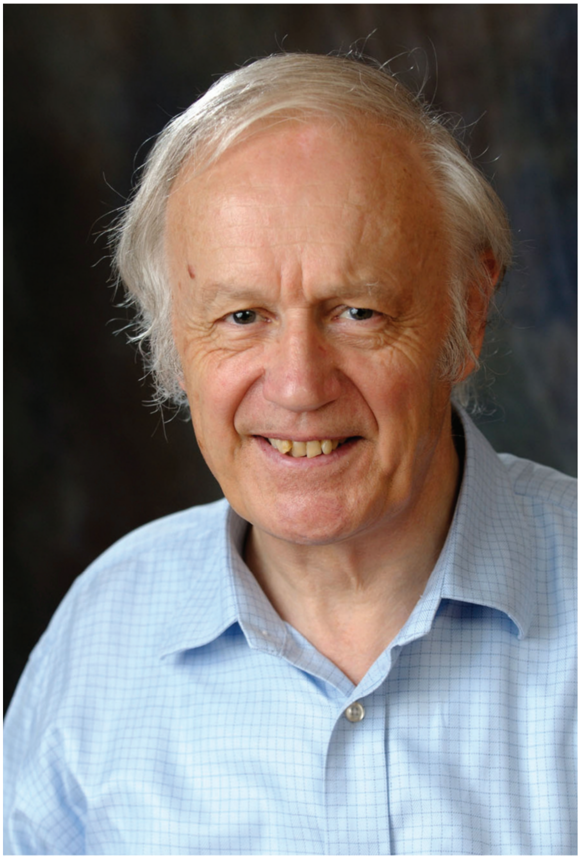

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Annual Review of Condensed Matter Physics

Matchmaking Between Condensed Matter and Quantum Foundations, and Other Stories: My Six Decades in Physics

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Abstract

I present some rather selective reminiscences of my long career in physics, from my doctoral work to the present. I do not spend time on topics such as the nuclear magnetic resonance behavior of ^{3}He, as I have reviewed the history extensively elsewhere, but rather concentrate, first, on my long-running project to make condensed matter physics relevant to questions in the foundations of quantum mechanics, and second, on various rather "quirky" problems such as an attempt to amplify the effects of the parity violation due to the weak interaction to a macroscopic level, and an unconventional proposal for the mechanism of the first-order phase transition between the A and B phases of superfluid liquid ^{3}He.

My prelude to a career in physics was not exactly typical: I was certainly not the kind of teenager who takes the radio set apart to see how it works. (Had I done so, I would certainly have been totally unable to reassemble it.) Indeed, until about the age of 20, I had essentially zero interest in physics or in any area of science. How I acquired such an interest, and how I was able to convert from my original education on the arts side of Oxford University to a second undergraduate degree in physics, is something I have recounted in some detail in the "autobiography" I wrote for the Nobel Foundation (1), so I will not repeat myself here; I only note the debt I owe in this context to the former Soviet general Sergei Korolev. (Readers intrigued by this remark may want to read Reference 1, and then look him up in Wikipedia).

At any rate, in the summer of 1961, after a foreshortened undergraduate degree that gave me a certain amount of intellectual indigestion, I found myself ready to embark on a doctorate in theoretical "solid state" physics at Oxford under the supervision of the late Dirk ter Haar. As related in Reference 1, Dirk's philosophy was to force his students, as far as their thesis research went, to fend for themselves from the word "Go"; their average graduation rate was I believe only around 50%, but I suspect that most of those who failed to achieve a doctorate (and thus, usually, a position in academic life) ended up happier doing other things than they would have been in academia. Indeed, when I survey the current North American scene, and particularly the huge imbalance between the available jobs and the number of applicants for them, I sometimes feel tempted to imitate Dirk's mode of operation (and am only deterred from doing so by the prospect of becoming the target of a lawsuit). Anyway, I was one of the lucky ones who survived this treatment, eventually graduating with a thesis on two more or less mutually unrelated (and in retrospect not terribly important) problems in the theory of liquid ^{4}He and ^{3}He, respectively. (In those days, for want of a more appropriate home, liquid helium tended to be pigeonholed under "solid state" physics, and several of Dirk's students worked on it). Along the way I picked up, as did my fellow students, a full panoply of field-theoretic methods, with our "bible" being the celebrated textbook of Abrikosov, Gor'kov & Dzyaloshinskii (AGD; 2), which still a half-century later adorns my bookshelves. Throughout my doctoral work, I was regularly teaching undergraduates, in individual or small-group tutorials, for the maximum time allowed (six hours per week), and in retrospect I feel that this activity had just as many long-term benefits as my research.

The natural next step was to apply for a postdoctoral position, and I was aware that at the time (1964) the leading institution in the world in my chosen field was the University of Illinois at Urbana-Champaign (UIUC). So, encouraged by Dirk, I applied there, and specifically to the late David Pines as a possible advisor. From the vantage point of more than a half-century later, my credentials seem laughable: Apart from my still-being-written thesis, the only publication I was able to list was an as yet unpublished one-page Physics Letter. But Dirk's letter of recommendation apparently carried the day, and David accepted me; I shall always be grateful for his confidence in me in the face of almost no hard evidence, and for the generosity he showed me during my postdoc year at UIUC, particularly in leaving me to explore my own interests free of constraints.

In the mid-1960s two major paradigms dominated theoretical condensed matter physics: one, which was well known to the community at large but of which I was, on my arrival in Urbana, largely ignorant, was the Bardeen–Cooper–Schrieffer (BCS) theory of superconductivity and, by extension, of Cooper pairing in other weakly interacting degenerate Fermi systems. The other, which was less well known in the West[1] but with which I had become enamored during my thesis work, was the Landau theory of a normal Fermi liquid, of which the prime (indeed at the time the

[1]It was known to the experimentalists in Oxford, but they tended to dismiss it as a mere parametrization of the experimental data.

only electrically neutral) example was liquid ^{3}He. Soon after I arrived in Urbana, John Bardeen and Leo Kadanoff had proposed to me, in connection with the experiments of our then experimental colleague John Wheatley, the calculation of the spin diffusion coefficient of the then putative Cooper-paired ("superfluid") phase of liquid ^{3}He in the hope that it might act as a diagnostic of the onset of the pairing. While thinking about this project (which I never completed, then or later), it occurred to me that to get a quantitative account of the behavior of ^{3}He in the conjectured superfluid phase of ^{3}He it would be necessary to combine the BCS and Landau paradigms. I knew at the time of one paper (3) that had used field-theoretic techniques to do this at zero temperature, but with unspectacular results [the only effect was to multiply the BCS (weak-coupling) results by a constant]; since Wheatley's experiments would be close to the transition temperature to the superfluid phase, I felt I should generalize the work of Reference 3 to this regime. When I started this calculation, I expected that the outcome would be a simple (and boring) generalization of the zero-temperature result, i.e., just a multiplicative constant. In the event I found something much more exciting: The Fermi-liquid effects changed the shape of the temperature dependence of "normal-component" quantities such as the spin susceptibility and normal density! Having originally obtained this result by field-theoretic methods, I felt it was very important to reproduce it by a more intuitive argument, and eventually succeeded in doing so by recasting the original Landau Fermi-liquid theory in terms of a set of Weiss-like "molecular fields," which was an idea that was rather aesthetically appealing to me. I think it was, more than anything, the adrenalin rush I experienced from this (prima facie[2]) novel and original piece of work that persuaded me to continue in academic life (something which, until that point, I had been not too sure about).

My second postdoctoral year was spent (thanks to the generosity of the Fellows of my by-then Oxford college, Magdalen) in the group of Professor Takeo Matsubara at the University of Kyoto in Japan. From a personal and social point of view, this was one of the most exciting years of my life (I said a little about it in Reference 1); from a purely academic angle, it was rather less so, but there were two developments worth recording: First, motivated by some experiments that seemed to show that elemental niobium was a "two-band" superconductor (i.e., one in which the locus of the Fermi energy intersects two different energy bands), I did a little calculation that predicted the existence of a collective excitation of the superconducting phase in which Cooper pairs "sloshed" between the two bands (the analog of the Josephson plasma resonance). No sooner had I sent this calculation off for publication than new experiments showed that niobium was not a two-band superconductor after all; but the work was to stand me in good stead later (1). The second aspect of my year in Kyoto that had long-term academic consequences was that, to supplement my income, I worked part-time in the office of the journal *Progress of Theoretical Physics*, with my main task being to check and, where necessary, correct the English of the papers submitted to it. At the end of that year, I wrote a short essay entitled "Notes on the Writing of Scientific English for Japanese Physicists," in which I tried to address some problems that seemed to me rather generic among writers whose native tongue is Japanese. For some reason, that essay, and especially one diagram in it (which acquired the name of the "Leggett tree"), caught the imagination of its readership, and it has been widely reprinted in Japan, not just in the physics literature. It occurs to me from time to time that, whatever the citation record may say, in terms of the real effect on what other people do (or refrain from doing!), this little essay has probably had more impact than any other of the 200-odd papers I have written over my career.

[2]It was only about 30 years later that I realized not only had my result for the spin susceptibility been anticipated (4) but so (at least arguably) had been that for the superfluid density (5). I sometimes wonder what would have been the effect on my motivation, and maybe my career, had I known that at the time. . . .

I spent one more postdoctoral year in "roving" mode (through Oxford, Harvard, Illinois, and Aspen), and in the course of it applied successfully for the position of lecturer at the University of Sussex. This was one of the group of so-called plateglass universities founded in Britain in the early sixties; I was attracted to it partly because the physics department was led by two people whom I knew and respected from my Oxford days, Roger Blin-Stoyle on the theoretical side and Douglas Brewer in the experimental area, and partly because of the reputation that the university had already acquired for its pioneering and interdisciplinary ethos. In the end, I spent 15 years at Sussex, from 1967 to the end of 1982, and despite the political and other upheavals that gave it a certain notoriety among the British public in those years found it rather congenial.[3] One thing that was very important to me, and was I think a determining factor in my career, was the very relaxed atmosphere in which I was able to work: In those (halcyon!) days even the position of lecturer at a British university effectively carried tenure, so I did not have to worry about publishing a lot of papers in order to obtain it. In fact, my perception of my conditions of employment was that so long as I did a good job in my teaching (which was heavy by the standards of a North American research university—up to 15 contact hours per week, not counting graduate students), I could go home on Friday evening feeling that I had earned my salary for the week; if I chose to do research on the evenings, weekends, or vacations, the university would encourage it, and provide me with a library, secretarial assistance and so on, but it was not part of my job.

As a result, during my early years at Sussex I felt able to explore a wide range of interests, not just within physics but across disciplinary boundaries. Two colleagues who influenced me a lot were Brian Easlea, who after a conventional education as a low-energy nuclear theorist migrated into the history and sociology of science (and actually became a very charismatic figure for some of the more radically minded students), and Aaron Sloman, again I believe a physicist by training, who was an early exponent of the new discipline of artificial intelligence. Partly because of conversations with them and other colleagues in the departments of history and philosophy of science, and partly because of my original training in philosophy, I began to reflect on what exactly it was that I and my colleagues in condensed matter physics were actually doing. At that time (the mid-1960s), much discussion in the philosophy and sociology of science centered on the alleged confrontation between the views of Thomas Kuhn as expressed in his classic book *The Structure of Scientific Revolutions* (6) and those of Karl Popper as set out in his earlier work *The Logic of Scientific Discovery*, (7) and this was crystallized in a rather influential conference proceedings (8) that appeared in 1970. When I was asked by an African colleague to contribute a paper to the inaugural issue of a journal he was starting, I volunteered to write a review (9) of this book (the subtitle could have been the one I used for some related talks, "The view from inside the zoo: a working physicist looks at some current philosophy of science"). One thing which struck me forcefully was a misconception that seemed to be shared by almost all the contributors to the book, whether they were pro-Kuhn, pro-Popper or neutral: In so far as they deigned to talk about condensed matter physics at all, they took the view (as I put it) that "all theoretical science which is not concerned with the formulation of basic hypotheses must reduce to the process of deduction from these hypotheses, and that this process, being basically of a logical or mathematical nature, is limited only by the intractability of the mathematics and the consequent need for approximate techniques." I commented that "what this ignores is the existence of vast areas of science—for instance a great part of the theory of condensed matter—where progress typically comes through conceptual innovation at the intermediate level, that is, through the invention of new 'ways of seeing things' which are neither derivable from the current microscopic theory nor (usually) directly

[3] Not all my contemporaries had such a positive experience: at least judging by his novel *Sweet Tooth*, which is largely set at Sussex in the 1970s, the author Ian McEwan evidently had more jaundiced recollections.

challenge it, but coexist amicably with it—at least for a time," and referred in this context to the work of Landau as typical. In retrospect I suppose this to some extent foreshadowed recent discussions of so-called "emergence"; however, I was assuming that while these things needed saying to the professional philosophers and sociologists of science, to anyone actually working in theoretical condensed matter physics they would seem glaringly obvious. Indeed, I have always regarded "emergence" as a rather common-sensical idea; I have been rather bemused by the fuss it seems to have engendered in the past few years (cf. 10) and irritated by the widespread and pleonastic[4] use of the corresponding adjective as a buzzword.

Within physics, although I continued to spend most of my time on researching problems in the traditional low-temperature area (mostly one or other of the helium isotopes), my interests started to shift. When I arrived at Sussex, my attitude toward issues in the conceptual foundations of quantum mechanics, and in particular the "measurement problem" was well summed up by a quote from a paper (11) by Bell and Nauenberg written at just about that time: "The average physicist feels that [these questions] have long been answered, and that he will fully understand just how if ever he can spare twenty minutes to think about it." However, soon after my arrival, Brian, who had been much influenced by the writings of David Bohm, gave a mini-course on the measurement problem, which convinced me that my prejudice, that the problem was simply a result of sloppy philosophical analysis, was too naive, and from then on, I became more and more intrigued by it and other foundational issues such as Bell's theorem, although for a reason I will come to in a moment, it was not until much later that I published anything in this area.

Early in the summer of 1972, thanks to a brief visit to Sussex by Bob Richardson, I heard about the Cornell nuclear magnetic resonance (NMR) experiments on liquid ^{3}He below 3 mK, and was immediately so intrigued that I postponed my projected foray into the foundations of physics to try to understand what they were telling us. The subsequent story is one I have told in Reference 1, so I will not repeat it here, but I will just make one remark: In retrospect, I think I was incredibly lucky in that at the point when I heard about these experiments I not only knew what I needed to know to think usefully about them (the experimental properties of ^{3}He at higher temperatures, and their interpretation in terms of Landau's Fermi-liquid theory) but, equally important, I did not know what I needed not to know, namely the standard theory of NMR; had I known this, it would almost certainly have sent me off in completely the wrong direction (as it did several other people who took a shot at the problem). Sometimes what one does not know is as important as what one does!

During my 15 years at Sussex, I did a fair amount of international traveling: Apart from a second year in Japan (this time in Tokyo, where my new wife, Haruko, and I stayed with her parents and I worked at Tokyo University in the group of Professor Yasushi Wada), I spent various periods in Germany and Finland in addition to attending a couple of the Karpacz winter schools in Poland, which was a valuable opportunity to meet, among others, some of my Soviet colleagues. But my most unusual stay abroad was in Ghana, where thanks to an exchange arrangement between Sussex and the University of Science and Technology in Kumasi I spent the autumn terms (fall semesters) of 1976 and 1977. This was an interesting and slightly surreal experience: I alternated between sitting in my office, doing much what I would have been doing at Sussex, and going out and beating the bushes for snakes or bargaining for vegetables with the market women at the Central Market. During my second visit, I volunteered to supervise the first-year labs (to the initial horror of my colleagues, who protested, "But you're a theorist!"), and in the course of doing so came to realize how much of what someone who has grown up in an industrialized country is apt to think of as just

[4]In the recent past I belonged to an organization called (not by any choice of mine) the "Center for Emergent Superconductivity." I sometimes wonder what <u>non</u>emergent superconductivity would be like!

mechanical common sense is actually the result of having grown up with various types of simple machines—an experience most of my students, who typically came from the surrounding villages, simply did not have. I also came to appreciate the tremendous handicaps under which my local colleagues were working and to respect enormously those who, while they could have taken jobs abroad, chose to stay and work in their native Ghana.

Throughout most of the 1970s I worked on the new (superfluid) phases of liquid ^{3}He, where there was plenty to do. One question that particularly intrigued me was the following: In my work on the NMR anomalies, I had argued that the electromagnetic dipole interaction between nuclear spins, which was tiny ($\sim 10^{-7}$ K) even at the distance of closest approach of two ^{3}He atoms, could have its effect enormously amplified by the onset of (quasi-)Bose condensation of the Cooper pairs and thus effectively manifest itself at the macroscopic level. The question then arose: Could the same mechanism similarly amplify other ultraweak effects? One possible such effect is the magnetic moment associated with the rotation of a homonuclear diatomic molecule because of the small polarization of the electrons toward the center of mass of the two nuclei. This is a very tiny effect (12) even in a molecule such as $^2C_{12}$; for a pair of ^{3}He atoms, which do not even form a bound state in free space, it would seem to be totally negligible. But the Cooper pairs that form in the superfluid A phase of the liquid are bound, albeit with a very large radius, and more crucially, the quasi-Bose condensation of the pairs can amplify the effect enormously. So, I tentatively predicted a "true" magnetic moment (not just an angular momentum) of the liquid that corresponded to a field of the order of a few gauss, and an experiment by Paulson & Wheatley (13) found effects consistent with this prediction; more recently, the beautiful experiments of Ikegami et al. (14) on deflection of electrons trapped on bubbles in liquid ^{3}He give rather stronger evidence for it. A similar but even more speculative proposal (15) was that it might be possible to display, at the macroscopic level, a violation of parity due to the effects of the neutral-current (and T-conserving) part of the weak interaction beloved of particle physicists (effects that, incidentally, had at the time not yet been seen in accelerator experiments); the idea was that whereas for a single elementary particle the Wigner–Eckart theorem tells us that any electric dipole moment (EDM) must be proportional to the total angular momentum J, which requires violation of time reversal T as well as spatial inversion P, the Cooper pairs in the superfluid B phase of liquid ^{3}He are characterized by a vector $\mathbf{L} \times \mathbf{S}$, which is odd under P but even under T, so symmetry allows an interaction that has this property to produce an EDM along this vector. Again, for a single pair the effect would be laughably small, but just as with the magnetic moment it can be enormously amplified. To estimate even a plausible order of magnitude of the effect I had to get into some somewhat esoteric chemical physics; my final estimate (about 10^{-12} e cm^{-2}) was just beyond what my experimental colleagues thought at the time that they could hope to measure, but I remain hopeful that with improved sensitivity the experiment may someday be feasible.

Toward the end of the decade, I began to get back to my interests in the foundations of quantum mechanics (QM) and specifically to the quantum measurement (or as I would prefer to call it, "realization") problem, as presented in Schroedinger's famous "Cat" paper. John Bell had, as usual, put the problem pithily: How does an "and" get converted into an "or"? In other words, if we believe that the quantum formalism applies in principle to the physical world at every level, how do the quantum mechanical superpositions that it seems necessary to postulate at the level of electrons and atoms get converted into apparently definite outcomes at the level of counters and cats? I could not convince myself that any of the resolutions being offered to this problem was satisfactory, and so gradually I became convinced that there was a real possibility that the premise might be wrong, i.e., that perhaps the formalism of QM could not be extrapolated in the way necessary to generate the Cat paradox. Then the question raised itself: Was there any possibility of examining this question experimentally? A positive answer would certainly fly in the face of the established wisdom, which held that by the time you get to the level of cats and counters decoherence will have killed any effects of QM superposition stone dead, so that the experimental predictions of the quantum description would be indistinguishable from those of a classical mixture, i.e., of a description in which each individual cat (etc.) of the ensemble is either alive or dead but we don't know which. So it would be necessary to find a system in which the effects of decoherence could be minimized by sufficiently careful engineering, and partly thanks to my interactions with a younger Sussex colleague, Terry Clark,[5] who had a lot of experience with what in those days were called rf SQUIDs (superconducting quantum interference device with a single junction) but nowadays go by the more glamorous name of flux qubits, concluded that this system—a simple superconducting ring interrupted by a Josephson junction—was among the most plausible candidates. But I was still skeptical both that QM would still work at the level of SQUIDs and that, even if it did, the decoherence objection could be avoided. Now there had already appeared in the literature the idea that, under the assumption that QM does apply to the dynamics of the flux variable in an rf SQUID (and to the related quantity in a current-biased Josephson junction, the phase drop of the Cooper pairs across the junction), and if we simply describe the system by a conservative Hamiltonian involving only the junction Josephson energy and capacitance and, for the SQUID case, the self-inductance of the ring, it should be possible to see two quintessentially quantum-mechanical effects, namely tunneling of the phase (or flux) out of a metastable potential well (which became known as macroscopic quantum tunneling or MQT) and, in the SQUID case, coherent oscillations between two degenerate potential wells (macroscopic quantum coherence or MQC); back-of-envelope calculations suggested that with realistic values of the parameters, both effects should occur at an observable level at not prohibitively low temperatures (for technical details, see 17). However, a realistic description of either system even in the classical regime requires one to add to the conservative equation of motion a dissipative term proportional to the conductance shunting the junction (thereby generating what is called in the literature the RSJC or resistively shunted junction with capacitance model). Because it seemed very likely that the physical mechanisms that give rise to this term also produce substantial decoherence, I framed the question as, "What is the effect of dissipation on MQT and MQC?" Toward the end of the 1970s I set out, with my then student Amir Caldeira, to make a serious effort to answer this question, starting with the hopefully simpler MQT phenomenon.

The problem of the tunneling of a system out of a metastable well while coupled to a complex environment of course arises in many different areas of physics and chemistry, perhaps most obviously in the theory of biochemical reactions. However, the special context in which Amir and I were interested dictated a rather unconventional approach. At the time we embarked on it, there were no experiments that probed the validity or not of the quantum formalism at the level of SQUIDs, and we regarded this as an open question. It was then clear to us that the situation was rather asymmetrical, in the following sense: Suppose we were to do a QM calculation of the effect of the environment on the rate of MQT that assumed (as do, for example, most recent calculations in a chemical-physics context) some definite Hamiltonian for the system–environment interaction; and suppose for the sake of argument that this calculation were to predict that the

[5] Terry himself, along with his theoretical collaborator Allan Widom, took a different approach that assumed that the naive results would be qualitatively unaffected by even substantial dissipation, and in fact published in 1983 a claim (16) to have observed (in effect) the MQC phenomenon in an rf SQUID at 4 K, a temperature orders of magnitude higher than those that more recent work has found to be necessary to make the same claim. I believed then (and believe to this day) that the raw data presented in Reference 16 and in subsequent papers by the same group probably have an alternative purely classical explanation (and in fact at one point produced a fairly detailed version of such an account, which was, however, never tested). For a more detailed discussion of this and other aspects of the early work on MQT and MQC, see Reference 17.

rate of tunneling, while perhaps suppressed by environmental effects, is still nonzero. If now the experiment is done and sees a nonzero rate, everyone goes away happy. But what if it sees nothing? In that case, given the very strong prejudice in the community in favor of the universal validity of QM, we would be open to the criticism that we had simply written down the wrong Hamiltonian. Indeed, a macroscopic device like an rf SQUID is a horrendously messy object—we know neither its exact chemical composition nor the position of the dislocations, impurities, etc.; it is sitting on a laboratory bench that is vibrating and interacting with the radiation field, the 50-Hz background, and who knows what else.... It seems hopelessly optimistic to think that we could ever write down a Hamiltonian that adequately takes these complications into account.[6]

Very fortunately, it turns out that there is a way around this problem, at least so long as following Feynman & Vernon (18) we are prepared to model the "environment" (i.e., whatever is causing the dissipation) as a set of simple harmonic oscillators coupled linearly to the system. Within this framework it turns out, serendipitously, that the (unknown) combination of energy levels and matrix elements that causes decoherence in the quantum dynamics is precisely that which leads to dissipation at the classical level, so that it is possible to express the decoherence uniquely in terms of the classical dissipation, which for any given system is an experimentally measurable quantity. In this way, Amir and I were able to predict that the Wentzel–Kramers–Brillouin (WKB) exponent in the MQT rate for a mechanical system would be increased by an additive factor of order $\eta \cdot (\Delta q)^2/\hbar$, where η is the (experimentally measured) friction coefficient and Δq is the width of the barrier. This result, when transcribed to a current-biased Josephson junction (where the analog of η is the conductance shunting the junction) was a great relief to us, because it meant that for experimentally reasonable values of the shunting conductance any failure to see MQT could not be blamed on decoherence resulting from the dissipative term in the RJSC model but must suggest a genuine breakdown of QM. In the end, our quantum-mechanical prediction [which was soon afterward confirmed by a more microscopic calculation by Eckern and colleagues (19, 20)] was experimentally verified a few years later in a beautiful set of experiments by Devoret et al. (21); our general technique, of relating decoherence at the quantum level to dissipation at the classical one, seems to have been found quite useful in recent discussions of the role of SQUIDs as flux qubits (see, e.g., 22).

But how good is in fact the Feynman–Vernon oscillator-bath model?[7] Although we had tried to make a start on this question in an appendix to our long paper (23), following the verification (24) of our predictions based on the model the steam seems rather to have gone out of the discussion, and over the past 40 years there have been only a handful of papers that return to it, most notably that of Prokof'ev & Stamp (25), which asserts that for certain kinds of problems involving spins it is qualitatively misleading. My own intuitive feeling is that to the extent that any one degree of freedom of the environment is only weakly perturbed (a condition that often fails to be satisfied in the kind of example discussed in Reference 25), it should be a good approximation; the principle is the same one that allowed nineteenth-century spectroscopists, who had no access to lasers, to get away with an oscillator model of the atom for so long (as long as one uses only the light sources then available, any one atom is only weakly perturbed even though the light beam may be totally absorbed). But I feel that the question could use further discussion.

In early 1982, while on my first visit to China, I received an invitation to take up the John D. and Catherine T. MacArthur Chair in the physics department of UIUC. I was of course familiar with the department from my postdoc year in 1964–65 and various short visits later, so the prospect

[6]These considerations apply to just about any experiment on a condensed matter system, and I once heard a colleague from a different area of physics proclaim that he would never accept any purported evidence for the breakdown of QM that relied on experiments on such a system.
[7]For which Amir and I not infrequently get undeserved credit in the literature.

was very attractive, and at the same time the morale in the British university system, following the massive cuts imposed by the Thatcher administration in the early 1980s, was at an all-time low; so after some initial hesitation, I accepted the offer, and after a previously arranged eight-month stay at Cornell moved to Urbana with my wife and four-year-old daughter in the late summer of 1983. Despite my continuing affection for Sussex, that is certainly one decision I have never regretted. One major difference between my conditions of work at Sussex and at UIUC was that on making the transition my formal teaching obligations, in terms of contact hours, decreased by a factor of eight; it would be nice to believe that my research output went up accordingly, but I doubt if that is the case—there does not seem to be a conservation law in these matters!

One thing that stayed constant across the Sussex–UIUC move was the range of courses I taught. I have always believed that rather than concentrating on one's research specialty, it makes more sense to volunteer to teach topics with which one has no previous familiarity, as one then sees more clearly the difficulties students are having in absorbing the material. Thus at Sussex, I covered just about all the material in the undergraduate course, with the exception of practical electronics and, oddly, basic quantum mechanics (though I did teach the latter in Ghana). At UIUC, apart from an interdisciplinary undergraduate "physics-for-philosophers" course, my teaching has all been at the graduate level, but over the past 35 years I have volunteered to teach, for example, general relativity and cosmology, fluid mechanics, Lie groups, and other topics far removed from my personal research interests. This breadth of experience came in very useful when I was invited, and accepted, to write a book titled *The Problems of Physics* (26) in the Oxford OPUS series; it was a stimulating challenge to try to put across some of the central ideas of various frontier areas of physics without using any equations at all. I cannot judge how far I was successful, except to remark that I believe that sales of the book clock in at a few millihawkings.[8]

During my stay at Cornell, and after arrival in Urbana, I continued the line of work I had started at Sussex on the effects of a dissipative environment on the quantum-mechanical behavior of a macroscopic system, but this time investigating how it affected the MQC phenomenon, i.e., the quantum superposition of states that are by some reasonable criterion macroscopically distinct. I collaborated on this initially with Sudip Chakravarty, who was also a visitor at Cornell concurrently with me, and subsequently also with my first UIUC students (Matthew Fisher and Alan Dorsey), my first postdoc (Anupam Garg), and an early visitor (Willi Zwerger). Eventually the six of us produced a lengthy (and I suspect in the view of many of its readers horribly turgid) paper (27) on the spin-boson problem, that is, the problem of a system tunneling between two nearly degenerate position eigenstates while interacting with a harmonic-oscillator environment whose coupling to it is diagonal in this representation. This is a classic problem that had been studied in many different subfields of physics and chemistry,[9] with wildly varying results; one of the main messages of our paper was that there is no "generic" spin-boson behavior; rather everything depends not just quantitatively but qualitatively on the form of the system-environment coupling spectrum [the quantity denoted by us, and often in the subsequent literature by $J(\omega)$]. A second conclusion, which was crucial for the ongoing MQT/MQC program, was that for experimentally reasonable values of temperature and dissipation not only MQT but the more delicate phenomenon of MQC might survive.

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[8]The millihawking, a unit for the sales of popular books on physics that I believe I am not alone in using, equals 10^3 copies.

[9]When we eventually submitted our paper to *Reviews of Modern Physics*, we got back two referee reports, each of which scolded us for our apparent ignorance of the existing literature of the subject, and submitted a list of a dozen or so "essential" references that the referee felt we should cite. We were able to rebut this criticism, to an extent, by pointing out that the two lists contained not a single reference in common!

At about the same time, Anupam and I published a protocol for the MQC experiment (which, remember, at that time was still on the drawing board) showing that if it could be conducted and turned out to give the results predicted by QM for two-time correlations, then this would refute a class of theories about the world that we christened "macrorealistic": Crudely speaking, such theories assert that when a macroscopic object has available to it two or more macroscopically distinct states, it always (apart from short transit times that can be taken into account in the analysis) occupies one or the other of these states, and furthermore that observation of which state it is in, if done with appropriate precautions, does not affect its subsequent behavior. (For a more careful definition of macrorealism, see, e.g., Reference 28). Although the analysis that led us to this result was a rather straightforward adaptation of one already familiar in the context of quantum nonlocality, the provocative aspect was the assertion that it should actually be feasible to do the MQC experiment on an rf SQUID in a regime that is sufficiently close to ideal that the predictions of QM are inconsistent with those of macrorealism. Because of the strong prejudice in the quantum foundations community that it would never be possible to demonstrate characteristically quantum-mechanical effects at the macroscopic level,[10] this assertion made us the target of repeated critical comments over the next few years. Fortunately, our experimental colleagues were more open-minded, and several groups started working toward a meaningful experiment along the lines we had suggested, resulting in the first demonstrations (29, 30) of MQC in rf SQUIDs (by then rechristened flux qubits) at the turn of the century. However, it would not be until 2016 that an experiment along the lines we had suggested (actually using a rather simpler protocol than our original one) was carried out (31) and, to my mind, definitively refuted macrorealism at that level. I find it rather amusing that nowadays the younger generation of experimentalists in the superconducting qubit area blithely writes papers with words like "artificial atom" in their titles, apparently unconscious of how controversial that claim once was.

As an aside, one may ask: Why was the effect of decoherence on macroscopic manifestations of QM so grossly overestimated for so many decades in the professional quantum measurement community? A typical argument (which I call the "electron-on-Sirius" argument—I forget where I first read it) goes something like this: By its very nature, any body that can reasonably be called macroscopic has a myriad of incredibly closely spaced energy levels; thus the (random) perturbation due to a single electron on a distant star is already large compared to the level splitting, and consequently no characteristically quantum-mechanical effects can occur. There are several reasons why this is a bad argument, of which the one I think is most interesting is that though any macroscopic system is strongly coupled to its environment, much of the coupling is what one might call adiabatic. To illustrate this point by a microscopic analogy, let's consider the working of the neutron interferometer: If we consider the correct QM description of the Universe at the time when the neutron is passing the two slits, one sees that because of the interaction of its magnetic moment with the blackbody radiation field, its state must be entangled with the latter (exactly how strongly depends on how we treat the high-energy cutoff and is irrelevant in the present context). Nevertheless, no one seriously believes that this entanglement (formally specified by a reduction of the off-diagonal elements of the neutron reduced density matrix) leads to a loss of visibility at the final screen, because as the two neutron states reconverge, so do the two states of the radiation field with which they are correlated. I believe that similar considerations apply in many experiments on MQC and related effects and are part of the explanation for the spectacular success of the flux-qubit experiments of the past 20 years; this emphasizes the importance of doing concrete

[10]Indeed, I am told that as late as 1999 the leader of one of the groups setting up the MQC experiment was solemnly assured by a very senior and distinguished colleague in that community that he was simply wasting the taxpayers' money.

calculations rather than just using "hand-waving" arguments, and I suspect that this lesson maybe relevant, for example, to current work in quantum biology.

A second aside: Do the flux-qubit experiments (and experiments of a similar nature conducted in other branches of physics such as optomechanics) "probe the quantum-classical interface" as is not infrequently claimed in the abstracts or conclusions of the relevant papers? I am afraid that the situation here is asymmetrical: If in these experiments one were to find evidence for a breakdown of the QM predictions (an outcome I would personally have given even odds on back in 1980), then indeed, irrespective of the nature of the breakdown, this would be a major contribution to the realization problem and indeed would constitute a major revolution. However, if we keep finding (as we have so far) the behavior predicted by the straightforward extrapolation of the QM formalism to this level, I am afraid we will really have learned nothing—the prima facie paradox will remain as acute as ever, in fact if anything more so.

Returning to the 1980s, following my move to UIUC and in parallel with the work on quantum dissipation described above, I continued to work on superfluid ^{3}He and other things. One thing that had puzzled me since the earliest experiments was the comparative ease with which the B phase of superfluid ^{3}He was nucleated from the metastable A phase: Because the Gibbs free energy difference between the two phases is so tiny ($\sim 10^{-8}$ of the energy of each separately!) and the surface tension of the interface between them so huge, any simple Gibbs–Cahn–Hilliard mechanism seemed inevitably to give an A-phase lifetime against this process greater than the age of the Universe, whereas no experimentalist, once he/she was well below the equilibrium transition temperature T_{AB}, ever seemed to have had to wait more than a few minutes for it to occur. After exhausting all the obvious possibilities, I finally came up (32) with a somewhat bizarre conjecture: that the mechanism of the transition was the passage of a cosmic-ray muon through the sample. The mere observation that such a muon (or more accurately the δ-electrons which it produces) could heat a small volume of the A phase across the transition temperature T_c into the normal state is by itself by no means adequate as an explanation: Since the A phase is thermodynamically stable between T_c and T_{AB}, this would not prima facie help, because one would expect that on recooling across T_c the A phase would be re-established and, when further cooled below T_{AB}, remain metastable with respect to the A $\rightarrow$ B transition.[11] The key insight is that because the heating and cooling occurs not over cryogenic timescales (~minutes or even hours) but in a far more rapid "quench," there is a nonzero probability that on crossing T_c a droplet of the unstable B phase nucleates, and if one can "protect" it against nucleation of the A phase until it has cooled below T_{AB}, then it can expand and fill the whole volume. To ensure the necessary protection, I conjectured that because of the unusual behavior of the transport coefficients of normal liquid ^{3}He at low temperatures one might form a "baked-Alaska" configuration, with a cold interior of the droplet surrounded by a higher-temperature shell. This conjecture seemed so outlandish to some of my colleagues that I am told that at least one worried about my mental state; however, a few years later, Doug Osheroff and his team at Stanford produced convincing evidence (33) that B-phase nucleation can indeed be produced both by slow neutrons and by gamma rays (and the numbers are consistent with the hypothesis that in the absence of laboratory radiation sources cosmic-ray muons can indeed induce it). Whether the details of the mechanism are as I originally conjectured remains controversial at this writing (cf. 34). What I find particularly pleasing about this explanation of the B-phase nucleation is that in the absence of an explicit instruction to consider cosmic rays, I doubt that any computer program, however sophisticated, would ever have found it.

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[11]The situation is very different in the case of nucleation of vorticity in rotating liquid ^{4}He, where I believe a cosmic-ray mechanism has also been invoked: In this case the equilibrium state immediately below T_c is the vortex one, so the problem does not arise.

Another piece of work related to the A-B transition, on which I collaborated with my student Sungkit Yip, was on the velocity of propagation of the A-B phase boundary once formed. The general principle seems clear: Because of the smallness of the free-energy difference between the two phases and the relatively deep degree of supercooling, the latent heat cannot heat the liquid back into the higher-temperature (in this case A) phase, so the transition is one of the relatively small number of known first-order phase transitions that are hypercooled[12] (35), and the progress of the interface, which is driven as always by the free-energy difference, is limited not by the usual thermal-diffusion considerations but more directly by the friction resulting from reflection of quasiparticles incident on it. This apparently simple problem actually involves a number of conceptual subtleties, and Sungkit and I batted ideas about it backward and forward for months; originally we did our calculation of the friction due to quasiparticle reflection under the implicit assumption that the latter was of the ordinary specular type, but this produced a value of the terminal velocity that turned out to be only $\sim 10^{-3}$ of the experimental one, and we then realized that this was because the reflection is of the Andreev type and transfers only a tiny fraction of the incident momentum. This gave predictions that were in reasonable agreement with the experiments; I think this is probably the best evidence to date for the smallness of the momentum transfer in Andreev reflection, and while our *Physical Review Letters* (*PRL*) article (36) did not make much of a splash then or later, it remains one of the papers I am most proud of.

In March 1989 much excitement was generated by the claims of Fleischmann & Pons (37) to have produced cold fusion in a table-top experiment with palladium electrochemically soaked with deuterium, and over the next three months many laboratories attempted to reproduce their results, mostly without success. Concurrently, many theoretical papers appeared that tried to find ways in which the solid-state environment could somehow enhance the vacuum fusion rate by the many orders of magnitude necessary to get agreement with the reported Pons–Fleischmann results. I felt intuitively that these claims could not be right, and with my UIUC colleague Gordon Baym succeeded in devising an argument (38) that showed that given a couple of eminently plausible and experimentally testable assumptions the proposed enhancement could not occur (a conclusion that has not prevented such claims being periodically repeated in the literature over the past 30 years). There is a curious coda to this story: In early June 1989, after most of the excitement had died down, I happened to be visiting the Universidad Autonoma in Madrid, where Carlos Sanchez was running a table-top experiment similar to that of Pons and Fleischmann and looking for the neutrons that should be a product of the fusion reaction. He had been seeing increasing bursts of activity in his detectors, and during my visit these culminated in a burst so violent that the university safety authorities temporarily shut the laboratory down. This did not recur, and was never to my knowledge explained; but what intrigued me was that after the power was switched off, the timescale over which the detector activity decayed was just about that which I calculated for the deuterium to diffuse out of the palladium. This coincidence, coupled with the fact that the period March–June 1989 was probably the period of maximum solar activity over the past 40 years, gives me a nagging suspicion that though the original claims of cold fusion were almost certainly invalid, there might nevertheless be something there. . . .[13]

Meanwhile, the topic that was preoccupying much of the condensed matter community was the high-temperature superconductivity (HTS) that had been discovered in the cuprates in late 1986. I was initially reluctant to get drawn into this field, because it seemed to be one in which theoretical speculation was vastly outrunning what could be tested by experiment, but by the early

[12]The best example of a hypercooled first-order transition is found not in real life but in fiction (normal $H_2O \rightarrow$ ice-IX; see Reference 35).

[13]I am not alone in this suspicion: Research on phenomena related to cold fusion (now rechristened low-energy nuclear reactions) continues to roll on, with more than 1,000 papers since 1989.

1990s I could no longer resist, particularly after I was coopted into the Science and Technology Center for Superconductivity, which had been set up at UIUC in 1989. My colleague David Pines was very active in this area and was an enthusiastic advocate of a theory in which exchange of spin fluctuations played the same role as phonons in the BCS theory in generating an effective electron–electron attraction. What I found particularly striking about this theory was that it made a confident assertion concerning the symmetry of the order parameter (Cooper-pair relative wave function), and one different from that of most of the competing approaches; while they mostly favored the same s-wave state as in the classical BCS theory, the spin–fluctuation model definitively predicted that the symmetry should be of the so-called $d_{x^2-y^2}$ type, where the sign of the (real) amplitude reverses on rotation through $\pi/2$. I always like to try to think of experimental ways of answering yes/no questions, and I came up with the idea of preparing, effectively, a dc SQUID with the Josephson junctions on mutually perpendicular faces of a cuprate superconductor[14]; I discussed this with my UIUC colleague Dale Van Harlingen, and after some highly nontrivial preparatory work he and his team succeeded in doing the experiment and showing that the symmetry is in fact $d_{x^2-y^2}$; since then, many similar experiments both at UIUC and by other groups have confirmed this result, and it is now accepted by the overwhelming majority of the community. Whether or not it is eventually accepted as "the" theory of HTS in the cuprates, the spin fluctuation theory can claim to be near-unique in having made a correct and nontrivial prediction about their behavior in advance of the experiment!

In 1984, motivated by what seemed to us a particularly foolish paper on Bell's theorem that had appeared in *PRL*, Anupam and I had written an indignant letter to the then editor of *PRL* admonishing him to apply the same standards to manuscripts in the area of quantum foundations as those used in other areas of physics. The result (which in retrospect I should have anticipated!) was that I was asked, and agreed, to become the first divisional associate editor (DAE) of *PRL* for the newly created division of quantum foundations, a post which I held until 1996. This was a valuable experience: Unlike (I suspect) in other divisions, disagreements between referees and authors in this area tend not to be about technical correctness but about the importance and significance of the content of the manuscript, something on which opinions can fluctuate wildly and the editor often must make a judgement call. Additionally, it developed in me very good discipline to have to identify myself to the author when making possibly critical comments on his/her work; nowadays when I act as an anonymous journal referee, I always try to pretend when composing my comments that I have to append my signature to them. One episode from my time as DAE particularly stands out in my memory: Sometime in the early 1990s a group of authors who had submitted a not particularly out-of-the-ordinary-seeming manuscript about Bell's theorem and related issues subsequently wrote in some embarrassment to say that they might have to withdraw it because their superiors (at what turned out to be a military-research installation) wanted to classify it. I still remember my sense of shock at the thought that Bell's theorem, which I had always thought of as the purest of pure physics, could possibly have any military relevance. Of course, in retrospect what I was seeing was the first stirrings of the quantum-information revolution that was to sweep through physics at the end of the twentieth century—certainly this was one of the most profound developments in my time, though one in which I did not really participate directly.

Following the realization of Bose condensation in the ultracold alkali gases in the mid-1990s, I decided to try to do some research in that field; despite (or perhaps because of) the fact that this

[14]I was not consciously aware of it at the time, but a similar proposal to detect p-wave pairing had been made by Geshkenbein, Larkin & Barone (39) several years earlier, and partly as a result at least two other groups came up with the idea simultaneously and independently. The d-wave case is actually somewhat simpler than the p-wave one, in the sense that in the former one does not have to work particularly hard to justify a reproducible lowest-order Josephson coupling.

is one area of physics where "Nature is doing exactly what the textbooks tell her to" and it thus does not have quite the excitement of (say) cuprate superconductivity, I felt that it would be a rich source of interesting thesis problems for my graduate students, and indeed it has turned out to be so; over the first decade of the twenty-first century, I graduated half a dozen students in that area with substantial theses.

Over the past ten years or so there have been two problems, coming in some sense from opposite ends of condensed matter physics, that have particularly obsessed me. One stems from the observation that the experimental properties of glasses (that is, solid noncrystalline materials) are far more universal, particularly below 1 K, than those of crystals, but that despite this we do not have a generic understanding of this class of materials that is anywhere near comparable with that of the latter. My instinct tells me that sometime in the next few decades we will acquire one, but I don't know how to get there. . . . The second problem concerns the attempt to realize topologically protected quantum computing in so-called $p+ip$ Fermi superfluids such as, putatively, strontium ruthenate or some variety of superconductor–semiconductor hybrid system, which is currently soaking up not only much effort but much funding. Almost without exception, theoretical work on this project has used, explicitly or implicitly, the concept of spontaneously broken U(1) symmetry; it is a concept that, despite its popularity, I believe to be fundamentally flawed. My question is simply: Will a theory that adequately respects fermion number conservation, when properly implemented, just give trivial corrections to the established wisdom, or will it indicate a need for a radically new approach? If I could either myself answer or hear from others the answer to even one of the above questions, let alone both of them, I would feel it would make a nice capstone to my career.

Over (parts of) the past 15 years, in addition to my regular UIUC employment, I have held summer appointments at the Institute for Quantum Computing (IQC) at the University of Waterloo, Ontario, at the National University of Singapore, and the University of Tokyo, and most recently, at Shanghai Jiaotong University, where I am director of the Shanghai Center for Complex Physics (SCCP). The IQC was ahead of its time in anticipating the explosive growth of the field quantum information; I very much enjoyed my stays there, although I sometimes got the feeling that my enthusiasms (which were and are more in the field of quantum foundations, and date from my entry into the field, which is now more than half-way back in time to the genesis of quantum mechanics) seemed outdated and rather quaint to the younger generation. In my capacity as director of the SCCP, I have been able to witness the astounding development over the past 30 years of both Chinese physics and Chinese society and also appreciate some of the problems to which this rapid development has inevitably given rise, for example, in the context of academic evaluation. In addition to these long-term appointments, I have been able to continue my world travels, including not only several visits to India (where my colleagues were kind enough to organize, recently, an extra 80th birthday symposium for me) but also some possibly less "standard" destinations such as Malaysia and Iran. We peripatetically inclined physicists, particularly the theorists among us, are indeed fortunate in this respect. . . .

Indeed, when I look back on what will be, by the time this article is published, a 60-year career in physics, I think I have been fortunate in many ways. I have had a marvelous constellation of graduate students and postdocs, from all corners of the globe, some of whom traveled long distances to attend my 80th birthday celebrations in March 2018, and I have had a remarkably supportive group of colleagues throughout my whole career (not to speak of sterling support from my family). But if I had to pick out one thing that made all the difference, particularly in the early stages, it would be the tolerant and relaxed environment that I experienced at Sussex when starting there in the late 1960s. When I recall this and then look around at the current environment for people at the postgraduate, postdoc or junior faculty level, I feel quite concerned: From conversations with this group not only in China (where the problem may be most obviously manifest) but even in North America, I get the impression that many of them feel that there will be no hope of obtaining the kind of postdoctoral/faculty/tenured position that is the natural next stage in their careers unless they have not only published three or four papers but published them in high-impact journals like *Nature*, *Science* or *PNAS*. Though I never cease to marvel at the ability of my young colleagues to survive, and even perform creditably, under this pressure, I fear that one almost ineluctable outcome is that there is a strong temptation to focus all one's energy on problems that can be reasonably guaranteed to yield results within the relevant time frame, typically two or three years. And almost by definition, these are not the really worthwhile problems! This situation is, of course, not an accident but one of the unfortunate results of what is in itself a good development, namely the partial leveling of the academic playing field that has resulted, inter alia, from the information revolution and led to the gross imbalance that I note earlier between jobs and aspirants, and I don't really know how one can deal with it; the best advice I can give to any younger colleagues who seek my opinion is deliberately to put aside some fraction (30%, 25%, even 20%) of their research time for problems that they not only are not sure they can solve within the two- or three-year deadline but are not even sure that they (or anyone) can solve at all. Apart from that I can only wish them well. . . .

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