content of this prediction, finding in general a good deal of evidence consistent with it: a non-exhaustive list includes Witte (1980), Meyers (1983), Grogger (1998), Gould et al. (2002) and Machin and Meghir (2004). We show that even among foreign immigrants, access to better legitimate income opportunities (through the acquisition of legal status) lowers the propensity to engage in crime.

The paper is organized as follows. In the next section we describe the institutional features of the Italian migration system that are mostly relevant for our analysis (for additional details see Mastrobuoni and Pinotti, 2011); Section 3 presents the theoretical framework and discusses the equilibrium of the model, leaving the details to the Appendix; Section 4 describes the natural experiment, while Section 5 presents the empirical results; Section 6 concludes.

2 Legal and illegal immigrants in Italy

Immigration in Italy is a very recent phenomenon, net inflows turning positive only since the late 1980s (after centuries of massive emigration). During the last two decades, the number of foreign official residents rose from less than 600 thousands to 4.5 million.

2.1 Work permits

The main way foreigners obtain legal residence permits is by receiving a work-related one (other ways are related to asylum, education, and family reunions). However, the application procedure greatly hampers the match between demand and supply of foreign workers. In principle, migration law (N. 189/2002) dictates that employers post job offers in the Italian consulates worldwide; upon finding an employer who is willing to act as a sponsor, the prospective migrant is supposed to apply for a residence permit; if such permit is granted, which depends on the yearly migration quotas fixed by the government, the prospective migrant receives a residence permit from the consulate, and can finally migrate into Italy.

Given that it is almost impossible to find a sponsor without a direct contact, many immigrants cross the border illegally or enter the country as tourists (potentially overstaying the tourist visa) to search for an employer who is willing to sponsor their application. If they succeed, they travel back to receive the work permit from the Italian consulate in their home country. In practice, the chances of later regularization after entering illegally are extremely low due to the tight rationing of residence permits. For instance, 170,000 permits were issued in 2007 against more than 740,000 applicants; the following year, the number of new residence permits even decreased to 150,000, to be primarily assigned to applications left pending the year before.

The chronic mismatch between applications and migration quotas creates a large pool of illegal immigrants, who have little or no chance to find a stable employment in the official sector and regularize their residence status. Most of them remain thus unemployed or work for lower wages in the unofficial economy, which in turn lowers the opportunity cost of engaging in outright criminal activities.

2.2 Crime rates

An official report by the Italian Ministry of Interiors shows that in 2006 (just before the EU enlargement) about 80 percent of all foreigners reported by the police for having committed violent or property crimes were illegally present in the country, while legal immigrants display crime rates in line with those of natives (MdI, 2007, p.360). However, to compare crime rates of legal and illegal immigrants we need also their respective shares over the total foreign population, which are difficult to obtain due to the very nature of illegal migration.

Still, amnesties of formerly undocumented immigrants provide some estimates in this respect. In these occasions, illegal immigrants can in fact apply for a valid residence permit under very mild conditions and have thus clear incentives to report their (illegal) status. General amnesties have been enacted every 4-5 years since 1986, growing in size from 100 to 200-250 thousand individuals during the 1990s, and reaching a peak of 700 thousand in 2002. Therefore, the growth of foreign residents in Italy was paralleled by that of unofficial immigrants, the latter representing about one third of the total foreign population in 2002 (Figure 2).[3]

Combining these pieces of information, the relative crime rate for legal and illegal immigrants is

$$\frac{E(C|L=1)}{E(C|L=0)} \approx 12.5\%, \tag{1}$$

where $C = 1$ for individuals committing a crime and $C = 0$ otherwise, while $L = 1$ and $L = 0$ denote the groups of legal and illegal immigrants, respectively. Therefore, the probability of committing a crime is about 8 times lower for the legal than for the illegal immigrants. We next examine which features of the Italian migration system may contribute to explain such difference.[4]

2.3 Deportations and selection into legal status

Illegal immigrants apprehended in Italy can *not* be sentenced to jail for the only reason of being illegally present on the territory.[5]. They should instead be transferred to a detention center for unauthorized

[3]Bianchi et al. (2010) and Fasani (2009) use applications for amnesty to estimate the size of the illegal population in Italy, while several studies adopted the same methodology to count the number of undocumented immigrants in the United States after the amnesty passed with the Immigration Reform and Control Act in 1986 (see, e.g., Winegarden and Khor, 1991).

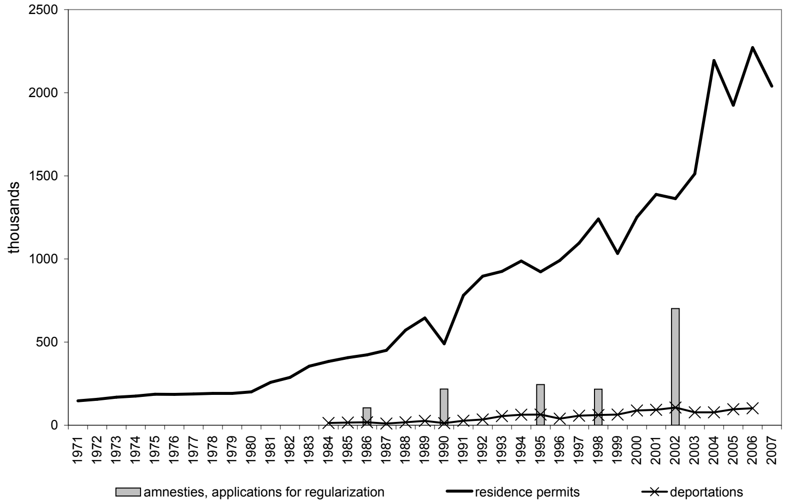
[4]To compute (1) notice that

$$\frac{E(C|L=1)}{E(C|L=0)} = \frac{c_L}{1-c_L} / \frac{r_L}{1-r_L},$$

where c_L and r_L are the shares of legal immigrants among all foreigners reported by the police and among all foreign residents, respectively, and let $c_L \approx 20\%$ and $r_L \approx 67\%$

[5]In 2002 the last reform of migration policy (Law 189/2002) introduced the possibility of incarceration for illegal immigrants who did not comply with a previous injunction to leave and were later re-apprehended by the police. However,

Figure 2: Residence permits, amnesties of illegal immigrants and deportations



Note: The figure shows the number of valid residence permits (since 1971), applications for 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.

aliens and, unless they qualify as asylum seekers, they should be deported back to their country of origin. In many cases the procedure is not enforced, due to the overcrowding of the detention centers and/or the cost of deportations, in which case the apprehended immigrant just receives an injunction to leave the country and is immediately released. Still, the fraction of immigrants who are deported is not negligible. Figure 2 shows that, focusing on the years in which there was an amnesty, the ratio of expulsions over the total number of illegal immigrants (as measured by the number of applications for amnesty) climbed from 17 percent in 1986 to 28 percent in 1998, eventually declining to 15 percent during the last amnesty of 2002.

Deportations reduce the number of crimes committed by illegal immigrants by expelling some of the potential offenders in this group. In particular, the ratio in Eq. (1) can be decomposed into

$$\frac{E(C|L=1)}{E(C|L=0)} = \frac{1}{1-E(D|L=0)} \times \frac{E(C|L=1)}{E(C|L=0,D=0)}, \tag{2}$$

where $D = 1$ for the illegal immigrants who are deported and $D = 0$ otherwise, so $E(D|L = 0)$ is the probability of deportation and $E(C|L = 0, D = 0)$ is the probability of committing a crime conditional on not being deported. The first term on the right hand side of equation (2) reflects thus the *incapacitation effect* of deportations on the criminal activity of illegal immigrants (as those who are deported cannot commit crimes in Italy), while for the legals $E(D|L = 1) = 0$ and $E(C|L = 1, D = 0) = E(C|L = 1)$.

The second term in Eq. (2) depends, instead, on the causal effect of legal status on the propensity to engage in criminal activity, as well as on selection bias. Framing the problem in the context of the potential outcome model (Rubin, 1974), the difference in crime rates conditional on not being

such norm was never enforced and was later deemed anti-constitutional (sentence 223/2004 of the Constitutional Court, discussed in Ghersi, 2005). Interestingly, in June 2012 the US Supreme Court rejected similar provisions contained in Arizona's immigration law.

deported may be rewritten as

$$\frac{E(C^1|L=1)}{E(C^0|L=0,D=0)} = \underbrace{\frac{E(C^1|L=1)}{E(C^0|L=1)}}_{causal\ effect} \times \underbrace{\frac{E(C^0|L=1)}{E(C^0|L=0,D=0)}}_{selection}, \tag{3}$$

where C^1 and C^0 are the potential outcomes conditional on having or not having legal status. Selection bias is the main threat to identifying the causal effect of legal status, because we observe C^0 only when $L = 0$ and, in general, legal and illegal immigrants have different characteristics to start with. For instance, individuals with higher labor ability are arguably more likely to find an employer willing to sponsor their work permit.

Such differences are indeed apparent from survey data collected in the North-West of Italy just one year before the 2007 EU enlargement. Starting in year 2001, the NGO ISMU, based in Milan, has conducted yearly interviews on a sample of about 9,000 immigrants, including legal and illegal immigrants.[6] Based on these data Table 1 shows that unauthorized immigrants tend to be less educated, earn lower earnings, and receive lower college premiums.[7] Part of these gaps certainly reflect a direct effect of legal status (i.e. the fact that legal immigrants can only work in the unofficial sector), but significant differences in predetermined observable characteristics (age, gender and household composition) point at an important role for selection into legal status.

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 difference in each variable. The source is the 2006 round of the ISMU survey, the sample is representative of the entire immigrant population of the Italian region of Lombardy. Robust standard errors are reported in parenthesis. *, ** and *** denote between-group differences that are statistically significant at the 90% confidence, 95% confidence and 99% confidence.

According to Table 1, illegal immigrants are typically young single males, have fewer kinds, and lower education than legal immigrants. In general, such characteristics are associated with a higher

[6]The sampling of illegal immigrants exploits the social networks among the foreign population around a number of "aggregation centers:" care centers, meeting points, shops, telephone centers, etc. (Blangiardo, 2008).

[7]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 US, see for instance Bratsberg et al. (2002), Kossoudji and Cobb-Clark (2002), Kaushal (2006) and Lozano and Sorensen (2011).

propensity to engage in criminal activities, so the selection effect in equation (2) might potentially explain much of the difference in criminal activity between legal and illegal immigrants. Our quasi-experimental design allows us to keep the composition of the two groups constant, in order to identify the causal effect of legal status.

Before moving to the empirical analysis, we characterize the relative probability of committing crimes from a theoretical point of view, distinguishing between the causal effect of legal status on criminal behavior, the incapacitation effect of deportations and selection into legal status. The model formalizes the intuitions above and justifies our identification strategy.

3 Theoretical framework

Consider a population of infinitely lived, risk-neutral agents who must decide whether to migrate. Official entry in the host country imposes an upfront cost B on legal immigrants ($L = 1$); such cost may include, for instance, the time and money spent to deal with paperwork, acquire health certifications, pay head taxes and so on. Alternatively, prospective immigrants can cross the border illegally, in which case they avoid the burden imposed by migration policy. However, illegal immigrants ($L = 0$) face the risk of being apprehended and deported back to their home country ($D = 1$, $D = 0$ otherwise) 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 according to the cumulative density $F(z)$, while π is the probability of being arrested and sent to jail (in the following period) conditional on having committed a crime. Assuming a constant discount factor $\rho < 1$, the expected utility of seizing a crime opportunity z is

$$V_C(L, z) = z + \rho\,(1 - \pi) \int V(L, z) dF(z), \tag{4}$$

where V is the individual utility in the next period conditional on avoiding arrest and receiving a crime opportunity worth z, and we normalized to 0 the utility of going to jail.

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 a denote the (heterogeneous) labor skills, the wage of each immigrant is $w(L)a$ and we assume that $\Delta w \equiv w(1) - w(0) \geq 0$.[8] While the strict inequality would be consistent with the empirical evidence discussed in the previous section, the conservative assumption that Δw is no lower than zero is sufficient for all the results that follow.

Individual utility, conditional on legal status and labor market ability, is thus

$$\begin{aligned} V(L, a, z) &= E(D|L)V_H + [1 - E(D|L)] \\ &\times \left[\max\left\{V_C(L, a, z); \rho \int V(L, a, z)dF(z)\right\} + w(L)a\right], \end{aligned} \tag{5}$$

where V_H is utility in the home country and $E(D|L)$ is the average probability of deportation conditional on legal status, which is positive only for illegal immigrants, $E(D|L = 0) > E(D|L = 1) = 0$.

Notice that we restrict such probability to be constant across individuals within each group (i.e. $E(D|L = 0)$ does not depend on a); the assumption is irrelevant for the purposes of our empirical analysis. To simplify notation we also assumed that (i) V_H is independent on a and (ii) immigrants

[8] From now on Δ denotes the difference between the (potential) outcomes of an individual when legal and illegal.

expelled from the country do not try to migrate anymore. Both assumptions can be relaxed as long as wages in the origin country are lower than in the destination country. Finally, we imposed a constant probability and (dis)utility of incarceration across legal and illegal immigrants. As we discuss at length in the next Section, this is indeed the case in the Italian legislative framework, particularly for our specific sample of pardoned inmates. In any case, the model could be easily extended to allow for differences by legal status along these dimensions.

Individuals face three decisions: whether to migrate or not; in case they do, whether to cross the border legally or illegally; finally, once in the host country, they must choose whether to accept or reject the crime opportunities available in each period. Next, we describe in greater detail the trade offs involved in each decision, and solve the problem backward, starting from the choice about criminal activity.

3.1 Criminal behavior

When deciding whether to engage or not in criminal activity each individual compares the expected returns from criminal activity, $V_C(L, a, z)$, with their opportunity cost, $\rho \int V(L, a, z)dF(z)$. Since the former depends, positively, on the value z of illicit income opportunities currently available (while the latter does not), there exists a reservation value $z^*(L, a)$ such that the individual engages in crime if and only if $z \geq z^*(L, a)$.[9] Letting $V^e(L, a) \equiv \int V(L, a, z)dF(z)$ denote the expected utility in the next period, the reservation value equates the payoffs from crime to its opportunity cost, $V_C(L, a, z) = \rho V(L, a)$; substituting into (4) we obtain

$$z^*(L, a) = \rho\pi V^e(L, a). \tag{6}$$

Intuitively, the payoff required for engaging in crime increases with the risk of being imprisoned and its (opportunity) cost.

Conditional on legal status, the reservation value completely characterizes criminal behavior. In particular, the probability of committing a crime equals the joint probability of not being deported and drawing a crime opportunity above the reservation value,

$$E(C|L, a) = [1 - E(D|L)] [1 - F(z^*(L, a))] . \tag{7}$$

3.2 Equilibrium

In the remainder of this section we provide a graphical representation of the equilibrium, and the intuition behind all results; formal proofs are presented in the Appendix. First notice that, since expected utility in the destination country is increasing in labor market ability, there exists a threshold for labor ability, $a = a_0$, above which individuals prefer to unofficially cross the border than staying home, see Figure 3a.[10]

What about the option of entering the destination country by complying with migration policy? Once in the host country, all immigrants prefer to be legal rather than illegal, i.e. $\Delta V^e(a) \equiv V^e(1, a) - V^e(0, a) > 0$. By a simple revealed-preference argument, all those willing to (illegally) migrate prefer to live in the destination than in the origin country; therefore, these same individuals are better off

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

[10]The same holds true if the utility in the home country does also depend on labor market ability, as long as the wage rate is lower than in the destination country.

Figure 3: Theoretical model of legal status and criminal activity

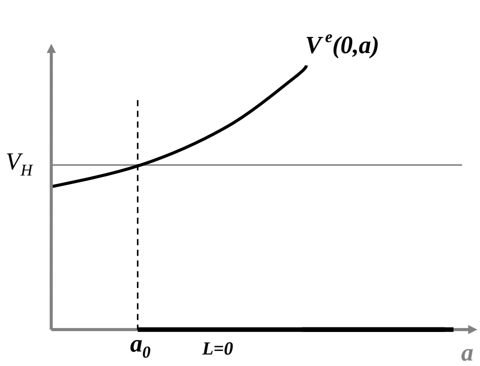


Figure 5a: illegal immigrants

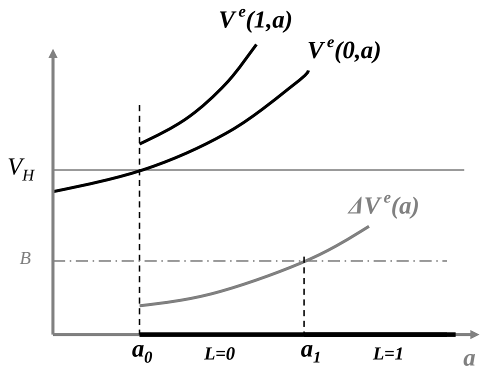


Figure 5b: self selection into legal status

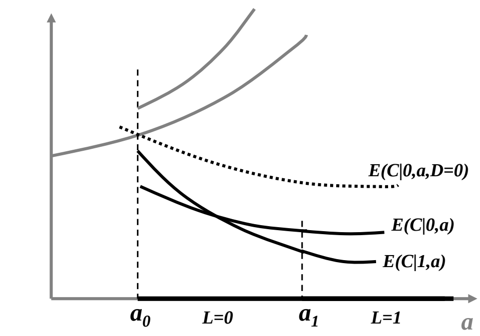


Figure 5c: criminal behavior

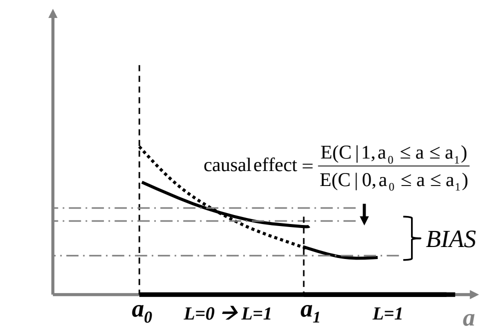


Figure 5d: selection bias

avoiding the risk of being deported back. Moreover, the differential increases with a; intuitively, better labor market opportunities in the destination country mean a greater utility loss in case of expulsion.[11]

After arrival, legal immigrants are thus better off holding a valid residence permit; however, they must bear the entry cost B upon arrival, and so apply for legal status if and only if $\Delta V^e(a) > B$. Since $\Delta V^e(a)$ increases with a, the latter condition must hold above some threshold a_1; see Figure 3b. The diagram clearly illustrates the selection of immigrants at the frontier: $L = 1$ for individuals in the upper tail of the skill distribution, $a \geq a_1$, while $L = 0$ for those in the intermediate range $[a_0, a_1]$, so that $E(a|L = 1) > E(a|L = 0)$. Positive selection in terms of labor market opportunities is consistent with the descriptive evidence presented in Table 1.[12]

According to equations (6) and (7), the equilibrium distribution of a determines within- and between-group differences in the probability of committing a crime, $E(C|L, a)$. Such probability decreases with a within each group; as for between-group differences, $\Delta V^e > 0$ makes for a higher criminal activity of illegal immigrants while deportations shift the crime rate for this group down from $E(C|0, a, D = 0)$ to $E(C|0, a)$, see Figure 3c.

3.3 The effect of legal status

Consider the effect of a policy granting legal status to all previously unofficial immigrants, i.e. those for whom $a \in [a_0, a_1]$. The causal effect of legal status on the probability of committing a crime is

$$\frac{E(C|1, a_0 \leq a \leq a_1)}{E(C|0, a_0 \leq a \leq a_1)} = \frac{1}{1 - E(D|L = 0)} \times \frac{E\left[1 - F(z^*(1, a))|a_0 \leq a \leq a_1\right]}{E\left[1 - F(z^*(0, a))|a_0 \leq a \leq a_1\right]}. \tag{8}$$

The denominator is the average probability of committing a crime before the policy change, while the numerator is the average probability after the policy change. The right hand side replicates the decomposition in Eq. (2) of Section 2.2. The first term is greater than 1 and reflects the incapacitation effect of deportations, which reduces the pool of illegals at risk of committing a crime before the legalization; the second term is lower than 1 because of the higher opportunity cost of crime, which brings a lower probability of committing crimes after legalization (i.e. $\Delta z^* \propto \Delta V^e > 0$). Therefore, the overall effect in (8) is ambiguous; Figure 3d depicts the case in which the probability of committing a crime decreases after legalization.

The same figure also shows that the relative crime rate of legal and illegal immigrants in the absence of the policy change is lower than the causal effect in (8) because of selection bias,

$$\frac{E(C|1, a \geq a_1)}{E(C|0, a_0 \leq a \leq a_1)} = \frac{E(C|1, a_0 \leq a \leq a_1)}{E(C|0, a_0 \leq a \leq a_1)} \times \underbrace{\frac{E\left[1 - F(z^*(1, a))|a \geq a_1\right]}{E\left[1 - F(z^*(1, a))|a_0 \leq a \leq a_1\right]}}_{selection\ bias} \tag{9}$$

where the second term on the right hand side is lower than 1 because the opportunity cost of crime, $z^*(L, a)$, increases with labor market ability a. The last expression provides a theoretical justification for the selection bias formula in Eq. (3) of Section 2.2.

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

[11]The Appendix presents the formal proof that $\Delta V^e(a) > 0$ and $\frac{\partial \Delta V^e(a) > 0}{\partial a} \geq 0 \ \forall a \geq a_0$.

[12]In the theoretical model selection is voluntarily in that only immigrants with better labor market opportunities decide to apply for legal status. However, the framework could be easily extended to allow for the possibility of involuntary selection, e.g. because immigrants with higher labor market ability have higher chances of being sponsored by Italian employers to apply for a work permit.

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 into Italy, accounting for half of foreign official residents in Italy in 2011. The first round of the enlargement took place in 2004 with the admission of Czech Republic, Estonia, Cyprus, Latvia, Lithuania, Hungary, Malta, Poland, Slovenia and Slovakia. Then, on January 1^{st}, 2007, Bulgaria and Romania also joined the EU.

4.1.1 New EU member countries

Romanians and Bulgarians had already been waived tourist visas to enter the EU with the Council Regulation N. 539/2001, and absence of border enforcement within the single market (after the Schengen Agreement of 1985) allowed for the free mobility of Bulgarians and Romanians already before 2007. But the waiver was limited to a maximum of 90 days in each EU member country, after which they had to move to another country; also, they could not work in the destination country.

Instead, Article 39 of the European Commission Treaty allows 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 until the end of the employment relationship, v) enjoy equal treatment with natives in access to employment, working conditions and all other social and tax advantages that may help integration inside the host country.

In practice, however, several countries in Europe maintained significant restrictions. In Italy as well, the application of the EU directives was at the center of a heated debate until the very last weeks before the enlargement, mostly because of the alleged impacts on crime. However, on December 28, 2006, the center-left government eventually guaranteed full rights 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, which account for the bulk of foreign employment in Italy. In the rest of the official economy (basically the manufacturing sector) migration quotas were eased in order to accommodate the larger number of workers from Romania and Bulgaria.

Therefore, admission to the the EU basically removed the migration barriers faced by Romanians and Bulgarians in Italy, granting them with full rights to reside and work (as opposed to a temporary residence permit, like it was before before 2007). They will thus constitute the *treated group* in our policy evaluation of the effects of legal status.

4.1.2 Candidate EU member countries

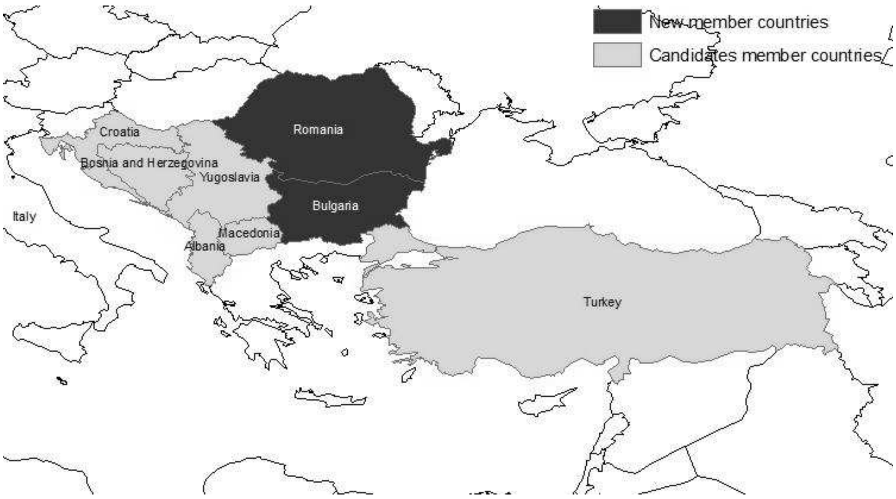
The process of enlargement is far from over, as several countries are in the process of joining the EU: Croatia already signed the Accession Treaty of the European Union and is going to join the EU on July 1, 2013, while Iceland, Turkey, Serbia, Montenegro and the Former Yugoslavian Republic of Macedonia are candidate members and are negotiating admission conditions. Such negotiations should start soon for a group of potential candidates: Albania, Bosnia and Herzegovina, and Kosovo.[13]

Citizens of EU prospective and candidate member countries provide a natural *control group* for Romanians and Bulgarians. Such countries have already started access negotiations with the EU, so

[13] More details about the EU enlargement can be found at http://ec.europa.eu/enlargement/.

they should be most comparable to Romania and Bulgaria along the economic and political criteria required for admission to the EU. With the exception of Iceland, they all belong to the same geographical area, see Figure 4; with the further exception of Turkey, they also share a great deal of linguistic, cultural and historical heritage. In practice, our sample of prison inmates pardoned in Italy will include just a few individuals from Turkey (less than 1% of the total sample) and no one from Iceland.

Figure 4: new EU member and candidate member countries



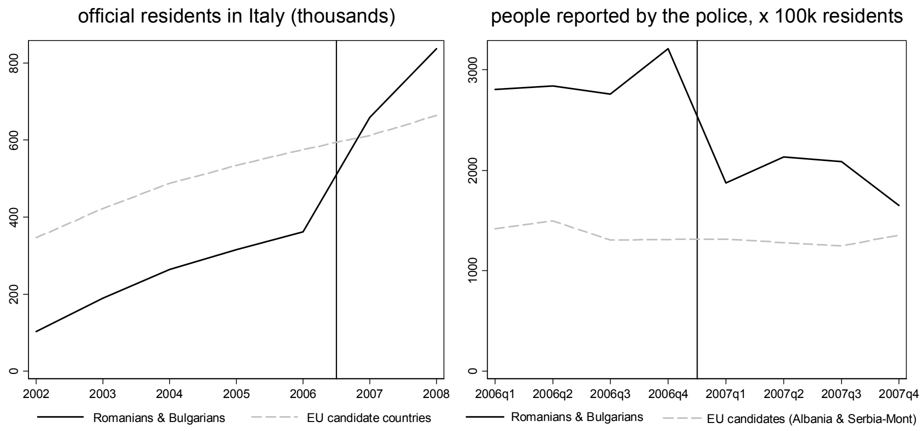
Note: The map shows the countries admitted to the EU during the last round of the enlargement (in black), as well as the group of candidate member countries (in gray). Source: European Commission.

4.1.3 The effects of the EU enlargement in Italy: preliminary evidence

Figure 5 compares the treated and control groups in terms of total official residents and number of people arrested by the police in Italy, before and after the EU enlargement. Until 2006, the combined size of Romanian and Bulgarian communities was about half of the other group, the difference remaining constant over the period. Then, in the wake of admission to the EU, the size of the treated group nearly doubled, while the size of the control group continued to grow at approximately the same rate as in the previous years.

The increase in the number of Romanians and Bulgarians arrested by the Italian police was much less pronounced, so the ratio of arrested over total official residents actually declined for the treated group, while no significant change was observed for the control group (see the right graph of Figure 5). At first sight, one might be tempted to conclude, just from this picture, that the removal of migration restrictions favored a decline in criminal activity. But the increase in the number of Romanians and Bulgarians officially residing in the country includes both new immigrants and old illegal immigrants who were already in the country but acquired legal status only after the enlargement, the two groups being indistinguishable from each other. If the fraction of previously unofficial immigrants is non-negligible, the statistics on official residents would under-estimate the denominator of the crime rate, so its decline after the enlargement would just be a statistical artifact of measurement error in the previous period. If the increase of official residents is instead driven by new inflows, the change in the crime rate would depend not only on the causal effect of legal status on previously unofficial immigrants, but also on the different selection of new immigrants after the enlargement.

Figure 5: Immigrants from new EU member and candidate member countries residing in Italy and number of arrests



Note: The left 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 graph shows instead the ratio of arrested by the police over 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.

One way to address these issues is to focus on a sample of immigrants who were already present in Italy before the enlargement.

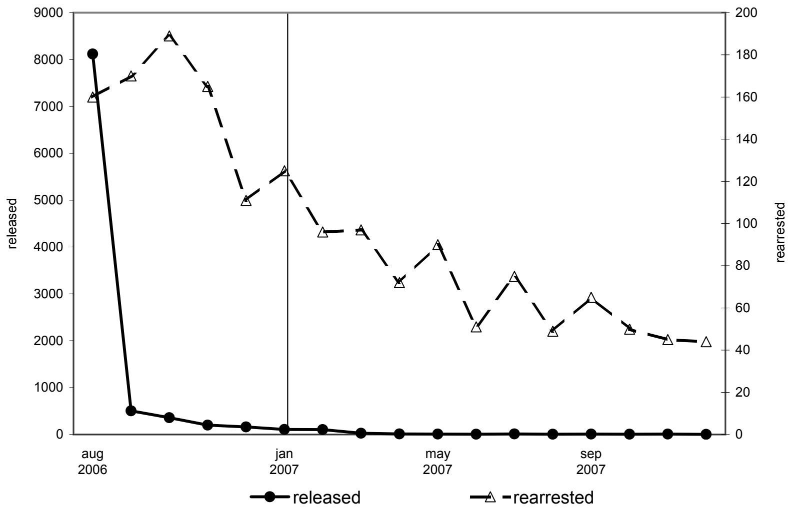
4.2 The July 2006 Collective Pardon

Upon a collective pardon that was passed July 31, 2006 several hundreds of Romanian, Bulgarian, and other Eastern European immigrants were released from prisons that are scattered all over Italy. These pardons eliminate part of the inmates' sentence, typically 2 or 3 years, and when the residual sentence is below such length, the inmates are immediately released. Given their wide reach and a skewed distribution of residual jail time pardons generate sudden releases of large numbers of inmates. The only offenders who do not receive pardons are mafia members, terrorists, kidnappers, and sexual offenders, but even violent offenders, like murderers and robbers, are released.

Such collective clemency bills are deeply rooted in Italian history; over the last 40 years there has been on average a pardon every 5 years (Barbarino and Mastrobuoni, 2010). The last one was voted by the Italian Parliament in July 2006 and enacted shortly after (on August 1). We were granted access to the criminal records of all prison inmates released in this occasion, including the exact dates of release and re-incarceration (if any) up until December 2007, their nationality and a few other individual characteristics.

About 22,000 individuals, corresponding to more than one third of total prison population, were freed within a few days. More than 8,000 of them were foreigners, reaching the figure of 9,642 by the end of 2006. Since a necessary condition for obtaining a residence permit in Italy is having a clean criminal record and a job, all (or most) of these individuals were illegal at the time of release and should have been deported immediately after. However, due to the massive size of the release, they just received a written injunction to leave the country. Most of them did not obey, as witnessed by the number of those re-incarcerated within a few days or weeks for having committed some type of crime on the Italian territory. About 800 were back in jail by the end of 2006, before the EU enlargement, growing to 1654 one year later, after the EU enlargement, see Figure 6. Our empirical strategy is to compare recidivism of pardoned inmates in the treated and control group, before and after the EU enlargement.

Figure 6: The Collective Clemency Bill, monthly number of inmates released and re-incarcerated



Note: The figure plots the number of foreign prison inmates released after the Collective Clemency Bill in August 2006 (on the left axis), as well as those re-incarcerated until December 2007 (right axis). The vertical line refers to the moment of the EU enlargement. Source: Ministry of Justice.

When interpreting the results, we are going to take advantage of two other important aspects of our quasi-experimental design. First, without clean criminal records the only pardoned inmates that acquired legal status between 2006 and 2007 are those in the treated group. And even if pardoned inmates in the control group managed to obtain a residence permit, this should bias our estimates toward zero.

Another feature of the quasi-experimental design is that whenever pardoned inmates recidivate and get arrested they are immediately sent to jail. This prevents the emergence of the following bias. While immigrants are not incarcerated for the only reason of being illegally present on the Italian territory (see Section 2.3), they may still experience a higher probability of incarceration conditional on having committed a crime. According to law, suspect offenders can be incarcerated before trial if caught in the act of committing an offence (*flagranza di reato*) or whenever there is a significant risk that they either pollute the evidence, recommit the same crime, or escape the judgment (upon decision of a special court, *Giudice per le indagini preliminari*). Since illegal immigrants are usually deemed at greater risk of bad conduct they might, in general, be more likely to be incarcerated than legal immigrants.

This is not true, however, in our sample of pardoned prison inmates, as previous offenders are always deemed at high risk and incarcerated immediately upon arrest (regardless of legal status). Moreover, the pardoned individual re-arrested within 5 years since the release must go back to jail to serve the residual sentence that was pardoned, with no possibility of benefiting from alternative forms of detention (e.g. home detention). Finally, no bail is allowed in the Italian judicial system. We may thus interpret relative changes in incarceration rates between the treated and control groups in terms of relative changes in criminal activity.

4.3 Sample

Our sample includes about 800 Romanians and Bulgarians, as well as 1,800 immigrants from candidate member countries (none from Iceland); in order to reduce heterogeneity we restrict our analysis to males, who represent about 90 percent of the total sample, and so we are left with 725 and 1,622 individuals in the treated and control group, respectively. The left columns of Table 2 compare the two groups in terms of the observable characteristics reported in our data: 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, so the group means of economic and violent crimes do not add up to one), the length of the original sentence and, finally, the months commuted with the pardon.[14]

While marital status is not significantly different, Romanians and Bulgarians appear to be on average younger (31 vs. 33 years of age) and more educated; they are also more (less) likely to commit violent (economic) crimes but, despite this, receive on average lighter sentences. One reason may be that the migration waves from some countries in the control group (notably Albania and Former Yugoslavia) predate those from Romania and Bulgaria, which makes for longer criminal careers in Italy, a higher incidence of repeated offenders and longer sentences among individuals in the control group. To the extent that such differences are correlated with criminal activity, the "parallel paths" assumption behind our difference-in-differences approach may be violated.

However, if we are willing to 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, notice that we are imposing it on *changes* in the crime rate over time (as opposed to *levels*); that is, we allow for (time-invariant) differences between groups to persist even after conditioning on observable characteristics. Most importantly, the availability of longitudinal data over repeated periods before the policy change provides us with the opportunity to investigate the empirical content of the unconfoundedness hypothesis.

Following Abadie (2005), we weight observations by the (inverse) propensity score of assignment, i.e. the probability of belonging to each group conditional on the observed covariates, to adjust for differences between the two groups. Specifically, we weight each unit by

$$new\ EU_i \frac{p}{P(X_i)} + (1 - new\ EU_i) \frac{1-p}{1-P(X_i)}, \tag{10}$$

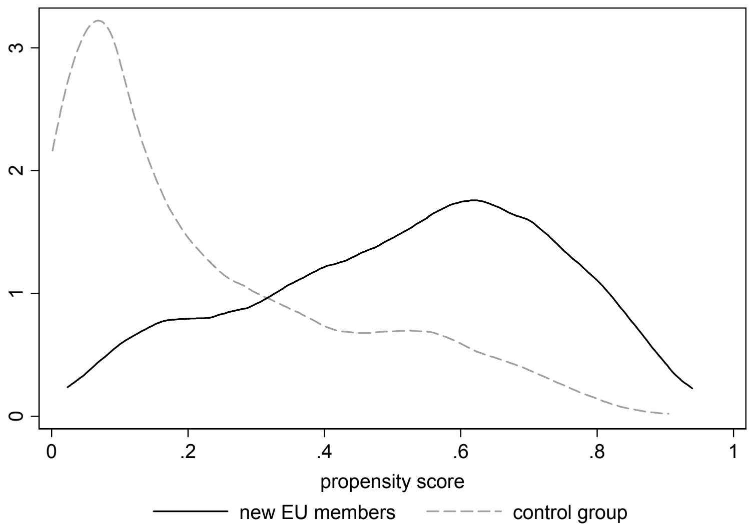
where $new\ EU_i$ is a dummy equal to 1 if the i-th individual is citizen of new EU member countries and 0 otherwise, p is the unconditional probability of belonging to the *new EU* group and $P(X_i)$ is the same probability conditional on the vector of individual characteristics X_i. The weighting scheme enhances comparability between the two groups by attaching more weight to units that are more similar to the other group relative to the average individual in the sample.[15]

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

[15] This approach was pioneered by Hirano et al. (2003) for the case of cross-sectional data, while Abadie (2005) extends it to difference-in-differences estimators. One important advantage relative to other propensity score-matching estimators is the possibility of computing asymptotically valid standard errors by simple bootstrapping methods (Abadie and Imbens, 2008).

As it is generally the case in non-experimental settings, the propensity score is unknown, so we estimate it exploiting all 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, marital status, education (indicator variables for illiteracy, primary and secondary school, as well as missing information on education), type of crime committed when first incarcerated (7 categories), a quadratic polynomial in sentence and commuted sentence and, finally, a full set of fixed effects for the region where the prison from which the individual was released is located.[16] As expected, there is a 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, see Figure 7. The inverse propensity score-weighting reduces the importance of such observations, while it increases the weight of observations in both groups that lie in the middle of the distribution of the estimated propensity score.

Figure 7: Propensity score weighting



Note: The 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 and Bulgarian on a flexible specification of the individual information included in our sample.

The right columns of Table 2 show that weighting observations according to Eq. (10) eliminates indeed all differences in average group characteristics; whether it eliminates also differences in unobservable characteristics is basically untestable. Yet, differences in pre-enlargement outcomes between the treated and control groups, which we examine next, allow us to assess the credibility of the unconfoundedness assumption.

5 Results

This section presents our estimates of the effect of legal status on the criminal behavior of immigrants. We start by comparing non-parametrically differences in the hazard rate of re-incarceration (i.e. the probability of being re-incarcerated in a given period conditional on not having been re-incarcerated before) between the treated and control groups, before and after the enlargement. Then, we are going to estimate semi-parametric and fully-parametric maximum likelihood hazard rate models. We

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

Table 2: Sample statistics by group, raw and propensity score-weighted data

	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 the group of citizens from candidate EU member countries. The first tree 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 Eq. (10). Robust standard errors are reported in parenthesis. *, ** and *** denote between-group differences that are statistically significant at the 90% confidence, 95% confidence and 99% confidence.

conclude with a series of robustness exercises and falsification tests.

5.1 Preliminary evidence

Figure 8 shows non-parametric estimates of the daily (log) hazard rates of re-incarceration for pardoned individuals from new EU member and candidate member countries. For the sake of graphical illustration, we focus on inmates released during the first week after the pardon (1,392 individuals out of 2,347 in our sample), so the horizontal axis represents (approximately) the same duration-at-risk for all individuals.[17] Since we are particularly interested in the effect of legal status through legitimate earning opportunities, we focus on individuals who were first arrested (before the pardon) for economically-motivated crimes (mainly property and drug-related offenses).[18]

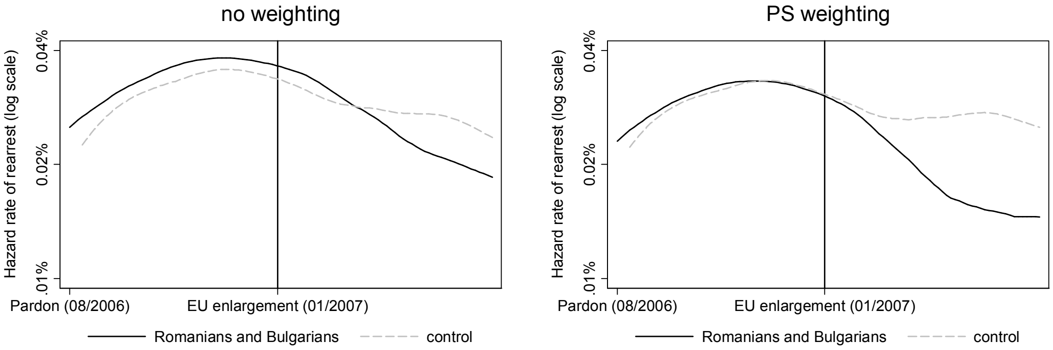
The left panel shows the results obtained using the raw data, i.e. before applying the weighting scheme. While Romanians and Bulgarians display greater recidivism during the first months after the pardon, the opposite is true after they obtain legal status. As to the plausibility of the identifying assumption, the evidence from the pre-enlargement period seems broadly consistent with the hypothesis of parallel outcomes (absent the policy change). After weighting observations by the inverse propensity score (right panel), the level of the hazard rate is also very similar between the two groups, which provides strong support for the unconfoundedness assumption.

To quantify the effect of legal status, in Table 3 we tabulate the hazard rate of re-arrest for each group over the last two trimesters of 2006 and the first two of 2007, their differences and the difference-in-differences; we report both robust and bootstrapped standard errors that are clustered based on

[17] All tables that are presented later in this section are based on the total sample.

[18] The data do not contain information on the crimes committed after the pardon.

Figure 8: Hazard rates of rearrest for pardoned inmates from new EU member and candidate member countries



Note: The figure plots the non-parametric (Nelson-Aalen) estimates of daily log hazard rates of re-incarceration between 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 re-incarceration in each day. In the right graph observations are weighted by the (estimated) propensity score according to formula (10).

Italian Regions and nationality.[19] This allows for criminal activity to be correlated for inmates of same nationality who spent time in jails that are located within a Region. While bootstrapping generally leads to valid standard errors and confidence intervals for propensity score-weighting estimators (Abadie and Imbens, 2008), Busso et al. (2009) suggest that, as the number of observations grows large, robust regression standard errors provide a good approximation. The two estimates of the standard errors are indeed always extremely similar: 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. For the sake of computational efficiency, inference on the maximum likelihood models presented next will be based on robust (clustered) standard errors.

The top left panel of the table 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. Then, in 2007 the hazard rate does not change significantly for the control group while it decreases, from 5.8 percent to 2.3 percent, for Romanians and Bulgarians; as a result, the difference-in-differences is also negative and very similar in absolute value (3.2 percent). These numbers imply that the hazard ratio of committing a crime between legal and illegal immigrants is close to 45 percent (0.023/0.054). In light of our theoretical model, the increase in the opportunity cost of crime after the acquisition of legal status (the second term on the right hand side of equation 8) outweighs the reduction in the deportation of potential criminals (the first term on the right hand side of the equation).

The opposite is true for violent offenders (top right panel), though the result is not statistically significant.[20] For such offenders, criminal activity should depend to a lesser extent on economic motives (Machin and Meghir, 2004), so changes in labor market opportunities play only a minor role and the deportation effect prevails.

[19] Italy is divided into 20 Regions and there are 8 different nationalities in our sample. Since not all nationalities are represented in all Regions the number of clusters is equal to 129.

[20] To be as conservative as possible, we include in this category individuals who were previously in prison for having committed *only* violent crimes; instead, those reported for both economic and violent crimes are included among economically-motivated offenders, because the latter type of crime could be instrumental to the first one (e.g. an assault during a robbery).

Table 3: Probability of re-incarceration for pardon inmates from new EU member and candidate member countries, before and after the EU enlargement

	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]
	ECONOMIC CRIMES, NORTH			ECONOMIC CRIMES, SOUTH		
	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)	(0.009)	(0.009)	(0.012)	(0.015)
	[0.007]	[0.011]	[0.013]	[0.010]	[0.012]	[0.015]
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)
	[0.020]	[0.010]	[0.022]	[0.019]	[0.013]	[0.023]
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)
	[0.021]	[0.015]	[0.025]	[0.021]	[0.018]	[0.027]

Note: This table reports the fraction of citizens of new EU member and candidate member countries that is re-incarcerated before ("pre") and after ("post") the enlargement as well as the difference and difference-in-differences between the two groups, for different sub-samples of pardoned inmates. The top left and right panels show the cross tabulation for the subsamples of individuals that were previously incarcerated (before the pardon) for economic and violent crimes, while the bottom left and right panels distinguish between economic offenders in northern and southern Italy. Observations are weighted by the inverse propensity score according to Eq. (10). Robust standard errors clustered by Italian region and country of origin are reported in parenthesis. *, ** and *** denote between-group differences that are statistically significant at the 90% confidence, 95% confidence and 99% confidence. Bootstrapped standard errors, based on 400 replications, are also reported in square brackets.

Heterogeneity in the effects across criminal types is thus consistent with the labor market-related mechanism emphasized in our theoretical model. And there is additional heterogeneity we can exploit in this respect. It is a well known fact that Northern Italy is economically much more developed than the South, with GDP per capita differences that are close to 50 percent.[21] One reason for such differences is that northern regions exhibit better income opportunities in the official sector, while a large share of the labor force in the south is employed in the unofficial sector: indeed, the relative size of the shadow economy is twice as large as that in the north. If legal status reduces crime because it improves economic prospects in the official sector, one would expect the reduction to be concentrated in the north: this is exactly the picture that emerges from the lower part of Table 3. The change in the fraction of Romanians and Bulgarians re-incarcerated for economic crimes in the north between 2006 and 2007, as well as the difference-in-differences relative to the control group, are almost twice as large as the ones based on the whole country, while no significant differences show up in the south.

5.2 Cox model

To probe these results further, and to exploit all the information about the dates of release and rearrest, we fit a maximum-likelihood model for the hazard rate of re-incarceration. Following most empirical applications of survival analysis (see Van den Berg, 2001, for a survey), we restrict ourselves to the class of proportional hazard models, in which the hazard rate is the product of a common function of time-at-risk and an exponential function of observable covariates. Specifically, the estimating equation is

$$h(J|t,x) = \lambda(t)f(x) = \lambda(t)\exp(\alpha_0 post + \alpha_1 new\ EU + \beta post \times new\ EU + z'\gamma), \tag{11}$$

where $h(J|t,x)$ is the hazard rate of re-incarceration after t days since the release from prison, conditional on a vector x of observable characteristics, $\lambda(t)$ is a common function of the time-at-risk and $f(.)$ is a function of a dummy *post* for the (calendar) period after the enlargement, a dummy *new EU* for the individuals in the treated group, and the interaction between the two, in addition to other individual characteristics z. Following the semi-parametric approach devised by Cox (1972), we leave the baseline hazard function $\lambda(t)$ totally unrestricted, and estimate the other coefficients by partial maximum likelihood. In this way we take advantage of the tractability of the proportional hazard model while allowing for a significant degree of flexibility in terms of functional form. Standard errors are clustered by Italian region and country of origin to allow for within-network correlation in criminal activity.[22]

The estimated coefficient β captures the difference-in-differences between the hazard rate of re-incarceration for the treated and control groups, before and after the EU enlargement, controlling for group- and period-specific effects, as well as for time-at-risk and other individual characteristics. The exponentiated coefficient of the interaction term, $\exp(\beta)$, provides an estimate for the *hazard ratio* of legal status, i.e. the (constant) percentage effect of legal status on the probability of re-incarceration. Such probability equals the hazard ratio of committing a crime times the probability of going to jail

[21] The economic divide between northern and southern Italy has long been studied, see for instance Eckaus (1961) and Helliwell and Putnam (1995). Estimates of GDP differences are provided by the Italian Statistical Office, http://www.istat.it/it/archivio/52316

[22] In Section 5.3.1 we show that the results change little when we generalize the proportional hazard model to allow for unobserved heterogeneity and when we go for a fully-parametric approach.

conditional on having committed a crime and on legal status,

$$\frac{h(J|L=1)}{h(J|L=0)} = \frac{Prob(J|C,L=1)}{Prob(J|C,L=0)} \frac{h(C|L=1)}{h(C|L=0)}. \tag{12}$$

As long as the probability of incarceration conditional on having committing a crime is constant across the individuals in our sample (as we discussed in Section 4.2), the first term on the right hand side is (approximately) equal to 1, so the exponentiated coefficient (12) provides an estimate for the causal effect of legal status on the probability of committing a crime in Equations (3) and (8).

The results are presented in Table 4. The coefficient of the interaction term is negative, statistically significant (at the 95 percent confidence level) and very high in absolute value. The exponentiated coefficient suggests that the hazard ratio of legal status is about 57 percent. These findings are unaffected when we control for the individual characteristics reported in our data (column 2), which is consistent with the propensity score weighting scheme enhancing the comparability of the two groups in terms of observable characteristics (notice also that the estimate for the group indicator is always very close to 0).

Table 4: Cox model for the hazard rate of re-incarceration

	(1)	(2)	(3)	(4)	(5)	(6)
	baseline		*econ*	*non-econ*	*north*	*south*
new EU	-0.016	-0.027	0.002	-0.215	0.234	-0.256
	(0.219)	(0.216)	(0.208)	(0.987)	(0.245)	(0.307)
post	-0.149	-0.171	-0.277	0.493	-0.343	-0.154
	(0.336)	(0.338)	(0.392)	(0.863)	(0.560)	(0.566)
new EU × *post*	-0.563***	-0.557**	-0.668**	0.243	-0.923**	-0.331
	(0.215)	(0.217)	(0.286)	(1.129)	(0.427)	(0.310)
$\exp(\beta) \approx \frac{h(C\|L=1)}{E(C\|L=0)}$	56.9%	57.3%	51.2%	127.5%	39.7%	71.8%
observations	4,177	4,177	3,653	524	2,013	1,640
controls	NO	YES	YES	YES	YES	YES
χ^2	10.55	30.23	32.78	3.67	29.34	11.60

Note: The table shows the semi-parametric estimates of the Cox model (11) for the hazard rate of re-incarceration. The main explanatory variables are a dummy for citizens of new EU member countries, *new EU*, a dummy for the period after the EU enlargement, *post*, and the interaction between the two. The exponentiated coefficient of the interaction term provides an estimate for the hazard ratio of legal status, see Eq. (12). The specifications in columns (2) to (6) include as additional regressors age, age squared, a dummy for being married and the length of the sentence commuted with the pardon. Regressions are weighted by the inverse propensity score according to Eq. (10). Robust standard errors clustered by Italian region and country of origin are reported in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence.

Columns (3) and (4) distinguish between different types of offenders. In line with the simple comparisons of means in Table 3, the overall effect is driven by a reduction in the criminal activity of individuals who were previously incarcerated for economic crimes, while the hazard rate of re-incarceration increases slightly for violent offenders (although this latter effect is very imprecisely estimated). Finally, columns (5) and (6) confirm that the reduction in the hazard rate for economically-motivated offenders is much stronger in northern regions, which is also consistent with the difference-in-differences shown in Table 3.

The results in Tables 3 and 4 uncover a strong, negative effect of legal status on the hazard rate of committing a crime in Italy. According to such results, the hazard ratio of legal status is slightly above

50 percent for economic crimes, and less than 40 percent in northern regions. This is a sizeable effect. In Section 2.2 we showed that, based on the available estimates of the unofficial foreign population in Italy, the incarceration rate for legal immigrants is about 12.5% of that of illegal immigrants (see Eq. 1). The causal effect of legal status would then account for 1/2 to 2/3 of such difference. The remaining part would be likely explained by (positive) selection into legal status (Eqs. 3 and 9) and differences in the probability of incarceration conditional on having committed a crime, which is constant across the former prison inmates in our sample but could vary by legal status for the rest of immigrants in Italy (see the discussion at the end of Section 4.2).

As for violent offenders, the maximum likelihood estimates are also qualitatively similar to the results in Tables 3; the hazard rate of re-incarceration increases, although the effect is small and is not statistically significant. Interestingly, if we plug a deportation rate of about 15% (see Section 2.3) in Eqs. (2) and (7), and we back out the hazard ratio conditional on not being deported (i.e. the second term on the right hand side of the equations), such number would be very close to zero, $1.27 * (1 - 0.15) \approx 1.07$. This is indeed consistent with the idea that legal status should have little or no effect of the behavior of violent criminals, and the only reason why the crime rate increases for this group is that they are not deported anymore after the acquisition of legal status.

5.3 Robustness

Next we confront our results against several robustness exercises, falsification tests and threats to the quasi-experimental design.

5.3.1 Functional form and unobserved heterogeneity

One first issue concerns the functional form and the estimation method. The flexibility allowed for by semi-parametric estimation of the Cox model comes at a cost in terms of statistical power. For this reason, we estimate a fully parametric model that imposes a logistic form on the probability of re-incarceration. To condition the probability of rearrest on not having being rearrested before, we follow Efron (1988) and estimate the model on the weekly (unbalanced) panel of inmates who are at risk of incarceration during each period.[23] The results, presented in Table 5, are in line with those obtained for the semi-parametric model, both in terms of statistical significance and magnitude.[24]

Another concern is unobserved heterogeneity. A fairly general result in the econometrics of survival analysis is that variation in omitted factors would bias the estimates of both the baseline hazard and the regressors' coefficients, no matter what the correlation between the included and excluded variables may be (see the discussion in Van den Berg, 2001).[25] Notice that our difference-in-differences specification allows for the presence of common unobserved heterogeneity in both the treated and control group. The only serious threat to identification would be the presence of a different degree of heterogeneity in the two groups (i.e. a different distribution of u), but this seems unlikely to the extent

[23] In practice, 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 re-incarcerated in the first week, and so on; in this way we end up with approximately 142 thousand person-week observations. Lee and McCrary (2009) also adopt this strategy to estimate an empirical model of recidivism, while Ashenfelter and Card (2002) apply the same methodology to study retirement choices.

[24] As shown in Allison (1982) and Card and Levine (2000) the two models give similar results as long as the hazard rates are low.

[25] The intuition is easier to see for the baseline hazard: whenever there is omitted heterogeneity in the hazard rates, having individuals with the highest (lowest) hazards leaving the pool at risk earlier (later) would be observationally equivalent to negative duration dependence. A similar argument applies to the derivative of the (log) hazard rate with respect to the vector of covariates.

Table 5: Logit model for the probability of re-incarceration

	(1)	(2)	(3)	(4)	(5)	(6)
	baseline		*econ*	*non-econ*	*north*	*south*
new EU	0.044	0.032	0.070	-0.215	0.245	-0.123
	(0.229)	(0.228)	(0.223)	(0.997)	(0.248)	(0.351)
post	-0.201	-0.135	-0.155	0.032	0.179	-0.611
	(0.237)	(0.296)	(0.354)	(1.004)	(0.424)	(0.527)
new EU $\times$ *post*	-0.623***	-0.598***	-0.711**	0.237	-0.880**	-0.464
	(0.219)	(0.215)	(0.284)	(1.134)	(0.406)	(0.346)
observations	142,124	142,124	124,019	18,105	68,151	55,868
time at risk (quad.)	YES	YES	YES	YES	YES	YES
calendar time (quad.)	NO	YES	YES	YES	YES	YES
controls	NO	YES	YES	YES	YES	YES
χ^2	52.26	81.22	54.73	12.75	73.55	47.07

Note: The table shows the parametric estimates of a Logit model for the probability of incarceration for immigrants from new EU member and candidate member countries before and after the EU enlargement. The main explanatory variables are a dummy for citizens of new EU member countries, *new EU*, a dummy for the period after the EU enlargement, *post*, and the interaction between the two. The panel of observations is unbalanced because we include only the individuals who are at risk of rearrest in any given week. The specification always include a quadratic polynomial for the duration of time at risk, columns (2) to (6) include in addition a quadratic polynomial for calendar time and additional individual characteristics, namely age, age squared, a dummy for being married and the length of the sentence commuted with the pardon. Regressions are weighted by the inverse propensity score according to Eq. (10). Robust standard errors clustered by Italian region and country of origin are reported in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence.

that such groups are similar in terms of observable characteristics and pre-enlargement outcomes (see Figure 8 and Table 3).

In any event, we augment the proportional hazard model in (12) with an unobserved component u, distributed according to the density function g,

$$h(J|t, x, u) = \lambda(t)f(x)g(u),$$

in order to take unobserved heterogeneity into account. The last expression is the general form of 'mixed proportional hazard' models, in which the hazard rate depends in a multiplicative way on a baseline function of time-at-risk, a vector of observable covariates and an unobserved component.

The model can be estimated parametrically assuming that $g(.)$ is a Gamma distribution and $\lambda(t)$ is a parametric function of the time-at-risk t. We estimated both a Weibull and an exponential model, and we could not reject the latter (i.e. λ is simply a constant). Table 6 compares the estimates of the exponential model with and without unobserved heterogeneity. No matter which specification and sample are chosen, without controlling for unobserved heterogeneity the estimated coefficients of the exponential model are very similar to those of the Cox model. Most importantly, allowing for unobserved heterogeneity increases the magnitude of the interaction coefficient, suggesting an even lower hazard ratio of legal status (about 40% for economic offenders, down to 27% in northern regions).

5.3.2 Placebo regressions

Another concern is that the results might depend on the choice of the control group. While candidate EU member countries represent a natural control group for new EU member countries, we want to make sure that such choice does not drive the results. For this reason, we randomly draw 1,000 samples

Table 6: Exponential hazard model with and without unobserved heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		baseline			*econ*	*non-econ*	*north*	*south*
new EU	-0.014	-0.021	-0.026	-0.057	-0.023	-0.301	0.248	-0.332
	(0.220)	(0.254)	(0.217)	(0.253)	(0.239)	(0.871)	(0.288)	(0.371)
	0.986	0.980	0.974	0.945	0.977	0.740	1.282	0.717
post	-0.469***	-0.161	-0.468***	-0.090	-0.127	1.054	0.058	-0.039
	(0.136)	(0.236)	(0.136)	(0.312)	(0.394)	(1.590)	(0.517)	(1.127)
	0.625	0.851	0.626	0.914	0.881	2.870	1.060	0.962
new EU × *post*	-0.559***	-0.779***	-0.554**	-0.803**	-0.916**	1.088	-1.321**	-0.573
	(0.216)	(0.292)	(0.218)	(0.331)	(0.458)	(2.393)	(0.671)	(0.637)
$\exp(\beta) \approx \frac{h(C\|L=1)}{E(C\|L=0)}$	0.572	0.459	0.575	0.448	0.400	2.969	0.267	0.564
heterogeneity	no	yes	no	yes	yes	yes	yes	yes
controls	no	no	yes	yes	yes	yes	yes	yes
χ^2	31.93	11.56	51.51	31.95	34.77	3.49	31.30	13.64
observations	4,177	4,177	4,177	4,177	3,653	524	2,013	1,640

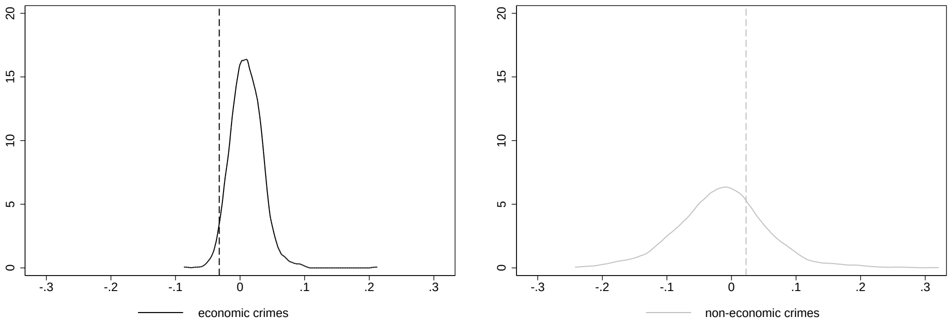
Note: The table shows the parametric estimates of an exponential hazard model for the probability of incarceration for immigrants from new EU member and candidate member countries before and after the EU enlargement. The main explanatory variables are a dummy for citizens of new EU member countries, *new EU*, a dummy for the period after the EU enlargement, *post*, and the interaction between the two. The specifications with additional controls include in addition a quadratic polynomial for calendar time and additional individual characteristics, namely age, age squared, a dummy for being married and the length of the sentence commuted with the pardon. The unobserved heterogeneity is assumed to be distributed according to the gamma distribution. Regressions are weighted by the inverse propensity score according to Eq. (10). Robust standard errors clustered by Italian region and country of origin are reported in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence.

of inmates from outside the new EU member or candidate member countries and re-run the estimates pretending that each of these placebo samples is the true treated group (we maintain the same group size, 724 individuals). Figure 9 shows that the densities of the placebo effects are centered around zero, suggesting that the change in recidivism experienced by the control group after the EU enlargement is not abnormal; what is clearly abnormal is the estimated effect for the true treated group (vertical line), but only for economically-motivated offenders. The p-value reported in the last column of Table 7 equals the probability that the effect estimated for a randomly drawn placebo sample is negative and larger, in absolute value, than the one estimated for the true treated group. Such probability is always very low for the sub-sample of economically motivated offenders (0.1 percent when estimated from the Cox model), while the p-value is 25 to 27 percent for the case of violent offenders. In general, the patterns observed for the Romanians and Bulgarians are clearly abnormal relative to those obtained for the placebos in terms of difference-in-differences before and after the EU enlargement. The placebo regressions confirm also our previous results about the differential effects between economic vs. violent offenders.

5.3.3 Placebo dates of enlargement

We also run a series of falsification tests at different placebo dates (rather than across placebo groups), so to exclude that the difference-in-differences coefficients capture the effects of other events besides the EU enlargement. In the spirit of the Andrews (1993) test for identifying structural changes with unknown break points, Figure 10 plots the ratio of the R^2 of the difference-in-differences model estimated at each placebo date in our sample period over the R^2 of a restricted specification without the interaction term, distinguishing between different subsamples. The most likely break point for Italy as a whole is December 12, which is very close to the official date of the enlargement and consistent with the fact that immigrants would rationally anticipate the policy change and modify their behavior

Figure 9: The density of placebo effects



Note: The figure plots the distribution of placebo effects obtained for 1,000 samples that have the same size of the treated group (724 individuals), but are randomly drawn from the population of former inmates that belong neither to the treated nor to the control group. The effects estimated for the true treated groups are shown with a vertical line.

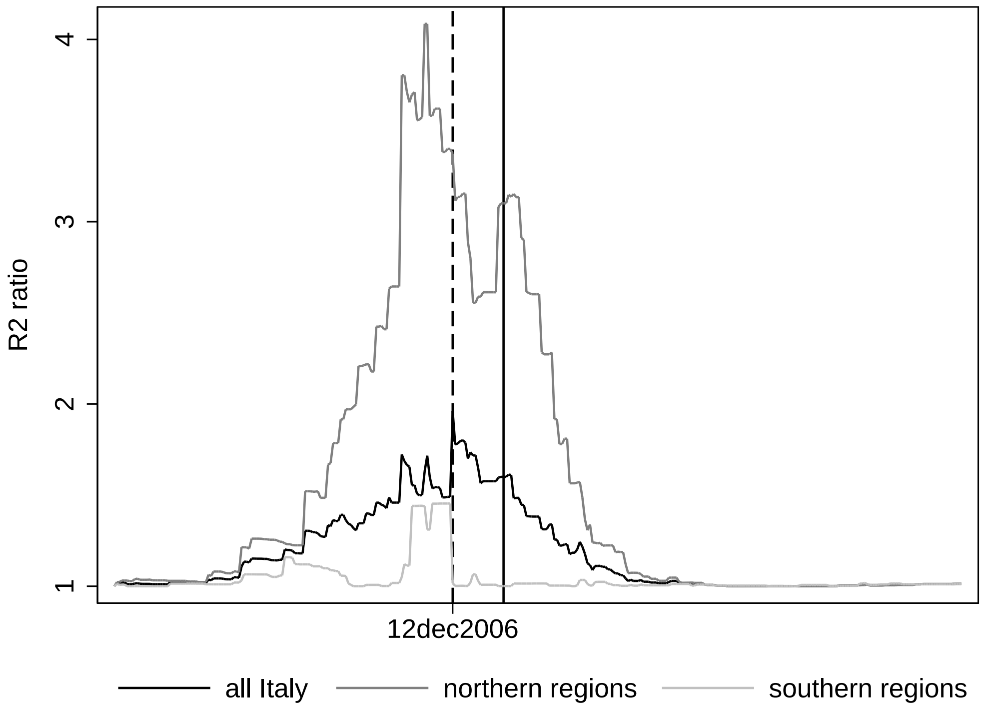
Table 7: Comparison between the actual treated group and the placebo samples

	PLACEBO SAMPLES					NEW EU	
	mean	**stdev**	**p5**	**p50**	**p95**	**coeff**	**p-value**
	difference in difference						
economic crimes	0.010	0.024	-0.027	0.009	0.048	-0.032	0.026
non-economic crimes	-0.014	0.071	-0.130	-0.013	0.098	0.023	0.273
	Cox model, interaction coefficient						
economic crimes	0.212	0.271	-0.225	0.205	0.687	-0.668	0.001
non-economic crimes	-0.166	2.883	-1.920	-0.405	1.458	0.243	0.249

Note: The table compares the estimates obtained for 1,000 placebo samples with those obtained for the treated group (Romanians and Bulgarians). The placebo samples have the same size of the treated group (724 individuals) but are randomly drawn from the population of former inmates that belong neither to the treated nor to the control group. The first two columns show the mean and standard deviation of the distribution of the difference-in-differences (first two rows) and interaction coefficients in the Cox model (last two rows) estimated for the placebo samples; the third to fifth column report the 5th, 50th and 95th percentiles of the same distribution. Finally, the last two columns report the coefficients estimated for the actual treated group and their p-values relative to the distribution of placebo estimates.

as uncertainty unravels during the last weeks of 2006 (see Section 4.1). The break-points estimated separately for northern and southern regions are very similar (December 1 and December 7), although the additional explanatory power is much greater in the former case (consistently with the evidence in Tables 3-5).

Figure 10: Structural break test



Note: The graph plots the ratio of the R^2 of the difference-in-differences in re-incarceration between treatment and control groups before and after each possible (placebo) treatment date in our sample period over the R^2 of a restricted specification without the difference-in-differences term. The vertical dashed line corresponds to the day that maximizes the R^2-ratio (i.e. the most likely break point), while the vertical solid line is the official date of the EU enlargement.

5.3.4 Attrition

In principle, the Romanians and Bulgarians in our sample could have moved to other EU countries after obtaining legal status in all the EU, in which case the reduction in the hazard rate of re-incarceration would be due to the fact that they spend less time in the pool at risk (rather than by changes in criminal behavior). Yet, there are at least three reasons why differential attrition between the treated and control group can not explain the results (or may actually bias our estimates toward zero). First, the tourist visa waiver granted in 2001 to Romanians and Bulgarians, and the absence of border enforcement within the EU area (see Section 4.1), allowed for a significant mobility already before 2007. Indeed, the 90-day term limit imposed on the visa waiver was computed separately for each destination country in the EU so, between 2001 and 2007, Romanians and Bulgarians had the greatest incentives to travel frequently across different countries within the EU in order to avoid falling into illegality. Therefore, the attrition rates for the treated group should have been higher *before* 2007, in which case our estimates of the difference-in-difference effect would be biased toward zero. Second, our findings point at a greater effect for economically-motivated offenders in regions that offer relatively better income opportunities to legal immigrants. There is no clear reason why outflows toward other

EU countries should have been greater for such categories of immigrants; if anything, we should expect the opposite. Finally, enhanced mobility after the acquisition of legal status would imply that the change in the observed re-arrests occurred after the admission to the EU, while the structural break test identifies it a few weeks before the enlargement.

5.3.5 Interactions in crime

Finally, we address the concern that the (change in) criminal behavior of Romanians and Bulgarians may have influenced the behavior of the control group, due to interactions in crime between different communities of immigrants. If immigrants in the control group increased their criminal activity in response to the decrease by Romanians and Bulgarians, i.e. if there are substitution effects, our estimates would be inflated; the opposite would occur in case of complementarities.

To address this issue we include explicitly interactions in crime into our empirical specification, allowing the hazard rate of each individual to depend on that of his peers, both in his own and in the other group, and we follow Drago et al. (2009) and Drago and Galbiati (2012) in exploiting quasi-random variation in commuted sentence to identify such effects. In Section 4.2 we mentioned that, whenever released inmates are re-arrested within 5 years, the commuted residual sentence gets added to the new one. Conditional on the initial sentence, the commuted sentence varies only by the date in which the first crime was committed, which in turn generates quasi-random variation in the expected future sentence. Drago et al. (2009) exploit this source of variation to identify the deterrence effect of expected sentence on the probability of re-incarceration, while Drago and Galbiati (2012) allow such probability to depend also on the residual sentence of an inmate's peers, defined as inmates of the same nationality serving time in the same prison.

We further extend their specification to allow also for interactions in crime between different communities. Specifically, for each individual in the treated group we form two distinct peer groups, including respectively all inmates in the treated and control group that served time in the same prison; we define analogously two peer groups for each individual in the control group. A negative coefficient for the average residual sentence of the peers from the other community would suggest that peer effects go over and beyond the same nationality, i.e. there are complementarities in crime across nationalities. A positive coefficient would indicate instead that inmates of a different nationality compete, i.e. there is substitution in criminal activity.

The results are presented in Table 8. Within our sample of new EU member and candidate member countries, we do find that (conditional on the initial sentence) one's own residual sentence length lowers the hazard rate of rearrest, but the coefficients on the average residual sentence of peers of the same nationality is not significantly different from zero (column 1). Only if we use a larger sample, including all foreign inmates we find that peer effects within the same nationality matter (column 4). Most importantly for our purposes, the average residual sentence of the peers who belong to the other group does not seem to matter at all. The corresponding coefficient is never significantly different from zero, no matter whether we control (columns 3 and 6) or not (columns 2 and 5) for the average sentence of the peers of the same community. The coefficient is particularly small for our sample of new EU member and candidate member countries (columns 2-3).

The absence of direct interactions between inmates in the treated and control groups and their peers in the other group does not exclude the possibility of other (indirect) influences in equilibrium. For instance, the other communities could overtake the criminal activities abandoned by Romanians and Bulgarians, in which case our estimates would be upward biased; alternatively, police forces

Table 8: Interactions in criminal activity between treated and controls, estimates of peer-effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Treatment and Control Group			All Foreign Inmates		
residual sentence	-0.015*	-0.014*	-0.015*	-0.012***	-0.013***	-0.012***
	(0.008)	(0.008)	(0.009)	(0.003)	(0.003)	(0.003)
peer residual sentence (same nationality)	0.010		0.011	-0.009**		-0.011**
	(0.007)		(0.007)	(0.005)		(0.005)
peer residual sentence (other nationalities)		-0.001	-0.004		0.014	0.013
		(0.011)	(0.011)		(0.010)	(0.010)
sentence	-0.007*	-0.006	-0.007*	-0.004**	-0.005***	-0.004**
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
controls	YES	YES	YES	YES	YES	YES
observations	1767	1746	1746	6567	6565	6565
pseudo R-sq.	0.0111	0.0103	0.0115	0.00689	0.00640	0.00699
N. clusters	331	320	320	1121	1120	1120
$\chi 2$	36.92	32.99	36.94	124.47	121.35	124.54

Note: The table shows hazard ratios of a Cox proportional hazard model. The sample for the first 3 columns includes all inmates from new EU member and candidate member countries released after the July 2006 collective pardon. The remaining columns use a sample that contains all foreign inmates. Robust standard errors clustered by Italian region and country of origin are reported in parenthesis. *, ** and *** denote coefficients significantly different from zero at the 90% confidence, 95% confidence and 99% confidence.

could target these other communities more intensively after the decrease in crime by Romanians and Bulgarians, in which case our estimates would be downward biased. Figure 11 plots the change (between 2006 and 2007) in the number of crimes committed by all Romanians in Italy against the same changes for the nationalities included in the control group, for different types of crime.[26] A clearly positive correlation emerges between the two. While we can hardly attach any causal interpretation to this finding, this would hardly be consistent with strong substitution effects between the treated and control group. If the positive correlation was driven by complementarities, our estimated effects of legal status would be biased toward zero.

6 Conclusions

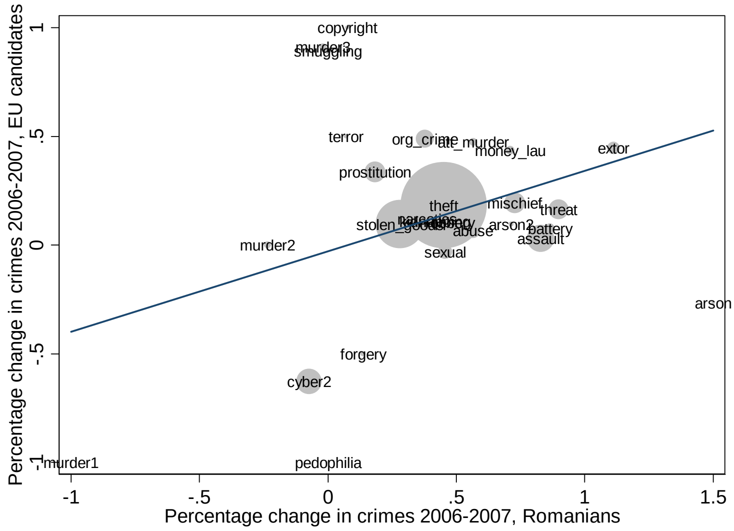
We use a natural experiment, namely the last round of the EU enlargement, to identify the causal effect of legal status on immigrants' crime. We provide a theoretical framework that illustrates the two main effects of legal status: on the one hand, it increases crime by precluding deportation of potential foreign criminals; on the other hand, it lowers the propensity to engage in crime by providing immigrants with alternative (legitimate) 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 hazard rate of committing a crime decreases by about 50 percent after obtaining legal status as a consequence of the EU enlargement.

This is indeed a large effect. Still, it does not account for the entire difference in crime rates between legal and illegal immigrants, as computed in Section 2.2. The remaining part (between 1/3 and 1/2) is likely driven by selection of immigrants into legal status and differences in the probability of incarceration conditional on having committed a crime (which is constant across the pardoned inmates in our sample but may vary in general between legal and illegal immigrants). This strengthens the argument for a quasi-experimental approach, in order to correctly identify the effect of legal status.

What about the external validity of our results? Admittedly, former prison inmates represent a very peculiar group, so these findings can not be easily generalized to the rest of the immigrant

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

Figure 11: criminal offenses committed by immigrants from new EU member and candidate member countries, change over the 2006-2007 period



Note: The 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.

population. But the great majority of people never engage in any type of serious crime, which is why previous offenders represent an interesting sample to look at. This segment of the population is more likely to lie at the margin between a criminal career and legitimate activity, which is why recidivism studies form the bulk of the individual-level evidence in the empirical crime literature (see for instance Witte, 1980; Lee and McCrary, 2009).

From a policy perspective, our findings suggest that the consequences of migration policy depend crucially on enforcement. Whatever level of migration quotas is fixed, it should be enforced, in order to prevent the formation of a pocket of illegal immigrants with a very low opportunity cost of engaging in crime. Starting in 2010 the US has been speeding deportations of convicted criminals and halting those of illegal immigrants without convictions. According to the US Immigration and Customs Enforcement agency, in fiscal year 2011, 216,698 immigrants who were convicted of felonies or misdemeanors were deported, representing 55 percent of all deportations.[27] On top of their incapacitation effect, such selective deportations are also likely to represent additional deterrence against crime committed by immigrants.

Finally, we focus only on the effects of the EU enlargement on the criminal activity of undocumented immigrants already in Italy before the enlargement. Yet, changes in migration policy have far-reaching consequences for the size and composition of migration inflows. In particular, subsequent rounds of the EU enlargement or amnesties of formerly undocumented immigrants are likely to attract new immigrants from abroad, whose characteristics may differ considerably from those of previous migration waves. Policy makers would also need to estimate the costs and benefits along this additional dimension, which, however, goes beyond the scope of the present paper.

[27]See http://www.ice.gov/removal-statistics/.

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Appendix

In this section we characterize the expected utility function $V^e(L,a) \equiv \int V(L,a,z)dF(z)$ and $\Delta V^e(a) \equiv V^e(1,a) - V^e(0,a)$ in Section 3. Starting from equation (5) and subtracting $\rho V^e(L,a)$ from both sides we obtain

$$\begin{aligned} V(L,a,z) - \rho V^e(L,a) = E(D|L)\left[V_H - \rho V^e(L,a)\right] + \left[1 - E(D|L)\right] \\ \times \left[\max\left\{V_C(L,a,z) - \rho V^e(L,a); 0\right\} + w(L)a\right] \end{aligned}$$

Integrating with respect to $F(z)$, noticing that

$$\int_0^{z^*(L,a)} \max\left\{V_C(L,a,z) - \rho V^e(L,a); 0\right\} dz = 0$$

and

$$\begin{aligned} \int_{z^*(L,a)}^{+\infty} \max\left\{V_C(L,a,z) - \rho V^e(L,a); 0\right\} dz &= \int_{z^*(L,a)}^{+\infty} \left[V_C(L,a,z) - \rho V^e(L,a)\right] dz \\ &= \int_{z^*(L,a)}^{+\infty} \left[z - z^*(L,a)\right] dz, \end{aligned}$$

and integrating by parts the above expression delivers

$$\begin{aligned} V^e(L,a)\left(1-\rho\right) = E(D|L)\left[V_H - \rho V^e(L,a)\right] + \left[1 - E(D|L)\right] \\ \times \left[\int_{z^*(L,a)}^{+\infty} \left[1 - F(z)\right] dz + w(L)a\right]. \end{aligned} \tag{13}$$

Proof 1: $\Delta V^e(a) \geq 0,\ \forall a \geq a_0$. By contradiction: assume that $\exists a' \geq a_0$ such that $\Delta V^e(a') < 0$. Then Eq. (13) would imply that

$$\begin{aligned} \int_{z^*(1,a')}^{+\infty} \left[1 - F(z)\right] dz + w(1)a' < E(D|0)\left[V_H - \rho V^e(0,a')\right] + \left[1 - E(D|0)\right] \\ \times \left[\int_{z^*(0,a')}^{+\infty} \left[1 - F(z)\right] dz + w(0)a'\right] \end{aligned} \tag{14}$$

Since $V_H < \rho V^e(0,a')$, $\forall a' \geq a_0$ and $\Delta w \geq 0$, a necessary condition for Eq. (14) to hold is that $\Delta z^* > 0 > \Delta V^e$; but Δz^* and ΔV^e having a different sign contradicts condition (6).

Proof 2: $\frac{\partial \Delta V^e}{\partial a} \geq 0$. Using again Eq. (6), $\frac{\partial \Delta V^e}{\partial a} \geq 0 \Leftrightarrow \frac{\partial z^*(1,a)}{\partial a} \geq \frac{\partial z^*(0,a)}{\partial a}$. Applying the implicit function theorem and the Leibniz rule to equation (13),

$$\frac{dz^*(L,a)}{da} = \frac{\rho \pi w(L)}{1 - \rho\left[1 - E(D|L)\right]\left\{1 - \pi\left[1 - F(z^*(L,a))\right]\right\}}$$

A sufficient condition for having $\frac{\partial \Delta z^*}{\partial a} \geq 0$ is that $\Delta z^* \propto \Delta V^e \geq 0$. But this is always true over the interval $a \geq a_0$ by the proof 1 above.