Does increasing compulsory education decrease or displace adolescent crime? New evidence from administrative and victimization data*

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Abstract

This paper estimates the contemporaneous effect of education on adolescent crime by exploiting the implementation a reform that increases the school leaving age in Italy by one year. We find that the Reform increases the enrollment rate of all ages, but decreases the offending rate of 14-year-olds only, who are the age group explicitly targeted by the Reform. The effect mainly comes from natives males, while females and immigrants are not affected. The Reform does not induce crime displacement in times of the year or of the day when the school is not in session, but it increases violent crimes at school. By using measures of enrollment and crime, as well as data at the aggregate and individual level, this paper shows that compulsory education reforms have a crime reducing effect induced by incapacitation, but may also lead to an increase of crimes in school facilities plausibly due to a higher concentration of students .

JEL Classification: I28, J13, K42, R10

Keywords: adolescent crime; school enrollment; crime displacement; incapacitation

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1 Introduction

In the last decade, a growing interest has been devoted to the positive externalities induced by an increase of the level of education of a society (Lochner 2011; Oded 2011). Juvenile crime is one of the non-economic outcomes potentially affected by educational policies.[1] Adolescents involved in criminal activities represent 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 22 percent in Canada and almost 27 percent in Germany and Sweden.[2] Adolescent crime is also a particular policy concern, because the victims of crimes committed by juveniles are mostly other juveniles.

In this paper, we examine the contemporaneous effects between education and adolescent crime, by exploiting a school leaving age reform implemented in Italy in 1999.[3] By exploiting both administrative records and individual-level data from victimization survey, we provide a comprehensive investigation of the relationship between educational policies and adolescent crime. The data that we have at disposal allows us to discuss the mechanisms driving the results, as well as to inquire into potential side effects that may arise, such as the temporal and spatial displacement of criminal activity.

Starting from the seminal works of Jacob and Lefgren (2003) and Lochner and Moretti (2004), several papers have assessed the effects of education on adult criminal behavior (Lochner 2011).[4] Only a few studies have looked at the contemporaneous effects of education policies on adolescent crime (Hjalmarsson and Lochner 2012). Luallen (2006) and Jacob and Lefgren (2003) estimate, in a reduced form setting, the effects of different school interventions which induced day-by-day variation in the time spent at school by teenagers: Luallen (2006) shows that total juvenile crime increases by 21.4 percent on days when teachers' 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. Anderson (2014) exploits changes in the minimum dropout age in the US, finding that exposure to a minimum dropout 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. Åslund et al.

[1] Other notable examples include health, civicness, political participation and religiosity. See, among others, Berinsky and Lenz (2010); Card and Giuliano (2013); Hungerman (2014); Lochner (2011).

[2] United Nations Survey of Crime Trends and Operations of Criminal Justice Systems, UN-CTS 2005.

[3] Hereafter, we refer to it as the *Reform*.

[4] Lochner and Moretti (2004) show that school attainment significantly reduces the probability of arrest and incarceration of adult males in the US. For the UK, Machin et al. (2011) find that a 10 percent increase in the average school leaving age lowers crimes of 18 to 40-year-olds by 2.1 percent. Hjalmarsson et al. (2015), by exploiting an education reform implemented in the 1950s in Sweden, document a decrease in the likelihood of future convictions and incarceration. For Italy, Buonanno and Leonida (2009) show a robust negative correlation between education and crime measures at the regional level.

(2015) study the impact of an educational reform in Sweden, which extended from two to three years the high school vocational track and improved its educational contents, on both juvenile and adult crime. They find that the reform reduced property crimes, especially in the ages interested by the increased time spent at school.

This literature suggests that education may affect juvenile crime through three main channels. First, there may be an incapacitation effect, due to the fact that adolescents are forced to stay at school, where they may have fewer opportunities to commit crimes than on the street (Lochner 2011). Second, longer periods of school attendance presumably increase students' human capital accumulation and change their expected probability of finding a job after completing school; this may make juvenile detention more costly, and should reduce the incentives to engage in crime. This human capital channel may affect youth crime immediately, but it may also last over time (Lochner and Moretti 2004; Machin et al. 2011). Third, social networks may play a role in shaping adolescent crime, because adolescents' risky and criminal behaviors are particularly influenced by peers, and compulsory education might increase juvenile concentration in the school buildings and thus foster such interactions (Card and Giuliano 2013).

Our work is among the few papers that look at the *contemporaneous effects* of education on crime, and it contributes to the literature in four ways. *First*, since we have at disposal measures of both enrollment and adolescents' involvement in criminal activity, we can estimate the reduced form effects of the Reform on both education and crime, as well as the causal effect of education on crime, by using the Reform as an instrument. The availability of a measure of education makes it possible to show how the implementation of the Reform affected adolescents' enrollment decisions, and thus to test the first stage. *Second*, we take advantage of several data sources (administrative yearly and quarterly aggregate data and individual-level victimization surveys) to investigate not only the aggregate contemporaneous effect of education on adolescent crime, but also the mechanisms driving the results. More precisely, the aggregate information on yearly enrollment and offending rate enables us to discuss whether the results can be consistent with an incapacitation or a human capital mechanism. The quarterly administrative data and the individual-level data, instead, allows us to shed lights on other potential side effects of the educational reform on crime: the temporal displacement of criminal activity to other times of the year, or of the day, or the spatial displacement from the street to the school facilities. *Third*, we provide evidence on the effects on immigrant adolescents. To the best of our knowledge, this is the first time that such an important issue is investigated, even though our results cannot be considered as conclusive, due to the limited share of immigrant students in the Italian school system at the time of the Reform. *Fourth*, the empirical evidence that we provide is also of particular interest because it regards a very recent educational reform, in a setting where the average enrollment rate was quite high (about 0.8 in the pre-reform year).[5] Thus, the adolescents induced to comply with the new compulsory age limit were most likely the *marginal students* more at risk of committing crimes. In this respect, our analysis provides evidence of the implications of policies implemented in contexts where the initial enrollment rate is already high, and aimed at keeping at school the entire population of students.

For our baseline analysis, we use administrative records of all 14-, 15-, 16-, and 17-year-olds reported by the police to the judicial authorities, matched with enrollment rates in the corresponding grades of high school (grades from 9 to 12), at the provincial level. The crime data precisely identify the age of all the offenders and the province where the offense took place, and include all the youths reported to the judicial authority, regardless of whether they were eventually sent to prison or punished in other ways (e.g. in social service programs).

The Reform was implemented in 1999, and increased the minimum dropout age by one year, so that, afterwards, all adolescents attending the last year of junior high school (at age 14) had to enroll and attend the first year of high school (at age 15). The Reform introduced only one additional year of compulsory education, so that adolescents could still drop out after the completion of the first year in high school. The identification is achieved in a Difference-In-Differences (Diff-In-Diff) framework, by exploiting the staggered application of the Reform to different age groups: the 14-, 15-, 16-year-olds constitute the treated group and they are observed both before and after the Reform; the 17-year-olds are the control group, as they are never affected by the Reform in the time window considered in the empirical analysis. We estimate the reduced form effect of the Reform on both the enrollment rate and the offending rate, and we use the Reform as an instrument, in a 2SLS framework, to retrieve the causal effect of education on adolescent crime. Furthermore, we take advantage of quarterly aggregate data to examine potential displacement effects to times of the year when the school is not in session, and individual-level data from the victimization survey to investigate temporal (morning versus afternoon) and spatial (from street to school) displacement of criminal activity.

While the Reform increases high school enrollment of about 4 percentage points for all treated ages, it determines different effects on adolescent crime depending on the age group. One additional year of compulsory education reduces the offending rate of 14-year-olds (with

[5]For example, Machin et al. (2011) use a school leaving age reform which took place in England and Wales in 1972.

respect to 16-year-olds, the benchmark group) by almost 2 incidences per 1,000 of the corresponding age group population, which corresponds, in relative terms, to a 11.5 percent decrease of the average offending rate. Similar results hold for the 2SLS model: an increase in the enrollment rate by one percentage point determines a decrease in the 14-year-old offending rate of about 0.28 incidences, a 1.6 percent reduction with respect to the average offending rate. The results are robust to various specifications and robustness tests, as well as to a placebo exercise in which we pretend that the Reform happened in the years before its actual implementation. The crime reducing effect is mainly driven by native males, and seems to be consistent with an incapacitation mechanism, where the students more at risk of committing a crime are prevented to do so by the enforcement of the additional year of education.

The crime reducing effect that we find in the baseline analysis could also be compatible with a framework where the criminal activity is shifted to other time periods, or to other locations. In our complementary analysis of the displacement effects, we do not find evidence of temporal displacement of the criminal activity in periods of the year when the school is not in session or in the hours of the day when adolescents are typically out of school. If anything, we document a decrease of drug-related crimes during the summer, compared to the subsequent months when the school year starts, as well as an increase in the probability of being victim of violent crimes in the school facilities. These results seem to suggest that also the social interactions channel plays a role in our context, plausibly because of the increased juvenile concentration that may foster altercations with school mates.

The rest of the paper is organized as follows. Section 2 describes the institutional setting and the 1999 Compulsory Education Reform; Section 3 describes the data sources used and provides descriptive statistics, and Section 4 presents the identification strategy for the baseline analysis. In Section 5 we present the baseline results, conduct several robustness checks, and discuss the potential mechanisms. Section 6 investigates the displacement effects. Section 7 concludes.

2 Institutional setting

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). The primary and junior high schools are identical for all students. At the end of the junior high school (i.e. after completing 8 years of education) students obtain the *junior high school diploma*, which entitles them to enroll in high school. High school can last two or five years, according to the track chosen: academic high schools last for five years and mainly prepare for college; technical high schools also last for five years, while the vocational ones last for two years and provide students with the technical skills necessary to start a job. 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.

The Italian Constitution, which came into force in 1948, establishes that compulsory education in Italy lasts for eight years, until the completion of junior high school. In 1999, the Italian Government approved and implemented a reform that extended compulsory education by one year (from 8 to 9 years of schooling).[6] The Reform aimed at increasing high school attendance and the minimum school leaving age, which were relatively low compared to most European countries (Benadusi and Niceforo 2010). The additional year of education was to be carried out by attending lessons in a high school (any type), and could not be spent in regional training centers nor with apprenticeship contracts.[7] The first cohort affected by the Reform consisted of adolescents turning fourteen in 1999 and born in 1985, i.e., enrolled in grade 8 in the school year 1998/99. From this cohort onward, all students enrolled in grade 8 could not drop out after the completion of junior high school and were obliged to enroll in and attend at least the first year of high school. Adolescents were then allowed to drop out after the completion of this additional year of schooling (i.e. they were not formally obliged to attend the following grades - from 10 to 13 - in the high school). The Reform provides an exogenous change in the educational choices of adolescents that can be exploited for the estimation of the contemporaneous effect of education on crime.

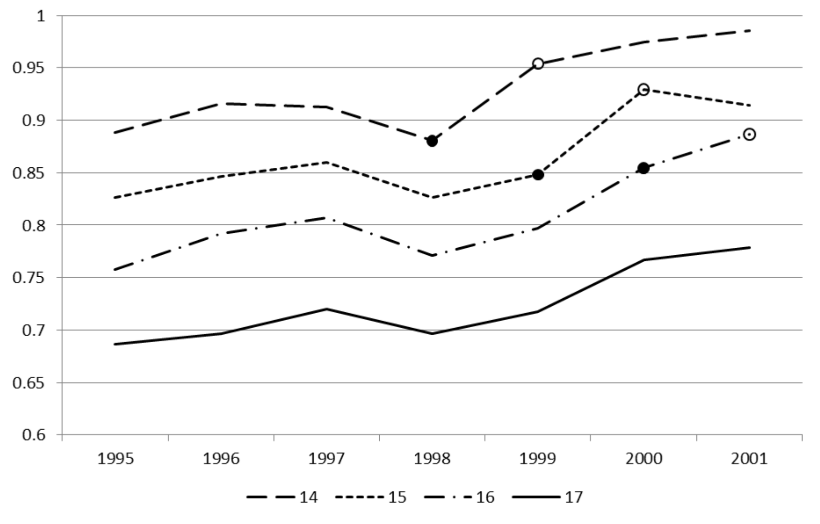
Figure 1 shows the trends in the enrollment rate of 14, 15, 16 and 17-year-olds in the years before and during the implementation of the Reform.[8] This figure also illustrates the basic intuition of our main difference-in-differences identification strategy. We consider 14-, 15- and 16-year-olds as the group of the treated units: the black dots indicate the school year just before the Reform affected each age group, and the circles the first school year of the Reform treatment. For all treated age groups the figure highlights the exogenous increase in the enrollment rate induced by the Reform in different school years. The Reform obliged the 14-year-olds to enroll and attend the first year of high school (grade 9). Indeed, the increase in the enrollment rate is more pronounced for them as compared to the 15- and 16-year-olds,

[6] Law No. 9/1999 and Ministry Decree No. 323/1999. Notice that ordinary laws, such as the Reform that we present here, can increase, but not decrease, the minimum compulsory education set by the Italian Constitutional Law.

[7] In accordance with the Reform, in the same year, the minimum legal age to start an apprenticeship contract increased from 14 to 15 (Law 345/1999).

[8] The enrollment rate (ER) is obtained as the share of students enrolled in a certain grade, net of those who dropped-out during the school year, with respect to the corresponding age group population.

Figure 1
Trends in the enrollment rate by age and school year



Notes: the graph shows the average enrollment rates of 14-,15-,16-, and 17-year-olds, by school year. The black dots indicate, for each age, the school year just before the Reform was implemented, while the circles the first school year when the Reform was implemented. On the horizontal axis the school years are labeled such that 1999 corresponds to the school year 1999/2000, and so forth. *Source*: own elaborations from MIUR.

who, however, also increase their attendance in the higher grades of the high school, meaning that a higher share of adolescents also decided to continue the high school path. The figure also shows the trend of the 17-year-olds, who are never affected by the Reform in the observed time window, and are considered the control group in our analysis.

The Reform was approved in January 1999, and came into effect in September 1999: as a consequence, there was no time to prepare the school system for the new intake of students (Berlinguer and Panara 2001). Figure A1 in the Appendix describes the main changes that occurred with the implementation of the Reform, by distinguishing by type of high school, and focusing on 9^{th} grade students. The largest increase in enrollment affected the vocational schools: the total number of students enrolled in this category the year after the implementation of the Reform is 15 percent larger than the corresponding figure in the year before. The vocational high schools also faced the highest increase in both students per teacher and students per school rates, confirming that the school system could not adjust to the new intake of students.

3 Data and descriptive statistics

In the empirical analysis we combine three main data sources: (i) yearly aggregate statistics from administrative records of school enrollments and adolescent crime, (ii) quarterly aggregate data on adolescent crime, and (iii) individual level data from victimization surveys.

3.1 Administrative data: yearly records on enrollments and offenders

We collected data on high school enrollments and on the number of offenders reported to the judicial authority, for 14-, 15-, 16- and 17-year-old adolescents in the years before and after the implementation of the Reform. The dataset is composed of cells defined by year (t), age ($a \in [14; 17]$), and province (p).[9] In the baseline analysis we consider a time span covering five school years (from 1997 to 2001), and eight cohorts of students (1980-1987).

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, which has the unique characteristic of offering aggregate administrative measures of the number of teenagers reported to the judicial authority for whom we know the exact age.[10] Our yearly measure of criminal activity (i.e. the *Offending Rate, OR*) is thus the number of adolescents reported by the police to the judicial authority ($offenders_{apt}$) over the corresponding age-year-province population (Pop_{apt}):[11]

$$OR_{apt} = \frac{offenders_{apt}}{Pop_{apt}} \times 1,000 \tag{1}$$

In order to measure the enrollment rate, we collect data on the number of students enrolled in high schools in each province for the school years from 1997/98 to 2001/02, from the Statistical Office of the Italian Ministry of Education (MIUR). We focus on 14 to 17-year-old students, enrolled, respectively, in grades 9 to 12 in high school. We define the *Enrollment Rate* (ER) as the number of adolescents who are enrolled in secondary education, net of those who drop-out during the school year ($enrollments_{apt}$), over the corresponding age-year-province population (Pop_{apt}):

[9] Since the number of provinces (NUTS 3) in Italy increased dramatically during the 90s, to maintain consistency of the data across years, we use the ISTAT definition of 95 provinces. Three regions (i.e. *Val d'Aosta*, *Trentino Alto Adige* and *Sicily*) are dropped from the analysis because, as Autonomous regions, they followed a different implementation of the Reform. The results of our analysis do not change if we exclude from the sample the provinces neighboring the excluded regions.

[10] ISTAT data on juvenile offenders are not available for adolescents aged 13 and below, because they cannot be formally sent to jail (Dipartimento di Giustizia Minorile 2007).

[11] Notice that this measure is an improvement over the other measures of criminal activities usually exploited in the literature, i.e. arrest rate and crime rate. In fact, the offending rate represents the proportion of adolescents for whom a legal action has been initiated, regardless of whether they are then condemned, arrested or imprisoned. As documented in the Appendix Table A1, the incarcerated youths (on whom the arrest rate is based) represent only a small part of the total number of youths prosecuted for a crime (on whom the offending rate is based): thus, the arrest rates may significantly underestimate the true magnitude of the phenomenon. On the other hand, measures like the crime rates, i.e. based on the number of crimes committed, would not be as accurate in the definition of the exact age of the offender (Jacob and Lefgren 2003).

$$ER_{apt} = \frac{enrollments_{apt}}{Pop_{apt}} \tag{2}$$

We then merge the two databases by year, age and province cells and link the resulting dataset to socio-economic information at the province level from different data sources, such as the Labor Force Survey (ISTAT), the Public Finance Database (Italian Ministry of the Interior) and the Italian Demographic Database (ISTAT).[12]

Panel A of Table 1 contains general descriptive statistics on the dependent and control variables used in the baseline analysis. The average enrollment rate is equal to 0.84, while the average offending rate is 17.08 adolescents reported to the judicial authority per 1,000 individuals of the corresponding age group population.

Table 1

Descriptive statistics: administrative aggregate data

	mean	sd	N
Panel A: yearly administrative records			
Offending rate (OR):			
All ages	17.08	10.62	1640
14-year-olds	10.42	6.05	410
15-year-olds	14.57	8.20	410
16-year-olds	20.01	9.93	410
17-year-olds	23.32	12.28	410
Enrollment rate (ER):			
All ages	0.84	0.12	1640
14-year-olds	0.94	0.07	410
15-year-olds	0.88	0.09	410
16-year-olds	0.82	0.10	410
17-year-olds	0.74	0.09	410
Control variables:			
Urban share	0.13	0.19	1640
Occupation rate (15-24)	32.99	10.30	1640
Provincial per capita VA	14,978.39	6,089.28	1640
Provincial population	618,668.18	666,762.46	1640
Panel B: quarterly administrative records			
Offenders count (OC):			
Theft	33.14	28.57	1360
Damage	5.84	5.52	1360
Assault and altercation	16.89	12.67	1360
Robbery	4.74	6.77	1360
Sexual offenses	1.27	1.80	1360

Notes: Panel A contains descriptive statistics of yearly, provincial by age level administrative records. 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. Panel B contains descriptive statistics of quarterly, regional by age level administrative records.
Source: ISTAT and MIUR.

[12] Since the ER measure refers to the academic year (from September until June) and the OR refers to the calendar year, we match the two in such a way to maximize the amount of time the measures overlap in each calendar year. For instance, the ER for the school year 1997/98 is linked to the OR in 1997. This matching ensures that the students enrolled in the school year 1997/98 have attended school not only from September until December, but also between January and June, because they have successfully completed the previous school year.

3.2 Administrative data: quarterly records on offenders

In order to analyze potential displacements of criminal activities in times of the year when the school is not in session, we obtain access to quarterly administrative records on adolescent offenders. These data are available for the same time window of the yearly administrative data, but have two main limitations, in order to comply with privacy protection policies: first, they are available for a limited number of offenses that do not overlap with the complete list available in the yearly records; second, offenses are aggregated at the regional level (NUTS 2).

The types of offenses available include property crimes (such as theft and damage), and violent crimes (such as assault and altercations, robbery, sexual offenses and drug-related crimes), which should account for a relevant share of the overall adolescent crimes (Dipartimento di Giustizia Minorile 2007). Given the above limitations, and the consequent high number of zeros, we express these dependent variables as the count of the offenders (OC^{j}_{arqt}) for each age ($a \in [14; 17]$), region (r), quarter (q), year (t), and type of offense (j):

$$OC^{j}_{arqt} = \sum offenders^{j}_{arqt} \tag{3}$$

3.3 Victimization survey

We exploit two waves of the Victimization Survey (VS) conducted on a representative sample of the Italian population by the National Institute of Statistics (ISTAT) in the years 1999 and 2008 (about 50,000 individuals interviewed in each wave). VS contains individual-level data on victimization, i.e. on whether the respondent or a family member has been victim of selected crimes in the 12 months preceding the interview.[13] They have the notable advantage of containing several individual level information about the victim (such as gender, education level, employment status, habits in the time spent outside the house, perceived level of control by the police forces in the neighborhood), and some information on the offender (when it is known to the victim), such as gender and an age range.

We select all the respondents who declared to be victim of a crime in the 12 months preceding the interview, and we express our dependent variable (VP_{ir}) as the probability for individual i living in region r of having being injured by a teenager. Since the survey provides the age of the offender in brackets, we define adolescents as offenders aged between 14 and

[13] VSs have been rarely exploited in the economics literature on crime. Two notable exceptions are Nunziata (2015), who uses the European Social Survey to show that victimization does not increase following large immigration waves, and Anderson et al. (2013), who exploit a VS on adolescents to test for crime displacement in the schools following an increase of the minimum drop-out age in the US.

20 (i.e., teenager). Thus, the dependent variable takes the following form:

$$VP_{ir}^{j} = Prob(\text{Injured by a teenager}|\text{Injured in the past 12 months}) \quad (4)$$

where r denotes the region where the offense takes place,[14] and j the type of crime. Given that the information concerning the age of the offender is self-reported by the respondent, we focus on crimes for which it is more likely that the victim has a good information about the offender, namely robbery and assault/altercations. As shown in Table 2, we conduct our analysis on 2,169 individuals victim of at least one robbery, assault or altercation in the 12 months preceding the interview: among them, 24 percent declared that the offender was under the age of 20.

Table 2

Descriptive statistics: victimization survey individual data

	mean	sd	N
Dependent variable:			
Victim of an adolescent (VP)	0.24	0.43	2169
Control variables:			
PostReform	0.48	0.50	2169
Female	0.40	0.49	2169
Age	35.09	15.99	2169
Non-native	0.01	0.10	2169
No education (illiterate)	0.01	0.12	2169
Primary school education	0.49	0.50	2169
Junior high or high school education	0.41	0.49	2169
University education	0.09	0.28	2169
Student	0.19	0.39	2169
Employed	0.49	0.50	2169
Not employed	0.32	0.47	2169
Going out to shop (No. of times, weekly)	2.14	1.38	2169
Going out at night (No. of times, weekly)	2.74	1.72	2169
Usage of public transportation (No. of times, weekly)	4.81	2.38	2169
Perceived control of the Police in the neighborhood	2.78	0.93	2169

Notes: immigrants are defined as individuals without Italian citizenship; individuals not employed also include those retired; the perceived control of the Police in the neighborhood is recorded in a scale between 1 (very low) and 4 (very high). Survey weights applied.
Source: ISTAT Victimization Survey.

4 Identification strategy

4.1 Diff-in-Diff specification

We estimate the causal effect of education on adolescent crime in a Difference-In-Differences (Diff-In-Diff) framework, by exploiting the staggered implementation of the Reform across ages and school years.[15] As illustrated in Section 2, the treated units in our analysis are 14-,

[14] Consistently with the analysis on the administrative data, we exclude the three aforementioned regions that had autonomy in the implementation of the Reform.

[15] The framework presented here refers to the baseline analysis of the contemporaneous effects of education on crime, using administrative yearly data. In later sections, we implement variations of the baseline identification strategy, using administrative quarterly data, and victimization survey data. The detailed specifications are

15- and 16-year-old adolescents, who are affected by the Reform (i.e. those born from 1985 to 1987) in different school years from 1999/2000 onwards. We consider a time window of five school years (from 1997/98 to 2001/02): over this time span the 17-year-olds are never affected by the Reform, and constitute the control group. We exploit this identification strategy to estimate the effect of the Reform on both the offending (OR_{apt}) and the enrollment rate (ER_{apt}). We thus estimate the following equations:

$$OR_{apt} = \beta_0 + \beta_1 DID_{at} + \beta_2 Treated_a + \beta_3 Post_{at} + \beta_4 X_{apt} + (1+\eta_j)\phi_t + \omega_p + \omega_a + t_a + \varsigma_{apt} \quad (5)$$

$$ER_{apt} = \delta_0 + \delta_1 DID_{at} + \delta_2 Treated_a + \delta_3 Post_{at} + \delta_4 X_{apt} + (1+\eta_j)\phi_t + \omega_p + \omega_a + t_a + \epsilon_{apt} \quad (6)$$

where a indicates adolescent age, p provinces and t years. The variable $Treated_a$ takes value 1 for 14-, 15- and 16-year-olds, and 0 for 17-year-olds; $Post_{at}$ is equal to 1 in the school years 1999/00, 2000/01 and 2001/02 for 14-year-olds, in the school years 2000/01 and 2002/01 for 15-year-olds, in the the school year 2002/01 for 16-year-olds, zero otherwise. The DID_{at} variable is then defined by the interaction $Treated_a \times Post_{at}$.[16]

In order to control for time invariant unobserved heterogeneity in provincial characteristics and for annual trends in adolescent crimes, the Diff-In-Diff baseline specification includes province (ω_p) and year (ϕ_t) fixed effects. We include age fixed effects (ω_a) and age-specific linear time trends (t_a), to control for the different propensity to commit crime across ages, and to account for time series variation specific to each age. Finally, we also add the interaction between year fixed effects (ϕ_t) and regional fixed effects (η_j), to capture relevant territorial dynamics that cannot be included in the vector of control variables.[17] We also control for several (time-variant) socio-economic characteristics X_{apt} that are potential determinants of education and juvenile crime. Among these, we include control variables varying across provinces and age groups, such as the share of youth living in urban areas, as well as indicators only varying at the province level, such as 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.

reported in Section 6.

[16] For a visual representation of the structure of the data and of the basic intuition behind the Diff-In-Diff identification strategy, see Appendix Table A2. Notice that our Diff-In-Diff framework is based on a cohort study. The model defined in equation 5 is equivalent to the following specification: $Y_{apt} = \alpha_0 + \alpha_1 Reform_{at} + \alpha_2 X_{apt} + (1+\eta_j)\phi_t + \omega_p + \omega_a + t_a + \varsigma_{apt}$, where the variable $Reform_{at}$ takes value 1 for the cohorts affected by the Reform (1985-1987) and 0 for those not affected (1980-1984).

[17] Italian regions (NUTS 2) correspond to a higher tier of territorial government as compared to Italian Provinces (NUTS 3).

Identification comes from two sources. First, we exploit across-ages and within-year variation in the treatment status, since in each year, after the introduction of the Reform, there are both treated and untreated adolescents according to their age. Second, we exploit within-age and across-years variation in the treatment status, since each age group is either treated or untreated depending on the year and on the cohort. The coefficients of the variable DID_{at} (β_1 for equation 5 - henceforth labeled $CrimeDID$ - and δ_1 for equation 6 - henceforth labeled $EduDID$) express the Difference-in-Differences parameters of interest. They can be interpreted as an intention-to-treat (ITT) providing the overall effect of raising compulsory education by one year, and representing the relevant policy parameters.[18] Furthermore, the coefficients from the $CrimeDID$ and the $EduDID$ should be interpreted in a setting where the initial enrollment rate is already high. In fact, the enrollment rate in high school before the implementation of the Reform ranges between 0.9 for 14-year-olds and 0.7 for 17-year-olds (see Figure 1). The different enrollment rate by age groups, as well as the design of the Reform, that, in practice, obliged students to enroll and attend only one additional year of high school at the age of 14, suggest that there might be differential effects across ages. For this reason, our baseline specification also allows for age-specific effects of the Reform.

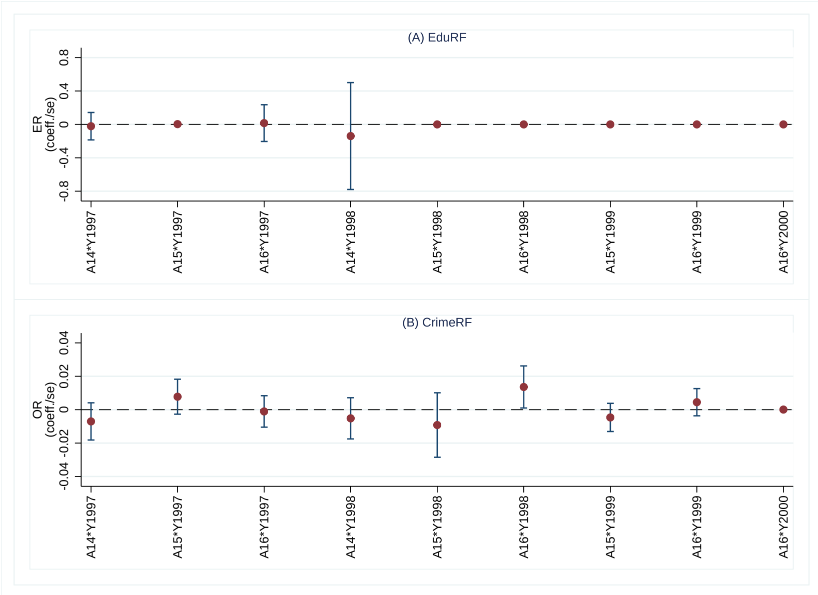
For these estimates to be causal, the assumption of parallel trends needs to be fulfilled. In order to test for this assumption, we estimate equations 5 and 6 by adding nine interaction terms defined interacting each age group indicator with each year in the pre-Reform period. In case the coefficients of such interaction terms were statistically significant, that would mean that, in the school years prior to the introduction of the Reform, the trends in enrollment and crime were not parallel across age groups, and that some of them already faced a peak before the implementation of the Reform. The coefficients of the interaction terms in both the $CrimeDID$ and the $EduDID$ are reported in Figure 2, and they are not statistically different from zero, thus confirming that the parallel trend assumption holds in our Diff-In-Diff estimation framework.

One critical empirical issue when using the Reform as a source of exogenous variation for education is that it was implemented with the same timing across all the country, without regional differences. Consequently, it is not possible for us to control for unobserved traits at the cohort level by comparing treated and untreated units.[19] Our main Diff-In-Diff identification strategy exploits 17-year-olds as a control group because those cohorts are never treated (see Figure 1). However, using 17-year-olds as the control group could be problematic

[18]Notice that the parameter β_1 in the $CrimeDID$ equation is the one usually estimated in the existing literature on the contemporaneous effects of school attendance on juvenile delinquency, since most studies lack specific or aggregate measures of school enrollment.

[19]We thank a referee for pointing out this aspect.

Figure 2
Regression test for the parallel trend assumption



Notes: the graphs show the estimated coefficients and the 99 percent confidence intervals from two separate regressions on enrollment and offending rate in the years before the implementation of the Reform ($N = 1148$). The regressions follow the baseline specifications of equation 6 (Panel A) and 5 (Panel B), and add nine interactions terms between each age group ($A14$, $A15$ and $A16$; $A17$ is the omitted category) and each school year (Y) in which the age group is not subject to the reform.
Source: own elaborations from ISTAT and MIUR.

if there are spill-over effects from criminal or schooling behavior of younger age groups onto the 17-year-olds. For example, having more marginal students of the affected age groups in the school could induce the 17-year-old students to engage more in crime, based on some social network effects. Or, 17-year-olds could be indirectly affected by the Reform through a lower quality in the education process due to an influx of many new students which absorb a larger share of school resources. In Appendix B we discuss this issue in greater detail. First, we show that the institutional features of the implementation of the Reform help to limit this concern. Indeed, aggregate statistics of school quality for the 17-year-olds do not show sizable changes when comparing the year before and after the implementation of the Reform (see Appendix Figure B1). Second, we provide a formal test of spill-over effects by regressing our measures of education or crime (i.e. the ER and OR) for the 17-year-olds on the same measures for the treated age groups (14, 15 and 16), and test whether there is a consistent pattern of correlations in the years when the Reform was implemented. Our results show that there is not a robust pattern of correlations between the crime nor the education measures, thus rejecting the hypothesis that spill-over effects might generate relevant bias in our estimated coefficients.

4.2 Instrumental variable estimation

As a further step in our baseline analysis, we identify the causal effect of education on juvenile crime by adopting a 2SLS estimation strategy. In the 2SLS framework, we estimate the effect of the enrollment rate on the offending rate, by exploiting the variable DID_{at} as an instrument, which captures the exogenous change in the enrollment as induced by the new compulsory education law.

For the 2SLS estimation, the main identifying assumption 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 significantly affects the adolescents' enrollment decisions, and (ii) it is not correlated with the unobservables that influence youth criminal behavior. The first point is confirmed by Figure 1, and also by the statistical significance of the instrument in the first stage regressions, reported in the following tables. The second point hinges on the assumption that the change in compulsory attendance laws was not aimed at reducing juvenile crime. To the best of our knowledge, the Reform aimed at increasing school leaving age in order to bring Italy in line with most European countries, and was not enacted in response to concerns about juvenile delinquency, youth unemployment or other crime-related factors (Benadusi and Niceforo 2010).[20] Indeed, the change in compulsory education was at the time envisioned as the first step in a comprehensive reform of the entire education system, which, in the end, was not approved by the Parliament.

5 Baseline results

5.1 The contemporaneous effect of education on adolescent crime

Table 3 reports the baseline results of our analysis, for which we use the yearly aggregate data on the enrollment and the offending rates. As mentioned in the previous section, we also extend the specifications of equations 6 and 5 by including a full set of interactions of the DID_{at} variable with age dummies. In this way, we aim at capturing any differential effect of the Reform on the 14-year-olds with respect to the 15- and 16-year-olds.[21] Panel A reports the Diff-in-Diff results on enrollment, Panel B reports the Diff-in-Diff results on the offending rate, and Panel C reports the results for the instrumental variable estimation, where the Reform acts as an instrument for enrollment.

[20] As the Minister of Education who proposed the Reform argued, '[...] the new compulsory education [...] has the purpose of increasing the culture and the level of knowledge of the country, to promote higher education, to increase the opportunities for everyone [...] '(Berlinguer and Panara 2001).

[21] The 16-year-olds are the omitted category in all specifications.

Focusing on the specifications that include the complete list of control variables and fixed effects, the $EduDID$ estimates (Panel A, column 2) show that the Reform effectively increases the enrollment rate by about 4 percentage points; the results from Panel A, column 3, where we add the interaction terms, show that the enrollment rate of 14-year-olds increases by an additional percentage point compared to the enrollment rate of 16-year-olds, after the introduction of the Reform; furthermore, we do not find any differences in the effect of the Reform on the enrollment rates between 15- and 16-year-olds.

Panel B of Table 3 reports instead the results for the crime reduced form, where the offending rate is the dependent variable. The results in column 2 show that, overall, the Reform did not affect significantly the offending rate of 14-, 15- and 16-year-olds, compared to 17-year-olds. However, we do find a crime reducing effect of the Reform on 14-year-olds (column 3, Panel B): the additional year of compulsory education reduces the offending rate of 14-year-olds (compared to the offending rate of 16-year-olds) by almost 2 incidences per 1,000 of the corresponding age group population, which corresponds to a 11.5 percent decrease of the average offending rate.

The results of the 2SLS model are shown in Panel C. Similarly to the reduced form results, we do not find any effects of the enrollment rate on the offending rate on average for all ages (column 2), but we do find an effect for 14-year-olds.[22] 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. The coefficient of the $ER \times Age14$ interaction is negative and statistically significant, implying that an increase in the enrollment rate by one percentage point determines a decrease in the 14-year-old offending rate of about 0.28 incidences per 1,000 of the corresponding age group population (as compared to 16-year-olds), which corresponds to a 1.6 percent reduction with respect to the average offending rate. To the best of our knowledge, this is the first causal estimate (i.e., in a 2SLS setting) of the contemporaneous effect of education on crime for young adolescents (14- to 17-year-olds). Finally, notice that the reduced form coefficient is slightly larger (in absolute value) than the corresponding 2SLS estimate, since it may incorporate any indirect or spillover effects of the Reform.[23]

The magnitude of the crime reducing effect that we estimate is in line with the other

[22] Notice that, in this case, for the estimation with the age dummies interactions, we have three endogenous variables (ER, $ER \times Age14$ and $ER \times Age15$) and three instrumental variables (DID, $DID \times Age14$ and $DID \times Age15$).

[23] The total effect of the Reform is given by -0.2815×5=-1.41, by multiplying the 2SLS parameter and the increase in the enrollment rate for the 14-year-olds as obtained from the $EduDID$ estimation results (5 percentage points). The $CrimeDID$ result indicates a reduced form effect of the Reform on the offending rate equal to -1.97.

Table 3
Contemporaneous effects of education on adolescent crime: baseline results

	(1)	(2)	(3)
Panel A: Edu DID			
DID	0.14***	0.04***	0.04***
	(0.01)	(0.00)	(0.01)
DID × age 14			0.01**
			(0.01)
DID × age 15			-0.00
			(0.00)
Panel B: Crime DID			
DID	-6.08***	-0.18	0.20
	(0.53)	(0.47)	(0.53)
DID × age 14			-1.97***
			(0.60)
DID × age 15			-0.22
			(0.54)
Panel C: 2SLS			
ER	-53.76***	-4.78	-0.20
	(3.96)	(11.56)	(12.32)
ER × age 14			-28.15***
			(9.75)
ER × age 15			-4.44
			(6.82)
First stage F-stat.	739.11	69.22	32.24
N.Observations	1640	1640	1640
Control variables	✓	✓	✓
Fixed effects		✓	✓

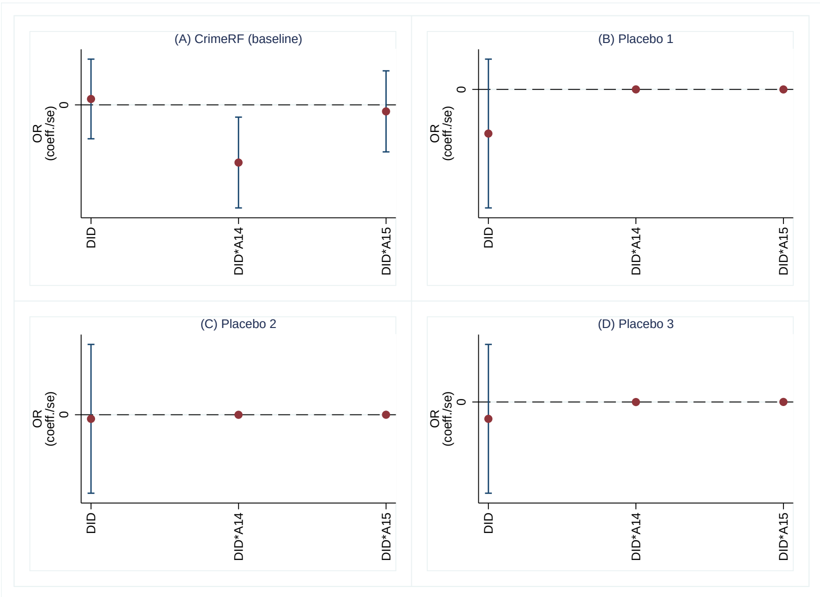
Notes: the dependent variable of the Diff-In-Diff crime reduced form (*CrimeDID*, Panel A) and 2SLS (Panel B) models is the offending rate (*OR*) of 14-,15-,16, and 17-year-olds; the dependent variable of the Diff-In-Diff education reduced form model (*EduDID*, Panel A) is the enrollment rate (*ER*) of 14-,15-,16, and 17-year-olds. The 16-year-olds are the omitted category in all specifications. 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 per capita VA)*), the logarithm of provincial population (*ln(provincial population)*). The fixed effects (FE) include province, age and year FE, region by year FE, and linear age trends. 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.
Source: own elaborations from ISTAT and MIUR.

studies which focus on the contemporaneous effects of education on youth crime and use identification strategies or sources of variability comparable to ours. In particular, Anderson (2014) finds that the exposure to a minimum drop-out age (MDA) of 18 reduces the arrest rate for 16- to 18-year-olds by 17.2 percent (relative to the mean arrest rates of states with MDA equal to 17 or 16). Åslund et al. (2015) find that offering a three-year vocational track determines a decrease in property crime of about 18 percent.

5.2 Robustness and specification tests

We first check the robustness of our baseline results, in the $CrimeDID$ estimation, by performing a placebo exercise, in which we assume that the Reform took place in prior school years. For this purpose, we use data on the offending rates that were made available for some years before the implementation of the Reform and we exclude adolescents who were really affected by the Reform (i.e. those born in 1985 and afterward). We perform three different placebo regressions exploiting different samples based on five-years time windows (as in the baseline specification).[24] Given that for those years the control variables are not available, we only include the complete set of fixed effects and, for comparison purpose, we replicate the *CrimeDID* specification without including the set of the control variables. The results are reported in Figure 3 and show that the coefficients of the placebo regressions are never statistically different from zero.

Figure 3
Placebo exercises



Notes: the graphs report the estimated coefficients and the 99 percent confidence intervals of three separate placebo regressions. The placebo regressions follow the baseline specifications expressed in equation 5 and are performed on three different time windows of five years, prior to the implementation of the Reform (N = 1640 in each regression). Panel A shows the results form the *CrimeRF* without including the set of control variables; *Placebo 1* (Panel B) is performed on the years 1992-1996, and the cohorts after 1980 are considered as treated by the placebo reform; *Placebo 2* (Panel C) is performed on the years 1993-1997, and the cohorts after 1981 are considered as treated by the placebo reform; *Placebo 3* (Panel D) is performed on the years 1994-1998, and the cohorts after 1982 are considered as treated by the placebo reform.
Source: own elaborations from ISTAT and MIUR.

In Table 4 we conduct several robustness and specification tests. In the specifications in columns (1) and (2) we alter the baseline estimation sample by restricting or increasing the time window. Specifications in columns (3), (4) and (5) modify the baseline set of fixed effects including, respectively, region by age fixed effects, region and province specific linear trends. Finally, specification in column (6) expresses the offending rate with its natural logarithm (replacing a zero when the log transformation is not defined). The results appear

[24]The first placebo regression (*Placebo 1*) is performed on the years 1992-1996, and the cohorts after 1980 are considered as treated by the placebo reform; the second placebo regression (*Placebo 2*) is performed on the years 1993-1997, and the cohorts after 1981 are considered as treated by the placebo reform; the third placebo regression (*Placebo 3*) is performed on the years 1994-1998, and the cohorts after 1982 are considered as treated by the placebo reform.

Table 4
Sensitivity checks

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Edu DID						
DID	0.04***	0.04***	0.04***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
DID × age 14	0.01**	0.02**	0.01**	0.01**	0.01**	0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
DID × age 15	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Crime DID						
DID	0.17	0.21	0.20	0.20	0.20	0.02
	(0.57)	(0.51)	(0.54)	(0.53)	(0.53)	(0.03)
DID × age 14	-2.01***	-1.11*	-1.97***	-1.97***	-1.97***	-0.17***
	(0.70)	(0.58)	(0.61)	(0.60)	(0.60)	(0.05)
DID × age 15	-0.14	0.17	-0.22	-0.22	-0.22	-0.04
	(0.57)	(0.55)	(0.55)	(0.54)	(0.54)	(0.04)
Panel C: 2SLS DID						
ER	0.20	1.96	-0.19	-0.20	-0.20	0.13
	(13.29)	(12.45)	(12.31)	(12.32)	(12.32)	(0.64)
ER × age 14	-27.35**	-15.83*	-28.15***	-28.15***	-28.15***	-2.41***
	(11.24)	(8.89)	(9.75)	(9.75)	(9.75)	(0.71)
ER × age 15	-5.15	2.40	-4.44	-4.44	-4.44	-0.59
	(7.43)	(6.85)	(6.83)	(6.82)	(6.82)	(0.48)
First stage F-stat.	34.66	29.77	28.87	32.24	32.22	32.22
N.Observations	1312	1968	1640	1640	1640	1640
Control variables and fixed effects	✓	✓	✓	✓	✓	✓
Specifications:						
4-years time window	✓					
6-years time window		✓				
Region by age fixed effects			✓			
Region linear trends				✓		
Province linear trends					✓	
Log of the offending rate						✓

Notes: the dependent variable of the Diff-In-Diff crime reduced form (*CrimeDID*, Panel A) and 2SLS (Panel C) models is the offending rate (OR) of 14-,15-,16, and 17-year-olds ; the dependent variable of the Diff-In-Diff education reduced form model (*EduDID*, Panel B) is the enrollment rate (ER) of 14-,15-,16, and 17-year-olds. For the list of the control variables and fixed effects included in all the specifications see Table 3. Specification in column (1) restricts the time window to the minimum (four school years, from 1998/99 to 2001/02); specification in column (2) increases the time window by one year (six school years, from 1996/97 to 2001/02); specification in column (3) includes region by age fixed effects; specifications in columns (4) and (5) include, respectively, region- and province-specific linear trends; specification in column (6) estimates the *CrimeDID* and 2SLS using the natural logarithm of the offending rate. 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.
Source: own elaborations from ISTAT and MIUR.

remarkably stable across the different specifications, thus confirming the goodness of our baseline estimates.

5.3 Heterogeneous effects by gender and immigrant status

Table 5 reports the results of the Diff-In-Diff crime reduced form analysis by distinguishing between male and female, and native and immigrant offenders.[25] While it is generally established that males, both when adolescents and adults, are usually those who engage more frequently in criminal activities (also in our data the offending rate of males is about four times those of females), previous findings in the literature show that adult male offenders are also more affected than female by exogenous changes in the time spent at school (see, among others, Hjalmarsson et al. (2015)). In line with previous findings in the literature, Panel A in Table 5 documents that the crime reducing effect of education on 14-year-olds comes exclusively from male adolescents.

While gender differences in criminal behavior are well established in the literature, the potential different effects of education on teenage immigrants crime have been less investigated. Studies from social sciences generally point out that first generation immigrants[26] are less incline to commit crime than natives, as usually documented by lower crime rates (Chen and Zhong 2013). There could be many behavioral reasons behind that, but the main argument is that immigrant youths, who have not yet assimilated to the youth subculture of the host society, are more law-abiding due to the innate characteristics from their traditional traits (i.e., being more realistic, stronger ties with family/schools, less access to delinquent friends). This cultural view also explains why second generation immigrants tend to commit crimes more than their first generation counterparts. In the years under study, the vast majority of adolescent immigrants in Italy were of first generation (Mencarini and Dalla Zuanna 2009).

Given that our data allows us to distinguish between native and immigrant adolescent offenders, in Panel B and C of Table 5 we repeat the baseline Diff-In-Diff specification on the two groups, separately. The results show that the effect on 14-year-olds comes exclusively from natives, while immigrants' crime do not seem influenced by the Reform. While this result seems to support the above-mentioned hypothesis, a caveat should be borne in mind. In fact, the effects could not be detected because of the small share of immigrants enrolled in secondary schools in those years (about 1 per cent of the overall student population).[27]

[25] Additional descriptive statistics on the offending rate by gender and immigrant status can be found in the Appendix Table A3.

[26] In line with the immigration literature, we define as first generation immigrants those born abroad; while second generations are born in the host country from parents born abroad.

[27] Unfortunately, our data on enrollment do not allow us to distinguish between the enrollment rate of natives and the one of immigrants, and neither to test the role played by the Reform for the immigrants' decisions to

Thus, our results, albeit informative, cannot be considered as conclusive under this aspect. More research is needed, especially for contexts where immigrant students represent a more relevant share of the student population.

Table 5
Heterogeneous effects by gender and immigrant status

	(1)	(2)	(3)
	Males and females	*Males*	*Females*
Panel A: natives and immigrants			
DID	0.20	0.69	-0.10
	(0.53)	(0.93)	(0.49)
DID × age 14	-1.97***	-2.99**	-0.95
	(0.60)	(1.15)	(0.70)
DID × age 15	-0.22	-0.95	0.48
	(0.54)	(1.09)	(0.49)
Panel B: natives			
DID	0.09	0.44	-0.08
	(0.46)	(0.85)	(0.31)
DID × age 14	-1.71***	-2.71***	-0.70
	(0.50)	(0.91)	(0.51)
DID × age 15	-0.23	-0.74	0.23
	(0.44)	(0.84)	(0.34)
Panel C: immigrants			
DID	0.12	0.26	-0.01
	(0.15)	(0.23)	(0.22)
DID × age 14	-0.26	-0.28	-0.25
	(0.25)	(0.48)	(0.24)
DID × age 15	0.01	-0.21	0.24
	(0.20)	(0.43)	(0.20)
N.Observations	1640	1640	1640
Control variables and fixed effects	✓	✓	✓

Notes: the dependent variable is the offending rate (OR) of 14-,15-,16, and 17-year-olds of both natives and immigrants (Panel A), natives only (Panel B), and immigrants only (Panel C). Immigrant adolescents are defined as those without an Italian citizenship. Robust standard errors in parenthesis, clustered at the province level. For the list of the control variables and fixed effects included in all the specifications see Table 3. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels.
Source: own elaborations from ISTAT and MIUR.

5.4 Potential mechanisms: evidence from aggregate data

The contemporaneous relationship between education and adolescent crime is likely to be driven by three main mechanisms: incapacitation, human capital accumulation and social interactions (Lochner 2011). The incapacitation effect is due to the fact that adolescents are forced to stay at school, where they may have fewer opportunities to commit crimes than on the street. However, the incapacitation of students at school can also increase their concentration, which may lead to more occurrences of violent crimes, such as altercations. The existing studies on the contemporaneous effect of education on crime have mainly stressed the importance of the incapacitation and the concentration channels, the latter being directly

enroll in high school. At the time of the Reform, immigrants students in Italy represented around 1.1 percent of the overall student population, while nowadays they represent a more sizable share of the overall student population, ranging between 6 and 13 percent, depending on the grade and school track (MIUR - Ufficio di Statistica 2014; Tonello 2016).

linked with a social interaction mechanism (Jacob and Lefgren 2003; Luallen 2006). However, in the context of compulsory schooling laws that require students to stay at school at least one additional year, the human capital channel may also play a role (Lochner 2011). The mechanism behind the human capital channel relies on the idea that longer periods of school attendance may presumably change the adolescents' expectations about their future prospects in the labor market, and make criminal activities and juvenile detention more costly.

While using aggregate data we cannot evaluate whether the social interaction channel play any role in our results, having at disposal both education and crime measures helps us to shed lights on the other two channels, i.e. human capital and incapacitation. Indeed, by comparing the results of the *EduDID* and the *CrimeDID* specifications, it appears that the enrollment rate of all 14-, 15- and 16-year-olds increase (on average, by 4 percentage points), while the crime reducing effect is concentrated among the 14-year-olds only and disappears at older ages. This pattern seems to rule out the human capital channel, at least in this short-run time horizon.[28] Since the Reform only required students to enroll and attend one additional year of education, it is plausible to think that the students who continue high school were probably better selected, and more motivated than those who drop out after complying with the new regulation. If a human capital effect played a role, even at such young ages, we would expect also the offending rates of 15- and 16-year-olds affected by the Reform to decrease. Given that this is not the case, and no effect is detected for ages that did not have to comply with a formal obligation to attend school, the incapacitation mechanism seems to be the main driver of our results with the aggregate data.

6 Displacement effects

The baseline results can also be consistent with a framework where the Reform did not affect the level of criminal activity, but only shifted its time of occurrence, for any age group. For instance, adolescents may displace crime in times of the year or of the day when they are not obliged to go to school. Alternatively, crimes can also increase during school time and at the school premises, plausibly fostered by social interactions and by the increase in juvenile concentration (Jacob and Lefgren 2003). Both these scenarios could not be captured by our baseline analysis with yearly aggregate data. In this section, we analyze whether temporal displacement occurs in our setting, by using quarterly administrative data on adolescent offenders and individual-level data from the victimization survey. The latter data source is

[28] Due to lack of data on criminal activity beyond age 20, we cannot evaluate whether a human capital channel operates in the long run.

also used to analyze whether spatial displacement takes place, after the introduction of the Reform, and whether the probability of being victim of a crime at school changes.

6.1 Temporal displacement when the school is not in session

In order to test whether, following the implementation of the Reform, adolescents displace their criminal activity to times of the year when the school is not in session, we exploit the quarterly administrative records on the number of adolescent offenders (OC^j_{arqt}) and estimate the following model:

$$\begin{aligned} OC^j_{arqt} = {} & \gamma_0 + \gamma_1 DID_{at} + \gamma_2 Treated_a + \gamma_3 Post_{at} + \gamma_4 X_{apt} \\ & + \gamma_5 DID_{at} \times Age14_a + \gamma_6 DID_{at} \times Age15_a + \gamma_7 Summer_q + \gamma_8 Spring_q \\ & + \gamma_9 DID_{at} \times Summer_q + \gamma_{10} DID_{at} \times Spring_q \\ & + \gamma_{11} DID_{at} \times Summer_q \times Age14_a + \gamma_{12} DID_{at} \times Spring_q \times Age14_a \\ & + \gamma_{13} DID_{at} \times Summer_q \times Age15_a + \gamma_{14} DID_{at} \times Spring_q \times Age15_a \\ & + \phi_r + \phi_t + \phi_a + t_a + t_r + \phi_q + \varsigma_{apt} \end{aligned} \tag{7}$$

where OC^j_{arqt} indicates the offenders count of adolescents of age $a \in [14; 17]$, in region r, in quarter q, and year t; j indexes the offense type (theft, damage, assault and altercations, robbery, sexual offenses and drug-related crimes). The variables DID_{at}, $Treated_a$ and $Post_{at}$ are defined as in the baseline analysis. The variable $Summer_q$ is equal to 1 for quarter 3 (July-September), and indicates the time of the year when the school is not in session.[29] Since the school year starts in September, the months of the year when the school is not in session ($Summer_q = 0$) refer to different grades, also for adolescents of the same age. For this reason, in our specification, we also distinguish between the first two quarters of the year ($Spring_q$, for the months January-June) and the last quarter ($Fall$, for the months October-December). Equation 7 includes region (ϕ_r), year (ϕ_t), quarter (ϕ_q) and age (ϕ_a) fixed effects to capture any region-, year-, quarter- or age-specific unobserved heterogeneity.

The variables of interests are the interactions between DID_{at} and $Summer_q$ or $Spring_q$, respectively, and their interactions with the age dummies, which capture any shifting effects of the Reform across quarters.[30] The identification of the coefficients of interests ($\gamma_9, \ldots, \gamma_{14}$) comes from the additional variations in criminal activity induced by Reform across quarters

[29] In Italy, summer holidays for students in all grades start in mid-June and end between the mid- and end-September (depending on the grade and on the region). In these months, schools of all grades (excluding universities) are closed.

[30] In the specification, the fourth quarter $Fall$ is the reference category for the time indicators, while 16-year-olds are the reference category for the age groups.

and within each year and age, and within quarter across cohorts. Positive coefficients of these interactions may capture any displacement effect induced by the Reform, as long as adolescents anticipate the lack of opportunity to commit criminal activities once the school year starts, i.e. in the Fall, and find it easier to engage in criminal activities either during the previous school year/grade, or during the summer. We might also expect heterogeneous effects depending on the types of offenses: property crimes could be more easily displaced to periods when the school is not in session; on the contrary, violent crimes might also decrease when the school is not in session following a decrease in juvenile concentration. In the latter case, the coefficients of the variables of interests would be negative.

Table 6

Adolescent crime displacement when the school is not in session

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Theft		*Damage*		*Assault/altercation*		*Robbery*		*Sex-related*		*Drug-related*	
DID	0.03	0.03	0.05	0.18*	-0.04	-0.08	0.16*	0.23**	-0.05	0.04	-0.01	-0.12
	(0.04)	(0.05)	(0.08)	(0.11)	(0.05)	(0.07)	(0.09)	(0.11)	(0.17)	(0.24)	(0.06)	(0.08)
DID× age 14	-0.02	-0.02	0.11	-0.07	-0.03	0.04	-0.14	-0.30*	-0.16	-0.29	-0.14	0.16
	(0.04)	(0.06)	(0.10)	(0.15)	(0.06)	(0.09)	(0.12)	(0.17)	(0.22)	(0.31)	(0.09)	(0.12)
DID× age 15	0.07**	0.07	0.07	-0.08	0.02	0.07	-0.09	-0.13	-0.08	-0.18	-0.10	0.00
	(0.04)	(0.06)	(0.08)	(0.13)	(0.05)	(0.08)	(0.09)	(0.14)	(0.18)	(0.30)	(0.06)	(0.10)
Spring	0.04***	0.04***	0.16***	0.16***	0.11***	0.11***	0.03	0.03	0.20**	0.20**	-0.07***	-0.07***
	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.08)	(0.08)	(0.02)	(0.02)
Summer	0.12***	0.12***	-0.07*	-0.07*	0.22***	0.22***	-0.03	-0.03	0.17**	0.17**	0.05**	0.05**
	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.08)	(0.08)	(0.02)	(0.02)
DID× Spring	-0.03	-0.06	0.04	-0.16	0.07*	0.14*	-0.04	-0.18	0.07	-0.10	0.01	0.14*
	(0.03)	(0.05)	(0.06)	(0.12)	(0.04)	(0.07)	(0.07)	(0.12)	(0.13)	(0.27)	(0.05)	(0.08)
DID× Summer	-0.09***	-0.03	0.08	-0.02	0.01	0.04	-0.13	-0.11	-0.02	-0.04	-0.07	0.13
	(0.03)	(0.06)	(0.08)	(0.14)	(0.04)	(0.08)	(0.09)	(0.14)	(0.15)	(0.30)	(0.06)	(0.09)
DID× Spring× age 14		0.05		0.28*		-0.07		0.24		0.20		-0.32***
		(0.06)		(0.14)		(0.09)		(0.16)		(0.30)		(0.12)
DID× Summer× age 14		-0.09		0.13		-0.09		0.11		0.10		-0.56***
		(0.07)		(0.17)		(0.10)		(0.19)		(0.34)		(0.15)
DID× Spring × age 15		0.02		0.22		-0.10		0.16		0.22		-0.12
		(0.07)		(0.15)		(0.09)		(0.15)		(0.33)		(0.11)
DID× Summer × age 15		-0.05		0.14		-0.00		-0.19		-0.07		-0.16
		(0.07)		(0.17)		(0.10)		(0.19)		(0.37)		(0.13)
Fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N.Observations	1360	1360	1360	1360	1360	1360	1360	1360	1360	1360	1360	1360

Notes: the dependent variable is the offenders count (OC_{aqrt}) of 14-,15-,16, and 17-year-olds adolescents (a), in each quarter (q) and region (r), for selected offenses. The variable *Summer* takes value 1 when the school is not in session ($q = 3$), and zero otherwise; the variable *Spring* takes value 1 for the months January-June ($q = 1, 2$), and zero otherwise. Estimates are performed using Poisson fixed effects regressions. The list of fixed effects includes age, region, quarter and year fixed effects. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels.
Source: own elaborations from ISTAT.

Given the nature of the data generating process, and the high number of zeros (from 8 to 35 percent depending on the offense type), we estimate equation 7 with a Poisson fixed effect estimator (Osgood 2000).[31] Table 6 reports the results, for the models with and without the interactions with the age dummies. Overall, there is no evidence of temporal displacement to periods when the school is not in session. If anything, we find support for a temporal displacement occurring from the Fall to the Spring, especially for damages for 14-year-olds.

[31] The results, albeit less precisely estimated, do not change using an OLS specification or alternative count-data models (e.g. negative binomial).

The negative coefficients of the triple interactions $DID \times Summer$ and $DID \times Spring$ for 14-year-olds' drug-related crimes seem instead to suggest that the Reform displaces the drug-related criminal activities to the last quarter of the year, when the school is in session, and the next grade starts. This effect can plausibly be driven by the increase in juvenile concentration at the school facilities: in fact the last quarter of the year for 14-year-olds corresponds to the beginning of the first grade in high school, which is the one directly targeted by the Reform, for which the increase in youth concentration has been more pronounced. However, the effect does not show up for the other violent offenses (assaults and altercations and sexual offenses). Similarly to the baseline analysis, we do not find any effects for 15-year-old adolescents.

6.2 Displacement in the afternoon and in the school facilities

We exploit individual level data from the VS to estimate whether the Reform induced some changes in the probability of being injured by a teenager along two main dimensions of displacement: first, a temporal displacement from the hours when the school is in session to the moments of the day after the school (typically, the late afternoon and the evening); second, the displacement of minor criminal activities to the school facilities. The first type of temporal displacement might be driven either by side effects of the school incapacitation, or by coordination effects in the organization of criminal activities to realize after school. Such a short-term displacement can also be more easily planned and manipulated than the temporal displacement discussed in the previous paragraph. The second type of displacement is more likely due to the increase of juvenile concentration in the school facilities.

While VS have the notable advantage of making available several information about the victim, the offender, the time and the place of the offense, as compared to the administrative data used so far, they have the drawback of not recording the exact age of the offender, which is provided in brackets. For this reason, we cannot implement the same Diff-in-Diff framework as in the previous sections and rather resort to *before and after* estimates. We thus use the two VS waves conducted in 1999 and 2008, define as adolescents the offenders aged between 14 and 20, and consider them as affected by the Reform in case they are born after 1985. The adolescents offenders reported in the first wave, i.e., aged 14-20 in 1999, are necessarily not treated, while the adolescents reported in the second wave, i.e., aged 14-20 in 2008, are necessarily treated. Thus the estimating equation becomes:

$$VP^{j}_{irt} = \lambda_0 + \lambda_1 Treated_{it} + \lambda_2 X_{ir} + \phi_r + \phi_j + \epsilon_{irt} \tag{8}$$

where the variable $Treated_{it}$ takes value 0 for the 1999 wave and 1 for the 2008 wave. The victimization probability VP_{irt} has been defined in section 3. We include region fixed effects (ϕ_r) to control for territorial unobserved heterogeneity, and type of offense fixed effects (ϕ_j) to control for any systematic difference in the victimization probability due to the type of offense. The vector X_{ir} includes individual level characteristics of the victim (such as gender, education level, employment status), and list of potential determinants of being victim of a teenager, such as the habits in the time spent outside the house and the frequency of public transportation usage. The parameter of interests λ_1 is obtained by comparing the victimization probability before and after the implementation of the Reform. As descriptive in nature, these results should be interpreted with adequate caution.

Table 7
Adolescent crime displacement after the school time and in the school facilities

	(1)	(2)	(3)	(4)	(5)	(6)
	Any offense	*Robbery*	*Assault and altercations*		*Any offense*	
Treated	-0.12***	-0.11***	-0.17***	-0.04	-0.05**	-0.13***
	(0.02)	(0.04)	(0.03)	(0.04)	(0.02)	(0.02)
Treated × AfterSchool				0.03		
				(0.06)		
AfterSchool				-0.01		
				(0.03)		
Treated × AtSchool					0.72***	
					(0.14)	
AtSchool					0.05	
					(0.11)	
Treated × Peer						0.44**
						(0.17)
Peer						0.34**
						(0.13)
Individual controls	✓	✓	✓	✓	✓	✓
Region fixed effects	✓	✓	✓	✓	✓	✓
Type of offense fixed effects	✓			✓	✓	✓
N. Observations	2169	1423	746	1393	1428	2169

Notes: the dependent variable is the victimization probability (VP_{ir}). The complete list of the control variables included is shown in the Appendix Table A4. *Treated* takes value 1 when the offenders have been subject to the Reform, and zero otherwise; *AfterSchool* takes value 1 if the offense took place between 3 PM and 9 PM, and zero otherwise; *AtSchool* takes value 1 if the offense took place at school; *Peer* takes value 1 if the offender is a school mate. Survey weights applied. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels.
Source: own elaborations from ISTAT Victimization Survey.

Table 7 reports the results. We find that the victimization probability decreases for the cohorts of offenders affected by the Reform by 12 percentage points (Table 7, column 1); the effect is similar across the types of offenses analyzed (robberies -11 percentage points and assaults and altercations -17 percentage points).[32] To test for the displacement effects in the afternoon, we interact the variable $Treated_{it}$ with the variable $AfterSchool_{it}$ which takes value 1 if the offense took place in the hours following the usual school daily schedule (i.e. from 3 to 9 PM). The results in column 3 do not show any increase in the victimization probability in this time span. The same results holds if we consider separately the offenses occurred between 3 and 5 PM and those occurred between 5 and 9 PM (see Appendix Table

[32]The extended results can be found in the Appendix Table A4.

A4).

In order to capture displacement effects of adolescent crime in the school facilities, we interact the variable $Treated_{it}$ with the variable $AtSchool_{it}$ which takes value 1 for all offenses occurred at school, and with the variable $Peer_{it}$, which takes value 1 if the offender is a school mate. Column 5 of Table 7 highlights an increase in the victimization probability for offenses taking place at school by 72 percentage points, while column 6 reports an increase in the victimization probability by 44 percentage points for the offenses perpetrated by a school mate. These results are in line with Anderson et al. (2013), who find that higher minimum dropout ages increase the likelihood that students report missing school for fear of their safety or for being threatened or injured on school premises.

Overall, both with quarterly administrative data and with VS, we do not find evidence of temporal displacement of the criminal activity in periods of the year when the school is not in session or in the hours of the day when adolescents are typically out of school. Conversely, we find evidence with both data sources that certain offenses are displaced to periods of the year when the school is in session, and in the school facilities, plausibly because of increased juvenile concentration and social interactions with school mates.

7 Concluding remarks

This paper estimates the contemporaneous effects of education on adolescent crime, by exploiting the implementation of a Reform that increases compulsory education by one year in Italy, and by using aggregate administrative data as well as individual level data from victimization survey.

We find that the Reform, while increasing the enrollment rate for all age groups, induces a reduction in the adolescent offending rate only for 14-year-olds, who are directly targeted by the compulsory education law. One additional year of compulsory education reduces the offending rate of 14-year-olds by almost 2 incidences per 1,000 of the corresponding age group population. In the 2SLS model, an increase in the enrollment rate by one percentage point determines a decrease in the 14-year-old offending rate by about 1.6 percent with respect to the average offending rate. In line with previous findings in the literature on adults, the crime reducing effect is mainly driven by native males. This first set of results is consistent with an incapacitation channel as the main mechanism explaining the drop in the 14-year-olds offending rate, while human capital accumulation does not seem to matter in such a short period of time.

While other studies have documented a long-lasting effect of increased education on adult delinquency (Hjalmarsson et al. 2015; Machin et al. 2011), our work considers only the contemporaneous effects of education on crime.[33] Thus, on the one hand, our estimates can be considered as a lower bound of the overall life-long effect of education on crime. On the other hand, given that the largest effects found in the literature are concentrated on the teenagers because of the incapacitation mechanism (Åslund et al. 2015), we can be confident that the crime reducing effect in adulthood would be lower than the contemporaneous one that we estimate.

In order to analyze the potential displacement effects of the increase in compulsory education on adolescent crime, we exploit quarterly data on selected adolescents' offenses. We do not find evidence of a shift in the criminal activity to times of the year when the school is not in session. If anything, we observe a decline in drug-related crimes in the summer with respect to the months of the first grade in high school, that is when the additional compulsory attendance starts, after the introduction of the Reform. Results from the analysis on the victimization survey, albeit more descriptive in nature, support these findings, as long as we do not detect any displacement to the hours of the day when the school is not in session, but we find evidence that, after the Reform, the probability of being victim of a violent crime in the school facilities increases.

These results can be consistent with a social interactions channel fostered by an increase in juvenile concentration in the school facilities. In fact, after the approval of the Reform, the school system could not be improved, either by building new schools, or by hiring new teachers, in order to deal with the new intake of students. A thorough implementation of compulsory education reforms, accompanied by larger infrastructure and personnel adjustments, might help to counter these side effects.

[33] Unfortunately, we cannot repeat our empirical exercise on the offending rates of the same cohorts at older ages because the information was not collected by ISTAT in those years (i.e. only offending rates for population aged *18 or more* are available).

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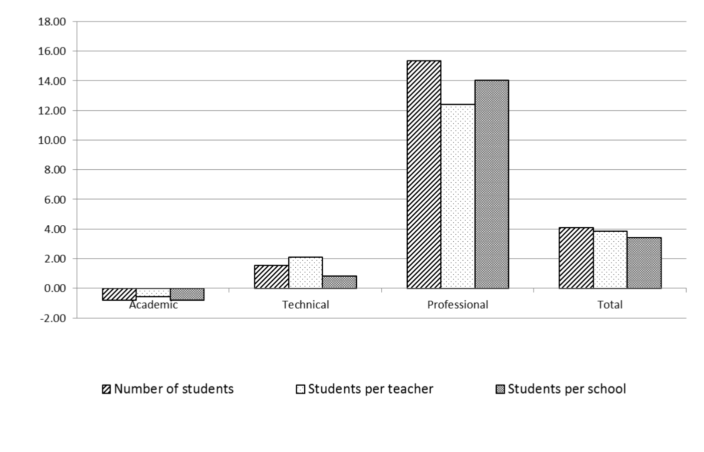
Appendix A Additional figures and tables

Figure A1
Percentage change of 9^{th} grade enrollment measures before and after the Reform, by school type



Notes: the statistics are constructed using aggregate data on enrollments, teachers and schools in the first grade of high school (grade 9) and by comparing the school year immediately before the introduction of the Reform with the school year immediately after (i.e. 1998/99 vs. 1999/2000). Each figure represents the percentage change with respect to the baseline of each category (i.e., in each cell) in the school year before the implementation of the Reform.
Source: own elaborations from MIUR.

Table A1
Characteristics of juvenile delinquency in selected countries

	Suspected		*Prosecuted*		*Incarcerated*		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); *Prosecuted individuals* are defined as persons against whom a legal action has been brought, regardless of the type of crime; *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).

Table A2
The data structure by age, years, cohorts and treatment status

Age		*Year*				
		1997	1998	1999	2000	2001
14	*Treatment Status*	0	0	1	1	1
	Cohort	1983	1984	1985	1986	1987
15	*Treatment Status*	0	0	0	1	1
	Cohort	1982	1983	1984	1985	1986
16	*Treatment Status*	0	0	0	0	1
	Cohort	1981	1982	1983	1984	1985
17	*Treatment Status*	0	0	0	0	0
	Cohort	1980	1981	1982	1983	1984

Notes: the Treatment Status values 1 and 0 indicate, respectively, whether the corresponding units in the cell defined by age and school year are affected (treated) or not affected (controls) by the Reform. For convenience, we also include the corresponding cohorts.

Table A3
Descriptive statistics: offending rate by gender and immigrant status

	mean	sd	N
Panel A: natives and immigrants			
Males and females	17.08	10.62	1640
Males	26.97	17.29	1640
Females	6.69	5.56	1640
Panel B: natives			
Males and females	14.27	8.25	1640
Males	23.04	13.76	1640
Females	5.05	3.84	1640
Panel C: immigrants			
Males and females	2.82	3.24	1640
Males	3.93	5.02	1640
Females	1.64	2.12	1640

Notes: offending rate of 14-, 15-, 16- and 17-year-olds by gender and immigrant status; immigrant adolescents are identified as those without Italian citizenship. *Source*: own elaborations from ISTAT.

Table A4
Adolescent crime displacement after the school time and to the school facilities: extended results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any offense	*Robbery*	*Assault and altercations*		*Any offense*		
Treated	-0.12***	-0.11***	-0.17***	-0.04	-0.04	-0.05**	-0.13***
	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)
Treated × AfterSchool				0.03			
				(0.06)			
Treated × EarlyAfterSchool					0.08		
					(0.09)		
Treated × LateAfterSchool					-0.01		
					(0.07)		
AfterSchool				-0.01			
				(0.03)			
EarlyAfterSchool					0.03		
					(0.04)		
LateAfterSchool					-0.04		
					(0.03)		
Treated × AtSchool						0.72***	
						(0.14)	
AtSchool						0.05	
						(0.11)	
Treated × Peer							0.44**
							(0.17)
Peer							0.34**
							(0.13)
Female	-0.04***	-0.04	-0.04	-0.05***	-0.05***	-0.05***	-0.04***
	(0.01)	(0.02)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
Age	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Non-native	0.10	0.20	-0.20**	0.09	0.08	0.09	0.10
	(0.19)	(0.23)	(0.07)	(0.18)	(0.17)	(0.18)	(0.19)
Education: junior high or primary	0.09*	0.12**	0.04	0.10**	0.10**	0.10**	0.09*
	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
Education: illiterate	0.08	0.06	0.12	0.07	0.06	0.08	0.04
	(0.15)	(0.14)	(0.20)	(0.15)	(0.15)	(0.15)	(0.14)
Education: college	0.03	0.07	-0.04	0.02	0.03	0.03	0.03
	(0.04)	(0.05)	(0.09)	(0.04)	(0.04)	(0.05)	(0.04)
Employed	-0.01	-0.03	0.05	-0.01	-0.01	-0.01	-0.01
	(0.03)	(0.04)	(0.07)	(0.03)	(0.03)	(0.03)	(0.03)
Student	0.18***	0.20***	0.12	0.18***	0.18***	0.17***	0.17***
	(0.05)	(0.05)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
Going outside for shopping (weekly frequency)	0.00	-0.01*	0.02**	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Going outside at night (weekly frequency)	0.02**	0.03**	-0.00	0.02**	0.02**	0.01*	0.02**
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Using public transportation (weekly frequency)	-0.00	0.00	-0.01	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Perceived control of police forces in the neighborhood	-0.03**	-0.01	-0.03	-0.03**	-0.03*	-0.03**	-0.02*
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.07	-0.07	0.28	0.13**	0.13**	0.30***	0.04
	(0.06)	(0.11)	(0.17)	(0.06)	(0.05)	(0.07)	(0.06)
N. Observations	2169	1423	746	1393	1393	1428	2169
Individual controls	✓	✓	✓	✓	✓	✓	✓
Region fixed effects	✓	✓	✓	✓	✓	✓	✓
Type of offence fixed effects	✓			✓	✓	✓	✓

Notes: the dependent variable is the victimization probability (VP_{ir}). The omitted categories are: *Education: high school*, *Employed*. *Treated* takes value 1 when the offenders have been subject to the Reform, and zero otherwise; $AfterSchool$ takes value 1 if the offense took place between 3 PM and 9 PM, and zero otherwise; $EarlyAfterSchool$ takes value 1 if the offense took place between 3 PM and 6 PM, and zero otherwise; $LateAfterSchool$ takes value 1 if the offense took place between 6 PM and 9 PM, and zero otherwise; $AtSchool$ takes value 1 if the offense took place at school; $Peer$ takes value 1 if the offender is a school mate. Survey weights applied. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels.
Source: own elaborations from ISTAT Victimization Survey.

Appendix B Spill-over effects of the Reform

In what follows we show that the concerns about spill-over effects of the Reform on the control group cab be considered negligible in our setting. First, we provide evidence that some institutional features of the implementation of the Reform reduce the possibility of spill-overs from the treated to the control groups. Second, we propose a formal test for spill-over effects of education and criminal behavior of the affected age groups on the 17-year-olds.

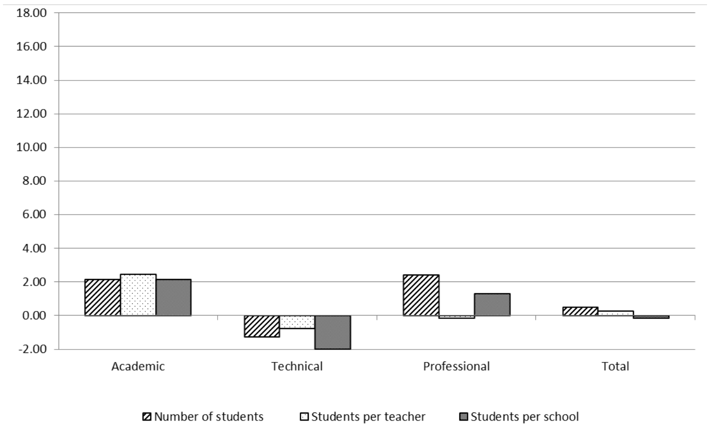
B.1 Interactions between 17-year-olds and other age groups and education quality

Following the huge and unexpected influx of new students after the implementation of the Reform, the schools were obliged to rent facilities in order to fulfill all the new enrollment requests. These were typically not in the same facility of the main school buildings (where, generally, all the other existing classrooms, including the 17-year-olds, were located), but, more frequently, in facilities distant from the main building. In fact, the number of additional facilities (so-called *sedi distaccate*) increased by almost 41 percent in the first year of implementation of the Reform (from 1,385 in the school year 1998/99 to 1,952 in the school year 1999/00).[34] This implies that interactions between the newly enrolled and the existing classrooms were actually limited. This institutional element helps to mitigate the concern that spill-over effects, either on the crime or on the education side, may have affected the control group.

Moreover, notice that choices of the 17-year-olds on the duration of the educational path cannot be influenced by the influx of new students, because they depend on the track chosen after the Junior-High School, at age 13. Indeed, students in the academic and technical tracks know, when enrolling in grade 9, that in order to gain the Diploma they have to pass successfully all the five grades of the High School. Students enrolled in the professional track can instead choose to drop out after having completed the first two grades. To corroborate these arguments, in the Appendix Figure B1 we show the same aggregate measures calculated for the 14-year-olds in Appendix Figure A1, for the 17-year-olds. There are not relevant changes in the number of students enrolled, nor in the measures of quality of the education system (such as the students per teacher ratio or the students per school ratio) from the school year 1998/99 to the 1999/00, when the Reform took place.

[34] Figures obtained from the annual publications of the Italian Ministry of Education (MIUR) on aggregate school statistics (in Italian: *La scuola statale: sintesi dei dati*) for the school years 1998/99 and 1999/00.

Figure B1
Percentage change of 12^{th} grade enrollment measures (i.e. 17-year-olds) before and after the Reform, by school type



Notes: the statistics are constructed using aggregate data on enrollments, teachers and schools in the fourth grade of high school (grade 12) and by comparing the school year immediately before the introduction of the Reform with the school year immediately after (i.e. 1998/99 vs. 1999/2000). Each figure represents the percentage change with respect to the baseline of each category (i.e., in each cell) in the school year before the implementation of the Reform.
Source: own elaborations from MIUR.

B.2 Testing for spill-over effects

Although the institutional setting and the descriptive evidence presented so far already mitigate the concerns about the spill-over effects on the control group, we take a further step and propose a more formal test. In details, to test whether younger age groups have induced some education or criminal behavior spill-overs on the control group, we regress our measures of education or crime (i.e. the ER and OR) for the 17-year-olds on the same measures for the treated ages (14, 15 and 16), and test whether there is a consistent pattern of correlations in the years when the Reform was implemented. We thus estimate the following equation:

$$\begin{aligned} OR_{pt}^{17} = \alpha_0 + \alpha_1 OR_{pt}^{14} + \alpha_2 OR_{pt}^{15} + \alpha_3 OR_{pt}^{16} + \\ + \beta_4 post_t^{14} + \beta_5 post_t^{15} + \beta_6 post_t^{16} + \\ + \gamma_1 OR_{pt}^{14} \times post_t^{14} + \gamma_2 OR_{pt}^{15} \times post_t^{15} + \gamma_3 OR_{pt}^{16} \times post_t^{16} \\ + \delta X_{pt} + (1 + \eta_j)\phi_t + \omega_p + \epsilon_{pt} \end{aligned} \tag{9}$$

where: OR_{pt}^{a} is the offending rate of the age group a in province p and year t; $post_t^a$ is a dummy variable equal to 1 for the years when the Reform affected age group a, and zero otherwise; $OR_{pt}^{a} \times post_t^a$ indicates the interaction term for each age group. As in the baseline analysis the vector X_{pt} includes the control variables defined in Table 1), and we also control for the province, year and region by year fixed effects. The same specification is also applied on the enrollment rates. The coefficients for the interaction terms (γ_1, γ_2 and γ_3) capture the correlations between the criminal or education measures of the treated units (i.e. the 14-, 15-, and 16-year-olds) and those of the control units (i.e. the 17-year-olds) in the years when the Reform is enforced.

It is also plausible that the enrollment rate of the affected age groups have an effect on the enrollment rate of the 17-year-olds in the following year. In other words, the enrollment decision of 14-, 15- and 16-year-olds in year t might affect the enrollment decisions of 17-year-olds in the same year t, but also in year $t + 1$. Thus, we also estimate a lagged specification of equation 9 in which all the independent variables are lagged by one year.

Table B1

Test for spill-over effects

	(1)	(2)	(3)	(4)
	Dependent variables:			
	OR17		ER17	
OR14 × post14	0.11	-0.05		
	(0.16)	(0.27)		
OR15 × post15	-0.12	-0.15		
	(0.09)	(0.19)		
OR16 × post16	-0.14	0.05		
	(0.12)	(0.15)		
ER14 × post14			0.12*	-0.13
			(0.06)	(0.09)
ER15 × post15			0.05	-0.03
			(0.07)	(0.06)
ER16 × post16			0.10	0.00
			(0.06)	(0.05)
N.Observations	410	410	410	410
Fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Specification:				
Contemporaneous	✓		✓	
Lagged		✓		✓

Notes: the dependent variable is the offending rate of the 17-year-olds in columns (1) and (2) (i.e. OR17) and the enrollment rate of the 17-year-olds in columns (3) and (4) (i.e. ER17). The fixed effects included are province, year and region by year; the control variables included are those listed in Table 1. In the *Contemporaneous specification* the OR17 of year t is regressed on the OR of the 14-, 15-, and 16-year-olds and other variables calculated in the same year; in the *Lagged specification* the OR17 in year t is regressed on the OR of the 14-, 15-, and 16-year-olds and other variables of the previous year ($t - 1$). The same applies for the enrollment rate specifications. Population weights applied. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels.
Source: own elaborations from ISTAT and MIUR.

Appendix Table B1 reports the results for the contemporaneous and lagged specifications. For both specifications we do not find any statistically significant correlation between the offending rates of the treated groups on those of the 17-year-olds, thus excluding that relevant spill-over effects are in place. Concerning the enrollment rate, we only observe a weak correlation with the enrollment decisions of 14-year-olds, which, however, should not be of concern. To conclude, rooting on the institutional setting, descriptive evidence and formal tests, we can be confident that the spill-over effects are not a concern in the present setting.