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## News &amp; Views

## Reflections on the past, present and future of condensed matter physics

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I will not go into the history of how I come to be giving a talk with this preposterously pretentious title. However, a couple of general comments before I start: first, I am a “pure” rather than an applied physicist, and I am afraid that my talk will give rather short shrift to the applied side of condensed matter physics (CMP), which of course has been hugely important over the last century or so. Second, I am a theorist, and so will tend to concentrate more on the conceptual advances than on the equally important progress on the experimental side.

I believe it may be helpful to view the history of CMP within the framework of the concept, popularized by the late historian of science Thomas Kuhn, of a “paradigm shift”. The dictionary definition (Merriam-Webster) of this concept is “an important change that happens when the usual way of thinking about or doing something is replaced by a new or different way”. According to Kuhn, in his classic book “The Structure of Scientific revolutions” (1962) [1], the history of science may be viewed as a series of periods of so-called “normal” science, in which a given paradigm (defined below) reigns unchallenged, punctuated by a number of “scientific revolutions” (paradigm shifts) in which the old paradigm is challenged by a new one which eventually emerges triumphant; examples which he frequently quotes, are the Copernican revolution, the birth of special relativity and that of quantum mechanics. What then is a “paradigm”? It is basically the overarching intellectual framework which, during a period of normal science, determines what are the legitimate questions, what kinds of answers to them are allowed, and what kinds of evidence may be adduced to support the latter. In a scientific revolution, all of these change, often quite violently; these are the “paradigm shifts” to which Kuhn devotes so much attention.

I believe that it may be possible to view much of the history of CMP (Fig. 1) as a series of (mini-)paradigm shifts, though the associated scientific revolutions are in many cases of the “velvet” variety: as in the political analog, the old ideas are not killed off, they stay around but their role following the revolution is much less

central, and a “new guard” is now in charge. I will try to give some examples of this in what follows.

I entered the university in 1955 (though I did not actually start doing physics until four years later); so let’s take that year as our approximate starting point. If I look back on the state of CMP (in those days called “solid state physics”) around 1955, I would say that we had a rather detailed understanding of a fairly narrow range of topics, mostly related to crystalline solids; liquid helium was off to the side, and glasses and “soft matter” were very little studied in physics departments (though rather more so in departments of chemistry). Our understanding was mostly based on a *single-electron* picture; it is remarkable in retrospect that one important concept, that of a topological insulator, whose basic features can be quite adequately analysed within such a picture, was to remain hidden for another 50 years. Exceptions to the “single-electron” picture were (of course!) phonons, magnetism (which however was mostly discussed within a mean-field model) and the Landau-Lifshitz phenomenological theory of second-order phase transitions; in addition, there was a quite well-developed phenomenology of superconductivity based on the work of the London brothers, Pippard, Ginzburg and Landau (though in the mid-fifties the latter was not that well known outside the former Soviet Union). One other hugely important attempt to take into account inter-particle interactions, and perhaps the first real example of what we would now call “many-body” theory, was the Bohm-Pines theory of the electron gas. However, with these exceptions, most theory in those days was of the “first-principles” variety, and since computational physics was in its infancy, mostly analytical in nature.

A few other characteristics of CMP in the mid-fifties: there was very little connection to other areas of physics, such as astrophysics (my Ph.D. advisor, Dirk ter Haar, was a rare example of someone who bridged the two fields) or biology; in the condensed matter community (and actually more generally in the physics community as a whole, or at least the Anglo-Saxon component of it) interest in the foundations of quantum mechanics was viewed as not quite “respectable”; and sociologically, at least in the US

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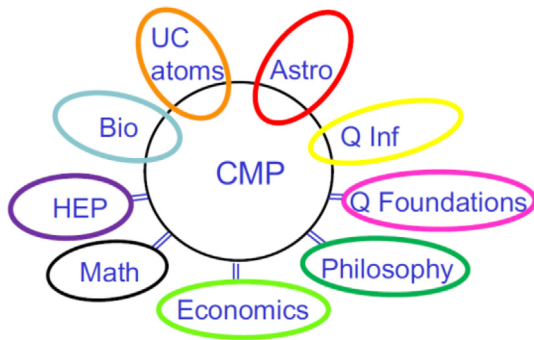


Fig. 1. CMP and its current interaction with other fields.

and the UK, the community was far from diverse (the proportion of female and ethnic-minority physicists was not zero, but it was pretty small). All in all, CMP in 1955 was a fairly typical example of Kuhnian “normal science”!

What is different in 2018? First, one rather obvious change is that the condensed matter community, while perhaps still not as diverse as we might wish, is much more so than it was 60 years ago. Secondly, a huge role has been played by the rise of computational physics, which, nowadays, has to be a component of any meaningful undergraduate physics degree. Third, while it is not the major subject of this talk, there have been spectacular advances in cryogenics, materials science, diagnostic techniques... As an example, in 1955 the lowest temperature attainable in the laboratory was about 0.1 K; in 2018 it is around  $10^{-10}$  K, an advance of 9 orders of magnitude in my physics lifetime (Fig. 2)! Finally, there has been an immense amount of “outreach” to other disciplines—mathematics, high-energy physics, biology, ultracold atoms, astrophysics, quantum information, quantum foundations, philosophy, economics... (Fig. 1). There is hardly an area of human knowledge these days on which CMP has not made at least a modest impact.

However, these are the “external relations” of the subject. An even more intriguing question is how condensed matter physics itself has changed over the last 60 years, and here I need to repeat the caveat that what you are going to hear in the next few minutes is the view of a theorist; rather than reviewing the impressive advances in experimental technique that underlie many of the developments I will mention, I shall ask the question: what have been the real paradigm shifts over this period in our overall view of the subject?

I suspect that if asked to name the first major conceptual development in their subject since 1955, most CM physicists would plump for the BCS theory of superconductivity [2]. While that is of course enormously important, and I will come to it in a moment, my answer would be different: the Landau theory of Fermi liquids (“LFL theory”), which predates BCS by about a year [3,4]. The importance of Landau’s seminal work was that, rather than asking, as most of his predecessors had done “how do we calculate the properties of a macroscopic condensed-matter system from its

microscopic Hamiltonian?” he asked a different question: “how do we relate the different physical properties of the macroscopic system?”. I well remember that when I was a graduate student in Oxford in the early 60’s, and tried to “sell” the Landau approach (not widely appreciated outside the former Soviet Union at that time) to some of the local experimentalists working on liquid  $^3\text{He}$ , its originally intended application, I tended to get the response that LFL was not a theory but simply a mere reparametrization of the experimental data, since every time one measured a new physical quantity, LFL came up with a new Landau parameter to fit it. Had this really been the case, the approach would indeed have been pointless; however, fortunately, within a few years, it became clear, first with the normal-state spin-echo experiments of Corruccini et al. and later with manifold experiments on the superfluid phase, that there are far more experimental data points than there are Landau parameters to fit them, so that LFL theory indeed makes some highly nontrivial predictions [5]. Of course, since then the LFL philosophy has been applied to many other systems besides  $^3\text{He}$ .

On to the BCS theory of superconductivity (1957) [2]. From the point of view of this talk, what is essential here is not so much the specific results and predictions but the whole idea that when confronted by a mysterious phenomenon one should try to seek out the fundamental physical factor involved (in this case the effective phonon-mediated electron-electron attraction), embody it in an effective low-energy Hamiltonian, albeit a grossly oversimplified one, and calculate specific physical properties based on the latter. (Of course, only a subset of all possible physical properties; no-one in his/her right mind would expect the BCS Hamiltonian to give even qualitatively correct results for e.g. the thermal expansion!) This procedure was of course in the case of BCS spectacularly successful, and I sometimes wonder whether this success has “spoiled” the CM theory community, in conditioning them to expect that other mysteries, such as high- $T_c$  superconductivity, will necessarily yield to the same technique.

The next paradigm shift was probably associated with the renormalization-group approach to second-order phase transitions developed in approximately the years 1963–71 [6] and the associated ideas of universality and broken symmetry [7] (though some aspects of the latter had actually been appreciated by Landau and Lifshitz thirty years earlier). In the words of the late Leo Kadanoff, “the practice of physics has changed, going from solving problems to discussing the relationship between problems”.

While an appreciation of the importance of topological considerations in CMP does not (contrary to some accounts!) originate with the quantum Hall effect (it is at least implicit in Bloch’s much earlier work on the stability of supercurrents in helium-4), the latter, and in particular the fractional version [8], gave it an enormous fillip and at the same time introduced the novel idea of quasiparticles, which bear no simple relation either to the underlying particles (as do Landau quasiparticles in a Fermi liquid) or to the underlying classical waves (as do the phonons in a typical insulator).

Finally, the most recent development in CMP which I would characterize as a paradigm shift is the impact, since around 2000, of the concept of quantum information: no longer can we be satisfied with calculating the properties of a many-body system averaged over a macroscopic number of different microscopic states, the individual wave functions themselves may be crucially important and must be taken deadly seriously [9]! The present author would query whether the majority of the community has yet fully caught up with the implications of this mini-revolution.

Of course, over the last 60 years, there have been several other important developments in the field; one thinks of superfluid  $^3\text{He}$  (1972), the integral quantum Hall effect (1980), cuprate superconductivity (1986), and most recently topological insulators (2004).

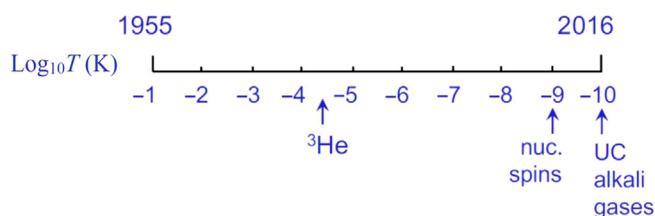


Fig. 2. The enormous expansion of CMP over 60 years — pushing towards absolute zero temperature as an indicator.

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