

RICHARD FEYNMAN AND CONDENSED MATTER PHYSICS

During the mid-1950s Feynman's interests concentrated on problems in condensed matter, including liquid helium, rotons, polarons and superconductivity.

David Pines

From 1953 to 1958 Richard Feynman worked primarily on problems in condensed matter physics. Of the 14 scientific papers he published during this period, ten are devoted to the physics of liquid helium, one discusses the relation between superconductivity and superfluidity, and one deals with the motion of slow electrons in polar crystals, the "polaron" problem; the remaining two describe work Feynman had carried out on quantum electrodynamics and hadron physics earlier at Cornell. He brought to the condensed matter problems the same remarkable originality and physical insight that characterized his earlier work on quantum electrodynamics and the path integral method, and through his contributions he made a lasting impact on the subfields of low-temperature physics and statistical mechanics.

Here I shall focus primarily on his contributions to the theory of liquid helium. Because they offer good insight into how Feynman approached a new problem, I shall also discuss briefly his ideas about superconductivity (which for Richard was "the one that got away"), his work on polarons and his lectures on statistical mechanics.

About 35 years ago, the condensed matter and low-temperature community of theorists was a small one; we all knew one another personally, listened to one another's lectures and discussed problems together whenever we met. Where possible, I shall try to convey some sense of what it was like to have had Richard Feynman as a member of that community.

However, just as it is not possible to capture in words the experience of listening to Feynman discuss his research, it also is not possible to capture the flavor of his written work. Reading Feynman is a joy and a delight, for in his papers, as in his talks, Feynman communicated very directly, as though the reader were watching him derive

the results at the blackboard. Thus, for those of us who had the pleasure of knowing Feynman, his papers bring vividly to life those discussions and lectures. He is explicit about how he has formulated the problem and what methods he has tried; he makes no attempt to gloss over difficulties; and he takes the reader fully into his confidence on such matters as research strategy, physical pictures vs mathematical calculations, unsolved aspects of a problem, promising approaches to their solution and so on. I cannot encourage the readers of this article too strongly to go forth and read the original; there is no substitute.

Liquid helium theory prior to 1953

To put Feynman's contributions to our understanding of liquid helium into perspective, it is useful to recall the work of Fritz London, Lazlo Tisza and Lev Landau. London¹ proposed that the λ transition between normal liquid helium, He I, and the superfluid liquid, He II, at 2.19 K had its physical origin in the formation of a Bose condensate, a state in which all the atoms in the liquid would be in a single quantum state at absolute zero. The mechanism was analogous to the condensation of an ideal Bose-Einstein gas. Tisza² proposed a phenomenological two-fluid model to explain the behavior of He II, consisting of a superfluid component of density ρ_s that flowed without resistance and was the sole component at absolute zero, and a normal component of density ρ_n that resembled an ordinary liquid or even a gas. The proportion of normal fluid increased with increasing temperature until, at the λ point, ρ_s was 0 and ρ_n was ρ , the helium density. Landau³ arrived at a similar point of view by developing a theory of quantum hydrodynamics, in which the normal fluid corresponded to a gas of two kinds of interacting excitations: phonons, which at long wavelengths are the usual quantized sound waves of a compressible liquid, and rotons, short-wavelength excitations (corresponding to a momentum $p \sim p_0 \sim 2 \text{ \AA}^{-1}$) possessing a finite energy Δ ($\sim 10 \text{ K}$). Landau obtained the explicit form of the roton spectrum by analyzing early experiments on specific heat

David Pines is a theoretical physicist at the University of Illinois, Urbana-Champaign. His current research interests include the helium liquids, neutron stars, heavy electron systems and high-temperature superconductivity.

and second sound in He II; his proposed spectrum had the form shown in figure 1.

Feynman remarked that London's idea "could be criticized on the grounds that the strong forces of interaction between the helium atoms might make the ideal-gas approximation even qualitatively incorrect,"⁴ while "Tisza's view is frankly phenomenological";⁵ further, "the role of statistics in [Landau's] arguments is not clear,"⁵ and, "since the rotons appear to correspond to only a few atoms, a complete understanding of the roton state can therefore only be achieved by way of an atomic viewpoint." Feynman then suggested that "a more complete study of liquid helium from first principles might attempt to answer at least three important questions: (a) Why does the liquid make a transition between two forms, He I and He II? (b) Why are there no states of very low energy, other than phonons which can be excited in helium II (i.e., below 0.5 K)? (c) What is the nature of the excitations which constitute the 'normal fluid component' at higher temperatures, say from 1 to 2.2 [K]?" With these words Feynman described his research program on helium, a program he carried to completion during the period 1953-57.

An atomic theory of the λ transition

To demonstrate that the strong interaction between helium atoms would not change the central features of the Bose condensation proposed by London, Feynman drew upon his space-time approach to quantum mechanics⁶ to write the exact partition function as an integral over trajectories.⁴ He used this expression to examine the character of the most important trajectories and concluded that despite the nearly hard-sphere character of the atomic interaction, the motion of a given atom would be little affected by the motions of others; the latter atoms would simply move out of its way, and so contribute to its effective mass, but would otherwise affect its motion little. Feynman was thus led to an approximate form of the partition function, which he analyzed in some detail to show that as the temperature is lowered, there must occur a transition that depends in an essential way on the Bose statistics of the helium atoms and that resembles closely the third-order transition (specific heat continuous, but possessing a discontinuous slope) found for the ideal Bose gas. He further noted that it would not be difficult to obtain a transition temperature of approximately 2 K and that the geometric correlations he neglected would likely turn the transition into a second-order one, in agreement with experiment.

Thirty-two years passed before Feynman's program was realized; in 1985 David Ceperley and Eugene Pollock⁷ used a Cray-1 computer to carry out the necessary path integrals and obtained excellent agreement with experiment for the specific heat (and other properties) through and below the λ transition.

Viewed in perspective, this first paper by Feynman on liquid helium displays clearly that blend of magic, mathematical ingenuity and sophistication, and physical insight that is almost uniquely Feynman's. In reading

this and his later papers one is struck by how differently Feynman's mind worked from that of other great physicists who studied—and in totally different ways solved—the same problem. Feynman took a very hands-on, direct computational point of view; Lars Onsager relied on field theory and Landau on extremely general arguments. Yet all came in the end to similar conclusions.

Feynman on low-lying excited states

Feynman followed the work discussed above with a paper in which he examined the nature of the ground-state wavefunction of liquid He II and the character of the low-lying excitations. This paper is a *tour de force* in that it contains only a single equation (for the changes in free energy when a He⁴ atom is replaced by He³); rather it contains a series of closely reasoned arguments that led Feynman, and lead the reader, to the conclusion that because liquid He⁴ obeys Bose statistics, there can be no low-lying excitations other than longitudinal phonons.

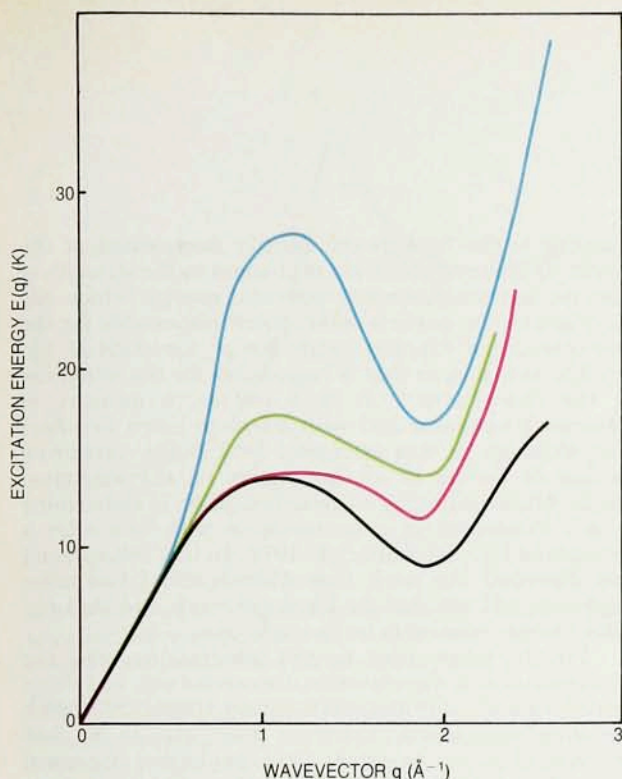
He began the paper with a qualitative description of the ground-state wavefunction, noting that if one freezes the motion of all the atoms at a given time, the wavefunction amplitude is negligible when any two atoms overlap (because of the strong hard-core-like repulsion) and is a maximum when a given atom is at the center of the "cage" formed by its neighbors. He then showed that extremely-low-energy excited states must involve large groups of atoms, and that compressional waves obeying a dispersion relation $\omega = sq$ (where ω is the frequency, s the sound velocity and q the wavevector) give a true mode of excitation. He demonstrated that this situation would not arise for an ideal Fermi gas or a classical (Boltzmann) gas, in both of which single-particle excitations yield the dominant low-lying mode. He clinched his case by showing that low-lying single-particle excitations cannot exist for a Bose liquid, because in this system the motion of a given atom from one location to another is "merely an interchange of which atom is which" and "cannot change the wavefunction." Feynman went on to examine the possible nature of "higher energy" excitations corresponding to the motion of a single atom or a small group of atoms, and argued that these might well correspond to Landau's rotons. He concluded the paper by introducing some of the themes to which he would return in his subsequent papers: the existence of a critical velocity for superfluid flow, and the motion of a small sphere or of a He³ atom in the liquid.

A wavefunction for phonons and rotons

In the third paper of the series,⁸ Feynman extended the physical arguments of the second to show that the wavefunction that represents an excitation in He II must be of the form

$$\Psi_{\text{exc}} = \Psi_0 \sum_j f(r_j) \quad (1a)$$

where Ψ_0 is the ground state wavefunction, f is some function of position, r , and the sum is over all the atoms.



Elementary excitation spectrum of liquid helium II as computed by Feynman⁸ (blue), Feynman and Cohen¹² (red), and the FC spectrum as corrected by Manousakis and Pandharipande¹⁶ (green). The spectrum as measured by Woods and Cowley¹⁴ is also shown (black). **Figure 1**

He then determined the form of $f(r)$ and the corresponding excited-state energy using the variational principle. He found $f(r) = \exp(i\mathbf{q} \cdot \mathbf{r})$, so that for an excitation of momentum $\mathbf{q}$ the wavefunction⁹ is

$$\Psi_{\text{exc}} = \Psi_0 \sum_j \exp(i\mathbf{q} \cdot \mathbf{r}_j) \equiv \rho_q^+ \Psi_0 \quad (1b)$$

while the corresponding energy is

$$E_q = q^2 / 2mS_q \quad (2)$$

Here ρ_q^+ is the Fourier transform of the density, $\rho(r)$, while S_q , the Fourier transform of the two-particle correlation function, may be written as

$$S_q = \langle \Psi_0 | \rho_q^+ \rho_q | \Psi_0 \rangle / N \quad (3)$$

where N is the number density of helium atoms. In this form it is seen that S_q is the liquid structure factor that determines the elastic scattering of neutrons or x rays and hence can be determined experimentally; for large q it approaches unity, while for small q , since ρ_q describes a phonon, it takes the form $S_q = q/2ms$, where s is the sound velocity. As shown in figure 2, for values of $q \sim 2 \text{ \AA}^{-1}$, S_q reaches a maximum, so that the corresponding value of the excitation energy, shown in figure 1, possesses a local minimum; the excitations in the vicinity of this minimum are Landau's rotons. Their energy Δ is, however, too large—some 18 K instead of the roughly 9.6 K that Landau found would fit the specific heat and second-sound velocity measurements. Feynman found this result discouraging, and attributed it to the inaccuracy of the wavefunction (equation 1b) in this momentum region.

Feynman went on in this paper to consider the thermodynamic properties of He II based on his form for the excitation spectrum, noting (quite correctly) that as one goes to temperatures near the λ point, the number of excitations becomes sufficiently large that their interac-

tions must be taken into account in any correct calculation. He then considered the motion of the fluid as a whole, noting that for irrotational motion the wavefunction must be of the form

$$\Psi = \Psi_0 \left\{ \exp \left[i \sum_j S(\mathbf{r}_j) \right] \right\} \quad (4)$$

where $S(r)$ is some function of position, and the velocity of the fluid is given by

$$\mathbf{v}_s(\mathbf{r}) = \nabla S(\mathbf{r}) / m \quad (5)$$

so that the motion is indeed irrotational, that is, $\nabla \times \mathbf{v}_s = 0$. He pointed out that one can have velocity fields that are not curl free, provided one considers regions that are not simply connected, as in a torus, because S then need not be single valued; he further remarked that under these circumstances, states of angular momentum in multiples of $N\hbar$ become possible—a clear indication that he was beginning to think about vorticity.

On considering excitations in a moving fluid, Feynman obtained the familiar result $E = E_q + \mathbf{q} \cdot \mathbf{v}_s$, where $\mathbf{v}_s$ is the superfluid velocity, and used this to establish the relation between his excitations and the normal-fluid density, ρ_n . He obtained the Landau results, and shared with the reader his unease about what the mathematically correct separation of the current into two parts is, using a gas of interacting rotons moving in a background fluid to illustrate his concern about how this separation is to be accomplished.

In the process of examining excitations in the moving fluid, Feynman identified a further problem with his excited-state wavefunction: It does not lead to particle conservation when one forms a wave packet to describe an excitation that carries current $\hbar\mathbf{q}/m$ and drifts at a group velocity $\nabla_q E_q$ (the roton, for which the group velocity is vanishing, being an extreme example). He therefore proposed a qualitative improvement in his wavefunction, in which the return flow of the background liquid about a moving excitation acts to conserve particles. This *backflow*, he noted, should be dipolar at large distances from the excitation, and the coupling of, for instance, the roton to this return field should both lead to a lower roton energy and provide a mechanism for roton-roton interaction. He proceeded to estimate the strength of the roton-roton interaction in order to demonstrate that such an interaction should lead to a correction of the Landau expression for the normal-fluid density, and hence should influence the calculation of the λ point. He further noted that Δ would decrease with pressure (thus providing a mechanism for phonon-roton interaction) and that the liquid would shrink if the number of rotons were increased. Feynman's arguments were well founded, and not a little prophetic. Some 25 years were to pass, however, before a more accurate theory of the consequences of roton-roton interactions was developed.^{10,11}

Feynman-Cohen wavefunction and backflow

Feynman and his student Michael Cohen took up the problem (posed by Feynman at the end of his third paper

in the series) of finding a better wavefunction to describe rotons, using as a basic building block the incorporation of backflow into the trial wavefunction.¹² As a trial problem they first considered the motion of an impurity that had the same mass m as He^4 , experienced the same forces and was not subject to Bose statistics. The trial wavefunction they chose for the motion of this impurity incorporated the backflow of the surrounding liquid through a function $g(r)$:

$$\Psi_A = \Psi_0 \exp[i\mathbf{q} \cdot \mathbf{r}_A] \exp\left[i \sum_{j \neq A} g(r_j - r_A)\right] \quad (6)$$

where $g(r)$ had the dipolar form $A\mathbf{q} \cdot \mathbf{r}/r^3$, which it will possess at large distances. Using the variational principle, they determined A to be about $3.8 \text{ \AA}^3$ (close to the classical value for perfect backflow, $3.6 \text{ \AA}^3$) and the impurity effective mass to be $1.54 m$ not far from the hard-sphere value of $1.5 m$. They therefore took for the excited-state wavefunction a symmetrical version of equation 6:

$$\Psi_{FC} = \Psi_0 \sum_j \exp(i\mathbf{q} \cdot \mathbf{r}_j) \exp i \sum_{j \neq k} g(r_{jk}) \quad (7a)$$

$$\equiv \Psi_0 \left\{ \sum_j \exp(i\mathbf{q} \cdot \mathbf{r}_j) \left[1 + i \sum_{j \neq k} g(r_{jk}) \right] \right\} \quad (7b)$$

where $g(r) = A\mathbf{q} \cdot \mathbf{r}/r^3$. The resulting energy spectrum was by no means easy to calculate, involving as it did lengthy numerical calculations that depended on both two-body and three-body correlation functions. As may be seen in figure 1, it led to a considerably improved value for the roton energy ($\Delta \approx 11.5 \text{ K}$).

Following their seminal work on the phonon-roton spectrum, Feynman and Cohen took up the question of measuring the excitation spectrum directly by studying the energy losses of monoenergetic neutrons scattered from liquid He II . They showed¹³ that most of the scattering at a fixed angle would result from production or annihilation of a single excitation from the condensate, so that one would expect to observe narrow peaks in the spectrum of scattered neutrons, superimposed on a broad background associated with energy losses in which two or more excitations are produced. They dealt in some detail with effects of finite temperature, instrumental resolution and linewidth. Their work not only accelerated substantially the experimental effort to measure this spectrum, but was singularly successful in setting forth, in advance of the first experiment, the major features of the neutron scattering experiments¹⁴ that are responsible for our present understanding of the excitation spectrum.

What is a roton?

Feynman and Cohen further proposed a physical picture of the roton based on their trial wavefunction—namely, that it resembled a classical vortex ring of such small radius that only one atom could pass through its center, with the backflow describing a slow drift of atoms returning for another passage through the ring. Charles Aldrich and I subsequently showed that this rather poetic picture is not quite right: A roton should rather be thought of as a He^4 quasiparticle and its associated backflow field, of mass $m^* \sim 2.8 m$, moving in an attractive potential well of energy -2 K , which originates in its

coupling to the background density fluctuations of the liquid. It is the combination of changes in the strength of that momentum-dependent potential energy (which has its origin in the particle interactions responsible for the pair-correlation function) with the p^2 variation of the quasiparticle energy that is responsible for the minimum in the $E(p)$ curve.¹⁵ It is, I believe, a measure of Feynman's openness and willingness to listen to others that although he was extremely fond of his vortex-ring picture (it figures in all his papers on the excitation spectrum), he evidenced no great hesitation in abandoning it as I discovered in a conversation with him after a colloquium I gave at Caltech in 1978. In that colloquium I had described the work that Aldrich and I had done; Feynman told me that he liked our work and that our roton picture seemed to be the right one.

Finally, when more careful integrations using the Feynman-Cohen wavefunction are carried out, and a sign error in one of their integrals is corrected, their results for the short-wavelength spectrum turn out to be less accurate than they reported. The mistake was discovered some 27 years later by Efstratios Manousakis, who was then a graduate student at the University of Illinois, Urbana-Champaign, working with Vijay Pandharipande on an improved variational wavefunction for excitations in liquid He^4 . As figure 1 shows, the corrected¹⁶ Feynman-Cohen result is sufficiently far off the mark that one cannot help but wonder if Feynman had known of the error, whether he might not have continued his work on rotons in an effort to improve that spectrum.

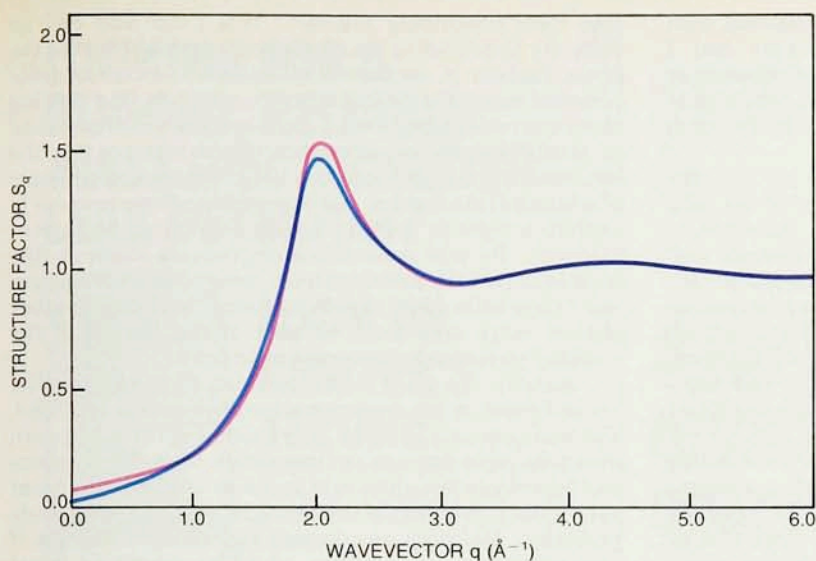
Feynman on superfluid flow

In an article in *Progress in Low Temperature Physics*,¹⁷ Feynman discussed in some detail the nature of superfluid flow, with particular attention to the quantization of circulation for rotational flow, the response of the superfluid in a bucket that is rotated, and the existence of a critical velocity for superfluid flow. He showed how the formation of the quantized vortex lines, suggested by Lars Onsager in 1949 (in a paper that Feynman read only after completing his own, independent work), could enable the superfluid to respond to rotation as though it were a solid body. The quantization of circulation takes the form

$$\oint \mathbf{v}_s \cdot d\mathbf{l} = nh/m$$

where the line integral is to be carried out over any closed path. By considering the kinetic energy contained in the vortex lines, Onsager and Feynman showed that when the superfluid is set in rotation, it would be energetically favorable to form singly quantized vortex lines in such a way as to mimic the solid-body rotation.

Feynman then considered the role that vortex lines play in more general fluid motions, with particular attention to the possibility that the critical velocity for superfluid flow could be determined by the production of vorticity. By estimating the energy required to create vortices for a typical experimental situation, he arrived at a quite reasonable estimate of the superfluid flow velocity ($\sim 1 \text{ m/sec}$). He then made a pioneering effort to consider the nature of superfluid turbulence. The paper is a classic:



Experimental results for the static structure factor for liquid helium. The red curve shows results for 2.29 K, the blue curve for 1.06 K. **Figure 2**

It addresses a range of extremely difficult problems in an imaginative, very physical and correct way; and it set the stage for all future work on superfluid flow.

Feynman on superconductivity

Feynman worked very hard on superconductivity during 1954–57. I was aware of this in a general way from reading references to superconductivity in several of his papers written during this period and from listening to his 1956 lecture to the International Congress on Theoretical Physics in Seattle. However, the extent to which this problem had become a central focus for his research became clear to me only when I visited Caltech for two weeks in December 1956. Feynman and I had lunch together nearly every day; walking to lunch, at lunch and after lunch, Feynman would try out a new set of ideas about superconductivity on me. (An incomplete list of these ideas may be found in his published Seattle lecture.¹⁸) It was clear from these conversations that he was thoroughly familiar with both the experimental and the theoretical literature on superconductivity. Independently of Arkady Migdal, Feynman had tried his diagrammatic techniques on the electron-phonon system and found that the system continued to behave normally. Cohen pointed out to him that this was perhaps not surprising, since the true ground state might not be obtainable using perturbation theory—a quite prophetic remark.

Feynman's strategy for solving superconductivity, as described in his 1956 lecture, is worth recounting:

I would like to maintain a philosophy about this problem which is a little different from usual: It does not make any difference what we explain, as long as we explain some property correctly from first principles. If we start honestly from first principles and make a deduction that such and such a property exists—some property that is different for superconductors than for normal conductors, of course—then undoubtedly we have our hand on the tail of the tiger because we have got the mechanism of at least one of the properties. If we have it correct we have the clue to the other properties, so it isn't very important which property we explain. Therefore, in making this attempt, the first thing to do is to choose the easiest property to handle with the kind of mathematics that is involved in the Schrödinger equation. I want to summarize some thoughts on

this question, although they do not represent a solution. They represent a statement of the problem and a little bit of a personal view.

I decided it would be easiest to explain the specific heat rather than the electrical properties. . . . But we do not have to explain the entire specific heat curve; we only have to explain any feature of it, like the existence of a transition, or that the specific heat near absolute zero is less than proportional to T . I chose the latter because being near absolute zero is a much simpler situation than being at any finite temperature. Thus the property we should study is this: Why does a superconductor have a specific heat less than T ?

Unlike many of the other great theoretical physicists who worked long and unsuccessfully trying to develop a microscopic theory of superconductivity, Feynman was quick to recognize that John Bardeen, Leon Cooper and Robert Schrieffer had indeed solved the problem in their epochal 1957 paper. Thus at the Kamerlingh Onnes Conference in 1958 he remarked that superconductivity had been solved, and in his 1961 lectures on statistical mechanics he described the BCS theory in considerable detail.

Another indication that he retained his interest in the field is the description of the Josephson effect in the *Feynman Lectures*, which indicated that he clearly realized the implications of this work, though he never worked on this subject himself.

Feynman on polarons

Electrons in ionic crystals are coupled (often strongly) to the nearly frequency-independent optical modes, so that as an electron moves it is accompanied by a polarization wave that acts to reduce its energy and increase its mass. The resulting entity, the electron plus its accompanying phonon cloud, is called a polaron. The polaron problem, first worked on by Landau and S. I. Pekar in the 1930s and revived by Herbert Fröhlich in the late 1940s, is of intrinsic theoretical interest because the dimensionless electron-phonon coupling constant α is typically large, so that perturbation-theoretic methods do not apply. It attracted the interest of many of us in the early 1950s because of the possibility that by understanding such a strong-coupling problem we might be able to make progress on a microscopic theory of superconductivity. Thus when I came to Urbana in 1952 to work as a postdoc

with Bardeen, he suggested that I look into polarons, with the result that Tsung-dao Lee, Francis Low and I developed an intermediate-coupling theory of polarons at the University of Illinois, Urbana-Champaign, while quite similar work was carried out independently by Fröhlich and his students in Liverpool.

Feynman became interested because he saw the polarons as an opportunity to test the power of his path integral approach by using a variational principle to compute the ground-state energy for the electron coupled to the phonon field. He was right; with comparatively little effort, he was able to reproduce our intermediate-coupling results and to obtain, by a different choice of parameters in the trial actions in his path integral, accurate results for the ground-state energy and effective mass that extend smoothly into the strong-coupling domain.¹⁹

(Incidentally, Bardeen's intuition that understanding polarons might help in the development of a microscopic theory of superconductivity proved to be correct. Schrieffer arrived at the BCS variational wavefunction by adapting the intermediate-coupling ground-state wavefunction, which Lee, Low and I had obtained, to the model Hamiltonian Bardeen, Cooper and Schrieffer had used to describe superconductivity.)

Feynman's interest in polarons continued into the 1960s. With his postdoc Robert Hellwarth and his students Carl Iddings and Philip Platzman, he extended his variational path integral approach to the calculation of the polaron response to an external field (in unpublished calculations, Feynman had independently arrived at the Kubo-Lax description of transport properties in terms of response functions), and so obtained an expression for the polaron mobility for arbitrary coupling constant. This further enabled them to examine transport under conditions for which the Boltzmann equation is not an adequate approximation.²⁰

The Feynman legacy

Following the seminal work of Migdal and his students Spartak Beliaev and Victor Galitskii, and of Murray Gell-Mann and Keith Brueckner, in the mid-1950s, Feynman diagrams have become one of the major methods used to calculate and describe physical processes in condensed matter physics. Yet they were almost never so used by Feynman. This seeming paradox is resolved when one realizes that the problems in condensed matter physics that interested Feynman were those of strongly interacting systems, for which a variational approach, either by itself or combined with his path integral formulation, was ideally suited. Summing Feynman diagrams offers a marvelously compact way of carrying out consistent perturbation-theory calculations and, in some cases, of giving general proofs. Thus for the problems that interested Feynman, diagrams were therefore not very useful, and Feynman made only sparing use of them in his papers on condensed matter physics.

For a number of years, Feynman spent a day each week at Hughes Research Laboratories; during this period he gave a series of lectures on topics that interested him, as well as consulted with colleagues there. In 1961 he lectured on statistical mechanics; the notes on his lectures, which were taken by R. Kikuchi and H. A. Feiveson, rapidly became a *samizdat* classic in the field. Some years later, when the publisher W. A. Benjamin approached Feynman seeking to publish these notes, he told me that given that some time had passed and he was no longer working in the field, he could no longer be sure that they would be of interest; he would therefore publish only on the condition that I first read them through and guaran-

teed their continuing interest. This I did, and had no difficulty in providing the requisite guarantee because the notes contain a remarkably lucid and at times quite personal exposition of statistical mechanics. On reading them one realizes anew what excellent taste Feynman had in identifying those topics that would best provide the beginner with insight into the key methods and concepts of statistical mechanics, and how willing Feynman was to explore a topic in depth, from its beginnings to current research. By now generations of graduate students (the book is in its 11th printing) have come to share this view, and I have little doubt that these notes²¹ will join his other lecture notes and books as part of the library of the working physicist for generations to come.

As with the other fields in which Feynman worked, his influence on condensed matter physics was profound, and will continue to be so. Feynman diagrams and path integrals have become indispensable tools for theorists and experimenters alike, and backflow is now a significant part of the vocabulary of the physicist studying many-body problems. His work on polarons represents a difficult, if not impossible, act to follow, while his papers on liquid helium set clearly the signposts for future work in the field, work that will incorporate at every level the physical picture he set forth some 35 years ago.

* * *

I should like to thank Phil Anderson, Gordon Baym and Chris Pethick for their careful reading of this manuscript and their helpful suggestions.

References

1. F. London, *Phys. Rev.* **54**, 947 (1938). For a further development of London's seminal ideas, see also F. London, *Superfluids*, vol. 2, Dover, New York (1954).
2. L. Tisza, *Nature* **141**, 913 (1938); *C. R. Acad. Sci.* **207**, 1035, 1186 (1938); *Phys. Rev.* **72**, 838 (1947).
3. L. D. Landau, *J. Phys. USSR* **5**, 71 (1941); *Phys. Rev.* **60**, 354 (1941); *J. Phys. USSR* **8**, 1 (1944); *J. Phys. USSR* **11**, 91 (1947).
4. R. P. Feynman, *Phys. Rev.* **91**, 1291 (1953).
5. R. P. Feynman, *Phys. Rev.* **91**, 1301 (1953).
6. R. P. Feynman, *Rev. Mod. Phys.* **20**, 367 (1948).
7. D. M. Ceperley, E. L. Pollock, *Phys. Rev. Lett.* **56**, 351 (1986).
8. R. P. Feynman, *Phys. Rev.* **94**, 262 (1954).
9. As Feynman notes, similar wavefunctions had been proposed earlier, for example, by A. Bijl, *Physica* **7**, 896 (1940).
10. K. S. Bedell, I. Fomin, D. Pines, *J. Low Temp. Phys.* **48**, 417 (1982).
11. K. S. Bedell, A. Zawadowski, D. Pines, *Phys. Rev. B* **29**, 102 (1984).
12. R. P. Feynman, M. Cohen, *Phys. Rev.* **102**, 1189 (1956).
13. M. Cohen, R. P. Feynman, *Phys. Rev.* **107**, 13 (1957).
14. The pioneering experiments were carried out by H. Palevsky and his collaborators at Brookhaven in 1957. For reviews, see A. D. B. Woods, R. A. Cowley, *Rep. Prog. Phys.* **36**, 1135 (1973); D. L. Price, in *Physics of Liquid and Solid Helium*, vol. 2, K. H. Bennemann, J. B. Ketterson, eds., Wiley, New York (1978), p. 675.
15. For a review of this approach, see D. Pines, *Can. J. Phys.* **65**, 1357 (1987).
16. E. Manousakis, V. R. Pandharipande, *Phys. Rev. B* **30**, 5062 (1984).
17. R. P. Feynman, in *Progress in Low Temperature Physics*, vol. 2, C. J. Gorter, ed., North-Holland, New York (1955), p. 17.
18. R. P. Feynman, *Rev. Mod. Phys.* **29**, 205 (1957).
19. R. P. Feynman, *Phys. Rev.* **97**, 660 (1955).
20. R. P. Feynman, R. W. Hellwarth, C. K. Iddings, P. M. Platzman, *Phys. Rev.* **127**, 1004 (1962).
21. R. P. Feynman, *Statistical Mechanics*, Addison-Wesley, Reading, Mass. (1972).