FOREWORD

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THESE PAPERS were selected for this collection by my mother, Joan McCord, after she discovered she had only a short time to live. They reflect the breadth and depth of the work she did from the middle 1970s until her death.

Many of the papers use the Cambridge-Somerville Youth Study data. The details of that study are well laid out in the collection, but it is worth providing a brief overview. The study was begun in the 1930s by Dr. Richard Cabot. Cabot was hopeful that a big brother/big sister/mentoring program could significantly improve a child's prospects of leading a good life, and he put in place an extensive program that offered support of a sort that seemed especially likely to help. At the same time, though, he was concerned to study the effectiveness of the treatment scientifically and with an eye to collecting data that might usefully be relied on to study the long-term impact of the intervention.

Identifying slightly over 500 boys between the ages of 5 and 13, half thought to be at risk and half not, Cabot had them paired up (matching, as much as possible, for risk factors, age, personal history, family background, and third-party reports) and then randomly assigned one member of each pair to a control group and the other to a treatment group. The treatment and control groups thus each had an equal number of boys who were at risk and boys who were not, and for each boy randomly assigned to the treatment group, there was a boy in the control group with whom he had been matched.

The treatment group was then assigned a counselor who provided a broad range of services, including tutoring, medical and psychiatric support, time in the country at summer camp, and exposure to YMCA, the Boy Scouts, and other groups. In short, the program was largely indistinguishable from the sort of mentoring programs that are now widely thought to be an effective way of helping kids in need. On average, the counselors worked with the boys for five years. The control group, in contrast, was simply left alone (although the same initial information had been gathered about them as about the treatment group). Extensive

records were kept about the boys, about what was done for them by the counselors and others, and about what the counselors observed in their interactions with the boys' families.

In 1975 and 1976, my mother, with the help of several dedicated researchers, tracked down 95 percent of the boys, now men, who had been in the study (with the treatment and the control groups more or less equally represented). Although some had died, most were still alive and the majority were living in Massachusetts, near where they had grown up. In addition to gathering various objective measures of these peoples' lives (from court records, mental hospital records, records from alcoholic treatment centers, and the registry of vital records and statistics in Massachusetts), they were all sent questionnaires. A little over half of the men filled out and returned the questionnaires.

I remember vividly when the data had been collected, and she began running some analyses. The results shocked her. She had long been impressed by the care and good sense behind the various treatments provided by the Cambridge-Somerville Program. So she had been thinking that she would be discovering some real benefits from the intervention, and had in any case assumed that, at worst, no significant benefit would be provable. What she discovered was that the treatment group was statistically significantly *worse off* than the control group along seven crucial dimensions: (1) the treatment group was more likely to have committed two or more crimes; (2) they were more likely to show signs of alcoholism; (3) they were more likely to show signs of mental illness; (4) they were likely to have died younger; (5) they were more likely to have health problems (specifically stress-related health problems); (6) they were more likely to have low-prestige jobs; and (7) they were more likely to be dissatisfied with their jobs. I should emphasize that the results were statistically significant along all seven dimensions, and the standard of relative harm is, in each case, set by a widely accepted objective measure. The longer the treatment, the more likely the harm.

These results were all the more surprising in light of the subjective reports of the treatment group, as revealed by the questionnaires. Those reports showed that two-thirds of those who filled out the questionnaires thought, in retrospect, that the program had helped them. Certainly, the fact that so many of the people who had counselors thought the program was helpful is important, and it helps to explain why people who serve as mentors are convinced that they are actually helping. But my mother regularly pointed out that the feeling of having been helped, and the satisfaction of thinking one has helped, are severely undercut by the clear evidence that this sort of intervention is likely, in fact, to harm the very people it is intended to help.

When the results of the Cambridge-Somerville Youth Study were published (1978),[1] they were met with two responses that are both puzzling and disturbing:

1. On the one hand, many people (especially practitioners actively involved in various mentoring programs) insisted that while the Cambridge-Somerville Program may have failed to help, and might actually have hurt the boys in the program, they were sure that their own programs were in fact helping. Yet their insistence on this point was

rarely accompanied by any real evidence that might deserve credibility. In some cases, of course, there were surveys and subjective reports and personal stories testifying to some perceived effectiveness of one program or another. The Cambridge-Somerville Youth Study shows, however, that such reports might go hand in hand with the program manifestly causing serious harm to those in it, so such reports should be suspect, to say the least. Indeed, the fact that people so readily claim, without appropriate evidence, that their own program is better, in the relevant respects, than the Cambridge-Somerville Program is disconcerting. It is as if a doctor were to claim that his own experience with Thalidomide, which he knows from experience cures morning sickness, constitute grounds for continuing to prescribe it despite the overwhelming scientific evidence that it causes birth defects in many cases. No one honestly concerned with the welfare of others should be ignoring objective evidence that what they are doing is harmful, even if their own experience suggests that it is having a good effect.

2. On the other hand, many people (especially those opposed to social programs introduced to help those at risk) claimed that the results of the Cambridge-Somerville Study showed that social intervention was bound to be ineffective. The Cambridge-Somerville Study shows no such thing. On the contrary, it establishes, with compelling evidence, that interventions can and do have an impact on the lives of people in the programs. The problem is that the impact might be harmful rather than beneficial. Although the Cambridge-Somerville Study is rightly seen as a cause of concern and caution, it establishes that social interventions have a measurable impact and thus gives some hope that continued efforts to introduce new programs, *followed by careful study of their effects*, are likely to lead to the discovery of some good way to help improve the lives of others.

Why was the program damaging? There are a number of possible explanations. It might be that receiving the mentoring and other help served to label the boys in ways that account for the damage; or it might be that the boys came to depend on the support and suffered from a sense of abandonment when the program ended; or it might be that the counselors conveyed to the boys a set of (upper middle-class) values that were ill-suited to the boys' circumstances and prospects. Or, perhaps, there was something about the specific form of the mentoring that explains the damage. Unfortunately, the Cambridge-Somerville data does not provide a basis for choosing among these hypotheses. They each have some plausibility, but it would be worse than unwarranted to embrace one or the other, or all of them taken together, as if we knew why the treatment program damaged the very people it was designed to help. The evidence simply is not in.

The main lesson my mother drew from her research was that anyone concerned to help others—and she was deeply committed to doing that—ought to work hard to study, in a scientifically credible way, the effect of their efforts.

Specifically, she thought it was important to establish control groups, to collect data in a way that would allow the use of objective measures of success, and to provide for a longitudinal follow-up to study the long-term effects of the intervention. She argued that when it comes to intervening in peoples' lives, we should insist on respecting the very same standards of scientific investigation as we rely on when it comes to intervening on their bodies with drugs and medical treatments. Anything less is, in effect, an abdication of our responsibility to care for those we seek to help.

A good deal of my mother's research after 1978 mined the rich fields of the Cambridge-Somerville Study—to investigate the effects of child abuse, neglect, and parental absence, the impact of the family on crime, and the etiology of alcoholism. Some of this work is reported in papers collected here. At the same time, she concentrated on questions of theory and methodology in the social sciences, working tirelessly to identify useful scientific tools and to help others design studies that will generate results likely to be taken seriously.

Throughout her career, my mother was interested in figuring out how we might properly combine an understanding of the causes of behavior with an appreciation of agents as (often) responsible for what they do. She was convinced that criminology, and psychology more broadly, needs to explore why it is that people take the various considerations they do as reasons. To focus on the etiology of behavior, on the influence of genes or socialization or happenstance, without regard to the standing of those who engage in the behavior as agents acting for (what they take to be reasons) is to ignore the very feature of humans that make them responsible for what they do. Consequently, she began working on a theory of motivation—and of what it is to act for a reason—designed to explain how human beings become agents able to act on reasons. It is a theory that takes seriously the idea that people, in acting as they do, are acting not merely as a result of various causal influences but also as a result of what they (understandably, although not always correctly) take to be reasons. Sadly, this is a research program that she was unable to pursue as far as she hoped, but it did result in what she called *The Construct Theory of Motivation*, which she articulated and defended in her "He Did It Because He Wanted To . . . ,"[2] "A Theory of Motivation and Life Course,"[3] and "Toward a Theory of Criminal Responsibility."[4]

All who knew Joan McCord knew her as a person of phenomenal intelligence, imagination, energy, and integrity. A few of us had as well the opportunity to know her as the extraordinarily loving, and incomparably wonderful, mother she was.

Notes

1. McCord, J. A thirty-year follow-up of treatment effects. *American Psychologist* 33(3): 284–289.

2. In Motivation and delinquency. Nebraska Symposium on Motivation, edited by W. Osgood, 44: 1–43. Lincoln, NE: University of Nebraska Press, 1997.

3. In *Social Dynamics of Crime and Control: New Theories for a World in Transition*, edited by Susanne Karstedt & Kai-D Bussmann, 229–241. Portland, OR: Hart Publishing, 2000.

4. In *Beyond Empiricism: Institutions and Intentions in the Study of Crime. Advances in Criminological Theory*, edited by J. McCord, 13: 147–176. Picataway, NJ: Transaction Publishers, 2004.