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Learning How to Learn and Its Sequelae*

Joan McCord

High school in Tucson, Arizona, left me thinking that education was a matter of learning how to repeat what others wrote. Fortunately, at Stanford, I had two lucky breaks that taught me otherwise. The first was in a philosophy course that challenged me to think critically about what I read. The professor assigned a series of incompatible theories. When we realized that, being incompatible, they couldn't all be right, we were forced to rethink our earlier conviction concerning each. What a lesson that was. Thirty years later, I returned to Stanford and let Professor John Mothershead know that his course had made a tremendous difference in my life; it had turned me from an acceptor of received opinions into an independent and skeptical thinker.

The second lucky break came in the form of an extraordinary professor who agreed to give me a tutorial course. He agreed on condition that I would read Spinoza, a notoriously difficult project. Having practically no background, but loaded with enthusiasm, I agreed. The reading was dense; the weekly sessions downright painful. Each week, I reported on the sections I had read and what they seemed to mean. Professor Davidson asked for more. Why had Spinoza made that specific argument? What issues were relevant to the questions it addressed? Had the argument been made by others? He conveyed the clear impression that reading the text, even carefully reading the text, was not enough. I remember no compliments, no recognition of effort spent, nothing but a careful exposure of what it means to master a topic. His teaching, I believe, put me on track to become a researcher in a broad and multi-disciplined field that investigates justice.

William ("Bud") McCord and I had been friends since high school. We went to Stanford together and were married as undergraduates. We

*I thank my son Geoffrey Sayre McCord for his careful editing of this manuscript.

decided on academic careers together. That decision was easy once we had been given the sensible advice to talk with people who occupied positions we envisioned ourselves as having in about 10 years. "Don't ask their advice," Professor Cowley said, "but talk with them about their daily routines. Ask them what they enjoy about their jobs. Then decide which lifestyle best fits you." We talked with business leaders in a variety of specialties, with lawyers who had become partners in firms, and with well-established bankers. These people were successful and most of them enjoyed what they were doing, but we found—hands down—that what the professors did was what we wanted to do. We also agreed that I would support Bud while he got his credentials before going on for my own.

After graduation from Stanford, Bud enrolled at Harvard to study with Sheldon and Eleanor Glueck and I got a position teaching children in Concord, Massachusetts. Classroom teaching provided an important opportunity for me to learn more about children. Occasionally, I used my sixth graders to experiment with theories about learning. The experiment I remember best involved teaching children what to value. At the beginning of the year, I asked the class to list their subjects in order of preference. Almost all the 30 children ranked arithmetic at the bottom. So arithmetic became my target. I instituted an Arithmetic Club. It would be open only to those who got 100 percent correct on an assigned paper. (Because I gave individualized assignments, everybody had a chance to join.) Members of the Arithmetic Club were given a special privilege: They were allowed to do weekly arithmetic homework which I would grade. At the end of the term, the class reevaluated their subjects. Confirming a theory about using rewards to shape preferences (based on my interpretation of some work done by Leon Festinger), arithmetic had moved upward for all the students and it had become a favorite for most. This classroom experiment exposed the folly of common practice in using homework as punishment, a practice that sends the message that learning is painful, rather than a privilege.

The classes also proved useful for pre-testing the measurement instruments Bud and I intended to use in our work for *Psychopathy and Delinquency* (1956). One of the tests, designed to measure aggression, involved a dog. The children were to identify the dog's preferred solution in a variety of problem situations, one solution being aggressive, one ameliorating, and one withdrawing from the situation. Because we knew some of the delinquent children to be tested would be illiterate, we used pictures showing the situations and each of the choices. Before giving the test the first time, in my Concord classroom, I had rated the children for their aggression. To honor our own dog, we referred to the one in the test we had designed as "Chumley," asking for each situation, "What would Chumley want to do?" An example is a picture showing Chumley's family displaying a new baby to an unhappy dog.

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The attached choices asked: "Does he want to bite the baby (picture)? Does he want to go off in a corner to show how badly he feels (picture)? Or does he want to make friends with the baby (picture)?" Almost all the children picked the friendly choices for just about all the situations, with no differences between the most aggressive and the least. Two months later, I reintroduced the test, giving the dog a different name. Same situations and same pictures. This time the children were asked, "What would Rover want to do?" It was Rover's family bringing home a new baby, having his bone stolen by another dog, seeing a birthday cake on the kitchen table, etc. Although the children had not differed in their perspectives of Chumley, with all finding him to be friendly, they differed in their assignment of preferences for Rover. The more aggressive ones indicated that Rover would want to bite the baby, fight the dog who took his bone, and take the birthday cake. We learned the importance of a name for attributing personality. We also had constructed a test that could be used for differentiating more aggressive from less aggressive children.

One of my aunts introduced us to Ernst Papanek, director of the reform school in upper New York State that took seriously delinquent boys from New York City. With Papanek's permission, Bud and I spent one summer working with and studying delinquents at Wiltwyck. In a reform school without bars, milieu therapy sought to change children by altering their environments. Papanek believed that using punishments would teach the children to use force to gain what they wanted. As an alternative, he manipulated the environment so that the children could learn the consequences of what they did without being punished. They were taught to repair damage. Their classrooms were models of compassion, as the specially trained teachers learned to cope with high degrees of maladaptation.

The whole day for each child was a part of his therapy. Cooks, for example, helped by teaching the children how to make pastries. Each child at Wiltwyck had a small plot of ground on which to grow flowers or vegetables. The boys were also taught to fight with gloves rather than fists. Floyd Patterson, the heavyweight boxing champion, was a graduate and returned annually to coach the boys. In the evenings, counselors retired to their quarters to read and play ping pong. I acquired a lifetime passion for ping pong. Much more important: I learned not to use punishments (i.e., the intentional giving of pain to control behavior).

Harvard had summer scholarships in Education. I was able to "test out" of most of the education courses and use the scholarships to take courses in philosophy. Once Bud received his Ph.D., I quit teaching in Concord and obtained a research assistantship at Harvard, working for a group of social scientists at the cutting edge of their fields. Eleanor MacCoby and Harry Levin were studying child development, using interviews

with mothers and doll play with the children. I "coded" the doll interactions, which involved marking on a sheet of paper what the child did with dolls that represented a boy, a girl, a mother, and a father. John and Beatrice Whiting were developing codes to check theories of child development against a broader diversity of customs than can be found in the United States. I coded reports on discipline for the Human Relations Area Files housed at Yale. Wesley Allensmith set up experiments to identify and then study children easily led by temptation. In one, children were told they could get prizes by hitting a hidden target with a beanbag. They were also told that it was against the rules to look behind the cloth to see the target. Through a one-way mirror we could identify which children nonetheless peeked, and the studies then explored how those children differed from others who obeyed the rules. Although I didn't work for him, Roger Brown was also at the Laboratory of Human Development. Brown was studying the influence of language on children's thought. Every noon, John Whiting stirred up a pot of soup in the kitchen, where faculty and students ate together. Whiting, director of the laboratory, had one clear rule: lunch conversation was to be about research. Students generally listened while the faculty talked. Listening to the discussion provided the most intense tutorial I have ever attended.

Most of the work for Sears, Maccoby, and Levin's *Patterns of Child Rearing* was done at the Laboratory of Human Development. They had used a questionnaire that involved detailed reporting by the mother about how parents treated their children and each other and how the children behaved. I still recall—and this was in 1955—the lunch in which Eleanor Maccoby described what they had learned about the relation between a mother's discipline and her child's misbehavior. I asked my first question: "How do you know that the mother's report is accurate?"

I had my doubts. Perhaps these came from having overheard my mother's frequent criticisms of me to her friends, criticisms that seemed to me unfair and inaccurate. Subsequent studies, by me and others, supported the skepticism.

My first son was born in December, 1956. The pleasures, for me, of parenthood are beyond description. Though I hated being pregnant, I loved caring for Geoffrey. A fellow teacher at Concord had a baby the year before, and because both of us were convinced that punishments were unnecessary, we discussed alternative ways to teach our infants how to act. My work at Wiltwyck, too, contributed to a commitment to rear my children without using punishment.

Years later, I wrote about the theoretical grounds for that commitment in two articles: "Unintended Consequences of Punishment," published in *Pediatrics Supplement*, 1996, and "Discipline and the Use of Sanctions," published in *Aggression and Violent Behavior*, 1997. I

hope the detailed explanations to be found in these articles will help many parents create comfortable nonpunitive environments in which to rear their children.

In the summer of 1957, I was offered a small sum of money to evaluate effects of the Cambridge-Somerville Youth Study on crime. Richard Clark Cabot, who had been one of Sheldon Glueck's professors, designed a randomized control study to test the idea that providing assistance to families of young boys in distressed neighborhoods would reduce delinquency. Each boy was matched to another with similar backgrounds, family structures, and parental behaviors. They also were matched in terms of such characteristics as early aggressiveness, intelligence, and physical strength. One boy in each pair was placed in the treatment group while the other was simply left to his community. The treatment program included tutoring, counseling, coaching in social skills, medical help, and leisure activities. Because of the matching, aggressive and non-aggressive kids were in both the treatment and the control groups.

Evaluation of the program back in 1948 had failed to find the anticipated benefits, and Gordon Allport believed that was because the evaluation had been carried out too soon. We tracked the records of the randomly assigned treatment and control boys and coded their case records on a variety of measures. *Origins of Crime* (1959) reported the results, which again failed to find anticipated benefits from the treatment. We did not even consider the possibility that the treatment might have been harmful!

The case records for the Cambridge-Somerville Youth Study were written with exquisite detail. Cabot, who had been president of the National Conference on Social Work in 1931, wanted to improve the practice of recording information so that case records would be more useful for designing treatments. He hired recorders to transcribe information about contacts between social workers and their clients within hours of the encounters. The case records included information about conversations and other interactions among family members. Such encounters typically took place about twice a month for more than five years, filling several hundred pages of narrative reports. I wanted to use the case records as a source of information about socialization and especially as a way of exploring the impact of child-rearing techniques.

Bud and I successfully applied for funds to code the case records for evaluations against independent information found in records of alcoholism and of crime. The result was *Origins of Alcoholism* (1960). This was the first longitudinal study of alcoholics and, because we learned about characteristics that preceded alcoholism, it enabled us to test which of them might be causal. The work benefited hugely from my training with Eleanor Maccoby. She had taught me to define variables carefully and avoid building evaluations into the coding scheme. The earlier codes, those used in *Origins of Crime*, reflected the categories used

by the Gluecks. After 1957, all work based on the Cambridge Somerville Youth Study case records used the redesigned coding scheme. Eleanor had also taught me to worry about reliability, so we tested reliability of ratings by having more than one person code a randomly selected group of case records and used only codes that attained a reasonable degree of reliability.

By Autumn of 1957, I was a full-fledged student in philosophy, with a Josiah Royce Fellowship at Harvard. Concerned to bolster my knowledge, I convinced the teaching assistant in Quantitative Logic to tutor me in exchange for my doing his grading. I was reading Descartes and Hobbes, Hume, and Locke while struggling with Hilbert and Ackermann. A.I. Meldon, J.L. Austin, and W.V.O. Quine were lecturing, and Wittgenstein's *Philosophical Investigations* excited us all. H.L.A. Hart gave a seminar to which all our professors came to argue. It was a wonderful time and place to be learning.

Around that time, my husband was offered a position as Assistant Dean of Humanities and Sciences at Stanford. He loved teaching at Harvard, but he was enticed by the opportunities at Stanford. Although I had been offered, and wanted to accept, a Teaching Assistantship with Roderick Firth in Ethics, we went to California, and I returned to empirical studies as a Research Associate at Stanford.

We rented, for 99 years, a plot of land on Stanford grounds. My cousin Ellen designed our new house, checking the blueprints with her architect father, and I supervised the building. Ellen later became a Senior Editor at *Architectural Forum* and wrote pioneer articles about the relationships between buildings and their occupants. The house was a perfect environment for us, with Bud's study off a private patio and away from noise, and mine within sight and hearing of the play area for our children. Shortly after we moved in, our second son, Robert, was born.

In 1961, we joined 80 Stanford students to spend almost a year in France. Stanford owned a building in Tours that included dormitories and classrooms. We had a small apartment, and I struggled to learn French, attempting to imitate the accents I heard from kids in the parks as I supervised my children playing. In the summer, we lived in a beautiful village, Croix-de-Vie, on the Atlantic coast. With only outdoor plumbing and two young children, living was complicated. But daily shopping brought me into contact with friendly merchants who were willing to listen to my dimly comprehensible French. Their willingness to converse helped me to understand a culture in which relaxation and discussion took precedence over earning money and being successful.

When we came back to California, I received a Stanford Wilson Fellowship to study philosophy. I returned to a field that continues to be the foundation for my thinking. Donald Davidson, Patrick Suppes, Jaegwon Kim, and Richard Jeffrey were the faculty from whom I learned a great deal. Davidson was working on problems linking reasons to

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actions and puzzling over issues in the philosophy of language. Suppes was in the process of converting set theory to methods for teaching arithmetic to young children, so that his course in logic included a clear exposition of Peano arithmetic as well as first order predicate logic and the axioms of set theory. Kim brought out the relevance of Plato and Aristotle to contemporary issues in philosophy. And Jeffrey conveyed the thrill of discovering Russell's Paradox at the foundation of mathematics, the Lobatchevski interpretation of one of the axioms in Euclidean geometry, and the role of transformations for understanding physics. The department had a "pro-seminar" in which first-year graduate students were required to produce a paper in order to exhibit what they knew. The faculty took the opportunity to show how little we knew by asking tough questions. Although the process was difficult, it helped teach us high standards.

I write about my education hoping it communicates the sense of adventure and the pleasures and challenges of learning. Some of this, it seems to me, is denied students when courses are too easy and are designed primarily to help students "feel good about themselves." But back to the autobiographical story.

I was combining being a student with raising my sons, though I had stopped doing research in sociology. I loved my studies and I was doing well. Soon, I began to think about having an independent career that would grow out of my love of philosophy. Yet shortly after the second semester began, Bud began drinking heavily and became abusive. Bud and I had written extensively about alcoholism, in part because both of his parents had been alcoholics. We had noted, in *Origins of Alcoholism*, that our work would be justified if it contributed to prevention of alcoholism. And both of us believed for a while that our work had indeed saved him. I hoped that Bud's attacks might end when he saw that my independence would not reduce my love for him. To this day, I do not know whether to consider alcoholism as a disease for which one should not be held responsible. I do know that Bud objected to me taking on an independent career. He convinced my family and some of our friends (who were his colleagues) to urge me to drop out of school. Not wanting to be in the middle of a domestic argument, the Stanford philosophy department withdrew its financial support for me. This was 1963. I sometimes wonder whether women today realize how different it is both to have support from other women and to have a legal system that allows for their independence. (I grew up at a time when women could be fired from teaching positions if they became pregnant, when airline hostesses could be fired if they got married, and when it was legal to tell women that they need not apply for a position because a company or university was interested only in hiring males.)

There is little worth writing about here except to say that sometimes divorce is the best option. Years later, I mentioned feeling guilty because

I had deprived my sons of living with their father. Both of them assured me that it had been the right decision. Interestingly, recent research has shown, contrary to popular opinion, that single-parent families are less likely to be damaging to children than are two-parent families in which the parents do not get along well.

Yet the period immediately following the divorce was difficult for me. Needing to support myself and two sons, I could not continue my studies. After a lean year of renting out a spare room in our house, tutoring kids, coaching tennis, and part-time consulting to put food on the table, I obtained a generous fellowship from the National Institute of Mental Health to finish graduate work. The fellowship gave me three years. My original plan had been to continue with philosophy. But I had a good publishing record in the social sciences, having co-authored four books and published 18 articles (some of which had been republished and had therefore reached a wide audience). Several professors counseled that it would be easier for me to find a position in the social sciences, so I decided to take courses in sociology. My sons often accompanied me to class, drawing quietly in the back of the room. I had jumped from being a Research Associate to becoming a student, from being a faculty wife to being a single mother. The shifts made dramatic differences in the way people interacted with me. Prompted by this sad experience, for my thesis, I ran an experiment to study the impact of status.

The experiment had me seen as acting in one of two roles simultaneously. So they would not see one another, a screen separated the two subjects in the experiment at the same time. One believed I was a Professor (with "Professor McCord" on the door); the secretary I hired used the title three times as she spoke about me while leading the subject to the experimental room. The other believed I was a student, with "practice room" on the door and the secretary referring to "Joan, one of the students here" and using just "Joan" to refer to me. I described the study to both at once, so whatever differences were uncovered could not be attributed to differences in my behavior. (In my mid-thirties, I could pass for either a professor or a student.) The subjects were of the same sex and had been randomly assigned to each status. I knew neither the order of entry nor which had been led to believe I was a professor and which believed I was a student.

I was testing a central tenet of sociologists at Stanford: the belief that status mattered a great deal to everyone. I designed a measure of influence that allowed me to use only the subtle pressure of expressing my preference. The task was to select the best sentence from among sets in which one used an adjective, one an adverb before the verb, and one an adverb after the verb. Pretests had shown that without influence, students were equally likely to pick each of the sentence forms in a set. In the experiment, I expressed a preference to both high- and low-status

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subjects simultaneously. That preference was rotated so that each type of sentence was chosen, though for different pairs. Other people had shown that women were more easily influenced than were men, so I evaluated the task to be sure there was room for influence, that is, that there would not be a ceiling effect. My study demonstrated that status descriptions—in the absence of any behavioral differences—made a difference in terms of influence for males. Yet, contrary to the universality assumption about status, status mattered little in terms of influence to the females. I received a Ph.D. in sociology in 1968.

June 1968 was before there were any legal barriers to discriminating against women, so it was particularly difficult for me to find employment. I wanted to move East, where theater and classical music beckoned, but my few offers came from western schools. Luckily, I had a friend from Harvard days teaching at Drexel University in Philadelphia. He remembered that I loved philosophy and thought that at Drexel I could teach philosophy as well as sociology. After a successful interview, the boys, two dogs, two cats, and I moved to a house just outside of Philadelphia. For a few years, I taught four courses per quarter and barely kept my head above water. Even so, my schedule was flexible enough to allow me to cheer my sons through baseball and soccer seasons. I married my second husband, Carl Silver, in 1970. A specialist in human factors, he delighted in classical music, opera, and fine food. He had courted all three of us, wisely, for my sons and I were good friends and I listened to their counsel.

My earlier work on the causes of alcoholism and crime had been carried out with a sample then in their early thirties. I knew that alcoholism may not develop until later in life and also wanted to use the same sample to learn more about criminal careers. But longitudinal research, research that requires tracing individuals, was expensive and decidedly not in style. It was difficult to convince agencies that the expenses involved would produce enough benefits to make them worthwhile. Since 1970, partly as a consequence of my work, several federal agencies as well as many researchers have come to recognize that if we are to understand behavior, we must come to understand how it develops. That involves learning the sequence of events, which in turn, requires longitudinal studies. Although some knowledge of sequencing can be gained retrospectively, studies of memory have shown the hazards in assuming one should believe what people claim to remember.

After several attempts to get financial support for the study, the National Institute of Mental Health again came to my rescue; this time, with a three-year grant to retrace the former clients of the Cambridge-Somerville Youth Study. As a result of the design, the Cambridge-Somerville Youth Study offered an opportunity for a strong test of the possibility that providing families with multiple forms of assistance would be beneficial to their sons. Feedback from the participants indi-

cated that they remembered the program and most believed it to have been helpful. I collected evidence from the courts, mental health facilities, alcoholism treatment centers, and death records.

Drexel, largely an engineering school, was not accustomed to social science research. There were no graduate students in sociology or any of the social sciences. Nevertheless, I managed to pull together a team of enthusiastic and capable researchers, and Drexel personnel included some exceedingly competent support staff. Michael Wadsworth, Jack Block, Jerry Bachman, and Glen Elder, among others, came to my advisory conferences, so I had colleagues with whom to discuss the project. The idea of having such annual conferences came from my Program Officer at NIMH, Tom Lalley, who realized that research thrives on discussions and was kind enough to provide funds to make such discussions possible for me.

My team managed to find 98 percent of the former members of the Cambridge-Somerville Youth Study despite there having been no contact for 30 years. That took some fine detective work by a staff of amateur sleuths. Moreover, we managed to get approximately 75 percent of the men, who were in their late forties and early fifties, to respond either to questionnaires or to interviews. Richard Parente did the interviewing. He traveled throughout the country, tirelessly adjusting his schedule to accommodate others, but unwilling to accept a refusal. Rich was able to put people at ease, and convince almost everyone to let him talk with them. To assure equivalence of treatment, Rich was not informed about which of the men had been in the treatment group and which had been in the control group.

The men who had been in the treatment program were compared with others reared in similar families up to the age when treatment began. Had there been no treatment program, they could be expected to have similar lives. As it turned out, those in treatment died an average of five years younger, were more likely to be recidivist criminals, and more likely to have become alcoholics, manic-depressives, or schizophrenics. The treatment had been harmful! Results of this provocative study have been well scrutinized. They have been published in *American Psychologist* and by the National Academy of Sciences. The most recent report appears in "The Cambridge-Somerville Study: A Pioneering Longitudinal Experimental Study of Delinquency Prevention," published in *Preventing Antisocial Behavior: Interventions from Birth through Adolescence* (1992).

Because the evidence cast doubt on so many social assistance programs, it was initially difficult to get the results published. Yet subsequently, many other researchers have reported similarly negative results from treatment programs that give the appearance of being beneficial. This line of research brought me invitations to speak in Canada, France, England, Sweden, and Switzerland. Recently, Tom Dishion and I pulled

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together evidence on effects of intervention programs to argue that the problem is related to mutual support among misbehaving young adolescents. The resulting article appeared in the September 1999 issue of *American Psychologist*.

A second major thrust of my research focused on the powerful impact of parental socialization. The studies showed that family interactions, rather than the presence or absence of parental figures, accounted for major differences between criminals and noncriminals (McCord, 1982). Parental conflict and aggressiveness (e.g., using harsh punishments or throwing things when angry) appeared to produce the more serious types of violent crimes (McCord, 1991a). Because the records included information about the parents, I was also able to show that some of the continuity of crime across generations was due to the tendency of criminal fathers to behave in criminogenic ways (McCord, 1991b).

In 1987, I accepted an offer to move from Drexel to Temple University. Shortly after, I became President of the American Society of Criminology—the first woman to have the honor. The presidency was particularly pleasing because my son, Geoff (who is a philosophy Professor at the University of North Carolina, Chapel Hill) came to Reno to introduce my Presidential Address (McCord, 1990). It has also been my good fortune to win recognition for my research, the Prix Emile Durkheim in 1993 from the International Society of Criminology and the Sutherland Award in 1994 from the American Society of Criminology.

Many of my newest projects focus on theory (McCord, 1997) or on methodology (McCord, 2000). Yet just about every topic related to criminology intrigues me. In recent years, thanks to Mark Haller, a colleague who is a historian, I have been reading social history and discovering the degree to which that history enlightens current issues in criminal justice. Effects of that reading appear in "Placing American Urban Violence in Context." (McCord, 1997).

For many years, my interests in criminology have been nourished through reviewing activities. Felice Levine (now Executive Secretary of the American Sociological Association) ran stimulating semiannual seminars to review applications sent to the National Science Foundation's Program in Law and Social Sciences on which I served from 1987 to 1991. I've also regularly been a reviewer for the Office of Juvenile Justice and Delinquency Prevention, the National Institute of Justice, the Center for Disease Control, the Department of Education, and several foreign granting agencies (as well as numerous journals and book publishers). Rarely, in my experience, are there sharp differences in evaluations at the top and bottom of the application piles. The interesting discussions occur in the middle ranges, as reviewing panels try to estimate the contributions an unseasoned applicant might make or the centrality of an issue being addressed by a well-qualified applicant. Rarely,

too, have any of the reviewing panels expressed preferences that were not scientifically supported.

At Drexel, most of my students were the first in their families to go to college. The students spent half the year in jobs, through a "Co-op" program. It was a challenge to help them see the value of learning from books. In teaching sociology, I selected books that I found interesting, and then designed activities that they could use to help them see the relevance to everyday life of what they read. One of the more fruitful assignments was tied to Goffman's *Stigma*. Students were asked to talk with a stranger for 10 minutes, then introduce a stigma about themselves and try to continue the same topic of conversation. The students were wonderfully imaginative. One was fixing his car with a stranger watching, commenting, and praising his work. After 10 minutes, the student mentioned that he had learned to be a mechanic "at Holmesburg" (a local reform school). The stranger slammed down the hood of his car and almost broke my student's hand. Another applied for a job and was clearly about to be hired when she handed in the health portion of her application: She had listed "epilepsy in remission." She was pushed out the door amidst much yelling and anger. Another was talking with a stranger while waiting for the train. Her friend stopped by, by design, and said how glad she was to see my student well and out of Byberry (the local mental hospital). Although the stranger had offered to show my student around New York City, she left as soon as she heard the word "Byberry." Another, while changing a tire on the highway, confessed that he was just out of reform school and looking for a job. He was given advice by, as it turned out, a parole officer (a member of the "wise" as Goffman described them) who counseled him not to mention his incarceration when applying for the job. It pleased me particularly to win the University's Lindback Award for Distinguished Teaching.

At Temple, I periodically teach in the Intellectual Heritage program, where first- and second-year students are introduced to classic texts. In addition, I have had the pleasure of teaching graduate students as well as undergraduates. I have particularly enjoyed the handful of graduate students who, as my students, have shared with me the adventures of discovery.

One of the benefits of an academic life is the opportunity to travel. Editing for the Harry Frank Guggenheim Foundation took me to a small town in the mountains north of Rome. A project on the impact of developing democracies took me to Budapest and Prague. My research on psychopathy brought me to Valencia. My work on alcoholism resulted in trips to Tel Aviv and to Moscow just before the Soviet Union collapsed. As Vice President of the International Society of Criminology, I regularly visit Paris for meetings of the Board, and joined those who went to Rio de Janeiro to provide one of the courses sponsored by the ISC. My focus on longitudinal methodology has, over the years, resulted in

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invitations to Bristol and Cambridge (UK), Voss, Stockholm, Warsaw, Poland; Vienna, Taipei, Groningen, Freudenstadt, and Rhodes. There have been other trips as well, but these stand out. Carl liked to travel and often joined me after my work was finished. Together, we ventured into Egypt and Turkey, Korea and Japan, and drove throughout France and Ireland. Even after Carl was wheelchair bound, we went to Bayreuth, Germany, for the Wagner festival. Carl died in 1998. My son Rob (now President and CEO of the Eastern Technology Council and a venture capitalist) accompanied me for a week in China, where I was giving a couple of talks before going on to Canberra, Australia. In Canberra, I was assigned an office down the hall from the philosophy department, where son Geoff was on a research fellowship.

Writing about my life has forced me to recall events I had not thought about for years. Of course I have regrets, but on major choices, I lucked out. As a career skeptic, I am reluctant to give advice. Yet because the editors of this book requested each author to include advice for the readers, let me draw to a close by complying with that request.

I would urge women to obtain credentials so they can find interesting things to do, activities that will not be heavily dependent on events over which they will have little control. I would not trade being a mother for any opportunity at all, though I recognize that a taste for being a parent is far from universal. I would suggest to anyone considering academe, male or female, that it is better to work on projects you believe to be important than to select with an eye to winning praise or prizes. And I would encourage friends at any time to find fields of inquiry that intrigue them so that they can always experience the satisfactions of learning.

Recommended Readings

The list is highly idiosyncratic, but I've identified four (maybe five, depending on how they are counted) that ought not be overlooked.

Plato. *Euthyphro* and *Meno* (various editions are available.)

Shaw, C.R. & McKay, H.D. *Juvenile Delinquency and Urban Areas*. The University of Chicago Press, 1942, revised edition, 1969.

Douglass, Frederick. *Narrative of the Life of An American Slave Written by Himself*. (various editions are available.)

Jackson, K.T. *Crabgrass Frontier: The Suburbanization of the United States*. Oxford: Oxford University Press, 1985.

A Dozen of My Best

- McCord, J. (2000). "Developmental Trajectories and Intentional Actions." *Journal of Quantitative Criminology*, 16(2):237-253.
- McCord, J. (1997). "Placing American Urban Violence in Context." In J. McCord (ed.) *Violence and Childhood in the Inner City*, pp. 78-115. New York, NY: Cambridge Press, 1997.
- McCord, J. (1997). "He Did It Because He Wanted To . . ." In W. Osgood (ed.) *Motivation and Delinquency, Vol. 44 in Nebraska Symposium on Motivation, Vol. 44*, pp. 1-43. Lincoln, NE: University of Nebraska Press.
- McCord, J. (1997). "Discipline and the Use of Sanctions." *Aggression and Violent Behavior*, 2, 4:313-319.
- McCord, J. (1996). "Unintended Consequences of Punishment." *Pediatrics Supplement*, 98(4):832-834.
- McCord, J. (1995). "Crime in the Shadow of History." In J. Hagan (ed.) *Current Perspectives on Aging and the Life Cycle: Delinquency and Disrepute in the Life Course*, pp. 105-118. Greenwich, CT: JAI Press.
- McCord, J. (1992). "The Cambridge-Somerville Study: A Pioneering Longitudinal Experimental Study of Delinquency Prevention." In J. McCord & R.E. Tremblay (eds.) *Preventing Antisocial Behavior: Interventions from Birth through Adolescence*, pp. 196-206. New York, NY: Guilford Press, 1992.
- McCord, J. (1991a). "Family Relationships, Juvenile Delinquency, and Adult Criminality." *Criminology*, 29, 3:397-417, 1991a. Republished in F.R. Scarpitti & A.L. Nielsen (eds.), *Crime and Criminals: Contemporary and Classic Readings in Criminology*. Los Angeles, CA: Roxbury Publishing.
- McCord, J. (1991b). "The Cycle of Crime and Socialization Practices." *Journal of Criminal Law and Criminology*, 82, 1(Spring):211-228.
- McCord, J. (1990). "Crime in Moral and Social Contexts." *Criminology*, 28, 1, 1-26. Republished in J. McCord & J. H. Laub (eds.) *Contemporary Masters of Criminology*, pp. 251-276. New York, NY: Plenum Press.
- McCord, J. (1989). "Theory, Pseudotheory, and Metatheory." In W.S. Laufer & F. Adler (eds.) *Advances in Criminological Theory*, Vol. 1, pp. 127-145. New Brunswick, NJ: Transaction.
- McCord, J. (1982). "A Longitudinal View of the Relationship Between Paternal Absence and Crime." In J. Gunn & D.P. Farrington (eds.) *Abnormal Offenders, Delinquency and the Criminal Justice System*, pp. 113-128. Chichester, England: Wiley.



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