History of RSB Interview: Sidney Nagel

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PC: Good morning, Professor Nagel. Thank you very much for joining us. As we were just discussing, the theme of this series of interviews is the history of replica symmetry breaking in physics, which we roughly bound from 1975 to 1995. During our conversation with you, we will bleed a bit on both ends for context and to better understand what happened afterwards. To get us started, can you first tell us a bit about your family and your studies before starting university?

SN: [0:00:33] My family. My father was a philosopher[1], and my mother was a physicist who got a PhD in physics[2] at a time when women were not supposed to get PhDs in physics. That was something I grew up with and I saw what the social strains were that that caused. That was one aspect of growing up. My brother is a mathematician[3]. He's the serious one in the family. I went off into physics, so I wouldn't have to compete with him. I [eventually] went off into physics, but that was not actually the first thing I wanted to do in life. The first thing I really wanted to do is be a poet. That kind of failed, so I had to take second choice as physicist. That's where I am now.

PC: How did your interest in science come about? Was it just always in the air in your family, or was there a particular draw?

SN: [0:02:15] Well, I actually was not interested in science when I started in college. I was, as I said, interested in literature. I guess two things

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[1] Ernest Nagel: https://en.wikipedia.org/wiki/Ernest_Nagel

[2] Edith Haggstrom, *β-ray spectra of rubidium-86 strontium-89, eka-tantalum and protactinium*, PhD Thesis, Columbia University (1942). https://clio.columbia.edu/catalog/1569213 (Accessed November 29, 2023.)

[3] Alexander Nagel: https://en.wikipedia.org/wiki/Alexander_Nagel

happened there. One was I got kind of disillusioned with what we're supposed to be doing if we're studying literature. The second thing is that I took a class that used this book by Purcell, which is for first-year undergraduates who are studying electricity and magnetism[4]. It's a beautiful book and it was a struggle to get through for me, but I was: "Ooh! There's something really beautiful here. Maybe I should continue taking a little bit more." So, I took a little bit more, and then a little bit more, and a little bit more. I still feel that I'm doing that now.

PC: What drew you to pursue graduate studies at Princeton, and with Steve Schnatterly[5] in experimental condensed matter in particular[6]?

SN: [0:03:35] I didn't really know what I wanted to do when I went to graduate school. I thought maybe something along the lines of biological physics was something, but there wasn't too much going on at that point there. I started off working with someone in particle physics and I realized I didn't like that aspect too much. Even though the groups were really small in those days, they were too big for me. I still like to have smaller scale things that I could work on. That was kind of the sociological local aspect of it. Until you've done it, you don't really know what it's like.

I was going into condensed matter, which is what Steve Schnatterly was doing. I knew I wanted to be an experimentalist, and the [reason why the] idea of condensed matter was very attractive—and still remains very attractive—to me is that it's not so separated from theory. Theoretical and experimental parts are not so far apart that you can somehow be aware of both at any time that you're doing a problem. That was getting less and less true in particle physics. It's getting probably somewhat less true in some parts of condensed matter physics now as well, but the area that I'm in, soft condensed matter, is beautiful that way. Right in the middle of it, you can do anything you want. They like to call the field mature if you can have a theory versus an experimental part, and I always think of maturity as the last step before death. This is the part that I like: the fact that we're not a mature field. We have ways to go before we have to partition ourselves off into confining areas.

PC: Was this really your perspective at the time? As a grad student, were you already aware of these subtleties?

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[4] E. M. Purcell, *Electricity and Magnetism* (New York: McGraw-Hill, 1965). https://en.wikipedia.org/wiki/Electricity_and_Magnetism_(book)

[5] Stephen Eugene Schnatterly (1938-2019). See, *e.g.*, "Stephen E. Schnatterly," *Neurotree* (n.d) https://neurotree.org/beta/peopleinfo.php?pid=54675 (Accessed December 8, 2023.)

[6] Sidney Robert Nagel, *Infrared properties of metals and wavevector dependent local field effects*, PhD Thesis, Princeton University (1974). https://catalog.princeton.edu/catalog/9919647053506421

SN: [0:06:15] I don't think that soft condensed matter existed as a field when I was a graduate student. I never heard of those words certainly. There were things such as liquid crystals around, which I heard of, but that was about it. The idea that you studied disorder for disorder's sake was certainly not an accepted aspect of things. We were taught that crystals were the be-all and end-all of everything. Why not deal with perfection all the time? I didn't realize that perfection is also disorder. That was something that I only understood later. But there was this idea that physics was somehow supposed to study particularly the things that we manufactured. That's something that changed very much in my worldview of this. I really think physics should be the study of what's out there and not what we can make to conform to what we would like. That's why I think studying things like disorder or things that are not in equilibrium or anything that's a true mess [is wonderful]. If physicists can come in and say things about the nature of that entity, then I think that is when they're doing their job the best.

PC: After your PhD, you went for postdoctoral studies with Jan Tauc[7] at Brown University on disorder. What brought you to Brown and with Jan Tauc in particular?

SN: [0:08:16] What I was interested in those days, what I thought I could do, was optical properties. What I did in graduate school was something about optical properties. Jan Tauc made his reputation with optical properties in semiconductors. So, that was what I was interested in or thought I could work in at the time. You should realize that my self-confidence in coming out of graduate school was about yay big, so I didn't really think that I could contribute much to anything. Something having to do with optical properties, I thought: "Oh, that would be neat if I could do that." So, that was following a little bit along in the path I had taken. What Jan was really known for was semiconductors. Not that I did any semiconductors at the time; we did metallic glasses. That was what I worked on as a postdoc. That was my first taste of real disorder in terms of glassiness.

PC: As you were just saying, your first paper on glasses was a work with Tauc. That work proposed a theory of metallic glass formability[8]. What led to the genesis of this paper?

SN: [0:10:14] (Gee, that's a long time ago.) The question that was in the air at the time, that we were trying to grapple with—one way or another—was

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[7] Jan Tauc: https://en.wikipedia.org/wiki/Jan_Tauc

[8] S. R. Nagel and J. Tauc, "Nearly-free-electron approach to the theory of metallic glass alloys," *Phys. Rev. Lett.* **35**, 380 (1975). https://doi.org/10.1103/PhysRevLett.35.380

why some materials were able to form a glass and others not, and why others would always crystallize on you. There's this ability of certain compositions of metallic alloys to form glasses, but you go away from that region and they don't do that so much. There had been a whole series of papers on this, basically from a packing point of view. Now, I'm a little bit hazy on what that was, but this was the group that was at Yale: Cargill[9], Thorpe[10], Weaire[11], and Polk[12]. They were talking about different kinds of packing arrangements for why these compositions would work, because it would form certain kinds of cavities that the smaller particles could fall into and so forth. This was the way that people were thinking about it at the time. I was interested in wedding what I knew about electronic structure and that. So, [I] just came naturally to think about it from that point of view. There was an argument to be made that this was energetically more favorable, because of the energetics of the free electrons. One of the most beautiful ideas that I ran into around that time was the idea of charge density waves[13]. I always thought that was just a lovely piece of physics. In some sense, this was a riff on those ideas, [explaining] why some [alloys] could be more stable by opening up a gap near the Fermi surface.

PC: Were these ideas that Jan Tauc was also interested in? Or was this your curiosity building up?

SN: [0:13:07] I don't think Jan was ever particularly in metals. He certainly understood the importance of metallic glasses as a new kind of glass. He was interested in it from that point of view, but his background was really very steeped in semiconductor physics. In semiconductors you've got a gap, while in metals you don't have a gap. I'm not quite sure if he knew what kind of questions to be asking. But he was one of these people who had tremendous taste in choosing problems. He smelled that there must be something there that's different and new, that's not there for [semiconductors]. So, he stepped away from what he had in semiconductor physics to go off to the metallic landscape. But I don't think he was interested in metals per se. That was probably not what his interest

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[9] See, *e.g.*, G. S. Cargill III, "Dense Random Packing of Hard Spheres as a Structural Model for Noncrystalline Metallic Solids," *J. Appl. Phys.* **41**, 2248–2250 (1970). https://doi.org/10.1063/1.1659198; G. S. Cargill III, "Structure of metallic alloy glasses," *Solid State Phys.* **30**, 227-320 (1975). https://doi.org/10.1016/S0081-1947(08)60337-9

[10] Michael Thorpe: https://en.wikipedia.org/wiki/Michael_Thorpe

[11] Denis Weaire: https://en.wikipedia.org/wiki/Denis_Weaire

[12] See, *e.g.*, D. E. Polk, "Structural model for amorphous metallic alloys," *Scripta Metallurgica* **4**, 117-122 (1970). https://doi.org/10.1016/0036-9748(70)90175-4; "Structural model for amorphous silicon and germanium," *J. Non-Cryst. Sol.* **5**, 365-376 (1971). https://doi.org/10.1016/0022-3093(71)90038-X; "The structure of glassy metallic alloys," *Acta Met.* **20**, 485-491 (1972). https://doi.org/10.1016/0001-6160(72)90003-X

[13] Charge density wave : https://en.wikipedia.org/wiki/Charge_density_wave

was, nor looking at it from the metallic perspective or instabilities due to something like the charge density wave phenomena. But he certainly appreciated it when we talked about it.

PC: This work acknowledges a conversation with Brian Bagley at Bell Labs[14]. How close were you with the Bell Labs research group and were you considering working at to Bell Labs at all?

SN: [0:15:15] Jan came out of Bell Labs. He was an émigré from Czechoslovakia. He was *in* Bell labs in 1968, at the time that the borders closed, so he stayed there. From Bell Labs, he then got his position at Brown. My connection with Bell Labs is very much from the outside. They had no use for anything I was doing. They never wanted to see me. I've always felt I was looking into the candy store from outside: "Oh! That must be a great place." But I never got to see it. I did give a talk there because Jan arranged it, but the people at Bell Labs were really not interested in what I was doing. Whether I wanted to go to Bell Labs or not was a moot point because they were not interested.

PC: What was the initial reception to that work on the theory of glass formability?

SN: [0:16:27] It was varied. Some people were intrigued, some people were dead set against it. One of the things is that the idea of how you could lower the energy is kind of an old one. It goes back to Hume-Rothery that certain alloys were more stable because of where they place the Fermi surface in view of where the structure goes. David Turnbull at Harvard[15], I remember, took it seriously, and he was Mr. Glass at the time. I mean he was Prof. Glass! He was certainly a very senior figure in the field. Jan told me that Phil Anderson[16] actually liked it, but I was so out of it I didn't even know who Phil Anderson was at the time. (Whoops! What does that say about me? I didn't realize that he was so incredibly important.) But people at Bell labs, they didn't want to have much to do with it. I remember Brian Bagley was dead set against it because it didn't conform their views of structure formation. The good question was always: "How would you go about testing for something like this?" I think with all of these things it's not particularly easy to figure out what a good test would be, what is the experimental way in which you would go and say: "Is this right or is that right? Which of these ideas?" The energy changes could be quite subtle in

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[14] See, *e.g.*, "Brian G. Bagley (1934-2022)," *Toledo Blade* (Jan. 1, 2023). https://www.legacy.com/us/obituaries/toledoblade/name/brian-bagley-obituary?id=38517294 (Accessed December 9, 2023.)

[15] David Turnbull: https://en.wikipedia.org/wiki/David_Turnbull_(materials_scientist)

[16] Philip W. Anderson: https://en.wikipedia.org/wiki/Philip_W._Anderson

order to give rise to a big effect. If you have something and it's in the exponent, then a small change in the exponent can make big differences to what the energetics really are implying for the system. So, that was part of what that was hard to make it easy to check.

PC: You described already a bit the structure of the glass community, but can you tell us more about how it worked at that point? Were there key meetings that everyone attended? Or what were the main questions that people were after?

SN: [0:20:07] There were a bunch of things that were kind of interesting at the time. I remember hearing a talk by Denis Weaire way back when I first [started]. He told us that no one really knew for sure that the hexagonal closed packed or the FCC lattice was most dense lattice. I thought that was amazing that that was a piece of thing that wasn't obvious to everyone. How could you even go about proving that? Then, I was told: “Oh! That's one of Hilbert's problems and goes back to Kepler and all of this stuff[17]. There's a rich history of this.” Then, 10 or 15 years later that was actually proved by Hales. So, that was something. I'm not sure, from the glass community, that that was an important thing, because I don't know that anything hung on that. But it kind of emphasized that even proofs about things having to do with crystals can be really knotty, difficult, and hard to have a conclusion of definite satisfaction.

That was one thing. But then there was a lot of stuff. There was one set of issues that came in because of a paper by Phil Anderson having to do with when you have interactions between particles[18]. Slowly the idea of interactions coming in to mediate what the energy is like gained attention. When two particles are close-by, you can’t start dealing with these things in a single-particle picture. The first time that that arose for me—it probably arose for lots of people long before that because this was impinging on the glass community—was Anderson’s work which was then followed up closely later by Marc Kasner and Helmut Fritzsche[19], [who] proposed valence alternation pairs as concrete model for how to see what the interactions could be. That was one set of issues. Then, there was a set of experiments that were by the [Naval Research Laboratory] group by Taylor, Strom and [Bishop] again on these same kind of issues[20]. Those

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[17] See, *e.g.*, Kepler Conjecture: https://en.wikipedia.org/wiki/Kepler_conjecture

[18] P. W. Anderson, “Model for the electronic structure of amorphous semiconductors,” *Phys. Rev. Lett.* **34**, 953 (1975). https://doi.org/10.1103/PhysRevLett.34.953

[19] M. Kastner, D. Adler and H. Fritzsche, "Valence-alternation model for localized gap states in lone-pair semiconductors," *Phys. Rev. Lett.* **37**, 1504 (1976). https://doi.org/10.1103/PhysRevLett.37.1504

[20] S. G. Bishop, U. Strom and P. C. Taylor, “Optically induced localized paramagnetic states in amorphous semiconductors,” *Phys. Rev. Lett.* **36**, 543 (1976). https://doi.org/10.1103/PhysRevLett.36.543.2;

were some of the issues, but those were all dealing with kind of semiconductor aspects of this. For whatever reasons, I didn't get into that. Partly because I was working on the metallic glass side of it, so those issues weren't at the fore then. But I think they became more in the fore for the amorphous semiconductor area.

PC: Once you joined Chicago, you became mostly free to choose your own research problems as an assistant professor. How did you select topics of interest? What was driving your program initially?

SN: [0:24:14] How honest do you want me to be?

PC: As honest as you can be.

SN: [0:24:21] Oh damn! Well, it started out really tough. I got here and—you're not supposed to say this out loud—but I got here, and I realized I had no idea what I wanted to do. Of course, I knew what I had done, and I was given certain startup funds. I spent them on something having to do with photoemission, and I realized, when it came to it, that I didn't want to do that. I had no interest in doing that. I was really trying to get rid of my career as quickly as you can possibly do it. So, I gave all that stuff away to colleagues. My senior colleagues were looking and saying: "Why aren't you doing something with this?" I don't know why it was their business, but they felt it was. So, I didn't follow the line that I thought I was going to.

I sat for months, just looking at the window trying to figure out what is it that I wanted to do. It was not a pleasant time. Slowly, I realized I was interested in a flexible lab. That is, I didn't want to be tied down to one big machine the way so many experimentalists do get tied down. So, the kind of stuff we did was—I should just say—flexible. I could use the same apparatus—there was electronics of various kinds that I could measure one thing with or another thing with—but it didn't require a specific idea that we're going to do the same thing over and over again with different examples.

We started off with doing transport in metallic glasses[21]. From that, I got into trying to say: "How about relating that to the structure?" Because of the ideas that I had about metallic glasses -- I said it was kind of like a

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"Optically induced metastable paramagnetic states in amorphous semiconductors," *Phys. Rev. B* **15**, 2278 (1977). https://doi.org/10.1103/PhysRevB.15.2278

[21] See, *e.g.*, S. R. Nagel, "Thermoelectric power and resistivity in a metallic glass," *Phys. Rev. Lett.* **41**, 990 (1978). https://doi.org/10.1103/PhysRevLett.41.990; S. R. Nagel, J. Vassiliou, P. M. Horn and B. C. Giessen, "Temperature dependence of the resistivity of Nb: Ni glasses," *Phys. Rev. B* **17**, 462 (1978). https://doi.org/10.1103/PhysRevB.17.462;

charge-density waves -- required knowing where the structure was with respect to the electronic structure. The mechanical particle structure of the system should be related to the electronic structure and I was hoping that we could see something about that. That was kind of what we started doing.

Then, there were few other things, but just in terms of the glassiness aspect of these, we started doing spectroscopy, dielectric response[22]. That became something that I didn't realize where it would go. We used it as a tool to do spectroscopy, which I don't think is exactly the way people had thought about it before. I don’t remember how many years it took us to do this, but it was a bunch of years. But we did this, so we had something like 14 decades or something close to 14 decades of frequency that we covered[23]. With that, you could really see that there were different regimes of the relaxation spectrum of supercooled liquids as they were cooling. You could look at different aspects of this, and different frequency regions. You could see what was going on. It really was kind of going back to what I had done—or learned about as a graduate student—which was optical properties. That is, you can see the different signatures of the electronic structure appear in different frequency ranges. That's kind of what came from this. You know that you have the whole spectrum, and so you have Kramers-Kronig relations[24] that tell you the real and imaginary parts are related, and you can use those to figure out something about what's happening at zero frequency if you know enough about the entire spectrum. If you covered all those decades of frequency, we could use that to get something about zero frequency.

We showed something there that I still like, but I'm still fighting the same battle and it's 25 years later! As you guys heard, I had to fight this with our good buddy Jean-Philippe [Bouchaud][25] at the Solvay conference[26]. Their view is that it couldn't possibly be right. But it predicts the same things that they found much later in the same thing and with the same scale of stuff. But they just don't want to believe it because it's linear response. It's not fancy theory land. (Sorry. I know you're both theorist, so I should

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[22] N. O. Birge, Y. H. Jeong, S. R. Nagel, S. Bhattacharya and S. Susman, “Distribution of relaxation times in $(KBr)_{0.5}(KCN)_{0.5}$,” *Phys. Rev. B* **30**, 2306 (1984). https://doi.org/10.1103/PhysRevB.30.2306

[23] See, *e.g.*, N. Menon, K. P. O'Brien, P. K. Dixon, L. Wu, S. R. Nagel, B. D. Williams and J. P. Carini, “Wide-frequency dielectric susceptibility measurements in glycerol,” *J. Non-Cryst. Solids* **141**, 61-65 (1992). https://doi.org/10.1016/S0022-3093(05)80519-8

[24] Kramers-Kronig Relations: https://en.wikipedia.org/wiki/Kramers%E2%80%93Kronig_relations

[25] Jean-Philippe Bouchaud: https://en.wikipedia.org/wiki/Jean-Philippe_Bouchaud

[26] *29th Solvay Conference in Physics "The structure and dynamics of disordered systems”*, David Gross, Marc Mézard and Giorgio Parisi, Brussels, Belgium, 19-21 October 2023. http://www.solvayinstitutes.be/html/29thsolvay_conf_physics.html (Accessed December 13, 2023.)

be careful about what I say.) But there's a bias that has come through. Don't look at the bias, look at what the data says! If you look at this, you see that there's something going on here, which is very odd and different from what would have been expected. [It] actually predicted that there was a temperature at which not only the relaxation stopped, which is what people had used with the Vogel-Fulcher law[27], and where the Kauzmann temperature was, which is where the entropy is going away, but also that there is a length scale. That's what this says because there is a static susceptibility which is diverging. It's at the same temperature, and all of the extrapolations lead to about the same temperature. This was a battle. We were able to show this[28], and I still feel like I'm fighting for legitimacy of that result even 25 years later. That's one of the things that we did at that time. But then that got us into other kinds of relaxation.

PC: I'd like to take you back a bit. Morrel Cohen[29] was working on a theory of glasses at a time you joined Chicago. How much did you interact with him on glasses?

SN: [0:32:05] Morrel was wonderful. Just mentioning Morrel's name makes me smile because of all the stories that took place at the time, of just dealing with Morrel. What was he like? I don't think he had a mean bone in his body, but he certainly also thought well of himself. So, it was both things at the same time, which is not always easy to appreciate.

A story just because where else will I get to tell the story? Here I was a young assistant professor. First year. Scared. I didn't know what end was up. Morrel was taking a whole group of people out for dinner with a distinguished speaker. In the middle of this dinner, with people all around the table, he stops the conversation with everybody and turns to me: "Sid, you're from Manhattan. How come you're not more sophisticated?" That was 40 years ago or more. I'm still trying to figure out an answer to that, and I have not come up with one. That's a Morrel story. You're asking for science and I'm giving you tidbits, but so be it. You asked about how I interacted with Morrel.

PC: If in any way, was there science between the two?

SN: [0:34:30] We talked about things. He certainly took an interest in what we were doing, and he left soon after. I've forgotten exactly when he was

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[27] Vogel-Fulcher-Tammann equation: https://en.wikipedia.org/wiki/Vogel%E2%80%93Fulcher%E2%80%93Tammann_equation

[28] N. Menon and S. R. Nagel, "Evidence for a divergent susceptibility at the glass transition," *Phys. Rev. Lett.* **74**, 1230 (1995). https://doi.org/10.1103/PhysRevLett.74.1230

[29] Morrel Cohen: https://en.wikipedia.org/wiki/Morrel_H._Cohen

there versus when I did various things, because he left to go to Exxon at some point not too long after I got to Chicago. He was interested, with Gary Grest[30], in doing the free volume theory of glasses[31]. It was certainly interesting, because he was doing this, and it legitimized that question as a good question to be asking about what was going on. What I found about it was that I didn't really know what to do with the theory. So, they have this theory and it looked like every time they wanted to get something out they had put something else in to begin with to get it to come out. This is maybe the way certain theories will go, but it didn't give a slam-dunk thing of: “If you do this, you can say something concrete about it.”

It was also the fact that I don't think of [experimentalists’] job as to go in and say which is correct and which is not correct. I mean that may be something it comes as a byproduct of what experiment does, but experiment is where you find new ways of looking at nature. You're not there to correct what other people are thinking about nature. You’re there to come out and figure: “Oh! Did you know that nature is doing this? And this is a different way of looking at it then you would have thought otherwise.” That's kind of what came out of that dielectric response stuff that I was telling you about. It showed that there was a whole new class of phenomena that [one] didn't really expect to be there. I think experiment and theory work best when they are kind of both independent and then touch base with each other at crucial times. But I don't know that there was ever a crucial time that free volume theory and our experiments had a way of touching base.

PC: You did nevertheless start to collaborate with Gary Grest and Aneesur Rahman[32] on molecular simulations of glasses at about that time[33].

SN: [0:37:28] That actually started before Morrel and Gary were working together which was after Gary left for Purdue. I started doing this work

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[30] Gary S. Grest: https://en.wikipedia.org/wiki/Gary_S._Grest

[31] See, *e.g.*, M. H. Cohen and G. S. Grest, "Liquid-glass transition, a free-volume approach," *Phys. Rev. B* **20**, 1077 (1979). https://doi.org/10.1103/PhysRevB.20.1077; G. S. Grest and M. H. Cohen, “Liquids, glasses, and the glass transition: a free-volume approach,” In: I. Prigogine, Stuart A. Rice, eds., *Adv. Chem. Phys., vol 48* (New York: Wiley, 1981): 455-525. https://doi.org/10.1002/9780470142684.ch6

[32] Aneesur Rahman: https://en.wikipedia.org/wiki/Aneesur_Rahman

[33] See, *e.g.*, G. S. Grest, S. R. Nagel and A. Rahman, "Quench echoes in molecular dynamics—A new phonon spectroscopy," Sol. State Comm. **36**, 875-879 (1980). https://doi.org/10.1016/0038-1098(80)90131-3; S. R. Nagel, A. Rahman and G. S. Grest, “Normal-mode analysis by quench-echo techniques: Localization in an amorphous solid,” *Phys. Rev. Lett.* **47**, 1665 (1981). https://doi.org/10.1103/PhysRevLett.47.1665; G. S. Grest, S. R. Nagel and A. Rahman, “Longitudinal and transverse excitations in a glass,” *Phys. Rev. Lett.* **49**, 1271 (1982). https://doi.org/10.1103/PhysRevLett.49.1271

with Gary while he was still here[34]. Then, I think Gary and Morrel kind of started working more together, either towards the end of Gary's time or after Gary had left his postdoc and had gone to Purdue[35].

PC: How did that that collaboration begin? How did you get involved in numerical simulations?

SN: [0:38:07] If any of you have ever met Anees Rahman you would be lucky people. Anees was a wonderful person. You were around him, and you just felt life is fun. He had a beauty in the stuff that he was doing. He was a very important scientist, but he was just great as a mentor to someone like me. I have been blessed with having certain mentors in my life who were those to whom I owe so much. Certainly Jan Tauc, whom I mentioned earlier, and Anees Rahman were two of them who, at that stage, helped me along. Someone who's more contemporaneous with me, Tom Witten[36], who I would also want to [name]. (We were more the same age. He was just a little bit older. Not very senior.) Those are the people who were really important. Leo Kadanoff[37] later, but not quite to the same level as Anees and Jan. These were people who were important. Your question was what? I've gotten got off your question.

PC: How did it come about? Did you reach out to them? Did they find out about you?

SN: [0:39:52] I reached out to Anees. I remember what I was interested in doing. I heard about two-level systems in the glasses. So, I thought: "We could go out and look for them in these systems". That was what the initial part was. Other people were doing similar things. Stillinger and Weber[38] were doing this at Bell Labs, and so forth. (They went off into their thing, the inherent structures[39].) We thought that we could do that—we didn't know they were doing it—but we got sidetracked by our own things.

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[34] G. S. Grest and S. R. Nagel, "Effective-field distributions and resistivity minima in amorphous ferromagnets," *Phys. Rev. B* **19**, 3571 (1979). https://doi.org/10.1103/PhysRevB.19.3571

[35] Grest was a Chaim Weitzmann Fellow and research assistant at the James Franck Institute of the University of Chicago in 1977-1978. He joined the Purdue faculty in 1979. See, *e.g.*, https://de.wikipedia.org/wiki/Gary_Grest

[36] Thomas Witten: https://en.wikipedia.org/wiki/Thomas_Witten

[37] Leo Kadanoff: https://en.wikipedia.org/wiki/Leo_Kadanoff

[38] See, *e.g.*, F. H. Stillinger and T. A. Weber, "Dynamics of structural transitions in liquids," *Phys. Rev. A* **28**, 2408 (1983). https://doi.org/10.1103/PhysRevA.28.2408

[39] See, *e.g.*, T. A. Weber and F. H. Stillinger, "Inherent structures and distribution functions for liquids that freeze into bcc crystals," *J. Chem. Phys.* **81**, 5089-5094 (1984). https://doi.org/10.1063/1.447498; F. H. Stillinger and T. A. Weber, "Inherent pair correlation in simple liquids," *J. Chem. Phys.* **80**, 4434-4437 (1984). https://doi.org/10.1063/1.447223; 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

What Gary and I got sidetracked by was echoes[40]. This was the thing that we did there. We wanted to get to very low temperatures, to be able to look at the inherent structure. We were just doing ordinary MD. We weren't doing something fancy. We weren't doing steepest descent or anything. We were just doing molecular dynamics. So, we quenched the system. We set all the velocities to zero, but of course the potential energy wasn't zero, so the temperature started to rise again. Then, we let it go and we hit it to zero again and it rose again. Then, lo and behold, there was something at the same time after our first [quench]. This time between the two quenches that we gave it, the temperature was going along and then had a dip and started to come back. I remember this is the first time I just thought: "This was so lovely."

I had a professor in undergraduate, who had been interested in echoes. He was one of the people who discovered the photon echo, Sven Hartmann[41]. We had a class from him where he talked about those. I always thought that echoes were one of the most beautiful phenomena that you can have.

Here it was. We were able to see this kind of echo in this system. Who would have thought? Then, we checked and it had all of the kinds of things that echoes were supposed to have. That was kind of where that came from. Then, of course, it's an echo. So what do you do with it? We tried to figure out what we could do with it. That was kind of what Anees and Gary and I did. It interrupted us going towards the important questions in glasses about low-temperature states and so forth, but probably computers weren't in those days up to the task of really looking for tunneling states. These echoes were an in-your-face phenomenon. That was something that made me very happy. I'm not sure it's very important, but it made me happy, which was important to me.

PC: How did this work logistically? Were you writing code at the time?

SN: [0:44:01] Gary and started something earlier than that, which was we were looking over tails of distribution. I'd have to look it up. I have forgotten what. That was the first thing that Gary and I had done. This was while he was a postdoc. This other, second set of stuff, was after he had gone off to Purdue.

PC: I was asking how did this collaboration work?

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[40] See also S. R. Nagel, G. S. Grest and A. Rahman, "Quench echoes," *Physics Today* **36**(10), 24-32 (1983). https://doi.org/10.1063/1.2915313

[41] Sven R. Hartmann: https://de.wikipedia.org/wiki/Sven_R._Hartmann

SN: [0:45:28] Anees was out at Argonne, and I was here at Chicago. Argonne is 30 minutes away by car. Anees knew I had a sweet tooth, so he would buy a whole box of Twinkies[42] and put it down next to the computer, so that I would come out there more often. This was how the collaboration took place. He was working with Michele Parrinello[43] at the time. That was also someone else in Anees' orbit then. Michaele was doing really important stuff[44], but Anees still found time to work with us on these echo phenomena.

PC: Were you doing your own coding?

SN: [0:46:27] I did some of it, but a lot of the coding was already done by Anees. He had already made glasses with McTague[45]. So, there was some density of states of glasses that existed. I kind of worked with that and just fiddled with some of the peripherals around the edges of the code. You don't want to ask me to do any coding. That's for sure.

PC: Was it Fortran based?

SN: [0:47:12] Yes, and as well there were still a computer cards. We didn't have screens. Those came in during that time.

PC: What was your general impression of numerical simulations at that time?

SN: [0:47:33] I loved it. I thought they were nice. You always had to be a little defensive because the world did not like computer simulations. They thought this theory but not really. It's theory, but not real theory. I never saw it as theory. I always thought of it as an experiment, so it really was kind of bucking the trend. As an assistant professor, I was called into the front office and told: "We didn't hire you to do this theorist junk. We hired you to be experimentalist, so go be an experimentalist." I thanked them very much and went back to doing what I was doing, because I didn't know how to do anything else. But it was what I was interested in, so I just followed that. I don't know if that answers the question. At the time simulation didn't get that much respect, although people [were] clearly intrigued. Quickly it caught on. It's just at the beginning it was: "Where

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[42] Twinkie: https://en.wikipedia.org/wiki/Twinkie

[43] Michele Parrinello: https://en.wikipedia.org/wiki/Michele_Parrinello

[44] Parrinello-Rahman barostat: http://www.sklogwiki.org/SklogWiki/index.php/Parrinello-Rahman_barostat

[45] 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

does this stand in the pantheon of what's worth doing? Or what does it show you?"

PC: Can you describe a bit the simulation community at that point, in the early to mid-80s? Were you regularly discussing with other simulators like Hans Andersen[46], Jean-Pierre Hansen[47], Dave Thirumalai[48] and Ray Mountain[49]?

SN: [0:49:33] I met some of them. Actually, I still, to this day, have never met Dave Thirumalai. I've never met him, but I met some of the others at the time. Certainly, Hans Anderson. There were various people, but I was not really in that community. I just happened to be doing something related but not in the mainstream. I had the good fortune of working with Anees. I never thought of myself as belonging to the simulation community. That was just what I was doing. Anees would tell me: "Oh! Did you know that Hans Andersen has figured how to do constant pressure ensembles[50] etc.?" That was interesting, but I didn't hear that because I went to conferences or anything. I heard because I worked with someone who was at the head of that field. Everyone looked up to him is my impression. My impression is —and how I felt about ever since—that whenever you say Anees's name everybody acknowledges that he was one of the main instigators of that whole area of science. There were other people like Stillinger[51] who were there. I did meet him one of the few times I went to Bell labs. He really tried to give me a hard time until Phil Anderson told him: "Shut up! Let that guy speak. So, I thought: "Oh! That was nice."

PC: In the early '90s, you specifically studied the question of a static correlation length in glasses using numerical simulations[52]. What brought you to this particular question and to this method for first studying it?

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[46] See, *e.g.*, J. R. Fox and H. C. Andersen, "Molecular dynamics simulations of a supercooled monatomic liquid and glass," *J. Phys. Chem.* **88,** 4019-4027 (1984). https://doi.org/10.1021/j150662a032

[47] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Jean-Pierre Hansen*, transcript of an oral history conducted 2023 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 5 p. https://doi.org/10.34847/nkl.8c89n6x5

[48] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Devarajan Thirumalai*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 19 p. https://doi.org/10.34847/nkl.a03aux8z

[49] See, *e.g.*, R. D. Mountain and D. Thirumalai, "Molecular-dynamics study of glassy and supercooled states of a binary mixture of soft spheres," *Phys. Rev. A* **36**, 3300 (1987). https://doi.org/10.1103/PhysRevA.36.3300

[50] H. C. Andersen, "Molecular dynamics simulations at constant pressure and/or temperature," *J. Chem. Phys.* **72**, 2384-2393 (1980). https://doi.org/10.1063/1.439486

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

[52] R. M. Ernst, S. R. Nagel and G. S. Grest, "Search for a correlation length in a simulation of the glass transition," *Phys. Rev. B* **43**, 8070 (1991). https://doi.org/10.1103/PhysRevB.43.8070

SN: [0:52:12] I don't exactly [remember]. I hope I've got the thing in the right order. The static correlation length, the dielectric response stuff indicated there was something like this. This was in the mid-‘90s, I think, probably not the early ‘90s. The question has always been: If there's a phase transition, you want some evidence of a length scale, not just a timescale, coming in. That was the issue. We had tried to measure a third-order correlation function, and we finally got to see something. Then, when we stopped and we looked at it, we realized: “Oh! The size of the signal that we see would have been [as] if the plates had been moving by an angstrom.” We decided you can’t trust that. So, we never published anything about that.

It was something that we were trying to do, because that was what people had seen this in the spin glass literature by looking at that[53]. This was one of the things that we were trying to study: to find that and then look at it in the simulations as well. But what those showed was that over the range that we could do simulations, we weren’t able to see anything. You could set up the expression for how to do it, but it didn’t show the phenomenon. But we learned how to get the energy to be conserved to all those last decimal points. We learned that part of it, how to do it.

I mentioned the dielectric response, [but] I should have said the other thing that was maybe more important was doing the time-dependent specific heat[54]. That was the experiment that we learned how to do. [We] invented that kind of spectroscopy where you do frequency-dependent specific heat, and you can see that the heat modes are going in at different frequencies. It's a spectroscopy just like dielectric response, but dielectric response is just infinitely easier to do and more precise. This [nevertheless] settled some issues as to what the specific heat was doing in these systems. That was another aspect. I should have mentioned that when I talked about what we started off doing, because that was an early thing.

PC: You mentioned spin glasses. How closely were you following the spin glass conversation, and how did you follow that conversation, if at all?

SN: [0:56:24] I was aware it was around. Certainly, I didn't know what to make of it, really. That is, to what extent are these the same or different kinds of phenomena. To be honest, I still don't really know the answer to that. I was

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[53] See Ref. 55.

[54] See, *e.g.*, N. O. Birge and S. R. Nagel, "Specific-heat spectroscopy of the glass transition," *Phys. Rev. Lett.* **54**, 2674 (1985). https://doi.org/10.1103/PhysRevLett.54.2674; "Wide-frequency specific heat spectrometer." *Rev. Sci. Instrum.* **58**, 1464-1470 (1987). https://doi.org/10.1063/1.1139434; P. K. Dixon, and S. R. Nagel, "Frequency-dependent specific heat and thermal conductivity at the glass transition in o-terphenyl mixtures," *Phys. Rev. Lett.* **61**, 341 (1988). https://doi.org/10.1103/PhysRevB.34.8667

aware enough of it that if you wanted to see something you really had to go to long times. You wait longer in order for things to get sharper, but you're never going to win by just going in that direction. Then, there was the work by the Bell Labs people. Laurent Lévy had done the higher-order susceptibility and showing that it did have a sharp peak and that it grew[55]. That's what made us think about trying to do that in the regular glasses, but we were not successful at it. What it did do is that we went off and worked ith Tom Rosenbaum[56], who was here at the time. With him, we did some spin glass stuff, trying to do a similar thing[57] as was done in structural glasses. Again, [we had] the same battle that we have with linear response in the glass community showing that there's evidence for some kind of a divergent susceptibility. There is also evidence in LiHoIFl glasses that Tom looked at—these are dipolar glasses—that it was a similar type of behavior. You could see effects in the linear response that could tell you where the transition temperature was going to be. What was nice about that case was that you could actually go and measure where the spin glass transition was because you could also do the nonlinear susceptibility there. But again, that's part of this 25-year thing, where we still are battling for acceptance. Will that ever be straightened out? We can see things, but the prevailing view of theorists is "You shouldn't see it, and therefore don't even pay attention to it"?

PC: Your interactions with Lévy and with Ray Orbach[58], who was also working in the field, was this all through the paper trail?

SN: [1:00:04] I don't know that I ever met Laurent Lévy, but I did know Ray. Ray was actually very good to me as a very young guy coming out. He invited me out to UCLA when I was just a youngster starting off my assistant professorship. He got to see some of the work I had done at Brown with Jan Tauc. I don't think we've talked much in the past 20 years about things, but early on he was one of the few who seemed to take what I said seriously.

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

[56] Thomas Felix Rosenbaum: https://en.wikipedia.org/wiki/Thomas_Felix_Rosenbaum

[57] W. Luo, S. R. Nagel, T. F. Rosenbaum and R. E. Rosensweig, "Dipole interactions with random anisotropy in a frozen ferrofluid," *Phys. Rev. Lett.* **67**, 2721 (1991). https://doi.org/10.1103/PhysRevLett.67.2721; D. Bitko, N. Menon, S. R. Nagel, T. F. Rosenbaum and G. Aeppli, "High-frequency dynamics and the spin-glass transition," *Europhys. Lett.* **33**, 489 (1996). https://doi.org/10.1209/epl/i1996-00368-1

[58] P. Charbonneau, *History of RSB Interview: Raymond Orbach*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 23 p. https://doi.org/10.34847/nkl.cfddyh9y

PC: In a 1996 study of the frequency-domain physical aging in a simple liquid, you found a qualitative agreement with spin glasses[59]. In that work you acknowledged conversations with Leticia Cugliandolo[60] and Jorge Kurchan[61]. How did these interactions and your appreciation of spin glass ideas take place?

SN: [1:01:35] 1996? I'm a little confused. I certainly got to know Leticia and Jorge when we ran the jamming workshop at ITP[62]. This seems to be just a little before. I'm forgetting where I would have run into them.

I remember. There was a conference that they held someplace in Europe. I remember being there. I forgot what the venue was, but I remember they were talking about waiting time studies. We talked a little bit about that, and I thought: "Hey! I can do that. We've got the stuff in glasses." They were talking about spin glasses. That came before the ITP workshop, because I remember getting up and telling everybody there: "There is this ITP workshop coming." The timing is about right for that. That's where I learned about it. I was at a conference. There was a whole European group of theorists there. Giorgio Parisi was there, and I'm pretty sure Jean-Philippe Bouchaud was there. I'm a little bit vague as to who [was there], but I remember it was those who were trying to understand what the meaning of the slow relaxation was, and how that interacted with the waiting time behavior.

PC: Was that your first time interacting with that community, or had you already been in touch?

SN: [1:03:52] I think that's probably right. There was the other community which has not been mentioned, which is Götze[63]'s mode coupling [theory group]. That was going on a lot of the time. That was the one that was probably much more prevalent in our understanding over here, at the

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[59] R. L. Leheny and S. R. Nagel, "Frequency-domain study of physical aging in a simple liquid," *Phys. Rev. B* **57**, 5154 (1998). https://doi.org/10.1103/PhysRevB.57.5154. **PC:** The article was submitted in September 1997, but a 1997 March Meeting abstract (submitted in fall 1996) cites a preprint by L. Cugliandolo. R. L. Leheny and S. R. Nagel, "Time Dependence of the A.C. Dielectric Susceptibility in Quenched Simple Liquids," *American Physical Society, Annual March Meeting*, March 17-21, 1997, abstract id. Q8.06 (1997) https://ui.adsabs.harvard.edu/abs/1997APS..MAR..Q806L/abstract (Accessed December 18, 2023.)

[60] Leticia Cugliandolo: https://en.wikipedia.org/wiki/Leticia_Cugliandolo

[61] Jorge Kurchan: https://fr.wikipedia.org/wiki/Jorge_Kurchan

[62] *Jamming and Rheology: Constrained Dynamics on Microscopic and Macroscopic Scales*, S. Edwards, A. J. Liu, S. Nagel and M. Robbins, Institute for Theoretical Physics, Santa Barbara, CA, USA, October 12-16, 1997. https://online.kitp.ucsb.edu/online/jamming2/ (Accessed December 18, 2023.)

[63] Wolfgang Götze: https://en.wikipedia.org/wiki/Wolfgang_G%C3%B6tze

time. I had a colleague, Gene Mazenko[64], who was part of that group of people studying mode coupling. I had a lot of trouble with that because I couldn't figure out how experiments related to what they saw. Here again, I had wonderful conversations with Dan Kivelson[65], who was part of the glass community at the time. We talked about that a fair amount, trying to figure out what could or couldn't be said about it.

FZ: Before the mid-'90s—say during the '80s, before this conference you mentioned—I'm not sure I understood how you were interacting with the spin glass community. Would you attend conferences or sessions, for example, in big meetings? Where were people talking about spin glasses? How did it work?

SN: [1:05:48] I don't think so. It's a long time ago, so I'm trying to remember, but I don't know how I even got invited to this one that I did mention. I don't know whether you guys were there.

FZ: I was in high school!

SN: [1:06:15] You could have been there. I should have been in high school. I would be so much happier now, had I been in high school then. I was certainly never part of that spin glass community. I was kind of involved in a variety of other types of things. During that time, there was some work that was on the glassy stuff and the susceptibilities, but at the same time I was also trying to understand the Aharonov-Bohm effect[66] in these weak-localization systems in 2D—it didn't need to be 2D—and how that's related to some symmetries. You could see this very simply in terms of symmetry argument, which had kind of gotten made opaque by doing these very sophisticated calculations. [If] you just looked at really small systems, you could see why you had to have that symmetry. There are different symmetries for even and odd number of particles. So, we had been doing things like that[67]. I have never really been a member of any of these communities. I was always kind of external to them.

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[64] See, *e.g.*, "Gene F. Mazenko," Academic Tree (n.d.). https://academictree.org/physics/peopleinfo.php?pid=165055 (Accessed December 18, 2023.) S. P. Das, G. F. Mazenko, S. Ramaswamy and J. J. Toner, "Hydrodynamic theory of the glass transition," *Phys. Rev. Lett.* **54**, 118 (1985). https://doi.org/10.1103/PhysRevLett.54.118

[65] See, *e.g.*, C. M. Knobler, A. J. Liu and R. L. Scott, "Daniel Kivelson," *Physics Today* **56**(12), 86–87 (2003). https://doi.org/10.1063/1.1650247

[66] Aharonov-Bohm effect: https://en.wikipedia.org/wiki/Aharonov%E2%80%93Bohm_effect

[67] See, *e.g.*, J. P. Carini, K. A. Muttalib and S. R. Nagel, "Origin of the Bohm-Aharonov effect with half flux quanta," *Phys. Rev. Lett.* **53**, 102 (1984). https://doi.org/10.1103/PhysRevLett.53.102; D. A. Browne, J. P. Carini, K. A. Muttalib and S. R. Nagel, "Periodicity of transport coefficients with half flux quanta in the Aharonov-Bohm effect," *Phys. Rev. B* **30**, 6798 (1984). https://doi.org/10.1103/PhysRevB.30.6798

FZ: This is why we are interested in your point of view. You were doing things that were related to what people were doing in spin glasses, but you were not a member of the community. We want to understand how the spin glass community was irradiating or not outside the inner circle of people who were doing spin glasses and 100% of their time. One possibility is that during the ‘80s and early ‘90s, spin glass people were mostly focused on equilibrium properties and equilibrium dynamics. Maybe the mid-‘90s is the moment when off-equilibrium entered the game, and so maybe that's why people started to make connections with other out-of-equilibrium systems. But I don't know if this is correct.

SN: [1:09:03] I'm interested in hearing what you have to say, because I haven't thought about this period for a long time. There were a couple of things that were going on right then. If we're talking about the end of the ‘80s, beginning of ‘90s, one other preoccupation that we had was going into granular matter -- sand piles. There were some of the same actors there. There was the work that Mike Cates and Jean-Philippe Bouchaud did about the elliptic, hyperbolic, and parabolic equations and so forth, about force chains going down to the bottom of a sand pile and so forth[68]. I remember being on the outside of that conversation and trying to understand what they were saying. To be honest, I didn't get very far. I'm not sure that it ever went very far after that, but it was a big hullabaloo in the field at the time, as they were trying to work this out. It was the physicists against the engineers. It wasn't the best time for either group, I think. I don't think it was a plus. But the point for us was that glasses and granular materials had a lot of things in common.

What was interesting about that was that I had all these friends who were doing simulations. It wasn’t that I was doing simulations, but all my best buddies were doing simulations. And they all seemed to be doing the same thing, but then I would have to ask them: “Are you doing a glass or are you doing granular material?” I had to ask them that because it wasn't obvious from what they were doing. It was by realizing that they were both doing these things, that I came to the realization that these things really had to be connected in some deep way. That's what brought about the idea of jamming. It came through this realization that: “Oh! All these people are doing these things. They're doing the same things, but not really realizing it.”

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[68] See, *e.g.*, J.-P. Bouchaud, M. E. Cates, J. R. Prakash and S. F. Edwards, “Hysteresis and metastability in a continuum sandpile model,” Phys. Rev. Lett. **74**, 1982 (1995). https://doi.org/10.1103/PhysRevLett.74.1982; J.-P. Bouchaud, M. E. Cates and P. Claudin, "Stress distribution in granular media and nonlinear wave equation," *J. Physique I* **5**, 639-656 (1995). https://doi.org/10.1051/jp1:1994195

You're asking specifically about where the spin glass stuff came in. Probably, I was more aware of the experimental stuff than the theoretical stuff. [I knew] enough to know why you want the higher-order susceptibility. That's a kind of theoretical argument, but this is what people do to measure it, and this is what they see. I got that. But what I really wanted to know about these things is—as you've heard me say often before—if we do all this theoretical work, what does it have to tell me about experiments? If it doesn't, not that it's wrong, it's just that it's probably not going to be what I'm catching. The fact that you have all of these wonderful results about infinite dimensions or replica symmetry breaking and so forth, those are words to me until I say: "Oh! This is what I can actually do to… Ah! I get it." I always feel totally inferior when I talk to you guys, because I'm not catching all the important ideas, but I do have it through this particular lens of where they're going to make an impact on me. I don't know if that's apologetic enough. I'm very sorry, but that's [what it is].

FZ: It's not necessary. But for some reason, the work about Cugliandolo and Kurchan, or not specifically their work but when people started talking about aging and dynamic, this resonated somehow better with your interests.

SN: [1:14:41] Because it was something I could see. As I recall, coming away from that conference: "Oh! They do this, and they should see this kind of behavior after the waiting time. Well, I can do that." I'm not a saying that I understood what the theory was, but I can certainly do an experiment on that. It was along the lines of what we were doing anyway, so it was an easy add onto to it. Bob Leheny has continued some of that, but I didn't do more than that after he left as a graduate student[69].

PC: We have a bit more on that theme. When you wrote we now know to be very influential review and supercooled liquids and glasses in 1996, you do refer in your conclusion to spin glasses as "important model systems which are being studied theoretically and are analogous to structural glasses and some respect"[70]. Was the intuition or the enthusiasm for spin glasses coming directly from that conference you had attended or was this a more general perspective?

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[69] Robert L. Leheny, *Dielectric susceptibility study of a strong glass forming liquid*, PhD Thesis, University of Chicago (1998). https://catalog.lib.uchicago.edu/vufind/Record/3558459. See, *e.g.*, H. Yardimci and R. L. Leheny, "Memory in an aging molecular glass," *Europhys. Lett.* **62**, 203 (2003). https://doi.org/10.1209/epl/i2003-00347-0

[70] M. D. Ediger, C. A. Angell and S. R. Nagel, "Supercooled liquids and glasses," *J. Phys. Chem.* **100**, 13200-13212 (1996).

SN: [1:16:07] It was probably more general. We'd all heard about spin glasses and the ideas from spin glasses for a long time. This conference was one of the places where I got to hear about some specific ideas that had resonance, but certainly we all thought that spin glasses were going to be the simpler model of glasses. Why did we think that? Well, very smart people have told us that they would be. So, how could they not be? Now, I realize maybe that's not the case, but I'm beginning to rethink what is the relationship between these. What makes them the same and what makes them different. But certainly, at that time, that was in the air, and it had been in the air since the ‘70s. That wasn't a new thing to come about. I think I'm remembering this right, but I remember on my flight from Providence to Chicago, when I was coming for my job, I heard about spin glasses. What are these things? Is it going to be really the simplified view of this? I remember it was some trip that I took between the two. It could have been a little bit later, but it was something that was in the air. It was glassiness in a simplified system, and therefore could be the basis on which things were to be understood. So, I don't think that came later. We all believed it but didn't know what to do with it.

PC: A group that made that connection quite explicitly in the late ‘80s was Kirkpatrick, Thirumalai and Wolynes[71]. How influential were these ideas on experimental research on structural glasses from your standpoint?

SN: [1:18:58] I understand that that work is tremendously important now for our understanding; I can't say that it influenced our experimental work. But it's also true that we had done what we thought was important. We spent a long time doing the specific heat spectroscopy and a long time doing the dielectric response work. Those were fairly long, drawn-out experimental efforts. If I had continued with those, I'm not quite sure what else I would have wanted to do with that. More interesting to me was getting into the granular stuff around that time, so the late ‘80s, early ‘90s. I forget what time was the Wolynes-Kirkpatrick-Thirumalai?

PC: It was the late ‘80s, roughly ‘87 to ’89.

SN: [1:20:29] It was around that time that our interests were beginning to shift a bit from that to the sand piles.

FZ: Maybe a last question on this before we move to the granular part. You gave a talk at the March Meeting in 1996 entitled “Is there a glass

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[71] See, *e.g.*, T. R. Kirkpatrick, D. Thirumalai and P. G. Wolynes, "Scaling concepts for the dynamics of viscous liquids near an ideal glassy state," *Phys. Rev. A* **40**, 1045 (1989). https://doi.org/10.1103/PhysRevA.40.1045

transition? Similarities with spin glasses"[72]. Do you recall what was the content of that talk and what was the discussion around it? How did people react to it?

SN: [1:21:24] I don't remember, to be honest, but my guess is what we were doing around that time—as I already mentioned—was the susceptibility over a wide range of frequencies. We can see that there is new behavior in the high frequency tail. It's a spectroscopy, so we can focus on the tail and see what the tail does as a function of temperature. We were getting something about that. That indicated that at some point the tail would become flat. When the tail becomes flat, and if it goes all the way down to zero frequency, then we have this divergent susceptibility. This was that that part. Also at that time, we had this work I mentioned with Tom Rosenbaum, Lithium Holmium Yttrium Fluoride spin glasses, which showed in the linear susceptibility that there was a similar structure. I know there was a period around that time that that's what I was trying to get people to realize. There is something analogous in the two. Not exactly the same because the structures appear in different places, but they are both low frequency phenomena. They enter in different ways, but the linear susceptibility is having this weird divergence not because there is a peak in ε'' or χ''. It's not a sharp peak in absorption part. It's that there is a very broad behavior, and the integral in frequency over that broad behavior is what's giving you the linear part. That's why it's so hard to see, and why you have to work harder to be able to gather that spectrum to be able to see how the integral over it is producing something very interesting. That was that picture that I recall at that time trying to push, and so that may have been what that that talk was about.

PC: As you've alluded to a couple times already, in the late '80s you started to be interested in granular systems. This work was initially done in collaboration with Tom Witten and Heinrich Jaeger[73]. How did this new direction come about in your lab, or in that group?

SN: [1:24:22] You all know Tom. Tom is great. Tom was at Exxon. He was going to start at Chicago some time later[74]. I knew Tom since I was a graduate

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[72] S. R. Nagel, "Is There a Glass Transition? Similarities with Spin Glasses," *American Physical Society, Annual March Meeting*, March 17-22, 1996, abstract id. O'3.002. https://ui.adsabs.harvard.edu/abs/1996APS..MAR.O3002N/abstract (Accessed December 19, 2023.)

[73] See, *e.g.*, H. M. Jaeger, C.-H. Liu and S. R. Nagel, "Relaxation at the angle of repose," *Phys. Rev. Lett.* **62**, 40 (1989). https://doi.org/10.1103/PhysRevLett.62.40; H. M. Jaeger, C.-H. Liu, S. R. Nagel, T. A. Witten, "Friction in granular flows," *Europhys. Lett.* **11**, 619 (1990). https://doi.org/10.1209/0295-5075/11/7/007

[74] Witten joined the University of Chicago in 1989. See, *e.g.*, "Interview of Thomas Witten by David Zierler on September 18, 2020," *Niels Bohr Library & Archives,* American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/47144

student, and it was always fun to talk science with him. He told me about self-organized criticality—type of arguments[75]. He was still at Exxon at the time. So, that was at the back of my head. I was getting a new postdoc, who was Heinrich Jaeger[76], at the time. I suggested to Heinrich: "Should we try this?" And Heinrich was game. So, we started to look at avalanches in granular materials, to look for power-law correlations.

Tom had told us about the theory, but he was not involved in those experiments. He was involved in a later paper having to do with how to understand friction in these systems. The first paper on that was *Relaxation at the angle of repose*[77], one of the titles I'm most proud of. It was to look at what kind of spectrum comes out of the avalanches of those systems. This had been very much popularized by Per Bak, Chao Tang and Kurt Weisenfeld[78], and particularly by Per Bak[79], who was not known for being shy about his work. This was supposed to be the theory of everything. *How Nature Works*[80] was the title of his book on that.

What we found was that there's no power-law distribution of avalanche sizes. It was relaxation oscillation. It was just a first-order phase transition. You haven't mentioned the sign I have above my lab door and at the top of my website which says: "Experiments: Where theory comes to die". This was one of the cases, where a number of theorists would come in, and we would show them what was going on. I would have an example on my desk to show them: "See, you move it like this, and suddenly there's an avalanche, and then nothing as you move it more and more, until there's another avalanche." And they'd say: "It's got to be wrong. The theory says this." [I'd say:] "Wait a minute, you've got this wrong. It's not what the theory says. It's what the experiments tell us." It made me realize that there's this supremacy in the world of theory over experiment. I have to do my best to battle that, to show that not only are the experiments the important thing, and the right thing. But it's also where new ideas come as well. Of course, I'm overstating this because they come from everywhere, but it gets forgotten that it comes from experiments. That's why I feel my job is to push that.

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[75] Self-organized criticality: https://en.wikipedia.org/wiki/Self-organized_criticality

[76] Heinrich Jaeger: https://de.wikipedia.org/wiki/Heinrich_Jaeger_(Physiker)

[77] See Ref. 73.

[78] P. Bak, C. Tang and K. Wiesenfeld, "Self-organized criticality," *Phys. Rev. A* **38**, 364 (1987). https://doi.org/10.1103/PhysRevA.38.364; "Self-organized criticality: An explanation of the 1/f noise," *Phys. Rev. Lett.* **59**, 381 (1987). https://doi.org/10.1103/PhysRevLett.59.381

[79] Per Bak: https://en.wikipedia.org/wiki/Per_Bak

[80] P. Bak, *How nature works: the science of self-organized criticality* (New York: Copernicus, 1996).

PC: You mentioned earlier how you had seen the analogy between people studying glasses and people studying granular systems, and essentially looking at the same systems and asking the same questions made you realize that there was a bridge that was missing. That's what led to the organization of the ITP Workshop in '97, I guess. Can you tell us a bit about how that came about with your co-organizers Sam Edwards[81], Andrea Liu[82], and Mark Robbins[83]?

SN: [1:29:18] I had been talking about these ideas with people here at Chicago. At the time, that was Tom Witten and Sue Coppersmith[84]. They were here. They both encouraged me to send this [as a proposal for an] ITP workshop. I'm not sure I would have thought of doing that myself. So, I put that in. I remember right in the middle of the NBA playoffs. How could they do this to me? NBA playoffs: the Bulls versus probably the Sixers. I don't know at the time. I get a call from the chair of ITP board, saying: "We can't let you do this because you're an experimentalist. We need to get another theorist to help on this. How about Andrea?" I said: "Sure. Just shut up and let me go back to watching the Bulls game." That's how it came. They did not want to have an experimentalist run that, because they didn't think an experimentalist could do it. Then, once we had Andrea and I together, that was a great thing. I was very delighted by that, but then we brought in Sam and Mark. But it started off with Andrea and myself.

PC: Did you know Andrea from before?

SN: [1:31:03] I had met her a few times, but I didn't really know her. I met her at UCLA. I probably met her at Exxon, when she was a postdoc there, but we weren't buddies in any sense then. We hadn't really started collaborating. The collaboration really started after the workshop was over. We worked hard to put the workshop on and make it good together, but we didn't have research plans together until afterwards.

PC: In an interview you gave earlier this year, you mentioned how not serious that work was perceived at least at the beginning, and maybe even by you[85]. When did granular work turn serious in your mind?

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[81] Sam Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)

[82] Andrea Liu: https://en.wikipedia.org/wiki/Andrea_Liu

[83] Mark Robbins: https://en.wikipedia.org/wiki/Mark_O._Robbins

[84] Susan Coppersmith: https://en.wikipedia.org/wiki/Susan_Coppersmith

[85] Rachel Crowell, "Sidney Nagel Delights in Disorder: An interview with the recipient of the 2023 APS Medal," *APS News* **32**(1) (2023). https://www.aps.org/publications/apsnews/202301/nagel.cfm (Accessed December 19, 2023.)

SN: [1:32:21] That's kind of a hard question. I'm not sure I've ever thought of anything I do as serious. So, to the extent to which I get to go against that, I always thought that framing the work in terms of self-organized criticality, that seems serious. How could it not be. Theorists had said something about that. So, we did that hard work, Heinrich and I and the student Chu-Heng Liu[86]. We worked on that, and we published on that. Then, that was kind of over, because there's nothing more to be done. You see the avalanches and they're not what they said. But in doing so, we had seen so many other phenomena in this system. I remember sitting down, saying: "Look, I can drop this and go back to doing other stuff. Or I can say: "No. There is something interesting here. The whole thing is behaving in weird ways. What can we do? Should we follow that up?" And I remember just saying: "I don't want to be the kind of physicist, who was just going to jump on something for a one-time thing and be off because it was an easy picking. It should be done with a certain sense of seriousness to this."

So, the sense of non-seriousness that I had was probably always because of the inferiority complex [I have]. If I'm doing it, it can't be serious. But that was true about everything I've ever done. If I think back to that time, I remember that afternoon thinking about it, that was kind of a serious discussion I had with myself. How could I not take this [further]? You can't just see these things, pretend they don't exist, and then say everything else is interesting. It wasn't as if I was doing something else that I felt was more interesting. I saw this stuff [and thought]: "Well, is there a way of looking at it?" What was surprising to me is that you look back on this and say: "That's an obvious question, that's an obvious question, that's an obvious question that we asked." But at the time, I had no idea what the right obvious questions were to ask. Somehow, that took a while. "Oh! Sound in granular materials[87]. What is that like?" That's kind of obvious one might want to ask that, but it took a while. Or "How does tapping make this thing collapse?[88]" Again, kind of obvious now, but it wasn't obvious to us then. What we liked was somehow to be able to ask some of these questions that we thought could then have larger ramifications. The stuff with the tapping, that started off with just tapping this tube containing sand. We have it right here. It's been around for a while. You just tap it, and the height goes down, it goes down, and then it goes down some more. To make an experiment out of that, we didn't think would be too hard, but it took a long time to do. Then, it was after doing that, that we realized: "Oh!

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[86] Chu-Heng Liu, *Spatial patterns of sound propagation in sand*, PhD Thesis, University of Chicago (1994). https://catalog.lib.uchicago.edu/vufind/Record/1668122

[87] C.-H. Liu and S. R. Nagel, "Sound in sand," *Phys. Rev. Lett.* **68**, 2301 (1992). https://doi.org/10.1103/PhysRevLett.68.2301

[88] J. B. Knight, C. G. Fandrich, C. N. Lau, H. M. Jaeger and S. R. Nagel, "Density relaxation in a vibrated granular material," *Phys. Rev. E* **51**, 3957 (1995). https://doi.org/10.1103/PhysRevE.51.3957

You can actually begin to test things like Edwards compactivity, temperature or whatever you want to call that[89]. It's a fluctuation-dissipation theorem about specific heat. Volume fluctuations and temperature. We could measure volume fluctuations; therefore we can get temperature, and we could see if it made sense. Again, I wish I could say we knew from the beginning that that was what we were doing, but no. It was that slowly we saw what you could do with this. We could see what nature trying to tell us. Then, thinking about it and realizing: "Oh! This is how to think about it, so that you can put it into the language of statistical mechanics or other kinds of physical systems."

PC: Your exposure to the Edwards ensemble, did that come from the ITP workshop as well?

SN: [1:37:57] No, I knew about that for longer. Anita Mehta[90] had come through Chicago earlier on. So, this must have been in the '80s, I would guess. I have forgotten when I first met her, but she came through. She came from Sir Sam's group. Certainly, I knew about it then if not before.

PC: But that had not gone into the design of the experiment. Is it after seeing the phenomenon that you became interested?

SN: [1:38:39] Who would have said: "Oh! That's how you're going to measure this." I'm not smart enough to have thought of that. It was that we thought: "How long does it take to reach equilibrium?" That's something that's glassy. That's a glassy question. There are time scales. That's where I was coming from, from the time scale part. This is part of why I saw these two things as part of the same kind of question.

PC: As you mentioned earlier, after the ITP workshop you started collaborating with Andrea. How was the program shaped at that point? What questions were you pursuing in the immediate aftermath? What was the project?

SN: [1:39:37] What was it that we were trying to do in the program? You've got the field of people doing granular material and so we brought those there. You have people who were doing glasses, and we brought them there. Can you get them to talk to one another? They're polite, but they didn't truly engage. It was a good workshop, because people did pay attention, but I think everybody was a bit confused as to: "What are we supposed to be doing? Give me something to calculate. Give me something

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[89] See, *e.g.*, S. F. Edwards and R. B. S. Oakeshott, "Theory of powders," *Physica A* **157**, 1080-1090 (1989). https://doi.org/10.1016/0378-4371(89)90034-4

[90] Anita Mehta: https://en.wikipedia.org/wiki/Anita_Mehta

else." We were trying to ask: "Is there a different way of looking at these phenomena?" That's kind of what we were trying to do. There were some people there who really pitched in and were very [involved]. Two of them were Shlomo Alexander[91] and Tom Witten. [I'm not sure] exactly how things went from there, but we left the program thinking: "That was kind of fun." But the ideas sat around, and they began to attract attention. This is myopic, only from my point of view, and other people may think about it differently.

Andrea and I were asked to look at this paper by Mike Cates and company about jamming[92]. We we're trying to figure out what the hell to say. During trying to figure out what to say, we came up with the idea of the phase diagram[93]. That's where that came out. That, somehow, put it in focus for us. And I think maybe for a lot of other people in field it's the same. "Yes! This is how these things are related." We have these axes, and you can vary the various things. You have this phenomenon around the origin, which has these general behaviors. You go far enough away, and you don't have those general behaviors anymore. That gave me a point for which I could start to think. I don't know if it did for others in the community. I can't speak for others, but for us it did. Then, we could begin to ask questions. We asked: "If you have a diagram like this, then there's got to be some point *J* in there. If there's a point *J*, then what are its properties?" You can do simulations in this in a very clean way, and so we could do this, and we could vary these things and begin to get clean results[94]. Thank god, we didn't put in friction, because then you'd never get clean results. Not that friction isn't important, but there were certain choices that we made that made it possible to say clean things about certain cases and we were able to get clean results.

PC: How did that collaboration work? You were two centers fairly distant from one another?

SN: [1:44:27] Two thousand miles apart. It was great, because she was earlier than me. When five o'clock came around, my life became really calm here,

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[91] See, *e.g.*, Z. Luz, R. Bruinsma, Y. Rabin and P.-G. DeGennes, "Shlomo Alexander," *Physics Today* **51**(12), 73–74 (1998). https://doi.org/10.1063/1.2805729

[92] M. E. Cates, J. P. Wittmer, J.-P. Bouchaud and P. Claudin, "Jamming, force chains, and fragile matter," *Phys. Rev. Lett.* **81**, 1841 (1998). https://doi.org/10.1103/PhysRevLett.81.1841

[93] A. J. Liu and S. R. Nagel, "Jamming is not just cool any more," *Nature* **396**, 21–22 (1998). https://doi.org/10.1038/23819

[94] See, *e.g.*, C. S. O'Hern, S. A. Langer, A. J. Liu and S. R. Nagel, "Random packings of frictionless particles," *Phys. Rev. Lett.* **88**, 075507 (2002). https://doi.org/10.1103/PhysRevLett.88.075507; "Jamming at zero temperature and zero applied stress: The epitome of disorder," *Phys. Rev. E* **68**, 011306 (2003). https://doi.org/10.1103/PhysRevE.68.011306

because no one was bothering me. It was in the middle of her afternoon before she had to go home. It was great. Every day we would talk for hours at a time. We hired a postdoc, Corey O'Hern[95]. It was delightful. No one cared about what we were doing. It was wonderful. No one gave a damn. It didn't matter whether we were saying important stuff or not important stuff or if someone was going to take us seriously or not. We weren't under the microscope for this. Then, unfortunately or fortunately, other people got interested and it became less fun, because people were paying attention.

PC: Was this all done over the phone? Or would the postdoc travel back and forth?

SN: [1:45:44] It was all done over the phone. It was before Skype, before Zoom. We just had telephone. Telephone was great. You could lie on the floor. No one would know you were lying on the floor whereas with Zoom you can't lie on the floor anymore.

PC: Beyond the point *J*, another important notion that came out of this study is that of marginal stability. How did that come about in your work? Did this also have deep roots in work you had done before, or was this really a new idea that came out?

SN: [1:46:30] What we were trying to understand was what is this crazy thing that we've got there. We were struggling because you change the potential and everything changes. That's not supposed to be the way phase transitions work. So, how do you understand this? We tried to study this. We were slowly piecing these things together to realize: "It's not the potentials, it's somehow a length scale that is driving this. So, where's the length scale?" That was when Leo Silbert came in[96]. He was our postdoc too. He looked at the normal modes of these systems, which is something I learned from Anees Rahman way back when. That was one of the lasting things that I got from this. So, we looked at that, and we realized: "This is how you can begin to see that there are two length scales in the problem: the transverse and longitudinal length scales."

Things began to fall into place, but you realize also that this density of states was flat all the way down to zero frequency, and nothing is supposed to be flat all the way down to zero frequency. That's not the way [it's

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[95] "Corey O'Hern," *Physics Tree* (n.d.) https://academictree.org/physics/peopleinfo.php?pid=171012

[96] M. Wyart, L. E. Silbert, S. R. Nagel and T. A. Witten, "Effects of compression on the vibrational modes of marginally jammed solids," *Phys. Rev. E* **72**, 051306 (2005). https://doi.org/10.1103/PhysRevE.72.051306; L. E. Silbert, A. J. Liu and Sidney R. Nagel, "Vibrations and diverging length scales near the unjamming transition," *Phys. Rev. Lett.* **95**, 098301 (2005). https://doi.org/10.1103/PhysRevLett.95.098301

supposed to be] in all the solid state courses that I'd ever taken. I remember I took one from Leo Kadanoff, and he said: “No, you can't get it going down to zero. It always has to go as ω^{d-1}.” Here, jamming was behaving very differently. This is where we realized that the jamming point is in some sense a pole. It’s a pole of disorder. It's the epitome of disorder. You have got some length scale which has gotten larger than any system size when you have gotten down to that point. You can't get elasticity in the normal sense. So, there's some sense in which averaging doesn't work anymore. That's where that aspect of the idea came from.

Matthieu Wyart[97] was a graduate student, and he was working on some of these things. Also, he was thinking about marginality in terms of what happened with pressure. This is now what's happening not exactly at the transition, which is marginal, but away from it. The system has to rearrange to withstand the pressure that it’s at. Those are ideas that Matthieu has ownership of. There are a lot of different pieces that come together here, but those are the ideas.

The length scales and the fact that there are two of them is still to me puzzling in a way. This is the thing that Andrea and I used to bemoan towards one another: “God, why can't we find a spin model for this? Because people then would start to take us seriously.” No one was paying any attention. We don't have a spin model, and if there is no spin model no physicist is going to be interested. That's basically true, but what I just realized is that it's because there's something really fundamentally different about this transition than the spin ones. You asked earlier about relations been spin glasses and this. It makes me think: Look, there's something about jamming which is not in spins. There's something fundamentally different here that we've now got hold of its tail feathers a bit. There is something new and different that's not just the spin glass, because otherwise I would have had a model for this. But if you were going to make a model of this with spins, it would be such a torturous model, it would be so fake that no one would particularly take it seriously. This is the right way to study this kind of transition. I'm not sure I know of other examples. I can get the critical phenomena of boiling and melting and freezing and so forth. Gas-liquid, gas-solid, all those transitions are just fine. I find spin models that are equivalent to those phenomena. But I don't know an equivalent spin model for jamming that allows long-range interactions and rearranges at the transition to make the transition into somehow new. It's not the same.

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[97] Matthieu Wyart: https://en.wikipedia.org/wiki/Matthieu_Wyart

PC: Did you ever get teach about structural glasses or jamming at Chicago or elsewhere?

SN: [1:53:46] I gave a couple of lectures. There was one thing at Eindhoven. They have these master classes. I came back to Chicago after that, and we were doing stuff on jamming with part of our MRSEC[98]. I was at some meeting, and I realized no one knew what jamming was. So, I then gave a whole bunch of lectures to teach jamming as we knew it then. (This was a bunch of years ago.) Then, at Giulio's Beg Rohu, I gave some lectures there[99]. They're very nuts and bolts-y because that's how I think. I'm sure it could be done in a much more fancy way.

PC: One can look through your career and your production as being expressed in sort of artful imagery, including coffee stain rings[100], figures and patterns revealed by pulling apart paint between plates[101], fluid droplet break off and splashing[102]. Was your glass and jamming work part of that? Was there some aesthetic drive for you, or was this just separate?

SN: [1:55:18] I think that aspect grew on me as time went on. At the beginning, when I was just doing glassy stuff, probably not. I don't think that there was an aspect there that had the visual appeal of what I'm after. One of the appeals of doing the granular was: "Oh! I start to do granular stuff, and I get to see it." I really got to see the damn thing. It wasn't hidden behind anything. Now, I think mustard seeds are beautiful[103]. And watching their flow along the surface, that's really neat. I get excited about that in a way that I probably wouldn't get excited if I'd only seen a signal. I did get excited when I saw echoes, I have to admit. The granular material was the beginning of saying: "Yeah, you can get the best of both worlds." I started

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[98] S. R. Nagel, "University of Chicago Materials Research Science and Engineering Center", *US National Science Foundation* Award Abstract # 0213745 (2002-2008). https://www.nsf.gov/awardsearch/showAward?AWD_ID=0213745&HistoricalAwards=false (Accessed December 20, 2023.)

[99] *The Beg Rohu Summer School: Glass and Jamming Transitions,* G. Biroli, Beg Rohu, France, 28 May-9 June 2012. https://ipht.cea.fr/Meetings/BegRohu2012/index.html (Accessed December 20, 2023.)

[100] See, *e.g.*, R. D. Deegan, O. Bakajin, T. F. Dupont, G. Huber, S. R. Nagel and T. A. Witten, "Capillary flow as the cause of ring stains from dried liquid drops," *Nature* **389**, 827-829 (1997). https://doi.org/10.1038/39827

[101] S. Alqatari, T. E. Videbæk, S. R. Nagel, A. E. Hosoi and I. Bischofberger, "Confinement-induced stabilization of the Rayleigh-Taylor instability and transition to the unconfined limit," *Sci. Adv.* **6**, eabd6605 (2020). https://doi.org/10.1126/sciadv.abd6605

[102] See, *e.g.*, L. Xu, W. W. Zhang and S. R. Nagel, "Drop splashing on a dry smooth surface," *Phys. Rev. Lett.* **94**, 184505 (2005). https://doi.org/10.1103/PhysRevLett.94.184505

[103] See, *e.g.*, H. M. Jaeger and S. R. Nagel, "Physics of the granular state," *Science* **255**, 1523-1531 (1992). https://doi.org/10.1126/science.255.5051.1523; D. M. Mueth, G. F. Debregeas, G. S. Karczmar, P. J. Eng, S. R. Nagel and H. M. Jaeger, "Signatures of granular microstructure in dense shear flows," *Nature* **406**, 385-389 (2000). https://doi.org/10.1038/35019032

out wanting to be a literature major and ended up in physics, which seemed to be as far away as I could get, but this let me have an entry back into the world of aesthetics. That was very important for me. Starting off with the granular experiments because we were watching it, we could see it. I didn't need terribly fast cameras and so forth. Then, we got into the drop pinch off problem[104], and we had to get really high-speed cameras to start doing this and do photography with high-speed strobes and so forth. I love that. I'm just so sorry that they invented digital photography because it was so much fun to do it the old-fashioned analogue way. It's not to say that digital photography isn't better, but as far as the enjoyment is for me, I just love going to the dark room and smelling all those chemicals.

PC: Is there anything else you would like to share with us about this era that we may have missed or overlooked?

SN: [1:57:55] I didn't realize you were going to be asking all these questions. I'm kind of in awe that someone went back to see that I gave a talk on these things. Jesus! That's amazing. Thank you for having done that. It's certainly fun to look back at all of the work. I feel a little bit bad in what I've said. So many people have been important in my career, and I have not been able to give appropriate credit to them in this. I just started off talking about the ideas rather than all the people who helped me. My graduate students, my postdocs have been great. I managed to mention a few when it came along certain topics, but really, they were [essential], so I want to acknowledge all of them. My colleagues have been great. We all take credit for ideas that somehow we had something to do with, but I can't take credit for most of these things. The whole ideas of jamming and rheology and so forth, we had started this [at the] ITP workshop. Tom was involved in that. I don't think he gets enough credit for that. Sue was involved in that. So, I feel guilty being the one being asked about this, but all of these things really go to all these people. Of course, there is Andrea, and then Corey and Leo Silbert. And the atmosphere here at Chicago is absolutely essential to my mental and spiritual health. People let me do this even though it was weird stuff. This was not supposed to be something a serious professor would do, and they let me do it, so I'm grateful.

PC: In closing have you kept notes, papers, or correspondence from over these times? If yes, do you have a plan that deposit them in an academic archive at some point?

SN: [2:01:09] I think this all came from the time of email.

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[104] See, *e.g.*, M. P. Brenner, J. Eggers, K. Joseph, S. R. Nagel and X. D. Shi, "Breakdown of scaling in droplet fission at high Reynolds number," *Phys. Fluids* **9**, 1573-1590 (1997). https://doi.org/10.1063/1.869279

PC: Not your glass work.

SN: [2:01:25] It's amazing how much we forget. I took a year off at one point, basically stopped doing research to take in a family from Cambodia during the 1980s. I thought I would never forget that because it was such an intense experience. Now, I can't even remember their names. Probably there's stuff around someplace, but where I'd find it, I'm not sure. I certainly haven't kept records of it in a in well-defined venue. That's too bad.

PC: Professor Nagel, thank you so much for this conversation.

SN: [2:02:29] Thank you so much for thinking of me. Thanks!