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Education Policy and Crime

Lance Lochner

10.1 Introduction

In 1997, over two-thirds of all prison inmates in the United States were high school dropouts (Harlow 2003). Although education policy has not been a major factor driving trends in crime over the past twenty-five years—high school completion rates have remained relatively stable since the 1980s, while crime has both risen and fallen dramatically during that time—it is natural to ask what role education policy does and should play in affecting crime rates in the United States. Put another way, where is the marginal dollar best spent: on police, prisons, or schools? All three appear to reduce crime, but education and training have many benefits that prisons and police do not. In fact, Donohue and Siegelman (1998) argue that well-targeted preschool-type programs might be more cost-effective criminal deterrents than raising incarceration rates. Lochner and Moretti (2004) make a strong case for increasing high school graduation rates as an alternative to increasing the size of police forces.

Despite promising evidence that education-based policies and early childhood interventions can play an important role in helping reduce crime, evidence is still limited and sometimes mixed. The link between schooling and crime is more complicated than simple prison statistics suggest. This chapter

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reviews evidence in this rapidly growing area and develops a human capital-based theory for interpreting much of this evidence.

We first discuss the relationship between education and crime from an economic perspective, developing a simple model that sheds light on key ways in which early childhood programs and policies that encourage schooling may affect both juvenile and adult crime. The model developed in section 10.2 is grounded in human capital theory and paints with a broad brush. It emphasizes the choice individuals face between legitimate work and criminal activity, with its associated punishments. By altering the relative rewards of work and crime, educational investments affect decisions to engage in crime. While the model does not incorporate all avenues through which education may affect crime, it serves as a useful point of reference.

In section 10.3, we discuss evidence on the impacts of educational attainment and school quality/choice on adult crime. The evidence from studies of educational attainment on crime is largely consistent with a human capital-based theory of crime, suggesting that increases in schooling reduce most types of adult crime (e.g., Lochner 2004; Lochner and Moretti 2004). Studies of school choice and increases in school quality paint a more nuanced picture: sizeable improvements in school quality produce minor (at best) improvements in student achievement and educational attainment, while they appear to substantially reduce crime during late adolescence and early adulthood (Cullen, Jacob, and Levitt 2006; Deming 2009a). It is unclear whether “better” schools largely improve social development or the peers and social networks of disadvantaged youth. We next discuss the contemporaneous relationship between school attendance and crime. Using exogenous policy changes and other events that effectively force students to stay in school or take extra days off (e.g., changes in compulsory schooling laws, teacher in-service days, and strikes), a few recent studies have shown that school attendance affects crime in rich and complex ways. Forcing some students to stay in school an extra year or two reduces both violent and property crime substantially (Anderson 2009), consistent with the time allocation human capital model developed in section 10.2. Yet day-to-day changes in school attendance have opposing effects on violent and property crime. An extra day of school appears to reduce property crime while *increasing* violent crime (Jacob and Lefgren 2003; Luallen 2006). The latter most likely reflects social interaction effects from bringing together hundreds of adolescents and letting them all loose at the same time.

Section 10.4 reviews a number of recent studies that examine the long-run impacts of early childhood, school-based, and young adult training interventions on juvenile and adult crime. While a few early preschool programs have produced sizeable long-run reductions in crime—most famously, Perry Preschool—other quite similar programs have not. School-based programs focused on improving social development among “risky” children have been shown to reduce crime through early adulthood. Finally, job training for

young adults (e.g., Job Corps) appears to reduce self-reported arrests and convictions during the period of intensive training, but it yields negligible lasting effects on crime. Altogether, the evidence suggests that reductions in crime can be achieved by a wide range of human capital-based intervention strategies.

We discuss a number of policy issues related to education and its potential role as a crime-fighting strategy in section 10.5 and offer concluding thoughts in section 10.6.

10.2 The Economics of Education and Crime

Why might education reduce crime, and should its effects vary across different types of crimes? How might education and human capital policies help reduce crime? To answer these questions, we develop a simple economic model that formalizes a number of key channels through which education may affect crime. We then provide a brief discussion of other factors that may help determine the relationship between education and crime.

10.2.1 A Two-Period Model of School, Work, and Crime

To better understand the effects of early childhood programs and education policy on criminal behavior, we consider a simple two-period model of human capital investment, work, and crime. The model developed here abstracts from many things to focus attention on the effects of education and human capital-based policies on crime.¹ It emphasizes the role of education as a human capital investment that increases future legitimate work opportunities, which discourages participation in crime. This is consistent with numerous recent studies that show that higher wages reduce crime (e.g., Grogger 1998; Machin and Meghir 2004; Gould, Mustard, and Weinberg 2002) and decades of research in labor economics showing that education increases wage rates (see, e.g., Card 1999).

The two key assumptions of this human capital-based approach are (a) individual rationality and (b) the fact that crime requires time: in terms of planning, simply “hanging around” waiting for something to happen, carrying out the activity, avoiding arrest, or incarceration. Regarding the second, a number of studies discuss the implicit “wage rates” for time spent engaging in property crimes like drug dealing or burglary.² Yet for many other offenses, especially violent offenses, the criminal act itself may require little time; however, expected time in police stations, courtrooms, and prison cells may be substantial. The total time associated with most criminal acts may, in fact, be dominated by expected incarceration time. Taking this into

1. For a more detailed treatment of the life-cycle human capital investment problem and the age-crime profile, see Lochner (2004).

2. See Freeman (1999) for a survey of this literature.

account, the expected time associated with many violent offenses is likely to exceed that for most property crimes as seen in table 10.1 (from Lochner 2004), which reports probabilities of arrest, probabilities of conviction conditional on arrest, probabilities of incarceration conditional on conviction, estimated time served if incarcerated, and overall expected time served per crime committed for common violent and property crimes.

Now consider the choices faced by adolescents and adults. In the first period (adolescence), individuals are assumed to allocate their time to crime ($c_1 \geq 0$), work ($L_1 \geq 0$), and human capital investment ($I \geq 0$) subject to the time constraint $c_1 + L_1 + I = 1$. In the second period (adulthood), individuals decide only between crime ($c_2 \geq 0$) and work ($L_2 \geq 0$) subject to $c_2 + L_2 = 1$. In considering time spent committing crime, it is useful to think generally about the total expected time spent planning and committing crimes, avoiding arrest, “hanging around” waiting for an opportunity to arise, and in court or jail/prison.

While we do not explicitly model childhood, we assume that individuals enter adolescence with a set of endowments that affect subsequent behavior. These endowments may be shaped by early family and public investments. As a result, they may be manipulated by early childhood interventions as well as school-based policies (e.g., elementary school quality, preschool programs). We explicitly consider three types of adolescent “endowments” developed throughout childhood: “learning productivity” A , initial human capital levels H_1 , and “criminal propensity” θ . It is useful to think of these three “endowments” quite generally, as parameters that embody individual characteristics as well as the environment faced by individuals. For example, A reflects anything that increases the productivity of adolescent human capital investments (either through formal schooling or more informal on-the-job training). This may include raw intelligence quotient (IQ), peers, or local middle or high school quality. Similarly, θ represents any factors that may affect the net expected returns to crime for an individual (e.g., criminal skill, preferences for risk, or a personal aversion to crime or prison).

Human capital investments through schooling and training improve adult skills H_2 :

$$(1) \quad H_2 = H_1 + h(I, H_1; A),$$

where $h(\cdot)$ is increasing in each of its arguments (i.e., $h_j > 0$ for $j = I, H_1, A$), and there are diminishing marginal returns to investment (i.e., $h_{II} < 0$). These conditions ensure that education and training increase human capital at a diminishing rate. We further assume that students with higher levels of human capital, H_1 , and learning productivity, A , produce more human capital for any amount of investment ($h_{IA}, h_{IH} > 0$). Both ability and initial skill levels are, therefore, complementary with skill investment.

For each unit of time spent working, L_t , an individual earns H_t . Thus, H_t reflects an individual’s potential earnings if he or she devotes all his time to

Table 10.1 Expected punishment associated with incarceration (Uniform Crime Reports)

Crime	Probability of arrest	Probability of conviction conditional on arrest	Probability of incarceration conditional on conviction	Unconditional probability of incarceration	Estimated months served if incarcerated	Expected days served per crime committed
<i>Violent crimes</i>	0.25	0.22	0.79	0.043	91	119.4
Murder and nonnegligent manslaughter	0.85	0.67	0.95	0.544	248	4,102.4
Forcible rape	0.15	0.39	0.90	0.051	136	212.2
Robbery	0.15	0.36	0.89	0.047	94	134.8
Aggravated assault	0.30	0.17	0.71	0.035	59	63.2
<i>Property crimes</i>	0.06	0.11	0.68	0.004	24	3.2
Burglary	0.07	0.27	0.76	0.015	29	13.2
Larceny-theft	0.05	0.08	0.61	0.002	20	1.4
Motor vehicle theft	0.10	0.08	0.73	0.006	17	3.1

Source: Lochner (2004).

Notes: Probability of arrest computed from crimes and arrests in the United States (Uniform Crime Reports, 2000) adjusted for nonreporting to the police (National Criminal Victimization Survey, 2000). It is assumed that all murders are reported to the police. Probability of conviction conditional on arrest divides total arrests in the United States by total state and federal convictions for 2000. Probability of incarceration conditional on conviction is based on reporting of state courts. Estimated months served if incarcerated applies to state prisoners and is estimated by the U.S. Department of Justice based on sentence lengths handed out that year and the average percent of sentences served by prisoners released that year. Unless otherwise noted, all criminal justice figures are for 2000 and are taken from Durose and Langan (2003).

work. Investment, I , has no immediate payoff; however, it may be subsidized by the government at rate s . These subsidies more generally represent any incentives the government may provide for schooling or training.

Assume that time spent committing crime each period, c_t , yields a net return of $N(c_t, H_t; \theta)$, where, for simplicity, we abstract from uncertainty about punishment.³ As noted earlier, the parameter θ represents any factors that may affect the net returns to crime for an individual. As such, θ is a function of early childhood investments, family background, and neighborhoods, as well as law enforcement and incarceration policies. In general, the net expected returns to crime, as well as the marginal returns to crime N_{c_t} , may be positive or negative. However, we assume that $N_{c_0} > 0$, so persons with a high θ have a greater total and marginal expected return from crime.

For criminals, the net marginal return to crime (N_{c_t}) must be positive, but this need not be the case for noncriminals. Many individuals commit crime while working or attending school. This suggests that $N_{cc} < 0$ whenever $N_{c_t} > 0$ (i.e., if net returns to crime increase with the amount of time spent committing crime, they do so at a diminishing rate).⁴ We, therefore, make this assumption throughout.

On the one hand, individuals with more human capital are likely to be better criminals as well as better workers. (White collar crimes like fraud and embezzlement are perfect examples.) On the other hand, more highly skilled workers experience greater losses in earnings while imprisoned, and they may also have a greater aversion to crime (as emphasized by Usher 1997). The following analysis assumes that the positive effects of human capital on criminal returns weakly outweigh the negative effects on expected costs associated with punishment, so $N_H \geq 0$, $N_{cH} \geq 0$ and $N_{HH} \leq 0$. Of course, human capital is likely to have negligible effects on the returns to many property crimes (i.e., $N_H = 0$, a case not ruled out in our analysis).

The Individual's Decision

Taking (A, H_1, θ) and s as given, individuals choose investment and time spent in work and crime to maximize the present value of lifetime earnings. Assuming a gross interest rate $R \geq 1$, and substituting in the time constraints, individuals

$$(2) \quad \max_{I, c_1, c_2} [H_1(1 - I - c_1) + sI_1 + N(c_1, H_1; \theta)] + R^{-1} [H_2(1 - c_2) + N(c_2, H_2; \theta)],$$

3. We implicitly assume that any expected punishments are incurred during the period the crime is committed. This is consistent with the fact that most juveniles caught committing crime face relatively short periods of incarceration. Dealing more explicitly with uncertainty and lags in punishment would not change the nature of most results discussed here. See Lochner (2004, 2010) for a life-cycle model that explicitly incorporates these features.

4. If net marginal returns were positive and increasing, individuals would specialize.

subject to the human accumulation equation (1) and the time constraints $I \geq 0$, $c_1 \geq 0$, $c_2 \geq 0$, and $I + c_1 \leq 1$.

While the individual decision problem is framed as an income maximization problem and directly applies to crimes with a financial motive, the framework can also be used to study violent crime. In the case of violent crime, the function $N(\cdot)$ reflects the monetary equivalent of any “psychic” or nonpecuniary benefits from violent crime.

We assume that $s < H_1$, so that investment subsidies are not large enough to make investment more lucrative than work unless there is some future return on investment. The problem yields the following interior first-order conditions for I , c_1 , and c_2 :

$$(3) \quad H_1 - s = R^{-1}[(1 - c_2) + N_H(c_2, H_2; \theta)] h_I(I, H_1; A)$$

$$(4) \quad H_1 = N_c(c_1, H_1; \theta)$$

$$(5) \quad H_2 = N_c(c_2, H_2; \theta).$$

These conditions hold for individuals who allocate some time to each activity during adolescence and adulthood and are useful for studying investment, work, and crime at the intensive margin.⁵ Individuals equate the marginal returns on investment and crime each period to their potential legitimate wage rate H_t (less any investment subsidies in the case of investment).⁶ Because it is fixed at any point in time, this wage rate reflects the opportunity cost for individuals in choosing how much time to spend investing in new skills or on the commission of crime. Among adolescents who spend some time working, small increases in investment (e.g., due to an increase in its return) will come at the expense of adolescent work and not juvenile crime; juvenile crime will also trade off with work (at the margin) and not investment. This suggests that we might not expect significant “incapacitation” effects of school on crime among students who also participate heavily in the labor market.

Equation (3) shows that schooling provides returns in the form of higher future earnings from work and potentially from crime through increased human capital. If education does not raise the returns from crime, youth that plan to spend more time committing crime as an adult will benefit less from school and should, therefore, choose to invest less in their human capital. Thus, a negative relationship between schooling and adult crime may result

5. The second-order conditions are not particularly informative. They do require that $N_{cc} < 0$, as assumed. While the second-order conditions do not necessarily hold everywhere for all possible parameterizations, we assume that they hold at any given interior solution for the (local) comparative static results derived in the following.

6. Equations (4) and (5) and diminishing marginal returns to crime ($N_{cc} < 0$) are consistent with higher average “wage rates” for many property crimes (relative to typical legitimate opportunities) as discussed in Freeman (1999).

from individual heterogeneity in tastes for crime or from local differences in criminal opportunities, law enforcement, and punishment regimes.

The effect of educational attainment on adult crime is embodied in equation (5). Anything that increases investment in human capital raises H_2 , which raises the returns from legitimate work and the opportunity cost of engaging in crime. Of course, human capital may also raise the return to crime, so the net effect of schooling on adult crime depends on the balance of these two effects. In general, we would expect education to provide greater returns in the labor market than for most types of crime, so education should reduce adult crime. Notice that individuals with a higher learning ability A will benefit more from school, so we might expect greater reductions in adult crime among smarter youth in response to school-based policies. Of course, there is little scope for school-based policies to reduce crime among those who would normally eschew crime as adults in the first place. As such, education-based initiatives aimed at adolescents are likely to achieve greater reductions in crime if they target relatively intelligent (high A) youth with low initial skill levels (H_1) and high returns to crime (i.e., high θ).

For youth with high enough returns to crime or investment such that they choose not to work at all during adolescence (i.e., $I^* + c_1^* = 1$), conditions (3) and (4) reduce to a single first-order condition equating the marginal returns on adolescent crime with the marginal returns on investment: $N_c(1 - I, H_1; \theta) - s = R^{-1} [(1 - c_2) + N_H(c_2, H_2; \theta)] h_I(I, H_1; A)$. Among these individuals, time spent investing and adolescent crime trade off one-for-one, so education-based policies may have sizeable impacts on crime among non-working juvenile criminals.

Education and Early Childhood Policies

We consider the implications of policies which may alter incentives to invest in human capital (i.e., changes in s) as well as earlier childhood or school-age policies that impact adolescent “endowments” (A, H_1, θ).⁷ Our results apply to individuals who spend some time in both school and on crime during adolescence and who spend some time committing crime and working during adulthood. In some cases, the effects of policies differ (as noted) between individuals who also spend some time working during adolescence and those who do not.

The following condition is useful for a number of results.

CONDITION 1. $N_{cH} \leq I$.

This condition implies that human capital does not raise the returns to crime more than it raises the returns to legitimate work. It may not hold in

7. Policies to improve high schools may also directly affect the productivity of time spent in school, A , and socialization, θ . In this sense, these parameters may be directly manipulated by policy; however, we assume that they are not freely chosen by adolescents. Of course, families may shape these parameters through earlier investments as discussed in the preceding.

the case of certain types of white-collar crimes, but it is likely to hold for most common “street” crimes like larceny, assault, or robbery.

We first discuss the effects of education subsidies, or policies that generally encourage schooling.⁸

RESULT 1. *A marginal increase in education subsidies, s (a) increases investment in human capital; (b) does not affect crime for working adolescents but reduces crime among nonworking adolescents; and (c) reduces adult crime if Condition 1 holds and increases adult crime otherwise.*

Education subsidies do not affect criminal behavior for adolescents who work because the amount of time spent committing crime is determined only by their potential wage rate. Time spent investing trades off one-for-one with time spent working.⁹ Nonworking adolescents increase their investment and reduce their criminal activity in response to higher investment subsidies. For them, criminal activity necessarily trades off with investment. As long as the returns to human capital are higher in the legitimate sector than the criminal sector, education subsidies will reduce adult crime rates.

It is worth noting, however, that crimes with a higher return to skill than legitimate work will tend to increase (among adults) in response to education subsidies. Thus, it is possible that some forms of white-collar crime may increase following policies that promote skill investments.

Because parental inputs, family background, early childhood programs, and school quality operate on the “endowment” parameters (A, H_1, θ) , understanding how these parameters affect individual decisions is important. We begin by studying the effects of changes in learning productivity, A .

RESULT 2. *A marginal increase in learning productivity, A (a) increases investment in human capital if N_{HH} is sufficiently close to zero; (b) does not affect crime for working adolescents but reduces crime among nonworking adolescents (if $N_{HH} \approx 0$); and (c) reduces adult crime if Condition 1 holds and increases adult crime otherwise.*

Policies that increase learning productivity or cognitive ability have qualitatively similar effects to an increase in education subsidies. Not surprisingly, an increase in the productivity of schooling (or learning ability) causes individuals to invest more in their skills. Adolescent criminal activity is unaffected by small changes in A for working adolescents. Because

8. All results are derived formally in the appendix.

9. The fact that wage rates are unaffected by hours worked but criminal earnings are declining in time spent committing crime is key to this result. If wage rates depend on the number of hours worked, time spent committing crime during adolescence will be affected by an investment subsidy even for those who are working. Additionally, if incarceration extends for many years into the future, an investment subsidy may reduce adolescent crime among workers because the expected costs from future incarceration are increasing in investment (Lochner 2004, 2010). Finally, large enough education subsidies could cause youth to stop working altogether, in which case they would also reduce their criminal activity.

initial potential wage rates are fixed, individuals simply substitute work for investment.¹⁰ More investment means higher levels of human capital and higher wage rates during adulthood. As long as the criminal returns to human capital are not too high, this lowers adult crime and increases time spent working. Nonworking adolescents commit less crime in response to an increase in A because higher investment must trade off with time spent committing crime.

Policies that raise initial skill levels (H_1) can yield different implications, especially for adolescents.

RESULT 3. *Among working adolescents, an increase initial skill levels, H_1 , reduces adolescent crime if Condition 1 holds; otherwise, it increases adolescent crime. Among nonworking adolescents, if human capital does not affect the net returns to crime (i.e., $N_H = 0$), then an increase in H_1 increases investment and reduces both adolescent and adult crime.*

As long as human capital is rewarded more in the labor market than the criminal sector, an increase in the skills of working youth reduces juvenile crime. However, it has ambiguous effects on investment because it raises both the opportunity cost of and return to education. This means that it is not possible to generally sign the effects of changes in H_1 on adult human capital and crime for working youth; however, we would typically expect adult crime to be decreasing in H_1 . Among nonworking adolescents, increases in skill have no effect on the opportunity cost of investment. As such, an increase in H_1 unambiguously raises their investment and reduces their participation in crime at all ages (if human capital does not affect criminal returns).

Finally, we discuss the effects of policies that alter the expected returns to crime. These policies may have their effects through socialization or simply through increasing the probability of arrest or incarceration.

RESULT 4. *A reduction in criminal returns, θ , reduces adolescent crime for working adolescents. If Condition 1 holds and $N_{H\theta} \leq 0$, then a reduction in θ also (a) increases schooling investments; (b) reduces adolescent crime for nonworking adolescents; and (c) reduces adult crime.*

A lower criminal return directly encourages individuals to work more at all ages. By shifting time from crime to work during adulthood, a reduction in criminal returns raises the return to investment (assuming criminal returns to skill are low). Increased schooling investment increases adult wage rates, which causes individuals to further reallocate time from crime to work as

10. As with education subsidies, large increases in A may cause youth to stop working altogether and substitute away from crime as well. It is also likely that individuals with a higher learning ability also possess a higher initial skill level H_1 by the time they reach adolescence, in which case criminal activity during adolescence would be lower for those with high A and H_1 . The effects of H_1 on crime are discussed further in the following.

adults. Thus, the endogeneity of schooling and work leads to larger reductions in adult crime than would be predicted if either were held fixed.

These results, particularly the last, highlight why cross-sectional comparisons of education and crime are difficult to interpret. On the one hand, youth who invest more through school should commit less crime as adults. On the other hand, youth planning to spend much of their adult lives on crime (and in jail) receive little return from school and will choose to invest little in school. Thus, a negative education–crime relationship can arise because education reduces crime or because a life of crime renders education useless.

10.2.2 Other Ways in Which Education May Affect Crime

Education may also teach individuals to be more patient (Becker and Mulligan 1997). This would discourage crime because forward-looking individuals place greater weight on any expected future punishment associated with their criminal activities. To the extent that time preferences are affected by schooling, crimes associated with long prison sentences (or other long-term consequences) should be most affected. Education may also affect preferences toward risk. If schooling makes individuals more risk averse, it should discourage crime with its greatest effects on offenses that entail considerable uncertainty in returns or punishment. Finally, schooling may affect the set of people individuals interact with on a daily basis in school, work, or their neighborhoods. Assuming more educated people interact more with other educated people who are less inclined to engage in crime, this is likely to compound any reductions in crime associated with schooling.¹¹ In most cases, mechanisms related to changes in preferences or social interactions suggest that educational attainment is likely to reduce most types of crime among adults.

10.2.3 School Attendance and Contemporaneous Crime

It is useful to distinguish between the effects of educational attainment on subsequent criminal activity and the way in which school attendance itself affects contemporaneous crime. The latter relationship is likely to be driven by three mechanisms. First, school may have an incapacitation effect—youth cannot be in two places at once, and many criminal opportunities are more limited in school than on the streets. Of course, school does not last all day, so this effect depends, in part, on the ease with which youth can engage in crime during nonschool hours. This mechanism is inherent in the time allocation problem in the preceding. Second, longer periods of school attendance should increase human capital levels and improve future employment prospects. This, in turn, may make juvenile arrests and long periods of detention more costly, reducing incentives to engage in crime while enrolled in

11. See Glaeser, Sacerdote, and Scheinkman (1996) for a model of crime where social interactions are important.

school.¹² Third, schools bring hundreds of adolescents together for the day and then let them all loose at the same time. The social interaction effects from doing this are far from obvious, but it is quite possible that this leads to altercations and more general group-based delinquency. The incapacitation and human capital effects are likely to imply negative effects of school attendance on crime, while the social interaction effect could be positive or negative.

10.3 Evidence on Education and Crime

We now discuss evidence on the effects of educational attainment and school quality and choice on subsequent criminal outcomes. We also review empirical studies that analyze the relationship between school attendance and contemporaneous crime.

10.3.1 Educational Attainment and Crime

We have discussed four primary reasons schooling might affect subsequent crime: (a) education raises wage rates, which raises the opportunity costs of crime; (b) education may directly affect the financial or “psychic” rewards from crime; (c) education may alter preferences for risk-taking or patience; and (d) schooling may affect the social networks or peers of individuals. For most crimes (except, possibly, white-collar crimes), one would expect these forces to induce a negative effect of schooling on adult crime.

Empirically, there is a strong negative correlation between educational attainment and various measures of crime. In 1997, 75 percent of state and 59 percent of federal prison inmates in the United States did not have a high school diploma (Harlow 2003).¹³ After controlling for age, state of birth, state of residence, year of birth, and year effects, Lochner and Moretti (2004) still find significant effects of schooling (especially high school completion) on the probability of incarceration in the United States as reported in figure 10.1.¹⁴ In 2001, more than 75 percent of convicted persons in Italy had not completed high school (Buonanno and Leonida 2006), while incarceration

12. The preceding model abstracts from this by implicitly assuming that punishments occur in the same period that crimes are committed and that there are no long-term effects of punishment on human capital or employment opportunities; however, it is straightforward to incorporate these effects in a life-cycle model with multiperiod punishments as in Lochner (2004, 2010).

13. These figures exclude those who received a General Educational Development (GED) diploma. As shown in Cameron and Heckman (2003) and Heckman and LaFontaine (2006), individuals with a GED perform like high school dropouts rather than graduates in the labor market. Roughly 35 percent of state inmates and 33 percent of federal inmates completed their GED with more than two-thirds of these inmates earning their GED while incarcerated. A small percentage of those who did not receive a high school diploma had participated in some vocational or postsecondary courses. See Harlow (2003).

14. These figures report the coefficients on indicators for different years of completed schooling from the 1960, 1970, and 1980 Censuses for white and black men ages twenty to sixty.

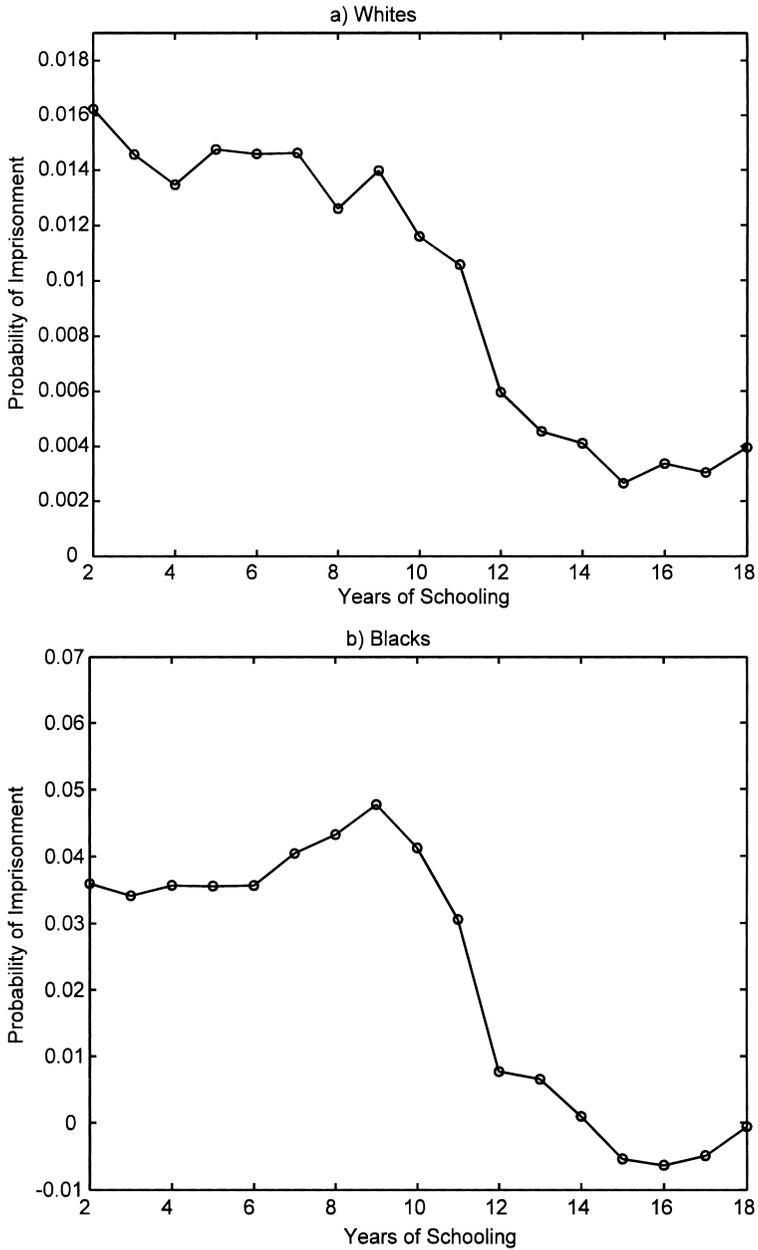


Fig. 10.1 Regression-adjusted probability of incarceration by education (men ages 20–60)

Source: Lochner and Moretti (2004).

Notes: From 1960, 1970, and 1980 U.S. Censuses. Regressions control for age, state of birth, state of residence, cohort of birth, state, and year effects.

rates among men ages twenty-one to twenty-five in the United Kingdom were more than eight times higher for those without an education qualification (i.e., dropouts), relative to those with a qualification (Machin, Marie, and Vujic 2010).

Differences by education are also apparent in self-reported survey measures of crime. For example, in the 1980 wave of the National Longitudinal Survey of Youth (NLSY), 34 percent of American men ages twenty to twenty-three with eleven or twelve years of completed schooling self-reported earning some income from crime, compared with 24 percent of those with twelve years of school, and only 17 percent of those with more than twelve years. The effect of education is magnified if we consider more active criminal engagement: 4.2 percent of twenty to twenty-three-year-old NLSY men completing ten or eleven years of school reported earning more than half their income from crime, compared with 1.4 percent of those with twelve years and 0.7% of those with at least some college education. Similar patterns are observed for violent crime in the NLSY. See Lochner (2004) for further details.

Early studies of the relationship between education and crime focused on their correlation conditional on measured individual and family characteristics using standard regression methods.¹⁵ For example, Witte and Tauchen (1994) find no significant relationship between educational attainment and crime after controlling for a number of individual characteristics. Grogger (1998) estimates a significant negative effect of wages on crime, but he finds no relationship between years of completed schooling and crime after controlling for individual wage rates. Of course, increased wages and earnings are important consequences of schooling. Thus, this study suggests that education may indirectly reduce crime through increased wage rates.

These earlier studies must be interpreted with caution. A negative cross-sectional correlation between education and crime, even after controlling for measured family background and neighborhood characteristics, does not necessarily imply that education reduces crime. Standard regression studies are unlikely to estimate the causal effect of education on crime (i.e., the effect of increasing someone's schooling on his criminal activity) for a number of reasons. First, unobserved individual characteristics like patience or risk aversion are likely to directly affect both schooling and criminal decisions. Individuals who choose more schooling (even after conditioning on observable characteristics) might also choose less crime regardless of their education level, in which case regression-based estimates do not identify a causal effect. Second, using variation in crime and education across states or local communities may also produce biased estimates. Governments may face a

15. Ehrlich (1975) provides an early empirical exploration of predicted effects of education on crime from a human capital perspective. See Witte (1997) for a survey of the early empirical literature on education and crime.

choice between funding police or good public schools, which would tend to produce a spurious positive correlation between education and crime. Alternatively, unobserved characteristics about communities or their residents may directly affect the costs or benefits of both education and crime. For example, communities with few job opportunities that reward schooling may also be faced with severe gang problems. While it is often possible to account for permanent unobserved differences across communities by examining the relationship between changes in schooling and crime over time, this approach does not account for the effects of changing unobserved community characteristics. Third, reverse causality is another important concern, for reasons discussed in section 10.2. Individuals who plan to heavily engage in crime (e.g., because they are particularly good at it, enjoy it, or live in areas with plenty of illicit opportunities) are likely to choose to leave school at a young age. Arrests or incarceration associated with juvenile crime may also cause some youth to drop out of school early (Hjalmarsson 2008).

Recently, economists have attempted to address these difficult issues and to estimate the causal effects of schooling on crime using instrumental variable (IV) methods. In the context of estimating the effect of educational attainment on crime, an instrument is valid if it induces variation in schooling but is uncorrelated with other factors that directly affect criminal proclivity (e.g., individual preferences or abilities, local law enforcement). Intuitively, this approach exploits differences in educational attainment across individuals that arise in response to factors that have no direct impact on criminal decisions. An ideal instrument would randomly assign some youth to drop out of high school and others to finish. Then, comparing the differences in crime rates across these groups would identify the causal effect of high school completion on crime. In practice, we typically do not observe such perfect experiments, but researchers can sometimes come close.

Because crime itself is difficult to measure, researchers are often forced to use measures of arrest or incarceration rather than actual crimes committed. It is possible that education reduces the probability of arrest and incarceration or the sentence lengths administered by judges, in which case estimates based on measures of arrest or incarceration incorporate these effects in addition to any effects of education on actual crime. While there is little direct evidence on these issues, Mustard (2001) finds negligible effects of defendant education levels on the sentence lengths they receive. Furthermore, results using self-reported measures of crime in the NLSY support the case that education reduces actual violent and property crime and not just the probability of arrest or incarceration conditional on crime (Lochner 2004; Lochner and Moretti 2004).¹⁶

16. There has been considerable debate among criminologists on the merits of self-reported measures of crime versus official measures of arrest. Most studies find a reasonably high correlation between the two; however, it is generally agreed that the two measures offer distinct

Many recent empirical studies analyze crime aggregated at some geographic level, exploring the effects of average educational attainment on crime, arrest, conviction, or incarceration rates. In order to address concerns with endogeneity or unobserved heterogeneity, researchers have typically turned to IV estimation or a differences-in-differences strategy using changes in state or national rules that affect schooling decisions. An aggregate-level regression is often specified as follows:

$$(6) \quad y_{calt} = \beta E_{alt} + \gamma X_{alt} + d_{lt} + d_{cl} + d_{al} + d_{ct} + d_{at} + d_{ca} + \varepsilon_{calt}$$

where y_{calt} is a measure of the crime, arrest, or incarceration rate for some offense type c , age group a , in location l , in year t . In some cases, only a single measure of crime is used (e.g., incarceration or total arrests), in which case the c subscript is extraneous. E_{alt} is an aggregate measure of educational attainment for age group a in location l at time t (e.g., average schooling attainment or high school completion rates). X_{alt} is a set of observable characteristics that may vary across age, location, and time (e.g., racial composition of an area). The d s represent indicator variables that account for unobserved differences by age/cohort, location, year, and criminal offense types. The term d_{lt} allows for location-specific time effects, which accounts for time varying unobserved location-specific differences that may reflect differences in local public spending, economic conditions, or law enforcement. The inclusion of d_{cl} allows the average distribution of crime or arrest types to differ across areas. For example, some states may focus arrests more heavily on one type of crime, while others focus on other types. Or some areas may be more amenable to certain crimes while others are not. Similarly, the age distribution of crime or arrests need not be the same across areas—some age groups may be more crime-prone in some areas or arrest policies with respect to age may differ across areas. The term d_{al} absorbs long-run differences in age-arrest patterns across locations. Crime-specific and age-specific time trends in arrest common to all areas are accounted for by d_{ct} and d_{at} , respectively. Finally, d_{ca} accounts for long-term differences in age-crime profiles across different types of criminal offenses. Given these fixed effects, identification of the effect of education on crime is achieved through time variation in cohort educational attainment levels across different locations. The absence of d_{alt} indicator variables in equation (6) is, therefore, central to identification.

Lochner and Moretti (2004) examine state-level male arrest rates by criminal offense and age (five-year age categories beginning at ages twenty

and complementary information about criminal activity. Comparisons of self-reported arrests versus official arrests tend to find a stronger correlation, with agreement increasing further for self-reported versus official measures of criminal convictions. A number of studies report greater underreporting of crimes and arrests by blacks; however, studies vary considerably on this. See the classic Hindelang, Hirschi, and Weis (1981) for comprehensive treatment of the issue or Thornberry and Krohn (2000) for a more recent survey of this literature.

to twenty-four through fifty-five to fifty-nine) from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR) for the United States in 1960, 1970, 1980, and 1990. This data is linked to 1960 to 1990 decennial U.S. Census data on educational attainment and race to estimate equation (6), where y_{calt} represents log arrest rates for a specific offense, age category, state, and Census year. They specifically analyze arrest rates for murder, rape, assault, robbery, burglary, larceny, auto theft, and arson. In using log arrest rates, the effect of education is assumed to be the same in percentage terms for each type of crime included in the regression. They explore the effects of both average years of schooling and high school completion rates at the cohort level (cohorts are defined by year of birth given year t and age a) in state l as of time t (i.e., E_{alt}). In addition to including all the d fixed effects in equation (6), they also include the percent of males that are black in age group a living in state l at time t (i.e., X_{alt}).

The main methodological contribution of Lochner and Moretti (2004) is the use of changes in state-specific compulsory schooling laws over time as IVs for schooling.¹⁷ Intuitively, this strategy measures the extent to which an increase in a state's compulsory schooling age leads to an immediate increase in educational attainment and reduction in subsequent crime rates for affected cohorts.¹⁸ Lochner and Moretti's (2004) analysis suggests that changes in compulsory schooling laws are exogenous and not related to prior trends in schooling or state expenditures on law enforcement, so it appears to be a valid instrument. Other studies reach similar conclusions about the exogenous nature of changes in compulsory schooling laws in other contexts (e.g., Acemoglu and Angrist 2001; Lleras-Muney 2002).

Lochner and Moretti (2004) estimate equation (6) using both ordinary least squares (OLS) and IV estimation. Using OLS, they find that a one-year increase in average education levels in a state reduces state-level arrest rates by 11 percent. Instrumental variable estimates suggest slightly larger effects although they are not statistically different. These estimated effects are very similar to the predicted effects derived from multiplying the estimated increase in wages associated with an additional year of school by the estimated effects of higher wage rates on crime (from Gould, Mustard, and Weinberg 2002). This suggests that much of the effect of schooling on crime may come through increased wage rates and opportunity costs as emphasized in the model of section 10.2. Given the strong relationship between high school completion and incarceration apparent in figure 10.1, Lochner and Moretti (2004) also estimate specifications using the high school

17. The relevant compulsory schooling age is based on the state law that applied when a cohort was age fourteen.

18. It is worth noting that this strategy (i.e., using compulsory schooling ages to instrument for average attainment) identifies the effects of raising average educational attainment levels via increases in schooling among high school dropouts. Policies that largely increase average attainment by increasing college completion rates could have very different effects.

completion rate as a measure of schooling. Ordinary least squares estimates suggest that a 10 percentage point increase in high school graduation rates would reduce arrest rates by 7 percent, while IV estimates suggest a slightly larger impact of 9 percent.

Lochner and Moretti (2004) also estimate separate effects of education for different types of crime using OLS (including interactions of criminal offense type with education in equation [6]). These results suggest similar effects across the broad categories of violent (murder, rape, robbery, and assault) and property (burglary, larceny, motor vehicle theft, and arson) crime—a one year increase in average years of schooling reduces both property and violent crime by about 11 to 12 percent. However, the effects vary considerably within these categories. A one-year increase in average years of schooling reduces murder and assault by almost 30 percent, motor vehicle theft by 20 percent, arson by 13 percent, and burglary and larceny by about 6 percent. Estimated effects on robbery are negligible, while those for rape are significantly positive. Additional specifications suggest quantitatively similar effects for a 10 to 20 percentage point increase in high school graduation rates. Their results for rape are surprising and not easily explained by standard economic models of crime.¹⁹

Lochner (2004) follows a very similar approach using the same UCR data from 1960 to 1980; however, he also examines white-collar crime. Ordinary least squares estimation of equation (6) produces *positive*, though statistically insignificant, effects of schooling on arrest rates for white-collar crimes (forgery and counterfeiting, fraud, and embezzlement). Estimates for violent and property crime are negative and similar to those of Lochner and Moretti (2004).

Lochner and Moretti (2004) also use individual-level data on incarceration and schooling from the 1960, 1970, and 1980 U.S. Censuses to estimate the effects of educational attainment on the probability of imprisonment separately for black and white men (ages twenty to sixty). Their estimates control for age of the respondent (three-year age categories), state of birth, state of residence, cohort of birth, and state-specific year effects. Most important, controlling for state-specific year effects allows for the possibility that different states may have different time trends for law enforcement policies or may simply exhibit different trends in aggregate criminal activity. Analogous to their analysis of state-level arrest rates, they use state-level changes in compulsory schooling ages as an instrument for educational attainment. Although this analysis uses individual-based measures of incarceration and schooling, variation in schooling laws at the state-year level effectively identifies the effect of education on crime. As with the estimates

19. However, the results are consistent with some specifications in Gould, Mustard, and Weinberg (2002), which suggests that local wage rates are positively correlated with local crime rates for rape.

for aggregate arrest rates, identification comes from the fact that in any given state and year, different age cohorts faced different compulsory schooling laws during their high school years, causing them to acquire different levels of schooling and to commit crime at different rates. Again, both OLS and IV estimates are very similar and suggest that, on average, an extra year of education reduces the probability of imprisonment by slightly more than .1 percentage point for whites and by about .4 percentage points for blacks. In their sample, the probability of incarceration for male whites (blacks) without a high school degree averaged .83 percent (3.6 percent), which translates into a 10 to 15 percent reduction in incarceration rates for both white and black males associated with an extra year of completed schooling. These estimated effects are comparable to those for arrest rates described earlier. Ordinary least squares results suggest that completion of the twelfth grade causes the greatest drop in incarceration, while there is little effect of schooling beyond high school (see figure 10.1).

Oreopoulos and Salvanes (2009) reproduce the Lochner and Moretti (2004) IV results for black males using the same estimation strategy with a slightly different specification and an expanded sample that includes men ages twenty-five to sixty-four from the 1950 to 1980 U.S. Censuses.²⁰ Their estimate suggests that an additional year of completed schooling reduces incarceration rates among black men by about 20 percent.

Machin, Marie, and Vujic (2010) exploit a 1972 increase in the minimum schooling age (from age fifteen to sixteen) in England and Wales to estimate the effects of schooling on criminal convictions for property and violent crimes over the period 1972 to 1996. Using both IV and regression discontinuity methods, identification effectively comes from cohort-level changes in schooling attainment and crime for cohorts turning fifteen immediately before and after the law change.²¹ Among men, they estimate that a one-year increase in average schooling levels reduces conviction rates for property crime by 20 to 30 percent and violent crime by roughly one-third to one-half as much.²² Compared to estimates for the United States by Lochner and Moretti (2004), the impacts of education on property crime appear to be greater in the United Kingdom, while the effects on violent crime are weaker.

20. Most notably, they do not include state and state-specific year effects in their specification. They also remove individuals with greater than twelve years of schooling from their sample. Their measures of compulsory schooling ages differ as well, incorporating the fact that some states allow for exceptions to the dropout age under certain conditions.

21. They estimate models aggregated to the year-age level for individuals ages eighteen to forty from 1972 to 1996. To alleviate concerns that other important economic or social factors may have changed at the same time the compulsory schooling age increased, they include a rich set of covariates: year and age indicators, fraction British-born, fraction employed, fraction nonwhite, and fraction living in London.

22. Estimated effects on male property crime are statistically significant, while effects on male violent crime are not. Estimated effects for women are, unfortunately, very imprecise.

Buonanno and Leonida (2006) estimate the effects of educational attainment on crime rates in Italy using regional panel data from 1980 to 1995. Their unit of observation is a region year (they examine twenty Italian regions), and they estimate a restricted form of equation (6) using OLS. Specifically, they control for region and time fixed effects (d_i and d_t), along with region-specific quadratic time trends (assuming $d_{it} = \delta_{1i}t + \delta_{2i}t^2$), and a rich set of time-varying region-specific covariates.²³ These estimates are identified from the relationship between changes in regional education levels and crime rates (around smooth regional time trends). Their estimates suggest that a 10 percentage point increase in high school graduation rates would reduce property crime rates by 4 percent and total crime rates by about 3 percent. (Effects on property crime are statistically significant, while effects on total crime are not.) They find no evidence to suggest that university completion reduces crime.²⁴

Merlo and Wolpin (2009) take a very different approach to estimating the relationship between schooling and subsequent crime. Using individual-level panel data on black males ages thirteen to twenty-two from the NLSY, they estimate a discrete choice vector autoregression model in which individuals can choose to engage in crime, attend school, or work each year.²⁵ These decisions are allowed to depend on unobserved individual-specific returns to each activity as well as crime, schooling, and work choices the previous year. Using estimates for their model, Merlo and Wolpin simulate the effects of changing youth schooling status at age sixteen on subsequent outcomes. Their estimates suggest that, on average, attending school at age sixteen reduces the probability a black male ever commits a crime over ages nineteen to twenty-two by 13 percentage points and the probability of an arrest over those ages by 6 percentage points. These represent 42 percent and 23 percent reductions in self-reported crime and arrest rates, respectively, for black males not in school at age sixteen.

A final study worth mentioning examines the effects of an explicit education subsidy on youth burglary rates in England. Between 1999 and 2002, England piloted Educational Maintenance Allowances (EMA), which provided subsidies of up to £40 per week (plus bonuses for completion of coursework) for low-income sixteen to eighteen-year-old youth to attend

23. Covariates include employment rates, GDP per capita, GDP growth rates, average wage rates, the fraction of crimes without an arrest, police per capita, and the length of time in the judicial process.

24. Buonanno and Leonida (2006) also generalize their econometric specification to allow for an effect of lagged crime rates on current crime rates, estimating this using a generalized method of moments estimator to account for the endogeneity of lagged crime rates. This specification produces similar estimated effects of schooling on crime.

25. Crime, work, and school are not mutually exclusive activities in this framework—individuals can do any combination of these three activities in each year. An individual is said to have engaged in crime in any year if they self-reported any of the following offenses: theft, other property crime, sold drugs, or assault.

school. The program was administered in fifteen local areas with low schooling participation rates. During the same time period, the Reducing Burglary Initiative (RBI) funded sixty-three different local burglary reduction schemes as a separate pilot project. Roughly half of all EMA pilot areas were also selected for the RBI. Sabates and Feinstein (2008) use a differences-in-differences strategy to identify the effects of each pilot program as well as the combination of the two on burglary. Specifically, they compare changes in burglary conviction rates before and after the introduction of RBI, EMA, or both against a set of comparison areas. While baseline burglary conviction rates were much higher in EMA and EMA-RBI combined areas relative to the comparison areas, annual growth rates in burglary conviction rates prior to the programs were quite similar across all three classifications. To reduce concerns about differences between the treated and untreated areas, Sabates and Feinstein control for a number of time varying area-specific factors likely to affect crime and limit their sample of comparison areas to those that best “match” the distribution of demographic characteristics in the pilot areas.²⁶ Their findings suggest that the combination of both the EMA and RBI significantly reduced burglary rates by 1.3 per 1,000 youth (about 5.5 percent), relative to the “matched” comparison areas. Effects of the EMA alone were slightly lower but still significant. While there are obvious concerns about the extent to which time varying determinants of burglary are the same for treated and comparison areas, Sabates and Feinstein (2008) show that estimated effects on burglary rates for nineteen to twenty-one-year-olds (who were not offered the education subsidy) were much lower and statistically insignificant.

10.3.2 School Quality and Crime

If human capital acquisition, socialization, or preference modification are important mechanisms determining the impacts of educational attainment on crime, then it is likely that school quality and the type of schools students attend also affect criminal behavior. While there are no studies that directly estimate the effects of measured school quality on crime, three recent studies on school choice and desegregation provide some useful insights.

Cullen, Jacob, and Levitt (2006) and Deming (2009a) examine the importance of school choice in large urban U.S. school districts (Chicago and Charlotte-Mecklenburg, respectively) on a variety of student outcomes, including delinquency and crime. Both studies examine the effects “winning” a randomized lottery for admission to schools children selectively

26. Their regressions control for unemployment rates for individuals under twenty-five, proportion of students eligible for free school meals, number of qualified teachers, pupil-teacher ratios, and the number of supplementary staff for ethnic minorities, percent of youth with no schooling qualifications as of age sixteen (i.e., dropouts), and the percent of unauthorized half-days missed in secondary school. We discuss results based on the “matched” sample of comparison areas.

apply to.²⁷ By comparing the outcomes for youth who win versus lose a particular school admission lottery, they estimate the effects of being offered admission to that school, relative to the preferred alternative. This reflects the “intention to treat” (i.e., the effects of being offered the opportunity to attend better schools) and not necessarily the effects of actually attending that school because many students did not ultimately enroll in schools for which they were admitted by lottery. However, both studies find that “winning” a lottery does significantly increase enrollment in that school. Because many students applying outside their assigned local school are from disadvantaged backgrounds and neighborhoods, on average, lottery winners end up attending better quality schools, as measured by such things as student achievement scores, value added (i.e., growth in achievement), student behavioral problems, or teacher quality. In this sense, these studies offer an opportunity to examine the effects of school quality, broadly defined, on delinquency and crime.

Cullen, Jacob, and Levitt (2006) find that winning a high school lottery in Chicago significantly raises peer graduation rates by 6 percent and the share of peers who test above national norms by about 14 percent; however, lottery winners appear to be placed in lower tracked classes within the better schools. Interestingly, they find no evidence that lottery winners perform better on a wide range of academic measures (e.g., math and reading tests, enrollment, days absent) and some evidence that they are more likely to drop out of high school. The latter may be due to a mismatch between student ability and school demands. Despite the disappointing findings regarding academic outcomes, students who won lotteries to high achievement Chicago public schools reported nearly 60 percent fewer arrests on a ninth grade student survey. These winners also reported getting into less trouble at school, and school administrative data suggest that they had lower incarceration rates during school ages. Of course, it is possible that schools themselves affect student arrest and incarceration rates through differential disciplinary policies (or criminal opportunities), so it is important to study whether these reductions in arrests/incarceration persist beyond high school.

To this end, Deming (2009a) examines the impacts of open enrollment lotteries (for middle and high schools) on adult criminal outcomes seven years after random assignment.²⁸ Given his interest in the effects of school choice

27. In both cases, students could always choose to attend their neighborhood school. If any additional positions were available in a school, an open enrollment lottery was run based on all other students who applied to that school/program. Lotteries were random within population subgroups (e.g., by race or family income).

28. He merges Charlotte-Mecklenburg school district data with data on adult (ages sixteen+) arrests and incarceration from Mecklenburg County and the North Carolina Department of Corrections.

on crime, he categorizes males based on their likelihood of arrest, which he estimates as a function of demographic characteristics, earlier math and reading test scores, and other school-related behaviors at young ages. For his entire sample of middle and high school lottery participants, “high-risk” youth (defined as those in the top quintile of predicted arrest probability) have seven times more felony arrests (seven years after random assignment) than the average student from the bottom four quintiles combined.

Like Cullen, Jacob, and Levitt (2006), Deming (2009a) estimates significant effects of winning a school lottery on the quality of school attended, especially among high-risk youth, but no effects on achievement tests. There appears to be some effect on student enrollment during high school years, but there is no evidence that high-risk lottery winners are more likely to graduate from high school.²⁹ Among high school lottery winners in the high-risk category, Deming (2009a) estimates a significant 0.35 (roughly 45 percent) reduction in the number of adult felony arrests (cumulative as of seven years after the lottery) with an associated savings in victimization costs of \$4,600 to \$16,600.³⁰ Because many crimes do not lead to an arrest, the total benefits to potential victims and society are likely to be much larger. His estimates suggest that winning middle school lotteries also reduces crime among high-risk youth with most effects of a similar order of magnitude.

Court-ordered school desegregation policies enacted since *Brown vs. Board of Education of Topeka* in 1954 dramatically altered the types of schools blacks attended in many American districts. In most cases, the resources and average student achievement of schools attended by blacks would have improved markedly.³¹ Guryan (2004) estimates that these desegregation efforts significantly increased high school graduation rates among blacks by 2 to 3 percentage points but had no effect on white graduation rates. Weiner, Lutz, and Ludwig (2009) examine whether these changes affected county-level homicide rates.³² Their estimates suggest that homicide deaths among blacks ages fifteen to nineteen declined by 17 percent in the first five years after court-ordered desegregation, while homicide deaths among white fifteen to nineteen-year-olds declined by about 23 percent. Homicide deaths among slightly older whites and blacks also declined. In looking at

29. There is more evidence of effects on high school graduation and college attendance among the lower-risk quintiles.

30. These victimization costs (in year 2009 dollars) assign costs based on the type of offense using cost estimates from Miller, Cohen, and Wiersema (1996). They do not include justice system or enforcement costs. The larger figure uses a cost of \$4.3 million for murder, while the smaller uses a value of \$125,000 (twice the cost of rape).

31. For example, Reber (2007) shows that integration efforts in Louisiana from 1965 to 1970 were accompanied by large increases in per-pupil funding for black students.

32. They use data on homicide death by year and county over the period 1958 to 1988 from vital statistics and data on homicide victims and arrestees from the Supplemental Homicide Report from 1976 to 2003.

offenders, they find that arrest rates for homicide declined by one-third for blacks ages fifteen to nineteen, while there was no decline for young whites. Combining Guryan's (2004) estimated effect on high school graduation rates with the estimated effects of schooling on crime from Lochner and Moretti (2004), they argue that much of the effect may be coming from the increased schooling among blacks associated with desegregation.

For some perspective, it is interesting to compare these findings with those from the Moving-to-Opportunity (MTO) experiment, which provided housing vouchers to low-income families to move out of high poverty neighborhoods. Evaluations of MTO report that families receiving the housing vouchers moved into neighborhoods with about 25 percent lower poverty rates; however, these moves only led to modest improvements in the quality of schools youth attended and no improvements in their cognitive achievement (Sanbonmatsu et al. 2006). Kling, Ludwig, and Katz (2005) report that the MTO housing vouchers led to lasting reductions in arrests for both violent and property offenses among young females, short-term reductions in violent crime arrests for males, and delayed *increases* in property crime arrests for males. Overall, any reductions in crime were modest at best.

Taken together, these studies suggest that simply improving the schools attended by disadvantaged youth appears to be much more successful in reducing criminal activity (though not necessarily in improving academic outcomes) than changing neighborhoods. Given the mixed findings on educational attainment levels (with modest positive effects at best), the impacts of better schools on crime appear to be driven largely by school quality and not "quantity." Whether it is the quality of teachers and instruction or of student peers is less obvious. The fact that test scores did not improve among lottery winners suggests that the main effects of attending "better" schools on delinquency and crime are likely to be attributed to better socialization, better peer interactions, improvements in noncognitive skills, or changes in preferences. It is, therefore, interesting that substantial improvements in "neighborhood peers" do not yield the same benefits in terms of crime reduction.

10.3.3 Contemporaneous Schooling and Crime

We now consider the relationship between contemporaneous schooling and crime. As noted earlier, there are three main ways in which altering youth's schooling attendance is likely to affect their contemporaneous engagement in crime: (a) incapacitation, (b) raising the costs of future punishment through human capital accumulation, and (c) social interactions facilitated by bringing youth together. The incapacitation and human capital effects of schooling on crime are likely to be negative, while the sign of the social interaction effect is theoretically ambiguous.

Three relatively recent studies shed light on these effects by estimating the impacts of different "interventions" that directly affect youth schooling

attendance.³³ Anderson (2009) examines the effects of increasing state compulsory schooling ages on crime among affected youth (i.e., forcing some youth to stay in school), while Jacob and Lefgren (2003) and Luallen (2006) estimate the effects of extra days off from school due to teacher in-service days or teacher strikes (i.e., keeping youth out of school). The policies analyzed by these studies differ in two important respects. First, increases in compulsory schooling ages typically “require” students to stay in school at least one additional year and sometimes more, whereas teacher in-service days and strikes are of very short duration. Second, while teacher strikes and in-service days release all students from school, changes in compulsory schooling laws typically affect a small set of marginal students. All three potential effects of school attendance on crime are likely to be relevant to changes in compulsory schooling, while the effects of in-service days and teacher strikes are likely to be limited to incapacitation and social interactions. Any social interaction effects are likely to be magnified in the latter cases due to the universal nature of the policy.

Rather than use changes in compulsory schooling laws as instruments for educational attainment, Anderson (2009) estimates the direct effect of these laws on contemporaneous county-level arrest rates (from the UCR) from 1980 to 2006 among affected youth ages sixteen to eighteen. Specifically, his estimates are identified from within-county fluctuations in arrests (around county-specific trends) for sixteen to eighteen-year-olds (relative to thirteen to fifteen-year-olds) over time as state compulsory schooling ages change.

Anderson’s estimates for total arrest rates imply that a compulsory schooling age of seventeen significantly reduces age seventeen arrests by about 8 percent (5.4 arrests per 1,000 youth) compared to a compulsory schooling age of sixteen or less. Similarly, an age eighteen compulsory schooling age significantly reduces arrests by 9.7 to 11.5 percent at ages sixteen to eighteen. Separating arrests by type of offense, he estimates that compulsory schooling laws significantly reduce both property and violent arrests for sixteen to eighteen-year-olds. Although estimated effects of schooling age laws on drug-related crimes are sizeable, the effects are typically not statistically significant. Overall, the estimates generally suggest that forcing youth to spend an extra year or two in high school significantly reduces their arrest rates over that period.

Jacob and Lefgren (2003) examine the effects of single-day changes in school wide attendance on juvenile crime and arrest rates in twenty-nine large American cities from 1995 to 1999. Exploiting teacher in-service days

33. Using individual-level data, earlier studies by Gottfredson (1985), Farrington et al. (1986), and Witte and Tauchen (1994) explore the cross-sectional relationship between time spent in school and contemporaneous crime, concluding that time spent in school significantly “reduces” criminal activity. Unfortunately, these findings are difficult to interpret given the simultaneous nature of the crime and schooling choices.

across jurisdictions over time as an exogenous source of variation in school days, they essentially compare local juvenile crime rates on days when school is not in session to those when it is.³⁴ Their findings suggest that an additional day of school reduces serious juvenile property crime by about 14 percent that day while it *increases* serious juvenile violent crime by 28 percent. These results are consistent with an “incapacitation effect” of school that limits participation in property crime. However, the increased level of interaction among adolescents facilitated through schools may raise the likelihood of violent conflicts (and other minor delinquency) after school. Interestingly, they find no evidence to suggest that school days simply shift crime to other days without changing overall crime rates.

Luallen (2006) follows a similar approach, using teacher strikes (typically lasting about five days) rather than in-service days as an exogenous source of school days. Using data from the state of Washington for 1980 to 2001, Luallen (2006) estimates that an extra day of school reduces arrests for property crimes by about 29 percent, while increasing arrests for violent crimes by about 32 percent in urban areas. The effect on property crime is roughly double the effect estimated in Jacob and Lefgren (2003), while the effect on violent crime is quite similar. In rural and suburban areas, Luallen finds insignificant effects on both violent and property crime arrests. Thus, the incapacitation and social interaction effects appear to be particularly strong in urban areas and negligible elsewhere.

10.4 Evidence on Human Capital-Based Interventions from Birth to Young Adulthood

A growing body of evidence suggests that early childhood and school-age interventions can reduce adult crime rates. Most famously, the High/Scope Perry Preschool Program substantially lowered arrest rates through age forty for a sample of low-income minority children in Ypsilanti, Michigan. Several other early childhood interventions have produced similar effects on delinquency; however, others have not. We briefly review studies of early childhood and school-age interventions that have analyzed educational and criminal/delinquency outcomes during late adolescence or adulthood.³⁵ We then discuss a few programs aimed at improving school participation among adolescents or that directly provide training to adolescents and young adults.

Table 10.2 summarizes four small-scale early childhood interventions (Abecedarian Project, Chicago Child Parent Center [CPC], High/Scope Perry

34. Their main specification includes controls to account for the possibility that crime may be higher on certain days of the week or that different cities may experience different monthly crime cycles.

35. See Karoly et al. (1998) or Blau and Currie (2006) for more comprehensive surveys of early childhood programs.

Table 10.2 Effects of selected early childhood programs on educational attainment and adult crime

Program	Program description	Program population	Methodology	Education effects	Crime effects
Abecedarian Project	Full-time full-year preschool from infancy to kindergarten	Developmentally at-risk children; Chapel Hill, NC	Random assignment	Increased high school graduation rate by 0.03 (0.70 vs. 0.67) and enrollment in 4-year college by 0.22** (0.36 vs. 0.14)	No significant effects by age 21
Chicago Child Parent Center	Half-day preschool (school year) ages 3 and 4	Low-income minority children; Chicago, IL	Matched sample	Increased high school completion rate by 0.09 (0.57 vs. 0.48) for females and 0.14** (0.43 vs. 0.29) for males	By age 18, reduced fraction arrested by 0.08** (0.17 vs. 0.25)
High/Scope Perry Preschool	Half-day preschool (school year) ages 3 and 4, biweekly home visits	Low-income black children at risk of school failure; Ypsilanti, MI	Random assignment	Increased high school graduation rates by 0.52** (0.84 vs. 0.32) for females and reduced graduation rates by 0.04 (0.50 vs. 0.54) for males	By age 40, reduced fraction arrested 5 or more times by 0.10** (0.24 vs. 0.34) for females and 0.24** (0.45 vs. 0.69) for males
Infant Health & Development Program	Weekly/biweekly home visits from 0-36 months, full-time full-year pre-school 12-36 months	Low birth weight preterm infants, 8 sites	Random assignment	No significant effect on high school dropout (approximately 10% dropout rate)	No significant effects on arrests by age 18

Sources: Effects for Abecedarian Project taken from Campbell et al. (2002) and Clarke and Campbell (1998). Effects for Chicago Child Parent Center taken from Reynolds et al. (2001). Effects for Perry Preschool taken from Schweinhart et al. (2005). Effects for Infant Health & Development Program taken from McCormick et al. (2006).

**Significant at the 5 percent level.

Preschool, and Infant Health and Development Program [IHDP]), their target populations, study methodology, and estimated effects on educational attainment and crime at ages eighteen or older. All of the programs included a preschool component, ranging from full-time, full-year care from birth to kindergarten (Abecedarian) to half-day preschool at ages three and four (Chicago CPC and Perry Preschool). Perry Preschool and IHDP also included regular home visits at preschool ages as part of their curriculums.³⁶ All of the programs targeted youth facing some form of disadvantage. Abecedarian and Perry Preschool specifically targeted children at-risk of having problems developing normally in school. Children enrolling in the Chicago CPC were all minorities selected from families with low socioeconomic status (SES). The IHDP drew from a more heterogeneous population, targeting preterm children born of low birth weight (less than 2,500g). Overall, these studies cover a reasonably broad range of potential preschool-based interventions and target populations. (We discuss findings for Head Start in the following.)

Youth from all four of these programs were followed until at least age eighteen, enough time to determine whether the programs have medium-term effects on the education and criminal behavior of participants. Only the Chicago CPC was not evaluated using randomized trials; however, Reynolds et al. (2001) use a strong design of matching treated children with other comparison children based on age of kindergarten entry, eligibility for and participation in government-funded programs, and neighborhood and family poverty. Children from the matched comparison sample would also have been eligible for the program had they lived in a neighborhood with a center. Sample sizes range from around 100 children for Perry Preschool to 1,300 for Chicago CPC.

Both Chicago CPC and Perry Preschool significantly increased high school completion rates overall; however, the Chicago CPC had more sizeable effects on male graduation rates, while Perry Preschool only raised female graduation rates (Reynolds et al. 2001; Schweinhart et al. 2005). The IHDP had no effect on high school dropout rates by age eighteen, while Abecedarian increased college attendance but not high school completion (McCormick et al. 2006; Campbell et al. 2002). These programs typically produced short-term gains in achievement scores and sometimes generated lasting gains.

The final column of table 10.2 reports estimated effects of these programs on late juvenile and adult crime. As alluded to in the preceding, Perry Preschool had significant effects on lifetime crime measured as of age forty

36. All of the programs typically provided other additional services to families and children (e.g., nutritional and health services). While a subsample of the Abecedarian participants received an extended school-age intervention for the first few years of school, we focus on the preschool component of the program. The additional school-age services did not substantially impact the educational attainment or crime outcomes discussed here.

(Schweinhart et al. 2005). Reductions in the fraction arrested five or more times were substantial for both males and females. Both showed reductions of about one-third; however, the size of the effect in absolute terms is much larger for males given their higher baseline crime rate. Reductions in crime for Perry Preschool students were observed across a broad range of crimes (e.g., drug, property, and violent crimes) and were apparent even at younger ages. The Chicago CPC also reduced arrest rates (by age eighteen) by about one-third (Reynolds et al. 2001). Another widely cited family support and preschool program, the Syracuse University Family Development Research Program, showed significant reductions in juvenile delinquency measured at a slightly earlier age: 6 percent of preschool participants had been placed under probation services by age fifteen compared to 22 percent of controls (Lally, Mangione, and Honig 1988).³⁷ The estimated savings in reduced criminal justice expenditures and victimization costs resulting from the crime reductions of Perry Preschool and Chicago CPC are sizeable. Using a 3 percent discount rate, Belfield et al. (2006) estimate that the Perry Preschool produced a social benefit of over \$150,000 (year 2000 dollars) per child from crime reduction alone.³⁸ Reynolds et al. (2001) estimate that reductions in juvenile crime through age eighteen associated with the Chicago CPC saved society roughly \$8,000. Findings like these, especially those for Perry Preschool, led Donohue and Siegelman (1998) to conclude that small, rigorous early intervention programs may pay for themselves through reduced crime rates alone if they can be targeted to high-crime groups.

Not all early childhood programs in table 10.2 yield reductions in crime. While modest reductions in self-reported convictions and incarceration through age twenty-one were observed for Abecedarian, none of these effects are statistically significant (Campbell et al. 2002). Based on administrative records of adult criminal charges in North Carolina, Clarke and Campbell (1998) report nearly identical rates of arrests and criminal charges (as of age twenty-one, on average) for treatment and control children in the Abecedarian study. Similarly, IHDP produced no significant effects on crime through age eighteen (McCormick et al. 2006).

What is different about Abecedarian and IHDP that these programs did not produce the same reductions in crime? It is difficult to point to any

37. The Elmira Nurse Home Visitation Program provided home visits by nurses to first-time mothers who were young, unmarried, or of low SES. Nurses visited homes for randomly assigned mothers during pregnancy and for the first two years of the child's life. Olds et al. (1998) report mixed but encouraging effects of the program on delinquency at age fifteen: treated youth were more likely to self-report being stopped by the police but had fewer incidences of arrests and convictions.

38. This figure is for benefits through age forty. Using a 7 percent discount rate produces a social benefit from crime of about \$67,000 (Belfield et al. 2006). Heckman et al. (2009) report that savings from crime reduction account for about 40 to 65 percent of the benefit-cost ratio for Perry Preschool, depending on assumptions about discount rates (0 to 7 percent) and the cost of murder.

particular curriculum difference although not all preschools are alike. Abecedarian began preschool at infancy and continued through kindergarten—the longest of any program. It was also full-day, year-round, unlike Perry Preschool or Chicago CPC. Like Perry Preschool, it showed sizeable gains in achievement and IQ, so it is difficult to attribute its lack of effects on crime to inadequate intervention. The only obvious program difference between Abecedarian and Perry Preschool or Chicago CPC that might explain the absence of any impact on crime is its lack of a “home visit” component, but IHDP included home visits by nurses from birth through three years of age. The IHDP began early but also ended when Perry Preschool and Chicago CPC began (age three), so it is possible that the early home visit combined with later preschool care is a key combination of services necessary for long-term impacts on delinquency and crime.

An alternative hypothesis is that the environments more than the specifics of the programs were important in determining impacts on crime. Chapel Hill is a midsized, mostly white and relatively affluent university city in the South, while Ypsilanti is a smaller industrial city with a sizeable minority population. Chicago CPC sites were in low-income neighborhoods in a large urban midwestern city. (The IHDP had sites throughout the United States.) It seems quite possible that the same program might have different effects in each city. As noted by Barnett and Masse (2007), crime rates were 70 percent higher in Ypsilanti than Chapel Hill when the respective program participants would have been age fifteen. They speculate that there may have been little crime to prevent among the Abecedarian sample; however, Clarke and Campbell (1998) report that the two control samples (Perry and Abecedarian) had very similar arrest rates (around 40 percent) by their early twenties. McCormick et al. (2006) report that juvenile arrest rates among controls were similar for the IHDP and Chicago CPC as well. So, among the target populations for these programs, crime rates were fairly similar even if local crime rates were quite different. Of course, it is possible that the long-term effects of early childhood programs depend as much on the environment in which participants grow up as on individual and family characteristics of the participants themselves. If so, it is important to exercise caution in extrapolating benefits from any single program or community to the wider population.

Despite the fact that children targeted by all programs were disadvantaged, there is a sizeable difference in baseline educational attainment levels between Abecedarian and IHDP on the one hand and Chicago CPC and Perry Preschool on the other. High school graduation rates were 70 percent among Abecedarian controls; dropout rates (as of age eighteen) were only 10 percent among the IHDP controls. These both compare quite favorably with Chicago CPC and Perry Preschool controls who had high school completion rates ranging from 30 to 50 percent. Neither IHDP nor Abecedarian increased high school graduation rates. While Abecedarian improved

college attendance rates, this does not appear to be an important margin for crime (see figure 10.1). Given the tight link between high school dropout and crime discussed earlier, it may not be particularly surprising that Abecedarian and IHDP did not reduce crime given their negligible effects on high school completion. Yet Perry Preschool substantially reduced male crime rates without raising educational attainment among males. Clearly, early interventions may reduce delinquency and criminal behavior without significantly improving final schooling outcomes.

In the end, there is no easy explanation for the different findings across studies. While the results from these studies are individually powerful given their research designs (most are based on random assignment), it is difficult to draw strong conclusions overall about the efficacy of early childhood interventions as a national crime-fighting strategy. The fact that sample sizes are quite modest and that program populations are not necessarily representative of the United States raises additional questions. This itself may explain some of the variation in findings across studies. It is natural to ask how these programs would affect other populations. Questions about scalability have also been raised: can these programs and their effects be reproduced at a larger scale? These issues have led a number of researchers to analyze the largest early childhood program in the United States: Head Start. This program targets children from low-income families usually living in low-income communities and has served hundreds of thousands of children throughout the United States since its inception in 1967.

Because no large-scale long-term random assignment studies of Head Start are available, researchers have employed nonexperimental methods. These studies generally examine impacts on national samples of individuals served by Head Start, using data from the Panel Survey of Income and Dynamics (PSID) or Children of the National Longitudinal Survey of Youth (CNLSY). We next discuss those studies that examine the impacts of Head Start on behavioral problems, delinquency, or measures of adult crime.

Garces, Thomas, and Currie (2002) and Deming (2009b) use a family fixed effects approach to estimate the effects of Head Start on a variety of long-term outcomes. By comparing siblings who did and did not attend a Head Start program at ages three to five, they address important concerns about permanent or long-run differences across families that may affect decisions about preschool or Head Start enrollment.³⁹ Garces, Thomas, and Currie (2002) use data from the PSID, examining adult outcomes for individuals born between 1964 and 1977, while Deming (2009b) uses data from the CNLSY and examines outcomes for individuals born in the late 1970s and

39. Of course, they leave unanswered the question as to why some siblings enroll in Head Start, while others from the same family do not and, more important, whether different enrollment decisions are related to underlying differences in child abilities or other factors that may affect outcomes later in life.

early 1980s. Despite using the same empirical approach, the two studies find quite different patterns for Head Start impacts on educational attainment and criminal behavior. Garces, Thomas, and Currie (2002) estimate significant increases in high school completion (by 20 percentage points) and college attendance (by 28 percentage points) for whites only, while Deming (2009b) estimates an 11 percentage point increase in high school completion rates and a 14 percentage point increase in college attendance for blacks only. Excluding GED recipients, Deming (2009b) estimates a smaller (7 percentage points) and statistically insignificant effect on high school completion for blacks, suggesting that much of their apparent improvement in high school completion is due to increases in the GED.⁴⁰ Regarding crime, estimates by Garces, Thomas, and Currie (2002) suggest that Head Start reduces the probability of being booked or charged with a crime by about 12 percentage points among blacks, with no effect on whites. Deming (2009b) finds no significant effects of Head Start on crime for blacks or whites.⁴¹

Carneiro and Ginja (2008) use a regression discontinuity design to estimate the effects of Head Start on adolescent outcomes, including the probability that someone is sentenced for a crime. Their approach exploits the fact that Head Start imposes strict eligibility criteria related to family income and structure: children ages three to five are eligible if family income is below the federal poverty guidelines or if the family is eligible for public assistance. Because these criteria vary across states and time, the income thresholds vary across these dimensions as well. They exploit this exogenous variation in eligibility, assuming the effects of family income (when children are ages three to five) on subsequent outcomes are continuous. Using data from the CNLSY on youth who would have enrolled in Head Start during the 1980s and 1990s, they estimate that participation in Head Start at ages three to five significantly reduces the probability (by 31 percentage points) a sixteen to seventeen-year-old male is sentenced for a crime (based on self-reports). They estimate similar effects for a sample of blacks only. These estimates measure the effect of Head Start on children who were at the margin of eligibility for the program and, therefore, represent the effects we might expect with modest expansions of the program.

Altogether, the nonexperimental evidence on Head Start appears to suggest some long-term effects on education and crime, but findings vary in important ways across studies.⁴² The strongest effects on crime appear to

40. The substitution between high school degrees and GED receipt is less relevant for the earlier cohort studied by Garces, Thomas, and Currie (2002) because the GED was much less common in the 1980s relative to more recent years.

41. His measure of crime is an indicator equal to one if the respondent reports having been convicted of a crime, been on probation, sentenced by a judge, or is in prison at the time of the interview.

42. While Head Start may affect juvenile and adult crime even if it has no effect on educational attainment (as with males in the Perry Preschool program), one might speculate that any increases in schooling (especially high school years) associated with Head Start should

exist for blacks although Deming (2009b) finds no effect on crime for either blacks or whites. Combined with the evidence from smaller-scale programs evaluated by randomized trials, there is limited but important evidence that early childhood interventions can reduce crime later in life for youth from disadvantaged backgrounds.

A recent program, Fast Track, introduced in four sites around the United States, provides group- and individual-based services to children from grades one through ten. The program specifically targets children from high crime and poverty neighborhoods who exhibit conduct problems in kindergarten, with the primary aim of preventing antisocial behavior and psychiatric disorders. The program focuses on three elements of development: social and cognitive skills, peer relationships, and parenting. During early grades, parents were offered training and home visits to help improve parenting skills, while children were engaged in group activities to foster friendships and tutoring sessions in reading. As children aged, more individualized services were provided, along with group sessions aimed at dealing with the transition to middle school, resistance to drugs, and so on. The program also incorporated a classroom intervention during grades one to five at schools with program children. Teachers implemented two to three sessions per week designed to promote social and emotional competence and to reduce aggression. Experimental estimates based on random assignment suggest that the program produced sustained improvements in conduct disorders and antisocial behavior over grades three to nine (Conduct Problems Prevention Research Group [CPPRG] 2007). As of grade nine, high risk youth (those from the top 3 percent of conduct problems in kindergarten) receiving the Fast Track program showed significant reductions in self-reported delinquency and criminal behavior; however, no significant effects on antisocial behavior were found for other youth.⁴³ Two recent follow-up studies (CPPRG, forthcoming, 2010) suggest that the reductions in crime and conduct problems extend at least two years beyond the conclusion of the program (last measured at grade twelve/age nineteen) and continue to be focused on youth that were initially “high risk.” Effects on juvenile conduct

lead to reductions in crime as estimated by Lochner and Moretti (2004). Under this assumption, estimates from Ludwig and Miller (2007), which suggest that roughly doubling Head Start spending (per capita) increases high school completion rates by as much as 4 percentage points, imply that this policy should also reduce arrest rates by up to 3 to 4 percent. Of course, multiplying the Garces, Thomas, and Currie (2002) estimated effects of Head Start on schooling attainment among whites by Lochner and Moretti's (2004) estimated effects of education on crime suggests that Head Start attendance should significantly reduce incarceration rates among whites, while analogous estimates from Deming (2009b) suggest that Head Start should reduce crime among blacks. Yet these studies estimated no effect of Head Start attendance on self-reported measures of arrest, conviction, or incarceration rates for these populations.

43. Results for antisocial behavior are based on an index created from self-reports of serious delinquent/criminal actions like stealing something worth more than \$100, assault, selling heroin or lysergic acid diethylamide (LSD), and sexual assault.

disorders did not appear to decline after the program, while effects on crime showed some fade-out.

Experimental evaluations of two earlier, more limited elementary school-age interventions are worth commenting on because they also focused largely on social development among high-risk children. The Montreal Longitudinal Experimental Study provided social skills training to first and second grade children, along with teacher and parent training over those same years. Boisjoli et al. (2007) report that by age twenty-four, children receiving the intervention (compared to control children) were twice as likely to have completed high school and only half as likely to have a criminal record. The Seattle Social Development Project intervened over a longer period (grades one to six); however, it only provided teacher and parent training (aimed at improving child social and emotional skill development). As of age twenty-one, Hawkins et al. (2005) estimate that the six-year intervention had increased high school graduation rates from 81 to 91 percent and significantly reduced self-reported crime and official lifetime court charges (from 53 to 42 percent).

Altogether, the evidence from Fast Track, the Montreal Longitudinal Experimental Study, and the Seattle Social Development Project suggests that comprehensive school-age programs designed to improve social development can produce lasting impacts on educational attainment, conduct disorders, and criminal behavior. In many ways, these programs emphasized social over cognitive development, relative to the preschool programs summarized in table 10.2. Of course, both sets of programs were broad-based and yielded improvements in both domains.⁴⁴

Programs targeted to older adolescents and young adults have shown mixed results. The Quantum Opportunity Program provided entering high school students with a mentor/tutor that aided them in schoolwork and community activities for four years. Financial incentives designed to encourage high school graduation and college enrollment were provided for educational, service, and developmental activities. A recent random assignment evaluation of the program reported no significant improvements in schooling or reductions in crime six years after scheduled high school graduation (Schirm, Stuart, and McKie 2006). In part, this may be due to the relatively low participation by youth in program activities.⁴⁵

The Job Corps provides intensive basic educational and vocational training for economically disadvantaged youth and young adults ages sixteen to twenty-four throughout the United States. The program also offers a wide range of other services (e.g., counseling, social skills training, health educa-

44. This is largely consistent with recent estimates of skill production functions for both cognitive and “noncognitive” skills (e.g., see Cunha and Heckman 2008).

45. On average, youth spent only 177 hours per year on educational, community, and developmental activities. Roughly one in four spent no time at all in these activities by the fourth year of the program.

tion, job placement services). The average participant is enrolled for about eight months, with most living in residence at training sites. The program's primary goal is to improve employment and earnings prospects. Based on a recent random assignment evaluation, Schochet, Burghardt, and Glazerman (2001) conclude that the program produced modest positive impacts on postprogram employment and earnings. The program also reduced self-reported arrest rates by about 30 percent during the first year after random assignment, when most youth would have been enrolled. Reductions in subsequent years were smaller and statistically insignificant. The program also significantly reduced conviction rates by about 17 percent during the four years following random assignment.⁴⁶ Conclusions from the less-expensive and nonresidential JOBSTART program are largely consistent with these findings (Cave et al. 1993).⁴⁷

Collectively, these studies indicate that human capital-based interventions from early childhood to early adulthood can reduce juvenile and adult crime, at least for some populations. To understand why, it is useful to briefly return to the model laid out in section 10.2 to aid in interpreting these findings. The model suggests that effective interventions may reduce juvenile and adult crime by improving child learning productivity, A , increasing adolescent human capital levels, H_1 , or by socializing children (i.e., lowering θ). While preschool programs highlighted in table 10.2 may raise learning abilities, achievement gains are generally short-lived and limited to primary school ages. Evidence of reduced criminal activity among adolescents attributed to early intervention programs, suggests that these programs raise initial market skills (H_1) or reduce criminal returns (θ) through socialization. School-based programs for high-risk youth like Fast Track emphasized social development (i.e., lowering θ) over cognitive achievement; yet they also likely improved adolescent human capital levels H_1 . Despite the difference in emphasis between the two types of programs, both have shown the ability to significantly reduce juvenile and adult crime. Job training programs for adolescents and young adults directly operate on the incentives to invest in human capital (analogous to an increase in the subsidy rate s in our model)

46. An earlier study by Long, Mallar, and Thornton (1981) estimated that the social benefits from reduced criminal activity among Job Corps participants amounted to over \$7,000 (in 2008 dollars) per participant—almost 30 percent of the total social benefit of the program.

47. JOBSTART offered many of the same basic components of the Job Corps to a similar population. Cave et al. (1993) find modest (and statistically insignificant) positive effects on earnings three to four years after random assignment for the full sample; however, earnings increased roughly 25 percent (in years three and four) for male participants with a prior arrest (i.e., had an arrest since age sixteen but prior to random assignment). Among male participants with no prior arrest, the program significantly reduced self-reports of an arrest (6.4 percentage points or 36 percent) during the first year after random assignment (i.e., the training year) but did not reduce the fraction arrested in subsequent years. Among males with a prior arrest, the program (insignificantly) reduced the fraction reporting an arrest over the first four years after random assignment by about 8 percent and had negligible effects on arrests during the first year. There were no significant effects on arrests for female participants.

and have led to modest reductions in crime during periods of heavy training. These programs have produced only modest increases in earnings and negligible long-run effects on crime, however, suggesting that simply training low-skilled adolescents does not provide the same promise as earlier interventions that act on individual endowments.

10.5 Policy Lessons

In this section, we discuss a number of important policy lessons regarding human capital policies and crime. First, we summarize evidence on the social savings from crime reduction that we might expect from policies that increase educational attainment or enrollment, improve school choice and quality, or expand access to early childhood interventions. Second, we highlight a few subpopulations and schooling margins that are likely to yield the greatest social gain from crime reduction. Finally, we discuss a few other lessons based on the evidence.

10.5.1 Valuing the Social Benefits from Crime Reduction

Lochner and Moretti (2004) estimate that increasing educational attainment levels in the population yields sizeable social benefits. Specifically, they calculate the social savings from crime reduction that would result from a 1 percentage point increase in high school graduation rates in the United States. Table 10.3 summarizes their exercise, translating all dollar values into 2008 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). Column (1) reports total costs per crime associated with murder, rape, robbery, assault, burglary, larceny/theft, motor vehicle theft, and arson.⁴⁸ Column (2) reports the predicted change in total U.S. arrests based on the Lochner and Moretti (2004) offense-specific arrest estimates discussed earlier and the total number of arrests in the 1990 UCRs. Column (3) adjusts the arrest effect in column (2) by the number of crimes per arrest. In total, nearly 100,000 fewer crimes would have taken place in 1990 if high school graduation rates had been 1 percentage point higher. The implied social savings from reduced crime are shown in column (4). Savings from murder alone are as high as \$1.7 billion. Savings from reduced assaults amount to nearly \$550 million. Because the estimates suggest that schooling increases rape and robbery offenses, increased costs associated with these crimes partially offset the benefits from reductions in other crimes.

The final row reports the total savings from reductions in all eight types of crime. Because these figures only include a partial list of crimes (e.g., nearly 25 percent of all prisoners in 1991 were incarcerated for drug offenses according to the U.S. Department of Justice [1994]) and do not include all

48. These costs include incarceration and victim costs. See notes to table 10.3 or Lochner and Moretti (2004) for details.

Table 10.3 Social benefits of increasing high school completion rates by 1 percent

	Total cost per crime	Estimated change in arrests	Estimated change in crimes	Social benefit
Violent crimes				
Murder	4,506,253	-373	-373	1,683,083,243
Rape	132,938	347	1,559	-207,270,899
Robbery	13,984	134	918	-12,839,495
Assault	14,776	-7,798	-37,135	548,690,721
Property crimes				
Burglary	1,471	-653	-9,467	13,920,409
Larceny/theft	295	-1,983	-35,105	10,347,853
Motor vehicle theft	1,855	-1,355	-14,238	26,414,558
Arson	58,171	-69	-469	27,302,131
Total		-11,750	-94,310	2,089,648,519

Source: Lochner and Moretti (2004).

Notes: These costs reflect incarceration and victim costs. Victim costs are taken from Miller, Cohen, and Wiersema (1996). Incarceration costs per crime equal the incarceration cost per inmate multiplied by the incarceration rate for that crime (approximately \$25,000). Incarceration rates by offense type are calculated as the total number of individuals in jail or prison (from U.S. Department of Justice 1994) divided by the total number of offenses that year (where the number of offenses are adjusted for nonreporting to the police). Incarceration costs per inmate are taken from U.S. Department of Justice (1999). All dollar figures are translated into 2008 dollars using the CPI-U.

costs associated with each crime (e.g., private security measures are omitted), these amounts are likely to underestimate the true social benefit associated with increasing high school graduation rates. Still, the savings are substantial: the social benefits of a 1 percentage point increase in male U.S. high school graduation rates (from reduced crime alone) in 1990 would have amounted to more than \$2 billion. This represents more than \$3,000 in annual savings *per additional male graduate*.

Open school enrollment lotteries and desegregation efforts also appear to reduce crime rates by improving school quality. Deming (2009a) estimates that reductions in arrests associated with offering better quality school options to a high-risk youth produces a roughly \$16,000 social savings to victims over the next seven years. Because better schools are also likely to have reduced crimes that never led to an arrest, total victimization savings are likely to be even greater. Total social savings should be still larger once savings on prisons and other crime prevention costs are factored in.

The effects of school attendance on contemporaneous juvenile crime rates are more complicated. Studies estimating the effects of day-to-day changes in attendance suggest that in urban communities additional school days reduce property crime while increasing violent crime (Jacob and Lefgren 2003; Luallen 2006). Overall, the social costs associated with increased violence are likely to dominate the benefits from reduced property crime. On the other

hand, Anderson (2009) estimates reductions in both violent and property juvenile crime associated with increases in compulsory schooling ages. Thus, his findings suggest an overall social savings from juvenile crime reduction although he does not attempt to put a dollar value on the effects.

Evidence on the effects of early childhood and school-age interventions are mixed. Long-run impacts on juvenile delinquency and adult crime can be substantial for disadvantaged youth. For example, estimates suggest that Perry Preschool produced a social benefit from crime reduction of roughly \$150,000 per child (through age forty). On the other hand, Abecedarian produced no significant impacts on crime. In choosing between programs or policies, it is, of course, important to incorporate the wide-ranging benefits of early childhood programs (e.g., higher earnings, better health, etc.). Even if early interventions are not more cost-effective in reducing crime when compared against more traditional law enforcement or justice system policies, they may provide greater total social value once all benefits are considered.

10.5.2 Where Are the Big Returns?

Given the most sizeable reductions in crime appear to result from the final years of high school, policies that encourage high school completion would seem to be most promising in terms of their impacts on crime.⁴⁹ Because crime rates are already quite low among high school graduates, policies that only encourage college attendance or completion are likely to yield much smaller social benefits from crime reduction although they may be desirable on other grounds.⁵⁰ To the extent that postsecondary education policies (e.g., lowering college costs) reduce crime, much of their effect may actually come through encouraging disaffected high school students to graduate rather than drop out.

In general, policies designed to encourage schooling among more crime-prone groups are likely to produce the greatest benefits from crime reduction. Consistent with this, the school-age Fast Track program appears to have reduced juvenile crime only among very high-risk children, showing little impact on even moderately high-risk children (CPPRG 2007, 2010). Similarly, Deming (2009a) estimates that improved school choice for middle and high school students leads to significant reductions in arrests for high-risk youth but not for others. As Donohue and Siegelman (1998) conclude, the overall efficiency of early childhood programs as a crime-fighting strat-

49. See Hanushek and Lindseth (2009), Jacob and Ludwig (2008), or Murnane (2008) for recent discussions of policies to improve schooling outcomes in the United States.

50. The fact that crime declines substantially with high school completion but not college attendance suggests that net expected returns from crime for most individuals lie somewhere between the wages of high school dropouts and graduates. See Freeman (1999) for a summary of evidence regarding criminal wages and earnings.

egy is likely to depend heavily on the ability to target high-risk children at very young ages. The same is likely to be true for school-age interventions.

Social benefits from crime reduction also vary across gender and race. Men commit much less crime than women, on average. Thus, it is not surprising that crime-related benefits from education policies and interventions are typically much smaller for females than males (e.g., Perry Preschool, Job Corps). This is true even though programs sometimes reduce female and male crime rates by similar amounts in percentage terms. While there are no studies to date comparing the impacts of educational attainment on female versus male crime rates, there would have to be a substantially larger proportional effect on female crime rates to produce overall crime reductions to rival those estimated for men. Among men, Lochner and Moretti (2004) estimate much larger effects of additional schooling on incarceration rates among blacks relative to whites. Garces, Thomas, and Currie (2002) estimate that Head Start significantly reduces crime for blacks but not whites; however, Deming (2009b) estimates no effect on crime for either group, while Carneiro and Ginja (2008) estimate similar large effects on both. Because crime rates are much higher among blacks than whites, on average, policies would generally need to produce much larger proportional reductions in white crime rates to achieve similar absolute reductions in crime. None of the evidence surveyed here suggests that this is the case.

10.5.3 Additional Policy Lessons

A few other useful lessons can be drawn from the studies surveyed here.

First, education policies can reduce property crime as well as violent crime. In the United States, the estimated effects of educational attainment or school enrollment on property and violent offenses appear to be quite similar in percentage terms (Lochner and Moretti 2004; Anderson 2009).⁵¹ Even murder appears to be quite responsive to changes in educational attainment and school quality (Lochner and Moretti 2004; Weiner, Lutz, and Ludwig 2009).

Second, higher wages increase the opportunity costs (including work foregone while incarcerated) of both property and violent crime. Lochner and Moretti (2004) show that the estimated effects of educational attainment on crime can largely be accounted for by the effects of schooling on wages and the effects of wages on crime. This is important because it suggests that policymakers can reduce crime simply by increasing labor market skills; they need not alter individual preferences or otherwise socialize youth. Of course, as evidence from the Job Corps and other training programs suggests, this is not necessarily an easy task. Training programs targeted at low-skill adolescents and young adults have modest (at best) effects on earnings and crime.

51. Estimates from Machin, Marie, and Vujic (2010) suggest that education reduces property crime more than violent crime in the United Kingdom.

On the other hand, encouraging youth to finish high school (e.g., through compulsory schooling laws) appears to substantially increase earnings and reduce crime. Preventing early school dropout is likely to be more successful than trying to compensate for dropout a few years later.

Third, education-based policies need not increase educational attainment to reduce crime. Studies on school choice lotteries (Cullen, Jacob, and Levitt 2006; Deming 2009a) suggest that providing disadvantaged urban youth with better schools can substantially reduce juvenile and adult crime, even if it has small effects on educational outcomes. Perry Preschool had no effect on male schooling levels but substantially reduced male crime rates through age forty (Schweinhart et al. 2005).

Fourth, evidence that violent crime is higher on school days than non-school days in urban districts suggests that social interaction effects are particularly important for juvenile violent crime (Jacob and Lefgren 2003; Luallen 2006). Smart policing efforts may be able to help address some of the problems associated with schools releasing lots of adolescents at the same time. For example, an increased police presence immediately after school or other major adolescent congregations let out may be warranted. Or, on nonschool days, it may be wise for police to focus more on targets or areas of juvenile property crime, worrying less about violent crime. The “hot spot” or “problem-oriented policing” literature in criminology suggests that informed targeting of police efforts to high crime areas (and, by extension, times) can be effective at reducing overall crime rates.⁵² Alternatively, it may be useful to consider ways of designing after-school youth programs or other weekend activities to minimize violent behavior afterward.

10.6 Conclusions

There is growing evidence that improvements in school quality and increases in educational attainment, especially high school completion, reduce adult violent and property crime rates. Policies that induce students to spend an extra year or more attending school also appear to reduce juvenile crime. These findings are broadly consistent with a human capital-based model of crime and work. For most types of crime, additional schooling is likely to raise legitimate wage rates much more than the returns to crime, thereby discouraging the latter. Lochner and Moretti (2004) argue that the reductions in violent and property crime associated with increased schooling is roughly equivalent to the effect of education on wages multiplied by the effect of increased wages on crime. Thus, most of the effect of education on violent and property crime may come from increased wages. By contrast, education may increase the returns to white-collar crime more than the returns to work.

52. For a recent survey of this literature, see Braga (2005).

Consistent with this, Lochner (2004) finds that arrest rates for white-collar crime increase when education levels rise.

Education-based programs may also socialize youth, reducing personal or psychic rewards from crime. Emphasizing social and emotional development, school-age programs like Fast Track have shown the ability to significantly reduce later conduct disorders and crime (among high-risk children). These programs also improved educational outcomes, which may explain some of their impacts on crime. Perry Preschool reduced male (and female) crime rates without affecting male schooling outcomes. Thus, the program appears to have improved social development or increased early skill levels (without noticeably affecting subsequent schooling investments). Evidence from school choice lotteries suggests that improvements in school and peer quality can lead to reductions in crime without raising student achievement or educational attainment. The most likely explanation for the reduction in crime is that higher quality schools better socialize youth or provide them with a better set of peers. Yet evidence from the MTO experiment suggests that moving families to lower poverty neighborhoods does not produce the same reductions in crime, complicating any explanation related to peer effects or social networks.

Education may also increase patience or alter preferences for risk; however, neither seems to be central to the estimated impacts on crime. Property crimes are generally associated with less than one month of expected time in jail or prison conditional on being sentenced (see table 10.1), hardly enough time for modest changes in patience to play much of a role. Property crimes also have very low expected probabilities of arrest (typically less than 10 percent chance) and even lower probabilities of incarceration (typically less than 1 percent), so there is little actual uncertainty in outcomes associated with these crimes (see table 10.1). Yet estimated impacts of schooling on property crime are similar to those for violent crime, which entails much longer and more uncertain prison sentences.

Altogether, the evidence suggests that while efforts to socialize youth can be effective, simply providing them with valuable market skills can discourage them from choosing a life of crime. In terms of crime reduction, human capital-based policies that target the most disadvantaged (and crime-prone) are likely to be the most efficient, while also promoting a more equitable society. To that end, increasing high school graduation rates and improving our nation's worst inner-city schools are likely to yield the greatest social return.

Although policies that increase school attendance for a year or more (e.g., increased compulsory schooling ages) appear to reduce both violent and property crime (Anderson 2009), a few extra days off from school may actually lead to reductions in violent crime, especially in urban areas (Jacob and Lefgren 2003; Luallen 2006). From a human capital perspective, the increased opportunities that open up for youth attending an additional year

of schooling should raise the future costs of incarceration associated with juvenile crime. This may serve as an important additional criminal deterrent that does not exist for day-to-day changes in the school calendar. In general, the effects of longer periods of attendance on contemporaneous juvenile crime are consistent with the subsequent effects of additional schooling on adult crime. The evidence on day-to-day changes in the school calendar highlights the possibility that by bringing many adolescents together, schools may foster negative interactions that lead to violence after school is out. Schools may also bring youth together who then look for trouble once they leave school grounds. Policies that find ways to address these problems may be effective at reducing juvenile violence after school. For example, after-school programs may help keep youth busy long enough to prevent some after-school violence, or they may simply delay the problems. Police might be deployed differently on school days and nonschool days, focusing on violent juvenile activity on school days and juvenile property crime on nonschool days.

There are many ways by which early childhood interventions may affect juvenile and adult crime. The human capital approach favored in this chapter highlights the potential effects of these programs on learning abilities, adolescent skill levels, and socialization or tastes for crime. These programs may also affect childhood preferences, including risk aversion, patience, or self-control. While a few early childhood programs have produced sizeable reductions in both juvenile and adult crime—most famously, Perry Pre-school—other quite similar programs have not. School-age interventions focused on developing social and emotional skills have proven successful at reducing later conduct disorders and crime, especially among very high-risk children. The benefits from reduced crime associated with successful programs certainly warrant the attention they have received, yet we still need to know much more about why other programs have not produced the same effects. Is it the curriculum, the population served, or the later school and postschool environment faced by program participants? Two things are clear. First, preschool and school-age programs have substantially reduced crime for some disadvantaged high-risk populations. Even if these gains cannot be expected in all cases, they are large enough to warrant careful consideration on a broader scale. Second, successful programs did not always increase educational attainment, even when they significantly reduced juvenile and adult crime rates. Thus, disappointing achievement or educational outcomes need not imply the absence of benefits from crime reduction.

Given current evidence, it is difficult to draw strong conclusions about the relative benefits of trying to target and “treat” children at very young ages versus intervening at later ages to keep adolescents from dropping out of high school. Of course, we need not choose one or the other. Indeed, both are likely to be important components of a broad-based national crime-

fighting agenda. Calculations by Lochner and Moretti (2004) and Donohue and Seigelman (1998) suggest that both human capital-oriented policies are competitive with more traditional law enforcement and incarceration efforts when all benefits are considered.

Appendix

Comparative Statics Results

In this appendix, we derive comparative static results for the model discussed in the chapter. We first derive results for the “fully interior” case where optimal investment and crime decisions satisfy $0 < I, c_1, I + c_1, c_2 < 1$. Then we derive results for the case when adolescents choose not to work, so optimal investment and crime satisfies $I + c_1 = 1$ and $0 < I, c_1, c_2 < 1$.

Following the text, assume that $N(\cdot)$ and $h(\cdot)$ are twice continuously differentiable in all arguments and that $N_c > 0, N_{cc} < 0, N_{c\theta} > 0, N_H \geq 0, N_{HH} \leq 0, N_{cH} \geq 0, h_I > 0, h_{II} < 0, h_{IH} > 0, h_{IA} > 0, h_H > 0$, and $h_A \geq 0$.

Case 1: Working Adolescents

Define the individual objective function to be maximized with respect to I, c_1, c_2 :

$$F(I, c_1, c_2; A, H_1, \theta, s) \equiv [H_1(1 - I - c_1) + sI + N(c_1, H_1; \theta)] + R^{-1}[H_2(1 - c_2) + N(c_2, H_2; \theta)].$$

At an interior optimum (i.e., optimal investment and crime satisfy $0 < I, c_1, I + c_1, c_2 < 1$):

$$F_I = -H_1 + s + R^{-1}[(1 - c_2) + N_H(c_2, H_2; \theta)]h_I(I, H_1; A) = 0$$

$$F_{c_1} = -H_1 + N_c(c_1, H_1; \theta) = 0$$

$$F_{c_2} = R^{-1}[-H_2 + N_c(c_2, H_2; \theta)] = 0.$$

Assuming $s < H_1$ combined with $F_I = 0$ implies that $1 - c_2 + N_{H_2} > 0$ at an optimum. Given $N_H \geq 0$, this is necessary for optimal $c_2 \in (0, 1)$ as required for an interior solution. We use the fact that $1 - c_2 + N_{H_2} > 0$ at an interior optimum repeatedly throughout this appendix without further reference.

Second-order conditions for a maximum require a negative definite hessian matrix of second derivatives for F :

$$H = \begin{pmatrix} F_{II} & F_{Ic_1} & F_{Ic_2} \\ F_{Ic_1} & F_{c_1c_1} & F_{c_1c_2} \\ F_{Ic_2} & F_{c_1c_2} & F_{c_2c_2} \end{pmatrix},$$

where $F_{Ic_1} = F_{c_1c_2} = 0$ and

$$\begin{aligned}
 F_{II} &= R^{-1}[(1 - c_2 + N_{H_2})h_{II} + N_{H_2H_2}h_I^2] < 0 \\
 F_{c_1c_1} &= N_{c_1c_1} < 0 \\
 F_{c_2c_2} &= R^{-1}N_{c_2c_2} < 0 \\
 F_{Ic_2} &= R^{-1}h_I(N_{H_2c_2} - 1).
 \end{aligned}$$

Condition 1 implies that $F_{Ic_2} \leq 0$; otherwise, $F_{Ic_2} > 0$ when Condition 1 does not hold.

Result 1: Effects of s

Using Cramer’s rule, observe that $\partial I/\partial s = -F_{c_1c_1}F_{c_2c_2}/|H| > 0$, because $F_{Is} = 1, F_{c_1s} = F_{c_2s} = 0$, and $|H| < 0$ at an optimum (second-order condition [SOC] for a maximum). We obtain $\partial c_1/\partial s = 0$ and $\partial c_2/\partial s = F_{c_1c_1}F_{Ic_2}/|H|$. If Condition 1 holds, then $\partial c_2/\partial s \leq 0$; otherwise, $\partial c_2/\partial s > 0$.

Result 2: Effects of A

Notice $F_{c_1A} = 0$:

$$\begin{aligned}
 F_{IA} &= R^{-1}h_{IA}(1 - c_2 + N_{H_2}) + R^{-1}h_Ih_A N_{H_2H_2} \\
 F_{c_2A} &= R^{-1}h_A(N_{c_2H_2} - 1).
 \end{aligned}$$

The first term in F_{IA} is greater than zero at an optimum; however, the second term is generally negative. Yet for $h_I < \infty$ and $h_A < \infty$, there exists some small $\varepsilon > 0$ for which $F_{IA} > 0$ if $N_{HH} > -\varepsilon$ by continuity. $F_{c_2A} \leq 0$ if Condition 1 holds; otherwise, it is strictly greater than zero.

Applying Cramer’s rule, we obtain $\partial I/\partial A = -F_{c_1c_1}(F_{IA}F_{c_2c_2} - F_{c_2A}F_{Ic_2})/|H|$. Thus, $\partial I/\partial A > 0$ if $N_{HH} > -\varepsilon$. We also obtain $\partial c_1/\partial s = 0$ and $\partial c_2/\partial A = -F_{c_1c_1}(F_{II}F_{c_2A} - F_{Ic_2}F_{IA})/|H|$. If Condition 1 holds, then $\partial c_2/\partial A \leq 0$; otherwise, $\partial c_2/\partial A > 0$.

Result 3: Effects of H_1

Notice:

$$\begin{aligned}
 F_{IHH_1} &= -1 + R^{-1}[h_{IHH_1}(1 - c_2 + N_{H_2}) + h_I N_{H_2H_2}(1 + h_{H_1})] \\
 F_{c_1H_1} &= N_{H_1c_1} - 1 \\
 F_{c_2H_1} &= R^{-1}(1 + h_{H_1})(N_{c_2H_2} - 1).
 \end{aligned}$$

While $F_{c_1H_1} \leq 0$ and $F_{c_2H_1} \leq 0$ if Condition 1 holds (otherwise, both are positive), it is not possible to generally sign F_{IHH_1} . As such, it is not possible to sign $\partial I/\partial H_1$ and $\partial c_2/\partial H_2$. Using Cramer’s rule, one can show that $\partial c_1/\partial H_1 = -F_{c_1H_1}(F_{II}F_{c_2c_2} - F_{Ic_2}^2)/|H|$. Because $F_{II}F_{c_2c_2} - F_{Ic_2}^2 > 0$ at a maximum, $\partial c_1/\partial H_1 \leq 0$ if Condition 1 holds; otherwise, $\partial c_1/\partial H_1 > 0$.

Result 4: Effects of θ

Notice:

$$\begin{aligned}
 F_{I\theta} &= R^{-1}h_I N_{\theta H_2} \\
 F_{c_1\theta} &= N_{c_1\theta} > 0 \\
 F_{c_2\theta} &= R^{-1}N_{c_2\theta} > 0.
 \end{aligned}$$

Cramer’s rule implies that $\partial c_1/\partial\theta = -F_{c_1\theta}(F_{II}F_{c_2c_2} - F_{Ic_2}^2)/|H| > 0$ because $F_{II}F_{c_2c_2} - F_{Ic_2}^2 > 0$ at a maximum. Furthermore, $\partial I/\partial\theta = -F_{c_1c_1}(F_{I\theta}F_{c_2c_2} - F_{Ic_2}F_{c_2\theta})/|H|$ and $\partial c_2/\partial\theta = -F_{c_1c_1}(F_{II}F_{c_2\theta} - F_{I\theta}F_{Ic_2})/|H|$. If Condition 1 holds and $N_{H\theta} \leq 0$, then $\partial I/\partial\theta \leq 0$ and $\partial c_2/\partial\theta > 0$. If Condition 1 does not hold and $N_{H\theta} > 0$, then $\partial I/\partial\theta > 0$ and $\partial c_2/\partial\theta > 0$.

Case 2: Nonworking Adolescents

We now consider the problem when optimal investment and first-period crime satisfy $I + c_1 = 1$. Imposing $c_1 = 1 - I$, the individual objective function to be maximized with respect to I and c_2 is

$$\begin{aligned}
 G(I, c_2; A, H_1, \theta, s) &\equiv [sI + N(1 - I, H_1; \theta)] \\
 &\quad + R^{-1}[H_2(1 - c_2) + N(c_2, H_2; \theta)].
 \end{aligned}$$

At an interior optimum (i.e., optimal investment and crime satisfy $0 < I, c_2 < 1$)

$$G_I = s - N_c(1 - I, H_1; \theta) + R^{-1}[(1 - c_2) + N_H(c_2, H_2; \theta)] h_I(I, H_1; A) = 0$$

$$G_{c_2} = R^{-1}[-H_2 + N_c(c_2, H_2; \theta)] = 0.$$

For optimal $c_2 \in (0, 1)$, it must be the case that $1 - c_2 + N_{H_2} > 0$. This implies that $s < N_c(1 - I, H_1, \theta)$ at an optimum because $G_I = 0$. We use the fact that $1 - c_2 + N_{H_2} > 0$ (at an optimum) repeatedly in the following.

Second order conditions for a maximum require negative definite

$$\tilde{H} = \begin{pmatrix} G_{II} & G_{Ic_2} \\ G_{Ic_2} & G_{c_2c_2} \end{pmatrix}$$

with $|\tilde{H}| > 0$, where

$$G_{II} = N_{c_1c_1} + R^{-1}[(1 - c_2 + N_{H_2}) h_{II} + N_{H_2H_2} h_I^2] < 0$$

$$G_{c_2c_2} = R^{-1}N_{c_2c_2} < 0$$

$$G_{Ic_2} = R^{-1}h_I(N_{H_2c_2} - 1).$$

Condition 1 implies that $G_{Ic_2} \leq 0$; otherwise, $G_{Ic_2} > 0$.

We derive comparative statics results for I and c_2 in the following. Because $c_1 = 1 - I$, $\partial c_1/\partial x = -\partial I/\partial x$ for any variable x .

Result 1: Effects of s

Clearly, $G_{Is} = 1$ and $G_{c_2s} = 0$, so Cramer's rule implies that $\partial I/\partial s = -G_{c_2c_2}/|\tilde{H}| > 0$ and $\partial c_2/\partial s = G_{Ic_2}/|\tilde{H}|$. If Condition 1 holds, then $\partial c_2/\partial s \leq 0$; otherwise, $\partial c_2/\partial s > 0$.

Result 2: Effects of A

Notice:

$$G_{IA} = R^{-1}h_{IA}(1 - c_2 + N_{H_2}) + R^{-1}h_I h_A N_{H_2 H_2}$$

$$G_{c_2 A} = R^{-1}h_A(N_{c_2 H_2} - 1).$$

As was the previous case, for $h_I < \infty$ and $h_A < \infty$, there exists some small $\varepsilon > 0$ for which $G_{IA} > 0$ if $N_{HH} > -\varepsilon$ by continuity. $G_{c_2 A} \leq 0$ if Condition 1 holds; otherwise, $G_{c_2 A} > 0$.

Applying Cramer's rule, we obtain $\partial I/\partial A = -(G_{IA}G_{c_2 c_2} - G_{c_2 A}G_{Ic_2})/|\tilde{H}|$. Thus, $\partial I/\partial A > 0$ if $N_{HH} > -\varepsilon$. We also obtain $\partial c_2/\partial A = -(G_{II}G_{c_2 A} - G_{Ic_2}G_{IA})/|\tilde{H}|$. If Condition 1 holds, then $\partial c_2/\partial A \leq 0$; otherwise, $\partial c_2/\partial A > 0$.

Result 3: Effects of H_1

Notice:

$$G_{IH_1} = -N_{c_1 H_1} + R^{-1}[h_{IH_1}(1 - c_2 + N_{H_2}) + h_I N_{H_2 H_2}(1 + h_{H_1})]$$

$$G_{c_2 H_1} = R^{-1}(1 + h_{H_1})(N_{c_2 H_2} - 1).$$

While $G_{c_2 H_1} \leq 0$ if Condition 1 holds (otherwise, it is positive), it is not possible to generally sign G_{IH_1} . If $N_H = 0$, then $G_{Ic_2} < 0$, $G_{IH_1} > 0$, and $G_{c_2 H_1} < 0$. In this case, applying Cramer's rule yields $\partial I/\partial H_1 > 0$ and $\partial c_2/\partial H_2 < 0$.

Result 4: Effects of θ

Notice:

$$G_{I\theta} = -N_{c_1 \theta} + R^{-1}h_I N_{\theta H_2}$$

$$G_{c_2 \theta} = R^{-1}N_{c_2 \theta} > 0.$$

Clearly, $G_{I\theta} < 0$ if $N_{H\theta} \leq 0$; otherwise, it cannot generally be signed. If Condition 1 holds and $N_{H\theta} \leq 0$, then applying Cramer's rule yields $\partial I/\partial \theta \leq 0$ and $\partial c_2/\partial \theta > 0$.

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Lochner's chapter provides theoretical and empirical support for the idea that education reduces crime. On the theoretical side, section 10.2 presents a two-period model emphasizing the trade-offs between work, school, crime, and leisure. A more detailed analysis along these lines may be found in Lochner (2004), but the two-period version nicely summarizes key trade-offs. On the empirical side, section 10.3 reviews the recent empirical literature on the relationship between education and crime. Section 10.3 is primarily, though not exclusively, focused on contributions utilizing quasi-experimental approaches such as instrumental variables. The articles reviewed cover a broad set of research questions:

1. What is the effect of an additional year of schooling on the future criminality of an individual?
2. What is the effect of attending a higher quality school on the future criminality of an individual?
3. What is the effect of an additional day of schooling on the contemporaneous criminality of an individual?
4. What is the effect of early childhood interventions on crime?

My comments cannot hope to address the breadth of topics covered in the chapter. Instead, I focus on two major points. First, I ask what might be meant, conceptually, by question (1) outlined in the preceding. I conclude that is notably more complicated than, for example, the second and third questions. Second, I consider the implications of short time horizons for the effect of education on crime.

What Is Meant by the Effect of Education on Crime?

Researchers often dispute the appropriate interpretation of estimated quantities, even when the estimates are based on randomized variation. When they can be defined in such a way as to avoid competing interpretations, counterfactual outcomes are a core device for clarifying meanings. Fix s at a reference level of schooling. Let $Y_i(0)$ denote the outcome that individual i would obtain under schooling level $S_i = s$, and let $Y_i(1)$ denote the outcome that the same individual would obtain under schooling level

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