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Witte, Ann D., and Helen Tauchen. 1994. "Work and Crime: An Exploration Using Panel Data." NBER Working Paper no. 4794. Cambridge, MA: National Bureau of Economic Research.

## Comment Justin McCrary

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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$S_i = s + 1$. With random assignment of a factor affecting schooling, we can usually estimate a quantity such as $E[Y_i(1) - Y_i(0)|D_i = 1]$ or the average of the causal effects $Y_i(1) - Y_i(0)$ for a subpopulation defined by $D_i = 1$.

In fortuitious situations, researchers agree on the meaning of the counterfactual outcome pair $[Y_i(0), Y_i(1)]$ and the subpopulation defined by $D_i = 1$ is of interest. Suppose that half of one group is randomly assigned to complete twelve years of education and the other half is randomly assigned to complete eleven years of education, and suppose there are no compliance problems (so that assigned schooling was equal to actual schooling). Then the difference in means for the two subgroups identifies $E[Y_i(1) - Y_i(0)]$. If researchers agree on the meaning of $[Y_i(0), Y_i(1)]$, then this is a meaningful quantity capturing the central tendency of the causal effect of schooling level $s + 1$, as compared to schooling level $s$. In this setting, all units have $D_i = 1$, and the subpopulation for which causal effects are measured is under the control of the researcher (who, I assume, determines the original subpopulation subject to random assignment). In settings where compliance with treatment assignments is not guaranteed, the researcher does not entirely control the subpopulation over whom causal effects are estimated. Suppose that because of the compliance problem, those randomly assigned to complete twelve years of education do not necessarily actually complete twelve years of education. For example, the researcher might be using an encouragement design, and encouragement may not be very effective. Then instrumental variables estimates $E[Y_i(1) - Y_i(0)|D_i = 1]$ under the auxiliary assumption of monotonicity, where now $D_i$ indicates whether the individual completed more schooling than he or she otherwise would have, by virtue of treatment assignment (Imbens and Angrist 1994). This subpopulation is defined both by the researcher's initial sampling choices as well as individual choice subsequent to random assignment. In many contexts, but not all, this type of subpopulation is of interest.

However, even if the subpopulation over whom causal effects are estimated is of interest, it may be hard to define the pair $[Y_i(0), Y_i(1)]$ in such a way that different researchers agree on the meaning. That this should occur is not surprising. After all, counterfactual outcomes are conceptual. Let us turn to a specific example.

A useful device for clarifying meanings of counterfactual outcomes is the description of hypothetical experiments. This approach, championed by the statisticians Paul W. Holland and Donald Rubin, even has a slogan: "no causation without manipulation" (Holland 1986; Rubin 1986).[1] Let us

1. Within economics, this approach to defining causality is sometimes met with derision. I interpret this approach somewhat more forgivingly. I view this approach not as much as being about *defining* causality and instead as being about *communicating* what parameter is under discussion. When researcher *a* can describe an experimental manipulation that would recover the effect researcher *a* is trying to measure, researchers *b*, *c*, and *d* cannot possibly be confused about what researcher *a* means. Researchers *b*, *c*, and *d* may disagree about whether the causal

use the device of the hypothetical experiment to clarify what might be meant by "the effect of an extra year of education on crime."

Suppose we randomly assigned half of a group of high school sophomores to an additional compulsory year of schooling. Suppose further that there are no compliance problems so that the difference in sample means between the two groups is a measure of the central tendency of the causal effects of an additional year of schooling for high school sophomores. Next, we define $Y_i(1)$ and $Y_i(0)$ as the level of crime that would obtain with and without the additional year of schooling, respectively. This begs the question: what aspect of crime is to be measured and at what date? Suppose we obtain agreement that the interesting feature of criminality is number of arrests in the last calendar year, and suppose moreover that we either have access to administrative or self-report arrest data that we trust (or perhaps we are simply willing to hold our nose). Even having reached consensus on what aspect of criminal involvement is to be measured, we still face the issue of the appropriate date of measurement.

Measuring crime at school exit is plainly undesirable: the age profile of criminal involvement rises extraordinarily rapidly between fifteen and nineteen. Such a measurement protocol would conflate the effects of the schooling intervention with the age profile. This is precisely the type of omitted variables bias that randomization seeks to overcome.

Measuring crime at the same date eliminates problems of noncomparability in terms of the age profile. In particular, this ensures that time because random assignment is balanced between the treatment and control groups. This simple, but powerful, consideration indicates that it is desirable to measure criminality as of the same date, regardless of how much schooling was obtained.

So let us choose a specific example of such a measurement protocol. Suppose that the outcome measured is the number of arrests in the last 365 days, as of May 1 of the year after randomization. By assumption, those in the treatment group are still attending school, and those in the control group are no longer in school. With this definition of counterfactual outcomes, the individual specific causal effect $Y_i(1) - Y_i(0)$ measures—indeed, may primarily measure—the incapacitation effect of schooling. By this, I mean simply that by its nature, crime is hard to commit while in school because much of schooling is spent sitting at a desk. Alternatively, the fact that school lets out at the same time and that youth may be engaged in similar transportation routes home, perhaps preceded by a period of "hanging out," could lead to *more* crime, by virtue of the prevalence of victims. In any event, the

effects in question are of interest, but all four researchers will be talking about the same concepts. Relatedly, when researcher *a* seeks to describe the effect of interest, yet finds it difficult to describe an experiment that would recover an estimate of that effect, researchers *b*, *c*, and *d* may be confused about what researcher *a* means. This is particularly true when researchers are working from different disciplinary traditions.

environments of the treatment and control groups differ—but in ways that have nothing to do with the accumulation of human capital (the putative mechanism for the schooling-crime causal connection).

Suppose that instead we seek to measure the number of arrests in the last $2 \times 365$ days, measured as of May 1 the year subsequent to randomization. Then, acknowledging that those in the treatment group had less criminal involvement in the first 365 days, we confront the reality that this fact has implications for all future levels of criminal involvement. It is widely understood that involvement in crime exhibits complex serial correlations. On the one hand, involvement in crime in one period leads to information networks that may aid in subsequent criminal involvement. This might lead the number of arrests in the last $2 \times 365$ days to exhibit a *larger* gap between treatment and control than the number of arrests in the first 365 days. On the other hand, involvement in crime in one period increases the probability of incarceration in jail or prison. This then leads to an offsetting mechanical shift in crime; those assigned to treatment may have a level of crime that is mechanically lower at first (because they are in school), but if pretrial detention or imprisonment is sufficiently prevalent, then this may in fact lead to a mechanically relatively *higher* level of crime in the medium term (because those in the control group are more likely to be detained or imprisoned). This is offset by the presumptively positive serial correlation in activity that would result were the criminal justice system to use fines—which do not take offenders out of circulation—in place of imprisonment.

These problems would seem to continue as the length of follow-up increases. Indeed, interpretation may even become more difficult with a longer follow-up. Economists tend to emphasize an offender psychology where individuals engaged in crime understand ex ante the consequences of such a choice; criminologists tend to emphasize an offender psychology where it is only after imprisonment that a potential offender will take seriously the idea that punishment is the logical consequence of repeated criminal behavior. For an economist, it is thus plausible that prison increases human capital and leads to a net increase in criminal activity upon release. Criminologists tend not to discount such a possibility but also are willing to think of an individual who, after an imprisonment spell, desists because of the realization that the system will, in fact, punish. Thus, in addition to the earlier difficulties of determining whether individuals are systematically incapacitated and for how long, we now additionally are forced to take a stand on the effects on subsequent criminality of having experienced punishment.

To me, this complicated mixture of mechanisms renders question (1) hopelessly confusing. The only thing I find myself able to make sense of is the narrow policy evaluation: "we spent $X$ dollars encouraging students to attend an additional year of school, and at a five-year follow-up, those in the treatment group seem to have $Y$ fewer arrests than those in the control group. In light of the rate of apprehension, $p$, believed to prevail for these types of crimes, this suggests that the program achieved an annual crime reduction per dollar spent of 0.2 $(Y/p)/X$." This kind of analysis is not necessarily connected to behavioral concepts but may, nonetheless, be practical and useful.

However, I find questions (2) and (3) to be quite easy to make sense of. Question (4) is confusing to me for the types of reasons outlined in the preceding, but the policy evaluation associated with question (4) is of obvious interest.

### The Effect of Education on Crime When Offenders Have a Taste for the Present

> Johnny Weeks: "It'll be better tomorrow, Bubbs."
> Reginald Cousins: (derisively) "Dope fiend talking about tomorrow . . . tomorrow ain't shit. Today, Johnny—today."
> —*The Wire,* Season 3

Short time horizons may well characterize the bulk of the offender population. Punishments for crime are experienced in the future, and the benefits are experienced largely in the present. Thus, the process of self-selection in the marketplace implies that those doing crime must be selected on a taste for the present. A majority of arrestees test positive for one or more serious drugs at the time of arrest; drug use is presumably both cause and consequence of a foreshortening of the planning horizon. Indeed, the dominant modern view within criminology is that crime is the result of self-control problems on the part of the potential offender (Gottfredson and Hirschi 1990). These considerations suggest that it is reasonable to model the potential offender as having a taste for the present and perhaps even dynamically inconsistent preferences (Strotz 1955).

If potential offenders have short time horizons, then it is implausible that education could exert large influences on criminal behavior through the types of channels emphasized in the human capital framework emphasized in section 10.2 of the chapter. In particular, the mechanisms emphasized in the chapter are as follows: (a) schooling leads to increased wage rates in the near term as well as in the future, and (b) crime becomes less attractive as the wage rate increases. I do not dispute mechanism (a). Several decades of careful research supports the idea that an increase of even one year of education results in a noticeable increase (e.g., 10 percent) in wage rates (Card 1999). However, I am not confident in mechanism (b). In particular, if potential offenders have short time horizons, then it would be surprising to find that the prospect for an increase in wages over the life cycle would affect decisions in the present.

Lee and McCrary (2009) present evidence that youth in Florida largely fail to respond to the prospect of adult sanctions and continue to participate in crime at the same rate after the transition to adulthood as they did prior to adulthood. There are two obvious interpretations of this finding: (a) individual offenders have short time horizons, or (b) individual offenders do not believe they will be caught. Under either interpretation, there should be little effect of education on crime as long as "the effect of education on crime" is understood to be about human capital. Of course, there is an important distinction between potential offenders and actual offenders. It could well be that potential offenders have relatively long time horizons and actual offenders have quite short time horizons, on average, and similar beliefs regarding the probability of apprehension.

A feature of this chapter is that it underscores throughout that education may affect crime through information networks. This may well be true. Indeed, my own sense is that this type of mechanism, while not the focus of current economic modeling, may well be the most important connection between education and crime. This likely has little to do with time preferences. If such a connection is important, then education could exert a powerful influence on crime even if schooling leads to scant human capital accumulation.

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