

With a new introduction by the author

Causes^{of} Delinquency

Travis
Hirschi



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Travis Hirschi

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For Anna, Kendal, Nathan, and Justine



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Introduction to the Transaction Edition

Some ten years after the publication of *Causes of Delinquency*, I was asked by the editors of *Current Contents* to account for the 215 times the book had been cited and its subsequent status as a “Citation Classic.” Flattered and disarmed by such attention, I distributed credit for the book’s popularity among four of its elements: “the [social control] theory of delinquency it advocates; its findings on the correlates of delinquency; the set of data on which it is based; and . . . the methodology it employs.” The full account, written in 1980, follows:

The ideas in the book were common in the literature of sociology and criminology at the time (1964) I decided to order them in some systematic fashion for a dissertation at Berkeley. I had been familiar with these ideas for some time, and had learned to respect them because they had been deemed worthy of explication by David Matza, Irving Piliavin, Erving Goffman, and Jackson Toby, among others.

The central findings in the book had been reported in the criminological literature over a period of many years. I was familiar with these findings because I had by then been working for several years with Hanan Selvin on a methodological critique of delinquency research.

My initial plan was simply to put the ideas and the research findings together. With this plan in mind, I went on the job market. I came home from my first trip east convinced there were more important things than regular employment. The ideas I found exciting and obviously consistent with available data had been treated as contrary to fact, passé, and even “appalling.” The only way to remedy this situation, it seemed, was to show the ability of the ideas to account for a single body of relevant data.

Despite the efforts of my dissertation advisor, Charles Y. Glock, I was unable to obtain data for secondary analysis. (In those days, large

scale data sets were rare and investigators perhaps understandably reluctant to release them before they had been thoroughly exploited.) Glock then put me in touch with Alan B. Wilson, whose Richmond Youth Project was just getting underway. Wilson agreed to let me add items to the research instruments in exchange for work on the project. (An NIMH predoctoral fellowship precluded gainful employment and provided large amounts of poverty-induced leisure.) Although I eventually became deputy director of the project, my contributions were mainly clerical (and physical—boxes of questionnaires are heavy) rather than intellectual.

The key to the book is the body of data on which it is based. I know from experience that the ideas could not otherwise have been sharpened sufficiently to impress sociologists. I know that most of the findings were available (though often ignored) before my work was published. I know too that the statistical analysis is not sufficiently sophisticated by itself to attract more than negative attention. It is therefore fitting that many of the citations to my work stem from the fact that it contains a convenient description of the Richmond Youth Project. Thanks to Wilson's generosity, the Richmond data have been available for secondary analysis of delinquency and related issues almost from the day they were transportable. In fact, my work was cited before it appeared in print in an article based on secondary analysis of Richmond data (Hirschi 1980).

With some elaboration, this history may help explain the tone and content of the book as well as its provisional claim to popularity or influence: The book was written in a warm and secure setting for what was assumed to be a basically hostile audience. Its author had spent some time and effort criticizing the work of others on the very topic he now planned to study. He knew in advance the kind of data he would use to test the theory he planned to develop. He also knew, he thought, what he would find in those data. From these circumstances, it is possible to predict the book's occasionally argumentative tone, its preoccupation with alternative interpretations of the data, its frequent use of supportive quotations, its confident hypotheses, and perhaps even the success of the theory it advocates and the failure of popular competing theories. What it does not predict is the subsequent success of the "passé and even 'appalling'" ideas of social control theory.¹

¹ My 1980 account suggests that I was able to dispel such criticism by deft argument and clever data analysis. This impression is not wholly accurate: "[Hirschi] takes his psychology straight from Thomas Hobbes and seems stuck in the eighteenth century. He seems quite unaware of the great body of psychological research and theory relevant to criminology" (Gibson 1970:452). The same reviewer later

Today, some 30 years after its publication, *Causes of Delinquency* is more frequently cited than ever, but it is not now so easy to lose myself in an understated self-congratulatory account of the origins and content of the book. Control theory has prospered in the interim, but *Causes* no longer stands virtually alone in its defense. The works on which the book was based are now better known and their relevance more widely appreciated; I have since published a sufficient number of articles on control theory to be labeled “the spokesperson of the microsociological perspective” (Adler et al. 1995, 61); dissertations on and formal tests of the theory have appeared “with perhaps unrivaled frequency” (Kempf 1993, 143); and several subsequent books have contributed in important ways to acceptance of the control perspective. These include: Ruth Kornhauser, *Social Sources of Delinquency* (1978 [1984]); Bob Roshier, *Controlling Crime: The Classical Perspective in Criminology* (1989); Michael Gottfredson and Travis Hirschi, *A General Theory of Crime* (1990); and Robert Sampson and John Laub, *Crime in the Making* (1993).

In the hope that the book may be better understood by examining its consequences as well as its origins, let me illustrate and briefly comment on the connections between *Causes of Delinquency* and each of the collections of scholarly work just identified.

Subsequent Books on Control Theory

Kornhauser’s work is considered by some to be the greatest book ever written on crime and delinquency. For reasons soon to be clear, I am not inclined to dispute that judgment. Kornhauser did what I was unable to do—champion the claims of social control theory over its major competitors using only published research findings. She was not overly impressed with the exposition of social control theory in *Causes of Delinquency*, but she was eventually *convinced by its data* that her favorite theory, the “strain model” of Merton, Cohen, and Cloward and Ohlin, was wrong. Because she had previously rejected Sutherland’s “cultural deviance” theory, social control theory was the only option open to her.

allowed that the results of the Richmond Youth Study had been “rendered nugatory” by the requirement of parental permission. (It is of course likely that the real reason for the success of my argument is that it at last found a receptive audience. The treatment I received the first time I attended the meetings of the American Society of Criminology [1976] was overwhelmingly friendly. So much so that I have had warm feelings toward the ASC and the site of that convention [Tucson] ever since.)

Once she recognized it as a variant of social disorganization theory, Kornhauser saw control theory everywhere and defended it in no uncertain terms against all comers.²

Roshier's book is an incisive intellectual history of criminology. He too does something I was unable to do—examine the role of social control theory in the rise of what he calls “postclassical criminology.” Roshier may not have been overly impressed by the particular version of control theory he found in *Causes of Delinquency*, but he gave the book credit for filling “important gaps” in classical theory and for “inviting expansion” along classical lines—something he then proceeded to do (1989, 46-49, 67 ff.). Perhaps more significantly, he treated strain and cultural deviance theories as relics of an earlier stage of theoretical evolution.

For purposes of exposition, I should mention Sampson and Laub before Gottfredson and Hirschi, whose book was published three years earlier. Once again we find scholars doing something I was unable to do—in this case analyze a large body of available data using the ideas of social control theory. Like Kornhauser, Sampson and Laub were not impressed with the exposition of social control theory found in *Causes of Delinquency*, but they were impressed (as were other criminologists) by the ability of traditional “informal social control theory” to organize and explain the most important set of data on crime yet collected, especially when they considered that those collecting it had disavowed interest in theories of any kind (Glueck and Glueck, 1950:3-9). At the same time, Sampson and Laub were sharply critical of the self-control version of control theory advanced by Gottfredson and Hirschi.

Gottfredson and Hirschi's self-control theory has attracted considerable research and critical attention, and is a major element in the current “popularity” of control theory. At the same time, as in the example just mentioned, it has contributed to acceptance of *social* control theory by acting as an “undesirable” alternative theory. This has made it possible for me to profit from criticism of my own work. I did not plan this outcome.

In 1979, Michael Gottfredson and I began a collaboration that lasted on an active basis for about ten years. Our first paper was a critique of the Sutherland tradition in criminology from a research or positivist point of

² Kornhauser acknowledges the influence of *Causes of Delinquency* in the preface to the paperback edition of her book (Kornhauser 1984, viii). In the dedication of the copy she sent to me, she wrote that the argument in *Causes* was “all that kept this from being a totally wrong-headed book.”

view. The stance we took then, and the stance we tried to maintain throughout our collaboration, was that the facts about crime and delinquency should take priority over all other considerations. Sutherland, we argued, had misled the field by his dismissal of the multiple factor approach and by his refusal to grant even provisional validity to non-sociological explanations of crime.

Consistent with this emphasis, our second paper focused on the effects of age on crime. We concluded that the decline in crime with age is one of the “brute facts” of criminology. From there, it was a few short steps (and a good many papers) to *A General Theory of Crime*, which argues that differences in self-control established early in life are highly stable, and account for a large array of criminal, delinquent, deviant, and reckless acts.

Not once during the course of our work together did Gottfredson ask how the position we were taking squared with *Causes of Delinquency*. Nor did I raise such questions. On occasion it may have crossed my mind that I had had a previous life, but it was strictly contrary to our oft-proclaimed position to worry about such things.

If Gottfredson and I felt an obligation to avoid questions about possible reconciliation between social control and self-control theory,³ others did not. The first reviews of *A General Theory of Crime* wondered about the connection between my old and current views (e.g., Akers 1991), and research comparing their validity followed in short order. The common conclusion of these comparisons seems to be that they reveal serious problems for one or both theories.

It may seem reasonable to ask how the two theories relate to one another, and which better serves some useful purpose. Such questions appear especially reasonable when both theories have been fully formulated and thus cannot claim that their development would be hindered by such concerns. But I cannot help thinking that they are misplaced and their appearance of constructive curiosity ultimately misleading.

My interpretation is that self-control theory rejects important insights from Hirschi’s original formulation of social control and is therefore a less adequate explanation. The adoption of the age-invariance thesis and the assumed stability of self-control beyond early childhood imply that individuals do not have the capacity to change over the life course. Thus, self-control theory completely neglects the impact of wider, structural forces on individuals in later life... (Taylor 2001, 384).

³ We have tried to explain the role of the age effect in the evolution of our thinking about crime, but we have not attempted to assess the relative merits of the schemes in place at the beginning and end of this process (Hirschi and Gottfredson 2001).

It may be useful to put Taylor's assessment in bald-faced terms: (1) social control theory is better than self-control theory; (2) the major factual premises of self-control theory are wrong; and (3) a major factual implication of self-control theory is also wrong. I think it less likely that she would make such assertions (and less likely that they would be published) were she comparing two theories by two authors. In that situation, the critic does not normally simply declare a winner without examining the claims of the loser. But when one author is on both sides, there is little risk in this exercise. Whatever the position taken on the relative merits of the theories, the critic can claim support from the author's own words. Should the common author prove cantankerous, it can be pointed out that such unreliability is nothing new. Should an uncommon co-author weigh in on the matter, it may be used to remind the reader that this author's views may be discounted because he or she is the likely source of inconsistency in the first place.

So, at issue here is the importance of consistency in theory. Everyone would agree that consistency is a virtue, so much so that inconsistency is *prima facie* evidence of hidden vice. Indeed, discovery of inconsistency rightfully allows the critic to discount other criteria of theoretical adequacy. Empirical data are of no value because they cannot simultaneously support inconsistent arguments. Pushed a little, even possible inconsistency may raise serious concerns about the claims of a theory.

How, then, could Gottfredson and I pay so little attention to this matter? The answer is this: We believe that consistency *within a theory* is crucial, so crucial that it may require conclusions that one would prefer to do without. At the same time, we believe that consistency *across theories* is no virtue at all. Reconciliation of separate theories of crime is either impossible or unnecessary. If they are the same theory, reconciliation is not required. If they are different theories, they cannot be made the same without doing violence (introducing inconsistency) to one or the other, or both (Hirschi 1979). Social control theory and self-control theory are not unique in this regard. They share important assumptions, but they are not the same theory, and should be judged on their merits, as should some future theory that attempts to encompass them both.

Sources of the Theory

The long-ago reviews of *Causes of Delinquency*, favorable and unfavorable, paid little attention to the sources of social control theory. Those

unconvinced by the argument of the book of course had little interest in its intellectual history, and tended to focus on alleged weaknesses in the measures or biases in the sample. As a class, those more favorably inclined mentioned my ties to Durkheim, Hobbes, Reiss, and even Freud, but none mentioned the broad intellectual heritage of control theory and none was concerned about issues of priority. Today, most discussions of the theory summarize works prior to *Causes* to illustrate that “control theories of crime have a long history” or that “there was a rich history of control theories of crime by the mid-1960s”—i.e. before I began to write (Paternoster and Bachman 2001, 73, 77). All of which suggests to me that one contribution of the book was to call attention to the substantial but scattered literature friendly to the control perspective. As the reader will see, I identify and often quote a lengthy list of control theorists—Matza, Nye, Reckless, Toby, Briar, Piliavin, and Reiss—and attribute important elements of the perspective to the likes of Hobbes and Durkheim. To top off the list of donors, I even claim that “the early sociologists in this country” were essentially control theorists. If most authors write their books by ransacking a library, not many are continually reminded of that fact. I am. And I deserve it. Social control theory was not the most popular perspective in the social sciences at the time I wrote, and I thought I needed all the help I could get.

This desire for legitimacy put me in somewhat of a bind. On the one hand, I had to praise famous men. On the other, I had to contribute something of my own. As a result, my version of social control theory is not merely a summary of prior work. I may have been thorough in my efforts to spread responsibility, but I was keenly aware that many of my supporters could not be counted on in a pinch. Many, if not most, had already shown themselves to be disloyal. The test I relied upon to detect unfaithfulness was, of course, inconsistency. To be a full-fledged control theorist, one could not accept assumptions contrary to the theory. Given the times, this was a tall order. Given the times, most criminologists knew that criminal behavior is caused (motivated), that criminals pursue careers in crime, and that cultural variability is virtually without limit. In their usual forms, all of these assumptions are inconsistent with control theory. They are the primary reasons control theory was so hard to sell. Not surprisingly, they are also the major source of discordant elements in the control theories of the time. I was as careful as I could be to avoid all of them.⁴

⁴ It may seem strange to suggest that a book titled *Causes of Delinquency* carefully avoided the idea that crime is caused, but—given the tendency of social

The assumption that theories must provide motives (causes) for criminal behavior shows itself in control theories in lists of basic human needs and in a view of human nature “in which man is active, moved to gratify strong wants, and receptive to efforts to socialize him primarily as they relate to the gratification of wants” (Kornhauser 1984, 39). According to Kornhauser, this assumption is common to American social disorganization theorists—from W. I. Thomas through Thrasher and Riess and Nye to Kornhauser herself. She takes me to task for missing this point, for arguing in favor of “constant [and undescribed] motivation to crime across persons” in the face of evidence and a statement by Durkheim to the contrary (1984, 48). Kornhauser is partially correct. I did not clearly see the motivational element in social disorganization theory. When I did see it, I acted properly. For example, I did not include W. I. Thomas as a control theorist for precisely the reason that he analyzed behavior as an attempt to realize his famous “four wishes.” I continue to believe that I was correct in rejecting the basic assumption of a competing perspective, and, if anything, I was too timid on this score. In control theory, strong wants are conducive not to crime but to conformity because they tie us to the future and because crime is an inefficient means of realizing one’s goals. As would then be expected, the view that crime is need-based or strongly motivated behavior has produced a series of concepts and hypotheses sharply at odds with the facts (Gottfredson and Hirschi 1993).

The idea that crime is a profession, a full-time role, or a way of life was also accepted by more than one scholar otherwise counted as a control theorist. The wish to locate career criminals, types of offenders, or specialists in particular crimes remains as strong today as when *Causes of Delinquency* was written. The motives behind this enterprise, if I may, are clear. Success would justify differential treatment and special handling within the criminal justice system. But the search for qualitative differences among offenders and between offenders and law-abiding citizens remains contrary to basic assumptions of control theory, and claims of discovery of such differences remain, in my view, premature.

The idea that cultures vary in myriad ways in their definitions of criminal and deviant behavior was just beginning to come under attack when I wrote *Causes of Delinquency*. Much to the glee of some of its critics, *Causes* purported to provide its own evidence of the universality of attitudes toward crime, of core values common to all cultures and scientists to equate causes with motives—such is the case. I address this issue in “Causes and Prevention of Juvenile Delinquency” (1977).

social groups. Interestingly enough, despite the current celebration of multiculturalism in the larger society, mainstream criminology now appears to accept cultural universalism with respect to crime. And I think for good reasons. Here is one of them: Cross-cultural studies of self-reported delinquency routinely find that the causes and correlates of delinquent behavior do not vary from country to country. In other words, the findings of research do not depend on, and are unaffected by, local legal definitions of the behavior in question (Junger-Tas et al 1994). By pointedly ignoring the operations of the criminal justice system, social control theory predicts this result. Still, even control theorists find it hard to resist the seemingly reasonable conclusion that some groups favor crime, and that for people in them lack of social control may be conducive to conformity (e.g., Roshier 1989, 89).

Tests of the Theory

Kempf (1993) identified seventy-one empirical tests of social control theory published between 1970 and 1991, and was able to locate twenty-seven dissertations on the theory completed during the same period. She concludes, I think it fair to say, that the theory has not been challenged by these “tests.” Together, they raise many issues and offer much advice, but they tend to see salvation in methodological refinement rather than in rethinking the problem.

What, then, makes social control theory particularly attractive to empirical researchers? LeBlanc (1983, quoted by Kempf 1993, 143-44) answers the question this way: “[A]t the beginning of the 1970’s, [Hirschi’s theory] was the only theoretical formulation that tried to synthesize in a coherent and complex theoretical plan a great deal of information on the causes of delinquent behavior.”

So, the mystery is no mystery at all. My version of social control theory has been so frequently called upon because it guides and justifies research on a broad range of topics, and has few competitors in this regard. Perhaps equally important, it has a special affinity to a well-known, widely practiced research method. It is easily shown that the theory’s virtues for research purposes are in its structure as much as in its content. It starts from the straightforward assumption that deviant behavior occurs when the bond of the individual to society is weak or broken. The bond has many potential dimensions or elements. And “society” turns out to encompass a potentially large array of persons, groups, insti-

tutions, and even futures states of the individual—e.g. parents, teachers, families, schools, peers, gangs, churches, education, marriage, children. To make things even better for those who would test or use the theory: (1) the strength of each element of the bond to each of the objects is assessed and reported by the individual—making the theory directly amenable to survey research and the large array of statistical devices available for the analysis of survey data; (2) the strength of each element of the bond is a function of characteristics of the individual and of the object in question—making the theory receptive to study of the role of individual differences and various institutional arrangements.

Looked at in this way, what is surprising is not how many researchers have tested the particular theory found in *Causes of Delinquency*, but how few have advanced alternative versions within the control theory framework.

My Career as Spokesperson

Books may tend to lose their force and cogency with age, but the same is true of lists of their alleged shortcomings. I spent long years assessing the validity of some of the major criticisms of the data on which *Causes of Delinquency* is based, and came away with the conclusion that I pretty much had it right in the first place (Hindelang, Hirschi, and Weis 1981). At the same time, much of my subsequent work was devoted to issues raised but not settled in the book. As a result of these efforts, I am not inclined to defend it further against its methodological critics, or to grant the accuracy or wisdom of the lists of deficiencies that have accumulated over the years. I have tried on occasion to force myself to think about fixing it, about bringing it into line with what we think we know now that we did not know then, but in every instance the pen has refused to move.

I think the pen may be on to something. What do we know now that I didn't know then? We know that the police respond to the behavior more than to the status of the offender. (At the same time, racial profiling has become a major policy issue.) We know that alcohol and drug use may well be manifestations rather than causes of delinquency. (But see almost any textbook.) We know that the age effect on delinquency is highly robust, and that differences in delinquency are reasonably stable over the life course. (As the journals swell with contrary theories.) We knew then that our measures of delinquency were suspect, and that our

results were dependent on the composition and representativeness of our samples. But there is good evidence and the testimony of *Causes of Delinquency* (“the empirical findings on delinquency fluctuate much less widely than the statements made about them” [p. 243]) that our fears were baseless. Still, we continue to think otherwise and to dismiss perfectly good findings for not very good reasons. All in all, then, *Causes of Delinquency* may stand as a reasonably balanced account of what we knew at the beginning of the research explosion of the last third of the 20th century—an account not too far from what we know at the end of it.

* * *

For some time now, John Laub has been prodding me to acknowledge the true sources of my views about crime and criminology and to propose fresh ways of resolving important issues in the field. This extended interview will become the introduction to a collection of my papers John is editing. A recurrent theme of our discussion has, of course, been the connection between social control and self-control theory, and I am afraid I may have taken more than one position on this issue. Nevertheless, I remain convinced that the befuddlement of a theorist, however profound, says nothing about the validity of his or her theories. As a not incidental byproduct of this discussion, we concluded that it was important to see *Causes of Delinquency* once again in print. John has taken the lead in this effort. I am grateful to him for that as well as for his good-natured insistence that I take seriously some of my long-standing velleities.

I am also grateful to Irving Louis Horowitz for his unfailing support. I can count many things I would not have done without it.

Travis Hirschi
March 2001

References

- Adler, Freda, Gerhard O. W. Mueller, and William S. Laufer. 1995. *Criminology*. New York: McGraw-Hill.
- Akers, Ronald. 1991. “Self-Control as a General Theory of Crime.” *Journal of Quantitative Criminology* 7:201-211.
- Gibson, H. B. 1970. Review of *Causes of Delinquency*. *Sociological Review* 18:452-453.
- Glueck, Sheldon, and Eleanor Glueck. 1950. *Unraveling Juvenile Delinquency*. Cambridge: Harvard University Press.

- Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford: Stanford University Press.
- _____. 1993. "A Control Theory Interpretation of Psychological Research on Aggression." In *Aggression and Violence: Social Interactionist Perspectives*, ed. R. B. Felson and J. T. Tedeschi, 47-68. Washington, DC: APA Monographs.
- Hindelang, Michael, Travis Hirschi, and Joseph Weis. 1981. *Measuring Delinquency*. Beverly Hills: Sage.
- Hirschi, Travis. 1977. "Causes and Prevention of Juvenile Delinquency." *Sociological Inquiry* 47:322-41.
- _____. 1979. "Separate and Unequal is Better." *Journal of Research in Crime and Delinquency* 16:34-38.
- _____. 1980. "This Week's Citation Classic." *Current Contents* 12-38:16.
- Hirschi, Travis, and Michael Gottfredson. 1983. "Age and the Explanation of Crime." *American Journal of Sociology* 89:552-84.
- _____. 2001. "Self Control Theory." In *Explaining Criminals and Crime*, ed. Raymond Paternoster and Ronet Bachman, 81-96. Los Angeles: Roxbury.
- Junger-Tas, Josine et al. 1994. *Delinquent Behavior Among Young People in the Western World*. New York: Kugler Publications.
- Kempf, Kimberly. 1993. "The Empirical Status of Hirschi's Control Theory." In *New Directions in Criminological Theory*, volume 4, ed. Freda Adler and William S. Laufer, 143-185. New Brunswick: Transaction Publishers.
- Kornhauser, Ruth. 1984 [1978]. *Social Sources of Delinquency*. Chicago: University of Chicago Press.
- LeBlanc, Marc. 1983. "Delinquency as an Epiphenomenon of Adolescence." In *Current Issues in Juvenile Justice*, ed. R. Corrado, M. LeBlanc, and J. Tre'panier. Toronto: Butterworths.
- Paternoster, Raymond, and Ronet Bachman. 2001. *Explaining Criminals and Crime*. Los Angeles: Roxbury.
- Roshier, Bob. 1989. *Controlling Crime: The Classical Perspective in Criminology*. Chicago: Lyceum.
- Taylor, Claire. 2001. "The Relationship between Social and Self-control: Tracing Hirschi's Criminological Career." *Theoretical Criminology* 5:369-88.

Preface

In this book I attempt to state and test a theory of delinquency. The theory I advocate sees in the delinquent a person relatively free of the intimate attachments, the aspirations, and the moral beliefs that bind most people to a life within the law. In prominent alternative theories, the delinquent appears either as a frustrated striver forced into delinquency by his acceptance of the goals common to us all, or as an innocent foreigner attempting to obey the rules of a society that is not in position to make the law or to define conduct as good or evil. Throughout the book, I stress the incompatibility of these images of the delinquent and the contrasting predictions to which they lead us.

Although this study is based on a large body of data collected with delinquency as a major focus of attention, I have tried to rely upon earlier investigations and to emphasize the extent to which the present findings are consistent with them. This consistency is, to my mind, remarkable. It is also a source of some difficulty. As anyone who has tried it knows, it is easier to construct theories "twenty years ahead of their time" than theories grounded on and consistent with data currently available. But the day we could pit one study of delinquency against another and then forget them both is gone. We are no longer free to construct the factual world as we construct our explanations of it. As a consequence, our theories do not have the elegance and simplicity of those of an earlier period. I take consolation in the certain hope that they are somehow nearer the truth.

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October 1968



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“But the philosopher doesn’t admit these relationships. Considering but himself alone, only to himself does he account for everything; and he prevails by his own strength. He has recourse to those fine systems of humanity and beneficence only at times for policy’s sake.”

“Such a man is a monster!” Justine said.

“Such a man is a man of nature.”

DeSade, Justine.



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Chapter I

Perspectives on Delinquency

Three fundamental perspectives on delinquency and deviant behavior dominate the current scene. According to *strain* or motivational theories, legitimate desires that conformity cannot satisfy force a person into deviance.¹ According to *control* or bond theories, a person is free to commit delinquent acts because his ties to the conventional order have somehow been broken.² According to *cultural deviance* theories, the deviant conforms to a set of standards not accepted by a larger or more powerful society.³ Although most current theories of crime and delinquency contain elements of at least two and occasionally all three of these perspectives,

¹ The purest example of a strain theory, contaminated only rarely by assumptions appropriate to a control theory, is found in Merton's "Social Structure and Anomie" (Robert K. Merton, *Social Theory and Social Structure* [New York: The Free Press, 1957], pp. 131-160). One characteristic of strain theory is that the motivation to crime overcomes or eliminates restraints—such as considerations of morality (see also Talcott Parsons, *The Social System* [New York: The Free Press, 1951], pp. 249-325). Because Merton traces his intellectual history to Durkheim, strain theories are often called "anomie" theories (see Richard A. Cloward and Lloyd E. Ohlin, *Delinquency and Opportunity* [New York: The Free Press, 1960], especially pp. 77-143). Actually, Durkheim's theory is one of the purest examples of control theory: both anomie and egoism are conditions of "deregulation," and the "aberrant" behavior that follows is an automatic consequence of such deregulation.

² Control theories take many forms, but all of the works by control theorists listed below explicitly adopt the assumption that I take as essential to this perspective. David Matza, *Delinquency and Drift* (New York: Wiley, 1964). F. Ivan Nye, *Family Relationships and Delinquent Behavior* (New York: Wiley, 1958). Walter C. Reckless, *The Crime Problem*, 4th ed. (New York: Appleton-Century-Crofts, 1967), Ch. 22. See also Jackson Toby, "Hoodlum or Businessman: An American Dilemma," *The Jews*, ed. Marshall Sklare (New York: The Free Press, 1958), pp. 542-550, and Albert J. Reiss, Jr., "Delinquency as the Failure of Personal and Social Controls," *American Sociological Review*, XVI (1951), 196-207.

³ I take the term "cultural deviance" from a paper by Ruth Kornhauser, "Theoretical Issues in the Sociological Study of Juvenile Delinquency," mimeographed, Center for the Study of Law and Society, Berkeley, 1963. Other terms for theories of this type are "cultural conflict," "transmission," "sub-cultural," and "differential association." The most influential theory of cultural deviance is Sutherland's theory of differential association (see Edwin H. Sutherland and Donald R. Cressey, *Principles of Criminology*, 7th ed. [Philadelphia: Lippincott, 1966], pp. 77-83). See also Walter B. Miller, "Lower

reconciliation of their assumptions is very difficult.⁴ If, as the control theorist assumes, the ties of many persons to the conventional order may be weak or virtually nonexistent, the strain theorist, in accounting for their deviance, builds into his explanation pressure that is unnecessary. If, on the other hand, it is reasonable to assume with the strain theorist that everyone is at some point strongly tied to *the* conventional system, then it is unreasonable to assume that many are not (control theories), or that many are tied to different "conventional" systems (cultural deviance theories).

In the present study I analyze a large body of data on delinquency collected in Western Contra Costa County, California, contrasting throughout the assumptions of the strain, control, and cultural deviance theories. I begin by outlining the assumptions of these theories and discussing the logical and empirical difficulties attributed to each of them. I then draw from many sources an outline of *social control* theory, the theory that informs the subsequent analysis and which is advocated here.

Strain Theories

Strain theories are the historical result of good answers to a bad question.⁵ The question was Hobbes's: "Why do men obey

Class Culture as a Generating Milieu of Gang Delinquency," *The Journal of Social Issues*, XIV (1958), 5-19.

⁴ The most forthright attempt to construct a theory combining assumptions from two of these perspectives is Cloward and Ohlin's synthesis of strain and cultural deviance theories (*Delinquency and Opportunity*). Kornhauser concludes that they have "constructed theories of subcultural delinquency that are congruent with social disorganization [strain] theories but unacceptable to cultural transmission theorists" ("Theoretical Issues," Part III, p. 4). My own analysis had led to the conclusion that the Cloward-Ohlin synthesis was unacceptable to both strain and cultural deviance theorists. In any event, the difficulties are clearer in Richard A. Cloward's "Illegitimate Means, Anomie, and Deviant Behavior," *American Sociological Review*, XXIV (1959), 164-176.

⁵ For the opposite and generally accepted view, see Talcott Parsons, *The Structure of Social Action* (New York: The Free Press, 1949), pp. 89-94, and Dennis H. Wrong, "The Oversocialized Conception of Man in Modern Sociology," *American Sociological Review*, XXVI (1961), 183-193. The question is bad because it assumes that something clearly variable is in fact constant. In their attempts to get out of this difficulty, sociologists are forced to pose the opposite question, which leads to the "interminable dialogue" which Wrong notes and approves of. Actually, of course, the problem of conformity and the problem of deviance are the same problem, and questions concerning conformity and deviance should be posed in such a manner that both problems can be solved at once.

the rules of society?" Although the Hobbesian question is granted a central place in the history of sociological theory, few have accepted the Hobbesian answer: "Of all passions, that which inclineth men least to break the laws, is fear. Nay, excepting some generous natures, it is the only thing, when there is appearance of profit or pleasure by breaking the laws, that makes men keep them."⁶ It is not so, the sociologist argued: there is more to conformity than fear. Man has an "attitude of respect" toward the rules of society; he "internalizes the norms." Since man has a conscience, he is not free simply to calculate the costs of illegal or deviant behavior. He feels *morally* obligated to conform, whether or not it is to his advantage to do so.⁷ As if this were not enough to show that Hobbes was wrong, the sociologist adduced yet another powerful source of conformity: "People are . . . profoundly sensitive to the expectations of others."⁸ Now, since others almost by definition expect one to conform, deviation can occur only at great cost to the deviator.

Having thus established that man is a moral animal who desires to obey the rules, the sociologist was then faced with the problem of explaining his deviance. Clearly, if men desire to conform, they must be under great pressure before they will resort to deviance. In the classic strain theories, this pressure is provided by *legitimate* desires.⁹ A man desires success, for example, as everyone tells him he should, but he cannot attain success conforming to the rules; consequently, in desperation, he turns to deviant behavior or crime to attain that which he considers rightfully his. The theoretical assumption that man is moral and the empirical fact that he violates rules in which he believes are thus made consistent.

Examples of this perspective are numerous:

⁶ Thomas Hobbes, *Leviathan* (Oxford: Basil Blackwell, 1957), p. 195.

⁷ Parsons, *Structure*, especially pp. 378-390.

⁸ Francis X. Sutton et al., *The American Business Creed* (Cambridge, Mass.: Harvard University Press, 1956), p. 264, quoted by Wrong, "The Oversocialized Conception," p. 188.

⁹ There was a more or less conscious attempt in one period of American sociology to avoid the evil-causes-evil "fallacy." Vices were consequently traced to prior virtues or to virtuous institutions: for example, crime to ambition, prostitution to marriage. The most sophisticated spokesman for this good-causes-evil view is Albert K. Cohen (see, for example, his "Multiple Factor Approaches" in Marvin E. Wolfgang et al., eds., *The Sociology of Crime and Delinquency* [New York: Wiley, 1962], pp. 77-80).

. . . a cardinal American virtue, "ambition," promotes a cardinal American vice, "deviant behavior."¹⁰

We suggest that many lower-class adolescents experience desperation born of the certainty that their position in the economic structure is relatively fixed and immutable—a desperation made all the more poignant by their exposure to a cultural ideology in which failure to orient oneself upward is regarded as a moral defect and failure to become mobile as proof of it.¹¹

Although the strain model has been applied to deviant acts that appear to be the result of cold calculation (the "decision" to become a professional criminal), it has been particularly appealing as an explanation of acts that are characterized by apparent irrationality or intense emotion (such as suicide and malicious and/or "pointless" destruction of property). Since the strain theorist uses such concepts as "discontent," "frustration," or "deprivation" as part of his explanation of deviant acts, he can easily transfer some of the emotion producing the act to the act itself. This virtue of strain theory is at the same time a source of difficulty.

Criticisms of Strain Theory

The strain theorist must provide motivation to delinquency sufficient to account for the neutralization of moral constraints. Once he builds motivation as powerful as this into his explanatory system, he usually has a plausible explanation of delinquency. "Intense frustration" would seem to provide sufficient motivational energy to account for "delinquency." In fact, given the seriousness of most delinquent acts, it provides a little too much pressure; and during the days, weeks, or months that the intensely frustrated boy is conforming to conventional expectations, its dormancy is hard to explain.

The fact that most delinquent boys eventually become law-abiding adults is also a source of embarrassment to the strain theorist. The conditions he builds into his model normally do not change during adolescence or, for that matter, at the attainment of adulthood. As the strain theorist himself contends, the lower-class boy's position in the economic structure is relatively fixed. His

¹⁰ Merton, *Social Theory*, p. 146.

¹¹ Cloward and Ohlin, *Delinquency and Opportunity*, pp. 106–107.

eventual reform, attested to by many empirical studies,¹² thus cannot be explained by changes in the conditions that initially forced him into delinquency.¹³

Delinquency is not confined to the lower classes. In order to get the pressure he needs, the strain theorist usually creates a perfect relation between social class and delinquency.¹⁴ This relation is "created": the strain theorist is interested in explaining only *lower-class delinquency*.¹⁵ Since there is no lower-class delinquency in the middle classes, the strain theorist may ask: What is it about the lower-class situation that produces delinquency? If he ever feels called upon to explain middle-class delinquency, the strain theorist has two options: he can argue that apparently middle-class boys committing delinquent acts are "really" lower-class boys;¹⁶ or he can reverse his original procedure and ask:

¹² Much of this literature is summarized and critically evaluated in Barbara Wootton, *Social Science and Social Pathology* (New York: Macmillan, 1959), pp. 157-172. Actually, one may derive contradictory statements from research on "reform." The statement in the text (most delinquent boys eventually become law-abiding adults) is true, but so, too, is the statement that most delinquent boys will be arrested for crimes as adults. The reconciliation of these statements is simple: the first relies upon a broader definition of delinquency than does the latter. To be meaningful, then, such statements must specify fairly carefully the degree of delinquency entailed. Follow-up studies of boys appearing in juvenile court suggest that a majority will be arrested for crimes as adults (see Henry D. McKay, "Report on the Criminal Careers of Male Delinquents in Chicago," *Juvenile Delinquency and Youth Crime*, Report of The President's Commission on Law Enforcement and Administration of Justice [Washington: USGPO, 1967], pp. 107-113, and Sheldon and Eleanor Glueck, *Juvenile Delinquents Grown Up* [New York: The Commonwealth Fund, 1940]. Follow-up studies of boys picked up by the police would show that the vast majority will not be arrested for crimes as adults (see Matza, *Delinquency and Drift*, pp. 22-26, nn. 30, 31).

Since strain theories attempt to explain the behavior of more or less serious offenders, the fact that the "great majority" of delinquent boys become law-abiding adults is not, strictly speaking, evidence against these theories. In the end, however, this criticism does not depend on a shift in the definition of delinquency. The fact is that strain theory has difficulty with "maturational reform" regardless of the proportion actually reforming.

¹³ The importance of maturational reform as a problem for most theories of delinquency is abundantly documented in Matza, *Delinquency and Drift*, pp. 22-26.

¹⁴ A perfect ecological correlation and/or a perfect individual relation as defined by such measures as Yule's *Q*. In short, low social class is a necessary but not sufficient condition for delinquency, as delinquency is defined.

¹⁵ Cloward and Ohlin, *Delinquency and Opportunity*, pp. 27-30; Albert K. Cohen, *Delinquent Boys* (New York: The Free Press, 1955), pp. 36-44.

¹⁶ Cohen, *Delinquent Boys*, pp. 157-161; Robert H. Bohlke, "Social Mobility, Stratification Inconsistency and Middle Class Delinquency," *Social Problems*, VIII (1961), 351-363.

what is it about the middle-class situation that produces (middle-class) delinquency?¹⁷

Every theorist discusses the actual class distribution of delinquency and attempts to base his decision to restrict his explanation to the lower class on an evaluation of available evidence. Implicit in such an effort is a criterion for deciding whether the relation between social class and delinquency is sufficiently strong to justify a class theory of delinquency. What is this criterion? How strong a relation between social class and delinquency is required to justify a class theory of delinquency?

The mere raising of these questions emphasizes the shakiness of the factual and logical foundations upon which strain theory is erected, since it is common among strain theorists themselves to question the correlation between class and crime.¹⁸ Indeed, recent delinquency research has tended to support this skepticism and more and more to undermine any theory that takes social class as the starting point for an explanation of juvenile delinquency.¹⁹

High aspirations are not conducive to delinquency. All strain theories generate pressure to delinquency from a *discrepancy* between aspirations and expectations. In order directly to test a strain theory, it is thus necessary to measure at least two independent variables simultaneously. (In Robert K. Merton's original theory, aspirations were assumed to be uniformly high within American society, and a discrepancy could thus be inferred directly from the realistically low expectations of segments of the population. However, subsequent research has undercut the assumption that all Americans place high and equal value on success, as Merton defined it.)²⁰ The need to measure two independent

¹⁷ Cohen, *Delinquent Boys*, pp. 162-169 (Cohen exercises both options); Ralph W. England, Jr., "A Theory of Middle Class Juvenile Delinquency," *Readings in Juvenile Delinquency*, ed. Ruth Shonle Cavan (Philadelphia: Lippincott, 1964), pp. 66-75.

¹⁸ See Merton, *Social Theory*, pp. 141-145, and Cloward, "Illegitimate Means," p. 174.

¹⁹ This literature is summarized and discussed in Chapter IV.

²⁰ Herbert H. Hyman, "The Value Systems of Different Classes," *Class, Status and Power*, ed. Reinhard Bendix and Seymour Martin Lipset (New York: The Free Press, 1953), pp. 426-442. Hyman shows that lower-class persons are less likely to have high aspirations. In a reply to Hyman, Merton argues (*Social Theory*, pp. 170-176) that the problem is one of absolute frequencies rather than proportions. Actually the problem is one of correlation: Are lower-class persons with high aspirations more likely to become criminal than lower-class persons with low aspirations?

variables at once has tended to shield strain theory from potentially falsifying evidence: for example, the finding that legitimate aspirations are negatively related to delinquency could be countered by the argument that the relation would become positive if expectations were held constant.²¹ At the same time, much indirect evidence that the desires upon which the strain theorist relies were at work has been provided by research which shows relations between delinquency and factors that presumably impede the realization of these desires, such as school failure. In fact, on the basis of this indirect evidence, strain theories appear to have substantial empirical support, and research articles in which one of them provides the interpretive framework appear regularly.²²

Nevertheless, there is some direct evidence that the relation between aspirations and delinquency does not reverse when expectations are held constant,²³ that many delinquents are not deprived in an objective sense,²⁴ and that many delinquents do not *feel* deprived in the ways suggested by strain theorists.²⁵

Therefore, I tentatively reject strain theory on the ground

²¹ Kornhauser, "Theoretical Issues," Part I, pp. 21-22.

²² For example, Erdman B. Palmore and Phillip E. Hammond, "Interacting Factors in Juvenile Delinquency," *American Sociological Review*, XXIX (1964), 848-854; Delbert S. Elliott, "Delinquency, School Attendance and Dropout," *Social Problems*, XIII (1966), 307-314. Palmore and Hammond rely on Cloward and Ohlin's theory, and Elliott relies on Cohen's.

²³ James F. Short, Jr., "Gang Delinquency and Anomie," *Anomie and Deviant Behavior*, ed. Marshall B. Clinard (New York: The Free Press, 1964), pp. 105-115. As far as I can determine, there is no good evidence to the contrary. Two studies apparently to the contrary are Arthur L. Stinchcombe, *Rebellion in a High School* (Chicago: Quadrangle, 1964), and Irving Spergel, *Racketville, Slumtown, Haulburg* (Chicago: University of Chicago Press, 1964). In Stinchcombe's study, the hypothesis that "whenever the goals of success are strongly internalized but inaccessible, expressive alienation results" is the subject of much analysis. I think it fair to say that this hypothesis explains very little of the "expressive alienation" in Stinchcombe's sample, and that the links between concepts and indicators with respect to this hypothesis are tenuous. Stinchcombe acknowledges the shakiness of his hypothesis and remains faithful to it only because he feels no alternative hypothesis is available.

Spergel's study is based on comparisons of three groups, each consisting of ten boys. Although his data often conform to minutely detailed hypotheses derived from strain theory, they are in effect ecological data since variation on delinquency within the groups is consistently ignored.

²⁴ Larry Karacki and Jackson Toby, "The Uncommitted Adolescent: Candidate for Gang Socialization," *Sociological Inquiry*, XXXII (1962), 203-215.

²⁵ Albert J. Reiss, Jr., and A. Lewis Rhodes, "Status Deprivation and Delinquent Behavior," *The Sociological Quarterly*, IV (1963), 135-149.