History of RSB Interview: Thomas A. Weber

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PC: Good morning, Professor Weber. Thank you very much for joining me. As we've discussed ahead of this interview, the theme of this series is the history of replica symmetry breaking in physics, which we roughly bound from 1975 to 1995. You've worked on structural glasses, so this is the work more germane to these ideas that we're going to be discussing. But before we get into that, I'd like to ask you a few questions on background, so that we can situate your contributions. Can you tell us a bit about your family and your studies before starting university?

TW: [0:00:37] I was an only child. I grew up in Ohio with a lot of relatives. It was a normal childhood. What can I say?

PC: Where did your interest in science emerge from?

TW: [0:00:56] I was always interested in science. I was pretty much a loner. I would do experiments in my basement. I had a fantastic chemistry teacher, and she would stay after school at night and let me run experiments. I calculated Avogadro's number, came home smelling like rotten butter. My mother was like: "What did you get into?" I was like: "Oh, I just did a science experiment." It was a basically growing up in a small town in Midwest Ohio.

PC: What led you to pursue chemistry studies at the University of Notre-Dame from there?

TW: [0:01:55]. Like I said, I was originally interested in biology, and I had this great chemistry teacher, so I decided to do that as a major. I guess my freshman year at Notre-Dame, I was: "Well, I really want to want to convert to mathematics." At that time, at Notre-Dame, for every freshman – if you had declared a major – they basically put you up with a professor. That Professor John Magee[1] said: "Would you like to have a job at the Radiation Laboratory on campus?" It was a facility funded by the Atomic Energy Commission[2] at the time.[3] So, I started working there at the princely sum of a $1.25 an hour and got interested in computers at the time. Basically, what happened is that although my interest in wet chemistry at that point declined, my interest in computers increased. So, I started doing calculations and stuff like that. That kept me as a chemistry major. At the time, I took every graduate course in physical chemistry that the university offered.

PC: What sort of computational resources were available to you at that time?

TW: [0:03:33] At that time, it was in the early '60s – it was 1963 – we had a Univac 1107.[4] It was one of the supercomputers of that era. It filled a whole room. I had a friend who was working at the computing center. He had keys to get into the building, and into the computer. We used to go in sometimes after dinner to play with the computing equipment they had there at the time.

PC: You had gone to Notre-Dame instead of, say, Ohio State. What was the draw? Was it a natural school for you to go to?

TW: [0:04:32] My father was a Notre-Dame football fan. My father never graduated from high school, but before he died, he had gone to over 400 football games. At the time, when I went to Notre-Dame, it was all male. I was like: "Oh! I really want to transfer to Ohio State." My father looked at me and he said: "Oh you're quitting college?" I said: "No, dad, I want to go to Ohio State." We said this back and forth a couple times. Then, he looked at me with this sad look in his eye, and he said: "Well, okay Tom, if you want to pay for it yourself." So, I stayed at Notre-Dame. Basically, I had many choices to go to college as long as they were all Notre-Dame.

I was smart enough that I got early admission to Notre-Dame, so it was kind of fated that I would go there. I got a good education. At the time, the professors there really took an interest in the students. It's not like the environment today, where they're all interested in doing research and stuff like that. It was more of a teaching atmosphere at that time,

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[1] John Magee: https://en.wikipedia.org/wiki/John_L._Magee_(chemist)
[2] US Atomic Energy Commission: https://en.wikipedia.org/wiki/United_States_Atomic_Energy_Commission
[3] See, *e.g.*, R. H. Schuler, "Radiation chemistry at Notre Dame 1943–1994," *Radiation Physics and Chemistry* **47**, 9-17 (1996). https://doi.org/10.1016/0969-806X(95)00072-6
[4] UNIVAC 1107: https://en.wikipedia.org/wiki/UNIVAC_1100/2200_series#1107

and it was a great fit. It was isolated, and there was nothing to do there except study. I was too young to drink. The Notre-Dame campus is actually its own little city and apart from everything else.

PC: From these, you went to pursue a PhD at Johns Hopkins.[5] What interested you there?

TW: [0:06:42] It was my research advisor. I was interested in quantum mechanics. At the time, Professor Parr[6] was there. It was one of the two locations. That's why I went to Hopkins. He later went as a professor at University of North Carolina. If he had been at North Carolina, I would have gone to North Carolina. I applied to graduate school at the University of Chicago – because they had a great quantum chemistry group there[7] – and Hopkins. I got in both of them and decided that I wanted to go to Hopkins. But I basically went there because of the professor that I had wanted to work with.

PC: So, you knew of his work from papers, or you had met him at a conference?

TW: [0:07:45] I knew his work from research papers that he had published.

PC: Were the computational resources vastly different from what you had at Notre-Dame or were they roughly the same?

TW: [0:08:05] They basically had less computational power. They had an IBM computer. It wasn't quite as powerful as a 1107, but they had adequate computations for the calculations that I wanted to do.

PC: After completing your PhD, you went to work at Bell labs. What drew you to a research lab instead of an academic position?

TW: [0:08:40] It was the only offer I had. At that time, I had a colleague who didn't get his PhD in four years. (I got my PhD in four years.) He ended up spending 10 years *postdoc'ing* because jobs were really far and few between at that point, in 1970. There had been a huge hiring of students in academia at that time and then, all of a sudden, all the positions were filled, and it was almost impossible to get an academic job. So, I applied to a couple of different universities, got no offers because nobody was

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[5] Thomas Andrew Weber, *Three studies in Hartree-Fock theory*, PhD Thesis, The Johns Hopkins University (1970). https://catalyst.library.jhu.edu/permalink/01JHU_INST/1lu78g9/alma991002612829707861

[6] Robert Parr: https://en.wikipedia.org/wiki/Robert_Parr

[7] For instance, Robert S. Mulliken: https://en.wikipedia.org/wiki/Robert_S._Mulliken

hiring, and Bell Labs offered me a position. In retrospect, it was probably the best thing I could possibly have done because I tend to have research ADHD. I don't work in any one area for any great length of time. Bell Labs was the perfect environment for that. We had a saying when they did the reviews every year. It was: "What do you do for us this year?" It was not: "What you did for us last year?" So, I was able to move from subject to subject: work on silicon, work on glasses, work on air pollution studies. I think it shows in one of the papers I wrote. I wrote the "[Molecular dynamics] simulation of polyethylene. 1. Structure"[8]. I had planned on writing another paper on dynamics. I never got to it because I got interested in glasses and started working on those calculations. For the things that interested me, Bell Labs was the perfect environment for that.

PC: Can you describe a bit the environment in the theoretical chemistry group at that point? How free were you to choose your research projects when you joined?

TW: [0:11:24] At Bell Labs, you chose your research projects. I was asked to do one thing by management at Bell Labs, that was the air pollution studies that Tom Graedel, Leonida Farrow and I did.[9] Other than that, it was a totally free environment. When you were doing research, you picked what you wanted to work on, and you just went and worked on that. At the end of the year, they would evaluate you and say yay or nay, how much of raise you got, and things like that. It was a very free environment at the time. Also, there were very few workers there that had postdocs, so mostly we relied on our colleagues. We worked with our colleagues because we didn't have postdocs to assist in the work.

PC: Who were the senior theoretical chemists in the group at that point, in the early '70s?

TW: [0:12:52] Frank Stillinger,[10] Gene Helfand, and John Tully,[11] they had all been hired about five or ten years before me. I don't really think of it as seniors in a sense. We were all on an equal footing. If somebody came to me, for example, and said: "Would you like to do a study on this sort of thing, or that sort of thing?", we'd say yay or nay and start doing the

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[8] T. A. Weber and E. Helfand, "Molecular dynamics simulation of polymers. I. Structure," *J. Chem. Phys.* **71**, 4760-4762 (1979). https://doi.org/10.1063/1.438263

[9] See, *e.g.*, T. E. Graedel, L. A. Farrow and T. A. Weber, "Kinetic studies of the photochemistry of the urban troposphere," *Atmospheric Environment (1967)* **10**, 1095-1116 (1976). https://doi.org/10.1016/0004-6981(76)90120-7; "Photochemistry of the" Sunday Effect"," *Environ. Sci. Technol.* **11**, 690-694 (1977). https://doi.org/10.1021/es60130a005

[10] Frank Stillinger: https://en.wikipedia.org/wiki/Frank_Stillinger

[11] John C. Tully: https://en.wikipedia.org/wiki/John_C._Tully

calculations. I was actually the one who knew how to get the most out of the computers.

PC: How did you get to know and interact with Dr. Stillinger, in particular?

TW: [0:13:49] I'm not sure. He might have been the department head at the time. We were in the same department basically.

PC: Would you see each other every day? Would you have shared offices? I don't know what was the geography of Bell Labs?

TW: [0:14:05] We all had private offices pretty much. Basically, we'd go to lunch together, a bunch of us, and talk about various and sundry areas. Bell Labs – at least at Murray Hills – was very isolated. If you wanted to go out to lunch, you'd have to get in a car and drive five or ten minutes to a restaurant. So, Bell Labs had its own cafeteria. They had long, long tables. People would just sit with physicists, chemists, mathematicians, engineers, and talk about various and sundry things. One of the reasons I was able to do many of the simulations I did was that I was sitting at the table complaining about the software library in the computer center because it was so slow, and I couldn't really integrate the equations of motion. Linda Kaufman was there, and she said: "Oh, I got this great software package to help you integrate. I'll send it to you. They didn't want to put it in the library but it's really fast." That's what enabled me to do a lot of the calculations I was doing on the Cray computer at the time.[12] We actually had to pay for our usage of computer time.

PC: Were the computational resources markedly different from what you'd had at Johns Hopkins?

TW: [0:15:55] I'm never quite sure how we paid for computation at Hopkins. They had a computer. Professor Parr had I don't how many hours of allocation of computer time, and we just used it. At Bell Labs, they tried to make the computer center not a profit center but a break-even center. We had to pay so much per hour. Actually, after midnight, the cost went down to 10%, so I would wait up until 12:01a and submit my programs over the internet to be the first in the queue and to get my work done.

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[12] See, *e.g.*, F. H. Stillinger and T. A. Weber, "Hidden structure in liquids," *Phys. Rev. A* **25**, 978 (1982). https://doi.org/10.1103/PhysRevA.25.978 The article thanks "Linda Kaufman of Bell Telephone Laboratories for suggesting the 'conjugate gradient method' which leads to drastic improvement in convergence rate for the quench procedure, and for providing the necessary computer software."

PC: In the late ‘70s, you became interested in inherent structures –structures obtained through rapid quench – of the Gaussian core model and, in particular, of the amorphous structures that were ensuing.[13] How did this question arise?

TW: [0:17:14] It arose, because I was… These structures also all have inherent crystal structures. What I was really trying to do is a crystallization of the dynamics that had occurred. So, I started quenching these things. That led to the interest of: “Oh, hey, we don't get the crystal structures forming or even crystallites, or anything like that. We get these amorphous structures.” So, we started looking at those, looking at nearest neighbors and things of that sort, and realized that these things were more like liquids than crystals. We realized that they weren't moving, so that they were more like glasses. That's where all that work began. We just became fascinated with all the structures that were occurring. We called these hidden structures in materials. Every time we did a simulation of anything, after that we would look at the hidden structures that were underlying where we were right now.

PC: How closely were you following the work of Rahman, Mendel, and McTague,[14] who worked on very similar issues at about the same time?

TW: [0:19:20] I wasn't, but I'm sure Frank was at the time, because Frank had done some of the initial pioneering work with Anees Rahman.[15]

PC: So, you were not in touch with these groups?

TW: [0:19:21] No, I was not. I personally was not in touch with these groups.

PC: If I understood correctly from your earlier comments, you were the computer specialist. You knew how to get this machine to work and be efficient. Is that correct?

TW: [0:19:33] That's correct. One of our classic calculations – it's now called the Stillinger—Weber potential.[16] It's really interesting because

[13] F. H. Stillinger and T. A. Weber, "Study of melting and freezing in the Gaussian core model by molecular dynamics simulation," *J. Chem. Phys.* **68**, 3837-3844 (1978). https://doi.org/10.1063/1.436191; "Amorphous state studies with the Gaussian core model." *J. Chem. Phys.* **70**, 4879-4883 (1979). https://doi.org/10.1063/1.437365

[14] See, *e.g.*, A. Rahman, M. J. Mandell and J. P. McTague, "Molecular dynamics study of an amorphous Lennard-Jones system at low temperature," *J. Chem. Phys.* **64**, 1564-1568 (1976). https://doi.org/10.1063/1.432380

[15] See, *e.g.*, A. Rahman and F. H. Stillinger, "Molecular dynamics study of liquid water," *J. Chem. Phys.* **55**, 3336-3359 (1971). https://doi.org/10.1063/1.1676585

somebody had come up with a similar form for that three-body potential many years before,[17] but they never were able to do calculations because they didn't have the computer speed. Also, they didn't have the cleverness to figure out…

You can either do a brute-force calculation or you can figure out how to speed up the calculation. That was one of the things that, since I realized I was paying real money for these calculations, I wanted to make them was fast as possible. That's where Linda Kaufman's computational algorithms came into play. I used those and those helped do calculations in real time. If you recall at the time, people, when they did simulations, they were… Martin Karplus,[18] I used to go to symposium and things and hear him talk about the calculations that he was doing, and they were like nanoseconds. That was the way calculations were done at the time. When we got the Cray-1[19] computer at Bell Labs, we had computational power which was far superior to almost any university at the time. We just went for it. I remember I became very popular when I'd go to conferences, because everybody would want to try to see if they could work with me, so we could use the Bell Labs computer to do whatever. They didn't have the national supercomputing center, so academics were pretty limited in their computational power.

PC: This effort on inherent structures eventually led to the description of a landscape of glasses in a *Science* paper "Packing structures and transitions in liquids and solids" which you published in 1984.[20] How did this landscape picture emerge from your studies? Was this a new idea at the time? What were the sources of inspiration for drawing it?

TW: [0:22:28] I think it's fair to say – maybe Frank would contradict me on this – that the structures were very interesting. They were something we just discovered when we went and did the simulations. At the time, I also had another effort to come up with some visualization software. I used to make 3d pictures of the structures and what was there. It was kind, in a sense, like a little kid looking at the landscape and say: "Oh, wow! Isn't that interesting?" I didn't have any preconceived notion of what we were

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[16] F. H. Stillinger and T. A. Weber, "Computer simulation of local order in condensed phases of silicon," *Phys. Rev. B* **31**, 5262 (1985). https://doi.org/10.1103/PhysRevB.31.5262
[17] See, *e.g.*, P. N. Keating, "Effect of invariance requirements on the elastic strain energy of crystals with application to the diamond structure," *Phys. Rev.* **145**, 637 (1966). https://doi.org/10.1103/PhysRev.145.637
[18] Martin Karplus: https://en.wikipedia.org/wiki/Martin_Karplus
[19] Cray-1: https://en.wikipedia.org/wiki/Cray-1
[20] F. H. Stillinger and T. A. Weber, "Packing structures and transitions in liquids and solids," *Science* **225**, 983-989 (1984). https://doi.org/10.1126/science.225.4666.983

going to find. We just did the quenchings, and that's what we found, and that's what we published.

PC: How was this particular work received at the time?

TW: [0:23:42] I have no idea. In ’87, I basically left Bell Labs and went to the National Science Foundation. I still did some research with Frank for a couple of years when I was there,[21] but eventually I decided: “I'm just playing. I'm not really serious about this, even doing that stuff.” So, I got out of the field. My career just went in a totally different direction.

You know, I got out of Bell Labs at a good time. I had very fine memories. Frank, a National Academy of Sciences member, eventually got fired by Bell Labs, because they were divesting of all the fundamental research that they did. They no longer did research, basically. This was the result of some court decisions when the federal government said: “AT&T is a monopoly and we're going to break it all up.” When it was a monopoly, AT&T and the telephone companies spent a certain percentage of their profits every year on funding research. A certain percentage of the research that they funded was geared to integrated circuit designs and things like that, but a good percentage of it was just fundamental research. Numerous Nobel prizes came out of Bell Labs. It was just an environment where they put together a bunch of smart people and said: “Do your research; see what happens.” And they let us have our head to do whatever we wanted to do. You won’t find that in any research lab today.

PC: I'd like to take you back to 1984, when these pictures came out. One person at Bell Labs who had worked on amorphous structures and excitations in amorphous structures is Phil Anderson.[22] Were you at any point in contact with him or discussed with him?

TW: [0:26:11] Not really. Phil only talked to Area 10 people. There was a hierarchy at Bell Labs. Area 10 was the pure physicists and Phil Anderson was one of those. We were in Area 20; we were sort of a lesser grade because we were chemists and physicists but mostly chemists in our

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[21] See, *e.g.*, F. H. Stillinger and T. A. Weber, “Fluorination of the dimerized Si (100) surface studied by molecular-dynamics simulation,” *Phys. Rev. Lett.* **62**, 2144 (1989). https://doi.org/10.1103/PhysRevLett.62.2144; T. A. Weber and F. H. Stillinger, "Melting of square crystals in two dimensions," *Phys. Rev. E* **48**, 4351 (1993). https://doi.org/10.1103/PhysRevE.48.4351

[22] See, *e.g.*, P. W. Anderson, B. I. Halperin and C. M. Varma, “Anomalous low-temperature thermal properties of glasses and spin glasses,” *Philo. Mag.* **25**, 1-9 (1972). https://doi.org/10.1080/14786437208229210

area. So, there was that hierarchy, and [we] didn't really associate with Anderson's group.

PC: Can you describe the glass theory and simulation community at the time? Were you participating in discussions or workshops on this theme?

TW: [0:27:11] I went to a Gordon conference and gave the keynote speech at the time.[23] Like I said, I had research ADD. So, I would go to meetings, and everybody would know everybody because they had been working in the same area, doing the same thing for 10 or 20 years, and all of a sudden, I'd be there, almost as an interloper. I went to polymer meetings; I went to Liquid's Gordon conferences. I became a fellow of the American Physical Society because of my work in polymers. I did that for one or two years, and then got into other areas. Like I said, I had research ADD. I was sort of a dilettante. I went from one area of research to another. The link was computation on high-speed computers. That was the thing that spurred my interest. Could I do a calculation? We did the first seminal calculation on air pollution, looking at the troposphere in New Jersey and figuring out that even if you took out all human sources of pollution in New Jersey you'd still have a problem because of the air mass that was coming over the forests of Pennsylvania which emitted terpenes in the hot summer heat, which eventually produced ozone in chemical reactions. I went from one area to another. I didn't stay. If I had been in a university, I probably would have never been able to get a research grant, because I never worked in one area long enough [for others] to say: "Oh, he knows what he's doing." I just went into an area, started doing research, got a couple of calculation results, and if my interest went to another area, I did something else. The Bell Labs environment was an environment that allowed you to do that because they were basically paying me to do research, and I picked the area of research.

PC: You nevertheless kept working on inherent structures for a bit longer. You notably looked at the hard sphere limit of inherent structures, trying to approximate…[24]

TW: [0:30:18] I never got into in that area. It started with the potentials that we were using, the Hulthen potential,[25] the Stillinger—Weber for silicon, that led to other things. But we basically started with the potentials and did the quenching. Every time I did a calculation, I'd save a few of the

[23] Conference details missing.

[24] F. H. Stillinger and T. A. Weber, "Inherent structure theory of liquids in the hard-sphere limit," *J. Chem. Phys.* **83**, 4767-4775 (1985). https://doi.org/10.1063/1.449840

[25] R. Roychoudhury, "Hulthen potential" *Encyclopedia of Mathematics* (2020). https://encyclopediaofmath.org/wiki/Hulthen_potential (Consulted October 24, 2024.)

results, quench them, and see what happened. We weren't doing, at least I wasn't doing, the fundamental theory behind all this. I was just looking at these fascinating structures when we quenched them on a computer. It was a discovery sort of thing.

PC: You proposed the lattice model for glasses also at about that time.[26] How did this idea come about?

TW: [0:31:25] That was Frank. That was basically Frank's idea, and we basically went from there to use our results to justify the modeling that we did. Frank was more… He had a knack for coming up with potentials, but he worked on a desk calculator, basically. He was not a computer person. A lot of people thought he was a computer person because a lot of computations are ascribed to his work, but it was always somebody else who knew how to run the computers. He was not able to submit a computation job if his life depended on it. It just wasn't his interest; it wasn't his skills. That's why it was a great collaboration, because that's where my interests were.

PC: Glenn Fredrikson and Hans Anderson proposed an idea similar to your lattice model at roughly the same time.[27] Do you remember becoming aware of this?

TW: [0:32:54] Glenn was working at Bell Labs at the time, as I recall. That's probably where that came from. It's hard to say. Bell Labs was such a collaborative environment at the time. People worked with one another, talked about research with one or another. I always say the great secret of Bell Labs was that they hired smart people and gave them the head to let them do whatever they wanted to do. They all ate lunch together, and we discussed stuff over lunch. The lunch hour was often more than an hour, because people would get in scientific discussions and start writing equations down on napkins and stuff like that. It was a very congenial environment. Whenever I would visit a university and would go to the faculty club… The faculty clubs typically had these lunch tables that would seat four, maybe six people at most. Bell Labs had these huge [tables]. They were like grammar school cafeterias, these huge tables. Maybe two or three would have been put together in a row and people would just sit down and put themselves in any open space, and we'd start talking to

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[26] See, *e.g.*, F. H. Stillinger and T. A. Weber, "Tiling, prime numbers, and the glass transition," Ann. N. Y. Acad. Sci. **484**, 1-12 (1986). https://doi.org/10.1111/j.1749-6632.1986.tb49557.x; T. A. Weber, G. H. Fredrickson and F. H. Stillinger, "Relaxation behavior in a tiling model for glasses," *Phys. Rev. B* **34**, 7641 (1986). https://doi.org/10.1103/PhysRevB.34.7641

[27] G. H. Fredrickson and H. C. Andersen, "Facilitated kinetic Ising models and the glass transition," J. Chem. Phys. **83**, 5822-5831 (1985). https://doi.org/10.1063/1.449662

whoever was next to us, whether it was a physicist, or a chemist, or a mathematician. You just found a place to sit, and you started talking to whoever was there. What can I say? It was a great environment at the time, where a lot of work and ideas were discussed. We used to have all these interesting discussions. People who came to Bell Labs to give talks were really horrified, because if somebody gave a talk, somebody would interrupt them. I remember one talk, [a physicist] – it wasn't Phil Anderson, but it was another one of the physicists who was there – kept interrupting this poor guy who was trying to give a talk. He said: "If you would stop interrupting me, I'll tell you because in three slides from now, I'm talking about this situation." The physicist shot back: "Well, if you don't talk to me right now, I'm leaving, so you're not gonna get to the third slide." It was just an environment where people… I remember John Tully had a picture on his door of a bunch of alligators, and he put a caption underneath it: "The Bell Labs' audience waiting for the seminar speakers." The alligators all had their mouths open. We were a rough crew at the time, but people weren't afraid to put forward an idea among their colleagues. Sometimes, the colleagues said: "That's a bunch of shit. You don't know what you're talking about." Other times, it would lead to something very productive. It was a kind of a no-holds-barred environment. It was great!

PC: As you mentioned earlier, in 1987 you left Bell Labs for NSF and became a program manager. Did you ever get the chance to support research work on glasses or the glass transition theory or simulations at NSF?

TW: [0:37:15] Yes! I supported a lot of that work. I first went as a program director in chemistry, and so I was supporting computations in the area. My second job, I became the division director for the advanced scientific computing program at National Science Foundation. I was running the five national supercomputer centers at the time. So, yeah, I supported… Then, one of my later jobs, I was in charge of materials research and supported a lot of work on polymers, glasses. That particular department was a blend of chemists, physicists, and engineers. It was basically the materials department where I had found my niche in science. It was sort of interesting, because for years I thought of myself as a chemist, but later I said I'm really a material scientist, because that was where all my interests… My computing interest and my interests at Bell Labs were in the area of materials. It was a transition, an evolution so to speak.

PC: We're approaching the end of the interview. Is there anything else you like to share with us about this era that we may have missed or overlooked?

TW: [0:38:57] No. I think I've really told you [about] the environment. The key was that the Bell Labs environment was one where you could do things like this. You didn't have to write a research grant to the National Science Foundation and say: "Oh! I want to work in this area." One of the problems that I saw with the federal funding of science was that when they sent it out to reviewers, reviewers would come back with: "Why should we fund this guy working in this area of science? He's never worked in this area previously." Guess what? Most of my work, I would have gotten that response if I had tried to get a federal funding agency to support it, because they would have said: "Well, he's never worked in glasses, he's never worked in silicon, he's never worked in air pollution, he's never worked in polymers." But at Bell Labs these were assets rather than marks against you.

PC: Finally, do you still have notes, papers, or correspondence from that epoch? If yes, do you have a plan to deposit them in an academic archive at some point?

TW: [0:40:40] I have no idea. I probably saved them somewhere. I have no idea where they would be, because after various and sundry moves I got rid of a lot of the stuff. I have no plans on… It's only recently that historians have come and asked me questions about any of this stuff. I had an interview with a guy that's publishing about the silicon work. He asked me a couple questions about a year ago. All of a sudden, I realized I'm one of the old fossils in science, when historians want to know about anything. I never quite thought of myself that way, but that's the way it is, I guess. To quote a famous author: "It was the best of times, it was the worst of times, it was the time that tries men's souls." Actually, that's the quote the beginning of my PhD thesis. I always thought that line from *A Tale of Two Cities*[28] was really great. I lucked out. The only job offer I got was at Bell Labs, and it was the perfect environment for me. It allowed me to flip from one area of science to another, just discovering things and doing things. I'm really happy I was there at the time. Bell Labs as I knew it does not exist, which is unfortunate, but that's the way of life. I think the United States is worse for the fact that research labs like that don't exist anymore. But they don't. That's another reality. Unfortunately, I'm not sure the environment for funding produces the equivalent of these kinds of research labs, where somebody can just go and work from area to area and bring new ideas. I think that's the thing. When we started doing a lot of the research at Bell Labs, we were able to

[28] A Tale of Two Cities: https://en.wikipedia.org/wiki/A_Tale_of_Two_Cities. The third part of the quote is in fact from derived Thomas Paine's *The American Crisis*: https://en.wikipedia.org/wiki/The_American_Crisis

discover things and bring up new ways of looking at things in areas of science that hadn't been looked at before. It was a great environment, and I was really lucky to be part of it.

PC: Dr. Weber, thank you very much for your time.

TW: [0:43:55] Okay!