

Stuart Altman

August 16, 2004

TAPE 1, SIDE A

Ted Brown: It is August 16th and I am in Stuart Altman's office at Brandeis University, in the Heller School.

Professor Altman, I note that you grew up in New York City and attended the City College of New York, with a major in economics and a bachelor's in business administration. Will you tell me a bit more about your early family influences and why that was the direction of your undergraduate studies?

Stuart Altman: I had at a very early age, sixteen, taken several jobs working for investment firms. An aunt of mine was an office manager of one of those big investment groups. My parents were essentially Depression parents. My father got a job out of high school as a draftsman, and we always thought of ourselves as middle class. We didn't have a phenomenal amount of money, but we never were poor. I grew up in the Bronx, and it was during the Second World War and the '50s. While there was some fleeting talk about potentially moving to New Jersey and my going to Rutgers, the City Colleges of New York had the allure of being good-quality places and more than acceptable places to go to, which I think you can appreciate. So, it wasn't like, gee, I'd love to go to Harvard or Yale: (a) It wasn't in the cards financially, and (b) it wasn't even something we thought about. My intention was to go to uptown City College. I didn't know what I would major in.

Then, I got a job at sixteen, while I was in high school, and would travel down to midtown Manhattan. I made \$1.04 an hour. It took me forty-five minutes to go to work and forty-five minutes to come home, to work three hours a day. So, I made \$3.12 and traveled an hour and a half a day, and I loved it. I wasn't particularly excited about high school. It was okay and I did well. But, this was the world of work, and there I was on 46th Street and Park Avenue with all these important people in suits, and a block away from Madison Avenue. It influenced me, and I decided to change my focus toward finance and go to downtown City College (the business school of City College) and major in financial accounting.

When I got to City College, I was majoring in accounting, and two things changed my life. One was I took my first accounting course, and I did well in it. But, I had such horrendous penmanship that the accounting professor--he was very good--said to me, "You know, we keep hearing about accountants that have bad penmanship, but you exceed the limits, and if I was you, I'd get into another field."

The second was I had an almost Masonic economics professor, I did well in the midterm, and he said to me, "You know, you should major in economics." To me, economics was finance, which is what I was going to major in. I'd moved from accounting to finance, but he said, "No, no, no. Economics is different." He had me take courses in economic history, and I switched fields.

His name was Bob Weintraub, and he managed to put together a group of maybe eight or ten of us. We all went on to graduate schools all over the country, among them the University of Chicago and Yale. I wound up going to UCLA. Some

did quite well. One became provost at Penn State.

Bob Weintraub had a very significant influence on our lives. He was one of what are called the “Chicago economists;” he believed in the marketplace, a disciple of Milton Friedman. I didn’t really appreciate the nuances. He convinced me to go to UCLA. When I looked back, it was a farm school of University of Chicago economics, it was very market oriented, it believed in minimum government, it was one of those classic, market-oriented economics departments. So, I left City College, graduated in ’59.

It was also an era which was post-Sputnik and the National Defense Education Act (NDEA). All of a sudden, if you went to graduate school and had something going for you, there was a lot of money.

I look back on this era, and think how fortunate we were. I always said that my life has been dictated as much by demography as anything else. I was born in 1937, during the depths of the Depression, and there were very few of us born in that era. Then, I came out of undergraduate school in ’59, when a lot of money was being spent on graduate schools. When I began teaching at the college level in the middle to late ‘60s, there was this glut of baby-boomers entering college, so it was very easy to get academic jobs. I consider myself very fortunate.

I went to UCLA and got caught up in this market-oriented approach and did well. UCLA was not what I would call a first-tier graduate school, but it was good. The training was solid. And, I graduated in five years with a Ph.D

TB: Well, one of the curiosities in your CV is that you got your Ph.D. in ’64,

when you completed your dissertation? Yet, from 1962 to 1964, you were already working as a labor economist for the Federal Reserve Board.

SA: Yes. You look back on your life, and there are these things that have a major impact on your career.

I was a typical graduate student from 1959 to 1962, did all my coursework, got my prelims out of the way, and got a master's degree on the side. I majored in a labor economics, which was the most social major of the market-oriented department. I already had the view that this market stuff had its limits. But, I played by the rules and could spout back the rhetoric. I believed it, more or less. But, the labor economists, were less Chicago oriented than the other professors..

My dissertation adviser was a young assistant professor, by the name of Lee Hansen, who ultimately went on to Wisconsin, which was the alternative to the Chicago school, a more institutional school. In 1962, Victor Fuchs convinced the Ford Foundation that there were not very many economists who were focusing on public policy. How ironic given where we are today. But, he convinced the Ford Foundation to put together a conference at Goucher College in Maryland for young economists. You had to be under the age of 40, and you had to have an interest in public policy. I had decided to write my dissertation on unemployed married women. It was partly personal and partly the nature of the times.

There was a major intellectual battle going on in the early '60s about whether the countries high unemployment was the result of structural problems or lack of economic growth. The structuralists believed we had individuals who were

permanently unemployed and we needed to have the government come in and retrain them. We had pockets of unemployment in West Virginia and places like that. This was also the end of the Eisenhower administration and ultimately beginnings of the Kennedy years and a much more activist-oriented government.

Then you had the more conservative, market-oriented people who believed these are normal times and we should not have the government intervene, and instead, we should use monetary policy to encourage more economic growth. Again, I was in a Chicago program that believed that there was no structural unemployment. There were these fringe workers, and one of the fringe worker groups were women. I'm exaggerating for effect, but the Chicago types believed women were not major breadwinners, and their unemployment was fictional or transitional. They were secondary workers. In my dissertation, I argued they were wrong. Women workers were here to stay and when they become unemployed it had serious negative effects for the family.

So, I applied for this fellowship and I was accepted, and I went to Goucher College, and there I was with a bunch of other young economists worrying about policy. One of the people who came up to speak to us was a labor economist by the name of Murray Warnick, who was in charge of the labor market branch of the Federal Reserve Board in Washington. And he took a liking to me. I went and I talked to him, and he offered me the ability to come to the Federal Reserve Board and have what's called a Federal Reserve fellowship to finish my dissertation. I was writing a dissertation on unemployed married women, and he was with the Federal

Reserve. I said, “Well, why are they interested?” They’re interested because the Fed was trying to decide how aggressive it should be in the marketplace, and, clearly, they looked at the unemployment rate. So, it made sense.

All the data I needed was in Washington, and I was spinning my wheels at UCLA. I was away from the data sources that I was trying to understand. I was reading these labor-market studies that had an annual report on women in the labor force. But, I wanted unpublished data. So, against the advice of my professors, I accepted the fellowship and came to Washington, D.C. in the midst of the Cuban-missile-crisis. That would be October 1962.

I was at the Fed and working away on my dissertation. I received tremendous support from the Fed. They were wonderful. I got paid, I had a secretary, and I began to learn how to use a computer. I even learned how to program, but it was complicated. You had to learn how to program in zeroes and ones. I did learn early on what a computer was like. So, it was wonderful.

I finished my dissertation in early 1964, graduated in June of '64. I liked the Fed. It was a nice place to be. And in late spring of '64, the Fed offered me a job. And then, as luck would have it, somebody called me up from the Pentagon and said that President Johnson was looking into creating an all-volunteer military. He was looking for labor economists to help study the labor market to see what it would take to recruit enough men. I flunked Boy Scouts, couldn't tie those knots, never was in the military, did my dissertation on unemployed married women, and I became one of the chief labor economists to study how to create an all-volunteer

army, and it was fascinating.

I went over to the McNamara Pentagon in June of 1964 and worked under a very able man who had come from the RAND Corporation by the name of Bill Gorham, who later became president of the Urban Institute and a very powerful force in social policy. At that time, he was a young hotshot out of the RAND Corporation. This was the era of the wiz kid, and I think of myself as a junior wiz kid. I didn't quite make the wiz-kid status, but it was both phenomenally interesting working with such capable and well trained economists. It had a lot to do with my future. It was an era where economists became *the* major discipline that influenced public policy in Washington. There have been books written about this era, and it had a lot to do with the individuals I worked with at the Pentagon.

TB: Didn't you meet Enthoven there?

SA: Yes. Enthoven was a wiz kid. I was a junior wiz kid. Enthoven was in what was called planning and evaluation. He was the strategic-planning guy there. I got to know Enthoven later, but I just saw him and feared him. He had under him some very smart, aggressive, tough economists. They were in one division; I was in another division called military manpower.

So, we were busy trying to develop plans for an all volunteer military as Vietnam began to heat up. In the beginning--this is early '64--we were sort of oblivious to Vietnam, although I begin to get a feeling that the U.S. was in much deeper than what we were hearing on TV. The Pentagon was an interesting and scary place. First of all, it was easy to get lost there, and there was a lot going on. I

got to work ultimately for several presidents and many cabinet secretaries. Nobody came close to--and I have known pretty smart people over my life—to Secretary McNamara. He was like a machine. He was analytical; you had to follow very precise rules in writing your reports.

I worked for six months on why it cost so much more money to train pilots in the Navy than it did in the Air Force or the Army. I finished the report on a Friday afternoon and thought I'd have the weekend off. Saturday afternoon, I get a call from one of his [McNamara's] military adjuncts who says, "The Secretary has a number of questions and wants the answers on his desk by 0900 on Monday." I came in Saturday afternoon around four o'clock and there are about sixty questions. That was the nature of McNamara and the Pentagon.

Anyway, I stayed there for a while, and then Vietnam began to have a big impact, and the idea of an all-volunteer military got pushed aside. I became the person responsible for deciding how many people should be drafted into the military, in the sense that I had to do the models that said we want to build up a force of this much, these are the number of people who are likely to leave, these are the number of people who . . . Unfortunately, the people are beginning to get killed. I had to figure out how many people would be needed to meet the force requirements.

I was hearing how we were going to phase out of Vietnam, and I was looking at the numbers that the military was projecting it would Need. It didn't add up.

For a while I would write the letters. I didn't sign them. You learn in the

Pentagon, you never write the letters you sign and you never sign the letters you write, which is a maxim of government. I'm writing the letters, "Dear General Hershey, Please have --- men drafted into the Army, so many drafted into the Marines." This was not what I bargained for.

I took a sabbatical--the first time I think anybody ever asked for a sabbatical in the Pentagon--and taught at the University of Illinois for a semester in labor economics, and then decided I wanted to try an academic career.

A side issue, but I think an important one.

Joe Califano, an assistant to McNamara, was picked by President Johnson to become Assistant to the President. He convinced the president that every department in the government should have was an office of Planning and Evaluation, designed along the lines of the planning and evaluation arm in the Pentagon that Enthoven was responsible for. So, they formed an Assistant Secretary of Planning and Evaluation in the Department of Health, Education and Welfare, and they asked Bill Gorham, who had been my boss, to become the Assistant Secretary for planning and evaluation at HEW. He took with him four or five of the senior economists and analysts from the Pentagon to staff this new office. They asked me to come and join them. But, I decided I wanted to see what academic life was like and accepted a position at Brown.

TB: You were there for four years, from '66 to 1970.

SA: Many of my associates from the Pentagon had moved over to HEW under Bill Gorham. Putting this into context, the hiring of young economists was

happening all over the government. There was the Office of Economic Opportunity (OEO), which was established by President Johnson to fight his “war on poverty.” Many of their recruits were young economists. They all were trained in the early 1960s. None of us had any training in the areas in which we wound up, except we were trained to understand markets, we were trained to think analytically and logically, and, most importantly, we were trained to use numbers and statistical models.

Econometrics was just in its early stages, and without sounding boastful, the economists of the ‘60s totally dominated the intellectual ferment of Washington. No discipline came close. Sociology, social work, history, legal, didn’t have the firepower. Now, it’s not that they weren’t smarter or nicer people, but it just was . . . It didn’t matter where you were, the Labor Department, Office of Economic Opportunity, Pentagon, or HEW. A lot of it had to do with McNamara, because many of the Pentagon analysts wound up all over the government, and I was part of that era.

I teach a course on policy analysis, and I recently reviewed a book that discussed that era. Back to my story, for a while at least I decided to leave the excitement of Washington and go to Brown and become a traditional faculty member.

TB: Do you remember your experiences as a faculty member, because you did go back to Washington soon after that?

SA: Yes, I remember it very well. The good and the bad; I like teaching. I

was a good teacher.

Brown was an interesting experience for me, being a City College and UCLA graduate, and from the Bronx. Brown had an elite class of kids who had come from Chauncey Hall and so-and-so. These were kids who drank sherry in the afternoon. The economics department was not like that. It turned out the chairman of the department had grown up around the corner from me, and another professor grew up in Brooklyn. The economics department looked like New York City moved to Providence. But, the university had a very different feel to it. I was enjoying that.

What I didn't like was the departmental politics. We got into huge personal, petty fights that led to a split within the department. Our split was ideological.

There was a group that wanted to be pure economists and believed in only publishing research in academic journals. Then there was another group that came from government, who wanted to apply economics to social problems. Of course, I was part of the second group.

Before it blew apart, I managed to get tenure. I had published enough to get tenure. Also, it was an era of fast growth in the academic community. It wasn't as hard to get tenure as it has become. I was also director of the graduate program

While at Brown, I continued to do research on military manpower issues. I was asked to participate in several commissions that continued to look into creating an all volunteer military. I ultimately worked on the final report that President Nixon approved. I was involved in the creation of what is now our voluntary military, which has lasted now for thirty-five years.

Also, during my stay at Brown, my friends in HEW called me and said, “You know, we’ve got a problem with nurses in the labor market. There’s the Nurse Training Act. You know all about women in the labor force. Come down and help us.” Being a good academic, I came down. I knew absolutely nothing about nursing, I knew nothing about health care, but I knew enough to say, “I need a grant in order to study this problem.” I began to look into the supply of nurses, and that was my introduction to health care.

With all of the ferment going on in Brown, I called my friend, Bill Gorham, who had now left HEW. When Nixon became President in ’68, Bill Gorham moved over and became head of the Urban Institute, which was established by the Ford Foundation.

TB: You became a fellow there.

SA: Exactly. I said to him, “Get me out of here. Let me come for a year, let me finish my book on nurses, give me a fellowship, give me an office.” And he did.

I came back to Washington for a year in 1970. For the first year, the Institute left me alone; I was writing my book. But, my best friend was the number-two person at the Urban Institute. And I still knew a lot of the people there. Then, two things happened. More people have asked me, “How did you ever get into the Nixon administration?”

TB: That was going to be one of my questions.

SA: All of my friends from HEW had left, except one guy I didn’t know that well who was in the education policy group. He had been on leave for a year at

Columbia, and came back into the department when the Nixon administration took over. The man who took Bill Gorham's place was a liberal California Republican.

Nixon brought with him from California two groups of people. One group went into HEW and a few other places. The Secretary was a fellow by the name of Finch; the undersecretary was Veneman, and their friend was Lou Butler. They were three Rockefeller type Republicans. These were liberal Republicans of means, they had been Republicans probably back to Lincoln, but they believed in helping people. They were really honorable, decent people.

Then there was another group. I call them the Prussians. I didn't like them, and I won't even mention their names. But, they also came into the Nixon administration.

Lou Butler was clearly of the first group. Finch, unfortunately, had a nervous breakdown, and in the President asked Elliot Richardson to become Secretary of HEW., Lou Butler, who replaced Bill Gorham as Assistant Secretary for Planning and Evaluation, had absolutely no experience in healthcare issues. By a fluke, Finch wanted to appoint as the Assistant Secretary for Health the former head of the Massachusetts General Hospital, Dr. Knowles, who was a real medical radical. At least as viewed by the conservative arm of organized medicine. Here was the Nixon administration asking a radical type doctor to head up and become Assistant Secretary for Health. The AMA became apoplectic; they fought with the Nixon administration to stop Knowles from becoming Assistant Secretary. They succeeded, but in the process, nobody was appointed Assistant Secretary for Health.

Lou Butler, by dint of a vacuum, became the equivalent of the chief health person in the Nixon administration, with no background.

The Country had gone through the '60s, and had passed the Medicare and Medicaid program in 1967. The Federal government became very action-oriented in health care for the first time. It passed all kind of legislation in the latter part of the '60s to expand the capacity of our health care system, to train new doctors, to intervene and to make our health system work better.

There was also a strong and growing force that wanted to create a single-payer system, led by Walter Reuther, young Senator Kennedy, and a group of people called the Committee of 100. The Nixon Republican administration became concerned that the U.S. would become a socialist country. They realized that they had to come up with an alternative. They couldn't just say no. So, health care became a major issue even though it was a Republican administration.

Lou Butler was in the middle of this. He had as his advisors two old-line Democrats who were career civil servants. He knew he couldn't get either of them appointed as his deputy. It was a political appointment. He had deputies for other social services and he needed one for health. There was nowhere to turn Within his staff. He needed to go outside.

He turned to a guy who was in education, which was also part of HEW. We hadn't split the Department of Education off yet. His name is Mike Timpane, and Mike Timpane says, "I know just the person to become your deputy for health." "Who is it?" he says. "His name is Stuart Altman, and he's at the Urban Institute,

and he knows all about health care, and he has a great sense of humor.” Lou Butler had this wonderful sense of humor, and Mike Timpane has one as well.

I was finishing my book on nurses. They invited me to lunch, and we had a great time for two and a half hours. We were laughing, talking about things, becoming friends. He said, “Oh, you’re great. Do you think there’s any way you can get appointed in a Republican administration?” I said, “I don’t know. I don’t have any idea. What’s a Republican?” Before he had time to try and appoint me, he decided to leave HEW. Secretary Richardson leaned out to someone he met when he was in the State Department, who worked for Henry Kissinger, who had been a deputy under Enthoven in the Pentagon. His name is Larry Lynn.

Larry Lynn had left the Pentagon to go to teach at Stanford -- smart, tough, arrogant. Graduated the same year I did, from Yale. Richardson asked him to become his Assistant Secretary for Planning and Evaluation.

. Lynn got Richardson to promise that if he could find good people, he could get them through even if they didn’t have the right political credentials. The first person out of the box was me. I was oblivious to this.

As an aside, you can throw this away, but it’s funny.

The political people under Richardson said, “Well, do you have any political Republican friends or influences?” I said, “Well . . .” It turned out that on my street in Providence was the only elected Republican in Rhode Island, and his daughter and my daughter played with each other. I called him up and I said, “Hi, I’m Stuart Altman. I’m Beth Altman’s father.” He went, “Oh, yeah. How are

you?” And I told him the story. I said, “Do you think that you can get the Republican Party of Rhode Island to back me?” “No problem.”

I found out later that people around Nixon took one look at my background, McNamara, Gorham, all those people, and they fought like hell to stop me. But, Richardson had promised Lynn that he would get me through, and he did. I became the deputy for health care. This was 1971.

I had no background in health care. I had no past. I just had the present and the future.

About ten things happened at once. I had no past, so I don't know this is not normal. August of 1971, Richard Nixon imposed wage and price controls. Controlling health care cost became a dominant issue. I became the staff director for the new, fledgling Economic Stabilization Program. It just so happened that my immediate bosses were Don Rumsfeld and Dick Cheney. Of course, I didn't know who they were. They didn't know who I was. They probably still don't remember me. And we created the program. But, I hadn't given up my HEW job. I had two jobs because I was still Deputy Assistant Secretary for Health Policy.

In 1971 Congress passed what became known as HR1, which was one of the largest reforms of the Medicare and Medicaid program ever attempted. Remember, Medicare was passed in '66-'67, and the country didn't know what had hit them. Health care spending went through the roof. The Congress start thinking about how to slow the spending growth rate by 1968-9; they passed the legislation in 1971. HR1 included all kinds of change: the first restrictions on the payments for

physicians; on hospitals; changing the way we pay for nursing homes; the PSRO program, which was the first government attempt to assure quality of care in the Medicare program. It was a huge piece of legislation. It was the biggest intervention of the federal government in the direct delivery health care system in the history of this country.

Medicare, when it passed, was designed to be a payment system and not to influence the practice of medicine. By 1971, that's over. And I was in the middle of it. Unlike today, there were only a handful of people throughout the government working on healthcare policy. So, here I was, the deputy assistant secretary for health care. By this time they had appointed an assistant secretary for health, Roger Egeberg. He was a nice person who also didn't know very much about health care policy. He was more interested in Russia and other international health issues. Then, they brought in a new Assistant Secretary of Health from Arizona by the name of Dr. Marty Duvall. Dr. Duvall became an important force in the later years of the tenure of Secretary Richardson.

The second thing that happened is Larry Lynn was now assistant secretary. A huge fight developed between Larry Lynn and others in the Department, about who was going to control the department's analytical agenda. Larry Lynn won and this permitted me, as his deputy for health to have a much bigger role in the departments health policy and budget decisions. Working in another section of HEW was a new employee of the Department who had just graduated with his Ph.D. from Johns Hopkins, by the name of Bob Blendon. Blendon didn't have a

home. So, I had him join our small staff. Blendon as you know has gone on to become one of the countries foremost authorities on healthcare policy. He was the one that convinced me that I should become involved in the Economic Stabilization Program to control healthcare spending. I was trained in economics at a market-oriented school. We don't regulate. Regulation is bad, it's evil, and it's the devil. He convinced me that the health care system didn't function like a market. Before I knew it, I was in the middle of the most extensive program to control healthcare costs ever undertaken by the Federal government. Then, Blendon left to help start up the Robert Wood Johnson Foundation.

Blendon was gone, and I was there. I had a great time. I had two jobs, and we began to fund a research agenda to find out how our health care system really worked?

If you look at the history of health care economics and health care research, there was a small cadre of health economists that were engaged in research in the 1960's. In 1969, they had their second meeting. A professor by the name of Herb Klarman was from NYU, Paul Feldstein was at Michigan. It was a small number. We had our first meeting when I was still at Brown.

But, health care research was not dominated by economists. There were many more health services researchers who came out of health administration. The economists studying health care were limited, to say the least.

TAPE 1, SIDE B

SA: My staff at HEW was in charge of what was called evaluation research. The Congress had established an evaluation budget for all HEW programs by requiring one percent of all appropriate funds to be used for evaluation research. NIH was furious that one percent of their money was going to evaluation. They said, "You can't evaluate research" "You shouldn't be doing that." We were beginning to question how they were spending their money on things like the war on cancer, which was established in 1971.

I had this crazy guy who questioned all the research of the cancer people. He was a Linus Pauling devotee and he was trying to convince me to take on the NIH on how they did research.

There was the FDA, an independent agency. We also questioned them. It wasn't only economics; it was NIH, FDA, and Medicare and Medicaid. We were beginning to push all of these groups. I can't say that we were always right. But, we became a force to promulgate evaluation research throughout HEW. One of the biggest research efforts of all time became known as the RAND experiment.

TB: How did Joe Newhouse get involved?

SA: Joe Newhouse was a young economist at the RAND Corporation. OEO somehow got involved under President Johnson. OEO had developed neighborhood health centers, and they were running their own health delivery system out of those centers.

A young economist at OEO, not a health care economist named Larry Orr

was convinced that we didn't understand the impact of financial incentives on how people use health care. Does it matter whether your health insurance includes deductibles and co-insurance? Being an economist, he tended to believe that if a patient had to pay for at least some of their care, it would influence how much they used. For healthcare professionals of that day, this was hard to believe.

He has contacts with the RAND Corporation. I believe it was a sole-source arrangement--to help design what became known as the RAND Experimental. It is almost impossible to believe that in the early 1970's the Federal government spent almost one hundred million dollars on this study. The study was headed by Joe Newhouse. It was designed to find out one simplest thing: What is the impact of deductibles and co-insurance on the demand for health care?

The reason why it was so expensive is they bought different insurance policies for people who paid for them, some of which were completely free, some others had different types of arrangements. Essentially, you gave people the equivalent of money. If I gave you a big deductible, I would give you an up-front payment so you're no worse off, but you would have a different set of incentives. I say, here's a \$1,000 deductible. I'm going to give you the \$1,000, but you don't have to use it on health care. You have a different set of incentives. That's why it added up to so much money.

RAND began to play around with this experiment. At the same time the Nixon administration decided that they wanted to close down OEO. They took all their health programs and they moved them into HEW. The first thing that

happened was the experiment came under my office.

I looked at it and said, “What a lot of money.” I knew the answer. It’s no big deal. The elasticity for hospital care is higher for outpatients care, then inpatient care and probably both have an elasticity of less than 1.0 (a ten percent increase in price would generate less than a ten percent decrease in the quantity used). Some at HEW said, “Let’s just shut it down. Let’s not do it.”

Larry Lynn, my boss, was also an economist. He and I were not sure we should end the experiment. “Maybe we should look at it.” So, we brought in the individuals from OEO and the researchers from RAND, including Joe Newhouse and discussed how to add more value to the study.

We started looking at it, and realized there was a lot to be learned from such a study; wouldn’t it be nice not only to learn about the demand for care, but what do people use and what they don’t use.” Following that meeting, the RAND team added a very capable physician to look at health care use. His name was Dr. Robert Brook.

Based on a redesign of the project, we allowed the National Health Insurance Experiment to go on. I think it’s fair to say that the health utilization part turned out to be, in some respects, more important than the financial component.

Joe Newhouse made a name for himself. This phenomenal guy, very analytically sophisticated, was part of a group that had been trained at Harvard under Professor Martin Feldstein, half a dozen of them are really the crème de crème of health economists today.

TB: Relate some of this to what sometimes is said about the development of the field of health services research in this period, where economics really took over that field as well. One of the critical figures was supposed to be Rosenthal, who . . .

SA: Jerry.

TB: . . . had been, I think he may have been Joe Newhouse's advisor at Harvard. In any case, he came from Harvard and became head of the National Center of Health Services Research.

SA: He was here at Brandeis.

TB: He was here?

SA: He was here. He was in the economics department at Brandeis.

TB: Oh, okay. I got that part wrong. He did, in any case, come to the center and, in an article that Jan Fox writes at the time, talks about a priority-setting process, trying to determine what should be the main issues in health services research. What seems to emerge from that description is that there was a real conflict between the older people who had been the first health services researchers and the newer economists.

SA: That's right. During the Johnson era, in the late '60s, the National Center for Health Services Research--I forget what it was called, but it was close to that--under Sanazaro, was formed and given a huge budget. It took us thirty years to get back to those funding amounts. Sanazaro, who was an old-line powerful health services researcher, was in charge. He was a very autocratic guy. He developed all kinds of research that ultimately led to the establishment of the Health

Planning Act, the Regional Medical Program, etc. The Federal government used its experimental authority 314E to establish many of these programs. It all was done in the late '60s. Jerry came in--I don't remember exactly when he came in but it was much later than 1971. You'd have to look.

TB: Seventy-three or '74.

SA: If you look at the structure of HEW at that time, you had the Secretary, and then you had several Assistant Secretaries. One was for Planning and Evaluation, and I was his deputy for health. Then, another one was the Assistant Secretary for Health, and under the Assistant Secretary for Health were all the healthcare agencies. The center that Jerry Rosenthal ran was in one of those agencies.

Secretary Elliott Richardson was a very, very good manager, which meant everything flowed through him. It meant everything in health care flowed through my office, because he would not sign off on anything unless he got our assessment.

We were a fairly small office. Jerry Rosenthal and I developed a relationship independent of all these other lines, because he was a bit of a crazy guy, but smart; and I liked him. We became good friends.

He didn't have anything to do with the RAND experiment, until later. The RAND experiment--it started in 1969, the changes were made in '71, '72. He didn't come until '73, '74. It was pretty far along. I'm not saying he didn't have a lot to do with it later on. I was much more involved in policy and much less involved in the actual flow of the dollars flowing out to the research community, than Jerry was.

No question about it. Except, his budget was determined indirectly by my office. Not because I was so special, but you had a budgeting process that require all the agencies budgets to be approved by the secretary. And the budgeting office had to get approval here, which meant they came to us. So, we had a lot to do with each other during that period.

Jerry had a couple of able people that worked for him. I wouldn't diminish his role, though he was much less involved in the policy research environment and much more involved in structuring the research involvement, and he was a force.

TB: That story is told but I don't see it. Even though he has some success in at least saving the field as it begins to form [unclear] the costs, he doesn't have anything directly to do with the RAND study, but he uses its legitimacy to gain some for the agency.

SA: Exactly. That is very true. Yet, so does everybody else.

TB: There were a number of budget cuts, and training grants were cut. Part of the same story is that health services research survived because of a number of other factors. Blendon, for example, at the Robert Wood Johnson Foundation started supporting health services research. Other foundations started supporting health services as the government funding was going away. And all the health government agencies, for example HCFA, when it comes on, began to incorporate health services research in some of their program evaluation. So, the field survived even if its principal agency was on the ropes.

SA: That's right. It survived as much because as we move into the '70s, a lot

of things were happening. The advocacy groups began to realize that they needed to have an analytical arm because they had to convince government. If I have had any influence over the years, it was establishing an analytical arm that had a lot to do with policy. Once you do that, you establish analytical research arms to combat you all over the place that didn't exist before. The AMA created a much bigger research group; the AHA created a much bigger research group.

When Medicare was established it had a small research group under a very capable one woman named Dorothy Rice. She was a wonderful source for information about our health system. Her office did all the data analysis for the Economic Stabilization Program in '71. We became very good friends. Her office was like a little gem and very important.

The Assistant Secretary for Health also developed a planning and evaluation arm out of this office. He recruited a senior official from the Kaiser Health plan to run the office.

NIH, which had no previous interest in health services research, also began some fledgling efforts. After my tenure in '76, Karen Davis took over the office of planning and evaluation. She expanded it tremendously. It became a much bigger office under Karen than it was under me--and again much bigger, under Califano. So, you're right. The '70s and '80s were very different eras than the '60s and '70s. But, it was in our era that this transition took place.

TB: Do you remember how you thought of yourself at that time analytically? Were you an economist, applied economist, were you a health service researcher?

Was that the better term?

SA: I definitely was not a health services researcher. I was trained in market-oriented economics. I thought like an economist, less like a health services researcher. I wouldn't typecast myself as a researcher per se, but more as a facilitator of research.

For example, when I was in the Economic Stabilization Program I brought in economists to figure out what we needed. How do we control health care costs by using markets versus regulation? I brought all the economists of the day in, and we had a two-day meeting, and then we supported new research to find out what we should do.

We began the grant process that eventually led to the DRG system, because the hospitals were screaming that every hospital was different and you couldn't regulate them, because one was a cancer hospital and the other was a heart hospital. We said, "Well, isn't there any way?" There was some work being done at UCLA and at Yale, and so we began the process of finding out how to combine hospitals that provided care to different types of patients.

TB: Let me fast-forward, and then we have to think about our time.

SA: You've now got me here.

TB: Later in your career, you received major awards as health service distinguished investigator.

SA: Right.

TB: Now, is there a transformation that occurs between the '70s and the

'90s? Or is this sort of a retrospective award?

SA: I think it's more a retrospective. I continue to be involved in this.

The reason why I got the award that was just given in June--if you read Blendon's remarks--what he attributes to me and to Uwe Reinhardt, who is very different than I in certain respects, but we see eye to eye on most issues, Uwe was never in the government, but Uwe is much more the social critic and writes much better than I do. I was the transmission belt. I understood the research; I tried to apply it to policy. I goaded the research community to do more things.

I also made it respectable for research to be done. That's what he said, and there's probably some truth to that.

Was I the quintessence of a researcher? No. Do I spend my life doing research? No. Do I do research? Yes. But, I'm more the person that the policy community turns to so that, in 1982, when they set up the DRG, the Prospective Payment Assessment Commission, I became its chairman. First of all, I was acceptable to the Republicans. They still remembered me in a Republican administration. By that time, I was beginning to morph into a Democrat, so I was acceptable to the Democrats, too. I chaired ProPAC for twelve years and we established a very significant research component as part of the Commission.

When President Clinton was elected, I became the head policy advisor to his transition team in '92, until we had a splitting of the ways. Nevertheless, Clinton appointed me to the Bipartisan Commission on the Future of Medicare.

I'm much more of an active player. I wouldn't call myself a politician.

There's the role in the middle, to use the research findings, to understand data, to promote it, but not to promote it just for its own sake but as an arm of making policy work better.

Does that make sense?

TB: Yes. In fact, one of the things really notable about your career was the tensions, the pulls between the government and the academic life, and clearly wanting to find some middle position. This position here as dean for sixteen, seventeen years is really not an academic position as a position in the economics department would be, because there's a very important policy dimension to that and you're training people to work in the world and not just to do research.

SA: That's exactly right. I wasn't going to go back to an economics department. I was not going to go back to that world of traditional economics. Traditional departments of economics eat up policy analysts. You can find and count on one hand health care economists that survive in an economics department. They get killed, because traditional economists think of themselves as above the policy environment. Policy is dirty, it's not theoretically pure.

David Cutler comes closest. Joe Newhouse even has three and four appointments. He's in the Kennedy School. Uwe is much more in the Woodrow Wilson School than he is in economics. I'm here. You can go down the line.

I was not going to go back to an economic department. I also was not going to go back to academia unless I was in a very senior position. Heller school, on a lot of levels, makes absolutely no sense for me to be here. It was originally a social-

work school. But, it has become much more and now one of the leading social policy schools in the country.

TB: How did it happen that you came here?

SA: It was one of these flukes again. When I left government in '76, I finally decided I had done it long enough. I could see the handwriting on the wall. I wasn't necessarily smart enough to know that the Republicans were going to lose and Carter was going to become President, but I had gotten tired.

TB: So, you left before the change in administration?

SA: Yes. I left in the summer of '76.

TB: And you went to Berkeley?

SA: I was looking around for something to do, and Berkeley offered me a visiting professorship in the School of Public Policy, which was great. For the first time in a long time, I had no administrative responsibilities. I could teach a little bit. I could wander around and talk to students and have lunch. I hadn't done that since Brown. Berkeley was wonderful. I call it my Shangri-La year. It was just wonderful.

While I was there, I had to decide what my life was going to be like. The School of Public Health offered me the potential of becoming a professor there, but they were dragging their feet. I got offered the chairmanship of the Department of Community Medicine in Seattle at the University of Washington. My friends in Washington DC said, "Come back and become a consultant." And a colleague of mine at Berkeley, who was on leave from Brandeis University, said, "Hey, there is

this deanship open at a school called the Heller School at Brandeis University.” So I looked into it and they and I took a chance, and I think it worked out well for the school and I found a home.

This is a very interesting place. Even though it has a long tradition of social work, it’s always been an interdisciplinary program with a strong economics focus. While it might look strange for someone like me to become its Dean, it wasn’t that strange. If you go back to the history of the school, Brandeis would not allow a social-work school on its campus. It didn’t want any professional schools. The only way Heller was accepted by the board of trustees was if it was social science applied to social policy. It always had a strong economics component.

So, I came in as the dean, and I created a health group. I convinced Stan Wallack to leave Washington and join me. Both of us have been at Heller for almost 30 years.

TB: You created the Health Institute at the same time . . .

SA: Well, after I was here about three or four months, there was a national competition to create several health centers at different Universities or think tanks. That’s when I brought Stan up here. I said, “Stan, would you run it?” He said yes, and we formed a consortium between Brandeis, Boston University Medical School, and MIT. We competed very successfully, came in second place to the University of California, Phil Lee’s group. They went to work for the national center, which was where the competition was. I convinced HCFA that they should have their own policy-analysis group and that our group and Northwestern, which came in second

and third, should become their policy-analysis group, and it worked.

It worked, and we started bringing in more health services researches, and the rest is history. Heller is now one of the biggest health policy research groups in the country. A lot of the credit goes to Stan Wallack. He's a great thinker about what types of reseach is needed to improve our healthcare system. So, that's it.

But, this place has allowed me to do just what I wanted. I was never going to be a full-time administrator. I watched too many full-time administrative deans get eaten up.

TB: This was understood at the beginning?

SA: No. No one understood anything. Sometimes you're lucky and sometimes you make your luck. I'm not sure which applies here. By '76, I was pretty well known, and I was appointed to several important national committees. I was appointed to the IOM.

In deciding to return to the east coast, I said to myself, much as I love working and living in Berkeley, it was too hard to commute to Washington every few weeks. I made two decisions that turned out to be right. One, if I'm going to play in Washington DC, I want to be close to Washington DC; being in California or the west coast was just too far. That's why I turned down Seattle. People have done it, but it's murderous.

Second, I can play a much more active role by being close but not in Washington. So, you know, I started being pulled into many interesting activities. And, of course, I didn't know that ProPAC was going to be created and I would be

asked to be its Chairman.

TB: Could you tell me how that did happen?

SA: ProPAC?

TB: Yes.

SA: The history of it was that the research that had been started during the Economic Stabilization Program to better understand how hospitals differ in terms of the patients they treat and the resources used. Researchers at Yale developed an analytical model to combine the various types of resources needed to treat patients in a limited number of categories called Diagnosis Related Groups.

TB: Did you do any of that research in the late '70s, early '80s?

SA: No. Some attribute the DRG system to me. I can take absolutely no credit, other than I set up some of the original research and shepherded it through once it got passed.

So, the Yale team was doing their research, and it was strictly a health services research activity. They understood how the health system worked, how hospitals worked. In the late 1970's the state of New Jersey adopted a modified version of the Yale DRG system for their hospital regulatory pricing system.. Then the Reagan people came in and they wanted to change the Medicare cost-based reimbursement system of the '70s. They pushed to redesign the Medicare hospital payment system by incorporating an expanded version of the Yale DRG system.

I had been involved in a lot of work on health cost controls over the years beginning with, the Economic Stabilization Program in 1971. For most of the 1970's

most of the cost control action focused on various forms of government regulation.

But, by the end of the '70s, the Reagan people made it clear that they believed regulation was dead. It's blown its cover, too many lawyers get involved. Economists were writing that it's a useless effort, that it costs people money. The regulated became the regulators.

In some of the early legislative discussions, the Reagan administration wanted to form a committee to do technology evaluation to decide how to operate a DRG type system. But staffers realized that very few people really understood the system. At the last minute, they put it into the legislation that there would be a new Federal agency called the Prospective Payment Assessment Commission and that its members would be appointed by OTA, Office of Technology Assessment, to keep it independent

OTA was responsible for setting up ProPAC, and OTA looked around for someone to head the commission. Apparently, I had been recommended but I didn't know anything about it. And then, out of the blue, they asked me to be the chairman. I didn't lobby for it; I didn't realize anything was going on. But, I was a good compromise candidate that had been in a Republican administration. Senator David Durenberger, a Republican Senator for Minnesota, was a major force in the creation of the DRG system. He knew me and I guess thought well enough of me to get OTA to appoint me as its Chair. I stayed as chairman from 1982-83 until 1996.

The reason I got bumped was because the Republicans took over the Congress in 1994, and they wanted a change in leadership. The Republicans said,

“we want to streamline government, so we’re going to combine ProPAC and PPRC, and Stuart’s been around too long.”

TB: Was that John Eisenberg in PPRC?

SA: No, not in the beginning. Phil Lee was the first chair.

TB: Then Eisenberg?

SA: And then Eisenberg, yes. Then, Eisenberg was replaced and Gail Wilensky took over. The Republicans pushed Eisenberg out and the Democrats were furious. Then, the Republicans pushed me out in ’96.

TB: Would you say that your relationship with the field of health services research is essentially as a sponsor and patron?

SA: And user, and also, as a participant as a researcher. I would put my activities as a researcher in a modest category. People always turn to me as the person to explain how research is used in policy; what kind of research should we be doing. How should we be formulating the research? How do you use research as opposed to how do you do it? There are better people more interested in theory of research techniques than I.

TB: That seems essential to the field. One of the things the best researchers in the field always worry about is that people aren’t paying attention to them,.

SA: That is a whole other story we can discuss about the role of research in policy. It has a role. It is not the only role. But, it’s a legitimizing force. Everybody has to have their research findings to support their arguments.

When I was in the Pentagon in the beginning, the generals sat around and

they would say, “This is what we do.” “Why do you do it that way?” “Because I say so. I am a general, I’ve been there. I believe this is what you do.” “Well, what kind of evidence do you have that your position is correct?”

Doctors would say the same thing. “I’m a doctor. I’ve been there. This is how you do things.” “What evidence do you have that that works?” “I don’t need evidence. I’ve been there.”

We talk about evidence-based medicine today. “What do you mean by evidence-based medicine? They’re all scientists.” It turns out they’re not. They’re practitioners.

Policy is determined by advocacy groups, by lobbying, by pressure, by politics, but everyone has to have their research to justify their position. That doesn’t mean research wins. Everybody has their research findings, but, some of the findings from different groups are better than others. They often build on the same research base but manipulate the findings to justify their position. But, you can’t play without having a research base. Sometimes, research results have a lot to do with the outcome, other times much less. You watch how legislation gets made, and how it gets manipulated by the policy process, by the lobbyists, and just by raw politics. I’ve watched it play out over a long period of time. We are a very different health community than we were in the 1960s--not necessarily better, because now you have so many health researchers that they often balance each other out. You wonder whether we wouldn’t be better off if we had less research going on.

TB: Should I end? I know you have an appointment waiting for you.

You've mentioned Uwe Reinhardt, and I notice that you've edited three books with him. My sense in reading his writing, knowing him slightly, and reading a number of your papers, is that there's a great deal that you share in terms of attitude.

SA: You're absolutely right. Uwe received his Ph.D. the same year I did. I take credit for convincing two people to come into the health field. One is Uwe Reinhardt and the other is Gail Wilensky. Both of them were not health economists. Neither was I, but I was there a week before they were, and convinced both to become health economists. When I left the Urban Institute to go over to become deputy assistant secretary at HEW, I convinced Gail to switch from studying welfare issues to health care.

Uwe and I both graduated in '64, and he is more of a moralist than I. His writings are on what's right and what's wrong. He does it as only he can do it, playing the immigrant and the foreigner. He's also a contradiction in terms, a very funny German. I think of myself as having a good sense of humor, but his is much better. He's a better speaker than I am and he charges more than I do. But, I love him. And you're right. We do see the world in similar ways. We believe in the market, but we don't. We both see ironies in how our health system operates. We get uncomfortable when the situation becomes too much dominated by "I'm the boss," "I'm the doctor," "I know best."

On the other hand, we are very intolerant of what I would call the arrogant researcher; particularly the economist who comes flying in from outer space and

doesn't know anything about health care but makes pronouncements.

Over the weekend, I read an interview from Michael Porter of the Harvard Business School. He's now the new guru. The business community is looking to him. He's a very smart guy. He's decided he has solved all the business problems of the world, and now he's going to solve the health care problems. Both of us look at his writings and say, "Not so fast. Health care is a lot more complicated than you think." So, yes, we are similar.

It was very fitting I think that AcademyHealth chose to give the award to both of us at the same time.

TB: You enjoy working together.

SA: He's wonderful. He's great, yes. I liked both of them. Gail's a good friend. We now fight each other. I'm on the Kerry health group and she's for Bush. I am sure we're going to be debating each other more and more as the campaign heats up.

TB: Any last thoughts that you'd like to share?

SA: No. I appreciate the opportunity to discuss my life.

TB: I enjoyed it.

SA: I hope it's of some use.

TB: I'm sure it will be.

SA: I've been very fortunate. I was there at the right time. I was particularly pleased that AcademyHealth gave me the award. Thank you.

TB: Thank you.

END OF INTERVIEW