CHEMICAL HERITAGE FOUNDATION

GLEN A. EVANS

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

Arnold Thackray

at

Salk Institute for Biological Studies
San Diego, California

on

20 November 1989

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

This oral history is part of a series supported by a grant from the Pew Charitable Trusts based on the Pew Scholars Program in the Biomedical Sciences. This collection is an important resource for the history of biomedicine, recording the life and careers of young, distinguished biomedical scientists and of Pew Biomedical Scholar Advisory Committee members.

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GLEN A. EVANS

1952	Born in San Diego, California on November 14

Education

1973	B.A., Biology, University of California, San Diego
1979	M.D., University of California, San Diego
1979	Ph.D., Chemistry, University of California, San Diego

Professional Experience

	Stanford University Medical Center, Stanford, California
1979-1980	Intern in Internal Medicine
1980-1983	National Institutes of Health, Bethesda, Maryland Research Associate
1983-present	The Salk Institute, La Jolla, California Assistant Professor

Honors

1973	B.A. in Biology with Highest Honors
1974-1979	University of California Regents Scholarship, UCSD School of Medicine
1974	California State Graduate Fellowship
1975	California Foundation for Biochemical Research Fellowship
1976	Mead Johnson Excellence of Research Award
1976	Roche Laboratories Award in Neurosciences
1977	American Cancer Society Award
1975-1979	NIH Pre-doctoral Trainee, NIGMS
1985	Pew Scholars Award

ABSTRACT

Glen A. Evans grew up in San Diego, California, the oldest of three children. His father was an illustrator and later an engineer working on airplanes, his mother a housewife. Both were of Welsh descent. All three of their children obtained degrees from University of California, San Diego (UCSD) and live in the area.

Evans first decided on a science career when he was in high school. An arrangement with UCSD allowed him to take courses at the University even while in high school, and during the summer before he matriculated at UCSD he worked in Renato Dulbecco's lab. As a result he was able to graduate in just three years, with a major in biology and enough credits for another major in chemistry, and with two published papers. Medical school beckoned, as did research, so Evans decided to combine the two in the Medical Scientist Training Program offered by the National Institutes of Health (NIH), choosing UCSD. There he was able to continue in Michael G. Rosenfeld's lab, where he had worked as an undergraduate on activation of hormone genes in the pituitary gland. He finished his MD and his PhD degrees together in just six years, with an internship at Stanford University and a thesis on the regulation of prolactin by TRF.

Evans' first job was in Philip Leder's lab at the NIH's Public Health Service, funded by the U.S. Navy. Finding the lab too large, Evans moved to Jonathan Seidman's lab to work on histocompatibility antigens. When Leder and Seidman left NIH for Harvard University, taking most of the lab with them, Evans decided to finish his third year and then move to the Salk Institute for Biological Studies. Though he has to fund his own work at the Salk he finds it intellectually free, smaller, and more efficient. He has little difficulty getting grants, except for expensive equipment, like a confocal microscope, so he attempts to share whenever possible. He keeps his lab small, preferring graduate students to postdocs, as he finds them are more curious, willing to stay longer, easier to teach, and willing to experiment. These days Evans is not working at the bench, as his lab is mostly involved with the Human Genome Project, and his time is better spent in administration, but he hopes to get back soon.

Evans' wife has degrees in both mathematics and music and is now a professional musician. The couple has two children, with another on the way. Evans' interests include skiing; playing piano, organ, and synthesizer; and building furniture.

To finish the interview Evans discusses his documentation, a typical day at work, his rolling contract, and his ideal lab environment.

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plays piano, organ, synthesizer. Manual labor; built furniture. Typical day at work. HGP keeps him out of lab, but he wants to get back. Discusses documentation; using other job offers as pressure; three-year rolling contract. Ideal workplace would be intellectually focused but still growing, e.g. Santa Fe Institute or universities in Boston, Massachusetts, area.

INTERVIEWEE: **Glen Evans**

INTERVIEWER: **Arnold Thackray**

LOCATION: **The Salk Institute, San Diego, California**

DATE: **20 November 1989**

THACKRAY: Glen, will you start us off by talking a little bit about your family background?

EVANS: My grandfather was born in Swansea, South Wales, where the Evans name comes from. And he came to the United States when he was about 10 or 11 years old and settled in the Midwest near Chicago, [Illinois]. He then ran away from home when he was about 15 and joined the Canadian Army and fought in World War I, and when he returned from that settled in Chicago where he met my grandmother on my father's side. My father then was born in St. Louis [Missouri] and as a teenager migrated to San Diego, [California], where he settled and remained the rest of his life and where he still lives.

My father, until he retired, was an illustrator who began working at an aircraft company during the war, doing designs and illustrations for airplanes. As time went on he became more involved in engineering aspects besides just the drawings. Finally before he retired he ended up doing some of the design drawings for the space shuttle. In fact, for a while one of his drawings was in the Smithsonian [Institution] in Washington. And he is now retired and lives in San Diego.

THACKRAY: What was his highest level of education?

EVANS: He graduated from high school and went to college for one year. He never finished college, though he attended college for about a year, where he took primarily art and design classes. For whatever reason he never continued, partly because of the necessity of working and partly because of working during the war in the aircraft industry and various things like that.

My mother's family derives from Texas and Arkansas. Her maiden name is Duty, an unusual name originating in England but which has been traced by a number of genealogists to the Texas/Arkansas area. She was born in Oklahoma, and moved to San Diego sometime during her teens, and essentially has been a homemaker. She also attended college for one year and never finished.

So let's see, my family life: I'm the oldest of three children. My younger sister, who's three years younger than I, is currently a graduate student at the University of California, San Diego, in the communications department, where she's finishing her PhD this year, and she works on the

sociology of media. Her specialty is Cold War ideology in television.

THACKRAY: A lively field.

EVANS: Very lively field. Extremely interesting, although I don't understand very much of it. My sister obviously graduated from UC-San Diego with a bachelor's degree, got a master's degree at the University of Massachusetts in Amherst and she's getting her PhD from UCSD.

My brother, who's ten years younger than I am, graduated about two years ago with a bachelor's degree in computer science from UCSD. He now works as a computer programmer and engineer for a small company in Sorrento Valley here called Quantum Design [Inc.], and they make something called a SQUID [superconductivity quantum interface device] magnetometer. Until recently I didn't know what that was, but it's a device for measuring superconductivity. So his main occupation is flying around the world installing software on these machines for large physics, superconductivity research.

THACKRAY: So UC-San Diego features very largely—

EVANS: In our family, yes—partly because it's local, partly because it's a great university, and also partly because it's a California state school, so it is somewhat less expensive to attend. In fact, all three of us have really put ourselves through college, so we've each worked during our college years and ended up paying for most of our education ourselves, although our parents clearly have contributed some.

THACKRAY: Was there a lot of family support? Where was the family push coming from, do you think? Three children heading through…

EVANS: Well, it's hard to know. I think both my parents have always very highly valued education, and my mother has probably been the driving force more than anything for her kids attending college and completing it. But I think in most cases it's more of an inner drive. It was very clearly understood from an early age that each of the children would attend college, and then once we were let go I don't think there was any difficulty in just continuing through it.

THACKRAY: So all the family is in the area.

EVANS: Yeah, let me think. My parents are still here. My brother—he was just recently married, last month—and his wife live here. My sister and her husband and her child live in San Diego. I

have two grandmothers, both my paternal and maternal grandmother, who live in San Diego, as well as my mother's brothers and several other cousins. So I think there is very little of our family that is elsewhere.

In the case of the children, each of us has spent some time in other parts of the country. My sister went to graduate school in Massachusetts. I was an intern at Stanford [University] and I spent some time at the National Institutes of Health in Maryland.

I think in terms of why we've all ended up here, it's partly out of choice because I think to all of us it's very important to be near family, and particularly, in my mind, having children, it's very important to me that my children grow up knowing their grandparents on more than just a once-a-year basis. So that's clearly by choice that we've ended up back in the same area.

THACKRAY: Just staying with the family for one more minute, if I may, what is the religious affiliation of the family?

EVANS: Well, my grandmother is a very devout Catholic. My father was raised as a very devout Catholic, though he is not as devout as his parents were. My mother was always of Protestant derivation, though not particularly religious. My brother recently married a girl of Polish extraction, and though he was never really educated or brought up as a Catholic has now gone back and become very religious again. So he's just been confirmed and so on. My sister, while being brought up in this very loosely Catholic family, married a Greek man, and she has then converted to Greek Orthodox, and in fact their wedding was in a Greek Orthodox church.

And I think there's really not a great religious drive in myself or in my close family, or really in my parents, so I think religious interest kind of drops with the grandparents. My wife was brought up in the Lutheran Church, and my children actually are not being brought up in a strictly religious home, though they're being exposed to a lot of religious ideologies and ideas, partly because my wife is a musician and she sings both in a church and in a synagogue. While her background is Lutheran and mine is Catholic, she sings in a Congregational church and we have a lot of friends in various backgrounds. Neither are particularly religious, though I think it's good for the children at least to know something about religion.

THACKRAY: Let me track back into your own education, if I may. When did science begin to seem the likely way to go?

EVANS: I think, probably, during high school is when I first had any kind of formal education in a science and where I really became interested in it, predominantly probably as a sophomore in high school, taking biology and chemistry and then physics. And there were lots of activities like science fairs and various things that I participated in.

I was actually in somewhat of a unique circumstance because, being in San Diego, as a senior in high school I was able to take courses at the University of California, San Diego—in fact was actually admitted to the university as I was beginning my last year of high school - and that year I took a number of courses in computer science, which was my first main interest.

And then once I became a full-time student here, it was quite clear that I was going to be involved in some science, though it was not completely clear which.

THACKRAY: Was that high-school linkage, was that common? Were other people doing it in your high school?

EVANS: No. The high school that I went to was a newly opened public school, and I was in the very first class. It was a new high school in a new area and it was designed to be very experimental. The initial few classes had a program that was very flexible and allowed students to proceed at their own pace.

It encouraged them to decide their own curriculum and pick out areas they were interested in. It allowed them the flexibility to actually leave the high school campus and take courses either at junior colleges or at the university. I think that program only lasted one or two years, actually. Whether it was successful for the majority of students I don't know, but it allowed me the ability to do things that I think very few other students did. I think I was the only student who took any courses at UCSD. There were several who took things at junior colleges.

THACKRAY: Was a particular teacher urging you to do that?

EVANS: There was one, who was actually a counselor, who was the driving force behind this experimental program. His name was Dr. David Hermanson. He was a recent Harvard [University] graduate, a very young guy. He had a doctoral degree in education and was very interested in new programs and very active in this area, and this whole concept was really his driving force, and he was a person who encouraged students to do a lot of these things. I wouldn't have thought of doing that on my own, I'm sure, of even being able to know how to arrange such things, but with a little bit of provocation and someone showing you the way, it was quite easy to go out and do such things.

THACKRAY: So UCSD was more or less automatic.

EVANS: Well, it was automatic partly because I was already enrolled as a student—I didn't even have to reapply to college—partly because it was local, and partly because of my parents. While they were very much encouraging the kids to go to college, it was really never considered that we would go away, and a lot of that, I think, was the expense in going to the East Coast to an Ivy

League school. A lot of it was also because they had really never experienced very much of the university themselves, and it's still to this day not completely clear to them what the difference between the University of California and San Diego State College or a junior college is. They see them all as pretty much equivalent in higher education. And again, as a high school student, one needs parental direction to decide on those things. It was pretty much understood that that's what I would do, and in the long run I think it worked out fine. One always has these insecurities about what would have happened if, but…

THACKRAY: How big was your high school class?

EVANS: It was very small. To tell you the truth, it was very small by a lot of standards. It was several hundred, but I can't tell you the exact number off the top of my head. I don't really recall, because it was the very first graduating class of this high school, and virtually no one who was graduating had started high school there. We had all come from other schools in the area and had pretty much been put together.

THACKRAY: So you came here and what took you in the biology direction? How did that occur?

EVANS: I was very much interested in biology and chemistry. At the time, either the year of my birth or the year afterwards was the year that the structure of DNA was discovered. I'd actually had a little bit of the beginnings of molecular biology as a high school student, which was 15 years later.

And the other thing that, strangely enough, made a big difference as to what I decided to do in college was that the year before I started college, which was 1970, I worked here for the summer, at the Salk Institute. And there was a program at that time, and there still is, for local high school students to come into the lab and to do something, usually under the direction of one of the postdocs or someone in the lab. This was actually designed as a program for minority and underprivileged high school students, and I was not necessarily a minority nor underprivileged, but that particular year they couldn't find enough students to fill up the places, so again I was pointed in this direction by this high school counselor and spent the summer here and ended up doing real benchtop molecular biology, knowing absolutely nothing about science. And it was pretty clear to me then that this was a really interesting thing to do.

When I then went to college, it was clear that what I wanted to do was initially directed to either biology or chemistry. I was a biology major, but I actually fulfilled all the requirements for the chemistry degree as well. And I took other classes—physics and mathematics and things, but really was primarily interested in biology after that—at least what I consider to be biology. What I mean by biology is really molecular biology, which is a combination of biology and chemistry.

THACKRAY: And was that an organized entity on the campus at that time?

EVANS: No, it was not. In fact, there was not even a biochemistry program or department. There were courses in molecular biology taught through the biology department, but essentially anyone interested in that would go either to biology or to chemistry and they would then take these other courses and focus themselves in that way. But my undergraduate degree is actually in biology. My PhD, which involves the same general research area, is actually in chemistry.

THACKRAY: When you were an undergraduate, did you get back into the lab somewhere in the summers?

EVANS: Yeah, when I worked at the Salk Institute during the summer, I worked actually in Renato Dulbecco's lab, who is now the president of our Institute. I spent a summer there. Then I actually came back a little bit during that fall and worked to finish up some of the experiments I was doing. The following year I got a job in the chemistry department as a lab assistant working for someone who was an inorganic chemist but was interested in applying those ideas and techniques to biology, so I was really working in the biochemistry area. I worked there throughout the rest of my undergraduate college career. I was working as a technician, not so much as a student. And I was paid. That's how I worked my way through college. We published two papers, or I was a co-author of two papers during that time as an undergraduate.[1]

THACKRAY: Co-author with…?

EVANS: With the graduate student and the professor that I worked with. So I was essentially the laboratory assistant working with the graduate student.

THACKRAY: So this was pretty unusual within your undergraduate class, wasn't it?

EVANS: I think so. It was very usual for a lot of the students to have jobs. Usually they worked as dishwashers or media makers. I actually started working as a dishwasher. I washed dishes for about two weeks, and then they actually found out that I had worked at the Salk Institute and had worked in the lab before, and that worked out to doing more valuable things—at least what was more valuable to them, also to me.

[1] G. N. Schrauzer, W. J. Rhead, and G. A. Evans,. Selenium and Cancer: Chemical interpretation of a plasma cancer test, *Bioinorganic Chemistry* 2(1973): 329-340 and Selenium in human plasma: Levels in blood proteins and behavior upon dialysis, acidification, and reduction, *Bioinorganic Chemistry* 3(1974): 217-223.

THACKRAY: So if I asked you about influences on you as an undergraduate, this sounds quite important.

EVANS: I think the people who probably were most influential as an undergraduate were, first of all, people I worked with at Salk. This was actually before I was an undergraduate: Dr. Dulbecco and the two postdocs I worked with in the lab, John Newbold and Jim Champeaux, who have now gone on elsewhere many years ago, as well as Marguerite Vogt, who is now a distinguished professor at the Salk Institute. And then Gerhard [N.] Schrauzer was a professor who's still at UCSD that I worked with as an undergraduate. Probably a number of the professors, but more directly the people that I worked for. If there was one professor who, in a more academic way, really influenced me, it was the freshman chemistry professor that I had. His name was Art [Arthur B.] Robinson. He was actually a protégé of Linus Pauling. Linus Pauling at that time was at UCSD and taught freshman chemistry but then left to go to Stanford. And Art Robinson, who was his postdoc, stayed here and took over his classes and was a very good teacher. I thought he was very good. Not everyone thought he was, because he was extremely tough on the students. It was a very hard course.

THACKRAY: How many students were there in your graduating group, biology majors?

EVANS: Again, it's hard for me to tell you exactly the number. There were probably two or three hundred of that major. The lectures of that major were these huge lectures. But, again, in some ways I was a little out of the ordinary in that I started, I finished my undergraduate degree in three years, partly because I had several credits from when I was a high school student and partly because I ended up taking a lot of classes. Because I was interested in both biology and chemistry, when I was approaching the middle of my junior year, I was told by the undergraduate adviser that I had too many credits to remain an undergraduate, and I could either do a double major in biology and chemistry and get two bachelor's degrees or I could graduate a year early.

This was one of the great decisions of my life, which of these things to do, which probably in the long run it wouldn't have made any difference whatsoever. What I ended up doing was graduating a year early and then spending a year as a graduate student. In fact I initially went to the chemistry department, because I was working for a professor in the chemistry department and I could do that without changing very much.

So I essentially graduated very prematurely from my point of view. I wasn't really prepared for what I was going to do at the end of that time. I think in the long run it was a good thing to do. I spent a year as a graduate student. My original plan was to apply to medical school after four years of college. But, I graduated a year early and had already applied for medical school for the following year. As you might see, that sort of pulled me in the direction of remaining at the same university, partly because I was not doing things the way you were supposed to.

THACKRAY: The thought in going to medical school was what?

EVANS: Well, the thought in going to medical school was predominantly to do medically related research. There was no question at that point that I wanted to do research, and research in an area like molecular biology. After working in some other areas like inorganic chemistry, it was still pretty clear that's what I wanted to do, so the question was whether to go to a PhD or to go to medical school and do it through an MD track. That time, which was the mid-seventies, the NIH [National Institutes of Health] was encouraging MDs to go into research. It was very easy to get research grants. Most of the things that I found very interesting and fascinating going on were going on in laboratories at the medical school.

THACKRAY: You knew some people there.

EVANS: I knew some people quite well. Geoff [Michael G.] Rosenfeld, who became my PhD thesis advisor, in fact, I knew when I was an undergraduate. He was a very young assistant professor in medicine who was working on hormone action. It seemed to me that that would be a way of not only being able to do the kind of research I wanted but also being in a position of having a little more human relevance to the work. That is, the things that were happening in molecular biology, recombinant DNA and a lot of that, were clearly something that was going to be of great medical use. Again, I now advise a lot of MD/PhD students who are considering the same question. I don't know what the right direction is, but I think they have to ask themselves what do they see themselves doing in the future, as to whether they should decide one or the other.

So I ended up doing both by being in an MD/PhD program, but much more along the medical lines than the PhD

THACKRAY: You felt retrospectively that was a good choice?

EVANS: I think for me retrospectively it was a very good choice, not so much because I do any clinical work, because I don't right now, but it very much changes the way you look at science, and it changes the kind of things you would do.

THACKRAY: In what sort of way?

EVANS: Well, molecular biology is a very rapidly moving field and there are a lot of exciting things going on with *Drosophila*, yeast, the worm *C. elegans*, but while I'm interested in all of those, I think that my approaches and interests and directions will always be very closely related to humans, so I'll be using human material or mammals of some kind. That was actually something

that Phil [Philip] Leder, many years later, told me since he had done something similar. While he really didn't do clinical work, having the medical education had a vast effect on the kinds of problems he would look into and the kinds of ways he would approach them.

THACKRAY: So what do you advise people now?

EVANS: I think, particularly for people that would anticipate doing both degrees, an MD/PhD, which would be to train a medical researcher, it's very different than either being a clinician or being a PhD The reason is it will provide you with the training to do very high quality research, but it will educate you in medicine. Medicine is an incredibly vast discipline. There's a lot of facts to learn. And what you will have is someone educated as a doctor who knows how to do the lab research. That's a very special kind of person, I think. It's not for everybody. But if that's the kind of thing they're interested in doing, then I would encourage them to do it. I wouldn't encourage them to go into that because they think it will either be easier for them or they'll get paid more or something like that. I think they have to realize exactly what it is that that will be training them to do.

THACKRAY: You went through the MD and PhD in quick time. Was getting that financed a problem?

EVANS: Well, the advantage for me being—I was in an NIH-funded MSTP, or medical scientist training program—the advantage to me was that I was paid a stipend, so essentially it was a funded position. I also got a scholarship from the University of California, which was another one of the things keeping me here, which paid anything that other programs didn't pick up, so in fact that funded my entire graduate career. This was funded from the NIH, and there was attached to that a payback agreement, which was that if the trainee decided not to do research as a career for what was then the equivalent number of years, then you had to pay them back with the amount of money it cost. That for me wasn't a problem, because it didn't change what I would do, but if I had changed my mind and decided to become a surgeon, I would have had to pay them back the cost of my education.

THACKRAY: Is that program still there?

EVANS: Yes, it is.

[END OF AUDIO, FILE 1.1]

THACKRAY: So taking an MD/PhD at San Diego had become almost foreordained by the time

you were doing it.

EVANS: It was actually not exactly planned, because when I applied to medical school, I applied to medical school with the thought of going elsewhere. In fact, I applied to similar programs at other places. I think I applied to six places, and I was accepted to all six, which were Harvard, Johns Hopkins [University], Washington University in St. Louis, Columbia [University], UCSF [University of California, San Francisco], and UCSD. I decided to stay in San Diego in part because I would finish quicker that way, and also in part because of the Regents Scholarship I was awarded. One of the disadvantages of some of the other programs is that, in order to be in their MSTP program, you had to first get into the medical school and then apply to the MSTP in your second year, so there wasn't the assurance you would actually be part of it, whereas here I was already doing it. I was already enrolled in the graduate program.

One of the curious things about this, that is the way I—not planned, of course, but the way this worked out—neither the graduate school nor the medical school knew that I was actually admitted to the other school. They actually thought there were two people with the same name. The problem came about when the chemistry department wanted me to do my oral qualifying exam, and I told them I couldn't do it because I was taking pathology in the medical school. Then they realized—this was before the program had actually started—they realized that it was the same person and they incorporated me in as the first official member of this MSTP program. So I actually ended up being in two departments simultaneously without the other one knowing about it.

THACKRAY: That was quite a juggling feat, wasn't it?

EVANS: It wasn't intentional. I didn't go out of my way to do it. But it was quite unusual.

THACKRAY: It kept you quite busy, didn't it?

EVANS: It was very busy.

THACKRAY: Will you talk just a bit more about those graduate student and MD years?

EVANS: Well, the equivalent of my senior year of college, where I was a first-year graduate student, I took the courses I was interested in in biochemistry and did quite a bit of lab work. At that point, I decided to work in Geoff Rosenfeld's lab at the medical school for my thesis work, and I began working on the activation of hormone genes in pituitary by other hormones. I was looking at the regulation of gene expression. This was just at the point where—it was actually slightly before recombinant DNA became a possibility. We were looking at messenger RNA levels and some

various techniques, as other people were beginning to clone things, beginning to pick up on some of the ideas of how to do that.

Then the next year when I began as a medical student, I had a huge amount of coursework to do. The first year actually went by okay. Towards the end of the first year and beginning of the second year, I realized that I was really not interested in everything they were teaching, so I ended up not going to class a lot of times and I'd work in the lab. As long as you passed the tests, there was no problem. I ended up spending a lot of the time when I should have been doing gross anatomy actually doing experiments in the lab.

THACKRAY: So how did you pass the tests?

EVANS: I found actually that I could pass the tests much easier by reading the books and studying than I could by going to lectures, and that some of the lectures were not very exciting or illuminating. A lot of the students would spend all their time in class and really have very little to show after being in the class. And it was pretty easy, since this was a body of knowledge, not really anything being generated new, and not really teaching one how to think, to solve problems. It was fairly easy—I say this in retrospect; it was a lot of work to learn the material fairly efficiently.

Then after the first two years of medical school, I then—which was planned in this program—took the time off and became a full-time graduate student. By that time I had enough work done and projects of my own going that I could finish in about a year and a half, I think. I finished my thesis work, went back and did the required clinical rotations, and finished both at the same time.

THACKRAY: And your thesis was...

EVANS: My thesis was on the regulation of prolactin gene expression by the hormone TRF [thyrotropin-releasing hormone]. TRF was a small peptide hormone, three amino acids, that was the first releasing factor discovered. A releasing factor is a hormone released by the brain, which controls the release of other hormones. It was actually discovered by Roger Guillemin, who was a professor here at the Salk Institute, who won the Nobel Prize for that discovery. It was not only an exciting area to be working, but having this very distinguished scientist across the street who actually initiated that study was very awe-inspiring.

THACKRAY: And this was in the chemistry department?

EVANS: It was in the chemistry department, though my work was actually done in the medical school. It was in the chemistry department partly for the following reason. In the medical scientist

program one could choose any department that one wanted to do one's thesis. Most of the students chose biology because it fit closer with what they were doing. Because I started in the chemistry department, the only requirement the chemistry department really had was to pass four examinations in four fields—physical chemistry, organic, inorganic and analytical chemistry. Most of the students didn't choose that because if they didn't pass physical chemistry they had to take classes in it. I happened to pass all four of them, so I didn't have any course requirements in that department. So it made really no difference to me to be in chemistry, even though what I was doing as a thesis was not really chemistry. It was really molecular biology.

THACKRAY: You mentioned you had a couple of publications as an undergraduate. Were you publishing as this work was going along, or not?

EVANS: The undergraduate work?

THACKRAY: No, while you were in medical school and doing graduate work.

EVANS: When I was in graduate work, I think the first paper I published from that was at the end of my first year of graduate work, and then as time went on there were eight or nine different papers that we wrote on various aspects during graduate work, some of which were primary research, and some of which were review articles.[2]

THACKRAY: The we…

EVANS: The we being myself and my thesis adviser. So what actually happened is, I ended up doing the work and then writing the papers and he would edit, telling me where I was wrong and things like that.

[2] G. A. Evans and M. G. Rosenfeld, Inhibitor of protein synthesis co-isolating with polyribosomal RNA, *Biochimica et Biophysica Acta* 390 (1975): 342-351; G.A. Evans and M.G. Rosenfeld, Cell-free synthesis of a prolactin precursor directed by mRNA from cultured rat pituitary cells, *Journal of Biological Chemistry* 251(1976): 2842-2847; G. A. Evans, J. Hucko, M. G. Rosenfeld, Preprolactin represents the initial product of prolactin mRNA translation, *Endicrinology* 101 (1977): 1807-1814; G. A. Evans, D. N. David, and M. G. Rosenfeld, Regulation of prolactin and somatropin mRNAs by thyrotropin. *Proceedings of the National Academy of Sciences USA* 75 (1978): 1294-1298; G. A. Evans and M. G. Rosenfeld, Regulation of prolactin mRNA analyzed using a specific cDNA probe. *Journal of Bioogical. Chemistry* 251 (1979): 8023-8030; G. A. Evans and M. G. Rosenfeld, Hormonal regulation of prolactin mRNA, in *Synthesis and Release of Adenohypophyseal Hormones*, eds. M. Jutisz and K. W. McKerns (New York: Springer Publishing, 1980), 295-309.

THACKRAY: Was that relationship a particularly crucial one?

EVANS: I think so. It was and still is a very important relationship, not purely from a scientific point of view but also, I think, on a warm, personal basis, partly because I had I think really known I wanted to do molecular biology. In the chemistry department I began working in the area of inorganic chemistry because that was the person that I had worked for as an undergraduate. The first few months as a graduate student I actually continued that with the thought of becoming an inorganic or bioinorganic chemist. It became very clear to me very quickly that that really wasn't the area I wanted to work in.

So I was then struggling with picking up, leaving one lab while on good terms. Not necessarily—I was leaving voluntarily, but they would have liked to have me stay very much, and I needed to find another lab I was interested in.

My girlfriend at the time, who's now my wife, worked at the medical school for a secretary in the endocrinology department, and we were talking about where I should go, what I should do. And she said, "You have to go talk to this young guy, Geoff Rosenfeld, who has just come as an assistant professor." I went to talk to him, and I was so excited by his enthusiasm in what he was doing that I then began to work in his lab and ultimately stayed there. I was his first graduate student. Most students would hesitate to go into a lab and work for someone who has no track record, has never done really anything in science. I think part of that was just the enthusiasm that he had himself and still has, and inspired in me. I think in the long run it was a good choice.

THACKRAY: So run me forward now. What happens as you graduate?

EVANS: Well, as I was graduating, there comes another great decision point of one's life in that kind of a program. Do you go and be a clinician for several years and do the internship, residency, clinical approach? Or the opposite is to say I want to do the PhD side of my life and go off and do a postdoctoral fellowship and then look for basic science positions. At that point no one is really sure what to do.

The decision I made was to go ahead and do, at least try being a clinical doctor. I'd never done that before except as a medical student, and I thought if I'm going to decide not to do that for the rest of my life, I'd better be darn sure that I know what it is that I'm not going to do. So I applied for internships, and decided to go to Stanford partly because it was, in my mind, a more intellectual atmosphere. The hospital and the basic science departments were in the same building.

I went as a medical intern in the department of internal medicine and did all the usual things that interns do, stay up all night for three weeks at a time. But it's clear my love is really in the laboratory and not in the clinic, although I actually enjoyed clinical medicine and was quite good at it.

But I had also—trying to figure out what to do at that point—and I had applied to Phil Leder's lab as a, it was then called a medical staff fellow at the NIH. That was a postdoc for MDs. He accepted me into his lab for his department. I found out about the acceptance about the same time I was starting my internship. I spent a month doing that and said no, I'm going to go to the NIH at the end of this year. So I finished up the internship year, which allows one to become licensed as a physician, and at that point moved to the NIH with the idea that I'll spend a year or two doing research and then maybe eventually will go back and do some more clinical work. But after being in that environment for a few months, it was very clear that that's really what I wanted to do, and that's what I've been doing ever since.

The other thing that was a big change for me in going from both medical school and as an intern and into the NIH was, in the earlier two experiences, I was very unusual. There was virtually nobody else with the background that I had or with the interests that I had. In medical school most of the people wanted to go into private practice. There were only a very few students interested in research. At Stanford, working in the hospital there was virtually no interest. Everyone's interested in the patients, number one, and their real estate ventures, number two.

And then when I went to the NIH, virtually everybody in the lab had the same background and the same enthusiasms that I did, because the lab was full of MD/PhDs who wanted to work in the lab and who had almost the same kind of background that I did. So it was like coming home. It was a really exciting time.

THACKRAY: That was good, not depressing.

EVANS: That was very good for me. It was nice to find kindred spirits. Where I had come from there really wasn't any kind of kindred spirit. It also gives you a lot of reassurance that people value what you do. You're not really so unusual.

THACKRAY: And that position, at NIH, that carried funding with it.

EVANS: Yes, that actually was one of these unusual positions that has since been discontinued, where one is appointed a commissioned officer in the military in the Public Health Service, which has a Navy rank with it. It became very popular during the Vietnam War as a way to keep physicians who didn't want to go into the military out of the military. It also paid a lot more than the usual postdoc, so it was a regular job, but it was at a clinical level for a young clinical person. And it had some other advantages—we could go to the officers' club at the Navy hospital across the street and get in, because we were officers and we had little cards that we would show them.

There are a few people who were in my same group then who are now still at the NIH, and they're now captains or lieutenant commanders or something, but they don't make any more appointments of that kind. One of my greatest pleasures in moving back here was showing

everybody my little Geneva Convention card, which says on the back that if you're captured by the enemy you only have to give your name, rank and serial number.

THACKRAY: What about the research that was going on in this lab?

EVANS: The thing that attracted me to that lab was primarily the work that Phil Leder and his colleagues had done by essentially solving the problem of antibody diversity, which is how one can make a virtually infinite number of antibodies from only a few genes. That was a major enigma, and the solution to that of course is that the genes rearrange in development, which no one had predicted but which turns out to be an elegant mechanism for doing that. And a number of labs, Susumu Tonegawa—he was a graduate student at UCSD at the same time I was an undergraduate—won the Nobel Prize for that [Physiology or Medicine, 1987]. A lot of us think that Phil Leder should have shared it with him, but the Nobel Committee didn't agree. But the piece of work in essence was to show that genes recombine. That discovery was made and published immediately before I began looking for places to go. And I found that one of the most exciting scientific discoveries that one could conceive of, because it was so elegant and so simple and such a beautiful mechanism. That was really the work that stimulated me to look in those areas.

When I entered the lab, it was at a transition point for that group. Phil Leder was a department chairman who had a number of other people in his department working in various areas. His laboratory worked on immunoglobulin genes. When I decided to go there, that's what I had thought I wanted to work with. When I got there, the first thing I realized was that the lab was very big and there were a lot of people working on various aspects, and the major problem of immunoglobulin diversity had been solved. In fact, there were a lot of other things that were interesting to work on. But the real beauty was in the one elegant mechanism.

At that time Jon [Jonathan G.] Seidman, who was a postdoctoral fellow of Phil Leder, had just finished and was offered a job at the NIH, accepted that in the same department. His idea was to begin to look at major histocompatibility antigens, and that is a class of proteins that determined tissue type. It determined whether skin grafts would be accepted or rejected between different mice, and very little was known about that.

I, after talking to a lot of people in the departments, decided to work with Jon because I thought that was such an important problem and also had the potential for uncovering something really unique. Thus, I went there actually to work on one problem, that of antibodies, and within a few weeks switched around to doing something else. And I think in the long run that was a good decision. I'm still fascinated by histocompatibility antigens. At that time no one had applied molecular biology to that problem. We were some of the first people to clone and understand the structure of those genes.

THACKRAY: At that moment you were still fully funded. Describe the lab structure to me, the number of people, the nature of the group…

EVANS: The laboratory was called the Laboratory of Molecular Genetics, which was the department that had Phil Leder as the head and about six other investigators, each with independent labs. Phil had a group that was very large, probably about twenty to twenty-five people. It occupied about half of the entire department.

THACKRAY: When you say people, how many of those were…

EVANS: Probably twelve to fifteen doctorate level, postdoctorals, several technicians, one graduate student; in fact, he is also a Pew Scholar now, Phil [Philip A.] Hieter. And in fact another postdoc, Jeff [Jeffrey V.] Ravetch, is also now a Pew Scholar. And then various other visiting scientists who were there. So the entire number of people in that group was about that.

The remainder of the people in the department, the principal investigators, would have maybe one or two people in their lab. Jon Seidman, when he started his lab, initially had two people, maybe three. But I came at almost the exact time he started his own lab, so initially he was the principal investigator and there were two postdoctoral fellows, myself and David [H.] Margulies. And then soon thereafter two more came. But the group was essentially five people.

THACKRAY: So you were a little spinoff group from this large group—somewhat like cell division.

EVANS: We were a little spinoff group. It is, but it was also very integrated. The lab meetings that we would have would be the entire group, Phil Leder's and Jon Seidman's, so we would present our findings and talk about our work with all the people working on immunoglobulin, and also people who were working on oncogenes at that time in Phil Leder's group. So we actually had the best of both worlds. We had all the connections and the contact with this world-famous group, yet we weren't just one of twenty people all doing the same thing. At least from my mind, that was an advantage.

THACKRAY: How is that sort of scale thing playing out? Do you see any changes in the business in the time that you've been in it?

EVANS: I think that there are changes in that it's more difficult, at least in my mind, it's more difficult now to work with a small group than it was at that time. Part of that is, I think science in some ways moves in waves. When a new technology is developed, or a new approach, a new idea, there's a wave of activity. For instance, protein sequencing, when that was derived. Then DNA sequencing, where all of a sudden a lot of things that were difficult before become very easy.

There's a huge amount of activity. Then it dies down over a period of years because one's waiting for the next breakthrough. What I see now is, in some ways some of the things that were easy before are passé, but there's this new set of things on the horizon. And one of the things it requires is more people, larger groups. The other thing it requires is automation.

THACKRAY: You're talking about—what's on the horizon?

EVANS: Well, what I see on the horizon—I guess I can step back and explain this. One of the major discoveries that affected the way I look at things was, first, the structure of DNA. After that there was a flurry of activity covering RNA and tRNA and many different things. Then it began to slow down. In the mid-1970s came recombinant DNA. We could clone genes, and we could begin to look at individual genes in detail. That's been going on now for fifteen years or so, and it's beginning to slow down. Now what's in the immediate future is the idea of analyzing entire genomes at once, and it's an increase of several orders of magnitude in scale. I say this partly because one of the things we're very heavily involved in now is how to map chromosomes, not just single genes. So I can optimistically, anyway, foresee that in ten years the kinds of things we do will be completely different than what we do now. We'll be dealing with millions of bases, not just thousands of bases. But in order to do that, we don't yet have all the tools that we need. We're developing them, we as the scientific community in general. But those kinds of approaches now require a lot of people. You can't do it with just one or two people. It requires a lot of hands and different kinds of expertise.

THACKRAY: Do you see that impinging on how life goes on at Salk, for instance?

EVANS: I think it clearly will impinge on how an institute like this works, because—there is a lot of talk about cottage industry versus big science. I think it will still be cottage industry, which is what we do now, but I think it's moving towards the idea of teams working, rather than individual investigators. It's not that the individual investigators can't work very effectively, but they have to work by coming up with entirely new creative ideas. If they want to do the same kind of thing everybody else is doing, it requires more people. I actually would rather be doing the clever things that one or two people would be doing, but it's difficult to come up with an entirely new approach.

THACKRAY: Can you actually see changes in the organization of the structure here and elsewhere and in the size of the groups and in the number of people in the cottage? We all look at clouds on the horizon and wonder.

EVANS: This Institute is actually a good example of the kinds of changes that occur. When it was first founded in 1965, the early 1960s, and was built and people were recruited to come here, there were really only six labs. There were six fellows at the Institute who were scientists of very high

reputation who had very, very large groups. Renato Dulbecco's lab, for instance, who was the person I originally worked with when I was in high school, was a group that took up this entire floor, or the floor above us. It was an entire wing and probably fifty people.

So there were several very large groups. One of the most successful ones was Roger Guillemin's group, which was probably at least fifty people, but that group was responsible for starting an entire new area of science, the area of neuroendocrinology. They were the group that, as I mentioned, discovered releasing hormones, and Guillemin won the Nobel Prize for that [Physiology or Medicine, 1977]. But to do that, to purify these things, one example is they collected 100,000 sheep hypothalami in order to extract the substance, which is a very small piece of tissue. It required a very large group.

The Institute was founded with that in mind. But as time went on, it turned out that many of those groups were financially not very efficient. They got very large, but they weren't very productive in bringing in grants. For a while the Salk Institute was in serious financial trouble because of these large commitments. So it then moved to the idea of the small laboratories, one investigator with a few people, which is now what the majority of the Institute is. The idea was that those were more productive per capita. They were also more financially feasible. You could raise more grants for these small groups. And I think that's probably true. But I almost see it now moving in the other direction, which is to compete on some of these larger projects, which is really the direction molecular biology is moving. It's moving toward the larger group again. It's almost evolving back. I'm not sure it's going to be only six laboratories, but you can see it change.

THACKRAY: So Salk actually was in big science almost before it was there.

EVANS: It was in big science from the beginning.

THACKRAY: Let me take you back into NIH and your little group. Can you talk a little about what you were doing there? Were those years . . . what are the crucial breakpoints intellectually in what you've been doing?

EVANS: I think the approach that one had at that time, and it's probably still true, is one can define what the problem is. That is, what piece of biology do we want to understand? And what that requires is understanding the structures of the proteins and the genes that are involved in it. Then from a technical point of view in order to do it, one has to clone the gene, so the entire search or effort is to clone the gene. It's getting easier as time goes on because there are many newer techniques, but at that time the entire effort was to clone the gene for the major histocompatibility protein.

This is when I went to the NIH, which was just after leaving a clinical internship, with all the insecurities that that involved, being out of the lab for a year, going into a new field, not knowing

whether you can compete and remember all the things you have to remember. I went into a lab that was very new. The director of the laboratory was about two years older than myself, so he was essentially a peer rather...

[END OF AUDIO, FILE 1.2]

EVANS: We knew what the problem was, and we knew what we needed in order to solve it, but the critical clone, the critical reagent was not at hand. So the first six, seven, or eight months was a very uncomfortable time of working very hard, trying a number of different approaches, some of which were utter failures, knowing that there was hot competition from the other laboratories, which was always a big consideration. You want to be the first to clone it. You don't want to read about it in *Nature*.

That was a great time, but also a very tough time, because we didn't know if we were ever going to get it. Around Christmastime, in fact, we got a clone, which turned out to be the right thing. That actually was the critical break point. After that time, everything went very quickly. We worked harder than ever, but it was very clear what to do, and everything went very smoothly. Before that time, we had no real sense that we would actually succeed.

The one thing I remember quite distinctly was, we were not alone doing this. There were several other groups working on it. We got a clone which we thought was the right thing, and another group at Harvard also got a clone that they thought was the right thing, and we actually arranged to exchange clones. Jon Seidman, the lab director—while it was competitive, it was not hostile. There were a lot of good feelings and a lot of exchange of information. And I distinctly remember that one of the people from that group was coming to the NIH to give a seminar and was bringing with him the clone from their group. We of course very much wanted their clone, to see what it was.

He walked into the lab with Jon and they were discussing and getting ready for the talk in a very relaxed way. I remember he said, "Oh, yes, I happen to have the other clone here," and handed it to me and walked out. I immediately put it into the ice bucket, had everything set, ready to analyze it. Within five minutes it was transforming bacteria. One day or two days later was Christmas Day—it was one of the few times I've gone into the lab on Christmas Day—pulled out the clones and began the analysis. It turned out in fact to be right. But this just shows you the kind of mindset into which one gets oneself. And after that time, after Christmas, the beginning of the year, everything went very smoothly, very quickly, and I think we wrote three or four papers within the next six months to a year.[3]

[3] D. H. Margulies, G. A. Evans, L. Flaherty, and J. G. Seidman. H–2-like genes in the Tla region of mouse chromosome 17, *Nature* (1982): 168-170; G. A. Evans, D. H. Margulies, R. D. Camerini-Otero, K. Ozato, and J. G. Seidman. Structure and expression of a mouse major histocompatibility antigen gene, H-2Ld, *Proceedings of the National Academy of Sciences* 79 (1982): 1994-1998; D. H. Margulies, G. A. Evans, K. Ozato, R. D. Camerini-Otero, K. Tanaka, E. Appella, and J. G. Seidman. Expression of H-2Dd and H-2Ld mouse major histocompatibility antigen genes in L cells

THACKRAY: So that really put this little group and you on the map.

EVANS: Exactly. It was that…

THACKRAY: This was in your first year?

EVANS: This was in the first year of my postdoctoral fellowship. Again, the period of time that I spent at the NIH was divided into two halves. The first two years I was in this group of people working, after our initial clone, very intensely on various aspects. The group got bigger. There were other people that came that were working on other projects not directly related to this.

But about a year after I arrived, it was announced to all of us that Phil Leder had been offered and accepted the job of the chairmanship of genetics at Harvard and he was leaving the NIH. And Jon Seidman announced that he had been offered a position there and was leaving also. Some of the people that had come a few weeks before that announcement were quite upset because they were going to have to move again.

From my point of view, I was offered a position there in Jon's new lab. But because I had already been there two years and was really considering, after a third year, going on and finding my own job rather than staying any longer, I decided to stay at the NIH. What happened was that the entire department picked up and left, and there were maybe three or four of us that were the more senior ones that stayed in the same space. That was actually the best year of the entire experience, because I'd already learned a great deal. I knew all the techniques, I had all the facilities, and then my boss left and I was left to myself. It was great fun to actually run my own lab for the first time. It was just me and another postdoc who was working on his project. But actually most of the things that I was most proud of and felt were the most creative were done during that time. At that time I was then looking at jobs in various places and deciding where to go.

THACKRAY: Was staying at NIH a real option?

EVANS: Yes. I was offered a position as the equivalent of an assistant professor at the NIH in the new laboratory of molecular genetics, which was being reorganized after Phil Leder and his group left. At the same time I applied to a number of other places. I was recruited by the Salk Institute in

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after DNA-mediated gene transfer. *The Journal of Immunology* 130 (1983): 463-470; and G. A. Evans, D. H. Margulies, B. Shykind, J. G. Seidman, and K. Ozato, Exon shuffling: mapping polymorphic determinants on hybrid mouse transplantation antigens, *Nature* 300 (1982): 755-757.

part because I knew a number of people here from having been here before, and my thesis advisor, Geoff Rosenfeld, was at that time actually here at the Institute and was lobbying very hard to have them recruit me back. So at that time the real decision came: would I stay at the NIH or would I move and come to the Salk Institute? That was another very difficult decision, to know which was the right thing to do.

THACKRAY: Talk about that choice.

EVANS: Well, the things I think that influenced me were partly not so much the Salk Institute, which I knew very well and had been at. But things changed so much at the NIH, at least in the building that I was in, because I was in the midst of this incredibly exciting, vibrant group of two dozen people exactly like myself, very enthusiastic, energetic, very bright people, and it picked up and left. While it was fun to remain behind, it was also a void. It was not as exciting. It was not as much fun to interact with the people next door, because they weren't there anymore. There were new people coming in, but I wasn't sure what that group was going to be like. I knew who the new lab director was, and it was someone I respect very much, but not quite in the same way. In some ways I was looking to where I wanted to be, who my next-door neighbors were going to be, what my lab was going to be set up like. I saw the two as fairly equivalent from an intellectual point of view. From a practical point of view there were a lot of differences.

The NIH is all internal funding, so you didn't have to write grants. On the other hand, your group was only as large as you could convince the administrative structure it should be, whereas here you're entirely on your own in terms of funding. You have freedom to do whatever you want. It's a very intellectually free area, and if you can write the grant to do it, you can do the experiment, which was somewhat different from the NIH. So I think they're very different places. This is also an extremely small Institute, and the NIH of course is very large.

THACKRAY: Is there a problem with being small?

EVANS: As far as the Institute? Actually, I think it has very great advantages. That's my own bias—in that a small place has the potential of being extremely efficient in how it does things. One of the things that is true of the Salk Institute that is not true of the NIH is the support facilities and personnel and purchasing department and photo labs and equipment labs and the building people that renovate lab space are incredibly efficient compared with the government, or even, I think, a large university like the University of California. So that's a very distinct advantage. And the Institute's philosophy originally was to make the place ideal for doing scientific research. They have lived up to many of those—maybe not every promise.

THACKRAY: So you chose to come here.

EVANS: I'd also mention that there were also personal things that, with my family being in California, also had a little bit to do with it. My wife at the time was pregnant, and we were deciding not only where we wanted to live and where I wanted to work, but where we wanted to bring our children up. I think that also played a big part.

THACKRAY: Let me go to Salk now. Your title is assistant professor, then associate professor. In that sense, how many faculty are there here?

EVANS: There are, I believe, about thirty-six principal investigators, meaning people with their own labs. There are several other faculty who have the title of staff scientist, who are faculty but within some other umbrella laboratory.

THACKRAY: Do you know what all these people are doing?

EVANS: I don't know the intimate details of exactly what it is, but I'm very interested in a lot of areas that are unrelated to what we do. For instance, one of the faculty members that I actually deal a lot with now, Ursula Bellugi, is a neuropsychologist, and she studies American Sign Language among the deaf. She also has been working with children that have a disease called Williams syndrome. Williams syndrome is a birth defect that leads to mentally retarded children—they're called pixie children. They have a very strange-looking face, but they have more advanced language ability than their peers. So she's very interested in higher cognitive functions. I would at least like to think about ways that one can apply the kinds of things we know how to do to understanding that. I'm not sure that we have the tools for that particular problem, but I'm very interested in it.

I have a fair idea of what most of the people here do, but I don't profess to understand it in any great depth. Terry Sejnowski, who is the head of a new laboratory called cogitational neurobiology, works on neural networks, which is actually a part of computer science. It's a way of mathematically linking objects to enable them to process information and to learn. He's using that to make models of how the brain might function. We were actually thinking about how we could use that to solve problems of comparing DNA sequences, because it allows you to write a computer program that can learn in various ways. Whether it can be done, I'm not sure.

THACKRAY: So you've come to Salk. This is the moment when you have to start funding yourself, isn't it? And also deciding what you're going to do.

EVANS: Well, both of those I think are important considerations. One of the things postdoctoral fellows always get very insecure about is, what are they going to do when they go and start their own lab? The most important thing is, can they take the project they're working on with them?

Various labs have different philosophies on that. One thing that can happen is you end up working on the same thing that you worked on as a postdoc, but now you're in competition with your old advisor rather than in collaboration. The other possibility is you decide to do something completely unrelated and have trouble getting a grant because you have no experience in that area.

Phil Leder's philosophy was always that the postdocs should start something entirely new and shouldn't work on the same thing he was working on, partly for his benefit, partly for their benefit. He encouraged postdocs as they were towards the end of their time to think about other things and actually do a few experiments on other things before they left.

Now my advisor, Jon Seidman—and I talked about this with him before leaving to decide what to do—he was of a similar philosophy that you should really work on whatever you're interested in, whatever you want to, but he's not going to give you your project. He's going to continue to work on it so it would be somewhat competitive. What John and I decided is that I would write a grant on whatever I wanted to, he would continue to do whatever he wanted and work on those areas, and if we ended up being competitors that's okay, but we'd maintain good relations.

What actually happened is that I wrote a grant on exactly that, got the grant, and then didn't do anything that I said I was going to do in the grant. I used it to do an entirely new area, and did very well at it. And what really matters to the granting agencies is that you do something interesting and creative along those lines, not that you do exactly what you said you were going to do. Jon, on the other hand, got areas of interest in other directions and dropped that project entirely, so in fact neither of us ended up doing that, rather than both of us working on the same thing.

THACKRAY: Who was the granting agency?

EVANS: NICHD, [National Institute of] Child Health and Human Development. And in fact, when I wrote the renewal for that grant, what I actually proposed to do was to continue the analysis of some of the same kinds of genes that I'd worked on as a postdoc, proposing models of how they might function. What I actually did was the same thing on a different class of genes, which we cloned ourselves, and which we weren't in any competition with. And then what the agency said when they reviewed it was that, "Dr. Evans did a very clever thing in that he began to work on one area and realized that that wouldn't answer the questions that he wanted to answer, and then applied the same technique to this other set of genes." And I was actually complimented for the fact that I had changed directions, not grossly, but at least redirected things in a way that was probably more successful. I think in the long run it was a good thing. And it's actually been a very successful set of experiments.

THACKRAY: That was a grant that started up in '83.

EVANS: Started up in '83, and I wrote that grant actually while I was still at the NIH.

THACKRAY: You also had a Keck grant in ’83.

EVANS: I had some funds from the Keck Foundation. That was actually arranged by Max [W. Maxwell] Cowan, who’s now the vice president of Howard Hughes Medical Institute. He used to be the scientific director at the Salk Institute. What happened, which I remember quite distinctly, was I was here in my own lab, very small. I had my first grant. And someone came that wanted to do a postdoctoral fellowship with me, and I said, “Well, I’d very much like to have you. I don’t have any money to pay you. But I’ll go ask.” I asked Max. Being very naive at the time, I didn’t realize that one probably doesn’t do these things in polite scientific circles, but I asked him if there was any possibility of having funding for a postdoc for a year. I remember his comments quite distinctly. He said, “Well, you’re from the NIH. Let me show you how fast and efficient things can be done at the Salk Institute.” Then he picked up the phone and within ten minutes I had a salary for this postdoc, which came from the Keck Foundation.

THACKRAY: Will you talk about three things: the size of the group you’ve had over the years from then till now, the funding of the group—because I see you’ve had multiple funding, and what you’ve been up to?

EVANS: Okay. The first thing is the size of the group. Until very recently, this was a very small group, which consisted of myself, one postdoctoral fellow, one technician—who was actually Geoff Rosenfeld’s wife, my thesis advisor, who has worked for me since I came here—and a number of graduate students. I very much enjoy dealing with graduate students, and while there aren’t as many students here as there are at the University, there are a few, and I’ve always gone out of my way to try to attract them.

THACKRAY: So you’ve had what, one or two at a time?

EVANS: Four, I have four now.

THACKRAY: They’re writing a thesis for you.

EVANS: They’re writing a thesis for me.

THACKRAY: So you’re licensed to...

EVANS: No, actually I'm not, but I will be soon. But we've managed to find our way around—the criteria for a student coming here is they have to really want to come here, because it's much more difficult for them than staying at the University.

THACKRAY: So this isn't typical for Salk people either.

EVANS: It's not typical for the senior Salk people. Senior professors all have adjunct appointments. The junior professors, none of them have adjunct appointments, because they don't make those easily any more. I should have such an appointment as of the first of the year. But so far during the last year it has been very difficult. And the students have been selected in that they're willing to go around the system in order to arrange to come here.

THACKRAY: It's a real bootlegging operation.

EVANS: It's very much on a shoestring.

THACKRAY: So you've really wanted to do it.

EVANS: Oh, yes. I very much enjoy students.

THACKRAY: What's the value added to you?

EVANS: Well, the practical value, which is really not very much, is that they stay longer than a postdoc. A postdoctoral fellowship is usually two years or three years. Students are here for usually four years or five years. But in actuality, they require such a large amount of attention and training that it really takes them a while before they can work in the lab effectively. The real value is that I enjoy teaching the students. They're much more willing to think about problems, and they're more willing to do some of the things I would suggest to them than postdocs. Postdocs are, in general, very focused on a particular problem. They want to grind out their work. They want to write the papers and begin looking for jobs. Graduate students are much more intellectual about it, actually. They're much more interested in science for science's sake. I find them much more fun to deal with. I think the real reason that there are students here is because I like having graduate students in the lab.

Until recently I've had only one postdoctoral fellow at a time, partly because as a young assistant professor without a reputation, you don't have a lot of people knocking on your door. In the

last year, for one reason or another, because the work is becoming very visible, there are a lot of people applying all the time, so I've had to—there it's quite the opposite. I have to pick and choose and be very careful who I have in the lab. And so now there are eight postdoctoral fellows in addition to graduate students, and a couple more who may be coming.

So just in the last year the group has expanded quite a lot. Part of that is the attention that the work—when the work goes well a lot of people want to come and help work on it. Partly, the kind of thing we're now beginning to do requires more people than the very small groups. It's shifting slightly to something else.

As far as funding, the Salk Institute has no internal funding of any kind, so I pay my entire salary myself. I've always been worried about having a single NIH grant supporting you, and then if you lose the grant you have no funding whatsoever. So it was clear to me when I originally came that I would have to have at least two grants, just so I could split things up that way. So I have the initial grant, which I wrote before I came. The day I arrived, I started writing the second one. I've been lucky in that they've been funded both times. Those have really supported the majority of the work.

There have been a number of other smaller things. The March of Dimes has supported our work for the last several years. The Pew award actually, as I've told them in the past, the thing that's nice about it is not that it's a large amount of money or anything, but they don't tell you what to do with it. It kind of gives you a little freedom that you don't have with NIH grants, either to buy something or hire a graduate student you couldn't have hired otherwise. They are not really looking to see that you do what you said you were going to do. I think they really want you to do whatever you want to do with it.

There have been a number of other smaller grants that I've applied for. In general, what I feel now is [that] it's such a lot of work to write an application that you might as well apply for a big one rather than a small one, because you have about the same chance of success. You also find that you spend a large amount of your time writing grants, which I don't particularly enjoy, but it's part of the business.

While the lab was small, there have essentially just been these two grants that have supported it. As it's gotten larger, the postdoctoral fellows all have their own fellowships, which they write after they get here or before they come, so they're really independently funded. The graduate students don't take a lot of funding, but I kind of get it off of one thing or another. The real limitation we have is equipment, because the equipment is becoming so expensive. It's hard to get 170,000 dollars for one microscope, so that I'm still working on, how to figure that out.

Now we are essentially funded by those same sources. We have another grant from the Department of Energy, which I got two years ago at the beginnings of the Human Genome Project, and that's what a lot of what our lab is now doing is related to. And I see that the funding for that is increasing. We actually began doing that work before that became such a visible thing, such a publicly exciting area.

THACKRAY: I see you're on one or two committees now that...

EVANS: I'm on a number of those review committees. Since this is a new project and it's very controversial, they seem to want to get everybody's opinion about what to do.

THACKRAY: Talk just about how that is going right now, since maybe thirty years from now that will…

EVANS: I'll tell you how it's going. It's very clear to those of us who are actually doing the nitty-gritty bench work that this project can really be done. There's no question. There's also no question in my mind that the rewards, scientifically and intellectually, are going to be far greater than anybody imagines. Among them, in the core of people who actually are in the doing of it, there is a great deal of enthusiasm. I can imagine it was like the time before the first linear accelerators were started in physics. It's the beginning of a new way of approaching it. I'm particularly excited about it. Not everyone shares that enthusiasm, not because they don't see the scientific benefits. It's because of the political and the cost involved. What they see, and I can't say that I disagree, is that this will take a large amount of the budget for science funding and will put it into something that will undoubtedly take away from other people's projects. Now a lot of the high-level administrators say that's not the case, that it really is a separate pot of money, but I don't think anybody is absolutely convinced of that. It is getting more difficult to fund research, even without this project, and I think this may contribute to making it a little more difficult. On the other hand, if I were a clinician in the hospital, and I was given a list of projects that I could say money should be put into based on what will come out to benefit my patients, I would put it into this project. There's no question about it.

There are a lot of areas of science that intellectually are very interesting, and they have the potential for generating things of great value. But this, there's no question it's going to generate things of great value.

THACKRAY: Is that argument going on in the committees that you're sitting on, or is that argument essentially over now? Are the committees staffed with people who are believers?

EVANS: This argument was going on several years ago, two years ago. It's now, the project is going to be done unless Congress changes its mind, which is always a possibility. They are fairly convinced that this is an important area that this magnitude of effort should go into it. Not only is it decided that it will be done, there's a five-year plan on how it's to proceed.

THACKRAY: I'll use a Swansea metaphor and say you're very much at the pole phase in this.

How many players are there like you, working on your level of competence? How do you number your peers as opposed to simply people working in the field?

EVANS: Well, I guess the way I would answer that is from all the meetings I've been to, there probably are thirty people or so who are as interested in this as I am and at the same level, that give talks at all the same meetings, that have groups or are putting together groups and are generating enthusiasm to do it. I'm not convinced they're all as focused. Because a lot of them, without naming names, are very senior scientists that are very well known in their fields, but they're also doing a lot of other things. In my mind, the people that are really serious, that clearly are going to do it even if the government does cut back all the money, there's probably a handful or so. It's hard to gauge the numbers. We actually were involved in this long before there was any idea of how to fund it. It was fun and an interesting approach and a new way to approach genetics. As the funding begins to appear, a lot of the people kind of jump on because it's clear that that's something you can get a grant to do. I try to at least convince myself I'm not one of those. But I think it's important to do science because you're enthusiastic and interested in it and not just because you can get a grant to do it.

THACKRAY: Which are the other labs you would be looking at right now, where is your hot competition?

EVANS: In this particular area there's not as much competition directly, that is, people doing exactly the same thing, but people doing the same thing in different areas. It's different, because while it's competition for attention or whatever, for appreciation of one's peers, which is really what this amounts to, there actually is a lot of collaboration, as we exchange things, the tools that you develop…

[END OF AUDIO, FILE 1.3]

EVANS: …the tools that you develop are exchanged freely between—we exchange them with everybody, even if they're working on the same thing. But the other areas in which there's a lot of, geographical areas, are St. Louis, MIT [Massachusetts Institute of Technology] in Boston, Michigan—the University of Michigan—UC, San Francisco, Baylor College in Houston [Texas]. There are probably a few that I haven't thought of. There's a group at Harvard, besides the group at MIT. Those are the areas where people have an active, ongoing program, and really very well into the work.

THACKRAY: What about, do you look overseas in the same way?

EVANS: Oh, yeah, in other countries the most active groups are in Great Britain. There are several, the MRC [Medical Research Council], the ICRF [Imperial Cancer Research Fund], and various portions of the MRC which is in different places. There are groups that have interest in other countries, some of which are very good but don't have the resources to do it. There's a group in Italy that is beginning to work on the human genome. The Italians don't have a great reputation in that area of science, and I'm beginning to realize why. It's not that they're not good scientists, because when you see the individuals there they are terrific. They are mainly trained in the U.S. or in Great Britain. But they don't have the resources and the government structure—how research is funded is so different in Italy that they can't possibly do it the same way we do. There's a group there. There's a group in the Soviet Union that apparently is working on it. They have a lot of reagents we gave them, though I don't know exactly what they're doing. There's a lot of discussion about the Japanese, but I think it's all discussion. I haven't really seen anybody there who's serious and hot into this area.

THACKRAY: Go back to the instruments. You mentioned the 170,000-dollar microscope that you were eyeing. But what have you actually, what have you got in your lab and what would you like to get in the lab?

EVANS: One of the things that is becoming very apparent is that this kind of biology, like certain kinds of physics, is very high-tech and it requires a lot of instrumentation. Probably more so than molecular biology has done in the last twenty years, by many orders of magnitude. The kinds of things we have access to now, we have a robot that the Department of Energy bought us, and one of the areas that is just beginning to come into the forefront is automating a lot of these repetitive procedures. So we have that. When we end up getting something like that, how you pay for it is never actually clear. When you buy something like that, you kind of look and put all these grant numbers on it and hope that they happen to have some money left in them and you can buy it. We have that. We have a number of complex gel apparatus for separating very large fragments of DNA. The institute has a cell sorter, which was built by someone here, which we're now using to purify chromosomes individually by passing them through several laser beams as they come out through an orifice.

That's the kind of high-tech equipment we have. Now there are other things that we need. The approach that I've taken is when we need something, the first thing is to find out who else has one. Is there anybody in the area that has one? This microscope, for instance, is called a confocal microscope. And what it is is it's a standard microscope that has a large laser on top. The laser beam goes down through the eyepiece, and rather than visually seeing it, the laser scans the image and it digitizes it and puts it into a computer. What one can do, and I'll show you some pictures, the things that we're in the process of doing, is determining some things about chromosome structure. This is a set of human chromosomes, and the gene we're looking at is labeled in yellow. This is what you would see visually. But once it's in the computer, you can then enlarge the chromosomes and measure various parameters. We measure lengths and diameters. And this is all done on the computer using the mouse. This is the gene we're looking at. It allows you to determine a lot of different statistics and do some very precise things, by using the computer to analyze the image. So

it requires a microscope, a big laser and a big computer system with several monitors to do the analysis.

We don't have one, but I found a neurobiologist who does, and so we've ended up doing some of the work anyway, but really in the most difficult way possible. We only work from ten at night to six in the morning when they're not using their microscope. And the postdocs in the lab that do it end up going up there. It requires completely disassembling the machine—because what we're doing is very different from what they're doing—setting it up for our studies, doing the studies, and then putting it back together before morning, and making them of course incredibly angry. It's partly an exercise in science. It's partly an exercise in interpersonal relationships to make sure we can keep them happy, that we're not destroying their experiments. In some ways, I keep telling people, we're doing this on a shoestring, because we're not a very big lab compared with some of the other places doing this. But it turns out to be fun, and I think we do it quite well.

The equipment for this kind of work—I'm not sure how the prices are decided, but it seems to just escalate as time goes on, in terms of what one has to spend to get a machine to do this or that.

THACKRAY: Yes, that's what Robert Hooke said to his colleagues. Okay, I think I'd like to switch you briefly into areas away from the science proper. You mentioned that your wife in an earlier incarnation played a critical role in pointing you in some direction. Can you just talk about your wife's background, what she does today and so on?

EVANS: Well, she and I are almost exactly the same age. Our birthdays are separated by about seven weeks. She was born in the Bay Area around San Francisco. Her father is a businessman who works in downtown San Francisco. Her mother has been a homemaker. We come from very similar backgrounds. She was the youngest of three children and came to UC-San Diego as an undergraduate. Her interest in doing that was in mathematics at the time. When she was deciding where to go to college, she found the school that had the best mathematics department that was in California but not too close to home, and ended up coming to UCSD. We actually met in a freshman calculus class. She went through college in parallel with myself, got a degree in mathematics. At that time we were very serious in deciding. We had decided to go to the same place so we could be near one another. We decided to stay in San Diego at the same time. This was when I was deciding where to go to graduate school and medical school. She became a graduate student in mathematics at UCSD and finished a master's degree and was deciding whether or not to go on to finish her PhD Now unlike myself, who was very focused on doing research, she decided that she didn't really like doing research in mathematics. She liked teaching mathematics, but what a research mathematician does was not—she decided that wasn't for her.

But in addition to that, she actually is a quite talented musician. She's a lyric soprano. So she applied and was accepted to graduate school in music and got a master's degree in music and now really is a professional musician, for all intents and purposes. When we were at the NIH, she had a faculty position at a college, in the music department. She performs quite a lot. She performed with a number of opera companies in Washington, DC, as I said had a faculty job there, and now here has

started a number of groups. She's the director of something called the Orpheus Ensemble, which is a chamber music group that does Bach, does baroque music. She both conducts the small orchestra and then she sings. They do a lot of Bach cantatas. She teaches music, teaches voice. That's her current interest, in that she spends most of her time doing that, although supporting yourself as a musician is even more difficult than supporting yourself as a scientist.

THACKRAY: Do you have children?

EVANS: We have two children. We have a nine-year-old daughter and a five-year-old son. Our daughter was born in Washington, and our son was born in San Diego. They both are very intelligent kids. My daughter, when she started kindergarten, was reading at about the seventh grade level. My son is not quite as advanced in reading, but he's in kindergarten this year and he's learning multiplication. We have them both in a private school here that allows children to proceed at their own rate, because we couldn't see having them in a public school where they would be learning their colors in kindergarten when they're already reading. We're both very serious about education, and that was very important to us.

THACKRAY: What about recreation? Do you ever get out of the lab?

EVANS: Yeah, it depends. What do you mean by recreation? I actually learned to ski several years ago. I'd never been skiing in my life. The story is that I was on a trip once. I was giving a seminar somewhere on the East Coast, and I told the lab that I wasn't going to be in on a particular day. And I came back a day early and I walked in the lab and there was nobody there except for one technician. I said, "Where is everyone?" and she wouldn't tell me. And after a while it turned out they all went skiing that day. I, of course, was rather upset, and I told them, "Now don't you ever do that again, but if you do, invite me." So they did, and one of my postdocs, who was a ski instructor, taught me how to ski. Ever since then I've been a real fanatic about it. I go several times a year at a minimum.

In addition to that, both my wife and I enjoy music a great deal. I play a number of musical instruments, none of them particularly well, but I studied piano when I was a high school student, a teenager and younger. I play the piano and organ and various other things. We have a lot of music in our house. My wife's a very accomplished pianist. My daughter now is a very accomplished pianist. I play, but because my hours are not, I'm not home during the day, and the kids go to bed early, I have an electronic synthesizer that I play, which can simulate virtually every instrument. It's not quite as nice as a grand piano, but you can use earphones. So I play the instrument quite a lot.

I also do a lot of building, both furniture making and, I recently built a room on our house, which was the first time I'd ever done anything like that. I really enjoy manual labor, if you will, making things. We make things in the lab, but the things we make you can't really see easily. We make DNA construction. I like to make furniture. Much of the furniture in our house I built,

actually.

THACKRAY: How does your typical day run?

EVANS: My typical day now, at the present time, is I would get to the lab, get up maybe 6:30 or seven, help get the kids off to school, get to the lab maybe at eight or 8:30. Unfortunately most of what I now do is talk on the phone, write papers, write grants, and meet with people. Then I'll have lunch and work till maybe five or six and then go home. The reason for that is not because I've run out of things to do, but my two young children usually go to bed about 7:30 to eight. My wife is very strict about that, and if I don't get home early I don't see them. It's very important for me to see the kids. Then after the kids go to bed, sometimes if I have energy left I'll do some reading or some writing at home. And if not, I'll do some reading for pleasure or I'll play the piano or something like that.

That's quite different than six months ago. Six months ago I was actually doing experiments in the lab myself. A lot of the thing that our lab is now working on, the Human Genome Project, is based immediately on vectors and techniques that I myself, with my own hands, made six months ago, which I'm very proud of, since I don't work in the lab anymore.

THACKRAY: So the Genome Project is really moving you in terms of what you do.

EVANS: Yes, it is definitely changing the way my day goes, partly because it's, unfortunately, it's the best use of my time. What I can do that postdocs can't do, I can get on the phone and call up Maynard [V.] Olson and I can negotiate for certain things that we need. Or I can arrange collaborations, or I can arrange information from others, and I can advise people on how they should proceed on their project. While it's much more fun for me to be able to do the experiment, it's actually faster and more efficient for me to tell one of the postdocs how to do it. Then he can go off and do it and I can tell the next—not tell them how to do it, but give them the benefit of my advice. I would much rather, however, go out and do the experiment myself, because it is more fun.

THACKRAY: What do you think the story's going to be in five or ten years? Do you know where you'd like to be?

EVANS: Do you mean geographically or you mean in terms of how my day is spent? What I would like—and whether this can be done remains to be seen—is, I would like to have more time to do my own experiments myself. What I mean by that is the main projects that are going on that are giving the results, that are funding the lab, that are the basis of grants, the postdocs are doing quite well, but there occasionally are these crazy ideas that one comes up with that are so far out I can't get someone else to do it, something I'd really like to try. It probably won't work. Those are the things

I'd like to have the time to do myself. I think as time goes on, with luck, I can arrange things so I can do that, at least spend a couple of days a week, half days a week doing something, and then have a technician that follows the cells. The kinds of things, for instance, one of the problems we have with the Genome Project is how to purify chromosomes. If you look at them, they all look the same. They're not very different. The way we can purify them is using this cell sorter, this large machine, but it purifies them based on size. Human chromosomes are small to large ones, but there's a lot that are exactly the same.

So the idea that I came up with that probably won't work was: people very frequently make hybrids. They engineer a cell that's a hamster cell but has one human chromosome. Hamster and human chromosomes are all the same. If we could engineer one where the chromosomes were of a very different size, it would be easier to purify.

So I found out that there's something called muntjac, which is a small barking deer. It's a primitive deer they have at the San Diego Zoo. It only has three chromosomes, but they're huge chromosomes, and the idea is to fuse a human cell with a muntjac cell. What you would have are these three immense chromosomes and a little teeny human one, so it would be easy to pick out the small human one. A postdoc and I actually went down to the Zoo, we got the people and they gave us some fibroblasts from the muntjac. We have them growing in the lab. Eventually, if I can find time, that would be an experiment I'd like to try.

There are some things like that that are a little bit off the beaten track that, they're pretty far out, but I think that that would be fun.

THACKRAY: One last question in a very different domain, and it's prompted by what I see up here, and I'd just like to get it in the record; it's about how you handle the documentation of what you do.

EVANS: Okay, these notebooks here are all the notebooks that I've generated since I've had my own lab. This is all my work, not the work of people in the lab, but my personal work. As you can see, it's evolved, because when I first started out they just had data. Then as I began being more specific, I've changed it to the exact project that it was working on.

THACKRAY: And there are about forty of these.

EVANS: There's about forty of those. These actually end off in [June 1988]. The reason for that is, number one, many of the most important things are now going on in the lab, and what I try to impinge on people in the lab is that they need to be very organized about their data. Some of these notebooks, some of them are very poor in terms of how the data's kept. Some of them, the earlier ones, are very good in that they have a table of contents and—I probably can't show you that because these are really pretty sketchy. These are very simple experiments and the date if you're

lucky. What we've been going to more is something like this which is using computer annotation. Everything from here on is actually not a notebook. It's in the computer.

THACKRAY: What about grant applications and all that sort of stuff? What do you do with that?

EVANS: The grant applications, there's a file cabinet in the other room where all the grants are kept, the final forms. Even the ones that don't get funded, they're all kept there, as well as, now they're all kept in the computer. I find that most useful because I can plagiarize from myself very easily, to pull sections out and cut and paste them together.

Everybody in the lab has this system, and in most cases they've done what I told them they should do, not what I actually did, which is be very organized about how they keep the data.

But actually those notebooks up there are from my postdoc work, which I still have.

THACKRAY: There are another twenty or thirty of those.

EVANS: And the ones on the left, the black-edged ones, are from my graduate work.

THACKRAY: There's another ten of those.

EVANS: So I have essentially all the records from every experiment that I've ever done.

THACKRAY: What about correspondence?

EVANS: Correspondence. I have recently gotten much more organized than I used to be in that I have files in the filing cabinet here with all the correspondence from a particular year. I have a secretary now, so she actually is very good about keeping those records up.

THACKRAY: How long have you had a secretary?

EVANS: Oh, about six months. The two most difficult things to get are, number one, space, and number two, a secretary.

THACKRAY: Is the secretary funded out of the grants essentially?

EVANS: The secretary will be funded out of my grant, but right now it's a present from the Institute, so she actually is the only thing funded by the Institute. It comes out of Institute coffers.

THACKRAY: Is that a function of seniority?

EVANS: It's a function of partly reward and partly complaining. The reason I have a secretary is in fact because, I was telling them, which is true, that there's so much of this routine stuff for me to do, I can't possibly do it. I'm going to quit and go become a real estate salesman. After enough of that, and suitable threats and suitable interpersonal skills, I ended up having a part-time secretary.

THACKRAY: Have outside offers been a necessary part of your life or has it run with publications and research results?

EVANS: Unfortunately, I wish it hadn't, but, and I don't know if this is true everywhere, but just doing good work is not sufficient. It is necessary to have suitable pressure to exert on an institution. I wish it were not the case.

THACKRAY: So you've been here half a dozen years. How many times have you had an outside offer drama?

EVANS: Oh, probably twice, I think. I think twice is really it. It's part of the way things operate. I'm not sure that that's the case in most places. But clearly at this institution, the way it is, it historically has a long history of people coming and going. That's the way things have worked. I'm not sure it's always actually good. Actually I think it's a waste of time. You can spend a lot more time doing the science.

THACKRAY: How long is your contract for now?

EVANS: It's for three years now. It's a rolling contract, so every year it's renewed for the following three years.

THACKRAY: It's a three-year early warning contract.

EVANS: Yes. Yes.

THACKRAY: Did you think the outside offer drama is necessarily bound into the scenery looking forward as well?

EVANS: I think it probably is at this particular institution. In some ways the outside offer drama is not as serious as the thing, "What I am doing here? I can go here. I can really be left alone and do the experiments." So it's not just putting pressure on the Institute. It's really serious thoughts about where is the best place to do the science, both scientifically and professionally and from a personal point of view. Of course, the grass always looks greener in other places, but I have feeling that that's probably not going to go away. I think that's just...

THACKRAY: If you, just to draw to a close, if, at least as an armchair experiment, you were going to move and the offer is from wherever you want, can you instantly identify the one or two places?

EVANS: Well, there actually is a balance between two opposing attractions. One attraction is to be in a place which is intellectually the most stimulating, meaning a concentration of other people who have similar interests and are enthusiastic, not necessarily doing the same thing. I don't relish being in a place where everybody is doing something very similar. I like being in a place where there are very smart people doing very diverse things.

The other is what one likes from a personal point of view. The kinds of things that now are becoming very valuable to me are, number one, to live close to where you work. I used to, a year ago, come back every night or come back on weekends, or commute back and forth, and now I can't do that. There's too much traffic and congestion in the area. So being able to live close to where you work to me would be a big selling point.

And also being in a place that is not just nice but kind of a focus[ed] and a growing place. I keep thinking about what the Salk Institute must have been like when it was first started here. What happened was that Jonas Salk was going to start an institute. He wanted to put it in a place that would have a university nearby so it would have the intellectual attachment but was really a new place. He wanted to start something new. So he came to San Diego, which no one had ever heard of at the time, started an institute, and somehow managed to attract these incredibly visible, famous, well-known scientists who were right on the cutting edge of their field at that time, and he got them all to come here at once. You could think that if one was willing to go somewhere new, you'd only go there if there were other people also going there, and kind of build up something.

THACKRAY: So it wouldn't be an established place.

EVANS: Well, it could. It could but I think one would be looking for an intellectual focus.

THACKRAY: If we limited ourselves to established places and print all these things, put all these things into a computer, what does it print out?

EVANS: Well, I think there are kind of two possibilities. One is the Harvard, MIT, Columbia area which is a very well established world center. The other would be something, and I keep thinking of a place like the Santa Fe Institute, which is not really a laboratory, but it's kind of a new idea in a new place that is getting a lot of very good people interested in it. I'm kind of always enticed by that. You think, well, maybe they really have the right idea. It's hard to know.

[END OF AUDIO, FILE 1.4]

[END OF INTERVIEW]

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