swiss physical society

Nr. 62
Oktober 2020

SPG MITTEILUNGEN
COMMUNICATIONS DE LA SSP

AUSZUG - EXTRAIT

Milestones in Physics (21)

Philip W. Anderson, a pioneer in modern condensed matter physics

The Gordon Conference on Superconductivity in 1995 in Les Diablerets with Phil Anderson, pionieer in modern condensed matter physics, who passed away on 29 March 2020. Read on p. 27 some personal remarks from his Swiss colleagues.

Milestones in Physics (21)

Philip W. Anderson, a pioneer in modern condensed matter physics

Phil Anderson, who with his dictum „more is different" and concept of „emergence", more than any other person showed that naive reductionism is insufficient to form a scientific understanding of the world around us, died on 29 March 2020 in New Jersey. As a mentor and colleague, he also touched the lives of many scientists around the world. There have been countless obituaries in the international press, so our aim here in the journal of the Swiss Physical Society, is to supplement rather than replicate these by providing impressions and recollections of some scientists in Switzerland who interacted with Phil, primarily at Bell Laboratories, but also via Maurice Rice, the ETH professor who was the closest Swiss-based associate of Phil.

Thierry Giamarchi (University of Geneva)

To say that Phil Anderson was a giant in physics, who has shaped what modern condensed matter physics is, is to state the obvious. He is one of the very rare theorists to have left a deep mark on the entire spectrum of subjects in the field ranging from disordered classical and quantum systems (spin glasses, pinning of vortices, localization, etc.) to quantum systems (magnetism in metals or insulators, disordered quantum systems, superconductivity, etc.). There is clearly not a theorist working in the field today that has not been deeply influenced by one of Phil's papers.

On my side he had a decisive impact on my career from the start: as a young undergrade student I heard a seminar on Anderson localization (the effect of disorder on quantum systems, one of the theories for which he got the Nobel prize), and found it so beautiful that I decided there and then to do my PhD on this topic (which I did). In addition to the work on Anderson localization, I had during my PhD the occasion to discover with awe several others of his papers such as the one on the Anderson orthogonality catastrophe or the one on the "poor man's scaling" of the Kondo problem, which were pure jewels. I only had later the occasion to meet the man, during a conference in Cargese, and found him surprisingly accessible, and totally driven by the scientific aspects of a discussion without bothering about any aspects of seniority or such considerations.

But it was only during my stay as a postdoc at Bell labs at the beginning of the 1990's that I had serious occasions to discuss with him. He had at the time the idea that the physics of 1D systems (so called Luttinger liquids) was a key to the understanding of high T_c superconductors. This led to many discussions with him, often heated, but always profoundly interesting and leading to deep thinking afterwards. Understanding him was not easy. He had this "impressionist" way of arriving to a result, a mixture of deep knowledge of experiments and some blazing intuition, that was very orthogonal to the very strict "analytical" training that I received during my PhD. This style of physics, also going for the most unconventional explanation to find the crack and new theories – something for which he has been tremendously successful – made it quite difficult to convince him just by calculation alone. He had to be convinced deep down at the physical level. Needless to say, this forced to think deep, and definitely generated a host of new ideas and research directions, even when the original idea was not successful as intended. So, discussing with Phil was always a tremendous source of inspiration, and I had the extreme fortune to be able to do so at various occasions till the last time I saw him in Geneva in 2006.

In addition to the pure scientific aspects, he had some impish manners that were infectious. One evening during a dinner at a conference in Trieste, I asked him whom he was considering as the best theorist he had met (wondering whether he would say Landau, or Feynman, or someone of the sort). His answer was with a twinkle in the eye (and although I think he was not at all religious): God!

For the 1991 Nobel Jubilee there was a very imposing and very solemn photo of the laureates, where one person – with a big smile on his face – purposefully displayed his name upside down. Guess who!

Dirk van der Marel (University of Geneva)

Anderson was in many ways the father of modern solid state physics. His thoughts and ideas have inspired generations of physicists, experimentalists and theoreticians alike. His works are pearls of original thinking and clarity of the scientific discourse. His papers on magnetism and disordered systems, the Anderson–Higgs–Kibble mechanism, disordered superconductors, resonating valence bond theory, and interlayer tunneling in the high T_c cuprates, have provided the scientific basis of my scientific research from the early eighties until now.

I had the tremendous privilege to have met and interacted with Anderson over the years on multiple occasions. Our interactions ranged from pizza lunch at Princeton while his colleagues and he were sorting hundreds of applications, dinner conversations on chamber music, hiking with a group of conference participants in the Swiss alps while picking mushrooms, and, of course discussions on theoretical and experimental condensed matter physics.

Anderson showed a human kindness and interest to the ideas of his colleagues regardless of their age and experience. He didn't hesitate to express his opinion about what is and what isn't relevant in scientific research and favored conceptual insights, analytical methods, and an intuitive approach over sheer number crunching.

His approach to science was strictly anchored in the scientific method, confronting theoretical predictions with experimental data and, after all checks and balances had been made, using experimental facts as a reference point for developing theoretical insights. Although probably other examples exist, from nearby experience I remember the heated debate about the pairing symmetry in the cuprates. In the late 80s and early 90s the dominating view shared by most theoreticians including Anderson was, that the gap in the cuprates was isotropic. As a result of improved sample quality and novel experimental methods experimental evidence started to accumulate in the early 90s that the cuprates have an isotropic gap with line nodes with a d-wave symmetry. Anderson's position on the issue of the pairing symmetry was largely experiment based. His account in his book "the theory of superconductivity in the high-T_c cuprates" (Princeton, 1997) gives the benefit of the doubt to d-wave pairing, in part motivated by experimental evidence from Josephson interference experiments, in part photoemission showing a large gap along (π,0) and deep nodes along (π, π), in part neutron scattering at 2Δ indicating that the gap changes sign. Anderson warns against oversimplification and points out the importance for repeating the experiments on additional members of the cuprate family.

A second episode concerned the interlayer tunneling theory of Chakravarty, Sudbø, Anderson, and Strong. The model started from the observation that the normal state is "strictly two-dimensional and coherent transport in the third dimension is blocked." Microscopically, Anderson explains this as a consequence of the normal state being a Luttinger liquid. In the superconducting state interplanar Josephson tunneling of Cooper pairs occurs as usual for coupled superconducting films, so that in this sense the superconducting state of the cuprates is more normal than the normal state. "Along the c-axis there is a great defect in conductivity: there is no coherent motion of electrons in the c-direction. This means that there is, in the normal state, a missing energy ... which is regained in the superconducting state". Interlayer hopping together with the "confinement" is either the mechanism of or at least a major contributor to the superconducting condensation energy. This state of affairs implied a simple relationship between the interlayer Josephson coupling (which could be determined from the c-axis penetration depth or the c-axis Josephson plasma resonance) and the condensation energy of the superconducting state which can be determined from specific heat experiments. This lead in the period 1996 - 1998 to a series of experiments by Kathryn Moler, John Kirtley, John Loram, and my group. Theoretical guidance in the form of discussions and scientific publications was provided by Anderson and, independently, by Leggett. Measurements of the aforementioned quantities in $Tl_2Ba_2CuO_6$ showed that the Josephson coupling was at least an order of magnitude too low to account for the superconducting pairing. In the field of high T_c superconductivity theoreticians rarely declare forfeit in the face of experimental evidence. Anderson, however, displayed true greatness; he didn't hesitate to defend our experiments to his theoretical colleagues, and he switched the attention of his great mind to different approaches of the high T_c puzzle.

In private discussions it was not always easy to understand everything he said. This was in part due to the fact that he tended to overestimate my understanding of theoretical physics, and in part due to the fact that he tended to speak softer and softer as the information that he conveyed became more important. I remember him with fondness.

Gabriel Aeppli (ETHZ, EPFL, PSI)

Anderson's approach to complex problems by identifying the relevant low energy degrees of freedom via consideration both of data and underlying physical principles has really been the defining paradigm for my entire life as a scientist. Apart from this influence obtained through his papers and talks, I did have the good fortune to interact personally with Phil. My first encounter with Phil was during my job interview – a two day process - at Bell Laboratories. He seemed to be asleep during most of the presentation, but at the end he asked a question, concerning "reentrant" spin glasses, about my thesis which could not have been asked had he been genuinely asleep. Of course, as the co-inventor of the Edwards-Anderson order parameter and replica trick, the concepts which together set the agenda for the study of frozen states in disordered media because they brought mathematical rigour to a messy corner of physics and chemistry, with eventual broad impact on fields from biomedicine to economics and computer science, he was well-positioned to formulate interesting questions on the subject of magnetic glasses.

The last real encounter was many years later on a trail at Aspen, where he was hiking - not struggling - in the opposite direction on the Buckskin Pass (3798 m) trail by himself (almost certainly against the advice of the authorities) at an age north of seventy. In between, there were interactions mainly in the tea room at Bell Laboratories, which he frequented even though he was spending most of his time at Princeton. Given his stature as Nobel laureate and my position as a starting scientist, it was remarkable that he had the patience to listen to me about problems which were by that time to a large extent peripheral (he had already solved them!) to his own contemporary research. My sense though is that this relationship was typical of that with other experimentalists - he had tremendous respect for and little fear of real data, which he probed thoroughly to establish trustworthiness.

Bertram Batlogg (ETHZ)

A keen interest in the latest results from the lab and close interactions with experimentalists were a characteristic of Phil's working style. He would sit down for hours analyzing data and suggesting new measurements. For young hires at Bell Labs in the 1980's this was particularly exciting and rewarding at the same time as Phil would patiently explain his latest theoretical concepts, such as his take on intermediate valence Rare Earth compounds, Heavy Fermions and Kondo lattices. With fellow theorists his patience might be shorter. Decades earlier in the 1950s the close contact with experiments on doped Silicon led him to the seminal theory of electron localization.

The traditional afternoon tea was a Bell Labs institution when dozens of researchers from the Physical Research Area would gather for informal, and quite often heated, chats on physics (or in early April on US tax law). When his turn would come Phil dutifully would put the huge aluminum kettle on the heater, brew the tea and supplied pounds of all sorts of cookies. Apparently he liked mixing with colleagues. Once at a workshop on superconductivity with numerous students participating, Phil presented a poster, in addition to the key note talk. And he did it in a most memorable way. In the dimly lit basement hall he was sitting on a chair next to the poster "camouflaged" with a hat, big glasses and a fake mustache. Thus junior scientists and students would indeed feel comfortable engaging this "just ordinary" presenter for explanations. His friends and colleagues will never forget these hours and will treasure the memory of Phi's humor and art of disguise.

Manfred Sigrist (ETHZ)

"More is different", a most remarkable and insightful article from 1972 was probably my first encounter with the author Phil Anderson. As an undergraduate student I did not appreciate fully the depth of the ideas yet. Only over time I started to appreciate his school of thought and feel a strong boost in my pride of being a condensed matter physicist.

Becoming a student in Maurice Rice's group then gave me the opportunity to also meet Phil Anderson, as they kept close ties since their common years at Bell Laboratories. In that time the news of the discovery of cuprate high-temperature superconductivity broke, which influenced much of the research in Zurich and I became a direct witness of many of the developments. Amazingly quickly Phil Anderson understood that the physics of a hole-doped Mott-insulator in the CuO_2 plane was the essence for superconductivity in these materials. The spins originating from one hole per Cu-ion coupled through superexchange would be starting point and the key ingredient of what would eventually become one of the most comprehensive paradigms of cuprates.

In a pioneering article in Science (1987) he introduced the notion of the resonating valence bond (RVB) state, which he developed together with Baskaran. The idea was so stunning that it needed a genius to find it. The RVB state constitutes a short-range correlated quantum liquid phase of strongly correlated electrons. Phil Anderson realized that such a state could be described by the Gutzwiller projection of a wave function of uncorrelated electrons, enforcing the presence of a single hole per Cu-ion, and would correspond to BCS-type of ground state, which then upon hole doping yields superconductivity. The so-called "t-J-model" incorporating the superexchange and doped mobile holes became the essential framework for the RVB physics. Despite being a rather simple model it is highly non-trivial to analyze due to correlation. The idea was taken up immediately at ETH and Fuchun Zhang and Maurice Rice provided with the Zhang-Rice singlet a solid microscopic basis for this model.

The Gutzwiller projection represents a real challenge and many groups invented techniques for this purpose, such as variational Monte Carlo calculations (Maurice Rice), slave boson mean field approaches (Gabriel Kotliar and Hide Fukuyama) or gauge field treatments (Patrick Lee, Xiao-gang Wen and Naoto Nagaosa). Interestingly, the superconducting phase predicted by the RVB concept has d-wave pairing symmetry, a fact which actually prompted Phil Anderson to abandon his idea for several years, as it seemed to contradict experiments, and to follow a completely different line to explain cuprate superconductivity. After d-wave pairing had eventually been established, however, Phil returned and promoted what is known nowadays as the "plain vanilla" version of the RVB paradigm, as reviewed in 2004 in the famous "A-to-Z" paper with the authors Anderson, Lee, Rainderia, Rice, Trivedi and Zhang.

The plain vanilla RVB theory not only predicts the correct pairing symmetry, it also gives a good account of the basic phase diagram of cuprates upon doping, such as the superconducting dome and the pseudogap phase. A theory developed in Maurice Rice's team, the YRZ propagator ansatz (Yang-Rice-Zhang), shows that the RVB picture incorporates features, which describe even more details of cuprate physics such as the Fermi arcs of the pseudogap phase. Cuprates are among the best studied material classes in condensed matter physics. Naturally it is not surprising, that many details have been observed which are not contained in the RVB picture. Nevertheless, it remains undoubtedly among the most convincing and beautiful guides to understand cuprates.

As Maurice's student I was a member of the family and it was easy for me to approach Phil. I remember one occasion when I met him in Japan in the late nineties. It was shortly after John Horgan's infamous book "The end of science" was published, for which also Phil had been interviewed. Unlike for most of the other people which are displayed most unfavorably, I had the impression that Horgan showed at least some respect for Phil. So I asked Phil how he got into this book. I learned from him that we scientists are naïve indeed when talking openly to a journalist whose intention it is to lure you into statements which promote his thesis. Surely, Phil was most unhappy about the book, whose views he absolutely did not share, and how his statements were distorted by very selective accounting of what he had said. Indeed he stated that he now also understands why politicians, who are specially trained for this, would give interviews without much content. In the same meeting I also learned that Phil and I share a common experience: both of us celebrated our 30th birthday as gaijins (visiting scientists) in Japan.

A section from the picture on the title page: In the first row Phil Anderson (left), Richard Greene and Øystein Fischer (right), the organisers of the Gordon Conference on Superconductivity from 17-22 September 1995 in Les Diablerets. In the last row the second from right: Maurice Rice.