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Interviewee: David A. Hamburg

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Interviewer: Mary Marshall Clark

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Q: Dr. Hamburg, thank you so much for being with us today.

Hamburg: It's a pleasure to be here.

Q: Thank you. I'm very much intrigued by the ways in which your early life experiences have influenced your evolution as a thinker, as a scientist, and as a director of the largest foundation in this country, really, general all-purpose foundation. Could you tell me a little bit something about your grandparents, your parents, where they came from, what were the important things about your identity that you gleaned from them? Start anywhere you want.

Hamburg: My grandfather came here -- it's a classic immigrant story -- in 1900 or 1901, I'm not sure. Pushcart peddler. Came first to New York, where there were too many pushcart peddlers. Went on to Cincinnati, where he had a cousin. That was better, but, still, there were quite a few. So he heard about a town up the Ohio River called Evansville, Indiana, and he went there. I believe he was the only pushcart peddler at the time.

My father had been born, as his first child, shortly before he left Latvia, so he was very eager to bring his wife and baby to the United States as soon as possible. So he got established in Evansville, Indiana, having really no formal education and knowing no

English. It's an irony that the way he got established was that his Yiddish meshed with the German spoken by a lot of German Americans in Evansville, and he made enough money to bring my father and his mother over about a year after he came. So that was part of the family legend, the courage. Put yourself in his position, a man without knowledge of the culture or the language, and no obvious way to make a living, and yet such ingenuity and dedication to do that.

So he was a very important figure for me, that whole story of how he came, and his commitment to bringing relatives. His belief was that Europe was not going to be a good place for Jews in the twentieth century, at least the early part of the twentieth century. He had this wonderful classic vision of America as the land of opportunity, so he made a few bucks and brought some relatives, made a few bucks and brought some more. He brought fifty-some to Evansville before World War II finally shut it down. It was a very moving occasion, I remember, at his funeral. They all came together, and I think they had a real sense, literally, they might very well not be alive, and certainly not in the United States, had it not been for him. So that was very important to me in a number of ways: I think the sense of what this country would stand for at its best, the opportunity, the democracy.

Then another corollary of it was the sense of family solidarity. They were all together in this small town and there was a very strong mutual support and mutual aid ethic. I grew up with that. My grandparents had a place on Second Street in Evansville, which was known in the family as just Second Street. You never used any names, it was just Second Street. And it was an open house, in effect. We would drop in a couple nights a week, and there would be some set of relatives there, a different configuration every time, but there was never any question that there would be people there who were interested in

each other and would help each other in any way possible, and a lot of humor and news of the day and so on.

My grandmother would hold forth about the news of the day. She got it from her Yiddish newspaper. I think it was a little bit behind, a few days behind the actual news, but in any case, that family solidarity certainly sensitized me to the value of social supports in life and later on in medicine and public health, child development, adolescent development. That certainly became kind of a red thread running through much of my work -- the value of social supports.

At a much later time, my wife, when she was Director of Studies for President Carter's Commission on Mental Health, working in the White House, got some colleagues together to put together a really first-rate scholarly paper on social supports, research evidence of what social supports did in relation to health, and really gave that whole field a big boost. But in any event, that's part of my heritage that was important.

Then I think another aspect of that was the premium on education, again a classical sort of belief within the immigrant culture. But it was real and vivid, that the way to get ahead was to get education. Everybody in the family, lacking formal education, had great intellectual curiosity. My father's claim to fame was he was the first member of the family to go to college. He was a rather shy and not altogether healthy child, but a perceptive teacher saw that he was gifted and helped him get a scholarship to the University of Chicago, and that, too, was part of the family lore. It sensitized me to the significance of the teaching profession. When I got to Carnegie, I finally had a chance to do something about that, take some steps that might tend to strengthen teaching as a profession. But

I'm sure that story about my father's experience, and some of my own personal experiences with teachers, influenced me very much in that orientation.

So those are a few examples of ways in which my early life sensitized me to certain themes that became important professionally. I guess I should add a couple more. One had to do with the status of medicine. There was a respect bordering on awe for the profession of medicine in the family, even though medicine really didn't have much to offer in those days. When I got accepted to medical school, to jump ahead a bit, our family doctor said, "Don't let it go to your head. All we have to offer is aspirin, morphine, and prayer." That was pretty much true. But despite that, there was this great respect for medicine.

My father himself had wanted to be a doctor, but my grandfather needed him to come back and help in the family business, to make money to bring the relatives over. They never accumulated wealth appreciably, but to bring more relatives, that was the passion. And my father did that. He always regretted that he hadn't had a chance to pursue medicine, so there was a sense in which I grew up with that and was going down a path that he had valued.

The other thing I would say is about intergroup relations in Evansville that was quite interesting because clearly the Jewish community was very sensitive to discrimination. It was still the era of, for example, quotas for medical schools and other quotas too. But, nevertheless, we were, on the whole, well treated and well accepted in Evansville. It gave me some sense of what was possible, despite intergroup tensions.

It cut two ways. One was, it heightened me to the meaning of social depreciation by virtue

of any kind of intergroup categorizations, the whole ingroup/outgroup problem which became very important to me professionally later in my career. No doubt there was sensitivity to that by virtue of growing up with the emerging Holocaust in the background. But there was also the sense of what was possible, how people could learn to live together amicably in this relatively small town and, in some ways, a tough industrial town which was not short on intergroup tensions, but, nevertheless, there was, on the whole, a kind of accepting climate. So those are some of the strands of early experience that bore on my later professional interests.

Q: When you talk about intellectual curiosity and the kind of intellectual curiosity that was manifested in your family, can you trace the development of your own intellectual curiosities and how it meshed with those? What did you discuss around the house in terms of what was happening in the world, what was happening in Evansville?

Hamburg: Certainly my father and his side of the family, generally, certainly my father had very strong intellectual curiosity. My father had lots of books. Even during the Depression, he had minimal income, he would always add to his book collection. I've been the same way, only more so, throughout my life. My wife says that I don't understand that there are libraries. After the Andrew Carnegie connection, I understand there are libraries. But I think if a book is worth reading, it's worth having and underlining and writing in the margin and so on. Anyway, I'm sure I got that from my father.

The part of it that really changed and was totally unexpected was turning on to science, and that came in college, with a busman's holiday. Well, I should explain. I was in a wartime speed-up program. I was in college during the war, headed toward becoming a

doctor. The government wanted as many doctors as rapidly as possible. There was a speed-up program, and the Army had a program, the Navy had a program. I was in the Army program, where they would pay for your education and, in turn, you would be available to be called up as a medical technician or, if you got through, called up as a doctor as soon as possible. In that wartime speed-up program, I in some ways felt I was missing the richness of the university education, because I had to concentrate on the pre-med courses so strictly, and I wanted very much to branch out.

But the busman's holiday was, I took a course in genetics, more biological science. But I heard such wonderful things about the professor, a man named Tracy [M.] Sonneborn, who was one of the pioneers in modern genetics. It was a non-field at the time. It was not recognized at all. It's hard to realize today that it's become such a preeminent field, but it wasn't then. Sonneborn was one of the people who made it an exciting field. In sensing the enthusiasm as well as the content, I got turned on to science, and that direction of intellectual curiosity was unexpected, and, again, shows at a higher level, a more advanced level, what teaching can do. It changed my whole life.

Q: Looking back a little bit, though, when the Holocaust was beginning to happen, what did you think about it? How did you respond to it, both intellectually and emotionally? How do you think that characterized your work later on?

Hamburg: It certainly influenced me probably much more than I realized at the time. There was a great perplexity to it, because I perceived Germany as one of the most advanced and civilized countries in the world. If you wanted to do advanced work in medicine or in many of the sciences, you went to Germany. There was a great tradition in

music and in many fields. So there was this enormous perplexity. How could a country that was in some ways so advanced be so incredibly barbaric in other ways? It's been a conundrum for me throughout my life. Oddly enough, this summer I came back to some reading, with the guidance of some of the leading historians of Germany, in order to try to say, could World War II and the Holocaust have been prevented? This is in the spirit of our Carnegie mission on preventing deadly conflict. But it led me to explore more deeply what did happen in Germany and some of the factors that were conducive to that awful turn of events.

But I guess I felt at some stage that if it could happen in Germany, it could happen anywhere. I simply couldn't believe that that was unique. It somehow was a part of human propensity that certain conditions would bring out, could bring to the surface. But it certainly influenced my interest in aggression and conflict and conflict resolution, heightened the awareness of the dangers basically of the historic paths of humanity, where there is so much slaughter and all that, that in the future we were going to have to find ways to deal with. It's a practical matter that as the killing power would increase, that we'd simply destroy each other unless we could find better ways to deal with ubiquitous tendencies to human conflict. I think that was a legacy it left me. I can't give the landmarks at which various things happened, but it was certainly a powerful shaping influence on my interests and my attitudes, I'm sure.

Q: So you went to college and then medical school. What led you into psychiatry, given that you had also this interest in genetics and certain kind of very specific issues in science?

Hamburg: Right. The evolution of my interest in psychiatry was in the fourth year of

medical school, in a course in psychiatry. In connection with it, I read Freud's general introductory lectures. I don't recall, it was probably on an optional reading list for the course. It certainly wasn't central to the course by any manner of means. But I found it quite fascinating that he was dealing with issues of motivation and emotion in human relationships, and he was also dealing with the influence of early experience on later behavior. That seemed to me just so fundamental, so fascinating.

I should say that the family was, to a certain degree, introspective. I think it's not altogether surprising that I would have been interested in psychological matters, because, for whatever reason, the family was kind of oriented that way. People tried to understand why others behaved as they did and so forth.

But then I got very intrigued with the concepts that Freud put forward. I didn't have the foggiest idea of how that would actually apply in practice in the field of psychiatry, and, indeed, what little I saw of it in Indianapolis City Hospital was not very attractive. In some ways it was more like a jail than a hospital. There wasn't very much that could be done at the time. Like I was saying about medicine in general, psychiatry had very little to offer. This was 1946 or so, maybe '47.

But then I read a book called *Men Under Stress*, by Roy [R.] Grinker and John [P.] Spiegel, which was their wartime experience, trying to understand the dynamics of emotional reaction to stress in combat. I found that really fascinating, and it suggested ways in which you could deal with the stress of combat, help people overcome the traumatic reactions they were having. So that added a kind of dimension of hope about the therapeutic side of the field.



I made up my mind to try to get to go to intern at the Michael Reese Hospital, where Grinker was working. He came back there after the war. I was lucky enough to get to go there and to get to know him, and he became a great shaping influence on me, and we became good friends until he died a few years ago. But that sort of translated into action the kind of concepts that I had come across in Freud's writing. Now, it all got very much more complicated later on, certainly, but in any event, that's how the interest evolved, I guess I'd say some sensitizing to psychological issues through the family, through introspection, and then the concepts of Freud and then the operational procedures and therapeutic interventions that Grinker and Spiegel had pioneered during the war.

Q: How did you move into an intellectual interest in stress itself? Can you talk about your own work, your own scientific pioneering work in that regard?

Hamburg: I had been turned on to science by genetics, as I said, and I wanted to hold onto that if I could. I had gone through a kind of transition of interest during the medical school years, from genetics -- this was single-cell genetics -- to cellular physiology, to general mammalian physiology, and then to behavior. And as I got into the field, I was looking for some way to connect those, and eventually did. For whatever reason, I've always had a kind of broadly integrative turn of mind, wanting to draw together ideas and information from different sources, from different disciplines, whatever, and see if I could try to make a coherent picture.

Now, in psychiatry, during the internship year, Grinker encouraged me to go to Yale for a year. He had no openings. He had a certain number of slots, and he didn't have any slots for that year, but he encouraged me to go to Yale for a year, then come

back to Chicago. And that's what I did. Early in that year at Yale, a very interesting Hungarian émigré to Canada, now Canadian, endocrinologist Hans Selye, gave a lecture in which he showed that a variety of conditions -- I forgot how many, fifteen or twenty -- different highly stressful conditions caused a great enlargement of the adrenal gland in rodents. Many of them were fractures and burns and so on that were clearly severe physical stress, but there were a couple that could be interpreted as psychological stresses, an immobilization condition which simply tied the animal down for a number of hours, and periodic sounding of loud noises, what appeared to elicit alarm responses on the part of the animals.

Those two conditions intrigued me very much, the question of whether psychological stress could have an impact on the adrenal gland. There was already a fair amount known about that with respect to one part of the adrenal gland, the adrenal medulla, which secretes adrenalin and other adrenalin-like hormones, but there was essentially nothing known about the other part of the adrenal gland, which is biochemically quite independent, the adrenal cortex that secretes the steroids, the cortico steroids, such as cortisone, the cortisol type of hormone.

So we set out to investigate that, to see whether psychological stress in humans could stimulate both the adrenal cortex and medulla. I spent some years then working on that sort of problem. It turned out, to my surprise, that there were not precise, reliable biochemical methods for measuring those hormones at the time. There were bioassays, which, although highly sensitive, were not so specific. So we spent years trying to get better methods both on the biochemical side and the psychological side to measure quantitatively different reactions to stress, first doing it in naturally occurring

circumstances and then in experimental situations, and, eventually demonstrating beyond a shadow of a doubt that the adrenal gland in both of its parts, the medulla and the cortex, respond strongly to psychological stress in humans.

Along the way, we made what I suppose was the most striking or dramatic discovery I was ever associated with in my career, and that was the discovery that the adrenal cortex also responded very powerfully when people were depressed, even if they were sitting, huddled, not moving, in psychomotor retardation, as the jargon went at the time, where you'd think it's almost like hibernation. But the alarm systems of the body were clearly responding physiologically, biochemically, under those conditions. It was counterintuitive. We actually repeated it in a different way before we published any of it, because we didn't trust our own findings. The prevailing conventional wisdom at the time was that, if anything, the alarm responses were dampened in that kind of depression. But it turned out that regardless of whether the person was overtly agitated or not, that the depressive emotion was associated with the powerful stimulation of the adrenal cortex.

A lot of very good people have made a living on that over several decades in elaborating and tracking down the implications of that discovery. The technical capacity to evaluate these systems has improved enormously since we made that discovery in the 1950s. Basically it wouldn't be too much to say it's filling in details, very important details, but the fundamental fact of the alarm systems being mobilized in depression, as well as other conditions of emotional distress, was something that we were able to establish.

It was an exciting time, and I did some of that work in several different places over a couple of decades, with a variety of colleagues. It was all interdisciplinary collaborative work, but

I perhaps provided a certain intellectual spark and a certain cohesiveness. One of the things that I've always enjoyed doing, and got to be pretty good at, was to elicit cooperation in people who bring different skills to the table, and getting a good collaborative enterprise going where people work together to get around the contours of a complex problem in a way that no individual could do alone.

Q: Along those lines of multidisciplinary, didn't you go back and do some further research in psychoanalytic theory, at the Chicago School?

Hamburg: Well, yes and no. The role that I played there, I guess, mainly had to do with coping. I was called in the Army again when the Korean War broke out, and I spent the whole Korean War in the Army, roughly half of it in Texas, Fort Sam Houston, and the other half at the Walter Reed Army Research Institute in Washington, which was then new. At least it was undergoing a phase of expansion and, for the first time, had a neuropsychiatry part to it.

Anyway, during the part in Texas, the Army had then, still now, today, the Army Burn Center was located at Fort Sam Houston, and it was always -- it has been, to the present time -- right at the cutting edge of research and treatment in the burn field. A surgeon and internist who were running that burn center asked me if I could see some of their patients, to help them clarify the problem they were having. I was not assigned to the burn center. I was running a big psychiatric service, in fact, at any given moment, 200-plus patients, so I was kind of running myself crazy at the time. But I was fascinated with this. Evenings and weekends, I would go over there to the burn unit, the burn center, and try to

make sense of it. The point was, they were at that time pioneering an open treatment of burns which required a lot of cooperation by the patients, and some of the patients were not cooperating, and that was the problem they wanted me to look at. When I looked at it, the thing that astonished me was that most of the patients were cooperating and were somehow coming to terms with this terrible injury that they had, second- and third-degree burns over much of their body surface. Some of them were like charred remnants of human beings. But yet most of them were adapting, were cooperating in this treatment. That was the most interesting part. I had no idea how to understand the situation.

I asked their permission to interview all the patients and all the staff over a period of time, to try to build up some context for getting at least a modicum of insight into this question of cooperation or non-cooperation. It turned out I was able to be helpful to them. I'm very happy about that. But mainly it led me to try to do some fairly systematic studies of how was it, in fact, that people come to terms with such severe injury.

I first went to the literature, the research literature, to try to get guidance. In a way, I hit the panic button. There was virtually no literature. Whether you took it from the standpoint of coping, adaptation, problem-solving in highly stressful conditions, or whether you took it from the standpoint of burn patients, there was a little here and there, but not much. So I really feel very lucky that I was able to play some role over a period of years in developing a field of coping studies. We even, I remember, had considerable hassle about what to call it, what term to use. Should we use coping, should we use adaptation, sometimes we used both, whatever term it was, to try to understand some of the psychological mechanisms by which people could come to terms with life-threatening situations or otherwise highly stressful situations.

So I was engaged in that over several decades, and I think it had some interest for people in the psychoanalytic field, particularly those concerned with what was called ego psychology, but I think it had a broader interest in psychiatry, in pediatrics, in various other parts of medicine, eventually in public health.

The series of studies on really life-threatening situations -- indeed, many of the patients died -- the burn situation, and then later patients with severe polio, before the vaccine, and then later studies of the families of children with leukemia at a time when leukemia was a uniformly fatal disease. It isn't today, happily, but it was then.

We tried to work on both biological and psychological aspects, but primarily in those studies psychological responses, how is it that people could maintain a sense of worth as a person, and maintain significant human relationships, and mobilize some hope for the future and come to terms with the immediate requirements of the situation, and think about some ways of getting out of this terrible box they were in and move ahead. So we tried to delineate a lot of strategies cognitive and emotional and interpersonal strategies, by which people would cope with very severe life-threatening situations.

Then I got to a point, really, on a personal basis where I didn't want to do that anymore. It was just too draining emotionally. It occurred to me that in a way what we were dealing with was major life transitions, and there were major life transitions that were not inherently life-threatening, but, nevertheless, difficult, almost required some new adaptive patterns. So then in later years, I spent considerable time studying those situations and every so many years would try to do a synthesis that would draw together the strands of

research on coping and adaptation. We put out one multi-author book on that, which had about a twenty-, twenty-five-year life in graduate studies and whatnot, I'm happy to say.

Q: What was it called?

Hamburg: It was called *Coping and Adaptation*. I had some very distinguished contributors, like Erik [H.] Erikson, for example, in that book. But that was an important part of my life, which was totally unexpected. But it seemed to me you couldn't really study biological responses to stress without getting interested also in psychological responses. And to the extent that we could link the two, we tried to do so.

Q: Partly what I'm fascinated about is, you've spent all these years in a medical environment, particularly, for studying almost maladaptive stress responses. I'd like to hear you talk a little bit about that in terms of your later views and insights into evolutionary perspectives, particularly after your time at the Center for Advanced Study in Behavioral Sciences. Then, on the other hand, you study what are the capacities, extreme capacities, of human beings to operate under conditions of extreme stress. How do you put those two things together, and how much did you begin to see human development, in particular, in these two ways, or did they ever compete with each other? Did you begin to see the world in these terms?

Hamburg: The different strands of my research interests were interrelated. Certainly the thing that struck me about the coping observations, one of the important things, against the background of the field, was that, understandably, medicine was very pathology oriented, and psychiatry inherited that tradition of pathology. You see symptoms, you see

disease, and you try to understand how the disease came about. I'd say it's largely in recent decades that medicine, generally, and psychiatry, in particular, has come to pay more serious attention to the body's own responses for adaptation. Take the enormous development of the field of immunology in medicine. There was really almost no immunology when I went to medical school. A little bit. And we've learned something about the very rich, complex capacities of the human body to deal with foreign agents, threatening influences at the microbial level.

So, too, I think that when I came into psychiatry, at first what we were really taught was to focus on breakdown of function, maladaptive behavior. Then as I began to make the coping observations, I sort of turned the thing on its head. In fact, I believe I started one of my papers with a sentence something like this: "Why doesn't everyone break down?" This listed many of the ubiquitous stresses of life, and if you went by the earlier tradition that emphasized so much the pathology and the vulnerability and the breakdown, you would think that there would be no way to escape it; sooner or later we'd all disintegrate. But we don't. I mean, we suffer, and by and large, we cope, we adapt, we solve problems, make the best of situations, transcend difficulties.

But both aspects, two sides of the coin, are real and significant. I did feel that I was able to maybe add something to the outlook of the field by pursuing the coping and adaptation in a serious way. Now, I may have been influenced in that by the early exposure to biology, including evolution. You have to bear in mind that adaptation is perhaps the central concept in evolution. I had been exposed to that kind of thinking as an undergraduate at Indiana University. One of the teachers of evolution in my day in Bloomington was Alfred Kinsey, who is known for the sex studies later, but was a great teacher of evolution.



Kinsey, Breneman, and other professors were very stimulating, no question about that, on the evolutionary perspective.

That came into my own life again in the stress work, because as we went along in that with a variety of collaborators and following the general research literature, it became very clear that the stress responses were a kind of mobilization for action, without action, in most cases, in contemporary circumstances. The energy metabolism of stress, cardiovascular responses of stress, the role of these hormones in stress, all pointed in the direction of the body's getting ready for some intensive exertion. Walter Cannon, years earlier, the great Harvard physiologist, had characterized that as the "fight or flight" response.

I got really perplexed about that. It seemed to me that must have served adaptive functions over a very long period of time, under the conditions in which we evolved, there were millions of years that the human organism and its predecessors must have been able to take the actions they had to take, and there would be a real value to anticipatory mobilization -- getting revved up and ready to go in the face of danger so that you could do whatever you had to do to survive, but that clearly didn't apply much to contemporary circumstances where stress does not often lead to exertion.

The people we were studying were mostly in more or less sedentary situations and, for that matter, largely lived sedentary lives. And all the drastic transformation essentially since the Industrial Revolution, just a moment of evolutionary history, you know, a hundred or two hundred years of time in which these things have taken place compared with millions of years in which we've evolved, so that it seemed to me that we probably carried over these responses from an earlier time, and they might no longer be adaptive. In fact, that became

a general orientation of mine, certainly about aggressive behavior, that much of what was adaptive earlier was no longer adaptive in contemporary circumstances. That led me to then actually get into some evolutionary studies, which I never in my wildest dreams thought I would do. That's what took me to Africa.

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Q: I think we wanted to back up a little bit, and you wanted to talk about the relationship between genetics and endocrinology in terms of you linking those two things and establishing a new area.

Hamburg: The interest in genetics and evolution were closely related, as they are conceptually and indeed operationally in the field of biology. Having gotten that bug as an undergraduate, it really never left me.

Now, with respect to genetics, as the stress research evolved, I got more and more intrigued with the question of individual differences in response to stress situations. They were very manifest on the psychological level, but then it seemed to me that given the enormous biological variability on almost any dimension that it's possible to measure, that there probably were big individual differences biologically in stress response. Let's say, for example, if you and I had an argument and we both got approximately equally upset, it might be that my level of adrenalin circulating in the blood would be three or four times yours, or it might last three or four times longer than yours. I really wanted to investigate that kind of possibility. There was reason to believe that that might well be true.

There are a lot of places in which the individual differences could be genetically determined. It might be in the rate of synthesis of the hormone in the adrenal gland, it might be something about the way it's transported through the blood to reach all the tissues of the body, it might be something about the way it's excreted in the kidney. There are a number of places where a genetic variation could influence an individual's biological response to stress.

So I recruited into our department at Stanford, when I went there in 1961, people with backgrounds in genetics and biochemistry and evolution, and brought the different disciplines together and began to develop what I came to call a behavior endocrine genetic response to stress. The acronym was BEG -- behavior endocrine genetic. We simply tried to get at that kind of question. It had to be done mostly in animals, where you could really look at the synthesis and transport and the metabolism of the hormones.

So, all that to say that we did manage to open up that field that's gone far beyond where it was in the beginning, but at least we were able to show that there were, in fact, major genetically determined variations in the way the body processed the stress-related hormones. I'm very pleased with that. It would have been fascinating to go on with it myself, but as in much of my career, for whatever reason, I would tend to stimulate very good people to get going on a line of inquiry, and they would carry it far beyond what I had done myself. Jack Barchas and Roland Ciaranello pursued this line of inquiry very effectively. They both went on to become leaders in neuroscience and psychiatry. Then, of course, the field as a whole, if the work was any good, would pick it up. Genetics has become very important in the stress field and in psychiatry generally at the present time.

On the evolutionary side of that, the way it was manifest was in trying to understand how human stress responses evolved. In 1956, I guess it was, '55, maybe, I got a letter out of the blue, it invited me to spend a year at the Center for Advanced Study in the Behavioral Sciences, located at Stanford, but an independent institution. It had been created by the Ford Foundation and it had its first operation in 1954. Real giants in the field were invited. I was very young, and certainly not a giant in the field. I almost felt like it was a mistake when the letter came, how they could be inviting me to come there. But turned out they wanted some young scholars of promise, and I was very lucky to be in one of the early groups. And doubly lucky because for reasons of the hospital and clinical responsibilities, I had to go, instead of for an academic year, for a chronological year instead, that meant I spanned two groups. Most of them came in September and left in September, but I came in January and left in January, so I spanned two groups. So I, therefore, got to know almost a hundred distinguished people in perhaps a dozen fields from the biological and the behavioral sciences to the social end, from all over the world.

Many friendships were formed and professional relationships that continued to the present day. For example, when I mentioned earlier that I had this summer spent time on German history, part of my guidance for that came from Fritz [R.] Stern, professor of history at Columbia, who I met at the Center in 1957.

I put on the informational sheet that they asked you to provide them with at the Center about your interests, I put down, "I'd like to learn something about evolution." I didn't know anything about evolution, really, about human evolution specifically. But I wanted to learn something about that in order to try to understand the evolution of human stress responses. So, the first or second day I was there, a wiry fellow appeared at the door,

introduced himself. His name was Sherwood Washburn, and he was one of the great -- I didn't know it at the time, but he was one of the great pioneers in modern study of human evolution. It's an irony that just this week I received in the mail from the Johns Hopkins [University] Press a volume in tribute to Washburn, for which I was one of the co-editors. It's called *The New Physical Anthropology* belatedly, we should have done it years before, but a wonderful former graduate student of his, Shirley C. Strum, was the driving force behind it, and that volume shows the intellectual history of Washburn's work and his stimulating effect on his students and others, and certainly on me.

So there began, then, a collaboration review with Washburn on many facets of human evolution bearing on stress and aggression and attachment and child development. But what that eventually led me to was to go to Africa. Washburn had begun the new wave of primate studies in the natural habitat, just in the early fifties, and it really caught on. I got interested, after a few years, in whether it would be possible to learn anything about chimpanzees in the natural habitat because of their very close biological relationship to humans. The new work in genetics and biochemistry was showing that over ninety-eight percent of the genes of chimpanzees and humans are identical. In fact, it's, in a way, hard to figure how we could be so different from chimpanzees, since we share so many genes. But be that as it may, it seemed to me a great advantage if you could learn something about chimpanzees.

Stanford had a lot of land, and they were wonderful to me and gave me, I think it was, twenty-seven acres set aside to build a semi-natural laboratory for chimpanzees. They could live in groups, except that we knew very little about how they lived in the wild. What's a day in the life of a chimp? Nobody knew at the time.

At first I was going to try to find a young zoologist to go out, and would set up a field station to do that, and, indeed, a medical school classmate of mine, who was a medical missionary in the Congo, had identified an area that seemed very promising. Then the civil war in the Congo broke out, and that clearly wasn't a feasible proposition. I wasn't going to ask anybody to go and do something like that.

Within, I don't know, a year or so, somebody brought Louis [S.B.] Leakey around to see me. This is before I moved to Stanford. I was still at the NIH [National Institutes of Health]. Leakey told me that he had started a young woman named Jane Goodall on studies just a few months before, or a year before, whatever, in Tanzania, on the other side of Lake Tanganyika from Congo. So I started corresponding with her. Then she came for a visit to the United States, and we became friends. She and her husband, a great photographer, Hugo von Lawick, had a marvelous treasure trove, even from an early point, of chimpanzees in their natural habitat.

So, to fast forward, I eventually got out there. In the earlier years I didn't go because my kids were young, and I just felt to go away for a long time wouldn't make sense. But I guess it was 1968, my wife said to me, "Why don't you take Eric with you?" He was the older of the kids. That would transform the situation. So I began --

Q: How old was he?

Hamburg: He was fifteen at the time. Then Peggy, our daughter, when she was fifteen, went with me. I began this pattern of going there more or less a couple of times a year.

When I first went out there, Jane was about to have to leave. She had finished the work for which she would get her Ph.D. at Cambridge [University], and it didn't seem a practical proposition to stay. Hugo needed to do filming elsewhere on other subjects. But it was clear that they really wanted to stay. They had no support for doing it. So I offered to try to get funding and some organizational support and make a relationship with the Tanzanian Government that would give them an official blessing.

So we made a real research station out of it, and it was a wonderful collaboration that lasted the better part of a decade, was active from '68 to '75. I think that's right. So we then were able to get graduate students and very good undergraduates and postdoctoral fellows, so at any given time we had perhaps twenty or so people working there from Stanford, from Cambridge University in England, and from the University of Dar es Salaam in Tanzania. I felt it was very important that it be international and interdisciplinary and that Tanzanians be included in it.

A lot was learned. It became much more systematic and, to the extent possible, quantitative observations of the behavior of chimps in their natural habitat, and moved from studying a single community to studying two adjacent communities so the interaction between the communities could be studied. That turned out to be extremely illuminating because of the violence between different communities.

But anyway, then we also built this semi-natural laboratory at Stanford, where we wanted to be able, for example, to train the chimps to hold out their arms to have blood drawn, which it's possible to do. So we had the semi-natural laboratory at Stanford and the natural habitat studies -- we informally called them Gombe East and Gombe West. People

went back and forth, students working first at one place, then the other. There are different things you could do in a laboratory than in a natural habitat. Each has its own limitations, each has its own strengths. It was, I think it's fair to say, unique in the world at that time.

It came to an end with a hostage episode in 1975. That's perhaps a great irony. We came back to the Congo. It was Mr. Kabila who last year became King of the Congo, is at the moment Chief of State of the Congo, who masterminded the hostage episode.

Q: Can you describe it for those who don't know about it?

Hamburg: Well, I was at the time spending a year at Caltech as a visiting professor, and my wife was a visiting professor at UCLA [University of California, Los Angeles] that year. We were living in Pasadena. I gave a lecture at University of California in San Diego, came back to my office at Caltech to check for messages toward the end of the day. The system they used at Caltech was, they would put up on your office door, they'd stick any messages there that you had. Normally I'd come back after a day or two away, I'd have two or three or four messages. This time it was a blizzard. The door was covered with, I don't know, sixty or eighty messages, so I thought it was some Caltech nerd's idea of a practical joke, you know. But it wasn't that at all. These were emergency messages that four of my students had disappeared in Africa. There were calls from the State Department and the press and families and so on.

All we knew was that about forty heavily armed men had come in off Lake Tanganyika into our camp on the lake shore, taken four people, disappeared on the lake. A few shots were



heard, and nobody had any idea. Were they killed and dumped in the lake? Who was it? Who took them? What was it about? We knew nothing.

So I decided I immediately would go over there and see if there was anything I could do. I hadn't the foggiest idea if there was anything I could do. We were all quite scared. It was just a fantastic experience, the kind of thing that you may read about, but doesn't happen to real people.

So it turned out, somewhat later, I guess it was some days before we found out that these were rebels against the government of Mobutu, the longtime dictator in Congo. He renamed the country Zaire. Now it's Congo again. But the reason we knew that is that they had sent a letter of demands to President Julius Nyerere of Tanzania. It turned out when I got there the story had been very secretive and complex, that these people, these rebels against Mobutu, lived very high in almost impenetrable mountains, nine, ten thousand feet, rising dramatically out of Lake Tanganyika on the other side of the shore. Lake Tanganyika is a lake about the size and shape of Lake Michigan, about thirty miles across, three hundred miles long, very deep mountain lake, rather dramatic, aesthetically appealing in some ways.

But anyhow, they'd come from across that lake. Nobody knew that they were there. They'd taken these kids and were holding them hostage. The point was that they had had a secret supply line for a thousand miles from the Indian Ocean at Dar es Salaam, the capital of Tanzania, across Tanzania to Lake Tanganyika, and they had been getting supplies that way. They were derivatives of the old Lumumba crowd that lost the civil war in the Congo, lost to Mobutu, and they were getting some supplies from a few Communist

countries, China, North Korea, and Cuba, so we came to understand later. That supply line had been shut down by Nyerere in a trade deal with Mobutu, so they were furious with Nyerere. We were pawns. We were pawns in it. We were meant to bring pressure on Nyerere. They saw us as very powerful quote Europeans, meaning white, and they attributed great power to me as an American professor, very amusing. American professors not thinking of ourselves as very powerful.

We were somehow supposed to get their people back who were operating the supply line, get the supply line reopened, or something. So it was quite a fantastic experience. All the way over on the long plane trip, I was very apprehensive about how I would react in the situation. I had no great confidence that I would have the foggiest idea what to do or that I would hold up well or any of that, but I was determined to do whatever I could.

It turned out that Nyerere was furious with them because they were very insulting in their note to him, in their demands on him and so on. Our own government, unknown to me, our own government was negotiating with Mobutu to be our strong man in Africa vis-a-vis the Angolan operation that was about to begin in Cold War fashion between us and the Soviets. I didn't know that at the time.

Q: A covert program?

Hamburg: Covert program, yes. So our own government had very little sympathy for the exercise. The last thing they wanted was for Americans to be talking to rebels against Mobutu when they were cultivating Mobutu as our great asset in Africa. I won't comment

on the irony of that situation.

But anyway, so we did not have a sympathetic response from the host country, Tanzania, nor from our own government, nor, of course, from the government of Zaire/Congo, need I say. So I was in business for myself. There were some wonderful people in our embassy in Dar es Salaam, then Ambassador Beverly Carter and one of his staff, Lewis MacFarlane, who were very resourceful, courageous people, who helped me, as it turned out, at the risk of their careers. So we set out to do whatever we could, first of all to find out who had taken them, and then to make contact with them. We didn't know how to do that. Eventually we made contact, and then to see if we had any conceivable negotiating leverage. What kind of leverage could we actually have?

It turned out it was possible to figure out different strands of negotiating possibilities, and that led, over a period of a couple of months, to three of the four kids were freed. But they were still holding the fourth one, and he was really in grave danger. Then President Nyerere helped us. That was absolutely vital. He helped us with that.

I was very happy, just two or three years ago, on multiple levels I was happy, but the International Peace Academy, on its twenty-fifth anniversary, gave awards to President Nyerere and Prime Minister Gro Brundtland of Norway and to me. I was flattered beyond imagination to be part of that company, but it also gave me a chance to pay tribute to him. He had asked at the time of the hostage episode that for ten years we would say nothing about it, so we honored that. But I was able, on that occasion, to say what it meant that he would make that intervention. Anyway, so after two and a half months, they all four were free. It was a very important experience. It was the end of our work in Africa, and it

certainly influenced the rest of my life.

Those years were extremely stimulating, and an opportunity to get some insights into human evolution, and I wrote quite a number of papers alone and with Goodall, and with Washburn and with others, and, more importantly, stimulated students, some of whom are now professors at Harvard and Michigan and Minnesota now, and making very good contributions to the field. So I view it as a very good time and a fantastic set of experiences of a kind that I never would have dreamt. I never dreamt of going to Africa in the first place. I never dreamt of doing primate research. I never dreamt of dealing with a hostage problem. But there it was.

Q: I have two questions about that period. One, I read somewhere that your wife, Beatrix, was convinced -- and rightly so -- that had Nyerere not helped you out in the end, you were prepared to go to the camp yourself --

Hamburg: Right.

Q: -- for the sake of becoming a hostage, to release the student.

Hamburg: It's a fascinating story. That's right.

Q: Could you tell the story?

Hamburg: It's a great tribute to Betty's ingenuity and sensitivity. We've been terrific collaborators for fifty years. We met fifty years ago this year and have, in many ways,

personally and professionally, worked closely together ever since. She's an extremely resourceful person.

During the hostage episode, when I first got to Dar es Salaam, our ambassador told me that the Tanzanian Secret Service was quite up in arms about this whole thing, and they were going to bug my hotel room, which they did, in a very clumsy way. They came in repeatedly to, quote, fix the light in the bathroom. But anyway, what I could say on the telephone connections, which were tenuous at best, was extremely limited by that fact, because we were going to do things that clearly the Tanzanians didn't want us to do. I was going to do whatever was necessary to save the kids, if I could.

So somehow or other, Betty and I were able to improvise a code. From our relationship, partly out of U.S. idiomatic language and partly out of some shared experiences that nobody could know about but us, we were able to get certain key symbols for the code to communicate what I needed her to do. Since it was an African political problem, then the U.S. side of it was very important in a number of ways, and she took charge of the U.S. side. But over a period of time, she did figure out -- and I didn't intend to tell her -- but she did figure out that I was considering going over there. There was an American journalist who --

Q: That you were considering, I'm sorry --

Hamburg: -- going over to the other side of Lake Tanganyika if necessary, as a last resort, to try to bargain for the fourth student.

There was an American journalist who knew that area very, very well, and had a number of scoops in the area. He told me that he thought he knew where the camp was. He was willing to go himself, but he didn't think it would amount to much. He thought if we went together, we would have a chance, and that was plausible. But there was also the chance they would simply take us hostage.

In my negotiating with the Kabila people, there was nothing that engendered confidence. I mean, they were not trustworthy negotiating partners. I wouldn't say they were bloodthirsty, but neither were they trustworthy. So it wasn't a congenial proposition. On the other hand, if that was all I had, the only card I had to play, then I would do it.

So she sensed that and got on an airplane and came over to sort of sit on me and prevent me from going and doing what she thought would be quite foolish. She thought they would simply take us hostage and they would have more bargaining power with us than with the student. And I suspect she was probably right about that.

Anyway, it did work out. It's a tribute to her as an individual, but it's also an interesting commentary on close human relationships, that you can invent, under great duress, you can invent a code and sense each other's responses over thousands of miles.

Q: I was also very intrigued by your response to the fact that Beverly Carter was fired summarily by Kissinger, is that right?

Hamburg: Yes.

Q: Could you talk about that?

Hamburg: Yes. That was very disturbing. Carter was only one of two remaining black ambassadors in our ambassadorial corps at that time, and was a giant of a man, six-eight, very smart and very resourceful, and had a career in the Foreign Service. He was called back to Washington after the fourth student was released. I was coming back, too. I was going to drop off the students in London at the excellent tropical disease hospital, to make sure that they were healthy, and then go on back to the States.

Betty and I were coming back to Stanford. He was going back to Washington. I, in my naiveté, thought maybe they would honor him for his resourceful help in this situation. But the first night back, after a marvelous, incredible reception at Stanford, where there were hundreds of students and others at the airport when we got back and so on, in the middle of the night I was awakened by a call from Beverly Carter in Washington, who got through a few sentences and then broke down. His wife came on the phone to tell me that he'd been fired, public humiliation, his career was over, ostensibly for violating U.S. terrorism policy, which hardly existed at the time. So all he wanted me to do was to come in a day or two and stand with him at a press conference and say he was a decent, honorable man, as he resigned his post and explained his situation.

I asked him to wait. I remember saying something to the effect that, "Those kids had been held hostage for several months, and now, Bev, you're hostage. Let us see if we can get you out." He felt he couldn't do anything himself. Somehow his code of conduct was such that he was just going to have to stay out of it, but that obviously if we could help him restore his career, he would be grateful.

So I spent the next six weeks trying to restore his career. We first went through the normal standard channels -- State Department proper. There were a number of sympathetic people in the Department, but we weren't getting anywhere. I came to the conclusion that if we could mobilize some media support, there might then be a chance to really put pressure on the President, because these kids, after all, were objects of great sympathy and great interest. We had been declining press interviews and all public commentary up to that point.

But I was able to get some help first from a friend, a much beloved friend, Fred Hechinger, of the *New York Times*, who died a few years ago. Fred, to make a long story short, put an editorial in the *Times* about the case, and it was a very good editorial. Then Carl Rowan, who was a columnist who knew Beverly Carter and had great respect for him, Carl Rowan and I talked. Carl Rowan did a column about him, and a few other things. So that was the background. It, in itself, didn't move much, but it gave a certain credibility that we could get media coverage.

Then finally I got to President Ford's Chief of Staff, highlighted the Hechinger and Rowan pieces, and said that we had invitations to go on "The Today Show" and "The Tonight Show" and a number of the TV interview shows, which we had been turning down, but there was great interest in these kids. Jane Goodall was well known and so on. I explained that if the ambassador didn't get reinstated, that we would have to go on the air, and said what we would say about the administration and its role in this whole thing, and that it's putting American citizens at risk through the action and inaction of the United States Government, and I would much prefer not to say that. I personally believe that



President Ford didn't know about it. All I asked was that he call it to President's Ford attention and either give me a chance, directly or through an intermediary, somehow to explain to President Ford our side of the picture. When I mentioned the media stuff, there was a very dramatic response, and within a matter of hours he got back to me and said, "You are right. It was all a misunderstanding. President Ford didn't know about it." I think it was legitimately true. President Ford was in the great Helsinki negotiations that led to the human rights landmark and all that during the time of our case, and I don't believe President Ford knew about it at all.

By the way, I left out there had been a short news piece on "ABC News" about these kids, too, and that was very helpful, just a vignette of the case, not about the ambassador, but about the students. So we had a certain credibility. I later got to know President Ford, and we had a talk about all this, and he couldn't have been nicer.

But anyway, suffice it to say that President Ford decided that the ambassador would be reinstated, he would have a choice of positions, because we were determined that he shouldn't have some Lower Slabovia kind of position; it had to be something decent. And he was reinstated.

Then a further irony. When President Carter was elected, I wrote a letter to Cyrus Vance which a mutual friend delivered to him. I didn't know Vance then. We later became great friends and collaborators. I explained, in delicate language, what had happened, and asked him to be considerate of Ambassador Carter, and he did actually create a position for him. Beverly Carter was a really wonderful human being. We all used to get together for the

first few years after the episode, on the anniversary of it, students and some of the parents and Ambassador Carter and his wife. We'd all get together and have a little celebration. So, another bizarre twist on that episode, but it did, I guess, lead me to my first serious experience with the media and the power of the media in American society, including the influence on Presidents.

Q: It turns out that a university professor does have some power, after all. [Laughter] I think it was Beverly Carter that challenged you, after that event, to think about using your mind and using your influence and using your creativity in spheres beyond the university. Is that correct? To take your work and try to put it in a larger, more global scope.

Hamburg: Yes.

Q: How did the hostage crisis affect you? How did it change you?

Hamburg: Ambassador Carter did make a rather powerful statement to me just before he left Dar es Salaam at the end of the episode, that he thought that I had capacities that I had really used, that this episode had brought out capacities that he thought I ought to pursue in the future, maybe in some domain relating to public policy or whatever.

Now, I had had an invitation early in May to become president of the Institute of Medicine, National Academy of Sciences. Or to put it another way, the search committee, looking for a president, wanted me to be the lead candidate. It would have had to be approved by the academy system, but they had taken informal soundings that, in fact, there was support in the academy system, and in all probability, if I said I would be interested, that I would be

president of the Institute of Medicine, which is the medical part of the National Academy of Sciences. The academy is a very special place in the American and, indeed, world scientific community.

I was enormously flattered and pleased and delighted, but I said no on the spot. It was a luncheon. My wife was there with me. It was in Pasadena. We were on leave that year. I can just remember the place and everything about it. I felt I couldn't leave Stanford, I couldn't leave the work in Africa. I had developed a configuration of responsibilities that was unique to me, and I thought if I pulled out of it, you know, it probably wouldn't be sustained. In any case, I loved what I was doing. Our kids had grown up on the Stanford campus. I never envisioned leaving at all.

That was early in May. May 19th, the students were taken. Two and a half months later, it was over. When I got back to Dar es Salaam at the end of it, there was a letter from the president of the National Academy of Sciences that essentially said something to the effect that, "An experience like this can be a deep experience. It can affect your whole life. It can make you rethink what you want to do. We're asking you to rethink. Would you like to consider the Institute of Medicine? Please come and visit and let's talk about it."

It rang a bell. I wasn't sure that that was the right thing to do, but that, combined with what Ambassador Carter had said, made me rethink. See, I had been immersed in the worst problems of the world during those few months -- of disease and abject poverty and ignorance and deception and violence. My nose was rubbed for months in all these dreadful problems. I had some feeling that maybe, maybe in some way, if I could turn my energies to settings and institutions that could do something about those problems, the tiniest little

bit, could affect policies that would have some kind of ameliorative or preventive effect on terrible problems of that kind, that maybe that would be a good thing to do. I wasn't quite sure how best to do it. I thought maybe to reconstruct something at Stanford different than what I'd been doing.

But I did go to visit with the president of the Academy and others, and decided that, in point of fact, that that was a very strong institution, had worldwide standing, was policy oriented, it basically had ways of bringing the strength of the scientific community to bear on great policy issues, policies within the scientific community and beyond the scientific community. So I was, in a way, a changed person, and from then on, both in that position and later at Carnegie, I was fundamentally trying to find some way to do the least little bit on these great issues.

Q: We can begin to fast forward a little bit towards Carnegie. If you want to talk substantively about what your work was at the National Institute for Medicine, you were focusing on health, in general, in developing countries, is that right?

Hamburg: We had several strands to work at the Institute of Medicine, and then later for a few years at Harvard when Derek Bok asked me to develop a university-wide health policy program modeled on the Institute of Medicine. So there was a total of about eight years at the Institute of Medicine and at Harvard where I was basically putting together sets of people who could bring great strengths from different angles on health policy issues.

Some of it had to do with innovations in health care, basically the application of the burgeoning life sciences to health care problems. Part of it had to do with disease

prevention, health promotion. Part of it had to do with health in early life, in childhood and adolescence, which was the main part that carried forward to Carnegie. Part of it had to do with the emergence of an aging society, health care and disease prevention in older people. And part of it had to do with health in developing countries, the immense disease burdens carried in developing countries, which I'd been exposed to, and there I had the great opportunity to create a Division of International Health in the National Academy of Sciences, and in recent years have been working with the academy on broadening the international functions at the academy, in health and other matters.

So that was very satisfying. In fact, we had the opportunity to bring the strengths of a variety of sciences to bear on clarifying policy issues and formulating policy alternatives on important fields, and some of that carried over to Carnegie, mainly the part on children and youth and the part on developing countries.

Q: You were focusing specifically at Harvard on children, is that right?

Hamburg: No, that was only one strand. We had six different categories, much like at the Institute of Medicine, of which one strand was that.

Q: How did you first come to be invited on the board of Carnegie? Because that's the way that you came.

Hamburg: Right. I think they wanted a health person, or at least they wanted somebody out of the scientific community, and I think to some degree preferred that it be in the health arena, and made some inquiries. At that time I was at the Institute of Medicine. I

think I was still there. I'm not certain. I may have been at Harvard already. In any case, it was that background of a broad-gauged interest in health. They didn't want to become a health foundation. They didn't want a deep specialist, however brilliant. They wanted a broad-gauged person in health, who could think about the relevance of health matters to the Carnegie agenda, whatever it might be.

Q: Had you been a Carnegie-watcher?

Hamburg: No, not particularly. I had a vaguely positive impression about it. I associated it with education.

Q: What were your perspectives on philanthropy at that time as opposed to research universities?

Hamburg: Hardly any, in point of fact. I mean, they couldn't have picked a less well-informed, less well-prepared board member. Maybe that was an attraction. I don't know. I certainly had no biases about philanthropy. See, when I was building the department at Stanford in those wonderful fifteen years there, it was government support. It was primarily the National Institutes of Health and, secondarily, the National Science Foundation and the Veterans Administration that provided our support. Within that, in my own department, the bulk of it was at the National Institute of Mental Health -- and they sometimes used to speak about us as NIMH West because we very rapidly built up major support. The universities lived on soft money, fundamentally a federal government enterprise. It still is, to a very large extent.

It's hard for me to imagine how that's grown. When I started out in medicine, there was, for all practical purposes, no National Institutes of Health, and today the budget is over fifteen billion dollars a year, and most of that goes into supporting research, university-based research. So that's what I knew.

We did not appreciably turn to foundations, with one major exception. I should say that the work in Africa, the chimpanzee work, did not have a natural niche for support in the federal government. There was some from the National Science Foundation, but I did turn to foundations for that. We had wonderful support from the Commonwealth Fund and the W. [William] T. Grant Foundation. It's a funny story, my wife later became president of the W.T. Grant Foundation, much later. They gave us wonderful support.

So, my little experience with foundations was very encouraging, that they had that kind of scope and flexibility that the government didn't have to support research in Africa of a very offbeat character; something they'd never done before but they were betting on people, an interesting idea. So to that extent, I had a sympathetic disposition toward philanthropy in terms of its scope and flexibility.

[END TAPE ONE, SIDE TWO; BEGIN TAPE TWO, SIDE ONE]

Q: We are at Carnegie now. You're on the board. Can you tell me some of your impressions of Carnegie under Alan [J.] Pifer's leadership? What was he like as a leader? What were the roles and expectations of board members? How did the board relate to the staff?

Hamburg: Well, I wasn't a terrific trustee. I wasn't as deeply engaged as I probably should have been. But I had a very positive impression. I thought it was very interesting. I learned a lot.

Alan was deeply committed to the issues of poverty and racism, the social agenda which had really come to the fore in the 1960s, and he was determined that Carnegie would do whatever it could to improve opportunities in education and the like. Those were values that I admired. The issues were quite interesting. They had a good deal more on elementary and secondary education than I'd been exposed to before, and I didn't have any problem with it.

There were, however, some problems in the board. There were members of the board who felt that the board had too little to say about the agenda, and so I guess Alan appointed a few of us to a committee to consider what to do about that, and we came up with the notion of an Agenda Committee, which has continued, as far as I know, to the present time, at least throughout my term, that is a combination of board members and senior staff who would keep rolling forward the agenda of future meetings, what did we need, what would be helpful, and so on. I thought that was a constructive response on his part to a certain discontent in the board.

There were some board members in that time who were, I think, rather controlling, who wanted to dip into management or, at any rate, didn't understand very well the distinction between policy and operations, that I think is appropriate, you know, where the board really does set policy, but that the operations are left to the chief executive and the staff. I was very sympathetic with the position of the president and the staff, and felt we oughtn't



to be too intrusive. We had ample opportunity to say what we wanted to say at board meetings. But the Agenda Committee was a good innovation.

It's also true -- I didn't realize it at the time, but Alan had had a nasty injury in falling from a ladder, so he wasn't as deeply engaged in those last few years as he had been earlier.

There was a certain amount of problem connected with that. I must say I wasn't aware of that at the time.

But from my standpoint, it was a well-functioning educational foundation that dealt with important issues, and I was happy to be a part of it.

Q: Were you surprised when they asked you to become president?

Hamburg: Yes, I was very surprised, and I thought it was inappropriate.

Q: Could you say that, phrase it so that you were surprised when they asked you to become president.

Hamburg: Yes, I was very surprised when they asked me to become president, and I at first said, almost reflexively, no, both from their standpoint and mine. That is, from the standpoint of the foundation, I felt that it would be odd to have a person of a medical background as president of the foundation. It really had never been a health foundation, though it had health components. It didn't intend to be a health foundation. The perception might be wrong, my background might be wrong.

Furthermore, I was getting increasingly concerned about the dangerous situation with the Cold War, and I felt that if I were to be associated with the foundation, that I would really want to do something about the Cold War. It was nowhere then near the current agenda of the foundation, and it seemed to me that would be kind of a wrenching transition to make.

From my own standpoint, I had only been at Harvard a couple of years, and Betty was very well established as a professor at Harvard, and I was building a program. Although I had very good people with me and it was clear that somebody could take over, that wasn't so much the issue. But I didn't myself feel right about leaving after what would only be a three-year stint. So I felt, on both counts, from the standpoint of the foundation and my standpoint, it wasn't very appropriate.

The then vice chair of the board, Helene Kaplan, later the chair of the board, was chairman of the Search Committee. Luckily for me, she was quite persistent, and at some point talked to Betty about it. She understood my reasoning was that -- part of the reasoning was, I didn't want to uproot Betty again. She'd been an awfully good sport about moving with me wherever I went, and being enormously supportive and helpful and collaborative, and I felt, "Enough already. She's got a very good position." Furthermore, by that time our daughter Peggy was a medical student at Harvard, and I loved being with her. The whole thing didn't seem sensible.

But Betty was the one who said, "Well, you really ought to think about it. You ought to open up your mind to the possibility." She wasn't recommending that I do it necessarily, but I really should think about it, particularly when the response of the search committee, or at least of Helene and Bud Taylor the chairman, was, "Why not do avoiding nuclear

war? Why shouldn't we?" Andrew Carnegie had a passion for peace. I mean, I knew that, too, but for a long time the foundation had gone away from that. Indeed, to a very considerable extent, the foundation left that when he died. Although it came back from time to time, as far as I could make out, it was really not central to the agenda of the foundation after Andrew Carnegie's death.

So I didn't really know whether it was appropriate or not, but the then trustees seemed to feel that, yes, the Cold War was the greatest conflict in history, the most dangerous conflict in history, and indeed was a threat to humanity, and why not see if there would be something useful to be done? So that was very significant to me, because that opened up a possibility to move into an area which I had otherwise no way I could do anything useful. So that was very exciting. I guess Betty's reaction plus the opportunity to tackle Cold War issues were the pivotal factors for me.

Q: When you came to Carnegie, what did you find in terms of the morale of the staff, the functioning of the staff? What did you observe about how people worked? You'd been mostly -- well, you'd been at the Institute, you'd been in university environments. Was it any kind of shift for you or transition for you to move into a different kind of world? And could you describe what it was like when you came in?

Hamburg: It was a shift.

Q: Your first impressions?

Hamburg: Coming to the foundation was a big transition for me and for the staff.

Remember I mentioned earlier that major transitions are inherently stressful. For myself, every time I made a move, I always had doubts as to whether I really would be able to respond well in the new situation or take advantage of the opportunities. And for the staff, I think they were a little apprehensive about me, though it helped that I'd been on the board and they knew me. I wasn't so threatening as an individual. I think there was some question whether I was going to medicalize the foundation. None of them had a background in the health field to speak of.

There was some apprehension about it, but I think we all threw ourselves into it very enthusiastically. I had certain issues that I wanted us to address, which had a lot of continuity with Carnegie tradition, and I put them on the table. We organized mixed staff and board groups, working groups, to look at these issues and to do as I'd done at the Institute of Medicine; that is, to get what I call terrain maps, papers that would in some depth examine a problem area and see where you could maybe find sort of a hot spot within the map, where you had an opportunity to make a contribution. Everybody, I think, got engaged.

We had a lot of outside consultants. I opened up the foundation quite a lot to outsiders to come in and tell us what they thought about these issues. I think it was stimulating. I think there was a zest to it that people got caught up in, and the staff could see they were going to have an input.

I had no feeling that I needed to make big changes in the staff. I felt that there were a lot of devoted people who were knowledgeable and good in philanthropy. I felt it might be necessary to move some of them around. They might have to change what they worked on,

test their adaptability, but I certainly didn't feel that I had to clean house to get my staff. I had some advice from the board, particularly from corporate members of the board, to do the corporate thing and get my own staff; that was Alan Pifer's staff. I just didn't feel any necessity for that. I thought I would look at it one by one, case by case, first year, second year, and there were some changes I made, very quietly, not blood on the floor. But most of the people were very smart and very experienced and very adaptable, and threw themselves into it. So there was that part of it, I think, was reassuring.

I guess operationally we did make a change. I organized program groups. I felt that the topics we were tackling were sufficiently complicated that we would need to have several people working together. That was my style anyway, from way back, as I mentioned to you. Rather than to have each staff member be a mini-foundation, we would need to have groups that would work out together the priorities for grant-making and the evaluation of specific grant proposals. And that's what we did, so we came to the working group system. I think that, on the whole, has been very fruitful.

So there was some transitional stress, but I don't think it was a very big deal. There was a lot of continuity and there were some changes, but the changes were worked out together. Now, the biggest change, of course, was in the Avoiding Nuclear War program. That was strange to everybody. I must say, I'm very, very glad, from the standpoint of tradition of the foundation, that we did that, not only in terms of the contribution we were able to make, but just in terms of what had been important to Andrew Carnegie. He didn't require that the foundation follow in his footsteps. He gave the broadest possible mandate. It's extraordinary what he did. He invented the concept of the general-purpose foundation, and he said, in effect, "Nobody can be wise enough to know what will be important in thirty

years or fifty years or a hundred years, and the greatest tribute the board can pay me is to decide what's important at a given time," etc.

Nevertheless, it's very clear that he had deep commitments to peace and education, and those problems have hardly gone away. It certainly seemed to me that it made a lot of sense to pursue these great themes. But how we were going to do it in avoiding nuclear war was not obvious at all. I didn't know if we could make a contribution. All I knew was that the Cold War had a relapse, the Soviets had gone into Afghanistan, and the feeling was, if they could make that stupid and dangerous a decision, who could tell what might happen? There was a certain amount of talk in both Washington and Moscow in those days that perhaps a nuclear war was inevitable and maybe somehow you could, quote, prevail. Nobody talked about actually winning, but you could prevail, maybe just 100 million dead or something like that. Just beyond imagination. One of the big arguments at the time was whether, in the case of a nuclear war, the human species might actually become extinct or not. Well, that's some argument to have.

So it was a dreadful situation. But how could a private foundation make any difference? Wasn't this a function for governments? So we had to figure out what we could do. And that was, I think, the most innovative thing.

The other thing for me that was a complex transition, I made the decision that we were going to focus on pre-collegiate education very largely. We were going to use the universities to help clarify those problems, but we were not going to concentrate on the well being of universities. We were going to try to concentrate on the well being of elementary, secondary, and pre-elementary. I used to say education doesn't begin with kindergarten, it

begins with prenatal care. We were going to go from conception through high school, because I felt the most serious problems were there. The foundation had a track record in that field, had some expertise, and very little was being done in other foundations on pre-collegiate education at that time. In fact, it scared me, because I asked the staff to do a little round-up, who's doing what in this field. The answer was, next to nothing at that time. I felt, well, maybe they knew something I didn't know. Maybe this was a poisonous field to enter.

Anyway, that's what we did. We developed fundamentally a sort of zero-to-fifteen strategy on the education, child development, adolescent development side, and a program in avoiding nuclear war, which later got just slightly modified into preventing deadly conflict. It's all of a piece. And then a developing countries program, in which Carnegie had some up-and-down, in-and-out history, but a serious interest in Africa. And I felt that suited me, of course, and we could build on that and pursue a development countries program focused primarily on Africa. Those were the main strands of what we did.

Q: Those three priorities are very clear in your introductory essay, "New Contexts for Grants," that you wrote, I guess, in 1983. How did you think of those three things together, those four things together, really? Especially avoiding nuclear war and the emphasis on development in education. Did you see those two things working hand in hand in terms of the education, the more serious education of the public?

Hamburg: I saw certainly points of contact. I didn't think of them as a single integrated program. I thought that was too much to handle. So they were separate and distinct programs, but with a lot of informal interplay. And certainly on every level education of

the public seemed to be fundamentally important. In a way what evolved was, in effect, a fourth program, a democracy program, the building of democratic institutions both in the United States and abroad. Very different problems in different places. South Africa, one problem. Russia, another problem. United States, another problem. But still, that fundamental commitment: the democratic institutions. And as a part of that, an informed public. An informed public.

So, for instance, vis-à-vis the universities, in all of our program areas we got major involvement of outstanding scientists and scholars in universities, and one of the things we tasked them with was what I called education beyond the campus. Why must education stop at the boundaries of the campus? If there is elucidation of an important problem, whether it be in arms control or crisis prevention or disadvantaged minority education, or the role of women in development, whatever the topic may be, if there's something to understand, why not explain it insofar as we can, in an objective, clear, and cogent way, at least to the American public and to such other publics as we might have opportunity to do?

One of the big struggles was to try to get access to the Soviet public in the bad old days, and we actually did get some, not nearly as much as we would have liked, but we got some. But anyway, that commitment to broad education of the public was a cross-cutting theme probably in the history of the foundation, but certainly during my time.

Q: Tackling a problem, let's just begin with, we'll talk about each of these areas, but beginning with avoiding nuclear war, I'm interested in how you brought in your own thinking to this set of issues which you were sort of bringing yourself up to speed on, particularly some of the writings that you've done on group survival, tendencies of groups



to do anything to protect their own identity. Could you talk a little bit about where that comes from in your own thinking and how that fit into the avoiding nuclear war concept?

Hamburg: Well, I had been very interested in intergroup relations from an early time, no doubt as a personal and family matter in the first place. But professionally it was very striking to me. In the primate work, I wanted to try to clarify relations between primate groups, and in some respects those are very menacing and even lethal relations, including relations between different communities of chimpanzees. You would see many indications of positive feeling and affiliation and proximity and all that in the higher primates, but you also saw that there was a condition of great risk and even lethality in the crowding of strangers in the presence of valued resources. That was a particular conjunction of deadly circumstances for primates.

It led me to do a lot of inquiry, starting with the year at the Center for Advanced Study in the fifties, the first time I was there, and going on after that, about research in a variety of behavioral sciences, particularly, I would say, in social psychology, but to some extent in other fields, about ingroup/outgroup relations and this remarkable human propensity to form distinctions between one's own group and other groups very quickly.

In fact, experimentalists in that field have found that even when they are neutral or want to avoid any negative implications, it's hard to avoid. Once you get a group forming, even a short-term transient group, the members begin to make invidious distinction between their group and other groups. And where you have more enduring groups, it seems to be rather a pervasive human attribute that is very easy to learn invidious distinctions between my group and others, between me and others. Egocentrism and ethnocentrism go hand in

hand. Mind you, there's a very positive side, affiliative side, loving, tender side to human nature, which has also been important to me, but I think there's no gainsaying the fact that we have this propensity to depreciating other groups.

A fundamental question in human adaptation for me is, can we get the sustaining quality of identification with our own group, without severely depreciating other groups? I think that ought to be possible. But the fact is that human cultures have mostly taught kids to grow up to some degree ethnocentric. Education everywhere is to some degree ethnocentric.

A particular case of that, that made a powerful impression on me was from Swedish friends and Finnish friends telling me about how Finns in Sweden are the object of some depreciation in Swedish society. It's a very tolerant society, but, nevertheless, that small difference gets amplified. I think humans have a tendency to amplify small differences and to find a basis for depreciation of others. Of course, that was carried to the nth degree by Hitler's maniacal attitude toward the Jews particularly, though not limited toward the Jews, but especially toward the Jews. It simply showed that there's almost no limit to which this human propensity can be carried. I don't think it dooms humanity or necessarily should be taken as a pessimistic note, but, rather, something we have to take account of. That's part of the human reality, and we have to learn how to cope with that tendency, which is more and more dangerous as our capacity to destroy is enhanced.

I felt that was a background feature of the Cold War, but there were many other aspects to it. And always in these situations it's an activated leader, a zealot, an ethnic entrepreneur, a pyromaniac who will put gasoline on the embers of intergroup hostility and cause a great

conflagration, whether it's Hitler or Stalin or Milosevic or you name it, but a political leader to activate people on the basis of these differences and to use the intergroup hostility for his own diabolical purposes. That's a very important part of it.

In any event, I felt we had to take that as a background for what, if anything, we could do about the Cold War issues. To make a long story short, what I set out to do was to get the maximum possible expertise on Cold War issues and to bring people together from different backgrounds to work on it. So I felt a great sense of urgency, and together with the staff, people like Fritz [Frederic A.] Mosher, we went to major universities and research institutes and tried to identify people, some from my prior knowledge and some not, who knew a lot about nuclear weapons, who knew a lot about arms control, who knew a lot about nuclear crises and confrontations and how we got out of them, people who knew a lot about decision-making in the Soviet Union and security decision-making in the United States, people who knew a lot about Third World flashpoints, people who knew a lot about Eastern Europe, where the Cold War began.

So when we faced up to it, there were different bodies of knowledge and skill and expertise that you needed to address Cold War issues. So, in effect, we went to mainly some major universities and said, "If you can get people who have a number of these competencies together, to work together in a sustained way, then we'll make a grant, a sizable grant by foundation standards, to get that kind of conjunction of talent brought to bear on Cold War problems."

So we had these interdisciplinary groups working at a number of universities and research institutes, and then pretty quickly moved to get some joint study groups between the U.S.

and the Soviet Union, mainly through their Academy of Sciences, the only chance we had to have some stature of independence beyond the political control of the dictatorship, not to have KGB hacks who would be controlling the process, but distinguished scientists and scholars. That was our only chance, and it worked pretty well, actually.

So, the U.S. Academy and the Soviet Academy, U.S. universities and the Soviet Academy, we got working groups starting with arms control and crisis prevention. Those were the main ones during the dark days. Later on, after Gorbachev came to power, it was possible to broaden out into joint study groups on a wider range of issues, and ultimately toward concentrating on building democratic institutions in the Soviet Union in its last phase, and then in Russia and Ukraine after that. So, the joint study groups then were a second strand. First were the interdisciplinary groups in the U.S., then the joint study groups. These groups were primarily scientists and scholars. Some other kinds of expertise were represented.

Then we moved toward what we called linkage, linkage with policy-makers, getting independent experts together with policy-makers in our own country, in both houses and both parties in the Congress and with the administration -- Reagan, Bush, on up -- and then after Gorbachev came to power, with policy-makers in the Soviet Union. Through some members of our joint study groups, scientists, I was able to meet Gorbachev early and form a relationship with him, could see that he wanted to have access to Western ideas and information and analysis, and to some degree I became a broker, to bring or send people to meet with him and some of his closest colleagues, to discuss arms control and crisis prevention and then the winding down of the Cold War altogether, what they might do about Eastern Europe and so on.

It was a fantastic opportunity which I never expected to have, but the upshot of it was, we not only had policy linkage with our own government, which we thought would be possible in our democracy, but also with the government of the Soviet Union, which had seemed, I must say, like a very long shot before Gorbachev.

So those were the three main strands of that program: the interdisciplinary study groups generating a wider range of policy options in this country, the joint study groups between us and the Soviets, and then the policy linkage with our own government and the Soviet government.

Q: Did you have any obstacles from the U.S. government in going over and doing this?

Hamburg: There were some obstacles all around. They turned out to be less severe than some had anticipated. There were one or two people in the Reagan administration early on, in his first term, who were not friendly to any kind of non-governmental involvement, be it foundations or universities or the National Academy of Sciences. They thought this was a governmental task and we ought not to be involved in it. We were basically outsiders in that perspective.

But it turned out that President Reagan himself didn't feel that way, and Secretary Shultz didn't feel that way, and so we had some pretty significant allies, as it turned out. By the spring of 1994, I must say, to my surprise, President Reagan himself was engaged in one of our activities that involved restarting the scientific and scholarly exchanges with the Soviet Union, so that was, of course, very reassuring.

In terms of the Congress, there had been people who anticipated there might be some resistance, but it turned out not at all. On the contrary, members of Congress welcomed having a chance to be with independent experts, and we began a much deeper engagement, particularly in a retreat format, of independent experts with members of Congress, than I had ever anticipated.

I must say that John Gardner had told me, when I took the position, that he thought there was potential for that sort of thing, and he encouraged me to pursue it. And he was right. It went even beyond what either he or I had envisioned, partly because members of Congress were so concerned about the Cold War, very concerned. Both parties, in both houses, they were really impressed with the danger and wanted to play a role. They didn't feel that it should be just the executive branch that had something to say about that. So we had pillars in both parties, like Senator Simpson on the Republican side, and Senator Nunn on the Democratic side, who were deeply engaged with this all the way through this program of policy linkage.

[END TAPE TWO, SIDE ONE; BEGIN TAPE TWO, SIDE TWO]

Q: Dr. Hamburg, we're still on the subject of avoiding nuclear war. We were just discussing, actually, the Pugwash Conference and its implications for you in terms of how you think about crisis management. Could you describe that, Graham Allison's work and how it influenced you?

Hamburg: It was a turning point for me. In 1978, when I was still president of Institute of

Medicine, I was keenly aware of the dangers of the Cold War, partly because the academy in which we were embedded was trying very hard to save Sakharov's life, and was very much aware of the pressures under which the Soviet scientific community was operating, and the dangers of their build-up of weapons of mass destruction.

What was particularly concerning is that there seemed to be an inclination over there, at least of some people, to take the nuclear issue right up to the brink, right to confrontation, like the Cuban Missile Crisis or the earlier Berlin situation. They were very, very, very resentful of the United States going on nuclear alert during the 1973 Israeli-Egyptian War. That was an unexpected development, and very dangerous. Nobody knows -- to this day, nobody knows for sure about the interacting effects of nuclear alerts on the two sides, and the possibility that it could get out of control in such a way that each side would have to fire, even though neither head of state wanted to do it.

So I felt we ought to get some people together who were expert on nuclear confrontation, on crisis management, and get them together in a pleasant, neutral setting, and talk about it, first of all, as a kind of technology transfer. We had learned something from the studies of excellent scholars, great scholars like Graham Allison at Harvard and Alex George at Stanford, on crises, above all, the Cuban Missile Crisis, but other crises of the Cold War, and, to some extent, earlier crises like the interaction of mobilizations in World War I that led inexorably to the start of the war, even though the leaders were in a great muddle about whether they really wanted to start the war.

So we had a body of knowledge, and I thought we ought to explain to the Soviets what we've learned. There were principles of crisis management that Alex and Graham had

formulated. So we got together a few Americans, a few West Europeans, and a few Soviets under Pugwash auspices in Geneva, spent the better part of a week there. I was the chair. We started out with a docudrama on the Cuban Missile Crisis that Graham Allison had adapted from an earlier docudrama made in this country, which was historically accurate to the extent possible, but also condensed and dramatic, and conveyed a sense of how close we were to having the whole thing go wrong. We were apprehensive how the Soviets would take it. Indeed, they were very perplexed. Were we trying to put them down? Because, in the end, they had backed down fundamentally. We weren't trying to put them down. We were trying to get across a sense of how dangerous and difficult these crises are to manage. So we spent two or three days on that.

And then we began to make the transition from crisis management to crisis prevention. I said to them at the turning-point day in the middle of the week, "Let us begin anew. Let us recognize how hard it is, when you get to the brink of a nuclear catastrophe, to prevent some accident, some inadvertent development, from leading us into a nuclear war. We don't really want one. So let's think. Could we prevent the crisis? We don't need to assume that the nuclear weapons stockpiles will be coming down soon. We don't need to assume that the level of animosity between you and us will come down soon. All we need to say is, we've got to be smart enough to keep back a few steps from the brink. We don't want to fall into the brink. The slope gets very slippery at the edge of the brink of nuclear confrontation.

"What could we do to avoid that? Could we strengthen the hot lines so that communication in urgent circumstances would be easier? Could we develop other mechanisms like the Incidents at Sea Agreement -- rules of the road at sea so that our vessels don't crash into



each other, so that we don't inadvertently shoot at each other? Could we have some rules of the road in the air, on the land? Could we move toward notification about troop movements or vessel movements so that it would minimize nasty surprises? Because in a nasty surprise, the decision-maker may panic and hit the button. Things like that. Let's begin thinking about preventing the crisis, even if we remain locked deeply into the Cold War."

Well, I should say in that week I thought we failed, although it was a very good, substantive meeting. By the way, toward the end of that week, we had an expert on Iran -- the Shah was still in office -- an expert who had been a consultant to Iran for many years, a Swiss scholar. We took Iran as a case in point. If the Shah falls, must we and the Soviets go to nuclear confrontation? There's a lot of oil there that we both care about, etc., a strategic position. We tried to look at ways of managing a crisis over the Shah's fall that would not lead us into a nuclear confrontation. But it was also very illuminating to me that this expert from Switzerland on Iran put it to us, "There's really no need to discuss if the Shah falls, it's when the Shah falls. He will fall within the year. I'm very sympathetic with the Shah. I've counseled him for fifteen or twenty years. He's finished."

That was news to me, to all of us. We were astonished. I came back and tried to get that word to President Carter, but I essentially failed. It got to him, all right, but not in a way that it was persuasive to him. That was a failure.

But the more fundamental part, from my standpoint, was that we need to continue this discourse, and it didn't look very promising. I thought we'd failed because the Soviet guy, the head of their delegation, was quite a nasty character. He wanted to talk to me in the

evenings, as was the style during the Cold War. The chairmen of the delegations would meet privately in the evening. And he wanted to know why we were so hostile to the Soviet Union. It was clear that his picture was of Jimmy Carter and Cyrus Vance as almost maniacal fiends who were looking for a nuclear first strike, and it was so crazy. If you knew Carter and Vance, you couldn't think of more peace-loving, reasonable people. I realized this is really dangerous, that these relatively sophisticated people have grotesque distortions in their understanding of our leaders and our country. So the crisis prevention need was greater than ever, but it looked to me like we'd struck out.

But about ten days after that, I got a cable from a man named [Georgy A.] Arbatov, who headed their Institute of USA and Canada, which was their main scholarly institute that interfaced with the United States, saying would I come and spend a week or two there and talk more about crisis prevention. Well, that made me know that they were interested, but I didn't feel that I was the big expert on the subject, and furthermore, I had a very demanding job at the Institute of Medicine. But I did persuade Graham Allison and Alex George to go separately -- we had no money -- on their own money, their own good nature. They went and spent time and began to arrange meetings in different places. There was a professional meeting of the Political Science Association, where they would meet with some Soviet counterparts.

At least a discourse got going, erratically, on crisis prevention, and that was one of the things that went through my mind. If I had some money at my disposal and some legitimate organizing capacity, I could make this crisis prevention stuff really go. We'd already had a very good thing under way at the academy, on arms control, which I personally helped to start and participated in, from 1980 onward. And so the Carnegie

option was very appealing.

As soon as I decided to take the job, which was in the middle of '82, I sat Graham Allison down. I was then at Harvard. I sat him down and said, "Graham, you're a very successful dean of the [John F.] Kennedy School [of Government, Harvard University]. You've built it up wonderfully. You cannot do that anymore, not just that. You've got to get a good deputy. You've got to get help administratively. You've got to come back to this crisis subject. You know more about it than anybody in the world. You've got to spearhead a joint crisis prevention group." I hadn't even moved to New York yet, but I felt it was such an emergency, that if I could get him committed to it, as soon as I got to New York I could persuade the board to make a grant. And that happened.

So the crisis prevention group got going, and then it was steady. It was twice a year formally meeting, once here and once there, and a lot of flow back and forth of younger scholars in between to prepare for the meetings and exchange materials. It got to be a broader discourse of not only crisis prevention, but ways to wind down the Cold War. That's what evolved from it. I think it was a significant part of the mechanism, with feedback to Gorbachev years down the road. We started before Gorbachev.

So that Pugwash '78, which was kind of improvised and which had no obvious follow-up, grew into this joint study mechanism and major follow-up in crisis prevention in other fields. And it gave me great encouragement that maybe I could do something when I got to Carnegie that wouldn't be just damn foolish.

Q: Would you lay out the structure of the Avoiding Nuclear War program in terms of what

your major efforts were, and, following on that, the concrete results that came out of the work?

Hamburg: We wanted to play a role in the analytical underpinnings for major reductions in nuclear weapons and the missiles that would carry nuclear weapons. We wanted particularly to focus on the first-strike capability. We wanted both sides to cut back on the capacity for first strike, because that's so enormously threatening. The more you build up first-strike surprise capability, the more I have to build up to match it. So we simply wanted to get the ablest people we could anywhere in the world to do the analytical work that might be useful, if and when the political leaders wanted it.

Now, it's interesting that in our early arms-control ventures it was not uncommon for me to say to the groups we were supporting something like this, "I don't know when, if ever, the work you put in will be put to use. It all depends on the political leaders. What I want is for us to have on the shelf, so to say, good analytical work that shows how we could both reduce the weapons and reduce the danger, both the structural arms control in reducing numbers and the functional arms control reducing the risk of an inadvertent firing, to do that work so that sooner or later some day, somehow it may be useful." And they were willing to do that. I mean, great physicists and engineers and other scientists spent a huge amount of time, energy, and aggravation in that work, knowing the importance of it potentially, but not knowing if it would ever be put to use.

Now as it turned out, it was hugely valuable once Gorbachev and Reagan got around to making the political decisions. That kind of analytical stuff which had diffused into the arms control community, into the government and elsewhere, was very valuable. It shows

you have to be patient and you have to take some risks, and people have to invest valuable time in a mission they believe to be terribly important. You feel it's so important, if you make the least little contribution, it's worth doing, and you have to be patient to wait for it to come to fruition.

So, the arms control is one piece. The crisis prevention was another. It got to the point where many of the proposals made in and around the crisis prevention group were adopted. We supported Senator Nunn and Senator [John W.] Warner, with Barry [M.] Blechman early on in my term to do a group looking at nuclear risk reduction centers, places where we and the Soviets would both have expert professional people, day in and day out, year in and year out, examining all the risks of inadvertent launch and accidental war and ways of reducing those risks, just in the same way we had challenged the scholarly groups early on to ask how could a nuclear war actually happen. How could it happen? And then think about preventive interventions on each pathway to make the slippery slope a little bit less slippery. That was the concept behind the nuclear risk reduction centers. They did eventually get established on a more modest scale. Senator Nunn has recently produced a proposal to strengthen those for somewhat other purposes, related to terrorism and weapons of mass destruction. The concept would be the same.

So a number of proposals, like strengthening the hot line and creating nuclear risk reduction centers, and having new rules of the road to avoid nasty surprises, and starting regional consultations. That was one of the ideas, primarily came from Alex George, and at first the governments were quite awkward, but then they became rather skillful. That is, we and the Soviets would meet about different regional conflicts. At the least, we could delineate what is our vital interest, and each side could go very carefully if they recognized

that the other side had a truly vital interest at stake. "Don't push me too hard there, because I can't let you." In other places we could bargain and we could give and take a more mutual accommodation. So the regional consultations were another part of that whole scheme of avoiding nasty surprises, getting gradually more transparent, troop notifications, major weapons movement notifications, so that you won't think we're going to war when we move some troops around.

So I think that whole field of crisis prevention grew and developed and provided an impetus for what came to be called confidence-building measures. That's still important on the international security stage today, confidence-building measures.

[tape interruption]

Q: Did you want to finish that thought on confidence-building?

Hamburg: Yes, more generally to respond to your question about assessing some of the outcomes of the Avoiding Nuclear War program. I think it's important to recognize that the studies we supported got out into the general discourse, certainly in the democratic countries at home and abroad, and to a certain extent in the Soviet Union through their scientific community.

For example, the studies that we supported on arms control and crisis prevention, the studies we supported on Star Wars [Defense Initiative], the studies we supported on Soviet decision-making, they were in newspaper columns and op ed pieces and magazine articles. In congressional hearings, I think it's fair to say that after, I don't know, let's say about

1985, in the ensuing decade, probably, there was hardly ever a congressional hearing that didn't have reference to one of the Carnegie-supported studies or having testimony from one of the Carnegie-supported experts or members of one of the Carnegie-supported panels. It was just part of the discourse. There were a lot of other sources. I'm not suggesting it was the only one. It is true we were, for a few years, the only foundation deeply engaged in this kind of work, and then we gave great encouragement to MacArthur [Foundation] to come into it. It's a long story, but we played a very active and cooperative role with MacArthur, and other foundations gradually did.

We were largely alone in the field in the darkest days of the Cold War, in the early and mid eighties. But anyway, there were many other influences, to be sure, and it's hard to track down. It's not like you can put a radioactive label in medicine and follow where the label goes, but you can tell that, for instance, the prevalence of Carnegie-related testimony in congressional hearings is one very good measure that this was significant.

When President Reagan announced his Star Wars proposal, we initiated contacts with major scientific organizations in this country to examine it objectively. It was the biggest proposal in history, of vast, enormous complexity, and it needed to be studied from the point of view of technical feasibility, it needed to be studied from the point of view of economic costs, and it needed to be studied from the point of view of its effect on international relations and on the stability of delicate balance in the arms race.

So we got many experts, mostly in conjunction, interdisciplinary groups of experts from major scientific organizations -- the American Physical Society, the American Association for the Advancement of Science, American Academy of Arts and Sciences, National

Academy of Sciences, and a few of the major universities, to look at that issue. It became very much a part of the currency. These were the independent studies that were very largely relied upon to compare with what the government was saying, compare and contrast sometimes, and work out for members of Congress and for the public what this proposal could and couldn't do.

I think it was proposed in very good faith and with a high aspiration and a high moral commitment to rid the world of the danger of nuclear war, but, unfortunately, it was an awful lot more complicated than it looked at first glance. In some ways it could be very dangerous, in other ways not feasible. In any case, a long, long, long way off. So all that needed to be understood. Again, it was public education as well as policy-maker education. The independent experts involved in these studies were mainly scientists and scholars, but there also were a number of excellent recently retired military -- admirals and generals -- who had the expertise, a different kind of expertise, who participated in these studies. The important thing was their independence -- independent military, science, scholar -- to look as objectively as humanly possible at these issues, whether it be the weapons themselves or defense against the weapons or various notions about military strategies.

So I think the contribution to broadening and elevating the discourse was considerable.

There's no way to be absolutely precise about it. That was one thing.

The other thing I want to call to your attention before I shut up on this topic is the linkage functions. That is, we know that major leading figures in our Congress, just to focus on the Congress for a moment, regularly participated in these Carnegie-supported linkage meetings. Some were through the Aspen Institute, some were through the American



Association for the Advancement of Science, some were through the National Academy of Sciences. There were different sponsoring groups to which we made grants, all of which conducted themselves at a very high caliber, objective analysis. They typically weren't there to get involved in a debate of the current policy issues, but rather to build a broader factual underpinning. We would say to members of Congress, "The idea is to help you get the facts straight for the long term. You're going to have to make momentous decisions this year, in three years, in ten years, and the more you have a solid factual basis, the better off you'll be." They were enthusiastic about that notion, always have been.

So what measures can you take of whether it was useful or not? We know that we got time and again the most respected people in the Congress over about a ten-year span -- the leader, senior members of the relevant committees, Armed Services and International Relations Committees. The other leaders, Speaker of the House and so on, people of that kind, came frequently. Some came regularly. So, is that a measure of success? Well, in a way it is. People with highly consequential responsibilities, leadership of people in the country were exposed to a wider range and a greater depth of knowledge on the subject from more independent sources than they would otherwise have had, and they would give testimonies, fervent testimonies, to the value of it. We periodically examined the question, should we back off now? Should we fade out of this business? We got the most enthusiastic responses. I was like shaken by the shoulders, "David, do not stop this. This is the only time we get this sort of thing." So I would say that is a measure of success.

You could argue on a different level, well, you don't know what they did with it, and maybe they messed it up and so on. I'm very, very happy to know that leading members of Congress from both parties and both houses got a lot of information they wouldn't have

had, and took it very seriously and valued it and tried to apply it in their work. If they failed, maybe we failed, but I think that some good was done.

Let me give you one concrete case that was a big one for us. There is something in the country that's come to be called the Nunn-Lugar program, the nuclear threat reduction program. It's been going on now for almost a decade. Senator Nunn and Senator Lugar would tell you -- they've often said publicly -- this grew directly out of some Carnegie-sponsored activities. What happened was the following. There was the coup against Gorbachev. I called two quick emergency meetings, one was in Budapest and then back in New York. The one in Budapest had a number of Russians involved, some of whom had been involved in the coup -- not coup plotters, but coup defenders, pro-Gorbachev, pro-Yeltsin, pro-democracy forces. We tried to make an assessment, members of Congress and independent experts in Budapest, to make an assessment of what had happened, how dangerous was it, what to make of it. We concluded it was very dangerous, that Gorbachev might not hold power much longer, that the Soviet Union might fall apart, with indeterminate consequences, with all those nuclear weapons and launching vehicles.

So we came back. I got together all of our grantees who were experts on the Soviet Union, plus a few consultants with great experience, like Bob [Robert S.] McNamara, who'd been Secretary of Defense during the Cuban Missile Crisis, and so forth, Mac [McGeorge] Bundy, who later was a resident scholar with us, and some retired military people, in addition to our grantees, and we spent a day or so trying to figure out what we could usefully do.

One of the things that became clear was that we needed to know to the extent possible where were the Soviet nuclear weapons. There was a lot of information available, and we

had a quick and dirty study, led primarily by Ashton [B.] Carter of Harvard, with a number of colleagues, and we therefore had a quite reliable map in a couple of months' time of where the weapons were and some knowledge of how well they were supervised and all that sort of thing, because our concern was, what would be the fate of the nuclear weapons in the disintegrating Soviet Union?

Meantime, earlier, a couple of years before, I had set up the steering committee on POP. POP was Prevention of Proliferation. The steering committee consisted of me and William Perry, who was then a professor at Stanford, later to be Secretary of Defense, and John [D.] Steinbruner from the Brookings Institution, who headed their international program, a very respected scholar, and Senator Nunn and Senator Lugar, the five of us. We met regularly to talk about what the United States could do in the world to minimize proliferation of weapons of mass destruction.

So I called an emergency meeting of that group to hear Ashton Carter's report about the Soviet nuclear weapons, and Nunn and Lugar were very excited about it and were very creative in thinking about how they could translate this knowledge into legislation. They asked us to come back in a few days to meet with a larger group. They convened twenty-some senators two or three days later for an emergency meeting on what they could do about this problem. The point was twofold. First of all, could we put up money through the Defense Department to work with the Ministry of Defense in the then Soviet Union, to make sure that their nuclear weapons were safeguarded to the maximum extent possible, that they were in secure places, they didn't fall into the hands of terrorists, that they weren't sold on the black market, any of that?

Secondly, to provide scientific work for their nuclear-capable scientists and engineers so they wouldn't be inclined to go to Iran or Iraq or Libya or some place like that -- and moreover help them to turn their work gradually to civilian uses. That high-tech skill could be a source of wealth for Russia in high-tech industry. It was most clear in the case of the aircraft industry, but, nevertheless, needed to be looked at in many other ways.

So they drew up legislation to do that, to help safeguard the weapons, move them to safe storage, with responsible stewardship, and set up the scientific centers in Russia and Ukraine to employ nuclear-capable scientists and engineers. That went through.

Then before long, Perry was Secretary of Defense, first Deputy, then Secretary, and Carter was with him as Assistant Secretary in charge of nuclear weapons policy, and there they were implementing the very thing that they had earlier analyzed. In implementation they had a lot of cooperation from Senators Nunn and Lugar. Some multiple billions of dollars have now gone into that over a decade. A huge amount of actual destruction of nuclear warheads and other nuclear-related technology has occurred under that program, as well as a lot of stabilization of their scientific and technical community. It's moved more slowly than we would have wished both here and there. There were problems with it.

In early '92, Nunn, Lugar, and a couple of other senators, Perry and I and Carter, went to Russia and Ukraine to see if we could speed up implementation there and back home. This was Nunn and Lugar's initiative. They met with President Bush, Secretary [Howard H.] Baker in return, to expedite it even before Perry was in the government. Perry and Ashton Carter gave great attention to that in government. They've just written a follow-up book, Perry and Carter have, just now, with Carnegie support on that whole program.

But you talk about leverage. Every foundation talks about leverage. I don't know what we spent, a very modest amount of money Carnegie spent, and leveraged billions of dollars, federal dollars, on a huge international program which materially reduced the nuclear danger, substantially reduced the nuclear danger. If there was ever a success story in the foundation community, this is one. This is one. Not my testimony -- Nunn and Lugar and the others have very often spoken about that. It's the ideal model. Nunn is now on the Carnegie board, by the way. He views Carnegie's achievements in this field as something very special.

I don't claim it personally. What I claim personally is that I had the good sense to have an affinity for terribly able people like Nunn and Lugar and Perry and Carter, and to feel a comfortable sense in the American democracy of moving back and forth across that permeable membrane of government and non-government. My attitude is, the President of the United States has at his disposal, if he wants it, all the expertise in our country. It's all his to use. And it ought to be not just those in government, it oughtn't to be turf issues, it oughtn't to be this foundation or that university or that department of government. The whole country is his to use, and we tried to put that kind of concept into operation as best we could. Sure, we had very good cooperation from President Reagan and Bush, a lot of stimulation from them, of course from President Clinton more recently. All of them have asked me about things the foundation world could do, "Could you do this faster than we could, or better than we could? Or could you stimulate something to happen across national boundaries that might be sensitive for the government to do?" And we've tried. Some of that goes under the name of track two diplomacy.

Q: Did you have any conflicts with Reagan about the Star Wars program?

Hamburg: No, no. I didn't engage a lot with him on it. But his attitude was, by the time I met him in the spring of '84, his attitude was, he'd learned a tremendous amount about nuclear weapons since he got there, he realized that they were incredibly dangerous. He did not consider them useful military weapons. He sincerely believed, as did Gorbachev, if there's a way to get rid of the damn things, you ought to get rid of them, and he hoped that Star Wars would do that, but if it wouldn't, then there ought to be other ways to do that. You ought to try the Star Wars track and you ought to try negotiation, reducing the levels track, and you ought to try public education. He came to be a true believer in the enormous dangers of these weapons.

He was a complicated guy. He wasn't what he seemed to be at first glance. We had thought that he might be very angry about our messing around in this field, but he wasn't. His attitude was, "I need all the help I can get." But that wasn't true of everybody in his administration. He told me one time that it troubled him very much that his administration was often divided on these issues, like reopening the scientific and scholarly exchanges with the Soviets in '84. A number of his people didn't want to do that, but he did. He wanted us to be strong, we could defend ourselves militarily if we had to, we didn't have to be afraid of anybody, but from that position we could negotiate quite radically.

Q: I'm thinking in terms of your discussions earlier about leadership and the importance of leadership and the ability of one individual to make a difference. You're dealing really in a highly constructed situation here with two superpowers in terms of avoiding nuclear war. Could you describe your first meeting with Gorbachev and talk a little bit about what role

his openness played in this whole process?

Hamburg: It was very dramatic, and it was one of many points in my career when I really had to pinch myself. I have to say I wish my grandfather could be around to see it. I always was concerned. I mean, to some degree there was always within me the kid who grew up in a small town in Indiana, close to people who had fled from persecution, and feeling a little bit marginal, a little bit insecure, and lacking in chutzpah. But I felt if you had a chance, you should try. President Kennedy once said something to the effect that what's influence for except to use it. If you have it, you should use it.

So when we had these arms-control meetings between the American Academy group and the Soviet Academy group from 1980 onward -- and I was a member of the group, I wasn't a sponsor. Carnegie wasn't in support of that, that was pre-Carnegie. But I was a participant. Later, I went off of it and Carnegie supported it. But in my participation, I became friendly with a couple of leading scientists in their group, the chairman at that time, a physicist named Velikhov, who was well respected by our physicists. So I would meet on the side with one or two of their leading people and have other discussions. I figured maybe there was something else I could do with the academy or with the foundations or universities, apart from the arms-control issue. What's happening? Are there openings to improve the relationship, to reduce one danger or another?

So it happened, by chance, at the Moscow meeting of the arms-control group, what we called CISAC, Committee on International Security and Arms Control, was in Moscow a few months after Gorbachev came to power. So I had my meeting, I remember, under some steps in their academy, with Velikhov, my ritual meeting. I said to him, "You've got a new

leader. I don't know anything about him. I apologize for my ignorance. I'd never heard of him before he was appointed. I'm certainly nowhere near a Soviet expert. I don't know if he's good for us, for the cause of peace, for the United States, for friendship, any of that. I don't know. Maybe good, maybe bad. But I do know this. New leaders, when they come to power, like to have a distinctive ecological niche, to say to yourself, 'There's some way in which I can make a contribution different from my predecessors or exceeding my predecessors, whatever. I wonder how he feels about that? I wonder if we could help him see that a contribution he could make might be to begin to wind down the Cold War. Is that possible?'

So Velikhov got very excited, said, "Let me think about it. I have to think about it. I'll get back to you in a while." What I didn't know was, he was going to talk to Gorbachev overnight. I had no way of knowing his relationship with Gorbachev.

The next morning, he was waiting for me when I came in. I was five minutes late. People said, "Velikhov's been looking for you all over." Very uncharacteristic behavior. The Soviets were never very punctual. He was all excited. So he had brought some ideas from Gorbachev. There were a whole series of rapid-fire interplays that first year.

But anyway, the first meeting with Gorbachev was one which was really dramatic because he gave me a critique of the Soviet Union the likes of which I have never heard from any right-winger in the United States. He told me for seventy years the damage that had been done, and then he would sort of beat me about the head and shoulders, as if I were criticizing, and say, "We are great people. We have great mathematicians and novelists and musicians and artists," and this and that and the other. "We are a great country.



We've just been hamstrung for these years by a dictatorial regime. Dictatorial regimes cannot bring to full flower the creativity of people, and our people are great people. Don't put us down." You know. And so forth.

So it was very funny. But he said, you're -- Velikhov had introduced me as a leader of the scientific community. I was at that time president of the American Association for the Advancement of Science. I guess he really told him about that. So, for instance, he said to me, "Your scientific community should be helping us." For example, he said, "In the social sciences, you should be advocating objective social science. Our social science has been distorted by ideology. I'm supposed to be a policy-maker. How can I decide when I have no honest facts before me? I don't know what to believe in economics or social trends. What is the truth? We have to have a disciplined collection of social information that's reliable, and you have to help us with that, because we have no background, no tradition in it. We have great mathematicians, you know. We can do it." It was extraordinary.

So, toward the end, I said to him, "It seems to me you're very interested in ideas outside. If I could be of any help -- " I felt he's going to kick me out of the room for being so presumptuous to say I could bring in people or ideas, but, no, he put his arm around me and he said, "That would be a great thing. Would you be willing to do that? Would you be willing to bring people to see me, send people to see me and my colleagues? We must open up. We have good ideas. We have very good ideas, but we don't have all the good ideas, you know."

So it was fantastic. I really couldn't tell, it could have been a terrific act. I went to see Senator Nunn, who was one of the few Americans at that time who'd had any extended

exposure to Gorbachev, and we had to compare notes. He had the same reaction I did. We weren't sure what to make of it. He could be a terrific actor. One thing that made me think that he wasn't an actor was, when we got talking about nuclear weapons, back to my research on stress, he began to tremble and sweat. Now, I don't know, maybe some very good actors can do that, some method acting that permits you to evoke these autonomic responses, all my old adrenalin stuff, but he had it. Let me tell you, when we talked about nuclear weapons, he said, somewhat like Reagan said, that he'd learned a tremendous amount about nuclear weapons after he came to power, and he just, you know, he couldn't believe that anybody could think of using a nuclear weapon. What responsible person would order a weapon to be fired that could cause millions of people to be killed in a minute? Just crazy.

So I didn't think it was an act, but, still, it took a while to sort out. But I saw no harm -- I did clear it with some people in the White House and the State Department, that they didn't think it was subversive if I were to begin to organize delegations to go and visit and so on.

I have up on my wall some photographs that were taken by Deana Arsenian, who was on our staff, and still is, who grew up there and left in adolescence from Moscow. Her first trip back was on a particular delegation that I took, that had five distinguished scientists and five members of the Senate. Some wonderful pictures of Nunn, Simpson, Cranston, a couple of others. Oh, Bill Cohen, who's now Secretary of Defense, was one of that delegation, and some very distinguished scientists who are arms-control experts. That was perhaps the peak one -- ten of us for a whole week meeting with his top military people and his top economic people and so on, and then a half day with him at the end.

There's a kind of amusing story. This was in March of 1988, as I remember. Early '88, anyway. At the end, he was talking to us in ones and twos, and I said to him, "Mr. President," or whatever it was, the terminology we used, "if you stay on the track you are now, I think the Cold War might just be over by the year 2000, by the turn of the century." And when I walked out with Nunn, I said, "Gee, Sam, did I get carried away by that?"

And he said, "No, no, no. The spirit was right. It was right for the occasion. You were probably just too optimistic about the timing."

Well, you know, a year later, depending on what criteria you use, one or two years later, it was all over. Two thousand was way too pessimistic, not too optimistic a projection. Who anticipated it?

But those delegations, I think, were useful. Later, in speeches, when he was still in office and since, in speeches at Stanford, at the Council on Foreign Relations in New York, he has said the new ideas, our new thinking, which is a term they use, didn't just arise in the Kremlin; it came elsewhere. Some of our best ideas came out of American universities, came out of American foundations like Carnegie. He's been very generous in giving us credit. At least we played a role, some kind of stimulating role in that ferment of a great leader. And the United States leadership met him more or less half way.

[END TAPE TWO, SIDE TWO; BEGIN TAPE THREE, SIDE ONE]

Q: I'd like to move to a discussion of preventing deadly conflict. I realize there's an interim program, but maybe if you could follow through with preventing deadly conflict.

Hamburg: The spirit of preventing the deadliest conflicts pervaded our work, and still does. I think we've really come back to one of the great spiritual heirlooms we have of Andrew Carnegie in that work.

As the Cold War ended, I felt that we were very likely to have an upsurge of ethnic and religious conflicts. I had written about that in my second essay, second annual report essay, 1984, and had talked with some of the Soviet experts during the Cold War about what might happen in the Soviet Union with the many different ethnic and religious conflicts within and around their borders.

So we adapted the program to focus more on ethnic, religious, regional conflicts, and less on the risk of international war, though not abandoning international war either, and to try to make grants that would help us to understand and, above all, to prevent the emergence of an enormous rash of ethnic and religious conflicts around the world.

I guess it's enough for the moment to focus on the most visible part of that program, which was the Carnegie Commission on Preventing Deadly Conflict. Now, the way that came about was that I had involved Cyrus Vance as an advisor, along with a number of other people, to our international program from the beginning of my term of office, and he'd been a very valuable advisor. Then I had tried to be helpful to him in some of the missions he did with U.N. Secretary General, providing him with background material and a little intellectual support here and there, to make it possible for him to do missions on short

notice. Again, the flexibility of foundations to respond. So he had done troubleshooting for the Secretary General in South Africa and Nagorno Karabakh, and then in Yugoslavia, first vis-à-vis the Serb-Croatia conflict, and then in Bosnia, of course, ultimately with the famous Vance-Owen Plan.

I visited him in Geneva toward the end of the time when he was working on Bosnia, and I said I thought he really ought to do a book when he got back, that we'd be happy to support a book, trying to consider how that conflict came about and how it might have been prevented, especially how it might have been prevented. Part of my motivation was the following, that one of our expert groups in 1987, in August 1987, in a meeting in Europe with members of Congress, had focused on Eastern Europe, with special attention to Yugoslavia, where [Josip Broz] Tito had died in 1980. In 1987, this expert group concluded emphatically that Yugoslavia was very likely to have a violent disintegration within five years, '87 to '92.

Members of Congress asked me to report that back to the Secretary of State. They took it very seriously. I did report it, and he took it seriously. The Europeans basically said to the Secretary of State, "It's a European problem. Don't be excitable. Leave it to us. We'll take care of it." And so for a number of years, as a matter of fact, the United States was happy to let the Europeans take care of it. Of course, they failed to take care of it.

But the point, you see, that was so striking, there was early warning. The experts knew that this violent conflagration was likely to come. Later down the line, Vance had written a letter, and the Secretary General of the United Nations, at Vance's urging, had written a letter to the Foreign Secretary of Germany, saying, "Don't recognize Croatia, because if you

recognize Croatia, then there will be a war in Bosnia." It's exactly what happened. They went ahead, and it happened.

All I'm saying is, there very often is early warning, but there's not effective action to respond to the early warning and prevent these terrible things. Then the Rubicon is crossed. Once Rubicon is crossed, it becomes infinitely harder, the revenge motives and all that. After the slaughter, after the mass raping and all that, it's so much harder to put Humpty Dumpty back together again, you know, and if you could prevent it in the first place, work towards some kind of just outcome over time, some mutual accommodation among the adversaries.

Vance came back to me after a couple of months and said, "It's too hard. I can't write a book, even with a collaborator. It's too complicated. What we should do is have an international commission to look at the whole problem." So that's what we did. We had an international commission. He was adamant that I should co-chair the commission with him, because we do work well together. So that's what we did. We had an international commission for three years, and now we're in the midst of a two-year follow-up explaining all over what we've said and why. What we've done, in short form, is to set out the nature of the problem, why it's so dangerous, why it's likely to get more dangerous in the next century when everybody will have weapons, everybody will be able to destroy everybody else at the path we're now on. No part of the world will be too remote to cause terrible damage somewhere else.

So we laid out, in a comprehensive way, the tools and strategies available for prevention, somewhat on a public health model, and then we asked who could do what, who could use

those tools to implement prevention strategies effectively in the future. That's what it is, it's an overview, in the so-called final report that we put out one year ago this month, at the White House, and we are in the midst of putting out forty additional reports and books. About half are out. The other half will be out by the end of next year. So you'll have a sort of two-foot shelf of what there is to know on prevention. There are also many related publications from the grant-making program.

Since this field, unfortunately, moves slowly, I suspect that it's going to be useful for decades to come. I don't know how long. But each one goes into depth on some aspect of the final report. It's like chapter by chapter you get depth on what is known or could be known about that part of the prevention problem. So this is a big, visible, worldwide enterprise which in its first year after the final report is concentrating on high-level government policy-makers at the U.N. and regional organizations like the Organization of American States, Organization of African Unity, that sort of thing. It's been adopted wholeheartedly by the U.N. Secretary General and by many leaders in a variety of countries, I'd say especially the United States, Sweden, Norway, Japan, Canada, one or two others. More to come. A lot of interest in Russia, a lot of interest in Germany. A lot of interest in the francophone countries, being spearheaded by Boutros-Ghali, who was part of this venture when he was at the U.N.

We were running ourselves ragged, trying to explain what we said and why, and to stimulate better ideas all over the world. We are shifting our emphasis to younger people, and now we want to develop some kind of global network of cooperating universities, not a vast network in numbers, but in geography, yes, universities, at least one on every continent that is teaching preventing deadly conflict more or less along the lines of our

publications, drawing on the resource of our publications.

Along with that, there have been many grants to learn more about these issues and to put into practice on a small scale ways of de-fusing emerging conflicts. That illustrates a principle of Carnegie's operation during my time, that I believe we have carried to a certain level that's unusual. I don't recommend it for every foundation every time, but for us I believe it's worked well, and that is an interplay between a commission-like body, a commission, a council, a task force, you name it, and the grant-making. So that ideas coming up out of the grant-making challenge and stimulate and inform the council or the commission, and, on the other hand, observations of the commission challenge the grant-makers to go out and find somebody who can dig more deeply into the subject and understand it better, or to try out on a small scale some conflict-prevention idea that the commission thinks is promising, that interplay back and forth between grant-making all over the country, all over the world, the grant-making on the one side and these commission-like bodies on the other hand.

One of our commission-like bodies that was enormously interesting and hopeful was the Carnegie Inquiry into Poverty and Development in Southern Africa. If the end of the Cold War brought great hope to the world, so, too, did the end of apartheid in South Africa. Neither was much predicted. There was every reason to believe there might be a horrible bloody war in both places, maybe both even conceivably nuclear. And it didn't happen. There were some lives lost, to be sure, but nothing like what might have happened. So it's a great source of hope in a bloody twentieth century which had the worst war in history and which had the Holocaust. There are these great sources of hope.



Our inquiry in South Africa, we believe -- and I think most South Africans believe -- played a significant role in helping to roll back apartheid. It was started, to his great credit, in Alan Pifer's time. It was early in its development when I came to office. The only plea that Alan Pifer made to me and to the board was to continue the work in South Africa.

Otherwise, he kept scrupulously out, as I have with Gregorian, but he did make this plea that it was at such an early stage and so important, that we should pursue it. I took that very seriously and did pursue it. We modified it some, and it developed a lot more in ways that we hadn't foreseen, but we carried it forward.

There are a few general points about that inquiry. First of all, as was characteristic of my time, one of my clichés at Carnegie, we wanted to get the facts straight, facts about poverty and development: how bad was poverty, who did it affect, who did it not affect, what possibilities were there for relieving poverty, how could you envision development in South Africa and Southern Africa, in the region. That was one piece of it.

The second piece was to do that in a way that would involve blacks and browns and not only whites, to embed it as much as possible in the black community. For all their deprivation, they were very gifted people, like Desmond Tutu and Mamphela Ramphele, not to speak of Nelson Mandela, and Thabo Mbeki, and Cyril Ramaphosa, and many, many others. There were many gifted people, even though a tiny fraction of blacks were getting educated at that time.

We tried to involve many accomplished and promising younger blacks and browns, coloreds, in the enterprise, right across the spectrum of all those maddening South African racial classifications. We based it at the University of Capetown, where a very courageous vice

chancellor, Stuart Saunders, a white man, protected it, nurtured it. A brilliant economist, Francis Wilson, white, was the first director of it. As soon as we could get Mamphela Ramphele freed from where she'd been banished out to the boondocks, to bring her back and to work on it, and then she became the co-director with Francis Wilson.

There were various task forces that covered South African society, a task force on education and poverty, a task force on religion and poverty, task force on business and poverty, you name it, twelve, fifteen task forces, and, where possible, headed by a prominent black. Desmond Tutu brilliantly spearheaded the task force on religion and poverty. In any case, headed by very competent people with leaders from different sectors, a mixture of people from different social backgrounds, trying to work out how could this sector could pull itself up by some bootstraps or other, how could we help to overcome racial discrimination and poverty in southern Africa.

Many volumes were put out of a factual nature. There was a lot of research training of blacks in the behavioral and social sciences, in vital statistics and the facts that would be needed to make social policy more wisely whenever the politics would permit that. And so as it went along, we used it as a vehicle for dissemination about South Africa in South Africa and in the United States and also in Western Europe. We did a number of collateral activities in which we drew upon the resources of the inquiry. We had an art exhibit in many parts of this country, photographs taken for the inquiry -- the face of poverty in South Africa.

I should say we built upon early Carnegie history. There had been a late '20s, early '30s study of a similar kind, but it was the Poor White study. Blacks simply didn't come into it

at all, they were non-persons at that time. We basically revisited that study, looking at the whole population, which, of course, meant in our time poverty was very largely black.

One of the things we did was to get very promising people and try to nurture their careers. We'd bring them here for a time or bring them to Europe for a time, provide research training in South Africa, provide networking in South Africa and outside. And I'm very happy and proud to say that many of those people are leaders in South Africa today, particularly in the government, also in universities, to some extent in business. There were a number of blacks, particularly, who got their first start or their main chance out of some experience with the Carnegie Inquiry. When you go to South Africa, you can explore that more fully.

Ramphela is a wonderful case in point. She was [Bantu] Stephen Biko's common-law wife. She bore children by him. She's a brilliant person. She was banned. When I visited her in 1984 out in the boondocks, I could see how brilliant and ingenious she was, and felt we ought to make every effort to get her back to the university, even if it would be initially in a very modest capacity, but to be there and to be part of the mix. Of course, she rose very rapidly once she came back.

I had the honor of being denounced by P.W. Botha in the Parliament. It wasn't a terrible denunciation. When I gave a speech in South Africa at a conference on the inquiry, he said something like, "Dr. Hamburg undoubtedly means well, but he doesn't understand South Africa. We have our own customs and tradition, our own culture, our own justice," and so on. "But meanwhile he should go away and leave us alone. He's only making trouble. We don't need outside agitators." [Laughter]

Q: You won an award two years ago, a year ago, and I'm wondering if it partly has to do with all your efforts in the peace area. Could you talk to us about that?

Hamburg: I've been much, much honored far beyond anything I could ever have dreamt, but there are two that are especially meaningful to me and just off the scale. Well, maybe three. I'd say the first honorary degree I ever got was from my alma mater a long time ago, in the 1970s, from Indiana University, and my scientific mentor, Tracy Sonneborn, was the instigator behind it. That was a very moving occasion.

A couple of years ago, President Clinton gave me the Medal of Freedom, which is meant to be the highest civilian honor we have, and I must say I was very deeply touched. In the citation they dealt with my whole career, but both the domestic and international side of it. I was touched, on the morning of the event, President Clinton, looking at the citation, told some of his colleagues that he felt there was not enough said about the international and peace side of things and the African side of things. There was a lot about the domestic side, education, child development, what have you, behavioral science, but he wanted a balance, a more balanced statement. So they scurried around and got that done. I was very touched by that. In any case, it was quite fantastic to get that award.

Then this year the National Academy of Sciences gave me its highest award, the Public Welfare Medal. Since the academy has been so important in my life, that was very, very meaningful. All three of these were total out-of-the-blue surprises, and they cited both domestic and international activities. The Public Welfare Medal is symbolic of our academy, that is, the academy not only recognizes great achievement in science, but also it

recognizes the uses of science for the benefit of society. In that, it really differs largely from any other academy in the world. So those awards were enormously meaningful to me, and I cherish them.

Q: On the subject of human welfare and back to Carnegie's programs, I wonder if you could talk for a few moments about your programs in the area of child development--.

Hamburg: Indeed, there is a bridge in the sense that I've long been interested in education for conflict resolution, education for mutual accommodation, learning to live together. That is, groups have to come to terms with their differences. I think that education is an important part of that -- schools, community organizations, religious institutions. That's, of course, only a modest but highly significant part of what education has to be in the next century.

When I came to Carnegie, as I said earlier, we tackled the problems of the fundamental underpinnings of child and adolescent development. So we developed, over those years, a developmental strategy, starting with conception and going on through adolescence. I'll give it to you developmentally, though we didn't do it in chronological sequence quite this way, for very practical reasons. It wasn't neat and tidy aesthetically as it might have been to do first zero to three, then three to ten. But in the end, we did all those.

I should say, the key structure of it was to make grants both to fill gaps in research, gaps in knowledge, and to support carefully assessed innovations in education, and to use our convening function to get together people from different sectors of the world of education and child development, to learn from each other, and then from time to time to have high-

visibility, high-quality groups from different sectors of American society that would try to make a synthesis. What is known about the zero to three age group? What is known about three to ten? What is known about ten to fifteen?

Now, those groups typically were from different sectors. About half the group would be experts on the subject -- educators, pediatricians, child psychiatrists, developmental psychologists, you name it. And the other half would be leaders in business -- the CEO of a pharmaceutical firm or Ted Koppel of ABC, Admiral [James D.] Watkins, who'd been head of the Navy and then Secretary of Energy. Leaders in different sectors. The idea of these intersectoral groups was both that you'd have a broader mix and more stimulation and more ideas, and also that you'd have more opportunities, when you finished your work, to open the door and get into different sectors, to say, "Look. It's important to reach out a helping hand to our children."

So those groups, I think, were powerful intellectually and perhaps even politically, and they also had a wonderful interplay with the grant program. First there were two grant programs chaired by [E.] Alden Dunham on the one hand, Vivien Stewart on the other, and then later a combined program chaired by Vivien Stewart. Each task force and grant-making stimulated and helped the other. I think it's an art form that I cherish, although it's hard to do, and I certainly don't recommend it as any kind of panacea for the foundation world. I don't even recommend it for the next phase of Carnegie's life. But in this particular phase, it was very useful.

Now, the zero to three task force was first chaired by Dick Riley, former governor of South Carolina, who had been a wonderful governor on these issues of young children. Then

when he became President Clinton's Secretary of Education, two distinguished scholars took over: Julius Richmond from Harvard, who'd been the father of Head Start, and Eleanor [E.] Maccoby, sort of the dean of developmental psychology, professor at Stanford. They took over and co-chaired and saw it through.

A very touching aspect of that was that my dear friend Jonas Salk served on it. He was ill - it was the last body of that kind that he served on during his life. He was too ill to attend meetings, but we talked on the phone regularly and we corresponded. He sent me a message to be read at the meeting, where we brought out this report, a meeting at which Hillary Clinton was the keynote speaker. That report emphasized the health side of it, prenatal care, and primary health care for young children in a broader conception, prenatal care, for example, that included an educational component and a social service component, not just a medical component in a narrow sense. And so, too, with early primary health care.

The second thrust of it was an educational component, not warehousing young children, but preschool education truly, more or less on the Head Start model, for poor kids and rich kids alike, some of the attributes of high quality preschool education, high quality child care.

Then the preparation for parenthood, various opportunities for preparing for responsible parenthood -- parent education in different settings, ways of getting the knowledge and skill to be a good parent, especially in this complicated time when both parents are likely to be in the paid work force outside the home, and a very complicated mix of caretaking by parents and by others, how to strengthen all the relevant institutions.

And, finally, the fourth strand of that report was on community mobilizations, how we can bring together the different sectors on behalf of our young children. If you can't get mobilized over babies, I don't know what could bring us together. So, that zero to three was one piece.

The second piece was middle childhood and mainly elementary school education, although it had some pre-elementary stuff in it, and some transition from elementary to junior high, but mainly around very important recent research particularly coming out of Yale, coming out of Johns Hopkins, and applied to school systems all over the country. Ways of raising standards, helping kids, especially poor kids, to meet the standards, improving teaching as a profession. So that was an important report, co-chaired by Admiral Watkins, who I referred to before, and Shirley Malcom, head of educational activities at the American Association for the Advancement of Science. Again, intersectoral group, high visibility, coming-out party. Tried to call national attention each time to get a lot of coverage. We were extraordinarily lucky in getting very broad and largely accurate and positive media coverage of these reports.

And then to have follow-up activities afterwards for several years to see what would happen if you implemented the recommendations. But the most ambitious of all of these was the Carnegie Council on Adolescent Development. Shall we just go straight through into that?

Q: Yes.

Hamburg: That was a ten-year effort. It overlapped with these others, '86 to '96. We



began it as a three-year effort, but it had such a powerful effect in the country that the board felt there was no way we should wind it down. It was intersectoral, it had leading people, terrific people on it, and they worked very hard. We had a staff in Washington, and they interacted with the grant-making staff in New York.

It was a stimulating and guiding body, the council itself was, and we set up, or stimulated others to set up, a number of convening functions and major studies and dissemination functions on various topics of adolescence. Last I heard, I think we were in the neighborhood of three-quarters of a million of our volumes as of a month or two ago, had been distributed, and there had been a vast amount of media coverage, a lot of interaction with governors and presidents and cabinet members and so forth. The creation of an Office of Adolescent Health in the Department of Health and Human Services, many ramifications of that council.

Basically what we did, we put out major reports on -- well, first I'll say we focused primarily on early adolescence, ages ten, eleven, twelve, thirteen, fourteen. We recognized and did some things on middle and later adolescence, but we mainly focused on early adolescence, because it's so neglected. I have to say -- I should have said before -- I learned a lot from my own kids about early adolescence, not only when they were early adolescents, because we were very close, but since. My kids, as well as my wife, have been collaborators in almost everything I've done, and they've gone on to wonderful accomplishments, of course, in their own right. I learn much more from them now than they learn from me. But the early adolescent focus, they undoubtedly helped me and my wife to make that focus, because it was so neglected and so important. You suddenly go from childhood to something like an adult, puberty pops out, and all these biological changes, psychological

changes. You go from a little elementary school in the neighborhood to a big junior high school. You go from being expected to be a child to be something like an adult, but you don't know what. Huge biological, psychological, social changes converging on this fascinated participant-observer, the young adolescent.

We felt we ought to look at schools, we ought to look at the health system with respect to those adolescents, we ought to look at community organizations, including religious ones, we ought to look at the media, all the front-line institutions that have an impact on them. That's the same philosophy -- we did that with younger children, too, but perhaps we did the most on the adolescents.

So there was one major report, the earliest one, called *Turning Points*, and what we did there was particularly significant, I think, perhaps as a model for other work. We got together an intersectoral task force again, on upgrading school education in early adolescence, junior high or middle schools, what we call generically the middle-grade schools. We looked at the curriculum, we looked at the organization of schools, we looked at the surround of schools. It was my belief, my passionate conviction, that it's not worth very much to just look in the classroom, important as that is. It's not enough to look just at what happens in the school building, important as that is. We as a society have to consider all the factors that influence learning in and out of the school. That means substance abuse and crime and television, a whole host of influences -- now, of course, the Internet. And not just what goes on in class, although it's terribly important, what goes on in class.

So that's the approach we took to this *Turning Points* report. One of the members of that task force was William Jefferson Clinton, then the governor of Arkansas. We had a set of

education governors that we involved from the very beginning in Carnegie activities, some Democrats, some Republicans. Tom Kean, Republican from New Jersey, is now the chairman of the Carnegie board. We had wonderful governors -- Jim [James B.] Hunt [Jr.] of North Carolina, Lawton Chiles of Florida, Dick Riley of South Carolina, Kit [Christopher S.] Bond of Missouri, Michael [S.] Dukakis of Massachusetts, and so on. The governors really awakened the country to education, and I'm happy to say we had a hand in that. When we began, it was considered not a good political issue, and now it is considered a very important political issue, I hope not too partisan. Sometimes it is too partisan now. But anyhow, Clinton made a good contribution to that report, and when we brought out the report he was the principal spokesman for it on national television and for the print media, as well.

But the substance of the report was formulated by experts on education and child development, and much of it is still going forward at the present time. We advocated smaller units. Break down these factory-like schools into smaller units, a school within a school, a house, whatever, various ways of doing it, but organize it in ways that each individual can get sustained individual attention. We advocated having an organizing principle intellectually, which should be the life sciences, because the kids are experiencing or have recently experienced puberty and their curiosity is enormous, and you want to direct that around a life sciences curriculum which includes a lot of health information in it. High-risk behavior is related to each functional system of the body. You talk about respiration, you talk about smoking. You talk about the brain, you talk about drugs. You talk about reproductive biology, you talk about sexually transmitted diseases. You talk about cardiovascular system, you talk about diet and exercise and smoking.

So among other things, the grant-making program stimulated a superb new curriculum, which just fully arrived on my desk last week, multiple volumes, an interactive lively curriculum, growing out of Stanford's human biology program, of which I was one of the founders thirty years ago. They've adapted it. Ten years of work adapting that curriculum for Stanford undergraduates to the middle-grade schools. So we had then this organizing principle for a curriculum which linked education and health, and we had the smaller units and ways of getting sustained individual attention.

We had life skills training, including systematic training in decision-making. Rather than making impulsive, ignorant decisions, learning how to make informed and deliberate decisions. Also life skills training -- which my wife, by the way, is a major contributor -- life skills training that deal with peers learning from slightly older peers about how to make friendships and how to resolve disputes without violence. These are practical skills for getting along in the world, that have not traditionally been a part of the curriculum. They can be taught in schools, they can be taught in community-based organizations like Ys or Boys and Girls Clubs, they can be taught in religious institutions. But they're important to connect with the traditional curricular material.

We talked about mentoring and other social supports, ways in which particularly lonely or isolated or very poor kids, especially kids in disadvantaged communities, could have some sense of attachment, of reliable human relationships, with adult mentors or with slightly older and more fortunate peers.

So that's all in there, and more -- the relationship between community organizations, supervised academic community service, learning to be useful to others beyond the self,

transcending ego-centered orientations.

Then we did a follow-up to that report. Partly thanks to Clinton and partly to others who participated in the coming-out events for that report, we got a lot of public attention on it. Then we set up a competition among states -- ultimately fifteen states participated -- in implementing these reports, the recommendations of the report "Turning Points." And then we had independent researchers, principally then from the University of Illinois and UCLA, assessing implementation. Number one, the fidelity of implementation of recommendations. Number two, did it make a difference? Were academic results better? Were interpersonal results better?

That's a very interesting model, because it might turn out the reports were plausible and attractive, but they didn't work. In this case, it did work. In general, the more nearly it was implemented, the more faithfully it was implemented, the better the results academically and otherwise. Of course, that's very heartening. It takes a period of years to do that. But this is an illustration of what I mean, an interplay between the grant-making program and the commission or task force.

The follow-up to a report, you don't just put it out there and sink without a trace. You put it out there, you disseminate, you explain, you stimulate interest, you stimulate better ideas, but you also stimulate studies to see does it work. Can you get it implemented? And if it's implemented, does it work? So that *Turning Points* was a good model.

I'll briefly mention, we also had an important report written by Fred Hechinger, who came up earlier in this story, written in a way that would be accessible to the general reader,

called *Fateful Choices*, decisions kids make about smoking and weapons and so on that affect their health and the health of others. So that's an adolescence health, disease prevention, health promotion approach, another volume parallel to *Turning Points*.

Then there was one called *A Matter of Time*, the time being out of school, the role of community organizations, as much time out of school as in school. A lot of bad things happen in the out-of-school hours these days. So we laid out what the potential is for community organizations, the general notion being to lure kids any way you can, make it attractive, with food, with music, with sports, with interested adults, with interested older peers, get them in, in the after-school hours, and then have educational activities and health-related activities, tutoring, mentoring, what have you, in community activities. The idea is to cover the waking hours of the kids with an opportunity, an array of constructive activities in school and out of school. If you put *Turning Points* side by side with *Matter of Time*, you've got the waking hours covered.

We also stimulated some other organizations to probe more deeply into adolescent health, so we went to health and education and the social environment of early adolescence, and covered it pretty comprehensively with these reports, all of which had the aspiration, at least the aspiration, to be intelligible and credible, intelligible because they were translated out of technical jargon into straightforward English, and credible because they were based, to the maximum extent possible, on research and the most carefully evaluated innovations in clinics and schools throughout the country.

That was the approach. It went ten years. I think it was probably the most serious sustained effort ever made on issues of adolescence in this country. It's now being followed

up by a forum on adolescence at the National Academy of Sciences, which I chair for a while. I'll pass the baton in a year or two to somebody else.

But adolescents are a hard sell. I'll stop with that message. We've got to keep thinking about adolescents. They tend to be perceived in a way that's frightening by adults -- wild animals, raging hormones, all that stuff. Young babies are a much easier sell, in my experience than the adolescents. Adolescence is a fateful time, a crucially formative time, just like infancy, and we've got to pay more sustained attention in this country and all over the world to the fate of our young people.

Q: Another question.

Hamburg: Sure, have no mercy.

[Laughter]

Q: When you first came to Carnegie, I asked you earlier what your impressions of philanthropy were, and they were rather vague because you hadn't thought a lot about it. Now what are your impressions of what role the philanthropy and the foundations should play in American society?

Hamburg: Well, it was Alan Pifer who said to me early on that the great thing about foundations is that they have such scope and flexibility. I learned that from him, and he's absolutely right. That scope and flexibility includes the opportunity to develop somewhat novel art forms, like the one I've been hammering on, of the interplay between a grant-

making program and commission-like bodies.

I think also foundations, at least the older ones that have good names like Carnegie, established names, but, in general, foundations have a wonderful convening power. You find you can get people of very high quality and dedication to come together from different backgrounds, to have some serious exchange on neutral turf about important issues. Another thing I would say is that a foundation like ours can be a wonderful interface between research and social action. You don't have to. Some foundations just support research and do it superbly. Some foundations just support social action, although I confess I wonder sometimes, "What are they advocating for?" I don't know where you get the knowledge on which to advocate except through research.

But anyway, our conception is that the interface between research and social action is a very good one because it gives you the best chance to have well-documented, well-formulated, constructive social actions which you can help to inform and advocate about. Those are some of the things.

I think you have to keep light on your feet. You have to keep looking for new opportunities. You have to see when a line of inquiry or innovation has been played out. You need to shake yourself up about every five years or so. But I think the scope and flexibility is unmatched. I think you can go into some areas of sensitivity where government might shy away from or otherwise it would be too partisan or too ideological. I'd say, for example, on matters of reproductive biology and women's health, some foundations are doing a lot of good things. We haven't done a great deal. We've done some. I think you can help



universities to look more at social problems than they traditionally have, apply some of their expertise to social problems. Those are some of the ways in which foundations can be helpful. I'm sure there are many others that I haven't thought of.

[END TAPE THREE, SIDE ONE; BEGIN TAPE THREE, SIDE TWO]

Q: You were talking about the big dependable foundations. Being a little bit of a foundation-watcher, I've heard a lot recently about partnering, foundations wanting to partner together. I was wondering -- I'd just like to get your historical perspective on that, when it's worked and when it hasn't worked, what you think about it generally.

Hamburg: I think there's a lot that can be done. We were very open to that in my time, and we have a number of interesting cases. Some are quite explicit, where two foundations, or conceivably more, get together to start something together. Like we and the Ford Foundation got together to start the Center for Children in Poverty, and placed it in the School of Public Health at Columbia University. I think it's been a very useful function. That's one way of doing it.

It's not easy to do. Each foundation has its own policy guidelines, its own identity, its own pride, its own turf sensitivities, sometimes the "not invented here" syndrome. It's not easy to start from scratch, but we've done it a number of times, and you can do it, but it takes a great deal of patience. You have to be very, very careful to the sensitivities of the other foundation and be prepared to let the lion's share of the credit go elsewhere, if there is credit.

Now, there are a lot of circumstances where you can share in an individual grant, informal contact between staff. "It looks promising to us, but we don't have enough to make it work. Does it look promising to you?" Sometimes it will, sometimes it won't. But where staff can have that kind of an interplay. We did a great deal of that. There are many, many of our grants that, from the start, were done jointly with some other foundation, but not in a very highly visible way. That's quite possible. In my experience, the higher-visibility ones are really hard to do, but there are many ways in which you can partner.

An example for us that was kind of dramatic was with the MacArthur Foundation. I had been advisor to MacArthur in its early years before I came to Carnegie, so I knew the board quite well. When I came to Carnegie, I hit the ground running on the Avoiding Nuclear War program. One of the MacArthur board members, Rod [John Roderick] MacArthur, the son of the founder, heard about it, took a great interest in it, and he and several other board members, namely Jonas Salk and Jerome Wiesner and Murray Gell-Mann, all distinguished scientists, the four of them called me together from a MacArthur board meeting and asked me if I could come out the next day for the second day of the board meeting, and tell them about what we were doing, and try to accelerate the process, help them persuade their board to move in to some similar program. Indeed, on the phone they proposed that simply they would adopt the Carnegie program in order to move rapidly.

I did that. I dropped everything and went out. On the plane going out, I thought, "It's not a good idea for them to adopt the Carnegie program. Even if they should do it at the moment, I think there might be repercussions that would not be suitable for their pride. Let me think about a way to do it that could be spiritually the same program or in the same big tent, but not exactly the same."

So I thought about some division of labor, and I thought about the device of a joint advisory committee for both foundations. This we worked out. It took a little doing, but we worked out a joint advisory committee chaired by McGeorge Bundy, and over a period of some months it made recommendations to both foundations. It started out some division of labor very informally. So their program grew directly out of ours, but it wasn't the same. It was a very good partnership for years. For instance, when they had their press conference to announce their program, they asked me to come, and I said, "No, I think it's better, it's your program. You announce it." I just felt their sense of pride, of ownership should be theirs. I said, "You say at the press conference, 'We're cooperating with Carnegie on this.' Fine."

So, that's what we've done. There are different ways of partnering. I do believe it takes a lot of sensitivity. It's easy to say these days, there's way more talk about it than there is practice. I'll give you a good example in our experience at the Carnegie Council on Adolescent Development. From day one, we had an open-door policy. We invited representatives from many foundations. Very few were in adolescence at the time. We hoped more would come. I looked over my notes from the first meeting. I said to them, "Please feel free to take up any part of this agenda that's interesting. Don't ask us any questions. If you want help, we'd be happy to help. You don't need our help, probably." A number of foundations did initiate or strengthen programs on adolescence over the years. Some of them referred to the Carnegie Council, some didn't. I know of one program where it was a direct offshoot of Carnegie and they never mentioned Carnegie, and I felt very proud. I told my staff, "Don't say anything. It's wonderful that they have done this, and it would be nice if they gave us a little credit, but if they don't, it's okay, too."

And all the way through its ten years, there were many kinds of foundation involvement. Ruby Takanishi, who was our staff director and now is the president of the Foundation for Child Development, Ruby would tell you better than I could all the many different foundations that came to our meetings or had informal contact with her or got data from our office in Washington. Or Vivien Stewart can tell you some of this. I think it had a profound effect on the foundation world, though we never, I never, at least, made an effort to map out. That's another kind of partnering, if you want.

I think the kind of partnering where you're going to get four or five foundation presidents to announce together some huge slam-bang initiative is going to be hard to do. When push comes to shove, I think that that will rarely happen. Once in a while it will. I don't know that it's important to happen. It can be important sometimes in the sense that it's a statement to the public at large, that this is so important that several foundations are really willing to commit real resources to it. But my main point to you about partnership is that there are many varieties of partnership and people like John Gardner and Alan Pifer and Vivien Stewart and Ruby Takanishi and others can tell you, Barbara Finberg, can tell you chapter and verse, Dave Robinson, of different ways of cooperative efforts among foundations, and also, by the way, between foundations and other institutions, government agencies and what have you.

Q: I guess another question that I have that we just simply haven't gotten to today is for you to talk briefly about the kind of board that you created at Carnegie and how you wanted that board to relate to the staff and issues of leadership.

Hamburg: I'm very proud of that. I invested a lot of time and energy. I was blessed to

have the opportunity to have a lot to say about board selection. I didn't make the selections alone, but I was very active in the process, I did a huge amount of homework, checking out very, very carefully how people had functioned in other board situations. The best predictor of behavior is behavior in similar situations. I wanted people who were extremely able, maybe visible, had earned a great deal of respect, came from different backgrounds, different professional backgrounds, different social backgrounds, black, white, green, whatever you want. Not only the traditional lawyers and bankers, although they're very important, but scientists particularly. I recommended more scientists, physical scientists like Sheila Widnall, who later became Secretary of the Air Force, biological scientists like Josh Lederberg, the great geneticist, as well as behavioral and social scientists. A variety of people from the sciences as well as from the professions on the board, people who accomplished a lot in their own right, people who were accustomed to working well with high-level people from other fields.

Of course, in the financial side, real experts, authentic experts. We were privileged to have people like John Whitehead, who had been the co-chair of Goldman Sachs, later Bob Rubin, now Secretary of the Treasury, who also was from Goldman Sachs, Dick [Richard B.] Fisher, who was the CEO of Morgan Stanley. Real leaders in that community, giving the guidance for the investment of the money. I felt we, by and large, ought to leave it to the real experts in that domain, although we wanted experts in that domain who were interested in education, interested in peace, etc., as most of them were.

So I think we had a board of very high caliber. Then I have to say I scared the staff initially by proposing to engage board members in our commissions, task forces, etc. Most of them were. If they wanted to be, they were. Either they were deeply engaged in that or

deeply engaged in the management of the assets. Some did both, but it's hard to do both. All I'm saying is that our board members were deeply engaged in the substance of the work of the foundation. I think they did superbly on the various commissions and whatnot. The Watkins-Malcolm Task Force, "Years of Promise: The Middle Child Education," they were both board members at the time. I didn't feel that they had to be board members, but no reason why not.

It's tricky. I admit you could have problems about that. I encouraged the maximum interplay between staff and board. I didn't want the board communications to just go through me. I invited virtually the whole professional staff to attend board meetings. I realize a lot of foundations don't think that's a good idea, a lot of universities don't think it's a good idea. It depends on your circumstances. For me, maximum interplay on a collegial basis, earned mutual respect between staff and board, was desirable. I thought there was mutual benefit in education, and it was good for morale. But it doesn't have to be that way.

Q: We have just a couple of minutes, but I realize we had an elliptical conversation earlier about utilizing large institutions, say, for example, in implementation like the World Bank. We were just speaking of the World Bank, in particular, to help with the concrete application, implementation of some of the initiatives that are happening, particularly with preventing deadly conflict. Could you just talk briefly about that?

Hamburg: When we would develop, through our grantees or consultants or staff or board, some ideas or innovations that seemed promising, we would discuss it along the way in an early stage with people from other institutions that would have much more resources and might take an interest or might not. So in the case of the World Bank, for instance, as we

were working on science and technology for development, or we were working on ways to prevent maternal mortality in developing countries, we would discuss it with relevant staff at the Bank. From time to time I would have the privilege of discussing it with the president of the Bank, sometimes take a staff member or board member with me to meet the president of the Bank. I was lucky to have such access going back to McNamara's time. There were times when things got picked up, where there would be a joint effort and typically the bank would do its thing in its way, and we would do our thing in some complementary fashion. They had more resources.

In the next few weeks, Cyrus Vance and I, as co-chairs of the commission, together with Jane Holl, the executive director of the Commission on Preventing Deadly Conflict, will meet with Jim [James D.] Wolfensohn, the president of the Bank, and probably he'll bring some of his senior staff, I expect, to convey our ideas that are reflected in a small book we've just published about how the World Bank could have a larger role in preventing deadly conflict. It may come of something, it may not. He certainly will be open-minded about it, Wolfensohn, I'm sure. So we've tried, where we can, to do that with the World Bank, with the National Science Foundation, with other institutions, governmental or intergovernmental organizations that have far more resources than we do, where the communication is free and open, and sometimes things take hold.

Q: Is there anything else you'd like to say or talk about?

Hamburg: Well, for the moment I'm pretty well talked out. I'll probably have some inspiration later. Thank you very much. It's been fascinating for me.

[END OF SESSION NINE]