History of RSB Interview: Andrew T. Ogielski

May 6, 2021, 10am-11:30am (EDT). Final revision: September 1, 2021

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Francesco Zamponi, ENS-Paris
Location:
Over Zoom, from Prof. Ogielski's home in Hanover, New Hampshire, USA.
How to cite:
P. Charbonneau, *History of RSB Interview: Andrew T. Ogielski*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École Normale Supérieure, Paris, 2021, 23 p.
https://doi.org/10.34847/nkl.86f6z55x

PC: Good morning, Dr. Ogielski. Thank you very much for joining us. As we've discussed ahead of this interview, the main purpose of our conversation is to talk about the period during which replica symmetry breaking was formulated and understood, which is roughly from 1975 to 1995. Before we get to that, I'd like to ask you a few questions on background to help situate your contributions. How did you first get interested in physics and then to pursue a research career in theoretical physics and quantum field theory, more specifically?

AO: It's easy to answer. I think I always liked science, and I always found things that I was learning in high school a little too easy. When I went to university and started studying physics—at first classical physics, which we studied in those days—it was also very easy and not challenging, until I hit quantum mechanics. Quantum mechanics became very difficult. At this time I took to reading everything I could and solving all problems I could solve to get a grasp of what it's all about. Then I naturally gravitated towards quantum physics, because that's what the majority of physicists at my university were doing. I started there as a postdoc, eventually immigrating to the United States. Quantum field theory was the dominant subject of studies, and I think it still is, at the University of Wrocław.

The nice thing about this place is that this was the hometown and the home university of Max Born[1]. Now, the Institute of Theoretical Physics is

[1] Max Born: https://en.wikipedia.org/wiki/Max_Born

called the Max Born Institute[2]. Born is one of my all-time favorite physicist, with his enormous range of interests.

Then, I travelled throughout Europe, where I held various visiting professorships and similar short-term positions until I got recruited by Bell Laboratories, in 1982.

PC: I wanted to talk to you about your peregrination before joining Bell Lab. Shortly after your PhD, you joined the Institute of Theoretical Physics at Wrocław. It seemed that you were working pretty independently at that point and you traveled abroad quite a lot. Was that a common path? And can you tell us a bit what drove your selection of research problems at that point?

AO: [0:02:55] I started with Lagrangian quantum field theory, which is what my advisor was doing[3]. The 1970s were the years of the discovery of asymptotic freedom and the renormalization group. I was very much drawn especially to Wilson's renormalization group theory and lattice field theories. I could leverage opportunities of certain grants or invitations and continue in that direction mostly in Western Europe: a year in Germany in Kaiserslautern, and earlier in France, in Dijon and several other places, and in C. N. Yang's institute in Stony Brook, NY. By the end of the '70s my primary interests were in lattice quantum field theory, renormalization group, and more and more geometric approaches to lattice field theory. I was well acquainted with the work of Sasha Polyakov[4], whom I met a number of times both in Russia and in Poland, and Sasha Migdal[5]. For a moment, I was very much interested in quantum loop theory and lattice formulations of that.

PC: How is it that you ended up at Bell Labs? What led to your recruitment there?

AO: [0:04:35] I immigrated to the United States and for a couple of months I was visiting Martin Veltman's group at the University of Michigan, Ann Arbor[6]. One day in late 1981, a recruiter from Bell Lab came for a visit.

[2] More accurately, the Institute of Theoretical Physics's address is plac Maxa Borna 9, in Wrocław. http://www.ift.uni.wroc.pl/ (Consulted on July 7, 2021.)
[3] See, *e.g.*, J. Lukierski and A. T. Ogielski, "Global scale transformations for renormalized field operators," *Phys. Lett. B* **58**, 57-60 (1975). https://doi.org/10.1016/0370-2693(75)90727-3; A. T. Ogielski, "On the dimensional symmetry rearrangement in renormalized quantum field theory," *Annales de l'IHP A* **25**, 59-65 (1976). http://www.numdam.org/item?id=AIHPA_1976__25_1_59_0 (Consulted July 8, 2021)
[4] Alexander Markovich Polyakov: https://en.wikipedia.org/wiki/Alexander_Markovich_Polyakov
[5] Alexander Arkadyevich Migdal: https://en.wikipedia.org/wiki/Alexander_Arkadyevich_Migdal
[6] Martin J. G. Veltman: https://en.wikipedia.org/wiki/Martinus_J._G._Veltman

The strategy they had was that senior scientists from Bell Labs, once or twice a year, would visit their own schools just to check with the senior professors whom they should talk to, who is available. Bell Labs operated on the principle of constant refreshment of talent. If you read Phil Anderson's book about his years at the Laboratory[7], he explains it pretty well. There was a magic concept of head count. Some percentage, which was not known to the members of the technical staff, but was known to management—which I became later, so I learned it—had to go every year and new people had to be brought in. This was a very strict mechanism. (I think it was more strict earlier, and maybe slightly less strict after Bell Labs was divided following the breakup of the Bell System.) So I met this guy. I was invited to Bell Laboratories for an interview, and I got a postdoc position, later becoming a Member of Technical Staff, which was the only research job title at Bell Labs. That's how I got there.

There were several interesting things about Bell Labs that you have to know. One was the organization numbering strategy. All departments had numbers, starting with physics. Of course, you are a theoretical physicist, so you know it's the most important science, right? So theoretical physics department was number 11111: 1 was for research; 11 was for basic research; 1111 was Physical Research**,** and the last digit identified specialized departments.

I had a second new interest. I got hooked up on software programming on a very basic level. When I was in Europe—you have to look back, it's way back in time—we were using programmable calculators to do basic symbolic calculations. Hewlett-Packard and Texas Instruments, big kind of engineering-style boxes. I was fascinated with symbolic calculations with these machines.

When I came to Bell labs, at first I began working with John Klauder[8], and my first project was to do Monte Carlo simulations to determine what possibly could be the renormalized limit of a variant of ϕ^4 scalar quantum field theory[9] that was proposed by John Klauder. (He was the editor of the Journal of Mathematical Physics at this time). I got my hands on the Cray-1 supercomputer. Today your iPhone has more computing power than a supercomputer in 1982, but then it was a fantastic machine that sort of looked futuristic: a partially-open hexadecagonal column with a ring of benches around it.

[7] Philip W. Anderson, *More and Different: Notes from a Thoughtful Curmudgeon* (Singapore: World Scientific, 2011).

[8] John R. Klauder: https://en.wikipedia.org/wiki/John_R._Klauder

[9] A. T. Ogielski, "Monte Carlo study of scale-covariant field theories," *Phys. Rev. D* **28**, 1461 (1983). https://doi.org/10.1103/PhysRevD.28.1461

I thought this project turned out pretty good, and I began to look where I should apply my newly discovered skills with computers.

At this time Phil Anderson was in the lab that I was in, in the department of theoretical physics, and also some people from Michael Fisher's group at Cornell[10]: there was David Huse and David Fisher, son of Michael Fisher, and several other people who made significant contributions in phase transitions and renormalization group theory and things related to this matter, including Pierre Hohenberg[11] and Ravi Bhatt[12]. Another part of the group worked in quantum many-body physics, hard core condensed matter theory of the Phil Anderson type, including Peter Littlewood[13], Chandra Varma[14] and Patrick Lee[15]. It was obvious to me that the biggest unanswered questions I could address were in the spin glass area, and more generally in the area of disordered magnetic systems: Ising spin glasses, random field ferromagnets, dilute antiferromagnets and several others.

Additional stimulation was that Bell Labs had this property that no matter what field you chose in physics, you could bet that there's going to be a world-class expert in one of those long hallways, and you could talk to them. There were several people doing beautiful experiments on various random magnetic materials. I eventually ended up collaborating with a really nice guy in Doug Osheroff's group[16], Laurent Lévy, who I think is now in Grenoble and who had been doing very high precision experiments[17], and with Stan Geschwind[18]

Having said that, it didn't take me very long to realize that one just could not compute anything interesting in the field of disordered magnetic systems on commercial computers because of the extremely long relaxation times exhibited in simulations.

[10] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher
[11] Pierre Hohenberg: https://en.wikipedia.org/wiki/Pierre_Hohenberg
[12] Ravindra Bhatt: https://academictree.org/physics/publications.php?pid=188110 (Consulted September 1, 2021)
[13] Peter Littlewood: https://en.wikipedia.org/wiki/Peter_Littlewood
[14] Chandra Varma: https://academictree.org/physics/peopleinfo.php?pid=395620 (Consulted September 1, 2021)
[15] Patrick A. Lee: https://en.wikipedia.org/wiki/Patrick_A._Lee
[16] Douglas Osheroff: https://en.wikipedia.org/wiki/Douglas_Osheroff
[17] L.-P. Lévy and A. T. Ogielski, "Nonlinear dynamic susceptibilities at the spin-glass transition of Ag: Mn," *Phys. Rev. Lett.* **57**, 3288 (1986). https://doi.org/10.1103/PhysRevLett.57.3288
[18] S. Geschwind, A. T. Ogielski and G. Devlin, "Activated Dynamic Scaling in Cd_{1-x} Mn_x Te: Is It a Spin Glass?" *J. Phys. Coll.* **49**, 1011 (1988). https://doi.org/10.1051/jphyscol:19888460

PC: You mentioned that Phil Anderson and others got you acquainted with spin glasses, but was that the first time you'd heard about spin glasses? Or were you already aware of them at that point?

OA: [0:11:07] It's hard to tell now. I really don't remember. Presumably I heard of spin glasses, but they were not part of my interests. I was primarily interested in Euclidean field theory on lattices. They were not random materials per se. I would honestly say that I knew about it, but I had not done anything about random materials before. On the other hand, I was very familiar with and I liked very much stochastic dynamics and stochastic processes in general. The concept of Monte Carlo simulations and ergodicity were very familiar to me. I could put two and two together quite quickly, and I quickly realized, and I was told pretty directly that if I wanted to make a career at Bell Labs I'd better take on the hardest problem there is.

At this time, nobody really knew anything about what short-range spin glasses actually do. There was the Sherrington-Kirkpatrick model, which was reasonably well understood, but its properties are highly nonphysical. There were several phenomenological papers, to the best of my memory, but the primary source of numerical information was simulations done, among others, by Kurt Binder in Germany[19]. Binder was doing a lot of simulations but they were too small size, too short timescales. So it was known that nothing was really sure of what came of those simulations. Actually, I later found that they were misleading, and their conclusions were wrong. Given these three ingredients—Phil Anderson and his colleagues who were theoretically interested in spin glasses and related phenomena, the presence of first-rate experimentalists literally next door from my office, and the necessity of doing something really challenging and interesting to survive the competition at Bell Labs—that led me to start thinking: "How on Earth could I make progress when I have as much access to a Cray supercomputer as I want, but it's not enough?" In September 1982 I wrote a technical proposal to my management on how we could overcome these problems with a special purpose computer.

Then, you see, what happened is that stars aligned. At that time, the director of the physics laboratory was Bill Brinkman[20], you may have heard his name in the context of his liquid crystal work. He was a Bell Labs oldtimer, who later went on to be a vice-president of research at Sandia Na-

[19] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Kurt Binder*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.5f2b685y

[20] William F. Brinkman: https://en.wikipedia.org/wiki/William_F._Brinkman

tional Labs. Bill arranged for me to meet Joe Condon[21]. You presumably know the name of Edward Condon[22], one of the physicists involved in the Manhattan project and in research on quantum mechanics of atoms and nuclei in the '30s and '40s. He later became a director of the National Institute of Standards and Technology, which was then called the National Bureau of Standards, of course. He gained a lot of respect because of his ethical position during the McCarthy era, and there were a lot of interesting stories about him.

Anyways, Joe Condon[, Edward's son,] was an extremely colorful character, a very generous man, and what he had was exactly what I needed. Joe Condon and Ken Thompson[23] had already built a second or third generation chess machine[24]. They were pioneering building chess-playing machines using off-the-shelf components and simple microprocessors. Don't forget that 32-bit microprocessors didn't appear until the 1980s, so their chess computers were very lean machines. It seemed that the architecture of the Bell Labs chess machine—that was called Belle—was ideal for a special purpose computer that could handle this broad class of disordered Ising systems in general—ferromagnets, antiferromagnets, spin glasses and such, dilution, no dilution. Such a machine would be ideal for applications to very large-scale Monte Carlo simulations.

I already had done enough of preliminary calculations and trials to realize that to succeed I would have to go to lattices of size maybe 64^3, which is even today very large in a simulation, and maybe even larger, and to extremely long timescales. Here, I have to be slightly numerical. (I just looked at the papers from this era yesterday.) If you take, say, a 64^3 3D lattice, you have quite a lot of spins—a quarter of a million, or something like it— and you define one Monte Carlo time step—at least, that's the way I did it—as a computing period when each spin in the lattice has the chance to change its state once. You take some path through the entire lattice and give one chance to each spin to change its state depending on the local field. That's the Monte Carlo heat bath algorithm. To get sensible physical results, where I could verify that the system was in thermal equilibrium—and this was the key to succeed—I had to go to calculations on the order of 10 to 100 million Monte Carlos steps. Each spin had to be given a chance to change its state at least 10 or 100 million times at the lower temperatures. We determined that this could be achieved in a reasonable time with a special purpose computer built with the latest pro-

[21] Joseph Henry Condon: https://en.wikipedia.org/wiki/Joseph_Henry_Condon
[22] Edward Condon: https://en.wikipedia.org/wiki/Edward_Condon
[23] Ken Thompson: https://en.wikipedia.org/wiki/Ken_Thompson
[24] Belle (chess machine): https://en.wikipedia.org/wiki/Belle_(chess_machine)

grammable integrated circuits, and with 1 ns TTL logic chips on the critical path.

Maybe I'll make a fork here, and I'll focus on this contribution and this work on the special computer for a moment, and explain what we did with it. In conceptual terms, the special purpose computer was essentially a hardwired subroutine. The person who wrote the skeleton of the operating system for this special purpose computer was none other than Ken Thompson, the inventor of Unix. I could not be luckier, essentially. I could work with Joe Condon with his knowledge of physics and computers on one hand, and with Ken Thompson with his Unix software[25] on another.

Working with Joe on our new project—with never-ceasing pressure from my physics colleagues to quickly begin using the new "spin glass machine" for solving the outstanding physics problems—was a roller-coaster experience. Joe was blunt, and often busy with Belle or other things, but when we sat down and he began explaining to me how to design computers it was an extraordinary experience that could compress months of studies into one intense overnight discussion. Both Joe and I worked often long into the night in the common area of the UNIX lab, smoking and drinking coffee to keep going. I did the manual labor of designing logic equations and finite state machines for the programmable chips, and assembling and wiring the chips on circuit boards to put this Ising machine together, and finally testing and debugging. Actually, in hindsight it all went extremely fast. I had a working computer in about a year, starting from translating the Monte Carlo subroutine to Boolean logic, and ordering a box of chips, essentially. I was also very motivated, let's put it this way, to finish it very quickly. The research analytics part, writing a C-language program that would call this special box which was attached to a conventional computer, a VAX[26] in this case, and this Monte Carlo subroutine loop over the entire lattice as many times as one wanted, in the configuration that one wanted could be done by this quite flexible machine, despite its simplicity.

With this special purpose computer I could bring to the discussions about how short-range disordered Ising spin systems actually behave the essential capability that I could run a simulation as long as necessary to converge to thermal equilibrium[27]. That was the key to the solution, because

[25] J. H. Condon and A. T. Ogielski, "Fast special purpose computer for Monte Carlo simulations in statistical physics," *Rev. Sci. Inst.* **56**, 1691-1696 (1985). https://doi.org/10.1063/1.1138125
[26] VAX: https://en.wikipedia.org/wiki/VAX
[27] A. T. Ogielski, "Dynamics of three-dimensional Ising spin glasses in thermal equilibrium," *Phys. Rev. B* **32**, 7384 (1985). https://doi.org/10.1103/PhysRevB.32.7384

I could demonstrate to all non-believers that I could bring the system to thermal equilibrium simply by running it from some initial starting configuration long enough to verify that it was longer than correlation times. Then I could space the sampling of any quantity I wanted to measure and estimate, any equilibrium correlation function, like the Edwards-Anderson order parameter and so forth—we measured a lot of things; I can tell you more about it—sample it in such a way that I could demonstrate reasonable independence, and apply statistics to the interpretation. I thus got a gigantic simulation. I was told that it was the largest ever attempted in statistical mechanics at this time.

Over three years this research led to a series of (I dare say) very good papers that are still cited to this day. The spin glass simulation work became a classic, and physical predictions computed in this early work remain accurate to this day in 2021–despite immense increases in computing power over the forty years since our "spin glass machine" was put to work.

PC: Before we dive into this aspect, I was curious about the context of it. Was this idea of building a special purpose computer novel on its own? Was it newly in the air at the time? Or was it something that people had been doing many times over already?

AO: [0:21:10] Not many times over. I think that at the same time there was a group on the West coast, in Santa Barbara[28]. They made different types of special purpose computers, but for reasons that I never understood they applied it only to standard Ising models, in particular the 2D model which is solvable[29]. I think that they just wanted to verify that it works. Their machine was not used for any new research. It was used more as a demonstration of a concept. I did not keep track of it later. One of the leading members of this group was Doug Toussaint. The strangest thing about that project, which was contemporaneous, is that they didn't apply it to anything interesting. They were just computing convergence of finite-size models to the exact solution of the 2D Ising model, and applied it to the 3D Ising model too.

The other researchers who used unusual computers in this research were actually my colleague from Bell Labs, Ravi Bhatt, together with Peter Young who was then at Imperial College and later went to UC Santa Cruz. They were using one of the very early massively parallel computers called

[28] See, *e.g.*, S. Gottlieb, "Guest Editor's Introduction: Special-Purpose Computing," *Computing in Science & Engineering* **8**, 15-17 (2006). https://doi.org/10.1109/MCSE.2006.7

[29] R. B. Pearson, J. L. Richardson and D. Toussaint, "A fast processor for Monte-Carlo simulation," *J. Comput. Phys.* **51**, 241-249 (1983). https://doi.org/10.1016/0021-9991(83)90090-6

DAP[30]. So, no, it was not a widely used approach. Building special purpose hardware has its benefits, but also its shortcomings. The benefit was that I could build myself a piece of laboratory equipment that I could use exclusively as much as I needed. That was the critical ingredient to get correct results. But I could not, for instance, switch and start calculating, say, problems in protein folding, which was another huge unsolved problem in those days that was talked about in the supercomputing community. I think that history showed that there was maybe a window of opportunity when building special purpose machines would win you the day. The third chess computer was definitely a good example. My little spin glass machine was another example. But then with continuously increasing speed of processors—Moore's law essentially—the general-purpose machines became unbeatable and that's why we use them today. Although the funny thing is that if you look inside your iPhone you'll realize that there are many specialized processors for, among other things, video processing or GPS. So in modern devices we ended up with parts that are special purpose and parts that are general purpose, but purely special purpose research computers have a short-lived window when there is nothing else that one could use as an alternative.

FZ: I'd like to ask you something related to this. Earlier, you mentioned the fact that you were familiar with lattice gauge theories. I was thus wondering about the following. I've heard several times that people were interested in spin glasses also because it was a simple example of a kind of lattice gauge theory where you have some variables that are dynamical and other variables that are quenched, like in certain approximation of QCD. I was thus wondering if this was an inspiration for you. Also, in the field of lattice gauge theories, people have been developing special purpose computers too. Did you have interactions with them? Was there any kind of interaction between these two fields at the time?

AO: [0:25:34] Quite close, actually, especially on the personal level. As soon as the news spread that I had this super-duper toy for Monte Carlo at Bell Labs, I got a large number of invitations to give invited talks, also in national labs and in academic groups doing large-scale Monte Carlo of QCD, of course. I was particularly close with Norman Christ[31], at Columbia, and with a group at Oak Ridge. I also had quite a bit of interactions with a group from Lawrence Livermore, and with other groups that unfortunately I don't recall right now.

[30] See, *e.g.*, P. Charbonneau, *History of RSB Interview: A. Peter Young*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.2fef8760

[31] Norman Christ: https://en.wikipedia.org/wiki/Norman_Christ

But there was a big distinction, because of the heavy emphasis on floating-point calculations in lattice quantum chromodynamics. They were rather proceeding in the direction of multi-processors. Essentially, they were taking single-card computers and putting dozens and sometimes hundreds if not thousands in a box. So it was a very different architecture. They were really networked general-purpose computers. So we were in touch discussing what could be done and what cannot be done, and what technologies we are using for design and so forth. So yes, I am familiar with these people and these groups, but just because of short physical distance between Bell Labs in Murray Hill and New York City, I was a frequent visitor and vice versa in Norm Christ's laboratory.

PC: The work you did and that you published on this machine—at least some of the first ones—was with a former Binder student, Ingo Morgenstern[32].

AO: [0:27:46] He actually came to Bell Labs when I was done with the construction of the special purpose Ising computer. He was a visiting postdoc, I think, at Bell Laboratories when I finished building this machine and it was already working full time. He and I wrote the first short communication about the initial results, but he did not participate in the special purpose computer project and in writing the software to operate it. Later he went back to Germany and we kept talking. I think that one of the more interesting conferences in spin glasses, from this perspective, was in Heidelberg in '86, when I saw him again[33].

PC: You mentioned how the computational physicists responded enthusiastically to your work. What was the spin glass community response to that work?

AO: [0:28:50] I would say very positive. First of all, I want to stress that it was not just spin glasses. There was this whole category of conceptually related materials, which in statistical mechanics context are generally called random field Ising models, dilute antiferromagnets, spin glasses of various kinds and so forth. I think that from the perspective of experimentalists, the biggest pleasure that I had—and contribution that I made—is that I had developed a parsimonious method of parameterizing various

[32] A. T. Ogielski and I. Morgenstern, "Critical behavior of three-dimensional Ising spin-glass model," *Phys. Rev. Lett.* **54**, 928 (1985). https://doi.org/10.1103/PhysRevLett.54.928; "Critical behavior of three-dimensional Ising model of spin glass," *J. Appl. Phys.* **57**, 3382-3385 (1985). https://doi.org/10.1063/1.335103

[33] Colloquium on Spin Glasses, Optimization and Neural Networks, Held at the University of Heidelberg, June 9-13, 1986. Proceedings: *Heidelberg Colloquium on Glassy Dynamics*, J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1987).

measurable functions. In particular, functions describing very slow, non-exponential relaxation that is characteristic of glassy materials. I used essentially Kohlrausch law[34], corrected with a power law, so essentially the product of a power-law decay and a Kohlrausch decay. Given that I had very accurate data with small error bars—precisely because I could achieve such high statistics in simulations—I could parameterize these curves pretty well. And there are good intuitive reasons to choose such things. Because when short-range and later long-range order develops, you do expect an inverse power law for the decay of correlation functions. When you have kind of cluster dynamics, which incidentally was being investigated at the same time by, among others, Dan Fisher, David Huse, myself, and Chris Henley[35], who was then at Bell Labs as a postdoc. Kohlrausch [scaling] was also kind of expected. Not to mention that researchers deeply engaged in the chemistry of glasses—real glasses, not spin glasses—had been using Kohlrausch functions for quite a bit of time. Anyway, to make a long story short, with this parameterization I was able to establish a common language for the experimentalists, who immediately could relate their data among themselves and to simulations. Some papers even began calling it the Ogielski function[36]. One particularly interesting case was the power-law decay in ordered or quasi-ordered states of those materials. When you have a very slow power-law decay, the exponents governing this decay are very large, about 7 or so. (That's the parameter *z* for the dynamic correlation function.)

I very quickly established a relationship with a number of experimental labs. Vince's lab in Santa Barbara[37], Mydosh's lab in Leiden[38], and several other labs in the United States and so forth. I think that we could express the correspondence between numerical findings and experimental findings, using the same type of formulas, the same type of parameterization. That was one. The second relation that I had with experimentalists was—in particular with Laurent Lévy, who worked in Doug Osheroff's lab—was the study of non-linear susceptibilities in spin glasses[39]. I think I men-

[34] Stretched exponential function: https://en.wikipedia.org/wiki/Stretched_exponential_function

[35] V. Elser and N. D. Mermin, "Christopher L. Henley," *Physics Today* (2015). https://doi.org/10.1063/PT.5.6160

[36] See, *e.g.*, R. M. Pickup, R. Cywinski, C. Pappas, B. Farago, and P. Fouquet, "Generalized Spin-Glass Relaxation," *Phys. Rev. Lett.* **102**, 097202 (2009). https://doi.org/10.1103/PhysRevLett.102.097202

[37] Vincent Jaccarino (1924 – 2019); See, *e.g.*, Henry T. Yang, "Sad News - Professor Emeritus Vincent Jaccarino," *UC Santa Barbara Office of the Chancellor*, September 3, 2019, https://chancellor.ucsb.edu/memos/2019-09-03-sad-news-professor-emeritus-vincent-jaccarino (Consulted January 13, 2021)

[38] See, *e.g.*, P. Charbonneau, *History of RSB Interview: John Mydosh*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.e1e3ob87

[39] See Ref. 17.

tioned to you my interest in stochastic dynamics. At some point, I went on an errand and I calculated from scratch the formulas for all dynamic correlation functions to a high order using the expansion of the generating functions. I found out there were some errors in the textbook formulas, which I corrected. Using these corrected formulas, Laurent Lévy did a beautiful experiment measuring up to the 7th order nonlinear susceptibility in the Ag:Mn spin glass[40]. It was a real *tour de force*. A demonstration that you could calculate higher-order susceptibilities, and that in well-done clean experiments you could measure them very accurately. Again you could relate one to another. That was another thing that we did.

In the theoretical community, I thought it was good to have a clean-cut answer that within limitations of dealing with finite systems—which were the largest anybody could do at this time—the various scaling laws, in particular, were completely consistent with the existence of a second-order phase transition in 3D spin glasses with a discrete distribution of bonds— $\pm J$ models—so that created quite a bit of interest.

The second thing was maybe scientifically not very deep, but it illuminated the shortcomings of less careful simulations. The most visible feature in simulations of spin glasses is that the specific heat develops a peak, but there is no long-range order of any kind in the system. People were thinking that that was the signal of a spin glass phase transition. It turns out that it's merely a signal of a short-range ferromagnetic order that develops at non-random Ising model critical temperature. Spin glass ordering appears at much lower temperatures than that of the peak of the specific heat. This peak, by the way, does not develop a singularity. It is just a round hump created by short-range magnetic ordering in the system.

The third very interesting thing in the interaction with theorists came from a different angle. I always had an interest in geometry and in stochastic dynamics, as I told you, so I started analyzing the low-temperature states of spin glasses, what exactly they are. Perhaps the best account of it is in the article I wrote for this 1986 conference on spin glasses in Heidelberg[41]. The rest is mostly unpublished notes, because at this time I was changing career. I could demonstrate a lot about the ideas how droplet models of hierarchical dynamics could be realized in Ising spin glasses, and how frustration networks actually help propagate long-

[40] L.-P. Lévy, "Critical dynamics of metallic spin glasses," *Phys. Rev. B* **38**, 4963 (1988). https://doi.org/10.1103/PhysRevB.38.4963

[41] A. T. Ogielski, "Phase transitions and equilibrium dynamics in strongly random Ising spin systems," J. L. van Hemmen and I. Morgenstern, eds. *Heidelberg Colloquium on Glassy Dynamics. Lecture Notes in Physics* **275**, 190-214 (Berlin: Springer, 1987). https://doi.org/10.1007/BFb0057517

range order in spin glasses. Again, some of this is in this Heidelberg lecture.

I also developed very close connections with the earliest work on neural networks. It just so happened again—that was the beauty of Bell Laboratories—that John Hopfield had an office exactly one flight of stairs above my office. When John Hopfield started talking and writing about his simple feedback neural networks and using them as model of memory—you are familiar with that—I could be looking at similar phenomena of cluster dynamics in my models of spin glasses.

I also started working with people directly in Phil Anderson's orbit—by then Phil already went full time to Princeton, after the mid-'80s. I just lucked out. I found that simple models of hierarchical relaxation can actually be solved exactly. You recall maybe that the problem was that most of theoretical physics known in the early '80s, or late '70s had a real difficulty with explaining non-exponential and non-power-law time decay phenomena. How to describe that? There were all these Kohlrausch functions, Vogel functions[42], and what not. We realized that random walks on trees are exactly solvable and can produce exactly these types of relaxation functions. This was a paper with Dan Stein, a short Physical Review Letters paper that generated a lot of interest too[43]. Somehow, that started very directly bridging spin glass research with other theoretical endeavors that dealt with complex systems with complicated energy landscapes of metastable states.

This was also Phil Anderson's interest. That's why he started talking about evolution theory, evolution of species. It was formulated in terms of energy landscapes, which I think for Phil were originally coming, of course, from many-body quantum mechanics with the Born-Oppenheimer approximation. This concept of energy landscapes was connecting very different systems

I very quickly related my work to work of people dealing with applications of spin glass thinking to neural networks, this application to spin glass-like thinking to evolution theory, and to the entire group that started creating the Santa Fe Institute[44]. That was very interesting. I also had the pleasure of spending a lot of time with Steve Wolfram[45]. Steve at the time was a postdoc at Bell Labs—for a year of two—before going to the Institute of

[42] Vogel-Fulcher-Tammann equation: https://en.wikipedia.org/wiki/Vogel-Fulcher-Tammann_equation

[43] A. T. Ogielski and D. L. Stein, "Dynamics on ultrametric spaces," *Phys. Rev. Lett.* **55**, 1634 (1985). https://doi.org/10.1103/PhysRevLett.55.1634

[44] Santa Fe Institute: https://en.wikipedia.org/wiki/Santa_Fe_Institute

[45] Stephen Wolfram: https://en.wikipedia.org/wiki/Stephen_Wolfram

Advanced Studies in Princeton. I would say it was a very good time to be able to come with complete answers, at least for this one category of spin glasses that I studied, because that field was related to so many other fields—not even neighboring fields, but quite distinctly separated.

It opened another door too. I don't know if you remember or if you read about it, but it was one of these very illogical phenomena in American mass media. In the early '80s, when I was doing this work, the mass media were filled with panic that Japan was going to overtake the United States in supercomputers. Japan announced something called the fifth generation supercomputer at that time[46]. Nobody else but Al Gore[47]—whom you know from a different perspective, presumably—became a great proponent and proselytizer of supercomputers. Somehow, I was used to some extent by people who were pushing for the growth of supercomputing in the United States. One of the amusing stories that happened to me was that I was featured in an article in Business Week with my little supercomputer[48]. At that time I was buying a house, and this article convinced my bank to approve my mortgage, which was really funny. There you go.

Yes, there have been really close connections between what I was doing and neighboring fields. Some results maybe could be transferred directly to what other people were doing. Some were maybe more conceptually related. I would say that in a research sense an interesting aspect of my work was the fact that there's all kinds of cluster dynamics taking place in spin glasses and disordered materials, it seems. After I stopped working in this field, I understand these ideas made quite a lot of progress.

PC: Before we move to that latter part, I was curious about something. You mentioned Phil Anderson's name a number of times. How much did you interact with him as part of that research, and more broadly at Bell Labs?

OA: [0:43:07] In a literal sense, often almost daily. In the theoretical physics department there was this nice custom of afternoon tea in the laboratory. You were supposed to show at 3pm sharp in a small room, close to our offices, in Murray Hill, and talk with everybody about the most intelligent thing that came to your mind. Phil's office was two doors down the hallway from this coffee room—as we called it—or small conference

[46] Fifth generation computer: https://en.wikipedia.org/wiki/Fifth_generation_computer
[47] Al Gore: https://en.wikipedia.org/wiki/Al_Gore
[48] "A Supercomputer Gap Has U.S. Scientists Up in Arms," *Business Week* **2818** (November 28, 1983), 109-110 (1983).

room. Whenever he was in the laboratories—he usually spent a lot of time at Bell Labs—he would show up and chat with people.

I learned a lot from him. The biggest lesson I learned from Phil was that it really pays to spend an enormous amount of time with data. He certainly helped me to build this motivation to spend so much time analyzing the data, making sure that it's right, that I have control over statistical errors, and, most importantly, that I have control over systematic errors, because they are the real danger**.** Pure statistics you can handle, but systematic errors can kill you. That was very helpful. I also went to visit him a few times for longer conversations also after he left Bell Labs and spent more of his time in Princeton. I extremely much enjoyed talking with him.

Other people that I spent a lot of time working with at Bell Labs, I would say David Huse[49], who is now professor in Princeton, and Dan Fisher. I also spent quite a lot of time with Chris Henley, who was a very interesting character. I don't know if you knew him. Did you?

PC: He was at Cornell, right?

OA: [0:45:14] Yeah. He later went to Cornell and as bad luck had it he died of brain cancer when he wasn't even 60. He did some very nice work with droplet and cluster dynamics, so we spent a lot of time working together. He was pressing me to dig deeper and deeper into data to identify the normal modes of the master equation, the eigenvectors of the generator matrix, to expose correlated clusters in frustrated systems.

Another good thing at Bell Labs was that at that time you were supposed to be in the office. There was no working from home. We all went to lunch together. Murray Hill has a very large cafeteria. The discussions across disciplines with people from various laboratories went on at lunches. That was again a fantastic vehicle to connect what I was doing with interests of other people. Incidentally, also with engineering groups at Bell Labs, not only in physics.

PC: You left spin glass and the disordered system work, at least from the physics standpoint, around 1987-1988. You mentioned that you then got interested in other problems. Was there a drive to that change of direction?

[49] See, *e.g.*, A. T. Ogielski and D. A. Huse, "Critical behavior of the three-dimensional dilute Ising antiferromagnet in a field," *Phys. Rev. Lett.* **56**, 1298 (1986). https://doi.org/10.1103/PhysRevLett.56.1298

OA: [0:46:58] Having invested such a big effort into designing with Joe Condon and then building and wiring this special purpose computer—it was a sizeable box with the VME bus, which was then a standard, installed on a rack, so it was a sizeable box with memory and processor cards inserted into it—I had to absorb essentially in one year all the latest research and everything that was done in automated logic design with programmable chips to create this machine, and software design, and small scale networking. Therefore, I naturally got very much interested in supercomputing and computer-related questions.

Before going there, however, I used my special computer to the max. With David Huse I did simulations of dilute antiferromagnets, and then I did more studies of random field Ising models[50] and dynamics of fluctuations in random and non-random Ising models[51]. I also collaborated on two experiments on dynamic behavior of certain spin glass materials. The period of 1985-86 was very productive – I authored or co-authored ten papers, five of them in Physical Review Letters, which was not bad.

In late 1987—I don't remember the date exactly—I transferred from theoretical physics to the mathematics center at Bell Laboratories. The math center at that time was run by Ron Graham[52]. You must have heard of Ron Graham, I suppose. He was not only an extraordinary discrete mathematician, but also a phenomenal juggler. He was actually the president of the world's union of jugglers. He was also a great supporter of Paul Erdős[53]. Paul Erdős essentially traveled with a suitcase, didn't have a home, and was just working with people, and then moving to the next place to write another paper. Because Ron Graham was a great supporter of Paul Erdős, Bell Labs had presumably one of the strongest groups in discrete mathematics at that time. It was also the birthplace of Unix[54]. There was always a need for more people in applied math area. So I spent a couple of years in the applied mathematics group there. It was a group dealing primarily with computer networks. I did some work on Boolean neural networks and stuff like this. Then, I became so engaged in this that when Bell Labs split—you may remember that Bell Labs lost its antitrust lawsuit filed by the Department of Justice[55]. (The irony was that IBM had won independence—it was not broken up—but AT&T was bro-

[50] A. T. Ogielski, "Integer optimization and zero-temperature fixed point in Ising random-field systems," *Phys. Rev. Lett.* **57**, 1251 (1986). https://doi.org/10.1103/PhysRevLett.57.1251
[51] A. T. Ogielski, "Dynamics of fluctuations in the ordered phase of kinetic Ising models," *Phys. Rev. B* **36**, 7315 (1987). https://doi.org/10.1103/PhysRevB.36.7315
[52] Ronald Graham: https://en.wikipedia.org/wiki/Ronald_Graham
[53] Paul Erdős: https://en.wikipedia.org/wiki/Paul_Erd%C5%91s
[54] Unix: https://en.wikipedia.org/wiki/Unix
[55] Breakup of the Bell system: https://en.wikipedia.org/wiki/Breakup_of_the_Bell_System

ken up into pieces by the Justice Department.) Bell Labs itself split into two. One part remained Bell Labs, and the other was called Bellcore[56]. It still exists, but it's not what it was. This created opportunities for advancement and I moved to Bellcore Research in 1989 and stayed there until 1996. In this time, I was 100% involved in supercomputing, with my primary interest in massively parallel computers[57]. At this point, I did essentially no physics anymore. I was fully busy with my new duties. I was a department head at some point. Then I had two departments under my management. As they say, at this point I may have crossed to the dark side, but it was not at all bad.

PC: Did you nonetheless remain in touch with some of the spin glass and optimization people in Europe who were looking more on the computer science side of these questions?

OA: [0:51:24] Very much so. I continued going to conferences on these subjects. Several of them I remember particularly pleasantly. For instance, Benoit Mandelbrot was a fantastic raconteur and a very colorful figure. I spent a lot of time with him at some conference. I think it was in Santa Cruz or Santa Barbara. I don't remember. I continued going for some time to the Institute of Theoretical Physics in Santa Barbara, but eventually I completely moved to supercomputing.

Then, when the government ban on using the Internet for commercial purposes was lifted, my life changed again. You remember that the US government did not permit corporations to use the internet because it was a government funded research program, so you had to be an academic or have a special exemption to use it. Bellcore got this special exemption, so we got Internet. Bell Labs did not use it until the late '80s, perhaps like '88-'89.

One of the features of big organizations which is not good is the not-invented-here syndrome[58]. Bell Labs invented their own internetworking. It was very elegant, but it was not Internet and it was not based on the IP protocol. Bell Labs also invented their own bit map terminal, but again it was not written in open source software. And Bell Labs invented its own email, which again was of a different kind.

---

[56] iconectiv: https://en.wikipedia.org/wiki/Iconectiv

[57] See, e.g., A. T. Ogielski and W. Aiello, "Sparse matrix computations on parallel processor arrays," *SIAM J. Sci. Comput.* **14**, 519-530 (1993). https://doi.org/10.1006/inco.1994.1070

[58] Not invented here: https://en.wikipedia.org/wiki/Not_invented_here

My eyes were opened to the possibilities of Internet protocols and implementation of the Internet in real life when I was at Bellcore. This was essentially my third career: physics, supercomputing, Internet engineering. It's quite a route, but it happened quite naturally.

PC: You mentioned how your experience with supercomputers served you in your second career, as you describe it. Was the physics of spin glasses influential at all? Or did that not persist?

AO: [0:53:58] It did, but in a different incarnation altogether. An incarnation that I practice to this day, actually. Through the first connections through the Santa Fe Institute, and Anderson's interest in evolution and related things, I got drawn to study biology as well. I was lucky enough to be able to spend several weeks each year at Woods Hole Marine Biology Laboratory[59] when I was at Bellcore. It was a perk that I could use. I could still lead my own research. This fascination persisted to this day. I would say that the understanding of this enormous group of problems that deals with disordered systems, collective behavior, slow relaxation—think proteins, of course here, that was a related problem at this time—kept me busy looking at real biological systems. I have retired from physics but I continued doing research in biology. I think it happened to more people than myself. Quite a few people went this way. I think that this big basket of problems related to spin glasses had quite an enormous impact on a multiplicity of disciplines. Not purely in a mathematical sense, but rather in a conceptual sense that we could carry over to other fields.

PC: I have a curiosity-driven, slightly tangent question. I noticed that you wrote a grant[60] with one of my now-Duke colleagues, Ingrid Daubechies[61]. Did you actually ever work together? Or was this just a one off?

AO: [0:56:00] Yes. Ingrid and I were both at Bell Laboratories, and we overlapped somewhat in time. I think I was there already when Ingrid joined. She did beautiful work on the wavelets at that time. We have remained in touch. This grant was in a different direction, related to Internet research. When I was at Bellcore, we were approached by several academic groups who wanted to participate in modeling of various aspects of the Internet. The key knowledge that I could bring was that I had the reputation of a guy who could take on enormously large problems, and I could

[59] Marine Biological Laboratory: https://en.wikipedia.org/wiki/Marine_Biological_Laboratory
[60] Amos Ron, Ingrid Daubechies, David Donoho, Walter Willinger and Andy Ogielski, "ITR: A Multiresolution Analysis for the Global Internet," NSF Division Of Computer and Network Systems, #0085984, https://www.nsf.gov/awardsearch/showAward?AWD_ID=0085984 (Consulted July 8, 2021)
[61] Ingrid Daubechies: https://en.wikipedia.org/wiki/Ingrid_Daubechies

somehow, one way or another, finagle resources and money to conduct very large numerical studies and very large simulations. And my group at Bellcore was collecting huge amounts of network data for statistical analysis[62]. The first grant proposal that we did was with Walter Willinger, who was then at Bellcore, Ingrid Daubechies, and a statistician from Stanford, Dave Donoho[63]. (I was later living near Dartmouth and he sent his son to Dartmouth, so we had some good discussions more recently.) The point of this proposal was to apply very large scale data analytics based on wavelets and other multiscale methods to analysis of Internet traffic simulations. My role was to put together a really big simulation testbed. Another partner in crime was David Nicol. (Last I heard of him he was a professor at Urbana-Champaign.) We actually did incredibly well. After basic research at Bellcore pretty much shut down—Bellcore was sold in 1996 to SAIC[64], a large defense contractor—it became a much less attractive place. I left and I became a professor at Rutgers for several years. All these years and a few years longer I had funding from DARPA and from NSF. This grant you discovered with Ingrid and Donoho and others as partners was renewed at least twice. Later I got several sizable DARPA grants for building the largest possible simulations of the global Internet.

One of the papers that made waves in this community was where we demonstrated that we can simulate one million computer nodes in a network the size of the United States[65]. This was a quite exciting period. That was another focus of my activities, where very large scale supercomputing was applied to a problem that was not resolved, and became successful. The problem was that typical simulations in networking those days were essentially dealing with issues of queue management. The researchers typically studied several processes creating packets. Small scale, small things. But the question we tried to answer was: What happens to packet traffic if I connect 10,000 or 100,000 sources of data flows and let them interact in various topologies. We actually got to a million nodes. We succeeded.

---

[62] For the context, see, *e.g.*, Walter Willinger, Ramesh Govindan, Sugih Jamin, Vern Paxson, and Scott Shenker, "Scaling phenomena in the Internet: Critically examining criticality," *Proc. Nat. Acad. Sci. U.S.A.* **99**, 2573 (2002). https://doi.org/10.1073/pnas.012583099

[63] David Donoho: https://en.wikipedia.org/wiki/David_Donoho

[64] Science Applications International Corporation: https://en.wikipedia.org/wiki/Science_Applications_International_Corporation

[65] J. H. Cowie, D. M. Nicol and A. T. Ogielski, "Modeling the global internet," *Comput. Sci & Eng.* **1**, 42-50 (1999). https://doi.org/10.1109/5992.743621; J. Cowie , H. Liu , J. Liu , D. Nicol and A. Ogielski, "Towards Realistic Million-Node Internet Simulation," *International Conference on Parallel and Distributed Processing Techniques and Applications* (1999). http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.30.2506 (Consulted July 9, 2021)

Another nice feature for people who were investigating critical phenomena was that power laws are ubiquitous in telecommunications as well, but through different mechanisms. You presumably saw somewhere papers showing that fluctuations of packet traffic are not gaussian. They are more like 1/*f* noise. They have this scale-invariant nature over very many decades, so again you have to simulate over very long times to achieve results that make sense. We could achieve this. This time, however, we were using commercially available multiprocessor machines, like very large Sun computers and other computers of this nature.

FZ: I wanted to know a little bit more about something. Toward the end of the '80s, a big part of the spin glass community moved toward the field of neural networks and computer science. In particular, in Paris, people around Marc Mézard and Elizabeth Gardner and Bernard Derrida. There was a group also in Israel. Did you have any connection or interaction with these people?

AO: [1:02:30] Very close actually. Regarding Israel, Haim Sompolinsky[66] was a very frequent visitor to Bell Laboratories. I spent hours and hours talking with Haim. I think that the area where we interacted most was the cluster structure of the low-temperature phase of Ising spin glasses. I already mentioned that the same kind of conversations were going on with John Hopfield, who was in the same building where I was before he moved full time to the West coast. Haim Sompolinsky and his group was definitely one type of interactions.

I was looking at some notes from the past yesterday evening just to remember the names better. I know that I had quite a bit of interactions with Gérard Toulouse[67], with Cirano de Dominicis[68], but I don't think we ever carried out a well-defined research project together. I would characterize it rather as mutually illuminating or stimulating conversations and such.

I did some work on neural networks themselves, when I was in the mathematics center at Bell Laboratories, but I was discouraged by the fact that the field was not progressing very well at the time. There was more hype than meat those days. I think that now we know the answer why through later work done at Google and Facebook. What was missing in those early days was an understanding that the power of neural networks emerges only when you use extraordinarily large amounts of data for training. I

[66] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky
[67] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse
[68] Cirano de Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis

don't think that was the understanding in those days, despite the availability of earlier Russian mathematics research on learning of functions from examples[69]. The neural networks that were studied those days were sort of toy models, as we would call it today. The reality is simply another way of saying that size matters. You really have to do it on a large scale with enormously large volumes of data for training and for cross-verification. That's what created what is now leading the computer industry with applications from Google, Apple, Facebook and such. I saw beautiful work very recently for solution of partial differential equation in density functional theory[70]. I love it. You have to do it big.

If there is one theme in my research life, it has been that if you deal with a big problem, you cannot really cut corners, and you have to deal with it in a proper size, on a proper time scale situation. That might be one common motif throughout. These three kinds of clusters of research I did: spin glasses and related disordered materials, supercomputing, and simulations and analysis of global Internet.

PC: During your time at Bell Labs, at Rutgers or elsewhere, did you ever get to teach about spin glasses, or replica symmetry breaking ideas or ultrametricity? If yes, can you detail a bit?

AO: [1:06:36] Not teach in the sense of teaching courses. I was then participating in various conferences or in the Aspen school of physics, or Telluride, where I would give lectures about my work, or about problems in the statistical mechanical approach to the spin glass and random magnetic material problems. But, no, I have not been teaching academic courses at that time. When I was at Rutgers—I was a professor there between 1996 and the end of 1999—I was in the electrical engineering department.

There was another type of big-scale computing that we did. It's also interesting in its own right. In the late '90s, the challenge for networking, in particular, was that it was becoming clear that the Internet was going wireless. At the same time, the protocols at the core of the Internet, in particular TCP, were developed for wired systems. The nature of the problem is completely different in wireless and wired networks. In wired systems, especially of the earlier era when people were building the first

[69] See, *e.g.*, A. T. Ogielski, "Information, Probability, and Learning from Examples". https://www.researchgate.net/publication/2825975_Information_Probability_and_Learning_from_Examples (Consulted September 1, 2021)

[70] See, *e.g.*, R. Nagai, R. Akashi and O. Sugino, "Completing density functional theory by machine learning hidden messages from molecules," *npj Comput. Mater*. **6**, 43 (2020). https://doi.org/10.1038/s41524-020-0310-0, and references therein.

Internet, packet losses were due to congestion. In wireless, they're mostly due to noise and corruption. Completely different mechanisms. Long story short, the Internet transport protocols and wireless link protocols are completely mismatched because they try to address different problems for quality control. So we have created a very large software testbed for investigating interactions of Internet protocols with wireless layer protocols [71,72,73].

I was at a group called WINLAB at Rutgers[74], a wireless network research laboratory, which included a number of ex-Bell Labs guys from the radio department and the cellular telephony departments. This has been a very successful project again. I keep hitting these big computational projects somehow. I was teaching maybe a couple dozen graduate students over this time—several of them got their PhD, the rest got master degrees—but I was not teaching courses. I was teaching apprentice-style how to analyze packet traffic in radio channels starting from Maxwell equations and ending up with computer defined radios and Internet protocols. Different things.

PC: Is there anything else about this era that you would like to share with us that we may have skipped over you think might be relevant.

AO: [1:10:16] I would keep coming back to this extraordinarily exciting environment, when people from different fields were spending lots of time together. In some way the idea of the Santa Fe Institute, which was supposed to look very different than what it is today, arose in the mid-'80s precisely to bring together people with interests in neurobiology, physics, economics and spin glasses etc. Phil Anderson, David Pines[75], Murray Gell-Mann[76] and several other people had more of an idea of an independent, novel type of university, rather than the visitor-driven lab as it is today. But I think that their expectations for funding have not materialized.

---

[71] J. Panchal, O. Kelly, J. Lai, N. Mandayam, A. T. Ogielski and R. Yates, "WiPPET, a virtual testbed for parallel simulations of wireless networks" *Proceedings of the Twelfth Workshop on Parallel and Distributed Simulation PADS'98*. https://doi.org/10.1109/PADS.1998.685282

[72] J. Panchal, O. Kelly, J. Lai, N. Mandayam, A. T. Ogielski and R. Yates, "Parallel simulations of wireless networks with TED: radio propagation, mobility and protocols," *ACM SIGMETRICS Performance Evaluation Review* **25**, 30-39 (1998). https://doi.org/10.1145/274084.274088

[73] Y. Bai, A. T. Ogielski and G. Wu, "Interactions of TCP and radio link ARQ protocol," *Gateway to 21st Century Communications Village. VTC 1999-Fall. IEEE VTS 50th Vehicular Technology Conference* **3** (1999). https://doi.org/10.1109/VETECF.1999.801596

[74] WINLAB: https://en.wikipedia.org/wiki/WINLAB_(Rutgers_University)

[75] David Pines: https://en.wikipedia.org/wiki/David_Pines

[76] Murray Gell-Mann: https://en.wikipedia.org/wiki/Murray_Gell-Mann

The most important items in my memory of the '80s are these broadly interdisciplinary connections and that I was able myself, which was extremely pleasant, to move between computer science, hardware design, physics, and touch a little on biology. It was very stimulating.

And all the people who were there were absolutely fascinating. When I was at theoretical physics at Bell Labs, Duncan Haldane[77] was there, Sue Coppersmith[78] was there. There were many more really good people: Piers Coleman[79], Chris Henley, Steve Wolfram, Boris Shraiman[80]. They were all physicists, all of them. They were young, and we were spending a lot of time talking about pretty much everything. That's the strongest impression that stayed with me.

PC: 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?

AO: [1:12:50] You see, this was not the email era yet. Email was just appearing. We were very much worried then that what's on email is going to disappear forever. Actually, the opposite happened. Paper letters disappeared with time, and email is forever. So, no, not very much correspondence survived. Just yesterday I unearthed some very pleasant letters that I received from Cirano de Dominicis and Gérard Toulouse. But then we usually had secretaries keeping them in folders. With moving between companies, and companies splitting up, much of this stuff sort of disappeared somewhere. However, I still have a big box with design notes for the Bell Labs special computer, some old spare chips and 10-inch reels of computer tapes, and the machine itself. Also a box of some handwritten notes and calculations for my various papers.

PC: I hope you find a way to preserve those in an academic archive. In any case, please let us know what happens to them. Thank you very much for your time. This was a very enjoyable conversation.

AO: [1:13:41] It was fun talking to you guys. I don't get as much to talk about spin glasses anymore. Presumably I forgot dozens of people who were helpful and with whom I enjoyed working, and dozens of laboratories that I visited, but maybe I'll have a chance to add some things in the notes.

[77] Duncan Haldane: https://en.wikipedia.org/wiki/Duncan_Haldane
[78] Susan Coppersmith: https://en.wikipedia.org/wiki/Susan_Coppersmith
[79] Piers Coleman: https://en.wikipedia.org/wiki/Piers_Coleman
[80] Boris Shraiman: https://en.wikipedia.org/wiki/Boris_Shraiman