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A Career in Physics

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Abstract

Over the course of my career, I have had the opportunity to work on a wide variety of problems in condensed matter physics, benefiting from superb collaborators and environments full of inspiring colleagues. I review here some highlights of my journey so far. Subjects include theories of dynamic critical phenomena, phase transitions in two-dimensional systems, systems with strong disorder, quantum physics of mesoscopic systems, one-dimensional quantum systems, and the quantum Hall effects.

1. BEGINNINGS

There are many ways of doing physics, and there are many paths to becoming a physicist. My particular path was not unusual. As a child, I developed an early interest in math and science. I enjoyed solving math problems, and I had a great interest in understanding how things worked. My interests were somewhat more theoretical than experimental; I was not particularly a tinkerer, but I did like to read about science.

My interest in science and math was certainly influenced by my home life. My father had done some graduate work in mathematics during the 1930s, and had hoped to pursue an academic career in the field, but he had been forced by economic realities of the Great Depression to abandon this goal in favor of a civil service job with the US Customs Service. Throughout his life, however, he maintained an interest in math; he loved to solve problems and he very much enjoyed tutoring the children of friends and relatives in all levels of the subject.

My schooling, prior to college, took place in the Brooklyn, New York, public school system. Although I attended a magnet school in fifth and sixth grades, I did not attend one of the famous New York selective high schools. Nevertheless, I benefited from the stimulation and encouragement of several outstanding teachers, and I was able to enter Harvard College, in 1958, with sophomore standing. After receiving my undergraduate degree in 1961, I stayed at Harvard for a fourth year as a graduate student, before transferring to the University of California, Berkeley, for my Ph.D. studies.

By the end of my first year at Harvard, I knew that I wanted to study theoretical physics. However, my fascination with condensed matter physics did not actually develop at Harvard but rather at Los Alamos National Laboratory, where I was an intern during the summer of 1961. I knew nothing about solid state physics at the time, but I was assigned to work with an experimental group that was using inelastic neutron scattering to measure the phonon spectrum of aluminum. As preparation, I was handed a copy of Brillouin's book on phonons and electrons in periodic structures, and I was struck by the beauty of the subject.

My thesis advisor at Berkeley was John Hopfield. The first project he gave me was to look for an explanation for the empirical observation, known as Urbach's rule, that in a wide variety of materials, including alkali halides, the optical absorption coefficient falls off exponentially as a function of frequency below the fundamental absorption edge, where the lowest energy exciton can be produced. In the model we considered, the optical absorption would be proportional to the spectral density for a tightly bound exciton interacting with a bath of thermally excited phonons. For the regime in which we were interested, I concluded that the phonon energies could be neglected, and the problem could be reduced to a calculation of the spectral density of a particle interacting with a static Gaussian random potential. When I got stuck on this, Hopfield suggested that if I could not solve the problem in three dimensions, perhaps I could solve it in one dimension. I think that the one-dimensional problem was actually more difficult than he suspected, but I eventually succeeded in solving it, finding an exact solution for the spectral density, at arbitrary energy and momentum. This led to my first published paper and gave me confidence that I could succeed in research (1).

Aided by insights gained from my one-dimensional solution, I returned to three dimensions, but now focused on just the low-energy tail of the spectral density. During the summer of 1964, I was an intern at Bell Telephone Laboratories, where I found that Melvin Lax had been working on the same problem. Each of us had partially solved the problem, from different points of view. Combining forces, we were able to obtain a fairly complete solution, which eventually led to a series of joint papers and the second half of my Ph.D. thesis (2, 3).

As it turned out, Hopfield was not a coauthor on any of my papers. I solved the one-dimensional problem largely on my own, and my work on the low-energy tail was done more closely with Lax than with him. Nevertheless, Hopfield's input was very important, as he suggested the problems to me and pointed me to the literature, and he helped me over several obstacles in early stages of the work. He certainly stimulated my interest in physics and he set a high intellectual standard, which I have held as a model.

Hopfield moved from Berkeley to Princeton in the fall of 1964. I moved with him, and I spent the academic year from 1964–65 as a visiting graduate student at Princeton while completing the work for my Berkeley degree. Following that, I spent a year as a postdoctoral fellow in the group of Philippe Nozières at the École Normale Supérieure in Paris. During that time, I met for the first time a variety of European physicists, and I learned a good deal from them. I also had some very stimulating interactions with Paul Martin, who was in Paris for a semester, on sabbatical from Harvard. The year in Paris was also an opportunity for me to travel to other parts of Europe, to see the great tourist sights as well as to visit laboratories.

2. BELL LABORATORIES

In 1966, I returned to Bell Laboratories as a member of the technical staff. Bell Labs was a very exciting place at the time, and I benefited from collaborations with a number of brilliant young people as well as from interactions with outstanding senior theorists including Conyers Herring and Phil Anderson, as well as Lax. My most important collaboration was with Pierre Hohenberg, with whom I coauthored some 16 papers over a 12-year period, primarily related to the dynamic behavior of systems at, or close to, a critical-point phase transition. Other colleagues with whom I collaborated on one or more projects included theorists Maurice (T. M.) Rice, Chandra Varma, and Dean McCumber and experimenters Denis McWhan, Stan Barker, and Brage Golding. Many other theorists and experimenters contributed to the vibrant atmosphere, including Gunter Ahlers, Phil Platzman, Don Hamann, and Patrick Lee, and they provided inspiration for my work in many cases. In addition, there was a constant stream of visitors, which led to several lasting collaborations.

2.1. Dynamic Critical Phenomena

The first of my joint publications with Hohenberg was a 1967 *Physical Review Letters* article, in which we proposed a generalization of scaling laws to dynamic critical phenomena in a variety of systems (4). Later papers, together with Shang-keng Ma and Eric Siggia, applied the newly developed renormalization-group methods to compute dynamic properties and delineated a set of universality classes for dynamic critical phenomena, determined by symmetries and conservation laws (5, 6). Our understanding of the subject was summarized in 1977 in a *Reviews of Modern Physics* article (7), largely written by Hohenberg, which has apparently become a basic reference for the field. In a precursor to this work, we also developed a hydrodynamic theory of spin waves at finite temperatures away from the critical point for magnets with O(2) or O(3) symmetry (8).

The object of all this effort was to understand behavior near a critical point at finite temperature. Because the singular behavior in such systems results from large fluctuations at frequencies ω with $\hbar\omega$ small compared to the temperature T, the problem is essentially governed by classical statistical mechanics, even if quantum mechanics may be important on the microscopic scale. In classical mechanics, the critical behavior of static phenomena, such as equal-time correlation functions and static response functions, can be understood without knowledge of the equations of motion. However, two models, such as the classical Heisenberg ferromagnet and antiferromagnet, that have equivalent energy functions and static properties can have very different dynamics. Thus, there can be several dynamic universality classes for a given static class. In contrast to our work, in the case of quantum critical points occurring at $T = 0$, which have been the focus of study in more recent years, static and dynamic properties are fundamentally linked and must be studied together (9).

2.2. Fermi Surface Instabilities and the Excitonic Insulator State

Discussions with Maurice Rice led to collaborations on several problems related to Fermi surface singularities and instabilities. The most important of these was an analysis of the so-called excitonic insulator state, which had been proposed to form at low temperatures in a semiconductor or semimetal with a very small energy gap and was loosely described as a Bose condensate of excitons. Our focus was in exploring the possible manifestations of broken crystal symmetry that should arise in such a state (10, 11). Other joint work, together with Barker and McWhan, concerned antiferromagnetism in chromium and its alloys.

2.3. Two-Level Systems in Glasses

In 1972, I collaborated with Phil Anderson and Chandra Varma on a paper arguing that the anomalous thermal conductivity, as well as the excess specific heat that had been observed in glasses at low temperatures, by Robert Pohl and others, could be explained by a statistical distribution of two-level systems associated with localized defects (12). An example of this might be an atom in a double-well potential, where the energy splitting and coupling to phonons would be determined by quantum tunneling through the barrier as well as asymmetry in the double well. It is now widely accepted that two-level defects are present in many disordered systems and are responsible for many anomalous phenomena in disordered systems at low temperatures. However, the precise nature of the two-level systems remains a matter of debate.

3. TWO-DIMENSIONAL MELTING

In 1976, I left Bell Laboratories to assume a faculty position at Harvard University. Soon after moving to Harvard, I developed a close collaboration with David R. Nelson on problems related to the theory of phase transitions in two-dimensional systems. It was David who first attracted my attention to the problem of melting in two dimensions. Kosterlitz and Thouless (13, 14) had earlier proposed that melting could occur by a mechanism similar to their model of the superfluid-to-normal transition, with dislocations in the solid taking the place of vortices in the superfluid. However, the interaction between dislocations has, in addition to a logarithmic term in the separation, an important anisotropic component, which is not present for the vortices. We found that this anisotropy engenders a modification of the formula for the divergence of the correlation length at the transition point, a fact that was noted independently by A. Peter Young (15). Nelson and I also observed, however, that though a small number of unbound dislocations would be sufficient to destroy the quasi-long-range translational order of a two-dimensional crystal, it would not destroy completely the orientational order (16). We therefore proposed the existence of a new phase, which we termed hexatic, with quasi-long-range six-fold orientational order. Specifically, if one defines a local order parameter $\psi(\mathbf{r}) = e^{6i\theta}$, where θ is the orientation of the bond between nearest neighboring atoms closest to the point $\mathbf{r}$, then the correlation function $\langle \psi^*(\mathbf{r})\psi(\mathbf{r}')\rangle$ should fall off as a power law, $\propto |\mathbf{r} - \mathbf{r}'|^{-\eta}$, at large separations. The exponent η would be vanishingly small at a temperature just above the transition from the crystal to the hexatic phase and would increase with increasing temperature to a maximum value of $1/4$, at which point there should be a transition of the Kosterlitz–Thouless type caused by unbinding of disclinations, which interact logarithmically in the hexatic phase. Above this second transition, the system would behave like an isotropic fluid, with exponential falloff of both orientational and translational order.

Of course, the two-stage melting scenario with a hexatic intermediate phase, predicted by a renormalization-group analysis, could be preempted by a direct first-order transition between the crystal and isotropic liquid phases. Numerical simulations and experiments with colloids suggest that both possibilities can occur, depending on microscopic details, but the hexatic phase does exist, in suitable cases, in a narrow range of density or temperature between the solid and liquid phases (17, 18). Also, as noted by Robert Birgeneau and David Litster, a hexatic phase with true long-range orientational order can occur in three-dimensional smectic liquid crystals (19, 20).

My collaboration with Nelson included research on dynamic properties of various two-dimensional systems. In addition to properties of two-dimensional solids and liquid crystals, we were interested in thin films of superfluids and superconductors (21).

4. QUANTUM HALL EFFECTS

4.1. The Integer Quantized Hall Effect

Beginning in 1981, a major part of my research has focused on quantum Hall effects, a large set of remarkable phenomena that have been observed in two-dimensional electron systems at low temperatures in strong magnetic fields. My interest in the subject, and indeed my awareness of it, was engendered by a telephone call I received from Gloria Lubkin, who was then an editor at *Physics Today*. She had heard about a theory of Bob Laughlin's, who was then at Bell Laboratories, purporting to explain some curious experimental results that had been published the previous year by Klaus von Klitzing, Gerhard Dorda, and Michael Pepper (22). The experimental paper reported the discovery of what is now called the integer quantized Hall effect, for which von Klitzing was later awarded the Nobel Prize. Specifically, he found intervals of magnetic field in which the inverse of the Hall resistance of his sample was constant and equal to an integer multiple of the quantity e^2/h, within an experimental uncertainty of the order of one part in 10^6. I had to confess to Gloria that I was not only ignorant of Laughlin's explanation but also unaware of the experimental paper. However, my interest was aroused.

When I had the opportunity to examine Laughlin's argument (23), I understood that it was correct, and I considered it to be a brilliant contribution. However, it raised several puzzling questions in my mind. I realized that Laughlin's explanation only made sense if there were one-dimensional electron states at the edge of the sample, which must remain extended even in the presence of arbitrary disorder along the edge. This seemed surprising, because it was widely believed that in a one-dimensional system, quantum states would necessarily be localized by even a small amount of disorder. Upon deeper reflection, however, I realized that the quantum Hall edge states were special, because they only carry current in one direction at a given edge, and they would therefore be protected from localization by current conservation. In my paper about this, I also observed that for the case of noninteracting electrons, one could have a quantized Hall current even in the absence of an actual electric field: If there was a difference in the electron population at the two edges of a ribbon-like sample, there would be a net current I along the sample that would be equal to ne^2V/h, where n is the number of bands at each edge and eV is the difference in electrochemical potentials between the two edges. Of course, it is the electrochemical potential difference, not the electrostatic potential, that is measured by an ideal voltmeter (24).

4.2. Fractional Quantized Hall Effects

Laughlin's analysis had established that for a two-dimensional electron system, if there are no delocalized states at the Fermi energy away from the sample edges, then the Hall conductance in the limit of zero temperature must be precisely equal to $\nu e^2/h$, where ν is an integer, possibly zero. The analysis was supposed to apply for interacting as well as noninteracting electrons, as long as states close to the Fermi energy could be derived adiabatically from noninteracting states, in analogy to Landau's description of a Fermi liquid. The physics community was therefore stunned when, in 1982, Dan Tsui, Horst Stormer, and Art Gossard announced the discovery of new quantized Hall plateaus with $\nu = 1/3$ and $2/3$, in high-quality samples based on GaAs (25).

Needless to say, I was fascinated by this, and, along with many other theorists, I tried very hard to find an explanation, but it was hard to know how to proceed. For noninteracting electrons, the energy levels in a magnetic field are quantized in discrete Landau levels, each having a large number of degenerate states. The plateau at $\nu = 1/3$, for example, occurs when the electron density is such that the lowest Landau level of electrons is 1/3 full. Thus, the many-body ground state would be highly degenerate in the absence of interactions. Consequently, if one wants to treat the electron–electron interaction using standard perturbation theory, one must first diagonalize the Hamiltonian within a very large Hilbert space, which is in principle a formidable task. Based on the Hartree–Fock approximation, it had previously been expected that the ground state in a partially filled Landau level would be some kind of Wigner crystal with broken translational symmetry. However, a Wigner crystal would not be tied to any particular filling factor and could not explain the observed Hall plateau.

It was understood that if, for some reason, the ground-state energy of a system of interacting electrons in the lowest Landau level in a fixed magnetic field had a discontinuity in its derivative with respect to electron density at filling factor equal to 1/3, this would lead to a quantized Hall state with $\nu = 1/3$. In a pure system, if the chemical potential were fixed, rather than the density, the system would exhibit a quantized Hall plateau over a range of magnetic fields, because the density would adjust to keep the state at filling fraction 1/3 over that range. In a system with some disorder present, localized states at the Fermi level would act as a reservoir, which would lead to a plateau in the Hall conductance, even at fixed density. I tried to find an explanation for such stability based on the ideas of a Wigner crystal melted by ring exchange, but I could not come up with anything convincing. Daijiro Yoshioka and Patrick Lee and I then undertook to calculate the ground-state energy, by exact diagonalization of the Hamiltonian, for up to six electrons in a rectangular box with periodic boundary conditions, with filling factors between 1/4 and 1/2 (26). We found that the actual ground-state energy at $\nu = 1/3$ was significantly lower than that of the Hartree–Fock Wigner crystal and that the electron pair-correlation function was very different from what one would expect in a Wigner crystal. In addition, we did find evidence of a kink in the ground-state energy at $\nu = 1/3$, but we gained little insight from this calculation about the physical origins of the effect.

Although our results were of sufficient interest to be published in *Physical Review Letters*, they were dwarfed by the brilliant work of Bob Laughlin on this problem, which appeared in the same issue (27). Laughlin introduced a many-electron wave function that satisfied the analyticity requirements for electrons in the lowest Landau level and had the correct density for filling factor $\nu = 1/3$, and was the unique state satisfying these requirements for which the amplitude vanishes as the cube of the separation, whenever two electrons come together. As a result, Laughlin's wave function is the exact ground state in the limit of short-range repulsive interactions, and numerical calculations on finite systems show that it has a very high overlap with the exact ground state for Coulomb interactions. Laughlin also argued that the elementary excitations from his ground state would be quasiparticles with charge $\pm e/3$ and that there would be an energy gap for deviations from $\nu = 1/3$, as required for a quantized Hall state.

In parallel with the development of Laughlin's theory, new experimental results showed the appearance of quantized Hall states at various additional fractional values of ν, including 2/5 and 3/5. Although Laughlin's wave function for $\nu = 1/3$ could be readily generalized to fractions such as $\nu = 1/5$ and particle–hole conjugates such as $\nu = 2/3$ and $4/5$, it did not provide a ready explanation for states such as $2/5$ and $3/5$. During the spring semester of 1983, I was on sabbatical at the Institute for Theoretical Physics at the University of California, Santa Barbara, and I worked hard on this problem. The result was a paper that was presented at a meeting of the European Physical Society and published in the conference proceedings in *Helvetica Physica Acta* (28). The paper explored a variety of generalizations of Laughlin's wave functions, which I argued might be good representations of the ground state at certain filling fractions. Among other possibilities, I considered states in which the partially filled Landau level contained electrons in both spin states, which I argued might have lower energy than a fully spin-polarized state at certain fractions, given the fact that g-factor for electrons in GaAs is anomalously small. Therefore, the cost in Zeeman energy for partial polarization might be outweighed by a gain in correlation energy in these cases. (Indeed, experiments have since shown that quantized Hall states of different polarization are possible at certain fractions, and phase transitions have been observed between states with different polarization.) I also argued that even-denominator fractional quantized Hall plateaus should be possible in principle, and I proposed trial wave functions for some even-denominator states, but I was not able to suggest any realistic situation in which this might occur.

In addition to proposals for microscopic electron wave functions to model ground states and charged excitations at various filling fractions, theorists made various attempts to construct odd-denominator fractions by a hierarchical procedure. In a hierarchy, states at one level would be described in terms of effective wave functions for the fractionally charged quasiparticles at the previous level. For example, one might try to construct the $\nu = 2/5$ state by adding an appropriate number of $e/3$ quasiparticles to the $\nu = 1/3$ state. I realized that if one tried to describe the quasiparticles by an effective wave function of the Laughlin analytic form, which would hopefully minimize their interaction energy, one could obtain the correct filling factor only if one assumed the quasiparticles obeyed fractional statistics. This means that one would have to employ a multivalued effective wave function, such that on interchanging the position of two quasiparticles, the wave function would be multiplied by a phase factor $e^{i\theta}$, where θ is a generally noninteger rational multiple of π, whose value would depend on the direction of the interchange and on the number of other quasiparticles enclosed by the path. I presented these ideas in a *Physical Review Letters* article that appeared in 1984, along with an explicit construction of microscopic trial wave functions for the allowed quantum states of a collection of negatively charged quasiparticles at $\nu = 1/3$, which realized the predicted fractional statistics (29). The idea that charged excitations in a fractional quantized Hall state should exhibit fractional statistics was also developed, independently, by Dan Arovas, Bob Schrieffer, and Frank Wilczek (30), who computed explicitly the Berry phase accumulated by the microscopic wave function for a Laughlin state with a pair of quasiholes (positively charged quasiparticles) as one quasihole is moved adiabatically around the other. Although the notion of fractional statistics in two dimensions had been proposed earlier as an abstract idea, the demonstration that this phenomena could be realized in an actual physical system was then a new development. More recently, ideas of fractional statistics, and the generalization to nonabelian statistics, have played a major conceptual role in the effort to characterize the possible states of quantum matter that are topologically distinguishable at $T = 0$.

In my 1984 *Physical Review Letters* article, I argued that by repeating the hierarchical procedure, one could construct, in principle, a quantized Hall state at every odd-denominator filling fraction, with quasiparticles exhibiting fractional charge and fractional statistics in each case. (A similar hierarchy had been proposed by Haldane; 31.) Of course, only a finite number of these could be seen in any real sample, because high-order fractions with small energy gaps would be easily destroyed by residual disorder. Even in an ideal sample without disorder, fractional quantized Hall states very close to an integer filling would be preempted by a state of lower energy in which the extra electrons or holes relative to integer filling would form a Wigner crystal due to their Coulomb interactions. Similarly, quantized states very close to a low-order odd-denominator fraction, such as $\nu = 1/3$, would be preempted by a Wigner crystal of charged quasiparticles stemming from the lower-order fraction. The stability of these Wigner crystals would then determine the width of the Hall plateau at the integer or lower-order fraction.

In the 1984 article, assuming that quasiparticles could be treated as point particles interacting with a simple Coulomb potential, I was able to obtain a curve of energy versus filling factor, which predicted quantized Hall fractions, plateau widths, and energy gaps for an electron system without disorder. It turns out, however, that treating the quasiparticles as point particles is generally a poor approximation, and the relative stabilities of various quantized states predicted by this simple model are typically far from the truth. A much better approach for understanding quantized Hall states, at least in the lowest Landau level, is based on the composite fermion formalism, introduced by Jainendra Jain in 1989 (32). Jain's ideas, or more precisely a further development of these ideas in which the electron problem is transformed into a system of composite fermions interacting with an emergent Chern–Simons gauge field, became the basis for much of my own work in the following decade.

4.3. The Unquantized Quantum Hall Effect

Although even-denominator fractional quantized Hall plateaus were observed in 1987 at $\nu = 5/2$ and $7/2$, corresponding to half-filling of one or the other spin state in the second Landau level, and in a wide quantum well at $\nu = 1/2$, no anomaly has ever been found in the electron transport near $\nu = 1/2$ in the narrow-well structures normally employed in GaAs. Nevertheless, measurements of the propagation of surface acoustic waves by Bob Willett and coworkers in 1990 found an anomaly near $\nu = 1/2$, which suggested that something interesting was actually going on (33). Patrick Lee, Nick Read, and I set to work on this problem and came up with a detailed manuscript, which was submitted to *Physical Review* in June 1992 and was eventually published in March 1993 (34). We noted that at $\nu = 1/2$, the average value of the Chern–Simons magnetic field felt by the composite fermions cancels precisely the external magnetic field, so that the mean-field ground state becomes simply a filled Fermi sea of composite fermions. In order to calculate dynamic properties, such as electrical transport or the response to a surface acoustic wave, however, one must take into account self-consistently fluctuations in the Chern–Simons electric and magnetic fields produced by density variations in the electron density and currents. Doing so, we were able to explain the previously observed propagation anomaly and to further predict that there should be observable oscillations in the behavior slightly away from $\nu = 1/2$. The anomaly at $\nu = 1/2$ reflects the fact that at $\nu = 1/2$, composite fermions in a clean system can travel in straight lines for distances much greater than the cyclotron radius of an electron in the lowest Landau level, which can lead to a nonlocal response to the electric field generated by a short-wavelength surface acoustic wave. Slightly away from $\nu = 1/2$, composite fermions travel in large circular orbits, whose diameter is given by the cyclotron diameter of a particle in an effective magnetic field equal to the difference between the applied field and the value at $\nu = 1/2$. The oscillatory behavior seen in the acoustic wave velocity, as a function of magnetic field, arises from a commensurability condition between this diameter and the wavelength of the surface acoustic wave.

Subsequent experiments, as well as calculations on finite systems, have supported these and a variety of other predictions of the Halperin–Lee–Read (HLR) paper (35, 36). However, there are many questions that the HLR theory cannot properly address. The effective mass for the composite fermions, which sets the overall energy scale for excitations at $\nu = 1/2$ and which determines the energy gaps for fractional quantized Hall states with ν near to $1/2$, is a free parameter in the theory that can only be obtained from a different microscopic calculation or estimated from experimental observations. Furthermore, as noted in the original paper, the HLR theory predicts infrared divergences close to the Fermi surface at $\nu = 1/2$, which may have little effect in practice but whose theoretical implications are only partially understood (37, 38).

The HLR theory has received renewed attention since 2015, when Dam T. Son published an alternate description of the $\nu = 1/2$ state, in which the composite fermions are treated as Dirac fermions rather than nonrelativistic particles, and where the Lagrangian for the gauge field lacks the HLR Chern–Simons term (39). It turns out that the predictions for physically observable quantities are mostly the same in the two theories, but there are a few cases in which there seem to be differences, where a simple calculation using the HLR theory appears to violate a requirement of particle–hole symmetry that should be present in the limit of no mixing between Landau levels. The Son–Dirac theory, which was constructed to be manifestly particle–hole symmetric, gets the correct answer in these cases (40, 41). It remains an open question whether higher-order corrections to the HLR theory can correct these discrepancies.

4.4. Further Developments in Quantum Hall Physics

The wider field of quantum Hall physics has produced a number of surprising experimental and theoretical developments over the past four decades, which have been a repeated source of interest and inspiration for me. In my continuing work in this area, I have benefited from close collaborations with several theorists, including particularly Ady Stern, Rudolf Morf, Steven Simon, Nigel Cooper, and Bernd Rosenow.

Quantum Hall effects lead to some fascinating manifestations in systems with two parallel layers that are close enough so that Coulomb interactions between electrons in different layers are comparable in strength to the interaction between a pair of neighboring electrons in the same layer. For example, coherent states exist in which a current flowing in one layer can produce a quantized Hall voltage in the second layer, which in various cases can be the same or different from the Hall voltage in the first layer. In some cases, one can observe strongly enhanced tunneling at zero bias between the layers, which is analogous in some respects to Josephson tunneling between superconductors. My interest in these problems was first stimulated by a set of beautiful experiments on GaAs systems in the laboratory of Jim Eisenstein at the California Institute of Technology (42), and, more recently, by experiments by groups based in the laboratories of Philip Kim and Cory Dean at Harvard and Columbia, respectively, on Coulomb-coupled layers of graphene separated by the insulator *h*-BN (43, 44).

Many fascinating quantum Hall phenomena are also to be found in a single layer of graphene or a Bernal-stacked bilayer, arising from their peculiar band structures, and these have also attracted my attention. Much of my work here has resulted from collaborations with Amir Yacoby, inspired by experiments in his group (45). In the most recent work, we studied the propagation of spin waves in quantized Hall states of graphene as well as the electrical production of spin waves by contacts biased at voltages larger than the Zeeman energy (46).

When electric current is carried in a quantized Hall state, there is a difference in the electrochemical potential between opposite edges of the device. If the current passes through a narrow constriction, there can be tunneling of charged quasiparticles from one edge of the sample to the other, resulting in dissipation. In small devices with two or more constrictions, there can be quantum interference effects, as a quasiparticle can follow several possible paths from one end of the sample to the other. Such experiments are interesting because in addition to the standard Aharonov–Bohm effect, there should be additional phase factors arising from the fractional statistics of quasiparticles in a fractional quantized state. Even more interesting effects are predicted in the case of special filling fractions, such as $\nu = 5/2$, where quasiparticles are believed to obey nonabelian statistics, reflecting the existence of hidden nonlocal degrees of freedom. In addition, the Coulomb interaction between quasiparticles can lead to oscillations of the interferometer area with varying magnetic field or gate voltage, which can mask the Aharonov–Bohm oscillations. The analysis of phenomena associated with interference and edge-state propagation in quantized Hall systems has remained an important part of my research in recent years (47–49).

5. OTHER INTERESTS

5.1. Cuprates and Two-Dimensional Antiferromagnets

Following the discovery of high-temperature cuprate superconductors in 1986, there was an open debate about whether long-range antiferromagnetic order could exist at zero temperature in a two-dimensional spin-1/2 Heisenberg system or whether it would necessarily be destroyed by quantum fluctuations. It was known from neutron studies that La_2CuO_4, the insulating parent compound of the original cuprate superconductor, showed long-range antiferromagnetic order at low temperatures, but that was presumably due to residual interactions between the copper-oxygen layers or residual spin anisotropy within a layer, so it did not give a direct answer to the question of what would happen at $T = 0$ in an isolated layer with full Heisenberg symmetry. In a 1988 paper with David Nelson and Sudip Chakravarty, we investigated the possibility that as a function of microscopic parameters there could be a zero-temperature quantum phase transition in a two-dimensional Heisenberg system between a state with antiferromagnetic order and a state with no broken spin symmetry, and we asked what this would mean for the behavior at finite temperatures and in the presence of weak interlayer coupling or spin anisotropy (50). The paper introduced the notion of a quantum critical regime at finite temperatures, where scaling laws for static and dynamical properties would be dominated by proximity to the zero-temperature critical point. We also argued that the spin-1/2 Heisenberg model should be on the ordered side of the transition at zero temperature and made some detailed predictions for its behavior at finite temperatures in the presence of interlayer couplings and anisotropy. Dynamic properties were discussed in follow-up papers with Chakravarty and my student Stephan Tyc (51). Subsequent neutron scattering measurements on La_2CuO_4 were in excellent agreement with our predictions (52).

In 1988, Laughlin proposed a theory of high-temperature superconductivity based on formation of quasiparticles with half-fermi statistics, which I found to be very novel and elegant but not necessarily convincing as the correct explanation for the actual systems (53). One feature of the proposal was that it was necessarily accompanied by broken time-reversal symmetry, and it seemed to me likely that this should have consequences that could be tested by experiments. Together with various collaborators, I published several papers on implications of the Laughlin model, including some size estimates for the effects of broken time-reversal symmetry (54, 55). In the end, experiments looking for a large broken time-reversal symmetry in the cuprates produced negative results, and Laughlin's theory fell into disfavor. I have not been actively involved in high-temperature superconductivity since that time.

5.2. Systems with Strong Disorder

In 1971, while I was still at Bell Laboratories, I collaborated with Vinay Ambegaokar and James Langer on an analysis of the low-temperature conductivity in an electron system that is sufficiently disordered that electron states at the Fermi level are all localized (56). It had been argued by N. F.

Mott that the conductivity in such a system should vanish at low temperatures T proportional to $\exp[-(T_0/T)^x]$, where the exponent x depends on the dimensionality of the system. [It was later shown by Efros & Shklovskii (57) that Mott's exponent would be modified if effects of long-range Coulomb interactions were properly considered.] Our contribution was to reformulate Mott's argument in terms of a peculiar percolation problem, neglecting the long-range interactions.

In the 1980s, I again became interested in percolation problems. Together with collaborators Pabitra Sen, Shechao Feng, and Christopher Lobb, we analyzed the behavior of elastic networks near a percolation threshold, and we demonstrated the differences between transport properties of various continuum percolation problems, including viscous flow through a porous medium, from those of previously studied discrete lattice models (58, 59).

The work on two-level systems in glasses, which I began at Bell Laboratories, continued during my first few years at Harvard. Together with my student James Black, we explored various implications of the two-level-system hypothesis for dynamic properties of glasses at low temperatures (60).

5.3. One-Dimensional Systems

As mentioned above, my first published paper concerned properties of a single quantum mechanical particle, or a set of noninteracting particles, in a one-dimensional system in the presence of disorder. At the time, I did not think that one-dimensional systems were particularly interesting in their own right; I worked on that problem in the hope that it would give me insight into the behavior of a three-dimensional system that I did not know how to solve. I did not appreciate that systems that are effectively one dimensional could actually be realized in the laboratory, and that, furthermore, in the presence of interactions, the properties of such systems could be highly nontrivial and different from those in higher dimension. I soon realized that my attitude was mistaken, however, and in later years I was drawn to study a number of fascinating phenomena in one-dimensional systems.

In 1990, Ian Affleck drew my attention to experiments, conducted by Koji Katsumata's group in Japan, on a molecular material NENP containing well-separated chains of antiferromagnetically coupled spin-1 nickel ions. In earlier theoretical work, Duncan Haldane, and Ian Affleck, Tom Kennedy, Elliot Lieb, and Hal Tasaki, had predicted that chains of this type should have an energy gap for spin excitations in the bulk of the chain but should have a zero-energy mode, essentially a decoupled emergent spin-1/2 degree of freedom, at each end (61, 62). The experimental observations could be explained if one assumed there was a small concentration of defects in the bulk of the crystal, which effectively split the chains into segments of finite length. Each defect would then be accompanied by a pair of weakly coupled spin-1/2 modes arising from the adjacent chain ends. The experiment was therefore the first experimental demonstration of the production of $S = 1/2$ excitations in a system whose constituents had only integer spins. In the end, I coauthored several papers with Affleck and Katsumata and others, including M. Hagiwara, Jean-Pierre Renard, and later, my student Partha Mitra, on various phenomena related to these systems (63, 64).

In 2002, I was approached by Amir Yacoby with some puzzling experimental results obtained in his laboratory. Amir and his students had developed a beautiful way to perform momentum-conserved tunneling between parallel one-dimensional wires on the cleaved edge of a GaAs heterostructure. An applied magnetic field perpendicular to the plane containing the wires was used to control the momentum boost acquired by the tunneling electrons while gate voltages could be used to control the electron density in the wires. The puzzles concerned several features in plots of the tunneling conductance as a function of these parameters. After considerable thought, together with my graduate student Yaroslav Tserkovnyak and Ophir M. Auslaender in Amir's laboratory, we were able to explain the data in terms of subtle effects arising from the confining potential at the ends of the wire segments, together with the phenomenon of spin-charge separation predicted for a one-dimensional electron system (65).

In 2008, I again teamed up with Amir on a problem concerning one-dimensional wires, this time together with Karyn LeHur. Our project, in this case, was to explore the meaning and consequences of charge fractionalization in a one-dimensional wire (66).

5.4. Mesoscopic Systems

Some of my earliest work on mesoscopic systems was inspired by experiments in the laboratories of Robert Westervelt and of Charlie Marcus, who was originally at Stanford University but later became a close colleague at Harvard for a period of a dozen years. In 1999, I collaborated with Piet Brouwer and Yuval Oreg, who were then postdoctoral fellows at Harvard, on a theory of grain-to-grain fluctuations in the ground-state spin of small metallic particles (67). In 2001, I collaborated with Ady Stern, Jan-Hein Cremers, Josh Folk, Oreg, and Marcus on an analysis of spin-orbit effects in a planar quantum dot in a parallel magnetic field (68). Subsequently, in collaboration with the Marcus and Yacoby groups, I was involved in projects related to an attempt to develop spin qubits in GaAs. A part of this research led to my involvement in a successful effort to explain puzzling observations related to the production of dynamic nuclear polarization in a process in which a single electron is shuttled between two connected GaAs quantum dots (69).

Another portion of my work has concerned spin transport in electronic conductors. Together with Hans-Andreas Engel, Emmanuel Rashba, Eugene Mishshchenko, and Andrey Shytov, I investigated problems related to the production of spin currents and spin polarization by charge currents in conductors with spin-orbit coupling (70). Discussions with my student Tserkovnyak and postdoctoral fellow Arne Brataas led to work on spintronic effects in hybrid systems containing metals and ferromagnets (71). I have also been involved in analyses of problems related to the Kondo effect in mesoscopic systems, *viz.*, problems in which a quantum dot with one or two low-energy degrees of freedom is coupled to leads with a continuum of low-energy excitations.

I have also been involved in the study of mesoscopic effects that were not directly related to spins or spin-orbit coupling. Examples were problems concerning effects of inhomogeneities in quantum Hall systems, splittings of Coulomb blockade peaks in transport through a quantum dot, and properties of small superconducting particles.

6. CLOSING REMARKS

Viewed from afar, it seems that my career has taken the form of a random walk through problems in condensed matter physics. For better or for worse, I have been largely motivated by a desire to understand specific phenomena that puzzled me at the time, rather than by a systematic goal to attack a singular overarching problem. Typically, I have tried to understand a problem from many points of view and have been driven to understand details at a quantitative level. Especially in recent years, I have enjoyed trying to solve puzzles posed by experiments in the laboratories of my colleagues. I would not argue that my approach is necessarily the best way to do science, and I would certainly not advocate this as an example for everyone to follow. However, I do believe it is important that researchers should enjoy their work and should conduct their careers in a way that gives them a high degree of personal satisfaction. In this regard, at least, my approach has worked for me.

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