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Rethinking the crime reducing effect of education: the  
role of social capital and organized crime

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**Max Weber Programme**

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## **Abstract**

This paper estimates the causal effect of education on adolescent crime by exploiting a compulsory education reform implemented in 1999 in Italy. To identify the causal relation we use the reform as an instrument for adolescent high school enrollment, and compare the offending rates of the cohorts affected by the reform with the ones not affected. We find that one percentage point increase in the enrollment ratio reduces adolescent crime by 2.47 percent, and that the effect is highly heterogeneous across areas and mostly influenced by the degree of social capital and by the presence of organized crime. In areas characterized by pervasive organized crime, preventing adolescents from staying on the streets, by keeping them at school, is not enough to ensure the same crime reduction effect as in areas where organized crime is not pervasive.

## **Keywords**

Adolescent crime; school attendance; organized crime; social capital.

## **JEL Classification**

I20, I28, J13, K42

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# 1 Introduction

Growing up in high criminal density areas means having to deal from an early age with altered economic, political and social contexts. The legal economy is distorted by the illegal rules, wiping out any possibility for clean market competition, while a parallel economy made of drug dealing, usury and extortion flourishes and subtracts human and financial resources from the legal economy, preventing its development (Saviano, 2012). Starting from the late 1990s, all industrialized countries have been characterized by a dramatic increase in juvenile crime; the lack or the insufficiency of supervision by parents or institutions has been seen as the main predictor of delinquency (UN, 2003). However, little is known about the determinants of criminal behavior in areas at risk of criminal associations and on the potential effects of policies to be used to counter adolescent crime in these areas.

In this paper we consider one characteristic, which has been broadly identified as an important policy instrument against criminal behavior, i.e. education. Education and school attendance may affect juvenile crime through several channels (Hjalmarsson and Lochner, 2012, Jacob and Lefgren, 2003). School may have an incapacitating effect, through which it prevents adolescents from committing crime, just because criminal opportunities are generally more limited at schools than on the streets. Of course, this effect depends on the ease with which students can engage in criminal activities or can displace them during the time outside school (Anderson et al., 2013). A second mechanism operates through the improvement of skills, which are remunerated in the labor market and should change the decisions to engage in criminal activities through a change in the expected probability of finding a legal job after school. This human capital effect may affect youth crime immediately, but it may also last over time. Finally, social networks may play a substantive role in shaping adolescent crime, mainly because risky and criminal behaviors are particularly influenced by peers, especially in the years of adolescence (Bayer et al., 2009, Card and Giuliano, 2013, Patacchini and Zenou, 2009). While the incapacitation and the human capital channels imply negative effects of education on crime, the

effects of social interaction cannot be established *a priori* as peer effects may either reinforce or attenuate the individual propensity to engage in risky behaviors.

In this paper we examine the empirical relationship between high school attendance and adolescent involvement in criminal activities by exploiting a school leaving age reform, implemented in Italy in 1999 (hereafter, simply referred to as the *Reform*), which increased compulsory education by one year. Italy is an interesting setting for the study of the effects of education on youth crime and for assessing whether the crime reducing effect of education, found in several pieces of this literature, might be affected by strong differences in social capital, institutions, school characteristics and, most importantly, the presence of Mafia-type criminal organizations. As documented by Guiso et al. (2013), Nannicini et al. (2013), Paccagnella and Sestito (2014), there is a large territorial heterogeneity in the social capital and cultural related traits that can be traced back to distant political and historical factors; the country is also characterized by significant differences in terms of Mafia pervasiveness (Bandiera, 2003, Barone and Narciso, 2013, Daniele and Marani, 2011, Pinotti, 2012). The latter point is crucial, since, as outlined in the quotation above, the school can be one of the few policy instruments to be used to counter adolescent involvement in criminal organizations (Saviano, 2012), which has dramatically increased in recent years. Recent data from the National Bureau of Statistics and the Italian Ministry of the Interior have shown that, from 2007 to 2011, the number of teenagers under 17 years old charged with the specific offense of *mafia-type* criminal association has increased by 23.7 percent in Italy.<sup>1</sup>

In the last decades, several empirical works have assessed the effects of education on criminal behavior (Lochner, 2011). The work closest to our empirical strategy is that of Machin et al. (2011), which assesses the causal effect of education on the crime rate exploiting a compulsory leaving age reform, implemented during the 70s in UK. Machin et al. (2011) focus on the long-term effect of education on crime, since they look at the crime rates of adult males. A few

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<sup>1</sup>Art. 416bis of the Italian Penal Code pertains to the specific offense of *mafia-type* criminal association. ISTAT Police Force database, national data from 2007 to 2011 (available at <http://dati.istat.it>).



empirical studies have looked at the contemporaneous effects of educational policies on adolescent crime and most of them focus on US data (Hjalmarsson and Lochner, 2012).<sup>2</sup> Anderson (2013), Bertheleon and Kruger (2011), Luallen (2006) and Jacob and Lefgren (2003) estimate, in a reduced form setting, the effects of different types of school policies which induced exogenous variation in the time spent at school by young students, while Heller et al. (2013) analyze the effect of a randomized experiment aimed at increasing adolescent time spent at school in high-crime neighborhoods of Chicago. Anderson (2013) estimates that the exposure to a minimum drop-out age of 18 reduces the arrest rate for 16 to 18 year olds by 10.27 incidences per 1,000 individuals of the age group population. Bertheleon and Kruger (2011) find that a school reform that lengthened the time spent at school from half to full-day in Chilean municipalities significantly decreases youth crime rates and teen pregnancies. Luallen (2006) and Jacob and Lefgren (2003) examine the effects of extra days off from school because of teachers in-service days or teacher strikes: Luallen (2006) shows that total juvenile crime increases by an average of 21.4 percent on days when strikes occur; Jacob and Lefgren (2003) find that property crimes decrease by 14 percent on days when the school is in session, while violent crimes increase by 28 percent over the same days. To the best of our knowledge, only Luallen (2006) takes into account how the effect of education on crime might differ because of territorial characteristics: in fact, he shows that the sizable effects on youth crime rates come from urban areas only. Recently, some studies have looked at the connections between social capital and crime. For instance, Akomak and ter Weel (2012), using data from the Netherlands, show that social capital is negatively correlated to measures of crime and it is able to explain a large part of the territorial heterogeneity in crime. Similarly, Buonanno et al. (2009) use data from Italian provinces to investigate the effects of social capital on crime rates. The authors find that both civic norms and associational networks have a negative and significant effect on property crimes.

This paper contributes to the literature in three significant ways. First, we provide causal

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<sup>2</sup>Only Bertheleon and Kruger (2011) use data from Chile.

estimates of the contemporaneous effects of high school attendance on adolescent crime. In fact, we provide both the reduced form effect of the Reform (i.e. the intention-to-treat parameter, ITT) and the local average treatment effect (LATE), exploiting the exogenous variation in the enrollment rate induced by the Reform in an instrumental variable setting. While the ITT may represent the most policy relevant parameter, only through 2SLS estimation we can provide evidence of the causal link between education and crime measures. Second, our estimates are based on newly available administrative records which precisely identify age and gender of all the offenders, as well as the province where the offense took place. This means that we can identify not only youths who have been jailed or arrested, but also all others who have been reported to the judicial authority and then have benefited from alternative measures to prison. In fact, most of the adolescents reported to the judicial authority benefit from *ad hoc* measures, such as correctional youth centers, family control or jail suspension (Dipartimento di Giustizia Minorile, 2007). Third, in addition to documenting a crime reducing effect of education induced by the reform under consideration, we also show how this effect changes across the Italian territory, and to which extent it depends on local institution, school characteristics and the presence of organized crime. To this purpose, we exploit classical measures of social capital as a proxy for civic capital, policy relevant measures of school characteristics, such as average class size and student-to-teacher ratio, and measures of the pervasiveness of mafia-type criminal organizations taken from the criminology literature.

To assess the causal effect of education on adolescent crime, we exploit a newly available dataset containing aggregated administrative records of all 14, 15 and 16 year olds reported by the police to the judicial authorities, matched with enrollment rates in the first, second and third year of high school. We provide causal estimates of the effects of youth enrollment ratio on adolescent offending rate, adopting a 2SLS empirical strategy and comparing the offending rates of the cohorts affected by the reform (our treatment group) with those of the cohorts not affected by the reform (our control group). We also exploit this exogenous shift in the enrollment ratio

to retrieve heterogeneous effects of education on crime in areas characterized by different levels of social capital or by the pervasive presence of organized crime. Italy and the 1999 Compulsory Education Reform provide an ideal setting for this study. While the 1999 Reform increased compulsory education across the country by one year, without any difference in its timing and modes of implementation, it exerted a higher incapacitation effect in the South of the country than it did the North. Southern regions are also the ones where organized crime and Mafias are highly pervasive in the social, political and economic contexts. The empirical evidence we provide is also particularly interesting as, differently from other works establishing causal links between minimum drop-out laws and youth crime, we have the opportunity to focus on a very recent educational reform.<sup>3</sup>

Our baseline results show that a one percentage point increase in enrollment rate reduces adolescent offending rate by 2.47 percent. The reduced form estimate of the effect of the reform on the juvenile offending rate indicates that increasing compulsory education by one year reduces adolescent crime by 22.4 percent. We also find that the crime reducing effect of education is highly heterogeneous across the Italian territory, and mostly influenced by social capital, school characteristics and the presence of organized crime. Although experiencing a higher increase in enrollment after the implementation of the Reform, provinces in Southern Italy are characterized by *lower* crime reducing effect of education: a one percentage point increase in enrollment rate would decrease the adolescent offending rate by 3.4 percent in Northern provinces and by less than a half in the Southern ones. In areas characterized by higher levels of social capital, the crime reducing effect of education is stronger (in absolute value) than in areas with lower levels of social capital; school characteristics indicate that areas with more students per class, less monitoring and a worse teaching environment are characterized by a lower crime reducing effect of education. Eventually, the crime reducing effect of education is found to be lower in regions characterized by a pervasive presence of Mafia-type organizations. When we distinguish between

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<sup>3</sup>For example, Machin et al. (2011) use a school leaving age reform which took place in England and Wales in 1972.

the North and the South of the country, we find that school characteristics play a role only in Northern areas, while social capital and organized crime matter only for provinces in the South.

The rest of the paper is organized as follows. Section 2 describes the Italian education system and the 1999 Compulsory Education Reform, Section 3 presents our identification strategy and Section 4 describes the data and provides descriptive statistics. In Section 5 we present the results, while in Section 6 we conduct several robustness checks. Section 7 concludes and derives policy implications.

## 2 The 1999 Compulsory Education Reform

The school system in Italy starts with five years of primary school (grades 1 to 5, corresponding to ISCED level 1) and three years of junior high school (grades 6 to 8, ISCED level 2). These two compose the *first cycle* of the school path which is identical and compulsory for all students. Secondary education lasts two, three or five years depending on the path chosen (vocational, technical, academic). Children enroll in the first grade of primary school the year they turn six, start junior high school when they turn eleven, and enroll in the first grade of high school the year they turn fourteen. Each student must attend school for at least 8 years and obtain the *junior high school Diploma* at the end of the compulsory education path (i.e. at the end of the *first cycle*).<sup>4</sup>

The 1999 Compulsory Education Reform extended compulsory education by one year (from 8 to 9 years).<sup>5</sup> The Reform was aimed at increasing high school attendance and the minimum drop-out age, which was considered relatively low compared to most European countries (Benadusi and Niceforo, 2010). The direct effect of the Reform was that, starting from the school year 1998/99, all students enrolled in 8th grade could not drop out after the completion of ju-

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<sup>4</sup>This limit was set by the Italian Constitutional Law (1948) which came into force in 1948, right after World War II. Ordinary laws, as the Reform we are going to illustrate, can increase, but not decrease, the minimum drop out age (Art. 34). Notice that students are not allowed to dropout before having completed this *first cycle*.

<sup>5</sup>Law No. 9/1999 and Ministry Decree 9 August 1999, No. 323. It is usually referred to as the *Berlinguer Reform*, from the surname of the Public Education Ministry who signed it. Throughout the paper we will simply refer to it as the *Reform*.

nior high school and were obliged to enroll and attend, at least, the first year of high school.<sup>6</sup> The additional year of education had to be carried out attending lessons in a high school (academic, technical or vocational), and could not be performed in regional training centers nor with apprenticeship contracts.

[Figure 1 here]

[Figure 2 here]

Figure 1 shows the increasing trend of the enrollment ratio for 14-18 year-old Italian adolescents over almost 20 years (from school year 1981/82 to school year 2009/10). The 14-18 year-old enrollment ratio experienced a sharp and steep increase in the years immediately after the introduction of the Reform. The vertical lines in the Figure indicate the period considered in the empirical analysis, ranging between school years 1997/98 and 2003/04.

Figure 2 focuses on the average enrollment ratio of 14-16 year-old students in the period of implementation of the Reform. The enrollment ratio is obtained as the share of students enrolled at a certain grade with respect to the corresponding age population.<sup>7</sup> The Reform was applied in the same way and with the same timing over the whole country but determined different effects across the territory, aligning the territorial differences in the enrollment rates, which were remarkably lower in the South of the country in the pre-Reform years. This evidence, together with the richness of the administrative records collected, allows not only to estimate the empirical relationship between enrollment and crime, but also to study how the same policy prescription which increased school incapacitation (i.e. one additional year of compulsory schooling) could have had differential effects on adolescent crime across the territory and which elements might help to explain such differences.

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<sup>6</sup>The first school year when the Reform came into force was 1999/00, and it was first applied to those students who were enrolled in the 8th grade in the school year 1998/99.

<sup>7</sup>For details, see Section 4.1

### 3 Identification strategy

Our aim is to estimate the causal effect of education on adolescent criminal behavior. The identification strategy considers the following equations:

$$Crime_{apt} = \theta_0 + \theta_1 Edu_{apt} + \theta_2 X_{apt} + \theta_3 W_{pt} + (1 + \eta_j)\phi_t + \omega_p + \nu_{apt} \quad (1)$$

$$Edu_{apt} = \delta_0 + \delta_1 Reform_{at} + \delta_2 X_{apt} + \delta_3 W_{pt} + (1 + \eta_j)\phi_t + \omega_p + \epsilon_{apt} \quad (2)$$

where  $Crime_{apt}$  represents a measure of criminal activity of the teenagers of age  $a$  living in province  $p$  in year  $t$ , and  $Edu_{apt}$  represents an aggregate educational measure of the cohort of age  $a$  living in province  $p$  in year  $t$ .<sup>8</sup> The vector  $X_{apt}$  includes control variables at province level varying over time and age group, while the vector  $W_{pt}$  includes several socio-economic indicators referred to each province  $p$  in each year  $t$ . More precisely, we control for several socio-economic characteristics, which have been considered potential determinants of juvenile crime by the existing literature (Anderson, 2013, Buonanno and Leonida, 2009, Machin et al., 2011): the vector  $X_{apt}$  includes demographic variables (i.e. the share of youth living in an urban area, the share of males), while the vector  $W_{pt}$  includes the population at the provincial level, the average labor force participation rate for 15-24 year olds, which is intended to proxy for the (legal) labor market opportunities, and provincial per capita value added as a proxy for the general level of wealth in each province.  $\phi_t$  are year fixed effects,  $\omega_p$  are province fixed effects and  $\eta_j$  are fixed effects at the macro-region level, with  $j = 1, \dots, 5$ .<sup>9</sup> Province and year fixed effects control for time invariant unobserved heterogeneity in provincial characteristics and for year trends in adolescent crimes, respectively. The interaction between year and macro-area dummies captures relevant territorial dynamics which cannot be included in the vector of control variables, such

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<sup>8</sup>See Section 4.1 for the detailed description of the education and crime measures used.

<sup>9</sup>The five macro areas include North-West, North-East, Center, South, Islands.

as macro shocks to the economy which may have different impacts in the areas of the country or population dynamics (i.e. immigration inflows).<sup>10</sup>

Equation 1 represents the structural equation describing the contemporaneous relationship between education and crime. However, estimating Equation 1 using OLS, even after controlling for several observed and unobserved characteristics, would lead to biased estimates of the effect of education on crime, because, still, there may be a correlation between adolescent unobserved traits and their educational choices. For instance, the estimate of education on adolescent crime might be biased by the fact that we are not able to control for all the determinants of criminal behaviors, such as the opportunity cost of crime and the profitability of criminal activities, which may also affect adolescent educational choices. Moreover, since we are interested in estimating the contemporaneous effects of education on crime, there may be reverse causality between adolescent criminal behavior and their educational decisions: in this case, we could observe a negative correlation between education and crime just because the returns of criminal activities are so high that investments in education appear not to be convenient (Machin et al., 2011).

To identify the causal effect of education on juvenile crime we adopt a 2SLS estimation, exploiting the exogenous variation in school attendance induced by the school leaving age reform described in Section 2. We first estimate Equation 2 that gives the enrollment rate of adolescents of age  $a$  in province  $p$  for each year  $t$  as a function of a binary variable  $Reform_{at}$ , which is equal to one for the cohorts affected by the 1999 Reform and zero otherwise. Our sample considers 14, 15 and 16 year-old students born between 1981 and 1988, where the cohorts affected by the Reform (i.e. our treated units) are those from 1985 onwards.<sup>11</sup> In the second step we estimate Equation 1 by 2SLS. Identification comes from the variation in enrollment across time and ages induced by the Reform.

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<sup>10</sup>For example, for the cohorts used in our sample there is not a consistent time series of education data on young immigrants attending high schools, nor a demographic time series of young immigrants by age and province cells. Macro area (i.e. five dummies for the North-West, North-East, Center, South, Islands) by year fixed effects are likely to capture the variation in offending rates induced by this sort of dynamics.

<sup>11</sup>We use a balanced sample of four treated cohorts (i.e. 1985 - 1986 - 1987 - 1988) and four control cohorts (i.e. 1981 - 1982 - 1983 - 1984).

The main identifying assumption for our estimation is that the change in compulsory schooling age legislation represents a valid and relevant instrument. An instrument for education satisfying these requirements is such that: (i) it affects significantly the variation in enrollment, and (ii) it is not correlated with the unobservables that influence youth criminal behavior. The first point is confirmed by the statistical significance of the instrument, as shown in the following sections and tables, meaning that the Reform is a strong predictor of adolescent decisions to stay one additional year at school. The second point hinges on the assumption that the *Reform<sub>at</sub>* variable could be legitimately omitted from Equation 1 and that the change in compulsory attendance laws was not concerned with problems of crime. The Reform was thought to increase the school leaving age so as to put Italy in line with most European countries. To our knowledge, it was not enacted in response to concerns for juvenile delinquency, youth unemployment or other crime related factors (Benadusi and Niceforo, 2010), but it was rather thought of as the first step in a comprehensive reform concerning the organization of the entire education system in Italy, which, eventually, did not even pass through Parliament. Other potential threats to identification are discussed in detail in Section 6.

Finally, it should be acknowledged that the variation induced by the instrument is very likely to be local, since it has an impact only on the bottom part of the education distribution and not at the top. When we estimate the parameter  $\theta_1$  using 2SLS, we are identifying a local average treatment effect (LATE), since only adolescents at the bottom part of the distribution, i.e., at the margin between dropping out and continuing education, are affected by the policy in their educational choices.

In the empirical analysis, we also estimate a reduced form model of the effects of the 1999 Reform on crime, which takes the following specification:

$$Crime_{apt} = \beta_0 + \beta_1 Reform_{at} + \beta_2 X_{apt} + \beta_3 W_{pt} + (1 + \eta_j)\phi_t + \omega_p + \varsigma_{apt} \quad (3)$$



The estimate from the *crime reduced form* (i.e.  $\beta_1$  in Equation 3) can be interpreted as an intention-to-treat (ITT) effect. It provides an overall estimate of the consequences of raising compulsory education by one year and it represents the relevant policy parameter.

## 4 Data and descriptive statistics

### 4.1 Data and variables construction

We collect data on the number of offenders reported to the judicial authority and high school enrollments (grades 9, 10 and 11), for 14, 15 and 16 year-old adolescents in the years before and after the implementation of the Reform. Our dataset is thus composed by cells defined by the year (from 1997 to 2002), the age (14, 15 and 16) and the province.<sup>12</sup> In the empirical analysis we consider a time span which covers six school years (from 1997 to 2002). In 2003 a new comprehensive educational reform was passed by the Italian Parliament and the minimum drop-out age was reduced to the Constitutional limit in force before the 1999 Reform. For this reason, we focus our empirical analysis on a balanced time window (before and after the introduction of the Reform) in the years before 2003.

For each cell, we construct a measure of the involvement in criminal activity based on the Italian National Institute of Statistics (ISTAT) official data on adolescent offenders. The ISTAT data on adolescent offenders have the unique characteristic of offering aggregate measures of the number of teenagers reported to the judicial authority for whom we know the exact age. Our measure of criminal activity (i.e. the *Offending Rate, OR*) is then the number of adolescents reported by the police to the judicial authority ( $offenders_{apt}$ ) over the corresponding age-year-province population ( $Pop_{apt}$ ):

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<sup>12</sup>Italian provinces correspond to NUTS 3 level. Since the number of provinces in Italy increased dramatically during the 90s, to maintain consistency of the data across years, we use the ISTAT definition of 95 provinces. Four provinces are dropped from the analysis: three of them because part of Special Statute Regions (i.e. *Val d'Aosta*, *Trentino* and *Alto Adige*) which followed a different implementation of the Reform, one (*Oristano*) because of missing data.

$$OR_{apt} = \frac{offenders_{apt}}{Pop_{apt}} \times 1,000 \quad (4)$$

where  $p$  indexes the province,  $a \in (14, 15, 16)$  the age, and  $t$  the year. Notice that the use of the number of adolescents reported by the police to the judicial authority is likely to be a more accurate measure of the involvement of teenagers in criminal activities. As outlined by Jacob and Lefgren (2003) and Machin et al. (2011), alternative measures, such as *crime rates* based on the number of crimes committed, would not be as accurate in the definition of the exact age of the offender. On the other hand, *arrest rates* obtained from the number of teenagers arrested could significantly underestimate the true magnitude of the phenomenon.<sup>13</sup> Finally, the use of provincial and year fixed effects in all the empirical specifications limit any systematic measurement error due to underreporting constant within geographical areas (over time) or within periods (across areas) (Bianchi et al., 2012).

In order to measure the enrollment ratio of the cohorts relevant for our analysis, we collect data on the number of students enrolled in high schools in each province from 1997 to 2002. We focus on 14, 15 and 16 year olds as they represent the age group which was directly affected by the Reform. We define the *Enrollment Ratio* ( $ER$ ) as the number of adolescents (14, 15 and 16 year olds) who are enrolled in secondary education ( $enrollments_{apt}$ ) as a percentage of the corresponding age group provincial population ( $Pop_{apt}$ ):<sup>14</sup>

$$ER_{apt} = \frac{enrollments_{apt}}{Pop_{apt}} \quad (5)$$

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<sup>13</sup>In the period 2001-2005 in Italy a formal jail was started for only 48 percent of juvenile offenders reported by the police to the judicial authority, and an even smaller figure (about 2.5 percent) were arrested and went into prison (Dipartimento di Giustizia Minorile, 2007). As a matter of fact, most of the adolescents reported to the judicial authority benefit from measures alternative to prison, such as correctional youth centers, family control, jail suspension. See Dipartimento di Giustizia Minorile (2007) for details on the peculiarities of jail for offenders under 18 in Italy.

<sup>14</sup>Conceptually, the  $ER$  can take values over 1 because of grade repeaters. For the empirical analysis, we use a censored version of the  $ER$  as we substitute 1 in all the cases in which the  $ER$  takes values above this threshold. While all the results also hold for the uncensored version of the  $ER$ , and are available upon request from the authors, in a later Section we make additional robustness checks calculating a *Net Enrollment Ratio*, i.e. without considering grade repeaters.

We then merge the two datasets by year, age and province cells and link socio-economic information at the province level from different data sources (Labor Force Survey - ISTAT, Public Finance Database - Italian Ministry of Interior, Demographic General Database - ISTAT).<sup>15</sup> In the analysis of the territorial heterogeneity of the effects of the Reform on adolescent crime we also exploit measures of the pervasiveness of organized crime and Mafia-type criminal associations, and measures of social capital taken from existing economic and criminology literature. Details on these variables are contained in Appendix A.

## 4.2 Descriptive statistics

[Table 1 here]

In Section 2 we described the introduction of the Reform, and illustrated rapidly the different effects it determined in the increase of the enrollment ratio across Italian regions. In this Section we go deeper into the descriptive analysis of the effects of the Reform both on the side of the educational outcomes (i.e. the enrollment ratio of 14-16 year olds) and on the side of the dependent variable (i.e. the offending rate of 14-16 year olds). First of all, Table 1 compares the enrollment ratio of the treatment and control groups. On average, the Reform increased by 7.6 percent the overall enrollment ratio. Although males result marginally more affected (+7.7 percent) than females (+7.3 percent), the most relevant differences in the effects of the Reform emerge splitting the sample between provinces located in the North of the country and those located in the South. The increase in the enrollment ratio is higher in the South (+9.3 percent) than in North (+6.7 percent). As we have already noted, this is probably due to the fact that the two areas started from different initial levels of adolescent enrollment in high schools (lower in the South than in the North) and converge after the implementation of the Reform. Thus, the introduction of the Reform determined a sharp expansion in the enrollment rate starting from the first cohort of students affected (i.e. students born in 1985). On the other hand, the same

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<sup>15</sup>The match between the two measures is such that, for example, the *ER* for the school year 1994/95 - which starts in mid September 1994 and ends in June 1995 - is linked to the offending rates of the year 1995.

policy instrument introduced by the Reform (one additional year of compulsory education) had stronger incapacitation effects in some regions (i.e. the South of the country) where a larger share of individuals at the margins of dropping out or continuing education was obliged to take this latter choice.

[Table 2 here]

Table 2 contains general descriptive statistics on all the dependent and independent variables included in the empirical specifications. The average enrollment ratio is equal to 0.93, while the average offending rate is of 20.51 adolescents reported to the judicial authority per 1,000 individuals of the corresponding age group population. Finally, in Figure 3, we show the average residuals from OLS regressions where the dependent variable is the offending rate of 14, 15 and 16 year olds ( $OR_{apt}$ ) and we include as control variables province and year fixed effects, macro area by year fixed effects and all the time-varying control variables included in the main empirical specification, apart from the enrollment rate (see Equation 1). The vertical lines denote the implementation of the Reform for each age group. The offending rates decrease after the Reform was implemented and such a decrease is sharper for 14 and 15 year olds, which were directly influenced by the extension of the compulsory education, while it is less pronounced for 16 year olds.

[Figure 3 here]

## 5 Results

### 5.1 The crime reducing effect of education

We estimate Equation 1 using a 2SLS regression, with robust standard errors clustered at the province level and weights equal to the corresponding age-year-province student population.<sup>16</sup>

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<sup>16</sup>The use of population weighted models increases the statistical precision of the estimates. All the results are robust to the use of alternative weights (see Section 6) and to the exclusion of weights.

Table 3 shows our baseline results. Columns (1), (2) and (3) contain the estimates of the parameter  $\theta_1$  from Equation 1 using OLS regressions, where we progressively add the control variables ( $X_{apt}$  and  $W_{pt}$ , column 2) and the fixed effects (column 3). Results show a negative and statistically significant correlation between the  $ER$  and the adolescent offending rate, increasing in magnitude (in absolute terms) and in statistical precision from the unconditional model (column 1) to the baseline specification (column 3). This result points to a framework where the omission of crime determinants and other control variables, as well as the inadequate control of province, year and macro-area time-varying unobserved heterogeneity bias the estimates towards zero.

[Table 3 here]

Column (4) presents the results from the first stage regression where we test the relevance of our instrumental variable ( $Reform_{at}$  in Equation 2). The coefficient is positive and statistically significant, meaning that the 1999 Reform induced a significant increase in the enrollment ratio of 14, 15 and 16 year olds. The first stage F-statistics lies largely above the thresholds specified by Stock and Yogo (2005), ensuring that estimates from the 2SLS regression are not poorly identified.

Column (5) contains our baseline 2SLS estimates: we find a negative and statistically significant effect of the  $ER$  on adolescent offending rates. An increase in the  $ER$  by one percentage point determines a decrease in the offending rate of 50.6 incidences per 1,000 of the corresponding age group population. This corresponds to a decrease of 2.47 percent with respect to the mean offending rate. Machin et al. (2011), focusing on the long run effect of education on adult offending rates, find that a 10 percent increase in the average school leaving age lowers crimes of 18-40 year olds by 2.1 percent. Taking together their estimates and ours, it could be that the crime reducing effect of education follows a decreasing path over the age of the individuals, where the incapacitation effect prevails during the school attendance period while the human

capital effect prevails at a later stage.

Notice that our 2SLS estimates deliver a coefficient which is larger, in absolute terms, with respect to the OLS. This may suggest that OLS underestimates (in absolute terms) the true effect. This result may derive from selection and composition effects. As we have already noted, the 2SLS parameter is likely to capture a LATE on individuals who are at the margin of continuing education or dropping out, which are also those who are potentially more at risk of engaging in criminal activities. For instance, suppose that there might be two types of adolescent: one type is characterized by a higher propensity to commit crime and has an opportunity cost of continuing education larger than the one of committing crime; the second type, instead, is characterized by a lower propensity to commit crime and a higher opportunity cost of engaging in criminal behavior than the one of being enrolled in high school. In this stylized scenario, it is straightforward to identify the group of *compliers*, who are more affected by the Reform, in the first type of adolescents. Hence, the effect of enrollment, as identified by the change in enrollment behavior after the Reform (i.e., the 2SLS parameter), is likely to be higher (in absolute terms) than the correlation found using OLS. This also means that the Reform has been more effective in the lower part of the education distribution and that a policy affecting the upper part of the distribution could not be as effective in decreasing crime.

In column (6) we report the OLS estimate for the crime reduced form effect of the 1999 Reform on adolescent offending rates ( $\beta_1$  in Equation 3). We find that one additional year of compulsory education reduces the offending rate of 14, 15 and 16 year olds by 4.6 incidences per 1,000 of the corresponding age group population which, in relative terms, represents a 22.4 percent decrease of the offending rate. The effect is in line with the existing literature. For instance, Anderson (2013) finds that the exposure to a minimum drop-out age of 18 reduces the arrest rate for 16 to 18 year olds by 17.2 percent. Our estimates are a bit larger in magnitude: this could be due to the use of offending rate rather than arrest rates, as the latter are likely to underestimate the true effect. This coefficient is also larger than the 2SLS estimate presented

before, since it may incorporate any indirect or spillover effect of the Reform.

So far, our analysis has provided evidence of the crime reducing effect of education (hereafter labeled *CREE* to ease the exposition) in the Italian context. However, we have also seen that the Reform has had different effects across the Italian territory. In other words, the Reform induced the same *incapacitation treatment* (i.e. one additional year of education to be spent at school) on all Italian students at the same instant in time, but it determined different percentage increases in the enrollment ratio, which are predominantly due to the enrollment rate level in each area before the implementation of the Reform. As a result, the Reform had different effects on the territory, increasing the enrollment ratio especially in Southern regions. Do these heterogeneous effects on enrollment translate also on differential effects on crime rates?

## 5.2 Territorial heterogeneity

The issue of the heterogeneity of the crime reducing effects of education has still found little attention in the literature. Only Luallen (2006) shows that sizable effects of increasing the number of days at school on youth crime rates are present in urban and not in rural areas. Hence, little is known about the role of territorial characteristics. In the Italian context, where we have documented deep differences in the effect of the Reform between the North and the South of the country, we may expect a stronger reduction in crime rates in areas where the incapacitation induced by the Reform has been higher. Thus, as a first step in the analysis of the heterogeneous effects, we test whether the *CREE* is statistically different in the South and in the North of the country. To this purpose, we estimate Equation 1 using a 2SLS fully interacted model where the interactions are constructed with a dummy variable taking value 1 for all provinces located in the South and 0 otherwise.<sup>17</sup>

[Table 4 here]

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<sup>17</sup>Our results do not change if we perform the analysis of the heterogeneous effects (Sections 5.2 and 5.3) with sample splits instead of using fully interacted models. Results are available upon request.

The first-stage regression in Table 4 (column 1) confirms that the Reform had larger effects in the South (+9 percent) than in the North (+7 percent). As the additional year of education imposed by the compulsory education Reform mostly influenced the decisions of those at the margin of dropping out before secondary education and most likely to engage in criminal activities, we would expect our coefficient for the *CREE* ( $\theta_1$  from Equation 1) to be larger in areas where the Reform pushed to school a larger share of these individuals (i.e., in the South). However, the 2SLS and the crime reduced form estimates in Table 4 (columns 2) show opposite results, revealing that the *CREE* is larger in the North than the South. In absolute terms, an increase by one percentage point of the enrollment ratio reduces adolescent offending rates by almost 3.4 percent in the North and by less than a half (1.6 percent) in the South. Similar results hold for the effect in the reduced form equation (column 3).

In order to understand this puzzling result, one should bear in mind that incapacitation (i.e., time to be spent at school instead of on the street) is not the only mechanism with which enrollment might affect crime. Another mechanism that applies to this framework is the so-called human capital effect, according to which the time spent at school contributes to increase the opportunity cost of committing a crime and enlarges the set of labor market opportunities that adolescents can find in the legal labor market. Finally, there might be an important role played by social interactions, even if it is not clear in which direction they could affect crime. For instance, staying at school an additional year may force adolescents with more propensity to commit crime to face their peers with lower criminal propensity and this may negatively affect their criminal behavior, either contemporaneously or over time. However, if at school adolescents only interact with bad peers engaged in criminal activities, this could reinforce, instead of decrease, the youth crime rate. In the next Section, we try to shed some lights on these mechanisms and try to explain why they have different effects across the Italian territory.



### 5.3 The role of institutions, social capital and organized crime

Why does the same incapacitation treatment yield different *CREE*, all things being equal, across the territory? And, more specifically, why do we observe the counter-intuitive results that in the areas where the policy induced a higher incapacitation effect, pushing to schools a larger fraction of marginal students, the *CREE* is lower? As a matter of fact, the same treatment of incapacitation has differential effects on the territory and this could plausibly be due to the different importance that alternative channels might have in the different areas of the country. In what follows, we test whether the quality of local institutions, as proxied by classical measures of social capital, the presence and pervasiveness of organized crime, and school system characteristics might help to explain the territorial heterogeneity in the *CREE* that we have documented in the previous Section. To this purpose, we collect different measures on social capital, on school characteristics and on the pervasiveness of organized crime, at the province level, and include them in the baseline 2SLS regressions using fully interacted models.

In particular, we use two measures of social capital largely exploited in the existing literature starting from Guiso et al. (2004), notably *Electoral participation* and *Blood donations*. We interpret social capital in the sense of *civic capital* (Guiso et al., 2004) and as a general proxy for the quality of local (formal and informal) institutions. In this sense, a higher social capital would increase the quality of social interactions as well as the expected returns from human capital accumulation: hence, areas with higher levels of social capital may be more likely to favor the development of a human capital effect, in addition to the direct incapacitation effect.

School characteristics are proxied by the average class size and student-to-teacher ratio. Areas with a lower average class size or a lower student-to-teacher ratio could be characterized by higher levels of social interactions within the classroom, but also better teaching environments and strong teacher monitoring, so that the development of the human capital effect or of the externalities induced by social interactions could be favored.

We use two measures to identify areas where organized crime is more pervasive and it is likely

to heavily influence everyday life and student behavior in the school and out of the school. The first is the *Mafia Index* taken from Calderoni (2011), which is a comprehensive measure of the presence of organized crime in each province. The second is a territorial rule such that we classify as *Mafia regions* all provinces located in the three regions where mafia-type organizations were historically born and are still heavily pervasive in the economic and social networks, notably Campania, Calabria and Sicily (Lupo, 1993, Pinotti, 2012). Detailed information and descriptive statistics on these variables are contained in Appendix A.<sup>18</sup>

[Table 5 here]

Each interaction variable, with the only exception of the dummy for *Mafia regions*, is first standardized with zero mean and unitary standard deviation and then included in the fully interacted model (Nannicini et al., 2013). Results are presented in Table 5. The estimates reveal that the effects of the *ER* are always negative and statistically significant, while the interaction terms highlight important heterogeneous patterns. The *CREE* increases with the degree of social capital at the province level. This result is statistically significant for the measure of electoral participation, but not for blood donation. According to column 1, an increase in social capital equal to 1 standard deviation would reduce the adolescent offending rate by 15.2 incidences per 1,000 of the corresponding age-group population (thus, increasing in absolute value the *CREE*). Moving from the lowest level of social capital (recorded in the Southern province of *Agrigento*) to the average level would reduce the offending rate by 33.98 additional incidences per 1,000 of the corresponding age-group population, or, in absolute terms, it would increase the *CREE* by 1.66 percent.<sup>19</sup>

<sup>18</sup>Our results are robust to the use of alternative measures of social capital, such as the *cheating scores* exploited by Paccagnella and Sestito (2014) and Guiso et al. (2013), as well as to other measures of the pervasiveness of organized crime.

<sup>19</sup>The lowest level of social capital according to the *Electoral participation* measure, recorded in the province of *Agrigento*, is equal to -2.23. Moving from this level to the zero average level implies increasing the level of social capital by 2.23 standard deviations, corresponding to 33.98 incidences per 1,000 ( $2.23 * 15.24 = 33.98$ ). The overall effect of enrollment rate on the adolescent offending rate is then equal to a reduction of  $55.28 + 33.98 = 91.26$  incidences per 1,000. The percentage reduction in adolescent offending rate induced by the change in social capital is equal to  $((-91.26 * 0.01)/20.51) * 100 = -4.45$  percent, while, without social capital, the percentage *CREE* is equal to  $((-57.28 * 0.01)/20.51) * 100 = -2.79$ . The difference between the two leads to a  $2.79 - 4.45 = 1.66$  percent increase, in absolute values, of the *CREE*.

In columns 3 and 4, we observe that increasing the average class size or the student-to-teacher ratio would reduce the *CREE*. This finding is consistent with several mechanisms. For example, increasing class size or student-to-teacher ratio would determine a lower level of monitoring, during school hours, and, plausibly, a lower quality of the education production process, but it could also increase occasions for social interactions with peers within the class or at school. The fact that we find a lower *CREE* (in absolute terms) in areas characterized by high class size or student-to-teacher ratio implies that social interactions at school may play a negative role and may also prevent the development of a human capital effect after the implementation of the Reform.

The estimates in columns 5 and 6 reveal that the *CREE* is highly influenced by the presence of organized crime, and this result holds for both measures of organized crime. For example, estimates in column 5 show that an increase in the Mafia Index equal to 1 standard deviation would increase the adolescent offending rate by 15.5 incidences per 1,000 of the corresponding age-group population (thus, decreasing in absolute value the *CREE*). Moving from the highest level of Mafia pervasiveness (recorded in the Southern province of *Reggio di Calabria*) to the average level would reduce the offending rate by 46.34 additional incidences per 1,000 of the corresponding age-group population, or, in absolute terms, it would increase the *CREE* by 2.25 percent. According to column 6, a one percentage point increase in the enrollment ratio would reduce the adolescent offending rate by 1.33 percent in *Mafia regions* compared to almost 3.18 percent in the rest of the country. This finding could be easily rationalized. In areas where Mafia-type criminal associations are pervasive, the presence of criminal networks (inside and outside the school) could facilitate the engagement in criminal behaviors of youths, increase the expected value of participating in criminal activities and reduce its social costs. Criminal networks and criminal organizations offer more opportunities to commit crimes, *via* social interactions mechanisms, but also they protect their own affiliates, thus lowering the perceived probability of being caught by the police, and guarantee a fixed amount of illegal rent which

could be competitive with respect to the legal labor market opportunities.

Turning back to the differential effects of the Reform across Italian regions that we have documented in Table 4, we now focus separately on the provinces located in Northern and Southern Italy, and repeat the analysis of the heterogeneous effects. This is because we want to test whether the channels that we disentangled affect the *CREE* differently in the two areas of the country, being, for instance, prevalent in one area and not in the other.

[Table 6 here]

Results in Table 6 present important differences with respect to those calculated on the whole sample of provinces (Table 5). The heterogeneous effects given by the social capital channel cannot be clearly related to one group of provinces or to the other, and the direction found in the previous analysis is confirmed for Southern provinces only. The effects of school characteristics are relevant in Northern provinces only, while the effect of the pervasiveness of Mafia-type criminal organizations is only statistically significant in the group of Southern provinces. Improving the quality of the educational system, in terms of simple policy indicators such as class size and student-to-teacher ratio, would not help to increase the *CREE* in areas at risk, while reducing the pervasiveness of organized crime and increasing *civiness* would entail benefits in the reduction of adolescent criminal behavior only in these areas.

Our results suggest that the incapacitation effect of one additional year of schooling can have different effects on adolescent offending rates, depending on several local characteristics. First of all, the fact that we always find lower effects (in absolute terms) in areas characterized by pervasive Mafia-type criminal organizations and lower levels of social capital may suggest that spending additional time at school is not enough to determine an effective incapacitation effect in areas at risk. In fact, we should expect in these areas higher rewards for criminal activities, so that adolescents may still find profitable to engage in criminal behavior when they are not at school. Second, the human capital effect could reinforce the incapacitation mechanism that

we observe. This effect may depend on school quality as well as on the activities performed at school. Both these implications point to the fact that in areas characterized by pervasive organized crime, preventing youths from staying on the streets is not enough to ensure the same level of crime reduction that we find in areas where organized crime is not pervasive.

## 6 Robustness checks

We test the robustness of our estimates over several dimensions and report the results in Table 7.

[Table 7 here]

In our empirical analysis, identification comes from the comparison of crime rates between ages and cohorts, exploiting the timing of the implementation of the Reform. One important assumption of our strategy is that cohorts are comparable across ages, namely between 14 and 16 years old. However, after the first two years of high school (i.e. grades 9 and 10), school characteristics may change as all types of high schools (academic, vocational, technical) may separate the educational path between the first two years (the so called *biennio*) and the remaining years. For this reason, we test our results adding in the baseline specification a dummy variable which takes value 1 for 9th and 10th graders (i.e. 14 and 15 year olds students), and 0 for 11th (16 year olds). Results are presented in column 1 of Table 7 and are still statistically significant, although smaller in magnitude. This is consistent with the fact that, in this case, identification comes only from the discontinuity in crime rates between 14 and 15 year olds.

Threats to our identification may also come from any sort of omitting variable bias. This could arise from territorial by year variations, which are not adequately captured by our control variables, and by the set of fixed effects and macro area by year fixed effects included in all the specifications. In this regard, in column 2 of Table 7, we repeat the analysis adding region by year fixed effects. The results do not change significantly from the baseline (Table 3), thus

reassuring us that macro area by year fixed effects adequately capture relevant territorial by year variations for which we cannot directly control for. In columns 3 and 4 we also add to the baseline specification linear and quadratic time trends: both the reduced form and the 2SLS results are always close to the baseline.<sup>20</sup>

Another aspect which may be a source of measurement error in our estimates is the presence of grade repeaters for whom we cannot establish the exact match between enrollments and offending measures. To test whether this is a relevant issue in our exercise, in column 5 of Table 7 we repeat the baseline analysis using the *Net Enrollment Ratio* (instead of the simple, or raw, enrollment ratio used so far), calculated as the enrollment ratio net of grade repeaters. The first stage is always statistically significant and the estimated effects are in line with the baseline in Table 3. We do not use the *Net Enrollment Ratio* in the main analysis as the methodology of recording grade repeaters in the official statistics is not consistent through the years used in the empirical analysis, and, in some years, the information is missing.<sup>21</sup>

In column 6 we implement a specification in which more weight is given to the cohorts closer to the year of implementation of the Reform (i.e. *Inverse Distance Weight, IDW*). The weight is equal to  $1/d$  where  $d$  is the distance in birth years from the discontinuity (Machin et al., 2011). The estimates do not substantially differ from the baseline: the coefficients are marginally larger, confirming that identification comes prevailingly from variation close to the discontinuity induced by the Reform.

The main analysis has been performed considering a period of time ranging between the school years 1997/98 and 2003/04, with 4 cohorts affected by the Reform and 4 cohorts unaffected. However, it could be that the longer the period of time used in the empirical specification, the more difficult it is to satisfy the main assumptions used for identification, namely the com-

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<sup>20</sup>We add 18 regional dummies interacted with the year dummies. Our results are also robust to the inclusion of province by year fixed effects and region-specific linear and quadratic time trends (available upon request).

<sup>21</sup>For the estimation presented in column 5 of Table 7 the number of grade repeaters in the school years 1998/99 and 1999/00 are missing and thus estimated with OLS regressions exploiting all the school years for which the information is available and imputed in the dataset. We are aware of the fact that this procedure applied in the first year of implementation of the Reform (1999/00) is likely to determine a downward bias which might explain why the estimates are sensibly smaller compared to the baseline.

parability between ages and cohorts. To test for this, we repeat the estimation keeping only 4 school years, from 1998/99 to 2001/02, but results presented in column 7 are close to our baseline (Table 3).

We perform additional specification tests concerning the functional form chosen for the regression model (results available upon request). As in most of the existing literature, we exploit OLS regressions which allow to estimate in a flexible and robust way instrumental variable models. However, our results do not change defining the dependent variable as the natural logarithm of the offending rate and replacing a zero when the logarithm is not defined (Bianchi et al., 2012, Fougere et al., 2009, Oster and Agell, 2007). It is also widely recognized that crime measures have a count-data nature and sometimes present distributions which are left skewed and might be appropriately estimated with count-data models (Jacob and Lefgren, 2003, Luallen, 2006, Osgood, 2000): our results are also robust tho the use of negative binomial or Poisson regressions.

## 7 Conclusions and policy implications

The causes of the territorial heterogeneity in the pattern of crime rates have long been debated in the literature. Glaeser et al. (1996) show that only 30 percent of the patterns in crime rates across areas are explained by observable area characteristics, and that social interactions may play a substantive role in explaining such variations. Akomak and ter Weel (2012) show that social capital may explain an important part of the territorial variability in crime rates. Concerning the causal relation between education and crime, Luallen (2006) finds that the incapacitation effect of school causes a statistically significant decrease in the youth crime rate only in urban areas.

In this paper, we estimate the causal link between school attendance and adolescent offending rate, exploiting the implementation of a Reform, which has increased compulsory education by one year. In our baseline analysis, adopting a 2SLS estimation using the Reform as an instru-

ment, we find that a one percentage point increase in enrollment rate at high school (grades 9 to 11) decreases the adolescent offending rate by 2.47 percent with respect to the mean. However, this crime reducing effect of education shows a substantial territorial heterogeneity. In fact, although experiencing a higher increase in enrollment after the implementation of the Reform, provinces in Southern Italy are characterized by a lower crime reducing effect of education: a one percentage point increase in enrollment rate would decrease the adolescent offending rate by 3.4 percent in Northern provinces and by less than a half in the South.

Education might affect youth criminal behavior through several channels, which can play different role across the territory. In addition to the incapacitation effect, so that adolescents are forced to stay at school, so that they are not able to commit crime, there might be at least two other mechanisms: the human capital effect hinges on the idea that being at school makes adolescents less willing to engage in criminal activities, increasing their return to education and their labor market opportunities in the legal system; social interactions at school may either decrease or increase adolescent involvement in criminal activities, depending on the types of interaction that take place.

We try to shed some light on these channels, and on how they change across Italian territory, interacting the variable of interest (i.e., enrollment rate) with measures of social capital, school characteristics and organized crime, and then performing these analyses for Southern and Northern provinces, separately. When we consider the entire country, we find that in areas characterized by higher levels of social capital the crime reducing effect of education is stronger (in absolute value) than in areas with lower levels of social capital; school characteristics, implying less monitoring and worse teaching environment, lower the crime reducing effect of education, in absolute terms. The crime reducing effect of education is found to be lower in regions characterized by a pervasive presence of Mafia-type organizations: a one percentage point increase in enrollment rate in the *Mafia regions* leads to a reduction in crime rate equal to 1.33 percent, while the same change in enrollment in *non-Mafia regions* determines a drop in crime rate equal



to 3.18 percent. When we estimate the fully interacted models separately for Northern and Southern regions, we find that school characteristics play a role only in the Northern regions, while social capital and organized crime matter only for provinces in the South.

Southern Italy is an area where Mafia-type criminal organizations deeply influence social life and economic activities (Mete, 2009, Pinotti, 2012). Growing up and attending school in such areas may have important effects on adolescent criminal behavior (Cederna, 2013, Ciccotti et al., 2007). Our results suggest that the incapacitation effect of one additional year of schooling can have different effects on adolescent offending rates depending on school characteristics, the quality of local institutions, and the pervasiveness of organized crime, so that spending additional time at school is not enough to determine strong incapacitation effects in areas at risk. In fact, in these areas young criminals might find illegal activities more easily and with higher reward, so that adolescents still find it profitable to engage in criminal behavior when they are not at school.

School quality, as proxied by relevant policy variables such as class size and student-to-teacher ratio, do not seem relevant in areas at risk. This could be due to the fact that the increased quality of the education production process is counterbalanced by an increase in social interactions with peers. On the contrary, the presence of organized crime and the quality of local institutions and social networks (as proxied by measures of social capital) do explain part of the heterogeneity in the crime reducing effect of education. These results may depend on different mechanisms. First, it could be the case that the incapacitation effect of school attendance on adolescent criminal behavior is more effective in areas where criminal organizations do not take control of the illegal market, or in areas where young criminals prevalently act in isolation (notably, the North of the country). Second, it could also be that the incapacitation effect of school in the Southern regions is not as effective as in the North because the time that adolescents spend out of school in the afternoons is sufficient to build a criminal career in areas where criminal opportunities are less scarce and more organized. Third, the human capital

channel might be less effective in areas where a criminal career is relatively easier to start and more diffused. Finally, the existence of criminal networks among adolescents or between adolescents and adults could also determine important differences according to the strength and pervasiveness of these networks on social life.

The evidence we present points to the fact that in areas characterized by pervasive organized crime, keeping youths at school simply to prevent them from staying on the street, is not enough to ensure the same level of crime reduction as could be reached in areas where organized crime is not pervasive. Still, the human capital effect could reinforce the incapacitation mechanism that we observe: for example, education policies should be aimed at increasing the sense of civic duty and legality and to design adequate school activities for students living in areas where illegal opportunities seem more profitable than the legal activities.

## A Appendix. Measures of social capital and organized crime

In the analysis of the heterogeneous effects, we use several measures of social capital and organized crime. In this Section we briefly illustrate each of these measures and provide some descriptive statistics.

We define social capital as *civic capital*, that is, *those persistent and shared beliefs and values that help a group overcome the free rider problem in the pursuit of socially valuable activities* Guiso et al. (2010). The two measures of social capital are taken from (Guiso et al., 2004) and are extensively used in the existing literature: *Electoral participation (Social Capital measure 1)* refers to the average electoral turnout for all the referenda that occurred in Italy between 1946 and 1989, *Blood donations (Social Capital measure 2)* refers to the number of blood bags collected per inhabitant in each province in 1995.

The *Mafia Index* is taken from Calderoni (2011) and it is obtained with a principal component technique exploiting several measures of the presence of organized crime in the Italian provinces (Mafia type criminal offenses, mafia murders, city councils dissolved for infiltration by organized crime, and assets confiscated from organized crime) covering the period between 1983 and 2009. The *Mafia Index* highlights not only the strong concentration of the mafias in their original territories but also their significant presence in the central and northern provinces. *Mafia regions* include the provinces in the Southern regions where Mafia-type organizations were historically born and developed (i.e. provinces in Campania, Calabria and Sicily); *Non-Mafia regions* include the provinces in all the other regions (Lupo, 1993, Mete, 2009).

Finally, the school characteristics (average *class size* and average *student-to-teacher ratio* at the provincial level) are taken from ISTAT (2007) and refer to the school year 1997-98 (i.e. before the introduction of the Reform). Figure A.1 shows the geographical distribution of each (raw) measure while in the empirical analysis we standardize each variable so as to have zero mean and unitary standard deviation.

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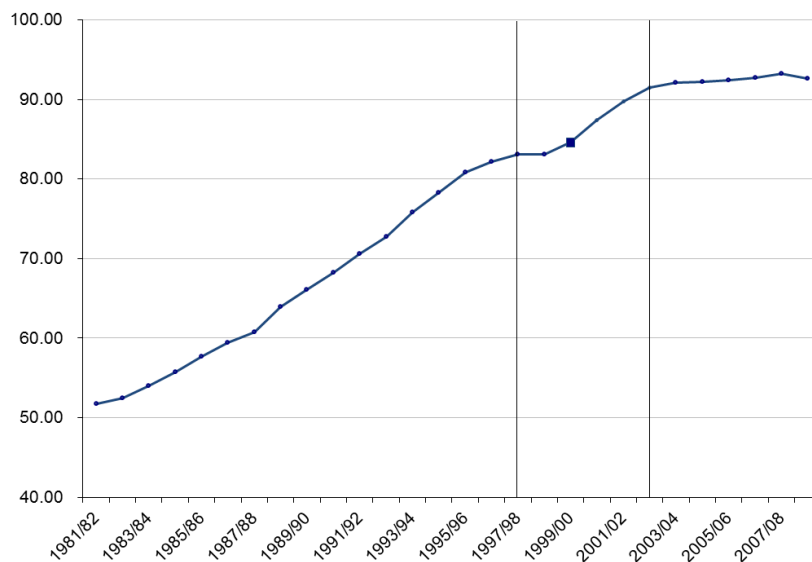
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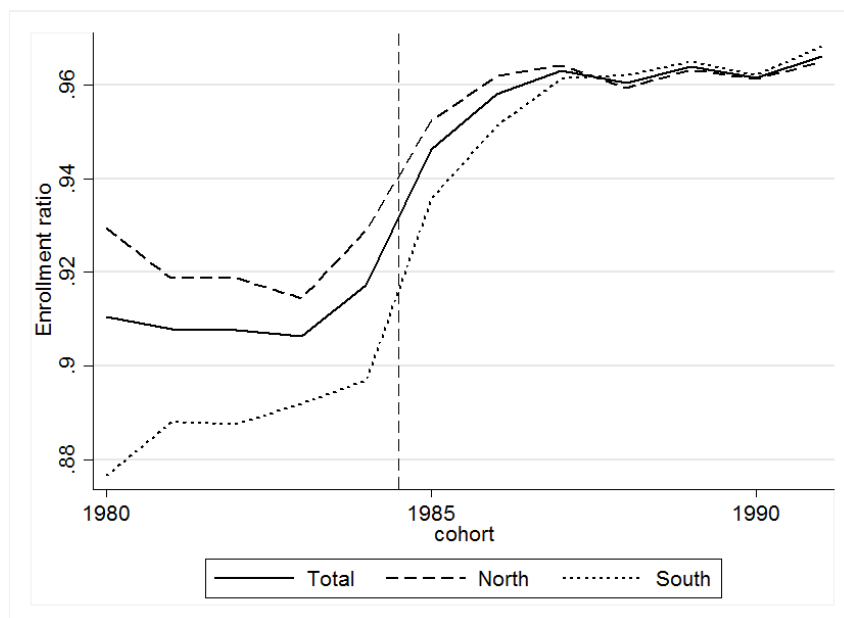
# Figures

Figure 1  
Secondary education enrollment ratio: historical trend.



NOTES: the graph shows the historical trend of the enrollment ratio for secondary education (grades 9 to 13). The historical series is provided by ISTAT (Italian Statistics Bureau) and it is calculated as the ratio between the number of students enrolled in secondary education and the number of resident citizens aged between 14 and 18. The squared dot highlights the school year immediately after the introduction of the 1999 compulsory schooling Reform, the vertical lines delimit the period of observation used in the baseline empirical analysis. SOURCE: ISTAT, Historical Series.

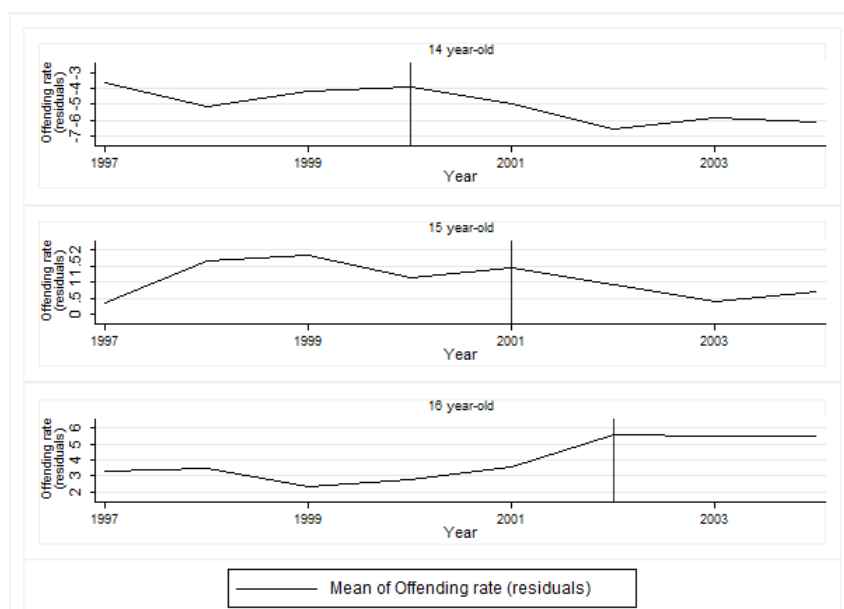
Figure 2  
Average enrollment ratio by cohort: geographical detail.



NOTES: the graph shows the average enrollment ratio of 14, 15 and 16 year olds students (grades 9 to 11) by cohort. The vertical line denotes the introduction of the 1999 Reform. SOURCE: ISTAT and MIUR.

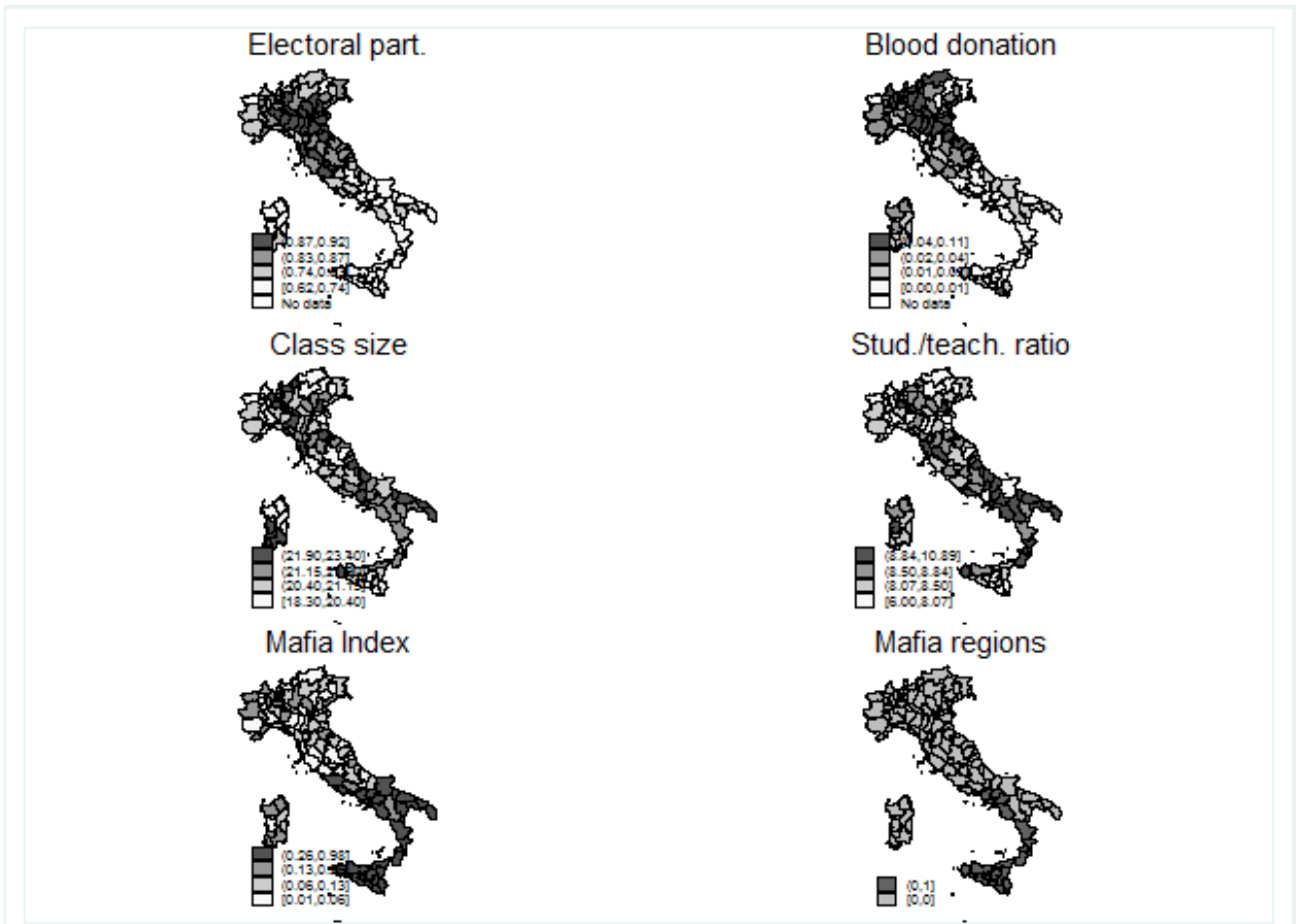


Figure 3  
Trends in the offending rates by age group.



NOTES: the graphs show residuals from OLS regressions of the offending rates on the set of time-variant control variables and fixed effects. Control variables include the share of youth living in an urban area (*Urban share*), the share of males (*Male share*), the average occupation rate for 15-14 year olds (*Occupation rate (15-24)*), the logarithm of the provincial per capita Value Added (2012 euros) ( $\ln(\text{provincial pc VA})$ ), the logarithm of provincial population ( $\ln(\text{provincial population})$ ). Fixed effects include year, province and macro area by year fixed effects. Vertical lines denote the year at which each age group has been affected by the Reform. SOURCE: own elaborations from ISTAT and MIUR.

Figure A.1  
Measures of social capital, school characteristics and organized crime pervasiveness.



NOTES: *Electoral participation* refers to the average electoral turnout for all the referenda that occurred in Italy between 1946 and 1989; *Blood donations* refers to the number of blood bags collected per inhabitant in each province in 1995. Average *class size* and average *student-to-teacher ratio* (i.e. stud./teach. ratio) at the provincial level are taken from ISTAT (2007) and refer to the school year 1997-98. The *Mafia Index* is taken from Calderoni (2011); *Mafia regions* include the provinces in the Southern regions where Mafia-type organizations were historically born and developed (i.e. provinces in Campania, Calabria and Sicily); *Non-Mafia regions* include the provinces in all the other regions.

## Tables

Table 1  
Descriptive statistics: the effects of the 1999 Reform on enrollment ratio.

	N	Cohorts not affected	Cohorts affected	Difference	Percentage increase
Overall	1638	0.898	0.967	0.068	7.614
Males	1638	0.894	0.963	0.069	7.694
Females	1638	0.897	0.962	0.066	7.322
North	1044	0.911	0.972	0.061	6.701
South	594	0.877	0.958	0.081	9.280

NOTES: all the differences are statistically significant at the  $p < 0.01$  confidence level.

Table 2  
Descriptive statistics.

	<i>mean</i>	<i>sd</i>	<i>max</i>	<i>min</i>	<i>N</i>
Offending rate	20.512	12.332	114.478	0.000	1638
Enrollment ratio	0.933	0.085	1.000	0.605	1638
Urban share	0.144	0.200	0.877	0.000	1638
Male share	0.506	0.018	0.585	0.432	1638
Occupation rate (15-24)	31.651	10.684	49.595	12.066	1638
Provincial per capita VA	14841.5	6374.4	43167.9	6463.4	1638
Provincial population	612978.7	642947.8	3922982	89775	1638

NOTES: Offending rates are calculated per 1,000 of the corresponding province-age population. *Urban share* refers to the share of youth living in an urban area, *Male share* to the share of males, *Occupation rate (15-24)* is the the average occupation rate for 15-24 year olds, *provincial pc VA* refers to the provincial per capita Value Added (deflated in 2012 Euros), *provincial population* refers to the total provincial population.

Table 3  
Baseline results.

	Offending rate					
	(1)	OLS (2)	(3)	First stage (4)	2SLS (5)	Reduced form (6)
ER	-10.58 (6.66)	-19.80** (7.68)	-46.00*** (4.04)		-50.60*** (6.76)	
Reform				0.08*** (0.01)		-4.60*** (0.41)
First stage F-stat.					105.54	
N.Clusters	91	91	91	91	91	91
N.Observations	1638	1638	1638	1638	1638	1638
Mean of dep. var.	20.51	20.51	20.51	20.51	20.51	20.51
Control variables		yes	yes	yes	yes	yes
Fixed effects			yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. Control variables include the share of youth living in an urban area (*Urban share*), the share of males (*Male share*), the average occupation rate for 15-24 year olds (*Occupation rate (15-24)*), the logarithm of the provincial per capita Value Added (2012 euros) ( $\ln(\text{provincial pc VA})$ ), the logarithm of provincial population ( $\ln(\text{provincial population})$ ). Fixed effects include year, province and macro area by year fixed effects. The *First stage F-stat.* refers to the Kleibergen-Paap rk Wald F-statistics. Robust standard errors in parenthesis, clustered at the province level. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.

Table 4  
Territorial differences between North and South

	Offending rate		
	First stage (1)	2SLS (2)	RF OLS (3)
ER		-69.63*** (10.86)	
South dummy * ER		36.39*** (12.64)	
Reform	0.07*** (0.01)		-5.36*** (0.45)
South dummy * Reform	0.02* (0.01)		1.86** (0.79)
First stage F-stat.		21.85	
Mean of dep. var.	20.51	20.51	20.51
N.Clusters	91	91	91
N.Observations	1638	1638	1638

NOTES: population weighted models estimated on age-year-province cells, fully interacted models. For the control variables and the fixed effects included in all the specifications see Table 3. The *South dummy* is equal to 1 for provinces in the South of Italy (0 otherwise). The *First stage F-stat.* refers to the Kleibergen-Paap rk Wald F-statistics. Robust standard errors in parenthesis, clustered at the province level. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.

Table 5  
Heterogeneous effects: social capital, school characteristics and organized crime.

	Offending rate (2SLS)					
	Social Capital		School characteristics		Organized crime	
	Electoral participation (1)	Blood donation (2)	Student/teacher ratio (3)	Class size (4)	Mafia Index (5)	Mafia regions (6)
ER	-57.28*** (6.78)	-51.64*** (5.80)	-55.24*** (7.52)	-57.33*** (7.48)	-61.90*** (7.71)	-65.21*** (9.22)
Interaction variable * ER	-15.24*** (4.66)	-6.74 (6.30)	19.51** (8.02)	17.76** (7.01)	15.50*** (3.63)	37.88*** (11.52)
First stage F-stat.	40.64	20.62	46.96	25.79	39.72	31.70
Mean of dep. var.	20.51	20.51	20.51	20.51	20.51	20.51
N.Clusters	89	89	91	91	91	91
N.Observations	1602	1602	1638	1638	1638	1638

NOTES: population weighted models estimated on age-year-province cells, fully interacted models. For the control variables and the fixed effects included in all the specifications see Table 3; for the definitions of social capital measures, school characteristics and organized crime measures see Section A. The *First stage F-stat.* refers to the Kleibergen-Paap rk Wald F-statistics. Robust standard errors in parenthesis, clustered at the province level. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.

Table 6  
Heterogeneous effects within North and South.

	Offending rate (2SLS)				
	Social Capital		School characteristics		Organized crime
	Electoral participation	Blood donation	Student/teacher ratio	Class size	Mafia Index
	(1)	(2)	(3)	(4)	(5)
Panel A: North					
ER	-70.77*** (11.25)	-73.51*** (11.91)	-79.74*** (13.19)	-79.93*** (13.41)	-64.73*** (8.51)
Interaction variable * ER	33.17* (19.56)	27.66 (18.01)	23.75** (11.37)	23.77** (11.91)	-9.14 (14.91)
First stage F-stat.	4.20	4.37	15.55	16.57	3.13
Mean of dep. var.	22.82	22.82	22.82	22.82	22.82
N.Clusters	57	57	58	58	58
N.Observations	1026	1026	1044	1044	1044
Panel B: South					
ER	-38.40*** (6.30)	-34.22*** (6.28)	-31.74*** (5.71)	-34.21*** (5.90)	-49.09*** (10.88)
Interaction variable * ER	-16.24*** (4.24)	-7.12 (5.93)	11.73 (7.64)	9.69 (6.38)	20.51** (8.95)
First stage F-stat.	29.77	52.78	52.03	51.59	16.77
Mean of dep. var.	16.45	16.45	16.45	16.45	16.45
N.Clusters	32	32	33	33	33
N.Observations	576	576	594	594	594

NOTES: population weighted models estimated on age-year-province cells, fully interacted models. For the control variables and the fixed effects included in all the specifications see Table 3; for the definitions of social capital measures, school characteristics and organized crime measures see Section A. The *First stage F-stat.* refers to the Kleibergen-Paap rk Wald F-statistics. Robust standard errors in parenthesis, clustered at the province level. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.

Table 7  
Robustness analysis.

	Offending rate						
	Alternative specifications				Net Enr	IDW	1998-2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Reduced form OLS							
Reform	-1.70*** (0.32)	-4.46*** (0.42)	-4.60*** (0.41)	-4.60*** (0.41)	-4.60*** (0.41)	-4.74*** (0.55)	-4.46*** (0.41)
Panel B: 2SLS							
ER	-29.59*** (6.26)	-49.30*** (6.68)	-50.60*** (6.76)	-50.62*** (6.75)	-40.69*** (4.16)	-62.55*** (8.02)	-50.02*** (6.48)
First stage F-stat.	76.90	100.27	105.54	105.52	258.00	169.40	107.95
N.Clusters	91	91	91	91	91	91	91
N.Observations	1638	1638	1638	1638	1638	1638	1092
Mean of dep. var.	20.51	20.51	20.51	20.51	20.51	20.51	20.08
Control variables	yes	yes	yes	yes	yes	yes	yes
Baseline fixed effects	yes	yes	yes	yes	yes	yes	yes
Age group FE	yes						
Region by year FE		yes					
Linear time trend			yes				
Quadratic time trend				yes			

NOTES: specification in column (1) includes fixed effects for age group (1 if age equal to 14 or 15 and 0 otherwise); specification in column (2) includes region by year fixed effects; specification in column (3) includes a linear time trend; specification in column (4) includes a quadratic time trend; specification in column (5) exploits the net enrollment ratio; specification in column (6) exploits inverse distance weights; specification in column (7) uses a subsample restricted to a time window of four years (1998-2001). For the control variables and the fixed effects included in all the specifications see Table 3. The *First stage F-stat.* refers to the Kleibergen-Paap rk Wald F-statistics. Robust standard errors in parenthesis, clustered at the province level. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.