

on how to handle this very unusual application. I suggested that we develop a special study section with specially chosen people to deal with this crisis and I was given permission to proceed. To create this special review committee, I invited a few people from my Study Section to serve, as well as some carefully chosen outsiders. I attempted to pick people who I thought could understand the radical idea that Dr. Breslow was proposing.

We went out to California for a two-day meeting. In the end, my specially picked people recommended that the proposal not be funded. It was too weird. For example, Breslow proposed to study the health of people but he was not going to do one physical exam or take any blood or urine. He was simply going to ask people to rate their own health! I recall he proposed a question that asked "Compared to other people your age, how would you rate your health? Excellent, good, fair, or poor?" This question has turned out to be one of the most powerful predictors of future health in dozens and dozens of studies but at that time it was a very bizarre question indeed. That the Breslow proposal was turned down was very disappointing but I urged Dr. Breslow to resubmit and a year later I assembled yet another group of specially picked reviewers to give it another try. And this time it worked. So Dr. Breslow was able to establish this crucial population study that has turned out to be one of the most significant studies in the history of social determinants [7]. A few years later, Dr. Tommy Francis of the School of Public Health in Ann Arbor was able to establish a similar population laboratory in Tecumseh, Michigan.

After three years of serving as Executive Secretary, I returned to do research in the Heart Disease Control Program, this time based in San Francisco, California. By then, it was clear that a field of research in social epidemiology was emerging, most of it focused on coronary heart disease. This research was not of very high quality, nor were the results compelling, but some interesting questions were beginning to emerge. I had a conversation with Professor Leo Reeder about this and we decided it might be good to bring together all the people who were engaged in this research to see what we were all doing and to think about next steps. Since I was a government employee, I prevailed upon my bosses to provide funds for the meeting. The Conference was held in Phoenix, Arizona in February 1966. We invited all of the social scientists and medical people in the country doing research on heart disease as well as some others who, while they were not doing such research, were nevertheless bright and potentially helpful. We scoured the country and came up with 27 people, including Reeder and myself.

The report of our conference was later published in 1967 as a special volume of the Milbank Memorial Quarterly

with the title "Social Stress and Cardiovascular Disease" [8]. It is a little embarrassing to read the book now and see the state of the art at that time but it was quite clear that, in spite of this, something important was happening. It was in one of the last papers of this little book, by the way, that I explored the issue of appropriate outcomes for social epidemiologic research. I argued in that piece, no doubt influenced by Lester Breslow's idea for the Alameda County Study, that we needed to look at a broader set of disease outcomes than the usual clinical entities. This idea stands as one of the key features in John Cassel's classic paper as well.

In 1968, I became a Professor of Epidemiology in the School of Public Health at Berkeley. I was, I think, the first sociologist to hold a position as an epidemiologist at any School of Public Health in the world. Leo Reeder was a sociologist at the UCLA School of Public Health but his was a normal position as a Professor of Behavioral Science. By then I was working with Reuell Stallones, who was also a Professor at Berkeley, to study coronary heart disease and stroke among Japanese migrants to Hawaii and California. Stallones was primarily interested in testing the dietary hypothesis. Did the Japanese in Japan have low rates of CHD due to their low fat diet? I was interested in testing the mobility hypothesis. Did rates of CHD go up among the migrants? We were both surprised by the findings.

It turned out that Japanese men who migrated to California had CHD rates five times higher than those in Japan, with migrants to Hawaii having intermediate rates. And this increase in CHD rate was not explained by any of the usual CHD risk factors such as diet, serum cholesterol, smoking or blood pressure. I assigned a doctoral student to figure out what was going on. Michael Marmot did his doctoral dissertation on this issue. He concluded that those Japanese men who had adopted Western cultural ways were the ones with the enormous increase in CHD while those California Japanese who had retained traditional ways had rates comparable to those still living in Japan [9]. Again, this observation was independent of diet and all the usual CHD risk factors. This clearly was not supportive of the mobility hypothesis since some migrants had no health consequence at all. Then Marmot left Berkeley to go to London to begin his work on the British civil servants and he left me with the question: what does it mean to say "Western ways" versus "Traditional ways"?

I went to Japan several times, interviewed dozen and dozens of people, and I read many books to get some understanding of this but all I could get out of this work was that my Japanese informants thought that Americans were lonely. I challenged this observation many times but doz-