

ited personal experience should therefore be considered within this very much broader context.

Analysis

My first attempt to think about my beginnings of work in social epidemiology was described in the Foreword to the Berkman/Kawachi textbook on Social Epidemiology [2]. As I indicated in that Foreword, my thinking about social determinants began in 1955 when I was accepted into a training program in the Department of Sociology at Yale University. That training program, called "medical sociology" was funded by the Commonwealth Fund and it was the first such formal training program in the world. There were four of us in the program. We were given a choice early on as to whether we would focus on what was then called the sociology of medicine or sociology *in* medicine. As I noted in the textbook, the logical choice was for me to choose to study the sociology of medicine because there already existed a relatively large and interesting literature on this topic dealing with the institution of medicine and medical care, the sick role, and attitudes and beliefs of patients regarding illness, pain, and medical treatment.

For reasons that are not clear to me, I decided to study sociology *in* medicine, which I took to mean the study of how social factors affect health and well-being. I now realize that what my Professors at Yale really meant by this term was nowhere near as grandiose as my version. Professors August Hollingshead and Frederick Redlich were at that time doing a large study of the link between social class and mental illness and that is what they meant by the term "sociology *in* medicine": They wanted me to help them with their research. I had no interest in the topic of mental illness because I took it for granted that social factors would somehow be related to mental illness. Looking back, I can see what a naïve view this was, but that was my uninformed position at that time. Instead, I wanted to know if social factors were related to diseases that were *not* so obviously connected to the social world, diseases such as heart disease, cancer and arthritis. Not only was this a naïve view, but it was also a reckless decision because there was virtually no literature on these topics at the time and no one was sure there ever would be.

When I graduated, I was scheduled to go into the Army to fight in Korea but Professor Hollingshead said that I might want to consider an alternative that would give me a military deferment: go to work for the U.S. Public Health Service in Washington. I agreed that that was a better idea. He said that he recently had talked to a statistician in the Heart Disease Control Program in Washington who wanted to hire a sociologist. So I went to Washington and met Phillip Enterline. I asked him why he wanted to hire a sociologist to study heart disease and he said that he had no idea. He and his group had just completed a study of

the geographic distribution of coronary heart disease mortality in the U.S. and they found very high rates on the East and West Coasts and in the Detroit-Chicago metropolitan area but low rates elsewhere. They had not been able to explain this finding and they thought that perhaps a sociologist might be able to help.

So I took the job. I was to be classified as a Statistician in the Civil Service because there was no category available for a Sociologist. I made the mistake of reporting this to Professor Hollingshead and he was not very happy. "If there's no category for a Sociologist, make one!" He raised such a fuss that they in fact did. So I was the first Sociologist labeled as such in the Civil Service.

Then I went to work and it was a disaster. I decided to begin my work by looking at data from a state with a very low death rate from CHD with the idea of then doing a similar study in a higher rate state. We obtained some wonderful data from North Dakota, a low rate State. In a six-county area of North Dakota, we were able to obtain information on every case of coronary heart disease that occurred in men, 35–64 years of age, in a one year period. Then we selected two age-matched men, free of CHD, from a representative sample of the 6 county area from which the cases came. I then set about testing all the hypotheses that I had learned in graduate school. In those days, we were thinking about marginality, status crystallization and many other concepts that no one can now remember. It must be recognized, of course, that there was no literature or previous research to rely on. This was, I think, the first such study of CHD ever done with social factors. So I based my work on the concepts that I had been studying in school. I spent a year doing this. Not one of the hypotheses worked out. The cases and controls did not differ from one another on any of the dozens and dozens of ideas that were then popular in Sociology. I think Enterline must have thought he made a major mistake in hiring me.

So I decided on a different tack. I would go through all of the data and see on which items there might be a difference between cases and controls. I had been taught that this type of fishing expedition was not a very good way to proceed, but I was desperate. In this analysis, I was able to see a considerably higher rate of CHD among men who had changed jobs and who had moved geographically and, especially, among men who had moved from farms to white collar jobs in the city [3]. I observed all of this, of course, after controlling for smoking, blood pressure, and many other CHD risk factors. I called this phenomenon "cultural mobility" [4]. I was then able to repeat this analysis with a remarkably similar data set in a State with a much higher rate of CHD, California [4]. And I found precisely the same thing as in North Dakota.