

Boshoff, Janine; Machin, Stephen; Sandi, Matteo

**Working Paper**

## Youth crime and delinquency in and out of school

Working Paper, No. 141

**Provided in Cooperation with:**

Università Cattolica del Sacro Cuore, Dipartimento di Economia e Finanza (DISCE)

*Suggested Citation:* Boshoff, Janine; Machin, Stephen; Sandi, Matteo (2025) : Youth crime and delinquency in and out of school, Working Paper, No. 141, Università Cattolica del Sacro Cuore, Dipartimento di Economia e Finanza (DISCE), Milano

This Version is available at:

<https://hdl.handle.net/10419/322217>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

UNIVERSITÀ CATTOLICA DEL SACRO CUORE  
**Dipartimento di Economia e Finanza**

**Working Paper Series**

***Youth Crime and Delinquency In And Out Of School***

*Janine Boshoff, Stephen Machin, Matteo Sandi*

**Working Paper n. 141**

**June 2025**



UNIVERSITÀ  
CATTOLICA  
del Sacro Cuore

# Youth Crime and Delinquency In And Out Of School

**Janine Boshoff**

*University of Manchester*

**Stephen Machin**

*London School of Economics*

**Matteo Sandi**

*Università Cattolica del Sacro Cuore*

Working Paper n. 141

June 2025

Dipartimento di Economia e Finanza  
Università Cattolica del Sacro Cuore  
Largo Gemelli 1 - 20123 Milano – Italy  
tel: +39.02.7234.2976  
e-mail: [dip.economiaefinanza@unicatt.it](mailto:dip.economiaefinanza@unicatt.it)

The Working Paper Series promotes the circulation of research results produced by the members and affiliates of the Dipartimento di Economia e Finanza, with the aim of encouraging their dissemination and discussion. Results may be in a preliminary or advanced stage. The Dipartimento di Economia e Finanza is part of the Dipartimenti e Istituti di Scienze Economiche (DISCE) of the Università Cattolica del Sacro Cuore.

# Youth Crime and Delinquency In And Out Of School

Janine Boshoff\*, Stephen Machin\*\* and Matteo Sandi\*\*\*

April 2025

\* *School of Social Sciences, University of Manchester*

\*\* *Department of Economics and Centre for Economic Performance, London School of Economics*

\*\*\* *Department of Economics and Finance, Catholic University of Milan and Centre for Economic Performance, London School of Economics*

## Abstract

This paper combines ten years of idiosyncratic variation in school closure dates for all secondary schools in England with administrative records of educational and criminal trajectories linked at the individual level to study the impact of the school schedule on the dynamics of youth crime. When school is not in session, students commit more property offences, more serious violent offences and fewer minor violent offences. Thefts, robberies and violent assaults drive these effects. This is novel evidence of strong incapacitation effects from the protective factor of schooling which affects not only the incidence of violence, but also its severity.

JEL keywords: Crime; School Attendance; Exclusion.

JEL classifications: I2, K14, K42, H44.

Corresponding author: Matteo Sandi. Correspondence address: [m.sandi@lse.ac.uk](mailto:m.sandi@lse.ac.uk)

## Acknowledgements

This study is part of the Education Policy and Youth Crime in England (EPYCE) project funded by ADR UK (see <https://www.adruk.org/our-work/browse-all-projects/education-policy-and-youth-crime-in-england-413/>), whose financial support is gratefully acknowledged. We thank the Department for Education (DfE) and the Ministry of Justice (MoJ) for guidance and enabling access to their linked administrative data extracts. This work was produced using statistical data from the Office for National Statistics (ONS). The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. The data do not allow us to compare “guilty” vs “non-guilty” individuals and the lack of a conviction should not be interpreted as “non-guilty”. Matteo Sandi also gratefully acknowledges financial support by the “D32-2022 Unpacking Inequalities: Mechanisms and Policies (UNEQUAL)” grant.

## 1. Introduction

Compulsory schooling is frequently viewed as a means of delivering societal benefits. One feature is that, because the economic and social costs of crime are high, the crime reduction that results from juveniles being in school, kept busy in a supervised environment and thus off the streets, generates significant social benefits. A link between crime and school attendance has been documented for most crime types and in a variety of settings, including the US (Jacob and Lefgren, 2003; Lochner and Moretti, 2004; Luallen, 2006; Anderson, 2014; Bell et al, 2016 and 2022; Cook and Kang, 2016; Cano-Urbina and Lochner, 2019), England and Wales (Machin et al, 2011) and Sweden (Hjalmarsson et al, 2015). The consensus is a property crime-reducing effect of school attendance (Machin and Sandi, 2025).

Currently less well-understood are the means by which in-school behaviour responds to legislative change mandating compulsory school attendance. While one of the main rationales behind compulsory schooling is to improve the human capital and labour market prospects of potential early dropouts, the primary experience of compulsory education for disruptive students is attending school surrounded by better-performing peers. Compulsory schooling laws often rest on a somewhat paternalistic view that unruly juveniles are actually better off staying on in class rather than leaving school at a younger age (Messacar and Oreopoulos, 2012).

However, when this occurs, relatively little is known about their behaviour in school. If juveniles are kept in school and disengaged from learning, delinquency in school may increase. Fellow students may suffer significant costs including increased bullying, gang activity, threats or a reduced perception of safety in school, which may affect their wellbeing and learning process. Disruptive peers may hinder school performance (Robertson and Symons, 2003; Figlio, 2007; Carrell and Hoekstra, 2010; Sarzosa, 2024) and decrease future earnings (Carrell et al, 2018), while raising the risk of drug use (Gaviria and Raphael, 2001; Kawaguchi, 2004; Lundborg, 2006; Powell et al, 2005), cheating (Carrell et al, 2008), indiscipline in the classroom (Carrell and Hoekstra, 2010), depression (Wang et al, 2011), anxiety (Kowalski and Limber, 2013) and suicidal thoughts and behaviours (Holt et al, 2015). These negative effects of bullying continue also after the abuse ends and they correlate with a broad set of physical, mental and economic difficulties in adulthood (Takizawa et al, 2014; Wolke et al, 2013; Wolke and Lereya, 2015). Among US high school students, over 20% reported being bullied in person at school and 16% reported being cyberbullied in 2018 (Basile et al, 2020). Thus, school bullying is widespread and causes substantial harm to the students involved.

This paper studies the causal impact of compulsory schooling on the day-to-day possibility, desire and actual act to commit crime by combining idiosyncratic variation in school closure dates for all secondary schools in England with administrative records of educational and criminal trajectories linked at the individual level. Administrative records of all school closure dates were collected for all state-maintained secondary schools in England from the 1<sup>st</sup> August 2010 to the 31<sup>st</sup> December 2019. The

school calendar data contain detailed information on when school is in session and the reason for school closures (e.g., winter, spring or summer school break, national holiday, or teacher training). Secondary schools in England set their half-term breaks and teacher training dates independently, generating extensive variation in the timing of school closures across schools and over time.

For all students in state-maintained secondary schools in England, administrative records of their complete schooling trajectories from the Department for Education were linked at the individual level with administrative records of their complete criminal trajectories from the Ministry of Justice. These very rich microdata allow us to overcome the quite severe previous data constraints in existing work on school incapacitation, resulting from use either of self-reported surveys that do not identify perpetrators or of locality-level aggregated data. The granularity of our data for the universe of students in England allows us to rectify this and offer novel, strong insights on school attendance and youth crime.<sup>1</sup> Our data enable us to follow pupils through from school discipline sanctions on to observing the severity of the crimes committed on these dates and their repercussions in the criminal justice system.

The panel dataset follows pupils through their entire school history in all state-maintained secondary schools in England with daily frequency from 2010 to 2019, and links to their criminal histories at the individual-level whilst in school and subsequently once they have left. The analysis starts by showing descriptively the trends in property and violent crime on “normal” schooldays, on weekdays when school is not in session, during weekends, and during the autumn, spring and summer breaks. The trends that emerge across the school calendar appear similar to those in school bullying and cyberbullying in the US prior to the Covid-19 pandemic (Bacher-Hicks et al, 2022).

The research design implemented in this paper isolates the causal impact on youth crime of idiosyncratic weekdays when school is not in session from the impact of weekends and other school breaks, which may not be comparable to “normal” weekdays when school is in session. The severity of crime is defined using the distinction in the British criminal justice system between indictable offences, which constitute the most serious offences, summary offences, which constitute minor offences, and offences that are triable either way as their severity depends on the specific case. By including a series of fixed effects for date and school, our analysis shows that property crime and indictable violence increase while summary violence decreases during weekdays when school is not in session. Although our focus is on youth aged 11-15, i.e., under-age, these offences can result in convictions in court, thus potentially having lasting consequences for the criminal trajectories of the juveniles involved. These conclusions are robust to the use of alternative estimation techniques.

---

<sup>1</sup> Throughout the paper, “school attendance” refers to the timing of school openings and not the actual school attendance decision by the students that is potentially endogenous in the crime equation.

Increased property crime is driven by increased thefts while the increased indictable violence is driven by increased robberies, which are typically committed outside school.<sup>2</sup> Since these results appear also when only court convictions are considered and they are unaffected by the number of schools in the locality that close on the same weekday, they do not seem to reflect differential crime clearance rates by the police when just a few students are off from school. Moreover, neither property or violent crime nor permanent exclusions by school directors for property or violent misconduct decrease in the days immediately before or after the weekday off. Thus, students do not seem to “substitute” delinquent acts from one day to another. In contrast, a genuine incapacitation effect of school on crime appears to be the prime factor governing the dynamics of thefts and robberies.

The reduced incidence of summary violent offences, i.e., minor acts of violence, is mostly driven by reduced assaults against the person, which are typically committed in proximity to other students (although cyberbullying may constitute an exception). Neither violent crime nor permanent exclusions by school directors for violent misconduct increase in the days immediately before or after the weekday off. Thus, students do not seem to hold grudges and “substitute” violence between one day and another. Unlike our conclusion for property crime, the reduction in summary violence during school closures is mitigated by the number of other schools in the locality that are not in session on the same day. This is coherent with a concentration effect of school on minor acts of violence, as it suggests that minor acts of violence increase when all students are forced in the same place and the greatest reduction in minor acts of violence appears when some students have to attend school and others do not.

These findings have four important implications. First, by showing that school attendance reduces the incidence of indictable violence and it increases the incidence of summary violence, they provide at least strongly suggestive evidence that schools may prevent not so much the *incidence* but rather the most serious types of physical violence. While the existing incapacitation literature shows that the incidence of violence increases when school is in session (Jacob and Lefgren, 2003; Anderson, 2014; Bacher-Hicks et al, 2022; Machin and Sandi, forthcoming), no study to date has examined the impact of schooling on the *severity* of physical violence. We provide novel evidence that the protective factor of schooling may act by reducing the *severity* of physical violence. This is important because, compared with victims of summary violence, victims of indictable violence often suffer more severe physical injuries and require greater access to legal and psychological support (Bureau of Justice Statistics, 2022), while also facing higher levels of psychological distress, post-traumatic stress disorder and medical expenses (Langton and Planty, 2024).

Second, by exploiting otherwise innocuous weekdays off from school, this study shows that in-person interactions at school are important drivers of minor acts of violence among students. Based on

---

<sup>2</sup> For evidence on robbery location figures, see, e.g., [FBI — Robbery](#).

our analysis, schools in the same locality should set their closure dates on different weekdays, as the contemporaneous school absence of students from different schools may result in increased assaults.

Third, by showing mixed evidence of school attendance on different crime categories, these results point towards one plausible mechanism behind COVID-19's mixed impacts on mental health. Brodeur et al (2021), for instance, show that COVID-19 reduced stress and suicidal thoughts. Bacher-Hicks et al (2022) show that online searches for both school bullying and cyberbullying in the US reduced by roughly 30–35% during the Covid-19 pandemic, suggesting that reduced bullying may have been an unexpected benefit of reduced in-person interaction. The findings of reduced minor acts of violence when school is not in session are consistent with the notion that reduced in-person interaction can have mixed effects on individual wellbeing.

Fourth, from a policy perspective, this study highlights the need to supplement youth justice policies and compulsory school policies with services that help keep students safe and engaged in learning and prevent the potential increase in minor acts of violence on school-days documented here. This intuition can also be extended to other settings (e.g., parks, recreation centres, youth centres, sport activities) to appreciate their potential unintended effects. To the extent that these settings attract several youths to the same place, they will increase the number of interactions between youths, and thus they might increase the potential number of violent conflicts.

## 2. Data

### *DfE-MoJ data linkage*

The empirical analysis uses individual-level administrative data matching education and criminal records from the Department for Education (DfE) and the Ministry of Justice (MoJ) from the 1<sup>st</sup> August 2010 to the 31<sup>st</sup> December 2019 for the entire population of students in state-maintained secondary schools in England. The DfE data extract contains administrative records of pupil-level achievement at various stages of the school trajectory and pupil characteristics, such as gender, free school meal eligibility, ethnicity, native language and school of enrolment at all stages of education.

The criminal records of juveniles in England are extracted from the MoJ's Police National Computer (PNC). The PNC stores criminal records' information across England and Wales. It is maintained by the UK Home Office and it holds approximately 13 million person records with precise information on all the criminal offences for which an individual has been cautioned or convicted, as well as detailed information on the nature and date of the offence and on the age of the offender.<sup>3</sup> This information is used to define our outcome of interest, namely the risk and count of offences committed by an individual on a given date. Data on the nature of the offence allow us to distinguish indictable

---

<sup>3</sup> Offences that are not recorded in the data include, e.g., TV evasion.



offences, i.e., more serious offences, from summary offences, for which the sentence cannot exceed six months of imprisonment. While the British justice system unambiguously treats some offences as summary, e.g., traveling on public transport without paying the fare, other crimes, such as theft, are triable either way and the distinction between indictable and summary offences depends on the specific case.

Criminal records are also grouped into property crime, which includes theft, criminal damage and arson, burglary; and violent crime, which includes violence against the person, sexual offence and robbery. Total crime is defined as the sum of property and violent crime.<sup>4</sup> Finally, our data allow us to follow individuals from the date of the offence through the criminal justice system, as they include information on the recorded result of the court hearing. The data do not allow us to compare “guilty” vs “non-guilty” individuals and the lack of a conviction should not be interpreted as “non-guilty” (e.g., a caution is not a court conviction but it is an official warning from the police that is usually given for a minor crime, where the offender has admitted to the offence and has agreed to be cautioned). However, this information is used to restrict our analysis to court convictions and check robustness of our findings.

#### *School term and holiday dates*

The DfE-MoJ data linkage was combined with data on the school term and holiday dates for every state-maintained secondary school in England from the 1<sup>st</sup> August 2010 to the end of 2019. In England, the school year is composed of three terms, i.e., the autumn term from September to December, the spring term from January to the Easter break (typically between March and April) and the summer term from the Easter break to mid-July, when the school year terminates. Within terms, schools have the power to set independently their half-term dates, which are school breaks ranging from five to ten days. While the school terms start and terminate at the same time for all schools (with few exceptions), the timing of half-terms are set independently and they display substantial variation across schools. Our analysis exploits this variation, as it allows us to compare the behaviour of youths in different schools when their respective schools are not in session.

An additional source of plausibly exogenous variation in the timing of compulsory school attendance is generated by IN-Service-Teacher-training (INSET) dates. INSET dates can be either used by the schools to train the school personnel on a specific matter or to provide teachers with time to focus on administrative or professional tasks. In a school year, there are typically five INSET dates and each school can decide independently when they are held and whether they will affect the whole school or just a few classes. INSET dates are posted on the school website and they are communicated to parents and educators well in advance. The variation in the timing of compulsory school attendance induced by the INSET dates is also exploited in our analysis.

---

<sup>4</sup> Other crime categories, e.g., drugs, are not studied here.

Information on the entire school calendar, including the start- and end-dates of half-terms and INSET dates, was scraped from the school websites and schools were contacted directly whenever this information could not be obtained from their websites. This resulted in the construction of a dataset that covers nearly the entire universe of state-maintained secondary schools in England and that only excludes private schools. This dataset was combined with the DfE-MoJ dataset and the count of offences by category was calculated for all students in each school and each date. Table 1 shows the resulting panel dataset on delinquent behaviour at the school level for all state-maintained secondary schools in England with daily frequency from 2010-19.<sup>5</sup> In Table 1 as in the remainder of our exhibits, all our displayed school\*day averages and estimated coefficients are weighted by the count of students enrolled each school year in each school and they are multiplied by 1000 for easier readability.

### *Descriptive patterns*

Our sample includes 3,264 secondary schools and 3,440 days (i.e., from the 1<sup>st</sup> August 2010 to the 31<sup>st</sup> December 2019), resulting in a total of 11,228,160 school\*day observations. Of these, Table 1 shows that 5,885,406 observations are on “normal” schooldays (52.42%), while 509,831 observations are on weekdays when school is not in session either because of half-term breaks or because of INSET dates: these are the dates that generate our identifying variation and they constitute approximately 4.54% of our sample. Of the remainder, 3,208,512 observations are on weekends (28.57%), 332,446 observations are during the winter breaks (2.96%), 314,381 observations are in the spring breaks (2.8%) and 977,584 observations are in the summer break (8.7%).<sup>6</sup>

Table 1 shows descriptively that total crime, defined as the sum of property and violence, significantly increases on weekdays when school is not in session and during spring breaks, while it reduces during weekends, winter breaks and summer breaks. Property crime significantly increases on weekdays when school is not in session, during weekends and spring breaks, while it reduces in winter breaks. In contrast, violent crime decreases both during weekends and in winter and summer breaks, while the evidence appears somewhat more mixed on weekdays when school is not in session and spring breaks. The violence reduction during summer breaks is likely explained by the fact that a six-week break is common in England and students may travel, thus changing the scope and venue of crime.

## **3. Empirical strategy**

The main challenge in the estimation of the day-to-day impact of school attendance on crime is that schooldays are not randomly distributed across the year. Crime may be systematically more or less common on days when school is not in session for several reasons. For instance, the days from mid-July

---

<sup>5</sup> Appendix Table A.1 shows descriptive statistics for property and violent crime separately by indictable (or triable either way) and summary offences.

<sup>6</sup> The count of different dates found in each group needs not add up to the total of 3,440 dates in our sample precisely because each school sets their calendar dates independently and, thus, not all schools close on all the dates when other schools close.

to the end of August are typically not schooldays in England: as criminality rises with temperature (Jacob et al, 2007), using the summer break to test how property crime changes when school is not in session would bias our estimates upwards. Similarly, weekends are not schooldays either. However, to the extent that more people are in the street and at social events on weekends, the potential returns from property crime might be higher, which in turn would also bias our estimates upwards.

We address these endogeneity concerns by focusing on the variation generated by the half-term dates and IN-Service Teacher (INSET) dates. A similar approach is used in Jacob and Lefgren (2003). Like in the US, INSET dates in England are weekdays where students do not attend school and that teachers use for professional development, teacher conventions, planning, or parent-teacher conferences. Similar to Jacob and Lefgren (2003), we argue that these days are very unlikely to correlate with any other systematic factors influencing the level of youth criminality. Our data include multiple school years and variation appears both within and between schools and over time in the dates of half-term breaks and INSET dates. Our analysis includes a set of date fixed effects, e.g., 1<sup>st</sup> September 2010 fixed effects, and school fixed effects, to account for anything specific to a particular date or a particular school that may correlate with youth crime. For secondary school  $i$  on day  $t$ , the equation of interest is:

$$Y_{it} = \alpha_i + \alpha_t + \beta_1 * OFF_{it} + \epsilon_{it}, \quad (1)$$

where  $Y_{it}$  is either the risk (0/1) or the count of criminal offences committed by the students enrolled in school  $i$  on day  $t$ ,  $\alpha_i$  is a set of school fixed effects and  $\alpha_t$  is a set of date fixed effects.  $OFF_{it}$  is a binary variable that takes value 1 if school  $i$  is not in session on a weekday  $t$  during the school year and value 0 otherwise. Therefore,  $OFF_{it}$  takes value 0 during weekends and during the winter break, spring break and summer break. The coefficient of interest is  $\beta_1$ , as it captures the effect of school closures on crime during weekdays when school is normally in session. Standard errors are clustered at the school level and  $\epsilon_i$  is the error term. For binary outcomes, equation (1) is estimated by OLS techniques (i.e., a linear probability model) and probit techniques. When the outcome variable is the count of crime, Poisson Pseudo-Maximum Likelihood (PPML) estimates are also shown since crime is a rare event and the count dependent variable has many zeros. The Appendix also shows robustness estimates with the count of crimes committed by the students at school  $i$  on weekday  $t$  deflated by the count of students in school  $i$ .

## 4. Results

### *Main results*

Table 2 shows the main OLS estimates of the study. Panel A shows estimates of the impact of school closure on the risk (0/1) of total crime in columns (1)-(3), the risk (0/1) of a conviction in court for total crime in columns (4)-(5), the count of total crime in columns (6)-(8) and the count of convictions in court

for total crime in columns (9)-(10). Panels B, C, D, E, F and G do likewise for the various crime categories included in our definition of total crime. In particular, Panel B focuses on property crime, Panels C and D break it down between indictable (or triable either way) and summary property crime, Panel E focuses on violent crime and Panels F and G break it down between indictable and summary violent crime. All estimates include fixed effects for date, winter break, spring break and summer break. While columns (1) and (5) omit school fixed effects, they are included in the estimation in all other columns, with columns (3), (5), (8) and (10) additionally controlling for the count of schools closed on a given date in the Local Education Authority (LEA).

Panel A shows that the count of criminal offences increases by roughly 9%-10% in weekdays when school is not in session.<sup>7</sup> This conclusion is robust to the use of alternative sets of fixed effects and to restricting the focus on court convictions. A smaller, statistically insignificant increase appears in the risk of committing crime, suggesting that this effect is not driven by marginal offenders. Panel B shows that both the risk and the count of property crimes and convictions for property crimes increase when school is not in session, as an 11.2%-12.6% increase in the risk of property crime and a 15%-25.7% increase in the count of property crime are estimated. Panels C and D split property crime between indictable (or triable either way) and summary property crime, showing that the former drive the results in Panel B. Indictable (or triable either way) property crimes include theft, criminal damage and arson and burglary, and in most cases the magnitudes of these effects are slightly larger than those in Panel B. Summary property crimes include, e.g., traveling on public transport without paying the correct fare, failing to show ticket, interference with motor vehicle/trailer, taking or riding a pedal cycle without consent, etc. The lack of significance for these petty crimes might reflect a genuine null effect or the fact that many of these go unnoticed, resulting in measurement error in our estimates.

One may wonder whether these results reflect the improved crime clearance rate by the police when just a few students are off from school and in the street, rather than a genuine increase in criminal activity. With most juveniles in school and just a few students in the street, it might be easier for the police to clear crime on these particular dates. However, the consistency in the results that focus on court convictions should mitigate the concern that our estimates reflect the differential crime clearance rate on these dates. Moreover, the count of schools closed on a weekday either because of half-term breaks or because of INSET dates was calculated for each date and each LEA, i.e., in each locality. If our results reflected the greater crime clearance rate, our effects should be mitigated or even cancelled out by the contemporaneous school closures of multiple schools in the same locality. As columns (3), (5), (8) and (10) in Panels A-D show, the interaction between our treatment status of interest and the count of schools closed in the locality is never significant and numerically very small, indicating that our conclusions are

---

<sup>7</sup> In Table 2, % effects are derived dividing the estimated coefficient by the mean of the outcome variable on school-days.

not affected by the simultaneous school closures of multiple schools in the same locality and in turn suggesting that our results do not reflect the increased crime clearance rate by the police when just a few students are off from school.

Panels E, F and G focus on violent crime. The estimates indicate that indictable and summary violence follow opposite dynamics when school is not in session. On these dates, the risk of indictable violence increases by 9.2%-10.1%, the risk of a court conviction for indictable violence increases by 20.4%-24.9%, the count of indictable violence increases by 15.1%-17.5% and the count of court convictions for indictable violence increase by 20.5%-23.7%. In contrast, when school is not in session, the risk of summary violence reduces by 14.8%-17.8%, the risk of a court conviction for summary violence reduces by 12.2%-18.5%, the count of summary violence reduces by 13.6%-18.5% and the count of court convictions for summary violence reduce by 9%-16.4%.

These results confirm that violence may increase when school is in session, but they also point towards a novel means by which school attendance may exert a protective factor on adolescents, namely by preventing the escalation of violence and reducing its severity. Moreover, unlike all other crime categories, the estimated significance in the interaction between our treatment of interest and the count of schools closed in the locality on a specific date indicates that the reduction in summary violence is partially offset when other schools in the same locality are closed on the same day. This finding appears consistent with the concentration effect of school on violence, as it suggests that separating juveniles also in the timing of their days off from school is likely to exert the greatest violence-reducing effect.<sup>8</sup>

#### *Crime displacement and school exclusions*

School attendance could merely alter the place or timing of youth delinquency, whilst not impacting its overall level. Indeed, as delinquency in school is not always reported to the police, the increase in property crime in Table 2 might reflect the greater likelihood of delinquency outside school to get reported. Also, when school is not in session, increased property crime may compensate the reduced property misconduct in school. Moreover, youth who plan to steal something from a local shop may think it sensible to do it on a day off from school. Similarly, juveniles planning to seek revenge on their rivals could choose to do it on a school day because the other juveniles will be nearby and/or because they may want others to witness the revenge. In these cases, our estimates may not reflect genuine changes in the amount of delinquency in aggregate over the longer run.

Our empirical analysis adopts two strategies to address these concerns. First, it examines permanent exclusions from school for property misconduct and violent misconduct. In England,

---

<sup>8</sup> Appendix Table A.2 displays our probit estimates of the impact of school closures on the risk of crime. Appendix Table A.3 displays our Poisson Pseudo-Maximum Likelihood (PPML) estimates of the impact of school closures on the count of crimes. Appendix Tables A.4 and A.5 also show OLS and PPML estimates when the count of crimes committed by the students enrolled in school  $i$  are deflated by the count of students enrolled in school  $i$ . Appendix Tables A.2, A.3, A.4 and A.5 are organised similarly to Table 2 and estimates appear consistent with those in Table 2.

permanent exclusions are equivalent to school expulsions in the US and therefore are the most directly comparable disciplinary actions a school director can take in response to bad behaviour. Second, it examines the level of youth crime and permanent exclusions on the days just before and just after the weekdays off from school. If juveniles simply saved all of their grudges, vendettas, and other violent acts for days when school was in session, one might expect there to be unusually high levels of crime on the days just before or after school was not in session to compensate for the unusually low levels of violent crime on non-school days. Since not all violence may result in a criminal offence, administrative records from the Department for Education on permanent exclusions from school constitute a valuable complement to the crime records of the Ministry of Justice.

Table 3 shows estimates of the impact of school closures on the count of permanent exclusions and crimes. To be precise, Panel A focuses on total permanent exclusions and total crimes, while Panels B and C break down the total into property misconduct and property crime (Panel B) and violent misconduct and violent crime (Panel C). Columns (1)-(2) show OLS estimates for permanent exclusions, while columns (3)-(5) and columns (6)-(8) show, respectively, OLS and PPML estimates for the combined permanent exclusions and criminal offences by category. In England, pupils can be excluded from school also for misconduct outside the school premises and on dates when school is not in session. Perhaps not surprisingly, columns (1)-(2) show that permanent exclusions reduce almost entirely by 97% when school is not in session, with permanent exclusions for property misconduct dropping by 68%-69% and exclusions for violent misconduct dropping by 98.8%-98.9%. When crime offences and permanent exclusions are combined, both OLS and PPML estimates in Panel A appear statistically insignificant, reflecting the contrasting effects of compulsory school attendance on different types of crime. Panel B shows that, when property offences and property exclusions are combined, the results look like a replication of those in Table 2. Thus, the evidence in Table 3 provides no empirical support to the concern that the increase in property crime may merely compensate reduced property misconduct when school is not in session. Panel C also reiterates the conclusion that violence reduces when school is not in session by showing that violent exclusions are affected in the same direction as summary violent crimes.

Chart 1 uses both crime records and permanent exclusions in school to test whether school attendance merely alters the timing of youth delinquency. Panel A focuses on total crime and total exclusions, Panel B focuses on property crime and property exclusions and Panel C focuses on violent crime and violent exclusions. The left-hand side charts display results for criminal records while the right-hand side charts combine criminal and exclusion records. Specifications (1) and (2) show OLS estimates with and without school fixed effects and specifications (3) and (4) do likewise for PPML estimates. Both the individual and joint tests of significance of the “Pre” and “Post” coefficients are displayed in each chart. In all cases, Chart 1 shows that crime and exclusion records did not vary significantly in the days just before or just after the school closure dates, suggesting that the increase in property crime and

indictable violence and the decrease in summary violence on the school closure dates reflect genuine changes in the amount of delinquency in aggregate over the longer run.

#### *Different crime categories*

What crimes do juveniles actually commit in and outside school? Table 4 shows estimates where the counts of property and violent crime are broken down into finer categories. Columns (1)-(3) display OLS estimates while columns (4)-(6) display PPML estimates. Panels A, B, and C focus on various categories of property offences. They show that the increased incidence of property crime when school is not in session is mainly driven by increased thefts, which are typically committed outside school. Thus, a genuine incapacitation effect of school on crime appears to be the prime factor governing the dynamics of these property crimes.

Panels D, E and F in Table 4 turn the attention to violence. Panel D shows that the incidence of robberies significantly increases when school is not in session. Although robbery is a violent offence, it is an indictable offence that is typically committed in the street. Therefore, an incapacitation effect of school appears also in this case to be the prime driver. Panels E and F show that, when school is not in session, the incidence of violence against the person significantly reduces, while a non-significant reduction in sexual violence also appears. Since violence against the person is typically committed in proximity to the victim (although cyberbullying may constitute an exception), the concentration effect of school on violence appears to be the main driver of this type of crime. Taken together, the results in Table 4 suggest that increased robberies are the main driver behind the increased indictable violence and reduced violence against the person is the main driver of the decreased summary violence in Table 2.

## **5. Conclusions**

A large literature has documented the beneficial effects of compulsory school attendance, one of which is reduced crime. This has been a major driver of policy in the US and elsewhere towards schooling and other youth intervention programmes aiming to support the path of human capital formation and labour market prospects, and discourage criminal participation of at-risk youth (e.g., Heller, 2014). However, little is known about the educational experience of juveniles when they are in school. Using administrative data linking education and criminal records at the individual level for juveniles in the state school system in England from 2010 to 2019, this study examines the impact of idiosyncratic school closure dates on the day-to-day propensity to commit crime of juveniles.

The empirical analysis shows that property offences and serious violence increase while minor acts of violence decrease during weekdays when school is not in session. The increased incidence of property crime is mostly driven by increased thefts. Similarly, when school is not in session a significant increase also appears in the incidence of robberies, which are serious violent offences typically committed

outside school. Thus, a genuine incapacitation effect of school appears to be the prime factor governing property crime and robberies.

The reduced incidence of minor acts of violence is mostly driven by reduced assaults against the person, which typically occur in proximity to the victim. Unlike other crime categories, this reduction during school closures is hindered by the number of other schools in the locality that are not in session on the same day. This appears coherent with a concentration effect of school on violence, as it shows that violence increases when all students are forced in the same place while the greatest reduction in violence occurs when some students have to attend school and others do not.

These findings have four important implications. First, by showing that school attendance reduces indictable violence and it increases summary violence, they provide novel evidence that schools may prevent not so much the incidence but rather the severity of violence. Our analysis shows that the property and indictable violent offences that occur on school closure dates can lead to convictions in court and thus potentially set up young offenders for a criminal career. Second, by exploiting otherwise innocuous weekdays off from school, they show that in-person interactions at school are important drivers of minor acts of violence. Schools in the same locality should set their school closure dates on different weekdays, as the contemporaneous school absence for students from different schools may lead to increased assaults. Third, by showing mixed evidence of school attendance on different crime categories, our findings are consistent with the notion that reduced in-person interaction can have mixed effects on individual wellbeing. Fourth, from a policy perspective, this study highlights the need to supplement youth justice policies and compulsory school policies with services that help keep students safe and engaged in learning and prevent the potential increase in minor acts of violence on school-days documented here.

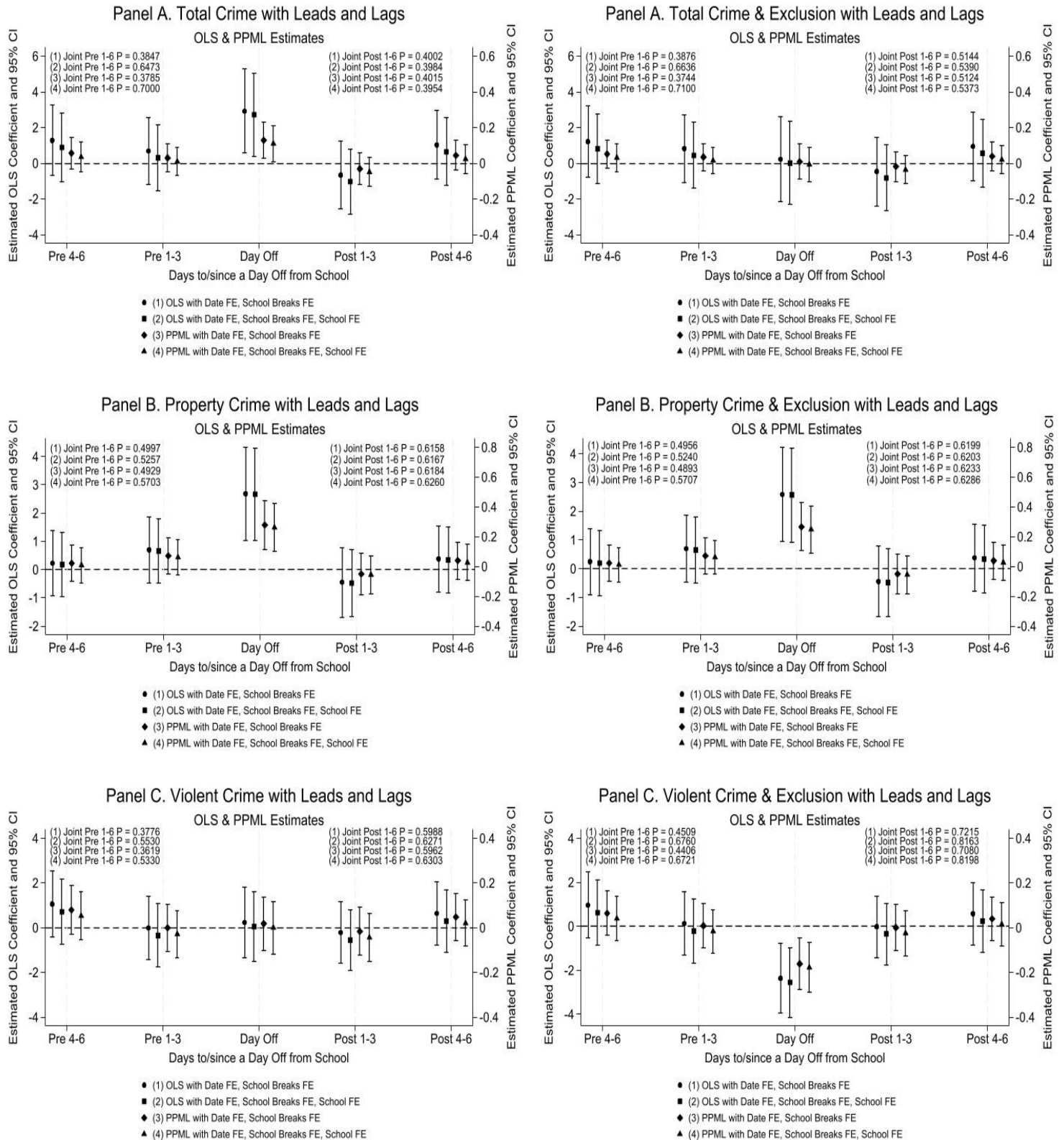


## References

- Anderson, D. (2014). In school and out of trouble? The minimum dropout age and juvenile crime, Review of Economics and Statistics, 96, 318-31.
- Bacher-Hicks, A., Goodman, J., Greif Green, J., and Holt, M. K. (2022). The COVID-19 pandemic disrupted both school bullying and cyberbullying, American Economic Review: Insights, 4, 353-70.
- Basile, K. C., Clayton, H. B., DeGue, S., Gilford, J. W., Vagi, K. J., Suarez, N. A., Zwald, M. L., and Lowry, R. (2020). Interpersonal violence victimization among high school students — Youth Risk Behavior Survey, United States, 2019. Morbidity and Mortality Weekly Report Supplements, 69, 28–37.
- Bell, B., Costa, R., and Machin, S. (2016). Crime, compulsory schooling laws and education, Economics of Education Review, 54, 214-26.
- Bell, B., Costa, R., and Machin, S. (2022). Why does education reduce crime?, Journal of Political Economy, 130, 732-765.
- Brodeur, A., Clark, A. E., Fleche, S., and Powdthavee, N. (2021). COVID-19, lockdowns and well-being: evidence from Google trends, Journal of Public Economics, 193: Article 104346.
- Bureau of Justice Statistics. (2022). Socio-emotional consequences of violent crime, 2022, NCJ 309041, Bureau of Justice Statistics report, August 2024.
- Cano-Urbina, J., and Lochner, L. (2019). The effect of education and school quality on female crime, Journal of Human Capital, 2019, 13, 188-235.
- Carrell, S., and Hoekstra, M. (2010). Externalities in the classroom: how children exposed to domestic violence affect everyone's kids, American Economic Journal: Applied Economics, 2, 211–228.
- Carrell, S. E., Hoekstra, M., and Kuka, E. (2018). The long-run effects of disruptive peers, American Economic Review, 108, 3377-3415.
- Carrell, S., Malmstrom, F., and West, J. (2008). Peer effects in academic cheating, Journal of Human Resources, 43, 173–207.
- Cook, P.J., and Kang, S. (2016). Birthdays, schooling, and crime: Regression-discontinuity analysis of school performance, delinquency, dropout, and crime initiation, American Economic Journal: Applied Economics, 8, 33–57
- Figlio, D. (2007). Boys named Sue: disruptive children and their peers, Education Finance and Policy, 2, 376–394.
- Gaviria, A., and Raphael, S. (2001). School-based peer effects and juvenile behavior, Review of Economics and Statistics, 83, 257–268.
- Heller, S. (2014). Summer jobs reduce violence among disadvantaged youth, Science, 346, 1219–1223.
- Hjalmarsson, R., Holmlund, H., and Lindquist, M. (2015). The effect of education on criminal convictions and incarceration: Causal evidence from micro-data, The Economic Journal, 125, 1290-1326.
- Holt, M. K., Vivolo-Kantor, A. M., Polanin, J. R., Holland, K. M., DeGue, S., Matjasko, J. L., Wolfe, M. and Reid, G. (2015). Bullying and suicidal ideation and behaviors: a meta-analysis, Pediatrics, 135, e496–e509.
- Jacob, B., Lefgren, L., and Moretti, E. (2007). The dynamics of criminal behavior: Evidence from weather shocks, Journal of Human Resources, 42, 489-527.
- Jacob, B. and Lefgren, L. (2003). Are idle hands the devil's workshop? Incapacitation, concentration and juvenile crime, American Economic Review, 93, 1560-77.
- Kawaguchi, D. (2004). Peer effects on substance use among American teenagers, Journal of Population Economics, 17, 351–367.
- Kowalski, R. M., and Limber, S. P. (2013). Psychological, physical, and academic correlates of cyberbullying and traditional bullying, Journal of Adolescent Health, 53, S13–S20.
- Langton, L., and Planty, M. (2024). Harms and consequences of victimization. Journal of Contemporary Criminal Justice, forthcoming.

- Lochner, L., and Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests and self-reports. American Economic Review, 94, 155-89.
- Luallen, J. (2006). School's Out... forever: a study of juvenile crime, at-risk youths and teacher strikes, Journal of Urban Economics, 59, 75-103.
- Lundborg, P. (2006). Having the wrong friends? Peer effects in adolescent substance use, Journal of Health Economics, 25, 214–233.
- Machin, S., Marie, O., and Vujic, S. (2011). The crime reducing effect of education, Economic Journal, 121, 463-84.
- Machin, S., and Sandi, M. (2025). Crime and education, Annual Review of Economics, forthcoming.
- Messacar, D., and Oreopoulos, P. (2012). Staying in school: a proposal to raise high school graduation rates, Discussion Paper No. 2012–07, The Hamilton Project, Brookings Institution: Washington, DC, USA, 2012.
- Robertson, D., and Symons, J. (2003). Do peer groups matter? Peer group versus schooling effects on academic attainment, Economica, 70, 31–53.
- Powell, L., Tauras, J., and Ross, H. (2005). The importance of peer effects, cigarette prices, and tobacco control policies for youth smoking behavior, Journal of Health Economics, 24, 950–968.
- Sarzosa, M. (2024). Victimization and skill accumulation: the case of school bullying, Journal of Human Resources, 59, 242-279.
- Takizawa, R., Maughan, B., and Arseneault, L. (2014). Adult health outcomes of childhood bullying victimization: evidence from a five-decade longitudinal British birth cohort, American Journal of Psychiatry, 171, 777–84.
- Wolke, D., and Lereya, S. T. (2015). Long-term effects of bullying, Archives of Disease in Childhood, 100, 879–85.
- Wolke, D., Copeland, W. E., Angold, A., and Costello, E. J. (2013). Impact of bullying in childhood on adult health, wealth, crime, and social outcomes, Psychological Science, 24, 1958–70.

**Chart 1. Estimates of the Impact of School Attendance on Count of Crimes and Permanent Exclusions with Leads & Lags**



**Notes:** Chart displays estimates of the impact of school closures on the count of criminal offences and permanent exclusions from school. Throughout the charts, specifications (1) and (2) show Ordinary Least Squares (OLS) estimates while specifications (3) and (4) show Poisson Pseudo-Maximum Likelihood (PPML) estimates. Total exclusions are defined as the sum of permanent exclusions for property misconduct and for violent misconduct. Total crime is defined as the sum of property crime and violent crime. The left-hand side charts show results for the count of crimes while the right-hand side charts show results for the sum of the count of permanent school exclusions and the count of crimes. Panel A focuses on total crime and permanent exclusion for total misconduct, Panel B focuses on property crime and permanent exclusion for property misconduct, and Panel C focuses on violent crime and permanent exclusion for violent misconduct. For each school  $i$ , the risk of crime (or conviction) is a (0/1) dummy variable that measures whether or not any student enrolled in school  $i$  committed a criminal offence (or received a conviction) on date  $t$ . For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Estimated coefficients and 95% confidence intervals are displayed in the chart. Standard errors are clustered at the school level for inference. Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Table 1. Average Crime in the Calendar Year**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	School in Session	School Not in Session	(2)-(1)	[(2)-(1)] / [(1)/100]	Weekend	(5)-(1)	[(5)-(1)] / [(1)/100]	Winter Break	(8)-(1)	[(8)-(1)] / [(1)/100]	Spring Break	(11)-(1)	[(11)-(1)] / [(1)/100]	Summer Break	(14)-(1)	[(14)-(1)] / [(1)/100]
Panel A. Total Crime																
Risk	11.829	12.291	0.462 (0.189)	3.91%	10.971	-0.858 (0.088)	-7.25%	8.287	-3.542 (0.202)	-29.94%	13.250	1.421 (0.244)	12.01%	10.025	-1.804 (0.142)	-15.25%
Count	26.229	28.821	2.592 (0.613)	9.88%	25.810	-0.419 (0.284)	-1.60%	20.762	-5.467 (0.712)	-20.84%	31.567	5.338 (0.839)	20.35%	22.283	-3.946 (0.440)	-15.04%
Risk of Conviction	5.001	5.422	0.421 (0.126)	8.42%	4.891	-0.110 (0.058)	-2.20%	3.889	-1.112 (0.133)	-22.23%	5.799	0.798 (0.162)	15.96%	4.207	-0.794 (0.092)	-15.88%
Count of Convictions	16.282	18.151	1.869 (0.521)	11.48%	16.388	0.106 (0.245)	0.65%	13.824	-2.458 (0.619)	-15.10%	20.044	3.762 (0.733)	23.10%	13.579	-2.703 (0.378)	-16.60%
Panel B. Property Crime																
Risk	5.676	6.582	0.906 (0.135)	15.96%	5.795	0.119 (0.061)	2.10%	4.187	-1.489 (0.139)	-26.23%	7.198	1.522 (0.181)	26.81%	5.760	0.084 (0.101)	1.48%
Count	10.791	13.519	2.728 (0.405)	25.28%	11.685	0.894 (0.169)	8.28%	8.286	-2.505 (0.376)	-23.21%	14.098	3.307 (0.478)	30.65%	11.124	0.333 (0.275)	3.09%
Risk of Conviction	2.332	2.661	0.329 (0.087)	14.11%	2.366	0.034 (0.040)	1.46%	1.754	-0.578 (0.088)	-24.79%	2.768	0.436 (0.113)	18.70%	2.239	-0.093 (0.066)	-3.99%
Count of Convictions	6.378	7.702	1.324 (0.329)	20.76%	6.770	0.392 (0.142)	6.15%	4.825	-1.553 (0.313)	-24.35%	7.924	1.546 (0.394)	24.24%	6.235	-0.143 (0.236)	-2.24%
Panel C. Violent Crime																
Risk	6.876	6.402	-0.474 (0.139)	-6.89%	5.839	-1.037 (0.067)	-15.08%	4.518	-2.358 (0.147)	-34.29%	6.909	0.033 (0.177)	0.48%	4.829	-2.047 (0.101)	-29.77%
Count	15.438	15.302	-0.136 (0.432)	-0.88%	14.125	-1.313 (0.224)	-8.50%	12.476	-2.962 (0.578)	-19.19%	17.469	2.031 (0.669)	13.15%	11.158	-4.280 (0.318)	-27.72%
Risk of Conviction	2.957	3.071	0.114 (0.094)	3.85%	2.809	-0.148 (0.044)	-5.00%	2.339	-0.618 (0.102)	-20.90%	3.378	0.421 (0.122)	14.24%	2.192	-0.765 (0.066)	-25.87%
Count of Convictions	9.904	10.449	0.545 (0.381)	5.50%	9.618	-0.286 (0.196)	-2.89%	8.999	-0.905 (0.519)	-9.14%	12.120	2.216 (0.606)	22.38%	7.344	-2.560 (0.276)	-25.85%
Observations	5,885,406	509,831			3,208,512			332,446			314,381			977,584		
Dates	2,276	2,121			983			472			332			605		
Schools	3,264	3,264			3,264			3,264			3,264			3,264		

*Notes:* Table shows average crime risk (0/1) and counts throughout the school calendar for the period from the 1<sup>st</sup> August 2010 to the end of December 2019, when our analysis terminates. For each school  $i$ , the risk of crime (or conviction) is a (0/1) dummy variable that measures whether or not any student enrolled in school  $i$  committed a criminal offence (or received a conviction) on date  $t$ . For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . Standard errors clustered at the school level are displayed in parentheses. % Changes are measured as the estimated change deflated by the mean of the variable when school is in session displayed in column (1). Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Table 2. OLS Estimates of the Impact of School Attendance on the Risk (0/1) and Count of Crimes**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Risk		Risk of Conviction		Count		Count of Convictions			
Panel A. Total Crime										
School Off	0.430 (0.347)	0.373 (0.338)	0.168 (0.385)	0.331 (0.226)	0.260 (0.254)	2.547 (1.147)	2.494 (1.137)	2.457 (1.299)	1.528 (0.881)	1.576 (0.997)
School Off x Schools Off in LEA			0.050 (0.035)		-0.003 (0.023)			-0.019 (0.108)		-0.046 (0.092)
% Effect	3.64%	3.15%	1.42%	6.62%	5.20%	9.71%	9.51%	9.37%	9.39%	9.68%
Panel B. Property Crime										
School Off	0.716 (0.258)	0.692 (0.255)	0.646 (0.292)	0.262 (0.162)	0.261 (0.182)	2.570 (0.805)	2.570 (0.803)	2.774 (0.949)	0.958 (0.524)	1.215 (0.614)
School Off x Schools Off in LEA					-0.011 (0.014)			-0.063 (0.071)		-0.064 (0.061)
% Effect	12.62%	12.19%	11.38%	11.24%	11.19%	23.82%	23.82%	25.71%	15.02%	19.05%
Panel C. Indictable Property Crime (or Triable Either Way)										
School Off	0.590 (0.220)	0.580 (0.218)	0.595 (0.251)	0.207 (0.135)	0.217 (0.154)	2.160 (0.718)	2.172 (0.716)	2.380 (0.851)	0.668 (0.420)	0.841 (0.501)
School Off x Schools Off in LEA			-0.003 (0.020)		-0.013 (0.013)			-0.044 (0.064)		-0.060 (0.058)
% Effect	14.88%	14.64%	15.01%	12.37%	12.96%	27.89%	28.04%	30.73%	14.28%	17.98%
Panel D. Summary Property Crime										
School Off	0.172 (0.136)	0.156 (0.136)	0.080 (0.154)	0.065 (0.089)	0.063 (0.098)	0.411 (0.306)	0.398 (0.306)	0.394 (0.342)	0.289 (0.275)	0.374 (0.303)
School Off x Schools Off in LEA			0.006 (0.012)		0.001 (0.006)			-0.019 (0.031)		-0.005 (0.018)
% Effect	9.39%	8.52%	4.37%	9.27%	8.99%	13.50%	13.07%	12.94%	17.00%	22.00%
Panel E. Violent Crime										
School Off	-0.386 (0.248)	-0.424 (0.245)	-0.543 (0.277)	0.104 (0.169)	0.036 (0.187)	-0.023 (0.763)	-0.076 (0.758)	-0.318 (0.845)	0.570 (0.681)	0.361 (0.753)
School Off x Schools Off in LEA			0.043 (0.026)		0.008 (0.018)			0.044 (0.078)		0.018 (0.066)
% Effect	-5.61%	-6.17%	-7.90%	3.52%	1.22%	-0.15%	-0.49%	-2.06%	5.76%	3.65%
Panel F. Indictable Violent Crime										
School Off	0.281 (0.161)	0.258 (0.160)	0.262 (0.180)	0.266 (0.119)	0.324 (0.132)	1.125 (0.573)	1.068 (0.571)	1.235 (0.640)	1.017 (0.533)	1.172 (0.595)
School Off x Schools Off in LEA			-0.005 (0.017)		-0.012 (0.013)			-0.034 (0.064)		-0.030 (0.056)
% Effect	10.07%	9.24%	9.39%	20.43%	24.89%	15.94%	15.13%	17.50%	20.53%	23.66%
Panel G. Summary Violent Crime										
School Off	-0.655 (0.198)	-0.668 (0.196)	-0.786 (0.216)	-0.213 (0.124)	-0.323 (0.138)	-1.148 (0.473)	-1.143 (0.469)	-1.552 (0.514)	-0.446 (0.407)	-0.811 (0.447)
School Off x Schools Off in LEA			0.047 (0.019)		0.018 (0.011)			0.079 (0.044)		0.048 (0.035)
% Effect	-14.83%	-15.13%	-17.8%	-12.21%	-18.51%	-13.70%	-13.64%	-18.52%	-9.01%	-16.38%
Date FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
School FE	N	Y	Y	Y	Y	N	Y	Y	Y	Y
Schools Off in LEA	N	N	Y	N	Y	N	N	Y	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
Schools	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays Ordinary Least Squares (OLS) estimates of the impact of school closures on crime. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(3) show results for the extensive margin of crime, i.e., for the risk (0/1) of crime, while columns (4) and (5) show results for the risk (0/1) of crime with a conviction in court. Columns (6)-(8) show results for the extensive and intensive margin of crime, i.e., for the count of crimes, while columns (9) and (10) show results for the count of crimes with a conviction in court. For each school  $i$ , the risk of crime (or conviction) is a (0/1) dummy variable that measures whether or not any student enrolled in school  $i$  committed a criminal offence (or received a conviction) on date  $t$ . For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Standard errors clustered at the school level are displayed in parentheses. For each estimated coefficient  $\beta$ , % effects are calculated as  $\beta$  deflated by the mean of the dependent variable when school is in session. Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Table 3. Estimates of the Impact of School Attendance on Count of Permanent Exclusions and Crimes**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ordinary Least Squares				Poisson Pseudo-Maximum Likelihood			
	Permanent Exclusion			Permanent Exclusion + Crime				
	Panel A. Total Exclusions & Total Crime							
School Off	-2.691	-2.694	-0.144	-0.200	-0.465	-0.005	-0.014	-0.028
	(0.138)	(0.139)	(1.157)	(1.146)	(1.308)	(0.048)	(0.047)	(0.051)
School Off x School Off in LEA					0.044			0.003
					(0.109)			(0.005)
% Effect	-97.11%	-97.22%	-0.50%	-0.69%	-1.58%	-0.50%	-1.39%	-2.76%
Mean of Var. when School is in Session	2.771	2.771	29.387	29.387	29.387	29.387	29.387	29.387
	Panel B. Property Exclusions & Property Crime							
School Off	-0.104	-0.105	2.466	2.465	2.660	0.256	0.243	0.252
	(0.027)	(0.027)	(0.806)	(0.803)	(0.950)	(0.080)	(0.078)	(0.085)
School Off x School Off in LEA					-0.062			-0.005
					(0.071)			(0.007)
% Effect	-68.42%	-69.08%	22.54%	22.53%	24.31%	29.17%	27.51%	28.66%
Mean of Var. when School is in Session	0.152	0.152	10.941	10.941	10.941	10.941	10.941	10.941
	Panel C. Violent Exclusions & Violent Crime							
School Off	-2.587	-2.590	-2.610	-2.665	-3.125	-0.178	-0.185	-0.213
	(0.134)	(0.135)	(0.777)	(0.772)	(0.860)	(0.056)	(0.055)	(0.060)
School Off x School Off in LEA					0.105			0.008
					(0.079)			(0.007)
% Effect	-98.78%	-98.89%	-14.45%	-14.76%	-17.31%	-16.31%	-16.89%	-19.18%
Mean of Var. when School is in Session	2.619	2.619	18.062	18.062	18.062	18.062	18.062	18.062
Date FE	Y	Y	Y	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	N	Y	N	Y	Y	N	Y	Y
Schools Off in LEA	N	N	N	N	Y	N	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
Schools	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays estimates of the impact of school closures on the count of permanent exclusions from school and criminal offences. Columns (1)-(5) show Ordinary Least Squares (OLS) estimates while columns (6)-(8) show Poisson Pseudo-Maximum Likelihood (PPML) Estimates. Total exclusions are defined as the sum of permanent exclusions for property misconduct and for violent misconduct. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(2) show results for the count of permanent school exclusions while columns (3)-(8) show results for the sum of the count of permanent school exclusions and the count of crimes. Panel A focuses on permanent exclusion for total misconduct and total crime, Panel B focuses on permanent exclusion for property misconduct and property crime, and Panel C focuses on permanent exclusion for violent misconduct and violent crime. For each school  $i$ , the *count* of criminal offences (or exclusions) measures the count of offences committed (or exclusions received) on date  $t$  by the students enrolled in school  $i$ . Standard errors clustered at the school level are displayed in parentheses. School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. In columns (1)-(5) that display OLS estimates and for each estimated coefficient  $\beta$ , % effects are calculated as  $\beta$  deflated by the mean of the dependent variable when school is in session. In columns (6)-(8) that display PPML estimates and for each estimated coefficient  $\beta$ , % effects are calculated as:  $\% Effect = (\exp(\beta) - 1) * 100$ . Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Table 4. Estimates of the Impact of School Attendance on Count of Crimes by Category**

	(1)	(2)	(3)	(4)	(5)	(6)
	Ordinary Least Squares			Poisson Pseudo-Maximum Likelihood		
Panel A. Theft						
School Off	1.605 (0.632)	1.620 (0.630)	1.652 (0.744)	0.330 (0.123)	0.321 (0.121)	0.314 (0.131)
School Off x Schools Off in LEA			0.014 (0.042)			0.005 (0.010)
% Effect	29.29%	29.57%	30.15%	39.10%	37.85%	36.89%
Mean Dep. Var When School is in Session	5.480	5.480	5.480	5.480	5.480	5.480
Panel B. Criminal Damage and Arson						
School Off	0.028 (0.127)	0.033 (0.127)	0.149 (0.132)	0.067 (0.264)	0.073 (0.259)	0.231 (0.236)
School Off x Schools Off in LEA			-0.035 (0.029)			-0.033 (0.013)
% Effect	4.67%	5.50%	24.83%	6.93%	7.57%	25.99%
Mean Dep. Var When School is in Session	0.600	0.600	0.600	0.600	0.600	0.600
Panel C. Burglary						
School Off	0.320 (0.243)	0.315 (0.243)	0.377 (0.299)	0.229 (0.170)	0.203 (0.164)	0.228 (0.187)
School Off x Schools Off in LEA			-0.020 (0.036)			-0.011 (0.022)
% Effect	21.11%	20.78%	24.87%	25.73%	22.51%	25.61%
Mean Dep. Var When School is in Session	1.516	1.516	1.516	1.516	1.516	1.516
Panel D. Robbery						
School Off	1.352 (0.366)	1.316 (0.363)	1.463 (0.427)	0.528 (0.125)	0.505 (0.137)	0.497 (0.145)
School Off x Schools Off in LEA			-0.038 (0.037)			-0.014 (0.026)
% Effect	54.69%	53.24%	59.18%	69.55%	65.70%	64.38%
Mean Dep. Var When School is in Session	2.472	2.472	2.472	2.472	2.472	2.472
Panel E. Violence Against a Person						
School Off	-0.978 (0.488)	-0.991 (0.485)	-1.250 (0.528)	-0.138 (0.071)	-0.141 (0.070)	-0.174 (0.074)
School Off x Schools Off in LEA			0.066 (0.045)			0.010 (0.008)
% Effect	-10.64%	-10.78%	-13.60%	-12.89%	-13.15%	-15.97%
Mean Dep. Var When School is in Session	9.192	9.192	9.192	9.192	9.192	9.192
Panel F. Sexual Offences						
School Off	-0.263 (0.368)	-0.277 (0.367)	-0.265 (0.395)	-0.223 (0.278)	-0.237 (0.264)	-0.226 (0.277)
School Off x Schools Off in LEA			0.004 (0.045)			-0.002 (0.025)
% Effect	-18.40%	-19.38%	-18.54%	-19.99%	-21.10%	-20.23%
Mean Dep. Var When School is in Session	1.429	1.429	1.429	1.429	1.429	1.429
Date FE	Y	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y	Y
School FE	N	Y	Y	N	Y	Y
Schools Off in LEA	N	N	Y	N	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
Schools	3,264	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays estimates of the impact of school closures on crime by category. Columns (1)-(3) show Ordinary Least Squares (OLS) estimates while columns (4)-(6) show Poisson Pseudo-Maximum Likelihood (PPML) Estimates. Panels A, B and C focus on the crime categories that are comprised in our definition of property crime. Panels D, E and F focus on the crime categories that are comprised in our definition of violent crime. Standard errors clustered at the school level are displayed in parentheses. For each school  $i$ , the *count* of criminal offences measures the count of offences committed on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. In columns (1)-(3) that display OLS estimates and for each estimated coefficient  $\beta$ , % effects are calculated as  $\beta$  deflated by the mean of the dependent variable when school is in session. In columns (4)-(6) that display PPML estimates and for each estimated coefficient  $\beta$ , % effects are calculated as:  $\% Effect = (\exp(\beta) - 1) * 100$ . Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

## **SUPPLEMENTAL APPENDIX**



**Appendix Table A.1. Average Indictable and Summary Crime in the Calendar Year by Crime Category**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	School in Session	School Not in Session	(2)-(1)	[(2)-(1)]/ [(1)/100]	Weekend	(5)-(1)	[(5)-(1)]/ [(1)/100]	Winter Break	(8)-(1)	[(8)-(1)]/ [(1)/100]	Spring Break	(11)-(1)	[(11)-(1)]/ [(1)/100]	Summer Break	(14)-(1)	[(14)-(1)]/ [(1)/100]
Panel A. Indictable Property Crime (or Triable Either Way)																
Risk	3.963	4.900	0.937 (0.116)	23.64%	4.085	0.122 (0.051)	3.08%	2.922	-1.041 (0.115)	-26.27%	5.219	1.256 (0.152)	31.69%	4.352	0.389 (0.087)	9.82%
Count	7.746	10.337	2.591 (0.351)	33.45%	8.284	0.538 (0.137)	6.95%	5.814	-1.932 (0.306)	-24.94%	10.444	2.698 (0.407)	34.83%	8.477	0.731 (0.224)	9.44%
Risk of Conviction	1.674	2.040	0.366 (0.076)	21.86%	1.703	0.029 (0.033)	1.73%	1.216	-0.458 (0.071)	-27.36%	2.088	0.414 (0.097)	24.73%	1.723	0.049 (0.056)	2.93%
Count of Convictions	4.678	5.924	1.246 (0.283)	26.64%	4.852	0.174 (0.116)	3.72%	3.469	-1.209 (0.263)	-25.84%	6.000	1.322 (0.336)	28.26%	4.779	0.101 (0.187)	2.16%
Panel B. Summary Property Crime																
Risk	1.831	1.858	0.027 (0.071)	1.47%	1.858	0.027 (0.034)	1.47%	1.357	-0.474 (0.076)	-25.89%	2.206	0.375 (0.101)	20.48%	1.540	-0.291 (0.050)	-15.89%
Count	3.045	3.181	0.136 (0.162)	4.47%	3.401	0.356 (0.091)	11.69%	2.471	-0.574 (0.185)	-18.85%	3.654	0.609 (0.220)	20.00%	2.648	-0.397 (0.144)	-13.04%
Risk of Conviction	0.701	0.685	-0.016 (0.045)	-2.28%	0.711	0.010 (0.021)	1.43%	0.573	-0.128 (0.049)	-18.26%	0.745	0.044 (0.056)	6.28%	0.564	-0.137 (0.031)	-19.54%
Count of Convictions	1.700	1.778	0.078 (0.139)	4.59%	1.918	0.218 (0.077)	12.82%	1.356	-0.344 (0.145)	-20.24%	1.924	0.224 (0.185)	13.18%	1.455	-0.245 (0.133)	-14.41%
Panel C. Indictable Violent Crime																
Risk	2.791	2.837	0.046 (0.090)	1.65%	2.514	-0.277 (0.042)	-9.92%	2.307	-0.484 (0.101)	-17.34%	2.986	0.195 (0.117)	6.99%	2.078	-0.713 (0.063)	-25.55%
Count	7.058	7.741	0.683 (0.337)	9.68%	6.947	-0.111 (0.171)	-1.57%	7.667	0.609 (0.481)	8.63%	8.324	1.266 (0.521)	17.94%	5.431	-1.627 (0.242)	-23.05%
Risk of Conviction	1.302	1.503	0.201 (0.067)	15.44%	1.362	0.060 (0.030)	4.61%	1.289	-0.013 (0.075)	-1.00%	1.512	0.210 (0.082)	16.13%	1.033	-0.269 (0.045)	-20.66%
Count of Convictions	4.953	5.775	0.822 (0.308)	16.60%	5.186	0.233 (0.153)	4.70%	5.826	0.873 (0.439)	17.63%	6.178	1.225 (0.480)	24.73%	3.899	-1.054 (0.217)	-21.28%
Panel D. Summary Violent Crime																
Risk	4.416	3.942	-0.474 (0.105)	-10.73%	3.621	-0.795 (0.052)	-18.00%	2.406	-2.010 (0.110)	-45.51%	4.304	-0.112 (0.141)	-2.54%	3.020	-1.396 (0.076)	-31.61%
Count	8.380	7.561	-0.819 (0.249)	-9.77%	7.178	-1.202 (0.135)	-14.34%	4.809	-3.571 (0.290)	-42.61%	9.145	0.765 (0.399)	9.13%	5.728	-2.652 (0.182)	-31.65%
Risk of Conviction	1.745	1.673	-0.072 (0.068)	-4.13%	1.531	-0.214 (0.032)	-12.26%	1.109	-0.636 (0.069)	-36.45%	1.958	0.213 (0.095)	12.20%	1.210	-0.535 (0.047)	-30.66%
Count of Convictions	4.952	4.674	-0.278 (0.214)	-5.61%	4.432	-0.520 (0.117)	-10.50%	3.173	-1.779 (0.258)	-35.92%	5.942	0.990 (0.362)	19.99%	3.445	-1.507 (0.159)	-30.43%
Observations	5,885,406	509,831			3,208,512			332,446			314,381			977,584		
Dates	2,276	2,121			983			472			332			605		
Schools	3,264	3,264			3,264			3,264			3,264			3,264		

*Notes:* Table shows average crime risk (0/1) and counts by category throughout the school calendar for the period from the 1<sup>st</sup> August 2010 to the end of December 2019, when our analysis terminates. For each school  $i$ , the risk of crime (or conviction) is a (0/1) dummy variable that measures whether or not any student enrolled in school  $i$  committed a criminal offence (or received a conviction) on date  $t$ . For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . Standard errors in parentheses are clustered at the school level. % Changes are measured as the estimated change deflated by the mean of the variable when school is in session displayed in column (1). Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Appendix Table A.2. Probit Estimates of the Impact of School Attendance on the Risk of Crime (0/1)**

	(1)	(2)	(3)	(4)	(5)
		Risk		Risk of Conviction	
Panel A. Total Crime					
School Off	0.017	0.015	0.008	0.028	0.021
	(0.014)	(0.014)	(0.015)	(0.019)	(0.020)
School Off x Schools Off in LEA			0.002		0.000
			(0.002)		(0.002)
% Effect	4.53%	3.63%	1.86%	7.86%	5.76%
Panel B. Property Crime					
School Off	0.053	0.052	0.050	0.045	0.044
	(0.019)	(0.019)	(0.020)	(0.026)	(0.028)
School Off x Schools Off in LEA			0.000		-0.001
			(0.002)		(0.002)
% Effect	15.47%	14.42%	14.08%	13.72%	13.26%
Panel C. Indicatable Property Crime (or Triable Either Way)					
School Off	0.060	0.061	0.062	0.049	0.049
	(0.022)	(0.022)	(0.024)	(0.031)	(0.033)
School Off x Schools Off in LEA			0.000		0.000
			(0.002)		(0.002)
% Effect	18.54%	17.79%	18.25%	15.42%	15.29%
Panel D. Summary Property Crime					
School Off	0.034	0.029	0.018	0.031	0.029
	(0.027)	(0.027)	(0.030)	(0.043)	(0.046)
School Off x Schools Off in LEA			0.001		0.001
			(0.003)		(0.004)
% Effect	11.26%	9.03%	5.59%	10.00%	9.36%
Panel E. Violent Crime					
School Off	-0.024	-0.028	-0.036	0.014	0.003
	(0.016)	(0.016)	(0.018)	(0.022)	(0.024)
School Off x Schools Off in LEA			0.003		0.002
			(0.002)		(0.003)
% Effect	-6.58%	-7.27%	-9.25%	4.09%	0.76%
Panel F. Indictable Violent Crime					
School Off	0.038	0.036	0.034	0.072	0.078
	(0.021)	(0.022)	(0.024)	(0.031)	(0.032)
School Off x Schools Off in LEA			0.000		-0.003
			(0.003)		(0.004)
% Effect	12.20%	10.83%	10.19%	23.90%	26.13%
Panel G. Summary Violent Crime					
School Off	-0.065	-0.069	-0.081	-0.048	-0.072
	(0.020)	(0.020)	(0.022)	(0.029)	(0.032)
School Off x Schools Off in LEA			0.006		0.005
			(0.003)		(0.004)
% Effect	-17.41%	-17.75%	-20.53%	-13.73%	-19.75%
Date FE	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y
School FE	N	Y	Y	Y	Y
Schools Off in LEA	N	N	Y	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
School	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays Probit estimates of the impact of school closures on crime. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(3) show results for the extensive margin of crime, i.e., for the count of crimes, while columns (4) and (5) show results for the extensive margin of crime with a conviction in court. For each school  $i$ , the risk of crime (or conviction) is a (0/1) dummy variable that measures whether or not any student enrolled in school  $i$  committed a criminal offence (or received a conviction) on date  $t$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Standard errors clustered at the school level are displayed in parentheses. For each estimated coefficient  $\beta$ , % effects are calculated as the marginal effect from the Probit estimates. Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Appendix Table A.3. PPML Estimates of the Impact of School Attendance on the Count of Crimes**

	(1)	(2)	(3)	(4)	(5)
		Count		Count of Convictions	
Panel A. Total Crime					
School Off	0.113 (0.050)	0.103 (0.049)	0.094 (0.053)	0.099 (0.060)	0.091 (0.064)
School Off x Schools Off in LEA			0.001 (0.005)		-0.002 (0.007)
% Effect	11.96%	10.85%	9.86%	10.41%	9.53%
Panel B. Property Crime					
School Off	0.269 (0.080)	0.256 (0.078)	0.266 (0.085)	0.160 (0.089)	0.182 (0.095)
School Off x Schools Off in LEA			-0.005 (0.007)		-0.008 (0.009)
% Effect	30.87%	29.18%	30.47%	17.35%	19.96%
Panel C. Indicatable Property Crime (or Triable Either Way)					
School Off	0.312 (0.098)	0.300 (0.096)	0.311 (0.104)	0.152 (0.098)	0.169 (0.106)
School Off x Schools Off in LEA			-0.004 (0.008)		-0.010 (0.010)
% Effect	36.62%	34.99%	36.48%	16.42%	18.41%
Panel D. Summary Property Crime					
School Off	0.159 (0.114)	0.141 (0.110)	0.141 (0.116)	0.184 (0.172)	0.221 (0.177)
School Off x Schools Off in LEA			-0.006 (0.010)		0.000 (0.014)
% Effect	17.23%	15.14%	15.14%	20.20%	24.73%
Panel E. Violent Crime					
School Off	-0.001 (0.058)	-0.009 (0.058)	-0.031 (0.062)	0.061 (0.077)	0.031 (0.082)
School Off x Schools Off in LEA			0.005 (0.007)		0.004 (0.010)
% Effect	-0.10%	-0.90%	-3.05%	6.29%	3.15%
Panel F. Indictable Violent Crime					
School Off	0.172 (0.084)	0.160 (0.085)	0.166 (0.090)	0.207 (0.109)	0.211 (0.115)
School Off x Schools Off in LEA			-0.005 (0.012)		-0.005 (0.015)
% Effect	18.77%	17.35%	18.06%	23.00%	23.49%
Panel G. Summary Violent Crime					
School Off	-0.173 (0.074)	-0.173 (0.073)	-0.229 (0.078)	-0.117 (0.103)	-0.196 (0.112)
School Off x Schools Off in LEA			0.014 (0.009)		0.014 (0.012)
% Effect	-15.89%	-15.89%	-20.47%	-11.04%	-17.80%
Date FE	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y
School FE	N	Y	Y	Y	Y
Schools Off in LEA	N	N	Y	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
School	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays Poisson Pseudo-Maximum Likelihood (PPML) estimates of the impact of school closures on crime. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(3) show results for the extensive and intensive margin of crime, i.e., for the count of crimes, while columns (4) and (5) show results for the count of crimes with a conviction in court. For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Standard errors clustered at the school level are displayed in parentheses. For each estimated coefficient  $\beta$ , % effects are calculated as:  $\% Effect = (\exp(\beta) - 1) * 100$ . Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 1000 for easier readability.

**Appendix Table A.4 OLS Estimates of the Impact of School Attendance on the Count of Crimes per Student**

	(1)	(2)	(3)	(4)	(5)
		Count		Count of Convictions	
Panel A. Total Crime					
School Off	0.025 (0.012)	0.027 (0.012)	0.026 (0.013)	0.018 (0.009)	0.018 (0.010)
School Off x Schools Off in LEA			0.000 (0.001)		-0.001 (0.001)
% Effect	9.09%	9.82%	9.46%	10.53%	10.53%
Panel B. Property Crime					
School Off	0.025 (0.008)	0.026 (0.008)	0.028 (0.010)	0.010 (0.005)	0.013 (0.006)
School Off x Schools Off in LEA			-0.001 (0.001)		-0.001 (0.001)
% Effect	22.12%	23.01%	24.78%	14.93%	19.40%
Panel C. Indictable Property Crime (or Triable Either Way)					
School Off	0.022 (0.007)	0.023 (0.007)	0.025 (0.009)	0.008 (0.004)	0.010 (0.005)
School Off x Schools Off in LEA			-0.001 (0.001)		-0.001 (0.001)
% Effect	27.16%	28.40%	30.86%	16.33%	20.41%
Panel D. Summary Property Crime					
School Off	0.003 (0.003)	0.003 (0.003)	0.003 (0.003)	0.002 (0.003)	0.003 (0.003)
School Off x Schools Off in LEA			0.000 (0.000)		0.000 (0.000)
% Effect	9.38%	9.38%	9.38%	11.11%	16.67%
Panel E. Violent Crime					
School Off	0.000 (0.008)	0.001 (0.008)	-0.002 (0.009)	0.007 (0.007)	0.005 (0.008)
School Off x Schools Off in LEA			0.001 (0.001)		0.000 (0.001)
% Effect	0.00%	0.62%	-1.24%	6.73%	4.81%
Panel F. Indictable Violent Crime					
School Off	0.012 (0.006)	0.012 (0.006)	0.014 (0.007)	0.011 (0.006)	0.013 (0.006)
School Off x Schools Off in LEA			0.000 (0.001)		0.000 (0.001)
% Effect	16.22%	16.22%	18.92%	21.15%	25.00%
Panel G. Summary Violent Crime					
School Off	-0.012 (0.005)	-0.011 (0.005)	-0.016 (0.005)	-0.004 (0.004)	-0.008 (0.005)
School Off x Schools Off in LEA			0.001 (0.000)		0.000 (0.000)
% Effect	-13.64%	-12.50%	-18.18%	-7.69%	-15.39%
Date FE	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y
School FE	N	Y	Y	Y	Y
Schools Off in LEA	N	N	Y	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
Schools	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays Ordinary Least Squares (OLS) estimates of the impact of school closures on crime deflated by the count of students enrolled in each school. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(3) show results for the extensive and intensive margin of crime, i.e., for the count of crimes, while columns (4) and (5) show results for the count of crimes with a conviction in court. For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Standard errors clustered at the school level are displayed in parentheses. For each estimated coefficient  $\beta$ , % effects are calculated as  $\beta$  deflated by the mean of the dependent variable when school is in session. Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 10000 for easier readability.

**Appendix Table A.5. PPML Estimates of the Impact of School Attendance on the Count of Crimes per Student**

	(1)	(2)	(3)	(4)	(5)
		Count		Count of Convictions	
Panel A. Total Crime					
School Off	0.105 (0.048)	0.110 (0.048)	0.102 (0.052)	0.111 (0.059)	0.105 (0.062)
School Off x Schools Off in LEA			0.000 (0.005)		-0.003 (0.007)
% Effect	11.07%	11.63%	10.74%	11.74%	11.07%
Panel B. Property Crime					
School Off	0.248 (0.079)	0.254 (0.078)	0.265 (0.085)	0.167 (0.083)	0.195 (0.089)
School Off x Schools Off in LEA			-0.006 (0.007)		-0.011 (0.009)
% Effect	28.15%	28.92%	30.34%	18.18%	21.53%
Panel C. Indictable Property Crime (or Triable Either Way)					
School Off	0.303 (0.098)	0.312 (0.097)	0.323 (0.105)	0.181 (0.095)	0.200 (0.102)
School Off x Schools Off in LEA			-0.007 (0.009)		-0.013 (0.011)
% Effect	35.39%	36.62%	38.13%	19.84%	22.14%
Panel D. Summary Property Crime					
School Off	0.103 (0.102)	0.102 (0.099)	0.108 (0.106)	0.127 (0.157)	0.193 (0.161)
School Off x Schools Off in LEA			-0.003 (0.009)		-0.002 (0.013)
% Effect	10.85%	10.74%	11.40%	13.54%	21.29%
Panel E. Violent Crime					
School Off	0.002 (0.057)	0.007 (0.057)	-0.016 (0.061)	0.076 (0.076)	0.045 (0.081)
School Off x Schools Off in LEA			0.005 (0.007)		0.004 (0.010)
% Effect	0.20%	0.70%	-1.59%	7.90%	4.60%
Panel F. Indictable Violent Crime					
School Off	0.173 (0.084)	0.174 (0.085)	0.182 (0.089)	0.223 (0.109)	0.229 (0.113)
School Off x Schools Off in LEA			-0.004 (0.011)		-0.004 (0.015)
% Effect	18.89%	19.01%	19.96%	24.98%	25.73%
Panel G. Summary Violent Crime					
School Off	-0.169 (0.072)	-0.157 (0.071)	-0.217 (0.077)	-0.104 (0.101)	-0.188 (0.110)
School Off x Schools Off in LEA			0.013 (0.008)		0.012 (0.011)
% Effect	-15.55%	-14.53%	-19.51%	-9.88%	-17.14%
Date FE	Y	Y	Y	Y	Y
School Breaks FE	Y	Y	Y	Y	Y
School FE	N	Y	Y	Y	Y
Schools Off in LEA	N	N	Y	N	Y
Observations	11,228,160	11,228,160	11,228,160	11,228,160	11,228,160
Schools	3,264	3,264	3,264	3,264	3,264

*Notes:* Table displays Poisson Pseudo-Maximum Likelihood (PPML) estimates of the impact of school closures on crime deflated by the count of students enrolled in each school. Total crime is defined as the sum of property crime and violent crime. Columns (1)-(3) show results for the extensive and intensive margin of crime, i.e., for the count of crimes, while columns (4) and (5) show results for the count of crimes with a conviction in court. For each school  $i$ , the *count* of criminal offences (or convictions) measures the count of offences committed (or convictions received) on date  $t$  by the students enrolled in school  $i$ . School Breaks Fixed Effects (FEs) include Winter Break FEs, Spring Break FEs, Summer Break FEs. Standard errors clustered at the school level are displayed in parentheses. For each estimated coefficient  $\beta$ , % effects are calculated as:  $\% Effect = (\exp(\beta) - 1) * 100$ . Estimates are weighted by the count of students enrolled in each school year in school. All our displayed averages and estimated coefficients are multiplied by 10000 for easier readability.



1. L. Colombo, H. Dawid, *Strategic Location Choice under Dynamic Oligopolistic Competition and Spillovers*, novembre 2013.
2. M. Bordignon, M. Gamalerio, G. Turati, *Decentralization, Vertical Fiscal Imbalance, and Political Selection*, novembre 2013.
3. M. Guerini, *Is the Friedman Rule Stabilizing? Some Unpleasant Results in a Heterogeneous Expectations Framework*, novembre 2013.
4. E. Brenna, C. Di Novi, *Is caring for elderly parents detrimental to women's mental health? The influence of the European North-South gradient*, novembre 2013.
5. F. Sobbrío, *Citizen-Editors' Endogenous Information Acquisition and News Accuracy*, novembre 2013.
6. P. Bingley, L. Cappellari, *Correlation of Brothers Earnings and Intergenerational Transmission*, novembre 2013.
7. T. Assenza, W. A. Brock, C. H. Hommes, *Animal Spirits, Heterogeneous Expectations and the Emergence of Booms and Busts*, dicembre 2013.
8. D. Parisi, *Is There Room for 'Fear' as a Human Passion in the Work by Adam Smith?*, gennaio 2014.
9. E. Brenna, F. Spandonaro, *Does federalism induce patients' mobility across regions? Evidence from the Italian experience*, febbraio 2014.
10. A. Monticini, F. Ravazzolo, *Forecasting the intraday market price of money*, febbraio 2014.
11. Tiziana Assenza, Jakob Grazzini, Cars Hommes, Domenico Massaro, *PQ Strategies in Monopolistic Competition: Some Insights from the Lab*, marzo 2014.
12. R. Davidson, A. Monticini, *Heteroskedasticity-and-Autocorrelation-Consistent Bootstrapping*, marzo 2014.
13. C. Lucifora, S. Moriconi, *Policy Myopia and Labour Market Institutions*, giugno 2014.
14. N. Pecora, A. Spelta, *Shareholding Network in the Euro Area Banking Market*, giugno 2014.
15. G. Mazzolini, *The economic consequences of accidents at work*, giugno 2014.
16. M. Ambrosanio, P. Balduzzi, M. Bordignon, *Economic crisis and fiscal federalism in Italy*, settembre 2014.
17. P. Bingley, L. Cappellari, K. Tatsiramos, *Family, Community and Long-Term Earnings Inequality*, ottobre 2014.
18. S. Frazzoni, M. L. Mancusi, Z. Rotondi, M. Sobrero, A. Vezzulli, *Innovation and export in SMEs: the role of relationship banking*, novembre 2014.
19. H. Gnutzmann, *Price Discrimination in Asymmetric Industries: Implications for Competition and Welfare*, novembre 2014.
20. A. Baglioni, A. Boitani, M. Bordignon, *Labor mobility and fiscal policy in a currency union*, novembre 2014.
21. C. Nielsen, *Rational Overconfidence and Social Security*, dicembre 2014.
22. M. Kurz, M. Motolese, G. Piccillo, H. Wu, *Monetary Policy with Diverse Private Expectations*, febbraio 2015.
23. S. Piccolo, P. Tedeschi, G. Ursino, *How Limiting Deceptive Practices Harms Consumers*, maggio 2015.
24. A.K.S. Chand, S. Currarini, G. Ursino, *Cheap Talk with Correlated Signals*, maggio 2015.
25. S. Piccolo, P. Tedeschi, G. Ursino, *Deceptive Advertising with Rational Buyers*, giugno 2015.

26. S. Piccolo, E. Tarantino, G. Ursino, *The Value of Transparency in Multidivisional Firms*, giugno 2015.
27. G. Ursino, *Supply Chain Control: a Theory of Vertical Integration*, giugno 2015.
28. I. Aldasoro, D. Delli Gatti, E. Faia, *Bank Networks: Contagion, Systemic Risk and Prudential Policy*, luglio 2015.
29. S. Moriconi, G. Peri, *Country-Specific Preferences and Employment Rates in Europe*, settembre 2015.
30. R. Crinò, L. Ogliari, *Financial Frictions, Product Quality, and International Trade*, settembre 2015.
31. J. Grazzini, A. Spelta, *An empirical analysis of the global input-output network and its evolution*, ottobre 2015.
32. L. Cappellari, A. Di Paolo, *Bilingual Schooling and Earnings: Evidence from a Language-in-Education Reform*, novembre 2015.
33. A. Litina, S. Moriconi, S. Zanjaj, *The Cultural Transmission of Environmental Preferences: Evidence from International Migration*, novembre 2015.
34. S. Moriconi, P. M. Picard, S. Zanjaj, *Commodity Taxation and Regulatory Competition*, novembre 2015.
35. M. Bordignon, V. Grembi, S. Piazza, *Who do you blame in local finance? An analysis of municipal financing in Italy*, dicembre 2015.
36. A. Spelta, *A unified view of systemic risk: detecting SIFIs and forecasting the financial cycle via EWSs*, gennaio 2016.
37. N. Pecora, A. Spelta, *Discovering SIFIs in interbank communities*, febbraio 2016.
38. M. Botta, L. Colombo, *Macroeconomic and Institutional Determinants of Capital Structure Decisions*, aprile 2016.
39. A. Gamba, G. Immordino, S. Piccolo, *Organized Crime and the Bright Side of Subversion of Law*, maggio 2016.
40. L. Corno, N. Hildebrandt, A. Voena, *Weather Shocks, Age of Marriage and the Direction of Marriage Payments*, maggio 2016.
41. A. Spelta, *Stock prices prediction via tensor decomposition and links forecast*, maggio 2016.
42. T. Assenza, D. Delli Gatti, J. Grazzini, G. Ricchiuti, *Heterogeneous Firms and International Trade: The role of productivity and financial fragility*, giugno 2016.
43. S. Moriconi, *Taxation, industry integration and production efficiency*, giugno 2016.
44. L. Fiorito, C. Orsi, *Survival Value and a Robust, Practical, Joyless Individualism: Thomas Nixon Carver, Social Justice, and Eugenics*, luglio 2016.
45. E. Cottini, P. Ghinetti, *Employment insecurity and employees' health in Denmark*, settembre 2016.
46. G. Cecere, N. Corrocher, M. L. Mancusi, *Financial constraints and public funding for eco-innovation: Empirical evidence on European SMEs*, settembre 2016.
47. E. Brenna, L. Gitto, *Financing elderly care in Italy and Europe. Is there a common vision?*, settembre 2016.
48. D. G. C. Britto, *Unemployment Insurance and the Duration of Employment: Theory and Evidence from a Regression Kink Design*, settembre 2016.
49. E. Caroli, C. Lucifora, D. Vigani, *Is there a Retirement-Health Care utilization puzzle? Evidence from SHARE data in Europe*, ottobre 2016.
50. G. Femminis, *From simple growth to numerical simulations: A primer in dynamic programming*, ottobre 2016.
51. C. Lucifora, M. Tonello, *Monitoring and sanctioning cheating at school: What works? Evidence from a national evaluation program*, ottobre 2016.



52. A. Baglioni, M. Esposito, *Modigliani-Miller Doesn't Hold in a "Bailinable" World: A New Capital Structure to Reduce the Banks' Funding Cost*, novembre 2016.
53. L. Cappellari, P. Castelnovo, D. Checchi, M. Leonardi, *Skilled or educated? Educational reforms, human capital and earnings*, novembre 2016.
54. D. Britto, S. Fiorin, *Corruption and Legislature Size: Evidence from Brazil*, dicembre 2016.
55. F. Andreoli, E. Peluso, *So close yet so unequal: Reconsidering spatial inequality in U.S. cities*, febbraio 2017.
56. E. Cottini, P. Ghinetti, *Is it the way you live or the job you have? Health effects of lifestyles and working conditions*, marzo 2017.
57. A. Albanese, L. Cappellari, M. Leonardi, *The Effects of Youth Labor Market Reforms: Evidence from Italian Apprenticeships*, maggio 2017.
58. S. Perdichizzi, *Estimating Fiscal multipliers in the Eurozone. A Nonlinear Panel Data Approach*, maggio 2017.
59. S. Perdichizzi, *The impact of ECBs conventional and unconventional monetary policies on European banking indexes returns*, maggio 2017.
60. E. Brenna, *Healthcare tax credits: financial help to taxpayers or support to higher income and better educated patients? Evidence from Italy*, giugno 2017.
61. G. Gokmen, T. Nannicini, M. G. Onorato, C. Papageorgiou, *Policies in Hard Times: Assessing the Impact of Financial Crises on Structural Reforms*, settembre 2017.
62. M. Tettamanzi, *E Many Pluribus Unum: A Behavioural Macro-Economic Agent Based Model*, novembre 2017.
63. A. Boitani, C. Punzo, *Banks' leverage behaviour in a two-agent New Keynesian model*, gennaio 2018.
64. M. Bertoni, G. Brunello, L. Cappellari, *Parents, Siblings and Schoolmates. The Effects of Family-School Interactions on Educational Achievement and Long-term Labor Market Outcomes*, gennaio 2018.
65. G. P. Barbetta, G. Sorrenti, G. Turati, *Multigrading and Child Achievement*, gennaio 2018.
66. S. Gagliarducci, M. G. Onorato, F. Sobbrío, G. Tabellini, *War of the Waves: Radio and Resistance During World War II*, febbraio 2018.
67. P. Bingley, L. Cappellari, *Workers, Firms and Life-Cycle Wage Dynamics*, marzo 2018.
68. A. Boitani, S. Perdichizzi, *Public Expenditure Multipliers in recessions. Evidence from the Eurozone*, marzo 2018.
69. M. Le Moglie, G. Turati, *Electoral Cycle Bias in the Media Coverage of Corruption News*, aprile 2018.
70. R. Davidson, A. Monticini, *Improvements in Bootstrap Inference*, aprile 2018.
71. R. Crinò, G. Immordino, S. Piccolo, *Fighting Mobile Crime*, giugno 2018.
72. R. Caminal, L. Cappellari, A. Di Paolo, *Linguistic skills and the intergenerational transmission of language*, agosto 2018.
73. E. Brenna, L. Gitto, *Adult education, the use of Information and Communication Technologies and the impact on quality of life: a case study*, settembre 2018.
74. M. Bordignon, Y. Deng, J. Huang, J. Yang, *Plunging into the Sea: Ideological Change, Institutional Environments and Private Entrepreneurship in China*, settembre 2018.
75. M. Bordignon, D. Xiang, L. Zhan, *Predicting the Effects of a Sugar Sweetened Beverage Tax in a Household Production Model*, settembre 2018.
76. C. Punzo, L. Rossi, *The Redistributive Effects of a Money-Financed Fiscal Stimulus*, gennaio 2019.
77. A. Baglioni, L. Colombo, P. Rossi, *Debt restructuring with multiple bank relationships*, gennaio 2019.

78. E. Cottini, P. Ghinetti, S. Moriconi, *Higher Education Supply, Neighbourhood effects and Economic Welfare*, febbraio 2019.
79. S. Della Lena, F. Panebianco, *Cultural Transmission with Incomplete Information: Parental Perceived Efficacy and Group Misrepresentation*, marzo 2019.
80. T. Colussi, Ingo E. Isphording, Nico Pestel, *Minority Salience and Political Extremism*, marzo 2019.
81. G. P. Barbetta, P. Canino, S. Cima, *Let's tweet again? The impact of social networks on literature achievement in high school students: Evidence from a randomized controlled trial*, maggio 2019.
82. Y. Brilli, C. Lucifora, A. Russo, M. Tonello, *Vaccination take-up and health: evidence from a flu vaccination program for the elderly*, giugno 2019.
83. C. Di Novi, M. Piacenza, S. Robone, G. Turati, *Does fiscal decentralization affect regional disparities in health? Quasi-experimental evidence from Italy*, luglio 2019.
84. L. Abrardi, L. Colombo, P. Tedeschi, *The Gains of Ignoring Risk: Insurance with Better Informed Principals*, luglio 2019.
85. A. Garnero, C. Lucifora, *Turning a Blind Eye? Compliance to Minimum Wages and Employment*, gennaio 2020.
86. M. Bordignon, M. Gamalerio, E. Slerca, G. Turati, *Stop invasion! The electoral tipping point in anti-immigrant voting*, marzo 2020.
87. D. Vigani, C. Lucifora, *Losing control? Unions' Representativeness, "Pirate" Collective Agreements and Wages*, marzo 2020.
88. S. L. Comi, E. Cottini, C. Lucifora, *The effect of retirement on social relationships: new evidence from SHARE*, maggio 2020.
89. A. Boitani, S. Perdichizzi, C. Punzo, *Nonlinearities and expenditure multipliers in the Eurozone*, giugno 2020.
90. R. A. Ramos, F. Bassi, D. Lang, *Bet against the trend and cash in profits*, ottobre 2020.
91. F. Bassi, *Chronic Excess Capacity and Unemployment Hysteresis in EU Countries. A Structural Approach*, ottobre 2020.
92. M. Bordignon, T. Colussi, *Dancing with the Populist. New Parties, Electoral Rules and Italian Municipal Elections*, ottobre 2020.
93. E. Cottini, C. Lucifora, G. Turati, D. Vigani, *Children Use of Emergency Care: Differences Between Natives and Migrants in Italy*, ottobre 2020.
94. B. Fanfani, *Tastes for Discrimination in Monopsonistic Labour Markets*, ottobre 2020.
95. B. Fanfani, *The Employment Effects of Collective Bargaining*, ottobre 2020.
96. O. Giuntella, J. Lonsky, F. Mazzonna, L. Stella, *Immigration Policy and Immigrants' Sleep. Evidence from DACA*, dicembre 2020.
97. E. Cottini, P. Ghinetti, E. Iossa, P. Sacco, *Stress and Incentives at Work*, gennaio 2021.
98. L. Pieroni, M. R. Roig, L. Salmasi, *Italy: immigration and the evolution of populism*, gennaio 2021.
99. L. Corno, E. La Ferrara, A. Voena, *Female Genital Cutting and the Slave Trade*, febbraio 2021.
100. O. Giuntella, L. Rotunno, L. Stella, *Trade Shocks, Fertility, and Marital Behavior*, marzo 2021.
101. P. Bingley, L. Cappellari, K. Tatsiramos, *Parental Assortative Mating and the Intergenerational Transmission of Human Capital*, aprile 2021.
102. F. Devicienti, B. Fanfani, *Firms' Margins of Adjustment to Wage Growth. The Case of Italian Collective Bargaining*, aprile 2021.
103. C. Lucifora, A. Russo, D. Vigani, *Does prescribing appropriateness reduce health expenditures? Main effects and unintended outcomes*, maggio 2021.

104. T. Colussi, *The Political Effects of Threats to the Nation: Evidence from the Cuban Missile Crisis*, giugno 2021.
105. M. Bordignon, N. Gatti, M. G. Onorato, *Getting closer or falling apart? Euro countries after the Euro crisis*, giugno 2021.
106. E. Battistin, M. Ovidi, *Rising Stars*, giugno 2021.
107. D. Checchi, A. Fenizia, C. Lucifora, *PUBLIC SECTOR JOBS: Working in the public sector in Europe and the US*, giugno 2021.
108. K. Aktas, G. Argentin, G. P. Barbetta, G. Barbieri, L. V. A. Colombo, *High School Choices by Immigrant Students in Italy: Evidence from Administrative Data*, luglio 2021.
109. B. Fanfani, C. Lucifora, D. Vigani, *Employer Association in Italy. Trends and Economic Outcomes*, luglio 2021.
110. F. Bassi, A. Boitani, *Monetary and macroprudential policy: The multiplier effects of cooperation*, settembre 2021.
111. S. Basiglio, A. Foresta, G. Turati, *Impatience and crime. Evidence from the NLSY97*, settembre 2021.
112. A. Baglioni, A. Monticini, D. Peel, *The Impact of the ECB Banking Supervision Announcements on the EU Stock Market*, novembre 2021.
113. E. Facchetti, L. Neri, M. Ovidi, *Should you Meet The Parents? The impact of information on non-test score attributes on school choice*, dicembre 2021.
114. M. Bratti, E. Cottini, P. Ghinetti, *Education, health and health-related behaviors: Evidence from higher education expansion*, febbraio 2022.
115. A. Boitani, C. Dragomirescu-Gaina, *News and narratives: A cointegration analysis of Russian economic policy uncertainty*, aprile 2022.
116. D. Delli Gatti, J. Grazzini, D. Massaro, F. Panebianco, *The Impact of Growth on the Transmission of Patience*, luglio 2022.
117. I. Torrini, C. Lucifora, A. Russo, *The Long-Term Effects of Hospitalization on Health Care Expenditures: An Empirical Analysis for the Young-Old Population*, luglio 2022.
118. T. Colussi, M. Romagnoli, E. Villar, *The Intended and Unintended Consequences of Taxing Waste*, settembre 2022.
119. D. Delli Gatti, G. Iannotta, *Behavioural credit cycles*, settembre 2022.
120. C. Punzo, G. Rivolta, *Money versus debt financed regime: Evidence from an estimated DSGE model*, novembre 2022.
121. M. Ovidi, *Parents Know Better: Sorting on Match Effects in Primary School*, novembre 2022.
122. L. Cappellari, D. Checchi, M. Ovidi, *The effects of schooling on cognitive skills: evidence from education expansions*, dicembre 2022.
123. M. Bertoni, G. Brunello, L. Cappellari, M. De Paola, *The long-run earnings effects of winning a mayoral election*, gennaio 2023.
124. M. Bordignon, F. Franzoni, M. Gamalerio, *Is Populism reversible? Evidence from Italian local elections during the pandemic*, gennaio 2023.
125. L. Colombo, G. Femminis, A. Pavan, *Subsidies to Technology Adoption when Firms' Information is Endogenous*, gennaio 2023.
126. L. Pieroni, M. Rosselló Roig, L. Salmasi, G. Turati, *Legal status and voluntary abortions by immigrants*, gennaio 2023.
127. F. Di Pace, G. Mangiante, R. Masolo, *Do firm expectations respond to Monetary Policy announcements?*, febbraio 2023.
128. R. Masolo, *Heterogeneity and the Equitable Rate of Interest*, febbraio 2023.
129. D. Delli Gatti, R. Terranova, E. M. Turco, *Mind the knowledge gap! The origins of declining business dynamism in a macro agent-based model*, ottobre 2023.

130. R. Davidson, A. Monticini, *Bootstrap Performance with Heteroskedasticity*, novembre 2023.
131. V. Colombo, A. Paccagnini, *Uncertainty and the Federal Reserve's Balance Sheet Monetary Policy*, febbraio 2024.
132. M. Bordignon, T. Colussi, F. Porcelli, *Populists at work. Italian municipal finance under M5s governments*, febbraio 2024.
133. A. Boitani, L. Di Domenico, S. Ricchiuti, *Monetary policy and inequality: an heterogenous agents' approach*, settembre 2024.
134. M. Di Giacomo, M. Piacenza, L. Salmasi, G. Turati, *Understanding productivity in maternity wards*, ottobre 2024.
135. D. Bonciani, R. Masolo, S. Sarpietro, *How Food Prices Shape Inflation Expectations and the Monetary Policy Response*, ottobre 2024.
136. G. Battiston, L. Corno, E. La Ferrara, *Informing Risky Migration: Evidence from a field experiment in Guinea*, dicembre 2024.
137. F. Di Pace, G. Mangiante, R. Masolo, *Monetary Policy Rules: the Market's View*, dicembre 2024.
138. L. Corno, E. La Ferrara, A. Voena, *Female Genital Cutting and the Slave Trade*, maggio 2025.
139. M. Carlana, L. Corno, *Peer influence in Educational Choices: Social Image Concerns and Same-Gender Interactions*, maggio 2025.
140. R. Disney, J. Gathergood, S. Machin, M. Sandi, *Human Capital from Childhood Exposure to Homeownership: Evidence from Right-to-Buy*, giugno 2025.
141. Stephen Machin, Matteo Sandi, *Youth Crime and Delinquency In And Out Of School*, giugno 2025.