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News & 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 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

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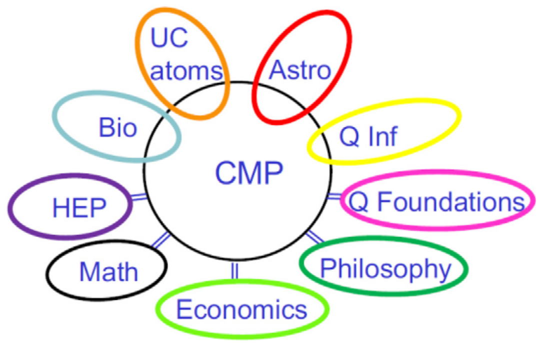


Fig. 1. (Color online) CMP and its current interaction with other fields.

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- to 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-He, its originally intended application, I tended to get the response that LFL was not a theory but simply a mere re-parametrization 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

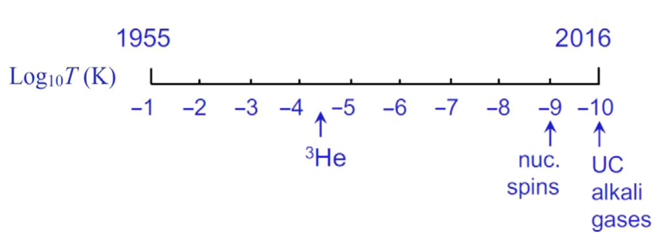


Fig. 2. (Color online) The enormous expansion of CMP over 60 years — pushing towards absolute zero temperature as an indicator.

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-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-He (1972), the integral quantum Hall effect (1980), cuprate superconductivity (1986), and most recently topological insulators (2004). However, while each of these was exciting, none of them has (yet) really changed the *kinds of questions we ask*, so that I do not think that any truly deserves the name of a paradigm shift, even a mini-one, in the Kuhnian sense.

One way of trying to trace the evolution of a given field over time may be to identify a few "buzzwords" and look at their frequency of appearance as a function of time. In the case of CMP, two obvious candidates for such a role are the words "emergent" and "topological", and I have tried to tabulate the total number of titles in the INSPEC index in which each of these words appears for the decades since 1960. In each case the number increases steadily from being almost negligible for 1960–70 to around half a million for 2010–20* (Fig. 3). Actually, from one point of view these increases are surprising, since just about *all* interesting phenomena in CMP, new and old, are both "emergent" and "topological"! As regards the former, I used to be a member of an institution called, not at my urging, the Center for Emergent Superconductivity; I challenge the reader to tell me what "non-emergent" superconductivity would be like! And as to the latter, it is topology, in the sense of the need for the wave function to be single-valued, which is at the root of just about every quantum phenomenon in many-body physics. So both terms are in my view mostly superfluous padding, and I personally believe that from now on certainly the former, and in most contexts also the latter, should be deleted from the CMP literature.

If one wants a metaphor for the overall state of CMP in 2018, I believe a good one might be a rugged seashore as the tide comes in; there are long strips of dry land (topics we think we understand) cheek by jowl with equally long inlets (topics we know we do not) (Fig. 4a). For example, we understand a great deal about crystalline solids, but much less about the various kinds of amorphous material which are their close cousins; while our quantitative understanding of "classical" superconductivity is impressive, to the extent that we can sometimes even predict approximate transition temperatures in novel compounds, the same cannot be said for the cuprates; and progress in laboratory photovoltaics has been of little help with the theory of natural photosynthesis (Fig. 4b).

Some miscellaneous thoughts about CMP in 2018 are given as follows:

First, it is important to distinguish the various levels of problem which we encounter. In some areas, such as the ultracold atomic gases, the microscopic Hamiltonian is not only known but tractable, at least computationally, so that their study reduces to Kuhnian "normal science" with a vengeance. In others, such as cuprate superconductivity, the Hamiltonian is at least partially known but is intractable both analytically and numerically; while in yet a third class, e.g. some types of glass, the Hamiltonian is not even known (To me, this last class is in many ways the most fascinating) (Fig. 5).

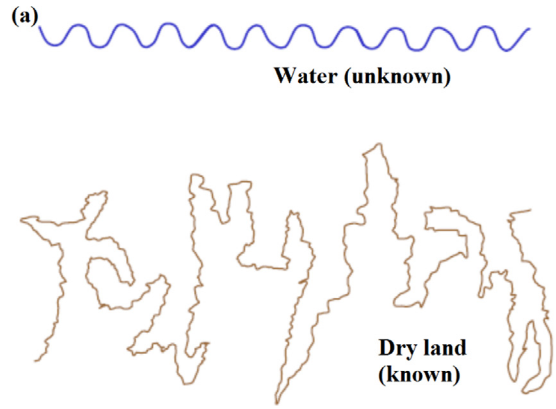
Are we indeed "spoiled" by the success of BCS theory as hinted above? That is, is it guaranteed that with sufficient ingenuity and effort we will be able, for any arbitrary CM system, to find a low-energy effective Hamiltonian which when solved to sufficient degree either analytically or computationally will explain the experimental properties? Many approaches to (e.g.) cuprate superconductivity seem implicitly to take for granted that this is the case; I am inclined to have my doubts.

Are analogies between CM phenomena and those occurring on particle physics, gravitation, etc. useful? I have little doubt that on the theoretical side they can be (one needs only think of broken symmetry, the renormalization group, AdS/CFT...). My doubts refer more to the experimental side: it has become quite fashionable in the last few years for a theoretical group to identify a supposed analogy between some phenomenon in high-energy physics (HEP), gravitation, or cosmology and one occurring, or predicted to occur, in CMP. An experimental group then does the experiment to verify the CM prediction, and claims "box-office" value for it on the basis if the alleged HEP (etc.) analogy. But had the CM experiment come out against the prediction, that would have had no implications for HEP; all it would have shown is that the analogy is mistaken. And if the negative result for an experiment has no conceptual significance, I do not see how the positive outcome can have any. Am I missing something? Probably...

Mathematical convenience versus physical insight: Philippe Nozieres has a quote with which I heartily agree: "only simple qualitative arguments can reveal the underlying physics"; in my opinion, theorists (and perhaps theorists in other areas of physics, too) are far too fond of fancy formalisms (e.g. imaginary-time functional integrals) which are mathematically streamlined but whose connection with the physics is at best at several removes. (Yes, I myself started off as a Green's function aficionado, but eventually saw the light...).

The impact of quantum information: as mentioned above, I believe this will in the long run be profound; in particular, many of the "tried and true" approximation schemes which have served us so well in CMP for the last 60 years, such as the Bogoliubov-de

(a)



(b)

"Known"	Versus	"Unknown"
Crystalline solids		Glasses (amorphous materials)
"Classical" superconductivity		High-temperature superconductivity
Laboratory photovoltaics		Natural photosynthesis

Fig. 4. (Color online) The "rugged-seashore" analogy from "known" to "unknown" (a) and CMP examples of "known" and "unknown" (b).

Open problems	
Hamiltonian known and tractable (At least computationally)	Ultracold atomic gases
Hamiltonian partially known but intractable	High-temperature superconductivity
Hamiltonian not even known	Amorphous materials

Fig. 5. (Color online) Types of unsolved problems in condensed matter physics.

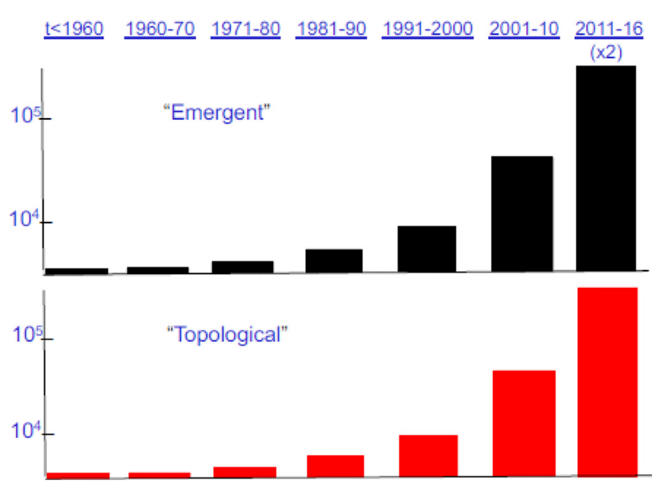


Fig. 3. (Color online) Entries in INSPEC index under "subject, title, abstract" (not all CMP). The figure for 2011–16 extrapolated to the full decade.

Gennes equations beloved of superconductivity theorists, may have to be discarded when we really face up to quantum-information problems in a CM context.

The scourge of bibliometrics and "high-impact" journals: this really is a point which goes way beyond condensed matter physics, or even physics; it is really a much more general point about the current scientific scene, but I felt I could not conclude my talk to this audience without at least mentioning it, because it involves an issue about which I feel very strongly. The reason that I do so may lie in part in my own early academic history (which is now more than a half-century old): my publication list at the time I applied for a postdoc position (at UIUC, no less) consisted of a one-page Physics Letter, and when I got my (effectively tenured) position at Sussex, my understanding was that my real job was to teach and that any research activities, while encouraged, were not a condition of my job. At no stage in my early career did I have to worry about publishing papers, let alone papers in "high-impact" journals. And had it not been for that relaxed atmosphere, I do not think I could have done the work I eventually did, on superfluid 3-He and other things.

How different is the scene for postgraduates and postdocs to-day! At least judging from overheard conversations between people at this stage of their careers to-day, even to be considered for a postdoc position, let alone a junior faculty one, at a prestigious university in the US, China or elsewhere it is mandatory to have published three or four papers in "high-impact" journal such as *Science*, *Nature*, *PNAS etc*. While this is in some sense the consequence of what is in itself a good thing, namely the much-increased access to academia which I noted earlier, it seems to me a truly terrible state of affairs: if your career prospects require you to publish in this way at (say) the postdoc stage, then it is almost automatic that you will seek out, or at least be strongly tempted to seek out, just those problems which you know you can do, or at least think you have a good chance to do, within the two- or three-year period available for them. And I could not think of a better recipe to guarantee that unless you are very lucky, the research you publish in this way, however "high-impact" the journal, will not in the end be the truly path-breaking stuff.

What to do? I guess that in a talk of this nature the speaker is allowed to give one unsolicited piece of advice to his audience. So here is mine, aimed mostly at those of you who are at the graduate student, postdoc or junior faculty stage: you have to be realistic, you cannot buck the system entirely, so you indeed will probably have to devote a fair fraction of your research time to "pot-boiling", short-term problems which will get you the necessary publications to move on to the next stage of your career. But whatever else you do, try to set aside some fraction of that research time — 20%, 25%, 30% — for thinking about problems which not only do you not know that you can solve, but that you do not know that anyone can solve, in 2–3 years or in infinite time. You may not in the end solve them, but in the long run they are the ones really worth doing.

Finally, what of the future would I advise my grandchild (assuming he/she were interested in academic life in the first place) to go into condensed matter physics? I think there is plenty of life left in the subject (Fig. 5). First, if we think about continuing in the existing mold, there are probably yet more sophisticated ordered phases to be discovered; the far-off equilibrium behavior of many CM systems is still almost 100% mysterious, there is the prospect of having to deal, particularly in the context of quantum computing, with even more strongly and delicately entangled states than the ones we now know…

However, the really slippery, and therefore really fascinating, issues in science are not the ones where we have well-defined questions and are simply trying to find the answers, but the ones where we literally do not know *what questions to ask*! And by definition, these are not to be found in periods of Kuhnian "normal" science, so we may have to actively push the boundaries of what we regard as "condensed matter physics" in order to find them. One direction in which this is already to some extent happening is that of biological organization, the brain, consciousness… Another possible one is the foundations of quantum and statistical mechanics in the context of macroscopic CM systems. For example: how do we (can we?) describe not just the outcome of an experiment but its *preparation* entirely in quantum-mechanical terms? Is the so-called "arrow of time" a spontaneously broken symmetry?… and so on. One modest step which is already being taken in this direction is the use of condensed matter physics to test the quantum mechanics of a macroscopic variable-(perhaps an "invisible" paradigm shift…).

So yes, I think I would have no compunction in encouraging my grandchildren to go into condensed matter physics; I do not think they will be bored or disappointed.

Conflict of interest

The author declares that he has no conflict of interest.

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Anthony J. Leggett is widely recognized as a world leader in the theory of low-temperature physics, and his pioneering work on superfluidity was recognized by the 2003 Nobel Prize in Physics. He set directions for research in the quantum physics of macroscopic dissipative systems and use of condensed systems to test the foundations of quantum mechanics. He has been particularly interested in the possibility of using special condensed-matter systems, such as Josephson devices, to test the validity of the extrapolation of the quantum formalism to the macroscopic level.