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## Temi di Discussione

(Working Papers)

Rethinking the crime reducing effect of education?  
Mechanisms and evidence from regional divides

by Ylenia Brilli and Marco Tonello

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# RETHINKING THE CRIME-REDUCING EFFECT OF EDUCATION? MECHANISMS AND EVIDENCE FROM REGIONAL DIVIDES

by Ylenia Brilli\* and Marco Tonello\*\*

## Abstract

We estimate the contemporaneous effect of education on adolescent crime by exploiting the variation in crime rates between different cohorts and at different ages that followed a reform that raised the school-leaving age in Italy. A 1 percentage-point increase of the enrollment rate reduces adolescent crime by 1.3 per cent in the North of Italy but increases it by 3.9 per cent in the South. The crime-reducing effect depends mainly on incapacitation (i.e. adolescents stay in school instead of on the street); the crime-increasing effect is consistent with a channel of criminal capital accumulation, operating through social interactions and organized-crime networks.

**JEL Classification:** I20, I28, J13, K42.

**Keywords:** adolescent crime, school enrollment, incapacitation, human capital.

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# 1 Introduction<sup>1</sup>

In the industrialized countries, increased prosperity and the availability of a growing range of consumer goods have led to increased opportunities for juvenile crime. With the social changes that have occurred over the past few decades, the informal traditional control exercised by adults (including parents, relatives and teachers) over young people has gradually declined, and adequate substitutes have not been provided (UN, 2003). Juvenile delinquency represents a sizable share of the overall delinquency rates: in the year 2005 in the US more than 18 percent of the individuals suspected to have committed a crime were under 18, and this figure was up to 22 percent in Canada and almost 27 percent in Germany and Sweden.<sup>2</sup> Adolescents' involvement in criminal behaviors is also of a particular policy concern because the victims of crimes committed by juveniles are mostly other juveniles. Moreover, adolescent crime entails a high social cost: Cohen and Piquero (2009) estimate that, discounted to present value at birth, the monetary value of *saving* a high risk youth ranges between \$2.6 and \$4.4 millions.

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 (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 completion. This human capital effect may affect youth crime immediately, but it may also last over time (Lochner and Moretti, 2004, Machin et al., 2011). 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

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<sup>2</sup>United Nations Survey of Crime Trends and Operations of Criminal Justice Systems, UN-CTS 2005.

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

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. The Italian context and the 1999 Reform represent an ideal 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 heterogeneous across the territory and which underlying channels might help to explain this heterogeneity. In fact, the Reform was implemented in the same way and with the same rules and timing in the whole country, which, on the contrary, is characterized by large territorial heterogeneity, both in formal and informal institutions, tracing back to distant political and historical factors (Barone and Mocetti, 2014, Guiso et al., 2013, Paccagnella and Sestito, 2014). A peculiarity of the Italian case is also illustrated in Figure 1, which shows the percentage change in the juvenile rate of suspected individuals from 1995 to 2003 (i.e. the same time window as the one used in our empirical analysis, see Section 4). Italy experienced a moderate increasing trend of the juvenile delinquency rate in the years 1995-2003 (+8.5 percent), while all the other countries (with the exception of Portugal) recorded low variations (Denmark, Spain, Japan), or even sizable decreases (Sweden, US, Canada, Germany and Greece).<sup>3</sup>

Starting from the seminal works of Jacob and Lefgren (2003) and Lochner and Moretti (2004), several empirical papers have assessed the effects of education on criminal behavior (Lochner, 2011).<sup>4</sup> However, only few 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>5</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 teenagers. 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

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<sup>3</sup>This pattern is not observed for adult delinquency, which increases for most of the countries in the same period (with the notable exception of the US, Denmark and Greece).

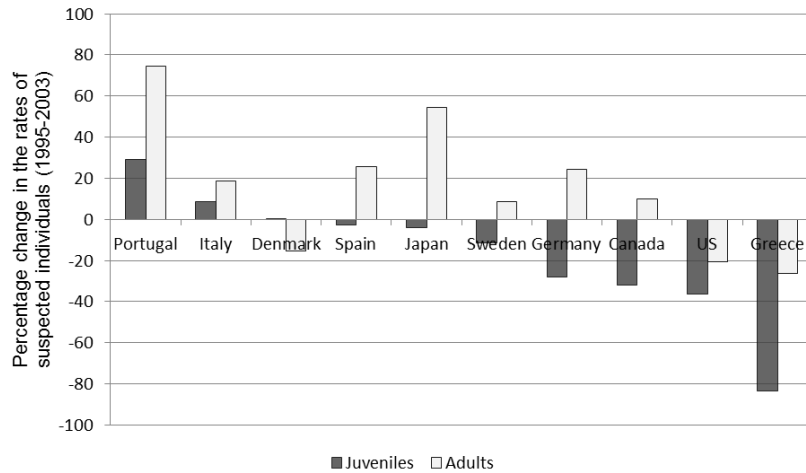
<sup>4</sup>Lochner and Moretti (2004) exploit aggregate (state-level) as well as individual-level data and find that school attainment significantly reduces the probability of arrest and incarceration of adult males. The work closest to our empirical strategy is that of Machin et al. (2011), which assesses the causal effect of education on adult crime rate exploiting a compulsory leaving age reform implemented during the 70s in the UK. The authors find that a 10 percent increase in the average school leaving age lowers crimes of 18 to 40-year-olds by 2.1 percent.

<sup>5</sup>Only Bertheleon and Kruger (2011) use data from Chile.



Figure 1

Percentage change in juvenile and adult rates of suspected individuals between 1995 and 2003



NOTES: the graphs shows the percentage change in the rate of suspected individuals (juveniles and adults) between 1995 and 2003. *Suspected individuals* are defined as persons brought into formal contact with the criminal justice system, regardless of the type of crime (where formal contact might include being suspected, arrested, cautioned). Juveniles are defined as individuals under the age of 18 in all the selected countries except: Sweden (under 21), Portugal (under 20), Japan (under 19). Data for the US, Spain, Japan and Greece refer to the year 2005 as the statistics for the years 2003 and 2004 are not available. SOURCE: United Nations Survey of Crime Trends and Operations of Criminal Justice Systems (UN-CTS), *United Nations Office for Drugs and Crime* (UNODOC) (available at <https://www.unodc.org/unodc/en/data-and-analysis/statistics/historic-data.html>).

(2006) and Jacob and Lefgren (2003) examine the effects of extra days off from school because of teachers' in-service days or teachers' 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. Indeed, the issue of the territorial heterogeneity of the effects of education on adolescent crime 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.

This paper contributes to the literature in three significant ways. First, we provide causal 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, which is generally found in the existing literature) 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. Second, we show that a *crime reducing effect of education* is not always present if the contemporaneous relation between adolescent crime and education is considered (Jacob and Lefgren, 2003, Luallen, 2006). Third, thanks to the availability of data on school enrollments and drop-out in the years close to the implementation of the Reform, we also discuss which

underlying mechanisms might play a role in explaining our results. Our estimates are based on newly available administrative records which precisely identify the age of all the offenders and 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 measures alternative to prison, thus reducing the scope for underestimating the true extent of the phenomenon (Jacob and Lefgren, 2003).

We exploit aggregated administrative records of all 14- and 15-year-olds reported by the police to the judicial authorities, matched with enrollment rates in the first and second year of high school to provide causal estimates of the effects of youth enrollment ratio on adolescent offending rate. We adopt a 2SLS empirical strategy and compare 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 the heterogeneous effects of education on adolescent crime in the different areas of the country. The empirical evidence we provide is 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>6</sup>

While the 1999 Reform increased compulsory education across the whole country by one year, it determined different effects on adolescent crime in the South than it did in the North. Our baseline results show that a one percentage point increase in enrollment rate reduces adolescent offending rate of 14- and 15-year-olds by 1.3 percent in the North, while increasing it by 3.9 percent in the South. 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 6.9 percent in the North, while increasing it by 13.5 percent in the South. We also find that the crime reducing effect of education in the North of the country is mainly driven by the incapacitation effect induced by the Reform. On the contrary, the crime increasing effect that we find in the South of the country is consistent with a *criminal capital* accumulation channel, mainly operating through social interactions and organized-crime networks. Finally, a human capital accumulation effect of the increase in compulsory education on adolescent crime is excluded to have played any role in the short run in both areas of the country.

The rest of the paper is organized as follows. Section 2 describes the institutional setting, Section 3 presents our identification strategy and Section 4 describes the data and provides descriptive statistics. In Section 5 we present the baseline results and we conduct several robustness and specification checks. Section 6 provides evidence and discusses the potential mechanisms

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<sup>6</sup>For example, Machin et al. (2011) use a school leaving age reform which took place in England and Wales in 1972.

that could explain our results. Section 7 concludes and derives policy implications.

## 2 Institutional background: 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>7</sup>

The 1999 Compulsory Education Reform extended compulsory education by one year (from 8 to 9 years). 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 the 8<sup>th</sup> grade could not drop-out after the completion of the junior high school and were obliged to enroll and attend, at least, the first year of high school. 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.

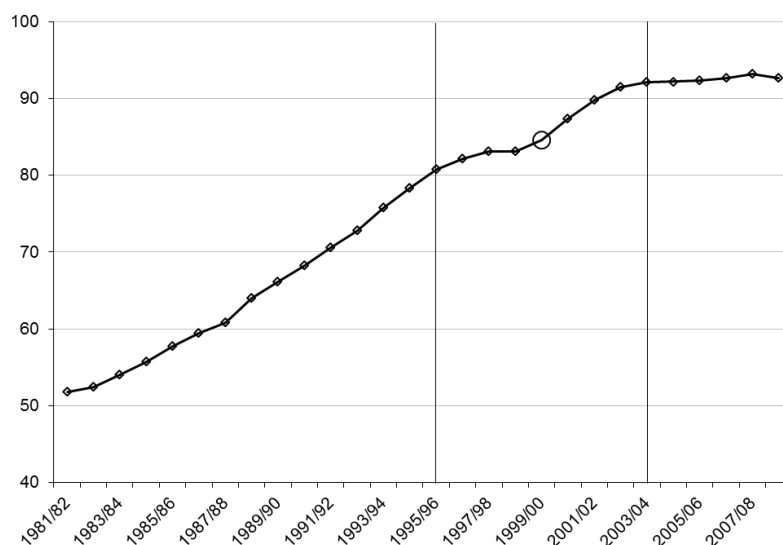
Figure 2 shows the increasing trend of the enrollment ratio for 14- to 18-year-old Italian adolescents over almost 20 years (from school year 1981/82 to school year 2009/10): a steeper increase can be detected in the years immediately after the introduction of the Reform. Figure 3 shows the increase of the average enrollment ratio of 14- and 15-year-old students (the targeted group) 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>8</sup> The Reform was applied in the same way and with the same timing across the whole country. This element, together with the richness of the administrative records collected, makes it possible not only to estimate the empirical relationship between enrollment and crime, but also to study

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<sup>7</sup>This limit was set by the Italian Constitutional Law established in 1948, immediately after World War II. Ordinary laws, as the Reform we are going to illustrate (Law No. 9/1999 and Ministry Decree No. 323/1999), can increase, but not decrease, this minimum drop out age.

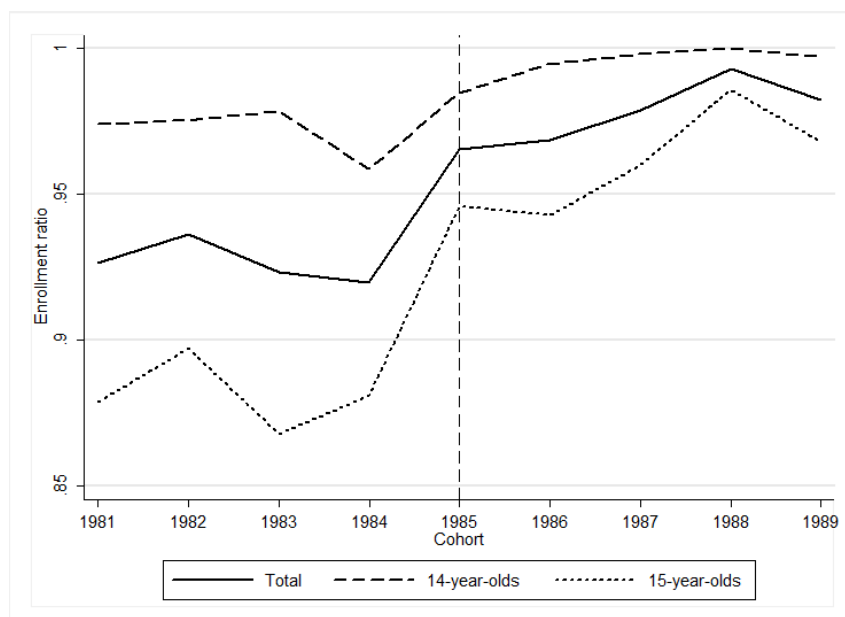
<sup>8</sup>For details, see Section 4.1

Figure 2  
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 National Institute of Statistics) 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 (multiplied by 100). The circle highlights the school year of the introduction of the 1999 Reform, the vertical lines delimit the period of observation used in the empirical analysis. SOURCE: ISTAT, Historical Series.

Figure 3  
Trends in the enrollment ratio of 14- and 15-year-olds



NOTES: the graph shows the average enrollment ratio of 14- and 15-year-old students (grades 9 and 10) by cohort. The vertical line denotes the first cohort affected by the 1999 Reform. SOURCE: own elaborations from ISTAT and MIUR.

how the same policy prescription could have had differential effects on adolescent crime across the territory and which underlying channels might help to explain such differences.

### 3 Identification strategy

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

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

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

where  $Crime_{apt}$  represents a measure of the criminal activity of the teenagers of age  $a$  living in province  $p$  in year  $t$ , and  $Edu_{apt}$  represents an educational measure of the teenagers of age  $a$  living in province  $p$  in year  $t$ ; in the baseline estimation we consider adolescents aged 14 and 15.<sup>9</sup> The vector  $X_{apt}$  includes control variables at province level varying over time and age group, while the vector  $W_{pt}$  includes 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, Machin et al., 2011): the vector  $X_{apt}$  includes demographic variables (i.e. the share of youth living in an urban area), while the vector  $W_{pt}$  includes the average labor force participation rate for 15- to 24-year-olds, which is intended to proxy for the (legal) labor market opportunities, the (natural logarithm of) provincial per capita value added as a proxy for the general level of wealth in each province, and the (natural logarithm of) provincial population, which, in combination with province fixed effects, implicitly controls for population density.

In order to control for time invariant unobserved heterogeneity in provincial characteristics and for annual trends in adolescent crimes, we use year ( $\phi_t$ ) and province ( $\omega_p$ ) fixed effects. We also include age fixed effects ( $\omega_a$ ) and age-specific linear time trends ( $t_a$ ), to control for the different propensity to commit crime across ages (Nagin and Land, 1993), and to account for time series variation particular to each age. Finally, we add the interaction between year fixed effects and regional fixed effects ( $\eta_j$ ), to capture relevant territorial dynamics which cannot be included in the vector of control variables, such as macro shocks to the economy which may have different impacts in the areas of the country or population dynamics.

Equation 1 describes the contemporaneous relationship between education and crime. Es-

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<sup>9</sup>That is:  $a \in [14; 15]$ ; Italian regions correspond to NUTS 2 level ( $j = 1 \dots 19$ ), each region includes a variable number of provinces (NUTS 3 level,  $p = 1 \dots 91$ ). See Section 4.1 for the detailed description of the education and crime measures used.

timating 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. 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 decisions. 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 the binary variable  $Reform_{at}$ , which is equal to one for the cohorts affected by the 1999 Reform and zero otherwise. Our sample considers 14- and 15-year-old students born between 1980 and 1989, where the cohorts affected by the Reform (i.e. our treated units) are those from 1985 onwards.<sup>10</sup> In the second step we estimate Equation 1 by 2SLS.

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 Figure 3 and by the statistical significance of the instrument, as shown in the following sections and tables, meaning that the Reform is a strong predictor of the adolescents' decisions to enroll at the first year of high school. The second point hinges on the assumption that the  $Reform_{at}$  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 the best of our knowledge, it was not enacted in response to concerns for juvenile delinquency, youth unemployment or other crime related factors (Benadusi and Niceforo, 2010); it was rather thought of as the first step in a comprehensive reform concerning the organization of the entire education system, which, eventually, did not even pass through the Parliament. A

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<sup>10</sup>We use a balanced sample of five treated cohorts (i.e. 1985 - 1989) and five control cohorts (i.e. 1980 - 1984).

validity threat for our identification strategy also relates to the potential correlation between unobserved crime determinants and time-varying area-specific unobserved characteristics also influencing enrollment decisions, if these are contemporaneous to the implementation of the Reform. For this reason, in all the specifications we control for region by year fixed effects, which allow to get rid of time-varying and area-specific phenomena. 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 of the education measure 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 margin* between dropping out and continuing education are affected by the policy in their educational decisions.

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:

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

The estimate from the crime reduced form (RF) (i.e.  $\beta_1$  in Equation 3) can be interpreted as an intention-to-treat (ITT) effect providing an overall estimate of the consequences on adolescent crime of raising compulsory education by one year, and representing the relevant policy parameter. Moreover, lacking specific or aggregated measures of school enrollment, this is the parameter usually estimated in the existing literature on the contemporaneous effects of school attendance on juvenile delinquency.

## 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, for 14- and 15-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 1995 to 2003), the age, and the province.<sup>11</sup> In the baseline empirical analysis we consider a time span covering nine school years, while in a later section we also check the robustness of our results using a

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<sup>11</sup>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 Autonomous Regions (i.e. *Val d'Aosta*, *Trentino* and *Alto Adige*) which followed a different implementation of the Reform, one (*Oristano*) because of missing data.

smaller time window.<sup>12</sup>

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.<sup>13</sup> 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 ( $Pop_{apt}$ ):

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

where  $p$  indexes the province,  $a$  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, as we also documented in the Appendix Table A.1. 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 for the school years 1995/96 to 2003/04, from the Statistical Office of the Italian Ministry of Education (MIUR). We focus on 14- and 15-year-old students as they represent the age group targeted by the Reform. We define the *Enrollment Ratio (ER)* as the number of adolescents who are enrolled in secondary education ( $enrollments_{apt}$ ) over the corresponding age-year-province population ( $Pop_{apt}$ ):

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

We then merge the two datasets by year, age and province cells and link socio-economic informa-

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<sup>12</sup>Starting from 2004 a new comprehensive educational reform was introduced and, among other things, 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 2004.

<sup>13</sup>ISTAT data on juvenile offenders are also available for 16- and 17-year-olds, while not for children aged 13 and below, because they cannot be formally sent to jail (Dipartimento di Giustizia Minorile, 2007). We will exploit this data in a later section.



tion at the province level from different data sources, such as the Labor Force Survey (ISTAT), the Public Finance Database (Italian Ministry of Interior), the Italian Demographic Database (ISTAT).<sup>14</sup>

## 4.2 Descriptive statistics

In Section 2 we describe the introduction of the Reform, and illustrate the effects it determined in the historical trend of the enrollment ratio. 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- and 15-year-olds) and on the side of the dependent variable (i.e. the offending rate of 14- and 15-year-olds).

Table 1  
Descriptive statistics

	mean	sd	max	min	N
Offending rate ( <i>OR</i> ) (overall)	10.41	6.06	56.50	0.00	1638
Offending rate: North	11.30	6.87	56.50	0.00	1044
Offending rate: South	8.86	3.82	24.31	0.00	594
Enrollment ratio ( <i>ER</i> ) (overall)	0.95	0.08	1.00	0.43	1638
Enrollment rate: North	0.96	0.07	1.00	0.43	1044
Enrollment rate: South	0.94	0.08	1.00	0.70	594
Urban share	0.13	0.19	0.87	0.00	1638
Occupation rate (15-24)	31.10	10.44	49.60	12.07	1638
Provincial per capita VA	14481.54	6487.91	45165.42	5148.52	1638
Provincial population	613731.79	643997.04	3980000	89775	1638

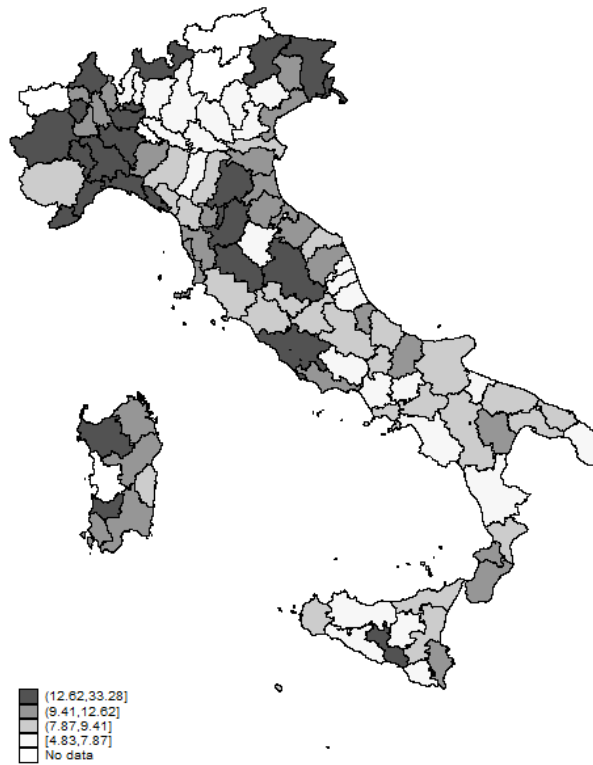
NOTES: offending rates are calculated per 1,000 of the corresponding province-age population. *Urban share* refers to the share of youth living in an urban area, *Occupation rate (15-24)* is the the average occupation rate for 15- to 24-year-olds, *provincial per capita VA* refers to the provincial per capita value added (in 2012 Euros), *provincial population* refers to the total provincial population. SOURCE: ISTAT and ISTAT-Demos.

Table 1 contains general descriptive statistics on the dependent and control variables included in the empirical specifications. The average enrollment ratio is equal to 0.95, while the average offending rate is of 10.41 adolescents reported to the judicial authority per 1,000 individuals of the corresponding age group population. The Reform increased by 6.3 percent the overall enrollment ratio: the raw increase in the enrollment ratio is slightly higher in the South (+8.2 percent) than in the North (+5.4 percent) of the country. 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 rapidly converge after the implementation of the Reform.

Figure 4 shows the geographical distribution across Italian provinces of the average offending rate in the years used in the empirical analysis. Higher levels are recorded in the Northern provinces and in the provinces with the main urban areas (e.g. Milan, Rome, Turin, Naples, Palermo). In Figure 5, we show the average residuals from OLS regressions where the dependent

<sup>14</sup>The match between the two measures is such that, for example, the enrollment ratio for the school year 1995/96 is linked to the offending rates of the year 1995.

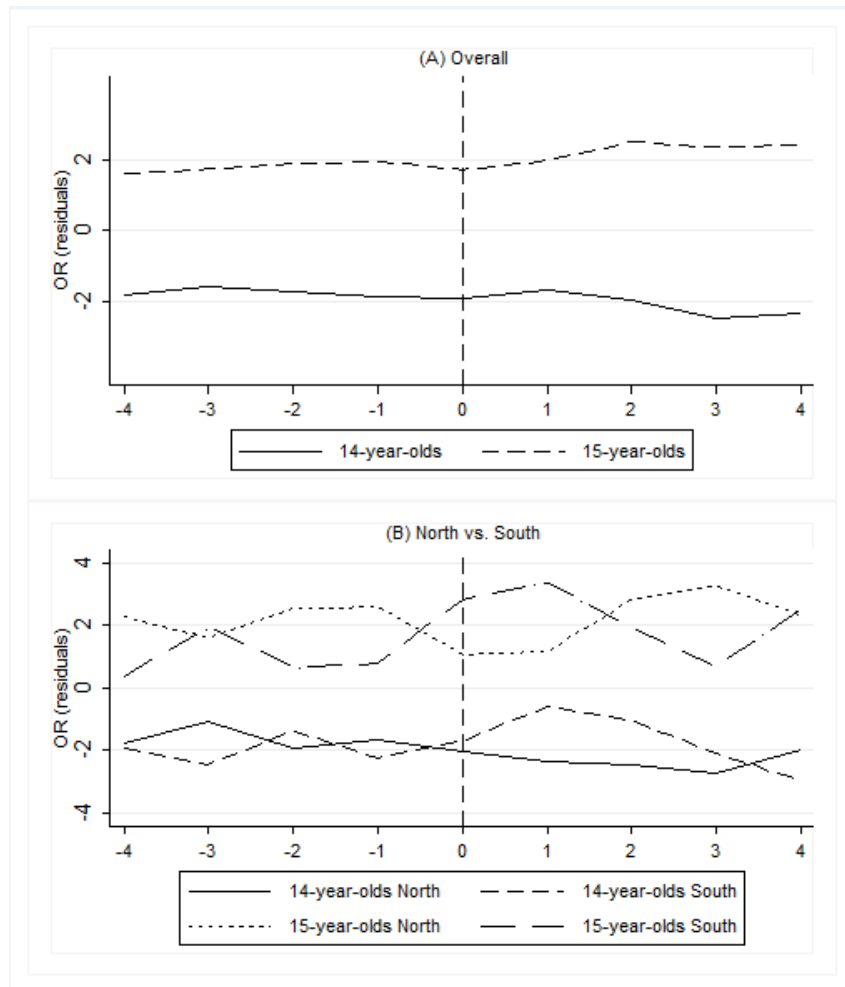
Figure 4  
Geographical distribution of the offending rate of 14- and 15-year-olds



NOTES: the map shows the average offending rate of 14- and 15-year-olds over the period 1995-2003 (rates expressed per 1,000 of the corresponding age group population). SOURCE: own elaborations from ISTAT.

variable is the offending rate and we include as regressors province and year fixed effects; the vertical lines denote the implementation of the Reform. Apparently, the introduction of the Reform does not determine a dramatic change in the pattern of the residuals (Figure 5.A). However, plotting the residuals distinguishing between the North and the South of the country makes it possible to uncover an interesting pattern (Figure 5.B): at the time of implementation of the Reform, the offending rate experiences a remarkable decrease in the North of the country, both for 14- and 15-year-olds, while the opposite occurs in the South, where a sharp increase can be detected. This descriptive evidence highlights crucial differences in the way in which juvenile delinquency reacted to the increase of the school leaving age between the two areas of the country that we aim to analyze in depth in the next section.

Figure 5  
Regional divides in the trends of the offending rates (residuals) by age



NOTES: the graphs show the average residuals from OLS regressions of the offending rates (indicated as  $OR$  (residuals) on the vertical axes) on year and province fixed effects. Panel (A) shows the overall trend for 14- and 15-year-olds; Panel (B) shows the trends for 14- and 15-year-olds, separately for the North and the South of the country. The vertical line denotes the first cohort affected by the 1999 Reform. SOURCE: own elaborations from ISTAT.

## 5 Baseline results

### 5.1 The contemporaneous effect of education on adolescent crime

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 population.<sup>15</sup>

Table 2  
Contemporaneous effects of education on adolescent crime: baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS			First stage	2SLS			Reduced Form		
ER	-1.59 (2.56)	-1.35 (1.72)	-0.82 (1.78)		4.66 (5.27)	2.65 (7.40)	2.79 (7.51)			
Reform				0.04*** (0.01)				0.31 (0.35)	0.12 (0.34)	0.12 (0.35)
First stage F-stat.					192.90	34.53	33.50			
N.Clusters	91	91	91	91	91	91	91	91	91	91
N.Observations	1638	1638	1638	1638	1638	1638	1638	1638	1638	1638
Province, year, age FE, age trend		yes	yes	yes		yes	yes		yes	yes
Region by year FE		yes	yes	yes		yes	yes		yes	yes
Control variables			yes	yes			yes			yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate of 14- and 15-year-olds (*OR*); the dependent variable of the first stage regression is the enrollment ratio (*ER*) of 14- and 15-year-olds. Control variables include the share of youth living in an urban area (*Urban share*), the average occupation rate for 15-24 year-olds (*Occupation rate (15-24)*), the logarithm of the provincial per capita value added (2012 Euro) (*ln(provincial per capita VA)*), the logarithm of provincial population (*ln(provincial population)*). 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 2 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 fixed effects (column 2) and the control variables ( $X_{apt}$  and  $W_{pt}$ , column 3). Results show a negative, but not statistically significant correlation, between the *ER* and the adolescent offending rate. Column (4) presents the results from the first stage regression where we test the relevance of our instrumental variable (*Reform<sub>at</sub>* 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- and 15-year-olds, while the first stage F-statistics lies above the thresholds specified by Stock and Yogo (2005), ensuring that estimates from the 2SLS regressions are not poorly identified. Columns (5), (6) and (7) contain the results from the 2SLS estimates: even if the model is well identified, we do not detect statistically significant effects. Similar results hold for the reduced form estimates (Equation 3) contained in columns (8), (9) and (10). Thus, considering the whole country, we find that the Reform had a positive effect on the enrollment ratio but we do not uncover any effects on the adolescents' offending rate, neither in the 2SLS, nor in the reduced form framework. However, the descriptive patterns illustrated in Figure 5 guide us to a deeper analysis considering separately the subsamples of provinces located in the North and

<sup>15</sup>The use of population weighted models increase the statistical precision of the estimates. The results are robust to the use of alternative weights and to the exclusion of weights (see Section 5.2).

in the South of the country.

Table 3  
Contemporaneous effects of education on adolescent crime: territorial divide

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage			2SLS			Reduced Form		
	Overall	North	South	Overall	North	South	Overall	North	South
ER				2.79 (7.51)	-15.12** (6.82)	34.98*** (12.13)			
Reform	0.04*** (0.01)	0.05*** (0.01)	0.03*** (0.01)				0.12 (0.35)	-0.78* (0.45)	1.20*** (0.40)
First stage F-stat.				33.50	17.17	29.75			
N.Clusters	91	58	33	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594	1638	1044	594
Control variables and FE	yes	yes	yes	yes	yes	yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate of 14- and 15-year-olds (*OR*); the dependent variable of the first stage regressions is the enrollment ratio (*ER*) of 14- and 15-year-olds. Control variables include the share of youth living in an urban area (*Urban 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(provincial pc VA)*), the logarithm of provincial population (*ln(provincial population)*); the fixed effects (FE) are the ones indicated in Table 2 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.

The 2SLS (columns 4-6) and the crime reduced form estimates (columns 7-9) in Table 3 show opposite results, revealing that there exists a crime reducing effect of education only in the Northern provinces, and that the Reform increased adolescent crime in the Southern ones. Specifically, an increase in the enrollment ratio (*ER*) by one percentage point determines a decrease in the offending rate of 15 incidences per 1,000 of the corresponding age group population in the North, while it determines an increase of 35 incidences in the South. In absolute terms, an increase by one percentage point of the enrollment ratio reduces the offending rate by almost 1.3 percent in the North, while increasing it by 3.9 percent in the South. Similar results hold for the reduced form equation: one additional year of compulsory education reduces the offending rate of 14- and 15-year-olds by 0.78 incidences per 1,000 of the corresponding age group population in the North, while increasing it by 1.2 incidences in the South. In relative terms, they represent, respectively, a 6.9 percent decrease and a 13.5 percent increase of the offending rate. Notice also that the reduced form coefficients are slightly larger (in absolute value) than the corresponding 2SLS estimates, since they may incorporate any indirect or spillover effect of the Reform.

The *crime reducing effect* we find in the North of the country is in line with Anderson (2013), who 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. Also the *crime increasing effect* is not new in the literature on the contemporaneous effects of school attendance on crime: Jacob and Lefgren (2003) and Luallen (2006) find that the level of property crime committed by juveniles decreases when school is in session, while the level of violent crime increases in the same days.

In order to understand these results, one should also bear in mind that incapacitation (i.e. the additional time to be spent at school) is not the only mechanism with which enrollment might affect crime. Another mechanism that applies to this framework is the 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 adolescents at school only interact with bad peers engaged in criminal activities, this could reinforce, instead of decrease, the youth crime rate. In Section 6, we will explore these potential mechanisms to explain whether they could have determined the different effects we observed across the Italian territory. Before doing this, in the following section we perform several robustness and sensitivity analysis to test the reliability of our baseline results.

## 5.2 Robustness and specification tests

As a first test of the robustness of our results, we take advantage of additional data that we collected for the crime and education measures in the years before the implementation of the Reform to perform a placebo exercise. In detail, we rerun our baseline regressions as if the Reform were implemented in the school year 1995/96 (instead of 1999/00), and thus contrasting the behaviors of adolescents born between 1981 and 1984 (our placebo treated group) with those born between 1977 and 1980 (our placebo control group).<sup>16</sup>

In Table 4 Panel A we present the results of a specification including the full set of fixed effects: the placebo reform does not have any statistically significant effect on the adolescent offending rate, neither overall nor in the two areas of the country, thus confirming the goodness of the identification strategy. In Table 4 Panel B we also include the time-varying control variables ( $X_{apt}$  and  $W_{pt}$ ). The inclusion of the control variables shrinks the number of observations: this is because some variables (i.e. the value added and the occupation rate) are not available for years before 1995 at the province level. The estimates are still not statistically different from zero.

Then, we test the robustness of our estimates over several dimensions and report the results

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<sup>16</sup>In this way, we exclude from the placebo exercise adolescents who were really affected by the Reform (i.e. those born in 1985 and afterwards). Since crime measures for older cohorts are not available, we use eight cohorts in the placebo instead of the ten cohorts used in the main analysis (i.e.  $\tilde{N} = 1,365$ ).

Table 4  
Placebo exercise

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage			2SLS			Reduced Form		
<i>Panel A</i>	Overall	North	South	Overall	North	South	Overall	North	South
ER				27.77 (58.74)	11.63 (23.29)	-43.93 (50.61)	0.29 (0.29)	0.28 (0.43)	0.30 (0.41)
Reform	0.01 (0.02)	0.02 (0.04)	-0.01 (0.00)						
First stage F-stat.				0.23	0.40	2.38			
N.Clusters	91	58	33	91	58	33	91	58	33
N.Observations	1365	870	495	819	522	297	819	522	297
Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
<i>Panel B</i>	Overall	North	South	Overall	North	South	Overall	North	South
ER				20.34 (25.93)	9.19 (13.99)	-275.96 (757.26)			
Reform	0.02 (0.02)	0.04 (0.04)	-0.00 (0.01)				0.37 (0.32)	0.33 (0.49)	0.49 (0.41)
First stage F-stat.				0.63	0.79	0.09			
N.Clusters	91	58	33	91	58	33	91	58	33
N.Observations	819	522	297	819	522	297	819	522	297
Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control variables	yes	yes	yes	yes	yes	yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate of 14- and 15-year-olds (*OR*). For the control variables and the fixed effects (FE) included in the specification see Table 2. 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.

in Table 5. The main analysis is performed considering a period of time ranging between the school years 1995/96 and 2003/04, with five cohorts affected by the Reform and five 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 comparability between cohorts. To test for this, we repeat the estimation keeping only seven school years, from 1996/97 to 2002/03. The results presented in Table 5, specification 1, are close to our baseline (Table 3): 2SLS coefficients are marginally larger, confirming that identification comes prevalingly from variation close to the discontinuity induced by the Reform.

Specifications 2, 3 and 4 of Table 5 implement different weighting schemes. Specification 2 shows the baseline estimates without weights: as expected, results deliver point estimates similar to the baseline, albeit less precisely estimated. Specification 3 implements a scheme in which more weight is given to the cohorts closer to the year of implementation of the Reform and to 14-year-old students, i.e. those directly affected (*Inverse Distance Weighting*, IDW, see Machin et al. (2011)).<sup>17</sup> As for specification 1, the coefficients are marginally larger, in absolute terms, with respect to the baseline. Specification 4 includes IDW interacted with population weights:

<sup>17</sup>IDW are constructed as follows:  $IDW = (1/d) \times W$ , where  $d$  is the distance in birth years from the discontinuity, and  $W$  is equal to 2 for 14-year-olds, and 1 for 15-year-olds.

Table 5  
Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS			Reduced Form		
<i>Specification 1</i>	Overall	North	South	Overall	North	South
ER	1.17 (8.22)	-19.17*** (7.09)	39.17*** (14.68)			
Reform				0.05 (0.35)	-0.90** (0.44)	1.20*** (0.42)
First stage F-stat.	25.97	13.27	23.15			
N.Clusters	91	58	33	91	58	33
N.Observations	1274	812	462	1274	812	462
<i>Specification 2</i>	Overall	North	South	Overall	North	South
ER	0.65 (9.72)	-12.69 (12.04)	30.74* (16.66)			
Reform				0.02 (0.40)	-0.52 (0.53)	0.99* (0.55)
First stage F-stat.	54.79	34.57	20.42			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
<i>Specification 3</i>	Overall	North	South	Overall	North	South
ER	0.30 (7.46)	-16.60** (7.07)	35.90*** (13.41)			
Reform				0.01 (0.34)	-0.87* (0.46)	1.07*** (0.38)
First stage F-stat.	37.45	22.48	22.88			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
<i>Specification 4</i>	Overall	North	South	Overall	North	South
ER	0.43 (7.36)	-16.70** (6.97)	35.21*** (12.99)			
Reform				0.02 (0.34)	-0.88* (0.46)	1.08*** (0.38)
First stage F-stat.	38.36	22.42	24.64			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
Control variables and FE	yes	yes	yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate of 14- and 15-year-olds (*OR*). For the control variables and the fixed effects (FE) included in the specification see Table 2. *Specification 1* conducts the analysis on a time window of 7 years; *Specification 2* shows the baseline specification without weights; *Specification 3* implements inverse distance weights (IDW); *Specification 4* implements inverse distance weights interacted with population weights. 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.



the results do not substantially differ from specification 3.

Table 6  
Sensitivity tests

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS			Reduced Form		
<i>Specification 1</i>	Overall	North	South	Overall	North	South
ER	2.74 (7.59)	-15.35** (6.91)	35.28*** (12.30)			
Reform				0.12 (0.35)	-0.78* (0.45)	1.20*** (0.40)
First stage F-stat.	33.30	17.13	28.39			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
<i>Specification 2</i>	Overall	North	South	Overall	North	South
ER	1.90 (7.25)	-14.58** (6.95)	32.36*** (11.68)			
Reform				0.08 (0.32)	-0.76* (0.44)	1.09*** (0.38)
First stage F-stat.	40.26	20.83	31.04			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
<i>Specification 3</i>	Overall	North	South	Overall	North	South
ER	2.84 (7.41)	-14.91** (6.76)	34.07*** (11.56)			
Reform				0.13 (0.35)	-0.78* (0.45)	1.21*** (0.40)
First stage F-stat.	34.86	17.40	32.52			
N.Clusters	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594
Control variables and FE	yes	yes	yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate of 14- and 15-year-olds (*OR*). For the control variables and the fixed effects included in the baseline specification see Table 2. *Specification 1* excludes from the baseline specification the age trends; *Specification 2* adds to the baseline specification regional linear time trends; *Specification 3* adds to the baseline specification region by age 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.

Finally, we perform a set of sensitivity checks for the specification used in the baseline analysis, reporting the results in Table 6. Age-specific time trends are added in the baseline specification to account for time series variation particular to each age; specification 1 of Table 6 relaxes this assumption and shows the estimates of the baseline model excluding them. In the baseline analysis, the region by year fixed effects account for time-variant unobserved factors. However, each region can be characterized by its own time trend for adolescent offending rates, and specification 2 controls for region-specific time trends. Specification 3 adds age-by-region fixed effects that can be included in the model to allow for separate shifts in the offending rates of 14- and 15-year-olds in different regions. The results for the three specifications do not

significantly differ from the estimates of the baseline model.<sup>18</sup>

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 might be appropriately estimated with count-data models (Jacob and Lefgren, 2003, Luallen, 2006, Osgood, 2000): our reduced form results are also robust to the use of negative binomial and Poisson regressions.

## 6 Potential mechanisms

As a matter of fact, the same treatment induced by the Reform had differential effects across the territory and this could be plausibly due to the relative role that the channels highlighted above might play in the different areas of the country. In what follows, we provide evidence on the underlying mechanisms that are likely to explain our results.

### 6.1 The human capital channel

Additional years of compulsory education might affect youth criminal behavior through a human capital accumulation effect, that is, staying longer at school should increase the expected value of a future wage, as well as skill acquisition and the subsequent expectation of finding a job. Moreover, additional school time could also convey social values and, in general, modify the individuals' time preferences and expectations (Lochner, 2011). All these elements are likely to reduce juvenile delinquency, through an increase in the expected cost of committing a crime.

If a human capital effect is at work we should expect the crime reducing effect of education to propagate in the years immediately after the school leaving age.<sup>19</sup> The relevance of this channel in our context can be assessed extending our empirical analysis exploiting the crime and education measures collected for 16- and 17-year-olds. In this case, we compare the same group of cohorts as the ones used in the main specification, but focusing on the offending rates when they are one or two years older. The results are presented in Panel A of Table 7. Overall, we

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<sup>18</sup>The results are also robust to alternative clustering (i.e. at the cohort and at the cohort by province level). Estimates are available upon request from the authors.

<sup>19</sup>Notice that seminal works on the crime reducing effect of education focus on the human capital channel at later ages (e.g. 18- to 40-year-olds for Machin et al. (2011); 20- to 60-year-olds for Lochner and Moretti (2004)). Thus, our exercise does not discard a human capital effect later in life, rather it focuses on the years immediately after compulsory education.

Table 7  
Potential mechanisms: human capital accumulation and incapacitation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	First stage			2SLS			Reduced Form		
<i>Panel A</i>	Overall	North	South	Overall	North	South	Overall	North	South
ER				42.41 (34.59)	-1.72 (24.94)	–			
Reform	0.01*** (0.00)	0.03*** (0.01)	-0.00 (0.01)				0.53 (0.41)	-0.04 (0.61)	1.19** (0.55)
First stage F-stat.				10.60	19.10	–			
N.Clusters	91	58	33	91	58	–	91	58	33
N.Observations	1638	1044	594	1638	1044	–	1638	1044	594
	OLS			First stage			2SLS		
<i>Panel B</i>	Overall	North	South	Overall	North	South	Overall	North	South
ER (Net)	-2.60 (1.78)	-4.65** (2.11)	-0.27 (2.25)				2.36 (6.90)	-11.56** (5.78)	49.47** (23.15)
Reform				0.05*** (0.01)	0.06*** (0.01)	0.03*** (0.01)			
First stage F-stat.							46.88	73.47	10.66
N.Clusters	91	58	33	91	58	33	91	58	33
N.Observations	1638	1044	594	1638	1044	594	1638	1044	594
Control variables and FE	yes	yes	yes	yes	yes	yes	yes	yes	yes

NOTES: population weighted models estimated on age-year-province cells. The dependent variable of the 2SLS and reduced form models is the offending rate (*OR*); the dependent variable of the first stage regressions is the enrollment ratio (*ER*). Panel A shows the estimates on 16- and 17-year-olds; Panel B shows the estimates using the enrollment ratio net of students dropped-out. This is obtained excluding from the number of students enrolled in each grade the fraction of students dropped-out during the school year. For the control variables and the fixed effects included see Table 2. 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.

do not find effects on 16- and 17-year-olds offending rates, both in the 2SLS and reduced form estimates (columns 4 and 7), although the estimates of the first stage regressions (columns 1, 2 and 3) indicate that the the Reform continued to have some minor effects in increasing the enrollment ratio, but in Northern regions only.

Concerning the subsamples corresponding to the two areas of the country, the 2SLS estimates are identified for the Northern subsample only (column 5), which, however, does not show any statistical significant effect, also in the reduced form estimates (column 8). In the Southern provinces, the cohorts treated by the Reform do not increase their enrollment in the years after the compulsory path ends, so that the 2SLS estimation is not even identified (column 6). Estimates of the crime reduced form model (column 9) show a statistically significant increase of 1.2 incidences per 1,000 of the corresponding age-group population, corresponding, in absolute terms, to an increase in the offending rate of 6.95 percent. This reduced form effect corresponds to almost a half of the effect that we have found in the baseline estimates for 14- and 15-year-olds (Table 3). This exercise shows that the human capital accumulation effect is not a relevant channel in our context. If anything, a crime reducing effect of education does not persist for 16- and 17-year-olds in the Northern regions, while the reduced form estimates show that the

increase in the offending rates of 14- and 15-year-olds in Southern regions is still present, although smaller, for 16- and 17-year-olds. This persistence in Southern provinces seems rather to point to the accumulation of a *criminal capital* when adolescents are forced to interact staying at school one additional year (Bayer et al., 2009). We will come back on social interactions effects in a later section.

## 6.2 The incapacitation channel

Incapacitation should decrease adolescent crime through a mechanical factor: while at school adolescents have less opportunities to commit crimes. However, an increased incapacitation induced by a compulsory education reform, as in our case, has the contemporaneous effect of increasing students' concentration in the school facilities, which, in turn, might increase social interactions among school mates (Jacob and Lefgren, 2003). While disentangling the separate contribution of the incapacitation and social interaction channel on adolescent criminal behavior is not possible in our setting, we provide evidence to understand whether each channel might be potentially at work in the two areas of the country.<sup>20</sup>

To understand whether the incapacitation channel is playing a relevant role in our estimates, we focus on the difference between students' enrollment and students' attendance. Indeed, the use of the enrollment ratio (*ER*) could not take into account the fact that a fraction of students forced to enroll in the first year of the high school (grade 9) could have then (illegally) dropped-out during the school year.<sup>21</sup> Figure 6 shows the share of 14- and 15-year-old students dropped-out during the school year with respect to corresponding age group population. The Reform reduced the drop-out rate, and this effect is stronger for 14-year-olds and in the North of the country (Figure 6.A).

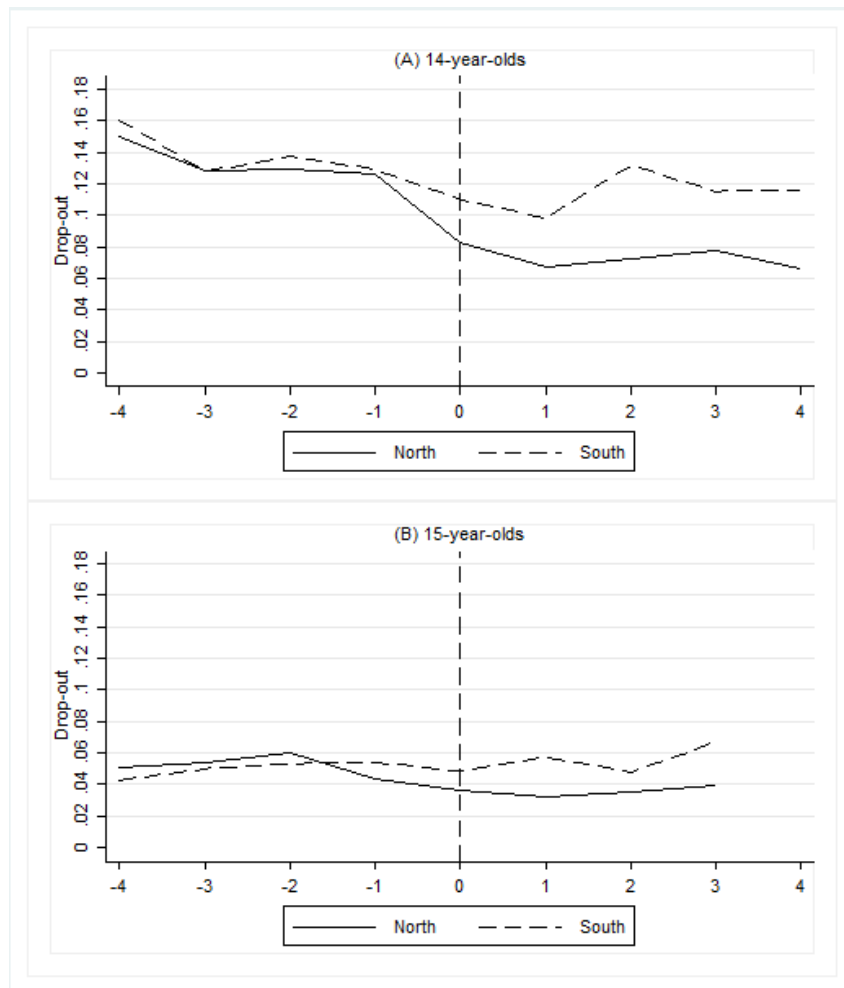
In Panel B of Table 7 we repeat our baseline analysis using a measure of enrollment ratio *net* of the share of students dropped out. This exercise makes it possible to focus on a different set of compliers, i.e. those students who changed their schooling behavior because of the Reform and remain at school for the entire school year. Comparing these results with our baseline estimates (Table 3) we observe that the crime reducing effect in the North is smaller, in absolute terms. On the contrary, the crime increasing effect in the South is higher: while the result for the North is in line with an incapacitation channel, the one for the South is supportive of a criminal capital accumulation mechanism. That is, pushing a large fraction of marginal students at school

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<sup>20</sup>To the best of our knowledge, this empirical issue still remains an open question in the existing literature on the contemporaneous effects of education on youth crime.

<sup>21</sup>Dropped-out students are defined by the Statistical Office of the Ministry of Education (MIUR) as those students who are enrolled at the beginning of the school year (in September), and who are missed during the school year, so that, at the end of the school year (in June), they are neither promoted to the next grade, nor retained. These measures are available only at the macro-area level.

Figure 6  
Trends in the drop-out of 14- and 15-year-olds



NOTES: the graphs show the ratio between the number of students dropped-out and the correspondent age group population (indicated as *drop-out* on the vertical axes). Students dropped-out are defined by the Statistical Office of the Ministry of Education as those who enrolled at the beginning of the school year (in September) and do not sit the final scrutiny (in June) in any school. Panel (A) shows the trend for 14-year-olds; Panel (B) shows the trend for 15-year-olds. The vertical line denotes the first cohort affected by the 1999 Reform. SOURCE: own elaborations from ISTAT and MIUR.

and increasing their concentration in the school facilities determined a crime increasing effect, especially on those who effectively stayed at school and took the entire *incapacitation treatment* imposed by the Reform. Differently, there seems to be a *deterrence effect* induced by the Reform on those who illegally dropped-out during the additional compulsory year (both in the North and in the South). While before the 1999 Reform, 14-year-olds were free to drop-out at the end of the junior high school (and, eventually, to start working), this was no longer admitted when the Reform came into force.<sup>22</sup> Thus, adolescents who left school during their additional compulsory year (in the 9<sup>th</sup> grade), received a different treatment with respect to the older cohorts, and the observed decrease in their engagement in criminal behaviors could be plausibly linked to

<sup>22</sup>The Law 977/1967 on *Children and Adolescent Labor* disciplined some kind of jobs (e.g. working in the agricultural sector) which could be legally started at the age of 14. The Reform set the limit of the age of 15 to legally start any kind of job.

their status of illegality determined by the introduction of the Reform, as a consequence of their decision to drop-out. Finally, given that a larger fraction of students in the South (as compared to the North of the country) dropped-out during the first year of implementation of the Reform, this unintended effect of deterrence on the dropped-out was plausibly larger in the South.

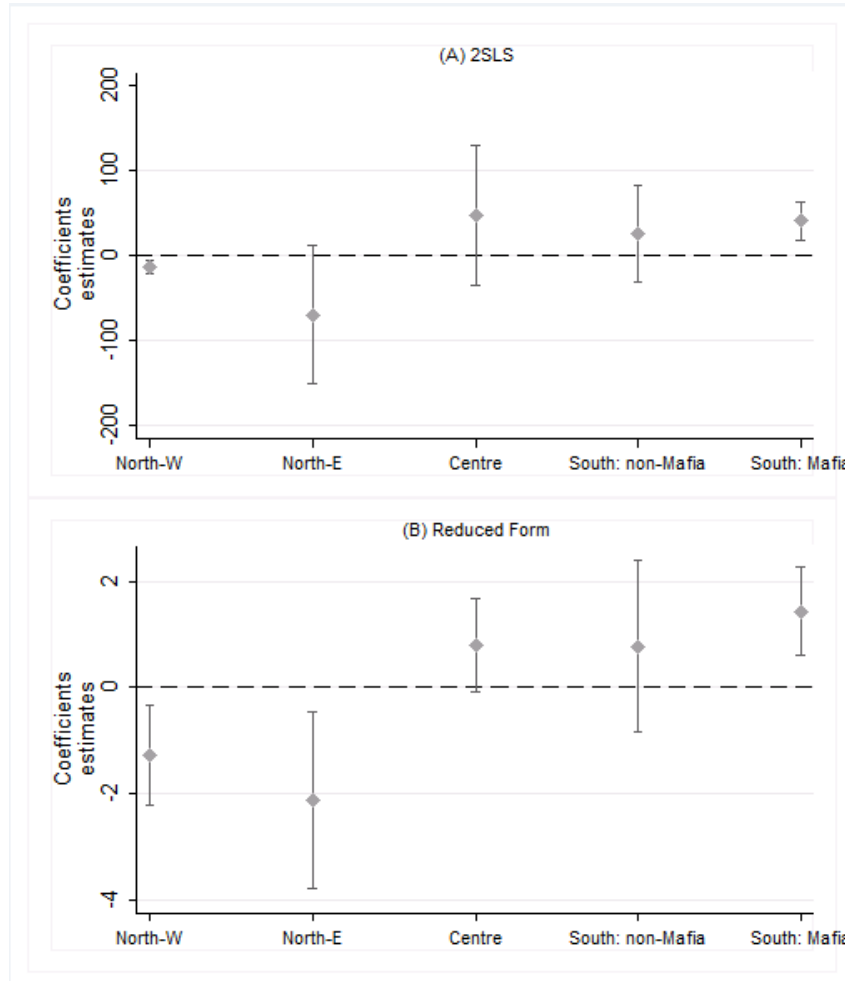
### 6.3 The social interactions and the criminal network channels

Increasing the amount of time adolescents spend at school may have effects on their criminal behavior which are *ex ante* undetermined, as they may strictly depend on the kind of peers they are forced to interact with in the additional time they spend at school. If increasing the time spent at school also increases the time an adolescent spends with *bad* peers, then the additional time spent at school might have the unintended consequence of helping the individuals to create criminal networks, or to get in contact more easily with organized criminal networks already existent outside the school, or it might help to reduce the cost of coordination for committing a crime with peers (Jacob and Lefgren, 2003). In this sense, additional time spent at school could also contribute to create or accumulate a *criminal capital* (Bayer et al., 2009). In Table 7 (Panel A, column 9) we have already documented that the increasing effect on adolescent offending rate in the South persists at older ages (i.e. 16 and 17). This can be interpreted as an indirect evidence of an increased criminal capital for those who were forced to spend more time at school. Italian Southern regions are also heavily influenced by the pervasive presence of Mafia-type criminal organizations (Calderoni, 2011, Pinotti, 2014). In these areas, 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 (Damm and Dustmann, 2014). 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.

We 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) on the basis of a simple 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 deeply rooted in the economic and social networks, notably: Campania, Calabria and Sicily (Lupo, 1993, Pinotti, 2014). Then we repeat our baseline analysis on the subsamples of provinces partitioned according to this criterion. As a sensitivity check, we use as a second measure to identify areas where Mafia is

more pervasive, the *Mafia Index* taken from Calderoni (2011), which is a comprehensive measure of the presence of organized crime in each province.

Figure 7  
Heterogeneous effects across macro-areas: focus on Mafia regions

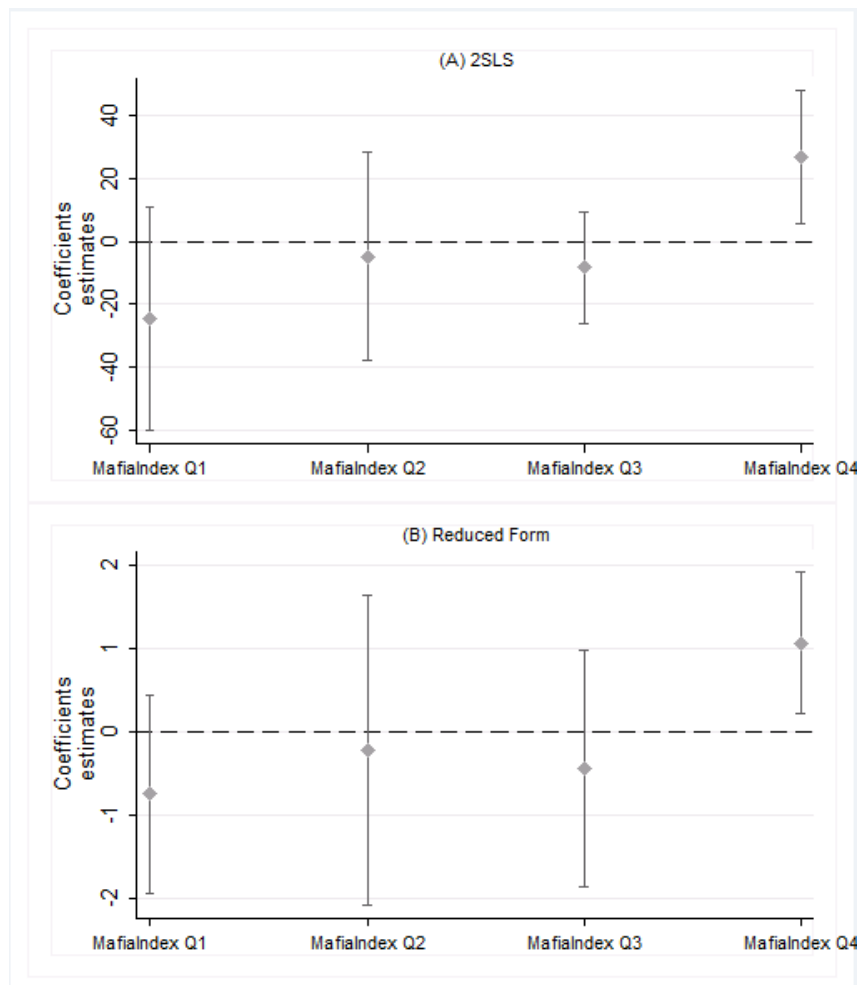


NOTES: the graphs show the estimated coefficients and 95% confidence intervals of population weighted models on the subsamples of provinces in the North-West, North-East, Center and South of Italy. Southern regions are divided into two groups: Mafia and non-Mafia regions. *Mafia regions* include the 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 Southern regions (Pinotti, 2014). Panel (A) shows results from the 2SLS model; Panel (B) from the crime reduced form model. SOURCE: own elaborations from ISTAT and MIUR.

Figure 7 plots the 2SLS and reduced form coefficients (i.e. the parameter  $\theta_1$  of Eq. 1 and  $\beta_1$  of Eq. 3, respectively), and the corresponding 95% confidence intervals, for the subsamples of provinces defined as explained above. Figure 8 repeats the same exercise partitioning all the provinces into the quartiles of the distribution of the *Mafia Index*. Focusing on Figure 7, it can be noticed that while the crime reducing effect of education is prevalently associated to the provinces in the North-West of the country, while the crime increasing effect is present in Mafia-regions only.<sup>23</sup>

<sup>23</sup>In particular, the negative and statistically significant effects (both in the 2SLS and in reduced form models) arise from the two biggest regions in the North-West (Lombardy and Piedmont), while the positive and statistically significant effects originate from two of the Mafia regions (Campania and Sicily). Detailed results are available

Figure 8  
Heterogeneous effects according to the Mafia Index



NOTES: the graphs show the estimated coefficients and 95% confidence intervals of population weighted models on the subsamples of provinces in the four quartiles of the distribution of the Mafia Index (Calderoni, 2011). Panel (A) shows results from the 2SLS model; Panel (B) from the crime reduced form model. SOURCE: own elaborations from ISTAT, MIUR and (Calderoni, 2011).



The increasing effects of education on juvenile crime documented in the literature are usually linked to the differences in the types of offenses committed (Jacob and Lefgren, 2003, Luallen, 2006). To test whether the 1999 Reform had a differential impact on the types of crimes in the South of the country, we take advantage of additional data which make it possible to distinguish between violent, property and drug related crimes. A drawback of these data is that, for confidentiality reasons, they allow to construct only region by year cells of the overall offending rate of adolescents aged 14 to 17. Thus, while proving useful to our purposes, the results must be interpreted with caution.<sup>24</sup>

Table 8  
Analysis by crime category

	(1)	(2)	(3)	(4)	(5)	(6)
	Violent			Property		
Reform	0.18 (0.33)	0.08 (0.35)	0.10 (0.34)	-0.85 (0.85)	-1.45 (0.90)	-1.21 (0.77)
Reform * South		0.20 (0.26)			1.20* (0.66)	
Reform * Mafia regions			0.23 (0.29)			0.07 (0.69)
R. sq.	0.72	0.72	0.72	0.71	0.71	0.70
N.Observations	178	178	178	178	178	178
	Drug and economic related			Total		
Reform	-0.65** (0.31)	-1.08*** (0.32)	-1.09*** (0.31)	-1.57 (1.36)	-2.82* (1.45)	-2.34 (1.42)
Reform * South		0.85*** (0.24)			2.47** (1.06)	
Reform * Mafia regions			1.19*** (0.26)			2.09* (1.20)
R. sq.	0.53	0.56	0.58	0.64	0.65	0.65
N.Observations	178	178	178	178	178	178
Region trends and year FE	yes	yes	yes	yes	yes	yes
Control variables	yes	yes	yes	yes	yes	yes

NOTES: population weighted OLS estimated on region-year cells. The dependent variable of the 2SLS and reduced form models are the regional offending rates calculated for 14- to 17-year-olds. For the control variables included in the baseline specification see Table 2. The specification also includes year fixed effects and regional linear trends. Robust standard errors in parenthesis. Asterisks denote statistical significance at the \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels.

We perform a reduced form analysis contrasting the adolescent offending rate before and after the implementation of the Reform, estimating OLS regressions where we include year fixed effects, regional-specific time trends, and the usual vector of time variant control variables calculated at the regional level ( $X_{jt}$ ).<sup>25</sup>

upon request.

<sup>24</sup>Based on the regressions performed on 16- and 17-year-olds, we may expect these results to provide the lower bound of the true effects, at least for the Northern provinces. We do not show the results for the residual category (*Other crimes*) as it does not display any statistically significant effect. They are available upon request.

<sup>25</sup>The sample of school years is the same used in the main analysis. Given that we cannot distinguish between the adolescent aged 14 or 15, and given that in the school year 1999/00 the Reform only applied to 14-year-olds (and not to 15-year-olds), we drop the observations for this year.

In line with our baseline analysis, we do not find statistically significant effects overall. To see whether a differential effect is present in the South, we interact the dummy *Reform* with a dummy variable indicating the regions located in the South. First, the estimates on the total offending rate confirm the findings in the main analysis: while the Reform has a crime decreasing effect in the North, this effect is considerably mitigated in the South. Then, while no statistically significant effects are found for violent crimes, drug and economic related crimes decrease in the North more than in the South. Given that drug and economic related crimes are especially sensitive to juvenile concentration and the creation and exploitation of criminal networks (Damm and Gorinas, 2014), these findings are consistent with the idea that the increase in the youth concentration and in the amount of time spent in school facilities, increased adolescents interactions and widened their illegal market prospects for petty crimes, such as smuggling and drug trafficking. Finally, in Table 8, columns 3 and 6, we also evaluate the effects of the Reform on the different types of offenses focusing on Mafia regions, instead of on the whole subsample of the Southern regions. The analysis confirms that the effects estimated for the South prevalently originate from Mafia-related regions: the total offending rate significantly increases in the Mafia regions as compared to the rest of the country, while drug and economic related crimes significantly decrease in the rest of the country but marginally increase in the Mafia regions.

## 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 local institutions and social capital may explain an important part of the territorial variability in crime rates. Concerning the causal relation between education and crime, only Luallen (2006) finds that the incapacitation effect of school causes a statistically significant effects in the youth crime rate in urban areas, while no effects are detected rural and sub-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, by adopting a 2SLS estimation and by using the Reform as an instrument, we find that a one percentage point increase in enrollment rate at high school (grades 9 and 10) decreases the adolescent offending rate by 1.34 percent with respect to the mean in the North of the country, while increasing it by almost 4 percent in the South. Edu-

cation might affect youth criminal behavior through several channels, which may play different role across the territory and help to explain the observed regional divide. Extending the analysis to the offending rates of older adolescents, we show that a human capital effect does not operate in the short run. We find that the crime reducing effect of education uncovered in the North of the country is mainly driven by the incapacitation channel, as typically shown in the recent literature (Anderson, 2013, Bertheleon and Kruger, 2011). Conversely, the increase in the adolescent offending rate observed in the South is consistent with a criminal capital accumulation mechanism, mainly operating through the channels of the social interactions and the organized criminal networks. Conducting the analysis by crime category also shows that drug and economic related crimes are the more influenced by the increasing effect observed in the South, consistently with the extension of the (illegal) market size for drug trafficking and smuggling through an increased concentration of peers in the school facilities (Damm and Gorinas, 2014, Jacob and Lefgren, 2003, Luallen, 2006).

Southern Italy is an area where Mafia-type criminal organizations deeply influence social life and economic activities (Mete, 2009, Pinotti, 2014). Growing up and attending school in such areas may have important effects on adolescent criminal behavior (Cederna, 2013, Damm and Dustmann, 2014). Our results suggest that one additional year of schooling can have different effects on adolescent offending rates depending on 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 and this seems to be even exacerbated through the increase youth concentration in the school facilities which followed the implementation of the Reform.

The evidence we present also points to the fact that keeping youths at school simply to prevent them from staying on the street in areas characterized by pervasive organized crime is not enough to ensure the same level of crime reduction as it could be reached in areas where organized crime is not pervasive. Still, education policies should be aimed at stimulating a role for the human capital effect to work, increasing the sense of civic duty and legality and designing adequate school activities for students living in areas where illegal opportunities seem more profitable than the legal activities. For example, Heller et al. (2013) analyze the effect of an intervention targeted for disadvantaged male youth attending grades 7-10 in high-crime Chicago neighborhoods. Interestingly, the intervention does not simply entail an increase of the time spent at school; rather it was composed by a vast array of programs including regular interactions with pro-social adults and in-school cognitive behavioral therapy.<sup>26</sup> The authors show that

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<sup>26</sup>Cognitive behavioral therapy (CBT) is psychological treatment designed to reduce common judgment and

program participation reduced violent-crime arrests during the program year 44 percent, a crime reducing effect that more than doubles what the existing literature on the incapacitation effect of schooling has generally found.

Further research is thus needed in order to disentangle the separate contribution of the incapacitation and social interactions channels in determining their effects on adolescent crime, and to understand how these channels might interact with the existing criminal networks and social institutions. Indeed, a deeper understanding of these mechanisms might also be particularly useful to design optimal behavioral policy interventions to counter adolescent crime.

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decision-making problems related to automatic behavior and biased beliefs.

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## A Juvenile delinquency in selected countries

Table A.1  
Characteristics of juvenile delinquency in selected countries

	<i>Suspected</i>		<i>Prosecuted</i>		<i>Incarcerated</i>		Incarceration rate	
	Juvenile	Adult	Juvenile	Adult	Juvenile	Adult	Juvenile	Adult
Italy	53.03	1,228.75	32.89	906.62	1.6	100.82	3.03	8.20
US	457.33	2,565.14						
Canada	265.65	1,666.35	174.37	1,156.30	6.91	99.54	2.60	5.97
Spain	55.25	620.91						
Germany	344.15	2,329.71	93.01	814.02				
England and Wales			246.6	3,275.72	4.35	122.41		
Denmark	136.77	907.06			0.37	74.23	0.27	8.18
Sweden	281.31	890.3	158.43		3.4	81.57	1.21	9.16
Greece	26.96	1,847.87			0.73	88.2	2.71	4.77
Japan	96.73	205.82	1.23	140.86	0.05	61.76	0.05	30.01
Portugal	44.79	2,025.08	111.61	862.52	5.02	117.4	11.22	5.80

NOTES: figures are expressed as rates per 100,000 of total population. *Suspected individuals* are defined as persons brought into formal contact with the criminal justice system, regardless of the type of crime (where formal contact might include being suspected, arrested, cautioned); *Incarcerated individuals* are defined as persons held in prisons, penal institutions or correctional institutions (including institutions for pretrial detention), regardless of the type of crime, in a selected date (June, 30); *Incarceration rate* is obtained as the percentage of incarcerated individuals over the total number of suspected individuals. Juveniles are defined as individuals under the age of 18 in all the selected countries except: Sweden (under 21), Portugal (under 20), Japan (under 19). SOURCE: United Nations Survey of Crime Trends and Operations of Criminal Justice Systems (UN-CTS), *United Nations Office for Drugs and Crime* (UNODOC) (available at <https://www.unodc.org/unodc/en/data-and-analysis/statistics/historic-data.html>).



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