SUPERFLUID 3-He: THE EARLY DAYS AS SEEN BY A THEORIST

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by

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It is needless to say that I feel it a great honor and privilege to have been selected for the 2003 Nobel prize in physics for my theoretical work on superfluid ^{3}He; I am particularly pleased to be sharing the award with Professors Ginzburg and Abrikosov, whom I have always looked up to as giants of the closely related fleld of superconductivity. The story of how, in roughly the twelve-month period July 1972–July 1973, we came to a theoretical understanding of the experimental data on what we now know as superfluid ^{3}He is a sort of complex detective tale, involving many actors besides me; for reasons of time I will concentrate in this lecture on my own involvement, and will have to omit several important developments in which I had no direct role.

The element helium comes in two (stable) forms, ^{4}He and ^{3}He; at low temperatures and pressures both form liquids rather than solids. The liquid phase of the common isotope, ^{4}He, was realized nearly a century ago, and since 1938 has been known to show, at temperatures below about 2K, the property of <u>superfluidity</u> – the ability to flow through the narrowest capillaries without apparent friction. By contrast, the liquid form of the rare isotope, ^{3}He, has been available only since about 1950, when enough of it was produced by the decay of the tritium manufactured in nuclear reactors. However, it was soon recognized that liquid ^{3}He is in many ways similar to a system which has been known for much longer, namely the electrons in metals. Although there is one obvious difference (the electrons in metals are electrically charged whereas the ^{3}He atom is electrically neutral), both systems are dense systems of particles which have spin 1/2 and are therefore expected to obey Fermi-Dirac statistics. (By contrast, the atoms of ^{4}He have spin zero and should therefore obey Bose-Einstein statistics). If we consider a non-interacting gas of such particles in thermal equilibrium at a temperature $T \ll T_F = \varepsilon_F / k_B$ (where ε_F is the "Fermi energy", determined by the mass and density), then all states lying well below ε_F in energy are occupied by a single particle and all those well above ε_F are empty; rearrangement of the particles can take place only in an energy "shell" of width $\sim k_B T$ around ε_F, and all the thermal, transport and response properties are thus determined by the properties of the states in this shell. In a famous 1956 paper L. D. Landau [1]

showed that under appropriate conditions this picture remains qualitatively valid even in the presence of strong inter-particle interactions; the system is then known as a "degenerate Fermi liquid". Experiments on liquid ^{3}He in the fifties and early sixties showed that this system indeed appeared to be behaving as a degenerate Fermi liquid below ~100 mK, down to the lowest temperatures then attainable (around 3mK).

Now, it has been known for nearly a century that the electrons in metals, which have a Fermi temperature of ~ 10^4–10^5K, may sometimes, at temperatures $\lesssim$ 20K, enter the so-called superconducting state, in which they can flow without apparent resistance; this is just the analog, for a charged system, of the superfluidity of liquid ^{4}He. Since for liquid ^{3}He the Fermi temperature is only a few K, it would have been reasonable to speculate that the atoms might undergo a similar transition at temperatures of the order of mK; since the atoms are electronically neutral, the result would be not superconductivity but rather superfluidity, as in ^{4}He. However, in the absence of a microscopic theory of superconductivity no quantitative approach to this question suggested itself.

Remarkable progress in the phenomenological description of superconductivity was made in the early fifties (as recognized in the awards to my co-laureates), in particular by introducing the concept of a "macroscopic wave function" or order parameter. The microscopic underpinning [2] of this concept was provided by my late colleague John Bardeen and his collaborators Leon Cooper and Bob Schrieffer in 1957, in what is now universally recognized as the correct microscopic theory of superconductivity (at least as it was known at that time), the "BCS" theory. They postulated that in the superconducting state the electrons within a "shell" of width ~ $k_B T_c$ around the Fermi energy (where T_c is the temperature of the superconducting transition) tend to form "Cooper pairs", a sort of giant "di-electronic molecule", whose radius is huge compared to the average distance between electrons (so that between any two electrons forming a Cooper pair there are billions of other electrons, each forming their own pairs). A very essential feature of the BCS theory of superconductivity is that the Cooper pairs, once formed, <u>must all behave in exactly the same way,</u> that is, they must have exactly the same wave function, as regards both the center of mass and the relative coordinate. In fact, the "macroscopic wave function" of Ginzburg and Landau turns out to be nothing but the common center-of-mass wave function of all the pairs. This wave function can have a very nontrivial spatial variation, and it is this variation which gives rise to the effects described by Professors Ginzburg and Abrikosov.

By contrast, in BCS theory the "internal" (relative) wave function of the pairs is rather boring; the two electrons have opposite spins, thus the pair has total spin zero, and the relative orbital angular momentum is also zero, so that in atomic notation the state of the "molecule" is $^1S_0$, and all properties of the system are completely isotropic; there are no "orientational" degrees of freedom.

When at the end of the fifties people started to extend the ideas of BCS to liquid ^{3}He, they soon realized an important difference between this system

and the electrons in metals: The interaction potential between two He atoms is strongly repulsive at short distances, and becomes attractive only for inter-atomic separation $r \sim r_o \sim 3$ Å; thus, the atoms forming a Cooper pair cannot approach much more closely than this. Since they come from states close to the Fermi surface, however, their relative momentum must be of the order of $p_F \equiv (2m\varepsilon_F)^{1/2} \sim 1$ Å^{-1}. This then means that their dimensionless relative angular momentum l must be of order $p_F r_o/\hbar$ and thus must be nonzero (most probably 1 or 2), in contrast to the case of metallic superconductors where as we have seen the pairs have $l = 0$. (A more quantitative calculation bears out the conclusion of this intuitive argument). The Fermi statistics then imply that if l is even (so that the orbital state is symmetric under exchange of the two particles) then the spin state must be a singlet (S = 0) as in the original BCS case, while if l is odd the spin state must be a triplet (S = 1). It was then not unreasonable to expect that in either case (provided $l \neq 0$) the orbital properties might be anisotropic, and that in the case of spin triplet pairing (odd l) the spin properties might also be so; however, as we shall see, this question is less straightforward than it might seem.

In the early sixties there was a good deal of theoretical interest in the possible existence and properties of a Cooper = paired (hence presumably superfluid) phase of liquid ^{3}He; in the present context two developments in particular are worthy of note. In a seminal 1961 paper [3], Anderson and Morel made a systematic adaptation of the ideas of BCS to this system: that is, they explicitly assumed (as had others) that in the $l \neq 0$ case, just as in the $l = 0$ one considered by BCS, all Cooper pairs form in exactly the same state, with respect not only to their center-of-mass motion <u>but also with respect to their internal (relative) state.</u> It is worth emphasizing that this assumption is not trivial; indeed there was at least one nearly contemporary paper [4] which made a quite different and perhaps prima facie more attractive assumption, namely that (in the $l = 2$ case) all the five Zeeman substates are equally populated, in such a way as to give a state of the system whose physical properties are totally isotropic.[1] By contrast, Anderson and Morel showed that with their assumption the physical properties would in general be anisotropic both in orbital and, in the odd-l (spin triplet) case, spin space. They studied in detail two cases: that of pairing in a relative d-state ($l = 2$), where they worked out various physical properties in considerable detail, and more briefly that of a p-state ($l = 1$). In the latter case they chose a particular state, namely one in which pairs form only in the $S_z = +1 (\uparrow\uparrow)$ and $S_z = -1 (\downarrow\downarrow)$ states (a so-called "equal-spin-pairing" (ESP) state), and furthermore these states are associated with the same orbital wave function, which intuitively corresponds to the pairs having an orbital angular momentum $\hbar$ along a direction conventionally denoted by the unit vector $\hat{\mathbf{l}}$; this state has subsequently, for reasons we shall see, acquired the name of the "Anderson-Brinkman-Morel" (ABM) state. As far as I know, there was no particular reason at the time to choose this state rather

[1] For arguments against this idea, see refs. [5] and [6]. Many years later the idea was revived [7] but again refuted [8].

than, for example, the state, which within the generalized BCS theory is degenerate with ABM, in which the $S_z = +1$ and $S_z = -1$ pairs have opposite angular momenta ("axial" state); so that the choice they actually made was serendipitous.

A second very important advance was made independently by Vdovin [9] in the former Soviet Union and Balian and Werthamer [10] (BW) in theWest; they observed that in the odd-l case it is possible to form pairs simultaneously in all three Zeeman substates in such a way that the pair wave function is a superposition, i.e., schematically of the form (where **r** denotes the relative coordinate)

$$\Psi_{pair} = F_{\uparrow\uparrow}(\mathbf{r}) |\uparrow\uparrow\rangle + F_{\downarrow\downarrow}(\mathbf{r}) |\downarrow\downarrow\rangle + F_{\uparrow\downarrow}(\mathbf{r})(\frac{1}{\sqrt{2}}(\uparrow\downarrow + \downarrow\uparrow)) \tag{1}$$

It should be emphasized that all the Cooper pairs still occupy the single superposition (1) (we do not have one-third of the pairs occupying each Zeeman substate independently!). The "ESP" states considered in earlier work such as that of Anderson and Morel are special cases of (1) with $F_{\uparrow\downarrow}(\mathbf{r}) \equiv 0$; in particular, the ABM state has (up to a phase) $F_{\uparrow\uparrow}(\mathbf{r}) = F_{\downarrow\downarrow}(\mathbf{r})$. In the specific case $l = 1$, Vdovin and BW showed that it is possible to choose $F_{\uparrow\uparrow}(\mathbf{r})$ to correspond to (apparent) angular momentum $L_z = -1$ and $F_{\downarrow\downarrow}$ and $F_{\uparrow\downarrow}$ similarly to correspond to $L_z = +1$ and $L_z = 0$ respectively; in this way they constructed a state with $L = S = 1$ but $J \equiv |\mathbf{L+S}| = 0$, i.e., in atomic notation a $^3P_0$ state. By the Wigner-Eckart theorem such a state should be isotropic in all its properties, whether involving orbital motion, spin or their combination; in particular, the spin susceptibility should be isotropic and equal to 2/3 of the normal-state value (reflecting the fact that a third of the spin state is $S_z = 0$ with respect to any axis). Vdovin and BW showed that within the generalized BCS calculation which they used this state (conventionally known as the BW state) should be more stable than any $l = 1$ ESP state.

As a result of these and other considerations, the general expectation in the theoretical community by around 1964 was that (a) liquid ^{3}He might enter a Cooper-paired state, which would then display inter alia the property of superfluidity, at a temperature which was difficult to predict, and (b) that the symmetry of such a state was most likely to be either $l = 2$, $S = 0$ or the BW ($^3P_0$) state; in either case the spin susceptibility should be isotropic and reduced from its normal-state value (to zero in the $l = 2$ case, by a factor of 1/3 in the BW case).

It was at this point that I became actively interested in the possible superfluid phase of ^{3}He. I had done a little work on normal ^{3}He in my D. Phil. thesis (using Landau Fermi-liquid theory which at that time was still something of a novelty in the West), and had subsequently gone to work as a postdoc with David Pines at the University of Illinois at Urbana-Champaign. One day, after I had been there for about a month, John Bardeen and Leo Kadanoff came into my office and said "Look, John Wheatley down in the basement is doing experiments on the spin diffusion coefficient of liquid ^{3}He, and is getting into the temperature regime where people think the superfluid transi-

tion might occur. Why don't you try to work out how this would be reflected in the spin diffusion behavior?" I started this calculation but never finished it; in retrospect this was probably just as well, since we now know that because of the anomalous NMR behavior of superfluid ^{3}He (on which much more below) the spin diffusion is an extraordinarily complicated problem, and it is unlikely in the extreme that I would have got it right in the absence of experimental clues.

The reason I never finished the spin diffusion calculation was that I got side-tracked on to a problem that struck me as much more interesting, namely how to combine the ideas of BCS on pairing in a weakly interacting Fermi system with Landau's "Fermi-liquid" theory of the normal state of ^{3}He. After some vicissitudes (see ref. [11]) I was able to reformulate the Landau theory in terms of a set of "molecular fields", a form in which it was then almost trivial to apply it to the hypothetical superfluid phase as well as to the normal one. I was able to show (for the $l = 0$ case) that while the spin susceptibility and normal density of a degenerate superfluid Fermi liquid, like that of a weakly interacting superfluid Fermi gas, both fall to zero in the limit $T \to 0$, their temperature dependence is in general quite different form that of the latter (and from one another)[2]. In the case of the spin susceptibility, I also applied the "molecular-field" idea to the BW state, and showed inter alia that in ^{3}He Fermi-liquid effects should depress the $T = 0$ susceptibility (relative to the normal-state value) from the "weakly interacting" value of 2/3 to about 1/3. In subsequent work I generalized Landau's calculation of the low-frequency collective excitations of a Fermi liquid, including zero sound, to the superfluid phase.

During the two years following my postdoctoral year at UIUC, I wandered about quite a bit, both geographically[3] and intellectually. Among other problems in the low-temperature area which I considered during this period was one concerning the possible collective excitations of a "two-band" superconductor, that is, a superconducting metal in which the Fermi surface interacts two different bands. I think I had probably read, and been influenced by, P. W. Anderson's elegant formulation of the theory of superfluidity in ^{4}He in terms of conjugate "number" and "phase" variables; at any rate, it occurred to me that a two-band superconductor should show a sort of "internal Josephson effect" corresponding to fluctuations of the relative number of electrons in the two bands and of the relative phase of the Cooper pairs in them, and in a paper published in Progress of Theoretical Physics [14] I discussed the appropriate microscopic definitions of the corresponding operators $\Delta N, \Delta\phi$ and their commutation relations, namely, $[\Delta N, \Delta\phi] = i$. At the time this work sank more or less without trace, in part because by the time it was published

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[2] Many years later I discovered that in both cases my conclusions had been to some extent anticipated, in refs. [12] and (arguably) [13] respectively. However, at the time they were a great boost to my morale!

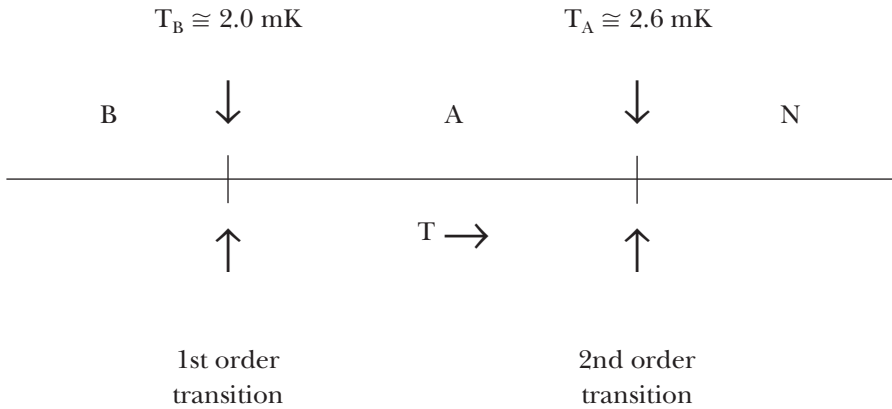[3] See the biographical note which precedes this lecture.

$T_B \cong 2.0$ mK $\qquad\qquad$ $T_A \cong 2.6$ mK

B $\qquad \downarrow \qquad$ A $\qquad \downarrow \qquad$ N

$\uparrow \qquad\qquad$ T $\rightarrow \qquad\qquad \uparrow$

1st order $\qquad\qquad$ 2nd order
transition $\qquad\qquad$ transition

Figure 1: The phase diagram of liquid ³He at melting pressure below 3mK.

it had already become clear that the experimental evidence for the existence of two-band superconductors in nature was dubious; however, it was to play a crucial role in the subsequent history.[4]

In the fall of 1967 I took up a lectureship at the University of Sussex, and for the next few years, in the intervals allowed by my teaching duties, I continued to work on various problems in low-temperature physics, including liquid ³He. However, I found myself becoming increasingly bored with this area of research, and indeed with much of conventional physics; at the same time, thanks in part to a remarkable series of lectures delivered by my colleague Brian Easlea, I got more and more intrigued by the conceptual foundations of quantum mechanics, and by the summer of 1972 had made a firm decision that I would abandon the sort of physics that gets published in Phys. Rev. B and devote myself full-time to foundational studies. (Fortunately, in those days even lectureship positions in British universities carried tenure, so that it was possible to make such a switch without drastically affecting one's career prospects!)

In July 1972 I was on a climbing holiday in Scotland when I heard that Bob Richardson, whom I knew at a distance as an experimentalist at Cornell working on, among other things, liquid and solid ³He, would be passing through Sussex for a day and would like to talk to me. I was certainly keen to meet Bob (I had had some correspondence with him concerning an effect in normal liquid ³He on which he had done the experiment and I the theory), but I was enjoying my holiday, and I remember debating with myself whether to go home a day early to meet him or not. In the end I think the weather made the decision for me: it rained on the morning in question, I went home early and spent several hours talking to Bob. What he told me that day changed my

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[4] In early 2003, Ponomarev *et al.* [15] reported experimental evidence for the existence in MgB_2 of the mode predicted in ref. [14].

whole research career and led, thirty years later, to my presence in Stockholm today.

In earlier work the Cornell group (at that time consisting of Doug Osheroff, Dave Lee and Bob himself) had studied the pressurization curve (that is, the graph of pressure versus time) of a mixture of liquid and solid ^{3}He, and had observed two small but reproducible anomalies, which with some hindsight indicate phase transitions in the liquid: a second-order transition at $T_A \sim$ 2.6 mK and a first-order one at about $T_B \sim$ 2 mK (see figure 1). Using the nomenclature which later became standard, the liquid phase which exists between 2 and 2.6 mK is the A phase and that below 2 mK is the B phase: thus the normal (N) – A transition is second order and the A–B one first order. The group had published their experimental results [16] in early 1972, but had erroneously interpreted the phase transitions as occurring in the solid rather than the liquid; I had vaguely known about this work but had not been particularly excited by it, in part because there was a general expectation that some kind of magnetic-ordering transition would occur in solid ^{3}He in the mK regime[5], and it did not seem that the resulting phase was likely to be particularly novel in its properties.

What Bob told me, however, was that the group (by now augmented by Willy Gully) had gone on to do nuclear magnetic resonance (NMR) experiments [18] on the solid-liquid mixture in the temperature regime in which the thermodynamic anomalies occurred. These were straightforward c.w. absorption measurements, with the rf field polarized perpendicular to the external dc field H_{ext}. The first, qualitative conclusion, which was drawn from an analysis of the spatial profile of the NMR, absorption, was quite unambiguously that the phase transitions were occurring in the liquid component of the mixture, not the solid. Now, what an NMR experiment in the above standard geometry measures is the rf power absorption as a function of the frequency ω of the rf field; provided a linear approximation is valid, from this one can directly infer the imaginary part of the (transverse) spin density response function of the system, $\chi(\omega)$, and application of a Kramers-Kronig relation to the latter (c.f. below) then allows one to infer the dc susceptibility χ_0.

In the normal (N) phase the absorption profile has a very sharp resonance at the (temperature-independent) Larmor frequency $\omega_{res} = \gamma H_{ext}$, where γ is the gyromagnetic ratio of the free ^{3}He atom, and the dc susceptibility χ_0 is also temperature-independent and of the general order of magnitude of that predicted for a free Fermi gas with the mass and density of liquid ^{3}He. These results were already well established and not considered at all surprising: the discrepancy between the experimental and free-gas values of χ_0 is entirely consistent with the interpretation of the N phase as a degenerate Landau Fermi liquid, and the fact that the observed resonance frequency is (within experimental error) the free-atom value tells us that any effective magnetic field, other than those of "exchange" origin, arising from the system itself are

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[5] A few years later, such a transition was indeed observed [17] at a temperature of about 1 mK.

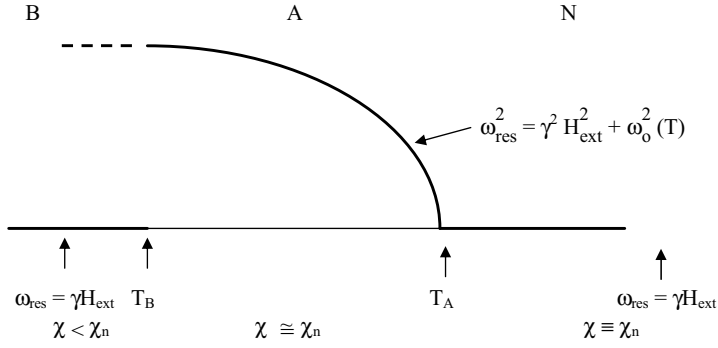*Figure 2:* The behavior of the NMR resonance frequency ω_{res} in liquid ^{3}He below 3mK. (The qualitative behavior of the static susceptibility χ is also noted.).

negligible. (The "exchange" fields, which are in part responsible for the renormalization of the *dc* susceptibility, are automatically parallel to the total spin of the system; thus they cannot exert a torque on the latter and thus cannot shift the resonance frequency, which is determined by the external field alone: cf. below). This is exactly what is expected, since the only possible origin of such a non-exchange field is the tiny dipole interaction (on which more below).

By contrast, the NMR behavior in the *A* and *B* phases is highly unconventional: see figure 2. As the temperature falls below the temperature T_A of the second-order transition, the resonance absorption peak remains very sharp but shifts upwards in frequency. The shift is not proportional to the external field H_{ext} but rather obeys a "Pythagorean" law:

$$\omega_{res}^2 = \gamma^2 \mathcal{H}_{ext}^2 + \omega_o^2(T) \tag{2}$$

with ω_o^2 having the approximate temperature-dependence

$$\omega_o^2(T) \cong A(1 - T/T_A), \qquad A/(2\pi)^2 \cong 5 \times 10^{10} Hz^2 \tag{3}$$

When the system undergoes the first-order phase transition into the B phase, the resonance frequency drops back discontinuously to the Larmor (N-state) value $\omega_{res} = \gamma H_{ext} \equiv \omega_L$. Meanwhile the *dc* susceptibility, obtained as described from the absorption data using a Kramers-Kronig relation, remains constant in the *A* phase at its *N*–state value;[6] however it drops discontinuously at the *A*–*B* transition by a factor of $\sim 50\%$, and thereafter decreases further with temperature in the *B* phase.

The behavior of the susceptibility struck me as surprising but not extraor-

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[6] Subsequent work [19] has shown that there is in fact a very small jump (a fraction of a percent) in χ_o at the *N*–*A* transition.

dinary. In particular, it could be fitted into the expected Cooper-pairing scenario if we make the assumption that the A phase is of the ESP type (since no $S_z = 0$ pairs are formed, the susceptibility should indeed be unreduced from the N–state value), while the B phase is either a spin singlet (even–l) state or the BW state. Of course, such an assumption leaves us with the question why then should there be two different Cooper-paired phases at all, and even more urgently, how any kind of ESP phase can be stable with respect to the BW phase, in clear contradiction to the predictions of the generalized BCS theory.

What struck me as truly extraordinary about the NMR data, however, was the shift of the resonance frequency in the A phase. To see why, let's try making the most obvious interpretation of the Pythagorean formula (2), namely that the system somehow generates a magnetic field $H_o(T) \equiv \omega_o(T)/\gamma$ in a direction perpendicular to the external one, so that the total field in which the spin precesses is $(H_{ext}^2 + H_o^2(T))^{1/2} \equiv \omega_{res}/\gamma$. As emphasized above, the origin of such a field cannot be exchange effects, which although strong cannot give rise to any extra precession. So where could it come from? The atomic electrons in liquid He form inert closed shells, with excitation energies of the order of 50 eV, so any contribution from them at temperatures in the mK range should be utterly negligible. This leaves the nuclear magnetic dipole moments: each ^{3}He nucleus can be thought of as a tiny magnet, with a magnetic moment $\mathbf{M} = \gamma\mathbf{S}$ parallel to its spin, and these magnets should then produce a field just as a macroscopic bar magnet would. Moreover, just as in the case of a bar magnet, the field is in general not parallel to the magnetic moment, i.e. to the spin, and thus could in principle shift the resonance frequency.

So far, so good. However, the problem is the magnitude of the effect. To fit the data to the above scenario, the field $H_o(T)$ would have to be of the order of 30 G at the A–B transition. Now even at the distance of closest approach of two He atoms ($r_o \sim 2.5$ Å) the maximum field exerted by a nuclear spin on its neighbor is less than 1G, and even taking into account the long range (r^{-3}) behavior of the field it seems impossible to envisage a stacking arrangement of the atoms which could give rise to the required value of $H_o(T)$ (quite apart from the fact that such a stacking would be expected both to cost a large amount of energy and to produce dramatic effects on the dc susceptibility).

Indeed, my initial reaction to these result were that they were so extraordinary that they might be the first evidence for a breakdown of some fundamental principle of quantum mechanics (such as the Pauli exclusion principle) under the very exotic conditions characterizing liquid ^{3}He in the mK regime. Given that, it seemed sensible to postpone my planned foray into the conceptual foundation of quantum mechanics until I could be quite sure that quantum mechanics was still actually working! So as soon as Bob had gone home, I sat down (fortunately it was still the Sussex vacation for another few weeks) to try to construct a formal proof that given the generally accepted laws of quantum and statistical mechanics, the shift observed in the experiments simply could not occur.

Now, there are very few things that can be proved rigorously in condensed-matter physics, and most of those refer to the linear-response behavior and are obtained by the use of various sum rules. In the case of the Cornell NMR data, it seemed prima facie plausible that the *rf* field was weak enough to justify a linear response approximation, so I asked myself what information could be extracted from the known sum rules involving the operator which couples to this field, namely the total *x*-component of nuclear spin $\hat{S}_x \equiv \Sigma_i \hat{\sigma}_{xi}$. (The *x*-direction is arbitrarily chosen as that of the (linearly polarized) *rf* field). If, as above, $\chi(\omega)$ denotes the frequency-dependent *rf* susceptibility, then there are two such well-known sum rules:

$$\frac{1}{\pi} \int_o^\infty \frac{Im\ \chi(\omega)}{\omega}\ d\omega = \chi_o. \tag{4}$$

$$\frac{1}{\pi} \int_o^\infty \omega Im\ \chi(\omega) d\omega = -<[\hat{S}_x[\hat{S}_x, \hat{H}]]>_o \tag{5}$$

where χ_o denotes the *dc* susceptibility and $\hat{H}$ the total Hamiltonian of the system in the absence of the *rf* field; the angular brackets denote the expectation value taken with respect to the unperturbed thermal equilibrium state, i.e., with density matrix proportional to $exp\ (-\hat{H}/k_BT)$. The first sum rule, (4), is just a special case of the Kramers-Kronig relation, while the second, (5), is the analog of the well-known *f*-sum rule of atomic physics and is obtained straightforwardly by writing down the second-order perturbation theory expression for $\chi(\omega)$ and evaluating the double commutator on the right-hand side explicitly. (c.f. e.g., ref. [20], section 2.2, for the corresponding procedure in the parallel case of the density sum rule).

The two sum rules (4) and (5) are quite generally valid. Now let us assume that the experimentally observed *rf* absorption is indeed a measure of the imaginary part of the linear *ac* susceptibility $\chi(\omega)$, and use the fact that the former is observed (in all three phases N, A, B) to be proportional to a delta-function ($\delta(\omega - \omega_{res})$), where however the resonance frequency ω_{res} is in general a function of the phase and of temperature.[7] Substituting this form of $Im\chi(\omega)$ into eqns. (4–5), we immediately obtain an expression for ω_{res}:

$$\omega_{res}^2 = -\chi_o^{-1}\ \langle[\hat{S}_x, [\hat{S}_x, \hat{H}]]\rangle_o \tag{6}$$

The double commutator has a simple physical significance: Imagine that we rotate the whole nuclear-spin system uniformly by a small angle θ_x around the *x*–axis (axis of the *rf* field), keeping the orbital variables fixed. By using the fact that the operator which generates such a rotation is $exp\ (i\hat{S}_x\theta_x)$, and that the quantity $<[\hat{S}_x, \hat{H}]>$ must be zero in thermal equilibrium, we find that

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[7] There is of course an implicit assumption here, namely that there are no contributions to the absorption which are too diffuse to be picked up in the experiments.

the double commutator is nothing but the negative of $\partial^2 < \hat{H} > / \partial \theta_x^2$, the second derivative of the mean energy under such a rotation.

Suppose, first, that apart from the Zeeman energy in the external field, $\mu_n \Sigma_i S_{zi} H$, the only spin-dependent energies in the problem are spin-conserving, i.e., they depend only on the magnitude of the total spin **S**, not on its direction; this is the case, in particular, for energies of "exchange" origin. Such terms clearly cannot contribute to the double commutator, which then turns out to be simply equal to $\gamma^2 S_z H_{ext} \equiv \gamma^2 \chi_o H_{ext}^2$; thus in this case the resonance is uniquely constrained to lie exactly at the Larmor frequency $\omega_L \equiv \gamma H_{ext}$. Thus, as already stated, any shift of ω_{res} away from the Larmor value, such as occurs in the A phase, is unambiguous evidence for the operation of some spin-non-conserving energy. In liquid ^{3}He, as we have already seen, the only known such energy is the nuclear dipole-dipole interaction, which has the standard form

$$\hat{H}_D = \mu_n^2 \sum_{ij} (\hat{r}_{ij}^{-3}) \left\{ \hat{\boldsymbol{\sigma}}_i \cdot \hat{\boldsymbol{\sigma}}_j - \frac{3 \hat{\boldsymbol{\sigma}}_i \cdot \mathbf{r}_{ij} \hat{\boldsymbol{\sigma}}_j \cdot \mathbf{r}_{ij}}{r_{ij}^2} \right\} \qquad (r_{ij} \equiv | \mathbf{r}_i - \mathbf{r}_j |) \qquad (7)$$

Evidently this expression is not invariant under rotation of the spins alone, without simultaneous rotation of the orbital coordinates, so in principle it can contribute to the double commutator in eqn. (6). In fact, this equation becomes explicitly:

$$\omega_{res}^2 = \gamma^2 \mathcal{H}_{ext}^2 + \chi_o^{-1} \partial^2 \langle H_D \rangle / \partial \theta_x^2 \qquad (8)$$

Comparing this expression with the experimental result, eqns. (2) and (3), we find that in the A phase we must have

$$\frac{\partial^2 \langle H_D \rangle (T)}{\partial \theta_x^2} = K(1 - T/T_A) \quad K \cong 10^{-3} ergs/cm^3 \qquad (9)$$

while in the N and B phases the quantity $\partial^2 \langle H_D \rangle / \partial \theta_x^2$ must be zero within the accuracy of the experiment.

Now, it is intuitively rather clear that the quantity $\partial^2 \langle H_D \rangle / \partial \theta_x^2$ cannot be larger in order of magnitude than $\langle H_D \rangle$ itself. Thus, the A-phase shift in the resonance frequency can be explained – but only if the expectation value of the nuclear dipole energy in that phase is of the order of magnitude (9). But how can this be? The maximum possible value (call it g_D) of the dipolar energy of a single pair of spins i, j, obtained by setting r_{ij} in (7) equal to the distance of closest approach (~ 2.5 Å) is only about $10^{-7} K$. Now it is true that if one multiplies g_D by the average density n (which should be a crude measure of the number of pairs which closely approach one another) one gets a quantity of the order of 1 erg/cm³, more than enough to account for the value (9); in principle, there is enough dipole energy available! The problem, however, lies with the thermal disorder: Given that two ^{3}He nuclei have an orbital separation $\mathbf{r}_{ij}$, the energy advantage of a "right" spin orientation (spins parallel to $\mathbf{r}_{ij}$) over a "wrong" one (spins perpendicular to $\mathbf{r}_{ij}$) is at most only $\sim g_D$, so

TABLE I. Analogy between SBSOS and Ferromagnetism

	FERROMAGNET	LIQUID ^{3}HE
	$\hat{H} = \hat{H}_o + \hat{H}_z$	$\hat{H} = \hat{H}_o + \hat{H}_D$
	invariant under simult. rotation of all spins	invariant under relative rotation of spin + orbital coordinate systems
	$\hat{H}_z = -\mu_B H \sum_i S_{zi}$ (extl. field)	$\equiv \mu_o \mu_n^2/r_o^3$ $\hat{H}_D = g_D \sum_{ij} \left(\dfrac{\sigma_i \cdot \sigma_j - 3\sigma_i \cdot \hat{r}_{ij}\, \sigma_j \cdot \hat{r}_{ij}}{(r_{ij}^3/r_o^3)} \right)$
	breaks spin-rot^n. symmetry	breaks relative spin-orbit rot^n. symmetry
	Paramagnetic phase ($T > T_c$): spins behave independently, kT competes with $\mu_B H$ $\Rightarrow$ polarization $\sim \mu_B H/kT \ll 1 \Rightarrow$ $\langle H_z \rangle \sim N(\mu_B H)^2/kT$	Normal phase ($T > T_A$): pairs of spins behave independently $\Rightarrow$ polarization $\sim g_D/kT \ll 1 \Rightarrow$ $\langle H_D \rangle \sim N g_D^2/kT$
	Ferromagnetic phase ($T < Tc$): $\hat{H}_o$ forces all spins to lie parallel $\Rightarrow k_B T$ competes with $N\mu_B H$ $\Rightarrow \langle S_z \rangle \sim 1 \Rightarrow \langle H_z \rangle \sim N\mu_B H$	Ordered phase ($T < T_A$): $\hat{H}_o$ forces all pairs to behave similarly $\Rightarrow$ kT competes with $N g_D$ $\Rightarrow \langle H_D \rangle \sim N g_D$ $\sim 10^{-3}$ ergs/cm^3 !

Table 1. Analogy between SBSOS and Ferromagnetism.

even in a classical model the preference for "right" orientations over "wrong" ones should only be a factor of order $g_D/k_B T$; this then gives an expectation value of $\langle H_D \rangle$ of order $ng_D^2/k_B T$, which is too small to account for (9). Actually, the situation is worse than this, since in a degenerate Fermi liquid such as ^{3}He the "polarizability" of the (pairs of) spins by the dipole interaction (as by an external field) is governed not by the thermal energy $k_B T$ but by the much larger Fermi energy $k_B T_F$, so that the resulting value of $\langle H_D \rangle$ is only of order $ng_D^2/k_B T_F$. An explicit calculation of $\langle H_D \rangle$ in the Fermi-liquid model of the normal phase confirms this estimate, showing that the associated contribution of the dipole forces to the right-hand side of eqn. (8) is too small for the resulting shift to be seen experimentally, as is indeed the case.[8]

Where could this argument possibly go wrong? I worried over it day and night for a couple of weeks, and then the solution suddenly hit me: it is an idea which I called "spontaneously broken spin-orbit symmetry", or SBSOS for short. It is most easily explained by analogy with the more familiar case of a magnetic system described by the (isotropic) Heisenberg Hamiltonian, the explicit correspondence is given in Table I. The crucial point, in the magnet-

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[8] Much more recently, a shift of the predicted order of magnitude in the N phase has in fact been seen, [21], although it is not clear that its detailed properties agree with theoretical expectations.
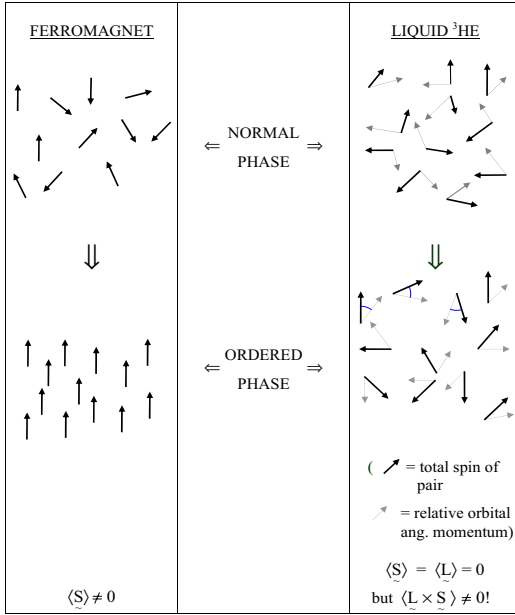
Figure 3: A comparison between ferromagnetism and SBSOS.

ic case, is that in the paramagnetic phase the competition of the thermal energy k_BT is with the <u>single-spin</u> Zeeman energy g_Z; since for reasonable fields we have $g_Z \ll k_BT$, the degree of polarization is small ($\sim g_Z/k_BT \ll 1$) and the expectation value of the total Zeeman energy is $\sim Ng_Z^2/k_BT$, i.e., second order in g_Z (N is the total number of spins). However, in the ferromagnetic phase, the <u>isotropic</u> (Heisenberg) part of the Hamiltonian, $\hat{H}_o$, while it cannot pick out a direction in space, nevertheless forces all (or most of) the spins to lie in <u>the same</u> direction. Now, instead of each spin having to choose individually to lie parallel or antiparallel to the field (corresponding to energies differing by g_Z) we need to choose (crudely speaking) between all spins lying parallel or all lying antiparallel; the energy difference between these two configurations is Ng_Z rather than g_Z, and it is with this large energy that the thermal energy k_BT has to compete. Since typically $Ng_Z \gg k_BT$, the polarization is almost 100%, and the resulting value of the total Zeeman energy is $\sim Ng_Z$, i.e., linear rather than quadratic in g_Z.

In a similar way, we may suppose that in the normal phase of liquid ^{3}He the different <u>pairs</u> of nuclear spins behave more or less independently; then, as sketched above, it is the single-pair energy dipole, g_D, which competes with the thermal energy k_BT, the degree of "polarization" is $\sim g_D/k_BT \ll 1$, and the resulting expectation value $\langle H_D \rangle$ is $\sim Ng_D^2/k_BT$. Now however suppose that in the A phase (on whose specific nature we do not at the moment speculate) the effect of the <u>spin-conserving</u> terms in the energy (kinetic energy, van der Waals potential energy etc.) is such that, while not favoring any particular configuration of the nuclear spins relative to the orbital coordinates, it forces

all pairs to have <u>the same</u> configuration. Then, crudely speaking, we are forced to choose between a configuration in which all pairs have the "right" configuration, and one in which they all have the "wrong" one. The corresponding energy difference is now not g_D but Ng_D, so that provided that $Ng_D \gg k_B T$ (or actually $\gg k_B T_F$), as is certainly the case in practice, the "degree of polarization" of the pairs is of the order of 1 and the resulting value of $\langle H_D \rangle$ can in principle be of order Ng_D, as required by the experimental value of the constant K in eqn. (9).

It is worth emphasizing that the concept of SBSOS is in some ways more subtle than the analogous one of the breaking of rotational symmetry in a ferromagnet. In particular, as illustrated in figure 3, it is entirely compatible with the total "spin of the pairs" $\mathbf{S}_{pair}$ and their relative orbital angular momentum $\mathbf{L}_{pair}$ both having expectation value zero (although in general quantities like $\langle (\mathbf{L} \times \mathbf{S})_{pair} \rangle$ may be non-zero). Even in cases in which $\mathbf{S}_{pair}$ is nonzero,[9] this need not automatically of itself imply that the total spin $\mathbf{S}$ of the system is nonzero; $\mathbf{S}_{pair}$ is a measure of the spin <u>correlations</u> of those pairs of atoms which are close in space. It is this consideration, incidentally, which explains the failure of the simple argument, above, which purports to show that the nuclear dipole moments cannot possibly produce the required "Pythagorean" field to explain the A-phase resonance shift: Consider the equation of motion of the total spin of the system in the presence of the external field and the dipole interaction. This has the form

$$\frac{d\mathbf{S}}{dt} \equiv \frac{d}{dt} \sum_i \mathbf{S}_i = \gamma \sum_i \mathbf{S}_i \times \underset{\sim}{\mathcal{H}}_i \tag{10}$$

$$\underset{\sim}{\mathcal{H}} \equiv \underset{\sim}{\mathcal{H}}_{ext} + \underset{\sim}{\mathcal{H}}_d, \quad \mathcal{H}_{d\alpha} \equiv \sum_j f_{\alpha\beta}(\mathbf{r}_{ij}) S_{j\beta} \tag{11}$$

where α and β indicate Cartesian components of the spin values. Eqn. (10) is a quantum-mechanical (operator) equation of motion, and we need to take the expectation values of the quantities on the two sides:

$$\frac{d\langle S \rangle}{dt} = \gamma \left\{ \langle \mathbf{S} \rangle \times \underset{\sim}{\mathcal{H}}_{ext} + \sum_i \langle \mathbf{S}_i \times \underset{\sim}{\mathcal{H}}_i \rangle \right\} \tag{12}$$

The argument given above implicitly assumes that we can make the approximation $\langle \mathbf{S}_i \times \underset{\sim}{H}_i \rangle \cong \langle \mathbf{S}_i \rangle \times \langle \underset{\sim}{H}_i \rangle$ in which case the maximum possible value of any precessional frequency associated with the dipole forces is indeed of the order of γ times the maximum value of $\langle \underset{\sim}{H}_i \rangle$, i.e., $\sim 1\,G$. In a state without SBSOS (and at temperatures such that $k_B T \gg g_D$) the "mean-field" approximation (13) is very good; however, in the presence of SBSOS it fails dramatically, and in fact the second term on the RHS of eqn. (12) can have a substantial value even when the total spin polarization $\mathbf{S} \equiv \Sigma_i \mathbf{S}_i$ is very small.

[9] e.g., the A_1 phase (see e.g., ref. [22] section XIII).

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Given, then, that the shift in the resonance frequency in the *A* phase of liquid ³He shows unambiguously that this phase must possess the property of SBSOS, what kind of microscopic model could give rise to this property? As a matter of fact there are several, including the crystalline antiferromagnetic phase subsequently shown experimentally [17] to exist in <u>solid</u> ³He below ~ $1\,mK$. However, in the light of pre-existing theoretical speculations the most obvious possibility is a spin triplet Cooper-paired phase (which must automatically have $l \neq 0$). We already know, from the unchanged susceptibility, that if it is indeed a Cooper-paired phase it must be of the ESP type, in which only ↑↑ and ↓↓ pairs form. For such a state, the dipole forces favor an orbital configuration in which the relative orbital separation of the correlated pairs is in the *z*–direction rather than the *xy*–plane, and any rotation of the spins away from their original orientation (such as is induced by an *rf* field) will cost energy as required by eqn. (7). In fact, I was able to estimate the quantity $\partial^2 \langle H_D \rangle / \partial \theta_x^2$, obtaining both the order of magnitude and the temperature-dependence required by eqn. (9). This calculation was presented by my Sussex colleague Mike Richards at the LT conference in Boulder, Colorado in August 1972,[10] and published [23] in Physical Review Letters later that year.[11]

By now the Sussex autumn term was under way, and I had little time over the next few months to work actively on the new phases (though I did write a longer paper [24] which explored the concept of SBSOS more thoroughly than had been possible in the letter). The experimental data still posed two very obvious problems. First, assuming that the *A* phase was indeed an ESP Cooper-paired phase, why should such a phase ever be stable with respect to a phase of the BW type in which all three Zeeman substates were populated? (In my Letter I had proposed a couple of tentative and, in retrospect, highly implausible explanations for this). Secondly, what about the mysterious *B* phase? The most obvious explanation, and one certainly consistent with its reduced susceptibility, was that it was indeed the theoretically expected *BW* phase, and most theorists apparently assumed so. However, at the end of my Letter I had remarked that the absence of a frequency shift showed that the *B* phase did not possess the property of SBSOS and hence could not be the BW phase, which would be expected to show SBSOS. (I speculated that it was either an even–*l* (spin singlet) BCS state or something of currently unknown nature). As we shall see, the first part of this argument turned out to be spectacularly wrong. Meanwhile, my SBSOS-based approach to the NMR data had itself not gone unchallenged; an alternative scenario [25] emphasized the anisotropy of the susceptibility in an ESP-type phase, something to which I had given no particular attention. It became increasingly clear that to de-

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[10] There was an amusing glitch because of my initial misreading of the experimental data, which I have recounted in ref. (13) and will not repeat here.

[11] It is interesting, historically, that this was to my knowledge the first calculation of any property of a Fermi superfluid in which it turned out to be necessary to worry about the behavior of the Cooper-pair wave function at distances of the order of the interatomic spacing.

velop a convincing theory of the NMR I needed to go well beyond the simple sum-rule arguments of the Letter, and do a believable calculation of the full microscopic dynamics.

In the early spring of 1973 the problem of the stability of the A phase (and simultaneously of the correct identification of the B phase) was beautifully solved in a seminal paper [26] by Anderson and Brinkman (AB). They took up an idea concerning superfluid ^{3}He which had been proposed prior to the experimental discovery of the new phases, namely that the effective attraction necessary to bind the Cooper pairs in ^{3}He does not simply come from the attractive part of the "bare" van der Waals potential, but has an important contribution also from the exchange of virtual spin fluctuations ("paramagnons", as they had been christened). Crudely speaking, just as in the BCS theory of superconductivity one electron polarizes the ionic lattice, and a second electron then feels the induced polarization and is thereby attracted to the first one (or more accurately to where the first one was in the recent past), so in liquid ^{3}He, with its strong tendency towards ferromagnetism, the spin of one ^{3}He atom would induce a parallel collective spin polarization of the liquid in the vicinity, which would then attract a second atom of the same spin. As pointed out in ref. [27], this mechanism favors pairing with parallel spins ($S = 1$), hence an odd value of l, and correspondingly disfavors even–l (spin singlet) states; it thus goes some way to explaining why the $l = 2$ state which most theoretical work had anticipated does not occur in real liquid ^{3}He. Now, however, AB went further and pointed out a crucial difference between this "spin-fluctuation exchange" mechanism and the "phonon-exchange" mechanism believed to operate in metallic superconductors: in the latter case, the virtual excitation exchanged is an excitation of a system (the ionic lattice) which is <u>different</u> from the objects (the electrons) which are thereby attracted to one another and thereby form Cooper pairs, so that its structure is very insensitive to the onset of pairing in the electron system. By contrast, in the case of liquid ^{3}He, the exchanged spin fluctuation is a collective excitation of precisely these objects which are forming the pairs (the ^{3}He atoms) and thus its structure, and hence the effective attraction itself, is in general modified by the onset of pairing. At first sight it is tempting to dismiss this effect as insignificant, however, AB were able to do a quantitative calculation (later extended [28] in collaboration with Serene) which showed that the effect can under certain circumstances be comparable to the differences in BCS theory between the energies of different pairing configurations. In fact they were able to show that while over most of the P-T phase diagram the BW phase should be stable just as in simple BCS theory, there is a comparatively small region of high pressure and temperature where the spin-fluctuation "feedback" effects render a particular ESP state stable. This conclusion precisely agrees with the experimental phase diagram (obtained soon thereafter by John Wheatley and his group) [29], if the B phase is identified with the BW phase and the A phase with the relevant ESP phase. Remarkably, the particular ESP phase which is favored is none other than the one explicitly investigated by Anderson and Morel in their 1961 paper, (which thus acquired the name of

the Anderson-Brinkman-Morel (ABM) phase). This work constitutes a major qualitative leap beyond the simple BCS theory, and was in my view a crucial contribution to the solution of the puzzle of the new phases.

April 1973 was the spring vacation at Sussex, and at the invitation of Bob Richardson I was able to spend the whole month as a visitor at the Laboratory of Atomic and Solid State Physics at Cornell. This month was without doubt the single most exciting month of my more than 40-year academic career: I was alone, and was able to spend sixteen hours a day, seven days a week, thinking about the microscopic basis of the NMR data. I spent much of the time commuting between the basement of Clark Hall, where the experiments were ongoing, and the sixth-floor offices of the theory group, who were of course equally excited about the problem and eager to exchange ideas about it.

The theory I came up with [30] during this month can actually be viewed as a natural generalization of my long-forgotten work on the "internal Josephson effect" in a two-band superconductor. Consider an ESP triplet state: the two spin configurations ($\uparrow\uparrow$ and $\downarrow\downarrow$) are then exactly analogous to the pairs formed, in the superconducting case, in the two bands, and the analog of the number difference ΔN is simply S_z, the z-component of the total (not the Cooper-pair!) spin of the system. What is the analog of the phase difference $\Delta\phi$ between the pairs in the two bands? If we consider for notational simplicity an ABM type state, so that the orbital wave function factors out, the spin state is of the form of a superposition:

$$\Psi_{pair} \sim a|\uparrow\uparrow\rangle + b|\downarrow\downarrow\rangle \equiv a|S_z = +1\rangle + b|S_z \equiv -1\rangle \tag{14}$$

so $\Delta\varphi \equiv arg(b/a)$. But for a spin-1 system, a change in the relative phase of the $S_z = 1$ and $S_z = -1$ components of the wave function is just equivalent to a rotation of the spin coordinates (or equivalently to the (inverted) rotation of the spin itself) around the z-axis. Hence, up to a factor of 2, $\Delta\varphi$ is just the angle θ_z of such a rotation. By the same arguments as used in ref. [14], one would then expect that S_z and θ_z satisfy the commutation relation

$$[S_z, \theta_z] = i \tag{15}$$

However, there is nothing special about the z-axis. One would therefore expect[12] that if **S** denotes the total spin vector and the vector **θ** specifies the operation of rotation through an angle $|\theta|$ about the axis $\hat{\theta}$, then we should have the generalized commutation relation, valid for an arbitrary pairing state,

$$[S_\alpha, \theta_\beta] = i\delta_{\alpha\beta} \tag{16}$$

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[12] The derivation of the dynamics in the original paper uses the standard $\underline{d}$ vector notation (see e.g., ref. [22], section (XI. D.)) and is rigorous. How I use a somewhat less precise but hopefully intuitive notation and argument.

Now comes the crunch: The commutation relations (16), while rigorous, will lead us to a useful dynamics only if the effective Hamiltonian for the latter can be expressed in terms of **S** and **θ** alone. Is this possible? I argued that it is, for the following reason: We know from experiment that the characteristic frequency associated with the dipole forces[13] (the quantity $\omega_0(T)$) is small compared to the other obvious characteristic frequencies in the problem, namely the "gap frequency" $\Delta(T)/\hbar$ and the (N-phase) quasiparticle relaxation rate τ^{-1}. Thus, during the NMR all the "microscopic" degrees of freedom (e.g., the distribution of normal quasiparticles, or the configuration of the Cooper pairs apart from their overall spin orientation) should follow the macroscopic degrees of freedom **S** and **θ** adiabatically, and one can make a sort of "Born-Oppenheimer" approximation in which the effective Hamiltonian is simply the minimum value of the (free) energy for the given values of those two variables.

Now the only energy, apart from the Zeeman term, which depends on the overall rotation angle of the nuclear spin system is the dipolar energy $H_d(\theta)$, which can be calculated explicitly as a function of θ for any given assumed Cooper-pair configuration (e.g., the ABM phase).[14] As to the **S**-dependent energy, it consists of the Zeeman energy plus a "polarization" energy which in a degenerate Fermi liquid has contributions from kinetic-energy (Pauli-principle) and "exchange" effects; in any event, for a given value of **S** the minimum value of this energy is simply $\frac{1}{2}\gamma^2\chi_0^{-1}\mathbf{S}^2$ where χ_0 is the static spin susceptibility (which is in general a function of temperature, etc.) Thus the total effective Hamiltonian in the Born-Oppenheimer (adiabatic) approximation takes the simple form

$$H(\mathbf{S}, \boldsymbol{\theta}) = \frac{1}{2}\gamma^2\chi_0^{-1}\mathbf{S}^2 - \gamma\mathbf{S}\cdot\mathbf{H}(t) + H_D(\theta) \tag{17}$$

(where $\mathcal{H}$ is the total external magnetic field, that is in general the sum of the dc field and the rf one). From eqns. (16) and (17) we immediately obtain the equations of motion of **S** and θ:

$$\frac{d\mathbf{S}}{dt} = \mathbf{S} \times \mathcal{H}(t) - \frac{\partial H_D}{\partial\boldsymbol{\theta}} \tag{18a}$$

$$\frac{d\boldsymbol{\theta}}{dt} = \mathcal{H}(t) - \chi_0^{-1}\mathbf{S} \tag{18b}$$

Note that in thermal equilibrium $d\boldsymbol{\theta}/dt$ is zero as it should be. Although strictly speaking the quantities **S, θ** appearing in eqns. (18) are quantum me-

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[13] In high fields the Larmor frequency may exceed τ^{-1}, but this does not matter since the relevant motion is simply uniform rotation of the spin system.
[14] It is implicitly assumed that the reference (equilibrium) state is the one which minimizes the expectation value of H_D.

chanical operators, once one has got to them it is an excellent approximation, under all conditions realized to date in liquid ^{3}He, to treat them semi-classically, i.e., to take the expectation value of eqns. (11) and replace $\langle \partial H_D/\partial \boldsymbol{\theta}\rangle$ by $(\partial/\partial \langle\boldsymbol{\theta}\rangle) H_D(\langle\boldsymbol{\theta}\rangle)$, thereafter treating $\langle \mathbf{S}\rangle \equiv \mathbf{S}$ and $\langle\boldsymbol{\theta}\rangle \equiv \boldsymbol{\theta}$ as classical variables.[15]

The equations of motion (18) should be exact within the Born-Oppenheimer approximation; it is clear that they are conservative and thus cannot account for the finite linewidths observed experimentally, but in 1973 I left that question for future work.[16] In principle they can be solved for any motion of the spin system which respects this approximation, whether or not it is linear in the rf field. In the linear case, which was at the time the most urgent for the interpretation of the existing experiments, it turns out that in the standard "transverse" geometry the values of the resonance frequencies (there may be more than one of them) are determined by the eigenvalues of the tensor quantity,

$$\Omega_{ij}^2 \equiv \partial^2 \langle H_D\rangle /\partial\theta_i\partial\theta_j \qquad\qquad (19)$$

Since the form of this tensor is characteristic of the particular pairing state assumed, we see that <u>the experimental NMR behavior is a "fingerprint" of the pairing state.</u> In particular, let us assume for definiteness $l = 1$ pairing and consider the three most frequently discussed states. For the ABM state, the form of the tensor $\hat{\Omega}$ is such that only a single resonance line is predicted in the NMR absorption, with a frequency which is correctly given by eqn. (31) as derived from the sum rules.[17] For the "axial" state, by contrast, the linearized versions of eqns. (18) predict <u>two</u> resonance lines, with a splitting which decreases rapidly with the dc external field and would have thus been invisible in the experiments existing in April 1973, but should be easily visible at lower fields. Although by this time it had already been shown [33],[26], that within the framework of a generalized Ginzburg-Landau theory the axial phase is never thermodynamically stable, the above observation suggested that it would be desirable to confirm, by NMR "fingerprinting" at lower dc fields, that the A phase shows no splitting of the resonance at any field, and such experiments were rapidly initiated both at Cornell and by Doug Osheroff at Bell Labs, to which he had by that time moved after completing his doctorate.

As regards the BW phase, the predictions made by the linearized versions of eqns. (18) are very intriguing. It turns out that the determination of the correct form (orientation) of the equilibrium state is quite delicate: The $^3P_0$

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[15] For a discussion of the NMR behavior under conditions where the semiclassical approximation might fail, see ref. [31].

[16] In fact, in later work with S. Takagi [32] I was able to generalize eqns. (18) so to produce an account of the dissipation.

[17] Strictly speaking this is true provided we assume that the so-called d-vector of lies perpendicular to the direction of the rf field. (There is a subtlety connected with eqn. (4) which there is no space to discuss here.)

state originally considered by Vdovin and by BW does not minimize the nuclear dipole energy, and in fact to do this is necessary to rotate the spin coordinates through an angle of $\cos^{-1}(\frac{1}{4}) = 104°$ relative to the orbital one. However, in zero dc external field it is obvious from the isotropy of the $^3P_0$ state that the axis $\hat{\boldsymbol{\omega}}$ of this rotation is completely arbitrary. A finite external field along the z-axis breaks this degeneracy by depopulating, very slightly, the $S_z = 0$ Zeeman component of the pair state relative to the $S_z = \pm 1$ components, and it turns out that this means that it is energetically advantageous to choose $\hat{\boldsymbol{\omega}}$ to be along the (positive or negative) z-axis. The relevant energy, while it thus determines the equilibrium state, is sufficiently small that, at least at first sight, it does not have to be taken into account explicitly in the dynamics, which can thus be calculated by assuming a "pseudo-isotropic" state, that is, one obtained from the (truly isotropic) $^3P_0$ state by rotating the spins relative to the orbital coordinates through $104°$ around the z-axis. Now the structure of the tensor $\hat{\Omega}_{ij}$ in (eqn. (19) for this state is rather striking: A small rotation of the nuclear spin system away from the equilibrium configuration around any axis in the xy-plane corresponds, up to the relevant order, simply to a change in the axis of rotation while keeping the angle fixed, and therefore brings into play only the very small energy which stabilizes $\hat{\Omega}$ in the z-direction; at first sight, at least, it is consistent to neglect this,[18] and the transverse resonance therefore remains at the Larmor value as in the normal phase. On the other hand a rotation around the z-axis involves a change of the angle of the spin-orbit rotation away from the equilibrium value of $104°$, and therefore should bring into play a dipole energy of the same order of magnitude as seen in the A-phase transverse resonance.

But how to see this effect? The answer is to look for a "longitudinal" resonance, that is, a finite-frequency resonance in the absorption spectrum of an rf field polarized parallel rather than perpendicular to the dc field. (In the normal phase, as in any phase lacking SBSOS, this absorption spectrum is simply a diffusive peak centered on zero frequency). The presence of such a resonance, at a frequency which could be estimated with some confidence on the basis of the "calibration" of the dipole energy from the A-phase shift, should when coupled with the absence of a transverse shift identify the B phase almost uniquely as the BW state. (A similar longitudinal resonance is predicted to occur in both the ABM and axial phases).

Although initially neither the Anderson-Brinkman theory of the stability of the ABM phase nor the spin dynamics which I had developed during my stay at Cornell found universal acceptance (cf. ref. [11]), by the late summer of 1973 the smoke had cleared suffciently that most researchers (including me) were convinced that both were correct and that the existing experimental data were entirely consistent with the identification of the A phase as ABM and the B phase as BW. The clinching NMR observations, of a lack of splitting of the A-phase transverse resonance at low fields [35] and of a longitudinal res-

[18] Though cf. ref. [35].

onance at roughly the predicted frequency in the B phase [34], came in within a year or so, and from then on these identifications, while occasionally challenged in the literature, appear to have stood the test of time, and are by now standard textbook material. It should of course be added that the hypothesis of Cooper pairing leads to many other predictions, above all that of superfluid behavior, and that this and the vast majority of other predictions are by now very well confirmed experimentally (see for example the comprehensive text [36] by Vollhardt and Wölfle).

Thirty years later, following the award of the Nobel prize for my participation in these events, one question which I am frequently and legitimately asked by journalists and others is "What is superfluid ^{3}He good for?" Particularly when standing next to my UIUC colleague and co-laureate Paul Lauterbur, whose MRI research has obviously been of such direct and immediate benefit to mankind, I find this question somewhat embarrassing, since in the most direct and practical sense the most honest answer is: nothing whatever! Helium is the most chemically inert of the elements; its application best known to the general public is probably the filling of balloons at children's parties, and its main scientific application is in cryogenics, an area in which the (rare and expensive) light isotope ^{3}He cannot compete with its much more readily available cousin ^{4}He. If this were not enough, the superfluid phases of ^{3}He occur only at less than one hundred-thousandth of room temperature, a regime which is hardly conducive to most kinds of practical application. So while it is conceivable that the "superfluid amplification" property to be discussed below may in the future be put to use in metrology (the determination of the fundamental constants), from all other points of view superfluid ^{3}He may well be the most practically useless system ever discovered.

If we take a broader view, however, and content ourselves with indirect applications, the picture is much rosier. With the arguable exception of the "fractional quantum Hall" systems discovered ten years later, the superfluid phases of liquid ^{3}He are probably the most sophisticated physical systems of which we can claim a quantitative understanding, showing a subtlety of correlation unprecedented in all of known physics; and the lessons learned from them have been very widely applied elsewhere, both in condensed matter physics (for example to the cuprate superconductors, which like ^{3}He are believed to form Cooper pairs in an "exotic" (non-s-wave) pairing state), and in particle physics and cosmology; indeed, whole books(e.g., ref. [37]) have been written on the analogies between various phenomena known experimentally to occur in superfluid ^{3}He and some postulated in particle physics and/or the cosmology of the early universe. A second area in which the uniquely rich structure of the order parameter (pair wave function) of superfluid ^{3}He has had fruitful consequences is in studies of chaos and turbulence, and particularly of the way in which topological defects in the order parameter are generated in quenching through a phase transition (a process which is in fact frequently regarded as a model for processes believed to occur in the early universe).

However, to my mind the single most exciting property characteristic of the superfluid phases of ^{3}He is the "superfluid amplification" associated with the existence of SBSOS. By "superfluid (or Bose) amplification" I mean that the fact that all the Cooper pairs have to behave identically has the consequence that tiny effects which in a normal system would be completely quenched by thermal disorder may become spectacularly visible. In itself this property is of course not peculiar to ^{3}He; in a BCS superconductor the Josephson effect and, less obviously, the Meissner effect itself can be regarded as illustrations of this behavior. What is new and unique[19] about the superfluid ^{3}He system is the fact that the Cooper pairs have one or more nontrivial internal (orientational) degrees of freedom, and must all behave identically with respect to these as well as to the center-of-mass motion (i.e., most show the property of SBSOS). As a result, the phenomenon of superfluid amplification occurs also in properties associated with the internal (relative) motion. We already saw one striking example of this in the NMR behavior; I would like to close with a second potential application of this principle which, if it can be realized experimentally, should be even more spectacular.

Unlike the gravitational and electromagnetic interactions, which have direct and obvious manifestations at the everyday (macroscopic) level, the weak interaction postulated in particle physics has so far manifested itself directly only at the microscopic level. One striking property of this interaction which distinguishes it from the other known interactions (gravitational, electromagnetic and strong) is that it violates the principle of invariance under spatial inversion (P); the consequences of this violation have been seen in scattering experiments, and more recently in the optical behavior of heavy atoms. It is interesting to ask: Is it possible to see the effects of this P-violation at the <u>macroscopic</u> level? In discussing this question it is important to bear in mind that, except for a very small "CP-violating" component (of order 10^{-3} of the total), the weak interaction respects the principle of invariance under time reversal (T). As a result, if we ignore for present purposes the small CP-violating component, the weak interaction cannot give rise to an electric dipole moment (EDM) on any "simple" quantum system (elementary particle, atom or molecule). The reason is as follows: by the Wigner-Eckart theorem, such an EDM (**d**) would have to be proportional to the only vector quantity characterizing this system, namely the total angular momentum **J**. But the ansatz **d** = const. **J** evidently violates T as well as P, and hence (in the absence of CP-violation) is unviable.

Consider however the Cooper pairs in the (pseudo-isotropic) B phase of superfluid ^{3}He. Because of the nonlinearity of the generalized BCS equation which describes the formation of pairs, the pseudo-isotropic state (as distinct from the original $^3P_0$ state of BW and Vdovin) is not characterized by a well-defined angular momentum **L**+**S** ≡ **J**. However, it does possess a characteris-

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[19] At least until the recent advent of BEC alkali gases with a hyperfine degree of freedom.

tic vector which has a finite expectation value in the ground state, namely the vector $\langle \hat{\mathbf{L}} \times \hat{\mathbf{S}} \rangle$, which turns out to be directed along the "spin-orbit rotation axis" $\hat{\omega}$ (see above). Since both **L** and **S** are axial vectors and odd under time reversal, the ansatz

$$\mathbf{d} = \text{const.}\ \langle (\mathbf{L} \times \mathbf{S}) \rangle$$

violates P but not T, and is therefore allowed by symmetry even in the absence of CP-violation.

A calculation [38] of the order of magnitude of the EDM expected to be produced by the weak interaction in ^{3}He-B indicates that it involves a number of dimensionless factors which are each individually small in this system, and that as a result the EDM on a single Cooper pair is something like ten orders of magnitude smaller than the smallest EDM which would be visible (if realized on, e.g., the neutron) in current beam experiments. However, now comes the crunch: because of the property of SBSOS, all the Cooper pairs must have exactly the <u>same</u> value of $\langle \mathbf{L} \times \mathbf{S} \rangle$ and hence of **d**, so that the total EDM of the system is in principle macroscopic! Although calculations indicate it is still extremely small, it is not obvious beyond the reach of existing measurement techniques, and Doug Osheroff at Stanford is currently building an experiment to look for it. If it can be seen, it will be the first ever example of a <u>direct</u> manifestation of the weak interaction, and in particular of its characteristic property of P-violation, at the level of everyday life.

In conclusion I would first like to thank those who did the hard work involved in nominating me for the Nobel prize, and the Physics Committee of the Royal Swedish Academy of Sciences for the even harder work which eventually resulted in my selection; I am very conscious that I am only one of a number of researchers whose work both before and after the experimental discovery helped to clarify the nature of the new phases, and in this context it is impossible not to mention one name in particular, that of Phil Anderson, who with various collaborators contributed so many vital insights during these years and later. Secondly, I would like to thank all those who in one way or another helped me along the path which has led me to stand here today. In particular I am grateful to the late Fr. Charles O'Hara for introducing me to the marvels of modern mathematics and giving me the confidence that I could do it if I ever had to; to David Brink and Michael Baker for accepting me, a classics graduate with zero formal training in physics, to do a second undergraduate degree in that subject, and to the Fellows of Merton College, Oxford, for providing the means for me to do it: to the late Dirk ter Haar for accepting me–still the proverbial dark horse–to do an advanced degree in theoretical physics, and to the faculty of Magdalen College, Oxford, for electing me to a fellowship which considerably more than kept body and soul together while I did it and thereafter; and to David Pines for choosing me as his postdoc at University of Illinois at Urbana-Champaign, and for his encouragement in my first ventures into the area of superfluid ^{3}He. I owe special thanks to Bob Richardson, Doug Osheroff, Dave Lee and Willy Gully for their

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