Education, Work and Crime: Evidence from Educational Reforms

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Declaration

I, Rui Miguel Vieira Marques da Costa, confirm that:

• The work presented in this thesis is my own and it has not been presented to any other university or institution for a degree,

• Where information has been derived from other sources, I confirm that this has been indicated in the thesis,

• Chapters one and four are a sole authored papers,

• Chapters two and three are based on conjoint work with Steve Machin (University College London and London School of Economics) and Brian Bell (King's College London),

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Abstract

This thesis studies the interactions between education, work and crime as a response to highly relevant and debated educational policy reforms: changes in compulsory school laws. In Chapter 1 a study of the recent trends of crime in the United States (US) is presented, along with a general theoretical model of crime as a rational individual decision shaped by different incentives. In the empirical section of the chapter, the use of individual-level data and exogenous variation in compulsory schooling laws helps to establish causality between educational attainment and incarceration in the US between 1960 to 2010 using an instrumental variable design. Chapter 2 looks more closely at the relationship between the policy reforms, education and crime in the recent period since 1980. Using arrest, incarceration and education data for males it establishes a fading response of educational attainment to changes in the laws, through a prevalent reduction effect stemming from the stricter law requirements adopted. In Chapter 3, the negative effect unveiled in Chapter 2 is carefully analysed through a multiple discontinuity design so as to better understand the channels through which compulsory schooling laws operate to reduce crime. Using detailed arrest data since 1974, evidence is found in favour of strong incapacitation effects in the short-run, complemented with dynamic incapacitation effects in the medium-run among young males. Finally, Chapter 4 looks at the response of females to these educational reforms in terms of crime and teenage pregnancy outcomes. Using a multiple discontinuity design, it is found that females respond similarly to their male counterparts with respect to crime and furthermore show a reduction in teenage pregnancy rates as a response to the same changes in compulsory schooling laws. Nevertheless in this chapter it is shown that the crime reducing effects of the laws are heterogeneous according to demographic, labour market and school quality regional conditions.

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Chapter 1

Crime and Economic Incentives: Evidence from Compulsory Schooling Reforms

Abstract

This paper aims to provide a motivation on why economics has been and should be used to study several features of crime. We show how different crime indicators have evolved in the US since 1960 across race and gender and offer a theoretical framework along with a short empirical literature review on the economics of crime. We then focus on the study of the impact of compulsory schooling laws on crime using individual data from 1960 to 2010 and find strong evidence in favour of both a reduced form effect as well as a causal educational impact through educational attainment.

1. Introduction

The premise that crime entails significant social costs seems to be consensual across fields of study. Despite the difficulty of accurately measuring all the direct and indirect costs that crime imposes on society[1], the increasing availability and quality of data has enabled economists to study and provide approximate measurement of the costs linked to crime behaviour. A review and meta-analysis of studies estimating costs of crime in the US by Chalfin (2015) , found losses ranging from 2% to 6% of gross domestic product (GDP) depending on the categorization of the nature of the costs and the conservativism of the assumptions. Likewise, the analysis of Jaitman (2017) estimates an average loss of 3.5% of GDP due to the direct costs of crime for 17 Latin American and Caribbean countries between 2010-2014. These figures are produced using conservative assumptions, in large part due to the difficulties of measuring: the long-running impacts of crime after imprisonment and potential release for individuals and the society alike; and the monetary value of psychological damages to the victims are among many other factors arising when one considers the whole range of consequences that criminal activity can have in society. Taking into account the previous limitations, it is clear that the costs of crime are unneglectable in both developed and developing economies; thus the study of the determinants of crime are of particular importance for informing and constructing effective policies to prevent and reduce criminal activities.

Started by Becker's use of rational utility models as a framework for understanding an individual's crime choices in 1968, research into the economics of crime has since developed and continues to be an ever-growing field of study, as reported by Draca and Machin (2015). Modern statistical methods combined with the normative analytical economic framework have proven useful in establishing the causal relationships between crime and incentives within an

[1] See Chalfin (2015) and Soares (2015) for extended discussion about the methodologies of measuring costs of crime

informative structure for policy design (Cook et al, 2013). A rich and extensive body of research has been produced in analysing economic and non-economic incentives as determinants of crime under different lenses: labour market factors and opportunities, criminal earnings and returns, deterrence of policing and safety technology, dissuasion power of sanctioning, consequences of sentencing and imprisonment, inter alia. A review of the literature focusing on labour market features and returns to illegal activities is offered in Draca and Machin (2015), whereas Chalfin and McCrary (2017) summarise and critically appraise the research produced on the analysis of sanctioning and sentencing. Later in this study, some of the literature presented in both of the previous reviews will be referenced in the context of the economic model proposed as a framework to analyse the potential of exogenous shifts in incentives for criminal engagement.

As stated previously, the presented study acknowledges crime as being a world phenomenon, however the focus of the proposed analysis will be the United States (US). The US has the highest incarceration rate per capita in the world[2] and ranks among the highest in terms of crime rates among OECD countries[3], consistently across crime types. The federative political structure of the country offers particularly interesting variation in policy adoption considering the relative legislative freedom of states in producing state law. In this work, we will be exploring the effects of reforms in compulsory school age laws and their potential to affect crime trends.

Using individual-level data from 1960-2010 combined with compulsory schooling laws enacted since 1915, with find evidence in favour of a crime reducing effect of these policy changes in the probability of an individual's incarceration. Exploring the educational attainment channel of the effect, we find strong evidence of a causal effect of education on

[2] Excluding the Seychelles Islands, according to the latest publication by Institute for Criminal Policy Research (http://www.prisonstudies.org/highest-to-lowest/prison-population-total)
[3] United Nations Office on Drugs and Crime, Statistics on Crime

crime as result of the exogeneous variation provided by changes in compulsory school laws. These results provide a bridge between two strands of the literature that evaluated these reforms in separate time periods.

The remainder of the paper is structured as follows: Section 2 introduces the reader to relevant descriptive facts about the evolution of crime and imprisonment in the US; Section 3 offers a theoretical framework to think about crime and its potential determinants linking it to the literature and the policy changes to be analysed; Section 4 describes the data, econometric modelling and presents the statistical results, and finally Section 5 offers some concluding remarks.

2. Background and Institutional Context

US in the International Context

As previously mentioned, the US has significantly higher levels of both crime and incarceration rates among developed economies. However, it is relevant to understand if trends in crime and incarceration rates have been similar between US and other world economies with comparable demographic and economic levels. Buonanno et al (2011) offered an interesting analysis of crime trends between the US and Europe[4]. They show that both blocks have experienced a similar upward trend until the early/mid-90s and have diverged since, with US crime dropping and Europe showing a continuation of the upward trend until the late 2000s in their analysis (Figure 1). They demonstrate that such deviation has been fuelled mainly by a

[4] Europe is defined in Buonanno et al (2011) as Austria, France, Germany, Italy, Netherlands, Spain and United Kingdom.

difference in the trends of violent crimes that has continued to trend up in Europe in contrast with the US, as we will present later in this Section.

Offenses Reported and Arrests

We start by analysing the evolution of crime as measured by offenses reported and arrests in the US over time. Figures 2A and 2B present the crime (offenses reported) and arrest rates for property and violent crimes in the US from 1960 (1980 for arrests[5]) to 2015.

The first point to note is the difference in scale between these types of crime: violent crime shows an average of 5 crimes per 1000 population over the period, almost one tenth of the average crime rate of property crimes which stands at approximately 40. Given the nature of the crimes, one can think of violent crime as more often motivated by emotional and non-monetary factors as compared with property crime which can be seen as a type of activity that yields monetary returns in many of its instances through the reselling of stolen goods in the illegal or poorly regulated markets. The former observation acts as support to the premise that crime engagement can be modelled as a rational incentive-based decision by individuals further developed in detail in Section 3.

Looking at the evolution of crime trends, one can observe the widely discussed[6] rise and decline of crime in the US over the past six decades. Crime rates have increased since 1960, reaching their peak in the early to mid-90s, at which time the trend reverted, showing a steady decline in crime rates until the most recent date, 2015. Additionally, it is noticeable that arrest rates have tracked crime rates in a fairly parallel fashion with intuitively lower rates (the police do not have the resources to solve and arrest the perpetrators of every crime reported)[7].

[5] Unfortunately, consistently defined data for arrests is only reported from 1980 onwards.
[6] See Levitt (2004).
[7] This paper will not discuss the effectiveness and efficiency of police in solving reported crimes, though it is a topic of interest in research.

Figure 3 presents a breakdown of arrest rates by crime types to better visualise the trends and differences across them. At this scale, it is easier to see how both violent and property arrests track the overall trends of their crime rates respectively. A more interesting fact is to note that drug-related arrests have upward trends since 1980, with a slowdown and reversal in the past decade of data. Unfortunately, one cannot conclude that the same has happened with the crime rates for drugs offenses given that these are not consistently reported in the US, however the strong correlation between arrests and offenses reported would point in favour of a parallel trajectory.

A further interesting dimension to analyse is how crime has evolved across genders. As discussed in Schwartz and Steffensmeier (2007, 2015), females have shown consistently lower levels in crime measurements (self-reported crime, arrests, imprisonments,...); Figure 4 shows that feature for arrest rates in the US[8]. Nonetheless, once properly scaled to the beginning of the period (Figure 5), we can conclude that female arrests rates have increased by approximately 10 percent, whereas male arrest rates have approximately halved since 1980 (specifically, a 54 percent drop). Based on the prior observations, one concludes that the gender gap in arrest rates has narrowed significantly by approximately 64 percent with respect to the initial level in 1980.

Next, we look at the racial breakdown in arrests rates between whites, blacks and all races[9]. In Figure 6 we plot the arrest rates for black, white and all arrestees for the same period of analysis 1980 to 2015. Despite the different races showing a common trend evolution consistent with the previous analysis for overall arrest rates, the level of arrest rates are strikingly dissimilar between whites and blacks. Black arrest rates were close to three times higher than white arrest rates in 1980 and about double by the end of the analysed time period,

[8] This is a common feature in several other developed countries: Sweden (Estrada et al (2016)) Australia (Beatton et al (2017))

[9] Arrest rates are not consistently reported separately for Hispanics over the entire period covered.

implying a reduction in the race gap between blacks and whites. Nonetheless, as of 2015, approximately 14 out of 1000 blacks were arrested, whereas the same statistic drops to 6 out of 1000 among the white population. When further accounting for the fact that in 2015 the black population was approximately 12.6% of the total US population (versus 73.6% for whites[10]), the previous difference in levels of arrest rates by race gain additional relevance.

Imprisonment and Correctional Supervision

After analysing the trends and composition of crime and arrest rates, we now shift the focus to imprisonment and correctional supervision numbers in the context of the US. As stated in the introduction, the US has the highest rate of incarceration per capita in the world. Figure 7 shows that the US prison population was not always as high as one observes most recently. Indeed, until the 80's, prison rates in the US were roughly constant, with around 1 out of 1000 individuals imprisoned. However, since the beginning of the 80's the incarceration rate has increased by a factor of close to 5, meaning by 2015 approximately 5 out of 1000 people living in the US were in prison. Moreover, when we extend the analysis to include forms of correctional supervision (probation and parole) in Figure 8, the number of people under any form of imprisonment or supervision increases to 21 out of 1000. One can see that the evolution of these forms of correctional supervision have tracked the boom in prison rates and more recently seem to have expanded in their relative importance compared to incarceration. The diverging feature between crime/arrest rates and incarceration rate is the fact that the latter does not seem to accompany the downward trend seen in both crime and arrest rates since the 90s, showing only a very mild decline since 2008. Buonanno et al (2011) suggest that incarceration

[10] This statistic includes Hispanics categorized as white in terms of race. Hispanic is treated as an ethnicity for Census proposes in the US. As of the year of 2015, 17.1% of the US population was of Hispanic ethnicity.

might have worked as an incapacitation and deterrence mechanism, and hence have had a causal effect on crime rates and thus explaining a small part of the divergence between the US and Europe as previously described. The discussion over the role of incarceration as a potential source of incapacitation and deterrence will be further developed in Section 3.

Analogously to the analysis of arrest rates, we study the gender breakdown in incarceration rates as shown in Figures 9 and 10. Similar to the trend shown in the arrest rates, females have far lower levels of incarceration compared to males throughout the time period, as visible in Figure 9. Nevertheless, when looking at the relative evolution of the incarceration rates shown in Figure 10, one observes again a stronger relative growth in incarceration rates among females as compared to their male counterparts (a 6 times increase versus a 2.5 times increase). Indeed, it should not be totally surprising that as more women seem to engage in crime and get arrested, the level of imprisonment would increase.

Finally, Figure 11 breaks down the incarceration rates by race in a compatible way to the analysis provided for arrests. Unsurprisingly, the racial differences between the incarceration rates of blacks and whites emerge with an alarming average of 12 out of 1000 blacks being incarcerated over the time period starting in 1980 and ending in 2015. The same figure for the white population drops to 2/3 per 1000. The time trends over the different races seem to be mostly similar and consistent with the overall incarceration evolution during the time period in analysis.

The massive increase in prison population has been described and analysed by Redburn et al (2014). They concluded that the contemporary levels of incarceration in the US are "unnecessarily high", pointing for a revision of the sentencing guidelines as a necessary step to reverse the trend. In 2016, the Obama Administration released an ambitious report "Economic Perspectives on Incarceration and the Criminal Justice System" where several proposals were laid out to tackle the incarceration rate problem as well as for reforming the criminal justice system.

3. <u>Modelling Crime and Reforms</u>

"I was not sympathetic to the assumption that criminals had radically different motivations from everyone else."

Gary Becker, Nobel Prize Lecture 1992

The seminal work by Gary Becker in 1968 offered the first formalisation of rational utility models to crime choices made by individuals. A first approach to modelling an individual's decisions with respect to engagement in criminal activities can be summarised by the following relationship:

$$(1-p)U(R) - pU(S) \gtreqless U(W)$$

where p stands for the probability of getting caught doing crime, $U(.)$ describes a standard utility function, W and R stand for the earnings derived from legal activities (labour markets) and illegal activities (crime) respectively and finally S reflects the sanction/fine applied to an individual when caught. In this model an agent will weigh the expected utility from committing crime, $(1-p)U(R) - pU(S)$, against the certain utility of working, $U(W)$ and decide as to whether to engage in crime or not.

The model offers a simple and intuitive framework to think about how economic incentives might play a role in the decision to commit crime; however, its discrete nature (an agent decides either to be exclusively in the illegal or legal sector) is not always the most useful approximation to the choices that individuals face. Aiming to bridge this potential gap, Ehrlich (1970, 1973) formalised a model of time allocation which allows individuals to choose their engagement in crime at the intensive margin and not exclusively at the extensive margin. Despite the main conclusions concerning the impacts of economic incentives remaining broadly the same as in Becker (1968), this model offers a flexibility better fitted for thinking about the shifters further presented in this work and the subsequent chapters. In a generic version of Ehrlich's model an individual will face the following problem:

$$\max_{\{t_l, t_c, t_w\}} \quad (1-p)U(rt_c + wt_w, t_l) - pU(rt_c - st_c + wt_w, t_l)$$

$$s.t. \quad t_l + t_c + t_w \leq T$$
$$t_l \geq 0\,, t_c \geq 0, t_w \geq 0$$

where t_l, t_c, and t_w describe the time allocated to leisure, crime and work respectively constrained to a time endowment T, and the rest of the parameters retain their previous meaning. Hence, the individual will choose a time allocation vector (t_l^*, t_c^*, t_w^*) as to maximise his/her expected utility. Optimal allocation between crime t_c and work t_w must satisfy the following first order condition, in case of an interior solution:

$$(1-p)(r-w)U'(rt_c + wt_w, t_l) = -p(r-s-w)U'(rt_c - st_c + wt_w, t_l)$$

At the optimal, the agent is going to equalise the expected marginal utility of allocating time to crime without being caught with the expected marginal utility of committing crime in case of being uncovered and sanctioned. Under a standard set of assumptions debated in detail in Ehrlich (1970, 1973), the comparative statics of this model deliver intuitive predictions about the way individual's crime engagement will respond to the different parameters included in the model:

i) $\frac{\partial t_c^*}{\partial p} \leq 0$ time allocated to crime decreases with the probability of getting caught p.

ii) $\frac{\partial t_c^*}{\partial w} \leq 0$ crime drops in response to an increase in returns to legal activities w.

iii) $\frac{\partial t_c^*}{\partial r} \geq 0$ individuals will increase their engagement in crime as the returns to crime rise r.

iv) $\frac{\partial t_c^*}{\partial s} \leq 0$ engagement in illegal activities declines as result of an increase in sanctions in case of being caught s.

The model as described before remains a static optimization problem, where an individual decides to allocate his/her time at a given moment without dynamic considerations. Further work by Lochner (2004) and Mocan (2005) in the economics literature and Gottfredson and Hirschi (1990) and Sampson and Laub (1993, 2005) in the criminology literature, have implemented dynamics in the standard crime models so as to provide a better framework to think about criminal careers and the idea of criminal capital accumulation[11]. Nonetheless, as shown in Bell, Costa and Machin (2017), Ehrlich's model can still easily be extended so as to incorporate some dynamic aspects sufficient to provide interesting predictions about the crime behaviour of individuals.

The predictions of the previously presented model have guided economists interested in the empirical analysis of crime. The main challenge for applied crime economists in testing the theoretical implications of the models has been the same as that of many empirical studies in other fields of economics: how to correctly model and measure the data so as to establish causation between economic incentives and criminal behaviour.

In the rest of this section we offer an considerable (but in no way exhaustive) description of empirical studies that have successfully tested some of the predictions of the previously presented model and further provide the rationale for how the model might offer insights about the effects of the reforms being analysed in this study.

[11] When considering crime decisions made collectively, models of networks and organized crime (gangs, mafia, …) have been proposed by Garoupa (2007), Baccara and Bar-Issac (2008), Kumar and Skaperdas (2009), inter alia.

Probability of Arrest (p) $\frac{\partial t_c^*}{\partial p} \leq 0$

The deployment of police in the streets is expected to deter crime, given that the probability of a criminal getting caught increases (p in context of the model), ceteris paribus. Using quasi-natural experimental settings Levitt (1997), Draca et al (2011), inter alia exploit unanticipated changes in police force placement and find empirical evidence consistent with a deterrence effect of policing.

Labour Market Returns (w) $\frac{\partial t_c^*}{\partial w} \leq 0$

An increase in the return of legal activities, labour markets, should tilt an individual away from crime as the cost of opportunity rises for those engaging their time in illegal activities. Grogger (1998), Gould et al (2002), and Machin and Meghir (2004) explore data in different settings and find evidence in favour of crime reductions being linked with labour wage improvements.

Criminal Returns (r) $\frac{\partial t_c^*}{\partial r} \geq 0$

Despite the relevance of the result that time allocated to crime increases according to the returns of criminal activities, measuring and observing the returns and earnings of crime is extremely difficult due to its intrinsic illegal nature. Empirical studies are hence rare in this topic; however, recently Draca et al (2018) show how criminals respond to direct and indirect measures of returns to crime through changes in prices of stolen goods. Despite not having a direct measure of time allocated to crime activities (property crime in this case), they find a significant reallocation across the portfolio of stolen goods consistent with criminals responding in line with higher returns to crime.

Sanctioning (s) $\frac{\partial t_c^*}{\partial s} \leq 0$

The sanctioning of crime should work as both punishment and direct and indirect deterrence. Most empirical studies have focused on the potential deterrence effects of sanctions: Helland and Tabarrok (2007) study the effect of the three-strike laws in California as a deterrence mechanism for individuals with two strikes on their record already; Bell et al (2014) explore the harsher sentencing by judges as result of the 2011 London riots. Both studies find evidence of a significant deterrence effect of higher effective or perceived sanctions on crime rates.

The cut-off introduced by legal juvenile age, the oldest age at which a juvenile court has original jurisdiction over an individual for law-violating behaviour, is another example of sanction variation which can be seen in the context of the model as the non-linearity of sanctions. One can model s as a function of age and hence derive implications for an individual's time allocation. Chalfin and McCrary (2017) point out that empirical studies on this topic have not reached a robust consensus on the effect of juvenile age as a crime deterrence mechanism.

Education $(w(E), p(E))$

According to the human capital approach, educational gains have the potential to increase labour market earnings (Becker (1962), Ben Porath (1967), Card (2001)). This proposes to model the return to legal activities as a function of education $w(E)$. Anchored on this assumption, Lochner and Moretti (2004), Machin et al (2011) and Hjalmarsson et al (2014) use exogeneous variation in education resulting from compulsory schooling reforms to unveil a negative effect of education on crime among adults with completed education. A potential simultaneous effect could be modelled concerning the link between education and probability of being caught, $p(E)$. If one would consider that better educated individuals would be less likely to be caught, such would potentially invalidate the result of a genuine crime reducing effect from education: decreases on reported crime from factual reduction on number of crimes or lower crime detection due to better avoidance would be, from econometric modelling perspective, observationally equivalent. However, Lochner and Moretti (2004) and Machin et al (2011) do not find evidence of effects through the previous channel when analysing changes in self-reported crime across education groups.

Incapacitation (T)

One can think of incapacitation as a change in the time endowment T of the model. Teacher strikes (Lualen (2006)), term length changes (Jacob and Lefgren (2003)), compulsory leaving age reforms (Anderson (2014) and Bell, Costa and Machin (2017)) have shown that exogeneous changes in the time endowment of young individuals resulted in reductions in the number of arrests at the ages affected.

Another strand of literature has looked at incarceration as a source of incapacitation for convicted criminals; Levitt (1996), Buonanno et al (2011) and Johnson and Raphael (2012) inter alia find evidence in support of this form of incapacitation in reducing crime. Despite these findings, the cost effectiveness of incarceration as a crime reducing policy has been widely debated in light of the recent evidence of pervasive long-running effects of incarceration on recidivism (Bayer et al 2009, Nagin et al 2009, Drago et al 2011 and Aizer and Doyle 2015), among other arguments.

In the remaining sections of this paper, we will analyse the relationship between compulsory school laws, education and crime and thus explore the potential mechanisms such as $w(E)$ and T. Recent literature, namely Stephens and Yang (2014), has questioned the validity of compulsory school laws as instruments to identify causal effects of education in several outcomes of interest. In their analysis, Stephens and Yang show that changes compulsory schooling laws are subject to potential confounders due to strong regional trends in school quality. Throughout the thesis, we propose both robust specifications with inclusion of fully flexible region-cohort trends, and a different identification model, discontinuity design, which by restricting the identifying variation within states addresses the problem of potential confounding trends in each state where reforms take place.

4. Data Description and Empirical Approach

Data

Individual-level data: 1960-2000 Census and 2010 ACS

We use microdata from Census and ACS from 1960 to 2010: 1960 Census 1% sample, 1970 Census combined state 1% samples (2% sample overall), 1980 5% sample, 1990 Census 5% sample, 2000 Census 5% sample and 2010 centered ACS (2008-2012 equivalent to 5% sample). The sample is restricted whites and blacks, ages 18 to 39, born in the US[12]. In the Census 1960 to 1980, we can identify institutionalized individuals in correctional facilities whereas from 1990 onwards only the institutionalized indicator is available. However, as explained in Bell, Costa and Machin (2016) and Bell et al (2018), once restricted to ages 18-39 the percentage of institutionalized individuals that were in correctional facilities average between 90 to 95% during the more recent period. We hence define an incarcerated individual as being in a correctional facility for years 1960 to 1980 and institutionalized for years 1990 to 2010[13].

Table 1 summarises some relevant statistics of the population sample broken down by sex and race. In line with the previously discussed points in Section 2, we observe considerably higher incarceration rates among the black population in both sexes when compared to whites:

[12] We exclude Alaska, Hawaii and the District of Columbia from the sample.
[13] Further improvements to the be applied to the definition of incarcerated individuals between 1990 and 2010 using the disability status of individuals and the strong correlation with other institutionalized population as explained in the Appendix.

7% versus 0.9% for white males and 0.4% versus 0.1% for white females. Restricting attention to the dropout population, we observe that the proportion of dropouts incarcerated is, overall, higher than in the graduate or higher schooling population, consistent with the premise that education can play a role in this crime outcome. Again, the racial difference in this statistic is striking with 11% of black male dropouts being incarcerated when compared to 2% among white dropouts (1% versus 0.2% for the female population). Finally, education measured as years of schooling and high school dropout shares shows once more a gap between races, irrespective of sex, despite a very mild attenuation among the female population. Blacks show close to 1 year less in average years of schooling (11.8 vs 12.9 males, 12.2 vs 12.9 females) and twice the dropout share (29% vs 17% males, 25% vs 15%).

Compulsory School Laws

Compulsory school laws were first enacted in the US in the late 19th century and beginning of the 20th century with different timings and binding ages across states. The early enacted laws combined with the child labour laws have been used in the literature: Angrist and Acemoglu (2001), Lochner and Moretti (2004), Lleras-Muney (2005), Goldin and Katz (2008), Stephen and Yang (2014), Bandiera et al (2018), inter alia. The use of more recent laws in the US context has been less common[14], partly due to the increasing complexity of these laws, as discussed in Goldin and Katz (2008) and Oreopoulos (2009), with several interactions being added to account for employment exemptions, parental consents and different degrees of non-compliance penalties.

For the purpose of this study we focus on the main three components of these laws: maximum start entry age, minimum leaving/dropout age and completed grade exemption. The

[14] Oreopoulos (2009) and Oreopoulos and Salvanes (2011) are rare examples of the use of most recent laws in the US context.

interactions between the previous three components and the added dimensions of current laws is beyond the scope of this study, despite its potential relevance[15]. We explore two measures that use and combine the three different elements described previously. Firstly, we define DA_{st} for state s and year t as the minimum between dropout age and the age implied by the grade exemption in place (i.e.: if the grade exemption is high school graduation (12th grade) then the implied age will be 18):

$$DA_{st} = min\{Dropout\ Age_{st}\ , Age\ at\ Grade\ Needed\ to\ Dropout_{st}\}$$

Secondly, we define CSL_{st} as per Goldin and Katz (2008), where the minimum of the difference between dropout age at time t and the entry age at time $t-8$ and the grade exemption and time:

$$CSL_{st} = min\{Dropout\ Age_{st} - Entry\ Age_{st-8}, Years\ of\ School\ Needed\ to\ Dropout_{st}\}$$

Figures 12A and 12B show "heat" maps of the US for the binding dropout age, DA_{st} in 1960 and 2010. In map 12A we can see that by 1960 there was a considerable level of state heterogeneity in dropout ages ranging from 14 to 18, with the particular cases of Mississippi and South Carolina that had abolished their compulsory school laws as a reaction to *Brown v. Broad of Education* 1954[16]. As of 2010, we see a clear tendency of higher dropout ages adopted across states with a significant amount of states establishing their binding dropout age at 18 years of age (22 states compared to 4 out of 48 as of 1960). Figures 13A and 13B show similar maps for CSL_{st}, where we can see a similar evolution as the one described previously for DA_{st}, with values now ranging from 8 to 12 years of compulsory schooling.

For the purpose of this study we match the laws to individuals at age 14 as in Lochner and Moretti (2004) and Goldin and Katz (2008): t will be hence equal to year of birth plus 14.

[15] Domnisoru (2015) shows that changes in work permits affect the level of compliance towards school leaving age reforms, however do not invalidate their significance.

[16] *Brown v. Broad of Education* 1954 marked a stepping stone towards equal access to public education between black and whites, by ruling that separate public schools based on race was unconstitutional.

Results are robust to matching individuals at ages 15 or 16, however since the analysis starts in 1960 when a few states still had dropout ages of 14 years of age and we want to draw comparisons to other studies, the age of 14 is sensible.

Empirical Approach

In this paper, we are interested in accessing and quantifying the impacts of the compulsory schooling laws enacted between 1920 and 2010 on education and crime, and the potential causal relationship between both. As described in Section 2, we acknowledge that incarceration is only one of the several outcomes of crime and that it might fail to capture the full effects of the reforms being analysed. Unfortunately, data on arrests prior to 1980 relies heavily on imputation due to the poor coverage rates observed in some states. Furthermore, disaggregated data is not available to facilitate the construction of a balanced panel able to be merged with more recent arrest data. Taking into account the fact that one of the aims of the analysis is to provide a bridge between the studies that have analysed such reforms at separate periods (Lochner and Moretti (2004) use 1960-1980 data; Bell, Costa and Machin (2016) focus on 1980-2010), the use of incarceration individual data looks the most harmonious solution to enable one to draw comparisons.

Reduced Form

Firstly, we look at the direct impact of compulsory schooling reforms on the probability of being incarcerated, commonly referred to in empirical literature as the reduced form equation. By modelling and estimating the reduced form before trying to establish a causal relationship between education and crime, one is able to gain a better grasp of the effects of these reforms on crime over and above the educational measurements that are available to researchers. As discussed in Section 3, these reforms have the potential to affect an individual's engagement in crime through channels (incapacitation and dynamic incapacitation) that are imperfectly measured by the existing educational attainment variables. The reduced form of compulsory schooling reforms on the probability of being incarcerated takes the form:

$$Incarcerated_i = \theta_1 School\ Law_i + \pi_1 X_i + \varepsilon_{1i} \tag{1}$$

where $Incarcerated_i$ stands for the indicator for whether the individual i is incarcerated or not. $School\ Law_i$ will be defined by functional forms of compulsory schooling laws, CSL_{st} and DA_{st}, affecting individual i. Relevant individual observable characteristics (year of birth, age, state of birth, state of residence, census year) are captured in X_i and finally ε_{1i} describes the error term.

Table 2A shows the estimates of θ_1 for CSL_{st} and DA_{st}, separated for whites and blacks as well as a pooled estimates. Under the main specification, θ_1 is identified by within-state variation in the laws and heterogeneity of the laws across states in each cohort. This is possible given that states adopted laws in different years with different requirements in terms of leaving age, entry age and grade exemptions. Aiming to answer the potential endogeneity concern raised Stephen and Yang (2014) with respect to the timing of adoption of the laws, we include estimates that control for a fully non-parametric set of region-year of birth indicators. The former specification will capture cohort-specific shocks or trends at the regional level that can be correlated with the timing of the adoption of the laws. This restricts the identifying variation to state changes in the laws within the same region at every given cohort; we believe this to be very demanding for the data given its decadal reporting nature.

Panel A of Table 2A shows broadly consistent negative estimates for the different indicators of DA_{st}, indicating that higher dropout ages have resulted in fewer incarcerated individuals. The same conclusion holds for the Panel B where CSL_{st} is used as a measure for compulsory schooling law changes. The statistical significance of the coefficients varies with race, with blacks showing more precisely estimated results. One can attribute part of this difference to the considerably lower mean number of whites incarcerated (0.9%). The results are not always monotonic as is the case of Lochner and Moretti (2004)[17] despite using a slightly different measurement of the laws. The effects on black males are consistently larger in magnitude than the ones of their white counterpart, which should be intuitive given the substantial difference in mean of incarceration between these groups. The inclusion of regional-cohort trends does not seem to affect the overall results in a considerable way despite a mild hampering down of the magnitude of some estimates.

Table 2B replicates the estimates of the reduced forms for females. Immediately we can observe that the magnitude of the estimates is considerably lower. Indeed, as stated previously, females have a considerably lower incarceration rate, thus making the relative effect of the reduced form coefficient to the mean more conformative with their male counterpart. The statistical significance of the coefficients is considerably lower with several of the coefficients being insignificant and even showing the positive sign, particularly in Panel A with DA_{st} indicators, which as we will see later have the potential to deliver counter-intuitive instrumental variable estimates. Despite the weaker link between compulsory school laws and incarceration status in the female samples, Panel B using CSL_{st} shows negative effects in all specifications with most of them being statistically significant.

Overall, the results on the reduced forms are consistent with a reducing effect in incarceration probability as result of stricter compulsory schooling laws. Larger effects are concentrated in the male population with particular emphasis among blacks.

Causal Effect of Education: Two-Stage Least Squares and First-Stage

[17] The authors do not provide the reduced form results in the published version, however using the replication data we were able to estimate them.

As discussed in Section 3, a significant part of the literature surrounding compulsory school laws and crime has focused its attention on trying to use the reforms in the laws as instrumental variables for education, hence aiming to establish a causal relation between education and crime. The basic relationship between the two variables can be summarized as follows:

$$Crime_i = \beta Schooling_i + \gamma X_i + \varepsilon_i \tag{2}$$

where $Schooling_i$ describes measures of educational attainment (years of schooling completed and high school dropout in the case of this paper) of individual i, and the rest of the variables retain the definitions provided in Equation (1). The use of compulsory school laws as instruments aims to solve the potential endogeneity between schooling attainment ($Schooling_i$) and individual unobserved characteristics captured in ε_i, thus failing to identify β and rendering its estimates biased. The model to solve the endogeneity problem, assuming the exogeneity of the laws, proceeds to isolate the variation in schooling that results from the changes in the schooling laws by estimating the first-stage equation:

$$Schooling_i = \theta_2 School\ Law_i + \pi_2 X_i + \varepsilon_{2i} \tag{3}$$

Finally, the exogenous variation in schooling steaming from the laws is used to identify β and estimate it using instrumental variable or two-stage least squares estimators (IV/2SLS). If the instrument, $School\ Law_i$, would be specified as continuous then β would be exactly $\frac{\theta_1}{\theta_2}$ (IV estimator), however given the use of non-linear functional forms of DA_{st} and CSL_{st}, β is estimated using the 2SLS estimator.

Years of Schooling

Tables 3A and 3B show the estimates for the OLS, 2SLS and first-stages using DA_{st} as instruments and years of schooling as a measurement of education for males and females, respectively. The estimates from OLS suggest that an additional year of schooling is associated with a 0.5 percentage points (pp) reduction in the probability of being incarcerated for males and 0.06 percentage points for females. The difference in magnitude of the OLS and 2SLS coefficient across sexes, is again attributable to the difference in means of the dependent variable (incarceration). When the OLS results are scaled according to percentage effect relative to the mean incarceration, one year of extra complete schooling delivers a 30 to 20% reduction for men and a 40 to 30% reduction for women in the probability of being incarcerated, depending on the samples. This shows that the relative effect of the results is actually not as different between sexes as the estimated coefficients suggest at first glance. The differences in magnitudes across races should also be analysed relative to their mean incarceration rate as discussed previously.

As far as the first-stages are concerned (Panels B), both sexes show consistently positive effects across race samples, suggesting that increases in compulsory schooling laws as measured by DA_{st} were indeed associated with increases in the years of schooling completed by individuals. The magnitude of the first-stage results is equivalent between sexes, although it is consistently higher among the black population that shows a lower mean educational attainment as described in Table 1. Most of the first-stages report F-Statistics above the threshold of 10 for test of weak instruments, with the black male samples falling slightly below 10, partly attributable to the smaller sample size, as the individual coefficients consistently show individual statistical significance. It is worth noting that our OLS and first-stage estimates lie between the estimates of Lochner and Moretti (2004) and Bell, Costa and Machin (2016), which is reassuring given that we use both time periods in this analysis.

The 2SLS estimates seem to suggest that indeed there were reasons to believe that endogeneity was producing an upward bias in the OLS estimates. This is because the vast majority of the 2SLS results show coefficients higher by factors of 2 to 3 times depending on the specification and sample, with these factors being not dissimilar to the ones estimated in Locher and Moretti (2004), Bell, Costa and Machin (2016) for males, and Lochner and Cano-Urbina (2016) for females. Looking at columns (2) of both Tables 3A and 3B, we observe that the 2SLS estimates are around 3 times higher than the OLS estimates (-0.538 vs -1.271 for males and -0.066 vs -0.193 for females). As discussed in the reduced form analysis the counter-intuitive signs of black females (Table 3B, Columns (5)-(6)) result in wrong and statistically insignificant 2SLS estimates despite a good first-stage. It is important to point out that differences between OLS and 2SLS estimates can span from the compliers population being affected by the instruments. The period of analysis is quite extended and as suggested in Bell, Costa and Machin (2016, 2017) the composition of compliers subject to recent reforms is likely to be different than in previous periods as secular trends in educational attainment are in place and individuals are, on average, more educated in the recent time periods. The former can contribute towards the difference between OLS and 2SLS results along with the potential endogeneity bias.

Tables 4A and 4B reproduce the analysis using CSL_{st} as instruments. Looking at the results of columns (1) and (2) for both sexes, we conclude that the instrument delivers similar results to the ones before (DA_{st} used as instrument). The first-stages are also consistent with the principle that stricter laws on schooling ages have delivered educational attainment gains and, additionally, all present F-statistics above 10 as required for a valid identification.

The results using the alternative instrument are generally consistent with the findings of Tables 3A and 3B, though with some variations and exceptions, such as in the case of white males with insignificant and often wrong sign estimates. On the other hand, black female results show the expected signs though lacking in statistical significance. It should not be completely surprising that some differences are found when using CSL_{st} as an instrument, given that this uses further variation in compulsory maximum entry age interacted with minimum leaving age. This has the potential to change the composition of the complier population and uses extra variation through the time period[18]. Indeed, the 2SLS results using CSL_{st} point towards a slightly lower magnitude of the bias suffered by the OLS estimates in some specifications.

High School Dropout

If one is to think about the potential composition of complier population in the current setting, we would be inclined to consider individuals at the margin between high school completion and failure to be the most likely to be affected by the compulsory school laws. Although perhaps less so in earlier periods, in recent periods, where educational attainment is generally higher among the general population, the relevant margin of education with potential to deeply affect labour market opportunities and hence incentivise/deter crime engagement is obtaining a high school diploma (Bell, Costa and Machin (2016, 2017)). Following the previous argument, we estimate the results using high school dropout status instead of total years of schooling completed in Tables 5A to 6B.

We start by looking at the OLS estimates in Tables 5A and 5B and conclude that these are significantly higher along this educational definition: failing to graduate from high school increases the probability of being incarcerated by 3.8 percentage points for males and 0.4 percentage points for females (Columns (1)-(2)). These figures are more than double the mean incarceration rates for both groups. Once more, one can observe that the effect on the black population (males and females) is significantly higher than on whites. Black males show a 10 percentage point increase in their probability of ending up in correctional facilities as compared to 2 percentage point increase in their white counterpart (1 pp versus 0.2 pp for females).

[18] Indeed, in the most recent studies on entry age reforms, McAdams (2016) and Landerson (2017) show a reduction in crime associated with older entry ages using a regression discontinuity design. If one is to believe the conclusions of such studies, this would suggest that using the entry age variation as in CSL_{st} might not be the most correct way to model impacts on crime outcomes.

Analogously to the case of years of schooling, we observe that first-stage effect on high school dropout is negative as expected and robust to the use of either of the instruments (DA_{st} Tables 5A-5B and CSL_{st} Tables 6A-6B) across sex and race, though the F-statistics fall below 10 for the blacks in Tables 5A and 5B.

When looking at the 2SLS estimates, one can conclude that overall there seems to be evidence of a downward bias in the OLS estimates as, after corrected for endogeneity, the coefficient seems to go up by a factor of 2 to 3 times depending on the sample and instrument used. The 2SLS results in columns (1) and (2) of the Tables 5A-6B are consistent with an effect of failing to graduate high school on incarceration probability between 10 and 4 percentage points for males and 1.5 and 0.8 for females, though a few estimates are not statistically significant. Once more, the cases of with "wrong" sings in the 2SLS estimates correlate perfectly with the cases where weaker reduced forms were found. As for the composition of complier population and its influence on the instrumented estimates, we believe that using this margin of educational attainment captures the variation of the population at the margins more consistently. However, we cannot rule out the fact that over time the composition of the population at this margin has not changed in its unobservable characteristics.

Overall, the results for both years of schooling and high school dropout as measurements of school completion are consistent with an improvement in human capital having a causal negative effect on crime as measured by incarceration status. It is well established that educational gains yield returns to wages through human capital accumulation (Card, 2001); furthermore more highly educated individuals might be more patient and risk-averse (Lochner and Moretti, 2004). All these factors can contribute as mechanisms explaining the negative relationship between education and crime. Nonetheless, as pointed out in Section 3 and the beginning of Section 4, the effects of school reforms such as changes in compulsory school ages can have effects that cannot be fully captured by education grade completion and that might affect other outcomes of crime not measured by incarceration.

5. Conclusions

In this introductory chapter we have provided a motivation for the study of crime using theoretical economic reasoning combined with solid econometric empirical models so as to test and quantify the predictions. We introduce the context of crime in the United States, making arguments in favour of it being a relevant ground for analysis and research given the magnitude of the phenomenon of crime in the country and the legal structure surrounding policy adoptions. We have shown that crime is significantly different across race and gender in the US and have provided the most up-to-date trends describing these gaps.

Additionally, this work introduced the reader to the common theoretical framework used in economics to think about the determinants of crime and how they interact with individual choices of the agents at the margin of engaging in illegal activities. A summary of several model predictions for individual criminal behaviour and how empirical literature has tried to test and quantify them was presented, before focusing on the relationship between education and individual crime engagement. Along this dimension, we used individual data from 1960 to 2010 to study the impact of compulsory schooling law reforms on the probability of being incarcerated and quantify how much of this effect can be attributed to educational attainment gains resulting from these policies. We find, by studying both reduced forms and instrumental variable estimates, support for the argument in favour of the potential of educational reforms (such as compulsory school laws) to shift individuals' decisions to engage in crime due to educational gains, whilst we retain the hypothesis that benefits from these reforms can work through other channels.

In the empirical analysis we aimed to bridge the literature studying the same policy changes though in separate time periods. The former, along with the theoretical and empirical literature review, provide a motivation and context for the work that follows in this thesis studying the several channels of the effects of compulsory schooling laws in different crime outcomes and along different dimensions such as age.

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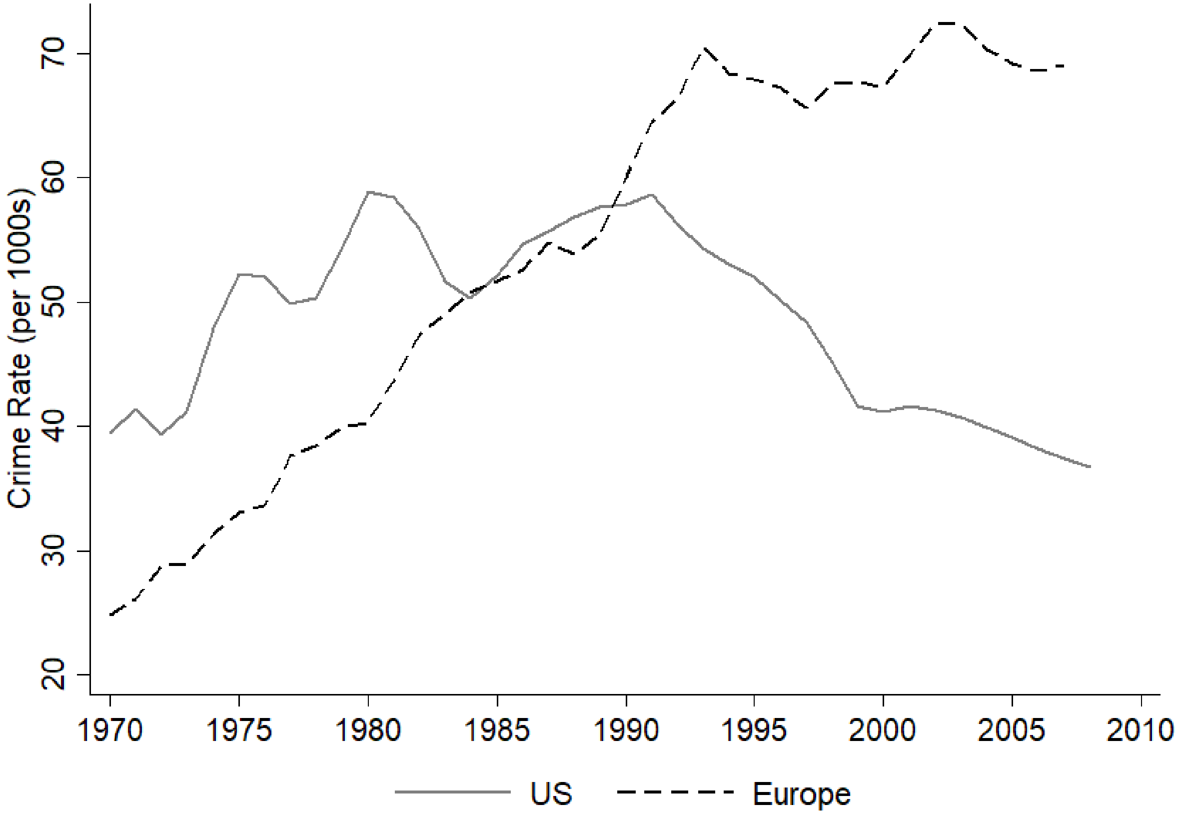
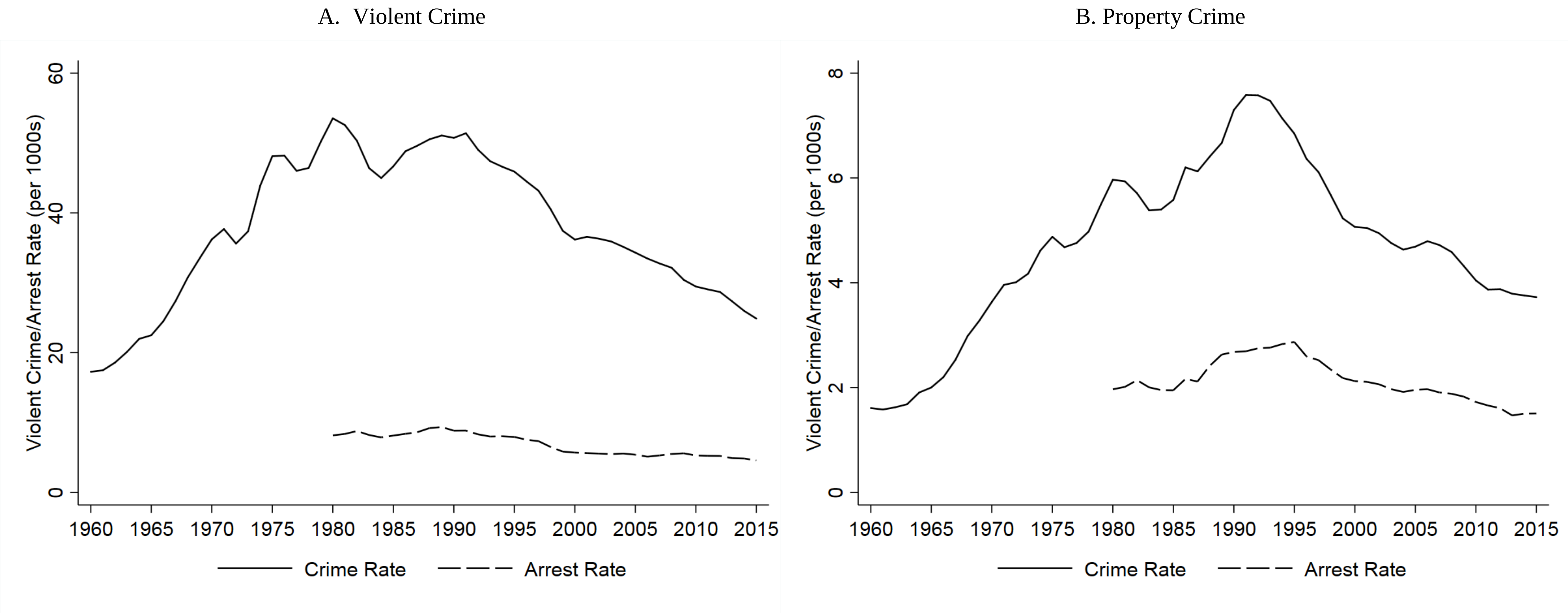
Figure 1: Crime Rates in United States and Europe



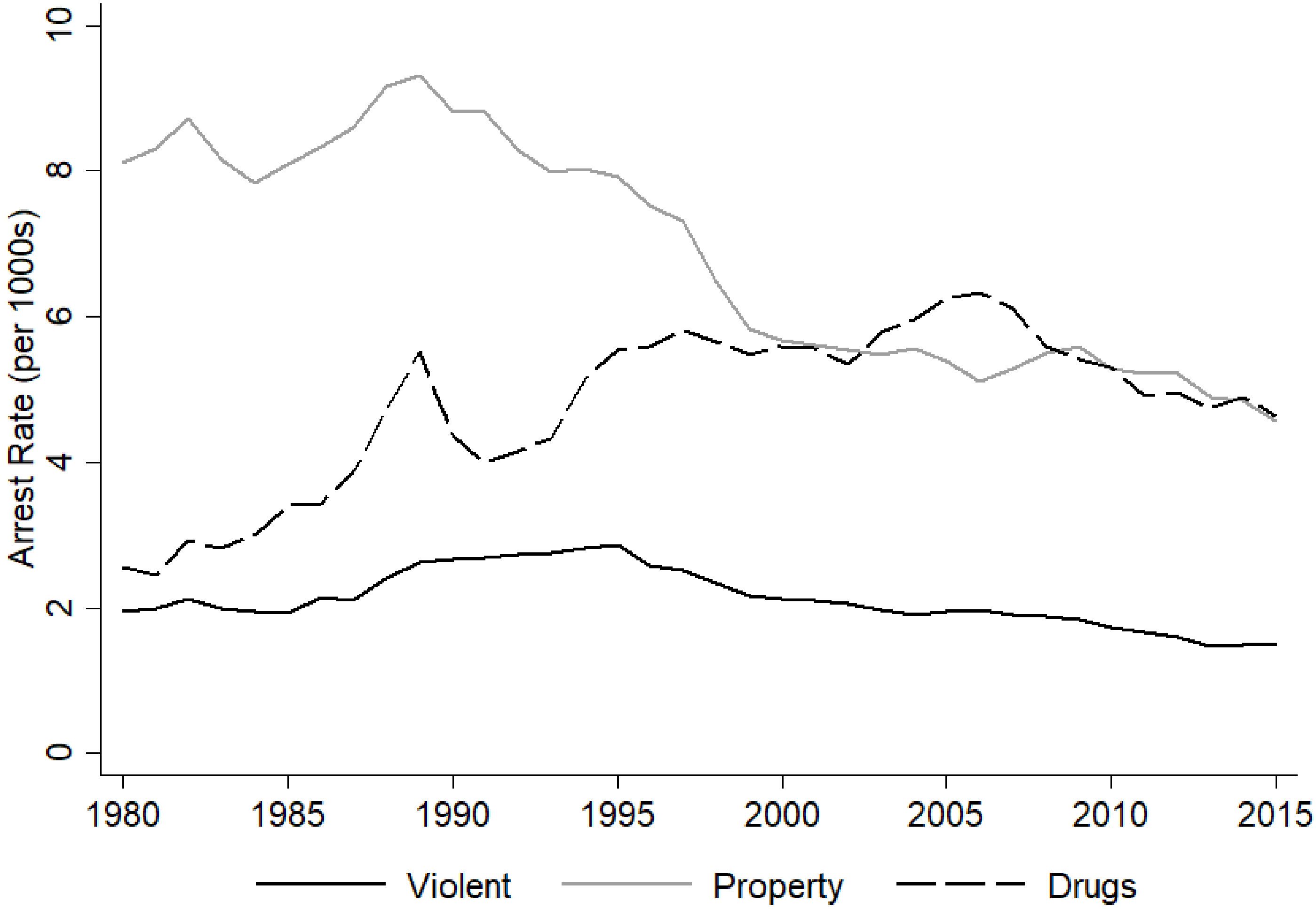
Notes: Crime rates are calculated and provided by Buonanno et al (2011) to whom the author is grateful.

Figure 2: Crime and Arrest Rates, 1960-2015



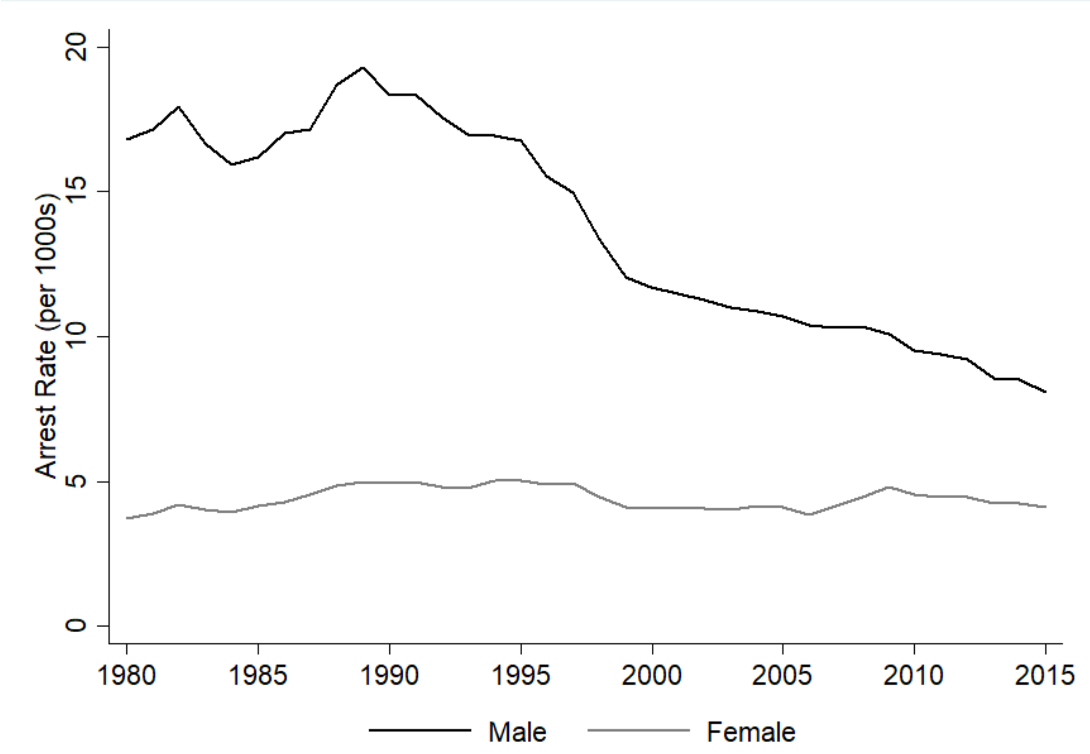
Notes: Crime and arrest rates are calculated by crime type: violent (murder and nonnegligent manslaughter, robbery and aggravated assault) and property (burglary, larceny and motor vehicle theft)*; Uniform Crime Report – Offense and Arrest Record*

Figure 3: Arrest Rates by Crime Type, 1980-2015



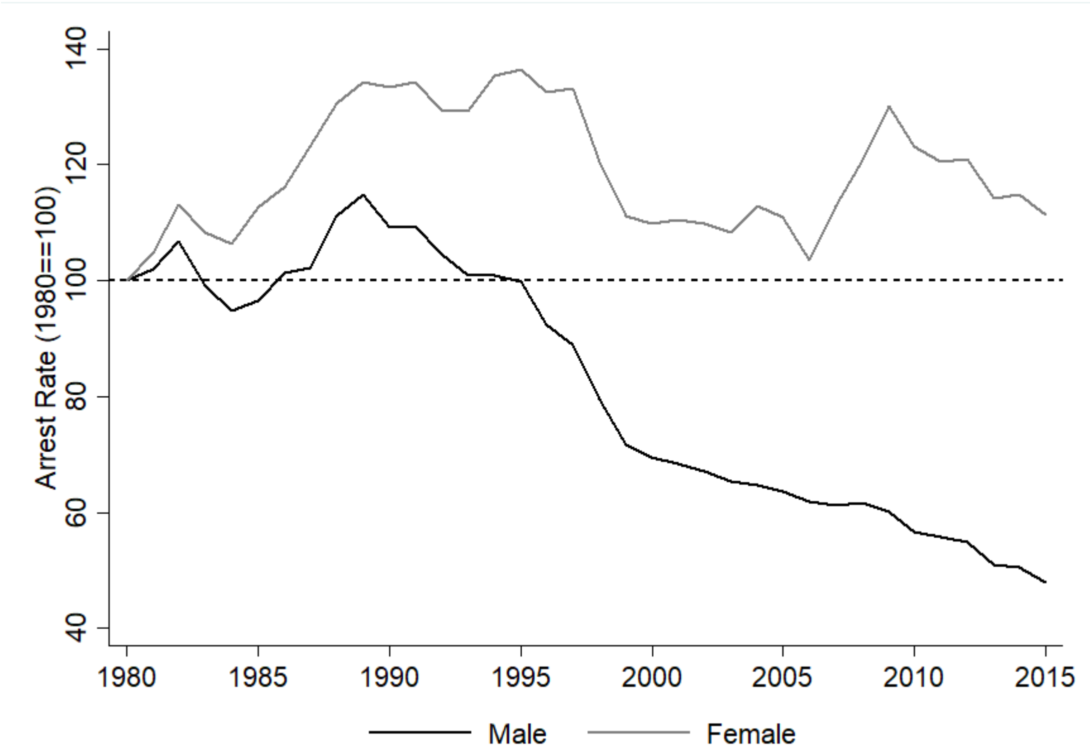
Notes: Arrest rates are by crime type: violent (murder and nonnegligent manslaughter, robbery and aggravated assault), property (burglary, larceny and motor vehicle theft) and drugs (possession and sales)*; Uniform Crime Report – Arrest Record*

Figure 4: Arrest Rates by Sex, 1980-2015



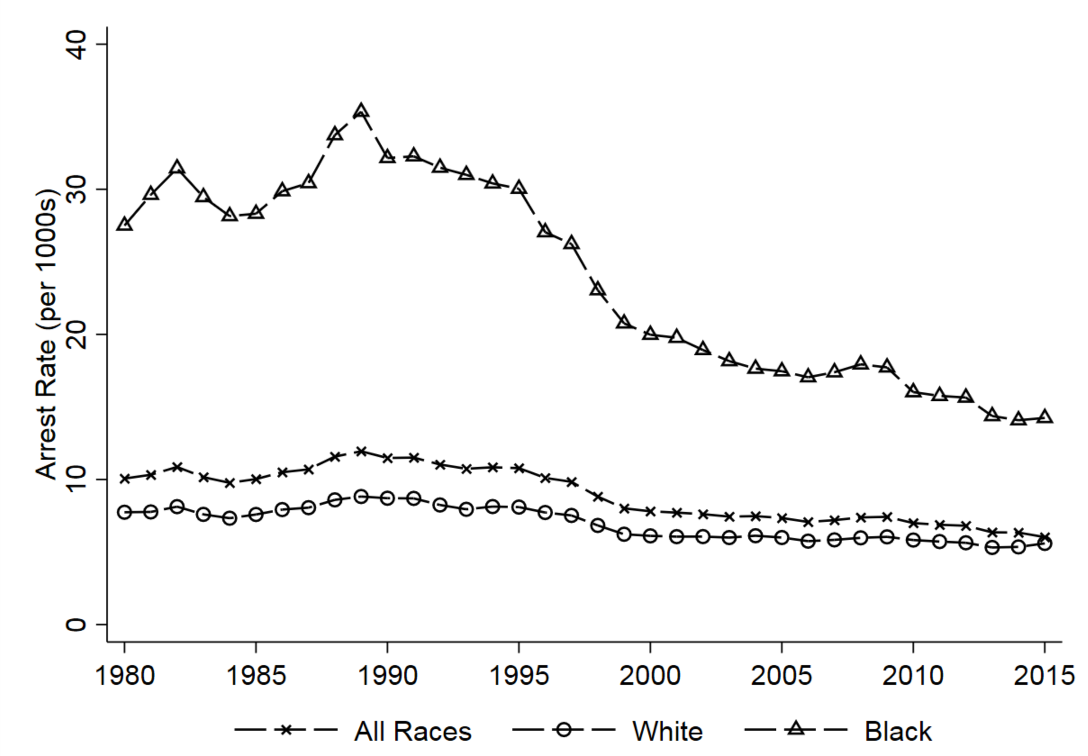
Notes: Arrest rates are calculated as the sum of violent and property crimes as defined in Figure 2; *Uniform Crime Report – Arrest Record*

Figure 5: Arrest Rates by Sex (Index 1980=100), 1980-2015



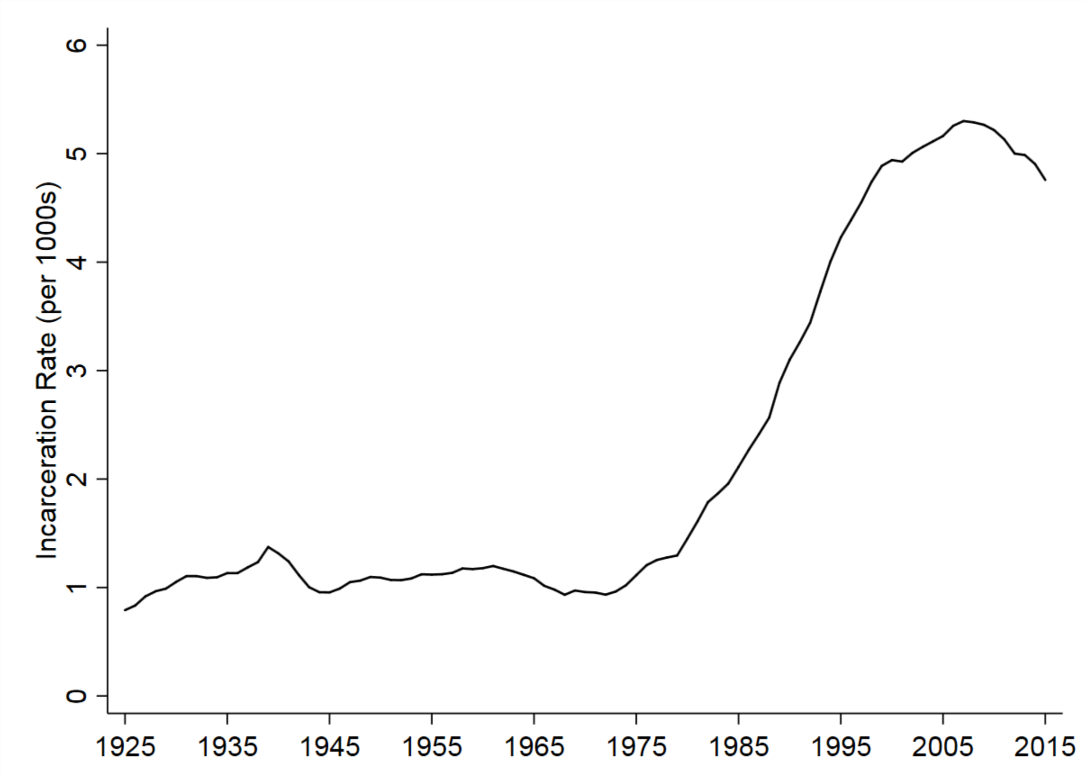
Notes: Arrest rates are calculated as the sum of violent and property crimes as defined in Figure 2; *Uniform Crime Report – Arrest Record*

Figure 6: Arrest Rates by Race, 1980-2015



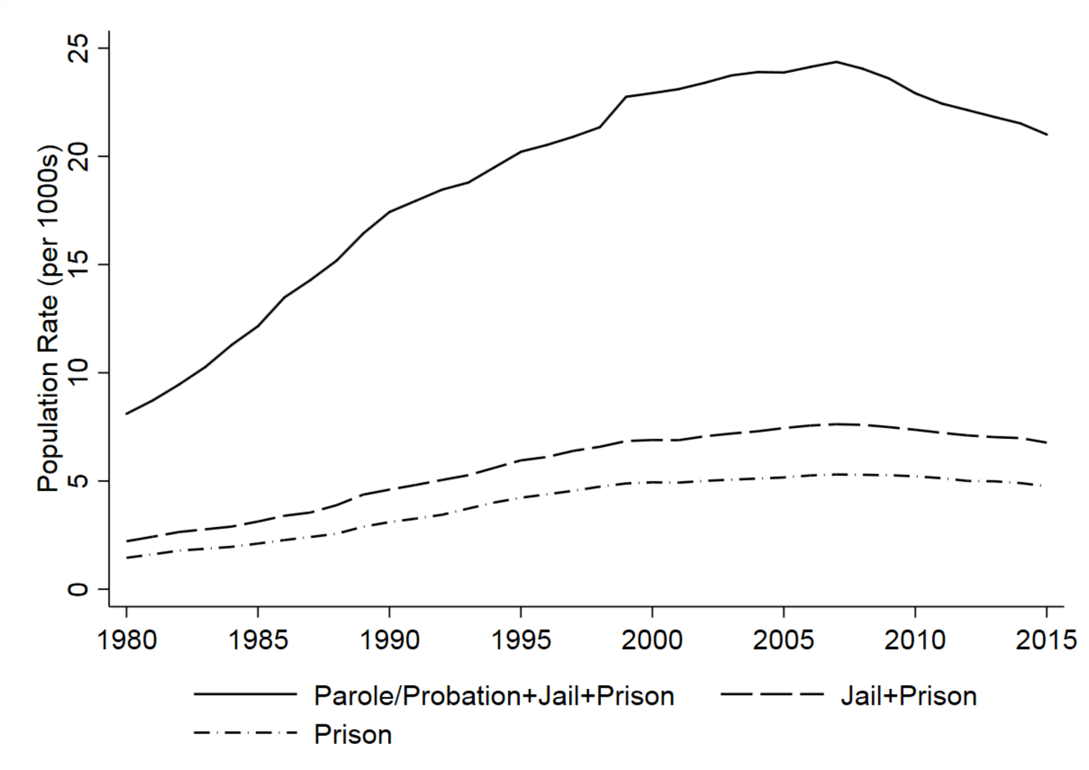
Notes: Arrest rates are calculated as the sum of violent and property crimes as defined in Figure 2; *Uniform Crime Report – Arrest Record*

Figure 7: Incarceration Rate in US, 1925-2015



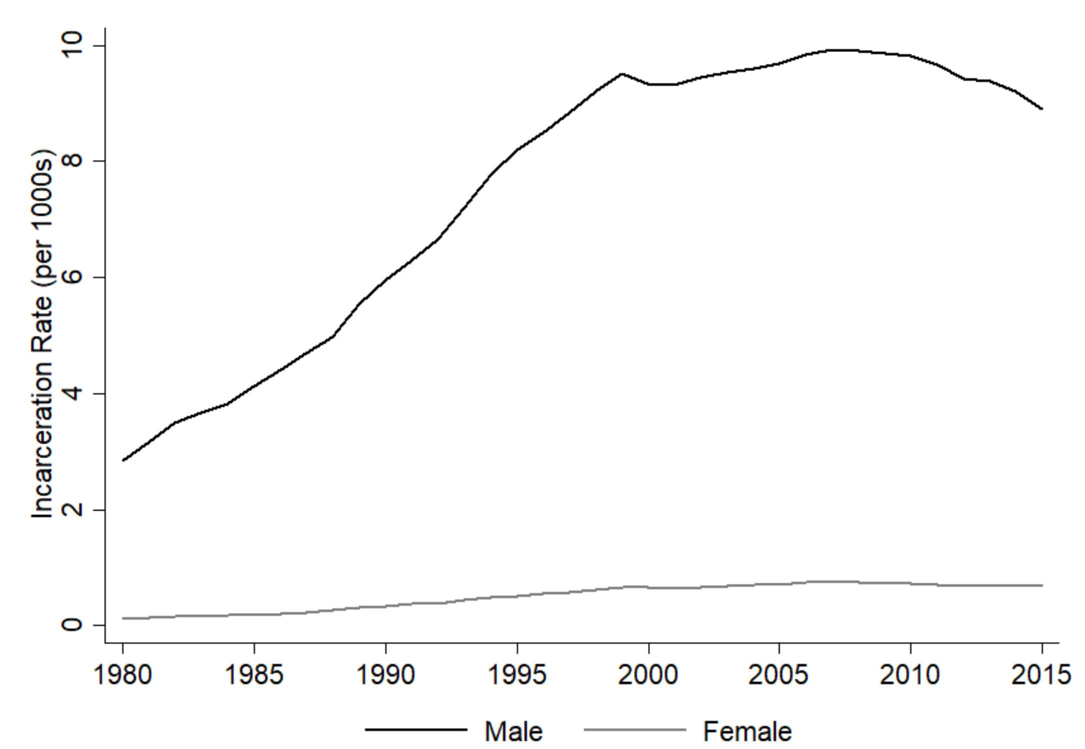
Notes: Incarceration rate is calculated for population of prisoners in state and federal prisons; *Bureau of Justice Statistics – Prisoners Series*

Figure 8: Correctional Population Rates Breakdown, 1980-2015



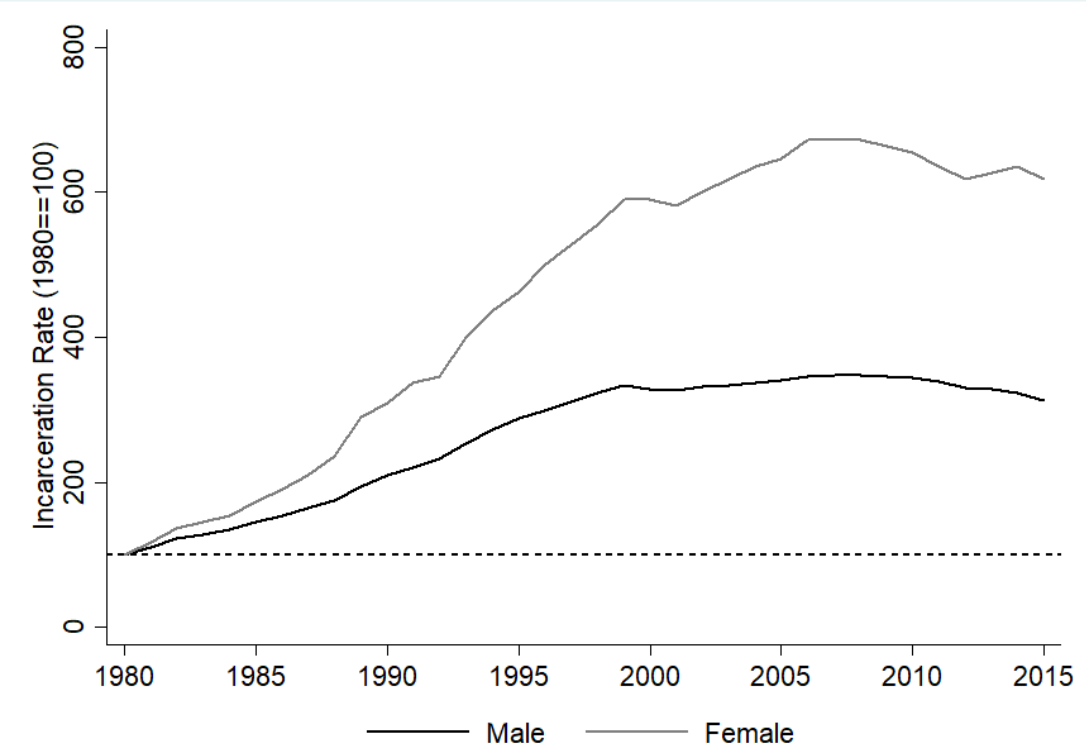
Notes: Population rates are calculated for population in specified correctional facilities/supervision programs and the US population; *Bureau of Justice Statistics – Total Correctional Population*

Figure 9: Incarceration Rates by Sex, 1980-2015



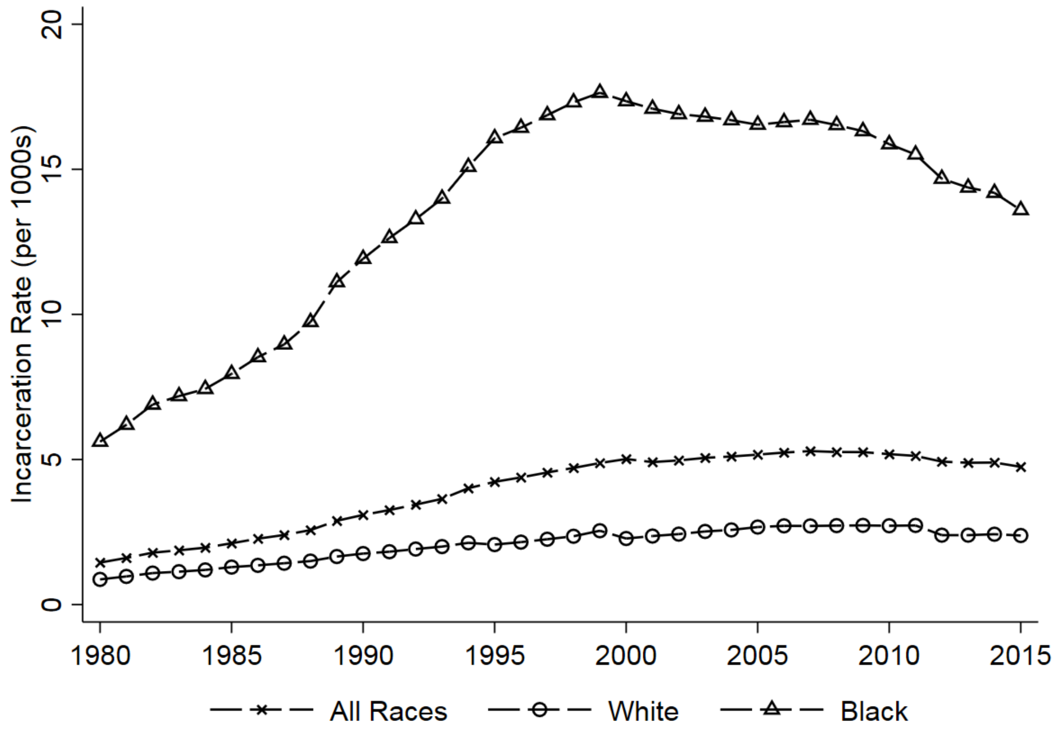
Notes: Incarceration rate is calculated for populations of prisoners in state and federal prisons; *Bureau of Justice Statistics – Prisoners Series*

Figure 10: Incarceration Rates by Sex (Index 1980=100), 1980-2015



Notes: Incarceration rate is calculated for populations of prisoners in state and federal prisons; *Bureau of Justice Statistics – Prisoners Series*

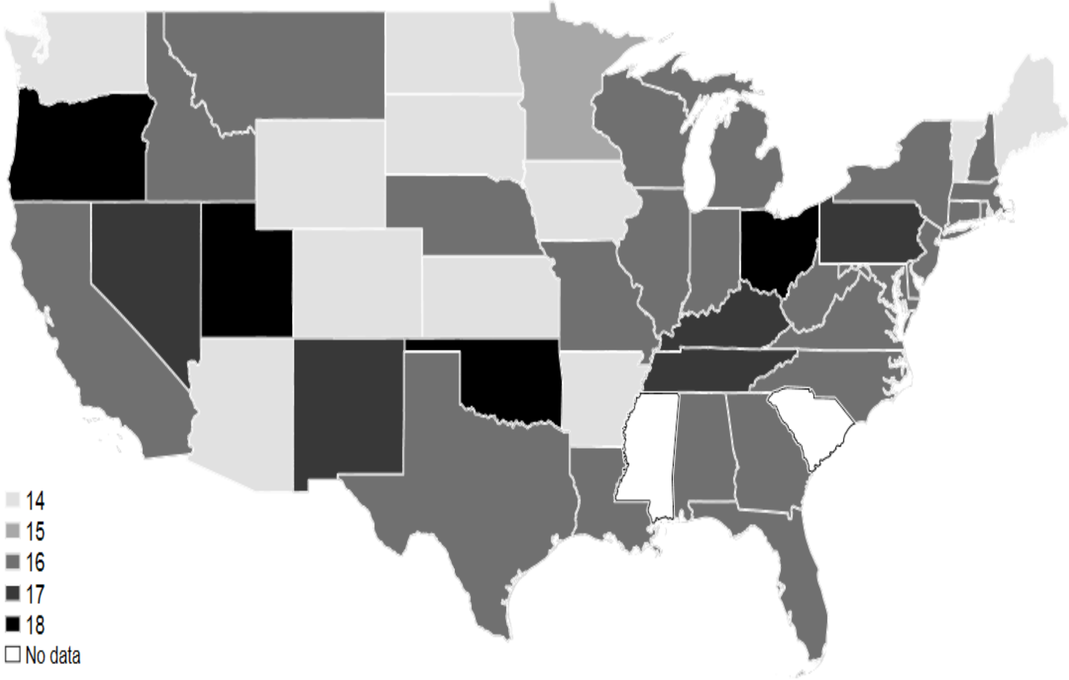
Figure 11: Incarceration Rates by Race, 1980-2015



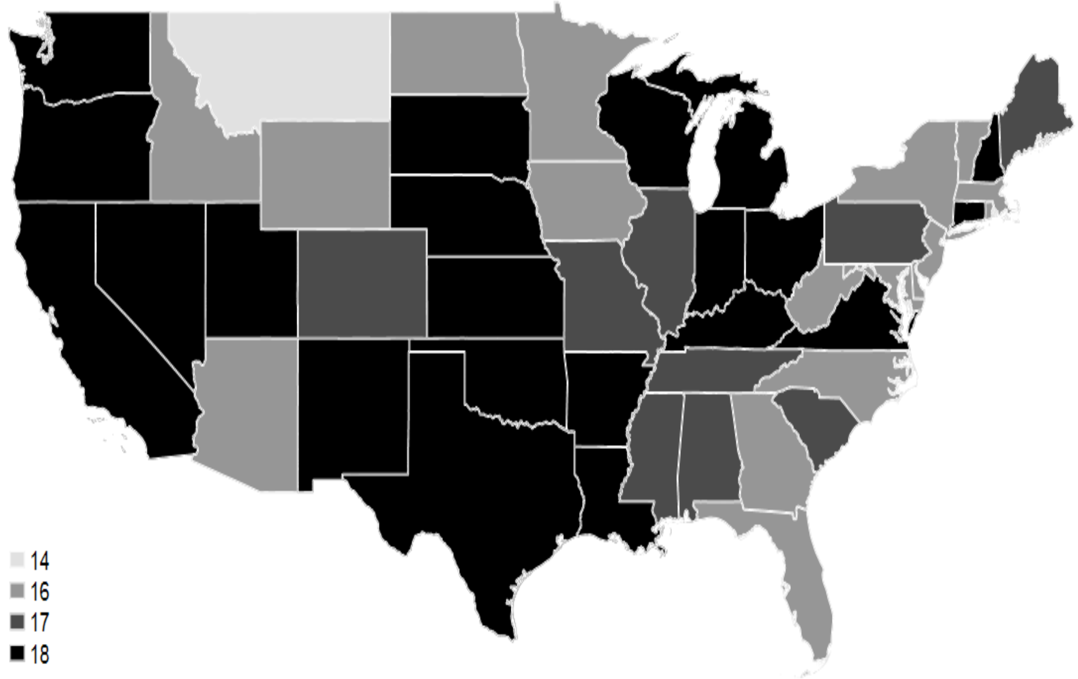
Notes: Incarceration rate is calculated for populations of prisoners in state and federal prisons; *Bureau of Justice Statistics – Prisoners Series*

Figures 12A and 12B: United States Maps - Dropout Age (DA_{st})

A. 1960



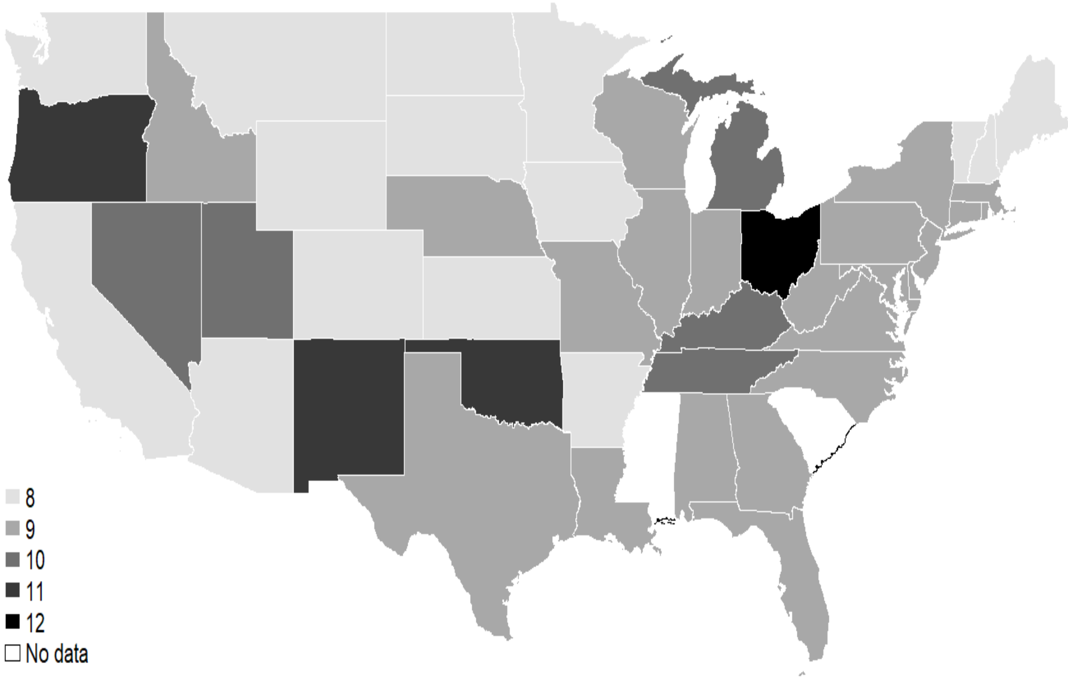
B. 2010



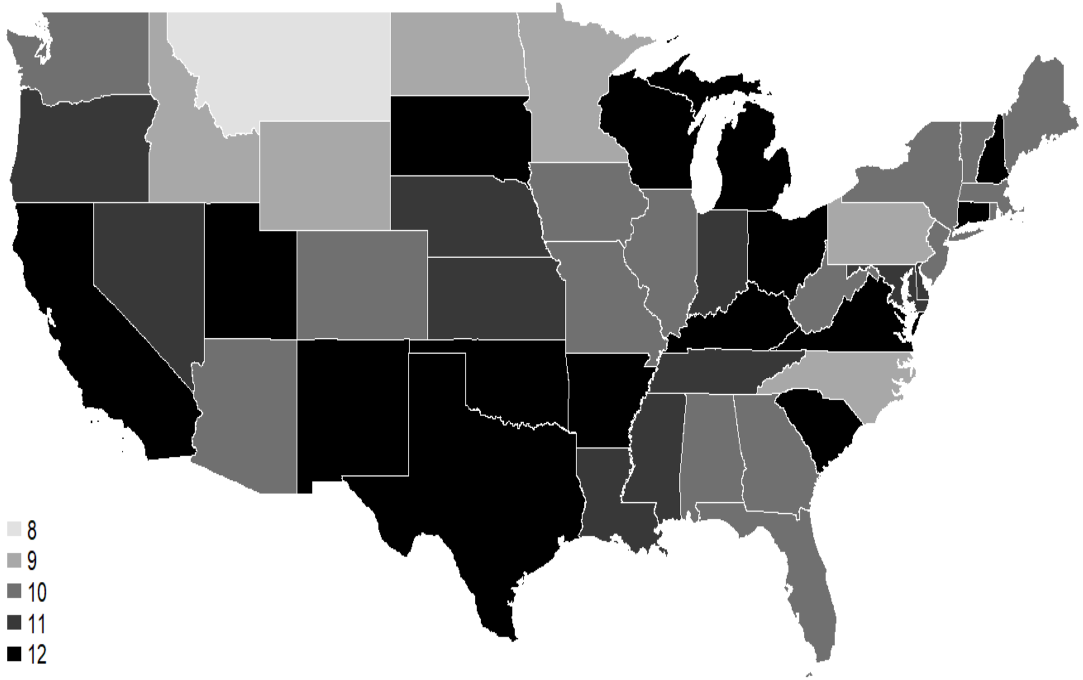
Notes: $DA_{st} \ = \ min\{Dropout\ Age_{st}\ ,\ Age\ at\ Grade\ Needed\ to\ Dropout_{st}\}$

Figure 13A and 13B: United States Maps - Dropout Age (CSL_{st})

A. 1960



B. 2010



Notes: $CSL_{st} = min\{Dropout\ Age_{st} - Entry\ Age_{st-8}, Years\ of\ School\ Needed\ to\ Dropout_{st}\}$

Table 1: Samples Summary Statistics

Variables	(1) All	(2) Whites	(3) Blacks
MALES			
Incarceration Rate (%)	1.713	0.940	7.005
	(12.975)	(9.650)	(25.524)
Dropouts Incarcerated (%)	4.014	2.152	11.391
	(19.628)	(14.510)	(31.770)
Years of Schooling	12.766	12.908	11.798
	(2.661)	(2.627)	(2.690)
Dropout Share (%)	18.587	17.008	29.396
	(38.900)	(37.571)	(45.557)
White Share (%)	87.259		
Black Share (%)	12.741		
Observations	7,072,699	6,225,072	847,627
FEMALES			
Incarceration Rate (%)	0.168	0.113	0.499
	(4.091)	(3.364)	(7.053)
Dropouts Incarcerated (%)	0.385	0.240	0.961
	(6.196)	(4.895)	(9.605)
Years of Schooling	12.766	12.908	12.184
	(2.460)	(2.401)	(2.569)
Dropout Share (%)	16.482	15.147	24.656
	(37.102)	(35.850)	(43.101)
White Share (%)	85.951		
Black Share (%)	14.049		
Observations	7,212,379	6,262,792	949,587

Notes: Samples includes all individuals aged 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Respondents whose state of birth was Alaska, Hawaii or the District of Columbia are excluded. Figures in parentheses are standard deviations. All results in the paper use population weights.

Table 2A: Males - Compulsory School Laws Effects on Incarcerated – Reduced Forms

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	1.713	1.713	0.940	0.940	7.001	7.001
A. Reduced Form (DA)						
DA=16	-0.135	-0.168	-0.035	-0.049	-0.448	-0.611
	(0.036)	(0.032)	(0.025)	(0.023)	(0.223)	(0.233)
DA=17	0.026	-0.039	0.017	-0.010	-0.518	-0.577
	(0.058)	(0.054)	(0.043)	(0.042)	(0.255)	(0.257)
DA=18	-0.258	-0.176	-0.071	-0.025	-0.846	-0.367
	(0.057)	(0.050)	(0.042)	(0.038)	(0.331)	(0.312)
B. Reduced Form (CSL)						
CSL=9	-0.038	-0.051	0.022	0.006	-0.429	-0.380
	(0.035)	(0.032)	(0.025)	(0.024)	(0.241)	(0.217)
CSL=10	0.062	0.002	-0.025	-0.057	-0.840	-0.862
	(0.044)	(0.039)	(0.031)	(0.028)	(0.283)	(0.251)
CSL=11	-0.089	-0.077	-0.005	-0.015	-0.956	-0.804
	(0.059)	(0.054)	(0.047)	(0.044)	(0.307)	(0.277)
CSL=12	-0.257	-0.126	-0.195	-0.128	-1.181	-0.559
	(0.066)	(0.059)	(0.046)	(0.041)	(0.396)	(0.330)
Observations	7,072,699	7,072,699	6,225,072	6,225,072	847,627	847,627
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all males ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate reduced-form linear probability model using alternative CSL instruments. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (all coefficient estimates are marginal effects multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 2B: Females - Compulsory School Laws Effects on Incarcerated – Reduced Forms

Female Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	0.168	0.168	0.113	0.113	0.499	0.499
A. Reduced Form (DA)						
DA=16	-0.016	-0.019	-0.016	-0.015	0.002	-0.028
	(0.008)	(0.008)	(0.007)	(0.007)	(0.050)	(0.050)
DA=17	0.010	0.007	-0.004	-0.003	0.058	0.040
	(0.014)	(0.013)	(0.012)	(0.012)	(0.059)	(0.059)
DA=18	-0.025	-0.007	-0.020	0.000	-0.121	-0.116
	(0.014)	(0.013)	(0.012)	(0.012)	(0.076)	(0.075)
B. Reduced Form (CSL)						
CSL=9	-0.017	-0.010	-0.008	-0.007	-0.088	-0.069
	(0.008)	(0.008)	(0.007)	(0.007)	(0.053)	(0.048)
CSL=10	-0.016	-0.004	-0.005	-0.008	-0.149	-0.126
	(0.011)	(0.010)	(0.009)	(0.009)	(0.063)	(0.056)
CSL=11	-0.025	-0.009	-0.012	-0.009	-0.112	-0.096
	(0.015)	(0.014)	(0.014)	(0.013)	(0.071)	(0.067)
CSL=12	-0.081	-0.039	-0.051	-0.029	-0.338	-0.304
	(0.017)	(0.015)	(0.014)	(0.014)	(0.085)	(0.078)
Observations	7,728,567	7,728,567	6,262,792	6,262,792	949,587	949,587
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all females ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate reduced-form linear probability model using alternative CSL instruments. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (all coefficient estimates are marginal effects multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 3A: Males - Causal Effect of Education on Incarceration – Years of Schooling (Instrument = Dropout Age)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	1.713	1.713	0.940	0.940	7.001	7.001
A. Years of Schooling						
OLS	-0.526	-0.528	-0.296	-0.296	-1.472	-1.476
	(0.010)	(0.010)	(0.006)	(0.006)	(0.034)	(0.034)
2SLS	-0.898	-1.271	-0.372	-0.358	-1.651	-3.004
	(0.214)	(0.228)	(0.181)	(0.182)	(0.862)	(1.156)
B. First-Stage (DA)						
DA=16	0.161	0.144	0.129	0.119	0.227	0.192
	(0.016)	(0.015)	(0.014)	(0.014)	(0.048)	(0.051)
DA=17	0.162	0.097	0.099	0.046	0.281	0.216
	(0.026)	(0.026)	(0.027)	(0.027)	(0.056)	(0.058)
DA=18	0.184	0.139	0.178	0.147	0.210	0.135
	(0.023)	(0.024)	(0.021)	(0.021)	(0.052)	(0.056)
F-Statistic	35.584	31.172	33.871	29.185	9.303	6.624
Observations	7,072,699	7,072,699	6,225,072	6,225,072	847,627	847,627
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all males ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS and 2SLS are multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 3B: Females - Causal Effect of Education on Incarceration – Years of Schooling (Instrument = Dropout Age)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	0.168	0.168	0.113	0.113	0.499	0.499
A. Years of Schooling						
OLS	-0.066	-0.066	-0.046	-0.046	-0.146	-0.147
	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)
2SLS	-0.154	-0.193	-0.180	-0.114	0.185	0.073
	(0.065)	(0.067)	(0.077)	(0.072)	(0.181)	(0.195)
B. First-Stage (DA)						
DA=16	0.125	0.120	0.088	0.089	0.268	0.246
	(0.013)	(0.012)	(0.012)	(0.011)	(0.048)	(0.053)
DA=17	0.104	0.061	0.049	0.014	0.271	0.228
	(0.020)	(0.019)	(0.021)	(0.020)	(0.052)	(0.055)
DA=18	0.136	0.105	0.129	0.109	0.171	0.123
	(0.018)	(0.018)	(0.016)	(0.016)	(0.051)	(0.054)
F-Statistic	30.790	30.952	26.738	27.404	14.319	11.768
Observations	7,728,567	7,728,567	6,262,792	6,262,792	949,587	949,587
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all females ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS and 2SLS are multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 4A: Males - Causal Effect of Education on Incarceration – Years of Schooling (Instrument = Goldin and Katz CSL)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	1.713	1.713	0.940	0.940	7.001	7.001
A. Years of Schooling						
OLS	-0.526	-0.528	-0.296	-0.296	-1.472	-1.476
	(0.010)	(0.010)	(0.006)	(0.006)	(0.034)	(0.034)
2SLS	-0.786	-0.452	-0.028	0.174	-1.131	-0.376
	(0.177)	(0.159)	(0.184)	(0.159)	(0.706)	(0.672)
B. First-Stage (CSL)						
CSL=9	0.149	0.143	0.095	0.098	0.286	0.271
	(0.016)	(0.015)	(0.015)	(0.014)	(0.048)	(0.050)
CSL=10	0.016	-0.001	-0.004	-0.016	0.200	0.194
	(0.018)	(0.016)	(0.017)	(0.016)	(0.050)	(0.051)
CSL=11	0.142	0.108	0.100	0.078	0.335	0.305
	(0.024)	(0.023)	(0.022)	(0.021)	(0.057)	(0.058)
CSL=12	0.126	0.113	0.107	0.108	0.317	0.294
	(0.026)	(0.024)	(0.025)	(0.023)	(0.055)	(0.057)
F-Statistic	36.535	43.836	23.650	30.098	17.449	12.896
Observations	7,072,699	7,072,699	6,225,072	6,225,072	847,627	847,627
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all males ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS and 2SLS are multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 4B: Females - Causal Effect of Education on Incarceration – Years of Schooling (Instrument = Goldin and Katz CSL)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	0.168	0.168	0.113	0.113	0.499	0.499
A. Years of Schooling						
OLS	-0.066	-0.066	-0.046	-0.046	-0.146	-0.147
	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)
2SLS	-0.188	-0.120	-0.239	-0.104	-0.238	-0.174
	(0.063)	(0.061)	(0.085)	(0.075)	(0.178)	(0.174)
B. First-Stage (CSL)						
CSL=9	0.107	0.102	0.058	0.063	0.254	0.228
	(0.013)	(0.011)	(0.012)	(0.011)	(0.042)	(0.044)
CSL=10	0.016	0.005	-0.004	-0.007	0.185	0.160
	(0.014)	(0.013)	(0.014)	(0.012)	(0.044)	(0.046)
CSL=11	0.090	0.066	0.041	0.031	0.322	0.287
	(0.018)	(0.018)	(0.017)	(0.017)	(0.052)	(0.053)
CSL=12	0.114	0.099	0.099	0.097	0.263	0.239
	(0.020)	(0.018)	(0.019)	(0.017)	(0.050)	(0.051)
F-Statistic	33.524	40.192	19.479	25.606	15.811	12.587
Observations	7,728,567	7,728,567	6,262,792	6,262,792	949,587	949,587
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all females ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS and 2SLS are multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 5A: Males - Causal Effect of Education on Incarceration – High School Dropout (Instrument = Dropout Age)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	1.713	1.713	0.940	0.940	7.001	7.001
A. High School Dropout						
OLS	3.788	3.788	1.949	1.949	10.535	10.543
	(0.078)	(0.078)	(0.038)	(0.038)	(0.219)	(0.219)
2SLS	11.520	11.078	3.968	2.648	25.677	34.728
	(2.458)	(2.138)	(1.790)	(1.514)	(11.375)	(15.702)
B. First-Stage (DA)						
DA=16	-1.509	-1.625	-1.259	-1.379	-1.767	-1.681
	(0.195)	(0.189)	(0.192)	(0.187)	(0.544)	(0.568)
DA=17	-1.236	-1.099	-0.685	-0.634	-2.385	-1.867
	(0.342)	(0.348)	(0.366	(0.366)	(0.682)	(0.690)
DA=18	-1.878	-1.879	-1.930	-2.036	-2.503	-1.622
	(0.292)	(0.300)	(0.284)	(0.288)	(0.699)	(0.695)
F-Statistic	22.026	25.604	20.303	23.799	4.782	3.002
Observations	7,072,699	7,072,699	6,225,072	6,225,072	847,627	847,627
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all males ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS, 2SLS and first-stages are multiplied by 100). High school dropout is defined as an indicator function for all individuals that fail to achieve an education level equal or higher than high school graduate. All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 5B: Females - Causal Effect of Education on Incarceration – High School Dropout (Instrument = Dropout Age)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	0.168	0.168	0.113	0.113	0.499	0.499
A. High School Dropout						
OLS	0.391	0.391	0.234	0.234	1.016	1.018
	(0.013)	(0.013)	(0.010)	(0.010)	(0.042)	(0.042)
2SLS	1.205	1.566	1.792	0.929	-2.284	-0.917
	(0.673)	(0.582)	(0.819)	(0.579)	(1.767)	(1.663)
B. First-Stage (DA)						
DA=16	-1.238	-1.473	-0.888	-1.142	-2.697	-2.823
	(0.206)	(0.209)	(0.196)	(0.196)	(0.615)	(0.670)
DA=17	-1.117	-0.854	-0.361	-0.194	-2.964	-2.677
	(0.325)	(0.320)	(0.360)	(0.345)	(0.674)	(0.705)
DA=18	-1.016	-1.031	-1.222	-1.356	-1.627	-1.238
	(0.290)	(0.302)	(0.277)	(0.281)	(0.695)	(0.750)
F-Statistic	12.484	17.326	9.234	14.129	9.924	9.771
Observations	7,728,567	7,728,567	6,262,792	6,262,792	949,587	949,587
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all females ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS, 2SLS and first-stages are multiplied by 100). High school dropout is defined as an indicator function for all individuals that fail to achieve an education level equal or higher than high school graduate. All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 6A: Males - Causal Effect of Education on Incarceration – High School Dropout (Instrument = Goldin and Katz CSL)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	1.713	1.713	0.940	0.940	7.001	7.001
A. High School Dropout						
OLS	3.788	3.788	1.949	1.948	10.535	10.543
	(0.078)	(0.078)	(0.038)	(0.038)	(0.219)	(0.218)
2SLS	8.381	4.084	2.641	-0.080	11.717	3.419
	(1.778)	(1.335)	(1.665)	(1.222)	(6.943)	(6.114)
B. First-Stage (DA)						
CSL=9	-1.603	-1.883	-1.058	-1.348	-2.633	-2.855
	(0.209)	(0.198)	(0.201)	(0.196)	(0.597)	(0.599)
CSL=10	-0.381	-0.497	-0.190	-0.261	-1.749	-2.092
	(0.226)	(0.219)	(0.226)	(0.220)	(0.635)	(0.641)
CSL=11	-1.443	-1.614	-1.099	-1.297	-3.412	-3.444
	(0.308)	(0.300)	(0.306)	(0.295)	(0.745)	(0.737)
CSL=12	-2.022	-2.269	-1.940	-2.216	-3.706	-3.581
	(0.321)	(0.310)	(0.320)	(0.309)	(0.749)	(0.731)
F-Statistic	28.558	38.299	18.998	27.311	11.762	10.540
Observations	7,072,699	7,072,699	6,225,072	6,225,072	847,627	847,627
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all males ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS, 2SLS and first-stages are multiplied by 100). High school dropout is defined as an indicator function for all individuals that fail to achieve an education level equal or higher than high school graduate. All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Table 6B: Females - Causal Effect of Education on Incarceration – High School Dropout (Instrument = Goldin and Katz CSL)

Male Incarcerated	(1) All	(2) All	(3) Whites	(4) Whites	(5) Blacks	(6) Blacks
Mean Dep Var	0.168	0.168	0.113	0.113	0.499	0.499
A. High School Dropout						
OLS	0.391	0.391	0.234	0.234	1.016	1.018
	(0.013)	(0.013)	(0.010)	(0.010)	(0.042)	(0.042)
2SLS	1.469	0.800	1.751	0.622	1.977	1.253
	(0.548)	(0.432)	(0.677)	(0.491)	(1.567)	(1.365)
B. First-Stage (DA)						
CSL=9	-1.240	-1.513	-0.615	-0.924	-3.054	-3.184
	(0.223)	(0.212)	(0.209)	(0.206)	(0.592)	(0.628)
CSL=10	-0.119	-0.169	0.256	0.204	-2.276	-2.451
	(0.236)	(0.220)	(0.238)	(0.220)	(0.601)	(0.644)
CSL=11	-0.604	-0.741	-0.067	-0.284	-3.464	-3.480
	(0.314)	(0.315)	(0.305)	(0.303)	(0.698)	(0.714)
CSL=12	-1.060	-1.216	-1.038	-1.264	-2.870	-2.923
	(0.310)	(0.288)	(0.314)	(0.287)	(0.668)	(0.706)
F-Statistic	17.596	25.611	12.743	19.912	9.530	8.747
Observations	7,728,567	7,728,567	6,262,792	6,262,792	949,587	949,587
Controls		Region*Cohort		Region*Cohort		Region*Cohort

Notes: Sample includes all females ages 18–39 in 1960, 1970, 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate OLS, 2SLS and first-stages models. The dependent variable is a dummy equal to 1 if the respondent is incarcerated as defined in the text (OLS, 2SLS and first-stages are multiplied by 100). High school dropout is defined as an indicator function for all individuals that fail to achieve an education level equal or higher than high school graduate. All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Standard errors corrected for state of birth-cohort clustering are in parenthesis.

Appendix

Individual-Level Micro Data on Incarceration

The micro data on US incarceration comes from the US Census. We sample all males aged 18-39 from the Integrated Public Use Microdata Series (IPUMS) for the 1 percent 1960, the two 1 percent state sample 1970, 5 percent 1980, 1990 and 2000 Census and the 5 percent 2008-2012 American Community Survey (ACS) 5-Year sample centred on 2010. We identify the institutionalized population using the Group Quarters (GQ) variable. The GQ variable consistently identifies the following categories:

a) Non-group quarter households;

b) Institutions (Correctional Institutions, Mental Institutions, Institutions for the elderly, handicapped and poor);

c) Non-institutional group quarters (Military, College dormitory, rooming house, other).

However only in the 1960, 1970 and 1980 IPUMS is the GQ variable detailed enough to uniquely identify those in correctional facilities. In subsequent Censuses (and the ACS), the institutionalized population includes the following categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. However, the share of the total institutionalized population accounted for by those in correctional facilities is very high in our sample.

Appendix Table A1 shows the institutionalized male population by GQ type and age. Note that this data comes from published aggregate Census reports that do break up the categories, though this is not available in the IPUMS data release. In 2000, for example, 95.3 percent of institutionalized males aged 18-39 where in correctional facilities. Two key points come from Table A1. First, incarcerated males aged less than 18 are much less well identified (since juvenile facilities are an important component for this group). We therefore restrict our analysis of the Census data to those aged 18-39.

Furthermore, since 1990 IPUMS reports variables on disabilities status of individuals. According to US Census Bureau as of 2010, only 2.4% and 4.8% of the adult population (18 or older) in correctional facilities report self-care and independent living disabilities respectively. Contrasting to 82% (independent living) and 70% (self-care) of adult population in the remainder of the institutionalized population (Mental Institutions, Institutions for the elderly, handicapped and poor). Given these numbers we can exclude people reporting self-care and independent living disabilities from our incarcerated definition, to narrow the match with correctional institutions). The results in the paper are robust to the absence of this further refinement in the definition.

Construction of Instrument

Data on compulsory schooling laws was retrieved from 2 main sources. Before 1978, we use the data compiled by Acemoglu and Angrist (2001). After 1978, we look at the official annotated statutes of each state in the Westlaw International Database for each of the corresponding years.

The data retrieved includes maximum entry age, minimum leaving age and education grade which exempts a child from staying in school. The laws have historically increased in their complexity adding several exemptions including work permits and early age parental consent letters to exemplify the most common. The Labor Standards Act 1939 harmonized child labour laws across states in the US, recent changes were not of a comparable order of magnitude as the ones seen during that period. To be consistent we ignore the possibility of parental consent authorizations to leave school at an age below the minimum dropout age, as these are often seen as exceptions rather than the rule.

Additional Table

Table A1: US Male Population in Group Quarters by Type and Age, 1980-2010

	Total Institutionalized	Correctional Institutions	Correctional as Percent of Total
1980 Census			
All	1232120	439720	35.7
15-17	68300	8460	12.4
18-21	123320	89600	72.7
22-24	104060	80240	77.1
25-39	301980	205780	68.1
1990 Census			
All	1801350	1030210	57.2
15-17	68480	16490	24.1
18-21	149780	128940	86.1
22-24	143890	133490	92.8
25-39	666690	581670	87.2
2000 Census			
All	2534060	1806260	71.3
15-17	87200	18960	21.7
18-21	221660	202470	91.3
22-24	201060	195660	97.3
25-39	951660	911050	95.7
2010 Census			
All	2716877	2059020	75.8
15-19	153924	74720	48.5
20-24	327760	308926	94.3
25-39	971581	945065	97.3

Notes: Data from 1980 are calculated from IPUMS data, figures for 1990, 2000 and 2010 come from the US Census Bureau.

Chapter 2

Crime, Compulsory Schooling Laws and Education*

Abstract

Do compulsory schooling laws reduce crime? Previous evidence for the U.S. from the 1960s and 1970s suggests they do, primarily working through their effect on educational attainment to generate a causal impact on crime. In this paper, we consider whether more recent experience replicates this. There are two key findings. First, there is a strong and consistent negative effect on crime from stricter compulsory schooling laws. Second, there is a weaker and sometimes non-existent link between such laws and educational attainment. As a result, credible causal estimates of the education-crime relationship cannot in general be identified for the more recent period, though they can for some groups with lower education levels (in particular, for blacks).

* This Chapter is based on Bell, B. D., Costa, R. and Machin, S. (2016) "Crime, Compulsory Schooling Laws and Education", Economics of Education Review, 54, 214-26.

1. Introduction

Few doubt that educational attainment and crime are related. Two examples illustrate this point. In the United States, 41% of inmates in prisons and jails in 1997 had not completed high school, compared to only 18% of the general population. Second, a survey of newly sentenced prisoners in the UK in 2005/6 showed that 47% had no educational qualifications, compared to 15% for the general population (Hopkins, 2012). What is less clear is whether this link represents a causal relationship running from educational attainment to criminal behaviour, or whether it merely reflects a whole set of personal characteristics associated with lower education levels that denote those on the margins of society. This is important for policymakers as they assess the potential social returns to education.

Significant research progress has been made on this question. Our overall reading is that there is quite strong evidence of the existence of a causal effect of education on crime, which is usually identified by using changes in compulsory schooling laws to generate exogenous variation in educational attainment.[19] Having said that, causal evidence using these laws that relates to recent experience is not available. For example, the most cited paper is that of Lochner and Moretti (2004) who study incarceration data that end in 1980, and arrest data that end in 1990. Anderson (2014) does study more recent arrest data for juveniles, but with a focus on whether changes in dropout age matter for crime, but not on education.

This paper adds to this literature in two key ways. First, we focus on both crime and education more recently. This is important since both crime and education patterns are different post-1980 than before. On the former, at least in the United States, the patterns of crime and incarceration are very different before and after 1980. The substantial rise in the prison population was predominantly in the later period, while total property crime has fallen substantially. On the latter, education levels are higher (in part reflecting longer run trends) and

[19] See also the review of the empirical literature on education and youth crime by Rud et al. (2013).

also part of the reason for the focus on the earlier period is that many of the major changes to compulsory schooling laws occurred prior to 1980, which is the variation in the data used to identify the exogenous movement in schooling (see Stephens and Yang, 2014). However we show that there is still substantial variation in these laws over the 1980-2010 period – though we emphasise that the group of students affected by the laws may be very different from those affected by earlier changes (i.e. the composition of the compliers may not be stable over time). This change in composition then may generate different effects on both education and crime.

A second feature is that we exploit a new set of geographical groupings for the continental United States that allows us to consistently combine data on arrests from the FBI Uniform Crime Reports (UCR) with micro-data from the US Census. One issue with the micro-data from the Census is that it has traditionally been hard to construct any consistent geography below the state-level that can be identified across successive Censuses. So for example, county-level data cannot be used because only the larger counties are uniquely identified in the micro-data. Autor and Dorn (2013) have painstakingly generated commuting zone areas for the US that can be consistently identified from the 1970 Census onwards. We have been able to match these areas to UCR reporting agencies, though the arrest data are only available on a consistent basis from 1974 onward, which restricts us to starting with the 1980 Census - so we study the 1980-2010 time period.

The rest of the paper is organised as follows. In Section 2 we outline an empirical framework for thinking about the empirics of education and crime, and briefly review the extant evidence on the causal relationship between education and crime. Section 3 discusses the data we use in this paper and the empirical methodology. In Section 4 we present a range of evidence on the reduced-form relationship between crime and compulsory schooling laws. We show that there is a consistently strong negative link between crime, measured either by arrests or incarceration, and schooling laws. We also present evidence to show that some of this effect appears to be a result of incapacitation, but this cannot explain all of the effect. In Section 5 we assess whether these results allow us to identify a causal effect of education on crime. The evidence turns out to be mixed. For whites, who on average have higher education levels, our answer is no. This is because of the difficulty in generating a strong and coherent first-stage for schooling from the school leaving laws, in contrast to the earlier evidence from the 1960s and 1970s. In contrast, for blacks we can still estimate a causal crime reducing effect of education – though even here the power of the first-stage is weaker than in the past. The conclusions are in Section 6.

2. A Framework and Previous Findings

Empirical Set Up

To motivate our analysis, consider the basic relationship between crime and schooling:

$$Crime_i = \beta Schooling_i + \gamma X_i + \varepsilon_i \tag{1}$$

where X is a set of control variables and ε_i an error term. At the moment (1) is pitched as an individual-level (denoted by i) regression of crime on schooling that holds constant the factors included in X. We will generalise this in several dimensions in our actual empirical work, as detailed below, but the relatively simple formulation in (1) serves its purpose for motivating our analysis.

Schooling is unlikely to be exogenous in (1). Thus to generate an estimate of β that yields a causal impact of schooling on crime we require an instrument that satisfies the usual conditions. In this paper, as with much of the literature, alternative measures of compulsory schooling laws (CSL) will serve as the instrument. Therefore underpinning an Instrumental Variable/Two Stage Least Squares (IV/2SLS) estimation of (1) we have reduced-form equations for crime and schooling (the first stage) which are given respectively as:

$$Crime_i = \theta_1 CSL_i + \pi_1 X_i + \varepsilon_{1i} \tag{2}$$

$$Schooling_i = \theta_2 CSL_i + \pi_2 X_i + \varepsilon_{2i} \tag{3}$$

where the IV (2SLS) estimate of the coefficient on the schooling variable in (1) is the ratio of the reduced-form coefficients in (2) and (3), so that $\beta = \theta_1/\theta_2$. In general, we would expect $\beta < 0$ since higher educational attainment is expected to reduce crime. This works through the two reduced-forms as θ_2 will be positive if schooling laws increase schooling, and the schooling laws reduce crime so that $\theta_1 < 0$.

In understanding a causal crime reducing impact of education in this set up, Lochner and Moretti (2004) note there is no simple relationship between the strength of the reduced-form for crime in (2) and the strength of the IV estimate in (1). On the former, θ_1 can be negative for at least two reasons. First, if there is indeed a causal link between education and crime *and* schooling laws increase education, then the coefficient on CSL in the reduced-form for crime will pick up this effect. An alternative, though in no sense mutually exclusive, possibility is that such laws can directly reduce crime, over and above their effect on educational attainment.

If there is a direct impact of school leaving laws on crime, to the extent that such laws force juveniles to be in a supervised environment rather than roaming the streets, then this can be interpreted as a straightforward incapacitation effect that reduces crime.[20] There is a body of evidence to support this which uses plausibly exogenous changes in the length of the school day or exploits random days in which schools do not open to identify incapacitation effects (Jacob and Lefgren, 2003, and Luallen, 2006).[21] However, we also know that criminal behaviour peaks in the late teenage years. If the incapacitation effect reduces criminal activity

[20] Other types of incapacitation effects that have been studied include Black at al.'s (2008) work on teenage births and Galiani et al. (2011) on conscription and crime.

[21] Earlier (non-causal) estimates of incapacitation (time spent in school) effects were presented in Gottfredson (1985), Farrington et al. (1986) and Witte and Tauchen (1994). Hjalmarsson (2008) looked at the opposite relationship, studying the impact of being arrested and incarcerated before finishing school on the probability of graduating high school and reported a strong negative association.

at these crucial ages, it may in addition generate a persistently lower crime rate as the cohort ages, since some of the cohort members will have avoided going down the wrong path at a crucial age. In other words, estimating $\theta_1 < 0$, is necessary but not sufficient to find a causal estimate of education on crime.

Existing Evidence

It is useful to briefly summarise the extant evidence on the links between crime, compulsory schooling laws and education. The key reference is Lochner and Moretti (2004) (hereafter LM). They present a wide range of evidence on the causal effect of education on crime in America, for the 1960s-1980s. They show that for various measures of criminal activity there is strong evidence that schooling laws, working through the effect on education, reduced crime. For example, the estimated OLS coefficient on years of schooling in a regression for incapacitation is -0.10 for whites and -0.37 for blacks. This implies that an additional year of schooling generates a 0.1 percentage point reduction in the probability of incarceration for whites. The corresponding IV estimates are -0.11 and -0.47. The schooling laws that they consider, which are the same source as the variation used in this paper, generate a strong first-stage. The *F*-statistic on the instruments ranges from 36 to 88 and all the coefficients in the dummy variable functional forms they adopt are positive. Though they do not present a full table of reduced-form crime estimates, they report that the link between the school-leaving age instruments and incarceration are fairly weak. The *F*-statistic on the instruments in the crime reduced-form gives p-values of 0.023 for Whites and 0.100 for Blacks.

Machin et al. (2011) consider the impact on education and crime of the national extension of the school leaving age in England and Wales from age 15 to 16 that occurred in the 1972/3 school year. Using data on criminal convictions, they show that the school-leaving age increase *both* reduced crime and increased educational attainment in the reduced-form and first-stage, and generated a significant 2SLS causal estimate for the effect of education on crime – roughly double the size of the corresponding OLS estimate. The reduced-form effect on crime was observed for both property and violent offences for men, but was neither substantial nor significant for women. While the instrument in the reduced-form for crime was less strong than in the first-stage (11.6 v 67.4 in Table 3 of their paper), it was still noticeably stronger in statistical terms than the estimates in LM.

Evidence from a schooling reform in Sweden has been analysed by Hjalmarsson et al. (2015) and Meghir et al. (2012). The reform extended compulsory schooling from seven to nine years and was implemented at different times across municipalities during the 1950s and 1960s. Results from Hjalmarsson et al. show that the reforms generated a strong first-stage relationship with years of schooling, with the reform increasing average years of schooling by between 0.3 and 0.6 years (*F*-statistic ranging from 93 to 171). Though the crime reduced-form is less powerful (generally only significant at the 10% level), it is consistently negative. Overall, the 2SLS estimates are generally of the same magnitude as the OLS.

The potential role of compulsory schooling laws such as those considered in this paper in generating a direct incapacitation effect has recently been examined by Anderson (2014). He exploits the state variation in the minimum dropout age to estimate the effect on arrest rates of juveniles over the period 1980-2008. Using county-level data he finds that minimum dropout age requirements have a significant and negative effect on property and violent crime arrest rates for individuals aged 16 to 18, compared to a control group of 13-15 year olds. He also shows that the effect is substantially larger in counties that have an above-median share of blacks in the population, suggesting that higher minimum dropout age requirements may be more effective in reducing crime in areas with a relatively large African American population.

3. <u>Data Description</u>

We report results from two main sources, studying age-specific arrest data in local labour markets through time, and imprisonment from individual-level Census data. To both of these we match data on compulsory school-leaving laws. In this section we discuss these in turn. More details are provided in the Data Appendix.

Local Labour Market Data

Our initial analysis makes use of local arrest rates across the United States that are measured annually for different age groups. The arrest data comes from the FBI Uniform Crime Reports (UCR). The UCR provides arrest totals by age and sex for a variety of offences for each reporting agency. We also need data on CSL and education (and any other control variables) at the same unit of analysis, and take these data from the US Census from the Integrated Public Use Microdata Series (IPUMS). It follows that the key issue is how to produce a consistent definition of locality whereby we can match arrest rates to schooling data. LM exploit state-level data to achieve this match, whilst Anderson (2014) uses county-level data but does not consider education data. County-level data has the distinct advantage of having a large cross-section, but counties are not consistently identified in the Census across years, so one cannot link the arrest data very well to the micro Census files. We therefore pursue an alternative geography that allows for us to achieve both objectives.

Autor and Dorn (2013) analyse commuting zone (CZ) level data to study local labour market evolutions and, in particular, the rise of service sector jobs in the United States. CZs are defined as clusters of counties that have strong commuting ties within the CZ and weak commuting ties across them. There are 741 CZs for the United States and they can be consistently constructed using Census Public Use Micro Areas (PUMAs) for all the years of our analysis. Excluding Alaska, Hawaii and the District of Columbia (for which we do not have the instruments), leaves 722 CZs. We have then matched each UCR reporting agency to their respective CZ. In cases where the reporting agency straddles more than one CZ, the arrest data is allocated in proportion to the population in each CZ.[22] We are able to successfully match 714 CZs to UCR data for at least one point in time.

We use arrest data by CZ, criminal offence and age for the Census years 1980, 1990, 2000 and 2010. We restrict the sample to males aged 16-39. In most of our analysis we distinguish between property crime (burglary, larceny, vehicle theft and arson) and violent crime (murder, rape, robbery and assault). One issue with the UCR data is that some reporting agencies do not always report (or do not report fully). Thus for example, there is no data recorded by the NYPD for 2010. In our empirical analysis we restrict attention to those CZs that have arrest data reported by the constituent reporting agencies to cover at least 90% of the population in a given year. We report various robustness checks on doing so below. For the main analysis (on a balanced panel with data for all four Census years) we can study 306 CZs between 1980 and 2010.

Unfortunately the UCR arrest data does not provide a racial breakdown by age. It only provides arrests by race for two age groups, juveniles and adults. As we will be interested in the differences between whites and blacks, we calculate the average share of blacks in the commuting zone population over the whole sample period and split the CZ sample into "low black share" CZones and "high black share" CZones, with the latter being all those above the 80^{th} percentile in terms of black share of the population. At the cut-off, the share of blacks in the population is 18.1% and the highest black share is 50.5%.

[22] More specifically, over the period from 1980-2010 about 12% of agencies cover overlapping areas. Since the recorded population of these areas overlap, the agency with highest population inside each commuting zone is kept for the purpose of calculating the covering ratios. Despite excluding lower population overlapping agencies to calculate covering ratios, the numbers of arrests are summed to the highest population agencies and there is no problem of double reporting of arrests. See the Appendix for further details on the matching procedure.

Census Individual-Level Data

We also report individual-level evidence on incarceration using the IPUMS Census/ACS data. The 1980 Census contains a detailed Group Quarters variable that allows identification of people in correctional facilities on the Census date. Unfortunately the micro-data for the 1990 and 2000 Census and the 2010 ACS does not provide this detailed breakdown. However, Bell et al. (2018) show that for the sample of males aged 18-39, those in correctional facilities account for between 90% and 95% of the broader Census Group Quarters variable of institutionalized quarters – which is identified in the later samples. We construct a sample of all males aged 18-39 in the 1980, 1990 and 2000 Censuses and the 2010 ACS (5% Sample). Note therefore that in contrast to the arrest data, we cannot include 16-17 year olds as too many of this age group who are in institutionalized quarters are not in correctional facilities.

There are two main benefits from using the incarceration data. First, since we can never measure underlying criminal propensity, it is usually beneficial to consider alternative measures of criminality. Second, we can break the sample by race and consider whether the effects differ between whites and blacks. As discussed above, we can only do this imperfectly with the CZ data.

Table 1 reports some summary statistics on the CZ panel and the Census micro-data that we use for our main results. The upper panel of the Table shows that in our fully balanced CZ sample over the whole sample period, the total arrest rate is 0.035, or 35 arrests per 1,000 population. The arrest rate is marginally higher in those commuting zones with a higher share of blacks in the population (at 0.042 v 0.034) and the decomposition by crime type shows that the division is broadly even between arrests for violent and property crime. On average, the sample of males have just over 13 years of schooling.

The individual-level micro-data from the Census, reported in the lower two panels of the Table, show a very stark, and well known, difference between whites and blacks in terms of incarceration. In the full sample descriptive statistics, shown in the middle panel of Table 1, 1.6% of white males aged 18-39 are incarcerated, compared to 8.8% of blacks. The Census summary statistics (for the full sample) also show that a remarkable 17.7% of blacks who failed to gain a high school diploma are in prison (compared to 4.7% of whites). On average, blacks have just under one year less schooling than whites and are almost twice as likely to have dropped out of high school prior to graduation.

Finally, the last panel of Table 1 shows that the characteristics of the population are almost identical when we restrict the Census sample to those resident in the commuting zones that are reported in the first panel. This suggests that the restrictions on which commuting zones we can use in our analysis, as a result primarily of missing UCR data, do not generate an unrepresentative sample.

*Compulsory School Leaving La*ws

We consider two alternative measures of the instrument.[23] One is an updated version of that used by Goldin and Katz (2008). This is constructed for those aged 14 in year t and born in state s as:

$$CSL_{st} = min\{Dropout\ Age_{st} - Enrollment\ Age_{st-8}, Years\ of\ School\ Needed\ to\ Dropout_{st}\} \quad (4)$$

The first term in (4) measures the minimum number of years required to attend school without using any of the exceptions to the law. The second term captures any exception that allows children to leave before the dropout age.[24] This is essentially the instrument introduced by Acemoglu and Angrist (2001) and used by LM. The only difference is that, as Goldin and Katz (2008) point out, since the second term is an exception that allows the child to do less

[23] See the discussions of different CSL definitions in Stephens and Yang (2014) and Oreopoulos (2007).
[24] See Oreoupoulos (2007) for more detailed discussion on the nature of exemptions and a Table summarizing them for some of the more recent law changes.

compulsory schooling than the first term requires, the CSL should be measured using a *min* function rather than a *max* – as Acemoglu and Angrist used.

In our empirical work, we follow what has become the normal practice of using dummy variables for different levels of CSL. For the Goldin-Katz instrument, we define dummies for values of the instrument of 10, 11 and 12 years respectively, with 9 or below as the baseline. Alternatively, the second instrument we use is the minimum Dropout Age, which has been used by Oreopoulos (2007) and Anderson (2014). Again we use two dummies to identify age 17 and age 18 as minimum dropout ages.

Figures 1A and 1B show the percentage of the population of males aged 18-39 that are allocated to the different values of the two alternative instruments. We split the sample period into 1980-90 and 2000-10 to capture changes within the sample, and include 1960-70 as a comparison. Two things are clear. First, there is a pronounced rightward shift in the distribution for both instruments over time. This reflects the general trend to raise schooling requirements across most states. For example, in the 1960-70 period, only 20% of males aged 18-39 had been subjected to a minimum dropout age above 16. By 2000-10, this proportion had risen to 43%. Second, there remains considerable cross-sectional variation in the schooling requirements. For our full sample, the minimum dropout age varies from 14 to 18, with a standard deviation of 0.8 years. Similarly, the Goldin-Katz instrument has a standard deviation of 1.3 years.

4. Crime and Compulsory Schooling

CZ Crime Reduced-Forms

We begin our analysis by focusing on the reduced-form crime equation (2) for the commuting zones arrests panel. Given the construction of the arrests panel, for individuals of different ages measured at the level of local labour markets (commuting zones) through time, the actual equation that is estimated takes the form:

$$Crime_{ast} = \theta_1 CSL_{ast} + \pi_1 X_{ast} + \varepsilon_{1ast} \tag{5}$$

where $\mathrm{Crime}_{\mathrm{ast}}$ is the log of the arrest rate for age group a in commuting zone s at time t. CSL denotes either the Goldin-Katz instrument or dummy variables capturing minimum dropout ages, whilst $\mathrm{X}_{\mathrm{ast}}$ controls for relevant observables. We also control for age, cohort (c = t – a), commuting zone and year effects by specifying the components of the error term as $\varepsilon_{1ast} = \alpha_a + \alpha_c + \alpha_s + \alpha_t + e_{1ast}$.[25]The Census micro-data allows us to construct commuting zone level measures of the CSL that account for the fact that individuals living in a particular commuting zone may have been exposed to schooling laws in another state.

We begin by considering estimates of the crime reduced-form in (5) for the balanced CZ panel. Table 2 presents the estimates, for the minimum dropout age in the upper panel and for the Goldin-Katz instrument in the lower panel. In each panel results from six specifications are reported, with reduced-form estimates shown separately for property and violent crime for all CZs and separately for low-black share and high-black share CZs.

The first point to make is that across all twelve specifications, the predominant picture is one of significantly negative coefficient estimates. Out of thirty reported coefficients, twenty four are significant and negative (at the 10 percent level or better). So regardless of which

[25] To address the standard issue of age, cohort and time effects, we fully saturate the model, leaving out the dummy for age = 39. Throughout we obtained very similar results if we placed more structure on the age effects, for example by including a quartic in age.

instrument we use, we find a generally consistent negative effect of compulsory schooling laws on arrest rates. Moreover, the magnitude of the estimated effects are substantial. As an example, if we take the coefficients in the first column of the upper panel, a one year rise in the minimum dropout age from 16 (the base) to 17, reduces the log arrest rate for property crimes by 0.126, or a 12.6% fall in arrests.

But there are some nuances in the results of Table 2. For the full sample of CZs, closer inspection of the reported patterns reveals a strong reduced-form for both instruments for property crime. A less strong relation for violent crimes is seen for the minimum dropout age instrument. There is also some evidence that the effect of the dropout age laws are more substantial in the high-black share CZs with respect to violent crime.[26] Finally, one should recall that this sample includes those aged 16 and 17, so it is possible that at least some of the effect that we are capturing here is a direct incapacitation effect, like that documented by Anderson (2014). We will return to address this explicitly later in this section.

Census Crime Reduced-Forms

In Table 3 we switch attention to the incarceration reduced-form based upon the individual-level Census data. Estimates from the individual-level reduced-form given in equation (2) of section 2 are reported, for specifications controlling for compositional variables (the X's) and also additionally for age, cohort, state of birth, state of residence and year effects in an analogous way to the CZ models. Table 3 shows estimates reported in a similar structure as for the CZ analysis of Table 2, except now we cannot delineate between property and violent crime and the race breakdowns are at the individual, rather than the spatial, level. We also show

[26] The results in Table 2 are for the balanced panel of 306 commuting zones. As noted above, we do lose a number of observations owing to reporting agencies not submitting data in particular years, especially in 2010. If we therefore look at an unbalanced panel that requires three continuous CZ observations between 1980 and 2010 we can raise the sample size to cover 461 commuting zones. Appendix Table A2 reproduces the same estimates as those in Table 2 for this unbalanced sample, which reassuringly produces very similar estimates.

results for the full sample and restricting to individuals in the CZs considered in Table 2. Since the dependent variable is dichotomous, we estimate probit models and report marginal effects.

As with the arrest reduced-form results from Table 2, the general pattern that emerges in Table 3 is of significantly negative estimated coefficients on the schooling law dummy variables. For all individuals, higher dropout age laws and longer compulsory schooling significantly reduce the probability of incarceration. Results prove similar for the full and CZ samples. But one key difference with the arrest results is that at the individual-level we get a strong difference between the races. The impact of the compulsory schooling laws is much stronger for blacks.

This immediately begs the question of why the results are starker in this regard for incarceration than arrests? One possibility is simply that arrests and incarceration are different outcomes. We suspect that a more compelling explanation is that the Census data can uniquely identify race, whilst the arrest data uses average share of blacks in the commuting zone to identify race. If, at the extreme, only blacks' criminal behaviour is affected by the schooling laws, we might still obtain significant negative effects in Table 2 for the white commuting zones since, on average, their population has 6.8% blacks.

Incapacitation Effects

As was noted above, in the CZ analysis we are able to study arrest rates of younger individuals (aged 16 and 17) who may still be attending school.[27] Thus we can explore whether the kinds of incapacitation effects identified in Anderson's (2014) analysis are present in our data. We do this by considering whether the minimum dropout age laws only have an effect for those still in school. Empirically we can test for this by estimating separate effects for individuals for whom an incapacitation effect may occur (see also Anderson, 2014), which we

[27] We cannot do this experiment with the Census data – for specific reasons to do with the Group Quarters variable we use, see the discussion in the Data Appendix.

define respectively for age less than or equal to 17 for the DA17 dummy variable and for ages equal to 17 or 18 for the DA18 dummy variable. These two are the pivotal ages for possible incapacitation effects for DA17 and DA18.

The results from including these interactions together with an effect for all other (non-incapacitation ages) are reported in Table 4. For all CZs, reported in the first two columns of the Table, the estimated coefficient on the DA17*(Age≤17) interaction is indeed significant, negative and larger in absolute value than the control group (those aged 18 and over), suggesting crime reduction from incapacitation. However, the overall DA17 effect is also significant and negative suggesting that, whilst incapacitation effects for the 17 year olds and younger are present (mirroring Anderson, 2014), this is not the whole story as the effect of a higher dropout age also reduces arrests for the older cohorts. This is the case for both the low- and high-black share CZs. There are also significant crime reducing effects from DA18 and the interaction with age equal to 17 or 18 is also negative, but no more sizable than for the DA18 for those over 18. Thus we are able to uncover evidence of incapacitation, but this is not the whole story as crime reduction effects connected to changes in dropout age persist for cohorts aged above 18.[28]

[28] The results are not directly comparable to Anderson (2014) both due to a different sample and because the control group in his analysis are the 13-15 year olds. In comparison our control group is those too old to be affected by incapacitation effects. If we restrict the comparison group to those closer in age to the treatment group (e.g. aged less than 25) we obtain similar but less precise estimates.

5. A Causal Effect of Education?

Having established that for the most part the crime reduced-forms do show systematically negative, statistically significant connections between crime and compulsory schooling laws, and that this does not only reflect an incapacitation effect, we now return to the question of the causal impact of education. In doing so, we focus on results from the Census imprisonment equations since only for this sample can we separately identify state of residence and state of birth which is crucial to correctly allocate the relevant school-leaving law.

OLS and 2SLS estimates of the crime-education relationship and the associated education first-stage are shown in Tables 5A and 5B. The Tables are structured identically for the two different instrument sets, the dropout age (Table 5A) and the Goldin-Katz compulsory schooling laws (Table 5B). Each Table reports six specifications, three each for whites and blacks which differ as to whether or not they include region x year and region x year of birth fixed effects over and above the age, cohort, state of birth, state of residence and year effects.

Consider first the specifications in columns (1) and (2) which show estimates analogous to the crime reduced-forms of Table 3 which include the age, cohort, state of birth, state of residence and year variables as controls. In (1) and (2), the OLS coefficients on years of education in all specifications are negative and are strongly significant. This reinforces the opening remarks of this paper – educational attainment and crime are indeed related. The estimates are of substantially larger magnitude (in absolute terms) for blacks than whites. For example, the OLS coefficient on years of schooling for incarceration is over four times as large for blacks as whites. Interestingly, it was over three times as large for the 1960-80 results reported in LM – though the magnitude of the coefficients is substantially higher in our sample as a result of higher incarceration rates.

Focusing next on the first-stage results, we see that overall there is no consistent pattern between the various measures of compulsory schooling and educational attainment. Whilst the first-stage *F*-statistics are mostly above 10, the individual coefficient estimates are occasionally the wrong sign (i.e. tougher schooling laws reducing education), particularly for whites using the Goldin-Katz instrument. This suggests that in general there is no strong link in the more recent period between compulsory schooling laws and attainment. In addition to this overall mixed picture, the individual coefficient estimates (regardless of sign) are also very small relative to the historical pattern. For example, LM found that a one-year increase in compulsory schooling raises average years of schooling by between 0.199 and 0.340 for whites in the 1960-80 period. None of the first stage coefficients get anywhere near that kind of magnitude.

There is however one dimension along which the picture of inconsistent and very small effects holds less. For blacks, the coefficients in the first-stage are consistently positive (using either the dropout or Goldin-Katz measure) and the *F*-statistic is always larger than the corresponding first-stage for whites, even though the sample is much smaller. Even here however, the estimated size of the effect of the laws on attainment are far smaller than in the past. Again for the 1980-2010 Census results (column (2) of Table 5B), the coefficient estimates imply *at most* a 0.143 increase in years of schooling from a one-year increase in the law. The corresponding ranges in LM are 0.454 to 0.528. So whilst we find evidence that the educational attainment of blacks is still being affected by compulsory schooling laws, the magnitude of the effect is smaller – implying that a smaller share of the population is being induced to comply.

What does this imply for the 2SLS estimates of the causal effect of education on crime? If the appropriately weighted estimated coefficients on the instrument set in the first-stage are positive overall, whilst the reduced-form for crime is negative (positive), one will obtain a negative (positive) 2SLS estimate. But none of these will have any real causal interpretation since they will have arisen, at least partly, as a result of a poor overall first-stage. This is indeed what Tables 5A and 5B show for whites, where the 2SLS coefficient estimate on years of schooling generally does take a negative sign, but is statistically insignificant.

For blacks, we make more progress. Consider for example the Census results for blacks reported in column (5) of Table 3 (the crime-reduced form) and column (2) of Table 5B (the first-stage). The estimated coefficients are consistently negative and significant in the reduced-form and they are positive and generally significant in the first-stage, with an *F*-statistic of 19.7. The resulting 2SLS estimate of -3.498 is significantly negative at the 5% level and a little larger in absolute terms than the comparable OLS estimate of -2.666. Thus for the group with lower education levels, the sample of black individuals, we do uncover a causal impact of education on crime.[29]

We have also investigated why the first stage relationship is relatively weak and occasionally produces coefficients of the wrong sign. It turns out that including region x year controls (columns (3) and (4)) and region x year plus region x year of birth controls (columns (5) and (6)) does improve matters somewhat.[30] The first stages in columns (3)-(6) show a more plausible pattern of estimated coefficients. However, they mostly do not affect the 2SLS estimates, with one exception where the estimate for blacks is driven to insignificance in column (6) of Table 5B for the Goldin-Katz instrument. Overall, however, much the same pattern remains, with it proving more difficult than in previous research to obtain across the board causal crime-reducing effects of education from variations in state compulsory school-leaving age laws.

[29] We have also produced estimates that measure education in terms of high school dropout rather than years of schooling. Overall, as in Lochner and Moretti's (2004) earlier period analysis, they show a similar pattern. For whites the implied coefficient estimates are very close to those that one would predict from the coefficients on years of schooling and the mean gap in years of schooling between those with and without a high school diploma. For blacks, the effect is somewhat larger for the dropout variable, suggesting that blacks criminal behaviour is more influenced by dropping out of high school than would be expected from the linear schooling model. These results are available on request from the authors.

[30] Stephens and Yang (2014) also show the importance of region x year of birth interactions.

6. Conclusions

In this paper we have offered a reassessment of the relationship between crime, compulsory school-leaving laws and education. Compared to existing research, innovations from our analysis include study of more recent time periods in which both crime (especially measured by imprisonment) and education evolutions are different from before, together with an analysis of panel data on age-specific arrests in local labour markets over time. We pay a lot of attention to the relationship between crime and changes in school-leaving laws and between education and changes in school-leaving laws. These two reduced-form relationships are the ones that, when put together if they work in appropriately hypothesised directions, enable identification of the causal impact of education on crime.

Our findings show there to be a systematic relationship between lower crime and increases in higher school dropout ages or mandated years of education. In other words, we report evidence of a significant reduced-form relationship between crime and compulsory schooling-leaving laws. This is the case in our analysis of commuting zone panel data and studying Census data between 1980 and 2010. When we dig a little deeper into these crime reduced-forms, however, a rather more nuanced picture emerges. Separate estimates by race, or for the commuting zones delineated by the proportions of black versus non-black residents, reveal that the crime reduced-form is much stronger for black individuals in the Census and for violent crime in those commuting zones with bigger proportions of resident blacks. When we study possible incapacitation effects resulting from higher mandated schooling or dropout ages, we do find evidence of such effects, but in addition from the reduced-forms there is evidence of lower crime rates for older groups that are associated with changes in the laws.

Having established these findings on the crime reduced-form, we then consider the schooling laws and education, and put the two together to consider the causal impact of education on crime. In the later period we study, where education levels are higher than in the past, the education reduced-form (the first stage in the causal analysis) turns out to be rather weaker than before. In fact, it proves rather difficult to identify a strong first-stage like those reported in earlier work. Overall there is no consistent pattern between the various measures of compulsory schooling and educational attainment. For white individuals, there is no evidence of a significant first-stage relationship for education. However, for black individuals, there is. Thus for the groups with lower education levels, where the crime reduced-forms were also significant, we are able to put the two together and so uncover a causal impact of education on crime.

Overall then, and to conclude, the findings from this paper shed more light on the possible crime reducing effect of education. It is evident from our statistical analysis of this issue that one can find evidence that crime and education are causally related (in our analysis for blacks). It is also the case that incapacitation effects that occur due to changes in school leaving age play a role in this relationship. This work also bears upon broader areas of research that seek to identify causal education effects using variations in compulsory school laws.[31] Our results suggest that the effects of such laws on educational attainment have weakened through time. This makes study of other factors that may lie behind the causal impact of education on crime and generating a better understanding of the patterns of heterogeneity that we see in our study thus form an important research agenda for the future.

[31] For example, see the fuller range of outcomes listed in the review of Oreopoulos and Salavanes (2011).

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Figure 1A: Population-Weighted Distribution of Compulsory Schooling Law (Minimum Dropout Age Measure)

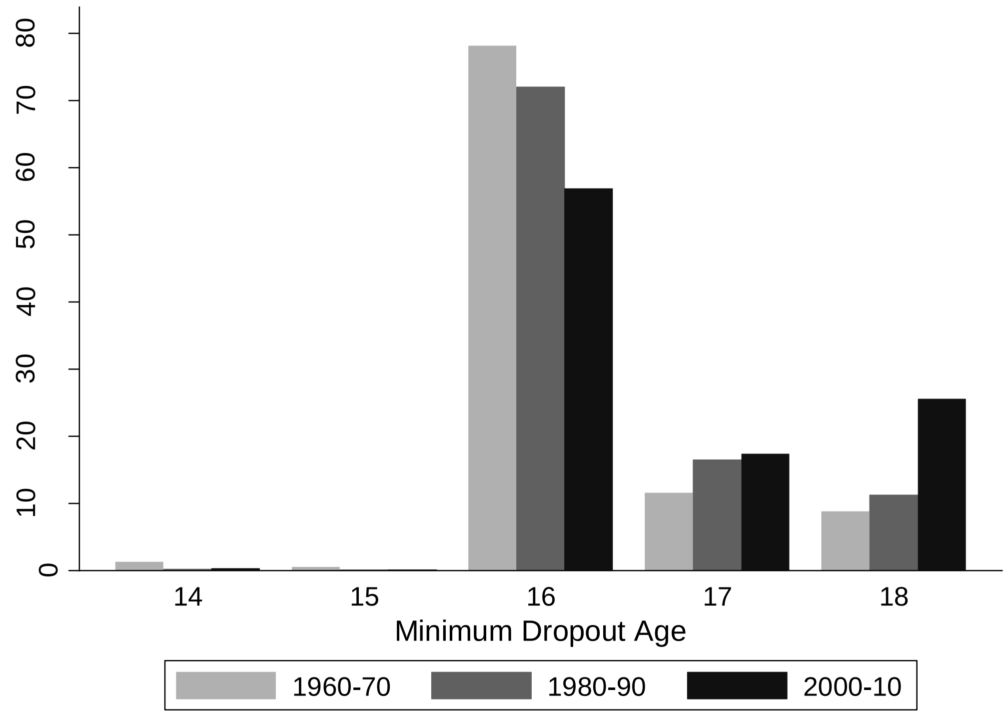

Figure 1B: Population-Weighted Distribution of Compulsory Schooling Law (Goldin-Katz Measure)

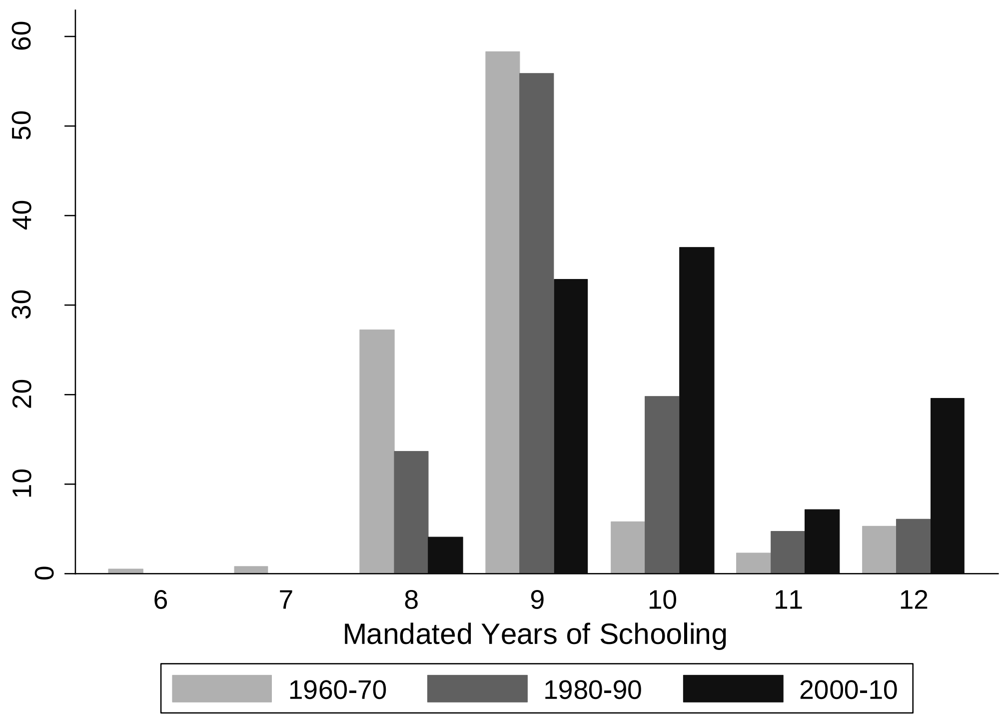

Table 1: Summary Statistics

CZone Sample:	All CZs	Low Black Share	High Black Share
Total Crime Rate	0.035	0.034	0.042
	(0.019)	(0.019)	(0.020)
Property Crime Rate	0.016	0.016	0.018
	(0.012)	(0.012)	(0.012)
Violent Crime Rate	0.019	0.018	0.023
	(0.009)	(0.008)	(0.011)
Years of Education	13.13	13.19	12.85
	(0.767)	(0.763)	(0.722)
Share of Blacks	0.101	0.068	0.251
	(0.086)	(0.048)	(0.067)
Sample Size	29376	25152	4224
Number of CZs	306	262	44
Census Sample:	All Individuals	Whites	Blacks
Incarceration Rate	0.025	0.016	0.088
	(0.156)	(0.124)	(0.283)
Dropout Incarceration Rate	0.073	0.047	0.177
	(0.261)	(0.211)	(0.382)
Years of Schooling	13.08	13.19	12.31
	(2.28)	(2.28)	(2.07)
Dropout Share	0.139	0.126	0.223
	(0.346)	(0.332)	(0.416)
Sample Size	7,006,642	6,214,803	791,839
Census Sample (Matched CZone):	All Individuals	Whites	Blacks
Incarceration Rate	0.026	0.017	0.091
	(0.158)	(0.128)	(0.288)
Dropout Incarceration Rate	0.077	0.051	0.186
	(0.266)	(0.220)	(0.389)
Years of Schooling	13.12	13.22	12.36
	(2.25)	(2.26)	(2.04)
Dropout Share	0.130	0.120	0.206
	(0.336)	(0.325)	(0.404)
Sample Size	3,563,596	3,182,529	381,067

Notes*:* CZone sample includes males aged 16-39. Census sample includes all males ages 18-39 in 1980-2000 Censuses and 2010 ACS (the ACS 2008-2012 5-Year 5% sample). Respondents whose state of residence or state of birth was Alaska, Hawaii or the District of Columbia are excluded. Census sample (matched CZone) includes only those residing in CZones used in the first panel. Figures in parentheses are standard deviations.

Table 2. Arrests and Schooling Laws, 1980-2010

CZone Sample:	All		Low Black Share		High Black Share	
Crime Type:	Property	Violent	Property	Violent	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
A: Reduced-Form (Minimum Dropout)						
DA17	-0.126	-0.157	-0.155	-0.124	0.025	-0.179
	(0.027)	(0.047)	(0.037)	(0.056)	(0.031)	(0.040)
DA18	-0.161	-0.049	-0.139	-0.033	-0.195	-0.456
	(0.026)	(0.028)	(0.028)	(0.029)	(0.052)	(0.069)
F-Statistic	22.5	6.4	15.1	2.8	14.3	22.7
B: Reduced-Form (Goldin-Katz)						
CSL1	-0.121	-0.164	-0.162	-0.197	0.071	0.014
	(0.021)	(0.022)	(0.026)	(0.022)	(0.032)	(0.038)
CSL2	-0.159	-0.171	-0.157	-0.103	0.018	-0.010
	(0.035)	(0.050)	(0.050)	(0.058)	(0.051)	(0.061)
CSL3	-0.244	-0.167	-0.247	-0.161	-0.131	-0.288
	(0.032)	(0.033)	(0.035)	(0.034)	(0.052)	(0.061)
F-Statistic	20.2	20.3	17.7	25.9	8.2	10.7
Sample Size	29376	29376	25152	25152	4224	4224

Notes: Sample includes males ages 16-39. Each panel represents a separate reduced-form model using alternative CSL instruments. The dependent variable is the log of the arrest rate for total violent or property crimes. All specifications control for age, year, year of birth and commuting zone fixed effects. Alaska, Hawaii and DC are excluded. Compositional controls are used in all specifications: the shares of female, migrant, black, married and young (16-24) in each commuting zone-year. Low black share commuting zones are defined as those below the 80^{th} percentile of the share of blacks in the population across the sample period. Robust standard errors are clustered at the state of residence – year of birth level.

Table 3. Incarceration and Schooling Laws, 1980-2010

Census Sample:	All		White		Black	
	Full	CZ	Full	CZ	Full	CZ
	(1)	(2)	(3)	(4)	(5)	(6)
A: Reduced-Form (Minimum Dropout)						
DA17	-0.152	-0.314	-0.071	-0.191	-0.266	-0.783
	(0.038)	(0.052)	(0.034)	(0.040)	(0.188)	(0.241)
DA18	-0.265	-0.441	-0.088	-0.218	-1.175	-1.343
	(0.045)	(0.051)	(0.032)	(0.037)	(0.247)	(0.298)
F-Statistic	17.2	36.4	4.4	20.3	10.4	10.3
B: Reduced-Form (Goldin-Katz)						
CSL1	-0.071	-0.015	-0.037	-0.048	-0.652	-0.432
	(0.031)	(0.039)	(0.023)	(0.028)	(0.163)	(0.207)
CSL2	-0.147	-0.306	-0.052	-0.237	-0.452	-0.609
	(0.048)	(0.069)	(0.038)	(0.059)	(0.250)	(0.350)
CSL3	-0.307	-0.367	-0.135	-0.227	-1.124	-0.902
	(0.050)	(0.057)	(0.037)	(0.041)	(0.283)	(0.334)
F-Statistic	11.7	17.2	4.1	11.6	7.1	2.6
Sample Size	7,006,642	3,563,596	6,214,803	3,182,529	791,839	381,067

Notes: Sample includes all males ages 18-39 in 1980, 1990 and 2000 Censuses and the 2010 ACS. Each panel represents a separate reduced-form linear probability model using alternative CSL instruments The dependent variable is a dummy equal to 1 if the respondent is in institutionalized quarters (all coefficient estimates are marginal effects multiplied by 100). All specifications control for age, census year, state of birth, state of residence and year of birth fixed effects. Alaska and Hawaii and the District of Columbia are excluded. The CZ sample covers only those individuals resident in the CZone areas used in Table 2. Robust standard errors are clustered at the state of birth – year of birth level.

Table 4. Arrests, Schooling Laws & Incapacitation, 1980-2010

CZone Sample:	All		Low Black Share		High Black Share	
Crime Type:	Property	Violent	Property	Violent	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced-Form (Minimum Dropout):						
DA17*(Age>17)	-0.096	-0.119	-0.125	-0.091	0.031	-0.125
	(0.025)	(0.045)	(0.034)	(0.052)	(0.030)	(0.036)
DA17*(Age≤17)	-0.383	-0.493	-0.447	-0.449	-0.009	-0.471
	(0.068)	(0.141)	(0.085)	(0.174)	(0.086)	(0.135)
DA18*(Age>18)	-0.173	-0.048	-0.150	-0.028	-0.189	-0.437
	(0.027)	(0.029)	(0.028)	(0.030)	(0.051)	(0.072)
DA18*(17≤Age≤18)	-0.134	-0.102	-0.121	-0.099	-0.216	-0.481
	(0.049)	(0.051)	(0.052)	(0.053)	(0.128)	(0.122)
Sample Size	29376	29376	25152	25152	4224	4224

Notes: See notes to Table 2. Both of the minimum dropout age variables have an additional interaction term with the relevant incapacitation age.

Table 5A: The Causal Effect of Education? – Minimum Dropout Age

Sample	Census White	Census Black	Census White	Census Black	Census White	Census Black
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	-0.592	-2.666	-0.592	-2.670	-0.593	-2.675
	(0.014)	(0.059)	(0.014)	(0.059)	(0.014)	(0.059)
2SLS	-0.692	-5.118	-0.527	-5.715	-0.401	-4.722
	(0.918)	(1.690)	(1.510)	(1.822)	(1.232)	(1.793)
First-Stage (Minimum Dropout):						
DA17	0.049	0.124	0.047	0.107	0.050	0.093
	(0.018)	(0.021)	(0.016)	(0.020)	(0.014)	(0.019)
DA18	-0.039	0.066	0.002	0.099	0.029	0.101
	(0.023)	(0.025)	(0.018)	(0.020)	(0.014)	(0.021)
F-Statistic	6.3	17.3	4.5	18.4	6.9	16.1
Region xYear			x	x	x	x
Region xYear of Birth					x	x
Sample Size	6,214,803	791,839	6,214,803	791,839	6,214,803	791,839

Notes: See notes to Table 3.

Table 5B: The Causal Effect of Education? - Goldin-Katz Instrument

Sample	Census White	Census Black	Census White	Census Black	Census White	Census Black
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	-0.592	-2.666	-0.592	-2.670	-0.593	-2.675
	(0.014)	(0.059)	(0.014)	(0.059)	(0.014)	(0.059)
2SLS	-0.179	-3.498	0.178	-2.647	0.326	-0.632
	(0.627)	(1.703)	(0.826)	(1.650)	(0.794)	(1.533)
First-Stage (Goldin-Katz):						
CSL1	-0.048	-0.007	-0.038	-0.001	-0.034	-0.000
	(0.012)	(0.017)	(0.012)	(0.016)	(0.011)	(0.015)
CSL2	0.025	0.143	0.025	0.124	0.024	0.119
	(0.019)	(0.023)	(0.018)	(0.021)	(0.016)	(0.020)
CSL3	-0.076	0.118	-0.027	0.139	0.005	0.143
	(0.029)	(0.030)	(0.023)	(0.024)	(0.018)	(0.023)
F-Statistic	10.0	19.7	5.9	23.7	5.5	25.2
Region xYear			x	x	x	x
Region xYear of Birth					x	x
Sample Size	6,214,803	791,839	6,214,803	791,839	6,214,803	791,839

Notes: See notes to Table 3.

Appendix A: Data Description

A1. Commuting Zone Panel Data on Arrests by Age

Panel data for the US come from the FBI Uniform Crime Reports (UCR). The measure of crime is arrests since we need to study age variations to match to compulsory school leaving laws. The UCR reports the number of arrests by year, agency, age, gender and type of crime. The original data identifies the number of arrests by law enforcement agencies.

Commuting zones are defined as in Autor and Dorn (2013) and consist of 741 (722 after excluding Alaska, District of Columbia and Hawaii) groups of counties that adequately describe local labour markets given their strong intra-group and weak inter-group ties. We construct a commuting zone-level panel on arrests by aggregating the number of arrests over law enforcement agencies within a commuting zone. This is feasible as the agencies are geographically identified by county and state, hence making it possible to aggregate the counties that constitute the commuting zones. Ignoring the problem of non-reporting agencies, one is able to consistently match 714 out of 722 commuting zones within the range of 95% to 105% population coverage balanced across 1980-2010. Commuting zones are not confined to a unique state, therefore when assigning a state identifier to a commuting zone we choose state that comprises the majority of population residing in a given commuting zone.

Within the UCR, data for certain law enforcement agencies is sometimes systematically missing either for the whole commuting zone area or for some important law enforcement agencies within that commuting zone. UCR details the total population covered by each law enforcement agency, hence making it possible to compare with the official population numbers from the Census. In order to minimize measurement errors the final balanced sample includes the 306 commuting zones which consistently report arrest data for all 4 years (1980-2010) and have a covering ratio of covered population and Census population between 90 to 110% of the population. We also consider an unbalanced sample that includes 461 commuting zones reporting at least 3 continuous years of data with the same covering ratio tolerance.

We sample males aged 16 to 39 from 1980 to 2010. The UCR data are grouped by age category. From age 16 up to the age of 24, the number of arrests is measured by single age year. For ages 25 and above, the arrests are aggregated to the number of arrests in a five-year age bracket, i.e. 25 to 29, 30 to 34, and 35 to 39. In order to be able to track the number of arrests per year-of-birth cohort, we therefore disaggregate the arrest measure to the number of arrests by single age year by dividing the arrest count by five. The underlying assumption is that year-of-birth cohorts are homogenous in terms of the number of arrests within these respective older age brackets.

Following the literature, we categorize arrests into those for property and violent crime using the UCR offense code variable as follows:

Violent crime:	Property crime:
01A = Murder and non-negligent manslaughter	05 = Burglary – breaking or entering
01B = Manslaughter by negligence	06 = Larceny – theft (except motor vehicle)
02 = Forcible rape	07 = Motor vehicle theft

03 = Robbery	09 = Arson
04 = Aggravated assault	
08 = Other assaults	

In order to produce arrest rates, we aggregate the number of arrests for the above categories and divide the resulting number of arrests by the annual commuting zone-age-year population. The population data for that purpose are retrieved from the US Census population estimates.

A2. Individual-Level Micro Data on Incarceration

The micro data on US incarceration comes from the US Census. We sample all males aged 16-39 from the Integrated Public Use Microdata Series (IPUMS) for the 5 percent 1980, 1990 and 2000 Census and the 5 percent 2008-2012 American Community Survey (ACS) 5-Year sample centred on 2010. We identify the institutionalized population using the Group Quarters (GQ) variable. The GQ variable consistently identifies the following categories:

a) Non-group quarter households;

b) Institutions (Correctional Institutions, Mental Institutions, Institutions for the elderly, handicapped and poor);

c) Non-institutional group quarters (Military, College dormitory, rooming house, other).

However only in the 1980 IPUMS is the GQ variable detailed enough to uniquely identify those in correctional facilities. In subsequent Censuses (and the ACS), the institutionalized population includes the following categories: correctional facilities, nursing homes and mental hospitals, and juvenile institutions. However, the share of the total institutionalized population accounted for by those in correctional facilities is very high in our sample.

Appendix Table A1 shows the institutionalized male population by GQ type and age. Note that this data comes from published aggregate Census reports that do break up the categories, though this is not available in the IPUMS data release. In 2000, for example, 95.3 percent of institutionalized males aged 18-39 where in correctional facilities. Two key points come from Table A1. First, incarcerated males aged less than 18 are much less well identified (since juvenile facilities are an important component for this group). We therefore restrict our analysis of the Census data to those aged 18-39. Second, the 1980 Census has a less tight correspondence between institutionalization and incarceration. Fortunately, this is the one Census that has the full GQ coding in the micro files, so we use only the correctional facility definition in the 1980 Census.

A3. Construction of Instrument

Data on compulsory schooling laws was retrieved from 2 main sources. Before 1978, we use the data compiled by Acemoglu and Angrist (2001). After 1978, we look at the official annotated statutes of each state in the Westlaw International Database for each of the corresponding years.

The data retrieved includes maximum entry age, minimum leaving age and education grade which exempts a child from staying in school. The laws have historically increased in their complexity adding several exemptions including work permits and early age parental consent letters to exemplify the most common. The Labor Standards Act 1939 harmonized child labour laws across states in the US, recent changes were not of a comparable order of magnitude as the ones seen during that period. To be consistent we ignore the possibility of parental consent authorizations to leave school at an age below the minimum dropout age, as these are often seen as exceptions rather than the rule.

Additional Tables

Table A1: US Male Population in Group Quarters by Type and Age, 1980-2010

	Total Institutionalized	Correctional Institutions	Correctional as Percent of Total
1980 Census			
All	1232120	439720	35.7
15-17	68300	8460	12.4
18-21	123320	89600	72.7
22-24	104060	80240	77.1
25-39	301980	205780	68.1
1990 Census			
All	1801350	1030210	57.2
15-17	68480	16490	24.1
18-21	149780	128940	86.1
22-24	143890	133490	92.8
25-39	666690	581670	87.2
2000 Census			
All	2534060	1806260	71.3
15-17	87200	18960	21.7
18-21	221660	202470	91.3
22-24	201060	195660	97.3
25-39	951660	911050	95.7
2010 Census			
All	2716877	2059020	75.8
15-19	153924	74720	48.5
20-24	327760	308926	94.3
25-39	971581	945065	97.3

Notes: Data from 1980 are calculated from IPUMS data, figures for 1990, 2000 and 2010 come from the US Census Bureau.

Table A2: Arrests and Schooling Laws - Robustness, 1980-2010

CZone Sample:	All		Low Black Share		High Black Share	
Crime Type:	Property	Violent	Property	Violent	Property	Violent
	(1)	(2)	(3)	(4)	(5)	(6)
A: Reduced-Form (Minimum Dropout)						
DA17	-0.086	-0.143	-0.119	-0.125	0.083	-0.108
	(0.025)	(0.043)	(0.035)	(0.053)	(0.032)	(0.035)
DA18	-0.116	0.020	-0.101	0.052	-0.186	-0.388
	(0.026)	(0.026)	(0.027)	(0.028)	(0.046)	(0.058)
F-Statistic	11.8	6.5	8.9	4.9	31.7	22.4
B: Reduced-Form (Goldin-Katz)						
CSL1	-0.109	-0.158	-0.151	-0.193	0.097	0.009
	(0.021)	(0.023)	(0.026)	(0.023)	(0.032)	(0.035)
CSL2	-0.112	-0.059	-0.105	-0.027	0.027	0.029
	(0.037)	(0.037)	(0.047)	(0.039)	(0.047)	(0.054)
CSL3	-0.210	-0.107	-0.219	-0.094	-0.155	-0.303
	(0.031)	(0.032)	(0.035)	(0.032)	(0.052)	(0.060)
F-Statistic	14.9	16.2	14.5	23.3	14.2	14.7
Sample Size	40536	40536	35088	35088	5448	5448

Notes: Sample includes males ages 16-39 in commuting zones that reported at least 3 continuous years of data. Each panel represents a separate reduced-form model using alternative CSL instruments. The dependent variable is the log of the arrest rate for total violent or property crimes. All specifications control for age, year, year of birth and commuting zone fixed effects. Alaska, Hawaii and DC are excluded. Compositional controls are used in all specifications: the shares of female, migrant, black, married and young (16-24) in each commuting zone-year. Low black share commuting zones are defined as those below the 80^{th} percentile of the share of blacks in the population across the sample period. Robust standard errors are clustered at the commuting zone – year of birth level.

Chapter 3

Crime-Age Profiles and School Dropout*

Abstract

Research on the economics of crime demonstrates that a beneficial unintended consequence of education policies that raise the school leaving age is reduced criminality. This paper studies the way in which these crime reductions come about by focussing in detail on how such dropout age policies have scope to alter the shape of the crime-age profile. Evidence from a sequence of state-level reforms enacted in the last four decades in the United States shows that these policies have significantly altered crime-age profiles. The observed change in the shape is consistent with there being both a temporary incapacitation effect and a more sustained crime reducing effect. These combine to generate sizable crime reductions from school dropout age policy reforms.

* This Chapter is based on Bell, B. D., Costa, R. and Machin, S. (2017) "Crime-Age Profiles and School Dropout".

1. Introduction

The crime-age profile is a well-established empirical regularity. Almost two hundred years ago, Adolphe Quetelet showed that crime in early nineteenth-century France peaked when individuals were in their late teens (Quetelet, 1831). Subsequent research has confirmed the existence of a strong crime-age pattern in many settings, with crime peaking in the late teens and declining quite rapidly thereafter.[32]

Figure 1 illustrates this profile for US males using arrest rates, showing a peak rate at age 18 and declines thereafter. In a very well-known study, Hirschi and Gottfredson (1983) conjecture that crime-age profiles are broadly invariant over time and space. They suggest that criminals can be identified by their lack of self-control, that this characteristic is determined well before adolescence, and that it subsequently persists throughout life. At first sight such a hypothesis would seem to imply that the crime-age profile should be reasonably flat. To avoid this conclusion, Hirschi and Gottfredson suggest that offenders burn-out over time – maturation – and that exposure to criminal opportunities decline as activity patterns change with age. By contrast, Sampson and Laub (1993, 2005) focus on the life-course of criminal activity and highlight how events such as family, relationships, schooling and employment change as one ages. These life cycle dynamics of crime generate the crime-age profile, with the inverse U-shape coming about from patterns of crime onset, specialisation and desistence that occur as individuals get older.

A large body of evidence in criminology has tried to assess the relative merits of these different views. Greenberg (1985) presents evidence that both the peak crime age and the rate of subsequent decline differs across crime types, localities, race and gender, whilst Hansen (2003) shows that the crime-age profile differs for those who leave school at the compulsory

[32] Sullivan (2012) offers a theoretical review and Siennick and Osgood (2008) present a review of empirical work and findings.

school leaving age and those who remain in education. Further discussion and additional evidence is given in Cohen and Vila (1996), and the meta-study of Pratt and Cullen (2000).

In the economics literature, Grogger (1998) examines how the changing returns to legal activity can explain the shape of the crime-age profile, whilst Lochner (2004) uses a human-capital model of crime to show that crime should indeed peak at around the time of labour market entry. More recently, Bindler and Hjalmarsson (2017) consider convictions from 19th century London to show that there was a U-shaped trend in the average age of male convicts over the century. They suggest that increased use of prison sentences, as opposed to the death penalty and penal transportation, may have raised the average age of conviction as a result of a rise of recidivism.

What has not been considered in this literature is whether policies that are known to shift the overall crime rate also have scope to alter the shape of the crime-age profile. One such set of policies that has been studied in the economics of crime field are education policies that raise the school leaving age. These have been studied in a range of settings to show that a beneficial unintended consequence of them is reduced criminality (see, inter alia, Lochner and Moretti, 2004; Machin et al., 2011; and Bell et al., 2016). However, the size of the benefits to society from the policy-induced crime reduction depends on whether the crime reduction that ensues from increases in the school dropout age reflect an incapacitation effect from keeping children in the classroom (i.e. off the streets and not committing crime) or whether the extra time spent in the education system has a long term effect on an individual's productivity (e.g. by enhancing their future labour market prospects, so deterring them from entering a life of crime).

Most existing research has focused either on direct incapacitation effects or on the longer-term effects, rather than examining both within the same empirical setting. On the former, studies of crime incapacitation by Jacob and Lefgren (2003) and Luallen (2006) look at teacher strikes and calendar year changes to show that changes in the requirement to be in school on a particular day have effects on crime on the same day. Anderson (2014) considers compulsory school leaving laws such as those examined in this paper and focusing directly on those who are kept in school during the day as a result of the reforms shows that there is a strong negative effect on crime.[33] On the latter, evidence of longer term benefits of crime reduction are provided by papers that study the causal impact of education on crime working through schooling laws for people who are old enough to have left the education system (Lochner and Moretti, 2004; Machin et al., 2011; Bell et al., 2016).

In this paper we provide evidence on both of these by directly testing whether crime-age profiles adapt in the face of policy-induced changes in the compulsory school leaving age. The focus is very much on the crime reduced form that has been used in the causal crime literature. This reduced form is modified to study the changing nature of crime-age profiles in a multiple regression discontinuity framework studying US state-level changes in the compulsory school leaving age. Evidence from these reforms enacted in the last four decades in the United States shows that these policies have significantly altered crime-age profiles. This change in the shape is shown to be consistent with there being both a temporary incapacitation effect and a more sustained crime reducing effect. These combine to generate sizable crime reductions from school dropout age policy reforms.[34] In contrast to the previous research on earlier US reforms this does not arise solely as a result of education improvements, and so the evidence of a longer run effect is interpreted as dynamic incapacitation.

The rest of the paper is structured as follows. Section 2 sets up a framework for thinking about how changes in school leaving ages may have scope to shift and alter the shape of crime-

[33] Other research has considered different forms of incapacitation, for example conscription (Galiani, Rossi and Schargrodsky, 2011), teen pregnancy (Black, Devereux and Salvanes, 2008) and violent movie screenings (Dahl and DellaVigna, 2009).

[34] Without placing as much focus on the scope to affect crime-age profiles Chan (2012) also studies crime reduced forms using US data. A related paper, based upon Danish register data, is Landerso et al. (2017) which studies the crime impact of reforming age of school entry.

age profiles. Section 3 describes the data that are used, offers some initial descriptive data analysis on compulsory school leaving laws and presents the research design used in the empirical work contained in the paper. Section 4 reports the main results on the impact of dropout age reforms on crime-age profiles. Section 5 provides further discussion and examines evidence on the mechanisms by which dropout reforms reduce criminality. Section 6 offfers conclusions.

2. A Framework For Studying Crime-Age Profiles

Since Becker (1968) formalized the economic approach to studying criminal behaviour, a variety of models have been developed in attempts to better help understand what lies behind individual criminality. For example, work by Ehrlich (1973), Witte (1980), and Witte and Tauchen (1994) thinks of engagement in crime as an allocation of time decision. More recently, models of crime have introduced dynamic aspects in attempts to more clearly represent life-course profiles of crime. The notion of criminal capital being a substitute for human capital, which can improve an individual's prospects in the crime market as compared to the labour market, has been a central feature of such models (see, for example, Lochner, 2004, and Mocan et al., 2005).

The model presented in this section incorporates this dynamic feature into the basic time allocation structure of Witte and Tauchen's framework. The aim of this is to consider how crime-age profiles have scope to be shifted by changes in mandatory dropout age. The starting point rests on the notion that an individual decides how to allocate time between the illegal sector, where they devote time to crime (c), and the legal sector, working ($t - c$), where t is his/her full time endowment. However, given that time for crime is constrained whilst individuals are enrolled in school, the full time endowment t will be a function of age, $t(a)$, most importantly being reduced if an individual is in school by being aged lower than the minimum age of school dropout a^d.

Normalizing *c* and *t* to the unit interval, one example of how the time endowment differs by age and dropout age is:

$$t(a) = \begin{cases} t_l \; for \; a < a^d \\ t_h \; for \; a \geq a^d \\ 0 < t_l < t_h < 1 \end{cases} \tag{1}$$

where the *l* and *h* subscripts index low and high free time available to allocate to crime. In this example t_h < 1 so 1 - t_h may be thought of as leisure time. The key feature of the model is that the younger individuals may do some crime (as t_l > 0) but because they are kept in school this acts as an incapacitation effect preventing them from engaging in as much crime as those older than the dropout age who have more available time for such activity.

The likelihood of these older individuals to do so depends on the relative returns to crime or work. The labour market returns to work are given by the wage, $w(e)$, which is a function of potential labour market experience, defined as e = max$\{0, a - a^d\}$, and which reflects on-the-job-training and learning-by-doing. Each individual faces a rate of return to crime $r(e)$ and a sanction $s(a)$ if they are caught doing crime. Given the probability of being caught by law enforcement $p(c)$ and a utility function $U(.)$ each individual maximizes his/her expected utility by choosing the optimal amount of time to spend on crime c as follows:

$$\max_{\{c\}} \; \big(1 - p(c)\big)U\big(r(e)c + w(e)(t(a) - c)\big) + p(c)U\big(r(e)c + w(e)(t(a) - c) - s(a)c\big) \tag{2}$$

$$s.t \;\; c \leq t(a)$$

Operationalising the model requires the following assumptions to be made:

i) $U'(.) \geq 0$, $U''(.) \leq 0$ – standard positive marginal utility and diminishing returns.

ii) $p_c' \geq 0$ – the probability of getting caught increases with time devoted to crime.

iii) $s_a' \geq 0$ – the sanction penalty increases as an individual approaches legal age and surpasses the extended age of the juvenile state court.

iv) $r_e' \geq 0$ – returns to criminal time increase as the individual gains potential experience and builds criminal capital.

v) $w_e' \geq 0, w_e'' \leq 0$ – there are concave wage profiles that are particularly prevalent in young and low-educated workers.

vi) $t_a' \geq 0$ - as the individual gets older and is older than the dropout age, the time endowment increases.

If assumptions i) to vi) hold then the solution to the individual's optimization problem in (1) is then given by a level of c that satisfies the following first order condition (in which the relative return to crime over work is defined as $n(e) \equiv r(e) - w(e)$):

$$\big(1-p(c)\big)U'(t(a)w(e)+n(e)c)n(e)+p(c)U'\big(t(a)w(e)+\big(n(e)-s(a)\big)c\big)\big(n(e)-s(a)\big)+p'(c)\big[U\big(t(a)w(e)+\big(n(e)-s(a)\big)c\big)-U(t(a)w(e)+n(e)c)\big]-\mu=0 \quad (3)$$

$$\mu\big(c-t(a)\big)=0,\ c-t(a)\leq 0,\ c\geq 0, \mu\geq 0$$

In (3), if $\mu \neq 0$, the constraint binds and $c = t(a)$ meaning the individual will use the full extent of his/her time endowment to engage in the illegal sector. On the other hand, if $\mu = 0$ the constraint does not bind and we are back to the unconstrained problem. The optimality condition equalizes the marginal net benefit of crime to the marginal benefit of working:

$$\left(1-p(c)\right)U'(t(a)w(e)+n(e)c)n(e)+p(c)U'\left(t(a)w(e)+\left(n(e)-s(a)\right)c\right)\left(n(e)-s(a)\right)+p'(c)\left[U\left(t(a)w(e)+\left(n(e)-s(a)\right)c\right)-U(t(a)w(e)+n(e)c)\right]=0 \quad (4)$$

$$c-t(a)<0,\ c\geq 0$$

Understanding how the optimal amount of crime varies with age, and thus generates a crime-age profile, and in particular how changes in school dropout age can affect this is the main goal of this paper. The implicit derivative of the optimal crime choice with respect to age is given by $\frac{dc^*}{da}=-\frac{\partial F}{\partial a}/\frac{\partial F}{\partial c}$, where *F* stands for the first order condition defined by equation (4).[35] In this model, the following proposition emerges:

<u>Proposition</u>

If (i) individuals are risk averse, $k=-\frac{U''}{U'}\geq 0$[36], (ii) wealth is non-decreasing in age $t'(a)w(e)+t(a)w'(e)+\left(n'(e)-s'(a)\right)c\geq 0$, (iii) the net rate of return to crime is non-negative $n(e)\geq 0$, and decreasing in age, $n'(e)-s'(a)\leq 0$, and (iv) $k\left(n(e)-s(a)\right)p(c)\geq p'(c)$[37], then the slope of the crime age-profile will be decreasing in age $\frac{dc^*}{da}\leq 0$.[38]

Proof: See Appendix B[39]

[35] See Appendix B for the formal derivations.

[36] For simplicity, we assume the risk aversion to be constant across age and that dropout age change will not affect this parameter. Despite the scarce evidence on the relation between risk aversion and age, the consensus seems to point to a positive relationship. Assuming that older individuals are more risk averse, the results of the model simulation would be strengthen in terms of crime reduction as individuals would be constrained in their crime engagement until older ages. Furthermore, if education is to have a similar relation with risk aversion the reducing effect over the age profile would be again more pronounced.

[37] This condition also ensures concavity of the objective function and existence of global maximum if $p(c)$ is convex, $p''(c)\geq 0$.

[38] The conditions stated are not exhaustive of all cases where $\frac{dc^*}{da}\leq 0$, however, they are the ones that are most in line with empirical evidence.

[39] Individual risk aversion is a key feature of standard economic models, whilst the non-decreasing wealth as function of age (at least until retirement approaches) seems supported by existing empirical evidence, though for older individuals than considered here. The positive net rate of return to crime needs to hold if an individual is ever to engage their time in criminal activities - intuitively if the return was to be negative the individual would choose to engage all of his/her time in the legal sector. Perhaps the most challenging assumption is that of the net return to crime decreasing with age. We would argue that this is most plausibly the case for the later ages studied in this paper – where there are increasing sanctions due to the loss of juvenile and extended age status in court (Levitt, 1998) and no evidence of convex age returns to crime.

The framework can be used to help understand some of the mechanisms behind the crime-age profiles that are observed in the data. The fact that optimal crime is decreasing in age matches the desistance stage (e.g. in the life course approach of Sampson and Laub, 1993, 2005) that typically starts in the late teens or early twenties. The onset age, with an increasing crime-age profile, can be thought of in light of this framework as a case in which the net return to crime is actually increasing in very early ages given the reduced level of sanctions commonly imposed on juveniles and relatively low legal wage opportunities.

The key practical dimension of this simple model is the way it can be used to examine how the crime-age profile may change when the minimum dropout age is increased. In the model this first implies a strengthening of the age constraint at younger ages[40] that will limit the allocation of available time to engage in criminal activities because of incapacitation. Second, there is a medium-term effect at later ages as the return to crime will be lower due to less investment being made in a criminal career (even if we make the extreme assumption that education is non-productive). A higher dropout age implies entrance into the unconstrained optimal crime time allocation at an older age, in which the dominant role is played by the net return to crime. If the latter is decreasing in age, by the time the individual is free to allocate his/her time the return will now be lower than it would under a counterfactual lower dropout age as the individual concerned faces potential tougher sanctions and the loss of the criminal experience premium that would have otherwise been accumulated.

A relevant point to be made when considering the model proposed and channels described is the distinction between intensive and extensive margin of crime reduction. The model as so far described should be view as the average effect over three sub-population groups: low, medium and high propensity criminal individuals. Low propensity case can be

[40] The magnitude of the effect of the higher dropout age will depend on its enforceability and on the extent of truancy.

seen as individuals for which the parameters of the model will always deliver the same result: zero allocation of time to criminal activities, $c^* = 0$. On the other hand, high propensity criminal individuals will engage all their time allocation to crime activities, $c^* = t(a)$. The most interesting case would be the "medium" propensity individuals for each the dynamics detailed previously are most pertinent. This group can show crime reductions through extensive and intensive margins of time allocation: total desistance of crime engagement ($c^* = 0$) – extensive margin; and reduction on the time allocation to criminal activities ($0 \leq c^* \leq t(a)$) – intensive margin. The total crime reduction effect will hence be a weighted sum over: the intensive margin reduction at the mechanical dropout age constraint for high propensity criminal individuals, and the joint intensive and extensive margin reductions of medium high propensity criminal agents. Without the access to some form of individual panel data, one cannot separately identify the different components of crime reduction described previously, only the total effect.

Figure 2 shows a simulation of the model where the minimum dropout age is increased.[41] It focusses on the age range 15-24, as will the empirical work, and considers a rise in the dropout age from 17 to 18. As previously described the effects of incapacitation (short-term) and criminal premium loss (medium-term) on time allocation are easily identified at the respective ages. These effects are congruent with the empirical crime-age profile shown in Figure 1, with the simulated reform showing strong effects of incapacitation, followed by a permanent hampering down of the crime-age profile at subsequent ages.

Various statistical characterizations of the shift in the profile can be described. For example, in Figure 2 the mean offending age rises a little, going from 19.36 to 19.42, the mode from 17 to 18, and the median from 18.72 to 18.85. As we shall see, the empirical results presented later in the paper broadly match these moment changes, though one difference is that

[41] The calibration parameters of the model are provided in Appendix B.

the incapacitation change is less pronounced in the data, whilst the longer-term benefits are stronger. Of course, the model is highly simplified so it should not be expected to perfectly reflect the empirical evidence – it is a tool for exposition that reveals theoretical mechanisms that may underpin shifts in crime age profiles induced by changes in the dropout age.

3. Data Description and Empirical Approach

Arrest Data

The crime data used in the analysis is provided by the FBI Uniform Crime Report (UCR) which compiles yearly arrest data by age and sex at local police enforcement agency level. This is currently available from 1974 to 2015. As most crime is committed at younger ages and the compulsory school laws also apply to these ages we choose to conduct our analysis on males aged 15 to 24 years old. For these ages, arrests are reported by single year of age.

For the purpose of the analysis, the geographical level of aggregation is at the county level as in Anderson (2014). This may initially seem odd since the reforms we are focused on occur at the state-level and it would therefore seem natural to analyse the impact at that level of aggregation. The problem we face in doing that is the substantial non-reporting of arrests by individual agencies to the UCR. This non-reporting changes over time and across states. To generate annual state-level arrest data therefore requires some method of imputation.[42] To give the most extreme example, consider the reform in Illinois that became effective from 2006 and increased the compulsory attendance age to 17. If we use a five-year window around the reform, only 1 of the 102 counties in Illinois consistently report arrest data every year – fortunately at least it is Chicago.

[42] One alternative approach that has been suggested is to only use the yearly observations on state-level arrest data when at least a minimum, say 95%, of the state population is reported on by the relevant agencies (see Bell et al., 2018). But this generates an unbalanced panel and is therefore not appropriate within our framework.

In the context of our research question and methodology using any such imputation would be inappropriate. We are seeking to exploit the discontinuity between cohorts over a short window and this requires consistent data to be available both pre- and post-reform. We therefore aggregate all agencies within a county and only include the county in our analysis if all the agencies report for all relevant years (or at most miss one year) around the reform window. Table A1 of Appendix A presents more detail on the numbers of covered and missing counties for each reform, together with information on the percentage of the state population covered.

Detailed county-level population numbers by sex, age and race are matched to arrest data and adjusted to the covering standards so as to produce precise age arrest rates and demographic composition controls. Unfortunately, the UCR data does not include a racial breakdown of arrests, making it impossible to evaluate the effect of the policies along a racial dimension.

Compulsory Schooling Laws

We have updated the compulsory schooling laws used in Bell, Costa and Machin (2016). The choice of how to measure the binding compulsory school age has been open to scrutiny: Stephens and Yang (2014) propose a refined version of the Goldin and Katz (2008) measurement combining start age, dropout age, grade requirement and child labour laws, whereas Oreopoulos (2009) and Anderson (2014) focus only on the dropout age enacted in the laws. Taking into account recent analysis by McAdams (2016) and Landerso et al (2017) pointing to a negative causal relation between starting age and crime (i.e. later starting ages are associated with reductions in crime propensities) we decide not to include these as a measure of variation in the laws as, if measured like Stephen and Yang (2014), they could have offsetting effects. On the other hand, we think that the binding school age is better measured taking into account the grade exemptions that often make up part of recent laws. For a given birth cohort $(t-a)$ where t denotes year and a is age, the measure of binding school age in state s is then given by:

$$DA_{s,(t-a)} = min\{Dropout\ Age_{s,t(t-a)}, Grade\ Required\ to\ Dropout_{s,(t-a)}\} \tag{5}$$

Figure 3 maps how changes in the dropout age enacted between 1980 and 2010 occurred between different states in the US. The map makes clear that some regions - such as the West South Central (Arkansas, Texas and Louisiana) and West Pacific (California and Washington) - have been most active over this period in introducing legislative changes to compulsory school age.

Defining the precise initial cohort that is affected by these change in compulsory schooling laws is not always as mechanical as subtracting the new dropout age from the year the law was enacted. It is also the case that some of the more recent laws studied in this paper contain a degree of complexity that is significantly higher than those enacted in the first three quarters of the twentieth century that have been considered in most previous research. In particular, some of the more recent law changes also feature employment exemptions, parental consents, mitigating circumstances and different effective dates. These all have some scope to add potential sources of measurement error to any attempt to code the laws. To reduce the noise around the cohort apportionment, all the changes have therefore been cross-validated empirically by analysing the data around the potential discontinuity and adjusting when needed.

Table 1 lists the 30 laws occurring between 1974 and 2010 that are used in the empirical analysis of this paper, together with detail on various relevant features of them including the particular dropout age change and new dropout age, and whether they feature exemptions by school grade.

Research Design

We study crime evolution before and after changes in compulsory school leaving laws based on arrest rates by individual year of age a for men in county c located in state s in time period t. A baseline crime reduced form is as follows:

$$Arrest_{acst} = \beta Reform_{s(t-a)} + \gamma X_{acst} + \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst} \qquad (6)$$

where *Arrest* is the log arrest rate, *Reform* is a dummy variable (to begin with) indicating whether or not there was a dropout age reform affecting birth cohort $(t - a)$ in state s, X is a set of county level controls and α_a, α_c and α_t respectively are fixed effects for age, county (also subsuming state fixed effects) and time, and ε is the equation error term.

When structured as in equation (6), this crime reduced form is essentially the one that has been estimated in existing work examining the causal impact of schooling laws by pooling together data across states which did and did not change their schooling laws over time (for wages, see for example, Acemoglu and Angrist (2001) and Oreoupoulos, (2009); for crime, see Lochner and Moretti (2004) and Bell et al. (2016); and for a range of outcomes probing robustness of the approach in detail see Stephens and Yang (2014)).

We begin by presenting estimates this way for comparison, but then move on to treat each of the reforms listed in Table 1 as a separate regression discontinuity (RD) around which we can examine what happens to crime before and after the reform takes place. To motivate the RD analysis, Figure 4 shows the discontinuity for the arrest rate for the pooled reforms (centred at t = 0). It shows a significant reduction in the arrest rate of 4 arrests per 1000 population (or 4.6 percent of the pre-reform mean of 0.086) relative to the earlier cohorts who were unaffected by the reform. The use of discontinuity design model to identify the effect of changes in compulsory school laws offers a robust way to capture within state time trends raised as a potential confounder by Stephens and Yang (2014). Furthermore, the combined use of narrow windows and differential trends at both sides of the threshold in the discontinuity design estimation offers both more precision, as well as dealing with the concerns of cohort-state specific trends as in the work by Stephens and Yang (2014).

More formally, for a given school dropout reform in a particular state, we can estimate the following specification for different time windows (w) around the dropout age policy changes:

$$Arrest_{acst} = \beta Reform_{s(t-a)} + f(t-a) + \gamma X_{acst} + \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst}$$
$$for \quad (t-a) - w \leq t-a \leq (t-a) + w, \qquad w = \{5, 7, 10\} \tag{7}$$

where the forcing variable in the classic RD design (see Imbens and Lemieux, 2008; Lee and Lemieux, 2010) is birth cohort $(t-a)$ and the general function $f(.)$allows for various functional forms that can be adopted for estimation.

To study the manner in which the policy change induces shifts in crime-age profiles, we further amend the RD design, allowing heterogeneity by age in the policy reform. This is precisely what the theoretical framework we described in Section 2 above argued needs to be done to see: a) how crime-age profiles may alter for different dropout ages; and b) to pin down the nature of incapacitation effects that occur when young people stay in school to later ages.

In practice, we estimate separate before/after policy effects in the crime reduced form for each age fixed effect, so that a more general estimating equation follows:

$$Arrest_{acst} = \theta_a(Reform_{s(t-a)}) + f(t-a) + \gamma X_{acst} + \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst} \tag{8}$$

$$\left.\frac{\partial Arrest_{acst}}{\partial Age_a}\right|_{a=j} = [\theta_j \times Reform_{s(t-a)}] + \alpha_j$$

where the partial derivative shows the impact for age *j* (*j* = 15, 16….24) as a function of the reform.

Controls

We match in a set of control measures that according to existing evidence (e.g. Levitt, 1997; Card and Krueger, 1992) may relate to both arrests and educational attainment and progress. Some of Card and Krueger’s (1992) school quality measures (pupil-teacher ratios, average teacher salary, number of schools) were updated at county-level using Common Core Date (CCD) data.[43] Police numbers were recovered from the FBI Law Enforcement Officers Killed and Assaulted (LEOKA) database and socio-demographic indicators were collected from the Local Area Personal Income (LAPI) data from Bureau of Economic Analysis. More details on these are provided in the Data Appendix.

4. <u>Crime-Age Profiles and Dropout Age</u>

Baseline Estimates of Crime Reduced Forms

Although the primary focus of the paper is on the crime-age profile, the empirical analysis begins by estimating the effect of the dropout reforms on the overall arrest rate. This is both because an overall effect is a necessary condition for the reforms to also alter the shape of the profile – since it is hard to think how the reform could increase the crime rate for those affected at any point in the profile – and because the prior literature has focused on such reduced forms and so it is useful to demonstrate that the reforms considered in this paper, which are more recent, generate similar effects as those examined previously.

Table 2 reports the baseline estimates of the crime reduced form. At this stage, all reforms across time and space are treated as equivalent and thus have a single indicator for reform. Later in this section separate estimates for each type of reform are presented (e.g. an increase in the school-leaving age from 16 to 17, 16 to 18, 17 to 18 etc). It turns out that the results are robust to allowing each type of reform to have separate estimates and it is therefore more straightforward to start with to present estimates for the weighted-average effect of all types of reforms, which is what Table 2 does. All standard errors are clustered at the state-cohort level, which is the dimension along which each reform occurs.

[43] Further details are given in Appendix A.

Implicit in the discussion thus far has been the assumption that each reform can be considered as exogenous to the parameters of interest. Crucially, we assume that school-leaving reforms where not instigated at a particular time and in a particular state in response to crime concerns related to the precise cohorts that would be affected by the reform. This seems unlikely to us because crime outcomes are generally viewed as an unintended consequence of school leaving age reforms. However, one way of assessing this is to consider balancing tests that compare observables between cohorts on either side of the discontinuity that the reform creates. Such tests are presented in Appendix Table A2 and there is no evidence to suggest any pattern around the discontinuity.

The first column in Table 2 presents estimates that simply turn on a reform dummy for particular cohorts in particular states using the dating provided in Table 1. This is therefore equivalent to the typical type of estimates that are presented in the reduced-form economics of crime literature such as Lochner and Moretti (2004) and given as equation (6) above. They do not explicitly take advantage of the discontinuity that each reform generates. The impact of the reform is significantly negative at the 1% level, and as such shows a strong crime reducing effect from higher dropout ages.[44]

The preferred estimates are those in the subsequent columns of the Table that are equivalent to equation (7) above and exploit the discontinuity across cohorts. They include a full set of state interactions with all the control variables and estimates are presented for different parametric forms for the running variable and for the length of the window around which we estimate the discontinuity. The first three estimates use a 10-year window around each discontinuity and each allows the running variable to have different parametric form on

[44] We have also estimated column (1) allowing for quadratic or cubic terms in the running variable and these produce coefficients very similar to the -0.099 reported in column (1): to be precise -0.086 and -0.091 respectively.

either side of the reform. It matters little what the functional form for the running variable is, so we proceed from now on with using a simple linear function.[45]

The discontinuity estimates are roughly half the size of column (1), but remain strongly significant. In the final two columns we experiment with a narrower window around the discontinuity. Again, there is not much to choose between these various specifications, so we proceed with a 5-year window on the basis that this more tightly focuses on the discontinuity.[46] This final estimate suggests a 6% fall in arrest rates for these young adults as a result of the dropout reform.

Different Types of Reform

The estimates presented in Table 2 pooled all the reform types together to estimate an average effect across the 30 reforms studied. In Table 3 estimates are reported separately for the 5 year window specifications for the five different reform types: the 29 reforms that featured an increase, either from 16 to 17, 17 to 18, 16 to 18, or any other increase; and the one reform in Texas in 1985 where the rewriting of the law lowered the dropout age from 17 to 16.

Column (1) begins by reporting an estimate for the weighted-average of the 29 reform types that involved an age increase. The estimated reduction in the Log(Arrest Rate) is -0.062 which, of course, is very similar to the column (6) specification of Table 2 of -0.060, although is slightly more negative. In column (2), the Texas increase attracts a significant positive

[45] All subsequent results are robust to using a quadratic or cubic function for the running variable, though such forms are computational feasible only for the longer windows around the discontinuity.

[46] The results in Table 2 follow the standard approach in the RDD literature of assuming that the reform is exogenous. We presented balancing tests on observables in Table A2 that are supportive of this assumption, but we recognise that this is a far from exhaustive list of observables and in any case one can never prove that all unobservables are balanced. A key concern may be that states decided to implement a reform at exactly the time it might have the most beneficial effect on crime. To examine this further we adopt a synthetic control approach and essentially combine the RDD design with a diff-in-diff approach. For each reform, we consider all other states as potential controls and use a five-year window prior to the reform to generate a synthetic control. Consider for example the reform in California that raised the leaving age in 1987. We use the average arrest rate for 15-24 year olds from 1982-1986 and match on arrest rate, percent black, percent young, personal income per head, employment-population rate and police officers per head. This then generates a set of weights for all other states that best matches the California arrest rate for 15-24 year olds in the pre-reform period. If we re-estimate the final column of Table 2 using this approach we obtain a coefficient estimate (and associated standard error) on the reform of -0.040 (0.007).

coefficient of 0.090. That the effect is positive in the case of a dropout age reduction offers a useful robustness test in line with crime reducing impacts of dropout age increases (and the opposite for this single case of lower dropout age) for the analysis.

Estimates for the four different groups of dropout age increases are presented in coulmns (3) through (6) of Table 3. There were respectively 8 reforms raising the dropout age from 16-17, 6 from 17 to 18, 8 from 16 to 18 and 8 in the catch-all 'Other' group.[47] In all four groups, there is a significant crime reduction effect, and the estimates are in a quite tight range between -0.041 and -0.071.

The use of county-level panel data means it is also possible to estimate the discontinuity for each reform separately. Estimates produced from doing this are presented in Table A4, but it is easier to visualise the various estimates as they are presented in Figure 5. Each point represents a separate reform labelled along the horizontal axis, and 95% confidence bands for each estimate are shown. Only one of the 30 reforms generates a significantly positive effect on arrest rates – the 1985 Texas reform. Of the other 29 reforms, 16 are significantly negative, and all but 4 have a negative estimate.

Different Crime Types

Table 4 present estimates for the 29 pooled reforms involving age increases that distinguish between different crime types (total, violent, property and drug arrests).[48] It also presents estimates that differ by two broad age groups (15-18 and 19-24). This second set of estimates offers a first indication as to whether the crime-age profile is altered by the reforms. The results of the Table suggest a fairly consistent pattern across crime types, though the effect is larger in magnitude (in absolute terms) for drug arrests than the other types of crime. Focusing on the age groups, in all cases the effect is larger for those contemporaneously

[47] The reforms in the 'Other' group are listed in the notes to Table 3.
[48] For the remainder of the empirical analysis, the focus is placed only upon the 29 dropout age increases, excluding the Texas increase. Results are however robust to including the increase.

affected by the reforms (i.e. in the younger 15-18 age range) than for those who were affected in the past. However this latter group still experiences a significantly lower arrest rate as a result of the reform that they were subject to when at school.[49]

The Impact on Crime-Age Profiles

Having demonstrated the crime-reducing effect of the reforms overall, and first identified some variation by broad age group, the focus is now directly placed on the effect on the entire crime-age profile, with an aim of studying the extent to which its shape may change in response to the education reforms. To begin, the specification for the 5 year window is generalised to have different reform effects at each single age – corresponding to equation (8) above. This then allows examination of the key question of the paper – can policy reforms alter the entire shape of the crime-age profile?

Consistent with the theoretical simulation presented in section 2, the results reported in Table 5 show that reforms have the largest effect for those directly incapacitated as a result of school attendance. However, they also show a significantly negative effect for later age groups that are not incapacitated in school as a result of the reform. These two findings emerge to varying degrees for different crime types.

Figure 6 shows the estimates, with 95% confidence bands, for each crime type. To highlight the effect on the crime-age profile overall, Figure 7 shows the estimated profiles pre- and post-reform by crime type. It is clear how the reforms are reducing crime at all stages of the life-cycle, though generally more heavily in the early years. Thus there is evidence of both a temporary incapacitation effect – when the young people are locked up in school – and a longer term crime reducing effect.

[49] For the total arrests specification in column (1) of the Table, the null hypothesis that the two age groups have the same arrest response to the reform can be rejected, with a p-value of 0.004.

Closer inspection of Figure 7 does reveal some differences in the balance between crime reductions at younger and older ages across crime types. When pooled, the total crime figure shows larger incapacitation effects. The same is true for property and drug crimes, and in the case of the former there is little in the way of an effect at older post-incapacitation ages. For violent crimes, the opposite holds: little in the way of incapacitation, but some crime reduction at older ages.

As we noted in the discussion of the theoretical model that in part motivates the empirical work, we can also look at how reforms affect various moments of the crime-age profile. Table 6 presents estimates for total crime and for the three sub-categories. All the moments are significantly shifted by the reform. The measures of central tendency (mean, mode) are shifted to the right as predicted and the standard deviation falls - thus the crime-age profile becomes more compressed after the dropout age is raised.

5. <u>Mechanisms and Discussion</u>

The reported results considered so far show a strong negative effect on arrest rates from school leaving age reforms. This operates both at the time an individual's behaviour is directly impacted by the policy, and also in subsequent years when they are not. The former effect is likely to be a result of incapacitation – a young person is constrained to remain in school, so they have less free time to allocate to crime. In this section, some potential mechanisms that may explain the latter longer run effect are considered.

Education Outcomes

There is by now a large literature that examines the causal effect of education on crime.[50] A natural interpretation of the dropout reform reducing criminality is that, in addition to the

[50] Many of these studies were cited earlier, but see also the review in Lochner (2011).

direct incapacitation effect that occurs from requiring students to remain in school for an additional year, the additional year also generates a productive educational benefit for those on the margin of criminal behaviour. This then raises their human capital, wages and employment and reduces the probability of committing crime in the future. This is consistent with the theoretical model outlined in Section 2, and with the earlier US research studying the impact of the earlier compulsory school leaving reforms from the 1960s, 1970s and 1980s.[51]

To assess this explanation of the results, the empirical connection between the reforms and different measures of education and work are considered. First of all, looking at the incapacitation side of things, we explore whether school attendance did in fact increase by utilising Current Population Survey (CPS) data on 16-18 year olds between 1974 and 2015 (see the Data Appendix for more details). Panel A of Table 7 shows the estimates, structured in the same way as the earlier baseline results for arrests. There is significant evidence of incapacitation, with the 5-year window specification in column (4) showing a 3.2 percentage point rise, or a 4.4 percent increase relative to the pre-reform mean. This reaffirms that school incapacitation effects were a key dimension of the dropout age reforms.

To explore what might lie behind the longer run crime reducing effects, the remainder of the Table reports results for education and job related outcomes for older individuals aged 19-60 in the American Community Survey (ACS) from 2006 onwards.[52] The outcomes are high-school dropout rates, whether or not an individual was in education or work, and log weekly real wages. Whilst there are statistically significant effects in the expected direction for a number of the specifications, the estimates are relatively small in magnitude. They do uncover education improvements that followed from dropout age reform, and an increased likelihood

[51] For crime, see Lochner and Moretti (2004) . For reviews of the sizable bodies of research on wage effects see Card (1999) and Oreoupoulos (2009). For a host of other non-wage outcomes variables – including health, voting behaviour and life satisfaction - see Oreopoulos and Salvanes (2011).
[52] ACS data is used because it is annual data that can be used to study the reforms across pooled birth cohorts. See the Data Appendix for more detail.

of being in school or work, but the effects are small – relative to the pre-reform mean, they respectively correspond to a 5.3 percent fall in high school dropout and a 0.4 percent increase in the likelihood of being in education or work. Unlike in the previous work on earlier reforms (e.g. Acemoglu and Angrist, 2001;Card, 1999), there is essentially no effect on wages in any specification.[53]

Interpretation

The positive effects of the reforms on educational attainment are therefore modest, certainly in comparison to Lochner and Moretti (2004) who find estimates that are quite a lot bigger than those reported in Table 7. Our previous work (Bell, Costa and Machin, 2016) has also demonstrated that the most recent reforms to compulsory schooling laws have substantially weaker effects on educational attainment than estimates identified using changes from dropout age reforms in the 1950s and 1960s. This is in line with the notion that the group of compliers – e.g. those that obtain a high-school diploma when the reform occurs who would not have done previously – is a smaller percentage of the eligible population for the period studied in this paper.

This interpretation makes sense as the high school dropout rate for those aged 16-24 fell from 27.2 percent in 1960 in Lochner and Moretti's data to 5.9 percent in 2015. This shrinks the group of potential compliers by a lot and makes it more likely that the dropouts are a hard core of individuals for whom such reforms are unlikely to have any effect (i.e. a higher share of never takers). This does not mean that there is no effect – after all a 0.5% percentage point (5.3 percent) fall in the dropout rate will certainly affect the criminal margin for some individuals. But it seems unlikely that the size of this change in educational attainment can explain the entire 3-4 percent reduction in arrest rates that we observe for 19-24 year olds.

[53] Lack of a wage effect from dropout age reforms is not unique to this paper. Pischke and Von Wachter (2008) report no wage gains from German compulsory school leaving age reforms. However, ****.

If the reforms do not substantively boost educational attainment or wages, what other mechanisms can explain the lower crime rate further along the age distribution when direct incapacitation effects cannot be operative? One possibility is dynamic incapacitation. This is where the direct incapacitation from being kept in the school classroom causes changes that affect future crime participation, independent of whether there is any educational value to the incapacitation. For example, suppose that being kept in school during the day prevents an individual from being on a street corner dealing drugs. This reduces arrests at the time, but also potentially means that the individual leaves school without the criminal record they would otherwise have had. They now find it easier to pursue a life as a law-abiding citizen. Put another way, for some individuals crime onset is stopped by incapacitation and they never commit crime subsequently. For other individuals who may already have committed crime, the incapacitation reduces their crime intensity in the incapacitation period and this persists as they get older – the reform acts to reduce their criminal capital accumulation as compared to the counterfactual of no reform.

Other evidence also suggests that interventions at this crucial period of potential criminal development can alter the life course of criminality. Bell, Bindler and Machin (2018), for example, show that leaving high school in a recession can significantly increase the affected cohorts' arrest rates well into adult life – in a sense the recession generates crime scars that persist beyond the period of direct impact. In a different setting, looking at random assignment of judges in Chicago to identify the causal effects of juvenile incarceration, Aizer and Doyle (2015) show that incarceration both reduces the probability of high-school graduation and increases the likelihood of subsequent incarceration as an adult. Both these studies are consistent with finding of dynamic incapacitation effects.

6. Conclusions

This paper presents the first evidence to show that compulsory schooling law reforms do not just affect the overall level of crime, but they also re-shape the crime-age profile. Focusing on changes in laws across US states since the 1980s, a multiple regression discontinuity framework is used to show that arrest rates for young men fall by around 6% on average as a result of these reforms. Whilst there is a larger negative effect for those in the age group that are directly constrained by the reforms – they are kept in school and incapacitated, hence having less time to devote to potential criminal activity – there is also a significant negative effect for those who are no longer directly constrained. The results are consistent with there being *both* an incapacitation effect *and* a longer-term beneficial crime reducing effect. This longer run effect is interpreted as a dynamic incapacitation effect because further evidence we present shows that these same reforms had very modest effects on average educational attainment and wages, though somewhat more substantial effects on the high-school dropout. Further study of how dynamic incapacitation may arise therefore forms an important future research challenge for better understanding how individual crime dynamics evolve over the life course, and how they may be affected by policy changes.

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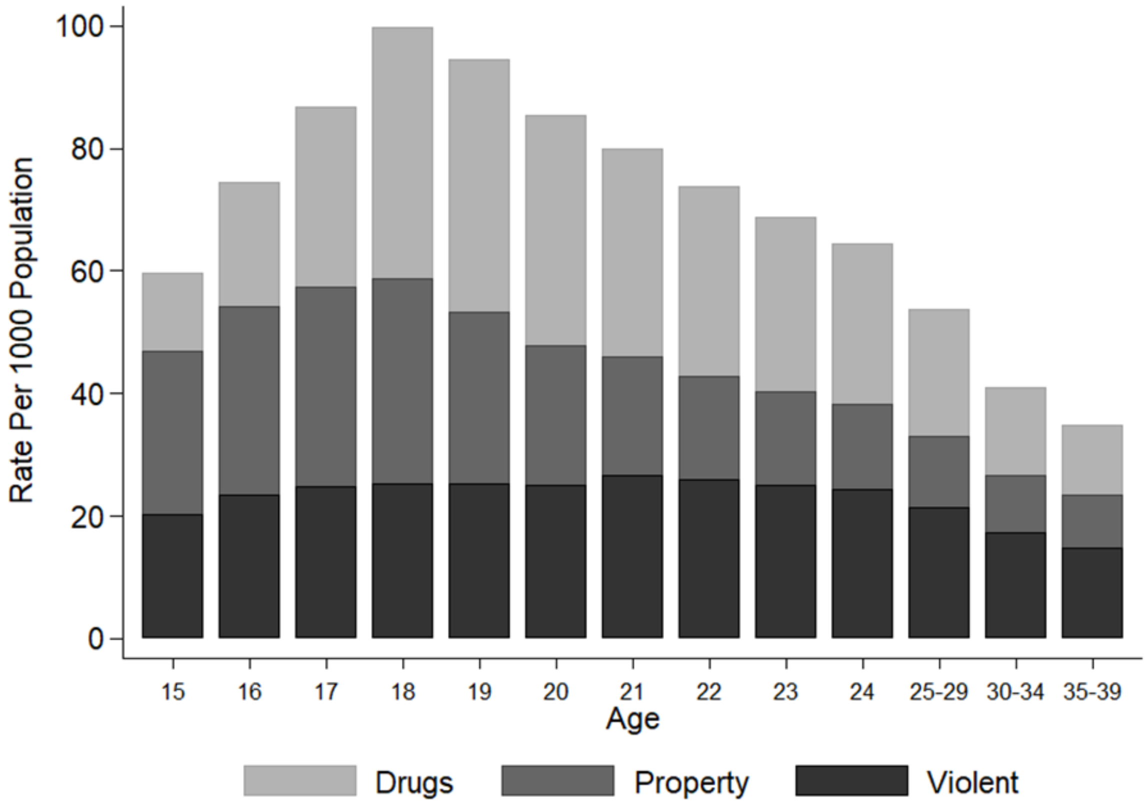
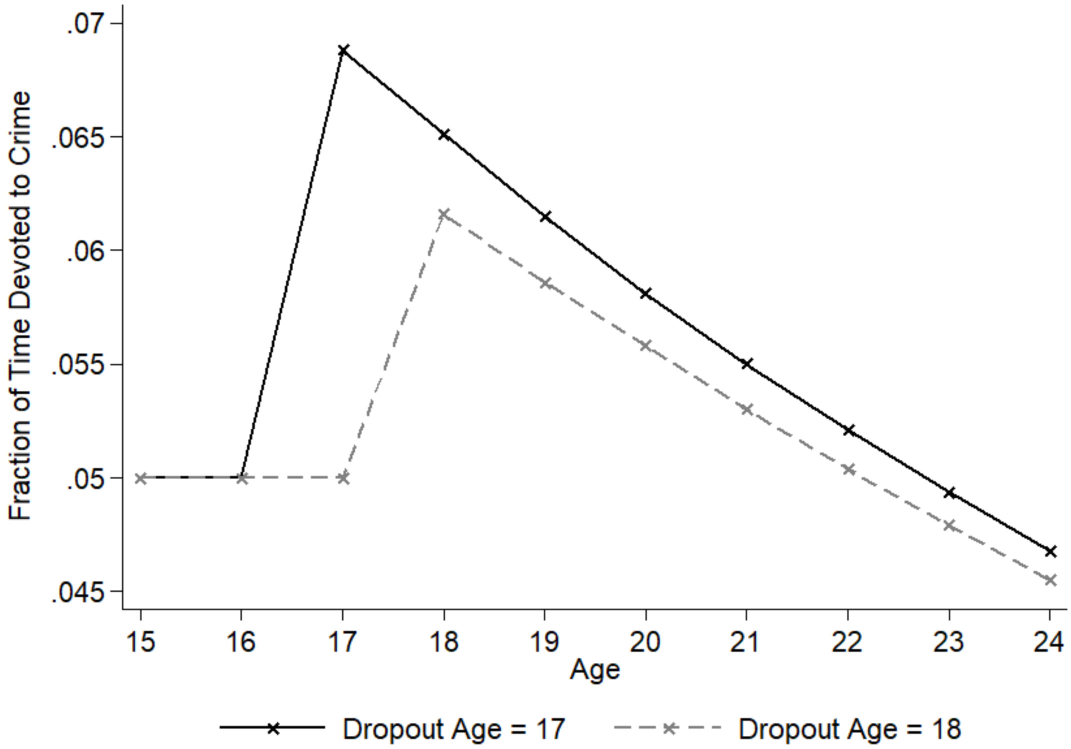
Figure 1: Male Offender Rates by Age, US



Notes: Male arrest rates by age, calculated for years 2000-2010 from UCR data. Only agencies reporting all years of the time period covered are included. The composition of the different type of crime is covered in the Appendix.

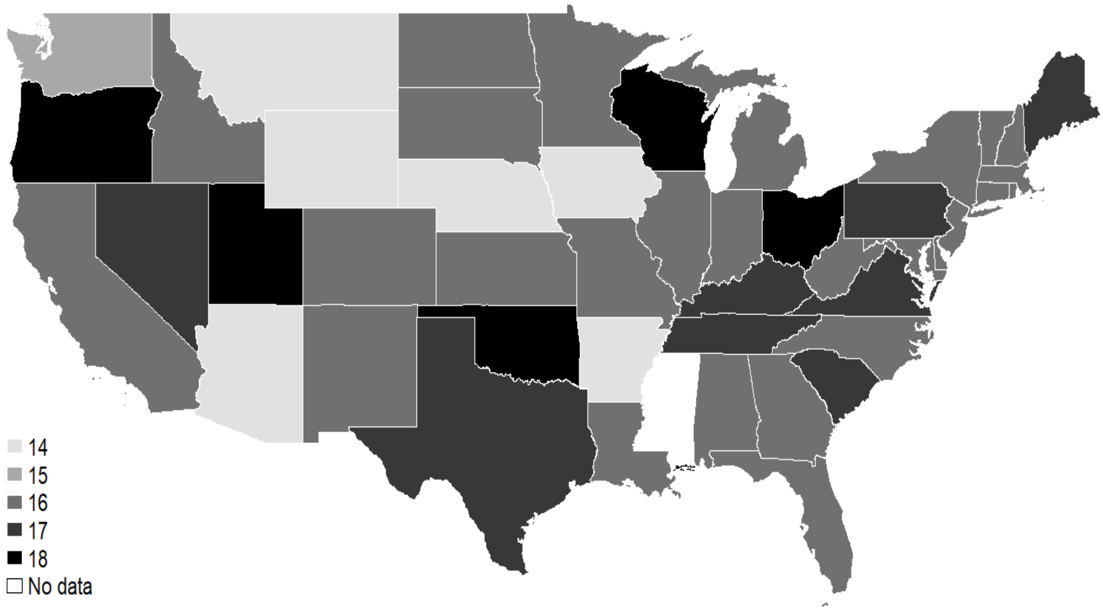
Figure 2: Model Simulation



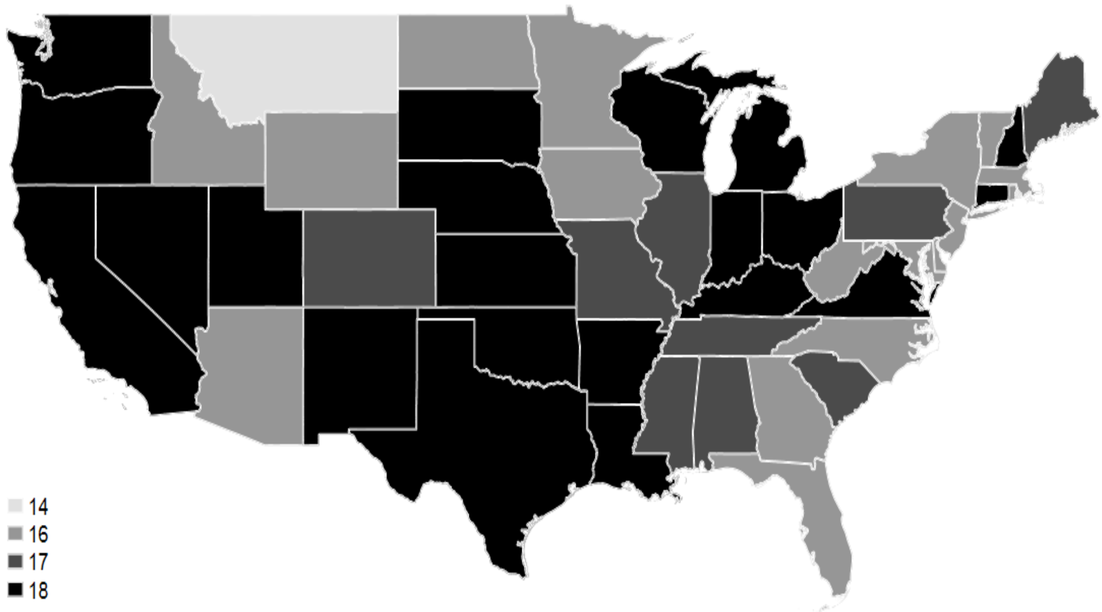
Notes: Details on the model simulation are presented in Appendix B. The ages 15 to 24 are those covered in the empirical analysis and the fraction of time devoted to crime is rescaled by a constant factor to (broadly) approximate the arrest rates observed in the data.

Figure 3: State Dropout Ages, 1980 and 2010

1980

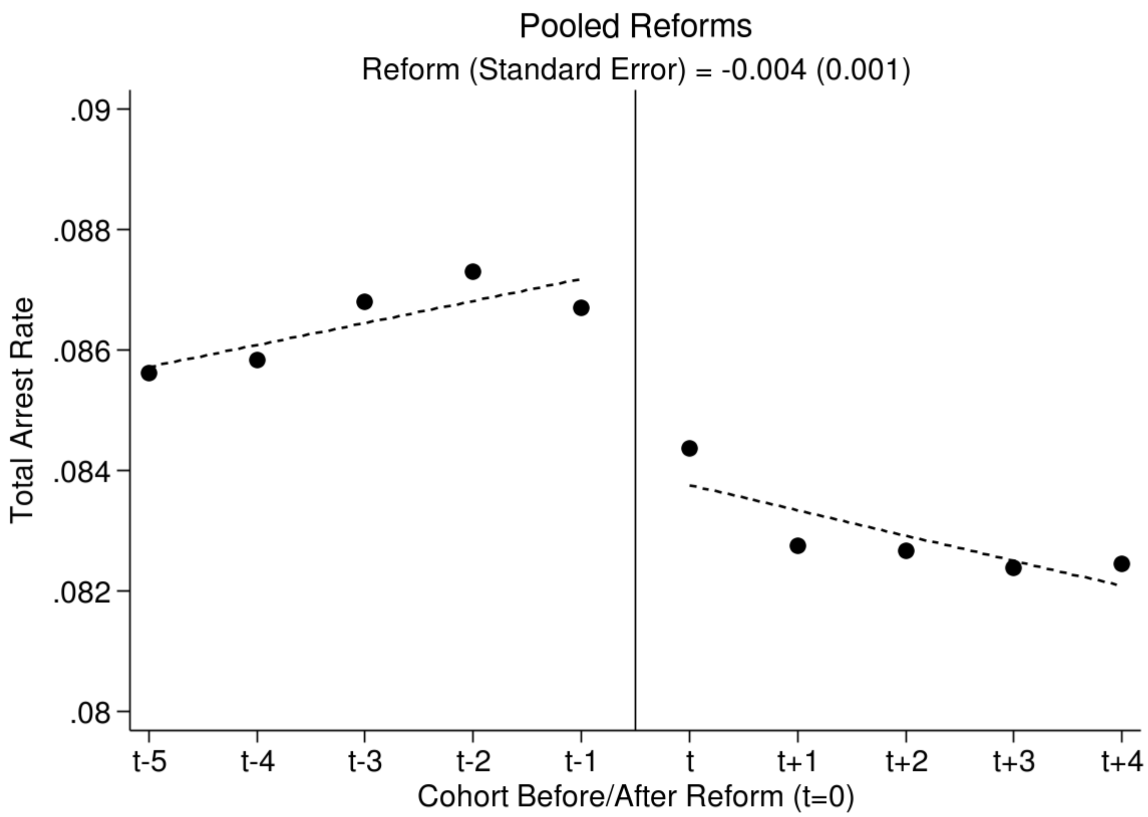


2010



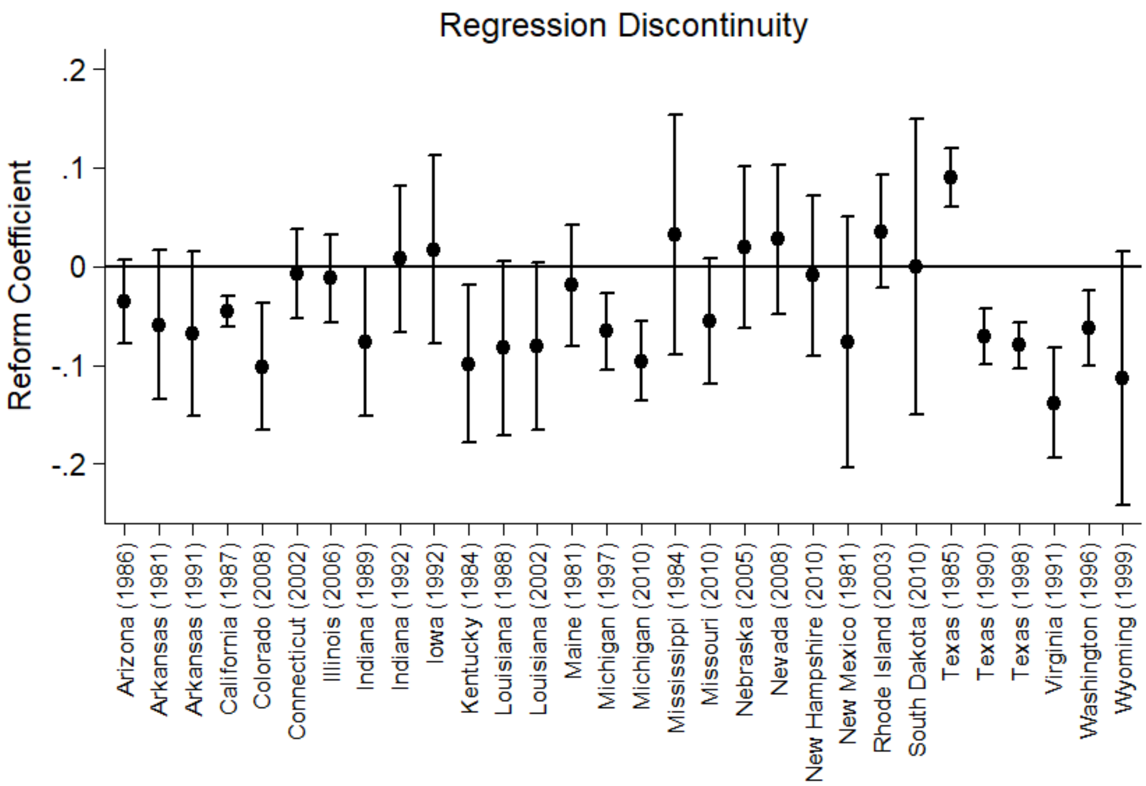
Notes: Dropout Age is calculated using the formula Dropout Age_{st} = min{Minimum Leaving Age_{st} , Grade $Exemption_{st}$ }

Figure 4: Arrest Rates Before/After Reforms



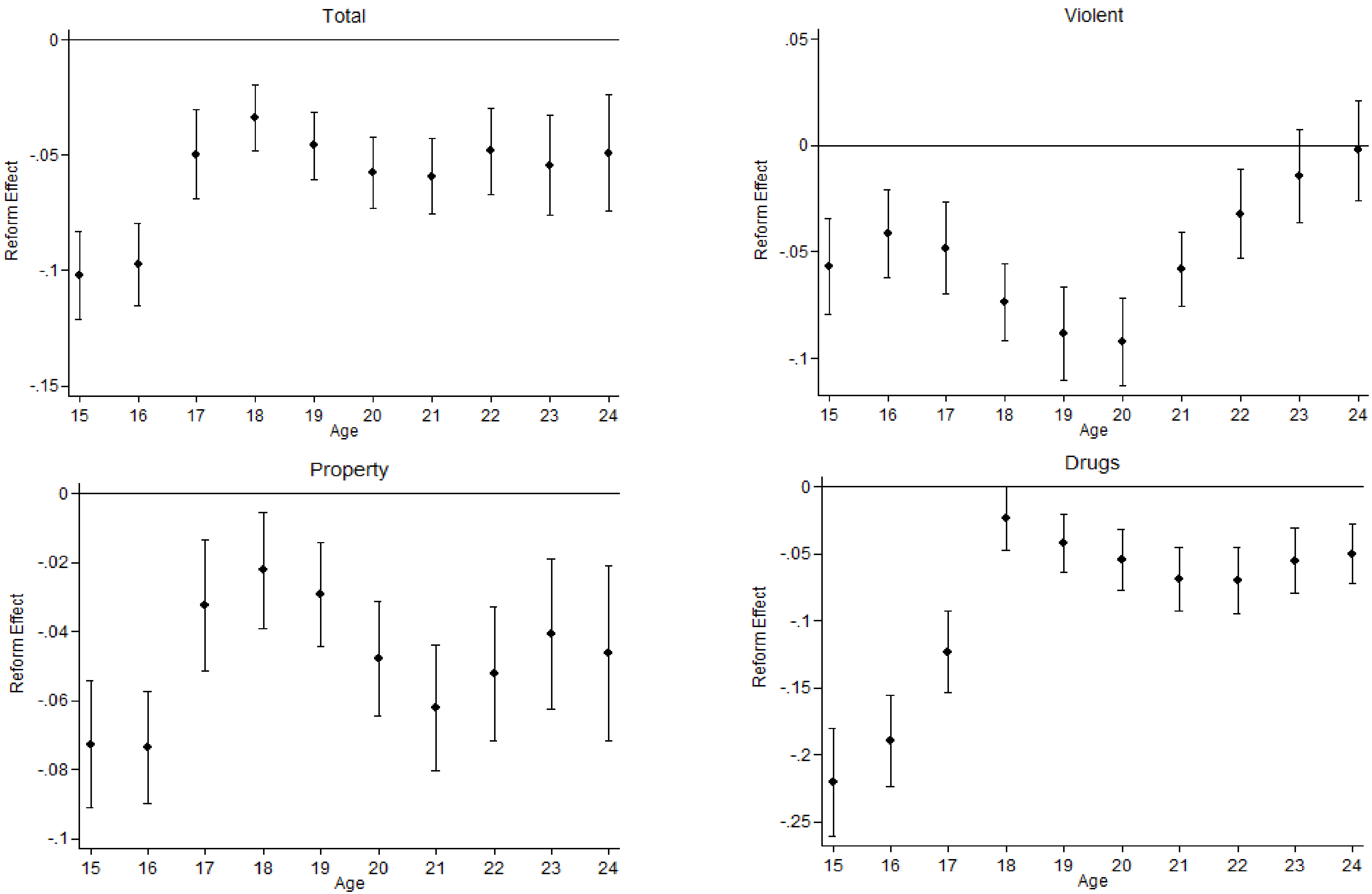
Notes: The reported discontinuity estimate (with associated standard error in parentheses) is the +/- 5 year mean difference pre and post-reform for the outcome variables.

Figure 5: Estimated Discontinuity Coefficients



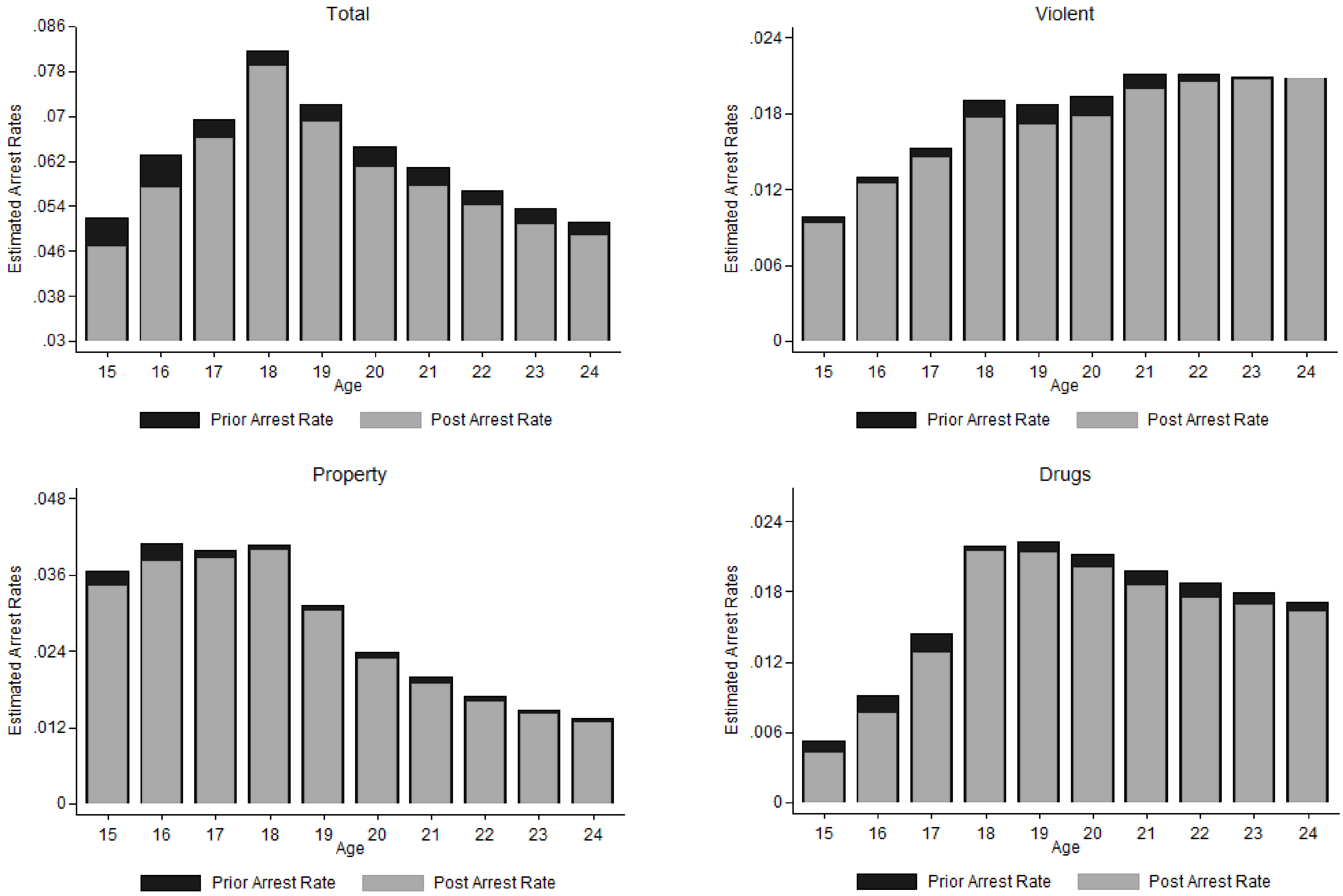
Notes: Coefficients from Table A4. 95% confidence intervals are shown.

Figure 6: Discontinuity Estimates by Age and Crime Type



Notes: From estimates in Table 5. Texas (1985) is excluded from the estimation given that is a decrease in dropout age. Confidence intervals at 95% significance, standard errors clustered at reform-cohort level.

Figure 7: Crime-Age Profile Shifts by Crime Type



Notes: Prior arrest rate is the mean of arrest rate by age prior to discontinuity using a 5-year bandwidth. Post arrest rate is the calculated by estimated age effects of Figure 6.

Table 1: State Dropout Age Reforms

State	Effective School Year From Statute	Type	Change	New Dropout Age
Arizona	1985 and 1986	Exemption	8^{th} to 10^{th} grade	16
Arkansas	1981	Leaving Age	16 to 17	17
Arkansas	1991	Leaving Age	17 to 18	18
California	1987	Leaving Age	16 to 18	18
Colorado	2008	Leaving Age	16 to 17	17
Connecticut	2002	Leaving Age	16 to 18	18
Illinois	2006	Leaving Age	16 to 18	17
Indiana	1989	Leaving Age	16 to 17	17
Indiana	1992	Leaving Age	17 to 18	18
Iowa	1992	Exemption	8^{th} to 12^{th} grade	16
Kentucky	1984	Leaving Age	17 to 18	18
Louisiana	1988	Leaving Age	16 to 17	17
Louisiana	2002	Leaving Age	17 to 18	18
Maine	1981	Exemption	9^{th} to 12^{th} grade	17
Michigan	1997	Exemption	NA to 12^{th} grade	16
Michigan	2010	Leaving Age	16 to 18	18
Mississippi	1984	Leaving Age	Reenactment	17
Missouri	2009	Leaving Age	16 to 17	17
Nebraska	2005	Leaving Age	16 to 18	18
Nevada	2008	Leaving Age	17 to 18	18
New Hampshire	2010	Leaving Age	16 to 18	18
New Mexico	1981	Exemption	10^{th} to 12^{th} grade	18
Rhode Island	2003	Exemption	NA to 12^{th} grade	16
South Dakota	2010	Leaving Age	16 to 18	18
Texas	1985	Leaving Age	Rewriting of law	16
Texas	1990	Leaving Age	16 to 17	17
Texas	1998	Leaving Age	17 to 18	18
Virginia	1991	Leaving Age	17 to 18	18
Washington	1996	Exemption	9^{th} to 12^{th} grade	18
Wyoming	1999	Exemption	8^{th} to 10^{th} grade	16

Notes: Mississippi abolished its compulsory school law in 1956, and reenacted it 1983/84 with an initial leaving age of 7 with progressive raise until 17 by the school year 1989/90. Texas has written its laws of 1984 and 1989 in a different way, stating the minimum leaving age was to include the completion of school year in which the birthday occurred in effect decreasing/increasing the leaving age by some months. Two other reforms occurred during the same period – in South Carolina (1987) and Kansas (1996). Missing arrests data precludes them from this study.

Table 2: Baseline Estimates of Crime Reduced Forms

	Log(Arrest Rate), 1974 to 2015					
	(1)	(2)	(3)	(4)	(5)	(6)
	All States	10-Year Window	10-Year Window	10-Year Window	7-Year Window	5-Year Window
Reform	-0.099 (0.018)	-0.047 (0.007)	-0.065 (0.009)	-0.038 (0.008)	-0.062 (0.007)	-0.060 (0.006)
Running Variable		Linear*Reform	Quadratic*Reform	Cubic*Reform	Linear*Reform	Linear*Reform
Reform Interactions		X	X	X	X	X
Sample Size	1,121,590	344,940	344,940	344,940	246,526	178,005
Number of States	48	24	24	24	24	24
Number of Counties	3,063	1,277	1,277	1,277	1,277	1,277
Pre-Reform Mean (Arrest Rate)	0.080	0.086	0.086	0.086	0.085	0.085

Notes: Sample includes males in each age group 15-24 inclusive for US counties. Estimates are weighted by population size and standard errors are clustered at state-cohort level (reform-cohort level for discontinuity windows). The dependent variable is the log of total arrest rate including violent, property and drug crimes. All specifications include age, year and county fixed effects. Covariates further include log of population, log of police force sworn and shares of female, black, non-white/non-black population. Reform Interactions means every covariate is made state-reform specific by adding an interaction with the state-reform indicator. Columns (2) to (6) include a centered running variable interacted with the dropout reform indicator as to allow differential trends at each side of the discontinuities.

Table 3: Estimates by Reform Type

	Log(Arrest Rate), 1974 to 2015, Discontinuity (+/- 5 years) Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
	All Age Increase Reforms	17 to 16, Texas	16 to 17	17 to 18	16 to 18	Other
Reform	-0.062 (0.006)	0.090 (0.015)	-0.071 (0.015)	-0.068 (0.013)	-0.060 (0.011)	-0.041 (0.007)
Sample Size	156,517	21,488	47,943	46,984	34,209	27,381
Number of States	24	1	7	6	8	8
Number of Counties	1,242	222	533	487	374	282
Pre-Reform Mean (Arrest Rate)	0.085	0.075	0.086	0.081	0.089	0.077

Notes: As for Table 2. Same specification as column (6) of Table 2. Each column shows separate regression according to the relevant reform sample. “Other” include the following reforms: Arizona (1985), Iowa (1992), Maine (1981), Michigan (1997), Mississippi (1984), Rhode Island (2003), Washington (1996) and Wyoming (1999).

Table 4: Estimates by Crime Type and Age

	Log(Arrest Rate), 1974 to 2015, Discontinuity (+/- 5 years) Sample, All Age Increase Reforms			
	(1)	(2)	(3)	(4)
	Total	Violent	Property	Drugs
A. Overall Reform Effect				
Reform	-0.062	-0.056	-0.053	-0.099
	(0.006)	(0.008)	(0.007)	(0.010)
B. Reform Effects By Broad Age Groups				
Reform*Age 15-18	-0.064	-0.059	-0.053	-0.128
	(0.005)	(0.008)	(0.010)	(0.013)
Reform*Age 19-24	-0.041	-0.046	-0.043	-0.042
	(0.005)	(0.008)	(0.008)	(0.009)
Sample Size	156,517	156,517	156,517	156,517
Number of States	24	24	24	24
Number of Counties	1,242	1,242	1,242	1,242
Pre-Reform Mean (Arrest Rate)	0.085	0.025	0.035	0.025

Notes: As for Table 2. Same specification as column (6) of Table 2. Sample excludes Texas (1985) reform given that is a decrease in compulsory schooling.

Table 5: Age Varying Reform Impacts

	Log(Arrest Rate), 1974 to 2015, Discontinuity (+/- 5 years) Sample, All Age Increase Reforms			
	(1)	(2)	(3)	(4)
	Total	Violent	Property	Drugs
Reform*Age = 15	-0.102	-0.059	-0.078	-0.221
	(0.010)	(0.014)	(0.011)	(0.024)
Reform*Age = 16	-0.097	-0.043	-0.075	-0.190
	(0.009)	(0.013)	(0.010)	(0.020)
Reform*Age = 17	-0.050	-0.049	-0.035	-0.123
	(0.010)	(0.013)	(0.012)	(0.016)
Reform*Age = 18	-0.034	-0.076	-0.023	-0.023
	(0.007)	(0.011)	(0.009)	(0.013)
Reform*Age = 19	-0.046	-0.092	-0.032	-0.041
	(0.007)	(0.013)	(0.009)	(0.013)
Reform*Age = 20	-0.057	-0.095	-0.052	-0.057
	(0.008)	(0.012)	(0.009)	(0.013)
Reform*Age = 21	-0.059	-0.061	-0.063	-0.072
	(0.008)	(0.010)	(0.011)	(0.015)
Reform*Age = 22	-0.048	-0.033	-0.051	-0.074
	(0.010)	(0.013)	(0.012)	(0.016)
Reform*Age = 23	-0.054	-0.013	-0.053	-0.068
	(0.011)	(0.015)	(0.015)	(0.017)
Reform*Age = 24	-0.049	-0.003	-0.061	-0.060
	(0.014)	(0.017)	(0.018)	(0.016)
Sample Size	156,517	156,517	156,517	156,517
Number of States	24	24	24	24
Number of Counties	1,242	1,242	1,242	1,242
Pre-Reform Mean (Arrest Rate)	0.085	0.025	0.035	0.025

Notes: As for Table 2. Same specification as column (6) of Table 2. Sample excludes Texas (1985) reform given that is a decrease in compulsory schooling.

Table 6: Crime-Age Profile Summary Measures Before and After Dropout Age Changes

	Changes in Crime Age Profile Summary Measures							
	Mean	Standard Deviation	Skewness	Kurtosis	20th Percentile	50th Percentile	80th Percentile	Mode
Total	0.035	-0.024	-0.012	0.014	0.136	0.075	0.090	0.315
	(0.010)	(0.004)	(0.004)	(0.005)	(0.018)	(0.019)	(0.015)	(0.065)
Violent	0.044	-0.004	-0.022	-0.012	0.117	0.123	0.165	0.787
	(0.013)	(0.006)	(0.007)	(0.018)	(0.024)	(0.024)	(0.021)	(0.093)
Property	-0.003	-0.033	0.007	0.034	0.039	-0.015	-0.039	0.400
	(0.013)	(0.005)	(0.007)	(0.020)	(0.021)	(0.021)	(0.017)	(0.070)
Drugs	0.156	-0.011	-0.051	-0.015	0.178	0.202	0.218	0.438
	(0.014)	(0.006)	(0.007)	(0.015)	(0.021)	(0.024)	(0.020)	(0.064)

Notes: Calculated with population weights. Estimates are computed based on the residual arrests after compositionally adjusting at state-level for year, log police employed, log population and share of females, black and non-white/non-black population. Texas (1985) is excluded given that is a decrease in compulsory schooling. Discontinuities after 2008 are excluded given the unavailability of data to balance ages covered on both sides of the discontinuities.

Table 7: Estimates for High School Attendance, Education, Employment and Wages

		(1)	(2)	(3)	(4)
	Pre-Reform Mean	All States	10-Year Window	7-Year Window	5-Year Window
A. High School Attendance (16-18)					
Reform	0.747	0.010	0.039	0.040	0.050
		(0.003)	(0.005)	(0.006)	(0.006)
B. High School Dropout					
Reform	0.109	-0.007	-0.004	-0.005	-0.006
		(0.001)	(0.001)	(0.001)	(0.001)
C. School or Work					
Reform	0.818	0.009	0.003	0.004	0.003
		(0.001)	(0.002)	(0.002)	(0.002)
D. Log Weekly Real Wages					
Reform	6.576	0.010	0.004	0.007	0.005
		(0.003)	(0.003)	(0.004)	(0.004)
Running Variable			Linear*Reform	Linear*Reform	Linear*Reform
Reform Interactions			X	X	X
Sample Size (Panel A)		1,026,804	254,257	181,689	131,001
Sample Size (Panels B and C)		6,816,430	1,716,601	1,201,659	861,019
Sample Size (Panel D)		4,854,245	1,272,952	893,008	640,527
Number of States (Panel A)		41	17	17	17
Number of States (Panels B to D)		48	24	24	24

Notes: CPS Basic Monthly (Panel A) sample includes all males, ages 16 to 18, from 1976-2015. Attendance in A is defined as an individual reporting to attend school full-time with education attainment lower than some college (See Appendix A).Panels B to E includes US born males in each age group 19-60 inclusive from 2006-2015 American Community Survey (ACS). Estimates are weighted by population weights and standard errors are clustered at state-cohort level. The dependent variables are: years of schooling, an indicator for high school dropout, an indicator for currently employed or attending school individuals (work or school) and log of real weekly wages. All specifications include age, year, black, hispanic and state of birth fixed effects (month fixed effects are added to row A). Reform Interactions means every covariate is made state-reform specific by adding an interaction with the state-reform indicator.

Appendix A: Data Description

A1. Panel Data on Arrests

Panel data for the US come from the FBI Uniform Crime Reports (UCR). The measure of crime is arrests. The UCR reports the number of arrests by year, state, age, gender and type of crime. The original data identifies the number of arrests by law enforcement agencies within states. We construct a county-level panel on arrests by aggregating the number of arrests over law enforcement agencies within a county. Within the UCR, data for certain agencies is systematically missing. For example, New York City systematically does not report arrest numbers. For the agencies used in our estimation we impose a reporting pattern consistent with a maximum tolerance of one missing year per discontinuity window (i.e. for 10-year bandwidth the agency needs to report 18 out of the 20 years)[54].

In addition, the UCR reports the total population for each law enforcement agency in the reported year. Aggregating the UCR population count to the county-year level and comparing that number to official population counts allows us to identify county-year covering ratio. The weighted average county-level covering ratio is 89% for the 5-year bandwidth. When estimating the population per age-sex cell, we use the SEER*Stat population estimates[55] at county level and apply the yearly covering ratios homogeneously across different ages. The implicit assumption is that the missing population has the same age breakdown as the overall county-year population. The weighted average share of state population covered in the 5-year bandwidth is 81% for reform states.

We sample males aged 15 to 24 from 1974 to 2015. The UCR data are grouped by age category. From age 15 up to the age of 24, the number of arrests is measured by single age year.

Following the literature, we categorize arrests into property and violent crime using the UCR offense code variable as follows:

Violent crime:	Property crime:	Drug Crime:
01A = Murder and non-negligent manslaughter	05 = Burglary – breaking or entering	18 = Drug Violations (Possession, Sale and Manufacturing)
01B = Manslaughter by negligence 02 = Forcible rape 03 = Robbery 04 = Aggravated assault 08 = Other assaults	06 = Larceny – theft (except motor vehicle) 07 = Motor vehicle theft 09 = Arson	

[54] Table 2, column (1) sample includes only agencies reporting at least 10 years over 1974-2015. The results are robust to a stricter reporting of all years in the bandwidth period.
[55] See Surveillance, Epidemiology, and End Results (SEER) Program of the National Cancer Institute (NCI) 1969-2014.

In order to produce arrest rates, we aggregate the number of arrests for the above categories and divide the resulting number of arrests by the annual county-age-year population. Some cells report 0% arrest rates, however for those we assume the lowest arrest rate reported in the sample period. Cell reporting arrest rates above 40% are excluded from the sample to avoid outliers influencing the analysis.[56]

A2. Racial Breakdown Covariates

An analogous method to one used to obtain population age-sex cell in *A1* is performed to estimate the racial breakdown of each cell. We use the racial population estimates collected from SEER*Stat at county-year level and the county covering ratio to estimate the number of population by race for each sex-county-year cell.

A3. Police Numbers

The police numbers used are collected from FBI LEOKA (which is available from 1960-2015). This data reports several police enforcement measures yearly for each enforcement agency. We use the total number of sworn officers per county-year as the measure of police force present at the geographic area of interest.

A4. County-Year Economic and Employment Covariates

Information on economic and employment indicators at county-year level are collected from Local Area Personal Income (LAPI) from Bureau of Economic Analysis (BEA) 1960-2015. Measures of total employment, personal income, wage income and several others are available at county-year level from LAPI.

A5. School Quality Measures

We use the Local Education Agency (LEA) level data available in the Common Core Data (CCD) from National Center for Education Statistics (NCES), both fiscal and non-fiscal[57], to produce the school quality measures. By aggregating both the number of students, teachers and instruction salary expenses at county-year level, we are able to compute estimates of pupil-teacher ratio and average teacher salary. We interpolate missing years in the data, these are not frequent and do not affect the general results.

A6. ACS 2006-2015: Education and Work

We sample all males aged 19-60 from Integrated Public Use Microdata Series (IPUMS) for the American Community Survey (ACS) 2006-2015[58]. The sample is restricted to US born individuals as to ensure the individuals are directly affected by the compulsory schooling laws enacted.

[56] The results are robust to the use of 30% and 50% thresholds alike.
[57] Unfortunately, fiscal information at LEA level is only available since 1989.
[58] Despite the fact that ACS started being collected as a 1% sample in 2005, this year is not included in the sample analysis given the empirical break in the education variables between 2005 and 2006.

Years of schooling are coded according to Acemoglu and Autor (2011). High school dropout is defined as an individual who has less than a high school graduation diploma or equivalent. Work and school indicator function is defined for an individual who is either classified as currently employed or attending school (college or high school) both full-time or part-time. Weekly and hourly wages are coded for both part-time and full-time workers (excluding self-employed and unpaid family workers) according to Acemoglu and Autor (2011) with minor improvements on top coding by making the adjustments state-specific according to the ACS sample design.

Reforms are matched by state of birth, as it is assumed that an individual born in a given state has attended school in that same state at least until dropout age. When matching the reforms to the individual data from ACS, a one-year sliding on the reform year is observed and adjusted for. The previous arises due to the inability to precisely estimate the year of birth for a given individual as data is collected over different months for each survey year and state, and only age is provided in the ACS hence making year of birth an approximated variable.

A7. CPS Monthly Basic 1976-2015: School Attendance

We sample all males aged 16-18 from the National Bureau of Economic Research (NBER) archive of the Basic Monthly Current Population Survey (CPS) 1976-2015. Unlike with the ACS sample, we are not able to distinguish between US born and migrants in the CPS consistently through the sample period. Summer months (June, July and August) are excluded from the sample as they consistently report significantly low enrollment in high school or college.

High school attendance definition before 1984 is based on the answer to the question "What was your main activity last week?"[59] being "School" conditional on the individual not having any education attainment superior to high school graduation. After 1984, individuals are directly questioned about their enrollment status differentially between high school and college[60]. We, therefore, define an individual as attending high school if he/she declares to be enrolled in high school conditional not having any education attainment superior to high school graduation. We analyze the sample period when both questions are available (1984-1993) and conclude that, conditional on the individual not having any education attainment superior to high school graduation, 91% of the individuals stating to be enrolled in high school answered "School" as their main activity last week. This attests for the strong correlation between both measures, dissipating concerns on significant jumps in the variable of school attendance between periods.

In CPS individuals do not report their state of birth, hence reforms are matched by state of residence. Considering that school attendance is being measured contemporaneously, we have no strong reason to believe that individuals between 16 and 18 years of age would not be subject to the school dropout age of their state of residence. When matching the reforms to the individual data from ACS and CPS, a one-year sliding on the reform year is observed

[59] This question was discontinued in 1994.
[60] This variable is only available for individuals 16 or older, hence the sample of the analysis starting at age 16 and not earlier despite a few reforms potentially affecting younger ages.

and adjusted for. The previous arises due to the inability to precisely estimate the year of birth for a given individual as data is collected over different months for each survey year and state, and only age is provided in the ACS and CPS hence making year of birth an approximated variable.

A8. Compulsory Schooling Laws

Compulsory schooling laws are collected directly from official annotated statutes of each state in the Westlaw International Database for each of the corresponding years. When provided in the statutes, the effective date of the new law is taken as the year of reform otherwise enactment year is assumed to be the most sensible approximation.

The data retrieved includes maximum entry age, minimum leaving age and education grade which exempts a child from staying in school. The laws have historically increased in their complexity adding several exemptions including work permits and early age parental consent letters to exemplify the most common. The Labor Standards Act 1939 harmonized child labour laws across states in the US, recent changes were not of a comparable order of magnitude as the ones seen during that period. To be consistent we ignore the possibility of parental consent authorizations to leave school at an age below the minimum dropout age, as these are often seen as exceptions rather than the rule.

Table A1. Coverage of Counties

State (Year)	Covered Counties / Total Counties	% Within County Coverage	% Overall State Coverage
Arizona (1985/86)	14 / 16	85.0%	91.3%
Arkansas (1981)	62 / 75	85.3%	83.2%
Arkansas (1991)	73 / 75	88.8%	90.6%
California (1987)	58 / 58	96.0%	95.1%
Colorado (2008)	55 / 64	87.9%	86.0%
Connecticut (2002)	8 / 8	71.6%	95.5%
Illinois (2006)[a]	1 / 102[a]	53.7%	22.2%
Indiana (1989)	37 / 92	51.5%	42.4%
Indiana (1992)	38 / 92	52.8%	43.6%
Iowa (1992)	89 / 99	86.1%	80.9%
Kentucky (1984)	94 / 120	74.7%	67.1%
Louisiana (1988)	36 / 64	72.7%	55.2%
Louisiana (2002)	42 / 64	70.4%	61.4%
Maine (1981)	16 / 16	94.3%	94.4%
Michigan (1997)	77 / 83	84.8%	85.0%
Michigan (2010)	78 / 83	91.9%	93.7%
Mississippi (1984)	24 / 82	40.3%	23.6%
Missouri (2009)	108 / 114	82.2%	90.4%
Nebraska (2005)	56 / 93	86.5%	84.2%
Nevada (2008)	15 / 17	95.8%	97.3%
New Hampshire (2010)	10 / 10	56.6%	55.6%
New Mexico (1981)	19 / 34	58.0%	50.8%
Rhode Island (2003)	5 / 5	98.6%	98.8%
South Dakota (2010)	29 / 66	84.3%	66.1%
Texas (1985)	223 / 254	89.1%	92.2%
Texas (1990)	235 / 254	94.9%	95.9%
Texas (1998)	230 / 254	95.1%	96.8%
Virginia (1991)	124 / 142	98.7%	93.4%
Washington (1996)	36 / 39	79.6%	51.9%
Wyoming (1999)	22/23	92.0%	94.1%

Notes: Coverage ratios are computed by dividing the population covered in the arrest data by the population estimated from the SEER*Stats for the respective geographies: county and state.

Table A2: Balancing Tests

Balancing Covariates			
	-5 years	+5 years	Difference (Standard Error)
Share of Black	0.136	0.136	-0.001
	(0.006)	(0.006)	(0.009)
Share of Others	0.049	0.059	0.009
	(0.005)	(0.007)	(0.009)
Share of Female	0.484	0.483	-0.001
	(0.001)	(0.001)	(0.002)
Log Police	6.872	7.002	0.130
	(0.140)	(0.133)	(0.193)
Log Population	8.028	8.041	0.013
	(0.136)	(0.126)	(0.185)
Teacher-Pupil Ratio	18.32	17.73	-0.581
	(0.537)	(0.573)	(0.776)

Notes: Sample includes cohorts of males aged 15-24 for US counties over time. Means across all counties in the balanced sample for each of the 30 reforms (as in Table 1), on each side of the +/- 5 bandwidth. Estimates are weighted by population size and standard errors are clustered at reform-cohort level.

Table A3: Sample Composition, Representative Analysis, 1974-2015

	Non-Reform States	Reform States
Share of Black	0.153	0.127
	(0.003)	(0.003)
Share of Others	0.032	0.059
	(0.001)	(0.003)
Share of Female	0.492	0.488
	(0.001)	(0.001)
Log Police	6.151	6.839
	(0.026)	(0.067)
Arrest Rate	0.075	0.081
	(0.001)	(0.001)

Notes: Sample includes cohorts of males aged 15-24 for US counties reporting over 1974-2015. Means across counties observed in the data for all states sample and the 24 states ever affected by a law change (as in Table 1) sample. Estimates are weighted by population size.

Table A4: Discontinuity Estimates by Individual Reform

State	Effective School Year	All Ages
Arizona	1985 and 1986	-0.036 (0.021)
Arkansas	1981	-0.059 (0.038)
Arkansas	1991	-0.068 (0.043)
California	1987	-0.046 (0.008)
Colorado	2008	-0.102 (0.033)
Connecticut	2002	-0.008 (0.023)
Illinois	2006	-0.012 (0.023)
Indiana	1989	-0.076 (0.039)
Indiana	1992	0.008 (0.038)
Iowa	1992	0.017 (0.048)
Kentucky	1984	-0.099 (0.041)
Louisiana	1988	-0.083 (0.044)
Louisiana	2002	-0.081 (0.043)
Maine	1981	-0.019 (0.031)
Michigan	1997	-0.066 (0.020)
Michigan	2010	-0.096 (0.021)
Mississippi	1984	0.032 (0.062)
Missouri	2009	-0.055 (0.033)
Nebraska	2005	0.019 (0.041)
Nevada	2008	0.027 (0.032)
New Hampshire	2010	-0.009 (0.041)
New Mexico	1981	-0.077 (0.065)
Rhode Island	2003	0.036 (0.028)
South Dakota	2010	-0.002 (0.077)
Texas	1985	0.090 (0.015)
Texas	1990	-0.071 (0.014)
Texas	1998	-0.080 (0.012)
Virginia	1991	-0.138 (0.029)
Washington	1996	-0.063 (0.019)
Wyoming	1999	-0.112 (0.066)

Notes: Same specification as column (6) of Table 2. Each row is estimated as separate regression for each reform with a 5-year window.

Appendix B

Theory - Derivations and Proof

According to the model presented in Section 2, one can derive the following expressions:

$$\frac{\partial F}{\partial c} = \left(1 - p(c)\right)[U''(t(a)w(e) + n(e)c)n(e)^2] - 2p'(c)[U'(t(a)w(e) + n(e)c)n(e)] + p(c)\left[U''\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right)\left(n(e) - s(a)\right)^2\right] + 2p'(c)\left[U'\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right)(n(e) - s(a))\right] + p''(c)\left[U\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right) - U(t(a)w(e) + n(e)c)\right]$$

$$\frac{\partial F}{\partial a} = \left(1 - p(c)\right)\left[U''(\,t(a)w(e) + n(e)c)(t'(a)w(e) + t(a)w'(e) + n'(e)c)n(e) + U'\left(t(a)w(e) + \left(n(e)\right)c\right)n'(e)\right] + p(c)\left[U''\left(\,t(a)w(e) + \left(n(e) - s(a)\right)c\right)\left(\,t'(a)w(e) + t(a)w'(e) + \left(n'(e) - s'(a)\right)c\right)\left(n(e) - s(a)\right) + U'\left(\,t(a)w(e) + \left(n(e) - s(a)\right)c\right)\left(n'(e) - s'(a)\right)\right] + p'(c)\left[U'\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right)\left(\,t'(a)w(e) + t(a)w'(e) + \left(n'(e) - s'(a)\right)c\right) - U'(\,t(a)w(e) + n(e)c)(t'(a)w(e) + t(a)w'(e) + n'(e)c)\right]$$

Let $k = -\frac{U''}{U'}$ be the coefficient of absolute risk aversion, $\frac{\partial F}{\partial c}$ and $\frac{\partial F}{\partial a}$ can be rewritten as:

$$\frac{\partial F}{\partial c} = U'(t(a)w(e) + n(e)c)\left[-k\left(1 - p(c)\right)n(e)^2 - 2p'(c)n(e)\right] + U'\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right)\left[-kp(c)\left(n(e) - s(a)\right)^2 + 2p'(c)(n(e) - s(a))\right] + p''(c)\left[U\left(t(a)w(e) + \left(n(e) - s(a)\right)c\right) - U(t(a)w(e) + n(e)c)\right]$$

$$\frac{\partial F}{\partial a} = U'(t(a)w(e) + n(e)c)\left[(t'(a)w(e) + t(a)w'(e) + n'(e)c)\left(-kn(e)\left(1 - p(c)\right) - p'(c)\right)\right] + U'(t(a)w(e) + n(e)c)\left[n'(e)\left(1 - p(c)\right)\right] + U'(t(a)w(e) + (n(e) - s(a))c)\left[\left(t'(a)w(e) + t(a)w'(e) + \left(n'(e) - s'(a)\right)c\right)\left(-k\left(n(e) - s(a)\right)p(c) + p'(c)\right)\right] + U'(t(a)w(e) + \left(n(e) - s(a)\right)c)\left[\left(n'(e) - s'(a)\right)p(c)\right]$$

$\frac{\partial F}{\partial c}$ equals the second derivative of the objective function, assuming we have an interior solution, $\frac{\partial F}{\partial c} \leq 0$ to ensure the concavity of the objective function. The sign of $\frac{dc^*}{da}$ depends then on the sign of $\frac{\partial F}{\partial a}$.

<u>Proposition</u>

If individuals are risk averse $k \geq 0$, wealth is non-decreasing in age $t'(a)w(e) + t(a)w'(e) + \left(n'(e) - s'(a)\right)c \geq 0$, the net rate of return to crime is non-negative and decreasing in age $n(e) - s(a) \geq 0, n'(e) - s'(a) \leq 0$, and $k\left(n(e) - s(a)\right)p(c) \geq p'(c)$[61], then the crime age-profile will be decreasing in age $\frac{dc^*}{da} \leq 0$

Proof:

[61] This condition ensures as well concavity of the objective function and existence of global maximum if $p(c)$ is convex, $p''(c) \geq 0$.

If

$$r(e) - w(e) - s(a) \geq 0 \;\Rightarrow\; r(e) - w(e) \;\geq 0$$
$$as \qquad s(a) \geq 0$$

$$t'(a)w(e) + t(a)w'(e) + \big(n'(e) - s'(a)\big)c \geq 0 \Rightarrow\; t'(a)w(e) + t(a)w'(e) + n'(e)c \geq 0$$
$$as \qquad s'(a) \geq 0 \quad and \quad c \geq 0$$

then it follows that

$$(t'(a)w(e) + (t(a) - c)w'(e) + r'(e)c)\big(-kn(e)(1 - p(c)) - p'(c)\big) \leq 0$$

$$\big(t'(a)w(e) + (t(a) - c)w'(e) + \big(r'(e) - s'(a)\big)c\big)\Big(-k\big(n(e) - s(a)\big)p(c) + p'(c)\Big) \leq 0$$

Hence,

$$\frac{\partial F}{\partial a} \leq 0 \Rightarrow \frac{dc^*}{da} \leq 0$$

Model Calibration

For the model simulation presented in Figure 2 the following functional forms were used:

$U(c) = \dfrac{(1 + (t(a) - c)w(e) + (r(e) - s(a))c)^{1-\sigma} - 1}{1 - \sigma}, where\, \sigma = 2$

$w(e) = log(1 + e)$

$r(e) = r + (1 + r) * log(1 + e), where\, r = 0.55$

$s(a) = \mathrm{s} + e^{(sa)} - 1, where\, s = 0.3$

$t(a) = \begin{cases} 0.5\, for\ a < a^d \\ \;1\;\; for\ a \geq a^d \end{cases}$

$a^d = 0.3, \qquad a'^d = 0.4$

$p(c) = c$

$0 \leq c \leq 1$

$0 \leq a \leq 1$

Chapter 4

A Step Closer to “Pomp and Circumstance”*: Teenage Crime, Pregnancy and School Dropout

Abstract

Quantitative study of female crime and its determinants has been relatively rare in social science and almost non-existent in the economics of crime research domain. This paper investigates the effects of compulsory school reforms on female crime and teenage pregnancy in the US. Using the Uniform Crime Report’s arrest data and the National Vital Statistics System’s birth data from 1974-2015, we apply a multiple regression discontinuity design to estimate that there is a sizeable impact of changes in compulsory schooling laws on juvenile female crime and teenage pregnancy. This empirical design allows us to address some limitations within the literature so far and produces novel insights on the contemporaneous and dynamic effects of such reforms on the age structure. Finally, we estimate and explore the within and between state heterogeneity and link it with school quality and socio-demographic measurements.

* March No.1 of *Pomp and Circumstance Marches* by Edward Elgar, came to be known as “Graduation March” and it is played in virtually all high school graduation ceremonies.

1. Introduction

The study of male crime behaviour has dominated much of the research agenda given its indisputable weight in the overall crime picture. Indeed, empirical study of female crime and its determinants remains rare in social science and is almost non-existent in the economics of crime research domain. Although the dominance of focus on male crime might have been justified in early studies, the trend since the mid-70s has highlighted an increase in female participation in criminal activities (Figure 1). With a 61.8% increase since the beginning of the period and an 11.2 percentage point decrease in the crime gender gap over the past three decades, the study of determinants, trends and structure of female crime have become topics of relevance for both research and policy.

Poor economic incentives are a key predictor of an individual's decision to commit crime according to the seminal work by Becker (1968) and following empirical work on labour markets. Since gender wage gaps have narrowed (Altonji and Blank, 1999) and female participation rates in employment increased (Blau and Kahn, 2000) one might begin to wonder why female crime has risen. An extensive amount of research has been focused on establishing the causal positive effects of an increase in education on lowering male crime (Freeman, 1996; Lochner, 2004; Machin et al., 2011; and Hjalmarsson et al., 2015). Yet, only recently, Cano-Urbina and Lochner (2018) [62] successfully produced causal estimates between education levels and female crime using compulsory school laws and individual census data between 1960 and 1980 for the US. In their work, Cano-Urbina and Lochner (2018) focus mainly on the effect of the reforms through the educational attainment offering a model for marriage decision as a form of endogenous selection for females.

[62] Both Hjalmarsson et al. 2015 and Machin et al. 2011 attempt to estimate the causal effect of education on crime for women, however their estimates are often imprecise.

Bell, Costa and Machin (2016) offer a study of the relations between compulsory schooling laws, education and crime for decades 1980-2010 in the US for males. They conclude that the link between compulsory school reforms and education has weakened with time, making the estimation of causal effects of education using variation of such education reforms imprecise and weakly identified. Nonetheless, like Jacob and Lefgren (2003), Luallen (2006) and Anderson (2014), they find that increases in compulsory schooling time and leaving age produce sizeable incapacitation effects (individuals in school hence not on the streets). More recently, Bell, Costa and Machin (2017) find the effects of such policies for the male population to be sizeable both contemporaneously (incapacitation) and in medium/long-term (dynamic incapacitation and productive educational effects).

Among the other outcomes of risky behaviour undertaken by teenagers and young adults, teenage pregnancy has been a topic receiving more attention in the literature. Starting with the seminal work by Becker (1960), optimal fertility choice has been part of both theoretical and empirical research agenda. According to Mincer (1963) and Becker and Lewis (1973) modelling, optimal fertility choices respond to permanent income shocks such as the ones associated with educational attainment by postponing births and reducing offspring size. In Kearney and Levine (2012), the teenage pregnancy rate in the US is shown to be the highest among the most developed economies, rendering it important for policy and research agenda. In their further work, Kearney and Levine (2014, 2015a) document a striking reduction in teenage pregnancy rates in the US between 1991 and 2010. Despite some reduction effects attributed to state welfare benefits and family planning reforms, they conclude that most of the downward trend remains unexplained even after accounting for racial and ethnic compositional changes in the population.

Notable work studying the effects of compulsory schooling laws as quasi-experimental set-ups to quantify effects on teenage pregnancy include Black et al (2008), and McCrary and

Royer (2011)[63]. Other quasi-experimental studies looking at fertility and infant birth health include: the impact of TV shows (Kearney and Levine 2015b) and college openings (Currie and Moretti 2003).

Making use of recent compulsory schooling law changes, we analyse the impact of these reforms on female arrest rates and pregnancy rates with data from 1974 to 2015. We explore the age heterogeneity of the effect by decomposing it across the age profile, building upon Bell, Costa and Machin (2017)[64].

Based on Uniform Crime Report and National Vital Statistics System data on arrests and births, we estimate an average 7% reduction on young female arrest rates and a 3.4% average decrease in birth rates among young women as a result of recent dropout age law reforms. For both outcomes, we find significant and large short-term (incapacitation) negative effects and additional reductions in the medium-term for older ages (early twenties in the context of the study). The combination of these effects results in the permanent reshape of the age profiles for both arrest rates and birth rates, with particularly clear results for the crime-age profile of females. The analysis shows that these effects is heterogeneous across and within states (at the county level), and often correlates with pre-reform school quality measures and socio-demographic local characteristics.

In Section 2, we discuss historical and recent trends in juvenile female crime and teenage childbearing; debate the gender differences and possible underlying determinants and mechanisms; and offer a description of the data. Section 3 presents the estimates of crime and teenage pregnancy reductions due to law changes and explores the age dynamics of effects,

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[63] Black et al (2008) use the minimum school leaving age reforms in the US to show a reduction effect on teenage pregnancy with increased schooling attainment among females. McCrary and Royer (2011) explore school starting age reforms in both California and Texas and find that, despite robust effects on educational outcomes, effects on teenage pregnancy are generally small and potential heterogeneous.

[64] Bell, Costa and Machin (2017) focus their analysis on male crime and its age structure. Landers et al (2016) provide a similar analysis for school starting age reform in Denmark.

underlying mechanisms and implications. The study of heterogeneity of the effects, between and within state, will be presented and discussed in Section 4. Finally, Section 5 will contain some concluding thoughts.

2. Background: Trends, Determinants and Data

Female Crime: Trends and Composition

There are several reasons to believe that the response to crime incentives and general criminal behaviour differ between women and men. Indeed, looking at the level of crime committed separately by gender, the difference is striking as can be shown in Figure 1. Females are responsible for only 22.7% of total arrest numbers and show a different age pattern, peaking at age 16, earlier than for their male counterparts which peak at 18 (Figure 2).

A further interesting fact in Figure 1 is the consistent upward trend for juvenile female arrest rates, only slightly hampered down in the mid-90s. Whereas male arrests have reached levels below those of 1974 by 2015 after almost two decades of a sharp decrease since the mid-90s, arrests among young women were 0.3 percentage points higher by 2015 relative to the beginning of the period (this translates to an increase of approximately 3 juveniles arrested per 1000 women). The previous narrowing of the gender gap in criminal engagement is a common feature across different countries as reported in Estrada et al (2016) for Sweden and Beatton et al (2017) for Australia.

The previously documented upward trend in overall arrests among young women was driven by an increasing female participation in violent and drug related crimes as shown in Figure 3. These types of crimes have been traditionally associated with males, although in recent years, there has been an apparent shift in the areas of criminal behaviour that women are engaging in. Property crime remains, however, the most significant crime type in terms of magnitude. Once more, Estrada et al (2016) and Beatton et al (2017) find a similar sorting pattern in Sweden and Australia, respectively.

Female Crime: Theory and Determinants

Most criminology theory has been male-specific (Gottfredson and Hirschi, 1990; Sampson and Laub, 1993 and 2005), a direct attempt to use such frameworks to explain female crime behaviour might be biased and/or insufficient to capture the singularities of female criminal engagement. Indeed, women traditionally played a role of secondary earners in families, and therefore being less affected by labour market factors if married and, additionally, as they are often the primary child care providers, they are less likely to engage in more risky activities[65].

Despite the obvious gender differences, Steffensmeier and Streifel (1992) and Schwartz and Steffensmeier (2007, 2015) argue that both males and females can be analysed under a common framework focused on five main life aspects that shape their probability of offending: gender norms, moral development and relational concerns, social control, physical strength and aggression, and sexuality. According to Schwartz and Steffensmeier (2007, 2015), the nature of these areas of life "(…) not only inhibit female crime and encourage male crime, but also shape the patterns of female offending that do occur". For example, females are documented to be less prompt to commit property crime that includes possible direct confrontation such as burglary, nevertheless the introduction of drug substance abuse, often through male partners, considerably increases their risk of committing larceny offenses (shoplifting, employee theft

[65] The difference between the labour market opportunities across gender makes the theoretical model proposed by Bell, Costa and Machin (2017) less adequate to explore the dynamics of female crime age profiles.

…) to finance their addiction[66]. However, as the paradigm of social norms and female emancipation shifts so do the gender differences in the underlying factors of criminal behaviour, making females more prone to engage in conventionally male-dominated criminal activities.

When considering the relationship between crime and age, the crime-age profile, Gottfredson and Hirschi (1990) argue that this relationship is time and place invariant and independent of other factors that correlate with crime. Sampson and Laub (1993, 2005) acknowledge and integrate the possibility of life-course changing factors, which make the link between age and crime propensity not unconditional while retaining the idea of a strong dependence between crime and age. According to both previous studies, crime-age profiles can be decomposed in three main stages: on-set age, peak/specialization age and desistance age. These stages relate closely to the empirically established inverse u-shape of crime profiles.

The possible explanations for the left shifted crime-age profile in women vary in their nature (Schwartz and Steffensmeier, 2007 and 2015) but no particular factor has proven to be the main determinant of this feature. This paper does not address this research question, instead it documents and analyses the impact of a type of education policy on the age structure of female crime without aiming to explain the underlying gender differences.

Teenage Pregnancy: Trends and Composition

As of 1973, 96 out of 1000 female teenagers in the US reported the start of a pregnancy. This number has seen a significant reduction starting from the early 90s through to the most recent available data which estimated a 4.3% pregnancy rate among teenagers in 2015. When analysing this phenomenon is important to distinguish between pregnancies resulting in births

[66] See Gavrilova and Campaniello (2015) for a discussion on gender differences in crime engagement and sorting across crime types.

and terminated pregnancies (intentionally – abortions, unintentionally – miscarriages and fetal deaths[67]). In Figure 1, we can see that until the 90's intentional abortion rates rose however, the downward trend is common across the different outcomes for the past three decades.

In this study, the analysis focuses on pregnancies resulting in births, as unfortunately data on abortions in the US is often scarce and is restricted to use by US-based researchers. To the best of our knowledge, no evidence has been found in favour of compulsory school laws having a causal effect in increasing the number or rate of abortions among young females in the US context. Taking into account the high correlation between pregnancies and births present in the aggregate data, it is hard to believe that even if pregnancies and abortions increased as a result of the policy they would reach a sufficient counterbalancing magnitude to invalidate the results of the analysis. Indeed, one would need assume a discontinuous change in the percentage of females deciding for abortion as result of the policy to undermine the validity of the results on the birth rate outcome.

Additionally, considering the disadvantage nature and age of sub-population likely to be affected by the policy, it is not clear in the current status of the policy debate that, conditional on pregnancy, abortion is an inferior outcome to birth from a welfare point of view. Despite our acknowledgement of abortions as important potential outcome in teenage pregnancies, we believe that our results will be underestimating the potential reducing effects of compulsory schooling laws on overall pregnancies by ignoring abortions as an outcome.

Teenage Pregnancy: Theory and Relevance

Teenage childbearing contradicts the main predictions of fertility models in developed economies under standard assumptions (Becker and Lewis, 1973). In these models, concerns

[67] Given the low proportion of unintentional abortions (miscarriages and foetal deaths) as an outcome of teenage pregnancies, the study does not include this in the analysis.

over the quality of childcare provision and education encourage females to give birth at older ages as their current and future income streams are more certain after human capital investments are realized. Nonetheless, if one models an individual female as impatient (high discount factor) and with significantly low prospects on economic opportunities, the short-term utility gain of being a mother can surpass the potential gains of delaying a pregnancy[68]. On the margin, it can lead to a state of "ambivalence" or indifference with respect to the timing of the childbearing and hence encourage unprotected behaviours towards sexual activity.

An extensive review of empirical work offered by Kearney and Levine (2012) points towards often insignificant and small negative effects of teenage childbearing on children's outcomes once selection is taken into account through different quasi-experimental settings. The previous conclusion, however, should not be interpreted as deeming any intervention that can directly or indirectly reduce teenage births as inconsequential or not beneficial to the individuals involved. This is particularly true for interventions that, beyond simply lowering rates of teen motherhood, potentiate future mothers to acquire more education and skills relevant to improve the prospects for themselves and their children. Compulsory schooling reforms may potentially result in such an effect. Therefore, despite acknowledging that teenage pregnancy is more often a consequence of social and economic disadvantage than its cause[69], one should consider investigating the potential impacts of compulsory school reforms on the age profile of fertility as relevant for policy and research alike.

Data Description

Arrest Data – Uniform Crime Report

[68] See Kearney and Levine (2012) for a detailed discussion.
[69] See Kearney and Levine (2012) for a detailed discussion.

The crime data used in the analysis is provided by the FBI Uniform Crime Report (UCR), which compiles yearly arrest data by age and sex at local police enforcement agency level. Acknowledging that most crime considered in the study is committed at juvenile ages and that compulsory school laws are, in theory, more likely to directly affect these ages we chose to produce our analysis on females aged 15 to 24 years old[70]. For the purposes of the analysis, county level was selected as the geographical level of aggregation, as in Anderson (2014). Bell, Costa and Machin (2016) produce their estimates at commuting zone level to better approximate the notion of local labour markets, however given the fact that a few commuting zones cross state boundaries and that we are interested in investigating the heterogeneity within each state, an aggregation at county level ensures a better design that addresses both limitations.

Detailed county-level population numbers by sex, age and race are matched to arrest data and adjusted to the covering standards so as to produce age arrest rates and demographic composition controls. Unfortunately, the UCR data does not include a racial breakdown of arrests making it impossible to evaluate the effect of the policies in a racial dimension.

Birth Data – US National Vital Statistics System

The study uses birth data provided by the National Center for Health Statistics, which compiles the birth certificates of all the different states in the US. Federal law mandates the collection of this data hence making its coverage close to universal for the US context. The data is made available to non-US based researchers at geographical aggregates consistent with at least 100,000 inhabitants being covered. The former causes limitations in the analysis when compared to the arrest data, as one is not able to identify every individual county in a given

[70] Ages below 15 are not identified separately by single age hence not making it suitable for the analysis requiring single cohort identification.

state and thus uses less rich local variation for more precise estimation[71]. Taking into account the previous restrictions, a panel of births is constructed for the years 1976 to 2015, including mothers between the ages of 16 and 24[72].

Compulsory Schooling Laws

We have updated and further corrected the compulsory schooling laws used in Bell, Costa and Machin (2016). The choice of how to measure the binding compulsory school age has been open to scrutiny: Stephens and Yang (2014) propose a refined version of Katz and Goldin (2008) measurement, combining start age, dropout age, grade requirement and child labour laws, whereas Oreopoulos (2009) and Anderson (2014) focus only on the dropout age enacted by law. Taking into account recent analysis by McAdams (2016) and Landers et al (2016) pointing to a negative causal relationship between starting age and crime (i.e. later starting ages associated with reductions in crime reductions in the propensity to commit crime) we decided not to include those as a measure of variation in the laws as, if measured like Stephen and Yang (2014), they could have offsetting effects. On the other hand, we think that the binding school age is better measured by taking into account the grade exemptions that often make part of recent laws. Our measure of binding school age is given by:

$$DA_{st} = min\{Dropout\ Age_{st}, Grade\ Required\ to\ Dropout_{st}\}$$

[71] It is the intention of the author to access and perform the analysis with fully geographically-identified data prior to publication of this study.
[72] Non-US based researchers are subject to data restrictions for births below the age of 16 for privacy protection reasons.

Figure 4 maps how changes in the *DA* enacted between 1980 and 2010 span through different states in the US. The map makes clear that areas like the West South Central (Arkansas, Texas and Louisiana) and West Pacific (California and Washington) have been the most active in making progress towards enacting their education legislation of compulsory school age.

Defining the exact initial cohort affected by the change in compulsory schooling laws is frequently not as straightforward as subtracting the new dropout age to the year the law has been enacted. Recent laws contain a degree of complexity significantly higher than their previous versions enacted in the first three quarters of the past century: employment exemptions, parental consents, mitigating circumstances and effective dates have all added potential sources of measurement error to any attempt to code the laws. To reduce the noise around the cohort appointments all the changes have been cross-validated empirically by analysing the data around the potential discontinuity and adjusted when needed. Table 1 summarises the 30 changes explored in this paper. Although there have been a number of changes enacted from 2010 onwards in several states, the unbalanced nature of the panel with respect to ages covered for recent reforms makes the identification of the effects both harder and imprecise and therefore we chose to exclude them from the analysis[73].

Police, School Quality and Socio-Demographic Indicators

To enrich the analysis of spatial heterogeneity within states, we collected data on some key elements that, according to existing evidence (Levitt (1997) and Card and Krueger (1992)), can relate to both arrests and educational attainment and progress. An update of some of the Card and Krueger (1992) school quality measures (pupil ratios, average teacher salary, number

[73] Once most recent data is released the analysis of such reforms will be possible.

of schools) is made at county-level using Common Core Date (CCD) data[74]. Police numbers are recovered from the FBI LEOKA database and socio-demographic indicators are collected from the Local Area Personal Income (LAPI) from Bureau of Economic Analysis. This data has the advantage of being collected at a level of disaggregation lower or equal to county level, hence making it possible to produce an analysis of spatial patterns at a detailed layer within each state.

3. <u>Empirical Framework: School Dropout and Age Profiles</u>

Statistical Modelling and Baseline Estimates

We begin our analysis by estimating the impact of compulsory school laws on arrests and teenage births by using the existing identification models in the literature. Similarly to Anderson (2014) and Bell, Costa and Machin (2016) we start by proposing a pooled differences-in-differences model:

$$Y_{acst} = \beta Reform_{s(t-a)} + \gamma X_{acst} + u_{acst} \tag{1}$$

where Y_{acst} denotes the log of arrest rate/log of birth rate for age *a*, in state *s*, county *c*, at time *t*. $Reform_{s(t-a)}$ denotes an indicator for the cohorts affected by the law change, $Reform_{s(t-a)} = 1\left[cohort_{st} \geq Reform_{s(t-a)}\right]$, whilst X_{acst} includes controls for relevant observables: police numbers, racial composition and population size. Finally, the error term is

[74] Further details can be found in the Data Appendix.

further decomposed in $u_{acst} = \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst}$ so as to control for age, time, county[75] fixed effects. The first two columns of panels A1 and B1 in Table 2 present estimates of the model presented in equation (1) for the sample including all states. The column (1) presents a commonly used specification in the literature[76], where column (2) adds region-cohort specific trends as a first approach to address the concerns of Stephens and Yang (2014) regarding omitted variable bias resulting from distinct regional trends in school quality and socio-economic conditions. Effects on arrest rates are estimated to be 13% and 9.2% (Panel A1) across the two specifications, consistent with a downward bias caused by differential cohort regional trends affecting control and treated states in this setting. More puzzling is the positive effect estimated for birth rates in column (1) of panel B1, though it is not robust to the inclusion of region-cohort trends, column (2). We believe that the former estimates point in favour of the existence of non-parallel trends in the potential control and treatment states, which are assumed away in the identification.

In order to address the potential threat to the model identification discussed previously, we decide to use the within state variation as plausibly a more accurate way to identify the effect of such policies. We suggest the use of a multiple regression discontinuity design as formulated in a model where all reforms are separately identified by the discontinuity design and pooled under the following empirical specification:

$$Y_{acst} = \beta Reform_{s(t-a)} + f_s(t-a) + \gamma_s X_{acst} + \alpha_{as} + \alpha_{cs} + \alpha_{ts} + \varepsilon_{acst} \qquad (2)$$

$$for \quad (t-a) - w \leq t - a \leq (t-a) + w, \qquad w = \{5, 7, 10\}$$

where $f_s(t-a)$ is a reform-specific function of the running variable, cohort $(t-a)$, and w defines the window length centered around the reformed cohort for a given state-

[75] Throughout the rest of the study, we will use county defined as both actual county for the crime data and smallest available aggregation of counties for birth data

[76] See Oreopoulos (2009), Chan (2012), Stephens and Yang (2014) and Bell, Costa and Machin (2016)

discontinuity s. The model will identify the effects of the reforms unless other observable and unobservable factors show a discontinuous change close to or at the cohort threshold. Table 3 shows that as far as a set of relevant observable variables are considered, there is no evidence of significant changes occurring around the running variable threshold. Contemporaneous reforms affecting cohorts close to the threshold analysed could constitute a potential confounder. Part of such events are capture by a fully flexible year trends present in all specifications, as well as cohort polynomials allowed to differ on each side of the discontinuity.

Columns (3)-(5) of panels A1 and B1 in Table 2 show the estimates for β, which identifies the average discontinuity estimate across reforms, over different choices of window for the discontinuity design. Most estimates are negative and significant across the window of choice, lending a level of robustness to the model estimation. Under the preferred specification of a 5-year bandwidth, the average effect of the reforms is a 7% reduction in arrest rates and a 3.4% decline in birth rates among young women. As expected, these figures are significantly lower than the differences-in-differences estimates but are still sizeable and statistically significant. Given the fact that the identifying variation is restricted to within-state level and that policies on dropout age do not aim directly to reduce crime or pregnancy rates, we can conclude the estimated effects should be interpreted as an unintended consequence of changes to compulsory schooling laws.

Unlike Cano-Urbina and Lochner (2018) and Stephens Yang (2014) which make use of "Class I" models[77], the model specification used does not include school quality measures directly in the reduced form equation. Our approach falls closer to "Class III" models by studying the effect of school quality measures on the estimated effect of the policy as presented

[77] For a detailed discussion of empirical school quality class of models we refer to Card and Krueger (1996)

in Section 4 of this paper[78]. Furthermore, the proposed identification of the model by discontinuity design already implies a restriction of the variation used by the inclusion of state-year non-parametric trends, significantly absorbing the underlying school quality and education trends.

Unlike Cano-Urbina and Lochner (2018) we are not solely focused on the crime reducing effects of education gains induced by these policy reforms. In this paper we try to explore how education policy reforms can have dynamic effects through the crime-age profile and hence help understanding the determinants and mechanics of criminal career choices. As first approach to disentangle the age patterns, Panels A2 and B2 of Table 2 mimic the previous estimation analysis, restricting the sample to teenage ages. These are plausible "incapacitation" ages and therefore concern individuals most directly affected by the contemporaneous mechanism of attending school. The results for these ages are, as expected, larger in magnitude, significant and robust across the discontinuity specifications (3)-(5). These estimates highlight the relevance of studying, in greater detail, the effects of these reforms across the age profile of young females.

Age Profile Effects

As shown previously, the effects of increasing the high school compulsory schooling need not be necessarily homogeneous across ages. Considering ages covered by compulsory attendance leaving ages, a mechanical effect of maintaining individuals in a controlled environment (school, classroom) is likely to result in a reduction of crime through

[78] Contrary to Card & Krueger (1992) we are not estimating wage returns to schooling, however the structure of the model is identical.

incapacitation[79]. Medium- to long-term effects can be an outcome of these reforms through human capital gains or dynamic incapacitation effects. An increase of compulsory schooling can be associated with real increases in human capital due to schooling, thus producing a long-term effect over the individuals' lifetime in terms of their opportunities and engagement in crime (Lochner and Moretti, 2004; Caro-Urbina and Lochner, 2018).

Complementarily or alternatively, preventing individuals from engaging in crime at early ages (on-set ages[80]) through incapacitation can result in a permanent change to the individual's potential criminal career. By limiting an individual's contact with criminal activities at ages of high risk, the individual might desist to engage in such behaviours at a later age when he/she is no longer constrained by compulsory attendance (dynamic incapacitation). Indeed, Aizer and Doyle (2015) found that youth crime engagement that results in arrest and subsequent incarceration can itself have negative effects on schooling completion in the long-run and further increase the probability of young adult incarceration. This gives ground to the belief that dynamic incapacitation effects can be a reality if one is to avoid incarceration at juvenile ages. As in the context of Sampson and Laub (1993 and 2005), school age reforms can be seen as a "turning point" in an individual's potential criminal career and therefore permanently change the crime-age profile.

In the case of motherhood at young ages, similar arguments can be made in favour of short and medium-term effects. The concept of an incapacitation effect at ages below the binding dropout age can easily be extended to the context of teenage pregnancy, where a controlled environment (school) has the potential to decrease risky behaviours such as unprotected sex, as well as limiting the time available to engage in such[81]. Human capital gains

[79] See Bell, Costa and Machin (2017) for a theoretical framework.

[80] See Laub and Sampson (1993 and 2005)

[81] Conversely, keeping young females in school can represent an increased opportunity to foster relationships and consequently engage in sexual activities as well. Indeed, Anderson (2014) finds some evidence in favour of an

through educational attainment can deliver medium- and long-term effects for birth rates at later ages as well. Unlike in the case of crime, childbearing is not a negative outcome irrespective of age[82] and therefore the social optimal reducing effects should not be negative across all ages. Nonetheless, one can argue with some confidence that delaying motherhood in cases where counterfactual births would occur at teenage years and even early twenties represents an improvement in overall welfare for the risk groups most likely affected. Namely, US campaigns for teenage pregnancy prevention have further extended their spectrum to unplanned pregnancy particularly among young women who are low-educated and at risk of poverty[83].

In order to explore the age effect dynamics, we propose an extension to the discontinuity design model so as to capture the effects of the policy at different ages by making the reform effects differ by age:

$$Y_{acst} = \theta_a Reform_{s(t-a)} + f_s(t-a) + \gamma_s X_{acst} + \alpha_{as} + \alpha_{cs} + \alpha_{ts} + \varepsilon_{acst} \quad (3)$$

where θ_a captures the age-specific effects of the dropout age reforms. Figures 6A and 6B show the estimated effects for different ages for both outcomes. One of the most obvious features of these results is the significantly larger effects at incapacitation ages (15-18) followed by a lower and roughly constant effect of the reforms on ages following the end of compulsory schooling. The former supports the argument that incapacitation effects can be particularly strong at ages of high propensity to risky behaviours. The cumulative effects for

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increase in sexual intercourse as a result of a selected number of dropout age reforms. Our results do not support the idea that the potential increase in sexual intercourse has resulted in an increase in births by teenagers.

[82] Under standard overlapping generations frameworks the optimal fertility choice should be positive as to ensure intergenerational redistribution effects. See Samuelson (1975) and Pestieau and Ponthiere (2016).

[83] See The National Campaign To Prevent Teenage and Unplanned Pregnancy, https://thenationalcampaign.org/why-it-matters/unplanned-pregnancy

the ages below the age of 18 (19 for births[84]) represent 53% and 35% of the overall arrest and birth reductions respectively.

Unfortunately, the separate identification of long-term effects of dynamic incapacitation and direct human capital investment is a challenge that would require further assumptions to decouple. Taking into account the documented erosion in the link between dropout age reforms and education in recent decades (Bell, Costa and Machin, 2016) we believe that some of the effect beyond the incapacitation age is a dynamic consequence of incapacitating crime engagement at the on-set ages (dynamic incapacitation).

Table 4 presents the estimated age effects broken down by type of crime. Overall, female crime reductions seem to share the same pattern found in males by Bell, Costa and Machin (2017), with higher percentage reductions on violent and drug-related crime arrests and pronounced age effects at early ages of incapacitation. The magnitude of the estimated reduction on property crimes needs to be contextualized in relative terms. As this is the type of crime most frequently committed, the percentage reduction translates in an actual larger absolute drop in arrest rates compared to other crimes.

In order to visually describe the estimated effects on the shape of the age profiles, we calculated new arrest rates and birth rates by applying the previous estimates to the baseline arrest and birth levels prior to the reform. These are presented in Figures 7A and 7B. As expected, the incapacitation effect has reduced significantly the engagement in crime at ages now covered by the compulsory school age, shifting the distribution of arrests to older ages (Figure 7A). Looking at the new age profile, we can see that the peak age of arrests has shifted from 16 to 18 years old, consistent with an increase in dropout ages. As for the age profile of

[84] Given the most pregnancies take 9 months, most teenagers starting a pregnancy at 18 will deliver birth by the time they are 19.

births, we can see an overall reduction across ages but no visible impact on shape of the profile given that it preserves its monotonically increasing nature (Figure 7B).

Table 5 present the breakdown of the age profile effects by reform type, according to the initial leaving age and new leaving age. Consistent with the previous results, we find strong incapacitation effects followed by medium-term reductions in both arrest and birth rates at older ages. Unsurprisingly, one can observe the opposite effects for the case of the reform taking place in Texas in 1985, as this reform actually lowered the dropout age as a consequence of the re-writing of the previous compulsory school age law. Average overall effects differ slightly across reforms types for both arrest and birth rate, though not significantly in terms of magnitude. As expected reforms that increase the compulsory schooling age from 16 to 18 (i.e.: two years of extra school attendance) present the largest reduction in arrest rates. We believe that data restrictions in terms of disaggregation for birth rates, combined with the more recent dates of reforms that increased leaving ages from 16 to 18 years of age, might help explain why the previous pattern is not present in the birth rate effects.

Overall, these results are consistent with both short and medium-term effects of reforms in compulsory school leaving age capable of shifting the age profiles of crime and significantly reducing undesirable outcomes across different ages of young females.

4. <u>County and State Level Heterogeneity</u>

State Level

Geographical patterns are of interest in the discussion of the reductions in birth and arrest rates as states across the US exhibit remarkable differences in levels of both outcomes as described in Table 6 for 2015. Indeed, the state of Vermont presents the lowest arrest rate among young women in the sample (8.7 per 1000s), roughly a quarter of the figure of the state with the highest rate nationally, South Dakota (35.4 per 1000s). The same pattern emerges when comparing birth rates for teenagers and those in their early twenties, where the ratio between the top (Massachusetts, 27.4 per 1000s) and bottom ranked states (Arkansas, 84.8 per 1000s) is about a third[85]. As advocated in this study, arrests and precocious childbearing can be thought of as consequences of common high-risk attitudes and behaviours and thus are expected to correlate across states as shown in Figure 8.

One of the significant advantages of the proposed identification model is that it enables one to point estimate each discontinuity separately with reasonable precision without relying on cross-state variation. The previous enables us to explore the heterogeneity of the impact in the different states under different changes to compulsory school leaving age. Figures 9A and 9B plots the different estimates for all the discontinuities include in our sample with their respective confidence intervals. Firstly, one can observe that there is significant variability in both the magnitude and precision of the estimated effects with 24 negative and 6 positive coefficients for arrest rates and 27 negatives and 3 positives for birth rates. The positive and significant coefficients in both outcomes of the reform in Texas 1985 are actually intuitive as they follow from the only reduction in dropout age present in the sample. A considerable amount of the statistically insignificant estimates belong to lowly-populated states which therefore lack precision in their estimation. It is reassuring to note that the two most populated states within the sample (California and Texas) consistently present more precise and intuitive estimates. Figure 10 plots the estimated reform-specific effect pairs (arrests and births), showing a strong relationship between the magnitudes of the estimates on the two outcomes, with a statistically significant correlation coefficient of 0.6. Overall, the results are consistent

[85] Table A1 of the Appendix presents the numbers for teenage ages and the main pattern remains unchanged.

with the prior prediction of reduction in arrests and births among young females in response to the toughening of school leaving laws.

We do not exclude that some of the policies might not have delivered significant reductions in practice due to state-specific characteristics and different institutional settings: school quality, employment levels, quality and quantity of law enforcement. In order to address that dimension we perform an analysis at county level in the following subsection.

County Level – School Quality, Law Enforcement and Demographics

The level of disaggregation of the data for arrests makes it possible to produce discontinuity estimates for the counties available within each state[86]. We produce the analysis of determinants of the magnitude of crime reduction in a two-step estimation analysis in the spirit of "Class III" models used and described by Card and Krueger (1992, 1996). First, we estimate county level discontinuity effects by making β a county-specific coefficient β_c:

$$Y_{acst} = \beta_c Reform_{s(t-a)} + f(t-a) + \gamma X_{acst} + \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst} \tag{4}$$

After obtaining the estimates $\widehat{\beta_c}$ we match them to the county variables collected for the cohort at age 15[87] and estimate the following reduced form equation:

$$\widehat{\beta_c} = \theta Z_c + \pi_s + \mu_c \tag{5}$$

where Z_c includes share of the black population, the share of the employed population, police per capita and the pupil-teacher ratio, whilst π_s is a reform-specific fixed effect and μ_c an error term. The standard deviation across counties of $\widehat{\beta_c}$ is 0.35 for a weighted mean effect of -0.05. This clearly illustrates the potential spatial heterogeneity of the effects and reinforces the pertinence of the analysis. The magnitude of standard deviation also reflects a significant

[86] Unfortunately, without access to detailed geographic birth data the analogous analysis is not possible.
[87] We choose to do the match at age 15 as to insure that covariates are not affected by the education reforms, reducing the endogeneity of the analysis.

heterogeneity in the precision of the estimates for different counties. To account for this dimension, the subsequent analysis is weighted by the absolute value of the t-statistics calculated in the first step of the estimation[88].

Table 7 shows the empirical estimates of the model covering different specifications and samples according to data availability. From the results, one can see a consistent positive relationship between the share of the black population and the magnitude of the arrest reduction in a given county. The black population in the US has traditionally presented lower levels of education and a higher risk of criminal engagement; Bell, Costa and Machin (2016) show it to be the racial group of the complier population benefiting the most from compulsory school reforms in terms of educational attainment. Likewise, the employment share of population seems to benefit the efficiency of compulsory school laws in reducing crime. The availability of employment opportunities reduces the trade-off with respect to crime (Raphael and Winter-Ember, 2001; Bell et al, 2018; and Bindler, 2016) Furthermore, females can be indirectly affected by the levels of male employment given their traditional role as secondary earners in the household and vulnerability to a partner's deviating behaviour (Blundell et al, 2016 and Pettiway, 1987). The magnitude of the estimates suggest that county with 10 percentage points higher black share of population are likely to experience a reduction in relative crime more than double average reduction, 7.9% added to the average 5% reduction estimated. As an example, this would be close to the difference between Jefferson County and Dallas County in Texas with 34.4 and 23.1 share of black population respectively. As for regions with striving labour markets, a 1 percentage point higher employment rate reflects a 0.3% higher reduction in crime as result of a dropout age increase.

[88] Results are robust to the use of the inverse of the standard errors as weights. However, standard errors are not scale invariant with respect to the magnitude of $\widehat{\beta_c}$ hence the preference towards the use of t-statistics instead.

Police deployment and its relationship with crime has long been an area of research (Levitt, 1997; Draca et al, 2011; Chalfin and McCrary, 2013). Estimates indicate a possible complementarity between the police density and policy crime reduction effects, making the latter more efficient. Providing some context the average police officers per 1000s in the sample analysed is of 2.8, hence an increase of one unit in this statistic (36% increase relative to the mean) translates on an enhanced crime reduction effect of approximately 4% relative to the baseline.

Finally, school quality measures like the pupil-teacher ratio seem to relate, as expected, to the reduction coefficients. Pupil-teacher ratio has a hindering effect on the crime reduction consistent with lower quality teaching standards and potentially crowded classrooms. Considering the estimate of Column (5), a difference of 1 unit in the pupil-teacher ratio is associated to an attenuation of the reducing effect of the reform of 2%, which translates on a loss of a little more than third of the mean effect considering of 5%.

To conclude, the reduction of crime in a local area (county) appears to be dependent on the quality, employment dynamics and socio-demographic composition of the area.

5. Conclusions

In this paper, we have offered a novel analysis of the relationship between compulsory school laws and female crime and teenage pregnancy rates. The findings of this research show there is a systematic relationship between lower crime and childbearing and higher compulsory schooling ages for young females. Our results provide evidence of significant and sizeable reduced form impacts of increases in compulsory school age on arrest rates and birth rates among women at teenage and early adult ages.

We also conducted a detailed analysis of the impact of these reforms on the age distribution of both arrests and births, over and above the general reduction in overall crime rates. Consistent with the criminology theory, we find that strong incapacitation effects dominate the age profile impacts. Furthermore, the analysis finds evidence in favour of constant dynamic incapacitation effects at later ages past the mandatory school leaving age. The combination of these short-term and medium-term effects serve to reshape the crime-age profile, shifting it upwards in age. Analogous effects are found with birth rates among young women, where both incapacitation and dynamic incapacitation play a role.

Upon expanding the analysis to study the heterogeneity of the effects across and within states, we find evidence of a differentiated impact of the reforms in reducing arrest and birth rates among young women. The magnitude of state-reform impacts is found to be strongly correlated across both the studied outcomes, strengthening the premise that common underlying individual characteristics and behaviours are shared by the sub-populations most affected by this type of schooling reform. Additionally, when focusing the analysis within each state we find evidence that local levels of employment, racial composition of the population (the black share) and police density affect the reducing effects of compulsory school age positively, as well as better quality of education as measured by pupil-teacher ratio.

The focus on crime and early motherhood among female youth in recent decades, joint with the use of a multiple discontinuity design to identify the policy effects across specific ages, states and counties, constitute a novel contribution to the existing literature. A detailed analysis of the underlying factors on the basis of gender differences in crime-age profiles, and the decoupling between educational attainment and dynamic incapacitation spillover effects look to be relevant topics for future research.

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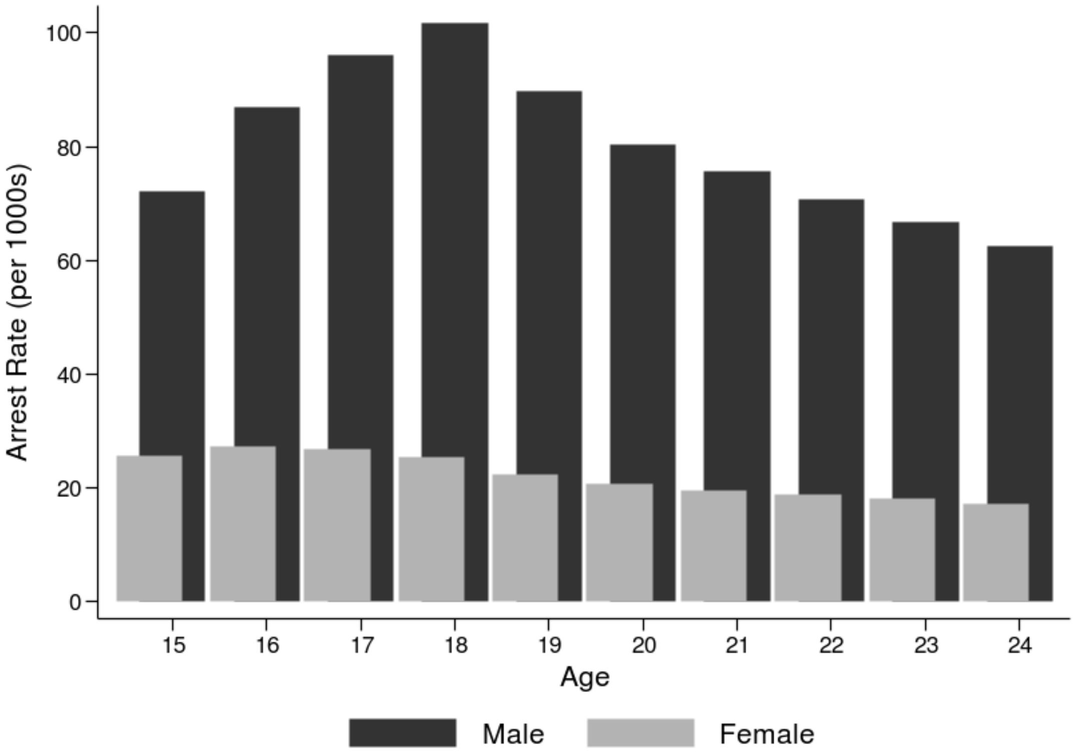
Figure 1: Trends in Arrest Rates by Sex, 1974-2015

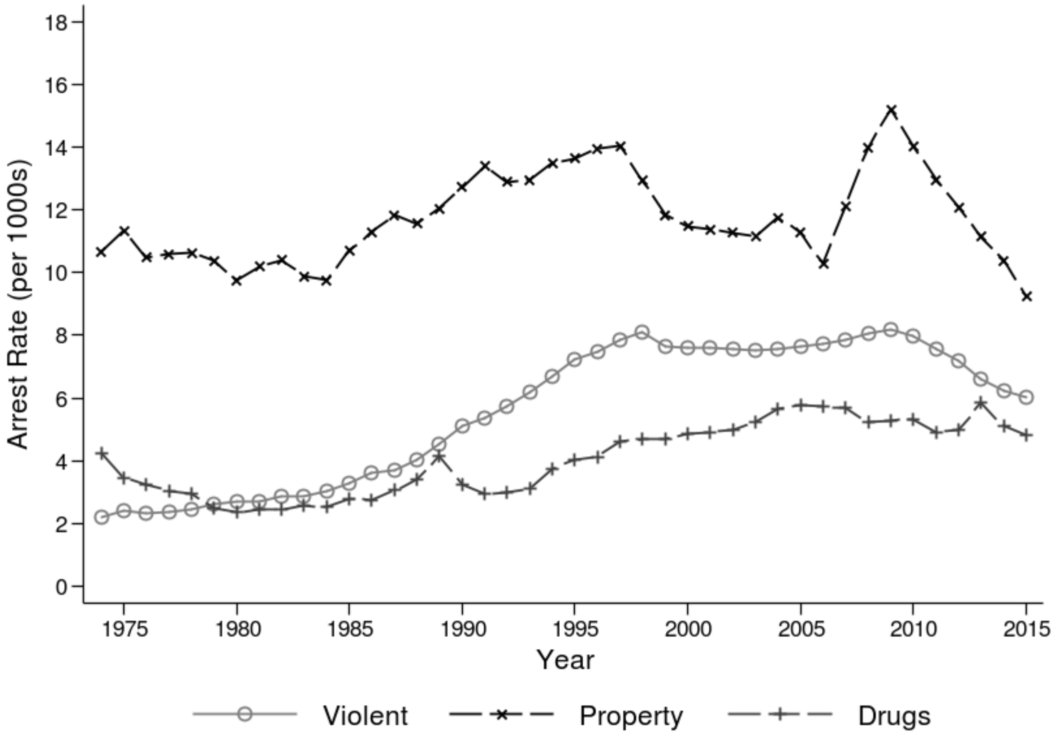
Notes: Total arrest rates are calculated from the UCR by summing up violent, property and drugs arrests. Reporting standards are that only agencies reporting all years are included.

Figure 2: Crime-Age Profiles by Sex, 1980-2010



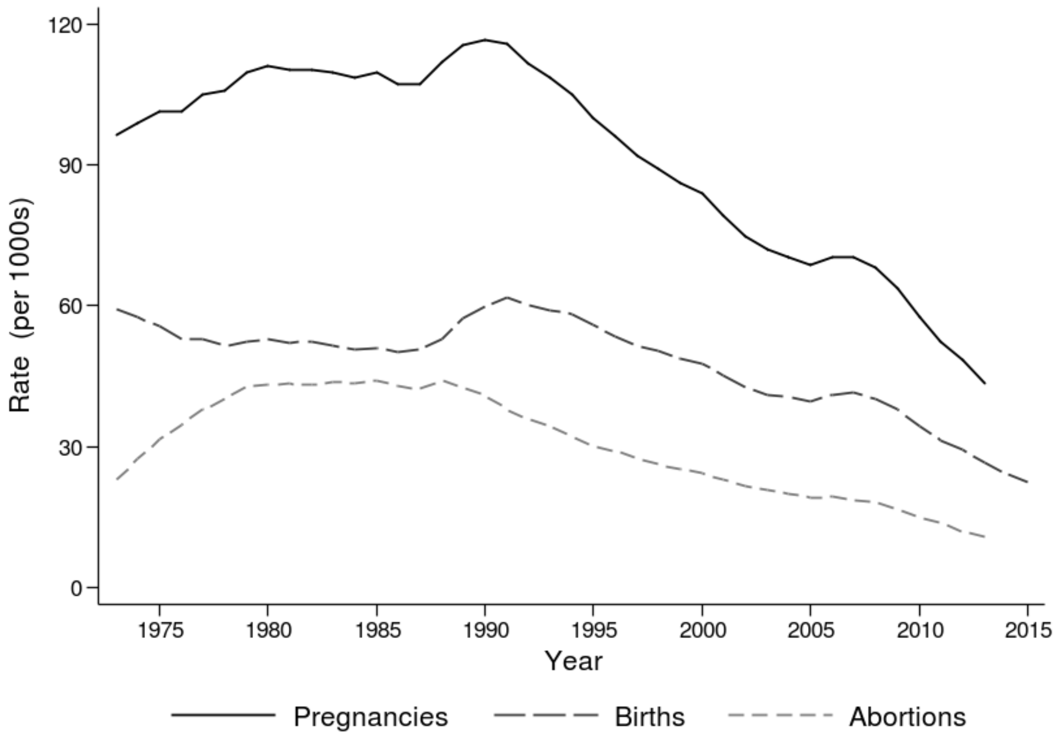
Notes: Total arrest rates are calculated from the UCR by summing up violent, property and drugs arrests. Reporting standards are that only agencies reporting all years are included.

Figure 3: Female Crime Arrest Rates Trends by Crime Type, 1974-2015



Notes: Arrest rates are calculated from the UCR for ages 15-24 by crime type. Reporting standards are that only agencies reporting all years are included.

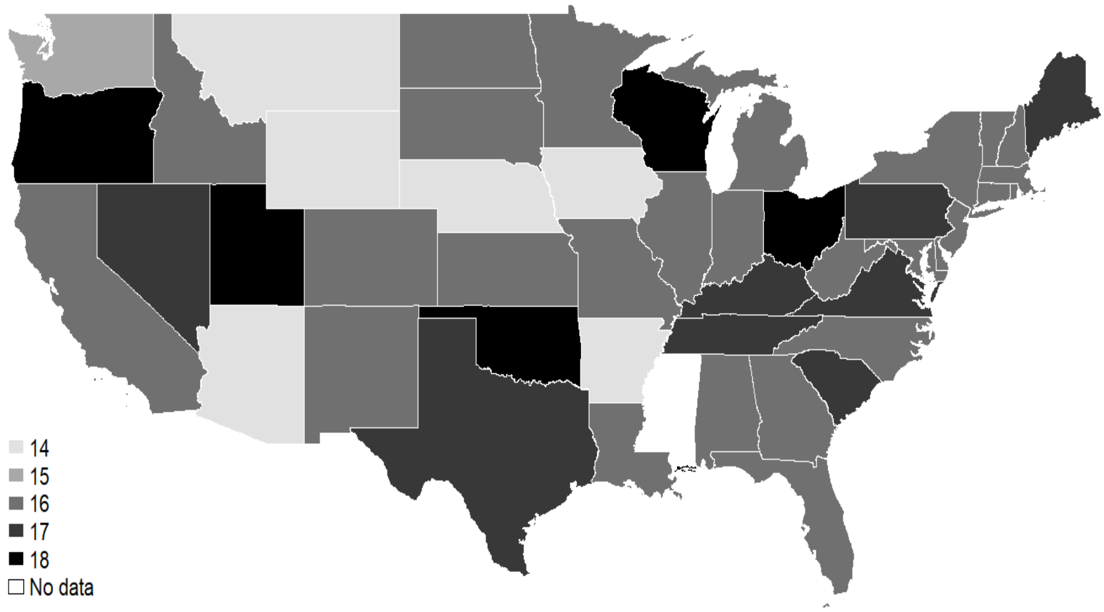
Figure 4: Teenage Pregnancies, Births and Abortion, 1973-2015



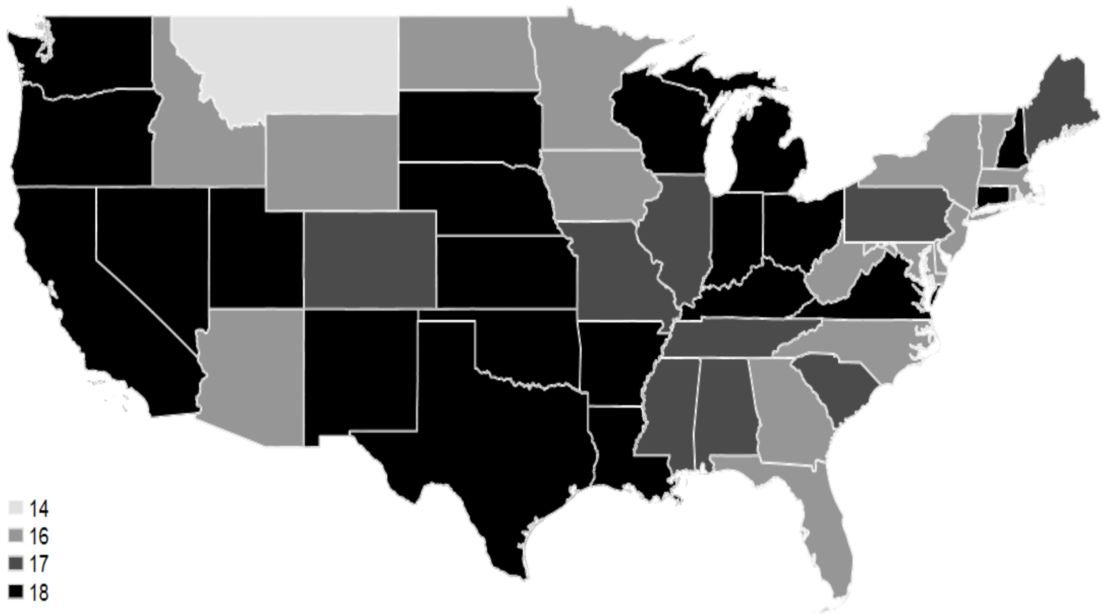
Notes: Pregnancies, births and abortion numbers are provided by National Center for Health Statistics and the Guttmacher Institute. Teenager is defined as individuals 15 to 19 years of age. Pregnancy and abortions number are not available for 2014 and 2015.

Figure 5: State Dropout Ages, 1980 and 2010

1980



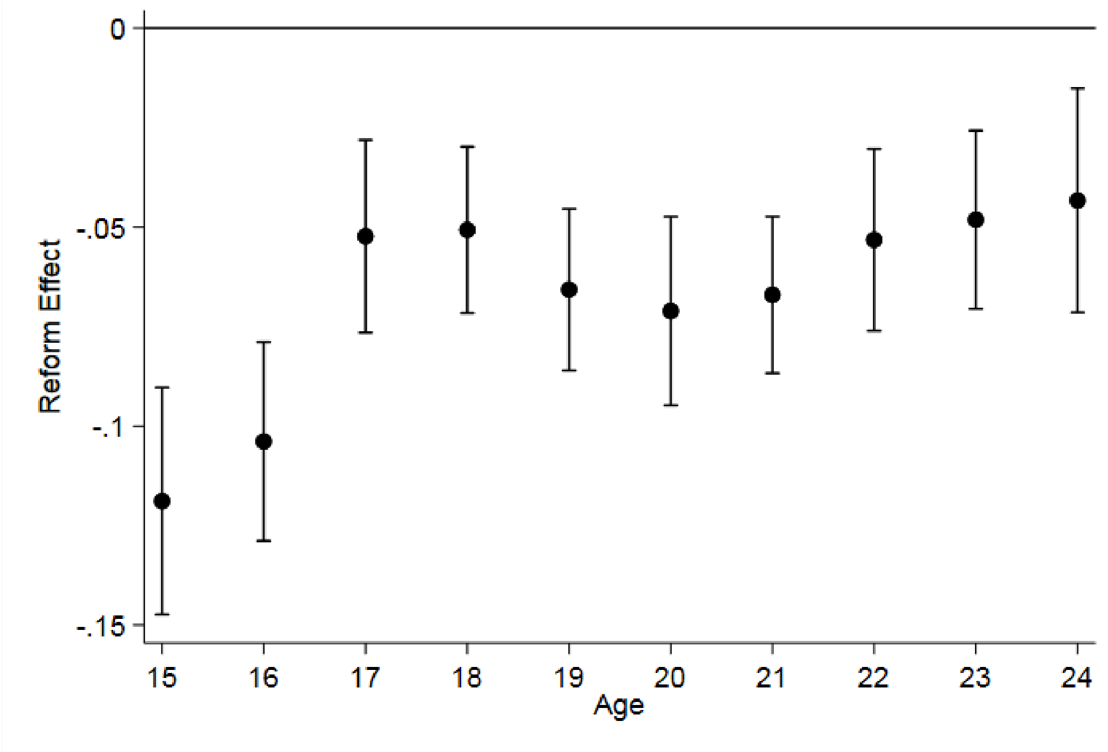
2010



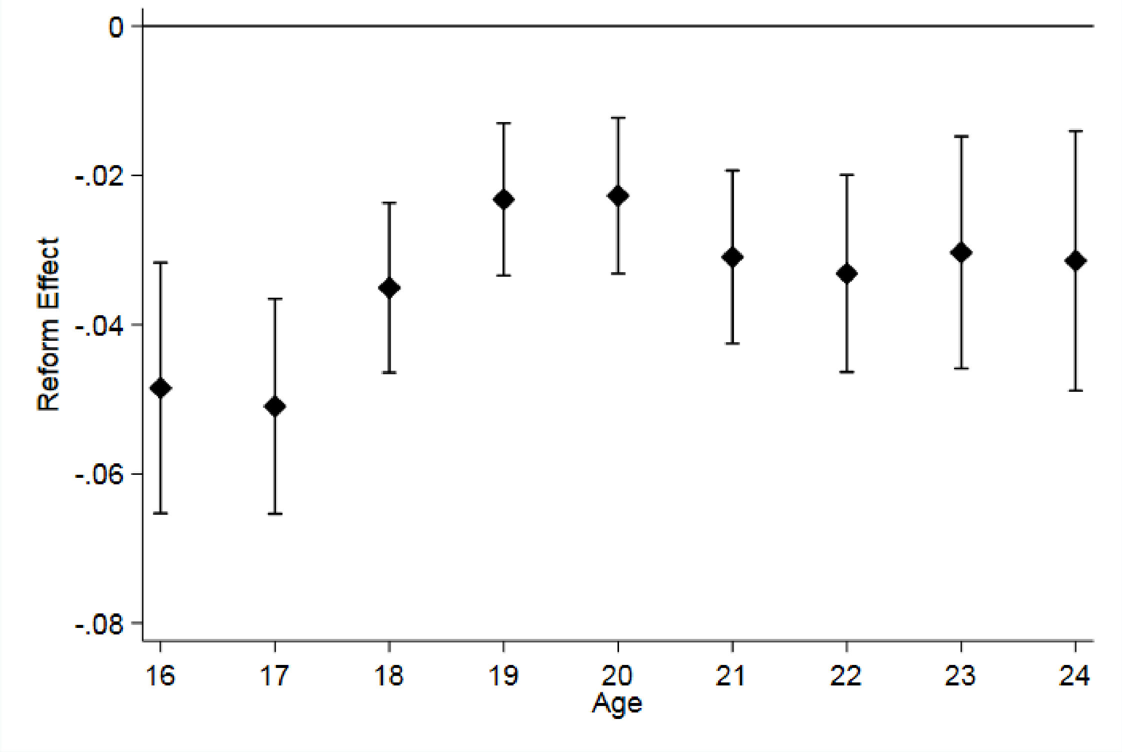
Notes: Dropout Age is calculated using the formula Dropout Age $_{st}$ = min{ Minimum Leaving Age $_{st}$, Grade Exemption $_{st}$ }

Figures 6A and 6B: Age-Specific Effects

A – Arrest Rate



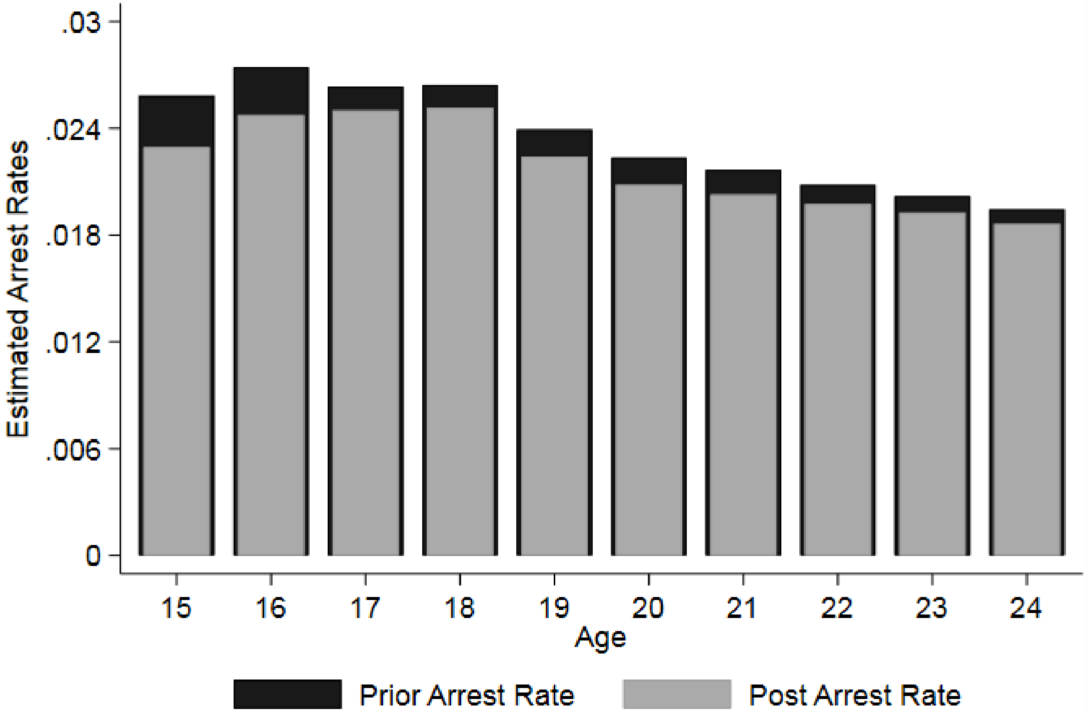
B – Birth Rate



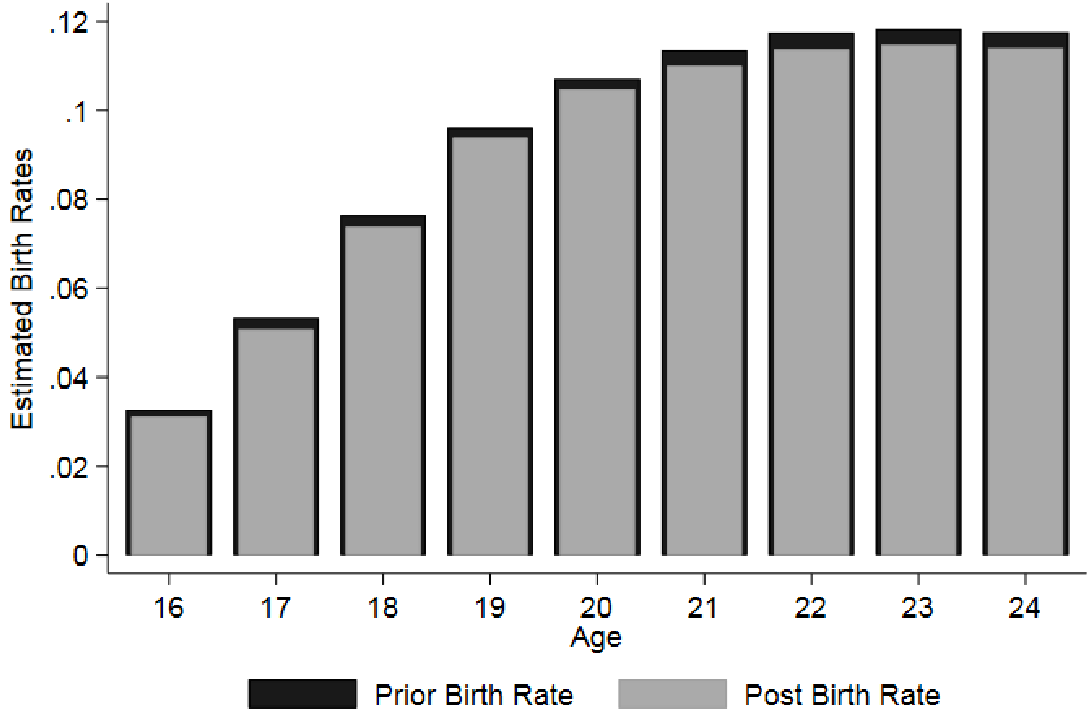
Notes: From estimates in Table 3. Texas (1985) is excluded from the estimation given that is a decrease in dropout age. Confidence intervals at 95% significance, standard errors clustered at reform-cohort level.

Figures 7A and 7B: Age Profile Shifts In Arrests and Births

A – Arrest Rate

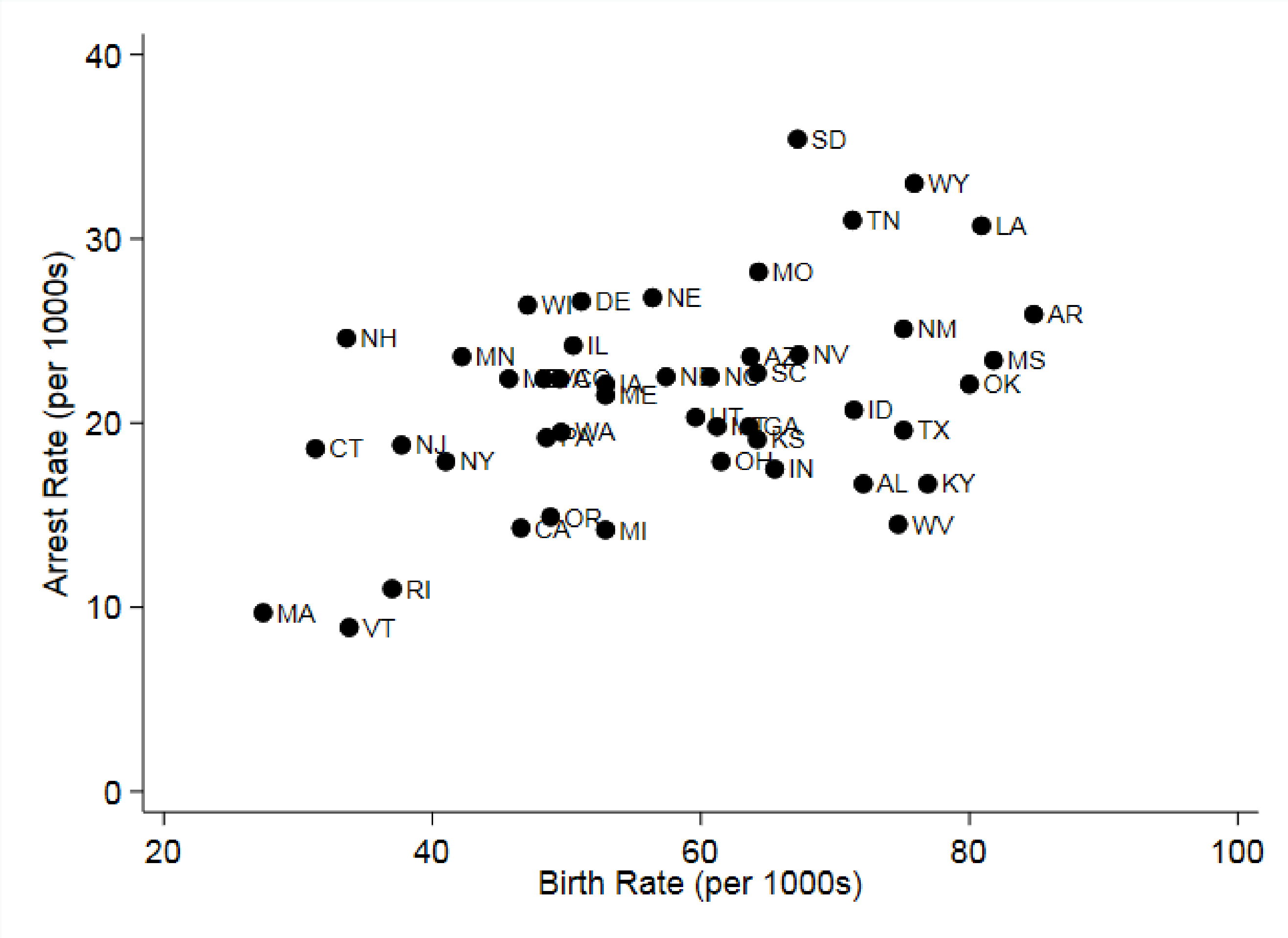


B – Birth Rate



Notes: Prior arrest rate is the mean of arrest rate by age prior to discontinuity using a 5-year bandwidth. Post arrest rate is the calculated by estimated age effects of Figures 6A and 6B.

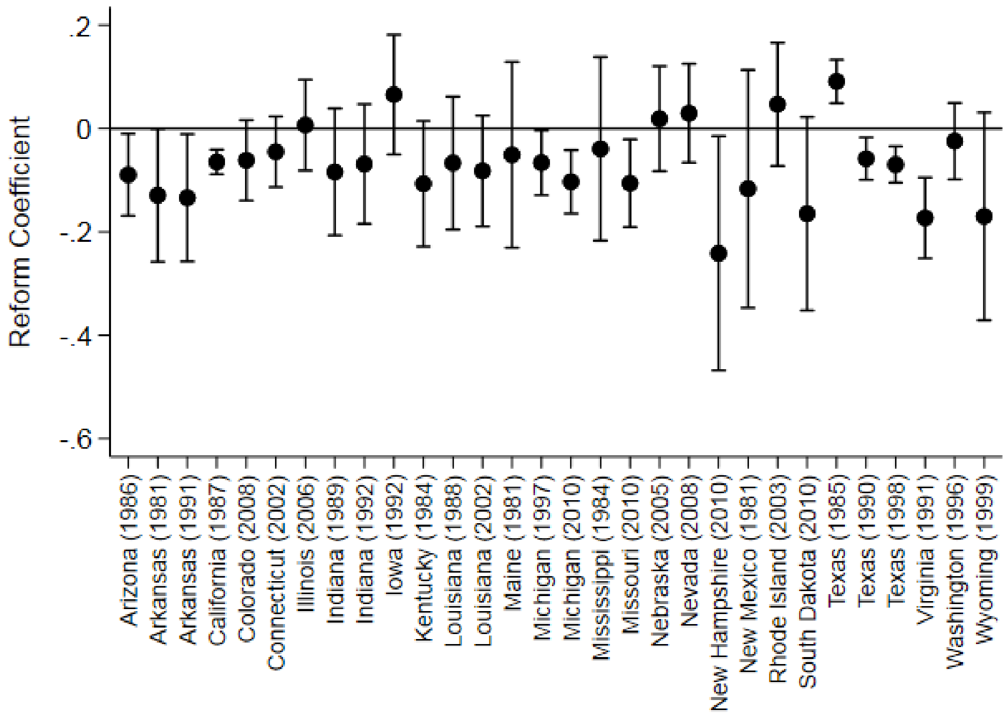
Figure 8: Arrest and Birth Rates by State (per 1000s)



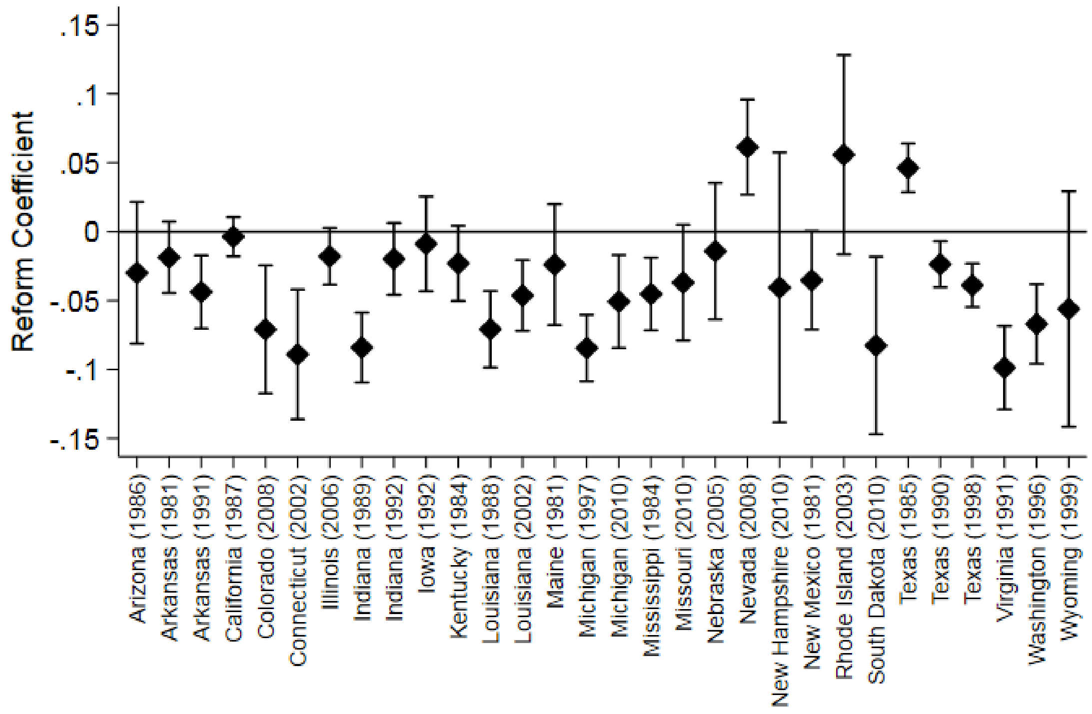
Notes: Numbers calculated based on Uniform Crime Report Master Arrest File and National Vital Statistics – Births as weighted averages over the period 1974 to 2015.

Figures 9A and 9B – Discontinuity Estimates by State

A – Arrest Rate

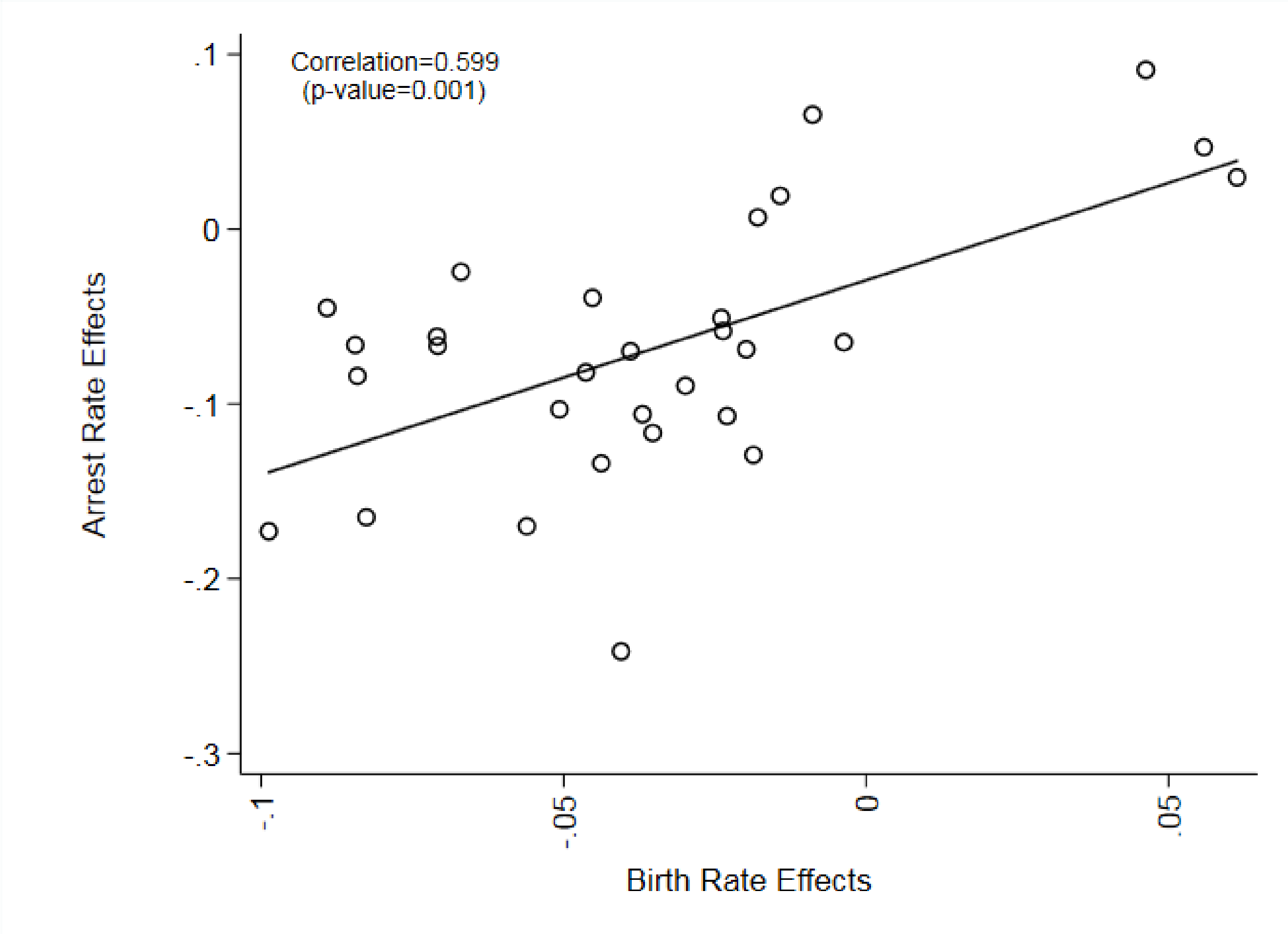


B – Birth Rate



Notes: Coefficients are estimated separately by reform samples using the same specification as column (5) of Table 2, 95% confidence intervals are shown.

Figures 10: Correlation Between Arrest and Birth Reform Effects



Notes: Coefficients from Figures 9A and 9B. No weights are used in the calculation of the correlation.

Table 1: State Dropout Age Reforms

State	Effective School Year From Statute	Type	Change	New Dropout Age
Arizona	1985 and 1986	Exemption	8^{th} to 10^{th} grade	16
Arkansas	1981	Leaving Age	16 to 17	17
Arkansas	1991	Leaving Age	17 to 18	18
California	1987	Leaving Age	16 to 18	18
Colorado	2008	Leaving Age	16 to 17	17
Connecticut	2002	Leaving Age	16 to 18	18
Illinois	2006	Leaving Age	16 to 18	17
Indiana	1989	Leaving Age	16 to 17	17
Indiana	1992	Leaving Age	17 to 18	18
Iowa	1992	Exemption	8^{th} to 12^{th} grade	16
Kentucky	1984	Leaving Age	17 to 18	18
Louisiana	1988	Leaving Age	16 to 17	17
Louisiana	2002	Leaving Age	17 to 18	18
Maine	1981	Exemption	9^{th} to 12^{th} grade	17
Michigan	1997	Exemption	NA to 12^{th} grade	16
Michigan	2010	Leaving Age	16 to 18	18
Mississippi	1984	Leaving Age	Reenactment	17
Missouri	2011	Leaving Age	16 to 17	17
Nebraska	2005	Leaving Age	16 to 18	18
Nevada	2008	Leaving Age	17 to 18	18
New Hampshire	2010	Leaving Age	16 to 18	18
New Mexico	1981	Exemption	10^{th} to 12^{th} grade	18
Rhode Island	2003	Exemption	NA to 12^{th} grade	16
South Dakota	2010	Leaving Age	16 to 18	18
Texas	1985	Leaving Age	Rewriting of law	16
Texas	1990	Leaving Age	16 to 17	17
Texas	1998	Leaving Age	17 to 18	18
Virginia	1991	Leaving Age	17 to 18	18
Washington	1996	Exemption	9^{th} to 12^{th} grade	18
Wyoming	1999	Exemption	8^{th} to 10^{th} grade	16

Notes: Mississippi abolished its compulsory school law in 1956, and re-enacted it 1983/84 with an initial leaving age of 7 with progressive raise until 17 by the school year 1989/90. Texas has written its laws of 1984 and 1989 in a different way, stating the minimum leaving age was to include the completion of school year in which the birthday occurred in effect decreasing/increasing the leaving age by some months. Two other reforms occurred during the same period South Carolina (1987) and Kansas (1996); unfortunately, missing data does not make their analysis feasible.

Table 2: Baseline Estimates of Female Reduced Forms

	(1) Pooled (All States)	(2) Pooled (All States)	(3) Discontinuity (+/- 10 years)	(4) Discontinuity (+/- 7 years)	(5) Discontinuity (+/- 5 years)
A1. Log Arrest Rate (15-24)					
Dropout Reform	-0.130	-0.092	-0.045	-0.068	-0.070
	(0.023)	(0.013)	(0.013)	(0.009)	(0.007)
Observations	1,143,168	1,143,168	339,971	242,694	175,206
A2. Log Teen Arrest Rate (15-18)					
Dropout Reform	-0.120	-0.122	-0.057	-0.072	-0.075
	(0.023)	(0.015)	(0.013)	(0.009)	(0.009)
Observations	457,387	457,387	139,683	100,243	72,222
B1. Log Birth Rate (16-24)					
Dropout Reform	0.031	-0.010	-0.007	-0.027	-0.034
	(0.011)	(0.007)	(0.006)	(0.006)	(0.005)
Observations	160,974	160,974	47,040	33,477	24,196
B2. Log Teen Birth Rate (16-19)					
Dropout Reform	0.026	-0.009	-0.032	-0.042	-0.042
	(0.013)	(0.008)	(0.007)	(0.006)	(0.005)
Observations	70,999	70,999	21,440	15,375	11,161
Controls		Region*Cohort	Linear*Reform	Linear*Reform	Linear*Reform

Notes: Estimates are weighted by population size and standard errors are clustered at state-cohort level. The dependent variables are the log of total arrest rates (includes violent, property and drugs) and log of birth rates. All specifications include age, year and county fixed effects. Covariates further include log of population, log of police force sworn in (in arrest estimates) and shares of female, black, non-white/non-black population. The discontinuity estimates include a further interaction term with the running variable.

Table 3: Balancing Tests

Balancing Covariates			
	-5 years	+5 years	Difference (Standard Error)
Share of Black	0.138	0.137	-0.001
	(0.006)	(0.006)	(0.008)
Share of Others	0.050	0.060	0.010
	(0.006)	(0.007)	(0.009)
Share of Female	0.488	0.486	-0.002
	(0.001)	(0.001)	(0.002)
Log Police	6.918	7.045	0.137
	(0.135)	(0.129)	(0.187)
Log Population	7.994	8.007	0.013
	(0.128)	(0.119)	(0.174)
Teacher-Pupil Ratio	18.29	17.77	-0.514
	(0.537)	(0.554)	(0.755)

Notes: Sample includes cohorts of females aged 15-24 for US counties over time. Means across all counties in the balanced sample for each of the 30 reforms (as in Table 1), on each side of the +/- 5 bandwidth. Estimates are weighted by population size and standard errors are clustered at reform-cohort level.

Table 4: Age Varying Reform Impacts by Crime Type

Log(Arrest Rate),
Discontinuity (+/- 5 years) Sample

	Total	Violent	Property	Drugs
A. Overall Effect				
Dropout Reform	-0.071	-0.063	-0.054	-0.115
	(0.008)	(0.011)	(0.009)	(0.014)
B. Age Effects				
Reform*Age = 15	-0.119	-0.121	-0.081	-0.323
	(0.015)	(0.022)	(0.018)	(0.036)
Reform*Age = 16	-0.104	-0.075	-0.072	-0.244
	(0.013)	(0.019)	(0.015)	(0.035)
Reform*Age = 17	-0.052	-0.046	-0.015	-0.104
	(0.012)	(0.018)	(0.015)	(0.030)
Reform*Age = 18	-0.050	-0.106	-0.027	-0.010
	(0.011)	(0.020)	(0.014)	(0.023)
Reform*Age = 19	-0.066	-0.090	-0.035	0.008
	(0.010)	(0.018)	(0.013)	(0.021)
Reform*Age = 20	-0.071	-0.078	-0.065	-0.039
	(0.012)	(0.020)	(0.016)	(0.021)
Reform*Age = 21	-0.067	-0.046	-0.056	-0.094
	(0.010)	(0.021)	(0.014)	(0.021)
Reform*Age = 22	-0.053	-0.021	-0.059	-0.090
	(0.012)	(0.022)	(0.015)	(0.022)
Reform*Age = 23	-0.048	-0.001	-0.050	-0.080
	(0.011)	(0.021)	(0.016)	(0.025)
Reform*Age = 24	-0.043	0.006	-0.073	-0.080
	(0.014)	(0.026)	(0.018)	(0.025)
Observations	153,114	153,114	153,114	153,114

Notes: Same specification as column (5) of Table 2. Sample excludes Texas (1985) reform given that is a decrease in compulsory schooling.

Table 5: Age Varying Reform Impacts by Reform Type

	Log(Arrest Rate), Discontinuity (+/- 5 years) Sample						Log(Birth Rate), Discontinuity (+/- 5 years) Sample					
	All Reforms	**16 to 17**	**17 to 18**	**16 to 18**	**Other**	**17 to 16 (Texas)**	**All Reforms**	**16 to 17**	**17 to 18**	**16 to 18**	**Other**	**17 to 16 (Texas)**
A. Overall												
Dropout	-0.071	-0.068	-0.071	-0.083	-0.043	0.091	-0.035	-0.037	-0.031	-0.027	-0.056	0.046
Reform	(0.008)	(0.014)	(0.014)	(0.017)	(0.011)	(0.022)	(0.005)	(0.008)	(0.008)	(0.010)	(0.009)	(0.011)
B. Age Effects												
Reform*Age = 15	-0.109	-0.075	-0.104	-0.172	-0.094	0.098						
	(0.015)	(0.025)	(0.022)	(0.031)	(0.029)	(0.047)						
Reform*Age = 16	-0.104	-0.057	-0.101	-0.145	-0.093	0.113	-0.049	-0.034	-0.097	-0.014	-0.083	-0.015
	(0.013)	(0.021)	(0.023)	(0.027)	(0.021)	(0.044)	(0.009)	(0.010)	(0.015)	(0.013)	(0.019)	(0.014)
Reform*Age = 17	-0.050	-0.069	-0.018	-0.082	-0.007	0.047	-0.051	-0.048	-0.061	-0.039	-0.070	0.011
	(0.012)	(0.020)	(0.023)	(0.025)	(0.023)	(0.042)	(0.007)	(0.010)	(0.013)	(0.015)	(0.015)	(0.013)
Reform*Age = 18	-0.041	-0.066	-0.042	-0.058	-0.020	0.124	-0.035	-0.038	-0.023	-0.032	-0.057	0.038
	(0.011)	(0.018)	(0.020)	(0.022)	(0.019)	(0.040)	(0.006)	(0.009)	(0.008)	(0.012)	(0.012)	(0.012)
Reform*Age = 19	-0.062	-0.063	-0.079	-0.082	-0.009	0.079	-0.023	-0.028	0.003	-0.037	-0.024	0.064
	(0.010)	(0.018)	(0.018)	(0.022)	(0.020)	(0.039)	(0.005)	(0.009)	(0.008)	(0.010)	(0.011)	(0.012)
Reform*Age = 20	-0.061	-0.085	-0.079	-0.073	-0.028	0.054	-0.023	-0.040	0.004	-0.029	-0.015	0.060
	(0.011)	(0.023)	(0.022)	(0.024)	(0.021)	(0.040)	(0.005)	(0.010)	(0.008)	(0.010)	(0.009)	(0.015)
Reform*Age = 21	-0.060	-0.056	-0.060	-0.086	-0.051	0.108	-0.031	-0.049	-0.006	-0.029	-0.035	0.054
	(0.010)	(0.022)	(0.019)	(0.018)	(0.024)	(0.042)	(0.006)	(0.011)	(0.008)	(0.011)	(0.009)	(0.018)
Reform*Age = 22	-0.046	-0.079	-0.063	-0.042	-0.021	0.091	-0.033	-0.039	-0.010	-0.033	-0.053	0.076
	(0.011)	(0.020)	(0.023)	(0.021)	(0.026)	(0.044)	(0.007)	(0.012)	(0.011)	(0.012)	(0.011)	(0.019)
Reform*Age = 23	-0.044	-0.065	-0.064	-0.022	-0.048	0.099	-0.030	-0.031	-0.021	-0.016	-0.064	0.075
	(0.011)	(0.023)	(0.025)	(0.016)	(0.022)	(0.047)	(0.008)	(0.013)	(0.014)	(0.011)	(0.016)	(0.015)
Reform*Age = 24	-0.035	-0.070	-0.079	0.003	-0.037	0.097*	-0.031	-0.025	-0.028	-0.008	-0.082	0.095
	(0.014)	(0.027)	(0.025)	(0.023)	(0.028)	(0.050)	(0.009)	(0.013)	(0.015)	(0.013)	(0.017)	(0.020)
Observations	153,114	46,268	46,416	33,551	26,879	22,092	22,156	6,916	4,752	6,094	4,394	2,160
State	24	7	6	8	8	1	24	7	6	8	8	1
Counties	1,175	495	469	362	272	221	192	83	53	76	49	24

Notes: Same specification as column (5) of Table 2. “All Reforms” excludes Texas (1985) reform given that is a decrease in compulsory schooling.

Table 6: Arrest and Birth Rates (per 1000s), Ages 15-24, 2015

State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)	State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)	State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)
Vermont	**8.9**	33.8	Washington	19.5	49.6	Arizona	23.6	63.7
Massachusetts	9.7	**27.4**	Texas	19.6	75.1	Minnesota	23.6	42.2
Rhode Island	11.0	37.0	Montana	19.8	61.2	Nevada	23.7	67.3
Michigan	14.2	52.9	Georgia	19.8	63.6	Illinois	24.2	50.5
California	14.3	46.6	Utah	20.3	59.6	New Hampshire	24.6	33.6
West Virginia	14.5	74.7	Idaho	20.7	71.4	New Mexico	25.1	75.1
Oregon	14.9	48.8	Maine	21.5	52.9	Arkansas	25.9	**84.8**
Alabama	16.7	72.1	Oklahoma	22.1	80.0	Wisconsin	26.4	47.1
Kentucky	16.7	76.9	Iowa	22.1	52.9	Delaware	26.6	51.1
Indiana	17.5	65.5	Colorado	22.4	49.5	Nebraska	26.8	56.4
Ohio	17.9	61.5	Virginia	22.4	48.3	Missouri	28.2	64.3
New York	17.9	41.0	Maryland	22.4	45.7	Louisiana	30.7	80.9
Connecticut	18.6	31.3	North Dakota	22.5	57.4	Tennessee	31.0	71.3
New Jersey	18.8	37.7	North Carolina	22.5	60.7	Wyoming	33.0	75.9
Kansas	19.1	64.2	South Carolina	22.7	64.2	South Dakota	**35.4**	67.2
Pennsylvania	19.2	48.5	Mississippi	23.4	81.8	Florida	-	54.9

Notes: Numbers calculated based on Uniform Crime Report Master Arrest File and National Vital Statistics - Births.

Table 7: Reform Effects Relation with County Economic and School Quality Indicators

Reform Effects (%)	(1)	(2)	(3)	(4)	(5)
% Black	-0.862				-0.799
	(0.187)				(0.189)
% Employed		-0.453			-0.294
		(0.127)			(0.137)
Police per capita (per 1000s)			-6.574		-3.987
			(2.002)		(2.136)
Pupil/Teacher Ratio				2.077	2.433
				(0.893)	(0.890)
Controls	Reform FE	Reform FE	Reform FE	Reform FE	Reform FE
Observations	1,798	1,798	1,798	1,798	1,798

Notes: The dependent variable is reform effect estimated according to model described in Equation (5) where demographic controls include log population size and further multiply by 100. Estimates are weighted by the absolute value to t-stat as to account for precision of the two-step estimation. All specifications include reform-specific fixed effects.

Appendix: Data Description

A1. Panel Data on Arrests

Panel data for the US come from the FBI Uniform Crime Reports (UCR). The measure of crime is arrests. The UCR reports the number of arrests by year, state, age, gender and type of crime. The original data identifies the number of arrests by law enforcement agencies within states. We construct a county-level panel on arrests by aggregating the number of arrests over law enforcement agencies within a county. Within the UCR, data for certain agencies is systematically missing. For example, New York City systematically does not report arrest numbers. For the agencies used in our estimation we impose a reporting pattern consistent with a maximum tolerance of one missing year per discontinuity window (i.e: for 10-year bandwidth the agency needs to report 18 out of the 20 years)[89].

In addition, the UCR reports the total population for each law enforcement agency in the reported year. Aggregating the UCR population count to the county-year level and comparing that number to official population counts allows us to identify county-year covering ratio. The weighted average county-level covering ratio is of 89% for the 5-year bandwidth. When estimating the population per age-sex cell, we use the SEER*Stat population estimates[90] at county level and apply the yearly covering ratios homogeneously across different ages. The implicit assumption is that the missing population has the same age breakdown as the overall county-year population. The weighted average share of state population covered in the 5-year bandwidth is of 81% for discontinuity states.

We sample males aged 15 to 24 from 1974 to 2015. The UCR data are grouped by age category. From age 15 up to the age of 24, the number of arrests is measured by single age year.

Following the literature, we categorize arrests into property and violent crime using the UCR offense code variable as follows:

Violent crime:	Property crime:	Drug Crime:
01A = Murder and non-negligent manslaughter	05 = Burglary – breaking or entering	18 = Drug Violations (Possession, Sale and Manufacturing)
01B = Manslaughter by negligence 02 = Forcible rape 03 = Robbery 04 = Aggravated assault 08 = Other assaults	06 = Larceny – theft (except motor vehicle) 07 = Motor vehicle theft 09 = Arson	

In order to produce arrest rates, we aggregate the number of arrests for the above categories and divide the resulting number of arrests by the annual county-age-year population. Some

[89] Table 2, column (1) sample includes only agencies reporting at least 10 years over 1974-2015. The results are robust to a stricter reporting of all years in the bandwidth period.

[90] See Surveillance, Epidemiology, and End Results (SEER) Program of the National Cancer Institute (NCI) 1969-2014.

cells report 0% arrest rates, however for those we assume the lowest arrest rate reported in the sample period. Cell reporting arrest rates above 40% are excluded from the sample to avoid outliers influencing the analysis.[91]

A2. Panel Data on Births

The National Center for Health Statistics (NCHS) provides access to the National Vital Statistics for births, which includes the universe of birth certificates across US territories. Microdata on birth certificates is provided publicly for research purposes, however starting in 1989 county identifiers are limited to geographical aggregates including at least 100000 inhabitants. Furthermore, from 2005 forwards no geographical identifier is included in the microdata made public (including state identifiers). The data from 2005 to 2015 is collected from WONDER-NCHS service that enables to produce aggregated statistics compatible with the 1989-2004 county aggregation criteria. Unfortunately, the data on births at ages below 16 is significantly restricted to small-populated geographies justifying henceforth the use of ages equal or above 16 to keep the panel data balanced. Unrestricted access to microdata including geographical identifiers and ages is provided exclusively to US-based researchers.

A3. Racial Breakdown Covariates

An analogous method to one used to obtain population age-sex cell in *A1* is performed to estimate the racial breakdown of each cell. We use the racial population estimates collected from SEER*Stat at county-year level and the county covering ratio to estimate the number of population by race for each sex-county-year cell.

A4. Police Numbers

The police numbers used are collected from FBI LEOKA (1960-2015). This data reports several police enforcement measures yearly for each enforcement agency. We use the total number of sworn officers per county-year as the measure of police force present at the geographic area of interest.

A5. County-Year Economic and Employment Covariates

Information on economic and employment indicators at county-year level are collected from Local Area Personal Income (LAPI) from Bureau of Economic Analysis (BEA) 1960-2015. Measures of total employment, personal income, wage income and several others are available at county-year level from LAPI.

A6. School Quality Measures

We use the Local Education Agency (LEA) level data available in the Common Core Data (CCD) from National Center for Education Statistics (NCES), both fiscal and non-fiscal[92], to produce the school quality measures. By aggregating both the number of students, teachers and

[91] The results are robust to the use of 30% and 50% thresholds alike.
[92] Unfortunately, fiscal information at LEA level is only available since 1989.

instruction salary expenses at county-year level, we are able to compute estimates of pupil-teacher ratio and average teacher salary. We interpolate missing years in the data, these are not frequent and do not affect the general results.

Table A1: Teenage Arrest and Birth Rates (per 1000s), 15-19, 2015

State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)	State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)	State	Arrest Rate (per 1000s)	Birth Rate (per 1000s)
Vermont	**6.0**	15.0	Texas	18.8	42.1	Delaware	23.1	21.4
Massachusetts	8.0	**12.7**	Kansas	18.8	31.1	Arizona	23.3	31.8
West Virginia	10.4	39.0	North Carolina	18.9	28.8	Arkansas	23.5	**46.2**
Rhode Island	11.0	18.4	Washington	19.1	21.9	Colorado	24.3	23.6
California	12.0	23.1	Maine	19.4	24.2	North Dakota	24.9	26.0
Alabama	13.0	36.3	Mississippi	19.5	42.3	Iowa	25.8	22.9
Kentucky	13.2	39.2	Virginia	19.8	20.4	Minnesota	26.1	17.2
Michigan	14.3	24.5	Nevada	20.5	34.2	Tennessee	28.1	37.0
New Jersey	15.4	16.3	Idaho	21.2	27.8	Missouri	28.2	31.4
Oregon	15.7	23.1	South Carolina	21.3	31.6	Wisconsin	28.5	21.4
Pennsylvania	16.2	22.8	Oklahoma	21.6	42.5	Louisiana	28.8	41.4
New York	16.3	17.8	New Hampshire	22.0	15.8	Illinois	29.1	25.9
Ohio	16.7	29.1	New Mexico	22.2	41.7	Nebraska	33.0	26.6
Connecticut	16.8	13.9	Utah	22.2	22.2	Wyoming	36.6	36.1
Georgia	17.6	30.7	Maryland	22.3	21.3	South Dakota	**42.1**	32.2
Indiana	18.0	32.1	Montana	22.5	31.2	Florida	-	25.6

Notes: Numbers calculated based on Uniform Crime Report Master Arrest File and National Vital Statistics - Births.

Chapter 5

Conclusion

"We also know that when students aren't allowed to walk away from their education, more of them walk the stage to get their diploma. When students are not allowed to drop out, they do better. So tonight, I call on every State – every State - to require that all students stay in high school until they graduate or turn eighteen."

President Barack Obama, State of Union Address of 2012

This thesis studies the different channels through which educational policies (compulsory schooling laws in particular) can affect an individual's decision to engage in crime and other risky behaviour such unprotected sex resulting in teenage pregnancy. Each of the preceding chapters have addressed pertinent questions in the policy debate: (i) does educational attainment help to prevent crime, and if so to what extent, (ii) do individuals necessarily need to gain higher levels of education for compulsory school laws to be effective at deterring crime, (iii) does female crime engagement react to educational reforms in a similar way as its male counterpart, (iv) are the magnitude of the effects dependent on local characteristics of labour markets and educational systems? The contribution of this thesis lies on a careful understanding of the context and research that previously addressed some of these questions, proposing new research designs that complement the limitations of previous literature and further expand the comprehension of the mechanisms through which educational policies can help to deter individuals from engaging in crime and risky behaviours that entail considerable costs for social and individual welfare in the short and long-run.

The first paper introduces the reader to the historical trends and most recent developments of crime in the United States. From crime rates to arrest rates and incarceration, the levels and evolution of crime as broadly defined are examined along dimensions as type of crime, gender and race. The chapter then continues to present a general model of rational individual decision-making used to study how incentives of different natures (monetary and non-monetary) can play a role in influencing an individual's choice to engage in crime and how empirical literature has used econometric identification models to establish and quantify causal relationships relevant for policy design.

This first work focuses on the particular interactions between compulsory schooling laws, educational attainment and incarceration through an extended period of modern US history: 1960 to 2010. Using an instrumental variable design and the collection of compulsory schooling laws from 1910 to 2016, the empirical section of the paper estimates reduced form effects of compulsory schooling laws and causal estimates between educational accreditations and incarceration probability. Evidence is found in support of educational gains resulting from changes in compulsory schooling having had a significant negative effect on the probability of incarceration across gender and race for this time period as a whole. Despite previous work using compulsory school age laws as instruments for completed educational levels, this is the first work to cover such a large period of data consistently and provides results across race and gender using different measures of the laws to check the robustness of the results.

The second paper looks at the recent effects of compulsory schooling reforms, questioning if they remain valid as a source of improvements in education completion nowadays. Making use of data since 1980 for both incarceration and arrests and an innovative local labour market definition of commuting zones, the paper makes relevant findings and contributions to the literature: recent compulsory schooling laws have shown an effect of reducing arrests and the probability of incarceration among males, however, not through educational attainment gains as revealed in previous literature focused on earlier periods. With the exception of populations with initially low educational levels (black males), there is no supporting evidence that recent laws have significantly raised educational levels. The idea of incapacitation effects being a source of the crime reduction in the data using reduced form models is then hypothesized and estimated. The paper concludes that there are beneficial effects of this type of educational reform that are not captured through progress in educational attainment, leaving the question about different acting channels open for more detailed empirical designs and further research.

The previous question concerning the channels through which compulsory school laws have affected crime motivates the third paper. In that paper, a theoretical model is formulated to help understand how mechanisms other than educational attainment might influence the individual's decision to commit crime along the age dimension. This model extends the general framework presented in the first paper of this thesis to specifically introduce the idea of contemporaneous incapacitation and dynamic incapacitation as result of disruption in criminal capital accumulation and its interaction with the sanctioning age profile. The model does not exclude the "traditional" productive educational effects but explains that these are not a necessary condition for changes in legal dropout age to reduce crime in both the short and medium-run.

Anchored on the predictions of the model, the paper proposes a multiple discontinuity design model as a tool to identify and quantify the previous effects. This design is novel in several dimensions: a) it enables the separate identification of a large set of reforms (30 to be precise) that previously have been identified jointly, b) it allows for more conservative modelling of confounding factors by using exclusive within-state variation in the identification of the model, and c) it reduces the measurement error associated with the matching of the laws to an approximate cohort affected as previously done in the literature. Using a detailed panel of arrest rates by county, year, age and type of crime covering the years 1974 to 2015, the multiple discontinuity model is estimated, presenting results compatible with strong incapacitation and medium-term crime reducing effects among young males. The results are shown to be robust for different types of crime and reform margins, and to be heterogeneous across states. To provide insight about the mechanisms underlying the incapacitation and medium-term effects, the paper analyses the effects of the reforms on high school attendance, educational attainment, college/employment and wages using the same empirical design. The strong positive results on high school attendance, combined with the low magnitude of effects on education completion and college/work, and the insignificant results on wages provided, suggest that a dynamic incapacitation effect is most likely to explain at least a significant part of the medium-term and potential long-term reduction in crime.

The final paper looks at the rather unexplored dimension of crime, female crime, as well as teenage pregnancy. The paper motivates the discussion about the increasing relevance of female crime engagement for the overall crime phenomenon by pointing out how the gap between genders has been steadily decreasing in the recent data for the US. Furthermore, it shows teenage pregnancy as a social outcome of high relevance in the context of the US, presenting extremely high levels when compared to other developed economies. Considering that both outcomes, crime engagement and teenage pregnancy, are socially undesirable and entail severe consequences for an individual's life opportunities, the paper follows by proposing to measure the effects of changes in dropout ages on both outcomes.

Making use of multiple discontinuity design and detailed data on arrests and birth rates between 1974 and 2015, the paper findings suggest that, like their male counterpart, females have reduced their engagement in crime and risky behaviours that would result in early childbearing as a result of changes in compulsory school laws. The magnitude of the crime reduction is, in relative terms, equivalent to that of males and the effects along the age profile look consistent with the findings of the third paper. Stronger incapacitation effects and medium-term reductions are found for teenage pregnancy as well. Finally, in this fourth paper, the question of whether local conditions matter for the efficacy of the reductions estimated previously is answered through a two-step estimation analysis at county level. This analysis shows that racial composition, employment rates, police density and school quality at local level significantly affect the magnitude of the potential of school laws to reduce socially undesirable outcomes such as crime.

In conclusion, this thesis provides insights on several relevant features surrounding educational reform impacts in general by looking at compulsory schooling laws and conducting comprehensive theoretical and empirical studies in this dimension. In a time when evidence-oriented policies gain ground in the political arena, reforms with capacity to affect education and safety, undoubtedly areas that remain vastly within state's sphere, are extremely valuable and should be guided by relevant and carefully designed empirical work.

The scope of relevant topics to be studied in the field of crime economics remains large and further research using innovative empirical designs and data can greatly contribute towards a better understanding of the determinants and dynamics underlying this social phenomenon. The new identification designs used in this thesis can help answer questions left without consensus as, for example, the effects of juvenile age on short-run and long-run crime outcomes or the extent to which crime is responsive to changes in local labour market earnings and income distribution. The author of this thesis has begun preliminary work on both topics, collecting data on juvenile age changes in the US since 1920 and on potential local labour market income shifters in the context of Brazil, so as to address these policy-relevant themes.