

Why Does Education Reduce Crime?

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We provide a unifying empirical framework to study why crime reductions occurred due to a sequence of state-level dropout age reforms enacted between 1980 and 2010 in the United States. Because the reforms changed the shape of crime-age profiles, they generate both a short-term incapacitation effect and a more sustained crime-reducing effect. In contrast to previous research looking at earlier US education reforms, we find that reform-induced crime reduction does not arise primarily from education improvements. Decomposing short- and long-run effects, the observed longer-run effect for the post-1980 education reforms is primarily attributed to dynamic incapacitation.

I. Introduction

For most crime types and in different settings, an established research finding is that education lowers criminality. In the causal crime education

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literature, this finding frequently emerges in studies when increases in school dropout age resulting from changes made to compulsory school leaving (CSL) laws simultaneously boost education and reduce crime.¹ What is currently less well understood is how and why this education policy-induced crime reduction comes about.

This paper makes the argument that additional insight can be gained by zooming in on the dynamics of policy-induced shifts in the age structure of criminality that occur from the enactment and implementation of CSL laws. In particular, the scope for law changes to alter crime-age profiles is studied to develop a better understanding of the reasons why education lowers crime. The critical insight is that because CSL reforms change the shape of crime-age profiles, they generate both a short-term incapacitation effect together with a more sustained, longer-run crime-reducing effect. This latter long-run effect can come about through education improving productivity or because short-run incapacitation displaces state dependence that generates dynamic incapacitation.

This paper presents empirical work to show that the balance between education-induced productivity boosts, on the one hand, and dynamic incapacitation, on the other, has shifted over time in the United States. The key factor in the way CSL reforms that occurred since 1980 explain the crime-education relation is dynamic incapacitation and not improved productivity from more education, as was the case in earlier research (most notably, Lochner and Moretti 2004).

Evidence from the school dropout age reforms enacted in the United States over the past four decades very clearly 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, postincapacitation age crime-reducing effect. These combine to generate sizable crime reductions from school dropout age policy reforms.² On the basis of empirical tests that decompose short- and long-run effects, the observed longer-run effect for the post-1980 education reforms is primarily attributed to dynamic incapacitation.

¹ Such law changes have been studied in a range of settings to show that a beneficial unintended consequence of them is reduced criminality—see Lochner and Moretti (2004), Machin, Marie, and Vujic (2011), Hjalmarsson, Holmlund, and Lindquist (2015), and Bell, Costa, and Machin (2016), among others. The latter paper was pretty much a replication of Lochner and Moretti (2004) studying more recent dropout age reforms. While this paper uses an update of data used there, the focus is very much on pushing the study of crime-reducing effect of education in a number of novel directions, including developing a new empirical framework based on crime-age profiles, pinning down better estimates, and looking closely at the reasons why dropout age reforms embodied in CSLs reduce crime.

² 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 on Danish register data, is by Landersø, Nielsen, and Simonsen (2017), who studied the crime impact of reforming the age of school entry.

The rest of the paper is structured as follows. Section II first discusses crime-age profiles and then outlines a framework where changes in school leaving ages have scope to shift and alter the shape and structure of crime-age profiles. This is then discussed in the context of existing research. Section III describes the data, offers some initial descriptive analysis of compulsory school leaving laws, and presents the research design used in the empirical work contained here. Section IV reports the main results on the impact of dropout age reforms on crime-age profiles. Section V provides further discussion and examines evidence on the mechanisms by which dropout reforms reduce criminality. Section VI offers conclusions.

II. Theoretical Considerations and Existing Research

A. *Crime-Age Profiles*

The crime-age profile is a well-established empirical regularity. Almost 200 years ago, Adolphe Quetelet presented evidence that crime in early-nineteenth-century France peaked when individuals were in their late teens (Quetelet 1831 [1984]). 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.³

Figure 1 shows this for US males using arrest rates, with a peak rate at age 18 and declines thereafter. In a well-known study, Hirschi and Gottfredson (1983) conjecture that crime-age profiles are broadly invariant over time and space. They suggest criminals can be identified by their lack of self-control, which is determined well before adolescence and 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 (1983) suggest that offenders burn out over time—via 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, specialization, and desistance that occur as individuals get older.⁴

³ Sullivan (2012) offers a theoretical review, and Siennick and Osgood (2008) present a review of empirical work and findings.

⁴ A large body of evidence in criminology has studied these issues (e.g., Greenberg [1985] presents evidence that peak crime age and the subsequent decline differs across crime types, localities, race, and gender, while Hansen [2003] shows that crime-age profile differs with education; for further discussion, see Cohen and Vila [1996] or the meta study of Pratt and Cullen [2000]). In economics, Grogger (1998) examines how changing returns to legal

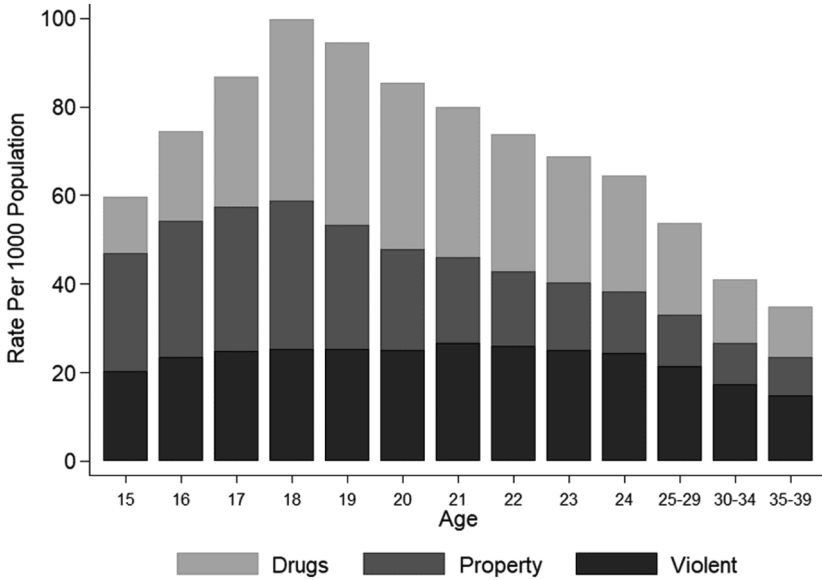


FIG. 1.—US male arrest rates by age, calculated for the years 2000–2010 from Uniform Crime Reporting data. Only agencies reporting all years of the time period covered are included. The composition of the different types of crime is covered in appendix A (apps. A–C are available online).

B. *Economic Models of Education Policy and Crime-Age Profiles*

Since Becker (1968) formalized the economic approach to studying criminal behavior, a variety of models have been developed. Work by Ehrlich (1973), Witte (1980), and Witte and Tauchen (1994) thinks of engagement in crime as an allocation of time decision. More recently, dynamic aspects have been introduced to more clearly represent real-world life-course profiles of crime. The notion that criminal capital is a substitute for human capital, which can improve an individual's prospects in the crime market vis-à-vis the labor market, has been a central feature (see, e.g., Lochner 2004; Mocan, Billups, and Overland 2005).

How can crime-age profiles be shifted by changes in the mandatory dropout age? An optimizing dynamic framework where crime participation alters as individuals grow older can frame a way to think about this.⁵ At a given age, individuals choose how to allocate time between the legal

activity can affect the shape of the crime-age profile, and Lochner (2004) uses a human-capital model to show that crime should peak at around the time of labor market entry.

⁵ Appendix B presents a formal model that incorporates a simple dynamic feature into the basic time allocation structure of Witte and Tauchen's (1994) framework.

and illegal sectors, depending on the relative returns in each sector. But schooling constrains the amount of time individuals can allocate to either activity when they are aged below the compulsory school leaving age.

A key feature, therefore, is that while younger individuals may commit some crime, because they are kept in school, there is 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.⁶ An increase in the mandatory dropout age will reduce the crime rate among those directly incapacitated in school as a result of the reform. Once the individual reaches the new, higher dropout age, the incapacitation effect will vanish, and if direct incapacitation is the only factor at work, then a higher dropout age alters the crime-age profile for individuals of age less than or equal to the dropout age but exerts no effect for those aged above the new dropout age.

However, a dynamic framework enables an additional effect from incapacitation, which we term dynamic incapacitation. This occurs when the direct incapacitation from being kept in the school classroom causes changes that also affect future crime participation, independent of whether there is any educational value to the incapacitation. For example, suppose 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 life as a law-abiding citizen. Put another way, some individuals' crime onset is stopped by incapacitation, and they never commit crime at a later age. For other individuals who may have already committed crime, the incapacitation reduces their crime intensity during the incapacitation period, and this persists as they get older—the reform acts to reduce their criminal capital accumulation as compared with the counterfactual of no reform. Lochner and Moretti (2004) describe this as follows: “It is possible that criminal behavior is characterized by strong state dependence, so that the probability of committing crime today depends on the amount of crime committed in the past. By keeping youth off the street and occupied during the day, school attendance may have long-lasting effects on criminal participation” (158).

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

⁶ While we are focusing here on individual's allocation of time independently of other individuals (peers), work by Patacchini and Zenou (2009), Deming (2011), Billings, Deming, and Rockoff (2014), and Billings, Deming, and Ross (2019) has shown evidence that peers affect the crime behavior of juveniles in school. In the context of the analysis present in this paper, peer effects would act to reinforce the effect of compulsory school laws on individuals of the same or similar age.

in a recession can significantly increase the affected cohorts' arrest rates well into adult life, and 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 a finding of dynamic incapacitation effects. Such dynamic incapacitation would be expected to shift the entire crime-age profile down, though we would expect the declines to be smaller than for the age groups additionally directly incapacitated.

Of course, the other source of crime reduction for individuals older than the dropout age is the focus of most of the existing literature. The extra schooling acquired by being made to stay on to an older age potentially increases the human capital of the individual and, in doing so, raises the returns to legal activity. Indeed, the key contribution of much of the compulsory school leaving literature has been to focus on the causal effect of education on wages. This productivity-enhancing effect of changes in the mandatory dropout age alters the crime-age profile in a substantially different way to incapacitation effects. There will be limited or no change in the crime-age profile for those of school age since the educational attainment will not have been completed at that point and the productivity effects on wages and employment will not be evident. However, looking at older individuals affected by the reform, there should be a shift down in the crime-age profile as the relative returns to legal activity rise.

To summarize, the discussion above suggests the following effects on crime-age profiles of a rise in the mandatory dropout age.

1. *Direct incapacitation.* A drop in the crime-age profile for individuals aged below the new dropout age and no change in the profile for those aged above.
2. *Dynamic incapacitation.* A drop in the crime-age profile for all individuals.
3. *Educational improvement.* The direct incapacitation raises education and induces a productivity-related fall in crime for those aged above the dropout age.

How the three competing channels can be distinguished empirically is further detailed in the analysis, which allows for state dependence in crime, presented in section V.

C. *Connections to Existing Research and Modeling Approach*

This more general structure relates to existing research on crime and education. To date, the impact of CSL laws on the age structure of crime features in two strands of crime economics research. The first of these

argues that the crime reduction from CSL law changes reflect an incapacitation effect that keeps children in the classroom to an older age (and so off the streets not committing crime); see Anderson (2014) for US research on this. Other studies of juvenile crime by Jacob and Lefgren (2003) and Luallen (2006) look at teacher strikes and calendar year changes, respectively, to show that changes in the requirement to be in school on a particular day have effects on crime that can plausibly be considered as incapacitation.

A second strand asks the question whether extra time spent in the education system induced by CSL law changes has a longer-term effect on an individual's productivity. The extra education can enhance future labor market prospects and so deterring individuals affected by the policy change from entering a life of crime. Indeed, 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, Marie, and Vujic 2011).

Most existing research has focused on one or the other of these by separately studying either direct incapacitation effects or longer-term effects. In this paper, we look at both in a unifying framework and draw implications from the findings about the means by which education reduces crime. In practice, this is done by developing a research design focusing in detail on the way in which CSL law changes alter the shape and structure of crime-age profiles. It directly tests whether crime-age profiles adapt in the face of policy-induced changes in the compulsory school leaving age. This more flexible specification of the crime reduced form than has generally been used by researchers in the causal crime literature is modified to study the changing nature of crime-age profiles in a multiple regression discontinuity framework studying state-level changes in the compulsory school leaving age.

III. Data Description and Empirical Approach

A. Arrest Data

The crime data used in the analysis are 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 onward. As most crime is committed by men and at younger ages and as the compulsory school laws also apply to these ages, we choose to conduct our analysis on males aged 15–24 years old. For this age range, arrests are reported by single year of age.

For the purpose of the analysis, the geographical level of aggregation is the county (e.g., as in Anderson 2014). Since the focus is on studying

reforms that occur at the state level, it would be legitimate to ask why county-level data are being used. The reason is the substantial nonreporting of arrests by individual agencies to the UCR, which changes over time and across states. To generate annual state-level arrest data, therefore, requires some method of imputation.⁷ To give the most extreme example, consider the CSL reform in Illinois that became effective from 2006 and increased the compulsory attendance age to 17. If a 5-year window is used around the reform, only 1 of the 102 counties in Illinois consistently reports arrest data every year—fortunately, at least, it is Cook County for Chicago, but this example makes clear the issue that arises.⁸

Because the focus is on exploiting the discontinuity induced by dropout reform across birth cohorts in a short window, this very much requires consistent data to be available both pre- and postreform, and so imputation could prove problematic. Therefore, all reporting agencies within a county are aggregated, and the county is included only if its agencies are included in the aggregation report for all relevant years (or, at most, miss 1 year) around the reform window. Table A2 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.⁹

B. Compulsory Schooling Laws

The compulsory schooling laws used by Bell, Costa, and Machin (2016) have been updated for the current analysis. Measurement and definitions are important because over time in empirical research using CSLs, the choice of how to measure the binding compulsory school age has been open to a lot of scrutiny and some disagreement. For example, Stephens and Yang (2014) propose a refined version of the Goldin and Katz (2008) measurement combining start age, dropout age, grade requirement, and child labor laws, whereas Oreopoulos (2009) and Anderson (2014) focus

⁷ One alternative approach is to use only the yearly observations on state-level arrest data when at least a minimum, e.g., 95%, of the state population is reported on by the relevant agencies (see Bell, Bindler, and Machin 2018). But this generates an unbalanced panel and is therefore not appropriate within the framework adopted here.

⁸ Another important point about reporting is the monthly coverage. We have considered a sample that includes only those reporting agencies that provide either a full set of 12 monthly observations in a given year or provide an annual/biannual count (i.e., some reporting agencies, e.g., Chicago, simply provide an annual total each year rather than monthly submissions). Second, we consider only those agencies that provide the full set of 12 monthly observations and remove those that provide just an annual figure. These two alternatives are presented in table A1 (tables A1–A10, C1, C2 are available online). The results are robust to these alternative samples.

⁹ 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.

only on the dropout age enacted in the laws. Moreover, it is important to take account of grade exemptions, as they often make up part of recent laws. Therefore, 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

$$DA_{s,(t-a)} = \min\{\text{Dropout Age}_{s,t(t-a)}, \text{Grade Required to Dropout}_{s,(t-a)}\}. \tag{1}$$

Figure 2 maps how changes in the dropout age enacted between 1980 and 2010 occurred between different American states. The map makes clear that some regions—such as West South Central (Arkansas, Texas,

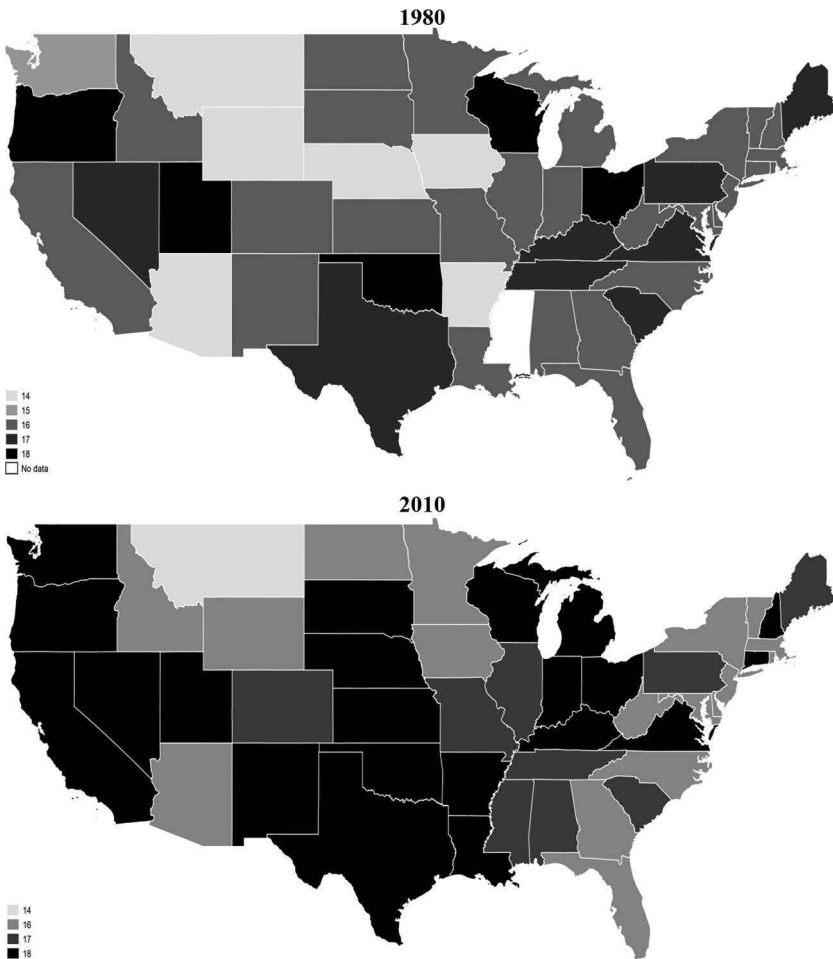


FIG. 2.—State dropout ages from 1980 to 2010, defined in equation (1).

and Louisiana) and the West Pacific (California and Washington)—have been more active over this period in introducing legislative changes.

Defining the precise initial cohort that is affected by these changes in compulsory schooling laws is not always as mechanical as subtracting the new dropout age from the year the law was enacted. 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.¹⁰

Table 1 lists the 30 laws between 1980 and 2010 that are studied in the empirical analysis, 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.¹¹

C. Research Design

Crime evolution is studied in settings of before-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

$$\text{Arrest}_{acst} = \beta \text{Reform}_{s(t-a)} + \gamma X_{acst} + \alpha_a + \alpha_c + \alpha_t + \varepsilon_{acst}, \quad (2)$$

where Arrest is the log arrest rate; Reform is a dummy variable (to begin with) indicating whether there was a dropout age reform affecting birth cohort $(t - a)$ in state s ; X is a set of county-level controls; 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.

The equation (2) crime reduced form is essentially the one that has been estimated in much of the existing work examining the causal impact of schooling laws by pooling together data across states that did and did not change their schooling laws over time. This has been done for a number of outcomes of interest: for wages, see, for example, Acemoglu and Angrist (2001) and Oreopoulos (2009); for crime, see Lochner and Moretti (2004) and Bell, Costa, and Machin (2016); and for a range of outcomes probing robustness of the approach in detail, see Stephens and Yang (2014).

¹⁰ When the time lapse between enactment and effective date of the law is more than 9 months, the changes have been cross-validated empirically by analyzing the data around the potential discontinuity to assert the binding date and cohorts affected.

¹¹ Table A3 offers more details on the 30 laws studied in the paper, e.g., including information on other exemptions on parental consents and employment. The results reported below remain robust to a variety of experiments that limit the sample to laws without exemptions; these are reported in table A4.

TABLE 1
STATE DROPOUT AGE REFORMS

State	Effective School Year from Statute	Type	Change	New Dropout Age
Arizona	1986	Exemption	Grades 8 to 10	16
Arkansas	1981	Leaving age	Ages 16 to 17	17
Arkansas	1991	Leaving age	Ages 17 to 18	18
California	1988	Leaving age	Ages 16 to 18	18
Colorado	2008	Leaving age	Ages 16 to 17	17
Connecticut	2002	Leaving age	Ages 16 to 18	18
Illinois	2005	Leaving age	Ages 16 to 18	17
Indiana	1989	Leaving age	Ages 16 to 17	17
Indiana	1992	Leaving age	Ages 17 to 18	18
Iowa	1992	Exemption	Grades 8 to 12	16
Kentucky	1984	Leaving age	Ages 17 to 18	18
Louisiana	1988	Leaving age	Ages 16 to 17	17
Louisiana	2002	Leaving age	Ages 17 to 18	18
Maine	1980	Exemption	Grades 9 to 12	17
Michigan	1997	Exemption	To grade 12	16
Michigan	2010	Leaving age	Ages 16 to 18	18
Mississippi	1984	Leaving age	Reenactment	17
Missouri	2010	Leaving age	Ages 16 to 17	17
Nebraska	2006	Leaving age	Ages 16 to 18	18
Nevada	2008	Leaving age	Ages 17 to 18	18
New Hampshire	2010	Leaving age	Ages 16 to 18	18
New Mexico	1981	Exemption	Grades 10 to 12	18
Rhode Island	2003	Exemption	To grade 12	16
South Dakota	2010	Leaving age	Ages 16 to 18	18
Texas	1985	Leaving age	Rewriting of law	16
Texas	1990	Leaving age	Ages 16 to 17	17
Texas	1998	Leaving age	Ages 17 to 18	18
Virginia	1991	Leaving age	Ages 17 to 18	18
Washington	1997	Exemption	Grades 9 to 12	18
Wyoming	1999	Exemption	Grades 8 to 10	16

NOTE.—Mississippi abolished its compulsory school law in 1956 and reenacted it in 1983–84 with an initial leaving age of 7 and progressive rise to 17 by the 1989–90 school year. Texas has written its laws of 1984 and 1989 in a different way, stating that the minimum leaving age was to include the completion of the 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.

Estimates are first presented in this way for comparison, but after that each of the reforms listed in table 1 is set up as a separate regression discontinuity (RD) around which what happens to crime before and after the reform takes place can be studied. To motivate the RD analysis, figure 3 shows the discontinuity for the arrest rate for the 30 pooled reforms (centred at $t = 0$). It shows a significant reduction in the arrest rate of 4 arrests per 1,000 population (or 4.6% of the prereform mean of 0.086) relative to the earlier cohorts who were unaffected by the reform.

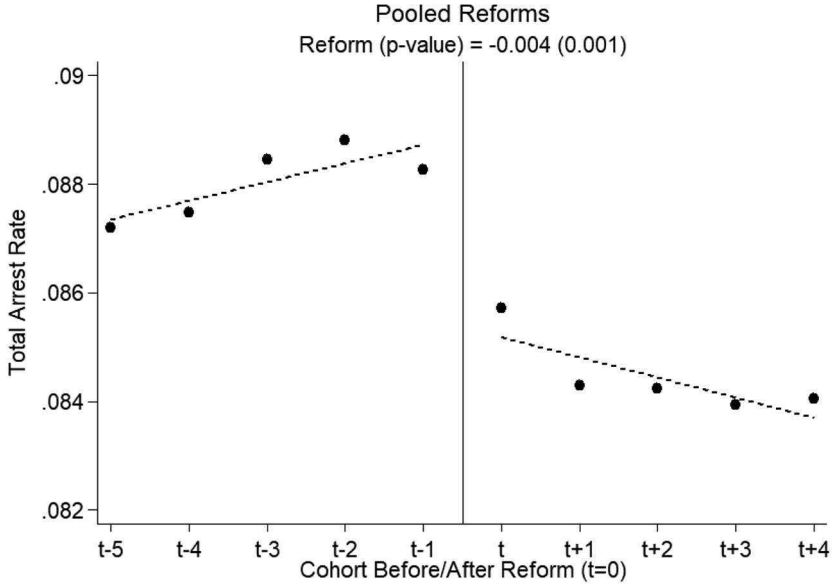


FIG. 3.—Arrest rates before/after reforms. The reported discontinuity estimate (with associated standard error in parentheses) is the ± 5 -year mean difference pre- and postreform for total arrest rate adjusted for population.

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

$$\text{Arrest}_{acst} = \beta \text{Reform}_{s(t-a)} + f_s(t-a) + \gamma_s X_{acst} + \alpha_{sa} + \alpha_c + \alpha_{st} + \varepsilon_{acst} \quad (3)$$

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

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 $f_s(\cdot)$ 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, the RD design is further generalized to allow heterogeneity by age in the policy reform. This is precisely what the framework introduced in section II above argued needs to be done (*a*) to see 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, separate before/after policy effects in the crime reduced form can be estimated for each age group, so that a more general estimating equation follows:

$$\text{Arrest}_{acst} = \theta_a(\text{Reform}_{s(t-a)}) + f_s(t - a) + \gamma_s X_{acst} + \alpha_{sa} + \alpha_c + \alpha_{st} + \varepsilon_{acst}$$

$$\left. \frac{\partial \text{Arrest}_{acst}}{\partial \text{Age}_a} \right|_{a=j} = [\theta_j \times \text{Reform}_{s(t-a)}] + \alpha_{sj}, \quad (4)$$

where the partial derivative shows the impact of the reform for age j ($j = 15, 16, \dots, 24$).

D. Controls

A set of control measures are included in X that according to existing evidence (e.g., Card and Krueger 1992; Levitt 1997) 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 the county level using Common Core Data (CCD) data. Police numbers were recovered from the FBI Law Enforcement Officers Killed and Assaulted database, and sociodemographic indicators were collected from Local Area Personal Income data from the Bureau of Economic Analysis. More details are provided in appendix A.

IV. Crime-Age Profiles and Dropout Age

A. Baseline Estimates of Crime Reduced Forms

Although the primary focus of this 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 reforms that affect different age groups are presented. It turns out that the results are robust to allowing different types of reform to have separate estimates, and it is therefore more straightforward to start with presenting estimates for the weighted-average effect of all types of reforms, which is what is done in table 2. Standard errors are clustered at the reform-state level, which is the dimension along which each reform occurs. Given the potentially low numbers of clusters (30), clustered standard errors will likely be biased downward, and for this reason, we report

TABLE 2
 BASELINE ESTIMATES OF CRIME REDUCED FORMS: LOG(ARREST RATE), 1974–2015

	All States						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform	-.107 [-.225, .001]	-.047 [-.080, .001]	-.067 [-.094, -.030]	-.040 [-.067, -.011]	-.063 [-.087, -.044]	-.061 [-.076, -.046]	-.061 [-.074, -.047]
Running variable	.053	Linear × reform	Quadratic × reform	Cubic × reform	Linear × reform	Linear × reform	Local linear regression discontinuity
Reform interactions		X	X	X	X	X	X
Sample size	1,130,145	351,497	351,497	351,497	251,377	181,492	351,497
Number of states	48	24	24	24	24	24	24
Number of counties	3,056	1,260	1,260	1,260	1,260	1,260	1,260

NOTE.—Sample includes males in each 15–24 age group inclusive for US counties. Estimates are weighted by population size, and 95% confidence intervals (in brackets) and *p*-values (below) are clustered at state level (state-reform 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, and nonwhite/nonblack population. “Reform interactions” means every covariate is made state-reform specific by adding an interaction with the state-reform indicator. Columns 2–6 include a centered running variable interacted with the dropout reform indicator so as to allow differential trends at each side of the discontinuities. Column 7 uses a local linear estimator by Calonico, Cattaneo, and Titiunik (2014).

the 95% equal-tailed confidence interval and p -value for the null hypothesis using a wild bootstrap (Roodman et al. 2018). Various alternative clustering approaches such as state and state-cohort level were considered, but these standard errors were in all cases less conservative than those reported.

Implicit in the discussion thus far has been the assumption that each reform can be considered as exogenous to the parameters of interest. The crucial assumption is that school leaving reforms were 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 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 table A5, and there is no evidence to suggest any systematic pattern around the discontinuity. To further address concerns about possible endogeneity and potential confounders of the timing of reforms, evidence of the validity of the identification approach is offered from placebo tests for several prereform cohorts, as reported in table A6. All the different lagged cohorts used as placebos—where the lag is sufficiently long to ensure no contamination from the reform can occur—show small and insignificant estimates.

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 that by Lochner and Moretti (2004) and Bell, Costa, and Machin (2016) and given in equation (2). They do not explicitly take advantage of the discontinuity that each reform generates. The impact of the reform has an estimated coefficient of -0.107 , with an associated p -value of .054, and as such shows a strong crime-reducing effect from higher dropout ages.¹²

The preferred estimates are those in the subsequent columns of table 2 that are equivalent to equation (3) and that 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 either side of the reform. It

¹² We have also estimated the col. 1, table 2, specification allowing for quadratic or cubic terms in the running variable, and these produce coefficients very similar to the -0.107 reported in col. 1, to be precise, -0.094 and -0.095 , respectively.

matters little what the functional form for the running variable is, so the subsequent analysis proceeds using a simple linear function.¹³

The discontinuity estimates are roughly half the size of the estimates presented in column 1 but remain strongly significant. In columns 5 and 6, results are reported with a narrower window around the discontinuity.¹⁴ Again, there is not much to choose between these various specifications, so the analysis now proceeds with a 5-year window on the basis that this more tightly focuses on the discontinuity.¹⁵ This estimate shows a 6.1% fall in log arrest rates for these young adults as a result of the dropout reform.

In the final column of the table, estimates come from a local linear regression approach as the use of polynomials for the running variable can yield biased estimates of the treatment effect. The approach of Calonico, Cattaneo, and Titiunik (2014) is used and focuses on a 10-year window given the additional demands of local nonparametric estimators. The estimated effect is almost precisely the same as in the other columns of the table.

B. 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, the reforms are divided into two groups, with a roughly equal number of reforms in each group to maintain adequate variation. The grouping of the reforms is by whether the increased school leaving age remains

¹³ This is what Gelman and Imbens (2019) strongly recommend, although all subsequent results do prove robust to using a quadratic or cubic function for the running variable, bearing in mind that the nonlinear functions are computationally feasible only for the longer windows around the discontinuity.

¹⁴ The narrowing of the window around the discontinuity addresses a potential concern as well about spillover effects in states with multiple reforms close to each other in time. Further robustness analysis with narrower windows and excluding states with multiple reforms shows results similar to those reported in the paper. The additional robustness analyses are available on request.

¹⁵ 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 A5 that are supportive of this assumption, but we recognize that this is far from an 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 difference-in-differences approach. For each reform, we consider all other states as potential controls and use a 5-year window prior to the reform to generate a synthetic control. Consider, e.g., the reform in California that raised the leaving age in 1988. We use the average arrest rate for 15–24-year-olds from 1983 to 1987 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 prereform period. If we reestimate col. 6 of table 2 using this approach, we obtain a coefficient estimate (and associated standard error) on the reform of -0.040 (0.007).

TABLE 3
ESTIMATES BY REFORM TYPE

LOG(ARREST RATE), 1974–2015, DISCONTINUITY (± 5 YEARS) SAMPLE, ALL AGE-INCREASE REFORMS			
	All Age-Increase Reforms (1)	Below 18 (2)	18 (3)
A. Overall Reform Effect			
Reform	-.063 [-.079, -.047] .000	-.062 [-.071, -.043] .000	-.064 [-.092, -.040] .009
Joint test (2) = (3) <i>p</i> -value: .865			
B. Reform Effects by Broad Age Groups			
Reform \times age 15–18	-.069 [-.084, -.057] .000	-.070 [-.092, -.046] .000	-.069 [-.102, -.052] .002
Joint test (2) = (3) <i>p</i> -value = .965			
Reform \times age 19–24	-.040 [-.061, -.025] .000	-.038 [-.067, -.019] .008	-.042 [-.067, -.020] .021
Joint test (2) = (3) <i>p</i> -value = .817			
Sample size	159,552	73,361	86,191
Number of states	24	14	15
Number of counties	1,256	787	908

NOTE.—See table 2 notes; same specification as col. 6 of table 2. Each column shows separate regressions according to the relevant reform sample. “Other” includes reforms resulting from grade exemption changes in Arizona (1986), Iowa (1992), Maine (1980), Michigan (1997), New Mexico (1981), Rhode Island (2003), Washington (1997), and Wyoming (1999) and the reenactment of law in Mississippi (1984).

below 18 (col. 2) or reaches 18 (col. 3).¹⁶ The table 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 overall effect across the two different groups of reforms proves to be similar in magnitude, and the hypothesis that they are equal cannot be rejected. However, focusing on the age groups, in all cases, the effect is larger for those contemporaneously affected by the reforms (i.e., in the younger 15–18 age range) than for those who were affected in the past. For the 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 .024. However, this latter group still experiences a significantly lower arrest rate as a result of the reform that

¹⁶ Table A7 contains estimates for five more finely defined 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.

they were subject to when at school. Again, there is very little difference in the effect on different age groups depending on whether the reform raised the leaving age to 18 or below. Overall, these results do not point to substantial heterogeneity across the reforms, which in any case is primarily only a difference of 1 year in the mandated leaving age. It should be remembered that these reforms are potentially very different from those considered by Lochner and Moretti (2004), which affected significantly younger cohorts in earlier time periods.¹⁷

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 A9, but it is easier to visualize the various estimates as they are presented in figure 4. Each point represents a separate reform labeled along the horizontal axis, and 95% confidence bands for each estimate are shown. Only 1 of the 30 reforms generates a significantly positive effect on arrest rates—the 1985 Texas reform. Of the other 29 reforms, 15 are significantly negative, and all but 4 have a negative estimate.

C. *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).¹⁸ It also again presents estimates that differ by two broad age groups (15–18 and 19–24). 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. This is particularly noticeable for those contemporaneously affected by the reform, as the drug arrest rate drops by almost 13%, compared with 7% for all types of arrest.

D. *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

¹⁷ We have also reestimated table 3 using the arrest rate in levels as the dependent variable rather than the log. Table A8 shows these estimates, again showing a clear pattern of larger reductions in arrests for the 15–18-year-olds together with a significant crime-reducing effect for the 19–24-year-olds.

¹⁸ For the remainder of the empirical analysis, the focus is placed only on the 29 dropout age increases, excluding the Texas decrease. Results are, however, robust to including the decrease.

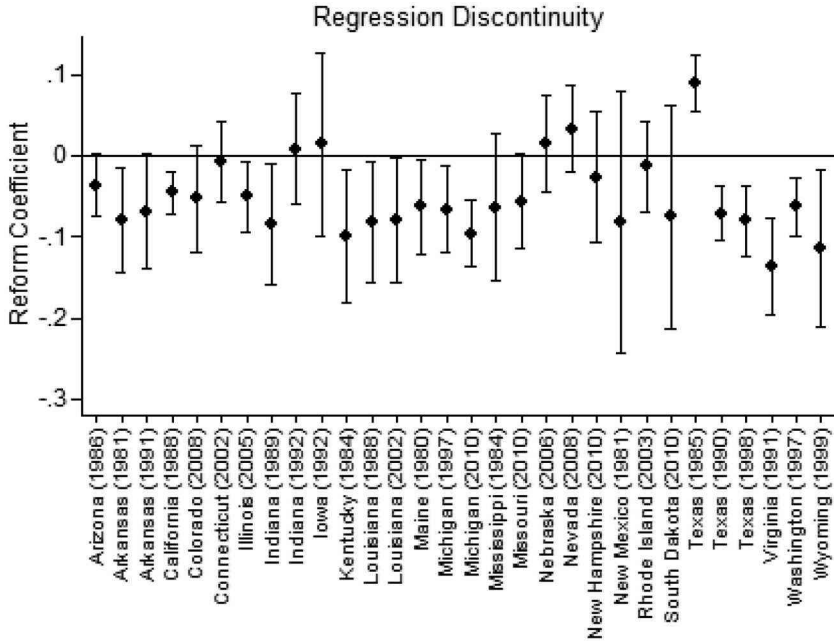


FIG. 4.—Estimated discontinuity coefficients. Coefficients are from table A8; shown are 95% confidence intervals.

TABLE 4
ESTIMATES BY CRIME TYPE AND AGE

	LOG(ARREST RATE), 1974–2015, DISCONTINUITY (± 5 YEARS) SAMPLE, ALL AGE-INCREASE REFORMS			
	Total (1)	Violent (2)	Property (3)	Drugs (4)
A. Overall Reform Effect				
Reform	-.063 [-.079, -.047] .000	-.052 [-.079, -.023] .000	-.053 [-.075, -.038] .000	-.101 [-.115, -.077] .000
B. Reform Effects by Broad Age Groups				
Reform \times age 15–18	-.069 [-.084, -.057] .000	-.055 [-.084, -.028] .002	-.056 [-.088, -.031] .000	-.129 [-.168, -.082] .000
Reform \times age 19–24	-.040 [-.061, -.025] .000	-.041 [-.069, -.014] .000	-.040 [-.073, -.014] .005	-.045 [-.071, -.018] .003
Sample size	159,552	159,552	159,552	159,552
Number of states	24	24	24	24
Number of counties	1,256	1,256	1,256	1,256

NOTE.—See table 2 notes; same specification as col. 6 of table 2. The sample excludes the Texas (1985) reform, as it saw a decrease in compulsory schooling.

is generalized to have different reform effects at each single age—corresponding to equation (3). This then allows examination of the key question of this paper: Can policy reforms alter the entire shape of the crime-age profile?

Consistent with the theoretical discussion presented in section II, 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.

TABLE 5
AGE-VARYING REFORM IMPACTS

	LOG(ARREST RATE), 1974–2015, DISCONTINUITY (± 5 YEARS) SAMPLE, ALL AGE-INCREASE REFORMS			
	Total (1)	Violent (2)	Property (3)	Drugs (4)
Reform \times age = 15	-.107 [-.127, -.085]	-.054 [-.083, -.022]	-.082 [-.119, -.051]	-.224 [-.307, -.132]
	.000	.007	.000	.000
Reform \times age = 16	-.099 [-.122, -.073]	-.044 [-.090, -.002]	-.075 [-.103, -.054]	-.185 [-.250, -.112]
	.000	.043	.000	.000
Reform \times age = 17	-.056 [-.088, -.027]	-.045 [-.083, -.015]	-.038 [-.086, .001]	-.130 [-.166, -.086]
	.000	.003	.053	.000
Reform \times age = 18	-.034 [-.054, -.021]	-.073 [-.111, -.028]	-.022 [-.044, -.008]	-.020 [-.052, .008]
	.000	.000	.001	.170
Reform \times age = 19	-.046 [-.065, -.033]	-.088 [-.144, -.039]	-.034 [-.066, -.010]	-.037 [-.081, -.003]
	.000	.000	.001	.030
Reform \times age = 20	-.059 [-.077, -.042]	-.093 [-.140, -.058]	-.052 [-.080, -.033]	-.057 [-.100, -.015]
	.000	.000	.001	.008
Reform \times age = 21	-.059 [-.079, -.042]	-.055 [-.083, -.017]	-.063 [-.096, -.039]	-.079 [-.114, -.048]
	.000	.010	.003	.002
Reform \times age = 22	-.046 [-.071, -.022]	-.028 [-.077, .013]	-.045 [-.080, -.016]	-.080 [-.107, -.053]
	.003	.183	.015	.000
Reform \times age = 23	-.054 [-.090, -.014]	-.010 [-.074, .038]	-.051 [-.091, -.010]	-.075 [-.096, -.041]
	.002	.751	.017	.001
Reform \times age = 24	-.047 [-.097, .006]	.001 [-.065, .058]	-.054 [-.137, .009]	-.067 [-.105, -.020]
	.101	.988	.099	.004
Sample size	159,552	159,552	159,552	159,552
Number of states	24	24	24	24
Number of counties	1,256	1,256	1,256	1,256

NOTE.—See table 2 notes; same specification as col. 6 of table 2. The sample excludes the Texas (1985) reform, as it saw a decrease in compulsory schooling.

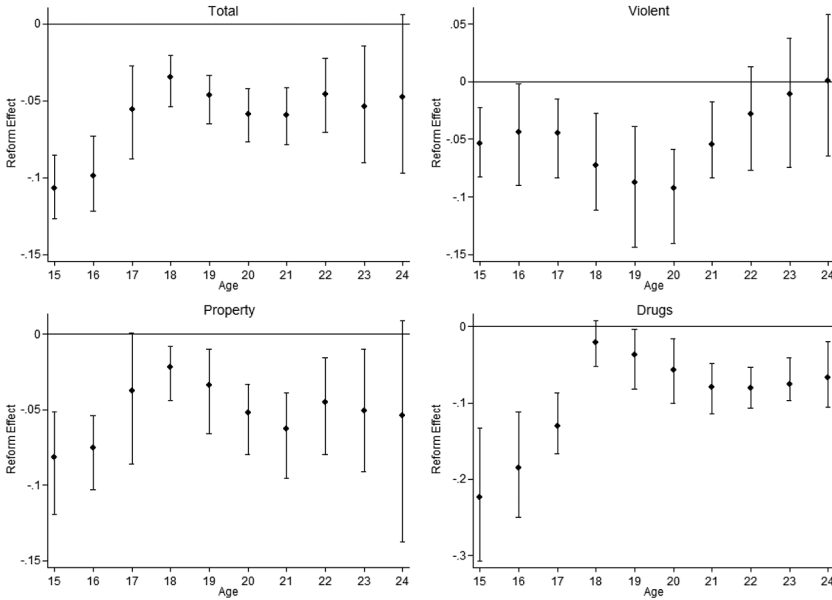


FIG. 5.—Discontinuity estimates by age and crime type, from estimates in table 5. Texas (1985) is excluded from the estimation given that it showed a decrease in dropout age. Bootstrapped confidence intervals at 95% significance are clustered at the state-reform level.

Figure 5 shows the estimates, with 95% confidence bands, for each crime type. To highlight the effect on the crime-age profile overall, figure 6 shows the estimated profiles pre- and postreform 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 men are locked up in school—and a longer-term crime-reducing effect.

Closer inspection of figure 6 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 postincapacitation ages. For violent crimes, the opposite holds: little in the way of incapacitation but some crime reduction at older ages.

V. Mechanisms and Discussion

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 behavior is directly impacted by the policy and in

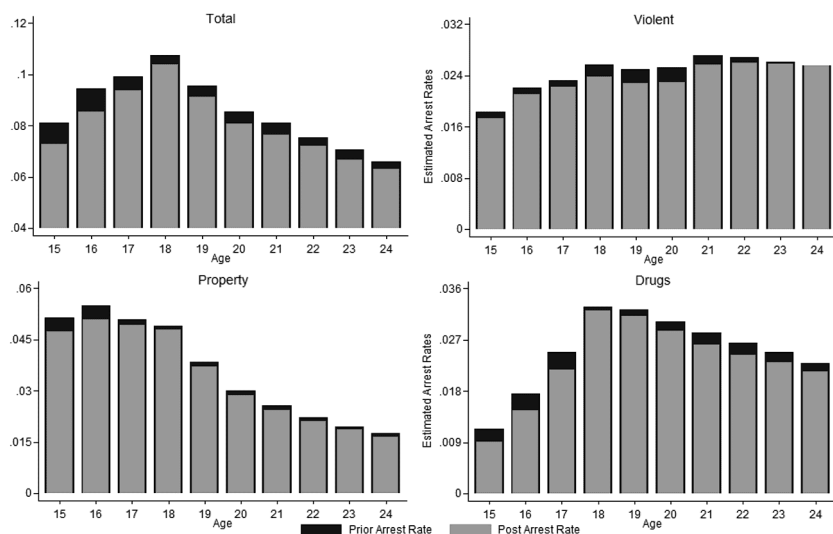


FIG. 6.—Crime-age profile shifts by crime type. Prior arrest rate is the mean of arrest rate by age prior to discontinuity using a 5-year bandwidth. The postarrest rate is calculated using the estimated age effects from figure 5.

subsequent years when they are not. The former effect is likely to be a result of incapacitation—when a young person is constrained to remain in school, they have less free time to allocate to crime. In this section, in line with the earlier discussion in section II, competing mechanisms that explain the latter longer-run effect are considered.

A. Education and Employment Outcomes

There is by now a large literature that examines the causal effect of education on crime.¹⁹ A natural interpretation of the dropout reform reducing criminality is that, in addition to the 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 behavior. This then raises their human capital, wages, and employment and reduces the probability they will commit crime in the future. This would be consistent with the theoretical discussion in section II and with the earlier US research studying the impact of the earlier compulsory school leaving reforms up to the 1980s.²⁰

¹⁹ Many of these studies were cited earlier, but see also the review by Lochner (2011).

²⁰ For crime, see Lochner and Moretti (2004). For reviews of the sizable bodies of research on wage effects, see Card (1999) and Oreopoulos (2009). For a host of other non-wage outcomes variables, including health, voting behavior, and life satisfaction, see Oreopoulos and Salvanes (2011).

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 utilizing Current Population Survey (CPS) data on 16–18-year-olds between 1974 and 2015 (see the appendix for more details). Panel A of table 6 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 5 percentage point rise in school attendance, or a 6.3% increase relative to the prereform 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 onward.²¹ The outcomes are high school dropout rates, whether an individual was in education or work, and log weekly real wages. While there are statistically significant effects in the expected direction for a few 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 of being in school or work, but the effects are small—relative to the prereform mean, they respectively correspond to a 4.6% fall in high school dropout and a 0.04% increase in the likelihood of being in education or work. Unlike in the previous work on earlier reforms (e.g., Card 1999; Acemoglu and Angrist 2001), there is essentially no effect on wages in any specification.²²

The positive effects of the reforms on economic and education outcomes are therefore modest, certainly in comparison with Lochner and Moretti (2004), who find education estimates that are quite a lot bigger than those reported in table 6. Lochner and Moretti argue in their context that the predicted increase in wages that resulted from the rising high school graduation rates caused by the education reforms combined with estimates of the elasticity of arrests with respect to wages can potentially explain the entire reduction in crime rates for those aged 20–59 caused by the reforms. Because we find no identifiable effect of the reforms on wages, we would obviously predict no effect on postschooling crime rates through this channel. Our previous work (Bell, Costa, and Machin 2016) has demonstrated that the most recent reforms to compulsory schooling laws have substantially weaker effects on educational attainment than

²¹ ACS data are used because they are annual data that can be used to study the reforms across pooled birth cohorts. See app. A for more details.

²² Lack of a wage effect from dropout age reforms is not unique to this paper. Pischke and Von Wachter (2008), e.g., report no wage gains from German compulsory school leaving age reforms.

TABLE 6
ESTIMATES FOR HIGH SCHOOL ATTENDANCE, EDUCATION, EMPLOYMENT, AND WAGES

	Prereform Mean	All States (1)	10-Year Window (2)	7-Year Window (3)	5-Year Window (4)
A. High School Attendance (16-18)					
Reform	.744	.010 [−.006, .028] .202	.038 [.019, .054] .000	.040 [.026, .056] .000	.049 [.035, .066] .000
B. High School Dropout					
Reform	.093	−.007 [−.018, .002] .131	−.005 [−.008, −.002] .004	−.005 [−.008, −.002] .002	−.004 [−.008, −.001] .023
C. School or Work					
Reform	.812	.010 [.004, .014] .002	.006 [−.002, .012] .113	.006 [.000, .011] .038	.003 [−.003, .010] .309
D. Log Weekly Real Wages					
Reform	6.769	.010 [−.011, .039] .388	.005 [−.005, .013] .288	.007 [−.003, .015] .145	.004 [−.002, .010] .128
Running variable			Linear × reform	Linear × reform	Linear × reform
Reform interactions			X	X	X
Sample size:					
Panel A		1,026,804	254,617	182,279	132,017
Panels B and C		6,816,430	1,754,012	1,236,070	889,652
Panel D		4,854,245	1,297,395	916,684	659,575
Number of states:					
Panel A		41	17	17	17
Panels B–D		48	24	24	24

NOTE.—Current Population Survey basic monthly (panel A) includes all males, ages 16–18, from 1976 to 2015. Attendance in panel A is defined as an individual reporting to attend school full-time with education attainment lower than some college (see app. A). Panels B–D include US-born males in each age group 19–60 inclusive from the 2006–2015 American Community Survey. Estimates are weighted by population weights, and 95% confidence intervals (in brackets) and *p*-values (below) are clustered at the state level (state-reform level for discontinuity windows). The dependent variables are 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 panel A). “Reform interactions” means every covariate is made state-reform specific by adding an interaction with the state-reform indicator.

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—for example, those who obtain a high school diploma when the reform occurs who would not have done so previously—are a smaller percentage of the eligible population for the period studied in this paper.

This interpretation makes sense in this paper as the high school dropout rate for those aged 16–24 fell from 27.2% in 1960 in Lochner and Moretti's (2004) data to 5.9% 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 0.4 percentage point (4.6%) 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% reduction in arrest rates that we observe for 19–24-year-olds.

Overall, how should we interpret the magnitude of the arrest estimates given the estimated impact on schooling? Column 4 of table 6 shows that CSL reforms generate a 4.9 percentage point increase in high school attendance of 16–18-year-olds, on average, compared with a prereform mean of 74.4%. For the 6.8 million male 16–18-year-olds in the United States in 2010, this generates an attendance increase of an additional 332,000 students staying in school. Column 1 of table 4 reports a -0.069 RD estimate in the log total number of arrests equation for the 15–18-year-old age group, and a comparable RD estimate for the 16–18-year-old group is -0.076 . The UCR data for 2010 show that the total arrests for this age group was 403,000, so $29,524 (= [\exp(-0.076) - 1] \times 403,423)$ fewer arrests are made as a result of the CSL reform. Overall, this therefore implies that for every 100 students kept in school, there would be approximately nine fewer crimes. For property crimes only, the reduction would be around three crimes for every 100 students kept in school. By comparison, Anderson (2014) reports a comparable back-of-the-envelope calculation (based on high school dropout), concluding that there would be seven fewer property crimes per 100 students affected by changes in the minimum dropout age.

B. Dynamic Incapacitation

In light of the modest economic and education effects, the discussion of section II that highlighted the potential for dynamic incapacitation effects to explain the reduced criminality of post-dropout-age individuals requires further exploration. To provide direct evidence on this channel, prereform time periods are used to estimate the extent of the state dependence between crime rates at school age (ages 15–18) and crime rates postschooling (ages 19–24), hence ensuring the estimate will not be affected by the reform. Then a counterfactual calculation is undertaken that uses the estimated impact of the education reforms on the school-age crime rate to estimate what the implied change in the postschooling crime rate would have been if the only mechanism at work was the impact

on school-age crime rates with state dependence. This gives an estimate of the dynamic incapacitation effect, whose magnitude can be compared with the overall estimated impact on postschooling crime rates from the RD research design.

Formally, the extent of state dependence (pre- and postreform) can be estimated from the following dynamic regression at county-cohort level:

$$\text{Arrest}_{c,s,t-a}^{19-24,p} = \rho^p \times \text{Arrest}_{c,s,t-a}^{15-18,p} + \pi_s^p X_{c,s,t-a} + \delta_s^p + u_{c,s,t-a}^p, \quad (5)$$

$$p = \{\text{Pre, Post}\},$$

where Arrest is the log arrest rate, X is a set of county-level controls, δ , are state fixed effects, and u is the equation error term.

From the previous analysis, the overall estimated change in the post-schooling arrest rate as a result of the education reform is simply the RD estimate for the 19–24-year-olds, $\hat{\beta}^{19-24}$ (where a hat denotes an estimate). An estimate of the importance of the dynamic incapacitation effect in this overall crime reduction can then be calculated from combining the estimated RD change in the arrest rate for 15–18-year-olds, $\hat{\beta}^{15-18}$, and the estimated state dependence prereform, $\hat{\rho}^{\text{Pre}}$.

In terms of the earlier discussion on crime-reducing effects resulting from a rise in mandatory schooling described in section II, this empirical approach links to the key theoretical underpinnings in three possible ways.

1. *Direct incapacitation.* The $\hat{\beta}^{19-24}$ estimate would be close to zero. Additionally, the state dependence parameter should be lower in the postreform period ($\hat{\rho}^{\text{Post}} \leq \hat{\rho}^{\text{Pre}}$) since direct incapacitation reduces crime rates at younger ages, with crime rates at older ages unchanged (in the absence of any other channel).
2. *Dynamic incapacitation.* Crime reductions occur throughout the crime-age profile but are more pronounced at younger ages, so that $0 \geq \hat{\beta}^{19-24} \geq \hat{\beta}^{15-18}$. The extent of state dependence determines how the earlier-age and later-age crime reductions result from dynamic incapacitation as they remain unchanged as a result of the reform ($\hat{\rho}^{\text{Post}} = \hat{\rho}^{\text{Pre}}$).
3. *Educational improvement.* A more pronounced reduction of the crime-age profile arises at later ages $0 \geq \hat{\beta}^{15-18} \geq \hat{\beta}^{19-24}$ as a consequence of indirect effects of the extra human capital accumulation on employment and wage outcomes. The state dependence estimate would likely be reduced comparing pre- and postreform cohorts ($\hat{\rho}^{\text{Post}} \leq \hat{\rho}^{\text{Pre}}$) from this channel.

Table 7 reports the results of this exercise. Each column contains an alternative specification for the prereform state dependence regression.

TABLE 7
DYNAMIC INCAPACITATION

	LOG (ARREST RATE), 1974–2015, DISCONTINUITY (± 5 YEARS) SAMPLE, ALL AGE-INCREASE REFORMS				
Log Arrest Rate ^{19–24,Pre}	(1)	(2)	(3)	(4)	(5)
Log Arrest Rate ^{15–18,Pre} ($\hat{\rho}^{\text{Pre}}$)	.485 [.352, .628]	.396 [.260, .548]	.425 [.345, .525]	.297 [.228, .381]	.293 [.204, .387]
$\hat{\beta}^{15-18}$.000	.000	.000	.000	.000
$\hat{\beta}^{19-24}$	-.069	-.069	-.069	-.069	-.069
$[(\hat{\beta}^{15-18} \times \hat{\rho}^{\text{Pre}}) / \hat{\beta}^{19-24}] \times 100$	-.040	-.040	-.040	-.040	-.040
100	84	68	73	51	51
Demographics		X		X	
Reform fixed effect			X	X	X
Reform interactions					X
Sample size	7,777	7,777	7,777	7,777	7,777
Number of states	24	24	24	24	24
Number of counties	1,256	1,256	1,256	1,256	1,256

NOTE.—Estimates are weighted by population size, and 95% confidence intervals (in brackets) and p -values (below) are clustered at the state-reform level. The dependent variable is the county-reform log of mean total arrest rate, including violent, property, and drug crimes between the ages of 19 and 24 by prereform cohorts and analogously for ages 15–18 in case of the independent variable. Here, $\hat{\beta}^{15-18}$ and $\hat{\beta}^{19-24}$ correspond to the estimates of col. 1 of table 3. All specifications include log of population. Covariates further include log of police force sworn and shares of female, black, and nonwhite/nonblack population. “Reform interactions” means every covariate is made state-reform specific by adding an interaction with the state-reform indicator.

The first column has no controls and all reforms pooled (with a 5-year prereform window for each reform). We then progressively add demographics (log of population, log of police force sworn, and shares of female, black, nonwhite/nonblack population), state fixed effects, and interactions between state fixed effects and demographics. Additionally, the postreform state dependence estimates align closely with the magnitude of their prereform counterparts; estimates of $\hat{\rho}^{\text{Post}}$ range from 0.48 to 0.25, compared with 0.49 to 0.29 for $\hat{\rho}^{\text{Pre}}$.²³ The row labeled $[(\hat{\beta}^{15-18} \times \hat{\rho}^{\text{Pre}}) / \hat{\beta}^{19-24}] \times 100$ is the share expressed in percentage terms of the change in the estimated postreform postschooling arrest rate that is the result of dynamic incapacitation.

The results in the table show that the extent of state dependence declines as more controls are added. This also implies that the share of the crime reduction attributed to dynamic incapacitation also declines. Nonetheless, across all the reported specifications, dynamic incapacitation is a significant portion of the longer-run crime-reducing effect of

²³ We do not completely exclude a small educational improvement effect, which would be in line with the small reduction in the magnitude of state persistence estimates for pre- and postreform cohorts.

TABLE 8
DYNAMIC INCAPACITATION BY REFORM TYPE

	ARREST RATE, 1974–2015, DISCONTINUITY (± 5 YEARS) SAMPLE, ALL AGE-INCREASE REFORMS		
	All (1)	Below 18 (2)	18 (3)
Log Arrest Rate ^{19–24,Pre}			
Log Arrest Rate ^{15–18,Pre} ($\hat{\rho}^{\text{Pre}}$)	.293 [.204, .387]	.326 [.297, .391]	.270 [.147, .441]
$\hat{\beta}^{15–18}$.000	.000	.001
$\hat{\beta}^{19–24}$	–.069	–.070	–.069
$[(\hat{\beta}^{15–18} \times \hat{\rho}^{\text{Pre}}) / \hat{\beta}^{19–24}] \times 100$	–.040	–.038	–.042
Sample size	51	60	44
Number of states	7,777	3,460	4,317
Number of counties	24	14	15
	1,256	787	908

NOTE.—Estimates are weighted by population size, and 95% confidence intervals (in brackets) and p -values (below) are clustered at the state-reform level. The dependent variable is the county-reform log of mean total arrest rate including violent, property, and drug crimes between the ages of 19 and 24 by prereform cohorts and analogously for ages 15–18 in case of the independent variable. Here, $\hat{\beta}^{15–18}$ and $\hat{\beta}^{19–24}$ correspond to the estimates of cols. 2 and 3 of table 3. All specifications are according to col. 5 of table 7.

education. Depending on the exact specification used, it explains between 51% and 84% of the crime reduction among 19–24-year-olds.²⁴

Tables 8 and 9 push the dynamic incapacitation analysis further to look at the different age reforms and to consider possible spillovers across crime types. Table 8 reports separate estimates for the two different groups of reforms—those that raised the school leaving age to 17 or below and those that raised it to 18—showing that while there are clear dynamic incapacitation effects for both groups of reforms, there is a somewhat stronger effect for reforms that affect the younger ages (i.e., that do not raise the leaving age to 18). This is a result both of a slightly higher estimate of state dependence and a slightly lower reduction in crime after direct incapacitation. It should be noted, however, that the differences are reasonably small and not statistically significant.

Table 9 explores the dynamic incapacitation effect by crime type (violent, property, and drugs) to allow for spillover effects across crime types. The age 19–24 crime rate for a particular crime type is generalized to depend on the age 15–18 crime rate for that crime type and the age 15–18

²⁴ We also reestimated table 7 using the arrest rate as the dependent variable. Across the same specifications, dynamic incapacitation accounts for between 41% and 73% of the crime reduction among the 19–24-year-olds. Additional evidence based on longitudinal data from a different setting, in Queensland, Australia, is given in app. C. The Australian microdata analysis also produces crime reductions and permits the analysis of intensive and extensive crime participation by individuals, and the results using these data corroborate the finding of dynamic incapacitation underpinning education policy-induced crime reduction.

TABLE 9
DYNAMIC INCAPACITATION BY CRIME TYPE

	Log Violent Arrest Rate ^{19-24,Pre} (1)	Log Property Arrest Rate ^{19-24,Pre} (2)	Log Drug Arrest Rate ^{19-24,Pre} (3)
Log Violent Arrest Rate ^{15-18,Pre} ($\hat{\rho}^{\text{Viol,Pre}}$)	.237 [.185, .294] .000	.039 [.012, .069] .008	.010 [-.019, .038] .470
Log Property Arrest Rate ^{15-18,Pre} ($\hat{\rho}^{\text{Prop,Pre}}$)	.091 [.023, .147] .018	.292 [.232, .368] .000	.027 [-.090, .119] .642
Log Drug Arrest Rate ^{15-18,Pre} ($\hat{\rho}^{\text{Drug,Pre}}$)	.017 [-.020, .060] .341	.030 [-.001, .064] .055	.255 [.204, .326] .000
$\hat{\beta}^{15-18}$	-.055	-.056	-.129
$\hat{\beta}^{19-24}$	-.041	-.040	-.045
$[(\hat{\beta}^{15-18,\text{Viol}} \times \hat{\rho}^{\text{Viol,Pre}}) / \hat{\beta}^{19-24}] \times 100$	32	5	1
$[(\hat{\beta}^{15-18,\text{Prop}} \times \hat{\rho}^{\text{Prop,Pre}}) / \hat{\beta}^{19-24}] \times 100$	12	41	3
$[(\hat{\beta}^{15-18,\text{Drug}} \times \hat{\rho}^{\text{Drug,Pre}}) / \hat{\beta}^{19-24}] \times 100$	5	10	73
Sample size	7,777	7,777	7,777
Number of states	24	24	24
Number of counties	1,256	1,256	1,256

NOTE.—Estimates are weighted by population size, and 95% confidence intervals (in brackets) and p -values (below) are clustered at state-reform level. The dependent variable is the county-reform log of mean arrest rate for each type of crime (violent, property, and drug) between the ages of 19 and 24 by prereform cohorts and analogously for ages 15–18 in case of the independent variable. Here, $\hat{\beta}^{15-18}$ and $\hat{\beta}^{19-24}$ correspond to the estimates of cols. 2–4 of table 4. All specifications are according to col. 5 of table 7.

crime rate for the other crime types. This allows a spillover from youth crime to adult crime across crime types. The dynamic incapacitation effect allowing for these spillovers can then be estimated. Overall, the own-crime type is always the dominant driver of dynamic incapacitation. However, there are differences in the extent of spillover across crime type. The dynamic incapacitation effect is stronger for drug crime, with very small spillover effects onto violent or property crime at an older age, suggesting crime specialization, and weakest for violent crime, where crime trajectories may vary more across crime types as individuals age.²⁵

C. Cost-Benefit Calculation

The larger contribution to the crime reduction from dynamic incapacitation as compared with a productivity effect does raise questions regarding whether economic benefits from raising the dropout age outweigh

²⁵ The notion that violent crime may not be the first crime type individuals engage in appears in the life course literature in criminology (see, e.g., the review by Piquero 2008).

costs. Lochner and Moretti's (2004) earlier results showed a significant economic benefit working through the productivity route, but this effect is much more modest for the recent reforms studied here. Table 10 therefore reports cost-benefit calculations on the estimated costs and benefits of the foregone crime using a similar methodology to that of Lochner and Moretti (2004) and also incorporates the costs of keeping students in high school for the additional school years.

By age 18, the policy just about breaks even, as the benefits from reduced crime just outweigh the costs, with the benefit-cost ratio of 1.04 meaning that \$1.04 results from crime reduction compared with each dollar spent on schools and their students in the extra school years. This result is the economic return to the direct incapacitation effect estimated in our analysis. However, because this persists via the state dependence generating dynamic incapacitation, the benefit-cost ratio rises when the crime reductions for the older 19–24 group are factored in. Indeed, when taking into account the effects of dynamic incapacitation for older ages (until age 24), the cost-benefit ratio shows a return of \$2.10 per dollar spent on the policy. Even here, then, this highlights how important the longer-term effects of the policy are to an evaluation of the cost-effectiveness of such education reforms, especially in an environment in which there appear to be scant productivity-enhancing effects from the reforms.

VI. Conclusions

By developing a more general way of modeling the impact of school dropout age reforms on crime, this paper presents the first evidence to show that compulsory schooling law reforms not only affect the overall level of crime, but they also reshape crime-age profiles. When placed into a more general modeling strategy than used in existing crime education research, this enables a better understanding of the reasons how and why education causally reduces crime.

Focusing on changes in state laws across the United 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. While 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.

The longer-run crime-reducing effect is interpreted as a dynamic incapacitation effect because further evidence shows that these same reforms had at best very modest effects on average educational attainment and

TABLE 10
COST-BENEFIT ANALYSIS

Victim Costs per Crime (1)	Property Loss per Crime (2)	Incarceration Costs per Crime (3)	Total Costs per Crime (4)	Estimated Change in Arrests (5)	Estimated Change in Crimes (6)	Estimated Change in Incarcerations (7)	Benefits (6) × (4) (8)	Estimated Change in School Enrollment (9)	Costs (9) × \$3,910 (10)	Benefit/ Cost Ratio (8)/(10) (11)
Violent crimes	135	7,399	40,697	-13,208	-28,042	-6,687	1,141,216,477			
Property crimes	1,141	201	941	-22,678	-121,926	-11,396	114,706,949			
Drug crimes ^a	NA	6,431	7,435	-18,638	-23,124	-8,734	171,928,858			
Total				-54,524	-173,092	-26,817	1,427,852,285	351,723	1,375,236,930	1.04
Violent crimes	135	7,399	40,697	-15,188	-32,247	-7,690	1,195,884,444			
Property crimes	1,141	201	941	-12,265	-65,943	-6,163	152,140,394			
Drug crimes ^a	NA	6,431	7,435	-12,465	-15,466	-5,841	114,987,276			
Total				-39,919	-113,656	-19,694	1,463,012,114			2.10

NOTE.—Costs of violent and property crimes are weighted averages of the breakdown costs from Lochner and Moretti (2004), using average share of crimes composing each of the categories as weights. Costs of drug crimes are based on the US Department of Justice (2011) victim costs and other crime costs, and incarceration costs are scaled in the same way used by Lochner and Moretti (2004). Estimated change in arrests are calculated based on the results from table 4, scaled using 1993 population within the age groups. Estimated crimes and incarcerations are calculated using 2009 clearances rates and conviction to incarceration rates, respectively, for each type of crime. Estimated change in enrollment is calculated using the results from col. 4 of table 7, scaled using the 1993 population within the age groups. Yearly costs per pupil (3,910) correspond to the average pupil costs (US Department of Education 2016) from 1974 to 2014. All figures are deflated to 1993 dollars.

wages, though somewhat more substantial effects on high school dropout. The overall evidence of dynamic incapacitation emerges because of state dependence that generates longer-run crime-reducing effects from incapacitation than those occurring just in the incapacitation period itself. This dynamic persistence is important both from the perspective of calculating the social benefits that crime reduction due to CSLs generates and for generating a better understanding of how individual crime dynamics evolve over the life course.

The analysis in this paper is based on panels of cohort-level arrest data rather than individual longitudinal data. This obviously prevents us from directly linking individuals, and so we cannot categorically show that those individuals who had reduced arrest rates during their direct incapacitation are the same individuals who had reduced arrests later in life, which is how we have interpreted our dynamic incapacitation results. However, analysis from the National Longitudinal Study of Youth 1997 (table A10) shows that 46% of all males arrested between the ages of 19 and 24 had already been arrested by age 18. Indeed, for those males who report being first arrested between the ages of 16 and 18—the age group affected by the reforms in this paper—52% are subsequently arrested when aged 19–24, while those not arrested at that point have a 26% probability of being arrested later. Furthermore, the importance of early-age arrests is much stronger for high school dropouts who are the likely compliers in our analysis of CSL changes. These results point to strong dependence between criminality at a formative age and later crime involvement and are consistent with the interpretation we have given to the results. We also consider it a major strength of this paper that it has a comparatively large set of policy changes to identify the causal effects. This contrasts with other settings where one typically might have access to individual-level data but with only a single policy reform.

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