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Crime, Compulsory Schooling Laws and Education

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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).

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JEL codes: I2; K42

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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.[1] 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

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

fallen substantially. On the latter, education levels are higher (in part reflecting longer run trends) and 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:

$$\text{Crime}_i = \beta \text{Schooling}_i + \gamma X_i + \varepsilon_i \qquad (1)$$

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:

$$\text{Crime}_i = \theta_1 \text{CSL}_i + \pi_1 X_i + \varepsilon_{1i} \tag{2}$$

$$\text{Schooling}_i = \theta_2 \text{CSL}_i + \pi_2 X_i + \varepsilon_{2i} \tag{3}$$

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.[2] 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

[2] 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.

incapacitation effects (Jacob and Lefgren, 2003, and Luallen, 2006).[3] However, we also know that criminal behaviour peaks in the late teenage years. If the incapacitation effect reduces criminal activity 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 through 1980s. For various measures of criminal activity, there is strong evidence that schooling laws, working through their 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

[3] 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.

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. Data Description

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 based on the agency population in each CZ.[4] 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.

[4] 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.

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 80th 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%.

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. (2014) 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 Laws

We consider two alternative measures of the instrument.[5] 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:

$$CSL_{st} = \min\left\{\text{Dropout Age}_{st} \text{ - Enrollment Age}_{st\text{-}8}, \text{Years of School Needed to Dropout}_{st}\right\} \quad (4)$$

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.[6] 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 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

[5] See the discussions of different CSL definitions in Stephens and Yang (2014) and Oreopoulos (2009).
[6] See Oreoupoulos (2009) for more detailed discussion on the nature of exceptions and exemptions, plus a Table summarizing them for some of the more recent laws.

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:

$$\text{Crime}_{ast} = \theta_1 \text{CSL}_{ast} + \pi_1 X_{ast} + \varepsilon_{1ast} \tag{5}$$

where Crime_{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 X_{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 $\varepsilon_{1ast} = \alpha_a + \alpha_c + \alpha_s + \alpha_t + e_{1ast}$.[7] 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.

[7] 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.

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 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.[8] 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

[8] 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.

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 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.[9] 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 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

[9] 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.

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.[10]

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

[10] 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.

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.[11]

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

[11] 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.

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.[12] 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.

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

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

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.[13] 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.

[13] 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)

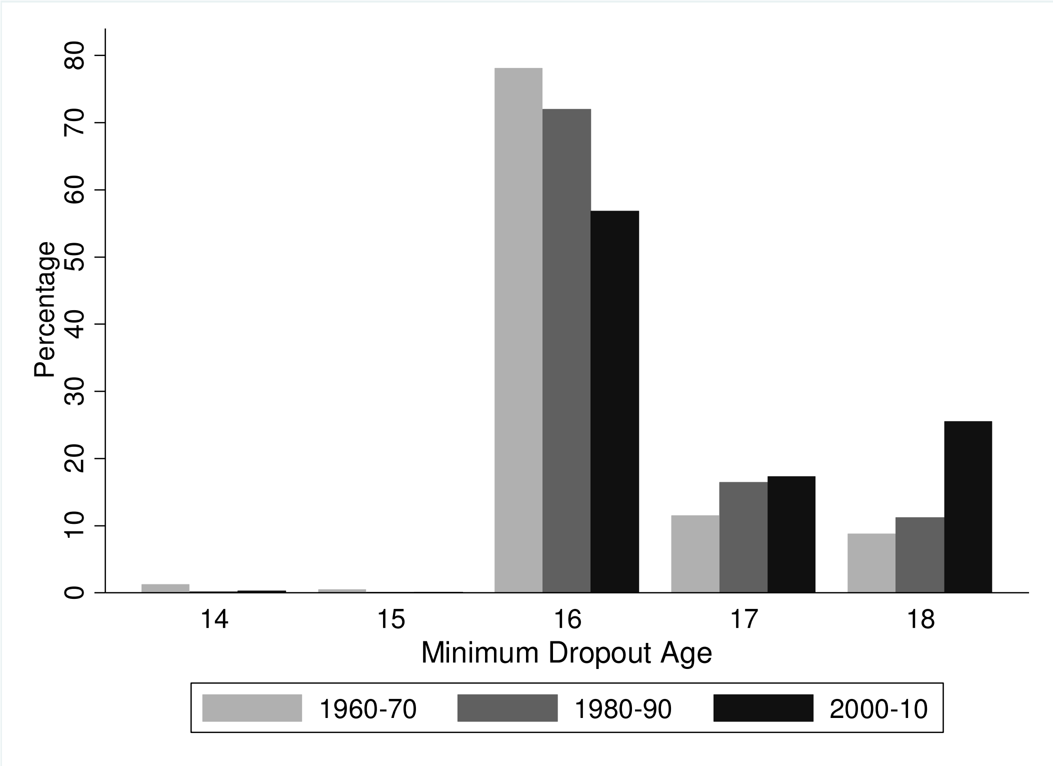

FIGURE 1B. POPULATION-WEIGHTED DISTRIBUTION OF COMPULSORY SCHOOLING LAW (GOLDIN-KATZ MEASURE)

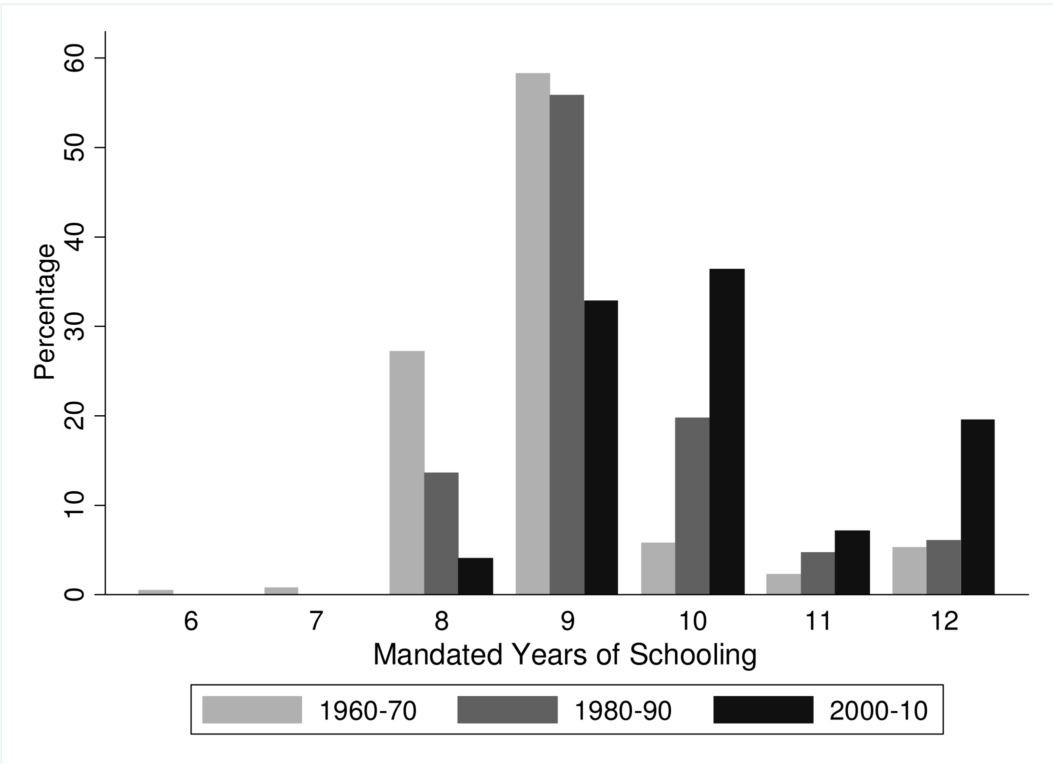

TABLE 1. SUMMARY STATISTICS

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 80th 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.

* signifies significance at the 10% level
** signifies significance at the 5% 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 probit 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.

* signifies significance at the 10% level
** signifies significance at the 5% 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.

* signifies significance at the 10% level
** signifies significance at the 5% level

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.

* signifies significance at the 10% level
** signifies significance at the 5% level

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.

* signifies significance at the 10% level
** signifies significance at the 5% level

Appendix

Data Description

A. 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:

01A = Murder and non-negligent manslaughter

Property crime:

05 = Burglary – breaking or entering

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 commuting zone-age-year population. The population data for that purpose are retrieved from the US Census population estimates.

B. 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.

C. 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 80th 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.

* signifies significance at the 10% level
** signifies significance at the 5% level

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