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Comment   Philip J. Cook

The chapter by Dills, Miron, and Summers (hereafter "DMS") grabs attention through provocative claims about economists' ignorance when it comes to "the main factors identified in the economics of crime literature as key determinants of crime." The exception offered to this nihilistic conclusion is a finding that drug prohibition generates violence, a result that has been documented by (among others) one of the authors of this chapter, Miron.

DMS's claim that forty years of empirical research by economists has been unproductive rests less on a careful review of the literature (see, e.g., Cook 1980; Nagin 1998; Levitt and Miles 2007) than on several time-series plots of national crime rates juxtaposed with a potentially causal variable. Two of these causal variables relate to the core issue in the economics of crime—the deterrent effect of the threat of criminal sanctions—and are plausibly important: the arrest rate, and the size of the police force. In my comments I will focus on these two variables. Two other variables, the execution rate and the imprisonment rate, are relevant to deterrence but of less interest. Execution is a very rare sanction in practice and the execution rate tells us very little about the likelihood or severity of punishment for the typical murder (Cook, in press). The imprisonment rate has a theoretically ambiguous relationship to crime.[1]

The method of empirical inquiry by which DMS reach their damning conclusion is, ironically, far less sophisticated than the literature they critique. From the first econometric studies that were published on the effect of sanction threat on crime (Ehrlich 1973; Carr-Hill and Stern 1973; Sjoquist

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1. As demonstrated by Blumstein and Nagin (1978), the relationship between crime and the imprisonment rate is not monotonic. At a low probability, an increase in the probability is likely to generate an increase in the imprisonment rate; at a high probability, an increase in probability may well generate a reduction in the imprisonment rate. The logic is identical to the relationship between price and revenue along a demand curve.

1973), economists have recognized and attempted to address the reasonable supposition that crime rates are determined by many factors, not just the sanction threat level. These pioneers also recognized that variables like the arrest rate and police per capita are simultaneously determined with crime rates, and estimating the deterrent effect requires dealing with a difficult identification problem. Contributors to this field have in recent years developed creative efforts to solve this identification problem, with some success (Levitt and Miles 2007). In any event, the current study is a giant step backward methodologically speaking—back to the early work of criminologists who were computing simple correlations (Gibbs 1968).

Indeed, DSM confess that their approach is problematic, in that it "suffers two large defects: the right model is multifactorial, and the raw correlation between crime and a potential determinant can be misleading in the presence of endogeneity." They justify their naïve approach with the odd claim that their approach "does not prove a multivariate, 'instrumented' analysis would not uncover a different effect, but it suggests this outcome is not especially likely . . ." In other words, a simple bivariate association is enough to tell us the big-picture story with a high degree of confidence. Not so.

Take the case of the variable "police per capita." What DMS seem to be arguing is that if police per capita has an important deterrent effect on crime, then the intertemporal correlation between crime rates and police variable will be negative pretty much regardless of socioeconomic, demographic, cultural, and policy changes that have occurred over the period. That is a particularly strange claim given that we know (and the early econometric studies recognized) that there is another relationship between crime rates and police per capita, that reflects the public demand for public safety. We thus have two relationships, both plausibly strong, that have opposite signs:

Supply of offenses: crime rates negatively related to police per capita.
Demand for safety: police per capita positively related to crime.

There is an obvious analogy here between supply and demand in goods markets. If the observed time series data are generated by shifts in the supply of offenses schedule (due to demographic or cultural change, say), then the data will trace out the positively sloped demand function. So if we in fact observe a positive relationship, that tells us nothing at all about whether the supply of offenses is responsive to the threat embodied in increases in police.

There is also a fundamental problem with DMS's analysis of the arrest rate. The authors use arrests per capita as their proxy for the theoretically correct variable, namely the probability of arrest given crime. What is the logical connection between arrests per capita and the probability of arrest given crime? Over time, arrest per capita will be *positively* correlated with crime rates if, for example, the true probability of arrest (given crime)

remains constant.[2] Finding that positive correlation (as do DMS) tells us literally nothing about the subject at hand, which is the deterrent effect of changes in the probability of arrest.

So where do DMS end up? They say they are focusing on the ". . . naïve 'if we hire more police, or make more arrests . . . that will necessarily reduce crime' perspective." They conclude that ". . . while one interpretation of our deterrence results is that economists do not have the right data to find these effects, an alternative, reasonable interpretation is that increases in the standard deterrence variables have small or perverse effects over the relevant range." In my judgment, DMS are 0 for 2: they have not provided a correction to what economists have actually shown (since DMS's method is far more primitive than the norm in the economics literature), and they have not shown that arrest rates and police resources have "small or perverse effects" (since their results are entirely compatible with a strong deterrent effect).

There is much reason to believe that crime rates tend to go down when the probability of punishment increases, other things equal. The review essays cited above include evidence from natural experiments, laboratory experiments, and credible econometric studies. Based on my reading of the evidence, my "naïve" prediction is that if we disbanded the police force the resulting crime surge would be unambiguous.

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2. Furthermore, it is easy to show that if crime is highly deterrable with respect to the arrest probability, then arrest per capita is inversely related to the arrest probability.