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Rise of Complexity, 1953–2002

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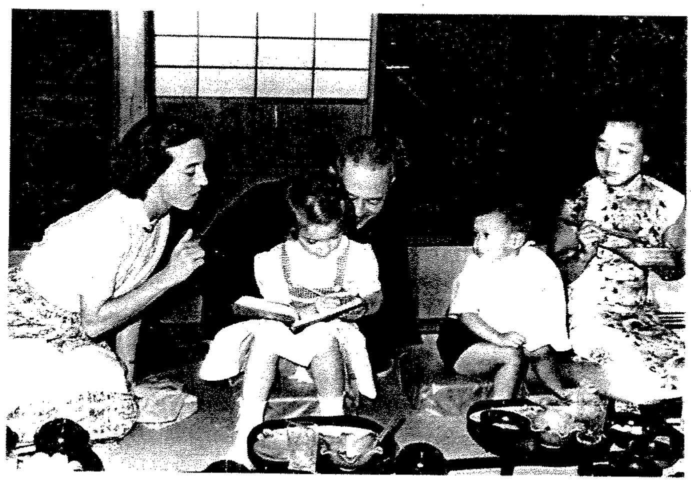
Here is a summary of the three rather different types of topic that I should like to cover today. Just because I am one of the few people here who has attended all four of the IUPAP meetings of all of theoretical physics, I'd like to start with a historical slant, first touching on some reminiscences of the first three meetings. Then I will, still speaking historically, try to outline some of the main intellectual themes of condensed matter theory – a very personal selection to be sure, one aiming at emphasizing the open frontier at each stage towards dealing with greater and greater real-world complexity. Finally, I'll introduce a more specialized, but still broad-brush, thread in one of those major themes.

Some reminiscences of the three predecessors of TH 2002 can be introduced by first showing some pictures from the world of theoretical physics as it appeared in 1953. The Japanese were the first to have the idea – or the temerity – of bringing together all flavors of theory in a single conference, their first postwar attempt at a major congress – the peace treaty had only just been signed, and their facilities were very strained. I was there by accident, rather, starting a Fulbright with Kubo, and for different reasons Frank Yang and I had both brought along our five-year olds, who turned out to be much more popular with press photographers than the panjandrums of physics – this is the Asahi's photograph of the greeting ceremony at the Kyoto station, with the coheads of IUPAP, Mott and Gorter, in the background behind my Susan.

For Yang's sake, I also show a snap from the ladies' program showing his Franklin and my Susan learning tea ceremony.



But not only the ladies were feted – here am I at the geisha party for the men – though Per-Olov Lowdin made a much greater impression on the geishas, a tall, handsome Swede.

A last non-physics photo: the pre-meeting festivities were interrupted by a typhoon, but nonetheless the intrepid Japanese bussed us to Nara over the rather bad roads of postwar Japan – a washout in the edge of one pitched our bus into the ditch. After much polylingual jabber, Onsager took over leadership of a crew of farmers and physicists and leveraged us out of the ditch with beams borrowed from a bridge – as you see. The photographer, incidentally, was John Bardeen.

Here, finally, is the whole group assembled, or most of them – Feynman was off somewhere, but there are Wheeler, Tomonaga, Maria Mayer, Bhabha, Wigner, Peierls, and many others who were in the process of making theoretical physics; only the Russians are missing, for obvious political reasons.

The Japanese were disappointed that they had not attracted any of the then Nobelists; but I think this meeting had possibly a uniquely large percentage of "Nobelists-to-be". I note a few of the faces which will play a special role in the discussion of condensed matter which follows: Slater, Onsager, Mott, Yang, Frank.

The successor meetings came at rapidly increasing intervals: 1956, 1968, 2002; I guessed at the algorithm and came up with around 2071 for the next one, which leaves us with a great responsibility here, I guess. Many people remember the magnificent talk at Seattle by Feynman on superfluid He (though after reflection one realizes that many of the ideas had been foreshadowed 3 years earlier in remarks by Onsager, who was not the expositor that Feynman was, to say the least), at the end of which he admitted complete bafflement about superconductivity – Blatt, representing an Australian group, claiming thereupon that the answer was electron pairs, which it was; but not in the form they were using. Some Russians appeared already at this one – Bogoliubov, for instance – looking very uncomfortable in their ill-fitting Soviet-style wardrobe. I do not know if there exists any printed material on this meeting.

Trieste was the grand opening shot of Salam's ICTP. It was magnificently managed but overshadowed in my mind by political events – Robert Kennedy was shot on the eve of the meeting, for instance. Trieste as a UN institution was hospitable to Russians – I remet Abrikosov after 10 years, and the Landau group was well represented. But I am afraid that in Condensed Matter, at least, the scientific program was a bit retro.

Now I would like to limn with a VERY broad brush some main intellectual strands of the progress of condensed matter physics during those 50 years. [1] John Slater, already in 1953, was obsessed with what I have rudely called the Great Solid State Dream machine. He envisioned that the then new electronic computer could be applied to the task of automatically providing the electronic structure of any desired solid; and he literally believed that then we would have all the answers to any conceivable question. The latter idea was wrongheaded; but the former has gradually become a reality, with the rise of what is now known as LDA. It is not enough appreciated that Slater himself provided the key element in that method, which in modern terms can be simply expressed as assuming that the self-energy is well approximated as purely local in both space and time. (Therefore determined by the only local electronic parameter, the density.) His problem was with the instruments (electronic and human) and the mathematical methods necessary to make the dream a reality.

The basic locality assumption is not quite right, particularly with regard to band gaps, which are often vital; the reason and the necessary improvements in tractable materials were worked out by a list of people too long to detail here, except to mention that the earliest and the least often credited was my student John Inkson. But there are many cases where it fails spectacularly: essentially all of the interesting class of substances with magnetic inner shell electrons, most of which exhibit what has been called the "Mott Phenomenon" – a dominance of the local repulsion between those inner shell electrons. (Mott's is another of those 1953 faces). A new version of the Dream Machine has recently been invented which is quite successful in most of these cases – DMFT, dynamic mean field theory, cooked up by Georges and Kotliar, where the assumption of locality of the self-energy in time is abandoned, generalizing more or less the scheme for dealing with magnetic impurities that was worked out for the Kondo effect.

A second theme is the wonderful theory of critical points and phase transitions which has been so heartening in bringing together statistical mechanics, condensed matter, and field theorists – but this is not the appropriate subject for me to talk about here, as a field in which I am not a player and the meeting's president emphatically is.

My third theme begins the march up the ladder of complexity from pure, perfect solids, which one can think of as one or another renormalized vacuum. Already in Kyoto in 1953 Mott spoke about what at the time was his chief concern, theory and experiment on dislocations in solids, and Frank was about to invent his famous growth mechanism which relied on spiral dislocations; while Onsager's cryptic remarks introduced the subject of quantized vortices in HeII. But it was not until much later that we began to realize that there were generalities relating all such phenomena, which came with the understanding of broken symmetry as a common description of condensed phases in the '60's, and with the general topological theory of defects originating with Volovik and Toulouse-Kleman in 1975. This mathematical apparatus was made necessary by the complexities of liquid crystal phases (brought out by Frank and de Gennes particularly) and of superfluid He-3. We recognized that essentially all dissipative and breakdown phenomena, from the noisy sliding of charge-density waves in "blue-bronzes" and Barkhausen noise in magnets to glitches in neutron stars, could be seen as related to the symmetry properties of the condensed phases.

Fourth Theme: From disorder to complexity. The original liberating idea may have been to stop treating the dirt and disorder in our condensed matter systems as a nuisance to be averaged over, and to try to renormalize the properties of the regular solid; instead to treat disorder as a subject of study in itself. This was the conceptual breakthrough of localization, which was simply a gleam in my eye in 1956, and had not yet achieved respectability in 1968 (Salam's program was firmly respectable). The next step was spin glass and the idea of frustration, appearing in the early '70s, and before we knew it – late '70s, early '80s – we were dabbling in complex optimization, neural networks, and many of the subjects which are parts of complexity theory (if there is such a thing.) Others were arriving in the same territory from different starting points: Edwards and de Gennes from polymers and glass, for instance, Bak from critical fluctuations applied to sandpiles, Hopfield from biophysics. We are only beginning to sample the enormous playground we see ahead of us.

A final major theme was the "supers" and macroscopic quantum coherence, from He-II in 1953–56, to BEC in 2002. In a way, this is climbing back down the ladder of complexity, but I wanted to make a small historical point having to do with a subset of this gigantic and active field. I'll call my subject "the rise and fall and rise again of 'anomalous' – non-BCS – superconductivity."

Within a year of the BCS paper which explained superconductivity as what we would now describe as a condensate of isotropic, "s-wave singlet" pairs made up roughly equally from all parts of the Fermi surface, there were suggestions of the possibility of anisotropic pairings, by Thouless, Pitaevskii and others. Formal theory (Morel and PWA) brought out the possibility of novel properties: one could have gapless superconductivity violating the then sacred, but wrong, Landau criterion for superconductivity, "orbital ferromagnetism" and other anisotropic properties, and, characteristically, one could see a complex phase diagram with several phases having the same critical point – or nearly so – because of the approximate degeneracy of the linearized gap equation for lower pair symmetries. Although singlet s is necessarily the lowest bound state for free particles, this is not the case in the presence of the Fermi sea, and in fact as all authors pointed out, basically repulsive pair potentials could have higher-L bound pair states, which might seem to open the prospect for a very common phenomenon. In fact Balian and Werthamer even showed that a particular $L = 1$ triplet state could masquerade as ordinary, isotropic superconductivity.

Unfortunately, that prospect seemed blocked by my own "dirty superconductor theorem" which showed that, except for the s-wave case of the original BCS which is protected by time-reversal invariance, all such states would be very sensitive to small quantities of disorder scattering – quantities which at the time were unavoidable in most metals. The psychological response to this and to unsuccessful searches for such states was a kind of "irrational non-exuberance" – an automatic assumption that superconductivity could only occur in BCS form, in response to the phonon mechanism of dynamic screening which had been elucidated in the '60s.

As Brueckner and Pitaevskii early pointed out, dirt problems would not occur in the Fermi liquid He-3, which brooks no impurities whatever; and as the late Vic Emery, in particular, pointed out, the spin fluctuation theory of Schrieffer and Doniach would favor triplet superfluidity in this case – BCS being impossible because the underlying potential is gigantically repulsive. As everyone knows, these predictions were borne out by experiment in 1972: the complex phase diagram, the anisotropy seen in NMR beautifully interpreted by Leggett, the spin-fluctuation theory partially confirmed by Brinkman & PWA.

In 1978 and 1979 the field of superconductivity was suddenly opened up by the discovery of two unexpected and exotic types of superconductor. First Frank Steglich discovered the superconductor $CeCu_2Si_2$, a material which at low temperatures was a metal with renormalized electrons having an effective mass of the order of hundreds of electron masses, such that the transition temperature of 2 degrees K is an appreciable fraction of the effective Fermi energy. The characteristic feature of this material which is most striking is the gigantic specific heat peak at the phase transition, since an entropy of order unity per atom is involved in the SC transition. These electrons are basically f-shell states, but at normal temperatures all f electrons are not free, but make up local magnetic moments. Soon Ott and Fisk had discovered three more of these "heavy-electron superconductors" based on uranium, the most exotic perhaps being UBe_{13}, and they and others showed that these indeed showed complex phase diagrams, impurity-sensitivity, and other characteristics of anomalous SC. Nonetheless the community displayed a deep reluctance to accept the reality of such states or of a non-phonon mechanism of pairing. Perhaps an important aspect causing this resistance was the general failure to realize that the quality of materials had steadily improved since the 1950's, so that the "dirty superconductor" barrier had quietly evaporated.

Shortly – in 1979 – Bechgaard discovered the first of another type of superconductor: an organic "charge-transfer" salt in which the metallic electrons – 1/2 per molecule – travel along one-dimensional or two-dimensional stacks of one kind of molecule (a selenium or sulfur-containing aromatic ring structure), the other simply providing a charge reservoir. It is very hard to imagine how the conventional phonon mechanism could cause superconductivity in these ultra-low density, highly anisotropic metals, in which the intramolecular Coulomb repulsion must be the major interaction, but nonetheless this seems to have been the mechanism of choice. Several more classes of similar compounds have come forward over the years, with Tc's ranging up to 13 degrees K. Thus there were already exotically interesting superconductors being studied before the great discovery of the high Tc cuprates in 1986.

It is often forgotten that the cuprates were suggested to be d-wave already in 1987–88, by Affleck, Kotliar, Rice and collaborators, and in a disguised form Laughlin. But that dirty superconductor argument deceived me, at least, into rejecting the existence of gap nodes until they were forced down my throat by massive experimental evidence in 1994. It is still one of their great mysteries that, alone among all known cases of non-BCS superconductivity, they still seem very insensitive to scattering. The reason seems very likely to be that some form of "spin-charge separation" protects the pairing, which is a singlet pairing of spins and only develops true electron pairing at lower temperatures.

The floodgates seem to have been thrown open by yet another exotic discovery, Sr_2RuO_4, proven conclusively by Maeno to be a triplet case. Since then it has been shown using NMR Knight shift that Bechgaard's original substance is a triplet case, while another organic is definitely singlet with nodes, according to several experiments.

Among the heavy-electron cases there is also considerable complexity. At least one new one, UGe_2, is superconducting and ferromagnetic with the same set of electrons, therefore necessarily a case of triplet equal-spin pairing. On the other hand, Lonzarich's group has found a new class of heavy electron cases of which $CeCoIn_5$ is typical, which share with the cuprates association with antiferromagnetism, and, apparently, a singlet gap with nodes – but not impurity immunity. I think it is time that one recognizes that these exotics must be treated as guilty unless proved innocent.

That being accepted, the question should be: why did we not assume this from the start? The magnetic phenomena associated with all of them strongly argue that the basic interaction is strong "Mott" local electronic repulsion, and one can argue persuasively that the dynamic screening mechanism of BCS which allows phonon-mediated SC cannot work in them. There seems to be, for reasons one thinks one understands, an association of antiferromagnetism with singlet, and of ferromagnetic interaction with triplet, cases.

I am aware that some heavyweight names have argued otherwise, that there is a forward peak of phonon scattering, but not at all convincingly to me. In a final figure I show where the fallacy of the school which argues for a strong peak in forward scattering by phonons comes from – such a strong peak combined with short-range repulsion could, according to Abrikosov and others, give anomalous SC. Eventually, the point is that long-wavelength phonons in a metal are charge neutral and do not couple to electrons. I think that in all cases the mechanism is electronic and related to magnetic interactions.

It is interesting that recent work of Capone et al, and Nozieres, seems to suggest that "Mottness" can also enhance Tc in a special class of singlet s cases – but here I am abandoning my historical theme.

In conclusion, it seems likely that non-BCS is not a strange, exotic phenomenon but, like BCS, dirt-common. After all, in nature there are huge classes of magnetic compounds, though the stablest materials tend to be nonmagnetic. It is always a mistake to underestimate the complexity of which Nature is capable. It also can be a mistake to believe too faithfully in theories, perhaps especially those promulgated by PWA.

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