David Mechanic

July 6, 2004

TAPE 1, SIDE A

Ted Brown: I'm here in David Mechanic's office at Rutgers. It's July 6, 2004, and we're beginning our interview as part of the history of health services research project.

Professor Mechanic, I wonder if we could just start with some reflections on your early family life, influences that directed you toward the study of sociology, your study of sociology at CCNY, winning the Ward medal, and other reflections that come to mind. I'll lead you systematically through the basic chronology of your career and then branch out as the conversation develops.

David Mechanic: I was a poor kid in New York City. I grew up in the Bronx. My parents were immigrants, had very little schooling, neither of them. They just had a few grades but were very much committed to education, especially my mother. My parents were moderately orthodox Jews, and we lived in an area which was very politically active, a place called the Coops. I don't know if you're familiar with it.

In any case, I grew up in the Bronx and was a pretty good student. All of my siblings were. And when it came time to go to college, we really didn't think of going anywhere

other than the city colleges. At the time, I went to CCNY. It was quite a strong school with very high standards. I didn't know that at the time. I went through college as a pretty naïve kid. I was also working while in high school and college. It was only when I got to Stanford, in graduate school, that I realized what a good education I received at CCNY, compared with many of my fellow students, who had been to what might be seen as more privileged universities.

At CCNY I got interested in sociology. I didn't know much about it when I first encountered it. In those days, they didn't teach sociology in the high schools. I didn't know what sociology was.

When I got into sociology, I found that it provided an analytic understanding of the sort of life I lived in an immigrant community and many of the social dynamics underlying that life--An understanding of the very unique neighborhood which combined immigrant Jews who were religious often in opposition to socialist and communist political groups. Sociology provided an excellent understanding of the underlying conflicts and the political activism that went on there; the stratification in the neighborhood between the Jewish community and the Italian community and some of the fights that took place across those communities. In a sense, sociology opened up that world to me at the time.

TB: Was there a particular professor at City College who was very influential?

DM: Actually, when I first took sociology at CCNY, the professor was awful. If professors would get you into a field, he would be the kind of person that would chase you out of the field. But, it was inherently interesting to me, and I started reading a lot on my own.

I later ended up with a number of professors who were very supportive. I did a senior honors thesis with an anthropologist by the name of Laura Thompson, who had been quite active as a research anthropologist, had done work on the Hopi and Navajo. She had done work in Fiji and many other places, and she kind of took me under her wing. She convinced me as a young, insecure kid that I had a lot to say and I should have high aspirations and so on.

I had a number of other very good professors in sociology, as well as other areas. I got a lot of support from a fellow by the name of--I don't remember his first name now--but a fellow by the name of Brotman, who was more interested in social action in the Harlem community. He got us involved in doing fieldwork in Harlem and going out and talking to people and that sort of thing.

Robert Bierstedt, who later became the chairman of the department, was very supportive, and he was much more of a mainstream sociologist, the son-in-law of Robert MacIver, who had been in his time a prominent sociologist. I was a good student, so I got recognition and support and a lot of encouragement.

Actually, I still was pretty naïve when I applied to graduate school. I still didn't have very sophisticated notions of how you select schools or what is a good program.

TB: How did you select Stanford, then?

DM: I applied to a number of schools. I was interested in getting to the other side of the country, to California. My brother already was a Ph.D. student at Berkeley, so I wasn't too anxious to be there. So, I applied to Stanford, Wisconsin, and many other places, and they all offered me some kind of financial arrangements. Stanford offered me a Ford Foundation fellowship.

My professors encouraged me to go to Stanford. I was very uncertain about Stanford or Wisconsin or some of the other places that I was thinking about. It was through their encouragement, and a desire to get out to California, that I went to Stanford. It was not the logical choice. Although it turned out, through a number of circumstances, to be a wonderful choice and served me rather well.

When I went there, I was interested in doing social psychology. At the time, a famous social psychologist at Stanford was Leon Festinger, who had just written his book on his theory of cognitive dissonance and was having a very big impact on social psych. There were a number of social psychologists in the sociology department as well.

My closest mentor and a close friend was a professor by the name of Edmund Volkart, who was just beginning to become involved with the emerging field of medical sociology, which at the time wasn't much of a field yet. He had worked with the Russell Sage Foundation, and was involved in the early efforts to develop medical sociology.

I was working with him in sociology on bereavement at the time--that was his interest. And I was working on experimental social psychology with Leon Festinger, and was thinking of myself then as an experimental social psychologist, someone who was going to do experimental social psychology.

TB: Was that a field that was between sociology and psychology? Was it understood to be a part of sociology?

DM: In the old days, the field was sort of split in both psychology and in sociology, and there was a lot of overlap. Some universities had joint departments. The University of Michigan had a very successful joint department for many years that broke up later. Harvard had a Department of Social Relations, which brought together these disciplines. But, it didn't really survive at any university. It led into psychological social psychology and sociological social psychology. Stanford was strong in both.

One of the Stanford professors in sociology, Richard T. LaPierre, had written a major book on social psychology. There had been a tradition of sociological social psychology

at Stanford, and there were also very good people in the psychology department which was a stronger department. It was then one of the very best in the country, and attracted really superb graduate students. I found myself at the time gravitating toward the best students in the group in experimental social psychology.

In 1956, John Thibaut and Harold Kelley came to the Center for Advanced Study in the Behavioral Sciences at Stanford. Hal may have been at Minnesota at the time, but he later ended up at UCLA. John Thibaut was at the University of North Carolina-Chapel Hill. They came to work on a book on a theory of the social psychology of groups. They were looking for someone to work with them. Leon Festinger recommended me so I ended up spending a lot of time at the Center. The Center was attracting the very major figures in all the social sciences. It had opened, I think, in '54 or '55, and many of the most noted social scientists of the day were passing through. I was helping Kelley and Thibaut on their book, (*The Social Psychology of Groups) and* would attend seminars and get to know many of the people at the Center.

It was an amazing experience for me because it gave me a lot of confidence in myself in interacting with these people who presumably were the "greats." I found I could hold my own pretty well as a graduate student among them, and I could argue convincingly with them over issues. That experience both gave me much more confidence in myself and a somewhat different view of myself, and it also built a network of people who I got to know and who were later important.

After I had completed the year with Thibaut and Kelley I went for a brief time to work with John Bowlby a psychiatrist or psychologist, maybe both, from England, who was very psychoanalytic. He's famous for his work on early separation reactions of infants in child development. And I had very strong opinions. I was a sassy young man. He was having me read and review the work of Melanie Klein, and I didn't think very much of her work. When I voiced my opinions about what I thought, he said, well, he had hired me to review her work and not for my opinions. I said, "Well, if that's the way you feel, I don't think I want to do this any longer," and I went to work for David Hamburg, who was a young psychiatrist from NIH who was spending a year at the Center.

David was a very supportive and very interesting person. He at the time was interested in coping with stress and adolescent development, which were areas I had gotten interested in as a graduate student. So, I started working on coping and adaptation when I was working with David. I later went on to do a fair amount of work in that area.

TB: Your first book was *Students Under Stress*. Was that your dissertation?

DM: No. My dissertation was a study of illness behavior. I forget what it was called. It had stress in the title, but it was a study which tried to look at an issue that I became fascinated with. I had read the work of Harold Wolff and Larry Hinkle. Wolff was an eminent neurologist at Cornell Medical School; Hinkle was in internal medicine. Wolff

had done lots of interesting work on stress and disease and headache and other areas. He was a very strong force in those areas in those days.

They had done what I found fascinating work on--I think they were telephone operators--large datasets from one of the big telephone companies, and they had used these data to make the case that stress was associated with illness in all bodily systems.

While I found that an interesting hypothesis, when I actually read their work, I thought there was a major flaw. They had been using the long-term records of these employees in terms of their medical utilization and medical diagnoses and so on. They made the assumption that all people use medical facilities and define illness in comparable ways when they faced illness, and these records were a true approximation of the incidence and prevalence of disease in these populations. I thought that was really a major impediment to taking their hypothesis or their conclusions seriously.

So, I set up a study in which I tried to look at the relationship between stress, the occurrence of illness and use of health services, trying to control for something I called propensity to seek medical care, which I developed a measure of. I was able to show that what I later called illness behavior was more influential in patterns of use of care than was the occurrence of illness itself. Then in this dissertation I looked at the relationship between stress and illness behavior in explaining the way in which people appeared in medical records and the outcomes of medical care.

At the time, I published several papers, one in the *American Sociological Review*, one in the *New England Journal of Medicine* on the sampling implications of using medical records. That's when I got into medical sociology initially.

But, my basic concern was to examine how the propensity to use medical care or illness behavior intervened between the occurrence of symptoms and actual use of medical care. I found that the illness behavior variable was even more important than the stress variable in accounting for these differences among people. Actually, it was a study of all of the freshman class at Stanford, which we followed over time. We got their medical records to see how their reports related to their subsequent medical records.

I developed this notion that we should be studying illness behavior and the ways in which people varied in their perceptions, identification of symptoms, and what to do about them. In the early '60s I wrote a concept-of-illness behavior paper in what then was the *Journal of Chronic Disease*, as well as several others, around the issue of cultural, social and ethnic variations in the way people defined illness and responded to it. These had to be taken into account as one looked at some of these other factors like stress and other hypothesized etiological factors in the occurrence of disease.

Illness behavior then became an area of research. Lots of people worked on it, and continue to work on it, in much more sophisticated ways than we had at that time.

TB: Can you say more about the emergence of medical sociology at this time? You've alluded to that.

DM: Yes. Medical sociology had its roots in much earlier periods. Henry Sigerist was interested in the sociology of medicine and wrote a good deal about it, as did other people. In the '50s, there began to emerge a variety of social scientists, and particularly sociologists, who were interested in how social factors related to disease and health behavior and so on.

This group began to get together with support from the Russell Sage Foundation, and they created a committee within the American Sociological Association which eventually became the medical sociology section. It has more than a thousand sociologists who now belong.

In the 1960s, the National Institute of Mental Health decided that it was going to take a public-health approach to health and illness, and mental health in particular, under Bob Felix's leadership. NIMH provided a lot of support for the development of the behavioral sciences and health. They funded training programs and provided much of the early research support.

In my final year at Stanford, I had an NIMH fellowship. They were supporting general social sciences on the belief that the social sciences were the basic sciences of mental health. In the '60s, they much increased the funding for these programs. So, my last year at Stanford was under an NIMH fellowship of that sort. Then, I went and did a NIMH postdoc at the University of North Carolina-Chapel Hill.

So, that was the beginning of the buildup of the social sciences through the National Institute of Mental Health as the major funder. That's why so much of early medical sociology was directed heavily towards mental health issues as compared with other general medical issues. It was only much later that the other NIH institutes got involved in these kinds of studies to any large extent. The NIMH was really the leader. My participation was funded through that mechanism, and medical sociology as a discipline developed primarily with NIMH support.

Do you know Sam Bloom's book?

TB: Yes, I do.

DM: Sam does a beautiful job of writing up the early history of medical sociology in his book, The Word as Scalpel. It is much more than an organizational narrative. It is an intellectual history that examines the development of medical sociology in the broader context of work in social medicine and public health.

TB: Could you say a little bit more about that year in North Carolina? Was John Cassell there at the time?

DM: Yes, John was there. I wasn't close to him, but I did go to seminars where he participated and was very much impressed by his point of view and the way he saw epidemiological issues relative to health.

I had gone to do a postdoc with someone who had a good reputation there, by the name of Harvey Smith, but that didn't really work out. He was anti-quantitative, and very threatened by quantitative work, and very controlling. So, I basically didn't work with him.

I went off and worked on my own and I did several studies while I was there. I did this book called *Students Under Stress*, which was a continuation of some of the work I had started with Dave Hamburg on building a theory of stress and adaptation.

If you go back, that was the period when psychodynamic theory and psychoanalysis was very powerful in psychiatry. It dominated American psychiatry. It also defined the way most people viewed stress and coping involving a very psychodynamic perspective. The purpose in doing this study was to look at how people dealt with stress from a social psychological perspective.

There was a very large number of graduate students going through the Ph.D. program in the psychology department at the time. They had a very arduous set of examinations to take and an extraordinary set of hurdles that had been set up. The students were under a great deal of stress as they went through the preliminary examination period.

What I did is pick up these students about three or four months prior to the examinations, and followed them weekly throughout that prior period and then through the examinations, and following the examinations. I interviewed them intensively repeatedly. I had them fill out various kinds of instruments I developed. I interviewed their families. I got data from all of the professors in the department. I looked at the ecology of their offices and who they talked to as it affected their perspectives and coping responses.

I ended up writing this book called *Students Under Stress: A Theory of Social Adaptation*, which was a critique of the Freudian notions of coping and presented an alternative view of stress, coping and adaptation. That perspective--and I wouldn't say I was the only one who contributed to it in this early period--but that perspective is now the dominant perspective of how people look at stress and coping.

So, I wrote that book and then went on to write other things on stress and disease and stress and coping. I would say that this was one of my major research areas in the early years.

TB: Let me ask a factual question. I want to come back to the significance of it later.

Did you have any interaction with Kerr White that year? Was he at UNC or was he on leave at that time? I know he was just leaving for Hopkins about that time.

DM: Yes. I had very close relations with Kerr later.

TB: But, not at that time.

DM: But, not at that time. I don't think I knew him when I was at Chapel Hill. It was 1959.

TB: He may have been on leave that year. I don't remember the exact chronology.

DM: There were some people who I later had very wonderful professional relationships with who were at Chapel Hill who I didn't know at the time. One was Kerr White; another was Cecil Sheps.

TB: Was he there at the time?

DM: He was there at the time, although I didn't know it. Later I had relationships with

many of these people. I maintained my relationships with John Thibaut (who was then chair of the Chapel Hill psychology department), who I had worked for at Stanford and he remained a good, supportive friend.

I especially got to know many of the people in psychology because that was a lot of my work's focus. And I knew various people in the medical field and in psychiatry. John Cassell was a major person, although I didn't have a very close relationship with him. But, there were other people. There was a psychoanalyst who I didn't agree with (Harley Shands) and often had lunch with him and found very stimulating and a variety of people in the public health school. But it was only later that I got to know some of the Chapel Hill people who played a major role in health services research like Kerr White, Cecil Sheps, Bernie Greenberg and Frank Williams.

TB: Let me now shift to your years at Wisconsin and what seems to be a very meteoric rise to full professor and chair of the department there, and the writing and publication of the first edition of *Medical Sociology*. What were some of those years like and what you remember most vividly?

DM: Going back to the history, prior to 1960, the NIMH was investing large resources in social science health research, particularly mental health research. Everyone was hungry to take advantage of the opportunity.

After a year at Carolina, I knew I wasn't going to stay there. It wasn't my kind of place and I was ready to move on. And I had a lot of offers. I had started to publish health related research papers at the time. Even though I was still thinking of myself as this social psychologist, I was being identified as a medical sociologist. I was beginning to write and some of those early papers were eventually published in the *American Sociological Review* and what was then called the *Journal of Human Behavior*, and some of the medical journals as well.

So, I had a number of offers and chose Wisconsin because of its long tradition as an outstanding university oriented to the state, on the one hand, and as a place which had traditionally very strong social sciences including sociology. They wanted me to come and build a medical sociology program, and I came and wrote a training grant as soon as I arrived. We got funded pretty quickly for a training program in medical sociology, with a very strong emphasis on social psychology and methodology.

One of my colleagues at the time was a very distinguished sociologist and social psychologist by the name of William Sewell. He was an excellent sociologist and a fantastic person. When I first came he had been away doing some work in India. When he returned he became chair of the sociology department and a major force in the university overall. Ultimately, and in many respects unfortunately, he became chancellor of the university during the Vietnam period, when the university was exploding.

So he and I developed this training program in social psychology and medical sociology. We attracted some fabulous students at Wisconsin for this program and it became one of the major training programs in medical sociology in the country. We provided very strong training in statistics, methodological issues, theory and so on, and I did the health stuff. Over the years, we built a large and successful program, and many of our students have gone on to great distinction.

When the National Center for Health Services Research and Development started developing training programs, we added a second training program so our program got quite large.

DM: In 1965, the NIMH gave me a special fellowship to go to England and work at the Institute of Psychiatry and the Medical Research Council. John Wing (who directed a social psychiatry unit in the MRC) had taken interest in the illness behavior work I had done, came to visit me at Wisconsin, and invited me to come to England and be in his unit. That's when I got the special fellowship to do that. John Wing was the up-and-coming psychiatrist in England at the time. He also was a sociologist. He did superb work on schizophrenia, and had a very big impact on the scientific development of the study of schizophrenia, diagnosis, and so on.

I went to England in '65, and while I was there I did my first study of the National Health Service. I had always been interested in the NHS as an alternative to American health care and wanted to know more about that. I did a large national study of general practice, and while in England I also wrote the first draft of the medical sociology book. When I came back from England, I applied to the National Center for Health Services R+D for funding for comparative work on English and American doctors and they funded me for that study.

TB: That was your first grant from the NCHSRD?

DM: I believe that was my first grant from the National Center for Health Services Research. And at the same time, I had grants from the National Institute of Mental Health and later the National Institute of Aging, and so on.

I was doing comparative work on physicians in the U.K. and the U.S. We built a strong program at Wisconsin, and we attracted some new medical sociology faculty. We were attracting really very outstanding students who have now gone on to be major figures in the field, people like Ron Kessler and Paul Cleary (now Dean of the School of Public Health at Yale) and Bill Eaton and Phil Leaf now at Johns Hopkins. Quite a range of people came into that program and went on to be major figures in mental health and health services research.

At the time, this was still a strong social psychology training program. There were other students who went through the program who didn't become medical sociologists. They did became prominent sociologists, like Alex Portes who was chair of sociology at Princeton and now a member of the National Academy of Sciences. He was one of the people who came through that program, and did beautiful work on how Cuban immigrants were coping and adapting to the challenges of relocation in the States.

I had a strong research program at the time, working in a range of areas. I continued with my work on coping and adaptation, particularly in relationship to the rehabilitation of people with mental illness. When I came back from England in 1966, I got a large grant from the National Institute of Mental Health to work on the coping of patients in the community. That was during the beginning period of deinstitutionalization and community care.

I needed a psychiatrist for this program. I tried to hire a young psychiatrist who had just come to Wisconsin from the University of Kentucky, Dr. Leonard Stein who became involved with us in our training program.

He didn't take the research job but later went on with Mary Ann Test to develop what is now the cutting-edge program for treatment of patients with serious mental illness in the community. It's called the Program in Assertive Community Treatment (PACT) which is now used in many states as state policy, and in many other countries, to deal with the community treatment of people with serious mental illness using this aggressive case-management program.

So, we had a very productive program at Wisconsin for many years. It kept on growing both in terms of research funding and also in terms of its training programs. We started with a lot of pre-doctoral training in the early years. In later years, we moved into postdoctoral training. At that point, the Robert Wood Johnson Foundation gave me money for postdoctoral training of people in health care.

At the time I finally left Wisconsin, we had funding from the National Center, NIMH, and the Robert Wood Johnson Foundation. We received a lot of recognition and trained excellent people and did some nice research.

Brown: Did the distinctions exist at that time, let's say late '60s, early '70s, between medical sociology, health policy, health services research, as distinctive fields or areas of concentration, or were the boundaries rather fluid between these? I'm looking ahead to when health services research as a field is trying to find its way with changes that you've written about in various places.

DM: There was no field of health services research at the time, and in many ways medical sociology was the first discipline on the block. It had its origins going way back, but organizationally it began to develop in the '50s, and then grew tremendously in the '60s within the American Sociological Association. In the '60s, the Section on Medical Sociology became the largest section in the Association, and the best funded section.

When I was chair of the Section, we went to Carnegie and they gave us a substantial grant for doing various kinds of projects related to health services.

Also, the National Center gave the section a large grant to look at models of medical education from a social science point of view. A variety of people--Sam Bloom and Ray Elling and others--worked on that program.

The section had a lot of research activities going as a section and produced some special issues of journals. For example, Sol Levine and I did a special issue of *Medical Care* on promoting health or something along those lines. Later, Linda Aiken and I edited a

section project on The Applications of Social Science to Clinical Medicine and Health Policy, which the Robert Wood Johnson Foundation funded. So, it was a very active section.

It was only later that other disciplines came into the area. Sociology was the dominant social science discipline, but then as health care became a much bigger issue, health economics gained greater traction and ultimately became, I think, the dominant discipline in health services research. Only more recently has political science become a player to a significant degree, largely due to the RWJF Health Policy Scholars Program and some of its other programs.

Medical sociology was dominant for a number of years. Then as economics became a core concern of federal agencies, much of the dominance then moved to health economics and the people like Victor Fuchs who had a major influence on the development of health economics.

TB: The RAND study...

DM: And, of course, the very big study, the health insurance experiment by Joe Newhouse and the Rand people, that was a remarkable effort. I happened to be on the advisory committee to the secretary on the Rand study and watched that study emerge from its inception. It had a very interesting history. Many of the people who sat on that

original advisory committee were very skeptical that the study could ever be pulled off. It was so ambitious within the context of the kind of research that had gone before. Nothing that big or complicated had ever been attempted.

There was also a lot of concern in the health services research community that it was going to take away all the money that was available for health services research, given the size and financial dimensions of the study. It was only later that the White House intervened to fund that study. At the time they were resisting the idea of national health insurance, and they argued that the study was needed because we didn't know enough.

The irony was that the health insurance experiment was not at all about national health insurance. It couldn't answer questions about national health insurance because of the way it was structured. It was a study of coinsurance. But, it had a massive influence. I became a great admirer of Joe Newhouse because of the way he was able to carry out that study and deal with the tremendous barriers to its implementation. He and his group did a remarkable job. That was, I think, a major watershed in the field of health services research.

TB: As I try to reconstruct the history of health services research as an emerging discipline of scholarship, I see some of the changes that you make clear were crystallizing in the '80s with the rising dominance of economists or economizers, as Dan Fox told me, already taking place to some extent in the '70s. Yet, they were creating some of the

questions about the future of the field, the downturn in funding of those. There was the leveling off and then the decreasing of grants, training grants, health services research. There seemed to be various groups and panels that were looking at the field and what its prospects may be.

You served, for example, on the President's Advisory Committee for Health Services Research in 1971-72. You not only ran your own center at Wisconsin, but you were on the Advisory Committee to the Hopkins center. About that same period, you were a member of the Technical Review Group of the Health Services Research Center at UCLA. You were very active in the field of medicine, on various committees and boards that looked at the health services research, all in the '70s and early '80s. I wonder if you remember any of the sense, or temper, or set of concerns at that time about health services research, about a possible displacement of sociologists and other social scientists by economists and other emerging priorities.

DM: The sociologists were not a significant part of this larger community and didn't think of themselves as in competition with this larger community. There were two kinds of medical sociology. There was a sociology of medicine and sociology in medicine, a distinction . . .

TB: You've written about that.

DM: . . . A distinction that Bob Strauss made originally. You ask, who were the early leaders in medical sociology? This was a period when I was sort of becoming a prominent medical sociologist. You had Elliot Freidson, who had a great deal of influence but Elliot never saw himself as an applied sociologist in any sense. His view was that medicine was a context that he could use to study the sociology of the professions. And he holds that view to this very day [Note: Eliot Freidson passed away in 2005]. He never saw himself as a person intending to change the health system. It was more to contribute to sociological theory.

Then we had Rene Fox, who was a leader in cultural sociology. That was very important in terms of studying how medical work got done and how medical uncertainty was managed. She was one of the early people to go into major issues of social ethics in medicine and she had considerable influence as a scholar.

These kinds of efforts were quite divorced from the economics of the health system and the types of work economists were doing.

When Kerr White was the chair of this President's Advisory Committee, and when I was involved in that, the concerns were, how do you build a field that has the kind of funding that could really effectively study and change the health-care system?

TAPE 1, SIDE B

DM: Recommendations have been made over and over again, and are still being made today, that some reasonable proportion of what we invest in health should go into health services research. That's always a much larger amount of money than anyone in the Congress is willing to give to health services research. The President's panel tried to define this large agenda. There was a lot of concern that the early National Center was not being effective, that it didn't have the influence necessary with the Congress, with the administration, and so on.

In an earlier period, Kerr White chaired the Study Section at the National Center for Health Services Research and Development. I notice in his interview for this series, he said I was on that group, but I actually wasn't. I think he confused that with the President's Advisory Committee, which he also chaired.

I think there was frustration with the failure to get much support for the efforts of the National Center and difficulties in turnover in the agency and issues about how should the agency best be organized to really affect the health care system.

Kerr White had very strong views about what health services research was. His perspective was very different from the kind of perspectives you get from medical

sociology, which were driven by social theory.

Kerr White was a physician interested originally in psychosomatic type issues. He was very interested in using epidemiology to change the health care system and how health care was carried out.

We had a very interesting group, but I'm not sure that that president's panel really had much influence. Most people don't know that such a panel ever existed.

It was during the period of the Nixon administration, and Nixon, although he was not seen as a liberal, did push toward some kind of national health insurance in the early '70s. He looks pretty liberal in a "Bush context."

It was interesting, because following the panel, a few of us were asked to work on Nixon's State of the Union from the health point of view, and I was one of the people who was asked to do that. I spent a lot of time in Washington. I regard this as the most unproductive of all of the endeavors I've ever been involved in.

We had an amazing group of people who worked for many months on the idea that we learned a lot in the space program and we could use a lot of the components of space technology to improve the health care system. The notion was to develop ideas, which then would be in Nixon's State of the Union address, and this group developed lots of

ideas. Ultimately, the only idea that was in the State of the Union, as I recall, was one that dealt with using technology for emergency medical systems, the 9-1-1 system.

I later heard, whether it's true or not, that the reason that got into the State of the Union was not our group. Rather, it was because there was an op-ed in the *Wall Street Journal* about a week before arguing that this would be a great thing to do to improve the health care system. I don't know if that's correct, but that's the story I heard.

TB: Are there any other remarks you'd like to make about your Wisconsin years? I would like to shift forward to your move to Rutgers and how your career may have changed. How the fields you were working in were changing at that point.

DM: At Wisconsin I started getting heavily involved in the mental health area. I continue with that. I've always had an enduring interest in mental health services, the organization of mental health care, ways of improving care for the most seriously mentally ill. I wrote a book in the late '60s called *Mental Health and Social Policy*, which has now gone through five editions. Actually, of all my books, that has been the very best seller. It's had a significant influence on social work in particular. But, I think more generally it's done extremely well. It, too, was directed against the current tide, making the case for the need to give priority to the needs of persons with the most serious mental illnesses.

Through the later years at Wisconsin, I developed a large study in Marshfield, Wisconsin, in connection with the Marshfield Clinic, which is a very large multi-specialty clinic sitting in a pretty isolated rural area. You could get a lot of control over the population and figure out what's going on. So, it was an unusual epidemiological opportunity. We had lots of different facets which we studied. We did a population study. We took patients from the Marshfield Clinic and then compared what we got through sampling of the clinic versus what we got sampling the larger population. We had a group who did wonderful work on the use of pharmaceuticals led by Bonnie Svarstad.

I had an excellent crew of people who were working with me, including Paul Cleary the program manager who later went on to do wonderful work in health services research. The Marshfield Study went on for a number of years. Jim Greenley was a co-investigator on that program and was a wonderful colleague. He sadly died early in his career. A number of investigators worked on that study, then went off and had their own careers. That was the last major study I did before I left Wisconsin. Actually, when we came to Rutgers in '79, Paul Cleary, Jim Greenley and I continued to work on the dataset that we had developed during those years.

TB: Could you say something about what attracted you to Rutgers?

DM: Yes. I came to Rutgers for personal reasons. I left Wisconsin after we had built a rather nice enterprise. It was very productive and we were getting marvelous students. I had wonderful colleagues. And so the shift really came because of personal reasons. When I got here, I was very frustrated. It was a real culture shock for me at the time concerning the difficulties of doing big research projects in an institution like Rutgers as compared to what had been the case at Wisconsin with its tremendous social science research infrastructure, survey research lab, you know.

TB: Has that changed over time?

DM: Yes. I see myself as a major force in changing that.

Once again, it's very far from health services research. Rutgers had a very bizarre institutional history, in that it started as a colonial private institution. It began as Queens College in 1766 and evolved only very late in its history into a state institution.

It evolved by putting together institutions that already existed and then adding them to a Rutgers confederation of sorts. So, Rutgers had this very funny structure. When I got here, there were five colleges in New Brunswick, each with its own dean, its own faculty. Everything was splintered into these tiny groups. We had several departments of history, several departments of sociology, and there wasn't any economy of scale sufficient to really doing anything ambitious because . . .

TB: And there weren't large grants as you had been . . .

DM: No, there hadn't been large grants and so on.

So, I was very frustrated. When I first came, I started to write grants, but I saw lots of barriers and I was thinking of moving on. The University of Pennsylvania was very anxious for me to come there, and so on. And I thought we had the most bizarre sort of administrative structure imaginable.

TB: Where you became a dean.

DM: Yes. Lots of other people sought changes for many years. We had to restructure the university. Life includes many accidents, and this is just one more accident, serendipity.

They asked me would I come in to plan the restructuring of arts and sciences. So, I became the first acting dean to create a structure for a new faculty of arts and sciences. Then, we merged all of these colleges and took the faculties away from the individual college deans and created a strong, large faculty of arts and sciences with 1,000 FTEs. We moved entire units around. All the departments had been split up in little pieces, a few historians here, a few historians there, because we have four physical campuses here in New Brunswick.

We consolidated departments and moved a large number of people to create these various consolidated departments. That was my task for a number of years. I built the faculty of arts and sciences and was its operating dean. Then, I got tired of doing that.

TB: Did you find it gratifying at the time, or was it just . . .

DM: It was a major challenge. I enjoyed the restructuring, and I feel it's one of the large accomplishments of my life because I think we made Rutgers a fantastically better institution. After the consolidation, Rutgers took off. There were other reasons it took off. We had good state funding; we had good leadership, new leadership and so on. But, the reorganization provided the infrastructure for taking off, because now you were able to launch much larger efforts with more economy of scale. You had larger resources that were consolidated, and so on.

I did that for several years, and then I decided I didn't want to do it any longer. You're running a faculty of a thousand people, with thirty departments and many institutes. I ended up spending my time doing things that I found were much less important to me than working on health services.

Now, even when I was dean, I continued to run a research operation. I continued to have research grants and so on. But, it's very hard, when you're running a faculty of that size, to also be a continuously productive researcher.

So, I decided I would go back and do something else. I was starting to look at where I might do it, and at that point Penn made me a fantastic offer. It was an extraordinary offer of a chair with substantial support, and I was pretty much going to go. And Rutgers came back and said, "Well, you've been such a force in changing this university, you can't possibly go."

I said, "Well, there are all these fantastic people working on health at Penn, and I see them sort of more as my colleagues than those here in terms of people to work with."

They said, "Well, we want you to stay here, so we'll give you lines and you bring in the people you want to work with," and so on.

That's how the institute started. I organized this institute in 1985. The university had given me start-up resources to bring in people, and I brought in some terrific faculty. It became a flourishing endeavor. We now have about 165 people, and we have faculty across twelve different disciplines. We have a robust research program. We're not gigantic in terms of funding, but last year we brought in a little over $13 million in extramural funding. So, now our university funding is minuscule. I mean, it's less than 10 percent of . . .

TB: But, it was critical in that first phase.

DM: Yes, it was absolutely critical, because what I did was I brought in research leaders to develop programs. We built a very strong interdisciplinary health services research program, which was very broad in its concerns. We do work on HIV, we do work on cancer, we do work on mental illness, we do work on asthma, we do work on heart disease. We do a lot of economics in terms of modeling the health care system. We basically have people engaged in most of the important issues that affect the health care system.

Then, about five years ago, I decided that a lot of the action is going to be in the states. The federal government was playing a much smaller role in health services organization. Yet, all of us in the health services field were pretty much oriented to the federal government and not the states. So, we had to do much more in understanding how states could affect health care services and policy.

I went to the Johnson Foundation and convinced them to give me $11 million to start a center on state health policy. We now have had it for the last four and a half years or so. It is a flourishing institution working with New Jersey and other states on a whole range of issues, long-term-care issues, SCHIP issues, health insurance coverage issues, and that's been quite a successful endeavor.

I brought in Joel Cantor, who had been the research director at the United Hospital Fund in New York. He's done a terrific job in building that state policy structure.

We've continued to build and move into other areas.

We have some people we would call straight health services researchers. We could ask them what their identification is, and they would say first that they were health services researchers. We have other people who think of themselves first as economists, or sociologists, but also as health services researchers.

I happen to be on the board of directors of Academy Health, and it's the basic issue we face. How do you grow an organization of health services researchers where for many people their first identification is with their discipline or some other area of study.

The way we're organized here is all of our people have to be appointed in departments. You can't just go out and hire a health services research professor on a permanent basis. You hire through departments. So, we have people in sociology, economics, psychology, statistics, political science, and so on.

That's the way we built this institute, which makes it a little different than other health services research institutes at other universities. They are perhaps less theory based, less academic in their orientation. But, we do have a school of public health. We also have a policy school. So, we have places around to place faculty who are more applied as well as those more disciplinary.

TB: Could you tell me something about any reckoning you came to about your own identity as a health services researcher, or health policy worker across disciplines, as opposed to a medical sociologist? I'm trying to make better sense of that transition. You talk about the need for revitalization of medical sociology, some of the values and principles in the field as opposed to the more applied and more economics-driven work of recent years. I'm wondering if you could reflect back on the '80s, when some of these identities and field definitions may have been changing.

DM: Yes. In the years I was at Wisconsin, I always saw myself first as a sociologist. I was in the top-ranked sociology department in the country and both my identity and my work were very heavily influenced. And if you go back, you see in those years I published much more in sociology journals than I do today. If you look at the last twenty years, you probably would find the journal I publish the most in is *Health Affairs*.

TB: Or the *Milbank Memorial Fund Quarterly*?

DM: Actually, I've published more in *Health Affairs* than in *Milbank*.

TB: It's a close second.

DM: I've been closely associated with both of those journals.

TB: I'd like to come back to that.

DM: I also published a fair amount in the *New England Journal of Medicine*. It is interesting because when I was at Wisconsin, I published some early papers in the *New England Journal of Medicine*. One of my colleagues came to me. He said, "Why are you wasting your time publishing in this provincial state medical journal?" That was the view. Actually, that's a view that still persists in many disciplines for people who want to work in health services. That's why, through these Robert Wood Johnson Foundation programs, we're trying to change the culture of some of these disciplines. I think we've been reasonably successful through the Scholars in Health Policy Research Program. Now, there's this new one in Health and Society, which is a population health program.

But, in those days I saw myself primarily as a sociologist. I was a person who could play in both fields. I was respected as a sociologist, and, also, I was having some impact on health and health services research. So, I had plenty of opportunity to work with my colleagues in medicine and in public health. But, I still saw myself and ran my program out of the sociology department. Even though there were opportunities to move into medical schools, I never did. We always kept it within sociology.

At Rutgers, I've created an autonomous institute which is outside the sociology department. We report to the Executive Vice President, and we are an all university institute. We have, as I said, more than twelve disciplines represented in the institute.

And, I have an interest in all those things. I find myself very, very interdisciplinary in what I read and what I pay attention to and much less focused on, let's say, more traditional sociology. So, I don't really publish much, at least more recently, in the basic sociology journals. I still publish now and then in the *Journal of Health and Social Behavior.*

TB: Is that a transition that was already under way before you left Wisconsin, or was it a more dramatic change?

DM: I think it's changed more dramatically since I moved out of the sociology department. But, I still see myself as a sociologist. I see myself as a social scientist of health more than either a sociologist or a health services researcher, but they all fit. I have no discomfort describing myself as a health services researcher. But, my interests still are very broadly social science applied to health and medicine.

For example, I'm very interested in medical history. Most health services researchers couldn't care a damn about medical history. When I took over the Robert Wood Johnson Foundation's Investigator Program, we had very few historians--I guess Rosemary Stevens was the only person to whom we had ever made an award who was a historian--even in the case of Rosemary, you could argue whether she's a historian or something else. I decided we would market the program strongly to history. I think we now have quite an extraordinary group of historians who are doing fascinating things. Every year

we manage to add a few more historians. The Rothmans just got an award and Barron Lerner also got an award this past year.

Within our program, when I started the institute, I built in a history component. I brought over Gerry Grob who was in the history department doing mental health history. Gerry was a distinguished historian and we started with mental health history. We have had a large postdoc program. We decided that we could fund at least one postdoc a year in history, within this program, who was doing mental health-related things. Charles Rosenberg would send us his best students. So, over the years, we have had an extraordinary group of people working in medical history who did their postdocs here: Nancy Tomes, Jack Pressman, just a variety of people.

TB: Liz Lunbeck.

DM: Liz Lunbeck. We just had a very, very wonderful group of . . .

TB: Sarah Tracy? Was she . . .

DM: Sarah Tracy was here. It's been a sizeable group over the years.

Gerry is a marvelous person, not only in working with the historians, but he helped to interest our people who never thought of history to take a broader view. And it was a

fight with the agencies. The agencies were saying, "Well, that's not research, that's not . . ." We fought back, and I think we were successful in convincing them that history had a great deal to contribute to understanding policy issues. I think over time, that's less of a fight.

Perhaps I now travel in a more insulated environment. I don't know what it is like when you get outside programs that I'm involved in.

For example, look at Academy Health as an organization, which would like to expand beyond its present group. It's not very oriented to theory in the disciplines, so it's very hard to attract even the medical sociologists to that organization. Among people in some of the other disciplines, like political science and sociology, and among people we think of as doing great work, not many of them come to Academy Health. We're trying to change that if we can.

TB: Can you comment a bit on the contested meanings of health policy as these issues may have been argued in the late '80s, early '90s? What was it, who controlled it? What was the role of political scientists, economists, other social sciences historians perhaps contributing to the field?

DM: I've always had a distinctive view, which I've written up over the years in various places, based on my own experiences. I got involved very early in the National Academy

of Sciences and the Institute of Medicine. I worked for various administrations in various capacities, worked with various agencies. I chaired a study section at the NIH; I was on the NIA National Advisory Council. So, I had a lot of different experiences. I became convinced, as I've written, that our influence comes from broader efforts to inform the climate of thinking and opinion-making. If we expect what we do to have any one-to-one relationship with policy, we're rather naïve. It's an unrealistic view.

But, we can have a one-to-one relationship on issues where people need a technical answer. So, you can figure out how to build a formula for funding under-doctored areas or under-resourced areas. Those kinds of things you can do. You might figure out a better case-adjustment system for hospital reimbursement.

But, if you're dealing with issues that are larger social policy issues, then obviously many broad interests are involved, including political interests. It's naïve to think that you're going to be anything but a very small player once the issues are at the political level.

There are people who do the technical work and help solve technical problems that are being faced. There are people who do health policy by trying to inform the larger climate of thinking about health and health care. By that I mean affecting how the media sees issues and presents them, affecting how students who later become health professionals and government policy makers see these issues, and so on. I think our role and influence is much larger in these arenas than in the direct, one-to-one policy arena.

My experience is, every time I've been involved in what is seen as the more prestigious, influential groups that could affect policy in the one-to-one sense, they never do. When I look at areas where I think I might have made a difference, they usually were in more technical areas. They were not politicized. There's a lot of room for making contributions. That's where the economists thrive, because they do a lot of work that's very useful in administering very large entitlement programs, Medicare, Medicaid, SSI, things like that.

TB: I looked through your publications, and I can identify some major areas to which you contributed: mental health, chronic illness, and disability. You've raised some interesting issues about rationing. What it means; and how we have to come to terms with it; about perceptions, realities of managed care. Are there other areas that you'd like to comment, that you think would be nice to have on tape? Any reflections you'd like to offer at this point on any of those or other areas?

DM: Over the last forty-five years, I've worked in a lot of different areas, and you say, "Where do I see my contributions as being most significant?" Interestingly enough, what the world picks up on isn't necessarily where you think your major contributions necessarily are. But, I think there are four or five areas where I have had a fair amount of influence on thinking.

Going back initially to the stress-and-coping idea, once again, lots of people contributed to that. But, I was part of the group that changed the model of how we think about adaptations to stress, sort of the social psychology of adaptation to stress.

I know when I got the Baxter Prize, they said I was one of the very first people to do outcomes research. The kind of outcomes research I did in those early days was quite pathetic compared to what gets done today. It is true that I tried very early to measure outcomes in a rudimentary way as a result of access to different services, and so on.

I think that my early work on rationing was quite important and helped push that discussion forward a good bit, much more so in the U.K. than here, because we don't talk about rationing here. But, I do think that helped push forward an important discussion.

My work on rehab and disability, particularly in the mental health area, I think was important. Some of the people I've influenced have gone on to make major contributions themselves in promoting mental health services, developing approaches to building service systems that provide better-quality care and better-quality outcomes.

I also take some pride in having helped build the field of medical sociology as a coherent discipline. There were people doing wonderful work before me. I mean, there were some magnificent studies. Howard Freeman and Sol Levine, for example, did a lot of interesting and useful research. Hollingshead and Redlich's Social Class and Mental

Illness was a classic study. There were lots of very good studies being done. But, if you go back, Hollingshead never thought of himself as a medical sociologist. He was a sociologist who was interested in social stratification.

What I tried to do when I wrote that first book in 1968 was create a more integrated field, which was theoretically, methodologically informed. I think that book had a lot of influence on people. I still hear from doctors who read that book when they were in medical school in the early '70s and late '60s and say it really changed their whole view of medicine, and so on. And, it was a successful book. It became a citation classic in terms of the citations index. So, I was very pleased with that.

When I did the '78 edition, I thought it was a much better, more comprehensive book, but it became too big a book and too hard a book. A lot of people said they wouldn't use it because it was too hard for the students. I find it ironic, but in any case, I think that was a good contribution.

Then there is this mental health social policy thing. I worked for decades with various groups around the country, trying to improve mental health services. We have a long way to go still. But, if I go back and say what services were when I first went into mental hospitals in the '50s to today, there have been dramatic improvements. While we can see everything that's wrong--and there are many things wrong--I think there has been a good bit of progress made along the way.

Then, I've been creating this institute.

What I did at Rutgers in reorganizing the University has nothing to do with health. The reorganization of the university is something I'm particularly proud of.

I think this institute has been a successful force. We've trained lots of good people. We have lots of people who we've encouraged to build careers in health.

And the stuff I do with the Robert Wood Johnson Foundation. I've been involved from their inception as a national foundation. I've been associated with the development of the Scholars Program for more than ten years. I've been involved in the development of the Health and Society Program and continue to be involved with that as well.

I now run the Health Policy Investigator Program, which is a terrific program, and I really enjoy the quality and diversity of the people we've brought together. We're thinking about health very broadly. A lot of these people are health services researchers, but they're coming at it from law, from sociology, from economics, from history, from different points of view. I think, as a consequence, we offer a much richer picture of the health care system, how it's evolving, the forces affecting it.

Actually, we've recently done a book which brings together some of our investigators' policy thinking around issues of inequalities, quality of health care, and promoting population health. Those are three major themes in that book.

TB: I wonder if you would comment on your relationship with *Health Affairs*, *Milbank Quarterly*, perhaps the *New England Journal*, *Social Science and Medicine*, all of which you've had a major voice in.

DM: Well, I've had a major voice in some, not others.

TB: [unclear].

DM: The journals I've had the biggest voice in probably are the *Journal of Health and Social Behavior*, and then *Health Affairs* and *Milbank.* I think in those three, I, over many years, not only wrote a lot for them, but also advised them in various ways.

Health Affairs, started by John Iglehart, was a very different kind of journal. It was a journal which saw itself as trying to reach, not the social scientists or typical health services researchers, but trying to reach policymakers. I think they've been reasonably successful.

Studies of readership show that *Health Affairs* has a bigger readership among policy type people than the other policy journals. Its subscriptions by *New England Journal of Medicine* and *JAMA* standards is low. It's only in the vicinity of ten or fifteen thousand. But compared to most of the other journals in social science, that is pretty good.

I write differently for these journals than I would write for typical social science journals. They are much less interested in theory and the social sciences, much more interested in impacting health, and that's the way I write when I write for them.

Milbank has been a more intellectual journal than *Health Affairs* historically. David Willis was editor for many years. David was an extraordinary editor. He devoted most, if not all, of his time to developing the Milbank Quarterly and to maintaining its quality. He was very interested in history and demography and international things.

So, it was a very cosmopolitan journal compared with the others. It had work on the U.K. and other countries, and it had much more of a historical perspective. It also had a strong quantitative orientation due to his interests in social demography. It's gone through periods of ups and downs. It's now competing with all these other journals for the very best papers. I'm still on the editorial board.

One of the big issues Milbank faces right now is the growth of the *Journal of Health Politics, Policy and Law*. It has a very similar outlook, interested in very much the same kinds of papers. So how do we maintain a distinctive niche and its unique character and identity?

Milbank Quarterly is also the oldest of the health policy journals. It's been around for some sixty-five years, something like that, and has had an important role historically in pushing forward some fields like psychiatric epidemiology and mental health services. So, it's a journal I have great fondness for. Because of its role, and also because it defines health studies the way I like to think of them. Broad interdisciplinary and international work, which is historically based and open to seeing that there are many ways to view the system and that we should be open to encouraging a lot of collaboration and interaction among these various viewpoints.

I published my first paper in the *New England Journal* in 1963 and over the years have published various things there. But I have not been involved in its editorial policies. Like many other people, I send things in and hope they'll get published.

The paper I published a couple of years ago on whether doctor visits were getting shorter created quite a stir. Of all the papers I've ever published, I think that got the most media attention. It went on for weeks and weeks. But, when I think of the *New England Journal*, it's a good place to publish work because it's influential. The press will pick up

on it immediately, and it reaches all sorts of audiences, that papers which might be as good or better elsewhere don't reach.

It's very interesting. The RAND Group published a paper in *Milbank* a couple of years ago, which said very much the same thing as this recent paper by Kathleen McGlynn published in the *New England Journal.* The *New England Journal* paper is more sophisticated in research terms and in how they arrive at their conclusions. The earlier paper was based on extensive review of the literature. But, basically the conclusions were the same.

The *Milbank* paper had very little impact. The *New England Journal* paper had a very large impact and is still having a large impact. The NEJM has that kind of social impact, which is important if you're trying to affect thinking about social policy.

I see myself largely as a researcher and analyst and not as an activist. I have very strong commitments to universal health insurance and believe in good caring as well as curing. I believe in a system that's more attuned to chronic illness and continuity of care, and so on. But, I don't see myself as pushing policy outcomes but rather trying to inform the policy making environment.

TB: I'm very interested in what you call the need for revitalization of medical sociology. I wonder if you can comment on that. I've read what you wrote in *Social Science and*

Medicine 1993 about that. I also wonder--this is maybe a long shot--if there's any connection between that call and perhaps some of the recognitions you've received recently from the health services field for your contributions.

My suspicion is that there's something of a contest for priority within the health services research field. Maybe a resonance between certain groups within or certain individuals within the health services research field and that call might have made a linkage. I may be off base.

DM: I'm not sure I see it quite that way. One of the things I feel reflects my success at presenting a broad point of view is I've received honors from the American Sociological Association, and from the Association of Health Services Research. I received the Baxter Prize and a couple of awards from the American Public Health Association. This year I got an award from the American Psychiatric Association. So, in a way, my work transcends any one area. Some people say I've contributed valuably to a lot of different areas, and I'm proud of that because I think we should.

I think too many medical sociologists are too insular. If they showed greater receptivity to developments in other areas, they would not only do better work, but would have larger influence.

I've written a couple of papers--I don't know if you've looked at them--talking about how economics has usurped the role of sociology.

TB: I am familiar with that.

DM: Yes. Part of it is inevitable in a system that's $1.6 trillion and where cost is ever present on people's minds.

TAPE 2, SIDE A

DM: Sociology, like all disciplines, tends to be insular. Most sociologists aspire to gain status in the discipline. The way you gain status is by making contributions to sociological theory and methodology and focusing on distinctive sociological perspectives.

Applied work is not seen as being of equal value by the discipline. I've always thought that that is not a productive way to view the work of the disciplines. I think sociology in medicine can do much more important work if it forms collaborations with people who understand the biology, the genetics, the economics of things.

You mentioned you had interviewed Vic Fuchs. When Victor Fuchs started the health unit at the National Bureau of Economic Research, he asked me, a sociologist, to be on the advisory committee of the National Bureau of Economic Research. I think I spent seven or eight years on this committee at the National Bureau.

I've always felt that the way you energize what you're doing and make it relevant to real issues is to maintain a dialogue with other disciplines and how people view the same kinds of problems in different ways. So, I don't have any sympathy for sociologists, for example, who want to talk about mental illness but don't want to pay attention to genetics, as if genetics is somehow a competing position. You don't pay any attention to

genetics or other biological factors because you're going to develop a social view of mental illness. I don't have much patience for that, and probably less patience as time goes on.

I think if we're going to do work that is lasting and valuable, we have to do work that takes account of the understanding and findings of other related disciplines and shows how our part is important within the larger context. Not by trying to explain away the other contexts and say they're not important; we have a better explanation. I think too many medical sociologists don't give enough attention to related disciplines and how what they have to say impacts how one should view theoretical issues and practical life issues.

So, my commitment is more to interdisciplinary social science, and increasingly, I think of myself as a social sciences medicine person. Much more so than a sociologist of medicine, because I draw on political science, I draw on economics, I draw on psychiatry. I'm interested in people who know the most about whatever it is I'm studying, and they're often not sociologists. They're people in other fields. The effort is to take what's best in each area and build that into your perspective.

But, the other side of it--and there is another side--is that you have to have a strong disciplinary understanding, or else very often the work has no context. I think that's one of the weaknesses of a lot of the work in public health and health services research.

Increasingly, we are turning out people who have good methodological skills and can do research, but they don't have a broader perspective and they don't have a theoretical orientation. A lot gets lost when you don't have a basic model which you can use to frame issues, think about them, and develop implications from them.

I think that health services research would be a much stronger area if it was more theory based, if it gave more attention to theoretical considerations and not just researching whatever the issue is at the moment.

If you ask me who are the most influential and lasting people in medical sociology in terms of their work, they're people who have developed this theoretical perspective. As I said, Elliott Freidson never saw himself as a person doing medical sociology, although he was one of the prominent medical sociologists. But, I think he contributed immeasurably to our understanding of how the medical profession operated. Rene Fox, from a very different point of view, does that very successfully.

Over the years, I've been asked to look at a variety of deanships of schools of public health. My reluctance has always been the feeling that in most schools of public health, students get a little bit of everything, but don't develop enough of a strong perspective to integrate thinking in a way that facilitates outstanding research. I think that's particularly a major weakness of the master's of public health. When you get to the doctoral level, public health makes much more use of social science disciplines and other work. But, I

think one of the weaknesses of the public health field has been the lack of a strong theoretical framework.

TB: What do you think of schools of public health which have created programs in health services research, or have that as a focus of master's in public health degree? For example, those which used to be much more epidemiologically focused, but now are very much health service research oriented in an econometric definition of health services research by and large.

DM: I have two minds about that. For those people who do that extraordinarily well--I would think of the RAND UCLA group as being one of the very best, if not the best--even though they provide degrees in health services, they seem to do well because they provide also good economic theory. It's my sense that the people who come out of their programs have a disciplinary strength, even though they may have not been trained in the traditional disciplinary departments. I think people coming out of the economics RAND program have that strength.

I see a lot of people from health services doctoral programs who don't have a strong perspective, and that's a weakness of the field. It's one of my concerns about the health services field relative to some of the basic disciplines.

Many of the younger people have a lot of skills, and some of the skills are very sophisticated, and they do very important work. But, very often they lack a broader conception, a framework that pulls their work together in a more meaningful way, thinking longitudinally, historically. At the moment, their technical work might be very valuable. They tell us who the uninsured are and important things of that sort. Then, the question remains as to what produces lasting contributions to the field?

So, I think back to someone like Avedis Donabedian, who had an enormous influence on the things we do today. What I really loved about him--we were friends during those days --Avedis always thought deeply about these issues. He didn't just go in and collect data. He always worried about the meaning of those data within a larger framework.

Vic Fuchs is another such person. I think he was more successful than other economists because he was able to take economics and put it into the larger, broader understanding of the health care system. His *Who Shall Live* was a very influential book--I don't know how influential on economists--it was a fantastically influential book on non-economists and had great impact on students.

He provided a framework. He also did the studies--he was a good empirical researcher. What made him different than many of the others who just did empirical research is he was able to incorporate his work into a larger picture of how the system worked. I think those are the people who have the larger influence, the people who are remembered once

the problems of today pass and we go on to the problems of tomorrow.

TB: That's a very interesting perspective. Many people in health services research lament the lack of influence for the field and think it's a problem of communicating the results of technical studies more effectively. They don't succeed in influencing policy because they're not sufficiently good at communicating the results of the technical studies. You're suggesting a very different perspective. Maybe the very definition of research needs to be broadened and put into a wider context.

DM: Not only that. That's a very naïve point of view. I know these people and I know how they think. If you had a broader perspective and understood how health policy gets made and how the Congress works and how the budgetary process works, and all of these kinds of issues, if you paid a little attention to political science and government and some of these other areas, you would understand that that view is naïve and short-sighted.

Lots of people have written on why the Clinton health plan failed. The little descriptions of failure are not terribly illuminating. Then, there are people like Theda Skocpol--I don't know if you're familiar with her work--who really provided a much better understanding. She may be right or she may be wrong in her final analysis of why it failed, but she provides much more context to understand how these health policy issues relate to other issues, and so on. I happen to be reading Clinton's book now. I don't think his perspective adds much to understanding the issue beyond what other people have said.

But, the point is that health services research would be a much more effective discipline if it gave more attention to this broader context.

A person I have great respect for is John Wennberg. If anybody has made a big splash in his thinking, it's John. And John does have a broader perspective. But we can still think broader and John and his group at Dartmouth have been doing that.

When they really get down to it, after years and years, we still don't understand these practice variations. Part of the reason we don't understand it very well is we don't have very good theory in health services research on how the culture and organization of professional medical groups work and how communication takes place within the context of medical care. I don't think ignoring those things allows you to answer some of these deeper questions about health services and where they should be going and how you get there.

But, it is true that, ultimately, many of the major decisions are made on both ideological and technical considerations. You first have to have the notion you want to get something done and then you have to know how to do it. The health services researchers often have a lot to contribute on how to implement it and developed ways of figuring out how you structure reimbursements and all of the specialty procedures. You can do that actuarial reimbursement kind of thing.

It's very important and very valuable. Slight technical differences can make a big difference in what happens in health policy. How we pay doctors for various Medicare procedures changes the whole configuration of Medicare, the whole configuration of how doctors organize themselves, what new enterprises they start, what new hospitals get started, and so on.

Interestingly enough, the amount of analysis of that is minimal compared with studies of what's happening at a microtechnical level. Jamie Robinson is doing nice work on system changes, and some others are beginning to do some work on that as well. It's an understudied area. There's too little of this larger perspective that is looking at the system, not only in terms of its processes, but also its culture, its incentives, and all those related issues.

TB: My sense of the health services research field is that it narrowed into the '80s and early '90s. Let's take advantage of the credibility of John Eisenberg's work and the transformation of the old center before the AHCPR which went through various incarnations became the AHRQ.

My sense is that once the field succeeded in proving its visibility and funding, there were those in the field who were quite concerned about overly narrow interpretation. I think of Karen Davis in this regard. I think of the book edited by Eli Ginsberg--I believe he reviewed it--which was a cross-section of work of various dimensions and areas in health

services research. It was published in the early '90s as an attempt to broaden the field again after its initial and perhaps strategic narrowing.

I'm wondering if you sense this yourself, whether you would agree with that interpretation. Did you play any role in accepting your awards and perhaps in your comments when you acknowledged the awards that were offered you in response to [unclear]?

DM: Well, one of the issues we have in AcademyHealth, as I mentioned earlier, is for many people, health services is their second area. They're physicians first or sociologists first or economists first, and so on. What they have in common is that they're interested in health services. They don't have in common where they're coming from, whether it be pediatrics or economics or social psychology or whatever. So, they don't share that part of their world. What they share is the health services research endpoints.

They all have organizations that represent the other side, the political science association and the sociological association and all these other organizations. So, health services, by its very nature, is a hybrid field. It's focused on just a piece of the effort. Trying to broaden it is very difficult in that context.

There have been people throughout the health research community who have always argued that we really have to use social sciences as basic sciences for biomedicine and

public health, that basic work in all of these areas can help understand better what we can do to promote health and improve health services. That was the position of the NIMH in the 1960s. NIMH said, "We're not going to just fund work on disease, we're going to fund all of the basic sciences, and the basic sciences include psychology, anthropology, and sociology."

Then Reagan became president in 1980, and the word social became a stigmatized word. If you wrote a grant with that perspective, you didn't get funded. I saw my students, who were sociologists, now calling themselves epidemiologists and health services researchers and other things. The political environment only allowed them to be funded under some labels and not funded under others, and we're still living in that world.

We're still living in a world in which, if you're studying mental illness, specific diagnoses are the key concepts, not broader measures. There's been a strong ideological reaction to funding broad social science.

Conservative representatives in the Congress aren't interested in studies of poverty or SES distributions. They don't want to hear about inequality unless it's very narrow inequality in access to traditional medical care. The agencies have steered away from broader population health determinants. Just like the AHRQ steered away from doing practice guidelines after they got beaten up and almost eliminated by the orthopedic surgeons.

You live in a real world and you have to survive in that real world. The social and behavioral sciences which in the '60s were seen as basic and deserving of support on their own terms, pretty much now are only seen as worthy of support when the work is disease based or related to some health services-issue of concern.

The National Science Foundation does support some basic social science. But, relative to the NIH, the National Science Foundation program is minuscule in terms of basic social and behavioral science. So, our field has largely depended on the NIH. The NIH supports research related to diseases, and that's the way people have come to think and how people increasingly have come to identify themselves.

If you look at the health services research careers of people who have Ph.D.s in sociology, a lot of times they don't identify themselves as sociologists. They may find it more productive to be seen as epidemiologists or as public health persons, or something else. So, in a sense, the larger environment also affects how professionals in these fields define themselves and select research problems.

TB: I wonder if you would comment--and this may be the last set of comments--on the relationship between your political or ethical commitments you've mentioned, and your belief in a universal health care program as something that may be desirable to achieve in this country, and your research. You commented before on the relationship between

research and change in policy. This is yet a broader context.

DM: Yes. I have two strong identities. One is, I feel very strongly about what a good health system is, and I don't think we have one here. To the extent that I can improve it through my work, I'm highly motivated to do so. I also, at the same time, believe that the work we do as researchers must be independent of any political ideology and political point of view. Some people say, "Well, that's naïve. Your work is always influenced by ideologies, problems you pick, problems you decide to attack," and so on.

It has to be real research. By real research, I mean that your idea has to be open to alternate interpretations and disconfirmation. You have to have people willing to say, "Well, I may feel ideologically strongly about this but my research indicates it isn't true." I've had several of these experiences in my own professional life. Some people on the liberal side have said I've betrayed the liberal cause, because of things I've written that were contrary to what I originally believed and what they like to still believe.

When I went to Britain in '65, I had read a lot about the National Health Service. It seemed to me to be the kind of health service that provided universal care for people at the point of need without any access barriers, and so on. Everyone could have their GP, and you'd get access to the system. By removing the fee for service--and this was a view that a lot of people held in those days--by removing the fee, you remove a barrier in terms of allowing the physician to make the best clinical judgments and do what's best for the

patient. So, getting the fee out of the nexus between patient and doctor was a real contribution to improving the quality of care itself.

Then, I went and studied British general practice. As I mentioned, I came back and had this grant from the National Center to do a comparative study of British and American doctors. What I found was contrary to what I went in believing. The form of capitation used in the National Health Service led doctors to be less responsive to patients than doctors in the U.S. who were being paid a fee-for-service.

I was able to replicate my British findings in the United States by comparing fee-for-service doctors and HMO salaried and capitated doctors. I found that the salaried and capitated doctors in the United States working in HMOs behaved just like the British doctors did, and very different than the fee-for-service doctors.

Because the fee-for-service doctors were being paid for each service they provided they were spending more time and they would stay in their office seeing patients longer. They were in a sense willing to provide responsiveness and amenities that they felt were key for patients paying a fee-for-service and in that sense the patient had some modest control.

Within the capitated practices in England, doctors behaved quite differently. They saw many of these patients who were coming back to them frequently as a nuisance. They tended to characterize them in negative ways if they were high users of care. They didn't

have the time to spend a large amount of time with patients' complaints, and they were not willing to expand their hours because they were paid a fixed amount.

I came back and wrote some papers about the disadvantages of capitation versus fee for service, even though I went in believing fee for service was a bad system. Over the years, as I've studied these things more, I have a more complicated view of how you might combine different incentive systems. At the time, I was willing to say that what I had believed so firmly was just wrong. I wrote a number of papers about this. I gave a major presentation at the Institute of Medicine where I summarized this research.

Some of my liberal friends who felt that HMOs were the way of the world and the only way to go, and capitated doctors were the only way to go, felt that I had sold out. My view was that it's what research is. Research is to find the truth as best you can and report it, whether it's what you believe or you don't believe.

That was the same thing with the *New England Journal* article a couple of years ago. I believed that, as many doctors were saying, that with managed care, doctor-patient visits were getting shorter. I studied it over a ten-year period. I found that they weren't getting shorter. They might even be getting a little longer over time. I went in believing the opposite was true, and I was going to demonstrate the opposite.

I think that's what good health services research does. It doesn't prove what you already

think you know. It asks a question that's open to confirmation, modification, or rejection. I've written a couple of papers about that, because I think a lot of people don't approach the field that way. A lot of people are out to sell a point of view. I'm open to the fact that I might be wrong about how things operate in reality.

So, I feel strongly about that. I have my values about what I would like to see the health system be, and I structure my research to examine how we can build programs to improve the care for people with chronic schizophrenia, for example; if we do things in different ways, are the outcomes better? One's views should be always open to disconfirmation.

I was involved in a large effort that the Robert Wood Johnson Foundation had some years ago to build mental health authorities in large cities. There were HUD housing vouchers involved. The whole thing was about a $125 million effort.

We believed firmly we could work with cities to encourage them to build integrated health authorities that would improve the care of people with serious mental illness. Then, when the evaluation studies were done, while the health authorities had developed, and some of them seemed to be doing good things, no one could show they had made any difference in terms of the outcomes for people with mental illness.

I go around and talk about why this failed, and let's try and understand why it is that these efforts didn't work. There are people who believe what they do is effective no matter

what. There's no research that's going to change their minds about how you do these things.

Assuming you have good studies, the data have to tell a story as to what's true and what isn't true. If you asked most people in the health industry if doctor-patient visits are getting shorter, two years ago the vast majority would have said, "Yes, managed care obviously is pushing doctors to see patients quicker and quicker." But, there's no evidence that that's the case. I think you have to live, ultimately, by the evidence if you believe that research is valuable and worth doing it in the first place.

TB: Any final reflections?

DM: I think this is a field that's going to grow and get better over time. When I go back to the crude studies we did in the early '60s compared to the quality of the stuff that gets done now, there have been massive advances in the sophistication and quality of the research, and the difficulty of the questions people are asking, and the complexity of questions they're asking. Health services research is a much more ambitious endeavor in this more narrow sense.

I would, however, like to see it done within a much broader framework of thinking. In some ways, the RWJF Investigator Program which I direct achieves that. We have funded a wide variety of outstanding people who have looked at the health care system

more broadly. I think in the long run they will make major contributions, and have made major contributions.

For example, we funded Michael Millenson to do a project on quality of care before anyone was really putting much emphasis on quality of care. I think his book on quality of care helped put this concern into the mainstream. Then, we have other people who are doing more academic things. But, I think in the long run, if you think and research broadly, such work contributes to building a better health care system.

TB: Thank you very much.

DM: My pleasure.

END OF INTERVIEW