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On the Nature of Research in Condensed-State Physics

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According to a commonly held view, the properties of condensed-matter systems are simply consequences of the properties of their atomic-level components, and all of theoretical research in condensed-matter physics consists essentially in deducing the former from the latter. I argue that this apparently plausible picture is totally misleading, and that condensed-matter physics is a discipline which is not only autonomous, but guaranteed in the long run to be fundamental.

To many philosophers, historians and sociologists of science, physics has served as the paradigm of natural science as a whole. If in the late twentieth century we were to glance around and count the number of physicists working, in universities, research institutions and industrial laboratories, on the various areas of the subject, then we should certainly find that a majority—my guess would be 70 to 80 percent—are working on the general area known as the physics of condensed matter, an area which by now embraces topics as diverse as traditional solid-state physics, neutron stars and the physics of biological matter, in fact by a liberal definition just about all of physics outside atomic, nuclear and particle physics and cosmology. Yet, oddly, this vast enterprise has attracted little attention from philosophers or others interested in the nature of scientific research.

The reason for this neglect is, of course, not far to seek. There is a widespread view around, which is current not just among professional students of science as an intellectual enterprise but, perhaps more surprisingly, among many working physicists, according to which the "fundamental" areas of physics in the late twentieth century are particle physics and cosmology, and all other areas of the discipline, including condensed-matter physics, are "derivative" and hence, by implication, less

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worthy of the attention of philosophers and others. A robust statement of this kind of attitude, as phrased by a working physicist outside the area, is the following quotation from the particle theorist S. Glashow[1]:

> "Important new theories *do* emerge in other sciences [than high-energy physics and cosmology]... How truly fundamental are they? Do they not result from a complex interplay among many atoms, about which Heisenberg and his friends taught us all we need to know long ago?"

A more sophisticated formulation of this point of view is sometimes based on the well-known "revolutionary/normal" dichotomy popularized by T. S. Kuhn,[2] according to which the history of science is characterized by long periods of "normal" research, interrupted by occasional violent upheavals ("revolutions") in which the whole framework ("paradigm") within which the questions are asked and solved is challenged and eventually overthrown. It is Kuhn's characterization of "normal" science which is relevant here: of the many quotations in his writings describing it, the following is fairly typical:

> "When engaged in a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given suficient brilliance, it can be solved. Of course, the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability, not the corpus of current science, is impugned."[3]

It is interesting that even those philosophers who have taken strong issue with other aspects of Kuhn's work have often appeared to accept his thesis concerning the nature and characteristics of "normal science"; for example, Sir Karl Popper, in whose view of science the element corresponding to this would presumably be, roughly, the deduction of consequences from the overriding "research program" acknowledges the importance of Kuhn's identification of the phenomenon and remarks "The success of the 'normal' scientist consists, entirely, in showing that the ruling theory can be properly and satisfactorily applied in order to reach a solution to the puzzle in question."[4] And many other philosophers of science of no particular allegiance seem to share those views. Once one has formed this picture, there seems to be little doubt in the minds of the vast majority that all of condensed matter physics falls firmly in the category of "normal science," and usually (though not always) that by implication it is something the philosopher or historian can afford to ignore. [I think, however, that some of Sir Karl's own writings (e.g., his remarks about the work of the pre-*Origin of Species* Charles Darwin[4]) indicate that he would be sympathetic towards the point of view I express below.] If nothing else, this is evidenced by the infrequency with which the subject is referred to in philosophical or historical discussions: Newton, Einstein, Heisenberg .. these are the staples of such discussions, but how many refer to Sadi Carnot or Willard Gibbs, let alone Lev Landau?

I believe that at the basis of this attitude, in whatever language it may be expressed, there lies a common belief: namely that all of science (or at least of theoretical science) which does not consist in the framing of "basic" hypotheses (or paradigms, research programs ... choose your favorite term) consists in the *deduction of consequences* from these hypotheses within certain fixed rules, a process which is analogous if not identical to the construction of a proof in mathematics or formal logic; and that the whole of condensed matter physics is a prime example of this latter enterprise.

In this essay I shall argue that this view of condensed-matter physics is largely if not totally misleading, and (by implication) that it is not only much more representative of science as a whole than the disciplines of particle physics and cosmology, but well worthy of philosophical study in its own right. The remarks which follow make no pretence to philosophical sophistication; they are merely the reflections of a longtime practitioner of the discipline who has tried to absorb at least some of the things said by philosophers, historians and sociologists about the practice of science and to test them against his own experience.

Let me start by making a distinction which seems rather obvious but seems often to be blurred in nontechnical discussions of these issues: One should clearly distinguish the question of whether a particular scientific discipline or subdiscipline is "derivative" (whatever that may mean: explained later) from the question of whether research in it ever, or regularly, or occasionally involves radical shifts of view of the kinds of questions it is legitimate or justified to ask, and the kinds of criteria which may be applied to judge the validity of the answers—crudely speaking, a "paradigm shift" in the language of Kuhn, or a radical restructuring of the theoretical framework in that of Popper. It may indeed possibly be argued that if a particular field is "derivative" in the fullest sense of the word, then almost by definition it cannot involve such radical reappraisals; but even if we grant this for the sake of argument, the converse is most certainly not true. For example, most current work in particle theory is strictly confined within the framework of quantum field theory, which essentially defines not only the language of discussion but the kinds of questions and answers which are legitimate. Many important and spectacular advances in the field, for example electroweak unification or quantum chromodynamics, in no way involve a challenge to this framework: what they involve is trying out different "moves on the chessboard," in this case different versions of Lagrangian field theory. That in this case one has no guidance from any "lower" or "more fundamental" level as to the correct form of Lagrangian is a quite different point. Thus, research of this type may well deserve the name of "fundamental" but it can hardly be seriously argued that in Kuhnian terms it is anything other than "normal" science.

Consider by contrast an example of an important advance in condensed matter theory, which while it may perhaps not be "typical" is certainly not unique in its nature, namely Landau's theory of a Fermi liquid.[5] Until Landau's work, most physicists, in their research on complicated systems of many particles, had implicitly and often unconsciously assumed that the only way to think about these systems was to start from a microscopic Hamiltonian (or Lagrangian), make the minimum possible number of "physical approximations" (on the nature of which see below) and produce quantitative predictions for the interesting experimental properties of the system. Landau in effect turned the question around by proposing that we *ignore* the question of the correct form of the microscopic Hamiltonian (except perhaps for a few qualitative features such as its symmetry properties) and ask rather: Suppose that we make a few qualitative assumptions about the energy spectrum, etc., of the system, then what interesting relations between the experimental properties can we infer? Needless to say, this restructuring of the framework does rely, to an extent at least, on information drawn from "lower-level" descriptions (one would hardly be tempted by the idea of a "Fermi" liquid if one did not believe that the component atoms involved obey Fermi statistics!), and to that extent may perhaps be legitimately argued not to be "fundamental"; but it certainly did change the whole way in which condensed-state physicists looked at their subject, and, interestingly, according to my recollections, evoked—admittedly in a relatively small community and on a small scale—precisely the kind of reactions which Kuhn describes as characteristic of scientific "revolutions" (including the tendency, once the dust has settled, to deny in retrospect that any paradigm shift ever really occurred!). Of course, I am not for a moment claiming that this kind of episode is peculiar to, or even particularly characteristic of, condensed matter physics—one can certainly find plenty of examples at a similar level in other cases, for example the debate concerning the S-matrix approach in particle physics; all I want to emphasize is that the question of "normal" versus "revolutionary," in so far as it is a sensible one at all, bears little logical relation to the question (also of debatable meaning) of "derivative" verses "fundamental."

What I am particularly concerned to address here, however, is the view according to which (a) the properties of condensed-matter systems are "obviously" simply the consequences of the properties of their atomic-level components, and (b) all of theoretical research in condensed-matter physics simply consists in deducing (or attempting to deduce) those properties from the "fundamental" laws governing the constituents, this process of deduction being essentially of a logical or mathematical nature and limited only by our failure in practice to be able to do the necessary mathematics. My argument against (b) is based on an analysis of the actual practice of present-day condensed-matter theory, and has, I believe, a reasonable chance of being accepted by other practitioners of the discipline; if it is accepted, then even if one assents to (a) its significance may seem to be somewhat modified. However, I also present a very much more radical and unorthodox argument against (a) in which I personally have great confidence but which I suspect will convince only a minority, if indeed any, of my colleagues.

Let's start by getting rid of a red herring. It is, of course, true that there exists *some* research which might naturally be classified under the heading of "condensed matter physics" which at first sight at least has the characteristics described under (b), e.g. the type of research usually known as "rigorous statistical mechanics." For example, consider the following problem, which is fairly typical of this kind of work: One starts with a Hamiltonian operator $\hat{\mathrm{H}}$ which describes a set of (quantum-mechanical) "spins" on a certain type of lattice with some specified form of interaction between them. One then specifies that the quantum-mechanical density matrix which describes the behavior of the system has the standard Gibbs form ($\propto \exp - \hat{\mathrm{H}}/kT$), and asks the question: Do any of the derivatives of the free energy, i.e. the quantity $-kT \ln Tr \exp - \hat{\mathrm{H}}/kT$, have a singularity as a function of (say) temperature? ("does the system undergo a phase transition?"). This kind of problem, while it may be attacked by people working in physics departments and published in physics journals, is of course a *purely mathematical problem*; it is easy to see that it could be successfully solved by a mathematician who had not the slightest inkling of the physical meaning of the quantity denoted T. (Needless to say, to interpret the *significance* of his results to physicists he would need to know this and quite a bit more.) It is indeed research in physics—in the same sense as writing a computer program to do stylistic analysis on the New Testament is research in theology. (How much would one learn about the nature of theology as a discipline by studying such a program?) Incidentally, and perhaps ironically, in the present context, this kind of purely mathematical work is much more numerically prevalent in supposedly "fundamental" disciplines such as cosmology—as when for example one tries to establish that given certain conditions the equations of general relativity are bound to lead to a singularity.

Is it correct to characterize work such as this as an example of "deducing the properties of macroscopic systems from the properties of their atomic-level constituents"? Not at all—at least not if the word "deduction" is understood to bear any relation to its use in mathematics or formal logic. Suppose that one is faced with the real-life problem of a piece of (say) europium sulfide of a certain shape and size, and wishes to know whether or not it will behave like a permanent magnet below a certain temperature. [I have to use this possibly arcane-sounding example (europium sulfide) because the best-known ferromagnetic materials (iron, nickel, cobalt) are metals and therefore not plausibly described by this model.] In order to make the calculation described here even relevant to this problem, one has first to abstract from the actually quite complicated structure and interactions of the europium and sulfur ions those particular features which one believes to be responsible for the magnetic behavior. In this abstraction many properties which would be vitally important in other contexts such as neutron scattering or plastic deformation studies (e.g. the isotopic composition of the elements involved, or the fact that the crystal lattice certainly contains dislocations) are simply ignored; one builds a *model* for the real-life system, that is, a representation which incorporates the features that one believes to be essential in the present context but leaves out everything thought to be irrelevant. I will argue later that it is precisely this model-building which is the crucial element. Secondly, one has to assume that in the real-life (laboratory bench) situation the interactions of the system with its environment are such that they can reasonably be taken into account by the standard statistical-mechanical techniques. This is actually very far from obvious *a priori* and needs considerable argument even to make it plausible.

> "Essentially, therefore, let us say that we do not believe that there is any object at equilibrium in the universe. What is in equilibrium is the local environment, the first neighbors. But correlations of billions of particles are not at equilibrium. In every object the arrow of time starting from the big bang is still present and will go on forever"—Petrosky and Prigogine.[6]

(I am tempted to believe that in certain examples of great current interest, e.g. involving so-called "mesoscopic" systems, these techniques may actually give spectacularly misleading results). Thirdly, one needs to be able to interpret the formal singularities which may appear in the free energy of the model system or its derivatives in terms of the occurrence of a spontaneous magnetization in the physical sample; this in turn involves a number of implicit assumptions, e.g. that the experimentally observed magnetic field associated with it is simply the sum of the magnetic fields produced by the individual spins, that the real-life inhomogeneity of the earth's magnetic field has no substantial effect on the results, etc., etc. That many of these assumptions are, to the working physicist in the area, eminently "plausible" is undoubted, but is not the point; no self-respecting mathematician or logician would look twice at anything which called itself a "proof" which had to invoke such steps.

Indeed, I would make an even stronger and perhaps at first sight more quixotic claim: that no significant advance in the theory of matter in bulk has ever come about through derivation from microscopic principles. At one level this perhaps hardly needs arguing: few people nowadays familiar with the concepts and results of statistical thermodynamics would seriously maintain that they can at present be derived from microscopic theories like Newtonian or quantum mechanics, let alone that they were originally found by this method. I would confidently argue further that it is in principle and forever impossible to carry out such a derivation: but the reasons for this are somewhat technical, so I will not go into them here. Statistical thermodynamics at least, then, must be considered a fully-fledged theory to be judged in its own right, or in Kuhnian terms, an independent paradigm. But someone might concede this point, and then say: But once you have statistical mechanics, and you have quantum mechanics, and electromagnetic theory, then in principle surely you can explain all the behavior, say, of solids? Isn't this just what solid-state physics is all about, turning the "in principle" into "in practice"? The answer is a resounding NO. Quite the opposite: the so-called derivations of the results of solid-state physics from microscopic principles *alone* are almost all bogus, if "derivation" is meant to have anything like its usual sense. Consider as elementary a principle as Ohm's law. As far as I know, no-one has ever come even remotely within reach of deriving Ohm's law from microscopic principles without a whole host of auxiliary assumptions ("physical approximations"), which one almost certainly would not have thought of making unless one knew in advance the result one wanted to get, (and some of which may be regarded as essentially begging the question). This situation is fairly typical: once you have reason to believe that a certain kind of model or theory will actually work at the macroscopic or intermediate level, then it is sometimes possible to show that you can "derive" it from microscopic theory, in the sense that you may be able to find the auxiliary assumptions or approximations you have to make to lead to the result you want. But you can practically never *justify* these auxiliary assumptions, and the whole process is highly dangerous anyway: very often you find that what you thought you had "proved" comes unstuck experimentally (for instance, you "prove" Ohm's law quite generally only to discover that superconductors don't obey it) and when you go back to your proof you discover as often as not that you had implicitly slipped in an assumption that begs the whole question. At best, then, such "microscopic derivations" have the essentially negative function of showing that there is no obvious *contradiction* between one's microscopic principles and the intermediate or high-level model. Indeed, some of the most important results in all of physics (the Bohr-van Leeuwen theorem, Bell's theorem...) are *negative* results, in the sense that they establish once and for all that there are *no* microscopic theories of a given class which can be used to build a higher-level model which will agree with experiment. Incidentally, as a psychological fact, it does occasionally happen that one is *led* to a new model by a microscopic calculation. But in that case one certainly doesn't believe the model *because* of the calculation: on the contrary, in my experience at least one disbelieves or distrusts the calculation unless and until one has a flash of insight and sees the result in terms of a model.

I claim then that the important advances in macroscopic physics come essentially in the construction of models *at an intermediate or macroscopic level*, and that these are logically (and psychologically) independent of microscopic physics. Examples of the kind of models I have in mind which may be familiar to some readers include the Debye model of a crystalline solid, the idea of a quasiparticle, the Ising or Heisenberg picture of a magnetic material, the two-fluid model of liquid helium, London's approach to superconductivity.... In some cases these models may be plausibly represented as "based on" microscopic physics, in the sense that they can be described as making *assumptions* about microscopic entities (e.g. "the atoms are arranged in a regular lattice"), but in other cases (such as the two-fluid model) they are independent even in this sense. What all have in common is that they can serve as some kind of concrete picture, or metaphor, which can guide our thinking about the subject in question. And they guide it *in their own right*, and not because they are a sort of crude shorthand for some underlying mathematics derived from "basic principles." Of course, the degree of abstractness varies very greatly, depending on the inventor or user of the model: some people visualize, in some sense, a quantum-mechanical wave function, while others find even the idea of a quasiparticle too abstract and are happy, if at all, only at the level of two interpenetrating fluids or atoms arranged in a regular lattice. But whatever the degree of abstractness, the invention of a new model is in essence the invention of *a new way of seeing things* which is not reducible to the old. Indeed I think most people who have had the experience either of inventing a new model, however humble, themselves, or of resisting one offered to them and finally accepting it, will recognize in these processes a great deal of what Kuhn says about the "gestalt shift" involved in conversion from one paradigm to another. (I think though "expansion" might be a better word than "shift.")

One obvious objection needs to be met: Agreed that *sometimes* in the theory of complex matter someone invents a new model. But surely the great bulk of work even in, say, solid-state physics is at the "puzzle-solving" level? That is, one takes a *given* model and works out its consequences, by some kind of deductive process. True: if you count pages of scientific journals you will certainly find that this level predominates. In fact, if you tried to estimate the time spent at the various levels even by someone like Lev Landau—perhaps the supreme model-inventor of the twentieth century—you might well find most of it was at this level. So what? Probably if you actually counted the time spent by an experimental physicist on various aspects of an experiment, you might find most of it was spent on plugging leaks in his vacuum apparatus. Does that prove that the plugging of leaks was the crucial part of the experiment?

By this time it will have become obvious that there is a strongly normative element in what I am saying. That is, I grant that if you count research even in the physics of bulk matter by weight, you will find a preponderance of the level of applied mathematics. Yet I am claiming that the *important* advances all come at the level of models, thereby to some extent defining what are my criteria for an advance to be important. This is quite deliberate: I simply don't believe that there is very much value in any piece of research which does not, at least by implication and even over a very small area, provide one with a "new way of seeing things." Whether this way of seeing things is valid or not, of course, may not be entirely clear until one has done a bit of the necessary applied mathematics—although in a surprising number of cases the mini-gestalt switch is itself, psychologically speaking at least, evidence enough.

I now turn to my second and much more radical thesis, namely that not only is there no good reason to believe that *all* the properties of condensed-matter systems are simply consequences of the properties of their atomic-level constituents, but that *there is a very good reason not to believe it.* This view may seem so startlingly antithetical to the "reductionist" point of view which has prevailed in science over the last three hundred years that the reader may be tempted to dismiss it out of hand. So let me acknowledge at once that while I believe that all that I have stated about the methodology and conceptual status of the theory of condensed matter is valid, I have no doubt that it is consistent to maintain that "in principle" *most* of the currently known properties of macroscopic condensed systems may be in some sense determined by the properties of the atomic-level entities composing them. Indeed, I would be perfectly happy to share the conventional reductionist prejudice were it not for a single fact (I call it that—others may demur) which is so overwhelming in its implications that it forces us to challenge even what we might think of as the most basic common sense. This fact is the existence of the quantum measurement paradox; and I believe that if we take it with the seriousness it deserves, and follow the argument to its logical conclusion, it will tell us in a quite *a priori* sense that research in condensed-matter physics, or more accurately some aspects of it, is guaranteed in the long run (and it may be a very long run!) to be fully both as "fundamental" and as "revolutionary" as that in particle physics or cosmology.

Since I have discussed the technical aspects of the argument at length elsewhere (e.g. Ref. (7)), I will here be quite brief. At the microscopic level we are by now accustomed to the idea of quantum-mechanical superpositions of states which can result in interference effects. A typical example might be the state of a photon at the intermediate screen in the classic Young's slits set up, or of a neutron at the second "ear" of a neutron interferometer; however, there are of course also cases where the two states in question do not correspond to physically separated positions but to something more abstract, as in a beam of neutral K-mesons or in the EPR experiment. The crucial point is that in every such case it seems impossible, without extreme contortions, to interpret the experimental results obtained on the ensemble by the hypothesis that each individual member of the ensemble took a particular "branch" of the superposition (e.g. that each individual neutron took one path or the other through the interferometer). In other words, at the microscopic level, given that we have a situation where two (or more) possibilities are left open, and where the formalism of quantum mechanics predicts a nonzero amplitude for each, then for each individual member of the ensemble neither possibility is definitively "realized." We should carefully distinguish this assertion, which if you like is metaphysical in nature, from the *evidence* for it, which is the physically observed phenomenon of interference.

Imagine now that the microscopic difference between the two states in question is amplified to the macroscopic level, as is done for example in the famous "Cat" thought-experiment of Schrödinger, or more realistically in a typical measurement process. It is a standard and well-known result that because of the strict linearity of the formalism of quantum mechanics, the formal quantum-mechanical description of the final state of the universe also corresponds to a superposition—but now a superposition of states corresponding to *macroscopically* distinct properties. The only qualitative difference with the situation at the microscopic level is that it would now almost certainly be impossible in practice, and would be argued by many to be impossible even in principle, to see the effects of interference between these two macroscopically distinct states. In the case of a realistic measurement process, or in any situation reasonably resembling that of Schrödinger's famous cat (as distinct from the *different* types of situation, also involving quantum superposition of macroscopically distinct states, discussed, e.g., in Ref. (8)) I have no quarrel with this assertion, and since the reasons for it have been (more than) adequately discussed in the quantum measurement literature (e.g. Ref. (9)) will not go into them here. The crucial question is, does this observation in any sense "solve" the quantum measurement paradox?

The basic point seems to me to be the following: At the microlevel, practically everyone[2] agrees that an interpretation of the quantum formalism which attributes a definite choice of one of the two or more relevant states to each member of the ensemble is unviable, the reason being, as already mentioned, that it is then impossible without extreme contortions to account for the phenomenon of interference. Now the quantum mechanical formalism is a seamless whole, and in no way changes as we progress from the microscopic to the macroscopic; we cannot, therefore, turn around and assign an interpretation to it at the macrolevel which we were unwilling, on good grounds, to assign at the microlevel. It is simply *irrelevant* to this argument that the evidence against the "realistic" interpretation which we were able to marshal at the microlevel is no longer available at the macrolevel—an observation which immediately makes about 95% of the literature on the quantum measurement paradox pointless. What is required is *not*, as these papers assume, to demonstrate the absence of interference between macroscopically distinct states, but to explain how one particular macrostate can be forced by the quantum formalism to be realized—a feature of the world which is so direct and basic a part of our experience that we cannot even imagine what life would be like without it. (For a clear and forceful statement of this point, see Ref. (11).) In the opinion of the present author (which is shared by a small but growing minority of physicists) no solution to this problem is possible within the framework of conventional quantum mechanics.

Obviously, to make this claim plausible one has to examine and criticize in detail the existing "solutions" to the quantum measurement paradox. This has been done elsewhere by many people, including the present author, and there is little point in repeating these arguments here. Let us rather ask: If the claim is accepted, what follows? We have excellent reason to believe that the quantum formalism provides an excellent quantitative account of the behavior of one, or a few, microscopic entities such as electrons and neutrons, and also of the properties of macroscopic bodies to the extent that these are in one sense or another the sum of single- or few-particle properties. Yet this argument implies that quantum mechanics, of its nature, cannot give a *complete* account of all the properties of the

[2] One must make an exception for adherents of the "pilot wave" interpretation (see e.g. Ref. (10)), which however seems to me to evade the conclusion only by implicitly redefining the concept of "definite choice."

world at the macroscopic level—at least if we take the description right up to the level of our direct experience. It follows that *somewhere* along the line from the atom to human consciousness quantum mechanics must break down, and some other principle beyond the Schrödinger equation must come into play. Exactly where and how this happens is of course a matter for speculation: at one extreme one could envisage a nonlinear theory of the general type considered by Pearle[12] and more recently by Weinberg,[13] which introduces effects which are (at least currently) unobservably small at the single-atom level but may be amplified vastly when large numbers of particles are involved, while at the other extreme one might postpone the transition to "actualization" right up to the point where human consciousness comes into play. A specific suggestion for a framework within which "actualization" may be realized has been made recently by Ghirardi *et al.*[14] Whether a theory of this type turns out to be ultimately correct, or whether the true solution to the problem introduces far more exotic elements, we can be sure of one thing: The theory of condensed matter cannot, on quite *a priori* grounds, reduce in *all* respects to the theory (or at least the current theory) of the microscopic elements composing it. In effect, we are in the position of the minority of physicists in the late eighteenth century who foresaw, correctly, on quite *a priori* grounds, that any theory of gravitation based on instantaneous action at a distance must some day break down. Like them, we know that quantum mechanics must break down, and moreover can see at least one direction (that oriented towards the macroscopic world) in which this must happen; like them, we cannot at present reasonably foretell when, why and how. All we can be sure of is that when the breakdown occurs, be it in the next decade or five hundred years from now, it will produce, just as did Einstein's general relativity, a fundamentally new and exciting realm of physics.

It is a pleasure to dedicate this essay to Sir Karl Popper on the occasion of his 90th birthday and to wish him many more happy years of research in the foundations of science.

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