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COMMENTARIES

THINKING ABOUT COHORTS

JAMES Q. WILSON*

The three groups of scholars who present their findings in this issue of the *Journal* have undertaken an important and herculean task. For this undertaking, every student of crime must be deeply grateful. Prospective cohort studies are essential if we are to advance, in any fundamental way, our understanding of the causes of crime and provide important new leads for crime-prevention strategies. The Office of Juvenile Justice and Delinquency Prevention (OJJDP) should be congratulated for having had the foresight to finance research projects that, although important, will not bear fruit for many years. I earnestly hope that OJJDP will nourish to maturity the infant studies that it has conceived.

As one who has long urged such an approach, I confess to my great sense of relief that somebody else is doing the actual work. These papers convey quite vividly the difficulty of gathering wave after wave of data, coding and tabulating it, and then making sense of it. Just keeping up with the flood of information while at the same time satisfying the reporting needs of the funding agency must be more than a full-time job, with precious little time left over for analysis and writing.

Having said all that, let me focus on what remains to done—which is practically everything. These papers are reports of work in progress; understandably, their authors are in no position yet to provide clear answers to any important questions. But it is vital that all concerned keep their eyes firmly fixed on those questions and remember that, having spent the public's money, they owe something to that public—guidance on what to do about preventing or treating delinquency and the host of problem behaviors that covary with delinquency.

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These papers are a beginning. The findings announced here provide new evidence on some old topics and raise some new issues that many criminologists have tended to neglect. The Huizinga group in Colorado has shown once again that there are different kinds of delinquents and probably different tracks to any given kind of delinquency. Though hardly a new idea, the analytical technique employed to make that point here may make that idea more persuasive to those researchers who have ignored the importance of making distinctions. I confess, however, to some unease about allowing the differences to emerge entirely from empirically identified clusters. We ought to know enough by now about patterns of delinquent behavior to make some *a priori* distinctions, such as that between children whose behavior is primarily sneaky (*i.e.*, they steal, lie, and conceal) as opposed to aggressive or confrontational (*i.e.*, they fight, argue, and rob). Rolf Loeber in earlier publications has made this and some related distinctions, and Gerald Patterson at the Oregon Social Learning Center has shown that the prospects for effective family therapy may depend heavily on what type of misconduct one seeks to correct. (Ending aggressive behavior is easier than ending sneaky behavior, because the parents are more aware of the former and more rewarded by progress in its reduction.)

The Thornberry group at SUNY Albany reminds us that children affect their parents just as parents affect their children. This will come as no surprise to developmental psychologists, who have been saying the same thing for thirty years (at least). For some reason, however, that insight has not penetrated as fully into criminology as one would expect. The interactionist perspective is a vital one; only by accepting it can we at last begin to lay to rest two mischievous errors: that the child is a blank slate on which family and society write at will, and that a family with one bad child must be a wholly bad family. The errors are obvious once one realizes that siblings raised by the same parents are almost as different in their behavior as children of the same age and status raised in two different families.

The Loeber group in Pittsburgh has shown once again that youngsters that begin offending at an early age tend to come from problem families and to display a variety of disruptive behaviors. That is something we have known since the first cohort studies by Sheldon and Eleanor Glueck and by William and Joan McCord, but here it is supported by data from the children themselves as well as from many other informants and not just by official records. Moreover, the amount of offending at an early age may be much greater than anyone had previously supposed. What is even more interest-

ing is the possibility that the factors that cause young boys to begin offending may be different from those that lead older ones to start. Finally, Loeber *et al.* have drawn our attention to the interesting case of early desisters and the forces that may have caused this desistance. (I assume, of course, that the desistance is real and not simply an artifact of having measured behavior for only a short time.) Marvin Wolfgang and his colleagues, in their classic Philadelphia cohort studies, have shown that a large fraction of boys who commit one serious delinquent act do not go on to get into further trouble with the police, but these pioneering studies were unable to say very much about what caused this desistance or even whether it was true desistance or just greater skill at evading detection. If there really are a significant number of early desisters, as Loeber *et al.* suggest, their data may provide us with some clues as to why it occurs.

What remains to be learned? Almost everything. There are, no doubt, different pathways to delinquency, but what is the first step on that pathway? Is it a discordant family that fails to supply love and consistent discipline to an ordinary child, or is it an ordinary family that does not know how to cope with a temperamentally rebellious or hard-to-love infant?

Clearly, delinquency (like almost all aspects of the human personality) is the result of a complex interaction between the child and his family, but what does each bring to that interaction? Two schools of thought exist on this issue. One holds that the parents bring almost everything; problem behaviors result when the parents, especially the mother, fail to produce a pattern of secure attachment. The other asserts that the child's own temperament, present at birth, affects the ability and possibly even the willingness of the parents to produce a secure attachment.

Nobody is surprised to learn that children who begin their anti-social conduct at a very early age are the most troublesome youth and likely to become the most serious delinquents. But what has happened in those first few years of life to generate so sad a trajectory? By "first few" I mean exactly that—ages zero to three. Do we have any measures of the child and his family—intelligence, temperament, familial discord, or social stresses—that best predict early onset? *Can* we find such measures retrospectively, without a birth cohort? I doubt it.

We are relieved to learn that there may be some desisters among even the most troubled youth, and we yearn to know what produces the desistance. What, if anything, happened to these children that did not happen to others that is correlated with desistance? And, just as important, what happened to these children that

did happen to others but that nonetheless produced desistance? If it turns out that desisters benefitted from some intervention or circumstance that did not benefit others, then we have a clue to the existence of some individual difference that is worth exploring—an exploration that is best done (and perhaps only done) by studying children from birth (or even from conception) on.

Each of these studies is limited in the generalizations it can produce by one central fact: none is a study of a birth cohort, and thus none can fully test any of the leads suggested by these papers. I am aware that the authors of these studies cannot be taken to task for not analyzing data they do not have. My remarks on the importance of a birth cohort studied with biomedical as well as sociological instruments are primarily advice to funding agencies and other scholars. But the advice is not limited entirely to them: even the present research groups could do us all a service by sharpening their questions and highlighting those that *cannot* be answered with their available data. The unexplained variance should not be left in the error term of their equations, for what is unexplained is only partly “error” (as in measurement error); it is in large part, I conjecture, some combination of individual differences and contextual effects.

Individual differences are not likely to be the whole story. Another part of the story, perhaps available in the existing data sets, is to be found in neighborhood effects. An average parent coping with a somewhat difficult child may succeed in a neighborhood environment of intact families, safe streets, industrious habits, and human hope; the same parent with the same child may fail in an environment of disrupted families, mean streets, self-indulgent habits, and widespread despair. I assume that such contextual information exists and will be reported in future papers.

In my opinion, the central task for these research groups is to clarify and broaden the questions they hope to answer. As they occur to me, those questions include the following:

1. What are the principal types of misconduct that we observe in this age group? (Loeber has already published a great deal on this topic and his distinctions, it seems to me, are a worthwhile place to start.)
2. What are the correlates that are common to all types and what are the correlates that are unique to each type?
3. How much unexplained variance is there, and what clues, if any, do we have as to its causes?
4. What early interventions, if any, made—and just as important, failed to make—a difference in the rate of misconduct? All of

these children went to school; some were exposed to special programs in or out of school; some had parents who were involved in an intervention program. What difference, if any, did different schools, teachers, or services make?

5. How does the development of delinquency (or more broadly, conduct disorders) in these samples parallel the development of similar conduct disorders as reported by developmental psychologists and research pediatricians whose interest is not in delinquency but empathy, reciprocity, attachment, and school achievement? I am struck by how few of the references cited in these papers are by non-criminologists. There is a vast scholarly literature on child development with obvious and important parallels to the early findings of the present authors. To ignore it, if that is what has occurred, is to risk not seeing the behavioral forest for the criminological trees.

I have one final suggestion: these are studies of real human beings, not of variables and parameters. Obviously, a great deal of difficult variable analysis is necessary before any findings can be presented. I would urge the authors to put into a technical appendix to their papers as much of that preparatory work as possible and focus their papers on key variables and categories illustrated with actual case histories.

For example, suppose it should turn out that a key category is a child with a high rate of conduct disorder, defined as sneaky and thieving behavior, that began at an early age and persists to the present. Out of all subjects within those categories, select two or three modal examples and tell their stories in a paragraph. Suppose also that another important category is the child who belongs in the preceding category in every respect save persistence: he stopped engaging (at least for a while) in delinquent acts. Tell a few of those stories.

Doing this will not only help the reader, especially the non-specialist reader, grasp what is going on in the data, but it will also, I suspect, lead the researchers themselves to see new ways of categorizing and explaining their results. As someone who has used cluster analysis and structural equations (albeit at the novice level), I am keenly aware of how easy it is to get lost in the necessary complexities of such techniques and to report all of the grisly details and even the "heartbreaking" (the term of Thornberry, *et al.*) but failed attempts. Once in that swamp, the road back to safe ground and bright sun is easily lost. But we must return there if we are to serve the people who are paying our bills—citizens desperately worried about crime in general and their own children in particular. I realize

that when scholars are asked, as they were here, to write academic papers for an academic audience, they will focus on academic issues. But the purpose of this research is not academic in the narrow sense; it is to find answers to some of the most troubling questions that beset our society. I hope the authors will not assume that their job is done when they have dotted every theoretical *i* and crossed every empirical *t*, leaving "others" to draw out the larger implications and to state them in plain language. If they have not already done so, they should immediately draw up a list of the most important practical questions their research can address and write their next set of papers in a form and style that answers, insofar as they can be answered, those questions.

Good luck.