Review Essay: Crime and Human Nature

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Crime and Human Nature

by James Q. Wilson and Richard J. Herrnstein.

New York: Simon and Schuster, 1985.

American sociologists tend to take a proprietary interest in criminology and to think of the leading theories about crime as "sociological." Hence, Wilson and Herrnstein's tome (639 pages but *only* $22.95) may startle numerous sociologists, and those who suffer from high blood pressure should be cautious in reading it. Sociologists are accustomed to occasional forays by economists into criminology, but many will be unprepared for a flank attack by formidable scholars from political science (Wilson) and psychology (Herrnstein).

The military analogy is appropriate because Wilson and Herrnstein formulated their theory primarily in opposition to sociological theories. Much to their credit, the authors avoid the usual shrillness in interdisciplinary warfare; but sociologists with interests in criminology should recognize the book for what it is—a challenge. They commonly appear unaware that for nearly two decades no sociologist has formulated a major theory about the *etiology* of crime. The great drought stemmed from a shift in focus (concomitant with the emergence of the labeling perspective) to crime as a reactive phenomenon and from endeavors to demonstrate that "positivist" criminology is the handmaiden of capitalism. Perhaps many sociologists are willing to surrender the etiology of crime to other disciplines, but a better way to sunder the sociology-criminology tie cannot be imagined.

An Operant-utilitarian Theory of Criminality

Wilson and Herrnstein do not use any particular label to identify their theory as to type. In recognition of their frequent use of concepts associated with operant psychology and also with economics, the label chosen here is operant-utilitarian, but without the peijorative connotations so dear to sociologists.

The Explicandum

The authors make a cogent argument for attempting to explain *criminality* rather than *crime,* but their conceptualization of criminality is defective. They seem to equate the crime-criminality distinction with the incidence-prevalence distinction; but prevalence is a property of populations—not of individuals—and the authors make it clear (p. 20) that their intent is to explain "why some individuals are more likely than others to commit crimes." That statement scarcely implies a criterion of criminality, and the problem is exacerbated when the authors describe their goal (p. 21) as "explaining why some persons commit serious crimes at a high rate and others do not." The mag-

nitude of a "high rate" is not stipulated, and any magnitude would be arbitrary. So it is puzzling why the authors do not treat the number of serious offenses per unit of time as a property of individuals, and then designate variation in that number as their *explicandum.* In any case, they appear to have contemplated some special measure of criminality as a property of individuals, which is one of several reasons why they should have engaged in research to support their theory rather than survey published research findings. In that survey they refer to diverse measures of crime or criminality, even population rates (incidence and prevalence), many of which are far removed from the authors' *explicandum.*

Although Wilson and Herrnstein propose to confine their attention chiefly to serious crimes, they never stipulate a defensible criterion of "seriousness." At one point (p. 22) they equate serious crimes with universal crimes and identify four: murder, theft, robbery, and incest. Yet there are doubts about some of these instances, incest in particular (see, e.g., Lieck, 1946: 48 and Newman, 1976: 233). Be that as it may, on the same page (22), the authors express an interest in street crimes and predatory crimes, their argument being that they must "report only what others have studied..." Perhaps so, but the labels in question are better suited for journalism than criminology. To illustrate the conceptual problem, contemplate two of many troublesome questions. First, are some white-collar crimes predatory? Second, is burglary a street crime?

So the only unambiguous feature of the *explicandum* is that it pertains to *individual differences* as regards serious criminality. That feature will repel numerous sociologists, including many with interests in criminology, especially when they read (p. 24): "But no explanation of social life explains anything until it explains individual behavior." However, should sociologists-criminologists dismiss the Wilson-Herrnstein theory because it pertains to individual differences, they will have proven themselves blind to two considerations. First, many of the theories about criminality that sociologists claim as their own, such as Sutherland's theory and Hirschi's theory (1969), purport to explain individual differences. And, second, to surrender individual differences to other disciplines is surely a high price to pay for antireductionism.

In Search of Premises

The Wilson-Herrnstein theory is stated discursively—that is, in accordance with only the rules and conventions of some natural language (for elaboration, see Gibbs, 1985). As a consequence, the Wilson-Herrnstein theory is even more amorphous than some of the contending sociological theories. Thus, nine particular declarative statements are clearly the premises of Sutherland's theory (Sutherland and Cressey, 1974:75–76), but the premises of the Wilson-Herrnstein theory cannot be identified with any confidence.

The Foremost Candidate

A sentence near the end of Chapter 2 ("A Theory of Criminal Behavior") appears to be a summary statement, and in that sense it is taken as *the* premise. "The larger the ratio of the rewards (material and nonmaterial) of noncrime to the rewards (material and nonmaterial) of crime, the weaker the tendency to commit crimes." That sentence is immediately followed by a passage evidently intended as an elaboration: "The bite of conscience, the approval of peers, and any sense of inequity will increase or decrease the total value of crime; the opinions of family, friends, and employers are important benefits of noncrime, as is the desire to avoid the penalties that can be imposed by the criminal justice system. The strength of any reward declines with time, but people differ in the rate at which they discount the future. The strength of a given reward is also affected by the total supply of reinforcers." Since numerous possibly relevant variables are not mentioned (for example, monetary gain, the perceived certainty of a penalty), the passage appears only illustrative. Even so, why are the "opinions of friends" *necessarily* important benefits of noncrime?

The *Appendix* provides a somewhat more coherent version of the theory in the form of equations. There the authors speak of reinforcement rather than rewards, evidently because (p. 44) rewards are what psychologists call "reinforcers." However, even if treating the terms as synonyms could be justified, it is a confusing irritation when both appear in the same sentence. Indeed, one must wonder about the purpose in using a predominantly operant terminology to define variables in the equations *(Appendix)* but a predominantly economics terminology in the verbal version (Chapter 2). The verbal version's logical structure is all the more difficult to discern because of the profusion of key terms—reward, value, cost, benefit, and so on.

Some Terminological Questions

In neither version do Wilson and Herrnstein display the slightest concern about the empirical applicability of their terms. Virtually all of those terms apparently denote quantitative phenomena, but the authors never stipulate formulas for the numerical expression of variables and describe the requisite data. Indeed, surely there must be doubts about even attempting to measure the phenomena denoted by such terms as the bite of conscience, sense of inequity, total value of crime, total supply of reinforcers, and impulsiveness. However, the criticism is a far cry from rabid operationalism. Terms that denote purely theoretical notions may be essential for powerful theories, and systematic tests of a theory do not require that all terms in the premises be empirically applicable. Systematic tests require only empirically applicable terms in the conclusions (theorems). Yet Wilson and Herrnstein appear indifferent to the empirical applicability of their key terms, and their theory's logical structure is so obscure that there is no basis for deducing testable conclusions.

Wilson and Herrnstein never emphasize the point that in traditional operant language there is a logical (necessary) connection between "reinforcer" or "reinforcement" and "changes in behavior." Briefly, a reinforcer is any consequence of a type of behavior by an organism that results in an increase in the emission of that behavior by the organism (see Akers, 1985: 45). Hence, it is vacuous to claim, as Wilson and Herrnstein do, that the frequency with which an individual commits a crime is a function of that behavior's reinforcement. The claim is vacuous unless extended to an identification of particular types of conditions or events as reinforcers of particular types of behavior by particular types of individuals, and it is precisely such identification that is missing in the Wilson-Herrnstein theory.

Sociologists who use operant terminology, Akers (1985) especially, are more circumspect in that use than are Wilson and Herrnstein. In that connection, the authors do not acknowledge Burgess and Aker's differential association-reinforcement theory of deviant behavior (see Akers, 1985). Yet the Wilson-Herrnstein theory and the Burgess-Akers theory are contenders, and the logical structure of the latter is much clearer if only because its premises are explicit.

The foregoing is not a denial of an argument that Wilson and Herrnstein pursue at length (e.g., pp. 43 and 59)—behavior is determined by its consequences. Nonetheless, in pursuing that argument the authors rarely speak of perceived or anticipated consequences, notions that could be essential in explaining an offender's first crime. Even when treating legal penalties in the mathematical version of the theory (p. 532), the authors refer to "probability" without considering perception. Perhaps most importantly, vicarious learning appears to be alien to the Wilson-Herrnstein theory.

The Use of Criminology's Literature

Whatever the merits of their theory, Wilson and Herrnstein's coverage of criminology's literature is extensive; and for that reason alone criminologists will profit from reading their work. However, their use of the literature is such as to raise additional doubts about the theory and even about the purpose of the book.

Constitutional Factors: Chapter 3

This chapter may prove more controversial than the Wilson-Herrnstein theory itself. Yet their conception of constitutional factors is hardly radical; they use the term in reference to factors (p. 69) "usually present at or soon after birth, whose behavioral consequences appear gradually during the child's development." The authors point out that some constitutional factors are not genetic or hereditary, and they emphasize that there is no "crime gene" or "born criminal." Even this assertion (p. 69) is unlikely to provoke a storm: "some traits that are to a degree heritable, such as intelligence

and temperament, affect to some extent the likelihood that individuals will engage in criminal activity."

In treating constitutional factors Wilson and Herrnstein appear determined to resurrect every factor that most criminologists now ignore. Only such determination could have prompted the authors to devote nearly 20 pages— space that could have been used for clarification of the theory or stipulation of test procedures—to rehash arguments and research on the anatomical correlates of criminality, including detailed observations on Lombroso, Hooton, Sheldon, and the Gluecks. And what do they conclude? "Wherever it has been examined, criminals on the average differ in physique from the population at large. They tend to be more mesomorphic (muscular) and less ectomorphic (linear)..." (p. 89). But Wilson and Herrnstein did not compute any measure of proportional reduction in error when predictions are made about the criminality of individuals from knowledge of their anatomical traits (for example, mesomorph versus ectomorph), and that omission is puzzling because of their avowed intention to explain *individual differences.* No less puzzling, the authors do not show how particular alleged anatomical correlates of criminality can be *formally deduced* from their theory.

Wilson and Herrnstein are on much more defensible ground when they survey the findings (pp. 90–100) of research pertaining to the criminality of twins (monozygotic vs. dizygotic) and the criminality of adopted children in relation to the criminality of their biological parents and adopted parents. The authors are justified in concluding that the findings suggest some genetic basis of some criminality, and criminologists should examine these findings more seriously than they now do. Yet these kinds of findings have not been banned from conventional journals, as witness Ellis's article (1982) in *Criminology,* an item not cited by the authors. More importantly, there is no basis whatever to argue that the findings in question are consistent with predictions *formally deduced* from the Wilson-Herrnstein theory.

Some Other Puzzling Topics

What has been said about Wilson and Hermstein's treatment of constitutional factors extends to their treatment of intelligence (Chapter 6) and personality and psychopathology (Chapter 7). Again, in assessing the relevance of the factors, the authors did not employ what they should have—a measure of proportional reduction in error when predicting the criminality of individuals. A difference between purported criminals and noncriminals in *average* I.Q. scores or personality scores is not an adequate substitute for such a measure. The stance of Wilson and Herrnstein on the personality-criminality connection is particularly telling. They acknowledge Schuessler and Cressey's review (1950) of numerous studies, only about 40% of which reported significant personality differences between criminals and noncriminals. What impact did that review have on Wilson and Herrnstein? Here is the answer (p. 185): "To the

authors of the study, Schuessler and Cressey, and others, the cup looked more than half empty rather than almost half full."

Lest the foregoing criticism be construed as reflecting a sociological bias, it extends to the chapters on families, broken and abusive families, schools, community, labor markets, television and mass media, and alcohol and heroin. Had Wilson and Herrnstein used a measure of proportional reduction in error when making predictions about the criminality of individuals, it is difficult to imagine them defending the treatment of those subjects at such length. In any case, the research findings reviewed in these chapters cannot be described as either consistent or inconsistent with the Wilson-Herrnstein theory because no predictions about such findings can be formally deduced from the theory.

The foregoing argument is actually consistent with Wilson and Herrnstein's subject of social class. They largely ignore the subject, with the rationale (pp. 27–28) being doubts as to whether there is anything like an appreciable association between social class and criminality *at the individual level* without all manner of complicated qualifications. The doubts are surely justified, but it is difficult to understand why the authors did not take the same position with regard to several other topics.

Defensible Topics

Had Wilson and Herrnstein used a measure of proportional reduction in error, their lengthy treatments of gender, age, and race (Chapters 4, 5, and 18) would have been even more justified. The presumption is that such a measure would reveal a substantial association at the individual level between each of these variables and criminality; but the associations would not be such as to demonstrate that even the combination of gender, age, and race provides an *adequate* explanation of *individual differences* in even one type of criminality. Nonetheless, this question poses a challenge for any theory about criminality: Why is there an association between criminality and gender, age, or race? In the case of the Wilson-Herrnstein theory, the more specific question is this: How can you demonstrate that in the United States the ratio of the rewards of noncrime to the rewards of crime is much greater for women, much less for teenagers, and much greater for whites? That question survives this book.

Chapter 15 may be the most relevant in assessing the Wilson-Herrnstein theory, because the subject is primarily perceptions of rewards or costs of crime. In particular, research on specific or general deterrence could be construed as a partial test of the Wilson-Herrnstein theory. Their assessment of deterrence findings is defensible, and they are aware of the serious evidential problems that haunt deterrence research. However, they never truly confront two crucial questions. First, how can the evidential problems be solved or circumvented? Second, if deterrence research is likely to remain inadequate for an assessment of the Wilson-Herrnstein theory, what is the alternative?

Even More Expendable Chapters

Chapters 16 ("Historical Trends in Crime"), 17 ("Crimes Across Cultures"), 19 ("Punishment and Personal Responsibility"), and 20 ("Human Nature and the Political Order") generate doubts as to what Wilson and Herrnstein hoped to accomplish. It would be a gross distortion to say that these chapters are substandard. Most criminologists will be much better informed on reading the chapters, but they are far removed from the ostensible goal of the authors.

It is unfortunate that Wilson and Herrnstein did not devote the space and energy invested in the four chapters to a clarification of their theory by stating explicit premises and deducing testable conclusions. Instead, we have another discursive, untestable theory, something that has never been in short supply. Hence, despite Wilson and Herrnstein's serious scholarship, their book will not lead criminologists out of the wilderness in which they now wander.

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