Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

Program in Bioscience and Biotechnology Studies

DONALD A. GLASER, Ph.D.
THE BUBBLE CHAMBER, BIOENGINEERING, BUSINESS CONSULTING, AND NEUROBIOLOGY

An Interview Conducted by
Eric Vettel
in 2003-2004

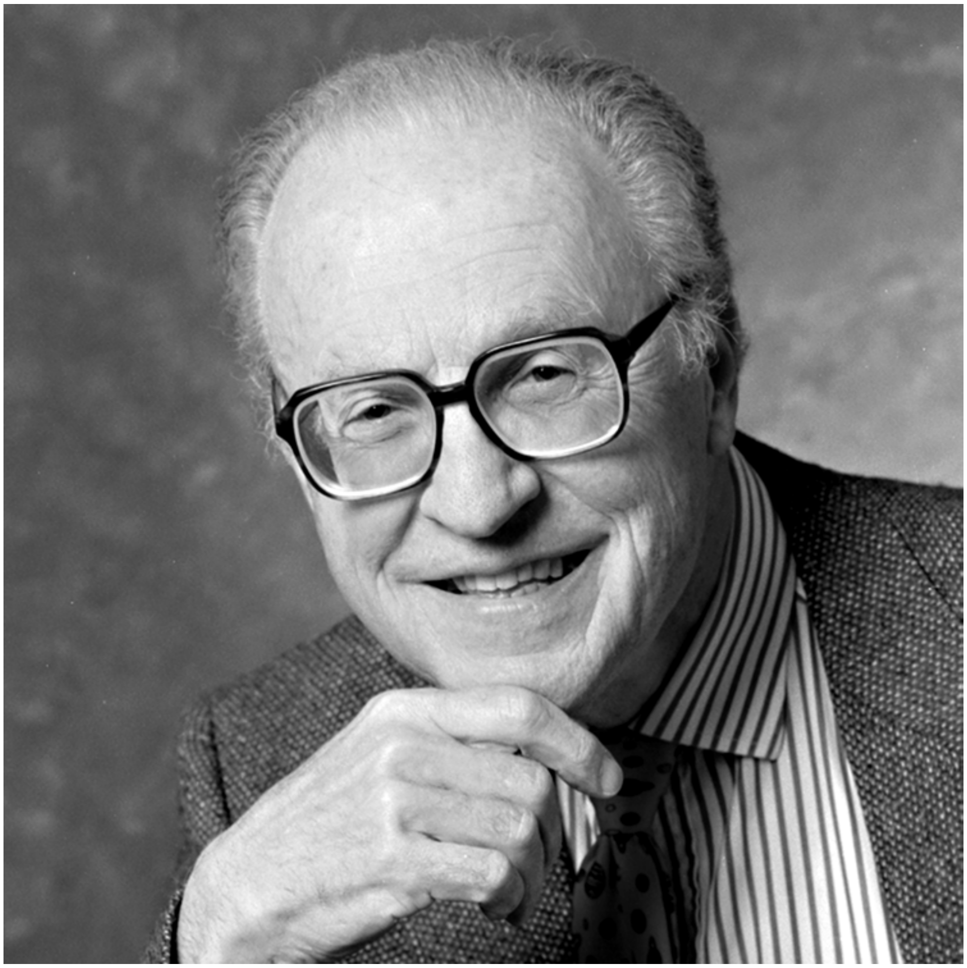
Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of northern California, the West, and the nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

****

It is recommended that this oral history be cited as follows:

Donald Glaser, “The Bubble Chamber, Bioengineering, Business Consulting, and Neurobiology,” an oral history conducted in 2003-2004 by Eric Vettel, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 2006.

Copy no. ______



Donald Glaser

Donald Glaser and Bubble Chamber
Lawrence Radiation Laboratory, UC Berkeley, early 1960s

Table of Contents—Donald Glaser

Interview 6, March 2, 2004

Biotechnology Series History—Sally Smith Hughes, Ph.D.

Genesis of the Program in Bioscience and Biotechnology Studies

In 1996 The Bancroft Library launched the forerunner of the Program in Bioscience and Biotechnology Studies. The Bancroft has strong holdings in the history of the physical sciences--the papers of E.O. Lawrence, Luis Alvarez, Edwin McMillan, and other campus figures in physics and chemistry, as well as a number of related oral histories. Yet, although the university is located next to the greatest concentration of biotechnology companies in the world, the Bancroft had no coordinated program to document the industry or its origins in academic biology.

When Charles Faulhaber arrived in 1995 as the Library's new director, he agreed on the need to establish a Bancroft program to capture and preserve the collective memory and papers of university and corporate scientists who created the biotechnology industry. Documenting and preserving the history of a science and industry which influences virtually every field of the life sciences and generates constant public interest and controversy is vital for a proper understanding of science and business in the late twentieth and early twenty-first centuries.

The Bancroft Library is the ideal location to carry out this historical endeavor. It offers the combination of experienced oral history and archival personnel and technical resources to execute a coordinated oral history, archival, and Internet program. It has an established oral history series in the biological sciences, an archival division called the History of Science and Technology Program, and the expertise to develop comprehensive records management and to digitalize documents for presentation on the Web in the California Digital Library. It also has longstanding cooperative arrangements with UC San Francisco and Stanford University, the other research universities in the San Francisco Bay Area.

In April 1996, Daniel E. Koshland, Jr. provided seed money for a center at The Bancroft Library for historical research on the biological sciences and biotechnology. And then, in early 2001, the Program in Bioscience and Biotechnology Studies was given great impetus by Genentech’s major pledge to support documentation of the biotechnology industry. Thanks to these generous gifts, the Bancroft is building an integrated collection of research materials--oral history transcripts, personal papers, and archival collections--related to the history of the biological sciences and biotechnology in university and industry settings. A board composed of distinguished figures in academia and industry advises on the direction of the oral history and archival components. The Program's initial concentration is on the San Francisco Bay Area and northern California. But its ultimate aim is to document the growth of molecular biology as an independent field of the life sciences, and the subsequent revolution which established biotechnology as a key contribution of American science and industry.

Oral History Process

The oral history methodology used in this program is that of the Regional Oral History Office, founded in 1954 and producer of over 2,000 oral histories. The method consists of research in primary and secondary sources; systematic recorded interviews; transcription, light editing by the interviewer, and review and approval by the interviewee; library deposition of bound volumes of transcripts with table of contents, introduction, interview history, and index; cataloging in UC Berkeley and national online library networks; and, in most cases, digital presentation at http://bancroft.berkeley.edu/ROHO/projects/biosci.

Sally Smith Hughes, Ph.D.
Historian of Science
Regional Oral History Office
The Bancroft Library
University of California, Berkeley
November 2005

ORAL HISTORIES ON BIOTECHNOLOGY

Program in Bioscience and Biotechnology Studies
Regional Oral History Office, The Bancroft Library
University of California, Berkeley

Paul Berg, Ph.D., *A Stanford Professor's Career in Biochemistry, Science Politics, and the Biotechnology Industry*, 2000

Mary Betlach, Ph.D., *Early Cloning and Recombinant DNA Technology at Herbert W. Boyer's UCSF Laboratory*, 2002

Herbert W. Boyer, Ph.D., *Recombinant DNA Science at UCSF and Its Commercialization at Genentech*, 2001

Roberto Crea, Ph.D., *DNA Chemistry at the Dawn of Commercial Biotechnology*, 2004

Donald A. Glaser, *The Bubble Chamber, Bioengineering, Business Consulting, and Neurobiology*, 2006

David V. Goeddel, Ph.D., *Scientist at Genentech*, *CEO at Tularik*, 2003

Herbert L. Heyneker, Ph.D., *Molecular Geneticist at UCSF and Genentech, Entrepreneur in Biotechnology*, 2004

Irving S. Johnson, Ph.D., *Eli Lilly & the Rise of Biotechnology*, 2006

Thomas J. Kiley, *Genentech Legal Counsel and Vice President, 1976-1988, and Entrepreneur*, 2002

Dennis G. Kleid, Ph.D., *Scientist and Patent Agent at Genentech*, 2002

Arthur Kornberg, M.D., *Biochemistry at Stanford, Biotechnology at DNAX*, 1998

Laurence Lasky, Ph.D., *Vaccine and Adhesion Molecule Research at Genentech*, 2005

Fred A. Middleton, *First Chief Financial Officer at Genentech, 1978-1984*, 2002

Diane Pennica, Ph.D., *t-PA and Other Research Contributions at Genentech*, 2003

Thomas J. Perkins, *Kleiner Perkins, Venture Capital, and the Chairmanship of Genentech*, *1976-1995,* 2002

G. Kirk Raab, *CEO at Genentech*, 1990-1995, 2003

George B. Rathmann, Ph.D., *Chairman, CEO, and President of Amgen, 1980–1988*, 2004

Regional Characteristics of Biotechnology in the United States: Perspectives of Three Industry Insiders (Hugh D'Andrade, David Holveck, and Edward Penhoet), 2001

Niels Reimers, *Stanford's Office of Technology Licensing and the Cohen/Boyer Cloning Patents*, 1998

William J. Rutter, Ph.D., *The Department of Biochemistry and the Molecular Approach to Biomedicine at the University of California, San Francisco, volume I*, 1998

Richard Scheller, Ph.D., *Conducting Research in Academia, Directing Research at Genentech*, 2002

Robert A. Swanson, *Co-founder, CEO, and Chairman of Genentech, 1976-1996*, 2001

Axel Ullrich, Ph. D., *Molecular Biologist at UCSF and Genentech,* 2006

Daniel G. Yansura, *Senior Scientist at Genentech*, 2002

William Young, *Director of Manufacturing at Genentech*, 2006

Oral histories in process:

Brook Byers
Ronald Cape
Stanley N. Cohen
James Gower
William Green
Keiichi Itakura
Daniel E. Koshland, Jr.
Arthur Levinson
Edward Penhoet
Arthur Riggs
William J. Rutter, volume II
Mickey Urdea
Pablo Valenzuela
Keith R. Yamamoto

INTERVIEW HISTORY—Donald A. Glaser

Donald Glaser, professor emeritus of molecular and cell biology at Berkeley and Nobel laureate in physics, provides in these interviews a personal account of his many faceted life, ranging from his family background and education, work on the bubble chamber, and career shift to biology, to his present research in neurobiology. His role as founder of and scientific advisor to Cetus Corporation and other companies is also documented. Eric Vettel, a postdoctoral fellow at the Bancroft Library, researched and conducted the six interviews for the Library's Program in Bioscience and Biotechnology Studies. Dr. Glaser reviewed and lightly edited the transcripts. We are grateful to the American Institute of Physics and Chiron Corporation for partial support of the oral history.

The Regional Oral History Office was established in 1954 to record the lives of persons who have contributed significantly to the history of California and the West. The Regional Oral History Office is a division of The Bancroft Library and is under the direction of Richard Cándida Smith.

Sally Smith Hughes, Ph.D.
Historian of Science and Program Director
The Bancroft Library
University of California, Berkeley
May 2006

Interview 1, November 18, 2003

[Tape 1, Side A] ##[1]

Vettel: Dr. Glaser, I want to say thank you very much for allowing us the opportunity to conduct this oral history.

Glaser: You're welcome.

Vettel: And by the way, you have a beautiful place, as I'm sure you've heard before.

Glaser: Thank you, yes, we enjoy it a lot.

Vettel: How long have you lived here?

Glaser: We moved in the day of the big earthquake in 1989. I can't remember when it was, October or November. That's when the construction was finished on the house.

Vettel: So that was October '89. When were the Berkeley fires?

Glaser: I don't remember, but it was about five years ago, I suppose. The big fire, you mean, near the Claremont Hotel?

Vettel: Yes.

Glaser: It was something like five years ago.

Vettel: The San Diego fires must be an awful reminder.

Glaser: It was pretty scary, but when we built this house, I installed a fire fighting system, so we have a 15-horse pump that operates at 90 PSI, and we have two one-and-five-eighths hoses, which is the biggest a single person can hold. Lynn was out in the backyard, I was on the front deck, and we watered down everything from our house to about 150 feet out, which was important because there was an independent arsonist in our neighborhood who set a grass fire on the next lot.

Vettel: Let's begin with the oral history now. There was an oral history done a few years ago with Arthur Kornberg, and the question was posed to him about what it was like to be a biochemist. And he hesitated because he said, "Describing my work and who I am as a biochemist is not very clear." So the interviewer said, "Well, how about molecular biologist?"

1.## This symbol indicates that a tape segment has begun or ended.

And he said, "Oh, no, no, no, that's too broad." And then he said, "What about an enzymologist?" And he said, "Oh, no, no, no, that's entirely too narrow." I paraphrase of course. Nevertheless, going over your papers and records and studying what you've done, I have to say that your career is perhaps even more difficult to describe. You have participated in a range of entirely different fields: molecular biology, biochemistry, neurobiology, biotechnology, and of course physics.

Glaser: I would leave out biochemistry.

Vettel: Leave out biochemistry. But your scientific interests and accomplishments are as varied as they are impressive, and your impulse to shift from one to another – perhaps that will be one of the themes that we return to throughout this interview.

But let's start at the beginning. How did you get here? Your grandparents, your genealogy.

Glaser: My parents were both immigrants from the Ukraine, I guess, southern part of Russia, and they came here independently—they didn't know each other. They were only eight or nine years old, I think. I never met most of my grandparents because they never came.

Vettel: When did they come?

Glaser: I would have to calculate back, but it was in the early 1900s.

Vettel: Why did they leave Russia and why did they come to the United States?

Glaser: Because there were pogroms against Jews and people were being killed in large numbers. Also, life wasn't so good, and so many families at that stage tried to escape from Russia.

Vettel: And why America? Do you know?

Glaser: I don't know and I don't know what the distribution was over other possible destinations.

Vettel: So they came over from the Ukraine in the early 1900s. What did they do? Your mother and father, what was their background?

Glaser: Well, they had no education, and so my father ended up a businessman and my mother helped him, essentially, some of the time, but she didn't have a profession of her own. They both finished high school by going to night school while they were working.

Vettel: And where did they arrive?

Glaser: Well, of course, Ellis Island, and then Cleveland – that is where their families settled.

Vettel: Oh, so they went straight to Cleveland.

Glaser: As far as I know. They both came from very large families. My father was one of twelve and my mother was one of eight, so they sent the kids over in pairs.

Vettel: So your father, what was he doing?

Glaser: He ran a wholesales notions store that supplied ten-cent stores and dry goods, that kind of stuff.

Vettel: So he came over when he was eight or nine, arrived in Ellis Island, then went straight to Cleveland to join up with his family.

Glaser: I assume so. Anyway, everybody ended up in Cleveland. After a while they scattered to the winds, but his generation were mostly in Cleveland.

Vettel: And then he went to night school and started his own business. And how many children did they have?

Glaser: I have only one sister, so two. And that was typical among the siblings of his generation of kids. So I have lots of cousins all over. And second cousins and so on.

Vettel: And is your sister older or younger?

Glaser: She's six years older. She lives in Chicago, and she had three children, and they have some grandchildren, I don't know how many.

Vettel: Your childhood. Your hobbies? What were you doing when you were young, with your sister and your parents as immigrants?

Glaser: Well, I wasn't a hero in school, and at one stage, I guess when I was in the first or second grade, my mother got a letter from the principal saying that I was a nice little boy and that I didn't cause any more trouble than other little boys, but I just wasn't able to keep up with the class, and she recommended I go to a school for retarded children.

Vettel: No!

Glaser: And that was a day when you didn't pull punches. You didn't say learning disadvantage, you said retarded, and so they were going to send me to a school for retarded children. My mother didn't have a strong background in education, but she knew that there was a thing called IQ, so she made a deal with them that she would take me to a psychologist and have my IQ measured and if I passed that, they wouldn't send me to a school for retarded children. I'll never forget that my mother told me, "You know that you are not allowed to show off at home, right?" I said yes. She said, "Well, now you can show off." And so I'll never forget, the first question that the psychologist asked me is what is orange. And I said, "Well, orange is a fruit that grows in warm places, and the color you can get by mixing yellow and red." And he said, "What did you say your name was?" And it went on from there. Anyway, they left me alone.

Vettel: I would hope.

Glaser: But I can understand it. I spent most of the time looking out the window. It was really very boring.

Vettel: I was going to ask, do you think you were bored?

Glaser: Yes, and I remember that they were learning all kinds of formal ways of adding numbers and carrying numbers and dividing and carry the number, and I didn't need to do that. I could see how it would go and got the answer without it. So the hell with it, I wasn't going to play their games, and that was trouble. Then they wanted me to sit there and learn how to write in script. I don't know whether you did, you are probably too young for that, but when I was a kid you had to do the standard thing, and I couldn't see that anybody would be interested in anything I would write, so the heck with learning to write. So that wasn't such a good idea either. They had grounds to think that I was retarded.

Vettel: You didn't think anyone would find anything—

Glaser: Well, one of my teachers sent a letter to my mother saying that she knew that this was insane, and she expected great things or something. So the teachers probably knew, but my school records—

Vettel: So you had not internalized this label of retarded.

Glaser: Oh no, I knew it was nonsense. But I sort of didn't realize what boredom meant. I knew it wasn't fun, but I didn't know the word boredom, I think.

Vettel: Did you like growing up in Cleveland?

Glaser: No. I mean the family was wonderful. Our family really was very, very wholesome and friendly and it was great, but it's a flat place, and you would know the word boredom if you grew up in Cleveland. But I had a lot of freedom. I had a bicycle, and I could go anywhere and get lost, so I went on long distance trips all over the place. I was mostly interested in building model airplanes, so I built a lot of model airplanes and designed them, and so on, and I did a lot of experiments, but nothing very profound.

Vettel: Why the experiments? Did they introduce you to it or was it just something that you—?

Glaser: No, I was really on my own. Aside from giving me music lessons, they didn't make any effort to stimulate me. They didn't inhibit me, but I was really free. My mother has told me that I benefited from the fact that she was too protective in raising my sister, and she felt she had squelched her a little bit. So when I came along I was given enormous freedom, which is wonderful.

Vettel: For some. It worked out this time.

Glaser: Well, I enjoyed it.

Vettel: That's interesting that the schools were overly rigid but you blossomed or found a way to blossom.

Glaser: It was, I think, two years later I skipped a grade, and I've never understood why. I was just told suddenly, "Well, you are in the fourth grade now." Reading back, I think it was because of the Great Depression and that they had to somehow cut their costs, and so they combined grades. That's a speculation, but whatever it was. So I ended up graduating from high school when I was about 16 or so, and I don't know exactly, but it was a series of accidents.

Vettel: What was it like growing up during the Depression in Cleveland?

Glaser: I don't have any grand picture of it, except I know that sometimes I would go to my father's store to help out just because it was fun to hang out. He mostly sold to other stores, to retail stores, but also there were a lot of peddlers who came in and bought pencils, or they bought something else that they were going to go and peddle from door to door. So I saw a lot of these guys who were drunk and really miserable, and I didn't realize that they were ordinary people. I thought they were defective and they couldn't make it, but there were a lot of them.

Vettel: Did that play into any of your—like when you were in school and you had teachers telling you that you were incapable?

Glaser: No, I never made a connection. And some of them were fun; they would teach me how to build towers out of boxes. But they drank witch hazel, for example—they couldn't afford drinking alcohol so they drank all kinds of cosmetic things, and some of those have wood alcohol in them and of course they are very poisonous, cause blindness at the very least. I don't know why this is turning out to be a psychotherapeutic session.

Vettel: No, I don't mean to.

Glaser: No, it's not your fault, but it brings up another thing. When I was very young and learning to play the violin, I had just gotten the violin and I couldn't tune it yet. One of these peddlers, quite independently, came to the door of our house in a nice neighborhood where we lived, sort of middle class, and wanted to sell pencils or something, and he saw the fiddle and his eyes lit up and he said, "Play something for me." And I said, "Well, I can't. I don't even know how to tune it." And then he said, "I'll tune it for you." So I gave him the fiddle, and he tuned it immediately, and that really shocked me because here was a guy that I regarded as a bum who could do something that I regarded as difficult and had some cultural experience. I haven't thought about that in years.

Vettel: It doesn't sound to me, the way you describe it, that you were without comforts. During the Depression it was not a matter of having a lot of want, but it sounds as if you and your family were comfortable.

Glaser: Yes, we were comfortable but we had to be very careful about turning off the lights, and my father bought a 60 horsepower Ford which could barely go uphill.

Vettel: Good thing Cleveland was flat, huh?

Glaser: Yes. And so we had to be careful. We almost never had steak, but we didn't suffer either, and there was enough money for the music lessons, which had a high priority from my mother's point of view.

Vettel: Why is that? Because I know that you play the viola.

Glaser: Yes. I started on the piano, but I found that very boring, and so I begged to go to the violin, which I enjoyed. Then we had a very good high school orchestra. We had a very good music conductor, he was really outstanding. Probably a dozen of my peers in that orchestra became first chair professionals in major symphonies. I mean really good people. I was the only one among the good players that really didn't want to be a professional. So some rich lady came to our high school and said that she would pay our way to go to Dearborn, Michigan for a nationwide music competition, and we'd never been out of Cleveland, so that was a hell of a deal. But we didn't have a quartet because there wasn't anybody who could play the viola, and so I said, "Well I'll learn to play the viola." So that's what I did.

Vettel: It sounds to me, once again, with respect to your music career, [you had] the freedom to choose the instrument and the opportunity and the open space to—

Glaser: It's true, but we all had that choice in that environment to chose what we wanted to play. That is what made this experience great. Then I discovered that I could play in a really high-quality quartet without having to practice as hard as the guy who played the first violin. And so I stuck with the viola and I played in a lot of very good orchestras and quartets and so on. I practiced of course, but it's nothing like the demands of a solo violinist.

Vettel: Do you still play today?

Glaser: Not very often, but occasionally some friends and I will decide that we are going to give a home concert, and then I have to practice like hell for three or four months to get up to speed. But I haven't played in public in a long time.

Vettel: Your high school, what was it like? It sounds like grade school, and rote memorization, was not stimulating. Your high school sounds impressive, since you are traveling and you have a music teacher taking you to national competitions and such—.

Glaser: The high school was superb, the music was outstanding, and everything else was really competent.

Vettel: What was the name of it?

Glaser: Cleveland Heights High School. There were about 2,000 students. It was a pretty big school. The physics teaching was rather bad. The guy who taught physics was also the football coach and that wasn't so good. The math teaching was excellent and English and history. The humanities were well done. But the physics guy, the only thing that was really good about him is that he trusted me and gave me a key to the lab so I could play around anytime I wanted. They had a little electric motor set up and I was playing with it, and it occurred to me that maybe you didn't need all those wires and if I pull one of them off, it might continue to work, and I did and it went faster. And he nearly blew his stack. He said, "You are wrecking my equipment." And I said, "No, look." And so I had discovered just by accident the so-called induction motor, and what we had before was a standard DC motor with commutator rings. I hadn't invented it; it was just from fooling around. But then I finally figured out how it worked.

Vettel: So why did he give you the keys?

Glaser: I don't know. I said I wanted to play around and he didn't think I was reckless and so he let me do that. I mean that was really an incredible tautological thing.

Vettel: Accidental.

Glaser: Sort of, but still.

Vettel: And again, the freedom.

Glaser: Yes, he must have had a spark and desire to be constructive.

Vettel: Do you think that was it?

Glaser: I don't know. He was sort of a dull guy, but he tried to convince us that the reason that a basketball bounces was that when it hits the floor it gets flat. Then all the molecules there get compressed together and get very dense and that pushes the ball up harder. He told us fairy stories like that.

Vettel: The physics teacher gives you the key and you go to the physics lab to fool around. You could have gone to the history teacher or the math teacher and asked for the same thing, so to speak. Chemistry would probably be more appropriate. Why physics?

Glaser: The thing that's always attracted me and led me into a career in physics is the desire to really understand what was going on, and physics is the most successful method that we know of understanding the material universe, leaving out biology. And even at an early stage chemistry was hocus-pocus, and it is less so now, but at that time it was cookbookery. And so there wasn't much that you could understand except by trial and error, and so thinking hard at that stage and my level of education wasn't part of the game, you just had to memorize a bunch of stuff. And so I memorized like I was supposed to and always got an A in chemistry, but I didn't like it. But in physics you could think hard and that would lead you to some idea which you could then test, and even at my low level that was the appeal and it still is.

Vettel: So the state of the sciences at that time, physics was where you could find answers to the physical world.

Glaser: That's right. You had the real hope that thinking hard plus mathematics plus a pretty high intellectual standard of what you accept as an explanation, those things combined really was a way to understand your surroundings, and still is.

Vettel: Did you understand the importance of physics to this depth in high school or was it just a sense?

Glaser: No. I thought it was fun, and what does fun mean? It means there's a reward for effort or there's a reward for impulse, and both of those things were there, and the reward was, "My God, I understand something!" It wasn't hedonistic in the sense of a sensual reward, but it was an extremely exciting thing to learn something new, and from that deduce something else which you didn't know. And then it turns out that's true, at least you can test whether it's true. That experience was and still is to me the guts of science generally, but physics particularly.

Vettel: And it's the field where physics gave you freedom. It's open ended, I guess. Did you appreciate that as well?

Glaser: I didn't know enough to know that at that time, and it isn't really so free in the sense that as the science gets more and more complicated it's more and more difficult to do something meaningful. It requires more and more equipment and a higher and higher level of understanding. There are encyclopedias of what's known. If you are at the frontier, if you do know enough, then yes, the freedom is incredible.

Vettel: So who were your mentors in high school? Who encouraged you?

Glaser: I don't really remember anybody that did. Nobody pushed me. I didn't feel any pressure, but I wasn't really close personally with any of the teachers, so I simply listened to what they said and did my homework like every other student. I don't remember any close relationship.

Vettel: But there weren't any other students fooling around in the physics lab? And no one pushing you into physics and no one pushing you into music?

Glaser: The only thing I was pushed into – pushed is a bit strong – but I was persuaded to do music and I liked it. I sort of have that attitude toward my grandchildren too. My children, I tried to encourage musical interests, and they just weren't interested, and they didn't seem to have a talent, and they regarded classical music as my kind of music anyway. It's a real disappointment to me, but that's how it is. But in my case, it took a couple of years of pushing. It either takes or it doesn't take. That is how I view it. I happened to really enjoy it.

Vettel: You were finishing up high school about the time that World War II had broken out. What was that like?

Glaser: :You know, I read these awful things in the paper all the time. I didn't know anybody personally who had been killed in the war.

Vettel: Your parents must have been really sensitive to what was going on in Europe.

Glaser:Glaser: Sure, everybody was. I suppose they were too. It had a direct effect on me in a funny way. Case Institute of Technology, where I was an undergraduate student, lost some faculty. They were drafted, and so I got to be a professor of mathematics at age 18. I had taken the course and I got all A's, and so they let me teach the classes.

Vettel: What class was it?

Glaser:Glaser: I taught a course in complex variables, and I think the other one was advanced analysis or something. Fairly sophisticated, but the sort of thing that every sophomore or every junior takes. But most of my students, or many of them at least, were returning veterans and they were much older than I and they'd tease me a lot, but they really wanted to learn so we had a very good relationship. They regarded me as able to help them and I regarded them as people I had to respect, and did. So we got along.

Vettel: So when you were finishing up high school on your way to Case Institute—

Glaser: In those days, by the way, it was called Case School of Applied Science, which is an ancient kind of title, and then it became Case Institute of Technology, and then much later it merged with Western Reserve University. The two are separated by a fence and they are right next to each other, long term rivalries, but now it's called Case Western.

Vettel: So you are finishing up high school, you are entering Case Institute of Applied Sciences. Did you have any idea that physicists were going to play a role as they did in the war, as you were finishing high school?

Glaser: There were rumors because we already knew from studying textbooks of nuclear physics that it was possible in principal to release the energy in the nucleus. And there's a thing called a "packing fraction," and I don't remember the exact definition, but it describes how much energy you get by putting a nucleus together compared with the separate parts. But then Lord Rutherford, a very famous physicist, said, "There will never be any energy coming out of this." Even though he knew that in principal it was there. I'm not quoting him exactly, but that was the gist of his comment, and people took him very seriously because he was a very successful physicist. But I didn't have any idea that the project was going on. We knew of the theoretical possibility and there were rumors and so on, but nothing in the way of —I mean, we knew some of our professors had disappeared somewhere. We didn't know where or what they were doing. Some probably went to the radar lab and some probably went to Los Alamos.

[End Tape 1, Side A] ##
[Begin Tape 1, Side B]

Vettel: Was there a pull or a draw or some sort of push at all to join the military or contribute as a physicist to the war effort?

Glaser: Well, I couldn't because I was only 16 at that time, but I felt guilty about it. At Case most of the undergraduates were in a V-12 program, which was officer training for the Navy, and they had a physical education program, which was much tougher than the civilian one, so I opted to take the Navy gym program out of a sense of guilt. And that was okay, except one of the things we had was the manly art of self-defense, wrestling and jujitsu and that kind of stuff.

Vettel: Jujitsu then or something like it?

Glaser: It was something like that. I remember I learned some really weird holds, and I broke the ankle of a friend of mine by mistake. I don't know if it was jujitsu, but the move was if somebody comes running at you to attack you, if you can grab an arm and lower your shoulder and catch him on yours, you can throw him. And I did that once, I didn't know I could, and the poor guy landed on his ankle. We were just horsing around. I also remember that I was too nearsighted for boxing, so I tried wrestling. Do you know about college wrestling? It has some very definite holds. It's quite different from what you see on television.

Vettel: It's more technical.

Glaser: Yes. The Navy guys in the wrestling program had a gentleman's agreement, "Yes, I will do what the chief says – the Chief Petty Officer was teaching the course – but let's not take it too seriously. But the chief decided that he was going to make a man out of me because he heard that I got all As and that I was playing in the Cleveland Philharmonic and I was very nearsighted, and so I was his partner. And so he was going to wipe the floor with me if I didn't learn some of these holds. He had half an ear and scars on his face—a real tough character. And so I had to learn how to do these holds because he was just throwing me around. He wasn't trying to hurt me, but he was showing me that I had to learn something. So I was the only one in the class that learned how to do it, and as a result I became the bantam weight wrestling champ.

Vettel: When was this?

Glaser: It would have been '44, '43, something like that.

Vettel: Early on or later on? You were there '42 to '46.

Glaser: I don't remember. I should say that because these guys were all in the V-12 program, they got a bachelor's degree in 30 months and they did that by going around the clock with no vacations. You took eight courses at a time and so when finals came you had two-hour exams, four in a row the first day, eight solid hours, and then eight solid hours the next day. It was a killer. But I stuck with it because I wanted to get going in the so-called "real world," and so I signed up for the fast-track V-12 sequence.

Vettel: Even the coursework; not just the physical education.

Glaser: No, the coursework, too. And so I got my degree in 30 months, two and a half years. I got my bachelor's degree when I was 18, 18½, something like that.

Vettel: So when were you playing in the Cleveland Philharmonic?

Glaser: Well I should tell you that there are two orchestras in Cleveland. There is the Cleveland Symphony, which is the real famous professional one. The Cleveland Philharmonic is a second orchestra, and about half of the people in it are also in the Cleveland Symphony, and the other half are trying to get into the symphony. So I was the other half. So I played very well, but I wasn't good enough, nor did I have any intention of being a professional.

Vettel: You were playing in the Cleveland Philharmonic when you were at Case?

Glaser: Yes, I guess so. It must have been, I can't remember.

Vettel: So you graduated from high school early, you went to Case, you taught a math—

Glaser: As soon as I graduated, after these 30 months, then I had to wait to get into Caltech for, I don't know, a semester or something because I was out of step. It was during that time that I taught the math at Case.

Vettel: —you were also in the Cleveland Philharmonic. At what point in your high school, when did you blossom? I'm just curious. Certainly something happened somewhere.

Glaser: I never blossomed. I didn't get all As in high school. I did the things that I was interested in and I did okay. I got A's and B's, but I wasn't any kind of superstar until my parents told me that, well you know they could send me to college, but they wouldn't be able to afford to send me to graduate school, so I better get good grades in college. So then I decided, I got all A's in college with one exception. I got one B.

Vettel: In what?

Glaser: It was a course in spectroscopy that was very badly taught, and I really didn't understand what was going on at that time.

Vettel: I still think something happened. I mean, you actually knew before you were an undergraduate that you had to get good grades to go to graduate school? Most people don't even think about graduate school when they are in high school. You were in the Cleveland Philharmonic and teaching a math class at age 18—

Glaser: You reminded me of something else. When I arrived at Case, I didn't know you could be a physicist. My parents didn't either. Nobody did. It wasn't until the bomb was announced. Until then, science and engineering were the same. Nobody knew the difference among ordinary citizens. So I was an engineer for six weeks, and then I discovered that physics was the only part that was fun. I was taking engineering courses, learning the building code of Chicago about how big a column has to be to support a certain weight and all that stuff, textbook engineering. And I could see that that wasn't for me, so I decided physics was the part that was fun. But the pace at Case was really different from my dismal high school, but in the first physics course I took I got zero on the first exam. Exactly zero, I couldn't work any of the problems. So I went to the professor and said, "You know, I really want to be a physicist —"

Vettel: With a zero in your hand?

Glaser: And I said, "— and I was wondering what I ought to be reading or studying besides the courses." He looked at my grade and almost fell off his chair. He said, "You can't be a physicist unless you get really good grades in physics." I said, "Yes, I know," and we chatted awhile. He was a stodgy old guy. He was not unfriendly, but he was the editor of the *Chemical Rubber Handbook* [*Handbook of Chemistry and Physics*]. Do you know this thing? It's a big, big, thick manual full of data, very useful. Anyway he was the editor of that and a very stodgy guy. He said, "But I'll make you a deal," I'm sure he'd use those words, "If you get 100% on all the rest—" He said, "You can't get an A in physics with this zero, but if you get 100% right on all the rest of the tests, I'll give you an A." So that's what I did. I really decided I'd better get it going. But it was a real shock. So I have a lot of sympathy with the freshmen who arrive here from even good schools. It takes awhile to get up to speed.

Vettel: Did you think about going anywhere else other than Case?

Glaser: I couldn't afford to. I had to live at home, so I never even thought about it ,and luckily Case gave me a really good education.

Vettel: And yet you knew that you wanted to go to graduate school right when you entered, or early on?

Glaser: No, it was pretty clear. Yes, I did.

Vettel: How did you know?

Glaser: I don't know. That's a good question. I don't know how I knew, and it may not have been; I may have been remembering wrong. It may not have been when I started as an undergrad. But after a semester or so I began to see that all the professors had PhDs, and the guys who did interesting research had PhDs, and that a Bachelor's degree was enough to get a job, but it wasn't enough to have an interesting scientific life. So it may have been three or four months after I already started, but it took awhile. I had no idea what was going on when I arrived as a freshman.

Vettel: You were drawn to math first? You majored in math and physics. It sounds like you went—

Glaser: It's a fairly common combination, and yes, I enjoyed math a lot, and at the level at which I was doing it, I was good at it, therefore it was fun.

Vettel: Then it was in college at as undergraduate that you discovered physics and that you could actually be a physicist?

Glaser: That's right.

Vettel: And that was just through trial and error?

Glaser: Yes, I mean I saw what kind of things I had to learn to be an engineer. Things are different now. Engineering is much more interesting than it was then, particularly computer engineering of various kinds, but also space engineering. Now it's really applied physics. It's not handbook stuff anymore. Engineers have to be extremely resourceful and creative and they have to know a lot of stuff and they have to be risk takers. It's not like building bridges using the formula. Now, many branches of engineering are very interesting, but when I was a student, I wasn't aware of any.

Vettel: And did you have any professors that you had as an undergrad who were pushed you, other than the physics professor who said, "You better get 100s on all of your papers?"

Glaser: No, I don't remember anybody pushing me, but there certainly were inspiring teachers.

Vettel: Do you remember who?

Vettel: Oh yes. A lot of them were really good. McClusky taught mathematics. We had an astronomy teacher whose name I can't remember who was Persian or Middle Eastern or something. He had an accent, and he was head of the observatory, the Warner Swayze Observatory in Cleveland, and he got me a job as a student demonstrator. I gave lectures at the observatory, "See all of the planets and all the constellations," and so on, very popular stuff. Jason was his first name. Anyway, I had a number of really good teachers in mathematics and in physics and in German. I can't remember any that were bad in all the different levels of mathematics. Really, the teaching was superb. Again, I didn't have a particular —well, I developed a personal relationship with them when I was on the faculty. They would be a little cautious about using scatological words in my presence.

Vettel: So when you were taking physics at Case, were you going in a particular direction? I mean physics at this time had quantum mechanics, nuclear physics, high energy. Could you tell you were going in a particular direction at this time as an undergrad, or were you generally interested in physics and not sure of a direction yet?

Glaser: I was generally interested, but I was particularly interested in what turned out to be particle physics, that is the question of, "What can we learn about the universe from physics?" So it would have been either astrophysics or particle physics. But also I had a desire to avoid becoming a member of a large team. That will come up later on, I guess.

Vettel: Astrophysics, at that time, was astrophysics more theoretical than particle physics?

Glaser: Yes. The instrumentation wasn't nearly what it is now. You could be an astronomer or you could be a theoretical astrophysicist, but there wasn't much in the way, well, there were no satellites. There was no Hubble and so on.

Vettel: A little bit more on your undergraduate physics, just a little bit more if we could. You went through undergrad pretty quickly under the accelerated program. You had an opportunity to teach while you were waiting to go to graduate school. Did you ever think about doing physics research as an undergrad?

Glaser: Again, there was a lot of freedom to play around in the lab.

Vettel: Even though you were taking the accelerated program, you still had freedom?

Glaser: There was time. I don't know how that happened, but there was time. I remember there was a dusty old acoustics lab, and you could play around and reproduce many of the phenomena in light that were known in optics, like diffraction, interference and focusing. But you could do it with sound waves so that the setup was big complicated blocks of things that you could see, and then you could set up geometries like you would see in optics but on an ordinary scale instead of a microscopic scale. And I remember that was a great pleasure fooling around with that and seeing how, not to discover anything new, but to develop intuition. But otherwise, we had a sort of standard—But I had one very serious lesson one day. Well, it was kind of funny too. I had a course in x-rays and so at a certain time I had to cut some x-ray film so it would fit into a camera, and there were some scissors there, and they had the point broken off of them. So I went to the professor and I said, "Hey, I can't cut this thing out because the scissors are broken." He said, "Glaser, you're such an idiot. Go to the shop, there's a grinding wheel. You can grind it down." That really made a big impression. Just do it. But it hadn't occurred to me that I could do that. And then something funny happened. He said, "Now don't stand too close to the x-ray machine for too long." All these Navy guys were standing around, and they said, "How come, Doc?" He said, "Well, you know, it leaks a little bit. It's not so well shielded." "What will happen if we stay there?" these guys said. And the professor said, "Well, it will make you sterile." So everybody jumped back. Then the professor said, "But it won't be permanent." So one guy said, "Say doc, how long do we have to stand here for it to last three weeks?" It was funny. That was Chuck Smith, the professor. He was a very good teacher, but they all were. I really was extremely lucky in both high school and college, except for this high school physics teacher who was a decent guy, but not very swift.

Vettel: Did you get a chance to work with the, I guess it's called the Wilson Cloud Chamber, when you were an undergrad?

Glaser: No, but I did eventually. I used them a lot for my thesis.

Vettel: But as an undergrad?

Glaser: No, I never saw one at that stage.

Vettel: This might be an awkward question. When you were an undergrad, did you know that you had a special talent at all?

Glaser: Well there were two or three of us who were the best in our class, in my opinion.

Vettel: As an undergraduate?

Glaser: As undergraduates, yes. And I got all A's and I didn't work terribly hard and I loved my courses. But the other guys were pretty good too. There are two others who I'm thinking of.

Vettel: Do you remember their names at all?

Glaser: David Dutton and the other one was Smith, but I can't remember his first name.

Vettel: Did you know you had somewhat talent in high school, or was it in college where you realized that, okay, you could do this?

Glaser: Well, I always knew I could do it, that I was good at it. But that's different from being on a different level from everybody else, which I didn't think I was, and I still don't think I am. I mean I'm good at what I do, but there are a lot of guys who are at that same level. You know, there's a big element of luck in it. That business about how I got through so fast, it wasn't my doing. It was just lucky.

Vettel: Was your social circle in high school and college physics?

Glaser: I didn't really have a social circle. I lived at home so I didn't hang out in any dorm, and I had a number of friends in my class, but I didn't see them when I wasn't on the campus. I had some other friends from high school, but after high school I didn't see them very much either because our interests diverged quite a bit. So I was sort of a loner. I never felt lonely, but I never was a groupie.

Vettel: Lots of things to study and learn.

Glaser: Yes, there was a lot of really good stuff, and just sitting around gossiping didn't appeal. I don't hang out very much, even now.

Vettel: Let's move on to your graduate school. I'm going to set aside for a moment your dissertation and your work. I'll have specific questions on that. Right now I would like to focus on graduate school. You said your parents weren't going to pay for graduate school, and that you had to do well as an undergraduate to go. You went locally as an undergrad to Case. So why Caltech?

Glaser: I wanted to go to California. It was always a romantic thing. I'd never been there. And Caltech and MIT were the two best places in science at that time; they probably may still be. So I applied to a number of places. I can't remember where else I applied, but I must have applied to a few places.

Vettel: And, for the most part, you got into all of them?

Glaser: Yes, I got in because I had a good record, and because I took a TA-ship, so they supported me totally as a graduate student. But it wasn't much; it was $600 a year or something. Plus there's a faculty club on the campus called the Atheneum which is very elegant, but on top of it, it had an open sleeping porch with 18 beds, and you could sleep there and have a locker for your clothes, just like a gym locker essentially, for very little money. So you could really make it on $600 if you slept on that open porch, which is what I did.

Vettel: Caltech, lots of opportunities. I mean California, this is kind of the romantic ideal of Southern California, Caltech, music and outdoors. Was this the time you got yourself oriented? I mean it's a big move.

Glaser: Yes, it was wonderful. I had never seen a mountain before, and we went hiking in the mountains every weekend. And I learned how to sail in a, I forget what you call those boats, but anyway, they were about a 24-foot sloop that was very common. So I learned how to sail; I learned how to ski. We started the first string quartet that Pasadena ever had. We were all amateurs at Caltech. So it was a real expansion of my life. I learned to do all kinds of things that I really love, and it was a wonderful bunch of students. I was the youngest in most situations because a lot of them, again, were returning veterans. So they knew a lot and had a very mature view of things. So I profited a lot from being among the other students. We ate dinner together every night at the club, which is a special deal. That's another reason the $600 could work. But you know you couldn't go off the campus. You couldn't afford to. But everything on the campus was within our budget. But yes, it was really a wonderful change. The only downside was the smog, of course.

Vettel: And there was smog then?

Glaser: It was bad.

Vettel: So you went to Caltech, and it sounds again like freedom, the opportunity to explore new things.

Glaser: Mostly lifestyle things, meaning I never did any rock climbing, any serious rock climbing, but strenuous mountain hiking we did regularly and skiing and sailing. It was beautiful.

Vettel: Was there a set program for physics when you got there? I tried to make a list of all the great professor at Caltech at that time: [Harry] Bateman was in math, biology had [George] Beadle, [Thomas Hunt] Morgan, the [Max] Delbruck seminars, chemistry had Linus Pauling. What an amazing time and place to be.

Glaser: Oh yes.

Vettel: Did you get to dabble in or take courses with some of these professors?

Glaser: Well yes, Ed Tolman was there. I took his course in cosmology, which was really fluffy. I encountered him once in the faculty club, and he said, "Glaser, you haven't been coming to my lectures." So I said, "Yes, I can't really take it anymore." He said, "What do you mean?" I said, "Well you're an extremely good teacher, but it's such a fluffy business." And he chuckled and said, "Well, that's how it is now." I really think that's funny. I remember I really was physically uncomfortable listening to his lecture and trying to figure out what was going on. "What can you believe and what can't you believe and why does he think that—" and so on and so on. While in the other classes I could understand that if I really thought hard and worked hard I could master the material and understand, and that would give me insights about the real world. But in cosmology in those days, it didn't fit into the, I don't know what to call it, I suppose the arrogance of the physicists, which is much described nowadays. But the belief that you could really understand something in cosmology? I didn't have that confidence. That's changed now, but in those days, that's how it was. But he was a superb teacher. Paul Sophus Epstein taught thermodynamics, and it was just spectacular. Morgan Ward taught mathematics. I used to go do Delbruck's seminars, but I wasn't in biology then, so I didn't take courses.

Vettel: But did you get to try a lot of these?

Glaser: No. You could go to seminars, but we had a pretty heavy load of required courses. Electricity and magnetism was the standard one that flunked out graduate students. You had to get a C or better, or maybe it was B or better, and you had three chances, and they still flunked about 5 or 10% of the class. And then we had the standard stuff, mechanics and relativity theory and quantum mechanics and nuclear physics. So you didn't really have time. Once you decided you were going to be a physicist, you couldn't sample other courses.

Vettel: Serious stuff at this point.

Glaser: Physics suddenly becomes—I mean it was a real change in pace again, going from undergraduate to Caltech. When I was a TA, I remember noticing that the undergraduates were sharp as hell. They were very selective. It's a small place; I don't remember exactly, but something like 900 graduate students, 900 undergraduates, or maybe 1,000 of each. It was small and very selective. The competition to get in was enormous at both levels. So both the graduate students and the undergraduates were really, really smart.

Vettel: I would imagine. You arrived at Caltech in '46. At this point Caltech is pretty competitive, and physics intensely so because of the consequences of the atomic bomb and the war. Did you get that sense then?

Glaser: No. I don't know early history, but Caltech always had a reputation of having very high standards and therefore very good students, and it was hard to get in, that's true. The bomb added a certain—It made physics more popular, so there were more people who wanted to study physics, but I don't think Caltech increased it's size in response.

[End Tape 1, Side B] ##

Interview 2, December 2. 2003

[Begin Tape 2, Side A]

Vettel: To remind you what we've covered, we ended the last interview session at that moment when you arrived at Caltech, and we talked about some of the other programs – biology, chemistry, math – and you commented that you were not able to explore some of these other scientific fields. You came to do physics. That was your focus.

Glaser: Yes, I don't remember that we discussed chemistry and biology very much except that while I was there I got very interested in molecular genetics.

Vettel: Oh, did you? I didn't know that. We didn't get to that. So what drew you to molecular genetics? It certainly wasn't a precise field back then. Was your interest instantaneous?

Glaser: No, it was because Max Delbruck, who was a distinguished German theoretical physicist, had turned his attention to the genetics of microorganisms, particularly bacterial viruses, so-called bacteriophages. He and other physicists around the world were having great success doing incredibly simple genetic experiments on organisms, which until that time caused great debate as to whether they had any genetics. Then it was discovered, first of all, that they had DNA in them, but then it was quite a while until it was generally agreed that DNA was the genetic substance. And so they had very interesting seminars describing what they were doing, and it was at such an elementary level technically that you could get an idea, do an experiment, and the next day you knew the answer, and whether you were right or wrong, and then you could go on to the next event. So it was extremely appealing to me, but I was already deeply committed and very excited about physics.

Vettel: So this was a time when a lot of the physicists were going into molecular biology. Schrodinger's book, *What Is Life,* really pushed—

Glaser: Yes. It was sort of a fake also. I read it and was very turned on by it, but I shouldn't have been. In reflection it wasn't that profound, but it set ideas which were novel to me then and to a lot of other people, too.

Vettel: So you were introduced to molecular genetics at this time. Delbruck and some of his graduate students perhaps, or visiting scholars, came and presented their subject to the Physics Department?

Glaser: No, they had their own seminars. It had nothing to do with physics. I wandered over there because I'd heard about it and found it interesting.

Vettel: And it was just an exciting field, exciting time, exciting place.

Glaser: Sure. And I would occasionally meet post-docs of his. In particular, I met Günther Stent at dinner at the faculty club one night, and I heard a little bit about what was going on from him and others.

Vettel: Did you keep up a correspondence with Dr. Stent when he came to Berkeley? Just out of curiosity. You were both at Berkeley at the same time.

Glaser: Yes, we became close friends once I got here, although I didn't know him very well at Caltech.

Vettel: I had no idea that you were getting involved in molecular genetics while at Caltech.

Glaser: I would go to seminars on things I knew nothing about because it's fun.

Vettel: So in physics obviously, there was [Robert A.] Millikan, [Robert J.] Oppenheimer with his half-appointment, Tolman, [Fritz] Zwicky, Ira Bowen, Carl Anderson—.

Glaser: Yes. All those people were my professors except for Oppenheimer. I never had a course from him. But I knew Tolman quite well, and Millikan was sort of an *eminence gris* who I saw in the distance, and I may have met him, but I really didn't know him.

Vettel: Did you come and start working with one or two individuals right away or did you—?

Glaser: Well, the first year they really loaded us heavily with difficult courses.

Vettel: Theoretical and applied, I guess?

Glaser: No, it was all theoretical. There was no laboratory work in the first year, essentially. And there was one very tough course in electricity and magnetism, which was the filter. You had to pass it with a grade of 70 or better, and you three chances. They flunked out a fair number, not a huge number, but more than several who couldn't pass the course. It was very, very theoretical and mathematical.

Vettel: Who taught it?

Glaser: W.R. Smythe. But the courses generally were quite demanding.

Vettel: So you sampled a variety of courses. How did you choose a direction? A field? Was it a particular group of professors, or did you choose it because of your interest in a particular field?

Glaser: You mean what did I choose to do my thesis in?

Vettel: Actually, *how* did you start to define or choose a direction within physics?

Glaser: Well, all of it was interesting. The teaching was superb and all of it was challenging, and it was remarkable how much you could know about the universe by mastering what you were taught. So it was very, very exciting intellectually, but when it came time to look for a thesis subject, the thing I was interested in most was elementary particles, nuclear physics, the composition and history of the universe, and such things.

Vettel: Physics was certainly a hot field at the time.

Glaser: Well, it still is hot, but it's become so expensive that's it's ponderous, but in those days it was more accessible. But what I didn't want to do was join a large group working on an accelerator. So that ruled out nuclear physics, and so the only field in which you could work on particle physics was cosmic rays at that time. So I asked Carl Anderson if I could join his group and he took me in, so that's where I did my thesis.

Vettel: So Anderson, had he won his Nobel Prize at that time?

Glaser: Long before. I think it was '32 or something. Anyway it was very early on, way before I arrived.

Vettel: You chose his field because of the scientific questions?

Glaser: Because of the scientific questions and the style of life. That is, you could work by yourself; you didn't have to travel to accelerators. You didn't have to attend endless meetings of people that argued about priorities and using common facilities like accelerators, and I didn't want to have anything to do with that if I could avoid it. There was once a model for doing physics and science which has almost disappeared now. That is, the individual with a few students, thinking hard, trying to be clever, and trying to pick out topics that were ready for development.

Vettel: Was Professor Anderson like that?

Glaser: Yes, he was – cosmic ray physics generally was. Each university had one or two professors and a small group, and they presided over a cloud chamber or two and a magnet or two, and there was always an array of Geiger counters. In fact, in those days it was really weird; we made our own Geiger counters. I don't mean the electronics, of course, but we made the tubes. So they arrived at the shipping dock one-inch in diameter, high-quality copper tubing, and we had to learn to cut it up and solder and weld and so on and fill it with just the right kind of gas. It was a standard joke that Uncle Bobby, which was how Millikan was referred to irreverently, was taking bids on a blast furnace. It wasn't that bad, but there was a lot of do-it-yourself stuff to do and we didn't grumble too much. We liked it, in fact. You could have the illusion of self-reliance, and I could turn threads on a lathe and do various things.

Vettel: Do you think that building your own equipment helped you in your understanding of physics? Did your understanding of physics improve because you understood its instrumentation?

Glaser: It helped me when it came time to design equipment, because at one stage I had forty-five engineers working for me, building big stuff for high energy. Well, that was later in molecular biology, but in physics it was the same thing. Life depended on the machine shop, the glass blowing shop, and I had to know how to do drawings for things that were makeable. So engineering design was a very important part, and still is, of being an experimental physicist.

Vettel: And, in turn, that helped you conceptually within experimental fields?

Glaser: Not so much. I mean the design of the equipment of course is motivated by the scientific goal and the scientific facts. But how thick the aluminum plate has to be to hold up the weight you want doesn't involve any physics, so it's both engineering and physics, and they're related but quite separate modes of thinking.

Vettel: But at the time, physics and engineering were parallel and closely related, closer then than they are today.

Glaser: Yes, but especially characteristic of small groups. That is, in the big cyclotron groups at the time, there were professional engineers and professional machinists, and so one knew how to make conceptual drawings, but not real drawings. I had to make real drawings and take them to the machinist directly. The guys who were the head of the machine shop and the head of the glass-blowing shop had sort of become father figures because I would go to them with my ideas, and they would say, "Well, that's great, but we can't do that. How about doing this?" So they contributed a lot to the engineering of what was makeable.

Vettel: They didn't have a problem with some young 24-year-old coming to them, telling them what to do?

Glaser: They were extremely generous. I don't know. I was never aware of any resentment because I really went to them as a supplicant, not as a boss. So we had a wonderful working relationship. I was really grateful to them, and they knew it. And I also had great respect, genuine respect both for their intelligence and their abilities.

Vettel: And they probably appreciated your approaching them as a supplicant.

Glaser: We understood each other. I mean we were a team really, not like the formal arrangements like it is today. In a way, we were probably more like a team than what they call teams today.

Vettel: There's a story about Oppenheimer's lectures – that so few people understood what he was talking about, that if you could understand just one of his lectures conceptually – that you could follow in his footsteps, but if you didn't understand him, ever, then—

Glaser: Did I tell you the funny story about Oppenheimer at Caltech? I guess I told it to John Adams. He has several commissions, he's working as hard as he possibly can, and he is in fact working on an opera about Oppenheimer. I have spoken with his wife, and she's more than happy to share his records, but I haven't done anything about it. I'll have to speak to him about that.

This is the story I told him, which is going to be in Adams' opera apparently. Oppenheimer came to Caltech at the height of his fame. Well, his fame lasted a long time but he was already very well known, more for his work at Los Alamos than for—He was a respectable physicist, but he wasn't particularly known as a man who would make powerful contributions. He had made solid contributions. So the lecture hall was jammed because everybody was there. Freshmen were sitting in the aisles. He came in and saw everyone. He worked his way through the crowd, and he climbed up on the lecture desk and stood there and slowly turned around, and he said, "My name is J. Robert Oppenheimer. Now you have all seen me. Today's topic is on pseudo-scalar meson theory. If you don't know what a pseudo-scalar meson is, you won't understand a word and should leave now." Nobody stirred. So he jumped down off the desk and grabbed some kid in the front row and said, "What's a pseudo-scalar meson?" The place emptied in seconds. But most of the graduate students like me sneaked in at the end, and it's true, I didn't understand a word. I was a first-year graduate student and I vaguely knew what a pseudo-scalar meson was, but I knew nothing about quantum theory and all of that other stuff. But I told this story to someone who said that Oppie had the reputation of doing that, of saying, "This is going to be very technical and very sophisticated and you probably won't enjoy it or you won't understand it."

Vettel: I've heard that his lectures served as a kind of filter in terms of which direction a graduate student might go. Understanding his lectures was almost a ritual, a badge of honor – if you could understand one of his lectures, then you might have a future in —.

Glaser: I hadn't heard that.

Vettel: So at Caltech, you received strong theoretical training. But there was also a large group around high energy —

Glaser: It was around the cyclotron.

Vettel: Centered around the cyclotron, and then you have smaller groups in particle —

Glaser: Cosmic ray physics. And they were quite different things. In those days cyclotron physics was nuclear structure that wasn't elementary particle physics; and cosmic rays was the only thing at Caltech where you could do particle physics, meaning the structure of the elementary particles, the discovery of the mesons and bosons and stuff.

Vettel: So it's almost a triangular relationship between the fields. I would imagine Oppenheimer and Millikan headed up the theoretical?

Glaser: No, Millikan was strictly an experimentalist. When I was there—

Vettel: But wasn't he on the cyclotron side?

Glaser: No. I think he was long retired by the time I arrived. His main claim to fame was the measurement of the charge of electron and showing that it was a discrete constant of nature. That was of great importance. He was apparently a very effective administrator and was President of Caltech even before, when it was called Throop Institute, before it was called Caltech. Anyway, he played no role in my education.

Vettel: Daniel Kevles and a few other historians of physics have mentioned that at Caltech in particular there was a divide, or competition, between theoretical and experimental physicists.

Glaser: Well, applied physics is certainly quite different, sort of post-engineering. But experimental physics—

Vettel: I mean did this divide exist before you got there or did you sense this division?

Glaser: No, there wasn't a divide, except being an experimentalist was a full-time job. To be a theorist was a full-time job because you had enormously complicated literature and mathematical skills to develop, so they were two professions that had different demands. And that was true everywhere; it wasn't only Caltech—that was the nature of the field. Until recently, molecular biology, or biology in general, there was no such thing as a theorist. I mean there were theorists, but they were ignored and they had no impact on the experiment.

Now in neurobiology, the field that I work in now, the field is getting complicated enough that the experimentalists really have a hard time keeping up with the growing theoretical literature, and their tools are so sophisticated that they have an enormous choice of what to measure and what to pursue. So they are eager to have theoretical insights, if they can be persuaded by them, to choose among the things that they might want to invest their time. So the rise of the distinctive theoretical side of science is a professional necessity when it becomes a full-time job, and the experimental side is also a full-time job.

Vettel: I have seen the necessity of collaborative possibilities between the two, but there is also competition, often in academia, between fields and within disciplines. And people have spoken about it or written about it at Caltech, and I'm wondering, did you get a sense of that competition?

Glaser: There's a different thing, which is within our own physics department here, there are a number of different specialties. Astrophysics is sort of a part of physics, condensed matter physics, nuclear structure, nuclear physics, elementary particle physics, and so on. And so there is a certain competition within the department because each specialist would like to have more colleagues. But you know in a mature department like ours, I'm no longer deeply involved, but there are calm negotiations about what's good for the students and what's needed in the world and so on. So we end up with a policy that is a result of discussion. So there's no blood-on-the-floor kind of competition of that sort.

Vettel: And when you were a student at Caltech, you didn't feel there was competition either?

Glaser: I was not on the faculty. I had no idea what went on there, but certainly as a student I wasn't aware of any fighting or anything like that.

Vettel: At the time when you were a student in search of a thesis, what field did you think you were going to enter? How did you define yourself as a physicist at the time that you were a student?

Glaser: I didn't have to. I took all of the courses that everybody else took and developed a general knowledge. I found cosmology to be fascinating, but very, very unsettling. I wanted to work hard on things that I can understand and in which I see more definite.

Vettel: You like precision and precise answers?

Glaser: Well, I wanted to know what was going on, and in cosmology one didn't. Things have improved a lot. I mean there is really hot stuff in the last several years, but in those days it was pretty wild speculation, and I didn't find it in the same league with the other things I was offered.

Vettel: I want to get to your thesis in a detailed manner, but a few more contextual questions. Again, returning to Kevles, there is a great line, I'll have to paraphrase. In the early '50s, about the time that you are finishing up, maybe late '40s, he cites a Gallup poll or public opinion poll that says physicists ranked second in terms of popular prestige, second to Supreme Court justices. I was wondering if you had a sense of the public's support for your field when you were a graduate student?

Glaser: Well, the Supreme Court justices have certainly fallen in prestige in my opinion in the last two years. Anyway, I think our popularity then was the result of the enormous success of nuclear power and nuclear weapons. Physicists were seen as people of great power who had made important contributions, positive and negative, but nevertheless, society-shaking advances. Ordinarily, the public didn't understand it, but they saw it as a source of great power, and they didn't see that it was limited and it was going to end because they saw the physicists and thought, "God, they can do anything." Today, if the public see somebody who's talented, they tend to say, "He must be a neurosurgeon or a rocket engineer," right? Well in those days it was physicists.

Vettel: Did you get a sense of the enormous prestige for your work then?

Glaser: Oh yes, sure.

Vettel: Did you have a sense of the close relationship between the federal government and physics research? Were you aware of the role physicists would play in the Cold War? It was a time when the policymakers felt or sought—

Glaser: No, I think I said something about society shaking. I'm not sure, but roughly that's what I had in mind. Well, you've asked several questions. Funding was relatively easy, and it's interesting that the first government agency that supported what has to be called pure research in physics was the Office of Naval Research. ONR was the first one, and they supported Carl Anderson and me and all of the other students, with no promise or no expectation that cosmic rays are going to turn up anything useful. And I don't know because I wasn't old enough what the motives were, but I could guess that they simply wanted to support the best places for training very good scientists because that's obviously, sooner or later, going to be useful. I mean they probably justified it on those grounds, but whatever it was, it was really hands off.

Vettel: And you could see it as a graduate student?

Glaser: Well, you wonder why is the federal government supporting this research? But they were very generous, not that money was thrown around, but we had what we needed, and even when I —well, that wasn't true. When I came to Ann Arbor, it was a little harder to get money, and it didn't last long. But when nobody had ever heard of me, it was very hard to get money.

Vettel: Last question on this particular subject, did you have your eye on Berkeley at the time that you were at Caltech?

Glaser: No, but I knew I wanted to stay in California if I could. But when I went looking for a job, it was easy to get a job. No problem. I had offers from, I think, Columbia, MIT, Michigan, and Madison, Wisconsin. Oh, and Columbia. But going to Columbia or MIT would have meant joining a big group with the cyclotron, and I did not want to do that. So at Michigan or Wisconsin, if that was the one, I was on my own and that's what I wanted, so I chose Michigan as the best of the ones available.

Vettel: Okay, if we could go to thesis and then I want to get to your job search in a minute.

Glaser: Well, the job search is boring. It wasn't hard like it is now.

Vettel: That must have been nice.

Glaser: I didn't realize how nice it was. I had no idea.

Vettel: The generosity of federal agencies like the ONR, their "hands off" policies, the jobs—I cannot imagine how nice it must have been.

Glaser: That's right. Those were really easy times to be a scientist.

Vettel: Good times?

Glaser: Yes, very good in the sense that the activity was respected, it was funded, and it was a time of growth. I don't know if it was generally true, but I had the feeling that the reason I had such an easy time getting a job was that departments were growing, and I'm sure I had good recommendations and that helped, but nowadays, good recommendations are only your first step.

Vettel: Good for research, too, at this time?

Glaser: Oh yes, at least that was my experience.

Vettel: Your doctoral thesis, I'd like to read this into the record. "The Momentum Distribution of Charged Cosmic Ray Particles Near Sea Level." That's the official title of your thesis. Some passages from it, some comments about it. "A new measurement of the momentum distribution of high energy charged cosmic ray particles at sea level has been made by means of a technique involving the use of two cloud chambers and a large electromagnet." Continuing, "Although the effect is barely outside statistical uncertainty, there seems to be an anomalous dip in the momentum distribution. In addition to this, there seems to have been a small variation in the spectrum during the time the experiments were performed, either day or night." Continuing still, "One of the remarkable and striking properties of the cosmic radiation is its extraordinarily great penetrating power." The passage discusses charged particles, mesons, protons, photons. How did you get to this topic? What were the obstacles? What was the question you were trying to answer?

Glaser: It was very unromantic. I wanted to get my degree as soon as I could and start working on my own, and when I went to see Carl Anderson he had a project going which needed someone to build two cloud chambers.

[End Tape 2, Side A] ##
[Begin Tape 2, Side B]

Glaser: There was a post-doc who had started on the project, and then he went away, and then there was a graduate student, and then he went away. So Carl asked me whether I'd like to take on this project. So in a way, the hard part had been dealt with. The boring part had been done, which is to construct the apparatus, and all I had to do was keep it running day and night for I don't know how long, six months, and collect all of the data, which is zillions of photographs, and analyze them.

Vettel: In sequence, analyzing photograph after photograph after photograph?

Glaser: Yes. The setup was really simple. First I should tell you that cosmic rays in general are high energy charged particles that are carousing all around the universe, and many of them are protons. And when the protons hit the atmosphere, and they have very high energies, we don't know how high, but certainly billions and billions of volts, gigavolts or teravolts, way up there. The limit is not known and people are still trying to find out what it is. But when they hit the atmosphere, they generate a shower of new particles, particles that are created. The way that you can think of it as running $E=MC^2$ backwards. If you think of $E=MC^2$ as a way of getting a big bomb and destroying some matter, if you start with a big amount of energy, you can generate matter. So it's a two-way street. So that's what was happening, and what was produced among other things was a mu meson. A mu meson can be thought of as a heavy electron.

Vettel: Now how did the two cloud chambers work, the Wilson Cloud Chambers

Glaser: There was a cloud chamber about 18 inches in diameter and a magnet about 2 feet deep. So when one of these new mesons arrived, it triggered a Geiger counter up here. It was probably one of the ones I made when others failed. Then it went in the whole apparatus, and then it triggered one underneath, and there's an electronic circuit called the coincidence circuit. When one fires and then the other one fires at the same time, you know that something went from one to the other. So therefore, something must have happened.

Vettel: Do you just wait?

Glaser: I don't wait—the machine waits.

Vettel: So the machine is waiting for mu mesons.

Glaser: It's waiting for a coincidence signal. The mu meson causes these two counters to fire together. They're connected to a piece of electronics which sends out a pulse whenever it gets two signals at the same time. That's a coincidence. Whenever it gets such a pulse it generates a large pulse which activates the cloud chambers. Then the camera's sitting there, it takes a picture, and you see it track up here and it track down here. Now, in between is a magnet, so this particle then is bent by an amount that depends on how fast it's going, or its momentum.

Vettel: Or the energy?

Glaser: Well, its momentum. The momentum is the mass times the velocity, and it's very similar to the energy which is the mass times velocity squared, ignoring relativity theory. It's that simple. Anyway, bottom line, you measure this thing, you measure the angle between them, you know the strength of the magnet, and it tells you the energy or the momentum of the particle. You do that for, I can't remember, many thousands of particles and you plot a curve, and that's called the momentum spectrum.

Vettel: So you're measuring the momentum of these particles passing through, and you are getting this curve that shows you the distribution of this momentum?

Glaser: Right. And the reason that's interesting is nobody had the faintest clue, and still doesn't, as to what causes these particles to have this humongous energy. Are they eruptions from distant stars? Are there things like huge accelerators naturally out there in the universe? Many people, including Fermi, believed that.

Vettel: This is where the theoretical comes in.

Glaser: That's the theoretical side of it. But what's needed is a measurement against which to test the theories, so the theories then predict what you should get. And on my PhD exam, one of the theoretical professors asked me, "What do you conclude from this?" and I mentioned that coy remark about the anomaly, that if I hadn't been so shy I would have claimed to have discovered the antiproton, because that thing had a peak just where you would expect an antiproton to come. But the data was not really sharp enough to claim that.

Vettel: Who picked up on the antiproton as being the anomaly?

Glaser: It wasn't seen in that way. It was done at Berkeley by [Emilio] Segre, [Owen] Chamberlain, and so on. That's why the Bevatron was built. The Bevatron had six billion volts of energy because that's what was required to produce an antiproton-proton pair if they exist. That's why it was built at that energy, for that purpose.

Vettel: In a sense, it was built to answer the question that you were asked on the exam?

Glaser: Yes. I mean I didn't expect to see that. One of the professors was really kind of upset with me that I hadn't gone through all of the mathematics to show that it was true. I just indicated it and said, "I didn't do the mathematics because the data aren't good enough, and it would take many years to collect enough data to test such a thing."

Vettel: Can you remember who it was that asked that?

Glaser: Yes, it was Christy, I think Bob Christy.

Vettel: So it sounds to me that your thesis used mathematics to measure the angle, and a lot of engineering. I'm struck by all that has to happen, and all that you have to know, to conduct measurements of something that happens in such a brief moment—the triggering, the cloud chamber, the equipment – everything just to take an image.

Glaser: Yes, but that was child's play compared to the bubble chamber in which you had to take a picture in a microsecond. That's quite a demand.

Vettel: You needed more time with this experiment?

Glaser: Oh, this was slow by comparison.

Vettel: How much?

Glaser: The tracks would persist for a second or two, but you had to take the picture within, oh, maybe a tenth of a second, 100 milliseconds or so. So it was no particular optical strain. While with the bubble chamber, I had to do a lot of very fancy optical photographic engineering to be able to catch the phenomenon.

Vettel: Now, in terms of your thesis, and forgive my ignorance, especially in the history of physics at this time, what was it that it contributed? What was unique about it? Was it unique or was this just a case of getting the degree, so to speak?

Glaser: Well, it was unique in the sense that nobody had done it. It was not very exciting in the sense that it didn't lead to any major insights.

Vettel: So then did you know at the time the direction you were going?

Glaser: Oh, yes.

Vettel: Were you thinking, “Oh, I could do this, but make it better.”

Glaser: Yes, I mean my real interest was in more interesting things that were going on in the same group, which is the so-called strange particles, which we now call the elementary particles. So there were cloud chambers on mountain peaks and cloud chambers in sea level labs that were very much like the ones I was using, but those chambers were immersed in the magnetic field, so the particles had curved trajectories, and by measuring the curvature, you could determine things about the particle.

Anyway, the probability of finding an interesting event in a cloud chamber was extremely low because here comes a particle, and going through a gas, and it has to have a collision with a molecule or atom in the gas, which almost never happened. So they used to put lead plates or platinum plates in the middle of the chamber, and the particle comes in and then out comes some stuff. But it all happened in the lead plate so you don’t see it. And again, once a day or something like that, you’d see something maybe every few days, and you’d sit there with your slide rule and do some special relativity to calculate what it was, and you could keep up with it. You know, there were calculators, but they were fancy mechanical things.

And that was the reason why I invented the bubble chamber, because in the bubble chamber you’re in a liquid and that’s a thousand times denser than air. So your chances of seeing an event happening in the liquid—no lead plate, no platinum plate—you see the actual thing happening, and it happens a thousand times more frequently so that you can collect data at an enormous rate. That was why I did it. Other people were trying to solve the same problem. At Brookhaven there was a group that had built an incredibly big cloud chamber at a very high pressure because they were trying to improve the chance again. But it was so syrupy inside, almost viscous, that it took 20 minutes before the chamber would settle down so you could take another picture. And I saw that and I said, “No, that’s no good.” And those two things are what motivated me to build a bubble chamber.

Vettel: So during your thesis, were you frustrated with the Wilson cloud chambers, that it just took so long to gather the necessary data?

Glaser: Not for my thesis, because the cloud chambers were doing their job, and that was okay. But the guys around me, some of the others, Bob Leighton for example was a post-doc, or was he already on the faculty? Or Bud Cowan, who was later on the faculty, were sitting there with their chambers waiting to get these very, very rare events. And each time they got one there was a paper, one event. And of course, nobody knew what they were. Carl Anderson had written on his blackboard, “What have we done about the pothooks today?” because a particle would come in, it would make a V-shaped thing, which he called a pothook. So you saw these. First they were called pothooks, then they were called V particles. Then the theorists looked into it and decided, “These particles live much longer than they’re supposed to, according to our theories. Therefore we’ll call them strange particles.” In fact they invented a number called the strangeness, which in some way described how much longer these particles lived than they were supposed to, according to the old theories. And we gave them names like V1, V2, V3, and once we knew what they were, then they began to get Greek names like lambda and theta, and each one of them had a very distinct identity, so they followed the tradition of alpha rays and beta rays. So a lambda was a particular particle.

Vettel: So while you’re working on your own with your two cloud chambers, you have others working with cloud chambers around you, some having less success or maybe not as frequent success as you. But certainly the same questions are coming up often enough to where your advisor is saying, “Look, this is our focus.”

Glaser: Yes, he didn’t have to say anything. He told me, “This is your job.” He never came into my lab and I was on my own, it was lovely. He was a friendly, nice man, but it was sort of a humdrum thing I did and he wasn’t deeply interested in it. You know, he thought it was a good thing to do, but neither one of us thought that it was great science.

Vettel: So your thesis, at the time you didn’t find it exhilarating, exciting?

Glaser: It was a measurement that somebody had to do, and it was set up and I did it, and I got my degree in three years because of that and I was very pleased with that.

Vettel: And you had the freedom to just do it. He didn’t come in. But also you said earlier, that this was a really intellectually stimulating time and place to be?

Glaser: Oh, yes.

Vettel: So was it a wonderful place because of the freedom you had to do what you wanted, or was it because you were sampling courses?

Glaser: Oh, because there was a lot going on around and some good seminars and smart students, a slew of students who were really a smart bunch. I was a TA for much of the time and the undergraduates were an extremely sharp bunch of kids.

Vettel: So you finish up, you defend your thesis, you take your PhD, you do your job search – Columbia, MIT – but you really like California, especially the outdoor activities.

Glaser: Sure, that's what I wanted to do. I learned to sail when I was a graduate student.

Vettel: And you're 23, and you are in California at this time. I'll bet southern California was a pretty nice place to be when you're 23.

Glaser: Yes, at any age.

Vettel: And yet, you went to Michigan. Why Michigan, again?

Glaser: Well, because MIT and Columbia would have meant joining a large group working on the cyclotron, and I didn't want to join a large group. And I wasn't that interested in nuclear physics, which is the structure of atoms. So Michigan was the best next in line in terms of the quality of the physics department and the university generally.

Vettel: Next in line behind MIT?

Glaser: Yes, and Columbia.

Vettel: But it's the best available for something that's not nuclear physics?

Glaser: Well, available for independents. They didn't have an accelerator there. Dick Crane was building a synchrotron, but it wasn't finished, and it never really succeeded.

Vettel: I think this passage gets repeated and told often in various genres for a number of reasons, partly because I think the public enjoys, almost finds comfort in this idea of scientific serendipity because it kind of confirms that great thinking can happen almost at any time, certainly in almost any place. When I read this I imagine a bunch of young men, new young faculty, having a few drinks, a few beers, perhaps a few too many. Maybe one person's staring at a glass of beer too long and just watching the bubbles go. But that's the image I get, that gets told. It even appeals to the public because they can say to themselves, "I've sat in a bar. I've had a beer. I've had to come up with good ideas when I'm in a bar." This story is a populist story, and its Horatio Alger, and its so straightforward that it's wonderful.

Now I've got to ask—Now we're getting to the bubble chamber. I would like to read another excerpt from an article that's about you. It's a pretty common piece; it appears in many different places, many different variations. I'm sure you've seen it. It reads, "One day in 1952, Donald Glaser and some colleagues at the University of Michigan were doing physics in a saloon. Someone observed that a stream of beer bubbles made a nice track. Glaser took the suggestion seriously and saw the process in a liquid that could register the path of a charged particle and then quickly expunge the marks. He thought that bubbles might be formed in a superheated liquid much as condensation droplets arise in a cloud chamber. He was right, and in April 1953, he showed a meeting of the American Physical Society pictures of tracks made by cosmic ray particles crossing a small vessel filled with hot ether."

Glaser: There's even a ballad written by some famous pop music star, I can't remember who wrote it, but anyway it's something like, "100 million men or more have stared into their beer before, but none before Don Glaser saw" and so on. I don't remember the rest of it. But you know, this is recorded widely in pop music.

Vettel: So it confirms again the point I want to raise—.

Glaser: It's just exactly what you're saying. It's totally wrong. The story is perverted by journalists.

Vettel: Because they want it to be true.

Glaser: Of course. And there are elements that are true. The essence of it was that I was trying to increase the rate at which we were collecting data about these so-called strange particles, and I had three or four ideas listed about how to do it. One of them was to use dacron, I don't know if dacron is used anymore for shirts?

Vettel: I know of it, yes, but I don't know if it is still used in shirts.

Glaser: But there's a monomer, dacron is a polymer, and there's a monomer, I think it's called acrylonitrile, I may be remembering it wrong. Nevertheless, the monomer is soluble in, I forget whether it's water or alcohol. And the fantasy I had was that you would have an aquarium filled with this monomer, and a particle would go through, and when it does, it makes ionizations, and those ionizations could polymerize locally and make sort of a plastic Christmas tree. Then I would fish out the Christmas tree and measure all of the angles, and that's how I would discover particles. So I tried it. And what do you know, it worked, except that it turned out that I didn't get Christmas trees; it gradually turned the solution brown. So what I discovered is a wonderful dosimeter for ionizing particles. So it did cause polymerization, but not connected particles, but diffuse.

Vettel: But it didn't manifest.

Glaser: It was no use to me. I didn't publish it. I mean I wasn't interested in dosimeters. But I probably should have patented it and gotten rich, but I was a man with a mission. So I ignored that one. Another idea I had is that if you have a pair of high-volt metal plates, high voltage between them, in a glass aquarium, sealed in a suitable environment, then if a particle goes through it ionizes the gas along its path and I could photograph sparks, and that was great. So I invented a thing called the spark chamber. Then I got some glass which was coated with a tin compound, I think it was stannic oxide or something. Anyway, you could make electrically conducting glass by coating glass with a conducting film. The fantasy was I'd have a stack of these things that I'd just photograph and I'd see the trajectory. That worked, that really worked; I have pictures of it. But I couldn't get the film to be durable enough. I would set up a little neon sign in there and gradually it would eat away that coating and failed. But now that's the basis for all high energy physics nowadays, and my friend Georges Charpak got the Nobel prize for building a spark chamber on a big scale.

Vettel: Had you talked to him about this?

Glaser: I don't remember whether I had or not, but it's a pretty obvious idea, and he did a lot of really clever engineering and invention. But these things, you know, they're ten times the size of this room. People walk inside of these arrays of spark gadgets. Anyhow, that's not what I wanted, but had I pursued it, I probably would have ended up the same way that he did.

And then I decided, "Okay, I'm going to use—" You see there's a general principle that if you want to detect an event which has only microscopic amounts of energy, you need to have a macroscopic amplification mechanism. So if you want to think of an old-fashioned photographic black and white film, immerse it in the developer in the dark—nothing happens. There is plenty of chemical energy around, but now shine light on it and it gets black. It wouldn't get black if it was by itself. There has to be a developer. So the general idea is that you need a resident source of energy and an amplification mechanism to convert a micro-phenomenon into a recordable macro-phenomenon.

Vettel: So that was one of the big problems. That's the problem.

Glaser: It was the fundamental problem. So I tried chemical instability, which was the dacron trick. I tried voltage instability, which is the spark chamber. I knew about photographic film as sort of a philosophic equivalent. The cloud chamber operates by having a super-saturated vapor like a humid room which badly wants to condense, and the charged particles go in through it, trigger, nucleate the condensation process. I knew that very well because in my thesis I had to really understand how the cloud chamber worked, and it was a very simple principle, but well known.

Vettel: Forced condensation?

Glaser: Nucleation-triggered condensation. So it was a pretty rational, not to say, cold-blooded search, for a meta-stable physical situation which could be triggered by a micro event. That's what I was after, and I tried everything I could think of. I even thought about freezing water, ice crystals, and I decided not to try it because it wouldn't be easy to reverse it, and I needed something that could cycle frequently.

Vettel: It would take too long to reverse it.

Glaser: It would take too long, right. Or concentrated sugar making rock candy. You know, you could think of a lot of things.

Vettel: I really am struck by all of your ideas, for instance, of pulling out "the Christmas tree." I mean, to have actual physical evidence, not simply a photograph, but it would be amazing to pull it out and say, "Okay, there's one. We'll do it again, and here's another and here's another," and just have lots of these Christmas trees all over.

Glaser: You got it. That was my fantasy.

Vettel: That would have been phenomenal.

Glaser: It would have been phenomenal.

Vettel: I mean a physical representation of what you just saw!

Glaser: Well, I tried all of the instabilities that I could think of, chemical, electrical, physical. And then came the bubble chamber and the basic question is, "Can I prepare a liquid with high density that is in a thermodynamically unstable state so that it can be triggered by a very tiny deposit of energy?" That was the problem.

[End Tape 2, Side B] ##
[Begin Tape 3, Side A]

Vettel: So you tried the chemical approach, the electrical approach, the physical approach, but all of these had the same inherent problem?

Glaser: Well, each one had a different problem, but none of them was really—

Vettel: Could trigger the micro event in the way that you wanted it to?

Glaser: Well no, they could trigger it, but you know, they cleaned off the glass and each one had its own problems. But whatever it was, it didn't produce a usable permanent record from which I could extract useful data.

Vettel: So that's the problem? They didn't record the way you wanted them to?

Glaser: That's what I was after. In the case of a liquid, one kind of instability is to heat it and then normally get to the boiling point and it boils, but in a pressure cooker you can heat it above the boiling point, which is the point of the pressure cooker. So my question was, "Could I put a liquid in a pressure cooker and get it sufficiently above it's boiling point that if I pulled the lid off very quickly, before the explosion occurs, would it be unstable enough that it will be sensitive to a particle?" That was the question. So I made a theoretical calculation. I asked myself, "What does it take to make a little bubble?" Well, you have to generate the energy to account for the surface tension to evaporate the vapor, the heat of vaporization. So you could calculate it. So I calculated what I had to do that would get it to make bubble growth, and for that I needed to know what the vapor pressure was inside of a bubble. That's complicated, because if you have a column of liquid, like in a thermometer, and there's a curved surface at the vapor/surface interface, then the vapor pressure at that curved surface is higher than the vapor pressure over a flat surface at the same temperature.

Now the question is, "What is it inside of a bubble?" The next question is, "If the bubble is tiny, am I allowed to take values out of the handbook that apply to gross substance, and [do they] apply?" So I struggled with all of these things, and I had little Carnot cycles, little pistons. Do you know about the Carnot cycle? In thermodynamics it's a heat engine that has a little piston going in and out. So I had to imagine little pistons going in and out of my bubble to calculate them. So I calculated, and then I decided that I had to have a pure liquid. I wasn't a chemist, so I went to the chemistry stockroom. I asked, "What pure liquids can I buy?"

Vettel: This was in Michigan?

Glaser: In Michigan. The guy looked at me like I was crazy. I said, "No, I need something that will boil around 30 or 40. So I looked up in the handbook, and diethyl ether, anesthetic ether, has just the right properties. Chemists use it all of the time, and it comes in sealed cans, high purity. That meant I could rely on the handbook values for it. So I got some of it, and the idea was to build a little, but first I had to ask myself, "I'm crazy. Am I going to spend all of my time when I don't even know if it will work?" So then I did an indescribably simple experiment. I'll show you a picture. I think this may be published somewhere.

Vettel: This is a picture of this experiment?

Glaser: Yes.

Vettel: I think I've seen this. Go ahead, keep going.

Glaser: Anyway, so the deal was that I put this in hot oil, and this is cooler oil, and then with the help—

Vettel: What's this?

Glaser: This is a piece of very heavy-wall glass tubing. This is thermometer tubing with a very fine bore, and this is maybe a 2 or 3 centimeter bore. And thanks to our German glassblower, he built this for me. Then we managed to fill it with the diethyl ether so that when it was hot, this was vapor, and then all the rest of this was liquid. Then I interchanged the two beakers and then this one got hot. So it got hotter and hotter, and this one got cooled. I interchanged them. So now this one wanted to boil real bad. It's like pulling the lid off of a pressure cooker. So then what I did was I got a cobalt 60 source, which is at the end of a little steel rod in a big lead case. Cobalt 60 is a pretty powerful gamma ray emitter. And this was around 3:00 in the morning when I was ready, and one of the guys, a graduate student, was there. He was out in the corridor and I was either 30 or 40 feet away, and I did my trick and then I told him, "Okay, now pull the stick out of the can," and he did it and it exploded immediately. Not exploded, but splashed; it didn't break anything. And we did it over and over again.

Vettel: And that was your experiment?

Glaser: And that was the experiment, no moving parts, didn't cost any money, but it did cost a lot of effort theoretically because I had to compute the boiling point of diethyl ether, which is around 30 centigrade, and as I recall this experiment required around 140. Now can you imagine raising the temperature to something four times its boiling point? It's ridiculous. And I calculated, this was 20 atmospheres. It was safe, the glass could still hold it. But still, there was high pressure, 300 PSI. Nothing like a SCUBA tank, but I mean, it's glass.

Even before I did this I wondered, "What's the history of this?" People have been wondering about superheat for a long time. It comes up with the steam engine all of the time. And I found a paper in a Canadian journal of physical chemistry 1924 article, and they had tried to do something like this—not for this reason, but they just wanted to know how hot could you get something before it would explode. And in those days, the editorial policies were much more lenient. So these guys said, "Well we did it. We got to 120, 130, it was really impressive. Couldn't go any higher, but the experiment became very erratic." And in fact, it was so bad, and they quoted 30 different times that they had to wait in doing a thing similar to what I did, sometimes five seconds, sometimes one second, and they gave those numbers. And they said, "This is a crummy experiment. We quit," and they published it. Now, you'd never be allowed to publish that kind of stuff nowadays, right? However I could take their 30 numbers, and I plotted them on a probability curve, and I saw it was a Poisson distribution, which means that whatever it was, it could be interpreted as a random event with a certain average rate. And for their apparatus it was one every second or one every two seconds. I knew all about cosmic rays, so I could compute the frequency with which a cosmic ray would go shooting through my little gadget.

Vettel: Using their figures that they published?

Glaser: Using their figures I got from this Poisson thing. And it turns out that their thing was firing off at about twice the rate predicted by cosmic rays. I also knew that the actual radioactivity level at sea level in a typical lab building is half cosmic rays and half it's the concrete.

Vettel: Was it exciting?

Glaser: Oh yes, it was mind blowing! It was extremely exciting.

Vettel: Everyday you must have known something was happening here?

Glaser: Well, you know, when I saw that paper, and they didn't know about cosmic rays then, it wasn't known, so you can't fault them. And probably they didn't know anything about cosmic rays anyhow because guys who work with boiling liquids don't study cosmic rays.

Vettel: Did you ever tell them? Did they ever find out?

Glaser: I have always regretted—I figured these guys are long gone, but as a young kid I shouldn't have thought that way because they were probably still around. Anyhow, that led me to design this experiment and to do it with an artificial gamma source. And then the question was, "Can I take pictures, or is this a general explosion that's going on? Is it local or not?" And I decided, "Well, if it's a gamma ray, it's a single local event," because the density, I could calculate that, but you know gamma rays were coming one at a time. It wasn't a shower bath.

Vettel: And so you could take the pictures?

Glaser: And so then I borrowed a very fast movie camera from the engineers, 3,000 frames a second, no longer a big deal, but in those days it was a big deal. And I built a little pressure cooker literally the size of my thumb, a couple cubic centimeters with a glass tube. This was sitting in a beaker of hot oil, and it was connected to a piston and a hand crank. And I sat there with two little Geiger counters, and I made them, and they were the same glass as the tube, but they had to work in hot oil, so it couldn't be the ordinary Geiger counter, but I knew how to make Geiger counters. I made Geiger counters, stuck them in, and so there was one above and one below and here's the chamber. Then I cranked it down, and the Geiger counters said whether there was a particle going through or not, and if there was, it flashed a light.

Vettel: So that's how you reduced the amount of time—because it is such a small scale.

Glaser: Yes, but that's not the relevant time, that doesn't matter. Anyway, the essential point is that I had this in a black box and the camera is not fast enough, so I had a xenon flash light like the kind that is used in cameras nowadays, standard flash. But those are too slow, those are a few hundred milliseconds.

So luckily there was a guy in the department who worked on IFF, Identification Friend or Foe, which was flashing xenon lights on fighter planes and bombers that flashed in a certain sequence so that flying at night you could tell whether it was a friendly aircraft or not. And he had leftover some tubes which I called flaming plus signs. Two electrodes here, two electrodes there and right at the intersection of the two arcs you could get a one microsecond duration by having a very small condenser hitting it. So I was able to get one microsecond thing with the shutter open all the time. And so I got a picture. That led to the talk at the New York meeting of the Physical Society. Up to that time I couldn't get any money. And I can't remember how this worked. I asked for $2,500, and I can't remember whether it was AEC [Atomic Energy Commission] or NSF [National Science Foundation] who funded.

Vettel: From your papers, it looked like the AEC.

Glaser: And I got a letter from somebody saying it would be an irresponsible use of public funds.

Vettel: Before you had done all this.

Glaser: Yes. But then even after, when it worked and I'd built a chamber big enough to see something happening, I wanted to take it to Brookhaven because we didn't have an accelerator, and so I applied for a chance to try my gadget and then I got this letter. I was confused. He said it would be an irresponsible use of public money—this is the guy who ran the Brookhaven accelerator.

But luckily one of the senior guys in the physics department at Ann Arbor knew what I was doing and we were friends, and he respected what I was doing. This guy was his friend so he wrote him, so he said, "Okay, you can come." When I got there I had my little chamber in a box, and they gave me a card table, and I had a camera, which no longer exists, called a Bolex. And the joke was they wouldn't let me have a beam line, an experimental area. But these accelerators are protected by huge blocks of concrete the size of a minivan and they stack them up like kids' blocks, and they don't fit perfectly, so they said I could put my card table outside of the crack between blocks, and I could get the background radiation that comes out. And so that's great, but I didn't know when the pulse was going to come. It comes every five seconds. So some guy from Columbia [who] was about 100 feet away took pity on me and let me connect a television cable to his electronics that I used to trigger my camera. And so the first roll of film that I ever took, 36 pictures, had on it, I can't remember, 30 or 40 examples of a pi-mu electron lead decay, and that had been seen very rarely in nuclear emulsions in balloon flights.

Vettel: So you saw it, and then it happened over and over.

Glaser: Over and over. So I came out of the darkroom with this film, pretty soon a huge mob collected.

Vettel: You must have known.

Glaser: Well, I didn't know exactly what I was going to get, but I knew I was going to get things—

Vettel: Or what it was going to get you into.

Glaser: Well, no, I knew if it worked it was going to be big. And I have on the wall just by chance something I can show you. This was done in a hydrogen bubble chamber in Berkeley, and here's a proton coming in with a couple billion volts of energy, and it makes a billiard ball collision, and it gets knocked off like this, and it makes a pi meson which comes around like this, and then the pi meson decays and makes a mu meson, the kind that I had measured the energy of, and then that decays and makes an electron which then stops. I didn't take this picture. This was taken in the big hydrogen chamber that Luis Alvarez built.

Vettel: It's beautiful. It's so elegant.

Glaser: It's really elegant, yes. One of the guys in his group gave me the picture.

Vettel: So you are finishing up your thesis at Michigan. Conceptually you are prepared to think about these sort of things. You were prepared. But you've said that you weren't specifically working on this problem because you were trying to get done.

Glaser: I was trying to finish my thesis at Caltech.

Vettel: So you were thinking about cloud chambers, but you weren't thinking about bubble chambers, but you saw the problems around you that others were having and you saw that there was a specific problem, and you could make things work better.

Glaser: I was also greatly influenced by a colleague of mine, Bud Cowan. We were graduate students together, and he said one day, "You know, we've been doing what we're told for most of our lives now. We solve problems that they put in front of us, and we get good at it. But are we trying to think of problems that they don't give us?" And he said he keeps a notebook of ideas that he has, and that set me to do the same thing, and so I've always been grateful to him. We were roommates for a while as students.

Vettel: Roommates at Caltech?

Glaser: Yes. He's retired now, but he was on the faculty at Caltech. And so I began to keep lists of things that whenever the professor would say something that I never knew about, I would always want to know, what good is it now that I now know that? What can I do that I couldn't do before? Not that I resented learning it, quite the opposite. Now that I know this, what new question can I follow?

Vettel: And so you go to Michigan. You have independence, it sounds like, at Michigan.

Glaser: I had an empty room. I didn't have any screwdrivers or pliers, nothing.

Vettel: You had an empty room, and that's what you appreciated.

Glaser: That's what I wanted, and Dick Crane who was building a synchrotron was extremely generous, and he let me steal stuff, they'd never miss it.

Vettel: So you get to Michigan late 1950.

Glaser: No, actually I left Caltech before I'd finished writing my thesis. I had all the data, and I was really in a hurry. So I went to Michigan and started working, and I wrote my thesis during my first year there. So I started in Michigan, I guess in the fall of '49, and I got my degree officially in '50.

Vettel: You were young, idealistic, ambitious, you had energy, you believed that you could actually do this.

Glaser: It takes arrogance, but it also takes naiveté. I was taking a tremendous chance when I went there. You started as an instructor in those days, not as assistant professor. I was an instructor for three or four years while I was fooling around with all these crazy ideas, and if I had joined a cyclotron group I would have been cranking out stuff.

Vettel: So I'm trying to understand. You had said you worked on the chemical chamber, and the physical approach, and the electrical approach. Were you actually creating machines for each of these approaches? And according to an article I read, you presented or finished the machine in April of 1953. So the rate at which you were building, creating, and thinking about all the different possible approaches, and these topics in general, it's all phenomenal to me.

Glaser: You have plenty of time when you are young. But there are a few amusing stories. You started me off by telling about the beer story. What actually happened was, in those days there were no summer salaries as there are now, and so faculty, professors everywhere, were sort of free—they went to Europe and whatever. And those that didn't want to go to Europe, they organized a physics symposium that met in Ann Arbor. So great men came: Serber and Bruno Rossi and Ramachandran and all those great guys, and it was very stimulating. And so we would listen to these wonderful lectures covering the whole field of astronomy and physics and so on. Then afterward, since the fraternity boys were gone—the physicists lived in the fraternity houses on very little money—then afterwards we went to the beer hall, it was called the Pretzel Bell, and we drank beer to continue the conversation. And it was a few young guys, but mostly it was these famous guys, and so it was incredible. Bruno Rossi is another one. They were from Caltech and MIT and all of those really hot places.

People knew what I was doing, and I took a lot of teasing, and they'd say, "Gee, Glaser, it should be easy, you can see tracks damn near anywhere." And that's the story about that beer thing. So it was after the fact, not before. But then—I can't remember; I guess when the Nobel Prize was announced—the owner of the Pretzel Bell claimed that I had invented the thing in his place. The walls of the Pretzel Bell were completely plastered with successful football players and football teams because all the big football games were celebrated afterwards there, and the picture they hung of me in the Pretzel Bell meant that I was the only non-football player on their walls. And then the place burnt down a few years ago, I was told, and there was some kind of plaque in there with my name on it. I never saw it until that time. And some friend of mine who lives in Ann Arbor saw it in the ashes and rescued it and sent it to me, and then I sent it back to the Physics Department.

Vettel: I can't imagine how exciting this time must have been.

Glaser: Well, it was extremely exciting. In the middle of it, I wanted to make a hydrogen bubble chamber which was really the point of the whole game because then you have only protons as targets. And if something exciting happens, you know that it's not breaking up a uranium atom or even a diethyl ether molecule. But I couldn't do hydrogen; there was no cryogenics, but there was in Chicago. Enrico Fermi had invited me to come and give a talk, and so I did. Then I made friends with the guys there, and I said, "Hey, let's try doing hydrogen. You guys have the hydrogen." And so we worked together on it. We first discovered and showed that hydrogen worked using exactly the same mathematics that worked for diethyl ether, and I tried propane and then I did xenon. The theory was absolutely accurate for everything. And then I had to go back home, and then guys in Berkeley started to fool around with hydrogen, and they had a big hydrogen establishment. So they were the first ones to see hydrogen tracks, but we were the first ones to show that hydrogen was surely going to work.

After my talk Fermi began to ask me, "Why did you think it would work?" And so I showed him roughly, and he said, "Tell us a little bit in detail." So I went to the theory, and he nodded and so on, and he was very polite but rather insistent, and I couldn't figure out why he cared. So afterwards I asked one of the young physicists who was more my size, "Why did Fermi give a damn about the theory?" He said, "You know, he also thought of a bubble chamber, and he proved that it could not work." Fermi never made a mistake, so I can't remember how this came about, but I mentioned this to one of my colleagues, Bill Nierenberg at Ann Arbor, and he said, "Well, Fermi wrote a book on thermodynamics. He's a real expert." So I got his book in thermodynamics, and I got to the place where it described the vapor pressure inside of a concave thing, and it was wrong. It was wrong for an unbelievably subtle reason. It was a diagram of a column of liquid, and they were calculating the pressures, and they ignored the pressure of the air column outside. It's just a small error, but that error made it possible for him to prove that it couldn't work. And luckily I didn't know about his book because it would have turned me off. Instead, I did my own calculation, and it was hard, but that was the critical difference. So it was a strange story.

Vettel: I think that's a better story than the story about inventing the bubble chamber in a saloon, looking at a glass of beer.

Glaser: Yes. There's another part of the story: once I had calculated this, I had to make sure that I wasn't being stupid, so I wondered if maybe water would work. Why fool around with all of these exotics, water is probably out of the question, but I decided, "What the hell?" So I smuggled a case of beer into the lab at night, and I put a bottle in a large beaker of hot oil, and I pulled off the bottle cap with the cobalt source next to it and without it. I wanted to see if there was going to be a gross difference in the foaming, just qualitatively. No difference at all. But when it was hot, the beer would hit the ceiling. So the next day the whole physics department stank like beer. And this was a problem for two reasons. One is that it was illegal to have any alcoholic beverage within 500 yards of the university. At Ann Arbor there is a street called Division Street, and that's 500 yards away or whatever it is. You're not allowed to have any saloons or beer or anything on the campus or closer [than Division Street]. The other problem was that the chairman was a very devout teetotaler, and he was furious. He almost fired me on the spot. So there are some funny beer stories, but not the popular one.

Vettel: No, not the popular one. Why don't we stop here? We've gone a little bit beyond where I thought we would be at this time. I think we'll discuss a little more of this bubble chamber in the next session. There are a lot of topics we have to talk about, but we'll pick up on the bubble chamber and then we'll probably discuss Berkeley.

[End Tape 3, Side A] ##

Interview 3, December 9, 2003

[Begin Tape 4, Side A]

Vettel: Today is Tuesday, December 9th, 2003. It's 2 o'clock and this is the third session, fourth tape, and we are meeting once again in Dr. Donald Glaser's home. Thank you again.

Glaser: You're welcome.

Vettel: This is becoming a regular occurrence. We ended the last session with a brief discussion of your development of the bubble chamber. Obviously, there are a lot of questions about the bubble chamber that I could ask, but at the same time, there are a lot of published articles about it too. I'll probably reference some of those in the oral history. Nevertheless, I wanted to ask you one more question. You approached the physics of the bubble chamber from an experimental and a theoretical point of view. Which aspect of your work with the bubble chamber was most challenging, the theoretical or the experimental?

Glaser: It's hard to separate them because the theory was rather difficult for me. I had to learn a lot of new things in order to carry it through and I had to trust some approximations, namely the values in the standard reference books and the handbooks describing the properties of liquids. I had to assume that they would hold even down to a micron size sample of liquid, and I had no way of really seriously testing that, so I assumed it. But there are a lot of theoretical things of that kind. Then I guess I mentioned to you that it was a rather delicate calculation and that Fermi got it wrong for that weird reason, that there was an error in his textbook, and I don't know whether it was his fault or not. So that was the first hurdle.

Then the second hurdle was the experimental one. But I described to you this extremely simple experiment. That went smoothly. Everything worked after that. Then it entered another phase which I didn't mention. I had two motivations, a professional one and a personal one. The professional one was that the field of particle physics was sort of stuck. We were getting new data about the existence of novel particles and the properties of ones they already knew about.

Vettel: Were these the strange particles?

Glaser: First they were called V particles, then they were called strange particles. We were learning about them at a painfully slow rate, so the theorists didn't have enough information to do much with it. We needed to have a lot more data before we could begin to understand it. So that was my main goal, but I had a secondary personal goal, which is that I didn't think that I wanted to work in large groups at big accelerators. So my fantasy was that if I developed a method that was sufficiently efficient, I could sit on a mountain and go skiing while the gadget worked and watch it now and then and think about it and work in sort of splendid, beautiful surroundings, which is sort of almost a tradition in cosmic ray physics with cloud chambers. I remember visiting a lab at the Jungfrauhoch high in the Swiss Alps where they did exactly that: they sat in a smallish place and they thought about physics and they collected data, albeit very slowly, and that looked like a very pleasant way of life – intellectual, aesthetic, and athletic. So that's sort of my fantasy.

It turned out that I was trapped, because when a particle goes through the bubble chamber, it can be thought of as putting a very hot needle into a liquid which is on the verge of boiling anyway. So that makes a very narrow, hot channel. But that heat dissipates very quickly. So what's known as the latent image, in analogy with a photographic film, has a very short life. And that means that you cannot do what was done with cloud chambers, that you have some Geiger counters above and below, and if the Geiger counters said something went through, then you can expand the chamber and catch it because the latent image lasts a long time. With a bubble chamber, you couldn't do that. So there was no way that the bubble chamber was going to be useful in cosmic rays because you didn't know when they were going to arrive. But it was perfect for the accelerators because you knew exactly when the next beam pulse was going to be. So what you did was when the beam pulse signal arrived on a coaxial cable, then you expanded the chamber and you knew you were going to get a picture of something. So it was very different and so I was trapped. I had to work at big accelerators if I wanted to exploit this development.

Vettel: Unintended consequences.

Glaser: Well, professionally it was great. Personally it was a disappointment. And also, it's a weird thing, but many of my friends of about my age and even somewhat older were very grateful to me because they said, "You know we were out of work. We weren't getting anywhere, and you generated enough work to keep all of us busy for the next God knows how many years."

Vettel: And the size of the bubble chamber also increased, too.

Glaser: I had never thought of it that way. I thought, "Well, if I don't think of something, somebody else will." But there was a tremendous amount of energy then, and so nearly every big center—Berkeley and Columbia and Chicago and CERN, among others, began to grow bubble chambers, and they got bigger and bigger, and I decided I wasn't a captain of industry type, which is the form that it took. I mean you had to get lots of money and administer large groups, many engineers, and so on. So I went far, but in Michigan I didn't have the resources to build either a hydrogen chamber because we had no cryogenics there, nor even a very large magnet, which would be needed. So instead I went in a weird direction. My fantasy was that we could only see charged particles in the bubble chamber, which was true of the cloud chamber and true also of all the other techniques. But there were a lot of events which emitted gamma rays, and I speculated about those and I were pretty sure—

Vettel: When did you speculate?

Glaser: I mean the whole community. For example, there is a meson called a pi^0, an uncharged meson, and then there's a pi^+ and pi^-. It's a triplet. And it decays into two gamma rays, which we don't see. So we never saw a pi^0 essentially. But the theory required it and we all believed it. So I decided I would try to see whether I could find these things. So my fantasy was that I wanted a bubble chamber which was like a block of lead, but transparent. Why a block of lead? Because a gamma ray converts itself into a pair electrons quickly in any material of high atomic number. So gamma rays go right through bodies, and they're used all of the time for x-rays and similar things, but they don't go through lead. So if you get a dental x-ray, the dentist puts a lead apron on your lap.

So the question was, "Could I make a bubble chamber that was of a high atomic number like liquid lead, but had to be transparent?" So I looked in all of the tables and the best candidate was tetraethyl lead, which until recently was used to make higher octane gasoline, and that was perfect. So I had lead in liquid form and it was transparent. But then I looked in the chemistry books, and it turns out that tetraethyl lead is extremely unstable, and it explodes spontaneously slightly above its boiling point. So it was great for fuel, but it wasn't great for what I wanted to do. Then the final one that I settled on was xenon, which is a pretty high atomic number, and it's the same stuff that's used in flash tubes for cameras nowadays. A rare gas, it's present in the atmosphere in one part in 10^7 and the only way to get xenon is to liquefy a cubic mile of air and boil off all of the other stuff.

Vettel: This was at Michigan, where you could not pursue the hydrogen chambers?

Glaser: I couldn't pursue hydrogen, nor could I pursue high magnetic fields.

Vettel: So rather than go to a place where you could, you stayed and pursued—

Glaser: It was a question of timing. Pretty soon Berkeley invited me to join the faculty and I was pleased to go, and there I could do that and more.

Vettel: But this was early 1950s, right?

Glaser: Yes, mid-'50s.

Vettel: Is this the evolution of—?

Glaser: That's the evolution of the bubble chamber in my lab.

Vettel: Okay, so you built a bubble chamber and then made improvements upon it, coming to zero.

Glaser: That's right. First it got bigger and bigger, and second, I had to pick a direction that didn't require liquid hydrogen and magnetic fields. But that was going forward on a huge scale in big centers everywhere, particularly Berkeley, but other places too. And Luis Alvarez was the guy who was the principal person who pushed it at Berkeley, and Jack Steinberg did it in Columbia.

Vettel: Built the larger ones?

Glaser: They built larger ones, and they were at places where they had built big accelerators –they had big staffs of engineers, and they could build big magnets and so on. So they had all of the technical resources, and until I joined such a place, I had to be clever to think of something else that would be useful.

Vettel: So in a sense, the limits of Michigan contributed to the further development of both you professionally and the development of the bubble chamber. You were looking for other chambers.

Glaser: Anyway, that's what I did. But it wasn't very big. It was only about a cubic foot inside, and that was a million dollar's worth, and that was the world's supply. We couldn't get anymore, so that's what determined how big the chamber was. But it was big enough because the radiation length is called the average conversion distance. That is, if you make a gamma ray of high energy, how far does it go before it turns into a pair of electrons? And the answer is, I don't remember exactly, but it was around two or three centimeters, while in hydrogen it would have been many, many meters. So there was no chance of seeing those in hydrogen, but in xenon we saw practically every one. So that was exciting and we had fun doing that. Actually, there was an electron-synchrotron being built by Dick Crane at Michigan, but it never was finished and I don't know all of the reasons, but it wasn't successful. And I think the reason was, there just wasn't the scale of industrialization—

Yes, I was looking for another kind of stability. I mean the first choice is clearly hydrogen because there you have only protons and that's the target. So if a beam of something, say protons, comes in and hits something else, you know it's a proton-proton collision, and therefore there's no monkey business about you smashing up an oxygen atom. But it's an elemental process which has the best hope of being understood. And we all knew that, that the hydrogen was really the important one. I couldn't do that, so I went with the xenon. I can't remember [exactly], but the Atomic Energy Commission gave me about $1 million to buy the world's supply of xenon, and half of it came from Germany, half of it came from the United States, and the Russians wouldn't sell theirs. So they then built a big xenon chamber after I showed how it would work. And that was very exciting. I mean we did see a lot of events in which something came in in sprays of electron-positron pairs. So I identified the pi^0 meson and showed its properties and found gamma rays in various other places. And it wasn't that important to physics because most people already believed it, but it certainly nailed it down. If we hadn't found it, it would have been really a shock.

Vettel: At Michigan?

Glaser: At Michigan, that there was at Berkeley, because Berkeley had been deeply involved in various aspects of the bomb project and separating uranium and so on and building cyclotrons. So they had a long tradition of people and machinery to build big stuff. I think it didn't work in Michigan because we didn't have what Berkeley had. Crane is a really smart guy and a very competent guy, but it was just too hard I think to do it in a place without the resources.

Anyhow, he did have an injector going that could generate a pulse of electrons at a known moment. So with that I measured the duration of the latent image in my bubble chambers. And theoretically I knew it was going to be no good, but you know you can't trust theory, so I tested it. And sure enough, it was way too short for me to sit on a mountain with a bubble chamber. So the night that we did that, the students and I all agreed, "Well, okay we're committed. We have to go to the big machines."

Vettel: When did you commit to the big machines?

Glaser: I don't know exactly, but it must have been '53, '54, something like that. You can check the dates better than I remember them, I'm sure.

Vettel: I'm trying to get the sequence then here. You developed the first bubble chamber in around '52?

Glaser: Something like that. Again, I don't know.

Vettel: Then had faced a professional decision in terms of which direction you're going and in terms of size and also what liquid form. I mean you faced many questions, and this is one of these unanticipated events. Because of the limited nature of experimental opportunities at Michigan, you had to pursue something bigger, so to speak?

Glaser: Right. And other than hydrogen, which I knew was, in fact, in some of my papers I said, "Well, we're using propane. We really know we ought to have hydrogen."

Vettel: And who was pursuing the hydrogen at this time?

Glaser: Luis was.

Vettel: At Berkeley and that's why he wins when he does that.

Glaser: That's right. But so was Steinberger in Columbia, and so was CERN in Geneva. They ended up building the largest bubble chamber there ever was with hydrogen, and I don't remember exactly, but it was two meters or a bit more long. It was sort of a huge bathtub.

Vettel: So then in around '53, after you had arrived at using the xenon in the chamber, that's when you started to find these strange particles, like the pi^0.

Glaser: Well the pi^0 was one, but that's not called a strange particle. The strange particles were ones that—

Vettel: But this is the period when you were—?

Glaser: This was the period when I was doing that work. Then I went to Berkeley with a trailer full of equipment. Literally I bought one of these humongous, big trailers. I never thought I would own one and somehow I owned it. I don't know, the Atomic Energy Commission bought it for me. So we hired a guy, a driver, and a tractor, and he pulled the thing to Brookhaven, and there we began working on the lambda and theta mesons which are strange particles. I shouldn't call them mesons, but they are strange particles. The most exciting question was whether their decay violates parity conservation, which was very much in the minds of physicists then. So we showed that it violated parity as well as time reversal invariance. That's another story. But there was strong motivation to collect a lot of them, and I did that, and then I went to Berkeley with the same trailer, and this time with graduate students and children's playpens and God knows what else, and we had to move the whole outfit.

Vettel: So you were at Brookhaven pursuing questions about time reversal invariance, you said it's another story?

Glaser: Well, the theoretical question of what means parity conservation and what means time reversal invariance – it's a bit of a story.

Vettel: Well, go ahead; I'd like to hear it. Then I want to get back to the children's cribs.

Glaser: Well, the very simplified version of it is that if you make a movie of somebody running down the street and play it backwards, you know it's silly, so you can tell whether time is going backwards or forwards. If you watch a ping pong game in which spins are allowed, and you play it backwards, it will look silly.

But if you can't see the players and spin is not allowed, then you can't tell—the ball is bouncing back and forth. So the general intuition is that as you go to less and less biological and simpler and simpler systems, you begin to lose the ability to distinguish time going forward and time backward. And that's an endless debate having to do with evolution and cosmic and so on. But that's the simple explanation of it.

So everybody assumed that if you're talking about these little submicroscopic particles, that they don't have any knowledge of time—And the other question is that there are right-handed and left-handed people, there are even right and left handed molecules. Dextrose, for example, is a right-handed sugar, and levulose is the same thing exactly, but it's a mirror image. And so it is with the amino acids. They're all left-handed in our biology, but they don't need to be. If you make them synthetically, you get what's called a racemic mixture—half are right-handed, half are left-handed. But if they come out of a biological process, they're all the same. So the notion was that as you get to simpler and simpler and less biological things, handedness also goes away. So you would think that, unlike a bowling ball, which has handedness because of the finger holes—I guess, maybe not, I'm not sure about that. But anyhow, as you get to billiard balls, then they're symmetrical, and the fantasy was that these little particles are billiard balls. They can't tell if they're getting older or not. They're not right-handed, they're not left-handed and Lee and Yang, a couple of really smart guys at Columbia, said, "You know, it's all very well that we have that intuition, but nobody's proved it, and it may be that these guys can tell if they're right-handed or left-handed."

To make a long story short, they were right that these particles do know if they're left-handed or right-handed, and there are different kinds. Time reversal invariance means that if you run the movie backwards, you can't tell which one is the right one, but you can with these guys. So they suddenly were more complicated than our intuition led us, so that opened a large question. Okay, they showed it for beta decay, in which a neutron goes to an electron, a proton, and a neutrino. But the question is, how about all of these other particles? Are they right-handed, left-handed? Are they getting older? Do they know they're getting older? We then mounted a number of experiments in which you look to see where the V's were and what the symmetry was and so on. Then we showed that they violated parity because they preferred one orientation instead of another.

Vettel: And this was at Brookhaven?

Glaser: This was begun at Brookhaven, but then especially at Berkeley at the Bevatron. And again, that's when we arrived with this enormous, I don't know, 40-foot long trailer or something like that. And I just discovered a few years ago that that trailer is still there, and they are asking, "please sign off on it?"

Vettel: I hope you didn't, you should just leave it there, for the sake of history. They seem to be tearing everything else down, even the Bevatron.

Glaser: Oh no, no. They were using it for storage and I said, "Well you guys can do what you want with it, but I am releasing my interest in it."

Vettel: What students were working with you? Did you take students from Michigan with you to Brookhaven?

Glaser: Brookhaven and then to Berkeley.

Vettel: Who?

Glaser: John Brown, Jack Vandervelde are two that come to mind. Oh gosh, there's another guy who stayed at Berkeley on the staff. I'm embarrassed. He's a good guy and I'm boffing his name. John Brown also. All of them were on the Berkeley staff for a while, and some of them stayed for their whole career.

Vettel: Now, you mentioned you were traveling with cribs from Michigan?

Glaser: So these guys were married and they had babies, so they had to move and they didn't have any money and we had this huge trailer, so we put all of our equipment in it and there was enough to put in a few things.

Vettel: And the Atomic Energy Commission picked it up, right?

Glaser: Well it wasn't going to cost them anything because they had already paid for the trailer, so we weren't hurting anybody.

Vettel: No. Given the experimental nature of your work during the '50s, not only what you're accomplishing, but the range that you're doing it in, it sounds as if it's not well suited for a large team. I mean just in the practical sense, you had to move. In the physical sense, it couldn't be large. I mean you're dealing with not a lot of space, not a lot of time, and also you have to combine in one experiment both the theoretical and the experimental. Could a large team do this?

Glaser: Well, another way to put it is that I picked problems that I could do myself with a few students, and those were ones that didn't require enormous amounts of money or enormous resources, and that's my present work. I've chosen partly for that same reason so that a lot of it depends on my own impulse and pleasure of invention. I need to have students to interact with, so that's an essential part of it. Not just fun, it's essential, the students and the post-docs. But that's how I like to work because I'm very impatient at having committee meetings and listening to endless speeches that have mixed motives sometimes. I'd rather just sit and agonize myself and then argue with the guys, and it gets better and better and pretty soon it gets somewhere.

Vettel: Had the direction of physics, in terms of the big, large team oriented laboratories bypassed a certain point of no return? The experimental world of physics was so large, almost cumbersome at this moment, that for someone like yourself who is intentionally seeking out opportunities where it's more intimate, this was a unique moment – the field was moving in one direction and you were moving in another.

Glaser: Yes. I mean there are some things that absolutely require large teams like building the big bubble chambers and running them and the big accelerators and telescopes and all kinds of stuff, even fMRI in biology. You know it's a multimillion-dollar gadget, and it takes a lot of engineering, and it takes a number of different kinds of competences. I'm going to spend all day tomorrow at a meeting reporting what the local guys have been doing with fMRI. I'm looking forward to that.

Vettel: Really?

Glaser: No. They had a lot of trouble, and it wasn't their fault. They were supplied with a defective instrument, a cutting edge thing that didn't work, and I think they have it working now, but I am looking forward to perhaps using it in some of our work.

Anyway, so many fields have evolved into ponderous activities. Astronomy always has, in the sense of Copernicus essentially. And obviously space science, rocket science, all of that stuff is big stuff. And yet at many junctions a critical invention is needed, and that usually isn't done by a committee; it's usually one guy or several, a small number who are immersed in it so they know intimately what the problems are, and then they make an invention. If it's important, then everybody picks it up, and it gets industrialized. That's sort of the dynamic, but the original ideas, well I have limited experience, but in my experience I'm not the only guy, there are many people who have the same experience—it's an individual who is well versed and who takes on a problem that he or she sees as very important and sometimes succeeds.

Vettel: Would you say these are the individuals who win the Nobel Prize in general?

Glaser: Yes, although Luis got a Nobel Prize for essentially industrializing the field, and he contributed a lot. He made a big group and wonderful technology that exploited the method and contributed a lot towards the knowledge of the particles. But there wasn't anything in there that was a real novel invention. It was just very high-class industrial engineering.

Vettel: So there is space for both.

Glaser: Yes. Both are essential, at least in physics. The same thing in astronomy. I mean some individual guy may have thought of the idea of an adaptive mirror, a mirror that is thin enough that it can fluctuate with time to compensate for the heat waves in the air. It's amazing you can do that. But somebody thought of that, and then it took a lot of engineering to make it happen. So that's kind of how it goes.

Vettel: It involves the personality of physics, the interaction between those that prefer the individual group and —

Glaser: That's right. Everybody contributes according to—It's almost like Marx said, "—according to his ability." But the other parts of Marx's quotes don't work.

[End Tape 4, Side A] ##
[Begin Tape 4, Side B]

Vettel: Did you feel like you were in competition at all with anybody else developing a bubble chamber during your research with mesons, strange particles?

Glaser: Oh sure. That is, I would have loved nothing more than to be able to build a hydrogen chamber and pursue the physics, but I couldn't. By the time I arrived at Berkeley the thing was well under way, and I couldn't have made any significant contribution. In fact, I came to Berkeley because Luis wanted me around, and he offered me a job. And I said, "Well, I'm a professor in Michigan. What are you offering me?" He said, "Well, I can't offer a professorship, but you could work in my group," and I said, "No."

Vettel: And what year was that about?

Glaser: '58, something like that. But he was really very interested. So I think he was one of the promoters that got me an appointment at Berkeley. We were never really friends, but he wanted me around, and I guess so did the other guys.

Vettel: In 1952 and '53, however, did you feel like you were an island? Were there others also trying to improve upon the Wilson cloud chamber or developing?

Glaser: Yes. There was a sizable group at Brookhaven, and everybody knew that we weren't getting data fast enough. The group at Brookhaven, Ralph Schutt was the director of it, a very capable guy, was building a high pressure cloud chamber. And he had mentioned that. Anyway, the main point was that if you make the pressure higher and higher, then you can have a better chance of seeing a collision, the density goes up. But I don't remember what pressures. If I had to guess I would say it was 20 atmospheres or something, 300 psi, which isn't a hard thing engineering wise. My SCUBA tank is 2200 PSI, so that's not such a big deal. But it was sort of like molasses. It took so long for the chamber to stabilize so that you could take another picture, like every 20 minutes. So I saw that, and I said, "My god, there's got to be a better way than this."

Vettel: Were you motivated by the competition at all or did it not matter?

Glaser: Well, there wasn't any competition until I was well under way, and then it was inevitable that everybody was going to do it. I wanted to work on hydrogen, so I went to Chicago, and I started a collaborative project because they had cryogenics, and it was near Ann Arbor. So I worked with them, and we were the first to prove that hydrogen behaved just like every other liquid, and that my theory, simple as it was, accurately described under what conditions it would. So some people were using helium and other people, you know, everything you could think of. So there was a lot of competition because everybody saw it was going to be useful. But I realized I couldn't compete in my circumstances, and I'm not sure I wanted to either because I didn't want to become a captain of industry. I decided if you're going to be a captain of industry, you have to get rich, and I decided that wasn't going to happen, not for me.

Vettel: Not here. And you went to Chicago in '50-what?

Glaser: I don't know. It must have been '56, something like that. I'm guessing.

Vettel: Your work, the bubble chamber, mesons, work at Brookhaven and Chicago in the hydrogen, where did you publish those? Did you publish all of your findings at this time?

Glaser: Yes, everything was published, unlike what I do now. I don't publish as much as I should now, actually. I'd better get going on it. I'm just lazy about writing it up. But yes, I mean it mostly would have been in the *Physical Review,* I would guess. But in high energy phsyics in those days, one of the more popular journals was the Italian journal, *Il Nuovo Cimento*, I don't know why, but those were the two places. Now there are zillions of journals, and I don't keep track of what's going on in high energy physics. But that's where I mostly published.

Vettel: So you published in those two?

Glaser: Mostly, yes.

Vettel: The Italian one as well?

Glaser: Yes.

Vettel: Did they translate it?

Glaser: I don't remember why, but that's how it was done. I don't think I published technical papers, but physics results. I remember I gave a talk in Italy one time early on. There was a very respectable guy, Bernardini, a very lovely man, a very good physicist, and I told about how we were starting to do the hydrogen experiments, and he was sort of flabbergasted that we were moving so fast. And then he gave a big speech about how wonderful it was and that I had proved that you didn't have to have a lot of money in order to make important contributions in high energy physics—a big philosophical interpretation. Then I was asked what I thought about this, and I said, "Well, I'm very flattered, but look out. These bubble chambers are going to cost almost as much as the accelerators by the time they're really useful. So it's good, the idea was cheap, but it's not going to be cheap," and he was a little upset by that.

Vettel: And your experience with the Atomic Energy Commission paying for all of the xenon in the world?

Glaser: Yes.

Vettel: Were your articles that you published instantly accepted in general?

Glaser: Yes.

Vettel: What was that like? I mean you were young.

Glaser: Right. Well, the first paper I wanted to publish was bounced by the *Physical Review* because I used the word bubblet, and the editor pointed out it wasn't in the *Unabridged Webster Dictionary*. And I was used to droplets in cloud chambers, so bubblets seemed like a very reasonable word. Nothing doing. But I had to rewrite the damn paper without bubblet in it. And then the first oral presentation was at the major physical society meeting of the year, which was always in Washington, I think at the Shoreham Hotel, I'm not sure of that. And the American Physical Society is very democratic. Anybody who declares his interests in physics can join, and anybody who's a member can volunteer to give a paper, and I think in those days they accepted all papers. So the meeting always went for a week. It started on Monday and ended on Saturday. So Saturday was well known. Everybody had gone home by then, and that was when the crackpot sessions took place because there are all kinds of crazy people. So I was in the crackpot session the first time that I was going to give a talk. I remember that the woman before me had just written out of her apartment in Manhattan and claimed that everybody was wrong: the charge on the electron was one-third of what everybody said, stuff like that.

Vettel: And that's what you followed?

Glaser: I was next, right. I can't remember how this was, but I was sitting at a table having a snack with some of the guys in the middle of the week, and some of them were from Berkeley. They were friends of mine or acquaintances, and Luis came up, and we were introduced for the first time. And I was telling about what I was going to talk about on Saturday, and Luis said, "Gee, that sounds important, but I have to leave. I won't be able to be there." So he sent a bunch of his guys, and he made them stay to come to my talk, and they took notes assiduously and so on. He recognized very quickly that it was going to be important, and that's when he tried to hire me on the spot.

Vettel: What year was that?

Glaser: That was about '58 or so, '59. Then he sent all of those guys and I thought, "My god, the spies are closing in on me," and I couldn't compete with this guy. He's a famous physicist and has the biggest lab outside of the government labs in the country. Berkeley was riding high then; the Bevatron was the foremost machine in the world. So I was in the crackpot session and my paper was rejected.

But then the big triumph was that, maybe a year later, I was invited to give a talk at the New York meeting, which was another major meeting. I'd never been to the New York meeting. At that time it was a very tidy story. Here's this kid with this little tiny glass tube, and here's this gadget where you can see cosmic rays and so on. It was a nice story, and it didn't take any special knowledge to see that it was interesting.

Vettel: It sounds like the New York meeting was in '59.

Glaser: I don't remember when it was. Everything is in '56 or '57. I don't have a good sense of dates.

Vettel: So around '56, '57. Looking back at this time period from '52 through '56, '57, '58 perhaps, what did you learn? What did you take from this professionally? Or personally?

Glaser: You mean scientifically?

Vettel: Professionally. Personally.

Glaser: Do you mean in terms of the way of working as a scientist or do you mean in terms of what I learned about physics?

Vettel: O.K., how about, working as a professional.

Glaser: I don't think I learned anything. That's just how I am. I don't recall that I changed very much. I had done a bachelor's thesis that was never published on basal pseudo-morphism which is a phenomenon in which I had annealed a copper surface which then was macrocrystals, and then I evaporated onto it aluminum from a very red-hot aluminum wire. What happens is that the copper has a cubic crystal pattern, and the size of the little cubes is not quite the same as the size of the aluminum cubes. The copper is a bit smaller, but the aluminum anyway follows the crystal habit or the copper, so it gets packed in slightly crowded. So instead of cubes now they're little parallelopipeds. So that was an interesting phenomenon. And then I had an electron diffraction camera. I had inherited all of this from some earlier student, but it didn't work. So my job was to make the damn thing work, and mostly it consisted of finding leaks in the vacuum system. That turned out to be very important because the aluminum that it deposited made very beautiful mirrors and because they were crowded in, they were tougher, resistant to scratching, and so on. It's apparently used now on big telescopes. I didn't publish it, I didn't know anything about publishing, but I think my advisor published it. But I was used to working by myself.

Vettel: Personally then, looking back, from the standpoint of the present, did you appreciate what you were doing and what was happening, the speed with which things were happening and the success you were having? Did you appreciate it at the time?

Glaser: Well, I don't know. I thought it was fun, and it was hard work, and I could do it. But I got one very important lesson. It was, I don't know if I told you, but I was taking a course in x-rays. Did I tell you the story about the x-rays?

Vettel: Go ahead.

Glaser: The professor was Chuck Smith. I was with a bunch of Navy guys. I think I mentioned this story. I was about 16, I think, and he said, "Hey, you guys. Don't stand too close to that x-ray machine." I've told you this story. Then later I had to cut some x-ray film so it would fit in a camera, and it didn't fit. Did I tell you about the broken scissors? So there was a pair of scissors there and I was supposed to use those scissors to cut the film, and I couldn't because the tip of the scissors was broken off, and I couldn't do it. So I went to him and I said, "Hey, do you have a good pair of scissors?" He said, "Glaser, don't be so helpless. There's a grinding wheel in the shop. Grind it down yourself." That would never have occurred to me. But one of the more important things I learned as an undergraduate was the notion that you shouldn't be helpless.

Vettel: Luis Alvarez was recruiting you, clearly. I'm sure it was an honor just to be recruited by Mr. Alvarez.

Glaser: Oh yes.

Vettel: Did you like Michigan at the time? Were you looking? Were you open to other offers?

Glaser: Well, Michigan was and is a very good university. It's not in the first rank, but right below it, and still is. And it was boring to live there. For example, nobody wanted to go to Detroit ever. We all wanted to go to San Francisco, but never to Detroit. It's flat, and there's no skiing anywhere close. So I wanted to go to a more interesting place, and I had loved Caltech, so I wanted to go back to California. But I think the fact that it was boring probably helped my work. So I worked, whereas, around here, I would have gone playing somewhere. In fact, for the first six months that I was here, if the sun was out, I would decide, "Gee, I don't have to work today." So I'd decide I could go and play because that was true in Ann Arbor, during the winter anyway.

Vettel: I noticed in your personal papers in the early to mid-'50s, there's correspondence between you and other universities. How do I put this? They're saying, in effect, that they don't have tenured space for you. That they don't have an interest in hiring you. So it almost appears as if either you are looking for a job in the mid-'50s, or they are asking you to come, but not as faculty? Were you looking for a job, or did other programs want you but not to the level that they would in just a few years?

Glaser: I don't remember that too well. The thing I do remember is that I wanted to go back to California. I don't think I would have been very enthusiastic about going to New York or Cambridge because I loved playing outdoors and sports and things, not competitive sports, but skiing and hiking and swimming and sailing and that kind of stuff. So I really like the way of life in California.

Vettel: Then in 1959 you were presented with an offer to join the faculty at UC Berkeley and you accepted?

Glaser: Yes, I accepted. But my friends in Ann Arbor begged me not to do it and said, "Please just go for a year's trial and see if you like it." So I couldn't not do that. So the first year I came in '59, I was a visiting professor, but that was simply out of loyalty to my friends.

Vettel: But you knew deep down you were probably gone?

Glaser: I was going to stay.

Vettel: Did you know in '59 or did Michigan know or did Berkeley know, or did anybody have an inkling that you were about to win the Nobel Prize?

Glaser: I think so.

Vettel: When did you have your first inkling?

Glaser: A friend of mine mentioned that he had nominated me, Jack Fry.

Vettel: Where was he from?

Glaser: He was a professor at either Wisconsin or Minnesota. I think it was Wisconsin.

Vettel: Do you have any idea when that was?

Glaser: No. You know, late ‘50s. Everything was the late ‘50s, but I can’t say exactly when. I wasn’t totally surprised. It was clear that what I did was having an enormous effect on the field, and that real physics was coming out of it, which is the criterion in my mind. So it seemed to be important. However, there were a lot of other important things that happen all of the time. So you never know in that sense.

Vettel: Did you feel pretty good about where you were?

Glaser: Oh yes, I was very pleased.

Vettel: I’m sure you were looked upon as a hotshot, but did you— ?

Glaser: Yes, I mean I enjoyed what I was doing, and I had a lot of friends and colleagues that had arisen from the professional interactions out of it. So it was successful and a career that I enjoyed. Sure.

Vettel: You took the job at Berkeley in ’59, and you had an inkling that the Nobel Prize might be in your future. You arrive at Berkeley. Did you accept a permanent position at Berkeley before or after you received the award?

Glaser: I don’t remember the legal business, but I guess I must have made it clear in conversation that I wanted to stay at Berkeley, and I was given a permanent position, but that officially I would be a visitor out of loyalty to my friends. I told them exactly how I felt. So I think legally I was probably a visiting professor, but with a commitment from them and from me. It’s not the real world, it’s academia, so people’s word counts.

Vettel: Where was your office and lab space, just out of curiosity?

Glaser: I had an office in—Well, Birge wasn’t here, so that would mean the old physics building. My lab was on the hill [Lawrence Berkeley Laboratory].

Vettel: And you do some work at the Bevatron?

Glaser: Oh yes. I mean that was what I did, worked the Bevatron, and I had a lab there and a group of, I don’t know, six or eight graduate students.

Vettel: So you were able to keep it somewhat small?

Glaser: Oh, yes. That was what I wanted to do. So we built our own bubble chambers, and we were sitting in a concrete house like everybody else. The typical thing at the big accelerators is that the beam comes out at a certain place, and these circular machines have beams at a number of different places, so you get a schedule to work at a particular beam site, and then you make a drawing, a layout of what your apparatus is going to look like. Then huge cranes appear with concrete blocks the size of a full size van, like the kind they use at the airport. So they stack those up, usually two or three high. Then you have a light bulb hanging from the ceiling and you sit there with your students and every five seconds there's a bang when the machine goes off, and you're constantly checking to make sure it works, and you sleep in shifts, and so on because it's experimental equipment, it's not a consumer product. They're reliable, but not perfect, so they take monitoring. But I remember, I'll never forget. We would sit around and we'd get sort of dopey, and we started to wonder, "You know the current theory is that the universe is expanding, and it's an accordion, and very likely it's going to come back, and it's going to be a big crunch, and that everything we're doing, all of the knowledge we're generating, will be finished. So is it really worth it?"

Vettel: Late at night.

Glaser: Late at night. So we decided, "Well, if that's true, then there must have been some guys like us at the previous cycle before the so-called "big bang" and they must have tried very hard to do something so we would know they were there. What could they have done?" Well, we decided what they could do is make a bunch of bombs or some energy situation that would generate an asymmetry. So there may be more protons than antiprotons because these guys had fixed it up that way. They knew enough to do that. And that's true. There are more protons than there are antiprotons. So we decided that we could take the unexpected asymmetries in the cosmos as it was known to be evidence that somebody had monkeyed the data. And it was just fun late at night. I guess it was a philosophical question. I'm sure many people ask the same question, "What is my life worth?" Well, we were asking, "What is the whole civilization worth?"

Vettel: And what will come of it.

[End Tape 4, Side B] ##
[Begin Tape 5, Side A]

Vettel: This is Tape 5 on December 9th, continuing. I'm probably not the only one who is struck by the irony of the contrasts that surround your Nobel Prize. I mean, everything that comes with the award is huge, and yet, the work that you did takes place in one hundredth of a billionth of a millimeter. Furthermore, you were a relatively young scientist at the time that you won the prize. What was this moment like? How did you interpret this time period?

Glaser: Well the scientific part was a time of great excitement, discoveries all of the time. And in fact whenever I had to go on a trip, I got a leather attaché case, and I designed and built a stereo film viewer in it which the machine shop made for me. We were generating millions of photographs all the time. So whenever I traveled I would have a few hundred feet of film that I could look at instead of reading a book, looking through it, discovering new particles. And it was very exciting because occasionally I found something that was unusual, and of course we had rooms full of people who we called scanners who were doing that too. It was a little weird because in those days there were no women physicists, so all of the graduate students were men, and they had very smart wives, very bright, lively bunch of people. Each one of them had a somewhat different visual system, so that some of them would be partial to different kinds of events than others, they had different efficiencies for discovering different kinds of things. And so the way we operated was that we told them roughly what we knew about particle physics and what they should see that was humdrum and ignore it, and if there was anything else they couldn't understand, then they should tell us about it.

Vettel: You are talking about people's propensity to see different things.

Glaser: So we had a standard piece of film, 100 feet or so, and we would periodically calibrate these women to see what their efficiency was, so we knew what the efficiency was for each one. But there was always turnover, so there were new people. So every now and then my secretary would tell me, "Suzy has missed her period. You better have her calibrated," because you know, they were getting pregnant at a tremendous rate. So that's how it was. So I suddenly realized, "What in the hell's going on? I've become a manager of, in fact, a sweat shop." I had my whole army. There was a large number of people—tens, it could have been even one hundred.

Vettel: This is at Berkeley?

Glaser: Berkeley. I had all these scanner people, so there was no point in my having a bright idea anymore because what I was doing now was going to take two years to collect enough data, analyze it, run this army, and so on. So I began to see this wasn't for me. That was the seed of my beginning to decide I was going to quit. That, plus the fact that Berkeley had the best accelerator, and I could do these interesting experiments, but it was pretty clear that pretty soon it was going to be Argonne at Chicago or it was going to be CERN or it was going to be some other place. And I didn't want to become that, and I don't like traveling either. These guys who stay in this field are committed. There was a physics department party a couple days ago, and these guys go back and forth to CERN two or three times a year. They have nine-hour jet lag, and they're away from their families, and so on. So anyhow, that's a sort of a side thing.

Vettel: At the time that you are either winning or are about to win the Nobel Prize, you're also tiring of the field. But let's go back to the actual prize, what was it like to receive it?

Glaser: Well, it was like a movie set. I mean, it was something really out of this world. [Alfred] Nobel named physics first in the list of prize awards. My wife, who was brand new at the time--we were just married--was the king's date as it were, and

I got the queen or princess or somebody, I forgot. Anyway, we entered the hall for the big dinner of a thousand people. So everybody bowed or curtsied. So to see a thousand heads going down, it was just like an MGM movie. It was really quite remarkable. And the whole thing was like that, so you know it was a fun party. It was sort of an incredible party. I gave a standard professional talk to the physics group and, you know, the medical guys [laureates] talked to the medical group and so on.

But then I remembered, oh, and I mustn't tell that. I had a lot of interactions with the royal family, which is not proper to repeat. But there was one funny one. I was sitting next to the King at this big banquet, lovely guy. He was a real archaeologist, and he really went crawling around on his knees in remote places and dug up stuff, a real guy. I loved that man. I was impressed. So it came to cognac time at the end of the meal, and I said, "You know, I'm not used to drinking very much, and I don't really want a cognac," and he said, "I have a special kind you've got to try." So I couldn't say no.

Vettel: No, you don't say no to the King.

Glaser: So the guy arrived with the silver platter with two cognacs, and he sipped his and I sipped mine. It was tea. He didn't want cognac either, so he had them brew tea that was just the same color as everybody else's cognac. I thought that was really charming. He was a lovely guy.

Vettel: In your acceptance speech, you make reference to those who contributed to the work that you did, and yet I'm still struck by how small and novel and individualized your contributions were. Why do you think you won the award?

Glaser: Well, you know, there's the official reason. Nobel said, "You're supposed to contribute to the welfare of mankind."

Vettel: But there has to be a single great thing.

Glaser: That's right. But the way that's interpreted is that you made an important contribution in this case to physics, which is judged essentially by your peers because they get nominations from everybody and then they have big debates. So I had started what was a major development in physics at the time, which gave us a handle on answering these profound questions, "What is the universe made of? What are the particles? Are they time reversal invariant? Do they know they're getting older or not? Are there right-handed and left-handed ones?" And all of this information was sort of, each part of it was like a piece of a huge jigsaw puzzle, and you had to fill in a lot of the pieces before you could get the picture. We only had a few pieces when I started, and we were collecting pieces very slowly. So I had a major effect, even though it was a humble invention. I mean it was a gadget, but this gadget had a huge effect on many, many professional physicists, and on the progress in the field.

Vettel: So from your perspective it was not just the invention of this gadget, the bubble chamber, not just the scientific understandings that you were able to arrive at using this gadget, but also how others used this gadget.

Glaser: Mainly that, because you know I was one guy with a little group and there were huge groups all over. We had a collaboration because we were generating, I don't remember, but millions of pictures a year. Every five seconds you'd take a picture, and the other groups were doing the same thing. So no way could I look at all of those pictures. I mean, I couldn't get the money or the people to do it. So I sent it all over the world, so there were groups in Italy and groups in Germany. We had to meet in Geneva to agree on the final draft of one of the papers, and there were 23 authors and I thought, this is awful. Now a paper can have 500 or more authors. But I thought already that that was ridiculous to have a committee to decide on a scientific result.

Vettel: This world of physics really is spinning out of control in terms of the way you wanted to practice science.

Glaser: It's not only physics. In astronomy it's the same thing. NASA's program of exploration requires enormous amounts of money and cooperation of huge numbers of people. We've talked about that before. And yet, the ideas come from individuals. I think that's very typical in every part of science. There may be in the groups two people who fire each other up, but it's not ten people.

Vettel: I'm getting a sense that discoveries are happening about as rapidly as this field is evolving and growing. So when you hear people say that you won the Nobel Prize for your invention of the bubble chamber, that almost sounds a little too narrow.

Glaser: Yes, it does. I'm always slightly offended by that, but that's what happened, because at the time, we had made some discoveries, but the potential of it was known to everybody. However, it's true in almost every part of science that a major technique or a major instrument has an enormous effect on productivity. The electron microscope, the ability to sequence DNA, you know, one thing after another. It's a sort of a little tiny thing to someone who doesn't know what the problem is, but which has an enormous lever. As an example, I mentioned this flexible mirror business, and there are many things like that. There are many things like that. So instruments are critical and theoretical ideas are critical too, but in between there is a lot of work that can be done by, and has to be done by, groups of trained, talented people.

Vettel: The field is growing in size, becoming almost cumbersome, which made it rather hard or undesirable for you to do to continue working in physics. You once said that the Nobel Prize made it hard for you to continue to do science. It's hard to discipline yourself to recognize that the next thing you do won't be as important. Did winning the Nobel Prize also contribute to your decision to try a different field?

Glaser: I don't know, but I had already decided to change fields for all of the reasons that I've mentioned. And there is a consolidation—it's not only the field is getting bigger, but the number of places where you can do it is getting smaller. So now at this meeting yesterday, this party, these guys are talking about one big accelerator that will be a big international collaboration that might be in Japan, it might be in Geneva, it might be in Kansas. It's a big political issue, but no one country wants to spend that much money. So they're making a very elaborate plan. In fact, the guy I was talking to is Italian. He had just been hired by Berkeley from Italy, and he's been working with CERN. So that's what's happening in that field. So I had already decided that I wanted to switch fields, but I think it's almost a matter of statistics that, to win a Nobel Prize, you have to do something that is very important but with a huge amount of luck in it too. So the chances that you hit the jackpot twice, meaning you have the right idea at the right time when the field is in the right stage to take advantage of it, that means that the idea has to be useful to everybody else.

Vettel: After winning it, did you feel like, "Now I have to do it again to prove my work?"

Glaser: No. I mean I know that it's a roulette wheel. So I know that I'm in it and I enjoy doing science and I'm going to keep doing science that suits my style, and I will undertake things that I think are important and to which I think I can make a contribution.

Vettel: But not in physics.

Glaser: No, it could have been physics. I mean I was considering going into cosmology and astronomy and that kind of thing, which is having a very exciting run now. It's very, very interesting. But again, that's a big team thing. But I find it very exciting what they're doing now.

Vettel: When you were young, when you said at the time, it's hard to discipline yourself to recognize that the next thing you do won't be as important, that's a significant thing to realize and learn at such a young age. I would think that is very very hard.

Glaser: Yes. I mean I'd be glad to be wrong, but it seems to me that to move forward with any enthusiasm, you have to face that overwhelming probability.

Vettel: And what did that mean to you?

Glaser: It meant that I'm going to pick something that I enjoy doing, that I think is respectable and legal, that I can earn a living doing, and that I'm going to like to work at it.

Vettel: And has opportunity.

Glaser: Yes, because I like working in science. I mean, I've often asked myself, "Suppose I feel that I'm no good at science anymore or that, for some reason, I decide not to do science. What else would I do?" Well, I could be a violist again. I love wildlife photography. I could spend my life outdoors stalking animals. That would be fun. So there are a few things which now I regard as hobbies and which other people regard as professions. I couldn't be a competitive violist anymore. It's just out of the question. There was a guy, Greenwalt I think, who was head of Du Pont. When he retired he decided that he was going to study hummingbirds. He had no shortage of money, so he bought the very best cameras and so on, and a couple of assistants, and he traveled around the world and generated the world's best book of color photographs of hummingbirds. Well, I think that's a very respectable thing to do. So I've thought about that, but I'm enjoying what I'm doing; and when I run out of gas, then maybe I'll try something else.

Vettel: New opportunities.

Glaser: Yes. I mean the goal is to have fun and be moderately productive and do no harm.

Vettel: And the easiest way to do that perhaps is, once you've won the Nobel Prize in one field, you could go into other fields where there are just as many great opportunities.

Glaser: Yes. There are great opportunities everywhere, but the people who have won the Nobel Prize in physics, in high energy successively, have discovered a new particle which took an enormous team and an enormous amount of money, and so on and so on and so on. They now designate a person as the spokesman for the team. They don't call him the leader or the director or whatever you would say, no, just a spokesman out of, you know, about 500 guys. One of them is the official spokesman. That's a weird way to do things.

Vettel: Well, the other story about the royalty? Do you want to give a hint at what it was about? I'm sure people would like to know more about the event itself, a behind-the-curtains look.

Glaser: No, not on tape.

[End Tape 5, Side A]##

Interview 4, December 16, 2003

[Begin Tape 6, Side A]

Vettel: Today is December 16th. This is the sixth tape in the interview with Dr. Glaser. We're in his home. It's two o'clock. We've covered a lot of ground, and we still have a ways to go.

Glaser: I guess so.

Vettel: You've had quite a professional run.

Glaser: Yes.

Vettel: We ended the last session talking about the Nobel Prize. I'd like to ask one more brief question, and then we'll move onto the next phase. I've noticed Nobel Prize winners react to the prize, or the award, or their career following the award, in very different ways. Some cling to the science and scientific field in which they won the award; others bow out somewhat gracefully; others not so gracefully. You, however, shifted into another field, roughly called molecular biology. Is that so?

Glaser: Yes.

Vettel: How long had you been thinking about leaving? When did you start thinking about leaving particle physics?

Glaser: Well, I was always interested in the fundamentals of molecular biology because of the courses and seminars given by Max Delbruck at Caltech when I was a graduate student. Then I began to be unhappy with high energy physics when it grew so enormous in scale, costing a lot of money and involving enormous teams and endless committee meetings and so on, as well as travel to just one of the two or three sites where you could really do cutting-edge experimental high energy physics. So those were the positive and the negative reasons that I switched. It was getting worse and worse, and the field is now awful in the sociological sense, but scientifically it's just as interesting and profound as ever.

But the Nobel Prize made it a lot easier for me to switch. I would have done it anyway, but it was clear that they wouldn't fire me if I didn't publish something for a couple of years while I was learning a new field. And a younger person without tenure could not do such a thing because if you're an assistant professor, you had better well continue publishing in the field which you were essentially hired to pursue. The whole point of tenure is to give faculty members the opportunity to take chances and switch fields, following their interests or following the developments in the field. So I was going to do it anyway, but the award in a way liberated me so that I could change fields more gracefully.

Vettel: So let's just say history had worked slightly different and you had won it in 1958, it's possible you would have made the switch in '58?

Glaser: That's very hard to answer because my sense of history isn't very good and I'm not sure. It may be that it was very fun and exciting and doable still in that day. It got steadily worse, but there are a lot of rewards to being in a very exciting field.

Vettel: Molecular biology at this time is certainly exciting, from '53 with Watson and Crick to the code in '60.

Glaser: It was very exciting and not only that, it was accessible. That is, in my opinion at that time it didn't take ten years of training to be able to make a contribution. That's no longer true, but in those days you could do very simple experiments with bacterial phages and with bacteria and even with mammalian cells, and I did all of those things.

Vettel: Even mammalian?

Glaser: Oh, yes.

Vettel: Really, early in the '60s?

Glaser: Oh, yes. I mean there were certain cell lines that everybody used which you could think of as the *E. coli* of the mammalian cell business. One of them was a so called hela cell which was a human cancer cell derived from a woman whose name was said to have been Helen Lane, hence hela. I think the name is probably not correct, but anyway, that was the pseudonym. So those cells were widely used for studying human cells. Then there were several strains of animal cells. There were T3 mouse cells. There was one that we used extensively. Another one we used a lot was the Chinese hamster ovary cell, the CHO cells. And those cells had the charm that they were almost normal. You could not grow normal cells in culture in those days because they inhibited their own growth naturally. Cancer cells grew wildly, but what you wanted to study, at least what I wanted to study, was what was the mechanism by which a normal cell became a cancer cell. Many other people had the same goal. So we were all looking for cells that were normal enough that they weren't frankly cancer cells yet. And yet, not so normal that they wouldn't grow. So it was a delicate business. So when somebody discovered a line like that, it became useful to everybody.

So those were the cells that we studied, and in particular we studied xeroderma pigmentosum, which is a type of cancer of the skin in which the patient loses the ability to repair UV-induced damage to the DNA. We showed, and other people did too, that there were about seven different repair steps, seven genes, and if any one of those genes was defective it would lead to skin cancer in these patients. But if they only went out at night, they could live normal lives. It's a weird kind of disease. So it was very strictly related to UV radiation from the sun or anything else.

Vettel: This is real interesting because you are going into a field, from my understanding of molecular biology in the late '50s, early '60s, where you are thinking about this mechanism in which a normal cell becomes cancerous. That is not the general trend taken by molecular biologists at this time. It was more theoretical then; it was a purer field. The question of cancer or tumor cells really comes into play a little bit later. So I was wondering, why would you ask this question before most of the field?

Glaser: Again, my sense of history isn't very good, but I don't think I was all alone. I think everybody was interested in that issue, or at least a few people. And of course, it was a mammalian cell issue, and most of molecular biology, all of molecular biology, began as a microbial field. Yeast, *E. coli*, simple cells.

Vettel: Purer questions.

Glaser: That's right. And indeed, the first experiment I ever did had to do with the loss of synchrony in the growth of bacteria. You start with one bacterium, then you have two, then you have four, and so on, but the division time of each individual cell is not accurate. So gradually they get out of step and I wondered, first, how long does it take for them to get out of step, and what's the mechanism. I thought it would be easy. Well, it wasn't easy. It took some little trickery to do the experiment, but I did the experiment. I can't remember the answer very well, but it was by four or five or six generations, they were pretty well out of step. And then you get the characteristic exponential growth, and before then you get stepwise growth.

Vettel: Was it just randomness?

Glaser: Well, that's the question. What is it? And so it has to be some element of randomness, and then you have to ask, "What's random?" And I decided--it was sort of a wild guess--that DNA polymerase is a very large, complicated molecule and that the cell would not want to have an enormous number of these molecules around. Therefore the number might be smallish, and therefore the chances that the DNA polymerase would make a hit on the receptive side, on the DNA where it could start duplicating, that wasn't 100 percent. So maybe there was a fluctuation in the roulette wheel that started the process. I can't remember now, but I made an estimate that was something like eight or ten molecules, but I didn't have the nerve to publish it so I never did, but then it turns out that it was true.

Vettel: The more frequent the event, the greater the number, the more likely you're going to get a hit, which evens out the curve.

Glaser: Which would keep it synchronous. Suppose it takes as it does, something like 20 minutes to go around the whole circular DNA molecule of *E. coli*, and if there are lots of polymerase around, then it's always 20 minutes. If there is only a few, this thing is waiting for two or three minutes before it gets a hit that starts it. So it's like a bunch of people with stopwatches, and they're all measuring the same event, but they start at different times, so they get out of step.

Vettel: Going back, I'm really curious about this, I apologize. The question of going into, say the research in tumor cells, in the early '60s, from my understanding, it's somewhat unusual. Arthur Kornberg at Stanford would not allow it, or at least made it clear he didn't want his lab to do it. Not at that time.

Glaser: Interesting.

Vettel: In fact, in '68, I think it was, Paul Berg told Kornberg, "I want to go into tumor cell research," and Kornberg said, "You're going to be the Pied Piper leading everyone astray." So that was about '68. In that late '50s, at Berkeley, [George] Gamow was trying to get programs and people and individuals going after the code, but not tumor cells and [Wendell] Stanley wanted viruses.

Glaser: Gamow wasn't at Berkeley then.

Vettel: He was, and then he left for GW probably in the early or mid '60s. But still the question of the code was on everyone's mind.

Glaser: Sure.

Vettel: UCSF, they wouldn't hire [William J.] Rutter until the late '60s because he wouldn't study the prokaryotes like the faculty wanted. So the question I'm wondering is, does your experimental background in physics, and you're willingness to get into how things work lead you into a particular question within molecular biology that others aren't necessarily going into?

Glaser: You know, there might be a much simpler explanation—I might be wrong about the dates. You know I would have to look in my notebooks because I don't remember. But I don't think that I was in any sense way out front as a pioneer because I didn't know enough to do that. But by the same token, I didn't know enough to know that I shouldn't be doing it either.

Vettel: Which is a lot like the way you created your bubble chamber.

Glaser: No, there I knew a lot.

Vettel: But you said you were so young, you didn't know about Fermi's articles, and you said you didn't know that you shouldn't be able to do it.

Glaser: I wasn't aware of Fermi's textbook of thermodynamics.

Vettel: Let me ask a few more questions then. How did you pick up speed in the biological sciences? What were you reading? Who were you studying with?

Glaser: Well, I spent a semester at MIT as a visiting professor. An old friend of mine, Cy Levinthal was a very active molecular biologist there, and we had been officemates when we were both young assistant professors at Ann Arbor, so we were good friends. So he invited me to come, and that's when I began to learn by going to seminars and talking to him and to Alex Rich and to a number of other people in the field. And then I spent a semester in Copenhagen with Ole Maaloe, a very prominent Danish molecular biologist. So I watched what people were doing and read what they told me to read and asked a lot of questions. I learned sort of by being in places which were very active and then I started doing experiments. One of the first experiments I did was suggested to me by Sidney Brenner, and he thought it would be a nice experiment for me to do. So I relied on friends and colleagues to tell me what to do. It would have been very hard to just go to a library and start.

Vettel: Standing on the shoulder of giants.

Glaser: That's really what it was.

Vettel: Do you have any idea when you spent your semester at say, MIT?

Glaser: It was very early '60s.

Vettel: So you had already come to Berkeley. Was there anything else about molecular biology that attracted you to it? Other than, your desire to find a field that is wide open, that offers freedom to explore.

Glaser: Well, I was looking for something that I found intellectually interesting, and I thought was accessible, and I thought I might be able to contribute to it because I had no idea how complicated it was going to get, but it was clearly going to get complicated. At that stage it was something that I could contribute to, because many of my friends who were also contributing were also physicists, Seymour Benzer and Cy Levinthal and Delbruck, many of them.

Vettel: This is really at the end of the era when the physicists are doing research with phage.

Glaser: The physicists were really leading the charge, and I could see what they were doing, and I could understand it without extensive background.

Vettel: Did you envision yourself as being in molecular biology much like Delbruck?

Glaser: Yes, in the sense that I would think hard and then I would try to do simple experiments.

Vettel: Was Alvarez or anyone in the physics department disappointed that you were testing the waters in molecular biology?

Glaser: I don't know, but when I came to Berkeley, I warned them that I was getting fed up with high energy physics and they couldn't count on me to stay in that field and that I was very interested in biology. So I told them that and that didn't seem to slow them down.

Vettel: And did you work with Wendell Stanley in the biosciences at Berkeley?

Glaser: He was the director of Stanley Hall, the lab I was in physically, but he had worked on the infantile paralysis virus, and he got his Nobel Prize for crystallizing tobacco mosaic virus, TMV, as you know. But we never worked together. I hardly knew him. He was a presence, and we had a cordial relationship, but not scientific.

Vettel: Now when Stanley was running the BVL in much of the '50s, there was no real formal molecular biology program. So when you introduced the idea of going into molecular biology to the Berkeley administration, how did that process work? What happened?

Glaser: Well, Günther Stent was an ally, and it bombed. I mean, it didn't exist. And then pretty soon other universities began to have molecular biology departments. Again, my sense of the history isn't reliable, but generally speaking I think that we agreed that that was a new field, and that we should change the name of the virus lab to the molecular biology department, which is what ultimately happened.

Vettel: And Stanley supported that? He was in support? I read in your papers that he said, "This sounds like a good idea."

Glaser: I don't remember that he was against it, I just don't remember discussing it. But the same thing happened with neurobiology a few years later, Günther [Stent] and I and Jerry Westheimer said, "You know, it's now time for us to have a neurobiology group." But Berkeley has been very, I don't know if it's general, but my experience is that Berkeley has been very conservative about starting new things, and one reason for that may have been that biology was enormously fragmented. I can't remember, but I think there were 15 or 12 departments of biology, and one of them had only one person in it. I may have mentioned that before. Because he was apparently a good scientist, but nobody could get along with him. So that's the solution. And then, only a few years ago, thanks in large part to Dan Koshland's effort and others, biology was reorganized into a couple of mega departments. So biochemistry and genetics and so on were poured into one, which was sort of the molecular cellular level. And it's called the Department of Molecular and Cell Biology with five divisions of which Neurobiology is one, and that's the one I'm in. Then there's another one, I'm not sure what it's called; I think it's called Organismal Biology, and that deals with evolution and whole animals and ecology and things like that.

Vettel: Focus on natural sciences.

Glaser: Yes, on the naturalist side of it.

Vettel: Now, do you believe that a critical reason why molecular biology bombed was because of the fragmented nature of the departments in biology? Was that unique to Berkeley?

Glaser: Again, I'm not involved with campus politics, so I don't know what was really going on, but all I know is that we proposed this and it did not happen. And the person who really knows is either Dan Koshland or Günther Stent because they are much more involved and have very good memories of what happened, particularly Günther, but both of them. And if you want to pursue that as a separate issue, I'm not the guy to know exactly who said what to whom about what, or who had the authority.

Vettel: I'm just trying to get a sense, because I've read in the archival papers about the process. You proposed it, and Clark Kerr said that you could bring Arn Engstrom and four other biophysicists at the assistant level. Once that got out, I saw in the papers an amazing evolution of who wanted to be in the program: two people in agricultural chemistry, one person in the bioorganic lab, one person in botany, one in bacteriology and immunology, cell physiology, nutritional science, plant pathology, soil and plant nutrition, all writing letters to be part of this molecular biology program. The list is phenomenal.

Glaser: I had no idea. I didn't know anything about that.

Vettel: So I was wondering what it was like to leave physics, because it was overwhelming in terms of this team process. And then you confronted an incredibly fragmented situation in molecular biology at Berkeley?

Glaser: No, I wasn't frustrated. I mean I was just commenting that that was the situation. I didn't finish the idea, but that led to the idea of competitiveness among the departments for resources, meaning space and FTEs. I think that the administration was delighted with the idea of forming two mega departments because then the debates would happen within the department, and then the administration would get a proposal from the whole department, not from the subdivisions. So it's a principle of administration that you don't want to have too many reports. And that made good sense because for the administration not being experts in the subtleties of the professions to be asked to make priority decisions was not reasonable. So it was much more efficient and likely correct to have these two mega departments, and that's what physics is too. Physics is a mega department of four or five major fields.

Vettel: You must have been somewhat frustrated though.

Glaser: Well, I didn't care that much. I just thought that it would be nice if we had a molecular biology department so that we could attract people who were interested in this cutting- edge field and so that we could offer students a coherent training program to be in this field and so that I would have other people to talk with. And that was my motive, but I could live without those things and did. So I was willing to say, "Hey, it's time for us to do this," but I'm not a real activist. I didn't go campaigning for it, actually.

Vettel: But you desired to be in it and be around people who could do it and talk about it, and you felt strongly enough about it that you entertained the possibility of joining MIT. Berkeley administration felt threatened that they were going to lose you. That I know.

Glaser: Well, I was at one point an institute professor.

Vettel: Oh, at MIT.

Glaser: Yes, and I really didn't want to leave, but yes, I had such an offer. I didn't solicit it. It came out of the blue, and I didn't use it as a bargaining chip because I really didn't want to leave. But in retrospect maybe we would have had molecular biology faster. But you know, I'm not a political type and I don't have much clout with the administration as far as what they want to do. Anyway, I just suggest things that I think are important and then hope somebody grabs the ball.

[End Tape 6, Side A] ##
[Begin Tape 6, Side B]

Vettel: So then you were simply presenting an idea. It was obviously a good idea. I mean everybody else follows it, and Berkeley tries to start a molecular biology program a number of times after that. But this particular instance, it bombed. What did you do? What happened next? I'm just curious.

Glaser: I don't remember that I did anything. I mean I had my own research program, and I continued to do it. I had a lab with some post-docs.

Vettel: And what was the project? You were working in the BVL?

Glaser: What's BVL?

Vettel: The Biochemistry and Virus Laboratory run by Stanley.

Glaser: Well, at that time it was called the Virus Lab. Well, we worked on—The first problem was the one I mentioned that I never published on the synchrony in bacteria and why they get out of sync. And I invented a gadget called the baby machine that was part of this experiment. I didn't publish that either. It was an incredibly simple thing. The idea was that I grew bacteria, sticking to a glass plate, and I let water drip over the plate so that every now and then one drop would fall off, and the only bacteria that could get loose from this plate were daughters because the mother was stuck. And when the cell divided in two, there was a chance that one of the daughters was sticking out into the flow. So the only things that came into the beaker below were babies. So that was a baby machine. So that's how I started, with bacteria that were guaranteed to be newborn and in sync more or less. So then I could watch them dividing and gradually getting out of sync. You know unbelievably simple experiment. Well, it turns out historically I had the right answer, but I didn't have any confidence at that time, and I didn't do enough controls. But anyway that's one thing.

Then later on, we began to work with the mammalian cells, but at about that time I was beginning to get impatient with the standard methods of molecular biology. There was a tremendous amount of scud work having to do with pouring out your plates and spreading culture and counting colonies and all of that. So I designed a machine for automating all of this, and it's a gadget that I call the dumbwaiter. And I got a substantial amount of money from the NIH because I said, "You know, this machine, my motive is for being able to do larger scale statistics on the properties of microorganisms and mammalian cells, but it would be an enormously valuable diagnostic tool medically. So they bought that, and so we built a prototype, and in fact I built a little building which is gone now, but it was right behind Stanley Hall. We called it the Cyclops Building, but they called it Stanley Hall Annex, and it was a super clean-room facility in which we could grow cells on open trays. The dumbwaiter itself was meant to accommodate 10^8 colonies on trays which were a meter square.

So you pour a one-meter-squared tray of agar, and then they were stacked up, and there were two stacks. So the trays went across here and then they went over here and then they went down the other stack. I can't remember which was which, but on the top there were cameras. So you took time-lapse photographs, and then on the bottom there were a series of what you could think of as eye droppers or pipettes that could administer chemicals ranging form amino acids to penicillin to vitamin B, to whatever. So what you could do is keep a dossier on each one of 10^8 colonies while you exposed it to some program of treatment. Treatment could have been light illumination and so on. It's a long story. There were a lot of inventions. There was a mechanical hand that could pick up colonies. I think it had 100 fingers, and in each finger was a little, tiny quartz rod that could stab a colony. So you could pick them up wherever they were, and then you could lay them down on a regular ten by ten array somewhere else. So it was a very efficient way of picking the ones you were interested in. People had done that before by hand, but this was all automated.

Vettel: This just increased the power of the experimental process by a factor of, who knows how much?

Glaser: An enormous amount. And then there was another gadget. You know how inkjet printers work, vibrating knobs and little rods. Well, long before the inkjet printer had been invented, I knew about this idea. It wasn't my idea: you could generate drops. So I had bacteria in suspension, and I adjusted the concentrations. On the average, each drop had a single bacterium, and the point was that I wanted to have colonies that were guaranteed to be descended from a single cell. So it was a clone, and I never wanted to have two or three, and I didn't want to have any empty drops because then I'd waste a lot of agar. So I used a vibrating nozzle for laying out the colonies. So then I put a laser beam that shone a beam of light on each droplet as it formed, and if there was no bacterium there, the light wasn't scattered. So I put an electric charge, and I threw that drop away, and if there were two of them, I threw it away. So I could generate a stream of droplets that were guaranteed to have one and only one bacterium. So I could end up with a sheet of colonies a few millimeters apart, each one guaranteed to be a clone. So that thing worked all of the time. So we had beautiful arrays. And the fact that we threw away the empties gained us about 1/e, something like 30% or so, and increased the size of our machine effectively by quite a bit. So with that we found zillions of mutants, way more than we could analyze. So I sent them to labs all over the world where they were still a couple of years ago being used. Then we did the same thing for the Chinese hamster ovary [cells], and that's how we picked up these seven genes that corresponded to the repair system.

Vettel: I mean this is a huge jump from your baby machine, which sounds like your first little experimental tool that you created in molecular biology. And then the dumbwaiter, the Cyclops—

Glaser: It had a single camera, so we named it the Cyclops Building, but it's the same thing.

Vettel: And then I've seen reference to Candid Camera, the Lazy Suzan, the Colony Picker. I mean these are all—

Glaser: Lazy Suzan was a prototype. It's complicated to make something. It's expensive. The thing was much bigger than this room.

Vettel: By the way, that's very big.

Glaser: So you want to make sure it's going to work. So I made a Lazy Susan. It was just a rotating thing, and I can't remember, but it must have had 40 or 50 trays on it, dishes. So we used conventional dishes, and then the Cyclops had one camera.

So we tested this idea of keeping the dossier, and that worked very well because we would have a growth curve for each colony. And in fact, then we did morphological studies, so we could recognize a colony. Essentially we had a scanner, which is now a very common gadget. But this scanner was a light beam that scanned the colony, so therefore we could get a picture of the colony's shape, and then we did a Fourier analysis on it. So we could describe each colony by something like eight or ten parameters. So it wasn't just grow/no grow or just how fast you grow, but what's the morphology, what's the color, and so on and so on.

Vettel: At incremental stages as it's happening.

Glaser: That's right.

Vettel: The process of development of these machines, the degree of complexity of the moving parts, how did you get from the baby machine to these?

Glaser: Well, I knew how to do things like that. I had a big group. There was a lot of money. I think there were 45 of us, and several of them were hotshot engineers, and we had one full time machinist and a number of technicians, and I guess the 45 included the biologists. But that's how I work. I start with an idea, and it's probably wrong, so I start with some unbelievably simple test of it, and if that works, then I go to the next—well, everybody, that's how you do things. That's how development goes. You don't start building a 747 until you've flown a kite. So it was like that, but then also in the high energy physics I had learned how to use large, professional machine shops to build big stuff. So I knew how to big build stuff from that. But I knew how to build small stuff from the machine stuff in Ann Arbor.

Vettel: Speaking of the relationship between physics and molecular biology and the tools and techniques and skills, it sounds like there's another step to this process in how you approached physics and how you approached molecular biology. The previous step where you describe it, it sounds like you get into the field, you get a sense of what people are doing and how they do it, and almost pick out the bottleneck in the experimental process and figure out, "I can make this better."

Glaser: Yes, I mean the first thing of course is to know what the science is, and then once you find the goal, then to say, "Well, how can I contribute to this goal?" And if you can see that one way to contribute is to speed the rate of doing it, then you'll have a lot more materials to choose from when you decide which ones to pursue in detail. So that's true. There's a very close parallel between the bubble chamber automation stuff and the dumbwaiter stuff.

Vettel: Microbial screeners, all that, the whole range of—.

Glaser: All that stuff, yes.

Vettel: Wow. That’s fascinating.

Glaser: And you know, the detailed inventions and gadgets that I built for biology, and now you can do it with the ones from physics. But the idea that you have a computer-controlled massive ability to produce lots of data and then analyze it. And we had a PDP10 computer, which was the biggest computer that Digital Equipment Corporation built in those days. And it required a full-time professional computer engineer to run it and maintain it and so on. And it had probably 1/10 the power of my Mac laptop.

Vettel: And the big thing ran hot.

Glaser: The whole room. I have enormous respect for it. I couldn’t do anything with it, but I had a very good guy who was very happy keeping it going.

Vettel: What was the obstacle to building these machines? Was it operational? Was it contamination?

Glaser: Well, there were scientific things, but ultimately the real obstacle was that the NIH wasn’t in the business of doing big biology, so they cut it off.

Vettel: That was the late ‘60s. I noticed that.

Glaser: And they were crazy. The thing was working well. It was generating mutants, but there was enormous resentment. Once I gave a talk for a group, and some guy in the back row started yelling at me, “Where do you get off destroying our field?” and so on. He was one guy. But to my friends in the field, I said, “You don’t really like this automation. Why not? It’s moronic work you’re doing much of the time.” Yes, but that’s when they get some peace and quiet, and that’s where they do their thinking, and they can spread the agar and they’re thinking about the experiment. It’s a craft. And many of them who were really friends, they weren’t angry at me at all; they could see the handwriting on the wall—this was going to happen. But there was the passing of an era. It’s sort of like going from a skilled woodcarver who generates artistic things to something that stamps out copies of whatever sculpture you want to sell.

Vettel: Kind of a mini-revolution within a period that one may call a revolutionary period.

Glaser: Right. And it’s interesting that when they [NIH] cut me off, nobody took it up until the last few years, and then apparently there is a very similar project right now at the LBL. They haven’t asked my advice, which I think is weird. And they reinvented the hand, for example. Now it may have been that one of the engineers up there was the guy who designed the details of the hand in the first place. And they’re doing it for all kinds of reasons. You know, I was not concerned with bioterrorism, but I certainly was concerned with automated diagnosis of infection, that the urine is usually sterile. If you have a bladder infection, it's one kind of organism dominating all the rest. The only game is, which is it, and it's essential to know which drug to use. So this gadget could do that. And now it's used for bioterrorism, I don't know. I hadn't thought about all of this until just this moment, but I guess I should volunteer to give some advice about how they could use this kind of technology.

Vettel: It would speed up the process.

Glaser: Well, it would allow a level of examining large areas or large samples or whatever particularly quickly, many in parallel, and they could know exactly how many they are, what's the level, and so on. And of course, there are all kinds of serological methods which are much cheaper, which wouldn't require a big machine. So I'm not sure that it's still a good idea.

Vettel: So the hardcore molecular biologists, the purists who work at a slower pace, alone, in small groups, they probably preferred the classical environment and admired and respected that kind of working environment. They were the ones who responded with hostility to you?

Glaser: No, not all of them.

Vettel: Not all, but I mean enough where you sensed it.

Glaser: But they were uncomfortable with it, and there was this one guy. I was really shocked because I had never encountered anybody like this.

Vettel: Where was this?

Glaser: It was at a big meeting. I don't remember where.

Vettel: So you have a team of 45. I've noticed in some of the files that you have these multilevel and complex organizational charts for your Cyclops project. You have Monday meetings. I mean this is a big project, with machines the size of buildings.

Glaser: Oh, yes. Well, this robot, it was—

Vettel: This is the size of some buildings.

Glaser: Yes, it was about the size of this room.

Vettel: Had you reached a point in the field where you saw that there was value to what you were contributing, and you decided that you were okay in a large experimental climate, one that perhaps you would not have been comfortable in a decade earlier?

Glaser: Well, this was nothing like the scale of high energy physics. I mean it was 45 people. We all met in my office, the design crew anyway, and the machinists weren't part of those meetings. So we didn't meet altogether, but there were a dozen people sitting around the table that was a door placed on filing cabinets. And we made our decisions. But that's different from high energy physics in which, again, there are 500 authors.

Vettel: So you were coming from a field that's massive--high energy physics. You saw growth, but the growth was on an entirely different scale. You were completely comfortable with it, however, comfortable with the traditional cottage industry approach?

Glaser: Well, I mean some of those guys were the leaders in the field. I mean, they weren't foot soldiers. They were really the hotshot biologists, and many of them, I would say most of them, thought what I was doing was going to be useful, but they sort of regretted the good old days. And the good old days are back again. I mean this machine is not being used. But now, I don't know what they have in mind, but I think it's used in industry for doing time codes and so on.

Vettel: Sure. They are probably manufactured by Beckman or some other company.

Glaser: Oh, yes. They could easily have done this themselves, or they could copy what I did. It's pretty straightforward.

Vettel: Who were some of the people on the team that you remember? I tried to get some names to help. Leif Hansen, Peterson, Ron Baker, Beck, John Couch, and Frazer Bonell.

Glaser: Yes, Frazer Bonell, I just ran into him the other day. He was our chief programmer, and John Berkovitz was one of the engineers. In fact, it's a funny story. I hired him. I put an ad somewhere, and he applied from LA, and he had hotshot recommendations, and I interviewed him and so on, and I hired him. Then I discovered that he was the brother of Lynn, who I was about to marry. It was really incredible.

Vettel: But that's early '70s?

Glaser: Yes.

Vettel: That's funny. And you had no idea?

Glaser: I had no idea.

Vettel: What about Spielman and Calvin Ward?

Glaser: Spielman was essentially a biological technician.

Vettel: Probably essential in this program, the technician.

Glaser: Yes. He did some of the lab stuff, but also he had good ideas every now and then about photography because he was an artistic kind of guy. Hansen was a Danish engineer, Leif Hansen who, I guess, was the head of the engineering group. And Larry Johnson was another engineer, a pretty good one.

Vettel: And Ward?

Glaser: Cal Ward was an interesting character. He had his Ph.D. degree in physics, but he came to me and he wanted to do biology, a really smart guy. But after a few years he quit and became a patent attorney, and that's what he's been doing ever since.

Vettel: So in 1968, the NIH is cutting funding, slashing funding virtually everywhere. Berkeley is forced to cut back too. It was a tough time economically, and it was socially challenging too. Something happens in '68 with a group called the Berkeley Scientific Laboratory?

Glaser: Yes. It's a company that I started.

Vettel: That you started?

Glaser: Together with a friend, Bill Wattenberg.

Vettel: Moshe Alafi was involved?

Glaser: No, Alafi was involved in Cetus, but he was not involved in BSL. There was another guy who was the financial guy who put up the money. I don't remember his name. Anyway, BSL didn't last very long. But Wattenberg had been a student of mine in nuclear physics. I'm trying to remember--he might have been on the faculty in Computer Science at Berkeley. He's a very active guy. He has a talk show on the radio now all the time, and he's a major consultant for Livermore and a very interesting, very energetic guy.

Vettel: Did he come to you or did you go to him?

Glaser: He came to me. So we did a number of small automation things for biological purposes, but I don't know. I'll have to look back and read about it because I don't remember any definite, single thing, but it didn't last long.

Vettel: What was the relationship between the NIH cutting your funds for Cyclops and starting a commercial biological company like the BSL?

Glaser: None. BSL wasn't in the picture when the cutting began, as far as I know.

Vettel: Because BSL started in ’68 and lasted until ’69, and then it was acquired by Tracor or something like that.

Glaser: That’s right. I forgot that.

Vettel: And the NIH was cutting funding in ’68. Certainly you don’t start a company overnight. So you must have been thinking about it; Wattenberg must have approached you before.

Glaser: No, I think actually the NIH funding cut had more to do with starting Cetus than it did with starting BSL. Cetus was in the ‘70s. It started as a partnership. I don’t know the exact dates. So I wanted to continue the general idea of trying to use computer automation for important goals in biology. They [NIH] told me that they were going to terminate my project at that time, and I was really furious because I’d put in an enormous amount of effort, and it was just beginning to be really productive enough so that we sent mutants everywhere, and it was working very well. So when we started Cetus, one of the ideas was that discovering new antibiotics and improving the yield of known antibiotics were very important commercial and health goals. I thought that I could tackle that with computer methods, and did, and that worked. But it wasn’t anything like the technology on the campus for the main reason that on the campus I was interested in identifying mutants in order to understand the function of the organism. For that I needed lots of parameters, and to get those you had to grow the cells on agar, one always did. In industry the question is that all of these drugs are grown in huge vats, 10,000 gallons or whatever it is. So you’re interested in the behavior of organisms in submerged culture, very different [than on a solid substrate]. So the automation that I developed for Cetus had to deal with growing them in little wells and so on.

Vettel: Which would improve the yield.

Glaser: And then we picked out the ones that grew best and did the best yield as measured by the presence of the product in the soup, and we got contracts with companies. Schering-Plough had a drug called Gentamycin, which is a very important antibiotic and is dangerous. Now it’s only used in the hospital under close supervision, but it’s the drug of last resort. It saves a lot of lives, but it’s not good in a doctor’s office. Schering-Plough, I don’t know if that’s the German or the American one. So I can’t remember how this came about, but somehow we connected with them and said, “Hey, we can improve the yield of this bug in your vats,” and they were planning at that time to build a plant in Puerto Rico and another one in Ireland as I recall, and we said, “You know, if we can double the yield of your bug, you won’t have to build any plants.” And they said, “Yes, but you can’t have our drug. It’s the crown jewel. You’re going to steal it. Why should we trust you?” and so on. That was before biotech was an industry. So anyway, we had a lawyer and they bargained with them and so on. Finally they agreed on some kind of security arrangement so you don’t take home the bug under your fingernail, which has been done by some people in other contexts. So they trusted us finally and gave us their bug, and we doubled it, and they didn't have to build their plants. And again, we were very naive, so I don't know exactly, but the deal was that we would get some fraction of the savings as our fee.

The usual way of solving this problem is to have 2,000 people in China or India sitting there, looking at colonies, measuring and so on, and that's how you do strain improvement. And then we came along with an automated method that creamed it, and that was how the biotech industry started. That was the first thing that I remember that we did that really had a big effect, and then we went on to other things.

Vettel: I want to keep going. We're going to come back. I do want to ask some questions about BSL, and then I'll have an entire session on just Cetus.

[End Tape 6, Side B] ##
[Begin Tape 7, Side A]

Vettel: The BSL. I'm curious, the Berkeley Scientific Laboratory – certainly by the late '60s your Cyclops, the dumbwaiter, all the different variations of this microbial screening project – you noticed that there was enormous value in terms of what it could contribute to the experimental process.

Glaser: That was the goal, but I knew that it would also have medical applications if it were used for that too.

Vettel: Many different areas—university laboratories to hospitals.

Glaser: Right.

Vettel: Did you know this as you were developing it, and then, once it was developed, did you look for opportunity in private industry? How did you get involved in starting a partnership with Wattenberg?

Glaser: Let's see. I guess Wattenberg was a co-author on one of the papers describing the dumbwaiter system. We worked together on that one. I can't remember whether he was a student. He might have been a professor in computer science in those days, but he had been a student in my course in nuclear physics.

Vettel: At Berkeley?

Glaser: At Berkeley, yes. Then he was a professor at one of the state colleges for a while. Anyway, I don't remember that BSL had anything to do with Cetus, and it had nothing to do with the dumbwaiter stuff. I don't remember exactly what

BSL was doing. It had to do with small instruments, but I don't remember much about that.

Vettel: Because there were contracts with hospitals, or at least someone at BSL was pursuing relationships with hospitals.

Glaser: Well, I'm really embarrassed that I don't remember.

Vettel: No, that's okay.

Glaser: Anyway, if you really want to know, you should talk to Bill Wattenberg. He's around. It was a small operation in which there were little things being made. So I imagine there were small test instruments of some sort, but I just don't remember much about them.

Vettel: I know this is a completely separate event from Cetus, but do you remember why you made the transition. This is a big. If you remember when we began this oral history we talked about the jumps that you took from field to field – starting the BSL is a very big jump. You may think that it looks small, but —

Glaser: You mean starting Cetus?

Vettel: No, from academia to the BSL. Just to go from academia into BSL. Knowing the context of the late 1960s, that's a—

Glaser: Well, what happened there was that Wattenberg was the real motivating force. He had the idea he wanted to do this, and he wanted me as a consultant and finally as a partner. So my contribution was, I can't remember, but I solved some little technical thing, and I made a gadget. I can't remember. That's really awful I don't remember that. But anyway, he was the prime mover. So the fact that I got involved was that he knew me as a professor and later as a colleague and asked me if I wanted to work with him on some aspect of this. But he ran it. I didn't run anything. And it wasn't a big company. I don't remember, eight or ten employees.

Vettel: And in this particular case, your work at Berkeley with the dumbwaiter, was it unrelated to the BSL or was it an offshoot? Did the BSL use the dumbwaiter or produce the dumbwaiter—or any other part of the entire system? Was it a product? Or was this a matter of having a tool like the dumbwaiter, and then someone coming to you and saying, "Dr. Glaser, I have this problem?"

Glaser: Well, it was something like that. It had nothing to do with the dumbwaiter that I recall. The dumbwaiter really grew out of high energy physics and the automation methods there. That's where I learned how to do engineering of moderately large things.

Vettel: Your ability to innovate or improve automation--that started way back with the bubble chamber; it carried through your work on the screening system and all the different machines and tools and gadgets and techniques that you improved or developed or built. Why do you think, just speaking from historical hindsight, why then? Why did you start the BSL in the late 1960s?

Glaser: Well, I think it was money—that was the desire to be financially more independent than you could be on a professor's salary. I had a daughter that I wanted to put through medical school and did, and now she's now Assistant Chief of Pediatrics at Kaiser, and they're trying to force her to be chief, and she doesn't want to because she wants to treat kids. We were just at her house last night in Sacramento. But I think that was it. And also it was a desire to see something of science doing some good. We discussed it--I guess I'm now remembering that. I think it was the discussions with Josh Lederberg and other biologists that I knew that, "You know, we know an awful lot about DNA," even then. "We know what it is and what it does and so on. And it hasn't done anybody any good yet." So we pondered that, what could we do with that. And if it's going to do good, it has to be something on a commercial scale, and you don't do that in universities. Now, I remember this guy, Cal Ward--I mentioned him before, very bright guy--I couldn't get a job for him. So I decided there must be a lot of people trained in molecular biology that there is no place for them to go. There aren't that many jobs in universities yet. So that was a way of finding productive work for him. So there were all of these sort of--there was the money thing, plus the altruistic side.

Vettel: From the humanitarian side, to contribute—

Glaser: The idea of doing some good out of biological knowledge that so far hadn't done any good. It's obvious that it was going to happen, but this was part of that.

Vettel: And this was an era where certain segments of society are asking, "How do we make the world a better place?" and this very idealistic sense.

Glaser: But that's always a motive from some sector of the population.

Vettel: But many of the students, obviously not all, but many of the students are asking, "How do we make a contribution?" whereas say a decade earlier it's, "What more can we know?" A more theoretical approach.

Glaser: Oh, I see.

Vettel: Were the antiwar, peace movements, poverty programs, medical care—this is a time when the [microbial] screeners could be used in a hospital. Why would you try to place your work in hospitals when there were so many other sectors that needed microbial automation? This is the '60s—this is a curious time in science and for science.

Glaser: I hadn't thought much about the general trend of this kind. I guess my view always has been very simple. That is, within our society, which is enormously diverse, there are people of diverse goals. I see a very unpleasant trend towards extreme materialism and particularly the lack of, I hesitate to use "spiritual," but humanistic goals or even goals devoted to the general welfare. To me it's a terrible thing that we're fighting all the time over things like our own careers, which amounts to getting re-elected, which amounts to the civic interests of a politician's constituents, and no one seems to have any concern for the general welfare. That I see is a very dangerous trend. I think that that is similar to the trend you're referring to, that there aren't too many people who are concerned about the general welfare in some way that is not selfish to their own situation, and I think that's been a trend over a long time. Maybe it's fluctuated, but I think that's one of the real weaknesses of our society. I had an example in mind which really horrified me, but almost everything the Bush administration does is like that.

Vettel: The politics of self-interest.

Glaser: Self-interest, which turns out to mean the interests of the donors. We're going to improve the mercury level in 29 years, but there's a scheme to do it in 7 years. We're not going to do anything about the automobile efficiency at all and so on and so on. They have children too, so the idea that they don't care about the deleterious things that they're doing to the environment or that that has lower priority than some short-term political advantage I find really terrifying. But Bush looks good to some people. So it's obvious that people aren't thinking, so then you come down to education.

Vettel: Why don't we stop.

[End Tape 7, Side A]

Interview 5, February 10, 2004

[Begin Tape 8, Side A]

Vettel: Again, thank you, Dr. Glaser. We're nearing the end. It's February 10, 2004. It's been a couple of months. We ended with the Berkeley Scientific Lab. That was the conclusion of our last conversation.

Glaser: When was our last session?

Vettel: December 16th. We got a little bit into the Berkeley Scientific Lab.

Glaser: That was a little company that I started with a friend that didn't last very long.

Vettel: Was it with your friend Alafi?

Glaser: No. That was with Bill Wattenberg.

Vettel: It says he was a co-author with you on a paper. Is that how you—?

Glaser: He was a student of mine in a course, I guess in nuclear physics. He was a graduate student in electrical engineering. We had some ideas for automating certain diagnostic procedures in medicine, so we were working on that and related topics.

Vettel: The primary objective with the screener, or was it a scanner for microbiology?

Glaser: I would have to look that up. But that was the theme of what I was doing on the campus, so I don't think that's what this was. I would have to check it. I really don't remember exactly.

Vettel: And then Berkeley Scientific Lab, was it primarily consulting?

Glaser: No. It was a place. It had facilities and half a dozen employees and was making some circuits by hand for that use. A very small operation.

Vettel: Making circuits for?

Glaser: Involved in medical diagnosis instrumentation.

Vettel: Do you know who some of your clients were?

Glaser: No. I didn't have anything to do with the business side of it.

Vettel: That was Wattenberg?

Glaser: Wattenberg, right. And now he's had a very varied career. He's a very inventive engineer, and he is in the newspaper almost every year for one reason or another. When BART [Bay Area Rapid Transit] first began to operate, he showed that you could trivially counterfeit their tickets. And at a later stage, he showed that the military could build bridges very quickly out of retired railroad cars which were strong. He's a very clever guy.

Vettel: Very inventive.

Glaser: Very inventive. And he's been a talk show host for years and years now. Anyway, let's get on with the main thing.

Vettel: Okay. That's all right. Just out of curiosity, and I should have asked this before, but if you were to say the percentage of professional time that you spent at Berkeley versus the Berkeley Scientific Lab—?

Glaser: Very little. I spent more time with Cetus.

Vettel: Did Wattenberg come to you, or did you go to him? Or you went to him with an idea, but said, "I don't want to run this"?

Glaser: I don't remember. We knew each other. I can't remember who initiated the conversation.

Vettel: Actually, Berkeley Scientific Lab in 1968-1969, roughly around there.

Glaser: I would have to check the records. As I told you before, I'm not very good at remembering dates.

Vettel: That's all right. But I'm trying to set the context between the Berkeley Scientific Lab and Cetus.

Glaser: That had a very short duration in my life. I think Wattenberg went on with it a little bit after I decided not to work with it anymore. There was no overlap in time that I remember. That was finished before Cetus began.

Vettel: What was happening between that time?

Glaser: I was a professor at the University of California at Berkeley. I was doing my campus research and I wasn't involved with any commercial activity.

Vettel: So Cetus. If you can take us back, Ron Cape, Farley, and yourself were the three founders.

Glaser: Right, and Moshe Alafi.

Vettel: And Moshe Alafi. How does this group of four individuals come together? Who approached whom?

Glaser: Cape was, I guess, a post doc in Günther Stent's lab in Stanley Hall, where I had my office. We met there casually. He and Farley were friends and colleagues. They had a venture capital consulting activity. That is, both of them had MBAs, as I recall. Farley had an MD, and Cape a Ph.D. in, I think, biochemistry. They were using their combined scientific and business experience to advise venture capitalists, and they looked over business plans. I don't know a lot about it, but that's roughly what I think they were doing.

I don't know exactly how they got to know Alafi. But then the three of them approached me to join them, as I recall. Alafi was an experienced venture capitalist, and continues to be, as far as I know. I met him at a home concert somewhere. We had a friend in common who was a reasonably good musician, but he had a lot of money so he hired professional musicians from the symphony to join him. They put on quite nice home concerts now and then. I don't remember any of the names. Anyway, I met Moshe one night at one of those concerts.

Vettel: And Moshe was a neighbor of yours? Is that right?

Glaser: No. That's the only context in which I met him. Then he approached me. I can't remember whether it was that evening. But anyway, we knew each other. So then he contacted me about that time to join the three of them. That's how it started.

Vettel: So Cape and Farley talked to you about their consulting work with venture capitalists? And at the same time, Moshe Alafi came to you from a slightly different angle, having spoken with those two?

Glaser: Yes, but I can't, again, be sure of the sequence of events.

Vettel: That's all right. If anybody's going to be using these oral histories, it's their responsibility to get the dates.

Glaser: I don't know about that.

Vettel: I'm not worried about that. So Cape is a post doc. I'm a post doc. It would be very awkward for me to go to a professor, especially of your stature, to be quite honest, and say, "Hey, I've got this great business idea. You want to join me?"

Glaser: It may have been Moshe that invited—I can't remember—that invited me to talk with them. I really don't remember. But no, in Stanley Hall, which was then known as the Virus Lab, the relationship between the professors and the post docs was quite informal. It wouldn't have been at all unreasonable for that to happen.

Vettel: If Cape and Farley's entrepreneurial energy was directed towards biology and medicine, and they approached you, how did they sell it? Did they say, "We know about your scanner. We'd like to take your scanner, use it, run it, sell it, make it —?"

Glaser: No, I don't remember that it was anything like that specific. If it had been, I would have declined because at that time especially, but even now, I think it's improper for an academically financed and executed research program involving students and the university to make money for somebody without some sort of a license. I wouldn't have done that. But the general idea that I proposed after they discussed the thing was that we could use modern methods of automation and computation to look at commercially important projects in molecular biology.

The technology that I had going on the campus, the so-called "dumbwaiter," as I called it, had to do with automating pattern recognition of the morphology of colonies growing on agar—so growing on a solid surface, or in some cases, growing on plastic directly. In industry, that is not the thing that's useful. That would be useful in medical diagnosis, looking for pathogens and so on. And indeed, I wrote a lot of papers on applications of that in the academic environment, none of which was patented. But in industry, you're looking for optimizing an organism to be the most productive in huge vats—tens to hundreds of thousands of gallons. The bug that does well in a liquid culture is very different from one that does well on a flat surface. The technique for automation, therefore, has to be enormously different.

What I did when we started the company was to design a system that used what, at that time, was a modern computer technology and marry it to various little inventions that we did at the company. We were in the black almost immediately because our first project was to improve the performance of organisms that were already commercially successful. I guess it was Schering-Plough, which is the American company, or Schering, which is the German parent. They had a drug called Gentamicin, which is a very, very effective antibiotic, but it's very dangerous. It's never given to a patient to take home. But if the patient is in the hospital and is seriously ill and nothing else works, then Gentamicin is a very, very valuable drug, and in the hospital only. The level of the drug in the patient's blood has to be monitored carefully, because too much and it's very dangerous, not enough and it's ineffective.

The demand for it was great, so Schering-Plough came to us, and I don't remember how we found each other, and said, "Could you improve the yield of this bug?" I suspect that we went to them because they were very paranoid. They wouldn't let us have their bug. That was standard in the industry. A prized bug that produced some valuable product in commercial conditions represented a big investment and a very valuable asset. Finally, we persuaded them with all kinds of confidentiality agreements. They were about to build a plant, I think in Puerto Rico, and another one in Ireland, to produce more of this stuff. We told them that we could easily double the yield of the bug they had.

Then we made some deal with them. I don't remember what it was. We didn't know how much our service was worth. Nobody ever had a business like this. We made a contract with them to be rewarded as some percent of their savings. I don't remember the percent. I think it was not a contract that was very much to our advantage. We were sort of had, I think, particularly since, as I recall, they were much shrewder than we were, because we were so naïve in saying that we would get a percentage of the profits. It's the same thing in the movie industry. Do you get a percent of the gross or do you get a percent of the profit? And the profit can be much manipulated by the accountants, so you never know quite— Anyway, that was naiveté.

Whatever it was, we did it. The result was that they did not build a plant in Puerto Rico or in Ireland. They didn't need to because their existing plants suddenly became twice as productive. I may not have the factor of two exactly accurate, but it was a significant improvement. Of course, they didn't think it could be done because they had tried their best in-house. They didn't really believe we could do it, but once they were convinced that we wouldn't steal their bug, then they let us try.

Vettel: Do you know who negotiated that contract with Schering-Plough?

Glaser: It was either Ron or Peter or the two of them. I don't think we had a lawyer on our staff at that time. We may have done. We had later, but I don't think we did then.

Vettel: How would it work? You're building the scanner for the screener, trying to improve the yield. You're dealing with large vats?

Glaser: It wasn't a scanner. It was a totally different technology.

Vettel: What was the technology, and then how did it work?

Glaser: It was really cute. I don't know if it's patented or whether it's an industrial secret or whatever it is. What we had was what's common now, but it wasn't at that time, which is plastic plates with a lot of little holes. Each bug was growing in a little tiny vat of its own. Then we had ways of assaying how much drug was produced in each vat. So it wasn't a scanning procedure.

Vettel: And then you would identify which one was the greatest yield?

Glaser: Then we would take that one, and then we'd mutate it, and go to the next one and see which one—You know, just like old-fashioned breeding crops. You mutate or wait for spontaneous mutations and select the product that does the best. So in a sense, this was genetic engineering. It was old-fashioned, mutate and hunt—engineering as a farmer might. Later on, we began to use more formal genetic engineering procedures. We were the first to discover and isolate the hepatitis C virus. Before that, the disease was known as non-A, non-B hepatitis. That was a major scientific achievement and led to patents, which led to the ability to assay the blood supply. When Chiron bought Cetus, that was one of the very valuable patents they got. Chiron now is the main contractor to the Red Cross and all the other blood banks because of the Hep C patent.

Vettel: Who identified or advanced that patent? Which scientist?

Glaser: I don't know.

Vettel: You said that Cetus was pretty much, roughly, in the black from early on.

Glaser: It wasn't in the black continuously, but it got in the black almost immediately. Then I don't know what the—

Vettel: Was that because you were offering consulting services, but also improving yields, industrial contracts?

Glaser: That's right.

Vettel: With alcohol beverage companies too.

Glaser: That's the one I remember. One of our first financial investors was National Distillers, who are responsible for Old Crow and all those, you know, Old Joe or whatever, a lot of alcohols. But their real money was in vinyl.

Vettel: In vinyl?

Glaser: Yeah. They made the raw stock from which polyvinyl chloride plastics are made. That was, I think, their biggest product. But anyway, it was a big company.

Vettel: Any industry that's in bugs would have to be interested in knowing what you can offer, because you're the only ones!

Glaser: Oh, yeah. But up until that time, it was an extremely labor-intensive thing. People just did all this stuff by hand, so there were sweat shops in China, I guess, and generally in the Far East, where labor was very cheap. That's where this work was done. Essentially, we were competing against the cost of doing this job there and the security issue of losing your bug. That was a big advantage.

Vettel: And you were the one that created this?

Glaser: I devised the system for doing it, but not alone. We talked about it.

Vettel: So you devised the system, but it was not the scanner?

Glaser: No.

Vettel: The innovation of this system—what did you call it, by the way?

Glaser: I don't think we ever gave it a name.

Vettel: Not Cyclops?

Glaser: No. Cyclops was the name we gave to work done on cancer cells and so on—quite different, nothing to do with this. The scanner was a campus thing, but I didn't invent the scanner. We built our own. In those days, the computers were so complicated that you couldn't run your own computer, so I had a full-time guy who ran the computer. We had the biggest computer that DEC offered commercially, which is much less than my little Mac now.

Vettel: And that was under the scanner?

Glaser: That drove the scanner.

Vettel: Who was that, by the way? Do you know who was the person?

Glaser: Bob Henry was the technical guy.

Vettel: The system that you developed for Cetus, had you worked on it prior to when Cape and Farley came to you?

Glaser: No, because I wasn't interested in fermentation in vats. My campus work had nothing to do with that. I was so naive, I didn't really realize that we were optimizing for quite different circumstances, and therefore—

Vettel: The vat, you mean?

Glaser: Yes, and liquid cultures. Submerged cultures, the way they call it.

Vettel: But innovating this system, Cetus— You worked on the campus with the scanner, with cancer tumors, the Cyclops, Berkeley Scientific Laboratory momentarily, just a brief moment in time. Then Cape and Farley come to you, and you say, “Industry hasn’t figured out how to automate. I think you can make this better.” You said, “I can do this. I don’t know a whole lot about fermentation, but I think I can do this better.” And then you did it?

Glaser: Yeah. But I had done that before, in physics. The bubble chamber was a gadget which increased enormously the rate at which we could gain information. The question is, how do you analyze it? I did a lot of engineering, designed pretty fancy equipment. The bubble chamber started this. I didn’t design the biggest ones; those had big teams of engineers. But I got up to the sort of minivan-size machines, or maybe full-size van-size machines. Then I quit professional engineering. I also designed systems for automating collecting the data. I’m not a programmer, so I had to have programmers. I had to design the logistics of the system and what, where, and how it worked and so on.

Vettel: This is remarkable. How long did it take you to create this new system to improve the yields of bugs?

Glaser: Again, I’m not so good on dates, but it wasn’t more than a year or two. It wasn’t a huge effort. Because Cetus was already incorporated in ’71 or ’72, something like that. This didn’t begin until—well, I’m not sure. It was a few years.

Vettel: But it was right after—maybe not a few months--but not many years after the incorporation of Cetus?

Glaser: Before incorporation. It was a partnership at first, and then incorporation came later. Then there was a limited partnership. There were various stages.

Vettel: And then your system is in play?

Glaser: Right.

Vettel: Before your system is in play, Cetus is in the black—how?

Glaser: No, it’s in the black because the system started working very quickly at a semi-manual level. You don’t do it all at once. Then gradually you got better and better and better, and that means an increase in capacity. But I’m embarrassed that I didn’t keep a diary, so I don’t have an accurate timeline. I generally don’t spend much time thinking about history. It’s a shame, but I just don’t. I don’t keep a diary. I don’t make historical rehearsals in my mind.

Vettel: Your formal role at Cetus was to create the system and then improve upon it, see ways to improve upon it.

Glaser: Sure.

Vettel: Did you have any other role? You did fundraising.

Glaser: Officially, I was chairman of the Science Advisory Board because pretty soon we got into real molecular biology, which was beyond my competence. I was teaching molecular biology, so I understood the book descriptions of things, but I didn't have any really hands-on lab experience in biology. Well, I had. I knew about making petri dishes and pouring agar. I could do some things, but the level of sophistication of real molecular biology I hadn't had any experience in. So my job, really, was to interview and hire real molecular biologists who knew how to do genetic engineering as it evolved. I had nothing at all to do with the identification of the hep C virus or of the invention of PCR [Polymerase chain reaction], which is a major thing.

Vettel: But that's much later. A decade in some cases.

Glaser: Much later.

Vettel: If your system is improving the yields of bugs even marginally, companies are going to be interested. The venture into molecular biology comes a bit later, then.

Glaser: Yes, because molecular biology was just being invented at that time.

Vettel: But you're in the black with this system in play.

Glaser: Right.

Vettel: Why branch out?

Glaser: [laughs] Because it was a limited amount of black. It was an okay business, but it was just, you know, chug along. The possibilities for real genetic engineering, for example, made my little mutate and hunt thing not very interesting, because now you could transfer genes. You don't mutate a bacterium to wait until it learns how to make insulin, but you make it. You know how it works. So molecular biology was an enormously more powerful method for achieving any kind of desired biological material or organism than simple mutate and hunt.

Vettel: I want to get to that transition in a bit because that is also interesting. We'll step back a bit just for a moment. You, Cape, Farley, and Alafi founded this biological company called Cetus. Is that how you would describe the process in which the company was formed? If so, what did Cetus do, generally, as a business in search of profits.

Glaser: Of course, very quickly it became a biotech in the modern terms when biotech was existing. Before that, it was a different kind of biotech company. I would make something up now if we needed it, but we didn't have a name for it. We didn't need a name for it. It was a company that did strain improvement.

Vettel: It was a new venture.

Glaser: Microbial strain improvement. The name, Cetus comes from the constellation the whale. Everybody wanted to call it Andromeda because of the movie *The Andromeda Strain*. I said, "No, that's too corny." And I was involved with that movie, indirectly. So finally, I don't know who thought of it. It wasn't me, but somebody else thought of Cetus as a gentle beast of great strength which lives by filtering huge volumes of material to find its food. It had sort of a nice— Anyway, that's where it came from.

Vettel: That completes the picture in terms of the concept. Your Nobel award is not in play at this moment. You are creating an engineering system. Is that true?

Glaser: Yes.

Vettel: The Nobel award is used to attract people, employees and investors.

Glaser: It's hard for me to evaluate that, but certainly it probably gained admittance and credence to the scientific side of things. We did, the three of us, go on fundraising tours to the big drug companies to get them to invest in our company. A typical reaction was, "We know that you're a very competent and well-respected scientist. We certainly believe that you're honest and that this stuff you're telling us about DNA is true. But we don't think it has anything to do with our business. Thanks for giving a seminar. We enjoyed it and we know about the science. But we're doing just fine in classical chemical modification of things to make drugs." A more cynical guy told me once after a few drinks that, "No, we're not going to mess around with you guys. If you're successful, we'll buy you. If not, we haven't wasted our time." To some extent, that's what happened. But they paid a lot for the more successful biotech companies. Chiron is half-owned or something like that by Novartis, and Genentech is three-quarters owned by Roche. There are a lot of other small firms that are independent, but as soon as they become really productive, they get snapped up.

Vettel: In this early phase of this microbial production system, the big pharmaceutical companies kept their eye on you, but did they want to see something happen? Did they want you to succeed?

Glaser: They knew about it, but it wasn't part of their business plan. It was of scientific interest. They're smart guys. I think they knew what they were doing. But it's the same with IBM. IBM will not take on some new invention unless it knows it will work. We weren't like that.

[End Tape 8, Side A] ##
[Begin Tape 8, Side B]

Glaser: Every now and then, one of our guys on the Scientific Advisory Board would invent something really cute, but it didn't look as though it was big enough to justify setting up a division of the company or hiring an executive to run it and so on. They very often allowed the guy to go off and make a little company of his own with very generous terms. I'm not sure if Cetus would have given them a patent, but it was very generous because they couldn't do anything with it.

Vettel: By the way, when were you on IBM's Scientific Advisory Board?

Glaser: I don't know.

Vettel: How about, "What decade?" [Laughs]

Glaser: That was the early '60s. I don't remember exactly. I was involved in it for about ten years. I don't know.

Vettel: You mentioned earlier that it was, I think the word that you used was "improper," for an academic or an academic program to be involved with a commercial industry, because it certainly tests—

Glaser: I regarded it, I still do, as illegal and immoral and improper in every way. Now the landscape has changed in the sense that universities now engage in formal contracts with companies. But earlier on, that custom hadn't developed. It was sort of understood by me and by most of us that you just don't do that. Then some people who were less scrupulous started doing it. They've been hauled into court and they've had to pay minor punishments. And then the formal scheme for licensing grew up.

Vettel: When did academia and industry have the right blend? The current era where academics are using their research for profit? When do you think it was?

Glaser: I remember some years ago, our Dean of Biology wistfully said that if you look in the Bay Area and see which labs are the best equipped and have the most sophisticated science in dealing with the biology of DNA, you would have to list Genentech and Cetus first, and then Stanford, UCSF, and Berkeley last. Because of course, Genentech and Cetus have big bucks! Once there's a possibility of great profit, then you can buy wonderful equipment and offer very high salaries and stock options and so on because the benefit of the outcome can be very profitable. A university can't do that.It's complicated, very complicated. Because in a university you can do all kinds of things which may not pan out. You don't have to show a profit. You can therefore take risks of all kinds, which lead you into novel directions, which means that you follow instincts and impulses. Risk-taking is not expensive. But when you get to the level of producing things on a commercial scale, then risk-taking becomes very costly. So perforce companies have to be more conservative about what they do and universities don't. It's very important to preserve the freedom of the university to do things which aren't profitable, or not obviously profitable. When it comes to the stage where something like mass production and so on, it's not appropriate for the university to set up a factory. It's two different kinds of parts of our society, which are both important. Where the boundary should be is something that you have to look at, I think, in a case-by-case situation.

Vettel: How did you negotiate that boundary, especially in this earlier period when biology and industry were kind of testing the waters of the relationship?

Glaser: I just made it a point that nothing that I did at the company took directly from what I was doing on the campus in the sense of developed technologies and biological materials and all that sort of thing. The commonality was, of course, my general knowledge of the science and my engineering experience to build things and invent things and so on. That was common. But anything which was identifiable as belonging to the result of some activity at the university, I felt, could not properly be—unless it was published. Then anybody can use it.

Vettel: What percentage of time did you spend at Bancroft Way for Cetus?

Glaser: The official university rule was forty hours a month, which turns out to be one day a week. I probably spent less than that on the average.

Vettel: Except you were creating a new system. You were building it. Engineering it.

Glaser: Sure.

Vettel: And that system you were innovating where?

Glaser: Again, I didn't keep a diary of physically where I was, when. I didn't physically put the thing together. I designed it and gave the idea to other people who built it. Certainly, I was physically at Cetus much less than one full day a week. But what did I think about when I was in the shower?

Vettel: Who knows where the mind can take you, or when?

Glaser: Sure.

Vettel: So you're creating a system for Cetus that is not part of your research at Berkeley. At Berkeley, you're probably working, I guess, on the scanner?

Glaser: The scanner was simply an instrument for looking at colonies.

Vettel: But that's the work you were doing? And you were looking at the colonies and writing papers about them?

Glaser: That's right. And isolating all kinds of mutants, which we sent freely all over the world. That was also naïve—not that I regret doing it, but one doesn't do that anymore. The university lawyers won't allow it. But in those days, we considered ourselves to be part of an international community of scientists. If you found a bug that had some interesting properties, the ethics were that you published it, and then you said what you were going to do with it. Anybody else who wanted the bug would ask for it, and you gave it to them with the understanding that they would not do the thing for which you developed the bug. But they could use it for anything else. That was the general rule. That rule was enforced by a few strong personalities, like Max Delbruck at Caltech and other sort of senior respected biologists. It was, I hesitate to use the phrase, a gentleman's agreement, but it was a socio-ethic consensus of how you operate.

Vettel: It was a particular generation that saw pure research as a communal—

Glaser: As a communal, and a valuable, that everyone benefited from free exchange. But once these bugs became really valuable, then the university attorneys got interested. Now you can't send anybody anything without permission and without a license.

Vettel: Were any of your bugs that you were sending out antibiotics, ever?

Glaser: I never tested them for that. They were mutants that were tested for their sensitivity to antibiotics for their requirement for this or that nutritional, their ability to grow at high temperature, low temperature, and so on. My fantasy was to run *E. coli* into the ground. By that, I meant we knew it had two to three thousand genes. I forget the number exactly. I think 3,000 is closer. It was sort of the prototype of the simplest autonomous living thing. A virus is not autonomous, obviously. What I wanted to know was, are there general principles about what it takes to be alive that are already evident in the simplest thing which we call living? What are those essentials? That's what I wanted to know. That's what this whole thing was about —building big machines so I could make a zillion mutants, hoping to get a mutation in most of these 3,000 and see what it did. Some of the mutations meant that the bug couldn't make its own arginine, so you had to put arginine in the agar. Or it couldn't make lysine, and you put lysine in the agar.

All of the biosynthetic pathways which lead from glucose ultimately to all the amino acids were worked out—not by me only, but by all of us. I don't know if the others had the same view. Many of the others were just biochemists. They wanted to know what are the chemical steps. I wanted to know, how did the whole system manage to be alive, to repair UV [ultraviolet] damage, to mutate, to improve itself when the environment required it.

Vettel: Almost trying to understand what each gene in *E. coli* did through the process of understanding its mutations?

Glaser: And its physiology and its repair. One lesson I learned very early on: I thought these *E. coli* guys had been around a long time. They divide every half hour. If anybody has optimized in biology, they have. So I decided I would measure their energy efficiency. I was going to put some bugs in a sealed glass vessel, a little bit of glucose, and a little bit of this and that, and then put that in a big water bath and measure the heat evolved as wasted energy to see how much of the energy that I gave them was wasted as heat and how much became bacteria.

As I was chugging along, suddenly somebody showed me a paper. God, I think it was 1926. Some German had had the same idea, but he was smarter. He used *Pseudomonas hydrogenomonas*, which is an organism that uses hydrogen as its only source of energy, and CO_2 as the only source of carbon, and water and sodium chloride—you know, absolutely chemically defined media. He put this in a sealed container—just like what I was going to do—and he measured the hell out of it. Bottom line, the energy efficiency was about 5%. I immediately learned that I was naïve as a physicist thinking that *E. coli* had optimized energetically. Then it turns out that they had invested an enormous amount of their resources in repairing UV damage from the sun and in sexual recombination.

Vettel: That's where the energy of the *E. coli* went?

Glaser: A lot of it. So energy efficiency wasn't so important, but survival by repair and improvement of the species by reproduction.

Vettel: —was where they spent their energy?

Glaser: Was a big fraction of it. I've never really written that up as a philosophical conclusion, but I suspect everybody in the field knows it.

Vettel: I'm going to push you for a moment here, if you don't mind. Some of the mutations that you were finding in the Berkeley lab must have been relevant to improving the yield of bugs.

Glaser: It could have been, but we weren't assaying for the yield of anything, so I wouldn't have noticed those. I didn't care about that. I was interested in what makes them tick. We never did an assay for anything useful.

Vettel: Going through some of your papers, I came across some names that came up often. I don't remember where they were, or what they were doing. Maybe you can help me out. Calvin Ward?

Glaser: He was a physicist, a post-doc, who decided he wanted to switch from physics into molecular biology. He was in my lab for a couple of years, a very, very bright young guy. Finally he decided no, that wasn't for him, and he went to law school in Stanford. He was our first employee at Cetus because he couldn't get a job. He was very good, very bright.

Vettel: This was before he went to law school?

Glaser: Before he went to law school. He was at Cetus for a few years and contributed a lot to all these developments—a very clever guy. Then he became a patent attorney. The last time I talked with him was a number of years ago. Now he bills his time, he's proud to tell me, at $500 an hour. He'd always come in in the morning and tell me what an idiot I was. By the end of the day, he was really deeply hurt if I didn't compliment him on something he had done. But the reason I mentioned it is that when he became a patent attorney, he got the reputation that a guy would come with some very clever patent, and Cal would say, "That's very good, but you idiot, you forgot to do this and this and this. You could improve it with this and this." I think he was valued as a part of the team as well as a—

Vettel: Did he work with the fermentation? A physicist with a background in molecular biology.

Glaser: He had already had two years experience in my lab in molecular biology.

Vettel: Was he working with the fermentation?

Glaser: I can't tell you specifically.

Vettel: Was he a computer scientist or a systems developer?

Glaser: He could have done any of those things. He's a very versatile guy. I can't say. He probably contributed to the software. He probably contributed to the automation system.

Vettel: If you're working one day a week, that's 20% of your working time.

Glaser: In principle, yes.

Vettel: In principle. But you were the one that identified the first employee for Cetus.

Glaser: Right.

Vettel: I'm trying to understand the relationship of Cape and Farley and yourself.

Glaser: In the beginning, the three of us would sit down and behave like businessmen. I would offer business solutions. At a certain moment, I said, "I think the stock is going to go to 18 within six months." They said, "You're nuts." And it did. But that was just lucky. We had no idea what we were doing. I used to chair a thing which we called the Blue Sky Session. Quite regularly, we would get together. As we got more and more scientists—a number of them came from my lab—sometimes we got big enough that we would be 30-40 people. We'd go to Asilomar and spend a few days together. The idea was to talk over crazy ideas, which was really fun.

Vettel: Which has been the point of Asilomar for many—

Glaser: I guess. But for us, it was certainly heaven.

Vettel: And Calvin Ward was in your molecular biology program when he was hired?

Glaser: As a post-doc. Then he went to Cetus.

Vettel: Did he do both for a while?

Glaser: No.

Vettel: Okay. So he was tired of molecular biology? At least in the academic—

Glaser: No. Two years post doc is sort of standard. Then he was looking for a job, which is reasonable.

Vettel: How did you talk him into taking a job at Cetus rather than academia?

Glaser: It wasn't hard. We offered him sort of a commercial salary, not like a post doc salary. He wanted to stay in the Bay Area. His wife had some kind of neuromuscular problems, so she didn't want to move to a place where there's lots of ice and snow in the winter. There were personal reasons. Also, I think he liked working with me. It was an exciting new thing. There weren't a lot of jobs in biotechnology or anything related to it. He wasn't a chemist, so he didn't want to work for a drug company.

Vettel: David Hansen.

Glaser: David Hansen. What connection do you have for him? As a co-author, or what?

Vettel: No. I just saw his name come up on occasion at Cetus and in your lab.

Glaser: I don't remember.

Vettel: Okay. Farley and Cape and yourself, and Moshe Alafi , who was the venture capitalist who launched Cetus.

Glaser: That's right. And each of us put in a little money. I think $25,000 was about all that we could—at least all I could afford at the time.

Vettel: But it’s primarily Cape, Farley, and yourself, with, say, Calvin Ward wearing many hats as well. What was Cape’s role? What was Farley’s role? I understand your role. I think I understand Calvin Ward’s role. What was Cape and Farley’s role?

Glaser: I don’t remember. One of them was CEO or president and the other was vice president. In my mind, they sort of did similar things.

Vettel: Who was the public face, and who was the system or operational person, would you say?

Glaser: It’s hard for me to say because I think they shared both duties. It wasn’t organized like a classical, grown-up company at that time. I was certainly not the public face except that I went on the tours. Maybe it was only one tour. But we went to four or five companies. Finally, the people who invested in us were very unlikely. They were two oil companies, Chevron and Amoco, which is Standard Oil of Indiana, same thing. Their reason is really peculiar. They were just rolling in money at that time. I don’t remember the economic—They were sort of tantalized by this new thing. What they wanted was an investment that looked promising but was guaranteed not to make any money for ten years. [laughs] So we could promise them that. National Distillers, same deal, although some of their business—well, their alcohol business—depended on fermentation. I think the vinyl chloride was strictly a chemical operation. Whatever it was, that’s where we got serious money—and none from a drug company, even though we tried. Strange.

Vettel: This really is off-the-cuff, this early operation. The companies you think you’re going to be working for, you aren’t, but business comes from unexpected sources.

Glaser: They could be our clients, as they were [in] at the beginning, but they didn’t want to be in the business. None of them—Johnson and Johnson, Merck, a few others that we visited.

Vettel: Do you think the loose organization, the spontaneity, the creativity, and the open-endedness of Cetus early on helped you because you could adapt? Or do you think it hurt? Looking back in hindsight.

Glaser: It helped because it was an ill-formed activity, ill-defined activity. We didn’t know what we ought to do and what we ought to be paid for it. We had to sort of invent things as we went. It may well be--I hadn’t thought of this before--that our loose structure was well-adapted for the loose situation and that our structure would not have worked well if we had tried to compete with a well-established company using well-established methods. Only later, of course, did Cetus become much more formal, and of course Chiron is even more formal now than it was five years ago when I was involved.

Vettel: Just before the Schering-Plough contract, there were some precarious moments, I guess, at Cetus, in terms of maybe dipping into the red and not being sure where money was going to come from.

Glaser: I don't remember it very well because I didn't have much to do with that part of it at that time, but I wouldn't be surprised. It was not a stable, flourishing business.

Vettel: Did you ever feel that the lack of focus was hurting Cetus?

Glaser: It's hard to say. We were roundly criticized in some quarters as being a playground for academic scientists. We did a lot of speculative stuff and a lot of new science and so on. PCR, of course, was invented later on. But there was a lot of freedom in a sense that scientists had a certain fraction of their time, either formally or informally, to try new things. The direction was not very stringent.

Vettel: That sounds like a great place to work if you're a scientist, but in terms of an investor and a business plan—

Glaser: That's exactly what we were criticized for. I don't know in balance how to evaluate that. I don't really have any wisdom on that. We were criticized. It's claimed that Genentech was better run from the beginning and more focused, and maybe Chiron also. But they started a fair amount later than we did, and I think they may have had more professional management than our guys. It's hard for me [to say]. I don't know how to evaluate that.

Vettel: Especially early on, businesses must look for opportunities but also remain focused, and the difficult part in all this is trying to find that balance. Cetus seems to have this tension between the interests of the scientists and the interests of the businessperson.

Glaser: And that continues. When I was on Chiron's board for many years, that tension was there. It wasn't really tension in the sense that they were opposing camps, because the chairman and the CEO at Chiron were both accomplished scientists, Bill Rutter and Ed Penhoet.

Vettel: But at Genentech, you had Bob Swanson, who—

Glaser: Who was not a scientist at all, and Perkins and others who were experienced business people. Evidently, they managed better than Cetus did because they're doing better. Or for example, Bob Fildes, who was sort of a professional manager brought in [at Cetus], really messed up by being arrogant toward the FDA. So some of the most profitable products that Chiron has now were started by Cetus, and he just mismanaged the interface with the FDA. It's a personality failure.

Vettel: Because the FDA is so crucial.

Glaser: Yes, getting drugs approved. He couldn't get things approved because he thought they were a bunch of jerks and he'd be damned if he's going to do what they say, and so on.

Vettel: From a scientist's perspective, this is, like you said, a playground.

Glaser: We were accused of that.

Vettel: Going back to the Scientific Advisory Board, the list of luminaries that advised Cetus is remarkable. This is another instance where you had a lot of scientific, innovative kinds of ideas. Who's there bringing, guiding those scientific ideas? Or did Cetus allow its scientists to pursue all these ideas?

Glaser: Well, choices had to be made, obviously. For a while, Fildes tried to kill PCR. I had to fight like hell, and some of the rest of them, to fight with him that they mustn't do that. None of us realized how powerful PCR was, but we all realized that this was really big stuff. That was, I think, incompetence on his part. But at Chiron and at Cetus, there were a lot of possible avenues and you couldn't pursue all of them, so judgments were made fairly early on.

Vettel: I was looking through the early Cetus papers, and Joshua Lederberg was giving some advice in terms of possibilities. I think you and Stanley Cohen—

Glaser: Stan Cohen, sure.

Vettel: The list goes on and on and on. This is early on. I don't even think Fildes was there yet.

Glaser: No, he wasn't.

Vettel: He was there later.

Glaser: That's right.

Vettel: Choices had to be made. Probably of all the ideas submitted by the Scientific Advisory Board, many were viable and reasonable. Who was selecting of those ideas, saying, "It's going to be hard, but we're going to go with that idea and not that one—" Who was doing that?

Glaser: My recollection of it is fuzzy. Therefore, I think it was a consensus of discussions among this group, and among the three of us, particularly. I don't think there was a single personality that was dominating all those discussions.

[End Tape 8, Side A] ##
[Begin Tape 9, Side A]

Vettel: Maybe people used the phrase "playground," and maybe some of them meant it in a derogatory way. It sounds like a playground, but it doesn't sound—

Glaser: It was for a while the best of both worlds. That is, it was scientifically very exciting, and there were a number of commercial opportunities, some of which turned out to be good. Whether we missed some others that we should have done, maybe, it probably happened.

Vettel: So for a while it was the best of both worlds.

Glaser: I remember Stan Cohen kept saying, "Look, I know how to clone things now. Tell me what to clone." None of us had the sense to say, "Insulin." I knew nothing about it. We all knew, after I learned about it, that it's a very small protein. It's an obvious one to try. Now, all the insulin in the world is made synthetically. We could have done that before anybody else.

Vettel: Do you think they relied too much on your system?

Glaser: No, because he kept saying, "Look, I can clone things."

Vettel: Not Stanley Cohen, but the other executives.

Glaser: No, I don't think so. We knew that the system was good for what we were using it for, but it wasn't in the league with real genetic engineering.

Vettel: But they didn't know—If Stanley Cohen comes to the Advisory Board and says, "I can clone things. Tell me what to clone," and yet they decide to continue with your—

Glaser: These things weren't exclusive. That is, we had some contracts. We did some strain improvement. We could have continued it. But it wasn't an all-encompassing—

Vettel: You could have shifted, but the decision to go into recombinant DNA at a much later date is what I'm trying to understand.

Glaser: I'm not very proud of that stage, because it wasn't a decision not to. It was just not a decision to do it. That is, there was an opportunity that we missed at that stage. It wasn't because of competition with other things.

Vettel: You see Cetus as a very real player in terms of having the potential to get into biotech, recombinant DNA.

Glaser: Oh, yeah. I didn't have anything like Stan's knowledge and competence, but that it was going to be important, I think, was clear to all of us. That we didn't grab the ball and run with it at that stage, I think, was one of our major mistakes. We did it later on, of course, but—

Vettel: Would more of a business sense have helped at that moment? Or was it the scientists who had an idea about medicine should have known?

Glaser: I think it's the latter.

Vettel: You said this was the best of both worlds, in terms of your time with Cetus. What were your colleagues at Berkeley, what was the administration at Berkeley saying about your involvement in the BSL and then later with Cetus. This is an entirely new venture for the biological sciences. Chemistry had been doing it for a while, and some other industries too. What was the UC administration or your colleagues saying about your one day a week at Cetus?

Glaser: I don't remember anybody saying anything. It was very common in computer science and in engineering and in chemistry. That rule came about because there was a lot of consulting from all of the science and engineering faculty in every field. Not so much in physics, as far as I know. But when I was on the board of IBM, that was a part of—I was limited to what I could do there. We didn't get anywhere near the limit. It wasn't a new thing. What was new was that it was happening in biology, but the rule came about because it was so common. The rule was not invented for biology--that's really what I'm trying to say. I don't know the history of the rule, but I'll bet it's forty to fifty years old.

Vettel: To go back just for a moment, the system that you had built to increase the yield of bugs in these vats, the system seems like it was difficult to operate. I've seen some papers that say the water was too warm, the bolts too loose, the —.

Glaser: Oh, yeah. You had to control things, sure.

Vettel: Was the operation of this system overly labor-intensive for a new industry?

Glaser: No. It was pretty amateurish in the beginning, but if it had enough really permanent staying power, the automation and the quality control could be improved. It wasn't anything that was beyond the standard industrial practice.

Vettel: It could have become standard industrial practice?

Glaser: Oh, sure.

Vettel: And from there, perhaps new biological opportunities would have come up?

Glaser: Could have, but let me say again that genetic engineering is a much more efficient way of getting the same goal.

Vettel: Did Cetus take genetic engineering seriously enough prior to Genentech?

Glaser: I don't know. I know that Genentech is claiming that they invented genetic engineering. I don't know the history of which happened when.

Vettel: But you were aware of it before? Stanley Cohen was certainly aware of it before Genentech.

Glaser: Oh yes.

Vettel: But Genentech comes along—What did Genentech—

Glaser: In fact, Cohen and Herb Boyer of Genentech co-authored a method for inserting genes into bacteria. They both knew about what was going on.

Vettel: What did Genentech's formation mean or do to Cetus and its operation and its organization?

Glaser: I don't know. I think they went public before we did. That was the evidence of better business management, I think.

Vettel: But Genentech formed in '76, and then they went public in around '80?

Glaser: I don't know the dates.

Vettel: And Cetus was about the same time. But in '76, did Genentech's formation or origins register on—?

Glaser: It's hard for me to know that. Swanson, the original CEO of Genentech, came to Cetus and wanted a job to learn the trade. He had decided that was the direction to go in. I never met him. I met him many years later. He said he'd do anything. He'll sweep the floor—whatever it is. He just wanted to be around. I guess Ron Cape decided he was not likely to be a useful employee and didn't hire him. I don't know of any direct interaction. Actually, I think we went public shortly after they did, probably because it never occurred to us to go public. When we did, we raised more money than any IPO up to that point. The number I remember is $106 million. I don't know if it's true. Ron probably told you or could tell you those numbers. Whatever it was, it was sort of surprising. What really shocked me is that we were worth more than Safeway [laughs]. You see these rows of huge trucks and so on. It was sort of a weird feeling that the monkey business that we had started loomed so large in terms of assets.

Vettel: When did the proverbial light bulb go on at Cetus that said, "We should be getting into genetic engineering"? You said genetic engineering was the more efficient way of doing what Cetus started out doing.

Glaser: Yeah. We were using classical, mutate and hunt strain improvement techniques.

Vettel: When did you realize that this [recombinant DNA] is the direction? Or when did Cetus realize it?

Glaser: Very hard for me to answer that question because I don't really know. It was in the air. It was in the journals. Everybody was talking about it. The timing between when you could read it in the journals and when Cetus made a decision to go after this or that product, I don't know. That's probably in the records that you could look at. I don't know that.

Vettel: Genentech's success was somatostatin.

Glaser: That's right. That had a big effect, sure.

Vettel: That had a big effect. Okay. That helps with timing. So Cetus heard about this?

Glaser: Oh, yeah. But the thing I can't evaluate is what was in the literature before and when did people say, "Okay. Now we've got to start doing this." I don't know.

Vettel: But somatostatin meant something?

Glaser: It was the first time anybody had cloned anything industrially, as far as I know. I may even be wrong about that, but that's what I think. I remember hearing about it. I felt that we were idiots not to—I'd had no idea what somatostatin was, but I felt like a real idiot that we hadn't gone after insulin, which was the next thing that Genentech did, or shortly after.

Vettel: I bet. Okay. At about the time that Genentech is doing its work with somatostatin, Cetus is playing around with Cetus Immune, Cetus Palo Alto, thinking about a venture in the UK. How do you explain the scope of Cetus? There's a difference here. Cetus is looking out and considering many options while Genentech is focused on one goal—.

Glaser: No, it's a big drug company. I don't know exactly what—

Vettel: How do you account for success at Cetus with so much distraction?

Glaser: We were climbing on the bandwagon. I think that we underestimated the political difficulties of plant genetics, that is, of making mutated versions of plants and animals for food. It looked like a very attractive thing to do. You could improve yields. You could improve shelf life. You could shorten the maturity time, and so on. All those things have been done now and could be done, but we hadn't—at least I personally wasn't aware of the enormous political hullabaloo that would arise over it.

Vettel: Surrounding FDA or even more than that?

Glaser: No, just the public is very nervous about genetically—They even have a name for it, GM Organisms, GMO [genetically manipulated organisms]. Now in Europe they want to label anything that's ever had any GMO in it. There's a tremendous, I don't want to call it, backlash, but it's not backlash against something that's happened. But there's a tremendous anxiety, which I think derives a little bit from the anxiety over radioactivity resulting from the bomb and the reactor. But whatever it is, there's a lot of hostility among many people, including the sort of extreme environmentalists, but also including people who aren't extreme, who are just worried about the unknown. That turned out to be a very difficult field in which to make a profit. I think it was an error in our part to go into it.

Vettel: To go into the genetic engineering?

Glaser: Genetic engineering of food organisms, plants and animals.

Vettel: When Cetus does focus on recombinant DNA, they focus on food and plants?

Glaser: No. That was a side issue. We didn't do that. But that was a real drain on resources and time and so on. I think that was a mistake.

Vettel: Let's quit now. We'll pick up perhaps with Cetus in the late '70s, and maybe then wrap it up.

[End Tape 9, Side A] ##

Interview 6, March 2, 2004

[Begin Tape 10, Side A]

Vettel: I thought we would wrap up our discussion of Cetus by taking a look at why you left the company.

Glaser: I was fully engaged, as always, on the campus. I was only the Chair of the [Cetus] Science Advisory Board, as I told you. I held regular meetings and I recruited experts and so on. I faithfully went to the board meetings, but I didn't do much work in between meetings.

Vettel: Right. Today I thought we would talk about the transition into your next phase, the neurobiology. I have one contextual question before we go to neurobiology. Then I have some questions, larger retrospective questions.

Glaser: All right.

Vettel: So today is March 2nd. It is the afternoon, two o'clock. We're in Dr. Glaser's home again. It's probably our last oral history session. We've had quite a few. I appreciate all of your time and cooperation.

Glaser: I've enjoyed it too. But I should mention, I'm scheduled to give a talk for the Faculty Forum. Do you know about the Faculty Forum?

Vettel: Yes.

Glaser: They want me to tell about how I went from physics into biology, and I don't want to. I'm just going to summarize physics in two or three slides, and then talk about neurobiology. I guess it has appeal to others for the same reason it appealed to you.

Vettel: It does, it really does.

Glaser: I guess it could be interesting to physicists. It's not easy to understand.

Vettel: Okay [laughs]. I am very intrigued by the transitions of your career and others are indeed too. Perhaps just one more question, maybe two, about the transitions. But before I do, if you were to write the history of Cetus, what would be your opening chapter? What would be that opening line, or the first person to appear in the story of Cetus?

Glaser: The impetus was given by Josh Lederberg, a very distinguished scientist, who remarked one day—"The basic thing is that we know a lot about DNA, but it hasn't done any good to mankind, and it should." In a way, that was the motivating theme. But there are all sorts of subplots, like I had a really bright post doc, and there weren't any jobs in what I would now call biotech.

Vettel: Who was your post-doc?

Glaser: Cal Ward was his name. He was our first employee; very, very versatile, talented guy. I couldn't get him a job. I suddenly realized we were training a lot of people who could make important contributions to this transition from the lab into medical and other socially valuable applications, and also that on the campus, I had developed a very sophisticated automation system for hunting for mutants. I had what many people thought was an excessively innocent goal, which was to run *E. coli* into the ground. I wasn't the only one. There were people all over the world. But I thought I could contribute by speeding up a lot of the really tedious parts of the work, with the result that we generated an enormous number of mutants of *E. coli*, and many more than we could possibly analyze and do the real genetics and the real biochemistry. So I sent them all over the world.

Vettel: That was at Berkeley, when you were doing that?

Glaser: At Berkeley, on the campus. There were many, many labs who did the real science of analyzing these mutants and what they meant in contributing towards understanding the 3,000 or so genes of *E. coli*.

Then, at a certain moment, the NIH said, "We can't give you that much money unless you work on cancer." So I began, and by using the same technology, with Chinese hamster ovary cells. I don't know whether that's already in your record. We began to study xeroderma pigmentosum, which is a skin cancer that is the result of an imperfect mechanism for repairing ultraviolet damage to the DNA from sunshine. All of us have seven enzymes that are busy at work all the time repairing the damage. If you're defective in any one, you get cancer if you're out in the sun—skin cancer. Those people live completely normal lives, but they can't go out during the day. We isolated seven different mutant sites, so therefore, seven different enzymes. We weren't the only ones working on it, but we contributed a number of examples.

At that moment, finally, NIH said, "We don't do big science anymore." They never did. They said, "We can't afford to support you more than" I guess it was $150,000 a year, which is less than a single lab. I said, "Well, then you're wasting your money. I can't do anything with that amount of money." So I quit. Then I realized that there was enormous benefit to the general application of modern computer automation to be done in the commercial world of biotechnology. That's how we started Cetus. I should emphasize that it wasn't at all the same technology that I developed on the campus, because on the campus, I guess, as I mentioned, we were growing things on agar. In the case of Chinese hamster ovary, it doesn't grow on agar. You have to grow it on specially treated plastic dishes. The surface requirements are quite specific. While in industry you're looking for submerged cultures—big vats, 100,000 or more gallons. We had to have a totally different setup, but the idea of using the computer and video technology to assay a tremendous number of candidates was the one unifying thread.

Vettel: And that's the technological skill that you brought to Cetus?

Glaser: Right. And I got this from high energy physics where I was used to handling huge amounts of data, which was visual data, that is bubble chambers in my case, but before that, cloud chambers, but they didn't contribute much. They were good in cosmic rays, but not in high energy physics.

Vettel: That is very helpful to understand the relationship between physics and your work in these massive, big science labs.

Glaser: If there are any threads that run through all of my career, it's a) to think about dealing with large amounts of data, which are essentially visual in nature, and b) to try to escape from big science. In some sense, I was blamed for industrializing high energy physics, because suddenly we had—But that's not true. It was just an inevitable trend.

Vettel: I would say there's probably a third: finding applications in terms of doing science and helping the scientific community and society do science more efficiently and more effectively.

Glaser: Yeah. In every case, my work was motivated by a certain arrogance in picking really important problems, like what is the universe made of? How does heredity work? How does the brain work? Ridiculous questions, and then hoping that there was some little bit of that that I could help or contribute to in a way that would benefit everybody else, also.

Vettel: That's great; thank you.

Finally, how did you downgrade or phase out Cetus? I know that you stayed on the board for quite some time.

Glaser: I stayed on the board until we merged with Chiron. Then I was the only one that Chiron invited to stay on as a member of the Chiron board. I think Ron [Cape] also, but for a short time.

Vettel: I think he went to one meeting and then quit because he saw where that was going in terms of his contributions.

Glaser: He told you his story. My view of it is that Ron is a very active guy, and he's very good at entrepreneurial things. He wanted to be free to be involved with start-ups in this field without any conflict of interest. I think that was a part of his motivation, which is wholesome. But I had no entrepreneurial desires at all. I thought it would be fun to be where the action was, so I was pleased to—And it was fun for some while.

Vettel: You were phasing out of Cetus, other than staying on the SAB, the Scientific Advisory Board.

Glaser: That was the end of Cetus. There was nothing to phase out of. I was in Cetus the whole time.

Vettel: The whole time? The same capacity, the same commitments?

Glaser: That's right. And then I transferred into Chiron with no break.

Vettel: And your contributions to Cetus in the later stages were similar to the contributions early on, the same kind of work. Cetus was going in the same direction, or later on, they were going to—

Glaser: No. Later on, the work of the company became less and less dependent on fancy automation and more and more like the other biotech companies—dependent on DNA splicing and what's real biotechnology, namely, manipulating genetics to produce useful medicine or other products.

Vettel: Last question about Cetus. Who pushed Cetus in the direction of biotech? Or what pushed Cetus?

Glaser: I think "what" is a better word. It was obvious that when gene splicing happened that then you could do things that you couldn't do before. We realized that, and so did a lot of other people at about the same time. Stan Cohen, at Stanford, was one of our science advisors. He was one of the inventors, together with Herb Boyer, of the gene splicing technique. He was fully aware of this. We knew from him what was now possible, even before it was published. We saw it. Everybody else saw it at the same time. We started before there really was biotechnology in the modern sense.

Vettel: Okay. At the time, you were also in the molecular biology department at Berkeley?

Glaser: Yeah. I had been in what was called the Virus Lab, which was the predecessor of molecular biology. That was in the old Stanley Hall.

Vettel: Right. I've pored through your papers. But the papers on your more recent work are not there, for obvious reasons. I may need some help.

Glaser: There's some on my web site, if you look there. I have an awful lot of stuff that's ready to be written up. It's more fun working on it than it is writing it up!

Vettel: Tell me about the transition from molecular biology to studying the brain: your timing, your motivation —

Glaser: As always, there's a carrot and there's a stick. The stick was that as molecular biology became more and more sophisticated, it required more and more knowledge and skills in biochemistry, and all kinds of, well, chemistry in general, but like biochemistry, organic chemistry, in which I don't have very deep training and which I don't like. I don't enjoy it. In the early days of molecular biology, a lot of physicists made major contributions, because then you didn't need to know so much chemistry. It was simply a matter of being clever in inventing easy experiments that answered critical questions. That was the early beginnings of molecular biology. By the time I was beginning to be seriously interested, it had already gotten way over my head in the directions that are the real modern molecular biology.

Vettel: The chemical side?

Glaser: Yeah, the biochemistry in general, molecular manipulations.

Vettel: Okay. That's the stick. What was the carrot?

Glaser: The carrot was, I had worked for a long time, first in high energy physics, then in molecular biology, on essentially trying to understand and automate visual tasks. In the high energy physics business and in molecular biology, I had a television camera connected with a computer, trying to understand what it was seeing. I began to see, which everybody knew, but it was personally obvious that it was exceedingly difficult to write a computer program to do what you and I do trivially. Then it began to dawn on me that the really interesting problem is how does the visual system work? That was the carrot.

I decided things have gone far enough that maybe I could contribute something. I had a very close friend, Werner Reichardt a German physicist, who worked a lot and made a lot of contributions toward vision in house flies. Beautiful work. He constantly was trying to get me to enter the field, but I felt that I didn't have anything to contribute at that stage and was heavily involved in other things. Later, I began to see that things had gotten far enough along that you could begin to make quantitative measurements. The circuit diagram of the visual system was fairly well known. It might be possible to make some testable predictions about how the system might work. That possibility is what really excited me. To this day, it does.

Vettel: So the next big project is vision and how the brain processes what we see. When did this transition take place?

Glaser: I don't know exactly, but I began teaching a course in computational models and vision research, theoretical biology. I did that for many years.

Vettel: That was the first step?

Glaser: The first step was teaching a class. I had a lot of faculty members who came to the course.

Vettel: It must have been wonderful in terms of just sharing and learning. When you teach and you're part of the learning process, it's very exciting.

Glaser: Absolutely. Suzanne McKee, who is an experienced psycho-physicist, came to my class. Every day, when I'd say something wrong, she would correct me and then we would discuss it. It was very, very exciting for me. Then I gave a course on computational models in vision. I must have had, well, I filled their hall with about 100-150 people, many of whom were computer science students and physics students, not so many biologists, and several faculty members. The faculty members were constantly making serious criticisms and remarks, as were the students.

Vettel: You had undergraduates, graduates, faculty—

Glaser: No, only graduates. I taught undergraduate courses in molecular biology, but not in that.

Vettel: I'm having a hard time imagining 100 graduate students.

Glaser: Oh yes. And they all had to do term papers. See all those papers there? I'm in the process of throwing those out [laughs].

Vettel: They must have come from all disciplines.

Glaser: No. It was mostly physics and computer science, and probably some mathematicians. I don't think there were many real biologists, because it was advertised as a course in computational approaches to vision.

Vettel: True interdisciplinary work.

Glaser: Oh yes.

Vettel: In the truest sense of the word.

Glaser: Sure. I had the task that I have every time I give a talk, of educating them about the biology in order to justify and set the stage for the theoretical speculations.

Vettel: Was the interdisciplinary style, when you transferred into neurobiology, similar to when you started out in physics, working with the bubble chamber? The way you described the bubble chamber, with engineers and physicists and mathematicians all coming together, is this—?

Glaser: Not so many mathematicians. The amount of mathematics that we had to do in the bubble chamber thing, I could do myself.

Vettel: But you had advanced training in that.

Glaser: Oh, yeah.

Vettel: It's not as if it was simple math —it's over my head! [laughs] You clearly didn't need a team to compensate for what you didn't know.

Glaser: No, it was some fairly sophisticated statistical mechanics and such things—not original, but applications of advanced methods. I didn't invent any mathematics.

Vettel: [laughs] Okay.

Glaser: What I'm doing now is more like inventing mathematics, because I don't really believe that the conventional mathematics, which was developed to deal with motion of rigid bodies and transmission of electromagnetic radiation, radio, and light, and transmission of sound and flow of fluids, I don't think that's going to do much good in biology, even though it always sets a limit on what's possible. You mustn't violate conservation of energy. You mustn't violate the second law, and so on. To make a constructive model for how the brain works is nothing like the mathematics of continuous media and those kinds of things that physics is good at. My models are really rules for how a system might proceed from one moment to the next. Those rules are not unlike the rules of chess in the sense that you cannot write an equation that predicts how a chess game will unfold.

Vettel: No. The decision tree is so wide at the very beginning.

Glaser: That's exactly so. In the case of visual systems, we have a very complicated circuit. Signals go everywhere, and the signals may be coded according to some Morse-like code. Or it may simply depend on the spike rate. What is the frequency of pulses per second? Big debate. In some cases, there seems to be a code. In other cases, it's simply the rate. The game is to figure out what signals could be sent along this meshwork of wires that could result in, "That's my mother."

Vettel: So you were honing in on the actual signal, trying to understand the signal, how it's processed into a signal, and how that signal is interpreted.

Glaser: Well, that's the passive side of it. The active side of it is to invent a signal, which has a maximum combination of robustness, paucity of resource requirements, minimal energy requirements, robustness against noise, invulnerability to death of some of the neurons. The game is to invent something which is as simple as possible, as robust as possible, and does the job.

Vettel: This work that you're doing could not have been done decades earlier. You need the technological power that we have currently to do this.

Glaser: That's right, because the number of variables is huge. But more than that, I cannot visualize beyond a few steps the consequences of some of these rules that I write down. Instead, I let the computer apply the rules, and then I generate a movie. Then I look at the movie and I say, "Oh, yeah. That's how it might work." Simulations is the right word, not computations. We simulate what would happen if these rules really worked. Then we can make a movie and see it unfolding.

Vettel: This is not artificial intelligence?

Glaser: No.

Vettel: Because that also is dealing with a massive number of variables—probably too many!

Glaser: No. I'm not trying to imagine how a chess master operates. But on a very simple scale, it is similar in the sense that I'm trying to imagine what happens in the first one or two synapses in from the retina that allows us to quickly detect motion.

Vettel: The simplest sight.

Glaser: The very simplest. First, I have to make a list of which things must be done first and which things can afford to wait. Anything that moves, you've got to pay attention to right away, before you know what it is. And the next game is to figure out what it is, and the next game you figure out is, is it an opportunity or is it a threat? I'm never going to get to the last stage, is it a tiger or a pussycat?

But the first step is, and I don't know if I showed you last time. I just invented a new—I hesitate to use the word—optical illusion, but a new stimulus, in which there's nothing moving, but you see motion. Anyway, there are some famous cases of ordinary pictures, paintings, or photographs in which things seem to be moving, but they aren't. The brain is being fooled. My task now is to figure out, how can that happen? It's not just a parlor game, because I visualize the brain as some sort of a scintillating mass. A lot of the scintillations are organized in a way that's useful, but many of them aren't and would be considered noise. I don't really believe that. I think the brain is a very efficient gadget. I think these scintillations have some function. I made a model speculating what function they might have. Surprisingly, the damn thing rotates, but there's nothing rotating in the picture.

Vettel: Could I stop the tape? I'd like to see them.

Glaser: Okay.

[End Tape 10, Side A] ##
[Begin Tape 10, Side B]

Vettel: Not this one, but I have seen things like this. Right away, you can see the image moving.

Glaser: Tell me what you see.

Vettel: I see movement back and forth, almost waves.

Glaser: Where?

Vettel: Oh, wow. In the colors. In the circles.

Glaser: Exactly.

Vettel: But then also, the black lines are almost radiating outward and getting more vivid.

Glaser: Right. This is a painting by a French artist named Leviant in about 1985, and he called it *Enigma*. Then he wrote a paper—an artist, not a scientist—which appeared in the *Proceedings of the Royal Society*, in which he tells how he adjusted all the parameters—the sizes of things, the number of radiating spokes, and so on—to get the maximum illusion. I don't think that's an accident. I think that is telling us something about how the brain works.

Vettel: Now, if I close one eye, the radiance is not as great as when I have two. It's almost as if, for some reason, when I have both eyes open, it's vivid.

Glaser: I think that's a brightness effect, but I don't know.

Vettel: Could I submit one of these into the record of this oral history? I don't have to submit this one.

Glaser: The problem with it is that it's a signed art work, so there might be copyright problems.

Vettel: Okay. I'll let you hold onto it then. Because it really should be in the oral history record.

Glaser: On the other hand, it's in a book that I have, it's in the lab now, of optical illusions, so it may be possible to get the copyright. But that's the problem. Here's another one that I got off the web. This one was done by a scientist.

Vettel: Oh!

Glaser: Isn't that amazing?

Vettel: That really is. But it's funny how it's pulsating almost, too.

Glaser: Oh, yeah.

Vettel: So this is what you're using to measure stimuli?

Glaser: Well, there are a lot of things going on. Here's one of my simulations. Usually I don't use it if it is static. They're usually movies. I start with a bunch of things like this, and then I let the chess game play itself out. I can make a chess rule which destroys various ones of those systematically. I can figure out how many plus signs are there, how many triangles, how many arrows in each picture by watching the thing destroy them one after the other in a certain order.

Vettel: How does that work? You do that. You watch them destroy.

Glaser: That's one way.

Vettel: What is that measuring? What are you looking for?

Glaser: I'm looking for an unbelievably simple rule by which the brain can figure out what's there by systematically going down a list and throwing out the obvious ones by a series of rules. You look out there. You see something move. Is it a rock, which is rolling down the hill? Is there a rabbit? Is it a grizzly bear? I'm convinced that you go through some kind of a list according to how much information you need at each moment to finally refine the thing. But first of all, if it moves, you'd better pay attention.

Vettel: But then that list is working fast?

Glaser: Very fast.

Vettel: Or does the brain figure out how to jump into the middle of the list because it knows that we'd better speed this thing up, processing?

Glaser: Now we're getting pretty much into speculation. My speculation is that when you go into a new setting, you do some kind of initialization, which determines the context. That context determines what's likely to happen in this kind of context. That sets your priority list.

Vettel: My sense of how science works, when we reach the end of a line of a particular topic, the disciplinary boundaries get jumbled up, and they reorganize again. Do you think that this is a new direction, a new field?

Glaser: You mean what I'm doing here?

Vettel: Yes. Neurobiology.

Glaser: Oh, neurobiology in general, yes. But that covers everything from neurochemistry to psychiatry.

Vettel: Is this a field, do you think? Or have you made room for yourself?

Glaser: What I'm doing now, most people think is crazy. Other people are doing mathematical methods and so on. There's going to be a big meeting at the Math Sciences Research Institute. The guy who runs it is a friend of mine, and he didn't invite me. I said, "Hey, I want to give a talk." "You can't. Sorry. It's all full." And then he said, "Anyway, it's only for mathematicians." It's crazy. But you know, that's good news, because I guess earlier on I told you that a government official wrote back that it would be an irresponsible use of public funds to support my work, the bubble chamber. $2,500 bucks I was asking for. Then we ran around to the big pharmaceutical companies, trying to start a little company. They said, "You're crazy. Don't bother us."

Vettel: And the NIH said the same thing, unless you're going to do cancer research.

Glaser: That's right. And now, same deal. "Have fun. It has nothing to do with us."

Vettel: How big is your team? How many people?

Glaser: It's not very big. I have at the moment two post-docs. Sometimes I have three. That's the size. I have two graduate students, and that's two or three, and usually something like ten or 12 undergraduates who sort of circulate through the lab. Mostly they help us observe these experiments. Now and then, one of them is really talented and gets excited. He can do a little project. A young lady once was a co-author on a paper when she was an undergraduate.

Vettel: Oh, good for her.

Glaser: They have a chance to be a co-author if they really do some science. Otherwise, they just have a chance to be part of the scene.

Vettel: How long have you been working in this field?

Glaser: You asked me that before. I first started teaching it before I started doing any real research. I guess the earliest papers, if you look at the website, are probably ten or eleven years ago. I think I've turned out five or six Ph.D.s, something like that, in this field. One of the students is getting quite close to a Ph.D.; the other one is just starting. The third one is doing a rotation. I don't know whether she's going to come back to the lab or not. I think she wants to do wet biology.

Vettel: What have you learned? What have you found? How have you contributed so far in this field? I know it's early.

Glaser: Yes. We've done several things. One of them is to make an extremely simple model of motion detection of a single point of light, to take a simple case. The current wisdom is that there are special little circuits called motion detectors. One of them was invented by this friend of mine, Reichardt. Each one of them takes ten or twelve neurons in a little circuit. But you can see motion anywhere in the field of view, any direction, any velocity. Each one of Reichardt's detectors is good for one direction and one position and one speed. The brain has to be plastered with these things. Pretty soon, you run out of neurons. I think it's silliness, except maybe for houseflies.

Instead, I have a very simple method, which is like a checkerboard. On each square, there's a neuron that can do a few simple things. But they're all connected together. It turns out that if a point of light moves across that checkerboard, it generates a bow wave, just like a speedboat. If it goes slowly, it has a wide angle. If it goes fast, it looks much narrower. A speedboat is a good analogy, but it's not accurate. The hydrodynamics of water is not that simple. But it is accurate for the nose cone of a supersonic jet or Cherenkov radiation in physics, ruled simply by causality. The thing goes along. The point of light has a certain speed, which you impose, and there is an intrinsic speed of propagation in this two-dimensional array. Those two velocities determine the angle. It's that simple.

Vettel: And this is where your physics comes in?

Glaser: Yeah, that's physics, but simple. I could teach it to a ten year old.

Vettel: Really now? [laughs]

Glaser: Really.

Vettel: Where are you publishing your articles? What journal?

Glaser: There's a journal called *Neurocomputing*, which is where most of it goes. Then the current stuff, I don't know. I may submit it to *PNAS* [*Proceedings of the National Academy of Sciences*] because I'm sure I'll have trouble with the referees if I send it to conventional journals. I've had that trouble. I never had much trouble in physics, and most people don't. But in biology, there are so many opinions that everybody, even famous people in biology, get their papers bounced three or four times before they satisfy a committee of reviewers.

Vettel: I wonder why that is for biology and not—

Glaser: I hate to speculate.

Vettel: No idea?

Glaser: I think professional jealousy is one ridiculous reason for it. Another reason is that there's a lot of physics which is really true and secure. A lot of biology isn't. People who have done experiments and know how hard it is to make them work are very critical of other peoples' experiments unless they've done every imaginable control. One time, I had a grant proposal turned down. The reviewer said, "Dr. Glaser's group has done very nice work with Chinese hamster ovary cells," which is a standard cell line, "in elucidating xerodermal pigmentosum. But in his new proposal, he proposes to use mouse 3T3 cells, and there's no evidence that he knows how to grow mouse cells." Can you believe that?

Vettel: You'd think there was a professional courtesy there.

Glaser: What, besides jealousy, could have motivated such a stupid remark? And 3T3 cells are another very standard cell that everybody uses. There are recipes everywhere on how to do it.

Vettel: Even if you didn't or couldn't or for some reason your lab couldn't figure it out, it's not hard to get over that hurdle.

Glaser: No! I just couldn't believe it. I wrote back to the NIH and they said, "We see your point, but we're sorry. We have a referee system. Go away."

Vettel: And the NIH listened to them? See, it's not just the person who wrote a foolish thing like that. What about the committee that would not just say, "This review is absurd"?

Glaser: I don't know.

Vettel: I want to get to that, the larger questions.

Glaser: Anyway, you asked me what have I done. The other thing is in stereo, as you're looking at the relative depth of things. Imagine you're looking down on a billiard table and all the balls are black. You're looking at them, so there are a bunch of little black ball images in this retina and a bunch in this one. Then it's trivial geometry that you draw lines through the pupil and you find out where they are, except that if they're all identical, which one goes with which one when you start this geometry? That's called the correspondence problem, and there's no solution to it so far. I've solved it for some simple cases.

Vettel: You've solved it?

Glaser: I've solved it. I have to show you graphics, but the main idea is that there is, in the back of your head, a literal map of the world. It's been shown with monkeys. A monkey stares at an archery target, bull's eye, and there's a bull's eye in the back of its head—it's really there; it's not just a figure of speech. You can measure it. What I've shown is that, with these propagating waves, if you put a single point of light you'll get a spreading circle, like dropping a stone in a pond. If I do that with the image at the back of your head, I now have an expanding wave going across the back of your head. It hits the billiard balls in order.

Vettel: Does it really?

Glaser: Yes. All you have to do is pair off when each billiard ball was hit, and it gives you the right answer. We haven't published that yet. That's on the verge.

Vettel: When you go over the transcripts, any of these graphics that you want to include in your oral history I think would be very helpful for a reader's understanding.

Glaser: They're essential. But it's a lot of work. I think the efficient way to deal with that is to simply give a reference to the publication. I've been feeling guilty that I haven't written this up yet. On the web site, you'll find, I don't know, six or eight papers, nearly all with co-authors. [http://mcb.berkeley.edu/faculty/NEU/glaserd.html]

Vettel: When you transitioned into neurobiology and how the brain works, it's not as if you jumped from one very important question to another very important question. Something guided you into this direction. Why vision? Why the brain? I know the brain is interesting, but there are a lot of interesting questions out there.

Glaser: Sure. My motivation is really simple-minded. That is, first, vision is roughly one-third of the cerebral cortex. It's the part for which the circuit diagram is best known, so the anatomy of the connectivity of different parts, and the specializations—motion is here, color is there, faces are here—is well known. It's getting better all the time. And there's a limit in sight. There are going to be about fifty specialized areas with specialized function, and they communicate with each other. So one has some idea of the structure of the brain.

The thing one doesn't know is what messages need to be sent. So I picked vision because it's the best known part of the brain. It's about one-third of it—very, very important to us, obviously. And because you can do very quantitative psychophysics, which is the other half of what I do. [Hermann] Helmholtz invented the word "psychophysics," in which the psycho is that you're measuring behavior of humans, and the physics is that you're giving a stimulus which is very well defined. Unlike psychotherapy, in which neither thing is true! We have a big long, black lab. It's about fifty feet long, totally black.

Vettel: Here at Berkeley?

Glaser: Yeah. We have two of them, two channels, like two bowling alleys, two bowling channels. At one end is an undergrad. At the other end is a computer with a screen. These students look at it, and they're supposed to indicate is it going to the right or is it going to the left? Which one is closer? So we make a lot of quantitative measurements. The name of the game is to speculate about what messages might be sent in this known wiring diagram, more or less known, that could predict what these people can see and what they can't see. In particular, these funny motions I just showed you. Not everybody sees them.

Vettel: That's the proverbial curve ball in this whole story!

Glaser: It's true. That's right.

Vettel: Because I can see, by predicting what they should see, that shows that you have a grasp of the data, if you know what the data means. But what does it mean when they don't see it? It's one thing to have color blindness and not see it, but we're not talking about that.

Glaser: Yes, color blindness is 5-6 percent of men. It's not a negligible thing. There is a group at MIT that measured stereo blindness. They said 20 percent of the population are stereo blind. They had a rather stringent test for stereo acuity. What it really meant was, who failed to pass their stereo acuity test? It could be that it's nearsighted people. They're not blind, but they can't see the chart. You have to examine these claims very carefully. Bottom line, there are 20 percnet of the population that are below some normal standard in depth perception.

Vettel: And once you find them, you're not going to use that 20 percent of the population in your experiment.

Glaser: No. We're careful to pick people who—I have a very bright young Indian grad student who just joined us. He couldn't see these things I just showed you, nor this new, I call it a racetrack illusion. But after he was around the lab for a few weeks, he began to see it. So that's really scary.

Vettel: [laughs] Dr. Glaser, now it's just too messy.

Glaser: It is. There are two possibilities. One of them is that it's social pressure. The other is that it's trainable. The trainability is not a shock. If I ask you which finger is closer and you're a guy off the street, you'd do pretty well. But after about a week's training in my lab, you can do ten times better. So stereo acuity can be trained.

Vettel: It's learned?

Glaser: Yes, learned. However, there's a thing called vernier acuity. You're familiar with the vernier scale. The question is which finger is to the right and which is to the left?

Vettel: But then if you have someone who has astigmatism—[laughs]

Glaser: Assume normal vision. Which we do. Either normal or corrected to normal. Otherwise, they're not in the experiment. People off the street are very good at that, too. No amount of training will improve it.

So what's going on? By looking at trainability and normal behavior, you get some clue as to what the mechanism might be. The Holy Grail in my lab that I've been talking about for years is, if somebody is bad at this, they've got to be bad at something else too. Because if there's a mechanism, it's going to be shared. You can't afford to have a whole piece of your brain doing nothing but vernier acuity.

Vettel: Or there will also be some part of your brain that overcompensates for being bad here and is very advanced in another area.

Glaser: That's right. That's another possibility.

Vettel: Is this, perhaps, one of your more challenging projects? Or did all of your projects have this many variables? Was the bubble chamber this challenging when you started out?

Glaser: The bubble chamber was much easier because I had about five ideas. I tested all five of them. It's the only one that really worked. But in every case—almost every case—I could predict, because the handbooks are full of reliable data in physics. In biology, that isn't true. There's a lot of stuff which is often believed by everybody based on one paper that happened, like hormone replacement therapy. Really bad data, and not much of it, and it led to a tremendous fad. That's worse than serious—Medicine is much worse than experimental biology, in my opinion. There often are—

Vettel: Just one or two papers that establish a rule?

Glaser: Yes. To be blunt, many, many physicians are not trained as critical scientists. So in all good faith, they report stuff which isn't really verifiable. And many are good scientists. There are both kinds.

Vettel: I've read some of their journal articles, and they're not reliable. I'm amazed that one or two articles will constitute clear and knowable information.

Glaser: Right. That's certainly what I think, too. But on the other hand, there are people dying, so they're grasping at straws. You can say, "God, if we have any clue about what it might be, grab it."

Vettel: Then it's the reader's responsibility to take it and—

Glaser: In a way.

Vettel: Is this your most challenging project to date?

Glaser: This is tough because of the difficulty and complexity of the subject, and the resulting unreliability of much of the literature. You have to be very critical. One of my colleagues, [Tribhawan] Kumar, who was a graduate student many years ago, is wonderful. He doesn't believe anything. He will dig up six reasons why this paper is nonsense. We have very critical discussions. His ability to find weaknesses is really a tremendous asset to the group. There are things which I tend to believe, which he shows me, "Here's the paper. You read it yourself." And he's almost always right.

Vettel: A critical thinker.

Glaser: Yes. But he's really good at everything, this guy. He's remarkable.

Vettel: And he's a grad student?

Glaser: No. He got his PhD in physics in my lab, and he's been with me ever since. Now he's an assistant research psychophysicist. I'm trying to make him an associate research psychophysicist. He's a professional. He's my closest colleague.

Vettel: What attributes, what talents, what knowledge, did you bring from physics to your current field in neurobiology? Neurobiology sounds, from the way you describe it, almost too broad.

Glaser: Yeah. It's a huge field. What I'm doing--I don't know what you'd call it—computational modeling of the visual system and visual psychophysics. Those are the two things I do. I limit myself to the front end, because there I feel pretty secure. We really know a lot about the retina, and we know quite a bit about the primary visual cortex. As you go higher and higher in the brain, which means roughly going from the back of the brain toward the front, when you get to the prefrontal areas, you're getting to decision making and artistic creativity and so on. I don't think we're going to get very far during my lifetime, in that direction.

Vettel: What came with you when you entered this field from physics?

Glaser: Certainly, as a graduate student in physics, I really learned what hard science is, that is, what constitutes a proof. I learned how valuable really sophisticated mathematics can be, which, as Einstein and Wigner and many other guys pointed out, is one of the real shocks, that we can understand physics. Why should our brain be mappable onto the so-called real world? Anyway, I learned that thinking hard and thinking critically and thinking carefully and being skillful in mathematics has enormous power to understand the physical world. Then the question is, how does it help in biology? In biology, also, if you limit yourself to the kinds of relatively simple things that I do, the same kind of discipline, and going back to fundamentals all the time to make sure that your assumptions are not based on something unreliable—I think that's part of it. The custom of thinking very hard and being very skeptical is characteristic, I think, of physics.

Vettel: This is the training that you received in physics?

Glaser: Yes. I don't know whether it's my personality which made me enjoy physics and be good at it, or whether I learned it because I went into physics. I have a standard wisecrack comparing biology and physics: Physics contains a lot of laws, which are very powerful rules with wide application and nearly always give the right answer. But when they don't, when there's an experiment which disagrees, it's called a paradox. Nearly always, if the paradox to the experiment is right, it leads to a new rule. Newtonian mechanics was replaced by Einsteinian mechanics in order to explain a lot of things that happened at the speed of light. A paradox is a useful thing that leads to a new rule, a new law.

In biology, there are no laws, because there's nothing that regular and specific and predictable. But there are mysteries. The solution to a mystery, it's not a new law, because there aren't any laws. The solution to a mystery is a miracle. A miracle means a new molecule, a new structure, a new process, which no one could have predicted in general. There are a few exceptions. So physics leads to paradoxes, leads to new laws. Biology has mysteries that lead to miracles that solve the mystery. That's my summary of the two fields.

Vettel: And there are challenges incorporated within each.

Glaser: Yes. And there are exceptions. I think Francis Crick more or less invented messenger RNA before it was discovered.

Vettel: And the Central Dogma?

Glaser: I'm not sure, but I think so. I think that's a relatively rare thing, for somebody to invent something in biology and then find it. It's more common that they're investigating how does this work, and then they encounter some surprising molecule they could isolate, or some other behavior that they can find. It's a little bit of an over-simplified summary, but I think it captures the spirit.

Vettel: On that note, I'd like to get into some broader topics that convey, if I could, your sense of these scientific fields. The first set of questions is about the culture of physics, in particular, but of science too, then going into big science, and then finally, some personal reflections.

[End Tape 10, Side B] ##
[Begin Tape 11, Side A]

Vettel: Simplify as much as you can, down to the most basic elements, each field and its—

Glaser: Sure. In the bubble chamber work, the fundamental question was that a charged particle at high energy leaves behind very little energy. Its track is the tiniest little tickle. There's no way you're going to detect that unless you have a system which has a lot of stored energy, like a keg of dynamite, ready to go with a spark. I made a list of all the instabilities I could think of and which ones are triggerable. That led to a list of experiments. I did all of them, and they sort of worked, but none of them was as useful as the bubble chamber.

Then in the molecular biology, what I really wanted to find out was what is the simplest autonomous living thing, and what tricks does it use to make it living? That was the idea of running *E. coli* into the ground, which I wasn't allowed to do because it was too expensive for the NIH. Now, in a way, it's happening by a different route, by genetics.

In neurobiology, I finally decided maybe I could have fun and contribute something because the wiring diagram is fairly well known. There's a huge body of psychophysics, accurate, reproducible, behavioral experiments, and that somehow, knowing the circuits, one might be able to invent the signals that might be running around in those circuits that correspond to the observations. That's a common theme, if there is one.

Vettel: Getting it down to its most basic constituent elements.

Glaser: And also to pick a modest piece of the problem. To pick the brain is just silliness. And yet, people do that.

Vettel: Right.

Glaser: In fact, people even pick consciousness. I'm sort of easing my way into it because I don't think I can show you the latest thing that I invented, this racetrack gadget. It's a motion illusion like the ones I showed you. This is a case in which there are a number of tiny, tiny subliminal cues of motion which you can't perceive. But if there are enough of them, then you can perceive a global motion. So below a certain number of these little tiny stimuli, they live only in the subconscious. As they get stronger or more numerous, they rise to consciousness. I see consciousness as a threshold. You must not be aware of all the other stuff, because otherwise, you'd go crazy. Now, I have a very quantitative way of measuring the threshold of consciousness, because I've increased the number of these little guys. So I think I can creep up on the question of consciousness.

Vettel: Starting from?

Glaser: When it applies to visual consciousness. Now, self-consciousness—wouldn't touch it.

Vettel: [laughs] Okay. [Some questions on] culture of science, culture of physics, in particular. From what I understand in the cyclotron/bubble chamber era, there were two possible paths that physics could take. One is that all the particles on the periodic table can be considered a single unit, consist of a composite of something, something larger and grander. This is early on in this era. Another path is, search for more data, search for more elements, kind of the reductionist approach. The field started out moving toward the second, more reductionist approach, it seems to me. The cyclotron/bubble chamber helped the entire field move in this particular direction. And then it was not until much later, maybe some of the work at CERN now, with weak interactions, quarks, and such, that they've backed off. Now they're trying to get a larger sense of "unity" again. I use these two paths as an example to illustrate something that I've been wondering about – how would you describe the culture of physics? Does it move as one hegemonic entity or are there opportunities to move in unique and individual directions? Is the field flexible or inflexible? Is it broad or narrow?

Glaser: We always used to wonder, what does a physicist do? The answer is a physicist does whatever he can. So physicists are willing to do anything, sort of, within the range of understanding the inanimate world, if you like. Yet, it's always been a goal of physics to understand the world from a reductionist point of view. That's really the only science of the hard sciences. Namely, can we understand fully some little tiny components which, put together, will explain gross behavior? It began with chemistry. The Greeks said, "If I cut up sand and make it finer and finer and finer, is there some limit?" Some guy named Democritus says, "No. There's an atom." What does atom mean? In Greek, it means "can't be cut." Tomography is cutting, "a" means no. And the other Greeks said, "No, look, there's water. If you keep going and cut up sand fine enough, you get water. Look, the ocean. Where do you think it came from?"

So this debate started long before modern science. It has been, really, the hallmark of science to suppose that understanding will come by the divide and conquer method. The periodic table was the first major success, which essentially described chemistry. It contained electrons, neutrons, and protons, and nothing else. Then in order to understand what was going on in the atoms, cyclotrons and high voltage accelerators, and all kinds of other gadgets, where the idea is you throw things at it, you bang it into pieces, and see what the pieces are. And what do you know? Out come protons, neutrons, and electrons. As long as you're in the range of ten or twenty or maybe 100 million volts, that's what you get. So [Ernest] Rutherford and his boys and everybody after that then did what was called nuclear physics, which is to blow up the nucleus and see what the parts are.

Then, occasionally in cosmic ray physics in the cloud chamber, you would see something weird. They were called in the beginning pothooks. Then they were called V particles. Then it turns out that they didn't decay as fast as they were supposed to. They were called strange particles. Once you knew what they were, they were called theta and lambda and given names. There was a whole new world of particles. The question is, were they there all the time, or did I make them? It's sort of an academic question, but the main point is that we think of $E = MC^2$, since C^2 is such a huge number, that you can get a huge bang out of a little tiny bit of matter. But you can also run it backwards. You can put in lots of energy and out come little particles. High energy physics meant that you had a big enough accelerator that you could put a large amount of energy into a collision, and things came out. Then you began to wonder, okay, now I'm manufacturing new things, and they decay very quickly, and so on.

When I started with the bubble chamber stuff, we knew there were protons, neutrons, electrons, maybe a couple of mesons, and that was the universe. When I quit, there were about 130 particles, and it's grown since then. So it isn't really in addition to the periodic table, which is only neutrons, protons, and electrons in various configurations. But it's now adding all kinds of things which don't occur in stable matter because they're all unstable. They decay. They're radioactive.

Vettel: Well, they're stable in our experimental system. That's what we see.

Glaser: I would say that the reductionist point of view has not by any means been a modern invention, nor is it running out of gas. It's running out of gas to the extent that the accelerators cost more money than governments are willing to spend, and you never know what's around the corner. A lot of these things were totally unexpected. But they're kind of like a jigsaw puzzle in that if you have a few pieces, or if it's a jigsaw puzzle with five hundred pieces, you can get a picture, but perhaps there is no way to get the big picture with just a few pieces put together. If you can put together two to three hundred out of five hundred, well, you can get parts of it. So the ability to think that you understand the whole thing requires that you have most of the pieces. That's what's driving this idea of going to higher and higher energies, more and more particles. Now, the question that's much debated is, what if there is an infinite regress and there's no limit? I don't know what the professionals think about that. But when would you, or should you, stop?Yeah. They last long enough to see them, but they don't sit there as part of that table. Now, more and more and more of these things, what do they have to do with anything? It turns out if you put some quarks together, what do you know? You get a neutron or you get a meson. So the reductionist philosophy continues as it started, with Democrates and the periodic table. It's gone to a different level now. Increasing the energy, it turns out, you can make things. Now you can say, "Well, these things you made, they were always there in some kind of virtual state. They made a little cloud, and that little cloud maybe is how come a neutron has its properties and magnetic moment, all the rest of it."

Vettel: But when you say that a physicist does what he or she can, in terms of the culture of science, if I want to get from Point A to Point B and people just before me, in the generation before me, and my contemporaries, are going from Point A to B on this one particular path, if I'm going to do what I can, it's easier for me to follow the same path, rather than blaze new trails, cut through all the shrubs, and get to the same place on a new path! And so the culture of physics, or the culture of hard science, is there a tendency within this culture to follow?

Glaser: Oh, sure. There's a tendency in every human activity to follow. That's a good thing. We learn from our ancestors and our colleagues and so on. However, luckily there are revolutionaries. I don't know what else to call them, deviationists. One prime example: this is my right hand, this is my left hand. Most people are right handed. There's a sugar called dextrose, which has the same structure as my—There's levulose, which is a mirror image. But now when you get down to hydrogen, there's no such thing as right-handed hydrogen and left-handed hydrogen. There was a general belief, your well-trodden path, that as you go to simpler and simpler and therefore smaller and smaller physical objects, they will have simpler and simpler personalities. And handedness is one that's gone.

Then Lee and Yang said, "Hey, it ain't necessarily so. We assume that because it sort of seems appealing, but there's no proof of it. Furthermore, it could be that it isn't true." What do you know, these little particles, the so-called strange particles, have handedness. That's called parity violation, and that's a Nobel Prize. That was a tremendous shock. But they pointed out that here we were following everybody on the same path, and going out in the shrubs, we found an interesting thing. So there are these people who are mavericks. They're not trying to destroy anything.

Vettel: Right. But in terms of the culture of science, the culture of physics or science, whatever, there's an experiment at CERN right now that's going to have 2,000 names on a single paper. Or the Human Genome Project. Is there a place for a maverick in big science? Or must they follow?

Glaser: That's why I kept running away from big science. I don't like it. I like to sit and think very hard and talk to a small number of guys and argue back and forth. Because I remembered at the Bevatron here in Berkeley, I had the bright idea for an experiment. Great. Everybody thought it was wonderful. Off we go. Then it dawns on me that my whole army and all of the resources I have are committed for three years. No point in my having another bright idea. What I like is having bright ideas, thinking, and no end-point. So I became sort of a junior captain of industry without a big income. I decided, "To hell with that," and that's why I quit. More and more of my friends are quitting now, of course, because the sociology of 1,000 or 2,000 authors— I quit when I was one of 23 authors. They resulted from my sending bubble chamber pictures all over the world because we couldn't analyze them all and we wanted the answer. We had to meet in Geneva—that was before e-mail—before we could agree on the final draft. I've just been involved with six of my colleagues in various parts of the university, not in science, on some project we have. We had to go through six or eight drafts by e-mail, and we did it in one day because it's urgent. But in those days, I flew to Geneva. That's when I quit. I decided the ratio of intellectual activity and pleasure to administrative garbage got too small.

Vettel: So there is a benefit to the reductionist approach?

Glaser: It's the only approach.

Vettel: It's the only approach. But the way science goes about it—I like your word, an army. In some cases, it really is the equivalent of an army. I've heard, I don't know who it was, maybe Kennan, equate an army to a dinosaur—large, brain the size of a pea, but once it gets moving it doesn't stop. So this big science, is there a place for a maverick in there? Or what does good science look like in big science? Can big science produce good science?

Glaser: I hate to call this group a dinosaur in the sense that they're all extremely intelligent, highly trained, and I imagine individualistic people. I think I would more describe them as members of a voluntary team, like a football team, in which they're trained, they have rules, and they work together. Every guy has to do his part or it doesn't work. I think that's more the way these big teams are.

Vettel: But when you have a three-year grant, halfway through, science might have gone off in another direction.

Glaser: Yeah, inertia is a problem in big systems. It's true, administratively. I worry about that, but I'm glad I don't have to really worry about it. For example, how does a guy get promoted at a university if he's a member of this team? And the answer is, unfortunately, it depends a lot on his seniors in the team writing recommendations to his chairman. I don't understand it. I'm glad I'm not involved in it. It's not an accident that I'm not involved. I was the head of the team. I didn't like it that way either. So I don't understand it. I think that I'm an old fogey in that sense. I was brought up to admire Einstein and all kinds of individuals who through individual effort and unique ideas and philosophy and whatever, talents—and composers, artists, writers—the people that I admired most were those who did incredible creative acts, not those who ran General Motors, although I'm coming to admire that there are guys who can do that.

Vettel: Right, that there is creativity in business as well.

Glaser: There are people who can be creative in business too. It's not trivial.

Vettel: But now it's teams. It's the Human Genome Project or CERN. It's not the individual.

Glaser: Yeah. I don't know. I think it's the nature of science. I think what's going to happen, the reality is that young scientists need jobs, and that's where the jobs are. So I think that there will be sociological adjustment somehow, and that the idea of personal heroism will remain. But added to it will be the values of the so-called "team player," which is well known in politics and in business. Those who have a maverick personality will go into other fields. Those that can make that adjustment and be happy with it are going to be the ones that are productive in these massive fields of science. It's not that high energy physics has become less interesting. It's really exciting. I miss the participation and the scientific excitement, but it's not worth the price. I'd just as soon read about it.

Vettel: Let's just focus on physics for a moment. What could physics do to promote or make space for the maverick? Just one simple solution.

Glaser: I used to be on the Science Policy Board, whatever it's called, of NSF [National Science Foundation], for physics. I worried that they had a rule that any of the referees, if there were five, who were against a project killed it. It didn't get funded. I said, "That's ridiculous. No company and no serious scientific research lab can survive if it doesn't take chances. Typically ten percent of the income of a company has to go in research and development, unless it's making safety pins, but if it's really technically based—And that we at the NSF ought to pick some fraction, like ten percent of our budget, to go into iffy—not crazy stuff, but somewhat crazy stuff." And they did.

I don't know if they still do now, but it was getting to be enormously conservative. To me, one of the solutions is to put aside—It occurred to me that one quantitative way to do that is if there were five referees and one of them objected, then ten percent of those ought to go through. If two object, one percent ought to go through. And if three object, it's probably crazy.

Vettel: NSF could have its own formula.

Glaser: It would be a formula which measures degree of craziness in some way. I don't blame the administrators. They've got a big responsibility. If they can point to some kind of—But then of course, the referees will start gaming the system. Anyway, I feel very strongly that you've raised an important question. There's got to be room for mavericks. You've got to measure degrees of craziness. And you've got to do things which are —Well, for example, the CEO of Cetus tried to kill PCR. We fought it like hell, and it's good that we did. If we had lost, okay, you waste a few million bucks. But that's not a big loss.

Vettel: Not only tolerate a little bit of craziness, but also have a little bit of patience too. You don't know what's right around the corner, to use your phrase, so follow the craziness and wait for a bit so it has a chance to play out.

Glaser: There's a famous remark attributed to Niels Bohr that some post doc or student or something came to him and said, "I've been working on the problem you gave me, and I have this really crazy idea. I don't know. It's probably too foolish to consider, but are you willing to listen to it?" And Bohr said, "Yeah. Let's see if it's crazy enough." To me, that's beautiful.

Vettel: You're right. Each transition that you made, was it crazy enough?

Glaser: Yeah, in the sense that I was happier after I made the transition than before. I was productive at some reasonable level, although some of my friends and critics think that I've been irresponsible, that I was really good at physics and I was contributing to that and I shouldn't have quit. So there's a hedonistic view and there's a social responsibility view, which I don't take that seriously.

Vettel: Looking back in retrospect, what do you hang your hat on? What do you feel that you've contributed to science? What would you say is your greatest accomplishments? I don't like that question myself because it's too loaded. How about, what accomplishments do you find comforting?

Glaser: It's simple. You can just look at the list of my publications. That covers the things that were worth formally announcing, and the accomplishments of my students, the starting of the biotech industry—but it would have started anyway. Nearly everything I did would have started anyway. In fact, people have said that Einstein was only about five years ahead with relativity, that other people were getting close, or maybe even three years. I think that the present environment, when there's so many well-trained, smart people in science, that if you're a year or two ahead of everybody else, that you're doing something original. If you're twenty years ahead, you don't have a chance.

Vettel: [Laughs] You're crazy.

Glaser: You're crazy. That's happened to me. I'm trying to think of a particular example. Yes, in building this big thing I called the dumbwaiter, this big automated machine, now they're trying to build it for bioterrorism. They're reinventing things. I hear at LBNL [Lawrence Berkeley National Laboratory] they've got a little [robotic] hand. Exactly the same as mine – I had a little hand. But nobody comes around to ask me. That's okay though.

Vettel: You would say publications, the students you've taught, perhaps your venture in industry?

Glaser: Those are the things that are publicly clear. I'm very proud of what I'm doing now. Every week or two, I get a bright idea that I think is clever, but a lot of them are wrong. I constantly am doing a recap. Typically, it'll be forty pages of writing. I look at it a week later and I'll boil it down to ten pages. Then finally, I'll put it on the computer, and then finally I'll get one of my guys to write a program. I'm having small pleasures through small successes. A success to me is to think of some way which is simple enough that I think the brain might be able to do it, and yet subtle enough that it'll be useful. Like this business of an expanding wave picking up the billiard balls in the retinotopic order, the mapping order. Such a simple idea, but I happened to have the thing in my head of the expanding wave because that's what my model led me to.

Vettel: What have you learned professionally that you would like more people to know?

Glaser: I think the main thing that concerns me now is politics. The most important thing is to distinguish between slogans and real, important facts that go into policy. I think that we're simply being drowned in meaningless slogans of all kinds, and that those who are trained in science have learned to make that distinction, but that the media are playing on fuzzing that distinction. I think it's extremely dangerous. I think that when we have really bad times people are forced to say, "The reality is this, and these are the things that I have to think about. Don't give me your slogans." But when we're well off, then we use slogans to sell toothpaste and everything else. The people who use slogans, now they're running our government. It's totally irrelevant to what we need to talk about.

Vettel: Perhaps it is relevant. I think the comparison with big science, but also with physics and what you've learned and how you approach it, I think it's very relevant.

Glaser: But the general idea that when things are tough, clear thinking is really important, and the trend for politicians is to muddy the waters whenever it's to their advantage. I don't know whether I mentioned earlier that I was negotiating with Russians over in Pugwash. Do you know about Pugwash? Pugwash is a town in Canada where there was a meeting of some scientists saying, "We've got to do something about the problems in the world. Let's consider things that have to do with avoiding a major war." Many countries signed up, including the Soviets. We would go to these meetings and discuss critical world affairs for which science had a major role to play. Of course, the Russians were always government operatives, and we were always deliberate to show that we had nothing to do with our government. Paid our own way, no briefing. They debriefed us, you know, "What did you learn?" But no instructions from anybody.

We were negotiating with the Russians about biological warfare, chemical warfare, and nuclear warfare and how we can avoid these things. It was all physicists at that time. Since then, more biologists have joined. We respected each other because we knew each other's papers and we knew each other as scientists. We played it like a chess game, saying, "Okay. You guys try to cheat, and we catch you." "Okay, that doesn't work. You do this, you do that." We played games, and we succeeded in finding a procedure and an inspection protocol in which neither side could beat up on the other one. On four other subjects, we couldn't find such a scheme. At that moment, Ralph Bunche wandered in. He was our representative to the UN. We told him that we agreed on three and couldn't agree on four, what do we do? He said, "Diplomats do the same thing all the time. You write a glowing report how you found this wonderful agreement and solution to the problem for these three problems, and you make the rest of the report as opaque as you can." So that's—

Vettel: That's your lesson.

Glaser: Anyway, the lesson that I draw is that people who have had scientific training or training in some other discipline, profession, law—there are a lot of others—are much more critical of what the politicians are saying, which is leading our country into a disaster, in my opinion.

Vettel: Well, thank you very much, Dr. Glaser. Personally, I've enjoyed this, and I do appreciate this very much.

[End of Interview]

SALLY SMITH HUGHES

Sally Smith Hughes is a historian of science at ROHO whose research focuses on the recent history of bioscience. She began work in oral history at the Bancroft Library in 1978 and joined ROHO in 1980. She has conducted interviews for over 100 oral histories, whose subjects range from the AIDS epidemic to medical physics. Her focus for the past decade has been on the biotechnology industry in northern California. She is the author of *The Virus: A History of the Concept* and an article in *Isis*, the journal of the History of Science Society, on the commercialization of molecular biology.