

Minnesota Paper

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In the fall of 1972 I entered the doctoral program in family sociology at the University of Minnesota because Reuben Hill, Ira Reiss, Murray Straus, and their colleagues had built such a great program in family scholarship. The Minnesota Family Study Center and the NIMH traineeship provided a rich learning environment, and I soon discovered that there were also other strong complementary programs in Family Social Science and the Institute of Child Development.

In my first year I had the good fortune to work closely with Reuben Hill and others on the Family Problem Solving project. I developed friendships with David Klein, who was Reuben's senior graduate assistant, and Steve Jorgensen who was in my graduate student cohort. Joan Aldous was my first year advisor, and after she moved from Minnesota Ira Reiss became my major professor. I benefited greatly from the personal qualities and professional abilities of many professors and graduate student colleagues at Minnesota.

I was a Utah guy in his early 20's who figuratively had fallen off of the potato truck into the Twin Cities. Fortunately for me, I was mentored through my doctoral program by several scholars who were (or were becoming) world famous. Reuben Hill, the most internationally renowned of my mentors, might have been particularly understanding of me because he also grew up in Logan, Utah where he completed a degree in chemistry before focusing his intellect on the social sciences. Reuben continued the critical editing of my writing begun by earlier mentors, but he added several new dimensions: he directed my NIMH fellowship program, invited me to assist him in teaching a course, and he modeled the multiple roles of an eminent family scholar. One thing that especially impressed me about Reuben was that, in spite of his

many commitments, he was incredibly timely with written feedback; almost without exception, he edited and returned my drafts within a week, and usually within a few days, after I had given them to him.

Ira Reiss, my major professor at Minnesota, also was a helpful mentor. He was especially straightforward about how to negotiate the doctoral program in sociology. Together we planned, and I then followed through with meeting the requirements. Ira was a steady and insightful advisor about departmental politics, a clear thinker, an efficient planner, and a coauthor who treated me and my work with respect. He was especially wise in helping me negotiate a major crisis in my graduate program, when one of the faculty on my supervisory committee took flight from reality and accused me of being his pilot.

The Director of Graduate Studies for the Department, who was rather formal in his interactions with students, was displeased that I had not followed the proper order by gaining approval from him for my prelim topic. Having cleared the topic with Ira and Reuben, I wanted to find a way to move ahead with my program; the offended Director of Graduate Studies suggested that he could resign and be replaced on my supervisory committee. With diplomatic intercession from Reuben and Ira he agreed at least to discuss the matter. When he came to the meeting I could see from his facial tics and coloration that he was highly agitated. Before even sitting down, he announced that he wanted to read a prepared statement, which went like this:

“In recent weeks, and especially in the past several days, I have been the victim of obscene phone calls in the middle of the night. Because of the difficulty we have been having with this committee, and its’ temporal proximity with these phone calls, I am resigning from Brent Miller’s committee.” It was quite a shock to have the Director of Graduate Studies indirectly accuse me of harassing him and resign from my doctoral committee. I was not responsible for the phone calls, but eventually we all agreed that it would be best to replace the offended faculty member on my committee, and my doctoral program moved ahead.

David Olson was another outstanding graduate mentor to me at the University of Minnesota. At the beginning of our relationship I was primarily a computer jock for David, submitting boxes and boxes of punched computer cards through the I/O window in the middle of the night to minimize the long turnarounds that started over if I made an error in the programming code. David modeled how to work with a research assistant and team, from conceptualization, through analyses and writing, and into publication. Of all my graduate mentors, Dave most liked to share fun with graduate students. With him (and also with some other graduate faculty), I played handball and racquetball; went skiing, jogging, and fishing; and shared dinners and potluck socials.

Some of my most memorable graduate school experiences reflect how naïve I was about academia. When I began the family sociology graduate program at Minnesota it didn't occur to me that family was only one of several emphases. Somewhat to my chagrin, I had to take foundation courses in social theory, complex organizations, population, deviance, and so on. Soon after my arrival I was enrolled in a foundational history of social thought course, taught by a senior professor who had written the textbook. He still lectured by turning pages of yellowed handwritten notes that he had delivered to thousands and thousands of students before. Entering the Social Science Tower elevator one day I found myself standing alongside this distinguished older professor with mutton chop whiskers. When I called him by his first name his face reddened with obvious displeasure. From the uncomfortable feeling in the elevator I learned that not all professors like to be on a first name basis with new students. That first fall, I also remember barging through Reuben Hill's closed office door one day without knocking, to find him meeting with one of his colleagues. Reuben was among the most patient of people; he just turned in his chair and regarded me quizzically. Although he didn't say anything I could tell that he wondered how long it would be before I learned professional manners. Now when a graduate student enters my office with little or no regard for what I might be doing, it helps to remember that I was once similarly naïve.

In the sociology doctoral program our training in research methods was first rate. For many years I used SSRC punch card handouts to help my research methods students understand data entry and formatting. As an RA responsible for data analysis, many nights wheeled carts filled with boxes of punched computer cards in to the mainframe computer center (there were no PCs!) between midnight and 4 a.m. so that I could do analyses without being interrupted during high demand times.

Social theory was also an important part of our training at Minnesota. A graduate seminar taught by Paul Reynolds was especially (in) famous; all students received incomplete grades because the papers he expected us to write could not be finished in one quarter. My paper for the course (which ran 100+ pages) was a reformulation of Ira Reiss' theory of premarital sexual permissiveness, which I presented at the NCFR Theory Construction pre-conference, and which later became the basis for a handbook chapter that Ira and I coauthored.

In the spring of 1975, anticipating graduation that summer, I interviewed for several jobs and decided to take a faculty position in the Department of Child and Family Studies at the University of Tennessee in Knoxville (UTK). It was an enjoyable position, teaching introductory marriage and family courses and graduate research methods. In Knoxville I conducted a short-term longitudinal study of the transition to parenthood, based on a literature review I had completed for my written prelims at Minnesota. We loved many things about Tennessee, but when a position came open at USU in the fall of 1979 we moved back to Logan – because having little children -- we wanted to be closer to our families, and I had a longing for the Rocky Mountains.

At USU I decided to develop a research program to address an applied, social problems type of marriage and family issue. Soon after my arrival at USU, the Utah State Office of Education issued a request for proposals to study what differentiated high school aged teens who began having sexual intercourse from those who remained sexually abstinent. I submitted a proposal that was funded in 1981, and that small sponsored project began two decades of research

from which I have published many papers about adolescent sexual behavior and pregnancy. My studies about adolescent sexual behavior led to many opportunities to become acquainted with researchers and federal agency officials, and to participate in national grant review panels usually focused on adolescent pregnancy issues.

Research on teen sexual behavior and pregnancy placed me in contact with people from whom I wanted to learn more, so I planned a sabbatic leave that took our family from coast to coast during the 1987-88 academic year. It began with me spending the summer of 1987 at the University of Michigan, taking quantitative methods courses. Next, I was a visiting scientist at Battelle Human Affairs Research Center, where I learned how full time social science researchers craft winning contracts and grants.

The final leg of our family sabbatical journey was across the country to Washington D.C. Wendy Baldwin was then Chief of the Demographic and Behavioral Sciences Branch at the National Institutes of Child Health and Human Development. I sought an opportunity to work at DBSB because they had funded much of the early research about adolescent pregnancy and childbearing, and Wendy was an important leader in that area. We worked out a role for me to help around DBSB, and in the process, I became acquainted with how the Branch, the Institute, and the NIH fosters research.

Returning to USU I resumed my research about adolescent sex and pregnancy, writing state of the field articles about adolescent sex, pregnancy, and childbearing. In 1991 I agreed to be Acting Head of the Department at USU while Jay Schvaneveldt was a Fulbright scholar in Thailand. When he returned I was glad to give the administrative headaches back to him. My memories of administrivia gradually faded, however, and by 1995, when Jay took another exotic leave to the Arab Gulf States, I became head of FHD on a permanent basis. Over the years I have reflected that Reuben Hill, Ira Reiss, and David Olson were never department heads, and believe they might have avoided it deliberately. Being a university administrator is a difficult balancing

act, trying to maintain a scholarly research program while responding to the daily and sometimes competing needs of students, faculty and staff, and external constituencies.

As our children entered their teens there was a different texture than we had expected, and it seemed to stem from their trying to come to grips with their adoptive identities. Gradually we began to realize that some adopted children have issues that neither they nor their adoptive families understand very well (Brodzinsky, and Schechter, 1990). This realization started me on a new line of research to better understand adopted children's feelings and behavior and to test possible mediating mechanisms that would explain why some adopted children do well, while others have more problems.

I have maintained especially close professional ties with the National Council on Family Relations over the years, starting with the Preconference and Theory workshop, the Research and Theory Section, and then becoming Vice President for Publications, Program Vice President, and NCFR President in 1991. I also have participated actively in the American Sociological Association, the Population Association of America, Society for Research on Adolescence, and the National Campaign to Prevent Teen Pregnancy.

Early in my professional career I studied marriage quality and the transition to parenthood. One of the shifts in my professional focus has been away from studying such normal developmental patterns in marriage and family life to an emphasis on social problems policy issues (teen pregnancy and adoption). By far I have written the most about teen sexual behavior and pregnancy, and this is one area where my work probably will have a lasting impact. I also have had some influence on students and colleagues in my teaching and writing about family research methods.

The adoption research I recently began also has the potential to influence the field, partly because our family's experiences have given me personal insights that complement my research training. It has been interesting (and refreshingly honest) to observe how the social scientific approach has shifted from denying the values and biases of investigators to recognizing that

values are inherent in all of our work. I don't think it is wise to overreact, however, by discarding a century of research methods. In current research I consciously combine a constructionist perspective with positivist methods of conducting research. This seems to be an appropriate reconciliation for me and perhaps for other researchers who seek to understand human behavior and family relationships.