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TRANSCRIPT OF A TAPE RECORDED INTERVIEW WITH ELIZABETH KUTTER

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Interview with Elizabeth Kutter

on June 17, 1977

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MIT ORAL HISTORY LABORATORY

Project on Potential Safety Hazards of Recombinant DNA Research

Interview with Elizabeth Kutter by Charles Weiner

Evergreen State College, Olympia, Washington May 19, 1976

transcribed by Janet Billane

Weiner: Today is the 19th of May, 1976, and this is Charles Weiner. Why don't you introduce yourself?

Kutter: I'm Betty Kutter, a member of the faculty in biophysics at Evergreen State College and a member of the NIH Recombinant DNA [Advisory] Committee.

Weiner: I want to start from the beginning. I know that you were born on August 11, 1939 in Chicago. I'd like to hear you talk about your family, about their background, where they came from and the occupation of one or both of your parents, education . . .

Kutter: Well, my father is an electrical engineer, and he was also born in Chicago. Both of my parents went to the University of Illinois. My mother got a degree in business but never used it later, and, in

fact, can't balance a checkbook and has no idea of what my father makes, so is not very interested in that kind of thing. She was a typical mother and housewife mainly, and was pushed even further into that role by the fact that my great-aunt, who was a complete invalid with arthritis, lived with us from the time I was five until the time I was in the ninth grade, when she died. So that was a lot of extra burden of care on the family. My father's father was a lawyer in Chicago. [Dad] went to the high school at the University of Chicago--a special high school there--graduated when he was sixteen, and then got his degree in engineering at the University of Illinois. That was clearly towards the end of the Depression when jobs were hard to come by. I'm not actually sure who he was working for in Chicago when I was born. He got a job when I was a year old with Lockheed, and we moved to California. He then went into the Army Air Force after we'd been there only a short time, so we did a lot of moving around when I was young. We moved to Tacoma, Washington, and then to Brownwood, Texas; Yellow Springs, Ohio; Dayton, Ohio; Boston, Massachusetts--

Weiner: These places were Air Force bases?

Kutter: Yes.

Weiner: And this was during the war?

Kutter: Yes. Well, we went to Boston when I was five, and he went to MIT for a year. After that we went back to Dayton, and then he got out of the Army Air Force and went to Jack and Heintz Co. in Cleveland.

We were there for three years, and then moved to Seattle. When he moved to Boeing, I was in fifth grade, and my parents still live in that same house. When I was young, I did a lot or reading, rather than trying to get to know people. After a while of moving so often, I tended to escape a lot into books. A lot of them were adventure stories, things like Howard Pease's ocean stores and also things like Les Miserables, Count of Monte Cristo. These were the kind of things I started reading in late grade school.

Weiner: Were there a lot of books around the house?

Kutter: Yes, there were a lot that had come down from my grandparents, classics mainly. I probably read less of those than ones from the library; I always spent a lot of time at the library. And I had some friends when I was younger. A lot of them were my sister's friends.

Weiner: What's the age difference between your sister and you?

Kutter: She's two and a half years younger than I am.

Weiner: And that's the only other sibling?

Kutter: Yes.

Weiner: How many different schools did you attend before you entered high school?

Kutter: A lot. Well, I went to nursery school in Yellow Springs, and kindergarten at first in Dayton, and then in Massachusetts. I went to

first grade in Yellow Springs, Ohio, and then second through fourth in Cleveland, and then started with fifth in Seattle and just did the regular progression from then on. Though, really, once I was in school we weren't moving that much. The pattern was more one that developed before. I was also pretty much interested in mechanical things and scientific things from the time I was fairly small. My Dad gave me a couple of electric trains for my second birthday which was before he even knew whether my sister was going to be a sister or a brother. And I got a third train for Christmas that year, and always enjoyed playing with them. I got an erector set when I was fairly young, too. It was something I did a lot with also. When I was in junior high, actually, I had the master bedroom because it was the only room big enough for my train tables, which were sixteen feet long and four feet wide, elaborately wired, and so forth.

And in many ways I was brought up as my father's "son." I always enjoyed most doing things with him and helped him a lot, whether it was papering or roofing the garage, or just talking. And since my mother wasn't much interested in what he was doing--and since he likes to talk--he'd often talk about whatever he was working on at that point with me. Things like defrosting systems for the Stratocruiser, and then, later on, electrical systems for one Boeing airplane after another. And he talked about ideas with me a lot. If I didn't understand, I'd ask him questions and he'd often reply with ten more words I didn't understand, but there was a very close rapport between the two of us. By the time I was in sixth grade, I was getting in trouble for using a sliderule at school. [Laughter]

About that point, I asked him what algebra was, and he sat down and taught me what you meant by an unknown in an equation, and how you'd solve one equation in one unknown, and two equations in two unknowns, and three equations in three unknowns. He made it all a perfectly logical process, and that took me through a year and a half of algebra in high school when I got there.

Weiner: And was that the start of your interest in mathematics?

Kutter: It had always been a game. I was interested always in a lot of different things. Probably if I'd been asked at that point what my profession would be, I would have said the theater. I was very much interested in dramatics. I'd gotten involved in the middle of grade school back in Ohio in a city-wide theater group that put on several plays, and even one rather scary radio program where the lights went out in the studio in the middle and we had to ad-lib the rest of the thing that we were giving because we didn't have our scripts to read from. And then later on, I was in some junior programs' plays in Seattle and some plays of a young people's theater group at the University of Washington, and a professional play at the Cirque Theater when I was in junior high.

Weiner: But does this--is still--while you're--

Kutter: That's still in junior high. That's still when I was relatively young. And I was also still very much interested in languages in junior high; I took Spanish. So I wasn't really leaning more towards science than anything else at that point.

Weiner: Was there a category of role that you would tend to play more than others?

Kutter: You mean in the plays, the category of role?

Weiner: Yes, that's right.

Kutter: A lot of different things. I played Kate in The Taming of the Shrew. In about eighth grade, I decided that I wanted to memorize Macbeth and got through the first two acts and part way into the third. I played some Greek things. Of course, there were a lot of young girl parts. The play at the Cirque was a play called Strange Bedfellows, where I played a young girl around the time of women's suffrage.

Weiner: How about your sister in all of this? Did she share these same interests?

Kutter: She was much more into socializing with people; she never got very much into dramatics at all. She was much more my mother's daughter, and did a lot of helping with women's kinds of things around the house, as those are commonly defined, more helping with the dusting or cooking. She liked to sew a lot. Actually we both liked to sew a lot, and there was one year when we each won the prize in our division in the Singer Junior Sewing Contest. She did, a little later, a lot of designing of her own clothes. And one other thing that affected the things I did around the house was the fact that my great-aunt lived with us. And starting in the fifth grade, I helped a lot

to take care of her. In fact, in the seventh grade I took care of her alone for two weeks while my parents were on their first vacation in years.

Weiner: She was really an invalid.

Kutter: Yes, a complete invalid. She could move her hands about an inch and was literally utterly immobilized; had to be gotten out of bed, put on a bed pan, fed, dressed, washed--I mean completely taken care of. My sister was quite a bit smaller than I was, partly because of the age difference, and my parents tended to think of her as frailer. And it took a certain amount of strength, so it meant that all through she never took any major responsibility for taking care of Nana. There were both advantages and disadvantages in having Nana live with us. It clearly tied us in certain ways, but also it meant that we always, from the time we were small, had someone who always had time to read us stories and tell us stories and listen to us and had nothing else to do and nowhere else to go, but to be there just for us. And that was very nice.

Weiner: She could do all of those things? Her speech wasn't--

Kutter: No, her speech wasn't impaired at all, and her mind was absolutely brilliant, and she kept up a correspondence with people all over the world. You'd put a pen into her hand, and she could move her hand just a very small amount with the pen in it to write. She couldn't bend her fingers actually, but--then she could hold a pointer in the other that she could use a little to slide the paper along gradually. She was an amazing person.

Weiner: Was there any encouragement from her about your interests?

Kutter: She was very much more into literature kinds of things, and also she was a devout Christian Scientist and did a lot of reading from Science and Health, books like that, that we did with her at times. She knew nothing about science either. I think [my] interests were really pretty broad at that point, although, particularly as I got into high school, I liked the more definiteness of science, that there was a right and a wrong. The literature was more for escape. I didn't like to analyze it particularly. I wrote stories sometimes, but I didn't like to tear apart a book. I tended more to just get utterly immersed in it and use it as an escape from life, perhaps. I had a lot of problems also in interactions with a few of the kids in my class, and, in fact, in fifth and sixth grade got beaten up almost every day. That definitely affected my trust of other people. And in junior high, I tended to sort of hide-out behind the lockers and read, rather than interact with people a lot. But by the ninth grade, I had decided not to worry about whether people liked me, to just assume that they did, because if I always assumed that they didn't then how could they possibly.

Weiner: Yes. Was there some crisis that provoked that evaluation?

Kutter: It seemed to happen after I'd gone from junior high to high school. There were only a few of us from the junior high who went to the high school in the ninth grade, so I was broken away from a lot of the people that I had been with. And I also liked a lot to ski and hike, and things like that, and was in a city-wide group of girl scouts that I joined in the ninth grade, rather than just a small local group. And it was partly

within the context of that group of people that I met several older girls that I became very close friends with, who sort of helped me a bit to improve my self-image. But I don't remember ever talking about it with anybody, about the negative sense that no one liked me, and that my being very good in school somehow interfered with people being able to relate to me. Nor do I remember any positive kind of stimulus that was there. I just remember one day deciding that I would never get anywhere doing it that way.

Weiner: Was one of the reasons that contributed to your feeling that others felt you were different--you mentioned scholarship and your interest in it--that you were a girl and doing the kinds of activities that weren't supposed to be appropriate for girls?

Kutter: Oh, I think so. I think that's probably true, almost certainly true. And that mathematics came so easily and that I liked it; it wasn't "in" for girls to like mathematics. We took a math exam at the end of the sixth grade, and those who did well on it were put into a separate track in junior high school. We took Spanish in seventh and eight grades and had math only one semester out of the two for the year, and then also lost one semester of home economics, or whatever. And actually, instead of doing the other semester of home economics, I played in the band, so I never did do any of those kinds of things that most girls did, and I missed part of gym to play in the band the other quarter.

Weiner: You played--

Kutter: I played flute. I'd started playing in seventh grade. I'm not

very musical, but I like music.

Weiner: How about artistic things? You mentioned that your sister was interested in design. Had you expressed any interest or ability in this direction?

Kutter: No, I hadn't at all. I am very poor at drawing; there are very few things that I can draw well. I can draw a beautifully convoluted tree with no leaves on it. All the branches intertwining and spread, and so forth. You know some things like that I can get into drawing, but I've never had any confidence in my ability to draw. The one art form that I really enjoy is clay, pottery, and particularly making small figures, but I've never done a lot of it. One that I liked best was a mother and child. It was just a very abstract kind of thing.

Weiner: When did you do that?

Kutter: I've done several versions of it. The first ones I did in junior high, and some things, like some little pig salt and pepper shakers that I'd wanted to do since I was young. But in general, I have no artistic confidence in myself. It's hard for me to let myself go and just do something, because I'm afraid of messing up the page and things won't look like they ought to, and that sort of thing. And so I have a lot of inhibitions about it which extend even to choosing drapes or decorating a house, to some degree. Although when it came to something like planning our own house much later, architectural design, that I enjoyed a lot. And even in junior high, I'd often sit down and design very elaborate houses.

Some of them would have a swimming pool that had an extension into the living room from outside, or something like that. [Laughter]

Weiner: Sure.

Kutter: But I really enjoyed that kind of design, but there it could be changed continually.

Weiner: It was more of a drafting kind of design.

Kutter: Yes, but taking into consideration also the artistic feeling, I thought. But I didn't try to draw in trees or anything like that. I never did much of that.

Weiner: You mentioned that later your interest in science developed, and you mentioned the idea of something that's right or something that's wrong. That brought a question to mind about religious education or training, or attitudes in your home.

Kutter: Well, I mentioned that my great-aunt was a Christian Scientist. My parents are both Episcopalians, and religion played a very basic part in their lives, an open and generous kind of religion. The house we chose in Seattle, for instance, was partly chosen because it was only a block and a half from the church and that was very convenient. And I sang in the choir from the time we moved to Seattle; I sang first in the youth choir and then eventually in the adult choir. I was often the only teenager in the adult choir. And I got a great deal of satisfaction and pleasure from the beautiful, familiar hymns. And my Dad has a very basic, just gentle faith that God tells him what to do. He became, later on,

quite involved in Moral Rearmament. I found myself in junior high starting to very much question a lot of the ideas on which religion was based and feel that there were things being said which I couldn't justify in terms of my own intellectual view of the world. And being concerned with something that I later on heard expressed in Marxist statements, religion being the opium of the people, you know that religion was something that it would be very nice to believe, and just because of that I had to look at it much more critically. Because I would like to believe in it. And I'd say that that was all the way through high school, and to some degree ever since, though much less now. I tended to both want to believe it and reject it. And periodically I got involved in something like Young Life or a little bit in Moral Rearmament too, trying to find an answer, which I never found. I've finally become generally very confortable with being an agnostic. Not someone who says, "It can't be true," but "I don't know whether it's true, and I'm not going to base my life, one way or the other, on whether it's true or not." And I have sort of, probably, a basic belief in some kind of creative force in the universe, which one can call God, some kind of logic and order and harmony in the universe. The idea of a personal God that one can call on and ask to shoot down one's enemies or somehow give one some special power, seems to me to be a very anthropomorphic thing. It's something that I can't at all believe in.

Weiner: There's one other thing about that early period; you mentioned girl scouts. But you also mentioned to me earlier that your father was involved with boy scouts and was the head of a boy scout troop.

Kutter: Yes, and in late grade school and junior high, I also went along on all of their hikes and did some other activities with Dad's boy scout troop. And in the summers, I'd spend two weeks at girl scout camp and two weeks at boy scout camp, for several years. A few summers, instead of girl scout camp, I went to the church camp, Camp Houston, but I basically preferred the girl scout camp. Actually, already at that point, a large fraction of my friends were boys and by the time we got into high school that was almost exclusively true. I had only, I'd say, at the most, three or four close girl friends, and I had a large number of good friends who were male. It was purely a friendship kind of thing, and at times, actually, it would rather distress me that they'd talk to me about problems like who to invite to a dance, and you know, never think about the fact that I was also a girl and might enjoy that.

In ninth grade, I dated a fellow who was a senior at the high school north of Seattle, in Edmunds. And from that point on, I usually had someone that I was dating reasonably steadily who was always someone far away, and always older than I was by several years, and always at a stage where they wound up getting much more serious than I was ready to get, which led to a termination of that relationship in each case. But only once did I have a boyfriend who was at the same high school where I was.

Weiner: Your friendship with boys, does this include playing with them as well, games, sports?

Kutter: It included hiking and things like that, and talking. None of them really were that much into team sports. And I never, at all, got into

baseball, or football, or any of those kinds of sports. In fact, baseball is still to me a total anathema. I once got hit on the head when I was in, about second grade, by a pop-fly over the back of a backstop that gashed my head open and sort of set my attitudes that were already fairly well-fixed. And we used to have to play baseball in grade school. And they put me in the furthest outfield, and if it happened to come in my direction, I'd run in the opposite direction as fast as I could. And if it accidentally hit the bat, I'd be so surprised I'd forget to run. [Laughter]

Weiner: Well, were there boys' teams and girls' teams? Or how did that work?

Kutter: Well, those were girls' teams. But I just didn't like baseball, period. In grade school there was also a game called "kickball" or something. It was played like baseball, but you kicked the ball instead of having to hit it. It was a big ball that you could catch in a reasonable way, and I rather enjoyed that. I hadn't thought of that for a long time, but I remember once getting so involved that I really slid into first base on a cinder court and took all the skin off my leg, but I went ahead and went around the rest of the bases as I was supposed to. Then once I got back, and took a good look at my leg and got hauled in to the nurse, and so forth. But generally, the doing things with boys was not that kind of thing. It was much more hiking, or skiing or kayaking. I did a lot of kayaking and really liked it. We were part of a kayak club in Seattle, and we'd go with my Dad, running white water rivers and things. I was never terribly aggressive sports-wise. I enjoyed the kayaking, but I wasn't about to try and Eskimo-roll or try competing in the slaloms.

And I enjoyed skiing, but I never at all was interested in racing or any of those kinds of things. I very seldom fell. I was always a fairly cautious skier; enjoyed it though.

Weiner: Somehow skiing brings to mind dancing, as a social activity as a teenager. Did you go to dances? I mean, there are dances where you don't have to be invited by the fellows who took you as one of the boys. [Laughter]

Kutter: My Dad really liked dancing, and so we used to dance together from the time I was in grade school. There was a dancing class that I took up at church in the seventh grade, the kind where the boys are supposed to take cookies to the girls, and so they stick them in their pockets and bring them out covered with threads. [Laughter] Things like that. And I also did some folk dancing. The noontime dances in junior high I very much avoided. I didn't like the social milieu. And there was a strange combination of people there. The school then, particularly in high school, ran the whole spectrum from debutantes to a majority of the black population and quite a large part of the oriental population of Seattle. And there was relatively few sort of in-betweens in terms of social class or economic values. The Bill Boeings, for instance, lived not far from there, and there tended to be a lot of cliquishness among certain groups, a lot of sororities, even in junior high and particularly in high school. It was not at all the kind of thing I was interested in, and those tended to be the kinds who went to dances and things. And there wasn't a lot of that kind of that thing done by the boys who tended more to be my friends, the ones who were involved with me in the science club, the chess club.

With the science club, we built a beta ray spectrometer, and they were very much involved in doing things. I went to the big dances, like, you know, my junior prom and senior prom. At that point, I had boys that I was sort of going with.

Weiner: Your science club you mentioned. When did that start? When did you get involved in it?

Kutter: When I was in ninth grade. Most of them were older than I was.

Weiner: What kinds of things did you do besides--Was that a group project?

Kutter: The beta ray spectrometer? Yes.

Weiner: Was there a purpose that you had in mind for it?

Kutter: Well, they started out wanting to build a cyclotron, and there I was, sort of a tagger-on. I just held tools and things like that. I didn't really have the background. Oh, we built some digital thing, sort of a modular device to help people learn the basic equations of physics. It was more a time, though, to get together and talk about science. The group who were ambitious enough to build something like a beta ray spectrometer graduated when I was sophomore. And the group sort of fell apart a little bit later.

Weiner: Why the cyclotron? I'm curious about that. That's quite an ambitious project.

Kutter: I don't know. They already were thinking about the idea when I got started with it, and the beta ray spectrometer was sort of a

scaling-down to something they could build. But there was a physics teacher at Garfield who had a great deal of influence on me, and I think most of the other people there, who had a masters in physics from MIT and was very much a hard scientist and yet very interested in people at the same time. And each year we'd usually send several students from Garfield to MIT.

Weiner: What was his name?

Kutter: James Mount. And he was the one who really emphasized the importance of just learning basic equations, doing dimensional analysis. Taught us the rudiments of calculus because we needed it to understand the physics. And had a very strong impression on most of us who had him. I had him for a year and a half of physics.

Weiner: What science did you take in high school?

Kutter: Well, at Garfield, I took a year and a half of chemistry and a year and a half of physics, and a year of biology and math through analytical geometry, and so forth. And then, I also had always been interested in other countries and languages and travel. Garfield has an exchange program with Braunschweig, Germany that had actually started in 1949 as part of the American Friends Service Committee Program. And the exchange student when I was in the eighth grade had lived with my flute teacher. I decided then that I wanted to be the exchange student. For several years we didn't have an exchange program because our sister school in Germany split into two schools, and there was a period of upheaval when it was impractical. And then in my junior year, the decision was

made that the following year there would be an exchange again. People could apply either in their sophomore or junior year, with the understanding that if you applied in your junior year and went for what was effectively your senior year, you'd have to come back for at least a semester. But I did go ahead and apply, and was chosen to go. That's sort of a pattern in my life. Everything I've ever applied for that mattered, I've gotten. [Laughs] I'm very much spoiled. At any rate, the year that I was in Germany, I had some differential calculus and analytical geometry and some organic chemistry and things like that in the high school there, even though it was a girls' high school specializing in languages.

Weiner: Was that at a more advanced level than you would have been able to get in high school?

Kutter: I think so. There was a basic calculus course that I could have taken when I got back, but I had that. The chemistry was certainly at least a different sort of thing and probably at a higher level. And I did some physics over there, too. And then when I came back, I, as you say, had to come back for a semester but wound up staying the whole year. There were a lot of good things still that I wanted to take there. And I didn't feel in any hurry. That was when I took the third semester of physics and the third semester of chemistry. One was each semester. I also took some creative writing. Had a chance to do more writing than I had done for a while. And a course in current events. And I didn't take as many courses as I usually had. Actually, I'd always taken extra courses, and so by the end of my junior year, I was only half a credit short to graduate. And then it took me two years to graduate [Laughs.]

But one thing I also did a lot during that year was to do a lot of public speaking. In fact, I think I counted up; it was something over a hundred talks I gave during that year. Of course, I'd been the exchange student, and I often showed slides or talked about various aspects at all of the different high schools in Seattle, and at Lion's Clubs, and American Association of University of Women and various kinds of social things, and a lot of different kinds of groups around. That year in Germany had done a great deal to open me up both emotionally and intellectually and make me much more aware of a lot of things going on in my own country, particularly when people would come up to me and say, "Why do you hate blacks?" I'd try to show them that I didn't, but I realized that there was a pervasive thing in our culture; so that did a lot of things. I also was on the debate team and did some traveling around to debates, both my junior year before I left and my senior year after I got back. And I played in the band and in the all-city orchestra.

Weiner: What about the drama work, the theater work? Was this [kept] up during the same year? Did you drop the drama?

Kutter: No. I'd completely dropped that in the ninth grade. I had a minor ski accident when I was in the play at the Cirque, where I'd gone over a jump without realizing it was there and cut my face below my eye. The director had gotten very angry at me for having the audacity to do something that might injure me and might take me out of the play. That made me very seriously question whether or not I really wanted to be a professional actress. When I pulled back from that, I started getting interested in a lot of other things. There were just too many things that

I wanted to do, and the people that were in the drama group at Garfield tended to mainly come from a certain very upper-class, white community that I felt much less comfortable with than I did with the completely inter-racial, very open, very life-oriented group that was in the band, particularly. And so when it came to making choices, I opted in that direction.

Weiner: The year of your graduation is somewhat confusing to me with this delay. What year was it.

Kutter: 1958.

Weiner: '58. You were nineteen.

Kutter: Well, I was nineteen the summer after I graduated.

Weiner: So you graduated in June and were nineteen in August. I see. And how did you do in your high school in terms of grades?

Kutter: Well, I had a four point. The one course that I really had to work at getting an A in was typing. Everything else had come easily, and then I took typing my junior year. I didn't think of myself as being particularly ambitious or concerned about such things. But when I look back, I realize that I was so determined not to let that spoil my record that I went half-an-hour early every day and practiced before school so I could be a good enough typist to get an A in typing too. It was rather a unique graduating class. There were eight of us with straight A's in that class.

Weiner: Out of how large a graduating class?

Kutter: About four-hundred and fifty. And that was the only year that that sort of thing had ever happened at Garfield; out of the top ten, seven were oriental. All except one went on to college, and most went on to do some really interesting and exciting things. I ran into a lot of them in later years. It was a very good class.

Weiner: How many women were among that top group?

Kutter: About seven, I think--six, or seven.

Weiner: Any others with interests in science?

Kutter: Yes, there were two others who were very strongly science-interested. And one who was not in the top ten but just behind that, who's remained one of my closest friends, eventually got a PhD in physical chemistry at the Lawrence Radiation Lab at Berkeley. This was the class I graduated with, not the class I'd been with most of the way going through.

Weiner: Yes, I see.

Kutter: I had these few close friends, including her. The daughter from one of my families in Germany came back and lived with me for that year, which was my senior year, and graduated then with me. She wasn't an official exchange student, just an unofficial exchange student, which was really nice.

Weiner: When did you make up your mind about the type of college that you would want to go to, and the type of studies that you would pursue?

Kutter: What I was planning on doing when I got out of high school was to teach mathematics in high school eventually, and I applied for a National Merit Scholarship and got one.

Weiner: Don't they usually rate people they have? To semi-finalists and then finalists? So that meant that you were a finalist and--

Kutter: Yes. I received an award.

Weiner: So that's the top list?

Kutter: Yes. Well, I took a trip with my parents looking at colleges, and the college I chose was Stanford. I also liked a couple of the other colleges down in Southern California a lot, and I was also quite impressed with Whitman. But that year after I'd gotten back, I'd met and fallen in love with a guy who was a sophomore at the University of Washington in pre-med. Partly because I was attached enough that I wasn't sure I wanted to go that far away, [and] partly because Stanford is very expensive and my Dad made just enough [so] that the National Merit Scholarship would pay only a hundred dollars a year, I decided to go to Whitman rather than going there, with the idea, initially, that I'd probably go to Whitman for two years and then transfer to the University of Washington which had much more possibilities at the higher levels of mathematics. Actually, I wound up transferring to the University after one year, partly because I was engaged at that point.

Weiner: Whitman had a good reputation for science, with some very good people in physics coming out of there.

Kutter: Yes. Yes, much earlier than when I was there.

Weiner: His name was Benjamin Brown, I guess. What about in the later period? Was it that same consciousness, that same tradition?

Kutter: Not nearly as much so, I don't think. I never got into physics there. I took chemistry and didn't get terribly excited about it. Mathematics, I jumped immediately into the sophomore-level course; there were three of us who were freshmen who did. I thoroughly enjoyed it, and I very much liked Hutchinson, who was my math teacher there; he was a really delightful person. And I also was doing other things. I was taking a course in comparative literature which was reading things like Faust and Dante. With Faust, I had a real advantage because I could read it in both German and English and found that I got an incredible amount more out of it from using the two languages than from only one. Languages also come extremely easily for me; I hadn't known any German before I got to Germany, but within six weeks I was dreaming in German. And by the time I came home, I couldn't speak English straight. People would not believe that I was an American. They'd try to figure out what part of Germany I was from--or maybe it was Sweden. But [laughs], I wasn't an American.

Weiner: You mean when you were over there?

Kutter: When I was in Germany.

Weiner: Well, your appearance, with light hair and blue eyes, might have helped.

Kutter: Oh, yes. Oh, clearly. And, you know, I had only a slight accent in my German. I very much liked that year at Whitman. I pledged a sorority, and for the first time sort of went through the experience of being part of that kind of a group of women, and in the atmosphere there, it was really nice because it was much less cut off from other people, and the independents were almost a separate sorority. It was a very different kind of atmosphere than at the University of Washington. And I made a few close friends who were women, and some good friends who were men, also, even though by Christmas I was engaged to my friend in Seattle. We got along best while we were far apart, and I had a lot of friends there. There were times when I wished that I had gone to Stanford. I didn't feel pushed in the same kind of way. I did some debating. I did well, but not incredibly well there. Particularly the chemistry--I did just fairly adequately. I had honors but not highest honors in chemistry, as I did in most of the other things. I wasn't that enthusiastic.

Weiner: Did you get any guidance in high school, regarding choice of college or of career field?

Kutter: No, except from this physicist who I very much respected, and remained close friends with after getting out of high school.

BEGIN TAPE ONE, SIDE TWO

Weiner: You mentioned the high school physics teacher and the good role he played. Did it ever come up in his conversations with you, either you or he bringing it up, about special problems that you might face as a woman

going into scientific training and a scientific career?

Kutter: Well, probably the fact that I was a woman affected my thinking in terms of wanting to go on to teach mathematics, rather than going on to become a mathematician. I mean I was thinking clearly always in terms of eventually having a family too, and a profession that I could do at the same time. And we talked a bit about whether MIT would be a good choice for me to consider. And one of the strong reasons why I didn't, was the fact that it's a very male-oriented place, and was, particularly, at that time. One of my classmates who went there wrote back to me that I really ought to have come. He said it was a desolate place, that there were only about a hundred women, and most of them were borderline cases; borderline on what, he refused to say. Which was rather a pathetic kind of thing. So in that sense it clearly affected it. And it clearly affected my relationships with other people. A lot of the guys in my class didn't like, really, the competition of a woman and a lot of the women that I knew clearly [classified] me as some different kind of animal.

I became much more comfortable with it, really, during the year I was in Germany even though again I was in a place where--well, there I was in a girls' school and there I really made a lot of women friends, partly because of being in that kind of an atmosphere. There I was easily, in general, the best in the class in the sciences. But I got a lot of encouragement there and a lot of support. I became very good friends with the person who taught mathematics and chemistry. It was a mixed-up situation there.

When I first got there they put me in a class that was a year behind

where I should have been so that I could learn the language. And so, while they were learning to use sliderules, I was learning the words for all the parts and helping them with the sliderule, while they'd help me with that, or in chemistry, the same kinds of things. And then after six weeks, I started taking German and history, and went on up to the higher grades for the physics and math and chemistry, and developed a very close rapport with this teacher who had very few people who were as scientifically-oriented as I was. And he really encouraged me to go on. And so there, of course, I felt no bias being a woman, and in high school I was never treated differently because of being a woman. I think, generally, it was almost more [that] people would lay a thing on me, that "you've got this fine mind; you have to use it; you have to go on; you have to do things that you're capable of doing."

I didn't seriously, at all, consider the biological sciences at that point. My experience with biology in high school left me with the feeling that that was the science for people who couldn't do real science. So I stuck with mathematics, which was very easy and straightforward and a great deal of fun, and went on. When I was at the University of Washington I again got a lot of encouragement in mathematics and was never, in any way, put down because of it. Again I tended to do very well, and usually there were only two or three women in my classes. Again, the other men didn't take me seriously as a woman, usually, which was hard at times, particularly since I'd decided to break my engagement, mainly because we were going pretty much [in] different directions. And med school was doing a lot of bad things to his head. I had always had to hide my intelligence around him; he felt awkward.

At any rate, back to mathematics. My junior year I took a course which was mainly graduate students, in analysis. There was one other woman in that class. That's actually where I met my husband, whom I married in the fall of my junior year. And I got a lot of encouragement. Edwin Hewitt had been my teacher my sophomore year. And he's one of those five percent of the mathematicians that does ninety-five percent of the work. He's a very excellent mathematician and had a lot of faith in my ability. And, in fact, [he] became very upset later on when I decided to switch and go on in biophysics. He wanted me to take more graduate-level mathematics [in] my senior year and felt that within two years I'd have my PhD in mathematics. He felt that I could do good things and really encouraged me to, but I questioned whether I had the intellectual ability--to some degree I questioned it--to be one of those five percent, partly because in this course in my junior year, for the first time, I ran into things where I could bang my head against a wall and maybe not even get the problem. I thought I'd perhaps had that, but I even more questioned whether I had that kind of single-minded dedication. And I knew that there were a lot of other things in life that I found exciting and interesting, and that I wanted to have a family.

Also, my sophomore year, I'd gotten talked into taking a course in biology--actually zoology--by my fiance, mainly because I'd understand better what he was doing. And so I took it, along with everything else. I was taking physics at the same time and organic chemistry, all three the same year. It was taught by a delightful Scottish woman named Mary Griffiths, who's still at the University and who just had sort of a part-time position. Her husband was in the history department there.

Her background had also initially been in mathematics, her bachelor's degree. And she taught it very much from a point of view of why things happen, and how things happen, and looking at mechanisms; and she made biology a completely different thing. It was a modern, somewhat molecular approach to biology, and it opened up incredible new vistas to me. Suddenly, biology stopped being this descriptive thing and became something where even though it wasn't as easy basically for me as mathematics, the questions that were being asked were so much more interesting--questions like how can you take a single egg and turn it into a human being. That very much caught my fancy, and that was pretty much when I decided that I would at least explore the options to go on in something vaguely called "biophysics," which at that point could have been neurophysiology, or biochemical genetics, or developmental biology, any one of those three.

Weiner: That's a big jump from an introductory zoology course to an interest in biophysics. Was there some special exposure to it that you had at the University to biophysics per se, or did it come out of her approach?

Kutter: Well, the molecular biology interest came out of her approach. I probably called it the three things I just mentioned, rather than biophysics, right at that point. Biophysics seemed to me to be a way to not be trapped in this classical biology thing. I knew I didn't want to graduate school in biology, and biophysics was just a newly-developing area which seemed to mean an ability to apply physical and chemical principles, which I liked very much, to biological systems. And that's what really attracted me. My junior year, then, I took electricity and magnetism, and

also I was taking other kinds of things, like The Far East in the Modern World. And I ran an international bazaar there in the Student Union Building for a couple of days. And I was active in the Civil Rights Movement through the YWCA, just in the days when it was just barely getting started, when no one was activist.

Weiner: On local issues in the Civil Rights Movement?

Kutter: That was when they were just starting to have the bussings down in the South.

Weiner: In Montgomery.

Kutter: Yes. And we were trying to raise money to help them out. It was more that kind of long-range thing than anything on a local level, at that point. So I was not by any means single-mindedly science-oriented, even at that point, although I was taking a lot of different sciences.

Weiner: When you became interested in those modern aspects of biology, did your job expectations change? Or were you still thinking of teaching high school?

Kutter: No, I was beginning to think, then, in terms of going to graduate school and possibly doing research. Actually, I pretty much stopped thinking of teaching high school by the end of my freshman year. I took a course in educational psychology over at Whitman, and I took a course in child psychology at the University in my sophomore year. It was mainly these kinds of stupid course requirements that just really turned me off. You know, everything that was done in those courses was so obvious.

There was no intellect involved, no real challenge. I gradually began to lose interest and set my sights somewhat higher. Even in mathematics, by the time I decided to go out of it, I was thinking about graduate school.

Weiner: Did you think, or did others talk to you, about the possibility that opportunities for a woman might be less in the field?

Kutter: No, and during that whole period in college, from, say the end of my freshman year on, when my own view of myself started changing, probably from the time that I began to see that I didn't just want to get married right away, if anything, I found being a woman an advantage in the sense that there were so few women in the things that I was doing that it almost set me apart from a lot of other people. I could more readily go up and talk to somebody about questions that I had. And I didn't feel the same kind of pressures that seemed to be hitting a lot of the men in my class. I noticed this even more in graduate school. There are certain images you have to uphold, certain ways in which you have to succeed. And I didn't have those kinds of pressures, but I certainly had a fair bit of support. I didn't run into any time again when I felt it was a handicap, until after I got out of graduate school and was at the University of Virginia. I'll talk about that later.

But up until that point, the only ways in which it was a handicap were partially self-imposed ones or very locally imposed, but certainly not in any way from the way I was treated by any of the faculty I worked with or the kind of respect I was given by any of my colleagues or students. Again, I often wasn't treated particularly as a woman, and I went through

that stage that so many women go through (which is really a pity) of feeling it's really a compliment to be told, "You're not like a woman. You don't think like a woman. You think like a man. You're like a man. You're as good as a man." You know, all those forms of "praise" quote, unquote. But it really, in terms of my expectations, ceased to be a disadvantage. That was probably partly because my Dad had brought me up with a really good view of myself, with a good deal of confidence in myself and a sense that I could go anywhere and do anything. I did question whether I wanted to go on to graduate school right out of college, whether I wanted to make that kind of decision. And the thing I was seriously considering towards the end of my junior year was going into the Peace Corps for two years, while I did some sorting out and thinking and seeing the world, and so forth. The main reason I wound up not doing that was because, as I said, I met my future husband during my junior year and then we got married that summer. He talked me out of it, and strongly encouraged me to go ahead and think about graduate school. Then the summer after my junior year I decided to try to get some experience to see exactly what I did want to do, because a lot of these things were still pretty nebulous concepts. I'd really only had the two quarters of zoology, plus one of genetics, and one of embryology; that was all the biology I had had. And I got a job working for Arnie Towe in neurophysiology for that summer.

Weiner: In a lab at the University?

Kutter: Yes. In a lab at the University of Washington. That was what convinced me that neurophysiology was not where I wanted to go.

Weiner: What about the work convinced you?

Kutter: Well, it involved taking a cat; holding it while we injected with chloralose, which supposedly destroys its ability to feel things but makes it hyper-reactive in terms of local sensory motor reactions; holding it while it went under; putting in a venal cannula and a trachial cannula while it was in this hyper, jumpy state; and then injecting it with a paralyzing agent. I always had this feeling that it was being paralyzed but it might still be feeling all these things. Then, cutting into the interior parts of the brain, through the skull, and putting in electrodes to measure, hopefully, from single cells. Then taking masses of recordings which we had to later try sorting out. By the time we tried to correlate things, it was just sort of statistical correlations between what we'd done and what we saw happening. There was no way to say, "Hey, that looks like an interesting pattern; let's repeat it," because by then the animal was long since dead. I didn't like at all working with live animals, particularly with cats--I love cats. It seemed to me to be unnecessary cruelty for what we got out of it. And I didn't particularly like the kinds of sorting through data, and it seemed like just not yet time for neurophysiology to really get a start, that the field wasn't ready yet. I mean, now Arnie has on-line computers. He's doing some of the same kinds of experiments, but the data is immediately being processed so you can see what the results are and then actually do an experiment, as opposed to observation.

Weiner: You're changing as a result of the recording.

Kutter: Well, the data is immediately processed and fed back, instead of

somebody spending two weeks going through it; it takes about three minutes.

Weiner: And so, with the same experimental animals you can vary the conditions and stimulus.

Kutter: Yes. I decided that developmental biology was equally unripe at that point, that the area of really great excitement was biochemical genetics, and that that was the kind of thing I'd like to do.

Weiner: Had you heard of recent work in the field? Were you aware of what was going on?

Kutter: Well, I'd heard of Watson and Crick and some of those things. I knew a little, not a lot. In fact, actually, I still was just applying to graduate school in biophysics. And Sig [her husband] and I talked about what schools to apply to. We had, of course, the situation where both of us had to get in. I applied to biophysics programs at Stanford and Berkeley and MIT, Harvard and Rochester, and the University of Washington. At that point, I wanted to apply to a couple of other places, like Colorado. Sig wanted to apply just to the best places in the field. And physics was, of course, super-competitive. (He's a physicist.) And so that partly dictated the choice of where we applied. And I didn't really have a very good sense of what was going on in any of these departments. I hadn't gotten to a point where I'd go out and read the literature or check who was in a department, really, and know what it meant. I was sort of applying blind and partly blind on the basis of Arnie's suggestions at a point where I was still thinking about neurophysiology. So the choices were only partly rational.

Weiner: But you were applying to the same places?

Kutter: Yes. So it was Sig who chose the best places in physics, and I looked to see if they were apparently okay in my field.

Weiner: When you got married, was there an explicit discussion of these kinds of educational and career decisions about whether there would be primacy for one over the other?

Kutter: Not really. Sig really encouraged me to go on. There was no real explicit discussion at all, in that there was the idea that we'd go somewhere to graduate school together. There was never any question that I would drop out at that point, but there was also no discussion at that point of whether we wanted children and what the effects of children would be, what would happen in terms of looking for jobs afterwards. We were pretty naive. Sig was several years older, but at the same year in school and an immigrant from Germany.

Weiner: He came over here in what year?

Kutter: He came over here in 1955.

Weiner: He was twenty years old?

Kutter: Yes, and came alone with a hundred dollars in his pocket. He had gone through an apprenticeship in the export-import business in Germany, and after having flunked out of the classical gymnasium there because he and languages didn't get along very well, came over here. After working for a few months in New York, he went into the Air Force and wanted to be trained as a jet airplane mechanic. He got the training, and then had

problems putting it into practice because they couldn't get security clearance for him--as a German--for about two years. But he got interested in the engineering behind the mechanics, and then in the physics behind the engineering, as he read more and more. He wound up finishing high school by correspondence and starting college, and then going into physics. And he thought what he wanted to go into was theoretical high energy physics; he didn't get into Stanford or Harvard; he didn't get money at MIT; and Berkeley--we hadn't heard about the money yet. I can't remember what happened there. But he decided that Berkeley was too big a place for him, that he'd get lost in the shuffle, that it was much better to go to a smaller place.

I was in a pretty flexible situation in terms of where I could go, because I had both a National Science Foundation Pre-doctoral Fellowship and also a Woodrow Wilson Fellowship. I had competed in both, and been awarded both. The Woodrow Wilson then became honorary, since I had the National Science Foundation. But with that kind of backing it's not hard to get in wherever you want to go, particularly if it's in a fairly new field. He felt that we should go to a different place than where we were. So that essentially left Rochester as the only choice. I'd applied to the Department of Radiation Biology there, which claimed to have a strong biophysics program. When I got there I discovered it was essentially all on paper, and the last person to get a degree in biophysics had just graduated. There were three of us just starting, of whom I was the only one who finished in biophysics. And the degree in biophysics simply meant you did the same thing as the radiation biology people, but you had taken graduate quantum mechanics from either the physics department or the

chemistry department, which is what scared most of them off. I was automatically put to working with Dr. Behle, who was working on radioactively labeled antibodies to fibrinogen as a way of treating cancer. He was considered the biophysicist, but that was not at all the sort of thing I had in mind. I was very unhappy that year. We had to do some research, trying to do some work with cutting open rats and slicing up their livers and various other parts, staining them with fluorescent dyes, and trying to do microscopy and see lesions, see differences in terms of fibrinogen deposits from the tumors that we had to experiment with on those rats.

Weiner: Were they a separate department or division?

Kutter: What, radiation biology? Yes, it was associated with the hospital. It's part of the original Manhattan Project, and they were doing some very interesting radiation kinds of studies. In fact, the dogs that had been irradiated at the time of the Manhattan Project were just dying off. It was rather interesting, because those that had gotten low doses of radiation were living longer than the controls. They were senile and sterile, and everything else--but they were living longer. [Laughs]

Weiner: The reason I asked is before the war the radiation and biology work essentially came out of the physics work with the cyclotron.

Kutter: Yes, this wasn't related at all. No, it was a completely separate department which was completely funded the the AEC, so that the faculty there didn't have to apply for grants, and so forth.

At any rate, the spring of that first year, a couple of things

happened. The department got a big grant from the NSF to build up a biophysics program there. And they used that to bring in ten graduate students on fellowships and to set up a course for the next year that brought in, for three to five days each, a lot of the leading people in the field. There was a whole series; it read sort of almost like a Who's Who in various aspects of biophysics. They also got several new faculty, among whom was John Wiberg, who was doing exactly the kind of thing that by then I was beginning to think I wanted to do.

That first year I was there, I took essentially the first year medical school curriculum, biochemistry and physiology. And I also took a course in molecular genetics from Allan Campbell. And that was just at the point where people were sorting out the genetic code, and papers were coming out trying to say why all codons had to have a "U" in them on theoretical grounds. It was a very exciting time. And I got really interested in phage work at that point, and then Wiberg was to be hired, who was working with T_4. When he came for his interview, Behle was astute enough, or whatever, to invite me to go to lunch with them. And the three of us went out to lunch. And after about two hours of listening to us, Behle left. [Laughs] And John and I talked for some four hours. And here was exactly the kind of thing I was looking for. He came, then, the following fall, and we clicked it off so well during that period that it was quite clear to both of us that when he came I would be his student. And that was very good. So the next year then, he was setting up the lab and I was still mainly taking courses. I took essentially the first year graduate physics curriculum in that year, the quantum mechanics and methods of theoretical physics, plus this course in the biophysics program that

brought [in] a lot of well-known people. I forget exactly what they called it--cell physiology or something--but we talked about muscle, about water, about ions, about a lot of different things at the molecular and cellular level. Irreversible thermodynamics and membranes.

Weiner: Good stuff. I mean up-to-date--

Kutter: Yes.

Weiner: In physics, did you have--was Marshak involved in the teaching of it?

Kutter: No, he wasn't involved in that course. I had a very excellent person, whose name, of course, I forget. Anyhow, I was very lucky because he was interested in biological applications of quantum mechanics. And he taught the whole course from a group theoretical approach and was really interested in the kinds of things I knew something about.

Weiner: You have to think of his name. Now it's getting more interesting to wonder who he is.

Kutter: He was interested in eventually trying to do some applications to biology, and eventually brought me some papers by Landau and some other people. He was a solid state physicist. In some ways, it was a problem for me to try to jump immediately into that program because I'd only had two years of undergraduate physics instead of the four-and-a-half required. I'd never had any classical mechanics, and we reviewed all of the most commonly-used classical [texts]--Goldstein's Classical Mechanics we reviewed in the first ten days of the quarter.

Weiner: Which you had never seen before.

Kutter: Which I had never seen before. [Laughs] I think on that first problem set, I got twenty-eight out of a hundred and thirty, and that was with some help from Sig. But from then on, we got into the quantum mechanics and my strong mathematics background helped a lot. I did very well, and the mathematical physics was a lot of fun. So the third year, then, I started getting involved in research and also prepared the problems for my prelim.

Weiner: How did you choose the research topics? Were you assigned as a result of something someone else was doing?

Kutter: No. I decided to work with John, and there again there was the advantage that John was just starting to build up his lab and build up his program, and we worked together. I tried a couple of dead ends as we were exploring things, before we got through. But in my simplistic mathematician's mind, the thing that really intrigued me most about T_4 was the whole question of why on earth it goes to the trouble to use hydroxy-methyl-cytosine in its DNA instead of cytosine, and what kinds of mechanisms there were that had given enough selective pressure that it had kept that. So we were interested in looking for dCTPase mutant, in order to try to see if we could get cytosine into the DNA. And that's essentially the research topic I'm still working on, various ramifications of that.

It was just at the time when the big collection of amber mutants and temperature-sensitive mutants had very recently been found. Wiberg had been involved in identifying the first two early genes; gene 42, the

hydroxymethylase, and 43, the polymerase. And so he was trying to identify some of the other early genes, and I got involved, sort of, from the ground floor with doing that. It was just a field that there were a lot of problems that expanded very naturally, and I took off in some of the directions that he wasn't as experienced with--things like sucrose gradients, for instance. I wound up having to look at host DNA breakdown, and size of DNA's, and it was a very nice problem. For my preliminary exam I had to do a paper on what I was trying to do and what I'd expect the results to be. [I] had to do two papers, one on that, and one on something else which I did with UV mutagenesis, and thymine dimers and repair; that was the second one I chose. But a lot of things that I tied together and suggested and put forth in that paper, in that initial thing before my thesis, gave a direction to my research and suggested options and possibilities. Everything I've done since has sort of evolved out of that. I laid out several directions of what the possibilities were, and the last of them I'm just now being able to test.

But it was the kind of problem where I could really, really outline different definite goals and sense of direction, and we got the mutants that we needed to be able to do it. Each stage of it led to another natural stage, and I wound up being much more involved because John was just getting started, and he and I and a technician were the only three in the lab until the fourth year I was in graduate school. It was just the last year or two that someone else came in and started, and so I wound up in effect doing two theses because there were two parts of the problem that really needed to be done.

Physics notoriously takes a long time, particularly at Rochester. Sig was in Marshak's department and somewhat involved with him. Sig, during this

time, went through a couple of crises where he was debating whether to go on. But he was basically doing well. He got just about a third of the way down on the qualifying exam and was convinced at that point by Marshak that theoretical high energy physics was a very unpromising direction in which to go, that in order for it to make sense to go there you should be extremely brilliant and do it easily, as well as being very hard-working. Sig was very hard-working and very good, very solid, but also jobs in high energy physics theory were very hard to get. And sort of the same kind of conclusions that I'd come to on my own about mathematics and what I wanted to do. Sig looked around a bit and thought about what he had said, and astrophysics was also very good there. That seemed to him to be a really good way to apply all these different aspects of physical laws that he really enjoyed, plus the mathematics, and to do some work in stellar evolution where you need to know hydrodynamics and you need to know various parts of thermodynamics and really apply a lot of these different ideas.

Weiner: And you need to know nuclear physics.

Kutter: Yes. You need to know the nuclear physics and are definitely applying those ideas too. He had to learn a lot of new things, though, which slowed him down a little bit more yet. So I knew I had plenty of time. I wasn't rushed in that sense.

And about that time, too, we decided to go ahead and have kids. At the time, I didn't tell John that it was a conscious decision. I don't think he knows to this day. We waited until we finished course work, where it would really interfere, but we decided graduate school was the logical time to have kids, that our time was much more flexible then than

at any other time, and Sig was rapidly approaching thirty. In fact, our first son was born just a few days after his thirtieth birthday. And he wanted to be able to hike and do other things with them. So within a period of ten days, Sig took his qualifying exam and I took my finals in theoretical physics and quantum mechanics, and Sig's sister arrived from Germany to spend a year with us, whom I'd never met before. We moved into a new apartment, and I got pregnant. His friends always teased him later [laughs], about what he was doing while everyone else was studying for the qualifying exam. But he's a very systematic person; he had laid out exactly what he was going to study a year in advance, week by week, and had systematically gone through it until the last week or two before, exactly following his schedule. So at any rate, the winter of my third year at graduate school, my first son was born.

Weiner: What year was that?

Kutter: That was 1965, February of 1965.

Weiner: How did that affect your studies? Well, by that time, your course work was over, but how did it affect research, preparation for the exams?

Kutter: It didn't slow me down much preparing for the exams. Actually, I was trying to remember the other day exactly when I took them. I prepared for them that fall. I guess I waited and took them the following fall. It didn't really slow me down very much. Bernard went to his first seminar when he was ten days old and to his last one before that at about minus-ten hours. [Laughter] The last two or three months I was slowing down a bit and getting tired more easily and not putting in as long hours, but it

wasn't any kind of serious problem. And, again, for the first few weeks, I was a little bit tired and so forth. My sister-in-law, of course, was going to help to take care of him. She was working as a teacher there, too, but she was there to help me. Except that both she and Sig came down with a really serious case of flu about the time I got home from the hospital, so I was nursing both of them plus a five-day-old child. [Laughter] That was very nice.

John was Bernard's godfather, later, when we finally had him baptized, and was very close to the kids. For the first four months I simply took him to the lab with me, had a little bed there for him, nursed him in John's office. If he got too fussy, I'd stick him in the pack on my back . . .

Weiner: While you were working--

Kutter: While I was working in the lab. And it was a very nice life, I think, really, for a child, in retrospect, because I was continually there; much like in a primitive tribe, he was close to my body if he needed it or sitting in the infant seat, watching what was going on and continually being entertained, part of things. And that summer, we went to Germany to visit Sig's parents for the first time and by the time we came back he was seven months old and much more mobile, too mobile to want to be in a lab. But I found a very good babysitter close by, an Indian woman who had several other kids she was taking care of, one from Uganda, one from Korea, and another American child. He was there about twenty hours a week and really enjoyed it. He called her "Mommy", too. And she was a very gentle, very quiet kind of person, which was a good balance to my more exuberant, tempestuous nature. And it really with him worked out exceedingly well.

I would say I may have lost three months at that point, probably not much more than that.

Weiner: Did you get any negative comments from people at the University?

Kutter: No.

Weiner: Regarding bringing the child in, or getting pregnant in the first place.

Kutter: No.

Weiner: As far as it would affect your career.

Kutter: John's reaction was, "Well, accidents do happen," [laughs], and simply from then on to be very supportive and do everything he could to make it possible for me to keep going, and I think in almost anyone else's lab it might have been far more difficult. But he's an incredible human being, besides being a very good and very thorough scientist.

Weiner: Where is he now?

Kutter: He's still at Rochester. And we're still good friends. It was interesting, he wound up with almost all women in his lab. Most of the graduate students he's had have been women, and I think partly [so] because he is basically very supportive. I've heard a lot of stories from other people at a number of other schools where that wasn't at all the case. They were made to feel that they had to make a choice; if they had a child, nothing was done to make things any easier. I took Bernard to seminars, sometimes, of if he was asleep I'd leave him in the lab. I

remember one day coming back and finding his little bed empty--which was in my office--and going into the lab, and here John was sitting, counting plates, and Bernard was sitting in his infant seat. They were talking to each other; they were holding this regular dialogue [laughter.] It was absolutely beautiful. He had waked up and so John just picked him up and put him in his infant seat and took him out [laughs.] In fact, at one point, John thanked me for going to all that trouble to provide entertainment for the lab. First of all, the third person in the lab was a woman in her fifties who was rather deaf and who was very fond of children, and Bernard clearly didn't bother her. And she enjoyed him, so there never seemed to be any interference just from that. Later on I was running a lot of sucrose gradients, and I'd do the experiment during the day while he was at the babysitter, and then go home and feed everybody and put him to bed, and then come back to take off the second sucrose gradient that had an eight-hour run to go.

Weiner: But you still, then, had a clearly defined household role, which had to do with keeping the house and preparing the meals? Or am I wrong to assume that?

Kutter: A lot of it, yes. Well, that year when Sig's sister was there, she pitched in too. And then most of the time I would say that Sig helped a lot both before and after the kids were born, but it was sort of two thirds-one third, which wasn't that bad in graduate school, because the academic things came more easily to me. In terms of balance, in terms of the amount to which it delayed us, it was fairly reasonable. Then, when Eric came along--first of all, he was born a few weeks early, which

threw my schedule off-schedule. [Laughs] I had two weeks of experiments still to do, and here I was in the hospital. I thought that those were the last experiments for my thesis and that I would just stay home and write. He was also planned; I wanted them to be two to two and a half years apart, and they were just twenty-six months apart.

While I was in the hospital, John was at a meeting and came back with some data which indicated that one assumption we'd made wasn't valid and I was going to have to do another set of experiments which involved making antibody by injecting rabbits, and you know--that whole period of time and so forth. So I was slowed down somewhat by that.

I did get delayed quite a bit then. When he was six months old I came down with mononucleosis, and so, of course, since I was still nursing him, he had a mild case of mono; and Bernard may have, I don't know. And here I was, trying to take care of house, kids; write thesis; everything else with mononucleosis. [Laughs] Well, for the first part, before I realized that that was what I had I kept fighting the idea that I was really sick. I kept saying, it's just in my head; it's just that I've got too much to do; and I want to escape from it; and it's all in my head. And, finally, when my Dad was coming to visit and I realized I didn't trust myself to drive the car to the airport to pick him up, I decided I must really be sick, went to the doctor, and he said, "How on earth are you still walking around?" So I just simply dropped the thesis-writing for a couple of months. It didn't matter. I was still through about six months before Sig was, in terms of just finishing, and then I finished some of the side work and got the first paper written during that period afterwards and continued to work half-time for John for that six months.

Weiner: The thesis exam was when?

Kutter: It was in February, I guess, of 1968.

Weiner: This was the qualifying orals or the defense?

Kutter: Oh, that was the defense.

Weiner: When were the orals?

Kutter: That was much earlier. That was that first fall after Bernard was born, when he was six months old.

Weiner: And the thesis committee for biophysics--I'd be curious to know how, since you have so much physics--did they put a physicist on it?

Kutter: Let's see. I'm trying to remember who it was. There was a physical chemist on it, and Dounce from biochemistry, who's quite well-known for his nucleic acid work (Dounce homogenizer and stuff like that); Allan Campbell from biology, who's done all the lambda work.

BEGIN TAPE TWO, SIDE ONE

Weiner: This is Side One of Tape Two. Well, just picking up on a few gaps, we were talking about your domestic responsibilities. And you did have some help from your husband's sister who was visiting.

Kutter: She was there for a year, and she was also working full-time teaching at the same time, so the three of us shared responsibilities.

Weiner: Yes. Did you have any household hired help?

Kutter: No.

Weiner: In that entire period. Was that because it wasn't necessary, or because there wasn't any money, or because you just didn't like to do that?

Kutter: We never really thought much about it. The things that had to be done, we all pitched in. We clearly didn't have a lot of money. If it had had a high priority, we might have done it. Partly, most of what had to be done was the day-to-day nit-picking kinds of things. We had a small apartment. It's almost more trouble then to have someone come in. It's not that you have big long hours full of things to be done. Some of those kinds of things that would take long hours Sig tended to do, cleaning the bathrooms, doing the floors; those kinds of things Sig tended to do. And to some degree we split the work of cooking. Having someone to take care of Bernard--we never talked about doing more than that.

Weiner: Well, did things change when your second child was born?

Kutter: By that time I was mainly writing my thesis, and what I did then was to still send Bernard to the babysitter, and Eric tended to sleep long hours. So I did a lot of the writing of it at home with Eric there. And when I did have to still do experiments, I did them mostly late at night.

Weiner: Getting back to the experiments, you said your first paper was published that came out of the research. What year was that published?

Kutter: 1968.

Weiner: We'll get a list of that. But that was a piece of your thesis.

Kutter: Yes. That was largely part of my thesis. It was a paper on general molecular biology, a good paper.

Weiner: When did you first get the sense of confidence that you could do original work and that it would be important, that it was a contribution?

Kutter: Probably about my third year of graduate school. I felt very discouraged for a while that first year when things were going so badly, and then that summer Wiberg wasn't there yet, and I wanted to try to learn membrane stuff. I did some things trying to profuse volvox to measure action-potentials and permeability changes, and I got nowhere. I spent most of the summer trying to make micro-electrodes small enough and then when I finally got the volvox I realized I didn't really know what it was like. But working with John was very good, and I gave my first paper at the phage meetings in 1965; it was well-received. This was the bacteriophage meetings at Cold Spring Harbor, which are a long-standing institution.

Weiner: This was the first time you'd been there?

Kutter: Yes. It was the first time I'd been there. The summer before I had gone to the International Biochemistry Meetings in New York; that was the first meeting I ever went to. I felt pretty lost and swamped there. But I developed a good sense of confidence at Rochester. There was a good group of other students to work with and I was treated, as I've said before, totally as an equal. [I] had a lot to do with curriculum planning, developing the new program, and had a good deal of confidence in myself at that point. It took me a long time, sometimes, to get into doing a new

kind of experiment. For example, I'll never forget when I first started doing sucrose gradients. Of course, I had to use a scintillation counter to count the results and we didn't have one. There was one other one in the department that was used full-time by the people who had it, but there was one available over in Chemistry--except it was one that had been bought and never used; bought used. So I wound up having to totally figure out how to program it for doing double-label things, how to set the windows. I probably spent a month where the only thing that I really got done was figuring out how to properly make the scintillation counter behave.

It also took me a long period of time working out a technique like separating the nucleotides to figure out what my percentage of cytosine was in my DNA. I didn't really enjoy working out new techniques and there was always a period when I would rather go back and read. I think I'm inherently more of a theoretician than an experimentalist. In fact, one thing that having the kids did for me, right when Bernard was born, was to tell me that I had to really settle down more and do more in the lab. I'd been continuing until then to to a lot of taking courses, going to every interesting seminar that came, doing a lot of reading, and a lot of thinking of different aspects in one area. I'd been making some progress, but I realized that suddenly time was much more at premium and I had to focus much more, once Bernard was born. And I didn't have too much contact with the graduate students who came in the last two or three years that I was in graduate school, although I've maintained close contact with some who came early.

Weiner: What about contacts outside, either graduate students elsewhere or people who had their doctorates? Did you start to develop any circle of

contacts?

Kutter: You mean outside of Rochester?

Weiner: Yes, right. In the field. I mean, you had a paper, for example--

Kutter: At the phage meetings, you mean. Well, once I had given my paper, then I started to make some friends. In fact, it's to then that my friendship with Waclaw Szybalski dates; he was there when I gave my first paper, and there was one particular experiment that I was having a lot of difficulty with, a particular thing I needed to know. I talked with him about it, and it was something they were set up to do in a very automated way. He said, "Well, just send me the samples and I'll run it for you." And two weeks later I had the answer.

Weiner: That's good.

Kutter: And he was all along one of the most supportive outside people. I started to develop friendships with some of the other phage people then and over the next few years. A lot of those friendships have remained.

Weiner: Did you keep going back to the meetings after the first one?

Kutter: Yes. I went to the phage meeting every summer through '67, for those three summers. And then, in '68, we were moving. In '69, I went back. Sort of every other year on the average since then, I've gone. I still have a special sense of "coming home" when I go there; it's sort of strange now, I'm sort of one of the "elder statesmen". At the phage meetings it's a lot of young people. It was a very nice feeling; there's always a very pleasant atmosphere there. You can go to the meetings in

meetings again in the evening, and then build a big bonfire and sing and go swimming at midnight. To me the whole feeling of science and the community of science has always been very important. Sig doesn't really like meetings or get much out of them, and has always been somewhat opposed to my going. But he always realized that he had to tolerate at least one meeting a year. It was usually just one meeting a year, and that was usually the meeting I chose. Once in a while I'd go to a biochemistry meeting, or something like that. I never went to the big Federation meetings, or any of the others. To me, the small, close contact of that kind of meeting is much more pleasant.

einer: But you didn't have a real circle of graduate students that you worked with, so you were starting, sort of, at some disadvantage in terms of contacts.

Kutter: Well, yes. The people I was close to were people who were in quite different fields. Probably my best friend there was Jack Burkey, who's now at the Lawrence Radiation Lab at Berkeley, who works on chromosome damage and repair and eukaryotic systems--the only one I've kept really close contact with. I had very few friends who were outside of the department. We had a few friends who were other people who lived there, a psychiatrist who lived across the way from us and so forth, but a very small circle of friends. [We] spent most of our time fairly seriously, or else off hiking, or something, sometimes.

Weiner: This period of, leading up to '68, was a period of unrest, as it's referred to now, on college campuses. Was there much of that in your

recollection of the period? Were you involved in any way, either by being aware of it, or responding to it, or participating in it?

Kutter: I certainly didn't respond or participate in any way. Of course, the period was a period after '65 when I had kids, already, to be responsible to, when I was really very much cut off. I remember the riots in Rochester in the black community while I was still there. Except for that, my awareness of what was going on was really relatively peripheral, both in terms of any kind of scientific thing or social awareness. I had largely dropped any kind of contact of that sort when we moved to Rochester. I really became very much the serious student of science. I lost most contact with any kind of politics, although I'd been somewhat active before I knew Sig in political things in Seattle, as well as other things. So that really was a period of intense focusing.

Weiner: So nothing about the Vietnam War protests, or anything like that, or any of its ramifications on campus affected you?

Kutter: Not really. Partly because I was over in Radiation Biology, which was part of the hospital, across the railroad tracks from the main part of campus. And I very seldom got over to the main part except for classes. I did, the year between, in '66, set up a joint program. I got a professor in physics and a professor in our program together and about ten students from each, and we set up a joint biophysics course for a year between the two departments where we took turns explaining things from our point of view, because we had very little communication back and forth between the physics department and our department. But that, again, was something that pretty much died after the year that I pushed it. There were fears of

showing ignorance on both sides that I think was one of the major problems. But in terms of political things, I really didn't get involved very much at all. One thing we did do, particularly during the first few years there, was that we had--through Foster Parents' Plan--we had a Vietnamese foster child who we sent things to and so forth.

Weiner: Oh, the child didn't live with you.

Kutter: No. The child didn't live with us; it was a child who was in Vietnam. We were writing back and forth and sending things, and a point came when that was cut off. He was of Chinese ancestry in Vietnam, and suddenly everything was rejected. I think that had to do with the developing tension in Vietnam. I'd say that we talked about all the reasons why the United States shouldn't be in Vietnam; and we certainly [were] very much opposed to the war in Vietnam. But I would say that it never led, really, to the point of doing anything or even feeling we ought to do anything.

Weiner: In '68, or even as early as '67, you must have been thinking about a job, about what would happen after you did get your PhD. It was pretty clear that you'd be getting it by June of '68 and that your husband would probably be getting his at the same time.

Kutter: Yes.

Weiner: Did you have discussions then about this next job-hunting stage and what the problems might be?

Kutter: Yes. Yes, we did at that point. And basically we did some

unfortunate things, I think, at that point. It had been a fair strain on Sig, probably more than on me, having kids, in an emotional sense, because I'm a person who does ten things at once and sort of manages to keep them all straight. I still tend to lead my life that way. But there were a lot of pressures on Sig, trying to help as much as he did at home, and so forth. He's basically very Germanic in his background; when we were in Germany when Bernard was a baby, his diapers were ironed. [Laughter] His idea of a happy medium in terms of degree of order, and so forth, in the house, and mine, were coming from very different points of view. My house tended to be--well--less than perfect, shall we say. There were a lot of other things that were given higher priority.

And Sig, at that point, made a very strong statement which I should have refuted but didn't, saying that he would like me to take a year off, now that we were getting out, and prove to him that I could keep the house the way he'd like it kept. And I accepted that, not without fear, but without saying, "Hey, that's crazy!" I never thought of questioning it, really. And the decision was simply made that at that point we would only be looking for one job; so Sig was the only one who was looking for a job. And frankly, we were taking geographical considerations into account much more than any kind of considerations of future jobs for me. We were first trying to look for a job on the West Coast; we had initially naively gone east with the idea that it was probably good for us to see a different part of the country for, maybe, three or four years. And here it had been six, instead. And that was just at a time when it was very hard to get jobs and particularly when the market was starting to really dry up. There were several jobs that looked very promising, post-doc's; one at the

University of British Columbia in Vancouver; a post-doctoral position at Lawrence Radiation Lab; a position at JILA in Colorado where the interviews went extremely well and Sig was very optimistic, and then something happened. The number of post-doctoral positions available was suddenly cut from twelve to two at JILA.

Weiner: This would have all been for your husband.

Kutter: That would have all been for Sig, yes. We weren't looking for jobs for me at that point. And I knew of a couple of people at UBC that I might eventually be able to work with. I was thinking about those things. Allan Campbell was moving to Stanford at that point, and I talked with him about the possibility of working, perhaps, part-time with him if the job at Berkeley worked out for Sig. Colorado--I thought there were several possibilities--but for various reasons those all fell through and then Sig started a new round of looking, and we really became fairly concerned. Six possible jobs came up at places like Brown, and Rice, and Washington University. I've forgotten right now what the others were, but in terms of being pleasant geographically, definitely the most promising one appeared to be the University of Virginia. Also, that was the one that appeared to be most permanent and to offer the best in terms of the department. Sig went for interviews; I didn't go along. That seemed to be the one that offered the most hope. Now I think one reason that I was a complacent as I was about the whole job-hunting thing was that the time he was initiating that seriously was the time when I had mono, so I was emotionally and physically very much down and ready for a rest. I think I might have been more aware of what was going to happen to me and to my own head, otherwise.

Washington University sounded like the most interesting in terms of possibilities for me, but geographically I really preferred Charlottesville, too, and I accepted that.

And it scared me a bit and frustrated me a bit--the one catalogue that we had from Virginia (it turned out it was a couple of years old)--it looked like there was nothing there that would be particularly close to what I had been doing. There was one fellow working there with human and sheep hemoglobin variants, but no one else seemed to be doing molecular biology things. So I went, without really realizing what it would do to me and without making any attempts to make any contacts in the department, to say nothing of looking for a job. I very quickly nearly went crazy. We had no money at all for babysitters, or anything. The whole expenses of moving, and some traveling we'd had to do--Sig had gone to a meeting in Czechoslovakia; we had gone out to California for Sig's sister's wedding--we'd just drained every possible reserve. We bought a three-story townhouse. The kids were one and three, and, as I realize now in retrospect, there's no way when kids are one and three that anyone keeps the house in order unless they have a whole flotilla of servants picking up at every step. [Laughs]

Certainly with the combination of coming from the scientific things I had been doing and getting a lot of positive feedback, and the very thankless thing there, and Sig being suddenly involved super-full-time, there was really very little that I could do. You know, it was clearly a very bad situation. I very nearly had a mental breakdown at that point. I found I could keep things reasonably for a day or two and then I'd just give up and do a lot of reading. I was giving my kids less real attention than I had

before. I felt my mind very quickly slipping away; I was just letting it go. I had the second paper from my thesis that I had to try to finish and wasn't making very much progress towards [doing so.] Partly, there were a few experiments that clearly ought to be done, and I had no place to do them, just a few small clean-up things, very trivial things. And I really became extremely depressed. At the same time, Sig was being highly successful in the department; his research was going very well. He was very well-received.

About that time one thing happened that helped a little to change the direction, and it was one of those kinds of coincidences that one wonders afterwards--you know--what would have happened if that hadn't happened. Sig was giving a seminar in physics and posters were posted around for it, and a couple of days afterwards I got a phone call. The phone call was from a man named Herbert Jehle, who's at George Washington University in Washington, D.C. When we had had that special program my second year at Rochester, he was the first person who came to talk in it. I was the person who had been assigned as the student in charge of him while he was there. We had talked a lot, and he had been doing a lot of papers on things like bilateral symmetry in animals and nucleic acid things. He had continued to keep in touch, and send me copies of drafts of the biological things he had done, ask me for opinions, taken some of my comments, and so forth.

Weiner: He's a physicist?

Kutter: He's a physicist, yes, but he was interested in biophysics. Then just the last year or so I had been there we had lost touch; just

the last year or so I was in graduate school, when things were so hectic. I hadn't realized that he lived in Charlottesville and just spent three days a week up in Washington, D.C. He had seen these posters around, and wondered if perchance that was the same Kutter. Indeed it was. He had not been able to go to the seminar, but called then just to check. We started talking, talking about science and all kinds of other things, and Rochester, and things I had been doing, and I think we spent two hours on the phone. It was just at a point where I was so depressed that I had no energy to try to pull myself out of it, and the next day I called the Microbiology Department and the Biology Department and asked to be put on the mailing lists for their seminar things. I hadn't even been to the departments at all.

Weiner: How long was this after you'd been there?

Kutter: This was November, October--probably early November. Something like that. Long enough to be frustrating. Also saw him several times later, but it was particularly that first conversation that started things clicking. A couple of weeks later there was a seminar in the Microbiology Department that was on a subject having to do with some interesting nucleases, by a very delightful Chinese girl whose name I don't remember, and which sounded interesting. I went to it. It was probably closer to the things I'd been doing than to that of anyone else there, and I asked a lot of questions because I was really interested. And it was also, you know, sort of like coming out of a vacuum. Afterwards several people came over to me and said, "Who are you?" [Laughter]

Weiner: You were the new girl on campus.

Kutter: "What are you doing? You clearly know something." I told them, you know, where I'd come from and the kind of work I'd done, and they were very interested. One of them was the one who arranged their seminars, and he asked me if I'd be willing to give a seminar in January. I assumed he'd at least pay me for it. Damn the buggers, they didn't. [She laughs] Didn't give me any kind of an honorarium.

Weiner: They would have paid your travel.

Kutter: No, but, you know, I thought that there would be some kind of very small honorarium. I needed a little reinforcement. But at least just giving the seminar was very good. Bob Kretsinger was also there, and offered to let me do a few experiments in his lab, what I needed to finish things up. And he was doing some things with T_4 lysosomes, so he was basically set up. He didn't have any money to pay me, but would be willing to let me do that. I didn't really have a chance to do anything before Christmas and this talk in January. I went in a couple of times and started talking with people a little bit, but the kids tied me down a lot and I still had a lot to draw me back. And Christmas is a busy time anywhere, particularly with small kids and lots of Christmas baking and things; it's something I've always enjoyed. Inevitably, the few weeks before Christmas you do a lot of that.

Anyhow, then I gave my seminar in January and one of the people who came was Rolf Benzinger, who's also in that department, and he became very excited by the possibilities of some of the things I'd been doing, particularly

the idea that T_4 was making some nucleases that appeared to be specific for stretches of cytosine, which at that point it looked like might be spacers between genes, stretches of pyrimidines. And he's a person who tends to get very excited. He asked me if I'd be willing to work for him half-time, that he had money actually to pay a graduate student full-time that he could use to pay me as a research associate half-time. My initial response was that I'd promised Sig that I would take this year off and I had no way to take care of the kids. I couldn't do it 'til summer. He was pushing, and saying, "Well, I have this money now and I don't know how long I'll have it. Such and such a grant runs out." And really pushing me to start right away, in fact to the point where he finally went to Sig, telling him that this was such exciting work and that if it worked out as he thought it might it would surely be a Nobel prize [laughs] and, you know, all this kind of stuff. He convinced Sig as much as he did me. Well Sig came home and said, "You know this is what he's saying and you're clearly utterly unhappy at home, and you can't function in this way." He had been becoming extremely angry with my inability to cope with things.

Weiner: Yes. Well, you haven't really clarified that. You mean to stay home, to take care of the house.

Kutter: Keep it in the way he would like it kept and keep my sanity. In other words, to be interested enough in the house to be really happy at least for a short period of time.

Weiner: And you were doing none of the above.

Kutter: [Laughter] No. That was bad. So anyhow, he said, "Why don't you go ahead." We had somebody who came in by that time, one day every other week, to help with the house, that started just about then. And about a month and a half later I started. It meant that once in a while I had to take the kids by the lab for a little while, which was not well-received there. In fact, a few months after I'd started, Dietrick Bodenstein went by once when I was just down the hall and saw Eric sitting on the centrifuge putting big plastic bottles in and out, not hurting anything, and he called in Rolf and told him that he didn't think it was wise for me to have the kids there--he didn't call me in--and that I shouldn't have them there at all, which made things then very difficult.

Weiner: Were there any other female faculty members of research associates?

Kutter: No. The whole atmosphere at Virginia, at that point particularly, was extremely male-oriented. In fact, there weren't even women allowed as undergraduates unless what they wanted wasn't available at any other institution in the state. The only two women around that people saw at all were the wives of a couple of faculty who had PhD's in their own rights, and were doing sort of small piddling things in their husband's labs, for which their husbands were getting most of the credit. One of them, at least, I think was very good. But the atmosphere was certainly not conducive. There were no women on the faculty. In fact, there were only two women on the faculty in all the sciences. They were quite a bit older, one in physics and one in astronomy, but none in biology. More that half of the entering students in biology were women, and the ones with the best credentials [were] coming in. And yet, very few of them finished and the

faculty took that as proof that women just weren't going to stick it out, and weren't going to hack it. They were mainly taken into that degree, I think, with the idea that they would be good to help teach the basic courses the first year or two in graduate school. And many of them wound up just quitting, and some got master's degrees.

It became to me, very soon, very apparent that one of the major problems was the total lack of role models and the total lack of any sense of real support on the part of the men, and the attitude that "you'll just get married anyway," and with that, one's career would have to stop, and just--you know--a general difference in the way men were treated from the way women were treated. If men had problems they were sort of pushed to keep going, and if women were having problems, "well, that's okay, don't worry about it. They don't need it, anyway." A lot of them started coming and talking to me. They'd been very much led to believe that it was impossible to have both a family and a career, that they had to make that choice. And so, the fact that I had both, even with the limited position that I had there, was something that was very interesting to them. You know, I can't say clearly how much of a role that played, but during the years after that a much higher percentage of the women started finishing. So I was in a very strange position, on the one hand feeling extremely grateful to Rolf and to the people there who were making it possible for me to keep in touch, to some degree at least, with research, and keep my membership card good, and keep my sanity. On the other hand, Bodenstein and others there made it very clear that I had no chance of a faculty position there, even later when I had a grant, and a good many of the faculty didn't.

It was hard to call [it] discrimination, even though I might have thought of it that way. After all, how many men could expect to go to a specific school and say, "Here I am. I want a job here. Give me one." They had just built up the faculty in that general area of molecular biology with a big training grant over the last couple of years. There were a lot of small ways in which I was made to feel very much like a second-rate citizen through all the years there. For instance, when speakers would come, Dietrick Bodenstein was chairman of the department, and he'd very often have the faculty over for some sort of a special get-together at his house. I was never once invited to any of those things at his house, even when the guest was someone whose field was fairly close to mine. Bodenstein was a very paternal sort of person, who, later when I was talking about applying for a grant, encouraged me and signed the papers, on a half-time basis, which was a very unprecedented step for the university to take to be willing to sign a grant which was applied for by me alone, without a faculty position, and so forth. And so at that level he was very supportive of me, to sort of keep in touch, to sort of keep "playing at" science. But to try to get them to take me seriously as a professional was much more difficult. I also made the mistake there of allowing Rolf to convince me to work on what was clearly to him the most exciting part of the problem, of trying to purify the nucleases and determine their specificity and see how useful they would be for other things. I made some progress with it, enough to be intriguing, but I was setting myself up to fall. I mean, trying to purify enzymes, particularly unstable enzymes, where you depend on spheroplast for an assay, when you're working part-time with kids at home that you have to get home at certain times for, and with a husband

who also is sort of willing to let you "play" but is certainly non-supportive of anything that involves intense commitment, was totally absurd. I mean it was really a mistake. I should have stuck with some of the other more genetic and control-of-gene-function aspects of the problem. There were a lot of those there, too. And I also was at that point somewhat concerned with the idea of being able to cut DNA between genes and use it for genetic manipulations. I had very mixed feelings at that point about the advisability of doing that, and whether that was something that I really wanted to contribute to.

Weiner: When you said you were concerned, you mean interested.

Kutter: No, I mean it intrigued me, but it bothered me. I was thinking then already, to some extent, of some long-range possibilities of being able to purify out a particular gene, and I wasn't comfortable with it. I both was fascinated with it, and uncomfortable with it in terms of its possible misuse.

Weiner: How did this come up in your thinking? I mean, was there a specific piece of scientific work that brought it to your attention?

Kutter: Well, it was the idea that if these enzymes that I had really cut between genes in any C-containing DNA, then that would mean that they could be used on eukaryotes as well as bacteria and so forth, and it might then somehow be possible to select for specific genes. Of course, I wasn't thinking then in terms of recombinant DNA per se, but I guess I was one of the first people who was separating DNA fragments on gels, for instance. I didn't know how to get them back out again, and the initial results with

FD phage, with the second enzyme which is purely single-strand specific, were intriguing because I got eight bands on the gels which were just about the right sizes for the eight genes of FD.

I went down at one point to talk with Edgell and Hutchinson, down at Duke, who were doing some work with DNA on gels towards the end of this period, and earlier I went up to NIH and talked with Peacock and Dingman, who'd done a lot of the RNA-on-gel work and got help from them in setting up my initial gels. But I was having trouble trying to get the DNA fragments back out, which I would need to do. I wanted to do some experiments to show whether the cuts were between genes or within genes. I wanted to do them by complementation experiments with mutants of the various genes of FD in Ø .

Weiner: Were you aware of other work in the area?

Kutter: No. There wasn't really any other direct work like that as far as I know, at that point. That was starting in early '69. That was at the point where people were first starting to figure out what kind of sequences the restriction enzymes saw and first having the idea that they made specific fragments. But that was before those fragments were really being separated on gels.

Weiner: But you hadn't separated them either, yet.

Kutter: Oh, I was separating my fragments on gels. That's what I was doing, but I wasn't getting them back out of the gels. Well, it was towards the end of that period that Edgell was starting also to do some of the same kinds of separation on gels that I was trying to do. And, of

course, he had a larger lab and more energy and was really--

Weiner: More time.

Kutter: More time, yes. Well, I think that more energy was more of a problem at that point. And I got involved in a couple of other things that sort of frustrated Rolf. For one thing, there were some people in the engineering department working on a very interesting technique for killing viruses in sewage, and that really appealed to me. I helped them do the initial experiments using T_4 as a model system. It actually turned out that the technique was working quite well, and it's being patented for certain applications now. It will also work to kill algae in fish tanks, or even killing schistosomes. So you know, there I felt I was doing something, or I was getting somewhere. A lot of the other stuff I was finding very frustrating and feeling embarrassed even talking to people about asking for help, because I wasn't making progress enough.

Weiner: Was your grant for that work, or what work was the grant for?

Kutter: The grant was for the purifying the nucleases and some of the other T_4 work.

Weiner: I see, that's it. And that was to pay your salary?

Kutter: Yes. I applied for that about six months after I'd gotten started working for Rolf, so it started a year after I started there, and it paid my salary and a little bit of money for a student to help, and it paid supply things; a lot of things I was sharing with Rolf.

Weiner: What part of NSF was this from? What panel, or division?

Kutter: I believe genetics biology then already. In fact, the person who was directly in charge of my grant at that point was Herman Lewis.

Weiner: Yes, right.

Kutter: Actually, I applied to NIH and NSF, but I didn't even want to apply. I didn't think there was any chance of either of them giving any money to somebody with as little background as I had, who was working part-time, had no faculty position. You know, the whole combination of things; I didn't think there was a chance. But Rolf very much pushed me to do it. I figured I couldn't ask him to keep supporting me if I wasn't even willing to try, so I made myself sit down and try. And when NSF came through offering me two-year support, I accepted it. As it turned out then, NIH also came through, offering me three years, but I'd already accepted the two years from NSF and turned down the three years from NIH. In retrospect, apparently, I could have accepted also the NIH one but just accepted money from them only up to their total recommended funding. But I had no idea of anything like that at that point. It was funny how I found out about getting it. We were skiing in Colorado, and I got back to the lodge and there was a message that there was a phone call from NSF for Dr. Kutter. We stood there for about an hour trying to figure out why on earth NSF was trying to get hold of Sig. We finally realized it was more likely to be me, and to have something to do with my grant. And they apologized for disturbing me on my vacation, but there were a few things they needed to know.

Weiner: When you said that you began to have concern over the possibilities that you saw coming up, did you discuss these with anyone?

Kutter: No, not really. It was a personal thing, and I really had no one to talk with about those kinds of things, especially there. I mean, Rolf was completely gung-ho to do it. And, in fact, these concerns expressed themselves more through foot-dragging, and doing other things. That, coupled with the frustrations of trying to do the work, than in any other way. It was hard to really go in with gusto and stand up to things at home as I would have had to have done, because I had these also, these concerns and fears. I spent more time then on a couple of the other projects. Rolf became understandably very frustrated with me, and there were sort of tensions in the relationship which I was very sorry for later. I could understand why they were there, and I could understand his frustrations. Basically, however, I've gotten much more science done since I came to Evergreen than I was really accomplishing there.

Weiner: I'd like to get to that. I just wondered--on the NSF grant--that was still half-time, though, right?

Kutter: That was half-time, yes.

Weiner: And you stayed at Virginia a total of three years, was it?

Kutter: A total of four. The first half-year I wasn't working.

Weiner: And your grant was for the two-year period, you said, only. But you had his grant.

Kutter: His grant supported me for a little over a year, and then there was that two-year period. Then at the end of that two-year period, I applied for renewal from NSF, and applied for a grant from NIH at the same time, and the NSF one at that point was approved but not funded. I hadn't gotten any publications out in that period. Well, I had the one more publication; I did get the second paper from my thesis out. But the NIH one was given apparently a very high priority. In fact, I learned much later through odd sources what the priority was that was given it, and it was an extremely high priority, which is strange. They awarded me a three-year grant at NIH. Well, I had very mixed feelings--

BEGIN TAPE TWO, SIDE TWO

Weiner: You hadn't accomplished very much?

Kutter: I felt that I hadn't really accomplished very much in the lab, not compared to what Rolf's expectations were, certainly, and a lot of things weren't going all that well. And I, on the one hand wanted to keep going, and on the other hand, I had this almost-wish that I just wouldn't be funded so I could stop trying to be three people at once and just an ordinary human being again. I'm not sure how serious those [feelings] were.

At the same time, the summer before I'd applied, I'd gone out to San Francisco to the Biochemistry Society meetings. While I was out there, I had come up to Evergreen to visit. We had been interested in Evergreen ever since there was first talk that it was going to be opening, which was well before it did open. Sig had written and applied for a job a year and a half earlier and been told that nothing was available at that point.

(That would have been the Planning Faculty.) My parents, who were in Seattle, had kept sending us things about Evergreen. And the whole educational concept intrigued us. Sig's research was going extremely well, but he really wanted to be closer to the mountains, the West, climbing, skiing, hiking. He was beginning to feel that maybe he'd jumped too far, that somehow he had really lost something and really sold himself short in going from a family where no one had even gone to high school, where his Dad is a chimney sweep, trying to go to the top levels in science. And he felt very frustrated that he wasn't getting the kind of recognition for his work, generally, that he felt that he ought to be getting. He'd go to a meeting, and nobody would really come over and talk with him, even though it was very clear that what they were doing was the leading work in stellar evolution at that point. He was getting, in several ways, sort of bad-mouthed by his ex-thesis advisor, which was partly related to the fact that Sig had been more concerned about geography than just the nature of the place where he was going, and would take time out to do hiking and skiing, and things like that, and therefore wasn't a serious enough scientist. There were certain misunderstandings that had been there to some degree all along. Perhaps even certain petty jealousies, although I don't know whether that's something I should leave in or not.

So at any rate, he was thinking that he was really enjoying the teaching and got top reviews among the students at Virginia for his teaching and that he might like to try a place like Evergreen. So while I was going out to San Francisco, he suggested I come up and visit my folks, and visit Evergreen. We hadn't been there before. I did; it was before it opened, by a few months, and none of the buildings were finished. I talked with Don

Humphrey, who was one of the three deans at the time, and a scientist. We hit it off very well right from the beginning, and he was really interested in my background and the combination of things I'd done and been interested in, and, I guess, in my enthusiasm and all, too. Up 'til then only Sig had applied, because he'd always; also when we were applying initially for jobs, felt that it would be much easier to get one job and then maybe worry about getting a second job later, for him only to apply. If they see two people, then they're going to run scared. But Don very much encouraged me to go ahead and fill out an application, which I did. We heard nothing more until a few weeks before Thanksgiving when we got a phone call from Evergreen saying that Don Humphrey and one of the other deans were going to be in Washington, D.C., and asking us if we could come up for an interview three days later. So we had our interview sitting in the park in front of the White House and watching the pigeons, and so forth, and really hit it off, all of us, very well, and wound up then going out for drinks and talking for quite a few hours. And we both had the sense that things had gone very well; we both had this utter sense of certainty that we would be offered jobs. Now at the time, during the interview, the question was raised of whether we would take a job if only one of us was offered a position. And it was very clear that if it was one of us, that it would be me rather than Sig; that some of the combinations of things I had done they were particularly interested in, and they were really concerned about equal opportunity for women and particularly looking for women in the sciences. They were also, though, basically interested in getting a lot of kinds of representative people on the faculty, sort of a broad distribution, and they had been looking for a couple. Actually,

I guess there had been several couples where they basically liked the women a lot but not the men, and one where it had been the other way around. But there were none where they'd made offers to both of them before.

But then we heard nothing until March, I guess it was. By the time we did hear, I had heard from NSF that it was approved but not funded, and hadn't heard anything from NIH. I was feeling sort of almost a sense of relief. Well, here was this job, then, being offered at Evergreen that came just a few days after the NSF thing. I assumed I wouldn't ever be able to do research here. I'd never talked with people about that. I was interested in exploring what they had, and I didn't want to give the impression that I was looking for a place where the main thing I would still do was research. I knew that it was basically an educationally-oriented place, and I had very, very mixed feelings about the possibility of giving up research. Well, good and bad. Research at times, seemed almost to be a drug. It's the kind of thing you get addicted to. The feedback you get is all for certain kinds of things, and you keep in those kinds of circles so you really can't let go of doing it because the people whose opinions you value, value that. You know, the same way I was with religion, I start questioning something when I realize that a lot of the reasons for it have to do with the fact that it's accepted by the small group of people that you're associated with. At any rate, we went ahead and--

Weiner: The offer came through to both of you?

Kutter: The offer came through to both of us, yes. Sig had been sort of almost hoping that it would come through first just to me. They had talked about a part-time thing working with the self-based learning thing perhaps for him for a year, and he was going to teach skiing and [laughter] let me

support him for a while.

Weiner: But that would have meant getting out of research for him, as well.

Kutter: Well, it did anyway. He was at that point very much ready to get out of research for a while. He had been spending his summers up in Washington, D.C., doing research at the Goddard Space Flight Center, that had a big enough computer. It was still at that point such that he thought, and it was perfectly taken for granted, that he could take off for the summer and leave the kids mainly to me, and do research, but that, you know, one meeting a year was enough as far as I was concerned. [Laughs] I again never questioned that much, but actually I got a lot more research usually done during the summers when I'd be with the kids during the day and put them to bed often and do some reading and all at home, have somebody put on my bacteria for me just before they went home at five o'clock, and then get the kids to bed about eight or so and go over and work. And our next door neighbor would listen for them to cry or something; but we could leave them alone. Then I'd work from nine 'til two or three.

Weiner: Now, this was where?

Kutter: This was at Virginia, when Sig would be gone in the summers. And then I could really intensely get into doing my work to a degree that I really couldn't when he was around. There were a lot of things, a lot of interactions, a lot of needs he had. I made progress. I made enough to keep my membership card good, so to speak.

Anyhow, then NIH came through and did fund it, and so I had a big choice to make. I wrote to the people here, telling them what had happened,

and, before just totally rejecting it, asking what the situation was in terms of research. Ed Karmondy, who now is provost here (but at that point was just acting dean for a year because of Don Humphrey's heart attack), wrote back very encouragingly and told me about the new science building that was going to soon be finished; told me about the large amounts of money to [be] put into equipment, and suggested that I do try to get the grant transferred here and that there would be a lot of emotional support, that he felt it was important that some research go on here. I had a lot of reservations. What I realized I would have to do, at least, was to postpone the start of the grant for nine months because the building wasn't finished, and I'd have to get adapted here, and there were a lot of things to be done.

So I wrote to NIH and asked them to transfer it and Virginia relinquished title to it, and I asked them to delay the start by nine months. They did all of these things. It meant completely rewriting the budget, because now it meant not paying my salary and to pay a technician's salary [instead]. I wrote it initially to get equipment, then after a while it became apparent that Evergreen had the money to buy, from certain funds that had been set aside in building a science building, all the equipment that I could want, and I only got a few small pieces of equipment out of mine. So I set it aside to pay money for two half-time technicians. Initially I set it up at Kormondy's suggestion to pay salary for me for one-quarter the second year so that I could work full-time. Between inflation and the fact that NIH had a general across-the-board cut of ten percent, at the point when the second year would have become due and feeling the importance of paying my technicians a decent wage, I

wound up cutting out that quarter as the one thing I could cut out, and cutting out the equipment money.

So at any rate, we decided to go ahead and move here. As I say, even though I thought at the point the decision was being made to give up research, it turned out it wasn't. When we came out, Sig also was very much pushing me to go ahead and take my grant, partly because he felt that we could really use the money that I'd get for my summer salary--which seemed strange to me, because we were getting two full salaries--we really could do without it; and partly because both he and Ed felt that if I turned down a grant at that point that it would be much harder to get one later. I really debated whether to try to do it even so. It was a lot of soul-searching, a lot of very serious soul-searching, because there were a lot of good things in my marriage but a lot of serious problems and I realized the strain of teaching full-time for the first time would already be a big strain. The kids were still small, kindergarten and second grade. Trying to do research, run a lab, run a grant was something that no one else at that time was trying to do at Evergreen, so it meant that I was doing essentially everything that other people were doing plus two extra jobs. I really seriously debated about it and finally decided to go ahead and try to do it, partly because the most final decision-making time was after we had gotten out here; I mean, the thinking kept going, after I'd come out here. I'd asked them to delay it, but I could still at that point have cancelled it. There were wives of two of the faculty who had master's degrees in science and had had nothing to be able to do with their skills here, and they were both very anxious to get jobs when they

heard about the grant. One had done chemistry related to certain bacterial systems, and the other had done mainly electron microscopy with insect viruses, both of which were quite a ways away. But they had a fairly strong background, so I saw that as a chance to have someone who could run the lab and help other women out at the same time, by giving them a job. That probably was one of the final straws that slightly tipped the balance in that direction.

Weiner: Is your teaching load reduced at all when you have a grant?

Kutter: No.

Weiner: So what is the load?

Kutter: Well, there's no way to reduce it because of the way the situation is at Evergreen. Actually what I did do was to say that the first year then that I had the grant, which was the second year I was here, one stipulation that I put was that what I wanted was something which was directly related to my own field so that I wasn't trying to think in completely different directions. The first year I was here I was teaching in a program called "Male-Female Roles in Society," and it was a complete turnaround from anything I'd ever done, working with another biologist and a historian, a psychologist and somebody in comparative literature. I found that for the first six months I had to even stop reading the scientific literature. I just had to put myself into a completely different space in order to be able to function in that program. I couldn't have started running a lab at the same time, so I arranged that I would do a group contract in molecular biology, which meant that I had twenty

kids working with me full time. I was full-time responsible for them, doing molecular biology, which we went through Lehninger with a lot of applications. We did a lot of lab things, many of them T_4 oriented. We used Watson's Molecular Biology of the Gene as supplementary reading. --[interview interrupted]--

Weiner: We are resuming now on the twenty-first of May, and when we left off you were talking about the contract in molecular biology that is the way for students to do work in molecular biology with you. And you were telling a little bit about the course, that you used Lehninger, and I think you were saying Watson. Didn't you use Watson?

Kutter: Yes, we used Watson's Molecular Biology of the Gene, Segel's Biochemical Calculation, and Biochemical Reasoning. I had a draft version of Wood, Wilson, Benbow and Hood's book, Biochemistry: A Problems Approach, which I used a lot for examples and problems. Actually in talking about that, though, we're getting quite a bit ahead of the story, and maybe this part should be delayed until later.

I just wanted to mention at that point really, that I decided the only way I would be able to do research here would be to be teaching at that time in a program where the orientation, subject matter and ideas were related to the things I was trying to do research on, at least in the first year or two, while I was getting the program off the ground. That statement only makes sense in the context of the ways things are often done at Evergreen and the whole orientation of Evergreen. The first year that I was here I taught in a program called "Male-Female Roles in Society." I heard first in the summer before coming that I was being

assigned to that program and I was extremely upset with that assignment. In fact, during my interview in the fall before, I'd been asked by one of the deans who was interested in doing a program of that sort if that's the kind of thing I would feel comfortable doing, and I told him, "Well, I could probably do it if I really had to, but it's not the kind of thing that I would look for or want to do." When a list came around of programs that were planned to be offered asking us to make choices of what we wanted to teach, I checked four different things, none of which were "Male-Female Roles in Society." I had sort of a feeling that they were likely to try to push me into there and I really didn't want it. That program was one designed to be what's called a "coordinated studies program" here, a full-time commitment for five faculty and about a hundred students. The five of us were a psychologist, a historian, someone in comparative literature, a more classical biologist, and myself. The program explored all the way from self-study, talking about individual feelings (almost an encounter group); and then on up into looking at autobiographies and biographies of a lot of different individuals, reading literature such as Sometimes a Great Notion and Manchild in the Promised Land, a number of different books about individuals and their roles and their feelings; and then going on up into exploring more societal things, Family in Transition, anthropological things like Margaret Mead, things like homosexuality, sexual fantasies and utopian ideas; and at the same time doing a little bit of biology, of human physiology; and going quite a bit also into the history, into the relationship between the industrial revolution and the development of the concept of the nuclear family, and the changing roles

for women and children that came out as a result of that. And the program was trying to tie all of these different ideas together.

As I say, I was extremely upset when I learned I'd been assigned to work in that, more upset than made sense, perhaps. Part of it was I got a letter that simply said, "You are going to do this," with no indication that, "Please, we understand that this isn't what you want," and from a dean who was just leaving for vacation as it was mailed and [would] not [be] going to be back from Greece until about the time I got here. And I knew no one; I was alone at home, and literally cried for a large number of hours. I think one of the major reasons was that I felt that I was being now put back into a situation of being assigned to something because of what I was, somehow, rather than because of anything I knew, and without my interests being taken into consideration. There were the fears, of course, of moving into a new situation at the same time. Part of it was that I knew there were certain very substantial difficulties in my own marriage and certain parts of our relationship that were very hard to justify, and that it would be very hard to teach in a program like that in an open and honest way without getting into really exploring and perhaps having to defend, at least to myself, if not to other people, some of those things that I was preferring to ignore.

And it became apparent that my expectation of difficulties was not totally unfounded. It was really a big jump from doing scientific research to suddenly being teaching full time, and teaching in something where I was expected to be able [to] lead a seminar in literature and lead a sort of encounter group, and all of these different kinds of things.

Three of us out of the five in the program were new to Evergreen, and each one of us came with our own particular background and fears and ideas; particularly, there was one faculty member, to some degree two, who whenever I would call for more substance in the program assumed I meant more science and became very upset with some of my ideas and attitudes. And it took a long time before we learned to really communicate with each other and realize what attitudes and ideas we were coming from. One thing that I very soon realized was that in order to do a good job of what I was doing and to sort out some of the things that were happening to me, I literally had to completely forget science for at least the first few months. That meant not trying to squeeze in an hour here or there to read a journal, not trying to think about what equipment needed to be bought for the new lab building, just simply throw myself heart and soul into trying to do a good job of what I was doing. There were a couple of other factors that made life both interesting and at times difficult that first year, particularly that first quarter. We were building a new house that we had largely designed ourselves, and we were doing a lot of the final designing as it went along. We weren't doing the actual physical building, but we were supervising the building and at times, helping; [it's] a strange, unusual house that's three hexagons put together and tucked out in the woods. It required a lot of thought and brought us together very closely. We were living in a one-bedroom apartment with two mattresses and a table and a desk, and weren't worried about order and neatness and that kind of thing there, which was very good for us.

The other complicating factor was that my older son was in second grade and in school a full day, which was fine, but the younger one was only in

kindergarten, which was half-days. I'd assumed he would be able to be in the Day Care Center that was run here at Evergreen for those half-days, but it turned [out] that they had put priorities in terms of who could be there. First of all, students had first priority and beyond that, families where there was only one parent had priority over families where only one parent was working. Families where both parents were working had bottom priority in terms of the idea that we could afford to pay for some more expensive kind of thing, I think. But the only other places that were available involved a good deal of running around and driving around and getting him to a nursery school early in the morning, getting him at noon, getting him to his kindergarten. Then he would take the bus home after school. A neighbor where we were building took him one day a week, but there was a lot of tension. He got tired of going to two different schools and after a few months I was able to arrange to switch him over to the Day Care Center. Fairly often later on, he would decide that it was too much going to two different schools and would want to come with me. When he wanted to badly enough to promise to be quiet, I would take him along and he would sit and read, or play with his blocks under the table in a very quiet way while I had my seminar with my students. And it worked out very well; I think it would work well in almost any kind of program, but it somehow seemed particularly appropriate to include my family in that way in a program talking about male-female roles in society.

That program was very good for me in some ways, in retrospect. It was the first time that I really learned to communicate with other women. I'd always had a few women as close friends who were basically women

scientists, but back in Virginia and Rochester there'd always been a problem knowing how to break through the superficial barrier between myself and most women who had chosen a straight homemaker's role. Inevitably, there are certain potentials for conflict there from several different points of view. First of all, I'm somehow managing to be both a housewife and a professional person, and that easily is perceived as being very threatening by women who have chosen only to be at home and take care of the children. They have to assume either than I'm goofing and neglecting some of the responsibilities that I ought to take care of, or that somehow they're goofing off and not really living up to what they could be doing and finding some kind of an escape. There seemed to be that kind of tacit assumptions. There was also generally the feeling that I must be too busy so they never call me to come over for coffee or come over to me to sit and talk, and back while we were at Virginia, when we'd be at parties in my husband's department I'd usually wind up talking with the men. Sometimes I'd make a point of trying to talk with the women. And we'd talk about children and recipes, and things like that. But I never knew how to get beyond that level to finding out the kinds of things that really interested them, the kinds of intellectual pursuits that they had perhaps followed earlier and were interested in following some day, the kinds of things they thought about as important, the kinds of fears about being trapped in a suburban housewife's role, or the kinds of ways they kept themselves alive intellectually.

There really are a lot of things going on there that I didn't know how to begin to get through to, and teaching in the program that I did,

which included both kids straight out of high school and housewives who were coming back to college for the first time, and a couple of older men, I learned to break past some of those barriers and I learned to become aware of much more of what was going on in other people, and much more sensitive. And I sort of felt that the whole half of humanity that had been closed to me for an awful lot of years when I'd been interacting mainly with men as a scientist and having almost no chance to have close contacts with women--that whole half of humanity had been in effect closed to me, so in that sense it was a really exciting thing and something that I'm very much grateful for.

At the same time, it did not make my marriage any easier. There were several things happening. I was under many more stresses from working hard and it was my first teaching experience, which inevitably meant it took more time and energy than Sig's teaching did for him, having had a good deal of experience at teaching. There were a lot of things that needed to be sorted out that weren't sorted out as much as they should have been; there were tensions and fears and feelings. Gradually in the spring I began to be able again to put together the different sides of my life more, to think about what I was going to be teaching the following year, to think about what equipment needed to be ordered for the lab, to once in a while do a little bit of reading of the literature again, take a few hours away. Particularly spring quarter when the program sort of split up into parts and we took students on contract. I had some students doing biological things and some students working in various kinds of clinical psychology settings, penal institutions and things like that, which was really an

exciting experience, but I wasn't so much dependent on continually interacting with four other faculty people, some of whom at least were very much afraid of my scientific interests. We had weekly seminars, this group of faculty, and talked very intensively about our feelings and our ideas and our emotional involvements and things that had made us what we were, as well as about the books we were reading and the ways we were dealing with students. Despite the tensions between us, these were very good. It was a strange dichotomy of having these fears and tensions, and yet this incredible degree of openness. At any rate, that's relatively irrelevant at this point.

Weiner: That ties in, in a positive way as reinforcement that you were getting by directly talking about these issues with colleagues, which you were apparently not able to do at home.

Kutter: Yes, that's largely true. We also moved into our house at Christmas time which was wonderful, to be in the house. But the problem it ran into was that my husband suddenly became much more uptight about things being well taken care of, in order. We had this beautiful home; we had to keep it up now. And for some reason that I don't understand at all, in retrospect, we didn't have the sense to realize that we had to get a housekeeper or someone who would help us with those things. Clearly, money was somewhat of a problem at that point, having just finished building. But, in retrospect, it would have been absolutely essential to have had someone come in once or twice a week and do some of the time-consuming household things. We tended to split chores, but inevitably more of the

responsibility fell on me and there were a lot of problems related to that.

And, actually, that spring for the first time since I'd been in junior high I saw a psychologist several times; I was at that point where I was very much concerned about a lot of things and got some support and some help from seeing a clinical psychologist at Group Health, just trying to sort out some of the things that were happening. At any rate, when I first applied for the grant my husband had been very supportive. When I first applied to transfer it to Evergreen he'd been very supportive of my going ahead and taking the grant for a variety of reasons. For one thing, he felt we needed the money; also, he felt that it was something that I shouldn't just drop. At that point, I'd said that if I were going to do it, there was one thing that I felt was really necessary and that it was going to take a chunk of time of undivided attention to try to set up a lab. He wasn't going to be working the following summer, the first summer after we were there, but instead, he was going to be doing some landscaping and work around our new house. I said that what I would like was that for that one three-month period, sort of for the first time in our marriage, to be able to not have any responsibility for what went on at home. I'd help, but to be in the kind of situation he'd been in when we were first at Virginia, where he would take the responsibility, asking me to help with things. If I felt I needed to go back in the evening, I would go back in the evening; if I were going to be late getting home, I'd simply call and say, "I'm going to be late getting home," and he'd take the major responsibility for the kids and the house. We talked about it. I'm not sure we talked about it in as specific terms as I've used just now, but

I felt that we'd reached a good agreement, that that was what would happen, that he would take the responsibility.

A major complication entered. His parents came and visited us from Germany for seven weeks. I dearly love his parents and we're very close, but at the same time this meant that now with two women in the house Sig had the feeling that he had all these things that had to be done outdoors: getting a lawn in, getting gardens in, making fences, and so forth, beautifying things basically, nothing that had to be done in any particular order at any particular time. He felt that he had all these other things and that with two women in the house he shouldn't have to help there. His mother was seventy then, and doesn't speak English and is feeling her age in some ways, and certainly helps, but there was still an awful lot to be done that she couldn't handle. I clearly felt certain pressures not to let things go too much. We allowed ourselves in that situation to fall back, to some degree, in a pattern of roles. I did a fair bit of work getting the lab organized and I put in a regular workday usually, though not always. I'd sometimes come home to fix the main meal at noon for them which was what they were used to, but it meant that I didn't have that chance to totally immerse myself and really get things going that I really needed.

Sig, at the same time, would get really angry that I didn't find time to help him with weeding or other things that he felt were important. I was too busy; he got the feeling then that my work was too important to me. I didn't sort out exactly what was happening until a year or two later, really. It's my own fault just as much for not simply saying at the time, "Hey, Sig, this is what we talked about. This is what we agreed. This is

what I have to do. And if I don't do it, we're going to be in problems later." I mean, it's as much my fault, certainly, as his for letting it fall into that kind of pattern, but it was a big mistake. It meant then that when fall came there was still a fair bit to be done in terms of getting the lab properly organized.

Just after Sig's parents left then, he had made arrangements before to take off for five weeks on a National Outdoor Leadership Course in the North Cascades. I was going East to go to the phage meetings, and took the kids along and went by train across the country to New York. Then I put them alone on the bus up to Boston to stay with friends there during the meeting and went and got them, stopped in Charlottesville and Minneapolis on the way back by train and got back just a couple of weeks before school started. But all in all, I hadn't had the time to really get everything in smooth-running order in the lab before school started. I was teaching a program which required really extensive use of the lab, so that meant that I was really trying to do double-duty once school started, putting in a lot of energy and long hours, for which Sig was only partially supportive.

Serious problems were developing that eventually led three years later--starting two years ago--to our deciding to separate. There was always the conflict there between my degree of professionalism and the kinds of things that he could really accept, which I think is very often a problem in marriages where the woman is really professionally involved. I see many marriages where it works well, but those are marriages where the people face that problem squarely and usually get outside help to do a lot of the more trivial jobs around the home, and really talk about what the roles are

and what's important in terms of the professional things, and what the expectations are both ways 'round. We talked at those problems a little bit, but we never really sat down and dealt with them.

On top of that came the problem that with Sig's being German, he had basically been raised in a home where the mother's only role was to take care of the household, where my older son's diapers were ironed the summer that we visited when he was a baby. And the floors were washed every day. There were conflicts in his own mind between his intellectual expectations that I ought to be able to be a fully independent professional person and lead a life in my own right, and very deep-seated emotional feelings that he never even allowed himself to admit or come to grips with until after we were out here, that tied back what a woman's role ought to be in terms of what his experiences of a woman's role had been as a child. There was a strong conflict in his own mind.

At any rate, the program in molecular biology went basically very well. The labs didn't run as smoothly as I would have liked at times, and I didn't have as much time as I would have liked to put into getting them to run really well, but the students learned a lot. It was a combination of a basic biochemistry course that went through Lehninger with a lot of supplementary work, and a journal club talking about current literature with different students taking turns reading one major paper and some of the background papers for it, then getting up and critically reviewing it, as is often done in graduate school. Did an amazingly good job. I was surprised. [Laughs] They put a large amount of work in each time, when they were going to do it, plus the biochemistry and molecular biology lab using E. coli and T_4 model systems for a lot of what we were doing. Then

in the winter quarter, in conjunction with the biochemistry, we talked about some of the applications, talking about synthesis of amino acids. We spent a lot of time on neurotransmitters and neuropharmacology; we talked about immunology. Spring quarter, we went quite extensively into developmental biology as it relates more at the molecular level than the biochemical level. By the middle of winter quarter, many were starting to get involved in projects, and quite a few did detailed projects the spring quarter, a few of which tied in with my phage things, and some of which were totally unrelated. At that point, I didn't try too much to push doing phage; I didn't want it to seem like I was just trying to get them to work for me, or something. In retrospect, I should have pushed more. Those who worked with the system already established clearly got a lot more out of the projects that they did and made much more progress. And phage, particularly T_4, really is one of the most ideal model systems around to learn the techniques of molecular biology and the ways of analyzing experiments, defining problems, and setting up ways to do something about them.

Weiner: How did your own research progress go, on your own grant?

Kutter: It made some definite progress. Partly, I did have these two technicians who were working part-time, each of them working half-time. We'd gotten a little bit done, even during the summer while things were being set up, and I'd presented a paper at the phage meetings at Cold Spring Harbor, a short paper. It was done under difficult circumstances.

BEGIN TAPE THREE, SIDE ONE

Weiner: This is the beginning of Tape Three with Betty Kutter and Charles Weiner. And you left off saying that the paper at the phage meeting was quite a feat because although the ultracentrifuges had arrived just a few weeks before, the scintillation counter had not. Right?

Kutter: So what I wound up doing was an experiment--Well, I needed to look at the sizes of certain DNA molecules to make a conclusion. It was continuing some work that I had started back in Virginia. To come to a finishing conclusion, I needed to look at the size of certain molecules. What I did was to run the experiments in the ultracentrifuge here. I had only time to do it once properly, plus one very "quicky" thing, and I took all of the scintillation vials up to the University of Washington. Actually, even getting the scintillation vials was somewhat a feat at that point. I won't go into it. [Laughs] I took them up and found time on a counter up at the University of Washington to count them. I had started making connections up there that helped enormously, and that's one of the few ways I was able to keep going. During the fall and winter of the first year when I'd been so intensively involved in the other program on sex roles, I hadn't tried to even go up there or talk with anyone even though I'd been there as an undergraduate and knew a couple of the people in the genetics department a little bit (no one doing anything related at all to phage.) But finally in the spring, I started to go up a little bit. And Sig was giving a seminar on his work in astrophysics up there. I just decided on the spur of the moment to go up with him for the day, and I wandered around

the genetics department, and said hello to someone I knew, and then was walking through the hall and saw a poster saying that half an hour from that time there was going to be a seminar by Bob Miller from Vancouver. And he happens to have been a person I knew fairly well from the phage meetings from quite a few years. I went in and went to the seminar, and before the seminar he saw me coming and was excited and we chatted. And it just turned out that what he does is somewhat similar to what I do; he was referring to my work several times [laughs] in the talk that he was giving. Then we started to talk afterwards and he introduced me to a lot of the other people in the department that I didn't know, including the people who work with phage.

They invited me at that point to come up and give a seminar later in the spring on the research that I had been doing back in Virginia before I came here. That, particularly, gave me a chance to show them the kinds of things I was interested in. Parts of it made a very pretty story, parts of the things I'd left hanging in mid-air nine months before. And they were extremely hospitable, took me out to lunch, took me out to dinner, and just generally were very supportive and very interested in what I was trying to do at Evergreen. And particularly, one of them, Ted Young, who also works with T_4, said that I would be welcome to come up and do a little work in his lab, learning one of the new techniques I needed to know, slab gel electrophoresis. And my technicians and I went up, then, in the early summer and did that. Often at the beginning when I was trying to get the lab off the ground and didn't have a stockroom to turn to, being at a completely new school, suddenly discovering that I didn't have any sodium

chloride or something very basic, periodically I'd wind up going up and borrowing some or getting a piece of advice or--

Weiner: Borrowing a cup of salt. [Laughter]

Kutter: Yes. Sodium chloride we did have, but there were things almost as trivial that we didn't--or just calling for a little bit of encouragement when I felt very alone and frustrated. Having someone who was willing to take some time and energy and seemed to be interested and care about what I was doing, I think made the difference between being able to get started and getting hopelessly lost at that point with all the things that were hanging over me.

Weiner: There was no one here who really could discuss the research at that level?

Kutter: No. There was one person who had worked with T_4 quite a while before but was much more interested at that point in educational methods and writing textbooks and things. We never really got into talking very much. Actually he's getting back now into being interested in research and has started just this last year working with me, but at that point he didn't seem to be even particularly interested in discussing the ideas so I really felt pretty much alone, pretty utterly alone.

Weiner: How many biologists were on the faculty at that time here?

Kutter: There are a lot now, something of the order of twenty. At that point there were probably eighteen out of a faculty of a hundred. It's

very lopsided in a sense on the side of biologists because it was found that among all the people in the sciences they were most flexible, adaptable at getting involved in these interdisciplinary kinds of programs. They were most likely to have had some kind of broadly interdisciplinary background. There were several marine biologists; a lot of them tended to be field biology, marine biology, ecology-oriented, but there was a biophysical chemist and someone who had worked in photosynthesis. A couple of the people in administrative positions had biological backgrounds also.

So generally, lack of biologists wasn't the problem. Particularly at that stage, in the school a lot of the problem was that everyone was so involved in getting a new educational enterprise off the ground and developing good ways of teaching that they were really having no time to sit down and think about or talk about the basic disciplines with other people in the same field. Our offices were set up in such a way that we were with the people we were teaching with. There were no departments at Evergreen, so there was no geographical localization to help us talk with each other. It's only now getting to the point where more people are starting to talk [about] their own disciplines with their colleagues. I was the only one at Evergreen who had a basic research grant. In fact, that's been true for the whole last three years. A pair of people have just gotten a large grant to do some work in photosynthesis, trying to use photosynthesis as a model for developing an energy source. There was a couple of years-period, particularly, and it generally has been true that there was a lot of pollution-oriented, ecology-oriented research going on which involved both students and faculty in an intensive program called "The Ecology and Chemistry of Pollution." But in terms of trying to

maintain an ongoing outside grant, trying to run a continuing lab and do basic research, I was simply alone. I got a good deal of support from certain people in the administration, emotional support, saying that they felt that that was absolutely essential to Evergreen, but at the same time, that didn't put any more hours into the day. There were only two of us for most of that time who were women in the sciences, and we were continually being asked to serve on various kinds of university committees. The first year I was on the President's Council and the representative to the Board of Trustees for awhile. That was before I was doing the research stuff. The third year I was chairman of Faculty Hiring for the whole campus. There were a lot of that kind of small things that I now have finally learned I'm going to have to say "no" to more often if I'm going to get research done and a good job of teaching done at the same time. But those things seemed to be very important, too, particularly in terms of strengthening the faculty, for example, with people who had commitments to both keeping up an excellence in their basic discipline as well as doing a good, solid really student-oriented job of teaching.

So where are we?

Weiner: Well, you've really set the stage for a number of things. Maybe we could just summarize the subsequent work on that research grant. I don't know whether it was completed, or whether you're still involved in it, or what.

Kutter: Okay. That first grant was a three-year grant from NIH. The three years were just completed a couple of weeks ago, May 1st. Like most research projects, it's the kind of thing that for every question you

answer, three more crop up. The work went slowly, particularly at the beginning, but solidly well. Two students out of the group that first year became rather solidly involved in the research and a couple of others did some peripheral, small, related projects. One of those students has been still working with me. Eventually, the work we were doing led to a quite long, substantial paper that came out this last December in the Journal of Molecular Biology. And there are a couple of other smaller papers that'll come out once I have a chance to write them [Laughs] But this tied together the basic ideas and presented a broad part of the field. Two of these students were very much involved in that work. Much of the writing was done in the summer a year and a half before it was published. Coming from John Wiberg's lab, there's a strong tradition of making sure a story is absolutely complete and doing a lot of polishing and checking small details, of a paper being something that you spend a fair bit of energy on, so I'd sent it around during the course of the fall to Wiberg and some of the other people I'd worked with. I rewrote it a couple of times in between, when I had a little bit of time, and finally submitted it in January. I got it back with some revisions to do which I wasn't able to get done until summer even though they weren't terribly large. Then I got it back [to them] in the summer, and it came out in December. I also later gave a second paper at the phage meetings. I felt generally fairly comfortable with what came out, particularly since it always takes a period of time to get a new lab off the ground. We're now in a position where we have several things really going strongly and hot, and a lot of excitement and enthusiasm. In the last year and a half I've had several

chances to give seminars on the work that we had been doing. I gave seminars at Michigan State, Harvard, MIT, La Jolla and Stanford in the last year and a half. (This, perhaps, should come out more later, but it's kind of ironic. It's turning out now that the one system with which one can do recombinant DNA work with T_4 was the system I've been developing for completely different reasons.) [Laughs]

Weiner: Yes. You're closer to it than you thought.

Kutter: [Laughs] Much closer to it than I thought, which is sort of frightening. That suddenly raised a good deal more interest in what I'd been doing.

Weiner: Yes. Are there any grant proposals pending?

Kutter: At first I had seriously considered not trying to renew it because I'd gotten to a point where I really needed a little bit of time off, a summer or two. There were too many things going on at once. At the same time, there's something highly addictive about doing research and about the response that comes from doing research well, in the sense of being part of the crowd. I enjoy going to meetings and talking science, and doing at least enough to keep my membership card good, and being able to learn the newest developments sort of "hot off the presses." But I was debating back and forth.

One thing was that my husband and I were having a lot of problems which were clearly not made easier by the fact that I was trying to do, in effect, three jobs at once. And in a lot of ways I was having an easier

time at Evergreen in terms of adapting more easily to the structure here, and he was being disturbed by a sense of my professionalism. I was questioning which had the higher priority. But finally a decision was made, largely by him, that we had to separate. Once that decision was made for me, I went ahead and with the other person here applied for competitive renewal of the NIH and also applied at the same time for an NSF grant. One problem that we ran into, being from a school like Evergreen that doesn't have an office set up to sort of keep track of all kinds of dates and deadlines and all that sort of thing, was that--well, I was back at the phage meetings when I was told that my grant as a competitive renewal was supposed to be in a month before the regular grants. I had no idea about that deadline. I hadn't gotten any sort of letter telling me that this was true, or what was happening. It's published in the NIH list of grant deadlines, and so forth. But I knew when the regular deadline was--I'd seen it listed in several places and just didn't realize that the competitive renewals were due earlier than that. I just happened to find it out by talking to someone who also had a competitive renewal due, which meant that the time the grant was due was approximately three days after I got back from the phage meetings. Well, we had been doing work and a lot of running around with recombinant DNA all during the summer, and hadn't gotten very much done towards the renewal, so I got back and arranged that it would be a week late. Burt and I put in a crash ten days and put together a grant proposal which in places need a little bit of polishing but the basic ideas were very good. We included some of the more peripheral things we'd like to do, and so forth. What resulted was

that it was approved but not funded from NIH, although they spoke highly of parts of it, large parts of it. But National Science Foundation did both approve and fund it.

Weiner: I don't understand that mechanism. You mentioned it before, as well, at Virginia. What does "approved and not funded" mean?

Kutter: Well, there are two steps in the process of getting a grant. The first step is that it goes to a study section and it's given to two of the people on the study section to go into in detail. My study section is virology, which means that there are rather few other phage papers, usually. But most of the papers have to do with tumor viruses, and other things like that. Then, as a result of the discussion at the panel meeting, everyone votes a certain number for it. The numbers go from one to four, one being highest priority, and four being "Don't fund." There's an average made of all of the numbers that people give it and this comes to some kind of a composite score. You're not told what that composite score is, ever. I heard that when my grant was first funded, during that period anyone with a composite score of 2.5 or better had a good chance of being funded. Now I think you have to have at least something like a 1.7, at least in NIAID in terms of the current situation, although that varies from institute to institute at NIH, which makes a good deal of disparities in terms of what actually gets funded. It also varies from session to session in terms of exactly what you need to get funded, depending on the number of grants and what's there. So "approved but not funded" means that if now the thing were 1.7, that would mean that if I got a 2.0 or

2.1, or something like that, it would have been approved as something that ought to be funded if there's enough money, but it's not high enough on the priority list to be funded.

Then NSF did approve it and funded it at about a little less than half of what I'd asked for, for two years, rather than longer. Now one thing I didn't know before was that even though I have the grant from NSF, I could have also accepted one from NIH and could have used it to make up the difference between what NSF is willing to pay and what NIH thought ought to be budgeted, which was less than what I first asked for but quite a bit more than the NSF [grant.] And what was suggested to me by the program director at NIH, whom I talked with when I was back there for a meeting, was that I ask for a three-month extension without funds, without additional funds, of my current grant, which means that rather than expiring May 1st it expires August 1st, and then put in a revised grant proposal. It would still be counted as a competitive renewal and I could take advantage of the comments that the people made and take a little bit more time putting it together. Of course, where the "more time" is coming from is a problem, but I probably will put in that revised application.

It's been interesting; it's been a combination of several times misunderstandings like that happening. One time when I wanted to carry funds over from one year to the next--I'd done a lot of things to save money because of cutbacks that were coming--in order to be able to carry it over I had simply put it into my non-competitive renewal that I wanted to do that, and got it. And the way it was listed, I questioned

whether they were letting me carry that over, and I called. They said, "Yes, everything's okay." But they kept not budgeting it to us in terms of the paper work, and finally six months later, I called again and it turned out that I hadn't filed a particular letter that I had to file. So they kept trying to tell me there was nothing they could do, the money was lost, (which was several thousand dollars--in fact, about seven thousand dollars total.) I did a lot of talking and calling people, you know, trying to explain the situation here and so forth. The people that were down there kept saying, "There's nothing you can do." In fact, I finally went so far as to say, "Well, I'll talk to Stetten and see if he can do anything to help me and pull the strings of being on the committee," which I hated doing. [Laughs] I thought afterwards, "That was stupid," after I'd talked to this one woman and said that. But the next day I got a call from the person who was in charge of the budget thing, who discussed the problem with me, saying, "In this case, since it seems to be a particular circumstance of misunderstanding, and so forth, write a letter explaining what the situation is and we'll see what we can do about it, with the understanding that that's totally un-precedent-setting and will never happen again." I wrote the letter, and they took care of it.

Weiner: Well, part of that problem, you imply, is due to your own lack of experience with grantsmanship and with the institutional lack of experience and lack of atmosphere for that in terms of colleagues, and also the institution itself.

Kutter: Yes.

Weiner: So that's one of the consequences of being relatively isolated.

Kutter: Yes. There's nobody here who automatically knows the steps that have to be done and the reports that have to be filed, and all that.

Weiner: That's an interesting episode as a commentary on the grant system.

Kutter: That's why I thought it was perhaps worth going into in a little bit of detail. But, just to finish that, I found both those problems with the system and with being outside of it. But at the same time, I've often found a good deal of support of what's clearly an unconventional setup and a certain degree of tolerance and ability in the long run to take into consideration, to be somewhat flexible, to be intrigued and interested at the idea of someone trying to do research at a small institution, and particularly in the beginning when I was transferring the grant here and really questioned whether I'd be able to do it, a great deal of support and cooperation, even though it involved completely restructuring the budget.

Weiner: That also has some advantage for NSF and/or NIH, which are often criticized for not distributing funds widely enough geographically and at the small institutions, which will come up again when we talk about your appointment to the NIH Committee.

Kutter: But it was also a statement in favor of the fact that there's no inherent bias against that kind of thing. When I initially applied for the grant and was only working part-time as a research associate at Virginia and had no definite position, I was granted the grant by both NSF

and NIH, so it appeared very clear that they were judging it on what they felt to be the merits of the proposal and not in terms of the stability of my position, or a name I might have, or a long list of publications. I mean, after these years I have only five publications. They're good ones, I think, but I don't have a long list of publications.

Weiner: Five years after, but longer after your PhD.

Kutter: Yes. How long is it? Eight years. [Laughs]

Weiner: Yes. Well, you've had other products.

Kutter: [Laughs] You're referring to two kids there.

Weiner: [Laughter] Yes.

Kutter: That happened earlier.

Weiner: I think it's time now that we've brought it up to date, to talk about the recombinant DNA.

Kutter: The one thing that I haven't talked about at all is how trying to do research fit in with my teaching responsibilities, and what I was doing over the last couple of years, which is a different kind of thing.

Weiner: Yes, that's right. You only talked about your initial hopes to make the two mesh.

Kutter: Yes.

Weiner: Well, what about it?

Kutter: One thing at Evergreen is it's clear you can't do the same thing year after year. You're normally doing a different thing every year. The second year, then, of my grant, I was still teaching in the sciences but in something which could not in any way be as directly related to what I was doing. That's a program called "Foundations of Natural Science." What the program is, is a combination of physics, through classical mechanics, electricity and magnetism; and chemistry, through thermodynamics and organic chemistry; cellular and molecular biology; neurophysiology; and calculus. It's a full-time program and there are four of us teaching it. The only lectures I do generally are the biology-related lectures. But in problem sessions I deal with the problems in all of those areas. It's designed so that I'm continually tying the biology together with the other sciences. For instance, they'll talk about capacitors in physics and the next lecture will be about the cell membrane as a capacitor and the way that's used to store the energy for nerve impulse, and the problems you'd run into if you tried to use simply core conduction as a method of transmitting sensory input, the reason you need this continual capacitor action, and a lot of related kinds of things. We were doing spectroscopy and organic chemistry, talking about molecules and the relationship to spectra, and doing optics and wave-particle duality in physics. I gave a lecture on biological transduction of energy, talking first about photosynthesis, and then going into vertebrate vision and tying them together with these other ideas.

So the teaching requires doing a lot of reading which is not related to my research, a lot of tying ideas together, which is one of the reasons

I wanted to come to Evergreen. I really enjoy keeping broadly in touch with what's going on in the world of biology. It also means that I have to be prepared to do problem sets in other things. In fact, I was helping to teach the calculus, winter quarter, and helping to conduct problem sessions in electricity and magnetism, and organic, and so forth, which has meant often for extended periods of time that I really couldn't keep in too close touch with the lab. Luckily, I had one student from that first year, as I said, who's worked clear up 'til now. He graduated a year ago, but his girlfriend was just finishing up and he stayed around and worked half-time. The first year I taught in this Foundation program there was no biochemistry program and the two of us together in the Foundations program--the biophysical chemist and I, together--took on a group of half a dozen very advanced students who worked substantially on their own, but [who] we helped. One of those students has gone on to work closely with me in the lab.

But it meant that really my technicians and these few students were quite extensively on their own. I'd take a couple of hours once a week and sit down and go fairly carefully with them through what they'd been doing, and drop into the lab now and then. But except during vacations, I could do very little work myself. Foundations is regarded as an extremely demanding program even for those people who are doing only that, students and faculty alike. And that's what I've been doing for the last two years. Now next year I'll be doing molecular biology again and then the year after I'll be going back to doing a basic program, something not quite so distant as Male-Female Roles in Society, but something

like Life and Health, or something of that sort. I think that timing is really important. It was possible to do that because I had a smoothly-flowing lab, a group of well-defined problems, technicians who could work relatively independently, and it was possible to keep going, and keep going in a productive sense, keep my finger enough in the pie to keep interested. The only time that that really broke down was last fall, the recombinant DNA thing, as we'll get into later, I'm sure.

Weiner: What about an estimate of how much time you spend in an average week--I know that's hard to define, and average week--but the amount of hours at Evergreen involved with teaching and the amount involved with research.

Kutter: Yes. That's really hard. We have sort of, once a quarter, something where we're supposed to take one week and try to tie together what we've done, and so forth. I would say that actually during the quarter, time directly involved with my lab is no more than, maybe, four hours a week. I spend an additional at least half-a-dozen hours a week reading scientific literature and things at home, sort of related stuff. It's hard to draw the line between what's related to my lab and what's related to my teaching. My teaching takes probably fifty or sixty hours a week.

Weiner: How much of that time is contact with students?

Kutter: That varies. In the fall and winter quarter parts of this Foundations program we have to go to each other's lectures in order to be able to tie things together. We have labs to run, plus seminars and problem

sessions, and a lot of individual conferences with students, and probably if you count all of that time together it's almost all day, four days a week. It's twenty-five or thirty hours; it's a lot.

Weiner: Well this will all be interesting background when you talk about the completely new activity that was superimposed on top of this, your work on the Recombinant DNA Committee. Want to take a break before we start?

Kutter: Yes. --[Interview interrupted]--

Weiner: Well, now we're at the point where we should bring in the issues of recombinant DNA, and the first thing, I guess, is what your recollections are of your first awareness of the relevant research.

Kutter: I remember sort of a vague hearing about what was going on. Actually, the first thing related to that that I was familiar with was while I was back in Virginia when people were first trying to show what kinds of ends the restriction nuclease had. And Rolf Benzinger was predicting that it would have some kind of a palindromic sequence at the place that it recognized and trying to work out a way to do it. And so I remember very well when the first paper came in that showed that you actually had this kind of a palindromic sequence that made staggered ends. I really was relatively unaware of what was going on, with moving out here and just getting started and everything. But the first thing I really remember paying strong attention to was the Berg letter to Nature and Science, which I proceeded to cut out and hang outside my door, and mention to students a bit. It struck an emotional chord because of the sort of

mixed feelings I'd been having in working with enzymes that cut DNA up into gene-sized pieces. I was intrigued by it.

I don't remember seeing anything more or hearing anything more about it until the following year. I went to a meeting on DNA replication at Montebello, which was just outside Montreal, in March of 1975. That meeting happened just a couple of weeks after Asilomar. I hadn't heard anything about the Asilomar Conference either before or at the time, but one of the people there at that meeting at Montebello was Waclaw Szybalski, who's been a good friend of mine since the first time I went to the phage meetings at Cold Spring Harbor and has always been very supportive in terms of research ideas. A couple of times I sent him things that needed to be analyzed for something, so we'd done a little bit of collaboration of that sort, research-wise, and had been good friends. Charlie Thomas was also there at the meeting at Montebello, and both of them had come almost directly from Asilomar and were still very much excited about what happened. We got into long discussions about Asilomar and the questions it raised, and the problems. This was the first time I'd heard in any kind of detail what was going on, or the specific concerns, the environmental concerns. I hadn't really thought particularly about those things. One of the things that Charlie and Waclaw were talking about was the pressure at Asilomar to put non-scientists on the committee. They felt very uncomfortable with that. They had the feeling these people couldn't possibly really contribute anything because this was mainly a technical question at this point and a non-scientist wouldn't have the expertise.

Then one evening they got to joking, or half-joking around that what

they should at least do is have somebody from outside the main science establishment, and perhaps have somebody from a small college. I think at that point they were sort of playing to me, in a way.

Well, at any rate, it wound up with Waclaw saying to Charlie, "Well, what about nominating Betty? She seems to have a lot of the qualifications we'd be looking for. We need another woman on the committee, and she's from a small college with a lot of emphasis on the social responsibility of scientists, and trying to get humanistic values along with science. She knows her science well." Charlie agreed that that might be a good idea, and Waclaw asked me if I'd mind if he nominated me. I think what he said, was that each member of the committee had been asked to nominate someone from outside, to bring up a name. I certainly didn't expect anything to come of it, but my reaction was, "Yes, sure. Why not?" Not so much a sense of a strong, burning desire to do it--I didn't know enough about it really at that point--but more a sense that I talked a lot about scientific responsibility and here was a potential chance to do something. I couldn't very well say no. It also might be really interesting; I might learn a lot. I had no particular position on one side or another, in terms of what had happened at Asilomar. I had a feeling that they were basically behaving very responsibly. I certainly didn't have any strong emotional commitment in any particular direction. So Waclaw wrote a letter to Stetten, the chairman of the committee, basically saying those things, that he thought I would be a good addition, and he mentioned both the small college and the emphasis on more responsibility of scientists, and also the scientific background and having worked with phage. He sent me a copy

of the letter. Actually, it surprised me a little that he'd even gone ahead and done it; I thought it was one of those suggestions made in the evening at the bar that nothing would ever come of. [Laughs] I still didn't expect anything further, and then awhile later Szybalski got a letter back from Stetten that he sent a copy of to me, saying that they wouldn't actually expand the committee until some sort of official action [was taken], and that would take a certain amount of time, but suggesting that I be invited as a consultant to the next meeting with the idea that I'd be put on the committee once it was opened up. So I did get an invitation to go to the meeting in Washington, D.C. in May. I wasn't quite sure what I was supposed to be; I mean, being a consultant seemed in a way a stranger position than being asked to be a member of the committee, because a consultant implies some sort of expert knowledge and that was not something that I really felt I had at that point. So I did a fair bit of reading then, before I went to the May meeting.

Weiner: Let me stop you at that point and ask first of all, had you seen the letter that Maxine Singer and Dieter Söll published in Science in '73?

Kutter: No.

Weiner: You hadn't heard any discussion of the issue either, in terms of concern being expressed?

Kutter: No. Nor in terms of possibilities or potentialities. I'd been completely out of that entire circle.

Weiner: So, of the people who were the signers of the letter, did you know

any of them personally? Had you ever met any of them?

Kutter: I'm trying to remember who--

Weiner: Berg, Baltimore, Roblin, Zinder, Watson, Stan Cohen--his name was added later--Herbert Boyer.

Kutter: Watson was the only one that I knew at all; I knew him from Cold Spring Harbor.

Weiner: So you hadn't had contact in your normal research or any other way professionally with them?

Kutter: No, not with any of them. I don't remember ever even having heard any of them speak. And I hadn't even heard of about half of them. Some of the names were vaguely familiar.

Weiner: And you didn't know that the Asilomar Conference was being held.

Kutter: No.

Weiner: There was an article in Science Magazine that followed up the Berg letter which you may recall.

Kutter: I seem to remember vaguely. Yes, I do remember vaguely that there was some talk about something, but I hadn't looked at it carefully.

Weiner: Right. And by the time you went to the meeting in Montebello, you hadn't seen any newspaper accounts of Asilomar?

Kutter: No, I hadn't. I don't think there was anything in the local

paper here about it. And when I first got there, I started hearing people talking about "Asilomar." You know, the word was sort of floating around. I really didn't know what they were talking about until I talked with Szybalski. I must seem very naive. [Laughs]

Weiner: Well, was there anything on the program or any discussions at the meeting where it came into open discussion?

Kutter: No. It was on replication of DNA, and there were a series of longer papers. People like Okazaki, and so on, but replication isn't something directly associated with that and I only remember it from the informal discussions that I had there with Szybalski. I didn't at that point really have any sense of the details of how they were doing it but I knew enough about the sites of the restriction enzymes that as soon as they said, "Well, you can just take them and cut them like this, and then cut other DNA's like this, and they'll go back together in ligase, I said, "Of course, why didn't I think of that?" [Laughs] I mean, it was a very obvious, obvious thing once it was mentioned in terms of the original letter.

BEGIN TAPE THREE, SIDE TWO

Kutter: I was more concerned with the Berg letter as an example of scientists taking responsible social action than I was with the specific issue that was involved.

Weiner: Yes, I see.

Kutter: It was more a model for a way of doing things.

Weiner: Do you recall the date of that Montreal meeting?

Kutter: It was about March 9th. It had been very much a period of turmoil in my own marriage, which I'm sure was one reason why I hadn't paid more attention. We had, starting in the spring before, off and on talked seriously about separating. In that fall my husband had bought a piece of land to build on, and the decision had gone back and forth several times as to whether to separate or not. And, in fact, if it hadn't been that we had decided to separate when I went to the meeting at Montebello, I would have simply said I couldn't possibly do it when Waclaw asked me if I'd be interested because that would have been something my husband very definitely would have considered as one thing too many, by far. So I really wouldn't have seriously considered it otherwise. Things were still going back and forth, and I still had mixed feelings as to whether it made sense to do it, but at the same time it seemed to come as a chance to break free from certain kinds of roles that I had put myself into--you know, certain kinds of being able to dabble at science, but not really take it seriously in a professional way. And so in that sense, it gave me a new window to the world.

Weiner: The letter that Szybalski wrote was March 17th to Stetten, and Stetten's reply was dated April 2nd. Did you say that Szybalski sent you a copy of the reply?

Kutter: Yes, Szybalski sent me a copy, both of the letter and then of the reply. I never actually got a copy of the reply directly.

Weiner: I see. Well, I don't think I found a letter appointing you consultant.

Kutter: I'm not sure whether I have that in there, either.

Weiner: Yes. I don't think I've found it.

Kutter: It didn't say very much. It just asked me to come as a consultant without saying anything about why.

Weiner: That was soon after the meeting, sometime in April.

Kutter: Yes. The [NIH Committee] meeting was in May. Yes, it came a week or two before. They called me, actually. The first thing that happened was that they called and said that Szybalski had nominated me and asked me if I'd be willing to come as a consultant so it was mainly done with a phone call, and the letter that came I probably didn't even keep. It was just a very short thing that simply requested my presence and said, you know, "Reservations have been made for you, so-and-so . . ."

Weiner: When you got back from the Montreal meeting was there any further discussion about the Asilomar issue that you got into, either here or with colleagues elsewhere, prior to the time of the May meeting?

Kutter: Not with colleagues elsewhere; and here, really not much either. There was very little. I talked some with a few of my students. I was doing developmental biology that quarter, and I talked with them about this new technique and some of the concerns about it. There was, as I remember, something that came out in Science, talking about the Asilomar meeting,

which I read. I got hold of two or three of the early papers related to the uses of the technique. Then there was a packet of things that were sent to me from NIH that got here just before.

But again, it was still a very difficult time, as I say. That was a period of time when the decision in terms of my marriage was going very much up and down continually. We went away for a few days to try to decide whether to stay together or separate. And the other thing that happened was that I had been asked right at that same time to serve on the Long-range Curriculum Planning Committee for the college here. We spent three days at a place over on the ocean going through the whole Long-Range Curriculum thing, so I was pouring a lot of energy into ideas for that, plans for that, and so forth. I didn't do very much more at all about the Asilomar thing until I got the reply to Szybalski. Up until then I just thought, "Well, gee. That was nice of him. That was a sweet gesture," that sort of thing. [Laughs] I had to leave for the meeting in Washington about six days after I got back from the Long-Range Curriculum Planning thing, so it was a very hectic time.

Weiner: When you read the materials, the type you mentioned, the early papers, the Science article, and maybe you saw by that time some other newspaper accounts of it--

Kutter: No, I never did see any newspaper accounts until much later. Our local newspapers around here really haven't done very much at all, as far as I know.

Weiner: You mean Seattle papers?

Kutter: Seattle papers, yes.

Weiner: When you did look into some of the materials, including the materials they sent, did that help you develop a viewpoint on the issues?

Kutter: Not particularly, and I felt slightly overwhelmed with the amount of material and went with the feeling that, particularly at this first meeting, I would mainly listen and be very open to all of the different sides and perhaps out of that would come a viewpoint. I went with the intention of not talking at all. I didn't live up to that. Periodically, something would come up where I did know something, or where something seemed to be missing, where I'd make a comment. I think that more than anything at that first meeting I began to sort of size up the people I was working with; the only one I had met before was Szybalski.

Weiner: Charlie Thomas, as well.

Kutter: Oh, and Charlie Thomas--sorry, yes. I'm trying to think whether there were any of the others. No. I'd read a lot of Jane Setlow's papers, but I hadn't met her before. Most of the others I didn't even know by reputation, which sounds naive. [Laughs] But most of them I hadn't heard of. Since then, I realize a lot of the things that they've done.

Very frankly, when I first got there I felt rather a bit like a five-year old at a grown-up's tea party. [Laughs] That really wasn't exactly helped by my initial reception, which I maybe shouldn't say anything about because it might embarrass somebody. I won't mention

any names. Well, I got there the evening before the meeting and went out to dinner with Waclaw and Roy Curtiss (I met him then), and a couple of other people, then went in the next morning for the meeting. A few minutes before it started a woman came over--sort of bustled over--and asked me to sign my name in the guestbook. And I did. She watched over my shoulder and then asked me to take a seat. I walked over to the table. The chairs were arranged so that there was a central table with the chairs of the committee members around it, and then a lot of other chairs around the sides for people who were there to observe. I went over to the table and started to sit down at my place which was at the end right at the beginning as I came to the foot of the table, where it belonged, of course. And she came bustling over, and said, "No, no, no, not at the table, not at the table!" I looked at her and said, "But I'm Dr. Kutter; this is my place." [Laughs] And she was most embarrassed, sort of flustered; she and I got to be very good friends after that. That sort of tended to reinforce the slight sense of unreality and "What on earth am I doing here with all of these people who know a great deal and have been involved for a long time? It must be some mistake." [Laughs]

Weiner: As the committee went into its deliberations for the day, did that first impression change, that feeling of your awkwardness in the situation?

Kutter: Yes. I have a tendency in any situation I get into, to very soon sort of throw myself in and become immersed in the situation and to sort of lose track of myself and to lose track of inhibitions about saying something; in fact, sometimes to say things that, if I'd stopped to think,

I would not have said. I remember when I was a graduate student, after a lecture of Arthur Kornberg's, going up to him and getting into a ten-minute long argument about him, that his really wasn't the DNA polymerase and that it shouldn't be from a bunch of properties it had. I thought he was mistaken, that it was some kind of repair enzyme, or something. And of course, in retrospect I turned out to be right; but at the time I felt utterly embarrassed once I realized after the conversation had ended the audacity of what I had done. [Laughs] That's not directly relevant to what's going on here, but it sort of expresses my general psychological make-up, I think.

Yes, I lost that sense of being totally out of it. I still felt very much that most things I had no business particularly participating in, and I had that same feeling that I had with Kornberg at times, of getting involved in listening, speaking up in terms of an opinion, and then feeling slightly embarrassed after I did it. But I didn't do a lot of talking at that meeting. Charlie Thomas wasn't there the first day of the meeting; he came only for the second day of that meeting. It was interesting, for example, for me to watch how the group dynamics changed after he got there from what they had been like before. I was very impressed right from the beginning with the very responsible and articulate role played by Roy Curtiss; he was probably the person more than anyone who impressed me at that first meeting. And there were a number of the people who seemed to be very quiet; it seemed that most of the talking was done by, perhaps, half a dozen people at the most. There wasn't much voting at that meeting; it was more setting up procedures. But still at some that were taken, it was hard to see quite on what basis they were making decisions. They

seemed to be simply following some of the things that were happening, but there seemed to be a group of people who were very actively involved and then a group of people who just sort of listened.

Weiner: You mentioned that the dynamics changed when Charlie Thomas came in. In what way?

Kutter: It's hard to put in words now that don't reflect what I saw of the dynamics between him and the rest of the committee at other meetings, but he seemed to have a very, very strong, very loud, very dogmatic way often of saying things. When he finished speaking it seemed to cut off all further discussion. A swaying kind of impression, whereas before [he came in] I had gotten more a feeling of a very well-balanced kind of discussion that would start and then go through a period of discussing back and forth. It was fairly clear that on most issues Roy Curtiss and Don Helinski and Stan Falkow were the more conservative members of the group, or at least the ones who seemed to be more aware of the potential hazards; then Dave Hogness and Jane Setlow and Charlie Thomas, when he came in, were among those who really felt that there was no danger. Szybalski was hard to put in any particular camp. The discussions he had on different topics I couldn't rectify in terms of any particular ideological bias or strong sense.

Actually, I'd met Stan Falkow also the day before the meeting. Oh, that is one thing I left out that I should have mentioned. Once, when I was up visiting at the University after I'd been nominated for the committee, I noticed that a lecture was going to be given in their Science and Medicine Series by Stan Falkow. I made a special trip up to hear it, to sort of

get a feeling for what I was getting into. He was talking about cloning, and also particularly about things like antibiotics in [animal] feed and the very serious problem of antibiotic resistance, and so forth. It was clear that that was the kind of problem that he was much more basically concerned with. I thought of going by and introducing myself, but felt a bit too shy. But then when I got to the airport I noticed him there, waiting for the same plane, and went over and introduced myself. We wound up talking for a couple of hours on the plane--rather, I wound up listening for a couple of hours on the plane, largely. I realized what an incredibly humane person [he was], and what an incredible store of knowledge and common sense he had. I was really extremely impressed with the sense of direct, right-outness. There were a lot of things that he didn't feel were particularly dangerous, but I got very much a feeling that he was someone I could trust.

I got that feeling also very much from Roy Curtiss. I have to say that I also, later, got it from Dave Hogness, in general. I felt that he very honestly did not believe these things were dangerous, very honestly felt that going ahead with the research in a lot of areas was something that was very important, and that he was trying very hard to be really fair and honest, and bending over backwards to [do so]. There were times when he got backed into a corner that stretched his ability to deal with that, when he felt he was being pushed too far, when emotionalism would take over, but I was very impressed in general--particularly with those three and Don Helinski, and with a lot of members of the committee in terms of the basic sense I had of their commitment to trying to be fair.

There were times, more so with Dave than with the others I've mentioned, where the inherent biases of their research and their interests inevitably came through and made them blind in certain ways, I felt, and some of that came through at that first meeting. By the end of the meeting I felt pretty comfortably a part of the group. One interesting thing, in terms of the dynamics, was that at that first meeting everybody who had a seat at the table voted. A vote would be taken, and I voted with them. That seemed the natural thing to do, and no one said I shouldn't. There were a couple of other people there who were not formal members of the committee. Herman Lewis was voting; the second day, Wally Rowe was brought in, who they were planning on putting on the committee later and who hadn't decided one way or another. There wasn't the same focus that came by the time of La Jolla, who's a voting member, and that kind of thing. It seemed more that the voting was a way of reaching consensus among the group of people who were there, trying to get a very important job done. There wasn't a combatative kind of thing. There was a good feeling to it. It was something that I felt very comfortable being part of.

Weiner: What did you feel was accomplished in the first meeting? Was it a two-day meeting?

Kutter: It was a two-day meeting.

Weiner: It was the 12th and 13th of May.

Kutter: We talked about the kinds of things that ought to be in the guidelines. I'd have to go back to my notes to see exactly, but--

Weiner: Well, we have the minutes. So I was just wondering about your recollections.

Kutter: Yes. In setting up a subcommittee to draft the guidelines, the subcommittee was simply appointed by Stetten. He simply sort of looked around the room and asked Helinski and Hogness, Szybalski and Chu to do it. That seemed to make sense. I was quite surprised by his suggesting that Dave Hogness should be chairman of that subcommittee, because it seemed to me that it was sort of at least a tactical, if not philosophical mistake to ask the person who was most directly involved in it to be chairman of the subcommittee to write up the guidelines. It made sense to have him on it, but it would have made much more sense to me, for example, to have asked Ernie Chu to be chairman, who was totally outside and had no involvement. That was one of those cases where I opened my mouth and closed it before I said something [laughs] instead of after. I felt that that was interfering. He seemed to do it just sort of arbitrarily. I mean, I don't think he was thinking in terms of that being any particularly important position. He just sort of went around, "Well, let's see. Hogness, and so forth, and so forth, and so forth--and Dave, why don't you be chairman." Well, actually, he didn't say, "Dave." That was one very interesting thing about the dynamics of the meeting in terms of the scientific thing. The scientists called each other by their first names, but the members of NIH, Hans Stetten, and so forth, called everybody on the committee, "Dr. Hogness," "Dr. Kutter," "Dr. Such-and-such," in a very formal way. And the other way around, too; we called him "Dr. Stetten." There was this formal distance there which seemed very strange to me, after coming from Evergreen. The

only people here who ever call me Dr. Kutter are the people who want to sell me something, and I wondered if there was any correlation. [Laughter]

But, what else happened at that meeting . . . We talked about [the fact] that one of the biggest problems was not that people wanted to do bad things, but that a lot of people didn't know good basic medical microbiological technique, and so forth. We talked about the importance of setting up courses to teach them to do those things. We talked about the committee's role in trying to get development of a safer vehicle, and about sending out requests for contract proposals to do that. We talked about setting up what now is the Nucleic Acid Research Scientific Memoranda. Actually, what we were talking about was the importance for some way for people in the field to very quickly know about accidents or about new technical advances that would make it safer, or, in general, about safety considerations. And there was a fellow there from NIAID, Earl Chamberlayne. He talked about their idea for putting out a scientific memoranda. We talked about expanding the scope in various ways and about how to start it out initially, that it should be sent to the list of all the people who'd been at Asilomar. We talked about getting a broader scientific discussion in general. That was when it was decided that Don Helinski would organize a meeting at La Jolla sometime the following fall. Actually, we'd said just before Christmas, probably, that we would have a meeting of the committee there at that time. So the next meeting was to be at Woods Hole. Then there was to be a meeting in conjunction with this session at La Jolla.

Weiner: With the workshop.

Kutter: With the workshop.

Weiner: Originally, that was scheduled earlier in the fall.

Kutter: No, that was scheduled all along for December. We also put in the possibility of a meeting in September, if it looked like we needed it. And that was a provisional thing. One of the problems with it, was that there was no time in that period when everybody could come, so it was very clear that whatever we set up there'd be several people missing. It wasn't clear that we had things that we would have to deal with at that point, but in terms of the regulations that meetings had to be announced two months in advance, and so forth, we decided to set up a tentative time.

Weiner: Was it clear to you at the time of the first meeting that you would be attending all of the other meetings and that you were essentially a member of the committee, although it couldn't be made official just yet?

Kutter: Well, I wasn't quite sure. I mean, nobody specifically expressed that. It was clear that I would be at the next meeting, at least. Well, nothing was expressed particularly one way or the other, actually. I wasn't quite sure how to handle it in that sense. One thing that we did discuss, by the way, in terms of a broader discussion of safer vehicles and vectors and guidelines, was I mentioned that I was going to be going to the phage meetings in August, and asked if they'd like to to try to organize a discussion of cloning vehicles and guidelines as part of one of the sessions there. Everybody seemed to think that was a good idea. The session that eventually happened at the phage meetings was already,

in effect, set at that time.

Weiner: But, they just authorized you to go ahead and do it if you wanted to. They didn't delegate you to go and do it for the committee.

Kutter: Yes. No, I think they may have voted on it but things were very informal in the way they were done then. It was taken in the additude, "It's a good idea, do it." The whole process was still more a "buddy system" kind of thing, a much less super-formalized process, I think. So the last thing we did was to go over and be given a tour of Building 41, which is the facility that's used for P4 containment there. They gave us a thorough tour of the whole facility and how it functions, and the problems with it, which was very interesting. At one point I then said to Bill Gartland, "You know, I'm slightly confused with what my status is now." He said that they would like to invite me again, at least as a consultant--implying that nothing had happened yet with the official thing of expanding it, but the way he talked indicating that my name would be submitted as it was expanded--but that they could invite me to, at most, one more meeting as a consultant. And I sort of wondered whether it was better to go to the Woods Hole meeting or perhaps wait and go to La Jolla, not knowing exactly what would be happening. But I decided to go ahead and go to the Woods Hole meeting.

Weiner: Well, then you decided when you got back, you mean?

Kutter: Yes, partly after. I guess I pretty well was thinking in terms of going to that. Thinking about it then, I decided that since the draft was going to be drawn up of the guidelines and we were going to be voting

on them at Woods Hole, that I ought to go to that. I was a little uncertain in terms of my long-range relationship, feeling a little bit like an intruder and not liking to feel like an intruder in terms of having to ask, "Well, am I on? What's my status?"--that kind of question. I found it very difficult to ask.

Before I went to that meeting, we had made a tentative decision at home that we wouldn't separate for the time being, shortly before I left. There had been something raised again by Sig, and I'd gotten feeling comfortable with what was happening there and spent a few days straightening some things out in Charlottesville while I was back East telling all of Sig's colleagues that we weren't separating after all. When I got home, Sig told me the next day that he had decided he had to move out. There was no discussion; things had to be dealt with completely, absolutely right away; and that he would be building that summer and the financial things had to be done immediately. So I had a week of work to catch up plus dealing with this and a lot of other things, and I pretty much went to pieces. It's hard to say anything very logical about what happened in my own thought patterns over the next month or so; very frankly, I'm not sure if that's something that I'll leave in. [Laughs] But the two things are sort of tied together. So he was going to be in the house and building his house, to be still living with me and building his house over the summer, [and] it seemed to make sense to try, at least, when I was going to be gone to be gone for awhile at a time and not try to continually be interacting. I talked then to Jon King, who I knew at MIT.

Weiner: How did you know him?

Kutter: He also works with T4 phage. A fellow who'd been a classmate of his was at Charlottesville and he came once to visit Bob Huthke and I met him then, and talked with him some. So I only knew him slightly, but I'd liked him. Anyhow, I mentioned I needed some information from him about a technique that I was using. I called and got the information from him, and mentioned that I was going to be in the Boston area and that I was on this recombinant DNA committee, or sort of on it, and asked if I could drop by while I was there. Actually, what I said was I was going to be in the area for the meeting of that committee, and asked him if I could drop by and pick up some information from him in the lab and talk to him a bit. I had no idea at that point of his involvement with Science for the People. He said that there was a genetics group who were very concerned with the recombinant DNA problem and they would like to know more about what was going on at the committee. He said it in a very neutral way, and asked if I would be willing to come a day or two early and have a meeting with the Science for People people and tell them about what was going on, and my views of what was happening on the committee, and, also, give a seminar on my research at MIT. And I said, "Sure," that I would like to do that. I actually wound up going to two meetings of the Science for the People group. The first one was just the genetics group, and they were discussing a lot of other issues. I wasn't talking mainly; I was listening, and getting a feeling for what the group was really doing. I'd heard about them a few years before and gotten some sort of sample subscription to their publication [that I had] not had much time to read. I found it somewhat more Marxist-oriented than my tastes were. They were

talking about some of the problems they were having in getting really balanced scientific things into the Science for the People Journal, and they were talking about a lot of different science-related issues, partly about the XYY chromosome business.

As I say, at that meeting I mostly listened and got a feeling for the people. Then some of those same people, plus quite a number of others, came to Jon's house the next evening for a big meeting about the recombinant DNA thing. I just went through everything I knew in terms of what was happening and where things were, and that was when they started really expressing some of their concerns. "Well, what about insects? You know so many diseases are carried by insect viruses; there's Drosophila." Somebody mentioned something that perhaps there was some evidence that something that Drosophila carried might under some strange circumstances be pathogenic--a lot of different kinds of concerns. Well, four or five days before the meeting I'd gotten Dave Hogness's subcommittee's draft of the final guidelines. [When I] got to Boston, I frankly can't remember whether they had a copy too. I guess just when I first got there they xeroxed a bunch of copies of the one I brought, and passed it around to people, so they had looked through some of the things and raised some of the issues that they were concerned with. The major ones they were concerned with were the composition of the local Biohazards Committee--Actually, somewhere, I should try to dig out for you the notes I made at that time of the issues that they were particularly concerned with, the copy of the draft I went through. I'm not sure at all whether I kept them. That was the kind of issues they were most concerned with, and the general philosophy. They

were concerned very much that the introductory part seemed to emphasize the advantages rather than emphasizing the cautious approach. They felt that the whole tone of the document was more in a tone that was an apologia for the scientific community rather than in a tone of "Hey, we've really got to be careful." The tone of the thing was more, "There is no danger." There were certain specific issues that they were concerned with, but not an over-all utter dissatisfaction with the document, or anything like that. I told them at the time that the meeting was going to be right out at Woods Hole and it was open to the public, and [since] they were so concerned they should have one or two people come. A lot of them had plans for the weekend already, but there was a couple who said that they would go, finally, after a lot of talking around. I said I thought it was important. So, I'd spent some time at MIT and with the Science for the People people. I'd also spent half a day over at Harvard Med School, talking mainly with Charlie Richardson and some of the people at his lab, and there had heard a lot of horror stores about things they felt were going on in unsafe ways in various other labs in the building. I talked with them about some of the issues in the guidelines, some of the things that had been raised over at MIT. Their feelings seemed to be very much the same that I'd gotten from Jon King and his friends. There were some of the more extreme things that they didn't espouse. (This part should be moved back in the transcript, back to the part before the meeting; it makes much more sense then.) It wasn't that I was going, simply feeling myself a representative in any sense at all of Science for the People, it was just that a lot of issues had been raised, and I'd discussed them with various people and come to

certain conclusions, a lot of which were not as strong as those that some of the Science for the People felt. In fact, I'd gotten a long harangue from Jon King about being in on it at all [laughs] and so forth. "Something should be done to stop the research. Putting together guidelines is just a useless waste of time," and why do I put my energies there. So at any rate, at that point there clearly was no particular sense.

While I was there I also talked quite a bit with Dave Botstein. He had some rather strong ideas related to certain aspects of the guidelines, and to the problem of ever getting them enforced at all. You know, "Nobody will live up to them, they're too strict," and, particularly, the problem of getting them enforced if they were too strict for people to accept. But he had good ideas.

Weiner: Where did you know Dave Botstein from?

Kutter: I didn't. [Laughs] He's upstairs from John King. I had seem him at the phage meetings, but I didn't actually know him. Jon mentioned to him that I was coming and that I was on the committee, and Dave had been very interested, I guess, in the guidelines' issues, and he mentioned that he might like to talk with me. And so Jon took me up to see him. Dave's initial attitude was, "What do you want from me?" [Laughs] "Well, what can I do for you?"--you know. I said, "Well, Jon thought you might be interested in talking with me about the guidelines," and he did get into a long discussion.

Then I went on to Woods Hole. The meeting there was held in an NSF summer residence. One very surprising thing was that there were essentially

no observers. There was one man from the University of Michigan (whose name I probably could find, but I would have to look it up) who I'd met also the day before giving a seminar at MIT. He gave one there the day after I did. He had been Botstein's thesis advisor, and had been sent by the University of Michigan as an observer to the meeting, because they were considering putting in P3 and P4 at that time and were concerned about finding out what was going on and what sorts of things were implicated. (I think he's chairman of the department there.) So they clearly, already at that point, were taking the whole question very seriously.

There were several people from NIH, representatives of different agencies. Chamberlayne was there again. Mal Martin. The heads of the different study sections who were going to be called on to administer grants, were there. But there were no reporters. The people from Science for the People did not come down. Later on, I really got very angry with them, saying, "You know, what right do you have now to be upset? You had a chance to interact." And at the last minute, Stan Falkow and Don Helinski were both unable to come because of family emergency situations. And Wally Rowe didn't come; he had made it clear from the beginning that he wouldn't attend meetings out of town.

So I went very naively expecting the same kind of spirit that I'd seen in Washington, with a few points that I felt needed strengthening in the guidelines, but basically a sense that Dave's subcommittee had done really a superb job of most of it, a sense that seemed to be shared by Botstein and the Science for the People people and others I'd talked with. There was a lot of concern about tumor viruses that Botstein and the Science for

the People people had expressed; that was one area that I was uncomfortable with. And there seemed to be very little on plant viruses. One person who was there as a consultant for the first time was Peter Day, who is a plant physiologist and whom I came to have a lot of respect for. He's now going to be a member of the committee, and I'm very pleased; we didn't have anyone who knew anything about plants. At any rate, what happened was that things went exactly in the opposite direction of what I expected. It started off okay for the first hour or two, and that was about the time that Charlie Thomas wandered in, late as usual. (I don't know whether to leave all these kinds of things in.)

Weiner: Why not? [Laughter]

Kutter: My memories of that meeting are somewhat blurred, because it was a rather traumatic experience. It seemed like thing after thing, when we got to essential points, rather than strengthening them, they were being weakened. There were a lot of points being kept, nothing was being strengthened, and quite a few were being weakened. And there were three of us who were consistently hard-liners from the beginning and being fairly vocal, initially; that was Roy Curtiss, Herman Lewis, and myself. I think that by the middle of the afternoon I'd gotten more and more a feeling that Roy was sort of giving up. I mean, he would clearly vote always with Herman and me, but what he'd say was almost in a what's-the-use tone. I think he felt utterly alone, because that was one place where it became very clear to me that Herman and I were really outsiders and that at that point nobody was really listening to anything we said. That was probably

particularly true for me. I kept saying it. I had to say it. I couldn't not say it. I think that if I hadn't talked with the people that I did in Boston just before, I might not have had the courage quite as much to go on saying things. But I did at least have a sense that there were other people, outside. I talked both with Boston people and with people out here in Seattle before I left about different ideas. It wasn't just the Science for the People group at all; it was other people I talked to there and here.

Weiner: Did you have a chance to talk with Falkow here since the May meeting, between the May meeting and the Woods Hole meeting?

Kutter: I talked with him in between, but not after I got the draft of the guidelines. He was in England, and I assumed he would be there. He had sent a letter to Stetten, saying that he couldn't be there, and saying something about some of his opinions. There was a long hassle at the beginning about small things of terminology that Charlie Thomas felt very strongly about. It had something to do with use of words--vehicle or vector of something--that sapped a lot of energy out of all the people there without getting anywhere. I mean, this must have gone on for forty-five minutes, a relatively unimportant thing. One thing I felt rather strongly was that there were certain places, at least, where the terminology ought to go from "should" to "shall." In other words, it should be an imperative statement, not simply a statement of "Well, it could be nice to, so we should do such-and such." I thought that there was certain tightening up that needed to be done in the definition of P2 containment, and most of it was fairly

small things like that. The testing needed to be spelled out more in EK2. And, particularly, the strongest thing was that the local biohazard committees should have a real function of some sort. It should not be stated in there that their only role was to give advice and to say whether a facility was really P3 or P4, that it was absolutely essential that that be also a place which was aware of bad uses of technique, where someone could feel free to go and say, "Hey, I'm concerned. In my lab, everybody's mouth pipetting, and it's supposed to be a P3 facility." I felt that it was absolutely essential also--

BEGIN TAPE FOUR, SIDE ONE

Kutter--that it should also include people who worked in the labs, technicians and graduate students, a plant engineer who knew something about how the building was put together and what would happen if you vented something into some particular duct, what other labs it would be likely to go back into.

Weiner: Did you say all this at the [Woods Hole] meeting?

Kutter: Yes. I said it fairly strongly. And the attitude was that first of all, these people would simply be obstructionist. They wouldn't know what was going on and they'd likely be obstructionist in terms of what could be done, in an unreasonable way; they wouldn't understand. Not the attitude that they would, for reasonable reasons, stop things. Not that we can't let them on because of that; but that they wouldn't really understand. People kept bringing up examples of what had happened places when so-and-so, who was just a firebrand out to cause trouble got on it, and

this guy was really a Marxist trying to stop everything.

Weiner: Who brought up such examples?

Kutter: Particularly, Charlie Thomas, Hogness--but that was one thing where the committee was almost unanimously against me. I mean, even Roy Curtiss, I think, was concerned with the Biohazards Committee having power over any kind of day-to-day decisions, or over what experiments could be done. There seemed to be a general distrust of people who are not scientists on the committee. And a continual statement that I heard reverberated from there through this last meeting, saying, "If the scientists don't believe in these guidelines, they'll never work anyway. We have to trust the scientists. The only way they can work is on a basis of trust." And it wasn't really 'til after the Washington meeting, this last meeting, that it finally just really snapped in my mind, the kind of myopia this represented, where the scientists were continually saying, "You have to trust the scientists," but being totally unwilling to trust anybody else, and saying, "If you gave them such-and-such a power, then they'll take such-and-such, and they'll take such-and-such, and they'll take such -and such." I mean, it was really a myopic, one-sided viewpoint, and that was the one issue that I really felt Herman was the only one who supported me on. There were some of the other issues that were split votes by a closer majority; for example, reducing EK2 from 10^8 to 10^6 was something that Szybalski was very much opposed to doing. I don't remember who else was very much opposed, but it turned out into a much closer split decision. There were several of the other points that were closer to split decisions.

By the way, I think there were tapes made of that whole meeting by Gartland. I think that's one meeting that it really would be worth getting the tapes of. The first meeting, the Washington meeting, I don't think there was anything significant that wasn't in the thing, and nothing much was happening. But that meeting at Woods Hole was a very key focal kind of thing. And you know, at that meeting, I continually had the feeling that I had to argue for these things, that there was a certain constituency out there that I had to represent in a way, almost. Well, not just a constituency, but that what they said seemed right, that I believed what they said, what I had heard from a lot of these outsiders, and that I felt uncomfortable with things that were happening. At the same time, this was a split with the feeling that these people have been working with this for all this time; they must know what's best. Am I being an utter obstructionist fool to continually say these things, and to continually fight these things? And I know some of the people on the committee, I'm sure, felt that that was true, that I was a naive outsider who was reading in dangers that couldn't possibly be there.

Weiner: Do you feel any problem of age; I don't know if you were the youngest person there, but--

Weiner: Definitely. Probably by at least ten or fifteen years.

Weiner: Well, no, Roy Curtiss is in his early forties.

Kutter: Well, I would guess Don Helinski is also probably in his early to mid-forties, but I'm substantially younger, I think, than anyone else

there.

Weiner: Stan Falkow was present at the May meeting, though, wasn't he?

Kutter: He had been present at the May meeting, and Helinski, too, but neither one of them was at that meeting.

Weiner: And they're relatively younger than some of the others.

Kutter: Yes, but they're still quite a bit older, I think. Or maybe it's just my perception of them; I perceive myself as much younger.

So, I felt discomfort but not anger in the same sense that Roy did, who had a much more valid, I think, utter perception of what was happening. I mean, I didn't trust my judgment enough to have the same kind of anger that Roy did with what was going on, that's expressed in a letter that he wrote after the meeting.

And one thing that happened was that evening there was a very nice party that Stetten had at his house for everybody, and I talked with some of the different people. I talked a lot with Herman Lewis about what we were feeling, and about how NSF runs, and the whole politics of science. This whole thing was a completely new thing for me. I still have no real sense of all the interrelationships between all of these people and sub-organizations within NIH, and how things fit together and work. I learned a little bit of that about NSF and NIH there, but I have sort of a mental block against it, too, I think [laughs] against wanting to know. But one thing that I think was very unfortunate was that Roy Curtiss didn't come that evening. There was a student of his who lived there in town, an

ex-student, and he went out with him. He had just really given up, because I think that if he had come and discussed [things] with some of the people who aren't as strong in one way or another, some of the things he was feeling, and really opened up and laid them out, and said, "Hey, I really feel uncomfortable with this, and this is why," in a one-to-one kind of interaction, I think that might have helped somewhat, at least. It couldn't have reversed the trend, but it could have helped some of the borderline decisions.

Weiner: I misunderstand one thing; I thought it was a one-day meeting.

Kutter: No, no. It was a two-day meeting; all of the meetings have been two-day meetings.

Weiner: I misunderstood that. So that would have been an opportunity for discussion and lobbying, perhaps, and convincing.

Kutter, Yes, yes.

Weiner: As it is, you don't feel that that was really happening.

Kutter: No, it wasn't really happening. The talk was on a lighter scale. I didn't have either the reinforcement of the power, in the sense of any kind of strength to lobby from.

Weiner: Did you get any feeling of resentment to you, anything explicit, overt?

Kutter: No, in fact, the feeling I picked up was something that was almost

more devastating than that. It was sort of a sense, almost, of being treated like a child. You know, "We'll let you have your say, and it's fine." And, "You'll grow up eventually." None of this said, but a very pleasant interaction with everybody, which was such as to indicate that they really weren't taking anything I was saying or doing seriously in the least. At least, that's the way I perceived it, particularly in retrospect. You know, certainly no animosity or enemies, just, "This is your role, and our role is to do what's right. Your role is to question things. Our role is to do what's right." You know, I think it would have been easier to take a little bit of a sense of, "Why don't you shut up?", or anger. I mean, nobody tried to argue me out of my position on anything. They just sort of had the attitude, "Well, there she goes again."

Weiner: Do you feel it was sort of patronizing?

Kutter: Yes. --[Interview interrupted]--

Weiner: While this patronizing was in effect, what about the next day? Was the next day pretty much the same as the first day?

Kutter: That was very much a repeat of the first day. And I ended it in simply a really negative mood, feeling that the whole things was hopeless. I still had no definite answer in terms of my status on the committee, whether I was even in a position to be doing anything. I had no sense that Roy was as upset as he clearly was from later, about what was happening. And one thing that never did happen was we never took a vote on the whole

guidelines. It was said they'd be sent around to all of us. I guess my feelings were a mixture of frustration and feeling slightly foolish at the same time, to always be on the losing side, you know, that something must be wrong with me a little bit. And almost having had enough of the whole business, and not caring too much one way or the other, whether they went ahead and put me on the committee or whether they decided that I was too stupid and obstructionistic to actually be on. And I spent another day our there and then went back into Boston and had been invited by one of the people I'd met at the Science for the People meeting to go over to the Microbiology Department at Harvard and do an informal seminar on my research stuff which they thought might be interesting to the people who are working there. He was a guy who was in Rich Goldstein's lab. I met Rich briefly there, and talked a little science with him.

Weiner: Where? At Harvard, at the lab?

Kutter: At Harvard, yes.

Weiner: Had you ever met him before that?

Kutter: No. And I gave my seminar, and didn't have much extra time there. I'd been over talking to Charlie Richardson, too, whom I know quite well, across the way. Actually, I'd been over to see him before I went out to Woods Hole. And we didn't talk at all about the guidelines; as far as I know, at that point he wasn't particularly involved or concerned. He knew I was there for an NIH committee meeting, and that was all. I mean, there was certainly no ax-grinding at that point on his part.

And I didn't talk with any of the Boston people, really, after the Woods Hole meeting about what had happened at that meeting. I saw Jon again, briefly. I was staying with some other friends there; just saw him to say "hello" to and pick up something. About the only comment I exchanged with him about the meeting was the chastisement that nobody from Science for the People had been there, and a statement that I'd send him a copy of the draft once it was finished getting put together. I didn't feel too comfortable with it, but I didn't talk to him. I just felt that I had done what I could; it had been useless. I had felt from the beginning that, at least in terms of strengthening anything, it was probably a useless battle--I hadn't expected it to go so far that way--but you know, a useless battle that ought to be done. It was sort of a letdown kind of state. I sort of pushed it back out of my mind at that point. You know, I did not think of trying to go to any other forum to get support for stopping the guidelines. I figured that it was done. It was fait accompli. That's the way they were going to be. I still have no real intellectual sense that most of what's going to happen is really dangerous. There's that very nebulous broad region. I didn't feel that if I didn't do something to stop it, the world would be likely to end, or something like that; I didn't have that kind of conviction. It was a matter that my judgment had been judged wrong by the people on the committee, and I'd live with it.

Weiner: Did you feel at that time that you had made any positive contributions at the meeting, that is, contributions that influenced them, that were accepted?

Kutter: No.

Weiner: For example, did you raise, or did someone raise the idea that an expert in industrial safety hazards should be involved on the committee?

Kutter: Oh, I raised that. And I raised a lot of those kinds of things, but the statement that was made repeatedly was that we were not going to dictate who should be on the local biohazards committees. We should not dictate anything to them; and that was not incorporated. In fact, it was voted down resoundingly. That was the particular section that was voted down resoundingly repeatedly, and people got very upset when it was raised again at La Jolla.

Weiner: Well, was the specific suggestion that you made that someone from the National Institute of Occupational Safety and Health be contacted and asked--

Kutter: I made that suggestion also, that we get someone from that as a consultant, and that was a suggestion that had come from the Science for the People people. That was something that was not part of the guideline discussion; it was part of the earlier discussion. One thing that was discussed at the Woods Hole meeting was the question of getting someone from outside, and we had discussed that before already. What was said at Woods Hole was that a number of people had been contacted by Stetten, including Gartner--Ralph Nader had been asked to suggest someone. A number of different groups like that had been asked to suggest a lay member of the committee. So before the Woods Hole meeting, a fairly intensive effort, they said, had gone into trying to find someone through sending letters, and had ranged from no response at all to something

saying, "We're not interested." I mentioned the possibility of trying to get someone from NIOSH to be on the committee; that might be a logical way to go. Or from the unions; actually, the union was already mentioned back at Washington, D.C. The other things I said was that maybe if you really want to try to get one of these people or people from one of these groups, the way to go about it is not to send a letter but to get on the 'phone. You know these people; they're in the same town. Talk with them, talk with them about how you feel [that] it's important we have someone. If they feel that they can't and don't want to do it, ask them for suggestions, really get into a two-way dialogue with these people rather than simply writing a letter. In a dialogue they might break past that initial barrier of saying "no" to anything that comes their way. So that's something I suggested, too, but I don't know to what degree that was followed. But by the next meeting we did have a person, who was a lay representative from Texas.

Weiner: You mean at La Jolla.

Kutter: Yes, at the La Jolla meeting.

Weiner: Emmette Redford. Well, then, what happened after your brief trip to Boston where you didn't discuss these things?

Kutter: I went from there to Colorado, where I was meeting some people in the Department of Molecular, Cellular and Developmental Biology to talk science and do a little research. And again, I didn't even bring up the issue of the guidelines there. I was asked by some of the graduate

students there to talk a little bit about the guidelines one afternoon. Most of the faculty didn't come, but quite a few of the graduate students did. And we talked about the need for safety. There I talked a little bit about some of the things I'd felt concerned about, and I felt supported that they shared my concern. Trying to remember, it seems to me--at the most--one or two of the faculty came, whereas the sessions we were having the rest of the time there, sort of informally talking science, much more of the faculty were much more interested. Even there, I didn't make any kind of strong point.

I had essentially no correspondence with anybody else on the committee, then, over the next few weeks. Again, I was coming back trying to take care of some things in my personal life. I wasn't home very long--less than a month, and trying to get some things done in the lab before I left to go to the phage meetings. As I mentioned earlier, we had before the Woods Hole meeting already arranged that there would be a part of a session in the phage meetings, talking about recombinant DNA. I had tried to get hold of some people who knew a little bit more about what was going on and might talk about alternate vectors; I'd tried to call Josh Lederberg several times but had not been able to get a hold of him; a couple of other people, too, whose names Waclaw Szybalski had given me. Szybalski was also going to be at this session and was going to be at the RNA polymerase meetings before, staying over for the first day of the phage meetings in order to be there for that session. He had to leave the next morning, so we scheduled it the first night of the phage meetings. In some ways it would have been better to have scheduled it later, but we did

it mainly so Szybalski could be there.

I got there a few days early for the end of the RNA polymerase meeting and to do a little work with a friend there, and was told that everybody there was talking about this recombinant DNA thing, and what a farce it was having such weak guidelines. Dave Botstein was there for the summer, and Tom Broker, my friend there, was telling about some of the discussions that had been going on among various groups there, of the people in residence mainly at Cold Spring Harbor, and said that Joe Sambrook would be interested in talking to me while I was there. The copy that they had, that they were concerned about issues in--and it was only certain, select issues--was the draft that had gone into Woods Hole, the Hogness draft. Just a few days before that, I had gotten the sort of final copy of what was to be the final version we were all supposed to write comments back on. Then, if those comments made major changes, we'd have the September meeting; if they didn't make major changes, then it would just simply be done editorially. And that would be the guidelines. So I decided, if we were going to have a reasonable discussion, it was probably good to have people see what we were discussing. I guess it was Tom's idea, actually; since I had a copy, he said, "Would you mind if we printed up some?" We had said that we were to get comments from other people, and all. So I went ahead, and they printed up two hundred, or two hundred and fifty copies of the guidelines that were ready just the day before the session discussing them. They [were] sold to people for twenty-five cents, to cover the printing costs. Most people hadn't had a chance to really read them by the time that first session happened; they'd read parts of them. And there must have been, probably, a hundred and twenty, a hundred and thirty--

well over a hundred people who came for that session.

I started out by simply spending a few minutes talking about what the committee was trying to do, what the guidelines were. I [had] decided before I went into that discussion that I was there, in effect, as a representative of the committee, and I was there to listen and try to give them a feeling of what the committee was doing. I was not going to give them any sense of my disagreements with the committee, at least at that point. That was partly, I guess, because I didn't feel totally secure about my feelings about what was happening, and what I really wanted was to listen without influencing the direction in which the discussion went. You know, I still felt I knew far too little to really be making decisions, and I didn't want to somehow be whipping up a constituency for my side, or anything like that. As far as I was concerned, it was essentially a fait accompli, and the only way to get the guidelines lived with, even as they were, was to present at least a more or less united front. In private I was perfectly willing to discuss my reservations, but I didn't think it was my role to get up in a public forum. Szybalski didn't want to moderate with me; he just wanted to listen. He took part in some of the discussion later.

Weiner: Were you sort of the moderator of the program?

Kutter: Yes, I was the moderator.

Weiner: This became a meeting to talk about guidelines, rather than one to talk about the development of a safer vehicle?

Kutter: Yes, what happened was that there were only about three papers that had been submitted that related at all to the vehicle, although he had set the time aside. Those papers were given first, and Waclaw moderated that part. Then there was a break, and then a discussion afterwards of the guidelines.

Weiner: Do you have the date of this meeting? Do you recall?

Kutter: I can readily find it for you; it was in late August, just shortly before Labor Day. I discussed, first of all, what had been done and sort of the basic criteria, the idea of biological and physical containment, and so forth. Then several people started raising some really strong philosophical considerations. At that point, the most vocal was Hatch Echols. And there were several other people who were doing a lot of talking. As the meeting went on, gradually Rich Goldstein also got more involved. Through the course of that meeting, I could see him listening to a lot of people and expressing concern. It wasn't that there were a few people who felt strongly about anything; there were a lot of discussions going on. It was almost an attack at me at times. An interesting thing at that meeting was that if either of the two of us came out as seeming moderate, it seemed to be Szybalski that kept saying, "Well, I agree with you. I disagreed with that on the guidelines." [Laughs] I was really keeping my mouth shut, so Szybalski at that point came out sounding as though he were on the side of much more stringent guidelines, which on some issues he had been, like the 10^6 or the 10^8. But the impression there was different from the impression I'd gotten earlier.

That session went 'til almost midnight, and then some of the people involved, including Hatch and Rich, decided to call a second meeting a couple of nights later. It was held over in the bar, to discuss more of what, if any, kind of action should be taken, and what should be done, and so forth. At first I wasn't going to go to that meeting at all; they hadn't specifically invited me or 'disinvited' me or anything like that. Then I was wandering by, sort of when they were halfway through it, and went over and sat down to listen. Finally I said, "I'm tired of playing devil's advocate," and told them that a lot of these things that they were most concerned about, I agreed with them a lot on and felt strongly about.

Weiner: How many people were present at that session that you walked in on?

Kutter: Probably forty.

Weiner: Forty? In the bar? There was room?

Kutter: Yes. Well, the bar is sort of at one end--it's sort of a big place where they do dances, and all kinds of things.

Weiner: I see, so they could just take over that.

Kutter: Yes.

Weiner: And was someone in particular leading it?

Kutter: Mainly, Hatch.

Weiner: Hatch. That's Harrison Echols.

Kutter: And Rich was involved, too. And several other people. I think Botstein, too, as I remember. Some of the local people were there, too, though Joe Sambrook wasn't, I don't think. That really wasn't my meeting, and I was mainly just listening. In between those two days, I had talked to the people at NIH. I had called Bill Gartland. After hearing the tone at the meeting, and after hearing some of the other things. We were supposed to give our responses by September 1st, and this was just a couple of days before that. I had called, saying that there were certain of those things that I had voted against and had a lot of reservations on; there seemed to be a really widespread concern about [it] here; and, you know, pointing out a few of them and asking what was happening in terms of other things coming in. He told me then about Roy's letter, and--

Weiner: Stan Falkow--

Kutter: Stan's letter. And that Maxine was very upset about it, and felt that she certainly at least would not be willing to have that be the guidelines that would apply to NIH; where she was chairman of the Biohazards Committee, she was going to insist on more stringent controls there. And I guess Paul Berg's letter had come by then, too.

Weiner: I'm not sure. That, I thought, was the first of September; I'm fairly sure.

Kutter: Yes. I don't remember. Yes. It may be; I don't remember. There was at least one or two. Anyhow, they were telling me--I know--about Maxine.

Weiner: His [Berg's] was September 2nd.

Kutter: Okay, then that wouldn't have been there yet. It was just before that time. Anyhow, Gartland said that the decision would be out in a few days as to whether to go ahead, and that there was some question on whether to have the meeting in September. I said I thought we ought to have a meeting in September; I would put in a strong vote that we really did need to reconsider some of those things, particularly since Helinski and Falkow had been gone. One thing I did say to the group was that the committee was just in the process of deciding whether to implement the guidelines then. Without saying anything to me, they'd gone ahead and drawn up that petition, and they wanted to show it to me and have me somehow correct it, or something. I said, "I'm staying out of that. I'm a member of the committee." I did tell them, though, that the decision was in the process of being reached during those few days and that if they wanted to have any kind of impact they should make a point of getting it off right away. They had this petition that some fifty-odd people there signed, the points of which weren't all totally accurate. And that was something that was really much attacked later. You know, there were sort of generalizations, rather than hitting the specific point, but they decided to send this general thing and then later send something that took the time and patience to go into specific points. In retrospect, maybe it would have been good if I had pointed out, "Hey, this isn't quite right. You should say this a little bit differently to have it be accurate in terms of the criticism you're making." [For example], like saying that most of the people were somehow involved in recombinant DNA work.

Weiner: You mean most of the people on the committee.

Kutter: On the committee, yes. Some of the criticisms they made precisely as worded weren't quite accurate, but in spirit they definitely were. But I really didn't feel that I wanted to have anything to do with that. I mean, I really was trying very hard still at that point, and I'm still trying very hard to be a listener and a mediator and try to figure out when both sides are really saying the same thing in different ways and what kinds of things can be tied together from what different people are saying. You know, and then focus on the points where there is real disagreement, which was much less than appeared then, I thought. I really still saw myself as a relatively neutral or conservative member of the committee. And I called them, and talked to Wally [Rowe] just about at that same time, who I'd met just briefly at the other meeting, and asked him what he thought of the Woods Hole draft. He said, well, he hadn't had a chance to look at it yet. And I said, "Well, there are some things that I'm concerned about, and a lot of people here, like Joe Sambrook and most of the people at the phage meeting, seem pretty concerned about. I'd really appreciate it if you would take the time to go through fairly carefully, and if there are things you're concerned about, say something to Gartland at this point before the guidelines have been made official." He had done some of that, and seemed to agree with my feelings. Then I talked to Bill Gartland just before the end of the meeting.

Weiner: You called Wally Rowe from Cold Spring Harbor?

Kutter: From Cold Spring Harbor, yes. I told Gartland then also about this petition. I think the decision was made before they actually got the

petition, but when I talked to him on the phone, I told him that "People were upset enough that some fifty people here have signed a petition which you should be getting in a few days, which was mailed sort of in the middle of the meeting." I talked to him again, then, just before the end of the meeting and he said that it looked as though we might be waiting, that they might be held up because there was a lot of flak coming in from different places. That was the time when he first said that he was going to ask those people who had strong disagreements with the guidelines to draft some alternatives. And he said he'd already talked with Don Helinski, and mentioned Falkow and Roy, and said that it would be good if I would work with them, too, if I would like to.

Weiner: Meanwhile, you're still not a member. Your letter of appointment didn't come, according to the file, until November.

Kutter: Yes. Well, there was a while before that when they said everything was okay. I mean, there were sort of two stages in that final appointment thing, apparently, and what he had told me when I talked to him just before the phage meetings was that the machinery had happened to actually expand the thing, and my appointment had been submitted and was in the works. Yes, it finally didn't come until November, and in fact, with it came a note saying, "Please sign all this stuff and get it back fast, so we don't have any problems at La Jolla about whether or not you can vote." [Laughs] Because it came about three weeks or something before that.

There was talk then about trying to get people together to tighten up some of those things. And after talking with him, and the day before I left I finally met Joe Sambrook, who I'd tried to see a couple of times before.

He hadn't been around, but Tom had said he wanted to talk with me. We sat down and talked for a couple of hours. Joe was mainly interested and concerned about the tumor virus parts of the guidelines, both tumor viruses as vectors and tumor viruses in plasmids. He first of all started giving me a little lecture on SV40 to give me an idea of what was going on, and what kinds of things they could do with it. He said that he was really concerned, that he felt that the guidelines for work with tumor viruses just were not realistic and really could pose some potential hazards. It was very clear that we'd had really no tumor virus person on the thing. We'd been listening some to Mal Martin; the main thing we had heard from him was a statement of what the guidelines were for working with just plain SV40, which was essentially very low containment--you know, the criteria that were normally used for working with SV40; and statements, particularly from Charlie Thomas, saying, "How can you possibly ask people to work with these things in more stringent conditions; if those are put through in that way, pretty soon people are going to have to do all their SV40 work in P3 or P4 facilities," and so forth.

Joe said that if that's the way the guidelines came out he'd go ahead and do it that way. They were all set to go ahead and clone SV40. They had the piece chopped out that they could have used, and so forth. But he said he really didn't feel very comfortable doing it. I asked him what he thought should be done to try to get a more realistic appraisal of what was going on, and he said, well, he thought the only reasonable thing to do would be to call together a small group meeting of the people who are actively involved in working with SV40, and as he said, there are very few labs at this point working with SV40, maybe seven labs. George Fareed at Harvard, and I think

somebody at Yale, somebody at Johns Hopkins. And he would suggest calling together that group and getting them to all talk about what ought to be done. Only with that kind of discussion back and forth, agreement back and forth, did he feel that anything could come out that would be accepted as useful and stringent enough. Anyhow, I said, "Well, if we tried to do something like that, would you be willing to help us? Would you be willing to come as a consultant to the committee to work on that particular thing?" And he said, "Well, I wouldn't be willing to be a member of the committee, but I would be willing to do that, you know, spend a period of time doing what had to be done." He had a lot of things that had to be done over the next period of time, but that was something that he couldn't very well say no to, if he were asked.

That whole period at Cold Spring Harbor for me represented an evolution from a point where I felt like a completely ineffectual outsider to a point where I started to some degree acting on my own, being willing to speak, in effect, as a member of the committee and not worrying about overstepping the bounds of my role or even doing something that wasn't directly officially sanctioned. I mean, if we only acted as a committee, nothing would ever get done. I'm sure the other people were often talking individually to people asking for help, suggestions, so this meant that I had someone whose views I knew, who I could suggest, at least. Well, the decision was made at the same time that to try to do anything by the September meeting was crazy, there wasn't time to rewrite or rework anything. That was one of the parts of the decision that Gartland had given me. I wasn't too comfortable with that, because I felt that waiting 'til December was a hell of a long time to

wait. But then I came back home--

Weiner: Let me ask this. How long were you at Cold Spring Harbor altogether?

Kutter: I was there ten to twelve days.

Weiner: And the first meeting on the thing was at the beginning of those ten days.

Kutter: No, it wasn't. I was there for the end of the RNA polymerase meeting, so it was about the fourth day. All these phone calls were over a period of about four days. I mean, that was just at the period when the final decision making was going on. The coincidence was kind of strange.

Weiner: So you went home after the meeting directly?

Kutter: Yes. I went directly home. It was interesting, in fact. It was Dave Botstein, who was concerned enough about what was happening, who called me and was talking with me, and gave me unlimited use of his phone to call back and forth to NIH to find out what was happening and really encouraged me to go ahead and try to do something. That was, I guess, the first time where I saw myself perceived by people outside of me as someone who had something to offer, as someone who was doing something.

BEGIN TAPE FOUR, SIDE TWO

Kutter: So at any rate, I came home and was coordinator of Foundations of Natural Science that fall, and had relatively little time before the start of the quarter, and a lot of work. We had one new member on the faculty

and there were definite problems within the team that was teaching together. I was teaching the same program but with different people; the year before it was extremely smooth-working. But this year there were conflicts in style and ways of doing things so it took all of my energy for the first few weeks. Then it became more and more obvious that nothing was being done in terms of trying to put together any kind of alternative things for the guidelines. Finally, I called and talked to Roy and to Don Helinski, and so forth. Don was saying that he really couldn't take any kind of leadership role in doing it because he was in the process of trying to put together the La Jolla meeting and that that was taking a lot of time and energy, but he'd be glad to be supportive of anything the rest of us decided to do. I talked to Roy, and he was so involved in trying to make the EK2 it was a night-and-day kind of thing. He said that he couldn't do it. And Wally wasn't willing to try. He hadn't been at Woods Hole. So things just sort of coasted, and nobody was willing to pick up the ball. Finally, a few weeks into the quarter two things happened that precipitated my decision to finally do something. The faculty here for the first time was trying a series of once-a-month noontime talks by people on the faculty about the kinds of things they'd been doing professionally, and so forth. The person who was organizing it knew that I was one of the few people doing research kinds of things and had heard a little bit about the recombinant DNA thing, and asked me if I'd be willing to do the first one of these. I talked with him a little bit about what I could do. And I said, "Okay, I'll go ahead and do it," and talked a little bit first about T_4 and how I'd gotten involved in working with phage, and what kinds of things had

led me to molecular biology or to doing science. Then I went on to talk a little bit about what DNA was, and passed around some DNA. A lot of the people there were artists, historians, and psychologists, and so forth. There were about forty people there.

Weiner: Faculty.

Kutter: Faculty mainly. The president of the college actually came. It was the first one; they were trying this thing out. They never got as good attendance later. It was in an informal lounge kind of setting. Then I went on to talk about the basic technique for making recombinant DNA and about the committee and the decisions that were trying to be made. I got a lot of feedback, a lot of questions being asked, and the questions weren't being asked by the scientists. They were being asked by the other people on the faculty. Really insightful questions, so I felt that that talk went really well and brought home to me really for the first time the sense of concern of anybody outside of the scientific community. At that point, I still had not seen any general public kind of articles. I had not even seen the Rolling Stone review of Asilomar. I really had no sense of what was going on outside the science community. So people were really concerned.

A night or two later we were having a faculty seminar and had been talking about some pretty heavy things in terms of the problems of decision making and having to live later with decisions, and had been drinking a fair bit of wine, too. This started out as a seminar among the four of us on the faculty, you know, talking about things related to teaching, but we'd stayed on to have dinner together. It was getting late in the evening, and it had

gotten fairly heavy, and Will Humphreys talked about the thing that he'd had to live with; his two older sons had been drowned the first year he came to Evergreen, the year before we came. They had taken a boat out without authorization--they were eleven and thirteen--to row across to an island that's across this part of Puget Sound, a couple of miles. And he had realized that they were gone and that that was probably what they had done, and debated whether to call the Coast Guard to go looking for them. Finally he decided, "Well, they'll be okay. They'll come back." They were very strong, independent kids and had done a lot of independent things. He waited and hour or two, and finally decided he'd better call the Coast Guard, and by that time it was too late. Somebody watching from the far shore had happened to be looking around with binoculars--at birds, actually--and had seen this small boat out there and had seen it break up. Apparently the boat had actually broken up and then disappeared from view. Whether it had been hit by some sea mammal or large fish, or had just collapsed, or what had happened, nobody ever knew and their bodies were never found. But for him, something that he'd had to live with and function with was having had that chance to make this decision, and having made the wrong one.

Sort of that whole week since--well it had actually been about three days since I'd given that talk--I'd talked in between with Roy and Don again, and was living with this thing that nobody was able to take on the load of doing that and I couldn't either. I told them I couldn't. And this was something that I said then to my three colleagues. I said, you know, "I'm feeling that same kind of pressure right now, that there really needs to be parts redrafted of the guidelines before the December meeting,

and none of the people, none of the more conservative people who care about it are able to do it for really valid reasons. And I can't possibly do it for really valid reasons. And it's just not being done. And I'm trying to live with that, and it's really hurting me, but it's just one more thing than I can possibly do." And to make a long story short, they said, "You've got to do it." And they said, "We'll try to help you. We'll take over some of the responsibilities as far as your teaching goes, and you've got to do it." And I think to some extent, they later regretted that statement. They were just then really supportive, saying first of all, "You've got to do it," and secondly, "You can do it." The second thing I said, you know, once they said, "You've got to do it"--I said, "Well, the other thing that really bothers me is how can I do it. I mean I don't know that much about a lot of those things. How can I be presumptuous enough to say that I'll do it? And I'm scared. I'm scared that I'm not good enough. I couldn't do an adequate job, and here's all this responsibility. What if I put the burden on you and take the time and do it, and try to do it, and then can't and really fail. You know then I'll feel three times as bad. And I'll feel a lot of pressure to try to do it--" And they just said, "You've got to" [laughs] "You're going to" [laughs] and set up a couple of things that I was going to do. For instance, I was not going to try to go to their lectures. Instead of doing that I was going to during that time do whatever phoning I needed to do when nobody was around to bother me. Certain things I'd still keep doing. There was a little discussion as to whether I should try to get someone to take my place in the program while doing it, but that didn't seem to be right. I also then talked with the one dean here, who

was particularly concerned with the sciences, and also with Ed Kormondy, the provost, who's an ecologist, and asked if they'd be willing to give me support if I tried to do it. They said yes.

I'm trying to remember now the exact time sequence. At some point in there I had called and talked with Dave Hogness. I think it was before I'd decided that I would be the one to do it. I was just trying to get us together a little bit, as a group. I aksed Dave if he'd like to be involved in trying to change anything, and he said "No," that he had put everything he had into that draft and he had a lot of other things that he simply had to do; he was not willing or interested to have anything to do with something like that. I also hadn't talked with Rich Goldstein for a while. I'd talked with him a bit earlier. Well, the next things I did before I did that was that then I called Bill Gartland and asked him what was happening. And he said that at that moment Stetten was in talking with Fredrickson, and a couple of other people were in there too. They were trying to figure out what to do because there was this real sense that nothing was happening, and something had to happen. And they were trying to figure out what to do to get something to happen. I said that I was willing to try to lead a group to do it. Apparently one thing that they were going to do at that point was to send a letter out to everybody saying, "You've got to do it. Somebody do it" to everybody on the whole committee. So as a result of that call, that letter didn't get sent. It probably would have been better if it had gotten sent, in retrospect. I called Wally Rowe and said, "Look, I'm willing to take on the over-all organizing of this, and I know that you don't like to travel, can't travel, and that that makes some things complicated." But, I said, "Clearly, one of the

biggest problems in terms of the present guidelines, the thing that people are most concerned about is the tumor virus stuff." And I said, "I think that that takes a separate thing," and I'd mentioned to him before Joe Sambrook's idea, and I mentioned it to him again in a lot more detail. And I said, "Would you be willing to get together some kind of small subgroup like that there at NIH?" In fact, I must have called him after I'd called Gartland, because Gartland had said that there would be money available for limited travel and meetings to put it together.

Weiner: He gave you the authorization to put together a--

Kutter: Yes.

Weiner: Was it his decision? Or had this--

Kutter: Well, all of them had been talking with us about doing it over a period of a month, but--

Weiner: And then you volunteered?

Kutter: But I volunteered to do it finally.

Weiner: In that same conversation, though, he said, "Fine, we'll give you a budget."

Kutter: He said, "Good, go ahead and do it." He said there would be some money available for travel. And one thing they couldn't have any money available for was phone calls and things like that. But it seems to me that that was in the same phone conversation. I'm not sure, but it may have also

been a later one. There were probably two within a couple of days, I would guess, because I think I told him that and then he talked to Stetten and called me back, as I remember. I really don't remember as well as I ought to exactly what happened there. I felt at the same time, very awkward to be taking it on my own shoulders in a way and saying, "I'll do it," when I wasn't even, in some ways, officially a member of the committee. I really didn't want to. [Laughs] I sort of felt pushed into it by the combination of circumstances, partly that the other people who could possibly have done it had already given such big hunks of their lives to it, and that I hadn't yet, and that there were some parts that they would have to do. I talked to Stan Falkow at that same time and asked him if I tried to do it and had a meeting here to do it, would he be willing to work with me on it and come down and work on it. And he said he'd be glad to.

I talked to Wally and asked him if he'd be willing to put together the subgroup, and it had to be done in pretty short order because we set outselves then a pretty tight time schedule. It must have been mid-October, already. I said we wanted to have everything put together to have a meeting here; I think it was November 3rd that we said. But the date was set partly because we felt that we had to be able to get them put together and distributed and get comments back before the December 5th meeting, and partly because Stan was going to be leaving a couple of days later to go away for a week or two. Well, I told Wally about that. I told him that Joe Sambrook had said he'd be willing to help. Then he and I both, for two or three days, tried to get hold of Joe, who's a hard person to get hold of.

Also during that time I called Maxine Singer. I'd met her just briefly when I was there for the Washington meeting and listened to a talk of hers.

Nancy Nossal, who's a good friend of mine, had worked fairly closely with Maxine and, I knew, thought the world of her. I was at that point trying to get this thing started, feeling really awkward and scared, having a hard time making myself pick up the phone and call people, for instance, to get the things that I needed. I called Maxine and told her how scared I was. I knew she'd been really involved in the whole thing, and I asked her about the things that she felt needed to be done. One thing that she had done, she said, was to put together something that had her concerns about the guidelines, that she put together in an informal way for Stetten, and she'd be glad to send me a copy of that. At this point we still hadn't gotten copies of any of the letters criticizing the guidelines. None of us had seen any of these, which had been one thing that we were sort of waiting for before starting anything. Roy sent me a copy of what he had written, and Stan sent me a copy of what he had written and the replies he'd gotten. I asked the people at NIH to please, at least, get those of us who were trying to work on the drafting a copy of those things right away. Maxine had copies of a few things that she sent me, and Wally did, but we never got the whole packet until just a few days before our meeting in November. It was clearly a job job to xerox them all, but this was probably close to a month after I'd first said, "Hey, we've got to have them." They'd been trying to put them together into a very nice, neat little booklet, and so forth; it would have been much better if they'd just sent them. [Laughs]

Weiner: Was there anything that indicated that after the petition and

similar letters being critical were received, that there was an effort to get letters in support of the guidelines, either official or unofficial?

Kutter: Well, they had talked with Dave and some people like that. There was one that came in that they got from Don Brown. Anyhow, one thing I asked Maxine [was] if she'd be willing to work with Wally on this subcommittee because Wally hadn't been at Asilomar or Woods Hole, and was a bit concerned about trying to draft something on the tumor virus stuff. He felt much more positive when Maxine agreed to work with him. Joe, Maxine and Wally organized that, and there was really good response of people coming to that. The draft they came up with, which they put together just before our meeting, was extremely tight. I mean, calling for much stricter controls than anybody had expected, and also extending itself to calling for stricter controls for all eukaryotes. They felt that even down to yeast, everything should be done under P3, EK2.

Weiner: This came out of a specific meeting that they had?

Kutter: Yes. They wrote around a bit, then had one meeting, just a one-day meeting in Washington.

Weiner: Was this September, October?

Kutter: This was the end of October and just a week before our meeting. Joe Sambrook went down for that, then he wrote up what they had decided on, made a second trip down to Washington to talk it over with Wally and Maxine, Mal Martin and the other people who were down there, and sent a copy around to everybody else on the subcommittee. Then Joe came out to take part in

putting together the final guidelines, so the final thing was being put together, then, by Stan, Joe and me. During this period while he had been trying to do that part, the other thing that was a concern was the whole problem of plant viruses. To try to deal with that, I called Peter Day, whom I'd met as a consultant at the Woods Hole meeting and asked him for suggestions. It turned out that he had been asked to be chairman of a subgroup set up by the American Phytopathological Association to look into cloning and recombinant DNA. And for him, it was too tight to try to get a group together to meet. But he had already sent a draft of the guidelines to quite a few of them and [had] got comments back from them. The biggest problem in terms of expenses connected with that were the phone bills we were running up. I asked Stetten what we could do about that; there was no real way to get paid for them, but Stetten wound up giving Peter and me his private charge number to charge calls. So part of the calls I made were paid for by the University here, and part of them were on Stetten's private official charge number. I don't know if he'd want anyone else to know that that happened. So he was being really supportive, and I talked to him once or twice a week during this period. I also got several students here who were really interested to help me start gathering material and taking the things that came in and organizing it, and so forth.

Then about this time, I made a call to Rich Goldstein to tell him that something was happening. When I called him he had already been sent that short blurb in Nature. ["DNA Committee has its Critics," Nature 257:637 (Oct. 23, 1975)] In fact, a reporter from Nature called me not long afterwards, but too late to get what I talked with him about in, correcting the blurb that finally went in. When Rich talked to me, he said, "Nothing's

happening. We've got to do something to stop what's happening. This group here in Boston is in the process of drafting a strong article denouncing the recombinant DNA thing." And I said, "Listen, what's happened now is the guidelines aren't being put into effect for right now, and they're going to be revised before they're put into effect. I'm chairman of the subgroup that's working on that, and the best help you can be now, if you're willing to, would be to channel your ideas and any concrete examples and any concrete help you can give--channel it to us and try to help us draw everybody together to work to something that's really good. If you're still upset with what happens after the La Jolla meeting, then go ahead with what it is you're thinking about doing." And to a fair extent, that's what they decided to do. But they had sent this copy of their petition, already, to Nature, [which] turned into that short editorial that came out in Nature. The day after I called, a reporter from Nature called him and somehow the story got garbled--I can't say at what point it got garbled--to say something to the effect that the guidelines had been scrapped, and that I was chairman of a subcommittee to draft new guidelines. Well, that certainly was never stated and never suggested, never felt. I mean, all of us felt that most of the guidelines were basically good and that Dave Hogness had done an incredibly good job on a lot of it, but that certain areas just really needed to be reworked before that became public policy. But that thing in Nature set off a real uproar, because what happened was, first of all, a letter was being sent out from Stetten, or from that central office, telling everybody on the committee that this subgroup of us, headed by me, was trying to draft suggested changes for the guide-

lines and inviting everybody to help. I don't know if you ever saw a copy of that letter.

Weiner: Yes. I think it's one of the ones we copied. Or if not, I copied it from Falkow, but I've seen it in the last two weeks.

Kutter: They sent me a copy of it right away, with the various articles which they sent about two weeks before the meeting; my copy of the letter was with that. But they were saving the other copies of it to send with everybody else's copies of these documents; they were taking their time to collate very carefully before [sending] them out, which was quite a bit later. What happened was that, for example, one morning Jane Setlow read in Nature that the guidelines had been scrapped. It's a quarter of a page which is simply a copy of the statement from the phage meetings that this group had signed, plus the statement at the end about the scrapping which had been added after the thing was originally sent in, just on the basis of that one phone call. It was crazy. So, anyhow, she had, in the morning, seen this thing in Nature which she assumed was totally garbled, saying the guidelines had been scrapped. Then in the afternoon, she'd gotten this packet of things with the letter. And she and a few other people, like Dave Hogness, were absolutely irate. You know, if anything was going to be done, why hadn't the whole committee been contacted, why hadn't everybody been asked, so forth and so on. I guess she called Szybalski to find out what on earth sort of whippersnapper I was. [Laughs] And so, you know, other people were equally irate.

Weiner: I've seen her letter. How was that smoothed over, though?

Kutter: I called her and told her the sequence of what had happened, and that if I had realized that the letter hadn't gone out I would have personally called everybody on the committee. I'd called quite a few who I thought might be interested, for example, all the people on the Hogness subcommittee, to ask them if they wanted to work--Ernie Chu and all.

Weiner: Well, at least you had input in this process from Berg, from Lederberg, from Curtiss--

Kutter: Oh, that was the other thing that I had been doing during this whole time. From Maxine, for one, I'd gotten a list of some names I should talk to. And everybody I talked to, I got names of other people I should talk to. And I called Josh Lederberg, listened to him for two hours, listened to all the reasons why he thought there should be, in effect, no guidelines. A lot of them were things like, they tended to be a very self-serving thing among a small group of scientists, because if you had really stringent controls it would mean that only those people who had access to these very expensive facilities could do the exciting, interesting experiments that he thought were really important; and all of his young people getting degrees were in positions now where they were going out to more everyday universities and they wouldn't be able to do things; and he had had trouble himself getting money for a P3 facility.

That was when we first started talking about the idea of having a few centrally-located facilities that anybody could go to. So I talked a lot with him, with Wally; I called particularly the few people who had written in letters saying they should be less tight, like Edgell, and I talked

several times with Edgell, and asked him to draft a more detailed response, which he did, that got there just in time for the other meeting. So I really did try to solicit other input. Once this started happening, suddenly there was a flurry of activity in the other camp. Hogness solicited something from Don Brown and a bunch of other people and started getting very irate, implying that the influx of letters that had come had been just from a very few people representing a high effort on the part of a few people to get the thing stopped; and what do you mean by a flurry of input; and that it was representing a very vocal minority, which I think he honestly believed. I don't think that's true in the long run, but I think that's what he honestly believed. So, initially, what we were trying to do was to just change certain sections. As I say, we got as much input as we could from everywhere. The other thing happened then, that horrible article came out in Science. That just really made me angry.

Weiner: The one by Nick Wade in November. [Science 190:767-769 (Nov. 21, 1975)]

Kutter: Yes. It was after the draft of the guidelines was put together; in fact, after it had been mailed to most people. I'd better go back to the process of putting it together, which I think is kind of an interesting story in itself. Joe Sambrook came out, as I said, and Stan Falkow came down, and we held the meeting at my house. A couple of students who had been involved a lot came and were involved also in the meeting, mainly listening, asking questions sometimes, running flunky errands when needed. Joe came in the night before, and we talked through some of the things then.

Then once Stan came down, we went into a marathon session going through trying to tie things together. [We] hadn't gotten all the input from Peter Day yet, but he called while we were meeting. On a couple of sessions we would end up calling Don Helinski at home to tie together a few points where there was something we weren't quite sure of. The students who'd been working with me had made a whole filing system of all of the suggestions, and where they came in terms of going through the guidelines.

We pretty much went through from beginning to end, trying to take into consideration at each point all of the issues that had been raised, both by people who felt they needed to be tighter and by people who felt they needed to be looser, and by people who were just concerned with tightening up wording, and so forth. At that point, in general, we tried to deal only with those things where there was some kind of major change, because we had in mind hoping to send out something that would just indicate very carefully those portions that had been changed, and, hopefully, there'd be large portions that wouldn't have to be talked about at all at La Jolla. There were different subparts that people had put in something for; for example, one of the problems was with trying to tighten up the definition of EK2. Taking into consideration Roy's concerns that there shouldn't be an EK2, and the concerns of other people, what I tried to do was to write down some details on exactly what Roy was doing and felt was important and put together a draft of the possible requirements for EK2. I had sent that then to Roy, and he penciled it up a little bit and said, "Hey, that EK2 I feel comfortable with." That was the kind of thing I'd been trying to do a lot, seeing where there were no real differences, and trying to act as

mediator. The thing was interrupted for a few minutes in the morning by a call from the Economist in London. They were trying to put together a story and wanted to ask us just a few questions. They had gotten my home phone by calling school.

It was a really productive day. We wound up putting together something that, I think, all three of us felt pretty good about. We were almost done by the time Stan had to leave about seven, and Joe and I worked fairly late into the night, finishing tying together some things and going back and rereading through.

At first I had intended to just send out that version as a final draft to everybody. Then I decided that in terms of the political hackles that had started to be raised, that it was probably smart to send that version to those people who were, in effect, signing it, to Don Helinski, Roy, Wally, and Peter Day, and send it again to Stan before it was sent out. So what we did was just quickly cut and paste and just retyped those things that had major changes, and put together a version that was sent around to the people on this subgroup.

Weiner: Where did you get these people in the subgroup? I mean, you just picked them up as you needed them, I gather.

Kutter: No. That had been the people who were originally conservative, and as far as the committee went, the ones that had been asked by Gartland to somehow put together something; in that letter that came out [where] it specified those people, you know, it said, "If there's anyone else who is interested in doing it, speak up and add yourself to it."

Weiner: Yes, I get it. I misunderstood.

Kutter: Well, the two people who weren't on that were probably Joe and Peter Day, but they'd been involved a lot in it. So we did that, and xeroxed it, [and] sent it off a day and a half later. Then the meeting was on Monday. Friday and Saturday I talked with Don and Roy. Actually, the talking on the phone went through Monday, before some of them had gotten through it. Roy, for example, had gone, item by item, through everything in the guidelines and felt really strongly that there were a lot of small changes in the parts we had tried to leave unchanged that ought to be made because this was sort of the last chance. Essentially, everything he was right about, I think, a lot of these small things. But that was a little bit against my better judgment, making so many changes, but at the same time, it was probably the right thing. Anyway, we went ahead and did it; he felt very strongly about it. He and I spent from approximately 10 P.M. to 1 A.M. on the phone one night, my time, which is about 1 to 4 A.M. his time. [Laughs]

Weiner: That's normal for him.

Kutter: Then we went on and finished it the next day. We must have spent eight hours--no, it couldn't have been that long--but a lot of time, going through the whole thing very carefully. He had an incredible ability to pick up every inaccuracy and slip, as well as some of the pretty important things. Then I put all those things together on this, and then one of my students and one of my colleagues, who's a good typist, too, Burt Guttman, did a marathon typing job and typed up the whole thing in the next twenty-four

hours. I'd arranged ahead to take it to the printer, and to get it done super-fast. So we took it to the printer Tuesday night, and got it Wednesday noon, printed and collated, and it was in the mails by Wednesday afternoon.

Weiner: How many pages was this? Do you recall? We have copies of it.

Kutter: Forty pages, or something like that. Plus the Appendix, so it was longer than that. The Appendix was something that Frank Young had put together on B. subtilis, as a possible alternative. I'd been trying to talk with people about alternative systems, and so forth. You know, I could give you my emergency list of phone numbers of all the people I was calling fairly regularly and getting help and support from.

One of the real concerns during that period had been with the question of how to do anything about industry, and one of the very good suggestions that Josh Lederberg had made was, "Well, why don't you just call somebody?" [Laughs] And he suggested particularly calling the fellow who's the president of the Association of Industrial Microbiologists, who's Max Stark at Eli Lilly. Or, actually the name he gave me was a different name who turned out no longer to be president, but I called there and got the name of the current president. He was very interested and supportive, and I sent him also, then, a copy of the guidelines. He hadn't had much chance to do anything with it before La Jolla, but I've kept in touch then, since he's sent it out to some other people; it's been an interesting dialogue back and forth, as to the role of industry. He seems to have quite a broad oversight. I've heard that from several other people I've talked with; for

example, down at Cetus they said the person to talk with about such things, who has the broadest influence among the industrial microbiologists, is Max Stark.

Weiner: So what did he ultimately have to say? This letter from November 26 is just that he'll circulate it and get some--

Kutter: Get some feedback, and then I talked to him later. In general, [his] attitude was that that's something that they ought to get a committee going on to talk about; that it was something that was not generally a concern yet in industry, but probably would be fairly soon; and that industry would be, as far as he could tell, very eager to follow whatever guidelines we put together. For industry, the extra money it costs to use P3 facilities, or whatever, is rather a drop in the bucket compared to the total costs that they're expending on anything. In terms of both the general public ideology and preventing legislation which they're as anxious to do as the scientists are, and the potential of legal suits if anything gets out, they were very concerned and he felt that it was really important to keep open a strong dialogue between industry and what we're doing. In fact, I talked to him just recently about the idea that had been brought up at our first meeting and then never talked about since, about using as antibiotic resistance markers, some that are non-clinically useful; I'll talk about this later. But he was supposed to call me back this week. He was going to ask quite a few people about it; he was very interested in the suggestion that they might get involved and help that way, and was going to talk with several people about the feasibility and more definite

ideas. So the dialogue seems to be basically a fruitful one, even though it hasn't gone very far yet. And a man from Eli Lilly was at the La Jolla meeting.

Weiner: Well, getting back to that period: the meeting, and then getting the document out, and you mentioned getting it out to industry as well.

Kutter: By the way, NIH did one very nice thing that helped speed up the process of getting it out and make it a little easier. I knew that they had all these mailing lists and they were telling me all these people that it ought to be sent to, and I said, "Well, why don't you send me a stack of addressed, franked envelopes with all the people that you think ought to get it." And that's what they did, and so we had mainly the job of sorting it out and stuffing the envelopes; where there were people that hadn't been involved before that, I added just a short note saying, "This is what we're doing. If you have any comments, we'd appreciate it. Send them to us at La Jolla." And it went out with a cover letter that was from the group of us speaking positively of the Hogness guidelines, saying that there were certain places that were felt to be a problem and so we've tried to make suggestions for change. In doing so, we've taken into consideration the following sources, and then it listed a whole sequence of different things: the phone calls, and all the documents that had come in; the various versions of the guidelines; the initial plasmid document that had been written by the people going into the Asilomar meeting, and some of that stuff.

Weiner: Well, this was received by people about a week before the first

workshop in La Jolla and about a week before the meeting itself?

Kutter: No, to the people on the committee we sent it out a couple of days before the other people, for policy reasons; it probably got there about two weeks before.

Weiner: Two weeks before. Okay.

Kutter: Two and a half weeks, something like that.

Weiner: Now the Science article came out about this time.

Kutter: Yes, the Science article came out that week when I had just gotten everything into the mail, or maybe the next week. I was feeling a real sense of satisfaction, that I'd taken a difficult job, and I felt pretty comfortable with the role that I'd played. We'd really come up with something that there would certainly be argument about and certainly there were people that were having problems, but I'd spent a long time on the phone, for instance, with Dave Hogness, in the process of finally putting it together, listening to some of his things. I took ideas from everybody, including Lederberg and people like that. You know, I felt that we had at least a reasonable chance of pulling it together into a working-together kind of thing, where even though there were disagreements they were in the spirit of working toward something good rather than in the spirit of--

BEGIN TAPE FIVE, SIDE ONE

Weiner: This was just the feeling that you had of what you had accomplished.

Kutter: I had this incredible sense of relief once I had sent them off.

I'd figured on having a few weeks to really concentrate on my students and my teaching; they were really feeling the pinch for those few weeks when I was so intensely involved. And it had gone for a week longer than I expected it to, by going through that draft stage. But I felt it had to be done. So I was just feeling this relief in getting back into really working, and feeling that I'd taken a difficult job and done about as good a job as I could have on it. I felt that actually in some ways I'd had certain advantages by being an outsider. You know, I could call in a very naive way and ask almost anyone for comments and help, and so forth, and the people in Boston trusted me; I could still talk to Josh Lederberg or Hatch Echols. Both ends of the spectrum, at least to some degree, were willing to talk and willing to communicate their ideas in a detail that they probably wouldn't have in talking to each other, because they would have a certain assumed, preconceived frame of reference. So Josh, for instance, had had to explain his thoughts in detail to me in a way that he might not have done just in talking to Hogness, even if Hogness had asked him about things like that, which he would have no reason to do, particularly.

So I was feeling really high. Just a few days later, the thing in Science came out. It made me particularly upset because during this time when I was so busy, Nick Wade had called several times before our meeting; then, when I was putting it together, I had been really open with him about what was happening and what I saw as our role. I also made the statement that I saw it as something that could go really easily, either way. He was telling me that he saw it as a confrontation; I mean, he came in,

picking up the short thing from Nature, saying, "What is this battle?"--insisting that it was going to be a battle, and a very intense battle. I told him, "I don't think it has to be, from the kinds of things that are happening; I think it's going to be very possible for it to be done in a strong and balanced way." I said, "I think it's going to be touchy. I think it's going to have the potential for blowing up." I told him that I felt that the way he wrote about it could really affect what happened. Some of the things he was talking about saying emphasized confrontation, and I said, "I don't think those are valid, first of all; and, secondly, I think presenting it that way, in that kind of inflammatory way, would make our job incredibly much more difficult at La Jolla." We talked a lot about it, and whatever he had questions about, he called back. I must have spent six hours, altogether, with him on the phone. He also had said that anything he quoted from me he'd check with me. The first time I knew that the article was in press was when he called on a Tuesday and said that the article had gone to press on Monday and he'd forgotten to call to check quotes. And he was calling to apologize for that. I had a little twinge of "Uh-oh!" But I said, "Well, you know, I'm really disappointed that you didn't do that, but I'll be interested in seeing the article." I said, "You promised, and I think that it's unfair to not do that if you promised, but--" And he said, "Well, I realized that, and I was just really busy. And I just forgot."

Then it was the next week, when the article came out. November 21, I think it was. Here it was--this incredible thing, with all sorts of statements taken out of context, taking the containment conditions in the guidelines and oversimplifying them to the point where they clearly were

utterly ludicrous. Just really an attack on the whole process and making it sound like I was contradicting Stetten, for example, by taking something I had said totally out of context and dropping half the sentence. I'd made the statement that those guidelines were not going to be implemented at this time, so he quoted me as saying that those guidelines were not going to be implemented and put that in juxtaposition with Stetten's statement that those guidelines from Woods Hole were serving as the basis for the new discussion, which they clearly were. There was no disagreement. Everyone I talked to had been misquoted, and quoted out of context. The whole thing was twisted in such a way as to make an incredibly inflammatory thing. I felt really kind of personally attacked, and very insecure, too. The picture it painted of my involvement with it was something which sounded as if I had picked up Szybalski in a bar at Montebello because I wanted to somehow gain influence on the committee; gone on my own as an observer to the first meeting because I was upset enough to go there, was what it rather sounded like. It called me an observer there, at any rate, not a consultant. Then, somehow, [I had] been pulled out of thin air by Stetten in July--in fact, it didn't mention that I'd been at the May meeting--as if I'd been an observer and this dissenting observer had been pulled out of the blue by Stetten to head a new group. Then it did funny plays on things that other people pointed out to me, that I didn't catch at the time; for instance, it talked about the fact that I had been put on, in effect, as a lay member of the committee. And then it used exactly the words of the Tang commercial to say, "But she has a PhD in biophysics, and she works with phage." The woman in the Tang commercial gets up and says, "I have a PhD in biophysics and I work with enzymes, but--" [Laughter]

Weiner: You think that was his intention? I mean, it never occurred to me.

Kutter: I mean, about four or five different people pointed it out to me.

Weiner: That's very funny. [Laughter]

Kutter: If he didn't do it on purpose, he very well could have. It was very much in keeping with the whole sort of putdown.

Weiner: Do you think other people reacted to that article in total in the same way, people who understood the issues?

Kutter: I clearly reacted more strongly than most people, but a lot of people called me afterwards and said, "Aren't you angry about that article in Science?" I was so furious that I called Nick and chewed him out for half an hour on the phone. I said that I was really upset with what he'd done. And he said, "Well, our job is to report things as we see them." I said, "You haven't done that. You've twisted it. You've made really distorted journalism." My feeling at that moment was that that article had blown any credibility I had, and any chance at compromise; it painted me as an arch-dissident. I was pretty exhausted at that point from all the energy that I'd put into it, and sort of felt that kind of a reception that everything had been blown, that all the energy I'd put into it had been blown. I started questioning, "Well, what am I? Am I somehow horning in on things? Have I been doing that?" You know, "Is there any grain of truth in any of these things?" I started really questioning myself. "What is it that I've subjected my students to this quarter in order to be able to do that?" I'd gone ahead and done the things that had to be done, but I hadn't had the time

that's usually spent here to really work with them on an individual basis, and do a lot of the kinds of things, teaching-wise, that ought to be done. And they were feeling that. They were also feeling some other things. There'd been very few lectures in biology, and that had been preplanned, that there wouldn't be many, fall quarter. But a lot of them interpreted that as being a result of my involvement. And Kaye V. Ladd, the other woman on our faculty team, had wound up taking a really rather tremendous burden of extra work that fall. It was her on whom the major part had fallen, and she's a person who does tend to really push herself in work, and had to drive herself to a point of almost utter exhaustion. So I was really feeling down for a few days. So again, I couldn't function very well in school for a few days.

Weiner: Wasn't this the period when your husband actually left the house?

Kutter: He had actually moved out at the beginning of October. He had built a house over the summer. There had been a period after I got back from the phage meetings when it was supposed to be done any day. The tensions were getting fairly high; it was a very awkward, difficult period, and things kept happening to delay his moving out. Finally, when he couldn't resist continually telling me to keep the house in better order, I finally said, "Get out." [Laughs] And he hurried it up, and was out shortly. But it was personally, still a pretty difficult period. In a sense, the intensity of involvement in the recombinant DNA thing probably helped keep me from doing quite as much brooding during that time. Well, the kids needed a lot of of extra attention, particularly my older son, who was suddenly feeling the

independence of not having Dad's strong arm there to be a strong, dominating influence. He was really testing his independence in every possible way. That was difficult at times, in combination with all the extra work. It was certainly the first time in my life when there ever would have been a possibility of considering trying to do something like that, and it turned into more of a job than I expected, because we wound up doing a more substantial revision than I had intended.

So we had gotten our draft. Actually, our draft must have been out three weeks beforehand because after we got our draft to NIH, for the only time I know of, they got a document out extremely rapidly. And that was Talbot. Bernie Talbot called me before the final--Well, I had sent a copy of the draft that I sent to the people on the subcommittee also to Stetten. Before he even got the actual copy of the final thing, he was starting [to] try to put together something which went through, item by item--what did they call it--

Weiner: The variorum.

Kutter: The variorum edition, which went through, item by item, including all three versions of every item.

Weiner: You mean, Hogness, Woods Hole and Kutter.

Kutter: Hogness and Woods Hole and Kutter. [Laughs] And he got that put together; they got that printed up and got that out to all of us about a week and a half before the meeting. That was after he got my draft. If only they could have gotten some of the other things out as fast as that. [Laughs]

But that was the one point where they were really kind of running scared about what was going to happen at La Jolla. I was really worried that that article would have completely blown things. I think, actually, what turned out happening was that they so grossly misquoted everybody that it almost had the effect, to some degree, of drawing people together. It also had the effect that there was a tremendous turnout of press and representatives from every conceivable agency, and scientists, and so forth at the meeting at La Jolla. Everything was represented, from Time magazine to Science and Nature to Rolling Stone; to the Los Angeles Times; and Environmental Protection Agency and NIOSH, and the National Endowment for the Humanities. Laughs (They have a program on science and society, or something like that.) And there were representatives from EMBO.

Weiner: Tooze was there.

Kutter: Yes, and from Britain and Canada. And, I think that the controversial nature of that article in Science, and all of those people bristling--the same sort of things drew the number of people that came and probably were whetted on by that Science article, that draws people to heavyweight boxing. [Laughter] And I think it wound up with a slight degree, at least, of pulling together in solidarity. It wound up as a meeting which I think was incredibly productive at La Jolla.

-- Interview interrupted --

Weiner: We're resuming now on the 22nd of May, picking up where we left off yesterday, which was on your way to La Jolla, essentially. The first question I want to ask you about it is what your expectation was about the meeting.

Did you have any feelings about whether it would be a knock-down-drag-out battle, as the newspaper and other magazine articles stated? How did you feel that your guidelines would be accepted?

Kutter: I really didn't know. I didn't feel a fear of a knock-down-drag-out battle, and I was very glad that the meeting on safer cloning vehicles was coming before the main part of the La Jolla meeting, because that gave at least some people on the committee a chance to be together for those few days before the meeting and talk about some of the data and issues and facts that had been relatively unavailable, and made writing the guidelines much more difficult. So, I felt that the tone of the actual La Jolla meeting would be affected by what went on during those few days before the start of the meeting. I had hoped that most of the committee would be there. As it turned out, most of the people that I most think should have been there, weren't. All of the people who were really involved directly with preparing vectors were there: Stan Falkow, Don Helinski, and Waclaw Szybalski. Hogness came down for a little while at the very end, and most of the other people from the committee weren't at that first three-day session, so they didn't get quite this intense amount of information. I thought at first that I wouldn't be able to go, and in a way, I probably shouldn't have. It meant skipping quite a bit of school, but I decided that in order to be well-enough informed to do my job properly that was something that I had to do, to go to that. And all of us were invited, and were basically supported during that period if we were willing to go--no consultant's fees, of course, or anything like that. So it was just a matter of being willing to take the time. I decided that it was important.

I had a few days of relaxation beforehand that I think helped me a lot. I took the train with my boys down to San Francisco just before Thanksgiving, the week before, and then spent Thanksgiving with good friends down there. I talked with some of the people at Berkeley who had been concerned at both extremes, including people like Hatch Echols, and sort of got a bit calmer in my own mind, and went with the kids on down to Disneyland with some friends from Berkeley. Then they went back up and stayed with other friends in Sacramento while I went on down to the meeting. That would have been impractical to have had their assistance there; but I had a few days of just being able to really relax with them beforehand, and that helped my mental state very substantially.

The three days at the meeting on safer vectors were also really both interesting and relaxing for me. The data looked encouraging, particularly the data indicating that in those cases where it had been very carefully tested, where they'd taken a mixture of all the clones from Drosophila, for instance, and mixed them with a wild type--these experiments had particularly been done with lambda Xenopus clones. They never saw any of the clones being able to compete, even if they put a very, very small fraction of wild-type lambda, one in 10^4 or 10^5, that by the end of twenty passages with the plasmids they had essentially nothing but wild-type lambda as detected by looking at bands on gels. So that kind of data was the kind of data that had been missing earlier in our deliberations of things taking over, and at least was encouraging. The obvious difficulty of getting accurate translation in making proteins from eukaryotic genes in prokaryotes was encouraging. It didn't look like you were going to get E. coli that were going to make

massive amounts of some hormone. I know it's not encouraging for those people who want to use them for specific things, but it seemed to imply that even if these got transferred somehow to a normal resident E. coli, that it would be unlikely to make a harmful product in significant quantity. So I felt a little bit more comfortable on the basis of that data, though there's still worlds of data to get. I could mainly be a listener; for once I said almost nothing.

I also then spent one afternoon over at the university and gave a seminar on my own research, which was a good change, too, from all the intense involvement with recombinant DNA all fall long.

So I was pretty well relaxed by Wednesday when people started getting there. Initially, Stetten had wanted me to lead the meeting, as Hogness had chaired the meeting at Woods Hole. Or he'd spoken with me about that and about meeting with me the Wednesday evening before. Then when all the flak started coming up, a lot of people who were clearly concerned about the process, and the edition came out then that contained all of the documents, there was talk first about meeting with Dave and me and a few of the other people. Then it got broadened to say that it might be really smart for the whole committee to meet privately Wednesday evening. This was something that people outside the committee weren't told about. It just consisted of the committee and those people who had been especially invited as consultants, people like Joe Sambrook, Sydney Brenner, and Maxine Singer. There was some talk about ideas and policy, but more, it was a chance for us just to all get together and relax and talk, and feel more comfortable with each other again, and a little discussion about the way in which the meeting would be run.

It wasn't at all a closed-door meeting making policy decisions, but more a feeling out the tenor of what was happening. The statement was very clearly made, then, that the world would be watching the next two days and that there were going to be a lot of people there from the press, from other agencies, and so forth. It was going to be a very public meeting, and the suggestion was made that we really think carefully about what we were saying and doing; I think that was an important thing. I think, in that sense, the presence of such a wide range of observers really did have a very strongly positive effect on what was happening the next day, too.

Weiner: Let me ask what you mean by that--to watch what you're saying, because the press may get the wrong idea? About what?

Kutter: This probably is a whole combination of statements I shouldn't try to make, because I can't say in terms of very specific details. It was more a sense that the kind of thing that Science had exploited, looking at strong divisions, much stronger than Stetten felt were really there or needful, and that if we could talk in a temperate way, very clearly listening to each other, it would be picked up in one way, and if there was clearly a knock-down-drag-out fight about what was happening, then it would be picked up in a very different way. It would be much harder to win final general support for the kinds of guidelines that came out. It was never made more specific. That's just the sense of what was being said.

Weiner: Wouldn't that be a desirable way to conduct committee business, even without the press's presence?

Kutter: Oh, of course it would. [Laughs] But what I'm saying is that the excuse of the press being present was being used as a gentle reminder that there was this added advantage. I think that the people at NIH were rather concerned. I actually don't think it would have been that knock-down-drag-out kind of a meeting. In retrospect, I don't think it would have been that even without [the press] . But there was the decision of Fredrickson, first to come and then not to come. There were a lot of things that indicated that NIH was very concerned about the tone of things after the tenor of some of the letters that had come in from committee members and the article in Science. They weren't quite sure just what to expect and were trying to gentle us along a bit, perhaps. I question, in retrospect, whether that kind of statement really would have been necessary. I don't like the way this is sounding, because it's coming out with a different feeling than I had.

Weiner: No, I think it's good, and I think you're clarifying.

Kutter: But it was more a chance to get together and relax together.

Weiner: Yes. Wouldn't you say that it amounted to a briefing by the NIH people about the nature of the meeting, and the atmosphere in which it would be conducted, and what the procedures might be? Not a caucus in the sense of determining policy or position, but just to try to maximize what was expected.

Kutter: Yes, yes. There was some of that, and some general discussion about the way to conduct the meeting. One thing that was decided then and repeated the next morning was that we would simply go through the variorum from beginning

to end, but we wouldn't try to stop and vote necessarily on every small change. He would simply ask where the things were that people wanted to discuss. Starting from beginning to end, we'd stop only at those things. The more trivial changes in the document, many of which were not much more than semantic, would be left to the discretion of Stetten. So that policy decision was basically made, which I think was a very important one. I would say that more than anything it was a chance to all interact with each other, and talk with each other as human beings, and it was a very nice, convivial kind of evening, with Sydney entertaining . . . [Laughs] But just a way of defusing what might have been a lot of tension, if the first time we'd seen each other was in front of a room full of people that had come, as I said before, to watch a prize fight.

Weiner: How long did this evening session last?

Kutter: It was a dinner. And I think it was John Spizizen whose institute [Scripps Institute] offered to foot the bill and invite us for a steak dinner. It was over by nine o'clock, or so; it wasn't a late evening thing.

Weiner: In a private diningroom some place.

Kutter: Yes. In a private diningroom there; in fact, I think it was the room where the meetings were held the next day.

Weiner: Was there any caucusing, any discussions later on specifically about the guidelines? And were you able to pick up what some of the problems might be that evening or that afternoon?

Kutter: I wasn't particularly involved in anything like that. Charlie Thomas wasn't there yet. He didn't get there until the middle of the morning of the next day. And Jane Setlow got there just at the very end. So two of the people who were likely to have the strongest objections weren't there yet. I didn't pick up any sense of caucusing or any kind of drawing sides carefully, or anything like that. There was also no particular politicking, or trying to win votes at that point, or saying, "Hey, I'm going to push this. You ought to." It was really very open, going into the meeting. I saw people sitting around, just talking in a convivial way. I wound up taking a walk with several people down along the beach; there was basically a fairly good feeling.

Weiner: Did you feel a significant change regarding your own status within the committee?

Kutter: Oh, I felt pretty accepted. I felt a little bit of hostility from a few people, which in itself was a positive sign, but not a serious hostility. I felt much more a part of the group than I had before. And you know, it helped again, having been there for those two or three days; it had sort of grown over that period of time. People at the meeting had come up and talked to me as a member of the committee and asked questions and given input. My own feeling, my own sense of professionalism or competence had grown a lot in the time in between. I'd sort of gotten over the anger and depression I'd felt after the Science thing came out, and had much more of a wait-and-see attitude. It had brought me back down to life-size in the sense of not having any incredible expectations, and yet at the same time

feeling that things were going to proceed in a reasonable way. At this meeting, as opposed to Woods Hole, there were a lot more people who basically held similar philosophies. Wally Rowe was there; he had arranged to come to that meeting even though he doesn't normally travel; and Don Helinski, of course, and Stan Falkow, and Roy Curtiss. So I really didn't feel the same kind of burden of [having to] defend my side; I mean, I felt that I was one of a group, rather than somehow isolated.

Weiner: You weren't treated with that same patronizing tolerance.

Kutter: No. [Laughs] No, that seemed to be completely gone.

Weiner: So the next day the meeting started, and I don't know the best way to treat it except the way you think is most appropriate, what you think is worth talking about.

Kutter: You've probably heard a lot of stories about it, details about it from other people, and you have the complete transcripts. You taped the meeting.

Weiner: Well, we have our own tapes, and also the NIH transcript.

Kutter: Yes. So in that sense it would be a mistake to try to go into too much detail about what happened there. Initially the tone was very good, and we got through the whole first introductory section very quickly. It seemed that on a large number of points, the draft that we had put together was the one that was accepted. On some key points later on it was very much fought, and in some cases rejected, but particularly on the early points, in

terms of the introductory sections and the general definitions of EK2, it was decided that so much information had come out of the meeting from the three days before that that ought to be revised, strengthened and lengthened, taking advantage of all of that information. So that was postponed, to be discussed the next day, with Roy, Don and Stan working on it that evening.

Then we started getting more into specifics of classifications, and so forth, and by that point Charlie Thomas had also arrived, which inevitably has a certain effect on the tone of any meeting. I think that that whole first afternoon was very negative in a lot of ways. There was a lot of, sort of, bitterness flying back and forth, a lot of non-communication going on. Charlie has a way of haggling for a long time over very small points and then retreating gracefully, then sort of pushing through something rather major as if it were trivial and obvious. That was going on quite a bit. There were times when I felt I was able to say things effectively, and there were other times when I had a strong feeling that no one was listening. But now, I think that was true not just for me but for almost everyone on the committee. It got into a period of non-listening and non-communication that afternoon. The biggest controversies came over the whole question of what the classification should be of the various kinds of eukaryotic species into E. coli. What we had suggested was moving the primates up to P4-EK2 instead of P3-EK2. That was accepted on a split vote. Then it was suggested, that since with primates one of the main concerns was viruses that might be carried, that embryonic material be placed in P3-EK2 instead of P4-EK2. That was a point that was then defended by Wally Rowe, with people listening very carefully, and [it] wound up passing. That came at a point when

almost everything was bringing tension; there was an incredible amount of tension back and forth in the whole thing. Nobody was feeling very comfortable with what was going on, and we very much needed something to break the air. That was provided by a combination of Brenner and Stetten. When Wally was talking about the embryos Brenner raised his hand laconically and asked, "And where do you place sperm?" And Stetten, with more quickness than I would have realized [laughs], before any of us had caught anything strange in that remark, said, "I don't think I'll let him answer that. That's a highly personal question." [Laughter] And that brought down the house. A good, solid laugh was exactly what we needed right then. That was very, very good for the whole tone.

Weiner: Wasn't there another line? Where did that other line come from?

Kutter: Well, Brenner was sitting sort of behind Wally, and Wally told me about this other line afterwards, that Sydney was sitting there, sort of mumbling under his breath and saying, "They ought to make everybody use their own sperm. That would keep the bloody women out of the business." But that's not what they'll have in the public transcript.

Weiner: So that came late in the day, and--

Kutter: Sort of mid-afternoon, and after that there were further decisions, several of which were very bitterly fought and very controversial. I remember at one point Dave looking over at me--Dave and I were sitting next to each other--saying, "You don't compromise on anything, do you." And I said, "That's not true, Dave." Something that I was arguing a lot back and forth;

oh, I know where that came. We started getting down to the question of lower eukaryotes, and cold-blooded vertebrates was the main matter at issue. They had been left in the Hogness draft at P2-EK1, and it seemed that going all the way down to that was really too low, so what we had suggested was P2-EK2 for cold-blooded vertebrates, and then lower eukaryotes being below that, although there was also an argument over insects. But the cold-blooded vertebrates were passed to be at P2-EK2, and passed on a fairly narrow split vote. That was the one place where Hogness kept arguing and arguing, and arguing and arguing, feeling that that was really a big mistake to move them up to that, that certain people's work would be very definitely impinged on, unnecessarily, by doing that.

Weiner: Did he have in mind whose?

Kutter: Oh, he named names.

Weiner: Don Brown?

Kutter: Yes, and a couple of other people. When Dave did lose that vote, he was very angry. But then he suggested, well, at least in the interest of uniformity, he thought that we should have embryonic tissue at P2-EK1. Now in retrospect, I'm not quite sure why he said "in the interest of uniformity" because with non-primate mammals we hadn't put embryonic tissue in a lower level. That's something that was never brought up, and that I haven't talked with anybody about since. But this seemed to be an issue that was continually raised and that people fought, to have a lower classification for embryonic tissue. What several of us said right away was,

"What do you mean? First of all, there's not the same question of viruses, horizontal transmission of viruses that might affect humans that you're worried about here. So what additional safety do you get from using embryos? How do you expect to have a frog embryo, which is a thing free in pond water, to be somehow a particularly clean thing to use?" These very definite logical inconsistencies were brought up by several of us, but question was called for almost immediately.

Weiner: By Stetten?

Kutter: No. Someone else calls for question; it's not Stetten.

Weiner: Oh, I see what you mean.

Kutter: I think it was Charlie Thomas--Charlie, or Jane, I don't remember--who called for the question right away, and that thing was passed. I couldn't believe it. I really felt that that was rather railroading. Oh, wait; I've gotten confused. The time when that discussion--this thing that we had moved up to P2-EK2, with the embryos still at P2-EK1, happened the next morning. What happened in the first discussion that late afternoon, was that the whole thing got left at P2-EK1.

Weiner: I see.

Kutter: That's right. It went back to the original Hogness version. It was again a fairly narrow split vote. A bit later, John Littlefield, who had voted for leaving it at P2-EK1, and Ernie Chu, both started thinking about that and a couple of other things that had been passed in the guidelines

and making comments about feeling uncomfortable about what they had done, about their votes.

Weiner: The previous day.

Kutter: Well, no, this is now about an hour later in the afternoon, after the P2-EK1 vote. You better switch this transcript around to put it in the right order. This is still Thursday afternoon. Just about the time we were quitting, John Littlefield was starting to raise this question again, and thinking about reopening it.

There was a reception that evening at McElroy's house. He'd also had a reception for the group that was there earlier in the week for the other meeting, and very nicely laid open his house to us. [I] got to talk to his wife, who's a physiologist working with firefly luciferase and things like that; very interesting people, and very, very hospitable. It was a nice place to have it, both times.

BEGIN TAPE FIVE, SIDE TWO

Kutter: I'll just repeat it. There was a lot of going around, people discussing issues with people. This was the first time I'd seen this kind of open, almost politicking. And, particularly, I think John Littlefield was the most involved in that. Stan Falkow later told me that Nick Wade had come up to him and said, "See, I wrote the right thing in my article. This is going to turn out to be an absolute fiasco, worse, even, than Woods Hole." Because at that point, in terms of the containment things in the guidelines, that's about what it was coming out to. Falkow told him, "It's

going to work out okay." But there was a lot of discussing back and forth about issues, and some of the people who had been relatively quiet on the committee--and there I particularly mean John and Ernie--were being very much more concerned about what they were doing. In fact, at one point I remember hearing John arguing--I'm trying to think of whether it was with Jane or with Charlie--and it was in this utterly hopeless kind of situation, in terms of polarization of views. But there was very definitely more discussion going on in private than I had ever heard before between the various dissenting opinions on the committee. As a result of a lot of the discussion that had gone on that evening and some of the changes of feeling by people who had voted on this narrow split vote, the next morning the issue was reopened of whether cold-blooded vertebrates should be P2-EK1 or P2-EK2, and Hogness, particularly, and Charlie Thomas, were very opposed to reopening the discussion on that and even more opposed when the voting went [in] the opposite direction. It was after that, then, that the embryonic DNA for cold-blooded vertebrates got in.

In my personal opinion, that's something that they tried to push again to still leave in at the final meeting in Washington, D.C., and on the vote, did still get left in, even at the meeting in Washington, D.C., but that I hope will be thrown out by Fredrickson because it's a complete logical inconsistency. [It] became very clear in the arguments that were made to defend that position that that was a logical inconsistency, and yet the vote still went that way. That's something that I really don't understand. That's noteworthy, though, for two reasons: because of the apparent illogic of the position and the lack of listening to data, and it's also noteworthy because

that's one of the very, very few cases in the whole guidelines that I can see that going on, that kind of ignoring one's senses.

Weiner: Do you feel that the motivation is special interest in this case, that it is protection of a class of experiments either they or colleagues had planned, or are doing?

Kutter: Yes. There are certainly no experiments that they have planned or are doing; there are colleagues who have planned and are doing it. And that's certainly involved. I don't think that they were intentionally ignoring the evidence, but I think their strong feelings about the value and safety of these other experiments, the specific experiments that they had in mind, were interfering with their sense of logic of what was happening.

Weiner: Let me ask about the previous evening, Thursday evening. How long did this reception last, where all of this politicking and deep issue discussion was going on?

Kutter: About nine-thirty. After that was when several of the people were working on the EK2 part. Different people were working on special parts after that. Roy worked essentially all night, and the other two worked with him until about two; I think some of the other groups that had some things to write worked quite late also. It was a hard-working meeting.

Weiner: Yes, okay. But at nine-thirty, they left McElroy's house. What time did you come? After dinner?

Kutter: No, we were invited for cocktails at five-thirty, I think it was, but it turned out to be an entire buffet dinner. Beautifully done buffet, with very elegant food and a curry with lots of sauces, and just a very, very nicely done, very, very hospitable dinner, which was the same thing they had done at the reception earlier in the week for the other group. It was an even nicer dinner; it was different food the two times.

Weiner: And this also included the observers and press?

Kutter: Yes.

Weiner: So that everyone present at the meeting was invited. Was that it?

Kutter: There were invitations, but most of the people that I had seen at the meeting were there.

Weiner: Well, so the four hours really provided time to have a chance for the kinds of discussion you mentioned--

Kutter: Yes, very much so. Some people left earlier than that. It was supposed to be from five-thirty to seven-thirty, but it was at least nine before half the people left.

Weiner: Well, it just gives me more of the atmosphere.

Kutter: Yes, yes. And, again, it wasn't an arm-twisting kind of arguments; it was trying to use reason and logic to convince people.

Weiner: Did you have any interaction with Nick Wade that meeting?

Kutter: Almost none. That was where I met him for the first time, and I treated him largely with stoney silence, I'm afraid. [Laughs] I talked with him for a couple of minutes, but I didn't make any attempt to get to know him better. Actually, I did wind up sitting at lunch, one day, with him and a couple of other people, and just listening. A lot of my hostility had decreased somewhat. I think that since then, he's done a very good job with the articles that he's written. He's been at the meetings. He's had more of a sense of what was going on. He was reporting things that were really happening, and he may have realized . . . I don't know whether he would agree now that the first article was inaccurate and intemperate, but he has much more feeling for what's really going on.

Weiner: Well, with this turn of events on Friday morning, what was the rest of the day like, in terms of the tone and the over-all accomplishments?

Kutter: Well, things were still fairly bitter that morning. Then we were getting to other things, for example, particularly the question of partially-purified DNA and whether it should be left in, and that was where something happened that sort of changed the tone for about half of that day that I'll try to report as accurately as I can, at least in terms of my sense of it. It would be interesting to check it back against the tapes of exactly what happened.

Dave Hogness said that there really was a significant safety factor even in only partially-purified DNA because you could work with just a few clones instead of a large number, to screen for something. And he said if

you can purify DNA such that you know that only one part in 10^4 is anything else, that's a lot safer. And I said, "Sure, all of us would agree with that, but my understanding is that you can't do that. You just can't check that." And that was what I'd gotten from Paul and from Josh, and from a lot of other people. And he said, "Well, there are a couple of instances now where things can be purified to that extent and we ought to leave room in the guidelines at least to encourage that kind of good science." I said, "If that's what you're looking at, I didn't think it had to be even one part in 10^4. If you had it one part in 10^3, say, I'd feel perfectly comfortable with only one part in 10^3 being something else." (He had said "one part in 10^4.") So people seemed to agree on that. And over a break, Charlie Thomas and--I've forgotten who it was, it may have been Stan Cohen--were asked to draft a statement which would incorporate the statment of this degree of purification and say a little about what the criteria would be in terms of purified.

Well, they came back with a statement which said that if it met certain criteria, if it had been 10^4-fold purified, it would be considered highly-purified DNA. Now that's clearly saying something completely different. If you were starting out with a sequence complexity of 10^7, it could still be only one part in 10^3 that you wanted, a point which I very rapidly pointed out, and a couple of other people did. Then Charlie said, "Well, I think if we just change this wording like this," and made a very small change in the wording, that still said the same thing, "then we're unanimously agreed." That was one point where he had gone too far; everybody caught it, and it was rejected.

Then we went ahead, and wound up with a statement, as I remember, that if something was purified so that it was only one part in a hundred anything else, that you could go down by one, because once you get it to that point, you still need to only work with three or four clones in order to find what you're looking for. Certainly the probability, if you're working with three or four clones, goes down orders of magnitude versus working with ten thousand, to look for a specific thing, even if something co-purifies, so I think most of us felt reasonably comfortable with that. It is just a lowering by one; I mean, I certainly felt that that made at least as much difference as going to embryonic tissue, even in primates, to say nothing of frogs.

My sense of what happened was that with that fiasco, Charlie lost a certain amount of his aura of invulnerability; I'd almost call it a power that he had. I really feel awkward about saying these very personal kinds of things, and yet I think it's very important in terms of the dynamics of the meeting.

Weiner: I think it is. I'm sure the different committee members will see things their own ways, and they'll have the opportunity to record their recollections, too. And there's also the transcript of the meeting itself.

Kutter: Yes, this was my perception of what happened. And I'd be interested in checking that against the transcript.

Weiner: Yes, well that would be a research task, and that is something that we should do, take a good look at the transcript, which we haven't done yet.

Kutter: At any rate, I felt that there was then a period where we made really good and rapid progress. There were some funny things that happened with the tumor virus part, because there were some new data that had come in that were raised; and some subgroup meeting, that included Joe Sambrook, Maxine Singer, and Mal Martin, doing some revising, trying to put in a few specialized conditions where SV40 could be used under P3 where it wasn't making any particles, and so forth. I think that that part was largely in response to a letter that had just come from Dan Nathans, the fellow at Baltimore [Johns Hopkins] that Tom Kelly works with, one of the very well-known people in the field--that indicated certain safety things that could be done. But I think probably there was at least one thing that got in that was a little bit touchy, so anyhow, that was refined to becoming very specific in terms of using SV40 as a vector. Most of the rest of the material was accepted largely in the form in which we had put it together in our draft. There were a lot of areas where there were no major differences between Woods Hole and Hogness and ours, once you got out of these areas where people had a lot of emotional stakes already. There was some reasonably extensive discussion of the plant virus material, with Peter Day still being the only plant virologist. He had been invited as a consultant again to the committee, at my suggestion. Since he had worked on it with me, I had requested that he be invited again, and I'd requested that Joe Sambrook be invited as a consultant. I think that the plant part was still the weakest part, and that's been since taken care of. I don't mean just plant viruses--the whole plant part of it--because we had no one on the committee who knew anything about plants.

Weiner: But you did consult Peter Day, and you got this long letter from him after circulating your version of the guidelines.

Kutter: Yes, we did. And that was definitely a start in the right direction. To jump ahead slightly, there was still some perception that there were things that needed more work, and Fredrickson asked Peter to call together a group that met with us at the meeting in April and made some changes in that part. It was already much better than the original document, I think, as a result of that input, but that had been a fairly hurried thing done mainly by telephone, and without a chance for a broad spectrum of people to look at it. I think that now most parts of the guidelines have been looked at fairly extensively by the people most likely to see loopholes and problems, which I think was a very important part of the final drafting procedure, going beyond the committee to do that. I brought up again some of the questions in terms of implementation that I felt very strongly about. Or actually, in fact I didn't bring those things up, I would just as soon left it to Stetten, but some of the others on the committee brought up those things again, stating that the local biohazards committee should have the authority only to approve facilities, and so forth. Again I was voted down.

Weiner: Let me just ask what else you think is significant in terms of the dynamics of that day. You've mentioned a number of specific things. Was it clear to you at some point in the second day that, in fact, most of the guidelines that you and the others had helped develop for that meeting were going to survive intact?

Kutter: Yes, it was. I began to feel more and more that the things that I cared most about, almost all of them were being accepted. You know there were clearly some points that I felt from the beginning were less likely to survive, but most of them weren't the most essential things. I think the only thing that I wound up still feeling needed strengthening enormously, was the idea of implementation--the local committees and the various things related to that. I talked some with Maxine and other people after the meeting, and I think we ended the meeting feeling really high. We felt it had been productive, that no unreasonable compromises had been made, and that things had gone much better than we'd had any reason to expect them to go. I think Maxine had been a little bit pessimistic coming into the meeting, too. And I remember various groups of us went out for dinner and then afterwards a bunch of us were sitting in the bar having a drink. It included Sydney, Maxine and Stetten and a couple of other people at our booth, and a lot of the rest of the committee at various booths around. There was generally a very optimistic feeling. I didn't talk that much with Charlie Thomas or Dave after the meeting to see whether they felt really uncomfortable with what had happened. I know they would have liked very strongly to have seen it go a different way, but I didn't pick up a sense of anger or bitterness from them either. And I felt pretty high; I felt that we had succeeded beyond my fondest dreams. And that's the way I came home feeling.

Weiner: Let me ask about the three versions of guidelines that were in the variorum edition. When you think about the Hogness draft and the Woods Hole guidelines and the Kutter guidelines, the ones that you prepared, how would

you rate the ones that you prepared in terms of conservative, as the term is used in this business; that is, more stringent. Would you say that of the three, that the ones that you submitted to the meeting were consistently more stringent?

Kutter: There were probably one or two places where we had worked things differently which would have been slightly more flexible, a couple of things in terms of P3 containment facilities, ways of introducing more flexibility; and there are a large number of areas where I'd say that at least the Hogness draft and ours were at a consistent level of being conservative. But in those places where there were major differences, ours was very definitely the more conservative, certainly much more conservative than Woods Hole, and the Hogness draft was also substantially more conservative than Woods Hole, across-the-board where there were major differences.

Weiner: Now, what about the guidelines as modified and as approved at the La Jolla meeting? How do you rate them compared to those three other versions that we were just talking about?

Kutter: Well, they're clearly more conservative than the Hogness or Woods Hole drafts. I don't like that kind of linear spectrum at all, but if you're trying to put it on it, a few areas a little bit less conservative than the draft we had put together, but relatively few; particularly, that was in the area of implementation and the power of the Biohazards Committee. And with that one exception, even though I would have disagreed with some of them and would have done it differently, I didn't feel in any way threatened or uncomfortable with the version that came out. I really felt

that it was at least as conservative a document as could possibly have been expected, and one which met most of the objections of the critics. You know, when you're dealing with this whole area where all of the dangers are purely speculative, imagined, as well as the benefits, there's a lot of room for flexibility. But there was nothing in it that I felt was just inviting disaster or a serious problem, or seriously uncomfortable with. Except for one other thing I felt really uncomfortable with was that P2-EK1 for frog embryos. Those were the two things that I was uncomfortable with in a significant way. Considering the length and the number of people involved in the process, and so forth, I was impressed that it could work as well as it did.

Weiner: So you were feeling good.

Kutter: Yes. And the very short articles that came out in Nature and Science immediately after that reflected that same kind of optimism. "By golly, they did it," [laughs] was the sense that I got out of Nick Wade's.

Weiner: What happened when you got back in terms of these issues? Did you feel that, "Well, that's it for awhile, and we can get back to normal, and we don't have to worry," or were there a lot of follow-ups that you were involved in?

Kutter: No, I felt that I could very much get back to normal. I had to. I had a lot of work that had really been let go. I had some problems that had to be straightened out with the program that I supposedly had been coordinating all fall. I just really got into my teaching, and talked periodically

with Wally Rowe and others on the committee about some of the things that were happening. He was right in the middle of things and would periodically call me for advice or just to think out an idea, because he was in the place where Stetten and Fredrickson, and so forth, were continually calling on him for advice. And so in that way, I kept in touch with what was going on.

Weiner: This activity in NIH was a follow-up on the fine details, is that right?

Kutter: Part of it was a follow-up on that. Part of it was the growing feeling on Fredrickson's part, partly for political reasons, partly I think simply because it seemed right, that there ought to be some opportunity for input from a much broader community in terms of whether this method of approach, these guidelines, the whole support of the research by NIH, was appropriate. And my initial reaction was, "Oh God, not another delay," because I think all of us felt that there was a great deal of urgency in getting some guidelines out before a lot of things just began to be done willy-nilly without waiting for any kind of guidelines. But as I thought about it, and even more in retrospect, I think that was very perceptive on the part of Fredrickson. I think it should have been possible to have done that a few weeks earlier than it was done and not stretched things out as long as they were stretched out. But that would imply some procedure for putting a high priority on the draft and their own printing presses, and getting it out to people fast. Somehow the wheels at NIH just don't seem to turn that way. The variorum edition is the only place where I've ever seen that done.

In this case, what happened was that they put together the draft of the guidelines, and they had to get certain more information from Roy Curtiss on EK2 and from some of the other people. So they sent out a draft within a couple of weeks after La Jolla, three weeks maybe; got feedback from all of us on that draft; put that into a final La Jolla version, the first copy of which I got from the NARSM, the Nucleic Acid Research Scientific Memoranda. They put it out both in that form and sent it to the committee separately at the same time. I don't know why they didn't just send us the NARSM version because it was printed in nice, small type in a thing that was reasonable-sized to be carried around, and the other one [laughs] was about three times the mass for no more information, and came two days later.

Weiner: I noticed in a letter that Roy Curtiss sent to Gartland, it was January 12th, '76, and he was still providing comments on the draft. It recommended revisions of the minutes, guidelines, and summary. And so, I imagine that that process was going on for quite awhile.

Kutter: Actually it may have been January 1st before we got that first draft out. It took them a long time. I don't understand why it took them that long to get a draft to us on the committee.

Weiner: Well, if the comments were as extensive as his, and generally they're not—it was a nine-page letter--

Kutter: Oh, well, yes. I'm not talking about the one after January.

Weiner: Oh, you mean just the direct result of La Jolla.

Kutter: It seemed the direct result of La Jolla should have gotten to us, you know, longer before Christmas. If it had gotten [here] two weeks earlier, it would have saved a month. That's one place, and there were one or two earlier times when I think that--I realize NIH is doing a lot of other things, but I think that giving some kind of priority at the printing press would [have] been in order there. The fact that they can do it, is clear from the fact that the very extensively-compiled variorum came out--I mean it was less than a week from the time they had my draft to the time that that was off the presses. So they can do it.

Weiner: Right. That was a staff thing, a staff comparison of the variorum, right?

Kutter: Right. Well, the first thing, too, from La Jolla was a matter of Gartland putting together everything from the minutes.

Weiner: Were you invited to the February meeting that Fredrickson called, his ad hoc advisory group?

Kutter: No, I wasn't. Intentionally, just a couple of people from the committee were invited. Well, Hogness was invited, and perhaps it might have made sense to invite me for our draft. Wally Rowe was there who had worked also with us, and it would have been a hard time for me to get away again that quarter.

Weiner: Well, Roy Curtiss was there, too.

Kutter: And Roy Curtiss was there. Yes.

Weiner: He and Hogness specifically reported on the committee.

Kutter: Right, and, you know, that made much more sense that way; it wasn't the whole committee there, and it looked to me like they did select a wide range of people. I would just as soon [have] seen a taxi driver or an auto worker there.

Weiner: Were you in touch with people other than the ones you mentioned, the ones from NIH and Wally Rowe, and others who were working on these problems, in the period after La Jolla?

Kutter: Yes. I talked with Rich Goldstein fairly extensively afterwards and told him I felt pretty comfortable. He and his group had prepared an extensive thing to try to help the discussion at La Jolla. Unfortunately, they spent so long preparing it that it didn't come out until I got there, so it couldn't have any effect on our draft of the guidelines, or really on the discussion at La Jolla. There was so much material added at that point, anything new that came in really couldn't be taken in. It was the same with Roy Curtiss's EK2 at the next meeting in April. We couldn't discuss it simply because we didn't get it 'til we got to the meeting, and there was too much volume to really deal effectively with anything new.

Weiner: Do you think it would have made any difference had it reached--

Kutter: Well, I think if it had reached us in time before putting together our things, there were some points they'd made that we could have tightened

up a bit. I don't know whether it would have made any difference in the in-between thing that had been in none of the three versions. It was hard to deal with things that were not in any of the three versions but [were] new alternatives. A lot of his points were more philosophical discussions that would involve, perhaps, broader shifts than just a change of two words here and there.

Weiner: Well, that's what I mean. Would there have been any constituency within the committee to make those major shifts? Or was the committee already committed to--

Kutter: No, I think that there are a lot of the things that they suggested that wouldn't have been changed, but I think that some of the things they pointed out in terms of shifts in tone might possibly have been incorporated. There were some points they made that I think have finally been taken more into consideration.

Weiner: As a result of their participation in the February meeting?

Kutter: Yes, as a result of their participation in Fredrickson's meeting. I think that was very important that they did it. I think one big problem, in terms of trying to get people like that to participate in the February meeting, was that they were so late getting the version of the guidelines out. They apparently sent them to the members of the committee, themselves, xeroxing them earlier. But three days before the meeting when I talked with Rich, still no one there had a copy. No one among that group had a copy of the guidelines yet, three or four days ahead, and yet they were

supposed to prepare a written statement. There were no funds at all available to bring any of these people in to testify. Those that came and testified had to go completely on their own time and their own funds. It seems to me that in order to really get the kind of broad representation of outsiders that was needed at that meeting, a few of those people who had really spent a lot of time and energy in an unpaid way should have been invited, very clearly invited, and they should have been supported and brought there.

Weiner: Were you consulted by the NIH people on that issue prior to the February meeting, of who might be invited and how should this participation be arranged?

Kutter: Again, I was talking periodically with Wally, who was involved in the discussions there. Particularly after I'd found out from Rich what was going on with the payment, I told Wally that I thought that was really a mistake. Rich very nearly didn't go, and decided only at the very last minute to go. He had three or four lectures that he had to do that week. It was an intense personal burden on him, as well as a financial burden. It wound up that they passed the hat at several meetings to help the two graduate students that went, and Rich paid it out of his own pocket. I know that he really can't afford that kind of thing. I think that it's very good that there have been a few people who've been willing to make that kind of time and energy commitment, time, energy and money, even, to do it, but I think that expecting that is a mistake and I think that it would have been very possible to identify a few people like that whose

way should have been paid.

Weiner: Who else were you in touch with in this period after La Jolla? Or, first of all, did you speak with people at the university about it? Did you give any reports back, either to students or faculty?

Kutter: When I got back I gave a brief report to the students and faculty in my own program, and a lecture about what had happened. They hadn't kept too closely in touch. There'd been a certain amount of ambiguity that had developed in their feelings about what I was doing. They, on the one hand, felt that it ought to be something important, and gradually as time has passed since, they've gotten more of a sense of what was going on. However, they also knew that it was taking a significant amount of time that they would have liked to have had, and, in effect, were paying for. I still had a lot of time to work with them, but there were certain dissensions that were developing in the college as a whole and in the program relating to the whole issue of governance, and who decides policies, and so forth. I think the one feeling was that I probably should have done it, but I should have asked the students first--told them what I was thinking about doing and told them what kind of a demand it would make, rather than simply telling them I was doing it. I think that would, in retrospect, have been at least a politic thing.

Weiner: You did ask the faculty, though?

Kutter: Yes, the faculty I was with I had asked, but it's an interesting thing in terms of balances of student-faculty interests. These governance

questions at Evergreen are relatively irrelevant to that. But it happened that that came to a sort of a big thing on campus and a two-day teach-in kind of thing, and all, just at the time I was leaving for La Jolla. Then somebody from a smaller radio station called KZAM in Seattle came down and interviewed me and Stan Falkow--this was, I think in January--and ran it on the radio repeatedly. The first time I knew that it was actually on, was that several students came in and said, "I heard you on the radio this morning!"

Weiner: Well, when was the interview, and when was the broadcast?

Kutter: I think in January, sometime, as I remember. I never heard the broadcast.

Weiner: Are you going to try to get a tape of it?

Kutter: Yes. I'll try to get it for you.

Weiner: So there was that more public involvement. Any other public involvement or newspapers or--There was a story that I have--a little thing on you from your file; that was somewhat later, though.

Kutter: Well, for the story that you have, she talked with me in November, and then the story came out much later. The story had to do only with the early things. There were stories that came out, of course, in the L.A. Times on La Jolla, and the reporter there sent me a copy. But I was able, except for these indirect links, to relatively drop out of sight for a few months. I sort of felt that it had been my ball for awhile, I'd

carried it, and now I'd passed the ball on; I clearly still had roles but that from now on, my role was simply as a member of the committee and not the same kind of major role that I had had. That was true between the two sets of meetings. I also talked to Rich a couple of times. I heard about the meeting that was going on at the University of Michigan, talked briefly with some of the people involved on the phone, but again had no major role.

During that time I was asked to go down in April to Berkeley. The biophysics students at Berkeley had decided to have their annual symposium on genetic engineering and invited me to be one of the three speakers. The invitation was made in February, and I said I'd be glad to do it, and felt slightly flattered. The other two people that wound up speaking were Herb Boyer and Chakrabarty. Actually, initially I was a little worried; Herb and Josh Lederberg and/or Dave Hogness were the people that had been invited, and then me. I felt a little bit like it was a lopsided thing in terms of talking about everything that could be done with it, versus some kind of safeguard thing, and I suggested they also ought to get someone who was very conservative about it. They did get Hatch Echols to moderate it, who's there also at Berkeley. I mentioned that one of the big concerns had been with industry and suggested they might want to get someone with industrial connections, and they wound up asking Chakrabarty to do it. But that's getting a little ahead of the story.

Weiner: Yes, we'll want to talk about that, about that meeting, now that we're on it.

Kutter: Okay, about the Berkeley meeting.

Weiner: Yes. The program shows that you were listed as a chairperson of an NIH subcommittee on recombinant DNA.

Kutter: [Laughs] I got very angry when I got down and saw that. They had taken that, apparently, from that Nature article, and I told them that they'd blown it. They were considering the subcommittee that had drafted the revised version of the guidelines, but I said that there was some controversy as to whether that was actually a committee or not, and whatever it was, it certainly was no longer in existence, and what I was, was simply a member of the recombinant DNA committee. But they had relatively little real sense of what was going on, clearly. I went down a couple of days early and talked with some people at Stanford and San Francisco; gave a seminar on my own research, at Stanford.

Actually, something that fits in here, I think, is a rather ironic twist to the whole story. When I was invited to be on the committee, and so forth, I felt that I, at least, had the advantage of being totally uninvolved and, therefore, a totally unbiased person in the whole thing. It turned out then that the system I'd been working with, T_4, trying to see what would happen if you put cytosine in this phage's DNA which normally has hydroxymethyl cytosine; the only way to clone that is to use T_4 that has cytosine. The restriction enzymes won't work on the glucosylated hydroxymethyl cytosine. You can make them with the plain hydroxymethyl cytosine--you can make the fragments, but nobody got any clones. So there was something in the bacteria excluding against that, whereas John Ableson

now has clones with my system. In fact, some of the people in Herb Boyer's lab are interested in a particular enzyme from T_4 that's a very powerful tool, the T_4 ligase, which will make butt joints which they can use to take any predefined fragment like the reverse transcriptase fragment for messenger RNA and then put onto it any specific restriction enzyme sequence in order to put it in where they want it, you know, without cutting anywhere into the middle of it. Well, the enzyme is a bit difficult to make. They're interested now in trying to clone the enzyme, so a guy from Herb Boyer's lab had come down to Stanford to hear my seminar. He gave me a ride back up, and we were talking. But that gives me kind of a strange, queasy feeling; I felt a little strange when I realized for cloning T_4 that it was my system that would be used. I made a point of staying out of any kind of direct involvement, but people have gotten mutants from me. Then to find the step beyond, being involved in the over-all cloning of all sorts of things, makes me think again of the whole question: "Is there any such thing as innocent science?"

Weiner: Well, presumably, if you're satisfied with the guidelines, then you should have no qualms about your research being used for recombinant DNA research.

Kutter: There's just the question of thinking of myself as a totally uninvolved outside party with no vested interests and discovering that--[Laughs] It's more the irony, than any kind of direct concern.

Weiner: At the same time, you haven't yet decided to proceed with recombinant DNA research?

Kutter: Oh, I wouldn't do it myself, but--

Weiner: Based on your knowledge of this particular system, which gives you a head start over other people . . .

Kutter: Yes, yes. I mean, I could do that.

Weiner: Why won't you do it yourself?

Kutter: I think there are a lot of other important questions.

BEGIN TAPE SIX, SIDE ONE

Weiner: We're on tape number six, and you were answering my probe about why you didn't feel you'd want to get into recombinant DNA research.

Kutter: The other thing is that I don't like bandwagons. Particularly, being at a place like at Evergreen, I'm not in a position to compete with those kinds of things that it makes a difference whether you do it today or tomorrow or yesterday. But I just don't like the general scientific atmosphere that develops around a field that's a bandwagon. I experienced it somewhat around 1968 to '70 when T_4 became temporarily the bandwagon, when the size of the phage meetings suddenly exploded and they had to split them into different meetings for temperate phage and virulent phage. You know, you'd have the same work coming out of four different labs in the same journal. There's a very different kind of a feeling that a field gets with that kind of bandwagon atmosphere. There's a great deal of competition, much less sharing and cooperative working with data. And that's not the way I like to do science. The same thing happened later

with tumor viruses and is happening now with recombinant DNA technology. That's not the kind of thing I want to become a part of.

Weiner: In addition to that, is it anything to do with the possible lack of trust that you might have in individuals either not doing experiments safely, or applying them to purposes that you wouldn't approve of?

Kutter: Oh, well, not in my own lab. I think that that's one thing that concerns me with any kind of guidelines; even though I trust the guidelines per se, I really do not trust a large fraction of the researchers in the field in the sense that I don't trust them to always be careful, to always make sure that all graduate students and undergraduate students and technicians know exactly what the problems are and exactly how to make sure nothing happens. What I see is that this is a field where nothing has gone wrong in the past with a lot of infectious agents because they've been worked with by medical microbiologists. But now you have a bunch of people who regard bacteria and other such things as just reagents off the shelf, and don't have that sort of ingrained sense of not sticking a loop straight into the flame but doing it slowly so it doesn't spatter all over the place, and that kind of thing. I never learned those things. Until I got on the committee, I always dumped my E. coli down the sink. I never thought about any of those things as potential hazards. It's just a different kind of mentality that's been developed in a molecular biologist, a different kind of attitude. That's the part I don't trust.

In addition, on the question of why don't I do it myself, I'm in a place that's mainly a teaching institution. I think that there are other

aspects of science that I'm much more anxious to teach my students than recombinant DNA technology. I don't want to give them the feeling that that's the major way to go about solving all problems. I'd much rather see them using a combination of genetic tricks and biochemical tricks, and biophysical tricks to try to work through nature, rather than creating new forms of nature. In addition, there comes the whole question of my credibility as a member of the committee, you know, sort of co-opting myself, in a way. I say there shouldn't be excessive proliferation of this kind of thing. I think, say, that people should be highly cautious, that people should look very carefully whether there are other ways to attack a given problem before they decide to use synthetic biology to do it. Then for me to jump in just because I have some kind of an 'in,' some kind of an advantage, both through my system and through connections on the committee, I think it would be a mistake. I have to admit, I was momentarily tempted by some of the ideas that I got when I was talking with Herb Boyer's students. I could think of several really nice experiments that you could do to select for particular things that could be useful, and so forth. And you know, the momentary thought flashed through my mind of--maybe--"Gee, I could just go down for a month or two in the summer and work with them, and bring my system, and so forth." But I think that would be a big mistake. I may not always feel that way, but that's the way I feel right now.

Weiner: That was a very interesting sideline from the story about your trip to Berkeley to go to a genetic engineering symposium.

Kutter: Yes. Anyhow, the first part of my time was spent with this involvement with my own research and my contribution to genetic engineering. The

second part was related to the concern I've felt all along about the industrial involvement in genetic engineering. I had been hearing stories from several different sources indicating that CETUS, a company in Berkeley, was getting all set to get off the ground in doing genetic engineering kinds of experiments and that they do have on their list of consultants who are very intimately involved, people like Josh Lederberg, and, I believe, Stan Cohen is the second one, who know all of the technology; and that some patents were being filed for, and that people were very, very concerned. So my reaction to those stories, rather than to call for legislation immediately, was to call a person I happened to know who worked at CETUS. It happened that that's the only thing related to a drug company where I had any kind of personal connection, and it was a guy who used to work with my phage system who had gone there. The president of the company is a former student of his, so they were fairly close. And I called him. His first response was that he didn't know exactly what was happening; they were talking about it. But he thought I ought to talk to the president of the company. This was when I called a month or so, before going down.

Weiner: Now, who was it you called?

Kutter: His name is Bruner--Bob Bruner. And his first reply sounded almost a little bit fuzzy. I couldn't quite make much of it. But then, not long after, I got a call back from the vice president of the company, who returned my call. I'll have to think of his name.

Anyhow, we talked and he made it very clear, first of all, that they were interested in the general problem and that they hadn't started

actually doing anything yet, that they were exploring the ideas, and that they were very interested in keeping in touch with the state-of-the-art as far as the guidelines went, and safety precautions; certainly extremely interested in using safety precautions. When he heard that I was going to be down in the Berkeley area, he asked if they could take me to lunch and talk about what was happening on the committee, what they were doing and what we were doing, and get ideas back and forth. This is a company which was mainly started by Don Glazer, the Nobel prize-winning physicist, who's still very much involved.

So on Tuesday I spent the afternoon talking with these people from CETUS, and they asked a lot of questions and gave some answers. They've been interested in the business, but they basically do things under contract for other companies, using genetic techniques to develop more efficient producer strains for antibiotics, a lot of specialized things like that. Genetic engineering would be a natural thing for them to do. But so far, they haven't found any industries willing to take the risk to support this kind of thing.

Weiner: You mean, because of the risk of financial return not being forthcoming, or the risk of a--

Kutter: Probably the risk of financial return not being forthcoming. I mean, nobody's interested enough to do it yet at this time--it's sort of very much in the exploratory stage. They said something that I have felt for a long time . . . --[Interview interrupted]--

Weiner: We were talking about CETUS desiring to get into work in recombinant

DNA but not being able to convince companies or to get companies to go along with it. What does CETUS stand for?

Kutter: I have no idea what it stands for. I think it's the Latin name for something, but, not speaking Latin, I don't know.

Weiner: Well, do you know how long they've existed?

Kutter: I think, something like five or six years. Relatively young company. I know that, well it's at least six years, and I don't think much longer than that. Okay, what they were saying was that they were very concerned about following any kinds of safety guidelines, and not so much in terms of necessarily thinking it's dangerous, but in terms both of legal liability in case of an accident--they were very concerned about the possible dangers of that--and general community relations, and [they were] very concerned about not getting into a position where Congress feels legislation is needed. If the industry can make it very clear that they will abide by any guidelines that are set, for example, by NIH, and that is an industry-wide policy, it's much less likely that it'll be felt necessary to impose some kind of controls from outside which are often hard to work out. And, as they said, the extra expense in working in P3 facilities or any extra problem in working with EK2 is really relatively small in terms of the total expense of an industry like that. In addition, most of the initial things they'd be likely to want to do in most industries are things that would involve only something like P2-EK1, and they do most of their industrial work in what amounts to P2 facilities, anyway.

When I was talking to the people in Herb Boyer's lab about a new cloning vehicle they'd made that they were happy with, it sort of shocked me to realize that it carried not one, but two antibiotic-resistant markers to clinically very useful antibiotics. One is Tetracycline and the other is Ampicillin; and using these two antibiotic-resistant markers, they're able to develop a very beautiful selection technique which allows them to first select which bacteria have picked up some plasmid and then to select which ones of those plasmids are carrying some kind of new DNA recombined into their genome. It's a very powerful technique. But it disturbed me that the antibiotics that they're using are clinically commonly used ones. One thing that we had been discussing since fall--Wally and I and some of the other people on the committee--and in fact, that had been discussed the spring before at the committee meeting, the first committee meeting I went to, was that a lot of drug companies had antibiotics stacked high and low on their shelves that they had isolated at various times and often for which resistant markers were available, but that it turned out for one reason or another, toxicity or something, to be clinically much less useful and had never been finished and marketed, and that it would be possible from all of those to select a few antibiotics which would be useful as detection tools and selection markers for recombinant DNA work. They [the people from CETUS] liked the idea. They personally didn't have any such collection, because they've been working for other industries rather than directly. But one thing that they did do was to suggest several people that I ought to get in touch with to explore the possibility of industry doing just that. It turned out that the first

person they suggested was Max Stark at Eli Lilly, who was the same person I had been in touch with before. I called him a couple of weeks ago, and he's supposed to get back in touch with me any day. He thought it sounded like a reasonable thing for industry to do. The other people I talked with said it would take something like three months, by people who knew what they were doing, to create a proper new strain. And he thought that most people in the field wouldn't want to take the time out to do that, which is too bad. I don't know whether it would be possible to get someone in industry to do it; you know, making people, then, dependent on their particular antibiotics. Actually, the more likely thing would be as a public relations gesture, saying, you know, "We're very concerned about the safety, and we're constructing this thing which will now allow us to do these selections without using clinically-sensitive antibiotics." That was the one thing that I mentioned a couple of times that drew an immediate apparently positive response. I don't know whether anything will come of it, and in a way again, I've been a little presumptuous, going ahead on my own to explore the idea of it. It seems like that's the only way to get ideas explored.

The other names he gave me were Jim Punch at Upjohn, Sidney Udenfriend at the Nutley Institute in New Jersey, and Nish at Ciba-Geigy in Basel. So at any rate, they were both very helpful and very supportive and said, "Keep in touch," and took time and energy.

Weiner: Did they indicate that they'd been engaging individuals who are involved in recombinant DNA as consultants?

Kutter: Oh, they definitely have, yes. Josh Lederberg-- and there are about three, I think, of the people who are some of their major consultants.

Weiner: So that's another thing you did on your own?

Kutter: Yes. Now, then, we can get to the main meeting. [Laughs]

Weiner: Well, while we're on this, let's just talk about the drug company. You've expressed by recalling your conversation with them, the major issues that are of concern to you. Is there anything else that you know of regarding drug companies' interest in this, in terms of people who have been involved either in the drug companies or who've been asked to be involved by them?

Kutter: Anything else that I've heard has been fifth-hand. I'm sure you've heard it from other people.

Weiner: Well, I intend to talk with people in the drug companies directly.

Kutter: Yes. Well, of course, there's the company that's supporting the Scottish group.

Weiner: Oh, the Imperial Chemical Industries is supporting the Murrays' work, but that's because they'd supported that lab all along. There is a joint research scheme that they have which may involve this facility, but it's a much wider-ranging thing than just this issue.

Kutter: I heard that there was some company in Britain that was constructing a major facility and I've heard something about some company up in--I don't

know whether it was in Minnesota or Michigan, somewhere. I think what they were trying to do was to work out ways of producing some of the antibiotics in strains that are easier to grow than the normal, crazy strains they come in.

Weiner: But, anyway, as far as the committee goes and your involvement, this was raised at the committee meetings, but the guidelines themselves say nothing about the idea of industry abiding by guidelines.

Kutter: No, it was made very clear from the beginning and repeatedly made clear that we're not charged to do anything for industry, and not charged to do anything for the military, or for anything else outside of NIH; the only job that we can have officially, as a committee, is to set up guidelines for NIH as advisors to the director of NIH. Now it seems to be very clear that nothing has to do with what we do in terms of moral suasion or making contact, or anything else. But one thing I began to realize was that as a committee, we couldn't do anything. That's why I started going ahead on my own.

Weiner: In going ahead, for example, there's no basis of reporting back to the committee; it's not on their agenda, anyway.

Kutter: No.

Weiner: I see.

Kutter: Some of the people on the committee know what I'm doing. I think I said something about it at one of the meetings, at least. I can't remember

for sure now; I intended to. The last couple of meetings have been so tied up with guideline discussions that it's been a serious problem that there's been no way to really discuss the broader policy issues, or the whole idea even of what the initial charge to the committee was, which (I discovered when I was drafting the guidelines) said it was our charge to stimulate ways of gathering data about potential hazards and then on the basis of that data to write up guidelines. It seems we've done things rather backwards. I know there was a lot of pressure to do that and I'm not sure it could have worked any other way. But at any rate, I've done that pretty much on my own, and I haven't done anything where I said I was speaking for the committee. I mean, what I've done has been as an indivdual talking with other individuals.

Weiner: That does bring us to the Berkeley meeting.

Kutter: [Laughs] By the way. Yes.

Weiner: You started it.

Kutter: [Laughs] I figured I should give a little of the background on that trip.

Weiner: No, that's important.

Kutter: Okay--the Berkeley meeting was Saturday afternoon.

Weiner: On the 24th of April, right.

Kutter: The 24th of April, yes, in a very large auditorium on the Berkeley

campus. And signs had been spread rather widely among the academic community there, but no announcements had been placed in the public newspapers or anything, partly because Herb Boyer had expressed some sensitivity about us getting made into some kind of a political rally. And we were competing with a gloriously sunny day, one of the first sunny Saturdays they'd had, I guess, for awhile. But still there were well over four hundred people there, I'd say.

Weiner: Mostly students, do you think?

Kutter: All ages. Some people I was introduced to were faculty. Some who I met afterwards were clearly community people. For example, one very delightful eloquent black man was the president of the Family Law Association there, who asked some very cogent questions. I gave his name, by the way, to Bill Gartland to put on the mailing list. He was talking to me afterwards and asked for more information on the subject and I mentioned it to Bill, and he suggested that we put him on the mailing list for all of the information. There seemed to be all ages, both sexes, a fair variety of people. It's hard to judge just looking at them, what kinds of backgrounds they're from. Some of them seemed to know a fair bit about science; and there were a number who didn't seem to know very much science.

First Hatch Echols gave an introduction, a brief introduction. And in that introduction he raised not only the recombinant DNA issue per se, but saying that what this led into was the broader, over-all question of genetic engineering and gene therapy, and all of the kinds of much broader and more serious, in a certain sense, societal issues, and saying that he hoped

that this growing discussion in this area would lead to further discussion in those other areas before those problems are on top of us, in terms of needing direct controls. Very nice short talk. And then Herb Boyer gave a good basic summary of the area. And then Chakrabarty talked about industrial applications. I think his talk was very illustrative. He was talking not just about recombinant DNA again, but about using more conventional genetic engineering techniques to try to do a variety of things. And the first thing that they had tried to do was to make E. coli that carried cellulase-- [Interview Interrupted]

Kutter: --with the idea that it would be very nice for people to be able to eat grass and trees and things like that. But fairly early in the work Chakrabarty got some kind of intestinal disorder and was afraid that that might be directly related to his picking up E. coli which were digesting the cellulose, and therefore interfering with the normal passage of bulk materials through the intestine. And they cut that off right there. The problem cleared itself up fairly quickly. Somebody asked later whether E. coli-carrying cellulase were found in the environment. I think the person meant naturally. Chakrabarty's answer was, "Well, as far as we know, there aren't any." Of course, there may have been some that they may have passed out into the environment, but whether any of these survived was a question. He was clearly slightly uneasy about it. So their next effort, and the one I had first read about him for in the newspapers, was to make Pseudomonas that could eat up oil spills. They have strains, and each of these traits is carried on plasmids that will eat each of the different major components of oil spills, one that will digest terpenes,

one that will use certain other kinds of cyclic hydrocarbons, one with alkanes of certain chain lengths. The obvious thing to do would be just to dump all of these together into an oil spill. But what happens is that one takes over and the others all die off, so that hasn't worked; they were trying to put them all together into one plasmid. Again they were successful, they showed in the lab situation that it would work fine, but before they used it on actual oil spills they thought of the potential catastrophe if some highly pathogenic natural organism happened to pick up this plasmid now. That [might give] it strong selective advantage so it could essentially go wherever it wanted to in the environment, creating some kind of serious disorders.

Weiner: In what way? Would this be that the plasmids' ability to consume those components of the oil would also give them the capacity to consume other components of the natural environment?

Kutter: Well, there are a lot of different places you find each of these different components, so it would have a much higher survival capacity in a lot of kinds of natural environments.

Weiner: Not necessarily oil environments. I see.

Kutter: Not necessarily just oil, no. But there are also a lot of places where there's oil. He didn't even mention the obvious other danger-advantage depending on who was looking at it, of having them eat up all our oil reserves. [Laughs] "Oops! Car putters to a stop. I got some bugs in my gas tank again." [Laughter] (Actually, this couldn't happen,

because the bugs need lots of water and air, as well.) There were a couple of other examples of that kind he gave of things they'd been trying to do. And it was really a better lesson in caution that I could have given, because each of these things they were talking about started out as incredible ideas, high potential, and wound up being both technically feasible and somehow apparently dangerous so they weren't actually applicable. Hearing this from someone in industry who was actually doing those things was, I think, a rather powerful statement. So I was glad that I'd suggested somebody from industry.

Weiner: What was the audience response?

Kutter: Very good; very good.

Weiner: Were there questions after each paper?

Kutter: No, the questions were at the end. There were first the papers, and then there was a panel discussion.

Weiner: What do you mean by very good response?

Kutter: Well, I'll talk about that when I get to the question period; but people were very attentive to what he was saying, and the questions later indicated a shared concern for some of the things he was doing, and a lot of interest.

Weiner: And your talk followed Chakrabarty's talk.

Kutter: Yes. I talked about the history of the development of the guidelines--

somewhere at school I've still also got the notes for that talk, if you're interested in having those. I started with the pre-Asilomar first letters and went generally through the development of the ideas in the guidelines, and the increased circle of people involved at the various stages. I spent the most time talking about the meeting that Fredrickson had, and the letters from various people that had come in after that, the variety of people who had been involved in it, the generally favorably response that they'd had to the guidelines at that point. Then I talked about the substance of the guidelines, and the problems in areas like implementation. Again there seemed to be a lot of interest on the part of people, listening. I had the feeling that there were more people that understood what I was saying; in other words, there was a constituency in addition to the scientific constituency. Then there was a short break after that, and a lot of people came up and were asking questions. In fact, I'd talked with a few people at the earlier break, but at that break there were, probably, ten people around me asking questions and asking for various things. One thing that I had done was to print up a hundred and fifty copies of the summary of the guidelines that Fredrickson had made up for his committee, which is about a fifteen-page summary.

Weiner: This was for the February meeting then?

Kutter: --that he had made for the February meeting. I had about a hundred and fifty copies printed up, which was all I felt like carrying, and took them down and had them there available for people. After they were all gone, people kept coming and saying, "Don't you have another copy? Don't

you have another copy?" And apparently the biophysics students were going to make some. I felt it was important that people also have a chance to see in a little bit more detail what they were about. That was written, you know, with many of the details, but in slightly more layman's language. I got that idea two days before, and again our press came through and did them for me, and got them in [out] in half a day.

Weiner: Who pays for that?

Kutter: Part of that came out of my grant money; part of it, Ed Kormondy, who's the provost of the college, said that he'd be willing to help pay for the phone bills and printing, and so forth. I mean this printing, I paid for; it wasn't that much. But he put in about two hundred dollars into the earlier things.

Weiner: Was this your first public presentation about the guidelines? You said that in various places you may have added ten or fifteen to a talk, you know, even on a faculty group, or just students, but--

Kutter: Yes, well the initial thing I'd given was the talk in the fall, just before I became chairman of that subgroup, to my own faculty, which was about forty people. No, that was more.

Weiner: Oh, that's the one where you got to speak--

Kutter: Where I got the feedback, yes. That was a good one. But this was the first time I'd ever spoken away from Evergreen, and the first time I'd ever spoken to a large group like that for any reason. I felt a little

bit nervous beforehand, and quite a sense of responsibility to try to keep a balanced presentation to show both the problems and also the strengths, and give a historical sense of the whole process, the whole process as a model for public involvement. I made the point that it had extended to things like the meeting at Michigan, which I talked about briefly, and this kind of meeting here, and that it was this kind of broader dialogue that was necessary. Then there was a question and answer period. I got about two-thirds of the questions.

Weiner: This was when you were assembled up there as a panel.

Kutter: Yes, we were assembled as a panel, after a brief break. Sydney Brenner was in town and they had invited him to also join us on the panel. He said he'd rather not, he'd rather not interfere; he might say something from the floor, but he asked when my talk was going to be. I didn't see him come in; I thought he hadn't come. But I was told then by the girl who had organized it that he came in shortly before my talk and had stayed through most of the question and answer period, up in the back, but he didn't get involved. I would have wished he had. [Laughs] I didn't have [any] chance to talk with him.

Weiner: What was the general nature of the questions?

Kutter: There were a lot of kinds of questions, a few technical questions, but very few. Most of them were questions like, "How can you be sure that something won't escape? Why do you feel it's important to go on with this kind of research?" One question that was asked was, "Why do you do this

kind of research, in research, in general? Why have you chosen that as a field?" These kinds of very broad questions. The question, for instance, of, "Are there any Third-World people involved in this decision-making process?"; and, "Shouldn't there be?"; and "How is this likely to affect underdeveloped countries?"

Weiner: Why don't we stop there and tell me what you answers were to these questions, just in summary.

Kutter: Well, in the question of the Third World things, what he suggested, also, is that the place for a broader forum would be through the United Nations, and that there ought to be some group set up there to discuss the whole general policy of technology of this sort and its effect on different countries. The first couple of questions, I wasn't the one who answered. But some of the questions were things like, "What about Europe? What about the military? What about biological warfare?" In terms of other countries, I mentioned what was happening with EMBO; with the MRC's in Britain and Canada; and a lot of the other national groups that are in the process of setting up guidelines. I said I didn't know about any work being done to set up guidelines in the Soviet Union, although I have some contact with somebody there who wrote me about six months ago and I have to answer it, but that, in general, the guidelines in these other countries were very closely following our guidelines. One question that was asked was, "Why not ban the work totally?" I said first of all, there's a feeling among many scientists that the potential benefits are much greater than the potential dangers. The second factor that's clearly entered into that kind of decision is that even if we banned it totally, that would have no effect

on its being banned in other countries. In fact, it would be more likely to be pushed, if anything, I would think. And that in the position we are now, it's been made very clear that if we come out with a set of guidelines that are reasonable enough in terms of relationship between hazard and caution, that a large part of the scientific community perceive as reasonable, they're much more likely to really be obeyed here and not, somehow, bootlegged through; that we still have a position of moral suasion in terms of the other countries of the world. EMBO, for instance, is largely following the guidelines we've been putting together, but have made it very clear that they feel they're pushed already to the very cautious extreme, and if we go much more extreme that that, they're not willing to follow them, which is a not-so-subtle way of putting pressure on us. They also asked the question about where these were being done, and about proliferation, numbers of peoples doing them. We talked a little bit about the idea that some groups have, that it would be good to do them mainly in a few central facilities, rather than scattered around the country.

Weiner: And in these cases, did you just discuss the dimensions of the issue, or did you come down and take a position on some of them?

Kutter: In that particular one, I said I thought that it would be a good idea to have at least a large part of the work going on in such facilities and discourage the building of P3 and P4 facilities in and near hospitals, where you have compromised patients, and other highly-congested areas. In general, though, I tried to discuss the scope of the issue rather than come down with a specific position. I think in terms of exactly what the

questions were and what my answers were, it would probably be more meaningful to try to get it from the tape, because it was an intense, emotional experience; it's hard to remember the details exactly, particularly of these questions I was answering. It's easier to remember the ones other people got.

Weiner: Is this the first time that you were in a situation where you were getting those kinds of questions right on the spot?

Kutter: Well, the first time I was in that kind of situation at all, was when I led the discussion at the phage meetings.

Weiner: Yes, but this was a different audience.

Kutter: Yes, this was a different audience; and there, it was more a discussion back and forth. Yes, that's the first time I'd done something like that. It felt like it went well, and a lot of people said afterwards it did. But it was draining; it was fun, but it was draining.

Weiner: What about Boyer and Chakrabarty, in response to these kinds of questions? Was it apparent in the panel that there was a real diversity of opinion on these kinds of questions?

Kutter: I don't think so. The whole tone of Chakrabarty's talk had been that caution is essential. And Boyer wasn't getting that many of the policy-type questions. I'm trying to remember exactly what he said, when asked why he was doing them. Actually, maybe I can reconstruct it, partly. He said, "All through the years, a lot of money comes in from society to

do research, and much of that research that one does seems to have no direct bearing, no direct usefulness. There's always this feeling of being supported, and why." Now, for the first time, he saw a chance with his own research to really do something useful, something where the payoff was immediate and clear, and that that was a good feeling.

I did talk with quite a few people afterwards. Then when I was on the plane coming home the next day, after we had stopped in Portland and before we came on up to Seattle, a man came back down the aisle who looked familiar. It took me a moment to realize who he was; it was Greg Hook's father (one of the organizers,) and he and his wife had been at the meeting. I'd been introduced to them briefly. He's an MD, an occupational health specialist, and he was on his way up to Portland for a meeting and had noticed me getting on the plane. He took the trouble of coming back and speaking to me, and telling me how much he had enjoyed the thing; he'd been very impressed, and so forth and so on. So that was a very pleasant kind of thing.

Weiner: Do you know if they intended to publish this, or it just was a symposium?

Kutter: I doubt very much if they intend to publish it.

Weiner: Okay. I have another question to ask, because you mentioned about the proliferation of facilities. In this letter from your file, on April 21st you wrote to Fredrickson. (It was a long letter, but I'll put it in front of you.) One of the major points is the question of proliferation of sites where such research is done, particularly at P3 and P4. Then

you talked about consolidating and centralizing. I'm curious to know what led to the sending of this letter to Fredrickson.

Kutter: Well, this was an issue that Wally and I, and Emmett Barkley from NIH, had been talking about very extensively in the fall when we were trying to put together the guidelines. One of the major concerns raised by people like Josh Lederberg was the fact that--

BEGIN TAPE SIX, SIDE TWO

--making tight guidelines was really being pushed by exactly those people who already had access to P3 facilities, at least, if not P4; and would tend to create very much a narrow elite who were the only people who had access, and were able to do these kinds of extremely interesting and productive experiments. At the same time, people at the other end of the spectrum, who wanted a great deal of caution, were saying that one of the major problems was having such facilities in places like hospitals, and so forth. From a lot of people I talked with later, it seemed to make a lot of sense to have at least available regional facilities: Fort Detrick, probably, in Maryland; there's a NASA facility potentially available in California; probably at least one additional facility in the Midwest. I don't know if you want me to go through all the things I put in the letter, as far as the reasons . . . but basically, it would be a lot less expensive to construct and staff a few such centers than to have them on every campus, when they'd be necessary for a relatively small part of the work. But more important, the primary responsibility of the staff would be conducting potentially hazardous experiments. They could be trained

and tested much more carefully on the techniques than could a graduate student or technician, for whom the actual cloning is only one small, rather technical part of a long-range problem. I went on to dicuss things like more uniform access, less likely to create an elite. It's much easier to make sure they're not in vermin-infested buildings, or near hospitals, or on heavily-traveled corridors. The most popular organisms could be shotgunned, once or a few times, and then various investigators could come in and look for specific clones, rather than Drosophila shotgunned fifteen different times in fifteen different labs with a clear increase in danger from sheer proliferation; it would be a lot easier to make sure that one still had the proper EK2 host strain if you had people working with it who were carefully trained. It would be much easier to do epidemiological monitoring, and to have properly-trained occupational health people there. There seemed to be a lot of advantages.

We had thought about that, already then. We had talked about availability of P3 facilities, actually, at the first Washington meeting and gotten a list of the something like thirty facilities available in the country, many of which were industrial. But that didn't seem to be actually a part of the guidelines per se, and so these ideas never again really got discussed at the next two meetings which were mainly guidelines-oriented. But at the second meeting the point was raised by a representative from Michigan. We discussed it very briefly, just as I was leaving to catch a plane. It seemed to be not such a guidelines issue as an issue which should be more a decision on the part of NIH as to where they wanted to put their funds. I mean, my feeling isn't that they should legislate against

any work in any other P3 facilities. A place like Stanford where a lot of such work is going on, I assume could set up and monitor perfectly adequately their own facilities. But in terms of their own [NIH] funding, their own emphases, they should establish a few such centers; they should give investigators money and grants to go to them for the few weeks it would be necessary, usually, to do that part of an experiment, and that they should not under any circumstances provide money for construction of local P3 facilities, would be my way of responding. In other words, it's more a policy decision than a guidelines decision.

Weiner: Well, there is a tendency in that direction in Europe, because EMBO and its laboratory is providing facilities for several European countries.

Kutter: Yes. The letter I got back from Fredrickson said that many witnesses at that Director's Advisory Committee meeting in February strongly recommended the NIH consider a program like that. He mentioned, specifically, in the P4 facilities. And he's gotten a lot of letters, and thinks that at least for the P4-type that they'll be able to set up something. I would like very much to see it for P3 also.

Weiner: So again, this is an individual input that you made to him, outside of the committee structure.

Kutter: Yes. I wrote first of all thanking him for the way he's handled the whole guidelines approach, and saying that I appreciate the effort and intelligence he'd put into it, and then saying that this was one issue that I didn't feel had been adequately discussed at our meetings, and therefore

wanted to express my opinion to him as an individual, but that in that case it makes it hard to divorce myself as an individual from myself as a member of the committee.

Weiner: Sure. One thing that we skipped over in order to pursue the industrial issue was the meeting of Advisory Committee on April 1st and 2nd. Now, that meeting was essentially to respond to a request that Fredrickson made to the committee, in response to some of the criticisms of the guidelines, and some comments that were given at the February meeting, and in letters subsequent to the February meeting. And the rest of the agenda is clear. We both know the agenda, and we have documents on it. What were your expectations in going to meetings regarding the guidelines. Did you feel that the guidelines that had been established at La Jolla were, in fact, the guidelines, and that this wasn't a meeting to deal with guidelines?

Kutter: No, I felt that it was very good that Fredrickson had clearly looked so carefully at all of the letters and comments, and listened very carefully to all the comments that had come in. And Perpich had put together a list of about seven points, which you have also, that he wanted further input from us on. I felt that would be the first item of discussion. I hoped that could be relatively brief and without going through the whole guidelines or anything like that again, and that we could get on to some major policy issues. It turned out we spent all of our time essentially discussing those points. I must say, that I was essentially in complete agreement with all the points he made. I felt very upset that the committee outright rejected a fair number of them. I think that most of those points that were

rejected were points that represented areas that were somehow close enough to home that the committee members were being somewhat myopic. Some things turned out very well. For instance, I felt pretty comfortable with the final decision on insects, that insects that had been raised in the lab for at least ten generations and shown to be pathogen-free can be done under P2-EK1, as long as they're not known common carriers of a pathogen, but that all other insects should be P3-EK2. That was a compromise suggested by Dave Hogness, rather than putting all insects up to P2-EK2, or something like that. I think that that was a reasonable suggestion.

Weiner: Did you feel that the committee was more polarized than it had been?

Kutter: Not so much, but I got a feeling of an immense amount of tiredness, being tired of going over the same things again and again. I didn't get any of the sense of buoyancy and enthusiasm that I did from the La Jolla meeting. There were a few new, significant, interesting things brought up, and a few sections that were really important. But in the bulk, I thought that it had the feeling of a meeting that was discussing something that had been discussed five times over to the point where people were unable and unwilling to really listen to new things. I'm not sure how it could be handled differently. It was sort of a downer.

Weiner: Yes. Do you think that in the committee's responses to Fredrickson, which were mostly negative, that in the process of rewriting certain parts of the guidelines, which is what they did--certain textual changes were proposed--that the La Jolla guidelines were on the whole weakened or

strengthened in terms of the safeguards?

Kutter: Oh, strengthened, definitely. Yes. I didn't think that any of the changes really weakened them. And strengthened is a good word, rather than being made more [stringent.]

Weiner: Yes. Strengthened in terms of safeguards--

Kutter: Well, strengthened in terms of being made into a more effective document.

Weiner: Yes. At the April '76 meeting, the committee was faced for the first time with approving proposed safe systems--safe hosts, safe vectors. As far as I know, the committee had not established criteria as to how to decide, by what mechanisms approval would be given, except that it would be taken up through the committee.

Kutter: Yes. We had said it would be taken up through the committee. We had the very detailed statements that were in the draft prepared for the guidelines, and to some extent, in the guidelines, of what criteria would be used. There was one part that was very clearly unworkable, in retrospect; we went through a discussion of that and somewhat changed the wording of that, which I think was an important thing to do. There was sort of an inconsistency. Then on the basis of that, we went ahead and approved a lambda vector to be used in a variety of systems which clearly met the criteria and which we had gotten information about a couple of weeks before.

Then there was the host that Roy Curtiss has been spending so much time

on. We had received a hundred and thirty page document when we got there, and there was no way to effectively go through that. Looking through bits of it, I assumed that it would be approved immediately. Some questions have been raised by a few people, like Adelberg. And there's going to be a subcommittee from the committee, which will be meeting at the Miles Symposium led by Adelberg, who had the doubts. They also have asked Stan Falkow to be there and work with them. (He's no longer a member of the committee.) And I don't really quite understand what the problems are. The basic issue that they're concerned about is that it will survive in tap water.

Weiner: Well, I'm really concerned more with the procedural things of the committee deciding on the basis of the proposal submitted to it. It was very close to deciding at that moment, for or against. It was proposed that the committee respond.

Kutter: It was proposed that we respond, yes. But I don't think that it was ever really very close. I mean, I don't think it ever would have been rejected at that point, on the basis of a vote. The question was whether we knew enough about it, and were ready yet to actually do it. The thing that made me feel somewhat positively was Helinski's statement that he had the strain and had checked out a number of the properties and that his results agreed, so that was a second corroborative set of experiments, and that seemed to work well. I think a lot of the questions raised since, I don't quite understand. I don't quite know where they come from, whether they're basically from people who are working with the strains and don't

know how, or whether--I don't know exactly what the thing is. But there was a question of whether we should vote for it. I think that if it had come up to a vote right then, I probably would [be] for going ahead. But I have to admit that that was more because of my extensive knowledge of Roy and the way he goes about doing things, than because of having carefully read the document. What I looked through, I agreed with. He was very cautious in his pronouncements about it. I think it's true that if he had been the kind of person who would energetically push it at that point, it would probably have been approved there.

Weiner: . . . Or had constructed the document in an advocacy way, because there was no real conclusion or summary or thrust.

Kutter: No, he wasn't willing to do that.

Weiner: That was a meeting of the committee in its working status, not in a drafting status, but you know, where you were evaluating in private sessions, although I guess the committee never got to it, but the agenda called for evaluating specific proposals.

Kutter: No. We never did. We did that later by phone.

Weiner: You were evaluating EK2 systems. You were--

Kutter: EK2 systems were public; I mean, that was a public evaluation.

Weiner: Right. And in the public sense, I'm talking about now, you were fulfilling the committee's charter of continually reexamining the guidelines within the spirit of keeping them flexible; you were doing all of that.

What was your impression of how effective the committee was, and also how it would function in the future? You have three more years to serve.

Kutter: Well, I think it wasn't yet an example of how it'll function in the future, because much of it was spent fighting this rearguard action to defend those most questionable parts of the guidelines, which is one of the most difficult things we've done. I think in terms of the other parts, by the time we got to them we were pretty exhausted from that part. I can't judge. I really don't want to even comment on that. I think that the nature and tone of the committee will almost inevitably be affected now by some of the changes in membership: Charlie Thomas having completed his term on the committee, he'll no longer be a member; with Peter Day added to the committee; and I've heard they're going to be adding several other people. I think it's becoming gradually more a committee with a variety of people who are not advocates of any extreme position. I think it's going to be a much more pleasant job.

Weiner: What about the so-called "lay representatives" on the committee? There was one from Texas appointed [Emmette Redford].

Kutter: He was at Fredrickson's meeting and wasn't able to make it to this meeting, but sent a letter that we-- What?

Weiner: I'm sorry. Was he at La Jolla?

Kutter: Oh, yes. And he definitely played a role at La Jolla. Yes. After Fredrickson's meeting, he wrote a letter to the committee, saying that he wouldn't be able to be at the [April] meeting but making points basically

in support of Fredrickson's suggested amendments. Even though he's not a scientist, at La Jolla, I think he raised several very cogent points, and he listens. He listens and pinpoints us at times when we're being illogical, or brings a different point of view. I think he does play a very definite role, though he hasn't had much chance yet.

Weiner: How about your own role? How do you see that?

Kutter: Protecting what we've won. [Laughter] The guidelines are subject to continual revision. I say that only partly facetiously. Sort of being as continually aware as I can of developments and what's happening, and I guess I see myself a little bit in a kind of watchdog role too--you know, trying to remember that monitoring things have to happen, and sort of checking whether they do, or that we follow up on some of the other kinds of things we've said are important, and doing things like getting in touch with industry and other kinds of things.

Weiner: We agreed that we'd talk about a couple of things. I just wanted to get back to the April meeting for a minute, though, to ask you what was the highlight of that meeting.

Kutter: I really can't say. Actually, I'd say it was a meeting with very few highlights.

Weiner: There was another thing that you wanted to add.

Kutter: Okay. The other thing that I've found myself caught up in as a member of the committee that some of the more concerned people have felt they could talk with, is that I've started getting people coming to me, with

talk about what they felt was violation of the guidelines. And what I've usually done, then, is try to track down what was happening and just directly talk to people and see if I could get some documentation. In general, it's turned out that the story, like the story of Mark Twain's death, was highly exaggerated. Other people have been in a similar position. Wally Rowe, at one point, was hearing things about Dulbecco, and simply wrote Dulbecco a letter and got a good response. But the one that kept recurring that people were very concerned about, concerned me too, and concerned me in terms of how to try to go about handling it. I heard this both from someone at Yale and from someone at Harvard. Well, there were two kinds of experiments that they felt were going on that were not appropriate, they felt. First of all, George Fareed was working with [Dean] Hamer, who's a graduate student of Charlie Thomas's. And they were putting a bacterial suppressor gene in SV40 under conditions where the SV40 could make particles. And they felt that this was being pushed ahead before the guidelines came out. They said the facilities that they were actually working in there were not even actually P3, whereas that kind of experiment would be only permitted in P4 under the new guidelines. And the other thing that was going on, I heard in those labs, was that the whole SV40 was being put into lambda.

It seemed to me to be highly unfortunate that this seemed to be such a widespread story, at least particularly in view of the fact that Charlie Thomas is on the committee; even though it might be technically permissible under the Asilomar guidelines, which were all that were in effect, in terms of his being a member of the committee and knowing very exactly what the correct guidelines were, that was definitely improper. I talked a bit

with Maxine Singer and Wally about the problem and the degree to which I felt on the spot about what to try to do with it, because I certainly did not feel like trying to hound anybody or make it any kind of personal vandetta against anybody. And I didn't know what to do. They said, "Well, try to get some actual data, some people to talk with." Then when I was at the meeting at Keystone, I did talk with a girl who was a graduate student of George Fareed's, who confirmed both that their facility did not meet up to P3 criteria and that these experiments were going on. She said if anyone wanted to come and ask her questions, she'd talk with them about it. She would not approach anybody. She was clearly in an awkward position. Not only was she a student of George's, but Charlies Thomas is on her committee. I didn't know what to do with it, as I say. (This was on my way to the meetings.)

It was clear to me that for me to confront Charlie Thomas would be a mistake. I had meant to talk it over with some of the members of the committee, like Don Helinski, who were solid middle, whom I think would be more in a position of authority there, and perhaps see if they'd like to raise it with him and find out what's going on, to simply dispel rumors and satisfy things. I wound up carefully suppressing the fact that I intended to do that, until the last afternoon. Then about an hour before my plane, I suddenly realized I hadn't done anything about it. The action I took was a mistake, in retrospect. I realized it just as Charlie was leaving, just after noon. He had to leave early, or I guess he had already left. I then gave a letter to Stetten saying that this had been raised by several people. I was concerned in terms of the credibility of the committee, and

wondered if it would be possible to somehow track that down, perhaps talk with the girl, and so forth. Well, I didn't hear anything more. A couple of weeks later I called Stetten. And that's the only time he's ever been really touchy with me on the phone, and really uneasy. What he said was that he had asked Charlie Thomas and he had denied that anything was going on that was against the guidelines. They had said that they do have a P3 facility, and there was no way he could pursue it any further.

I feel uncomfortable about the whole situation. I think he probably included the girl's name in the letter to Charlie, which would put her in a very difficult situation, without doing any good. And it seems to me that if we're having any kind of credibility that we need to do something more to check out that kind of thing, particularly in that sort of situation, more than to ask only the person who is directly involved.

Weiner: Presumably, there's a mechanism set up within the guidelines themselves, and that is that the local biohazards committees are the ones with the responsibilities, as well as the principal investigator.

Kutter: No, that's not at all clear yet because that was not true in any of our versions of the guidelines, that the local biohazards committee has any such responsibility. It's only in what I had tried to push in the Kutter guidelines, and also in Fredrickson's suggestion, but otherwise the local biohazards committee had only responsibility for certifying that something was P2 or P3, or whatever.

Weiner: Well, that's one of the issues.

Kutter: That's one of the issues, yes. That part would be. Well, apparently, as far as I could tell from Rich Goldstein, there was no really functional local biohazard committee there [at Harvard Medical School].

Weiner: There was a meeting held recently to discuss a P3 facility at Harvard, and Mark Ptashne was the major person involved in organizing that. And there was considerable discussion, and Mary Terrall from [our project was there].

Kutter: Of course, this was out of Harvard Med School. Or was that included also?

Weiner: I think so. I think it was. I don't know if this issue was really voiced. I'd have to check with her. But it seems to me that that's the place for the complaint to be lodged in the first place, at the local level, with any institution. I mean, according to the way I interpret the guidelines, and even as constituted.

Kutter: As constituted, well--

Weiner: Otherwise, then, if there is confusion on this then what is the meaning of the rest of it? You know, on the major point of implementation.

Kutter: Well, the thing is that they have to certify it before the grant is applied for, but their responsibility is between that and NIH. Yes, I think you're right, in terms of the certification. Stetten should have also asked, then, the local biohazards committee. And that's something Stetten probably should have done, whether it was P3.

Weiner: Where do you stand on this matter? Is it closed as far as you're concerned?

Kutter: It's not closed in my mind, but, I mean, as Stetten [has] repeatedly said, he doesn't know where to go from there. I've been trying to call Rich to find out a little bit more, whether he knows anything. I feel very much in a very awkward position there. I would like to at least make sure that that girl doesn't suffer any adverse circumstances. If I could find some proper mechanism, like you [laughs] to try to contact, you know, just to try to find out what's happening. . .

Weiner: Well, your major concern, though, is with the credibility of the committee, and that apparently hasn't been taken care of by Stetten's response.

Kutter: Yes, I really feel that. And I feel that I flubbed the whole thing, somehow. You know, Stetten said, "It is closed now," and made a very strong issue of it. And I think that if I pursue it, I have to pursue it in a very, very cautious way. I certainly can't now go back and pursue it in the way I initially intended to. I haven't talked any more with people on the committee about it. As I say, that's one of those areas where I felt upset and badly enough about it, I sort of internalized it. This is the first time I've really talked about it.

By the way, one thing that was mentioned was that they would have available, if they were willing to take a little bit of effort, twenty minutes away, the Harvard Primate Center, which has a P4 facility, where they could perfectly well go ahead and do those experiments.

Weiner: But from what I heard at the committee meeting to discuss much simpler questions, the question of inconvenience and a slight delay seemed to be a major issue, on the part of many people who objected to some of the aspects of the guidelines.

Kutter: I think that people should be pushed into taking a twenty-minute drive to have that significant safety. Yes, they also raised the issue that at the Harvard Bio Labs people are upset with what will be going on there, and that there's a possibility of bringing a class action suit to try to stop the construction of a P3 or P4 facility at the Harvard Bio Labs.

Weiner: Well, apparently, since neither of us know what happened, there was this meeting, and we can fill in the documentation--

Kutter: I think that's worth--

Weiner: That's one of the local biohazards committees that we're going to try to look into for historical documentation. It's just the two nearby, MIT and Harvard, so you know, we'll get to that.

Kutter: Actually, what I was told was that the local biohazards committee was essentially ignoring the group. They hadn't had a meeting yet at that point. Essentially, that was what Mark Ptashne told Bernie Fields. The other people who were involved in the discussion included John Sedat, from Yale, if you want that.

Weiner: Well, you know, we can look into that directly as a part of the project.

Kutter: What interests me is among all the people who are out looking for violations--and there are a fair number--that's the only one that seems to have any real credibility. There's one or two other borderline cases. In fact, the most questionable one, at UCLA, was stopped by the action of a couple of people on the local biohazards committee, even though the person managed to get a written statement that he could go ahead with it from somebody at NIH. That had to do with the human hemoglobin genes. I'm trying to think of the name of the person at UCLA who did that good job; it was someone who used to be a student of Hogness', Mike Gristein, I think his name is. You could check it. I'm almost sure that that's it, Mike Gristein, who, against a fair bit of opposition there, sort of went ahead and actually stopped certain work from being approved at the local level.

Weiner: Well, isn't that the ultimate test of the guidelines to see for an individual case is how effective they are and how effective the implementation procedures are?

Kutter: Yes, yes. And this is going to be the most crucial and most difficult time, right at the point where they're being started. It's going to be the most difficult because the procedures aren't really set up yet. And it's going to be the most crucial, because if it's clear there's no enforcement then everybody'll go ahead and violate them, and it'll be impossible to ever get . . .

Weiner: Do you see this as coming to be a major part of the agenda of the committee in its future meetings?

Kutter: I hope not. I hope it's going to be handled at more local levels. But I think that we, at least, are going to need to not stick our heads in the sand and perhaps at each meeting take an hour or two to discuss local flak, what we've heard, get impressions from people of how things are working, and if they're not, why. I think that that's something that we're really going to have to take responsibility for, if it turns out to be a problem.

Weiner: Well, I was thinking especially of your point, that in this transition interim period, it may very well be important.

Kutter: Yes. And I think we're going to all have to continue to act as individuals with a sense of responsibility, as Wally did in writing to Dulbecco, and as I've done in trying to follow up some of this. You know, I think that's a place where we're not just members of the committee, but we're individuals. And we should set an example for other individuals in the academic community. It shouldn't be just us, but anyone who sees or hears about something going on that they feel is highly questionable, needs to gradually get the sense of support and ability to function to go ahead and raise those questions with the people involved. And it's only when it gets to the point where people feel supported in challenging potentially dangerous research--and people, meaning the whole spectrum, from technicians up to fellow-scientists--that the guidelines will really work.

Weiner: I want to ask a final question. And that is, your own evaluation of the effect of this involvement that you've had for just a little more than a year, which we have reviewed in great detail, on you as an individual

and also on your career.

Kutter: Yes. In an individual sense, it's been an incredible education. I've learned a lot of scientific things. But even more, I've learned a lot about the politics of science and the kinds of human interactions that play really crucial roles in things that I had taken for granted in a much more absolute sense, before. It's made me much more aware of broader issues. I'd always had an intellectual sense of the responsibility of the scientist, but I'd also never had a feeling that anything that I could do would ever make any difference. So it was just kind of a sense I was trying to give my students, rather than something that affected my daily life. And I'm really thinking now a lot in terms of how I want to spend my time in the next few years, in terms of relative energies for my own basic science versus political science; you know, how much do I want to get involved in those kinds of issues and learn more about them, and try to take an effective role in increasing public involvement in scientific decisions. That very clearly has sort of changed my headset. I think it's made me much more effective as a teacher in talking about these kinds of issues; I can talk now from firsthand experience. And I can make things really meaningful and relevant, and not just in terms of recombinant DNA, but I've gained a much broader perspective on a lot of the issues facing science today, so I think it's been really incredible that way.

It's also clearly turned me from being someone that a few friends knew, and whose papers are still quoted now and then by a small circle of people, into someone that a lot of people know. And that's a strange kind of phenomenon. At the Keystone meeting, I walked up to a group of people and

was introduced to Mark Ptashne, and he said "hello" --and then carefully ignored me. Then someone came up and started asking me a question about whether they could do a certain experiment. And all at once, Mark's ears perked up, and he turned around and said, "Oh, you're that Elizabeth Kutter." And that was a very strange feeling. Or, being invited to Berkeley. So it's given me a different sense of myself, partly, as a result.

A lot of people had made the statement that now that I'm well-known, won't I, of course, use that to go to a research-oriented school and get a different kind of a position, a more prestigious position, and leave Evergreen. And my feeling there is that I have no particular desire to do that kind of thing. I think that Evergreen is an incredible place to be, and it's a good balance between public involvement and being able to teach in a place that really emphasizes teaching and attitudes, and being able to have really good facilities to do some research. I won't say that under no circumstances could I be lured away anywhere else; I mean, there will be a lot of factors, I'm sure, in the future. And, perhaps, I will at some point go away for a year or two. But I certainly don't feel any major compunction to make any changes in my life. I feel a lot of reason for staying right where I am. I like it here. I like it here a lot. And I really like the balance now of having mobility, which the committee has given me in a lot of ways, to get to know people who are doing really good science in a lot of other places, to talk about my research in some of the leading schools in the country at times, to get that kind of feedback that's usually missing at a small school, and at the same time come back to this kind of a place where I can really function well. And that's a nice place to be.

There's one thing that I've thought a lot about that I should have put in a little bit earlier, that I'd like to add. One really both encouraging and frightening sense that this whole involvement has given me has been the sense of how much of the major things that happen in various aspects of history, I'm sure, have to do with a single individual, and how much influence at times a single individual can have by happening to get involved in one direction or another. And I've felt at times that things have happened in relationship to the guidelines, for instance, that probably wouldn't have happened in quite the same way without me. I see things happening in a very different way, for example, with Charlie Thomas being a member of the committee, than would have happened otherwise. Maybe in the long run, all these things smooth over. But it's both very encouraging in terms of giving an individual the strength to take the time and energy to get politically involved. It can make a difference. And it's frightening in the sense of realizing how few people really do make most of the major input to most kinds of decisions. You know, this is clearly a general political problem in our society that was evidenced very much by Watergate and other things. But none of us can say that we have the right to just keep in our own little world, that what we do doesn't make any difference. I mean, it's not often we're put in the kind of position that I've been put in right now, where we do suddenly have a chance to get somewhere close to some kind of major council of power. But it's really frightening, the power of the single individual.

END OF INTERVIEW