A Critique of Two Metals

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I argue that Anderson’s identification of the conflict between the fermi-liquid and non-fermi-liquid metallic states as the central issue of cuprate superconductivity is fundamentally wrong. All experimental evidence points to adiabatic continuability of the strange metal into a conventional one, and thus to one metallic phase rather than two, and all attempts to account theoretically for the existence of a luttinger-liquid at zero temperature in spatial dimension greater than 1 have failed. I discuss the underlying reasons for this failure and then argue that the true higher-dimensional generalization of the luttinger-liquid behavior is a propensity of the system to order. This implies that the central issue is actually the conflict between different kinds of order, i.e. exactly the idea implicit in Zhang’s paper. I then speculate about how the conflict between antiferromagnetism and superconductivity, the two principal kinds of order in this problem, might result in both the observed zero-temperature phase diagram of the cuprates and the luttinger-liquid phenomenology, i.e. the breakup of the electron into spinons and holons in certain regimes of doping and energy.

The key idea is a quantum critical point regulating a first-order transition between these phases, and toward which one is first attracted under renormalization before bifurcating between the two phases. I speculate that this critical point lies on the insulating line, and that the difference between the Mott-insulator and fermi-liquid approaches to the high-$T_c$ problem comes down to whether or not the superconducting states made by n- and p-type doping can be continued into each other. A candidate for the second fixed point required for distinct superconducting phases is the P- and T-violating chiral spin liquid state invented by me.

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In a recent paper Baskaran and Anderson have criticized Zhang’s SO(5) theory of cuprate superconductivity on various microscopic grounds following the general thinking of Greiter and also on the much more serious grounds that the entire idea of ascribing the behavior of the cuprates to quantum criticality is physically wrong. The right idea, according to them, is that a second kind of metallic state, the luttinger-liquid, is present in the cuprates, and that the strange phenomenology of these materials is due to the presence of this new state of matter. The existence and importance of the non-fermi-liquid state has been the central feature of Anderson’s ideas on cuprate superconductivity from the very beginning, and has had a powerful influence on the development of the subject by virtue of being the only genuinely new idea in the field. But it is now obvious that we have reached an impasse on this matter, and I think the controversy surrounding Zhang’s paper provides a much-needed opportunity to question whether the conflict between the fermi-liquid and the non-fermi-liquid might have been the wrong issue. There are a great many reasons to be worried about this. What is the evidence that the non-fermi-liquid state is actually different from the fermi-liquid in the sense of finite-temperature adiabatic continuability? Why is it so difficult to write down a luttinger-liquid in spatial dimension greater than 1, much less find a Hamiltonian that stabilizes such a state? Why does existence of the luttinger-liquid help identify the cause of cuprate superconductivity? What is the experiment that would resolve the key controversies of the luttinger-liquid state in a definitive way? There is still reason to take Anderson’s phenomenological observations seriously, in particular the interpretation of certain experiments in terms of spinon and holon excitations into which the electron decays, but there are also reasons to suspect that the central issue he identified is not quite right. Zhang’s ideas, which are not completely right either in my view, have had the salubrious effect of articulating an alternate view of the underlying physics, namely the quantum criticality idea Baskaran and Anderson are so quick to dismiss, in a particularly simple and elegant way using equations that everyone can understand. As a result there is now a second important idea on the table, one that I think makes considerably more sense than the luttinger-liquid idea, namely that cuprate phenomenology might be fundamentally due to a conflict between different kinds of order.

The antiferromagnetic and superconducting phases each derive, according to Baskaran and Anderson, from a more fundamental thermodynamic phase, the Mott insulator and the metal, respectively. Let me for a moment defer the question of which metallic state is intended here and concentrate on the existence of the Mott insulator, a paramagnetic spin singlet with an energy gap for charged
excitations and no antiferromagnetic long-range order modeled after the ground state of the Hubbard model at half-filling in 1 spatial dimension. Basakaran and Anderson go further to say that the antiferromagnet is a Mott insulator, and it is an antiferromagnet because it is a Mott insulator, not vice versa; superexchange is a consequence of the insulating state. Unfortunately, ten years of work by some of the best minds in theoretical physics have failed to produce any formal demonstration of the existence of such a state at zero temperature - essential here because everything conducts a little at finite temperature - and dimension greater than 1. Probably the closest anyone came was my own work which produced a state with a spin gap and discrete broken symmetries at the price of long-range interactions, and which had a phenomenology inconsistent with that of the cuprates. Anderson’s views to the contrary, this matters a great deal because one’s inability to back up phenomenological observations with a simple model that is easy to solve and makes sense usually means that an important physical idea is either missing or improperly understood. Another indicator that something is deeply wrong is the inability of anyone to describe the elementary excitation spectrum of the Mott insulator precisely even as pure phenomenology. Nowhere can one find quantitative band structure of the elementary particle whose spectrum becomes gapped. Nowhere can one find precise information about the particle whose gapless spectrum causes the paramagnetism. Nowhere can one find information about the interactions among these particles or of their potential bound state spectroscopies. Nowhere can one find precise definitions of Mott insulator terminology. The upper and lower Hubbard bands, for example, are vague analogues of the valence and conduction bands of a semiconductor, except that they coexist and mix with soft magnetic excitations no one knows how to describe very well.

In light of the magnitude and scope of these problems it is rather ironic that a zero-temperature state with order possessing all of these properties, namely the conventional Hartree-Fock spin density wave, has existed all along and can be written down and explained easily.

Why is it so hard to construct a Mott insulating vacuum that makes sense in 2 or more spatial dimensions when it can be done so readily in 1? I would like to address this question in the context of the pure spin limit of the problem, as the difficulty is exhibited already there, but the meaningfulness of this limit is not obvious and is one of the things we need eventually to address. Consider a spin Hamiltonian of the form

$$
\mathcal{H} = \sum_{<j,k>} J_{jk} \vec{S}_j \cdot \vec{S}_k ,
$$

where \(<j,k>\) denotes a sum over lattice pairs, not necessarily near neighbors, and \(J_{jk}\) is a translationally-invariant Heisenberg exchange interaction of finite range. When the total spin per site is integral it is possible to find exact solutions in any number of dimensions that are legitimate spin liquids, in the sense of having exponentially decaying correlations, an energy gap, and a common-sense relationship between this gap and the correlation length. When the spin per site is half-integral, on the other hand, no such solution has even been found, and such computer work as we have indicates either order or inadequate sample-size convergence, i.e. that the simulation is not large enough to determine one way or the other whether ordering occurs. This fundamental disparity between integral and half-integral spins was anticipated by Lieb, Schultz, and Mattis long before the discovery of high-\(T_c\) superconductivity and is manifested as the Haldane effect in 1 dimension. They introduced the unitary operator

$$
U = \exp \left\{ i \sum_j \frac{2\pi x_j}{L} S_j^z \right\} ,
$$

where \(x_j\) denotes the x-coordinate of the \(j^{th}\) lattice site and \(L\) denotes the sample size, which has the effect of rotating each spin about the z-axis in a way that twists by \(2\pi\) as one advances across the sample. This operator is defined in any number of dimensions, but for the arguments to work properly in dimension greater than 1 it is necessary to imagine a sample that is long and skinny, say 50 light-years wide and \(10^5\) light-years long, and to have an odd number of sites in the plane perpendicular to the long axis. Since \(U\) rotates all the spins in a given region together it is almost a symmetry operator and therefore increases the expected energy by an amount that vanishes as the sample size grows. Denoting the exact ground state by \(|\Psi_0>\), we have specifically

$$
\frac{<\Psi_0|U^\dagger \mathcal{H} U|\Psi_0>}{<\Psi_0|\Psi_0>} - \frac{<\Psi_0|\mathcal{H}|\Psi_0>}{<\Psi_0|\Psi_0>} \propto 1/L^2 ,
$$

where \(L\) denotes the sample length. However, in a half-integral spin system we also have

$$
<\Psi_0|U|\Psi_0> = 0 ,
$$

this following from the minus sign acquired by a spinor when it is rotated by \(2\pi\). So \(U|\Psi_0>\) is exactly orthogonal to \(|\Psi_0>\) when the spin per unit cell is half-integral. Since \(U\) does not conserve total spin, this implies that half-integral spin systems have arbitrarily low-energy excitations in every spin channel and are thus fundamentally infrared-degenerate. This is inconsistent with the energy gap characteristic of a legitimate quantum spin liquid but an expected and necessary consequence of ordering. So the simplest explanation of the computer experiments, the one I believe to be right, is that half-integral spin systems have a powerful propensity to order and do so almost always. The case of 1 dimension is an exception for the simple reason that continuous symmetry breaking is impossible in 1 dimension. The quantum spin liquid in 1 dimension is not a new state of matter at all but a still-born antiferromagnet. The higher-dimensional analogue
of the Haldane effect is the nonexistence of the Mott insulator state at zero temperature.

It is very important to emphasize that this line of reasoning does not contradict any of Anderson’s phenomenological observations, but simply contradicts the deeper physical meaning he assigns to them. As a side effect they also relieve us of a great intellectual burden we should not have been carrying in the first place. When one represents that a distinct quantum phase exists, one is not allowed to adjust the Hamiltonian to make the desired behavior occur. A good modeler starts from the correct equations, computes honestly, and produces plots that match experiment. If, on the other hand, one represents the behavior to be due to proximity to a quantum critical point or line, it is mandatory to adjust the Hamiltonian, as this is the only way to correctly identify the physical principle unifying the behavior. Furthermore it is quite possible for the critical Hamiltonian to be “unphysical” in the sense of containing parameters one would never find in nature. The tasks of demonstrating the existence of a phase and demonstrating the existence of a critical surface have exactly opposite strategies and are mutually incompatible. So the pique shown by Baskaran and Anderson toward the Zhang’s work, particularly the lengths they go to criticize model assumptions, specific values of parameters, computational strategies, and so forth comes down to hostility toward the possibility that the Mott insulator phase might not exist.

It is a tall order for anyone to demonstrate the existence of a phase of matter without finding a transition to it. I cannot, in fact, think of a single instance in which this has been done. If we were considering a system at zero temperature the issue of two metallic phases could be resolved very cleanly, as the Landau quasiparticle either becomes arbitrarily well-defined as the energy scale is lowered or it does not. At zero temperature the luttinger-liquid could be distinguished from the conventional fermi-liquid, and there would have to be a phase phase transition between them. However, in the cuprates the temperature of the ostensible luttinger-liquid phase - the normal state at optimal doping - cannot be lowered to zero because superconductivity intervenes, so this test cannot be applied. This is unfortunate because above the superconducting dome there is no evidence that this state cannot be continued adiabatically into the metallic state at extreme overdoping. The latter is thought by most of us to be conventional. It is possible that a critical point separating the two metallic phases exists at zero temperature and is just covered up by the inopportune occurrence of superconductivity, but I think this is incorrect. It requires the superconductivity to be unrelated to the more important struggle between the two metallic states, and this is inconsistent with the violent change in the carrier scattering rates the superconducting transition found by Bonn et al. in microwave skin conductivity experiments.

The continuability of the strange metal into the nonso-strange one does, in fact, imply that starting from a weakly-interacting fermi sea and summing Feynman graphs makes formal sense, and this implies that fermi-liquid modeling also makes sense. It does not imply that this is a good thing to do, however, because this approach relegates all the strange phenomenology to the category of complicated detail, whereas there is every reason to believe that some as-yet undiscovered physical principle is at work. Anderson has, in fact, made an excellent case for this.

The critical point idea could account for Anderson’s phenomenology quite completely if spinons and holons are the true elementary excitations at the critical point. There are several reasons for thinking this might be true, but all are necessarily indirect because no critical point Hamiltonian with the requisite properties has yet been discovered.

1. If spin-1/2 systems like to order then the critical point is the only place in the phase diagram where order does not occur. Absence of order is a sufficient condition, at least in a pure magnet, for spinons and holons to exist.

2. The two ordered phases in question have Goldstone modes which disperse linearly at long wavelengths. This applies to all Hamiltonians in the basin of attraction of a given phase, including those arbitrarily close to the critical point. The modes in question do not, however, exist as sharp excitations at the critical point itself because there is no physical principle left preventing them from mixing. The critical Hamiltonian must therefore be characterized by a large number of strange low-lying excitations that can be organized by an arbitrarily small perturbation into these modes. This occurs, for example, in the Hubbard model at half-filling, where ordinary electrons are organized by an arbitrarily small Hubbard U into the collective modes of either s-wave superconductivity or antiferromagnetism depending on the sign of U. This is a somewhat unfortunate example because the conventional metal is usually understood to be phase, i.e. an attractive fixed point, which cannot be a critical point by definition. So we must have low-lying excitations that live at the critical point and nowhere else, that involve mixing of the Goldstones of the two phases, and are not conventional particles and holes.

3. There is reason to suspect that the principles of conformal symmetry can be abstracted to critical points in more than 2 dimensions. The assumptions of conformal invariance and dynamical scaling together lead to functional forms for response functions like those of the luttinger-liquid.

4. Phenomenology suspiciously similar to that of the cuprates is observed in heavy-fermion materials under circumstances in which it can be attributed unambiguously to proximity to a critical point.
Let me now proceed to speculate a bit on the nature of the zero-temperature critical point or line which might be responsible for the behavior Anderson has identified. While there are many different kinds of order potentially present in this system the big conflict is obviously between superconductivity and antiferromagnetism. I would therefore like to assume that the critical point in question regulates the transition between these, just as Zhang has done. I will also ignore microscopics and start from the empirical fact that both antiferromagnetic and d-wave superconducting order occur in the cuprates and do, in fact, conflict. This is seen, for example, in experiment in the advance with doping from Ne`el order to "spin glass" to superconductivity without intervention of a normal-metal phase. The spin glass is not a phase but a region of increased sensitivity to disorder, i.e. exactly the kind of thing one would expect to find at a first-order phase transition. The transition from isotropic antiferromagnetism to superconductivity must be first-order because the corresponding order parameters lie in different irreducible representations of the lattice point group. By elevating this conflict to a matter of importance we are, of course, promoting the view that these two kinds of order have the same microscopic cause.

In Fig. 1 I show a model renormalization group flow that might be associated with such a transition. I imagine that the first-order transition extrapolates in Hamiltonian space into a surface, drawn here as a line to emphasize the analogy with the quantum hall transition, terminating at a critical point. The critical Hamiltonian need not be experimentally accessible, and I have accordingly labeled the second axis "X". When the sample is small there is no order and no interesting change to the behavior as doping is varied. As the sample size is increased, however, the low-energy behavior flows toward that of the repulsive fixed point regulating the transition, slows down there, and then finally bifurcates toward the attractive fixed points characterizing the two phases. The first major effect seen as the sample size is increased is therefore not the onset of order but the onset of criticality. It is appropriate to call this "quantum disorder" because the critical point Hamiltonian is the only one that does not renormalize, i.e. does not order. The spectroscopic signatures of the critical point may also be seen in the ordered phases by conducting experiments at intermediate momentum and energy scales, as raising the energy scale is equivalent to making the sample smaller and thus flowing backward in Fig. 1. It has been my view for quite some time that such reverse scaling can be seen in many experiments, for example in the photoemission "spin gap", the strange intermediate-energy phenomenon characteristic of these materials which thus far has no discoverable relevance to the superconductivity itself.

Having identified a reasonable topology for the flow let us now distort it, as shown in Fig. 2, so as to place the critical point at zero doping. Baskaran and Anderson have correctly pointed out that a continuous transition from superconductivity to antiferromagnetism is not expected, even at a point, unless an additional physical principle is at work. The physical effect I propose to exploit is the vanishing of the superfluid density at half-filling. If the carrier density becomes arbitrarily small then so do the superconducting order parameter and the nonzero latent heat it usually necessitates. Locating the critical point at the insulating boundary is also consistent with the great body of RVB work, which always found an RVB vacuum, the obvious prototype for the critical-point ground state, easy to define at half-filling but nearly impossible to define for $\delta \neq 0$. It is also relevant that the RVB states became disreputable precisely because they showed signs of describing a quantum critical point instead of a phase. Consider a wavefunction of the form

$$|\Psi> = P_{\alpha} |\Phi> ,$$

where $\Phi$ is a single-Slater-determinant electron wavefunc-

\[ \text{FIG. 1. Generic renormalization group flow for the proposed first-order line phase transition in the cuprates. X is an as-yet unidentified Hamiltonian parameter that is normally assumed to be zero. } \]

\[ \text{FIG. 2. Modified renormalization group flow for the cuprates that takes into account particle-hole symmetry, distinct superconducting states for n- and p-type doping, and a second fixed point representing a quantum-disordered state of the magnet. Note that } \delta \text{ here means total oscillator strength below the ultraviolet cutoff, not the doping density.} \]
Putting it at the critical point, which of course was the reason for would be required to make the superfluid density vanish the dome in Fig. 3 conducts, we find that a miracle is the key problem with Fig. 3. Since everything above the first one to bifurcate between the n-type and p-type regions. This is required by particle-hole symmetry, such as the Hubbard model in the large-U limit. The physical idea being expressed here is that the system is like a semiconductor, and that the conducting states made by n-type and p-type doping are mirror images of each other because the quantum mechanics of the carriers is the same. It is certainly the case in the experimentally accessible parameter range that the cuprates conduct only when doped and have the violently doping-dependent optical sum rule expected of a semiconductor.

Fig. 2 also has two superconducting states, one each for n-type and p-type doping, which cannot be deformed into each other. This is more controversial. All experiments done to date on the cuprates have found two superconducting states, but it is not clear that they are adiabatically distinct. Indeed many people believe that there is only one superconducting state and that this is continu-able into a BCS state at half-filling. Fig. 3 shows the flow expected if this were the case. I do not think this flow is right, but I include it to make the dichotomy clear. Fig. 2 also differs from Fig. 1 in possessing a second fixed point above the first one to bifurcate between the n-type and p-type regions. This is required by particle-hole symmetry if there are assumed to be two distinct superconducting states, but more importantly by the physical idea that the δ = 0 line is insulating, for then the state to which one flows at δ = 0 cannot be a superconductor but must be something else. The absence of this second fixed point is the key problem with Fig. 3. Since everything above the dome in Fig. 3 conducts, we find that a miracle would be required to make the superfluid density vanish at the critical point, which of course was the reason for putting it at δ = 0 in the first place. The superconductor-antiferromagnet transition would also become synonymous with the metal-insulator transition, as occurs in competition between spin density wave and BCS ground states in a traditional metal, and we would be faced with the old problem of explaining how such a transition could be continuous. Finally, our use of a spin model, such as that of Eq. (1), to motivate our physical thinking would not be justified because it is valid only within the basin of attraction of a phase containing that model. A spin model can obviously never be attracted to a conducting fixed point, so Fig. 3 would imply that no spin model could tell us anything about the critical point, and also that the difference between integral and half-integral spin is irrelevant because it is invisible in the antiferromagnetically ordered state.

Let us now consider the physical nature of the second fixed point. At δ = 0 it is attractive and represents a quantum phase of the pure spin system different from the antiferromagnetically ordered one. This phase must be characterized by some kind of order, as the relevant spin model is half-integral, but the symmetry breaking can be only discrete because the transition to it is second-order. Spin-Peirels order immediately comes to mind, particularly since it is known to occur 1 dimension and to be associated with a scaling diagram similar to the one I have drawn. There is, however, no obvious reason for a spin-Peirels state to be absolutely unstable to d-wave superconductivity when doped, and this is essential for the system to be a superconductor or an antiferromagnet, but nothing else, in the thermodynamic limit. For this reason I favor identifying the second fixed point with the chiral spin liquid, the insulating state characterized by short-range antiferromagnetic correlations, an energy gap, and the P- and T-odd 3-spin order parameter \( \vec{S} \cdot (\vec{S}_j \times \vec{S}_k) \). The chiral spin liquid is absolutely unstable to superconductivity when doped, by virtue of the principles of anyon superconductivity.

It is with great reluctance that I introduce P and T
violation to this discussion. Cuprate superconductors do not appear to violate T and P in the bulk, although they do so at surfaces, and while the absence of such an effect in a real material can be ascribed to the magnetic fields such a state would tend to generate, the fact is that spontaneous breaking of P and T in the bulk has become disreputable because it has not been seen experimentally. Unfortunately the chiral spin liquid is the only insulating vacuum known to be unstable to superconductivity for an identifiable physical reason, and I believe that a reason is required, so I continue to think that this vacuum is right despite the discouraging experimental situation. The implication is that there is a tendency, perhaps only a subtle one, for the cuprates to break P and T spontaneously, and that the conclusion that they do not is incorrect. This would require yet another fixed point regulating the bifurcation of the n-type and p-type superconducting states into right-handed and left-handed versions.

The chiral spin liquid has always had the serious difficulty of requiring long-range interactions to be stable. This is a consequence of the Lieb-Schultz-Mattis theorem, and it applies equally well to the RVB vacuum, although it is less obvious in that case because an antiferromagnetic vacuum with a small moment and a long correlation length is a passable approximation to a state with no order and power-law correlations. However, this problem disappears once the liquid becomes a repulsive fixed point rather than a phase, for then it is no longer distinct from the superconducting state into which it flows, and the requisite long-range forces can be attributed to superfluid order. The chiral spin liquid thus construed is fundamentally different from the quantum-disordered state of integral-spin systems as represented, say, by a nonlinear sigma model, in that it cannot exist in isolation from its adjacent stabilizing superfluid state. The superconductor into which the chiral spin liquid flows has a small $d_{xy}$ order parameter superimposed on the usual $d_{x^2-y^2}$ one with a relative phase of $\pi/2$, so that the fermionic spectrum has an energy gap. This gap measures the amount of T-violation in the ground state and is the experimental signature distinguishing such a state from a conventional d-wave superconductor. The relationship also works in reverse. The action of $P_{\alpha}$ for $\alpha = 1$ on this state at half-filling produces the chiral spin liquid state.

I began this article with the proposition that the idea being defended by Baskaran and Anderson in their attack on Zhang is fundamentally wrong because the luttinger-liquid does not exist as a legitimate state of matter in spatial dimension greater than 1. At the level of interpreting experiments this might be construed as pedantry, for phenomenology based on a critical point is not so different from phenomenology based on a phase if the energy resolution is sufficiently crude. However, this is not right because high-$T_c$ is not the sort of problem in which modeling leads inexorably to understanding. We already know that the superconducting state is deformable into a BCS state, albeit with d-wave symmetry, and we also know that there is essentially no agreement with any of the experimental minutiae usually cited as proof that the BCS theory is correct. So the BCS paradigm is not the right one. High-$T_c$ is more like the strong interactions in that it presents us with an abundance of experimental facts that are difficult to calculate from first principles for known reasons, for are for the most part unimportant, and which require prioritization based on their ability to test questions of principle. In this context a misidentification of the principle is the worst mistake one can possibly make, for it causes unimportant things to be categorized as important and vice versa.

Here is a summary of the important experimental implications of my speculations:

1. There should be two distinct superconducting states, one each for n- and p-type doping, that cannot be deformed into each other without crossing a phase boundary.

2. There should be two and only two electronic phases - superconductivity and antiferromagnetism. Striped phases count as antiferromagnetism.

3. The transition from superconductivity to antiferromagnetism as doping is reduced should be first-order, with no coexistence of the two kinds of order except through phase separation. In other words, the two phases are antagonistic.

4. Luttinger-liquid behavior should be observable at lower and lower energy scales as the phase transition is approached from either direction and should persist to zero temperature at the transition.

5. There should be a tendency for the superconductor to develop a small $d_{xy}$ order parameter on top of the usual $d_{x^2-y^2}$ one.

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AFTERWORD

Shortly after this paper was released as a preprint Baskaran and Anderson issued a reply to which I will now respond.

Baskaran and Anderson begin by conceding my main point that the Mott insulator “is not a zero-temperature fixed-point”, i.e. not a legitimate state of matter at zero
temperature, and then go on to declare this unimportant because the zero-temperature constraint is too “restricted”. It is not too restricted. If the Mott insulator is not a phase at any temperature by any known definition then we must either say what it is or stop writing papers about Mott insulators. Like Baskaran and Anderson, I believe that the Mott phenomenon is a real effect, so I have attempted here to offer ideas as to what it might be. Declaring this to be a waste of time is untenable in light of the situation in cuprate superconductivity, and the matter is certainly not dealt with in Anderson’s 1959 paper 7.

The examples of Mott insulators given by Baskaran and Anderson - CuCl2· 2H2O, CuSO4· 5H2O, iron oxides, and hemoglobin - are all beautifully consistent with my views as they are all adiabatically deformable to conventional conductors along high-temperature paths and all order magnetically at zero temperature, as Baskaran and Anderson concede. The ordering temperature is admittedly low, and as-yet undetected in the case of the sulfate, and why it is low is indeed the important matter. Low-temperature magnetic ordering in real materials is easily disrupted by disorder, of course, so no report of intrinsically disordered spin ground states can be believed until spin glass behavior is meticulously searched for and not found.

Baskaran and Anderson are right about the near equivalence of 3He and the cuprates. Indeed their assertion that “the Mott insulator is a form of quantum solid, and the melting transition in 3He is our best example of a Mott transition” is quite consistent with my views in being a first-order transition between two ordered states - the antiferromagnetic crystal and the p-wave superfluid - potentially analogous to a first-order transition between the antiferromagnet and the d-wave superconductor in the cuprates. However their criticism that “no critical point ... connects solid and liquid” and that this is “well-known” is incorrect. The only thing required is an extra symmetry, such as that proposed by Zhang, that makes the two kinds of order equivalent, for it is the incompatibility of the broken symmetries that requires the transition to be first-order. It is true that an extra symmetry of this kind would be physically unnatural, but nothing prevents a physical first-order transition from being regulated by an “unphysical” critical point nearby in Hamiltonian space. It should also be noted that the phase diagram of 3He is more complicated than that of the cuprates, so there are more competing phases to reconcile.

The most important matter raised by Baskaran and Anderson is the meaning of the fermi surface. The quasi-particle spectrum of the cuprates in extreme underdoping, i.e. near the ostensible critical point, develops a “pseudogap” of order J, that is 10 times Tc, near the Bril-louin zone face at (π, 0), and evolves smoothly into the insulating state, where is possess a deep, isotropic minimum at (π/2, π/2) predicted by a number of us on the basis of “relativistic” theories lacking a fermi surface. 5

So it is simply not true that “the low-energy excitations must be described in terms of a fermi surface”. I predicted a non-trivial photoemission result quite nicely without it. The fact that the quasiparticle spectrum evolves continuously from the relativistic behavior at low doping to the metallic behavior at high doping has the following simple interpretation: There are two sets of excitations - one appropriate to the critical point and one appropriate to the cold metal - that can used perturbatively to compute measured spectra across the region of interest. In either case the “elementary” excitations of the perturbation theory scatter strongly at the temperatures and dopings of interest and lose their integrity as a result. Neither the fermi surface nor the relativistic point is right; the whole question of the nature of the elementary excitations is meaningless because the temperature cannot be lowered to zero. However, regardless of whether this interpretation is correct it is experimentally the case the fermi surface loses definition by degrees as the doping is reduced, and it is therefore not characteristic of anything.

APPENDIX: DISORDERED SPIN VACUA

Both the quantum disordered spin vacua discussed in this paper have prototypes written as projected BCS states at half-filling. Both may be generated using fictitious Hamiltonians of the form

\[ \mathcal{H} = \sum_{<j,k>} \Psi_j^\dagger \left\{ t_{jk} \tau_3 + \Delta_{jk}^R \tau_1 + \Delta_{jk}^I \tau_2 \right\} \Psi_k, \quad (A1) \]

where

\[ \Psi_j = \begin{pmatrix} c_{j1}^\dagger \\ c_{j1} \end{pmatrix} \]

\[ \tau_1 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \tau_2 = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \tau_3 = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}. \quad (A2) \]

For the specific case of

\[ t_{jk} = \begin{cases} t & j \text{ and } k \text{ near neighbors} \\ 0 & \text{otherwise} \end{cases} \]

\[ \Delta_{jk}^R = \begin{cases} \Delta & j \text{ and } k \text{ x near neighbors} \\ -\Delta & j \text{ and } k \text{ y near neighbors} \\ 0 & \text{otherwise} \end{cases} \]

\[ \Delta_{jk}^I = \begin{cases} \delta & j \text{ and } k \text{ + + second neighbors} \\ -\delta & j \text{ and } k \text{ + - second neighbors} \\ 0 & \text{otherwise} \end{cases} \quad (A3) \]

we have
\[ \Psi_q = \sum_j \exp(iq \cdot \vec{r}_j) \Psi_j \quad (A4) \]

\[ \mathcal{H} = \sum_q \Psi_q^\dagger \mathcal{H}_q \Psi_q \quad (A5) \]

\[ \mathcal{H}_q = 2t \left[ \cos(q_x) + \cos(q_y) \right] \tau_3 \]

\[ + 2\Delta \left[ \cos(q_x) - \cos(q_y) \right] \tau_1 + 4\Delta' \sin(q_x) \sin(q_y) \tau_2 \quad (A6) \]

\[ \mathcal{H}_q^2 = E_q^2 \quad (A7) \]

\[ |\Phi> = \prod_q \left[ \Psi_q^\dagger \left( \frac{E_q - \mathcal{H}_q}{2E_q} \right) \Psi_q \right] |0> \quad (A8) \]

The ground state \(|\Phi>\) thus generated represents a \(d_{x^2-y^2} + i\epsilon d_{xy}\) superconductor with \(\epsilon = 2\Delta'/\Delta\). When acted upon by \(P_\alpha\) with \(\alpha = 1\) at half-filling per Eq. (3) it results in the chiral spin liquid vacuum \(|\Psi>\). The RVB vacuum is the \(\epsilon \to 0\) limit of this.

There is a local SU(2) gauge symmetry contained in this construction procedure at half-filling and \(\alpha = 1\) caused by overcompleteness of the representation. If \(|\Psi> = P_\alpha |\Phi>\) for this case then it is also true that

\[ |\Psi> = P_\alpha U |\Phi> \quad (A9) \]

where

\[ U = \exp \left\{ i \sum_j \Psi_j^\dagger (\vec{\theta}_j \cdot \vec{r}) \Psi_j \right\} \quad (A10) \]

for any choice of the variables \(\vec{\theta}_j\). Thus specializing for simplicity to the case of \(\Delta = t\) and taking

\[ \vec{\theta}_j \cdot \vec{r} = \frac{\pi}{4} \left[ 1 - 2 \cos(\pi x_j) + \cos(\pi x_j) \cos(\pi y_j) \right] \tau_2 \quad (A11) \]

we find that

\[ U \mathcal{H} U^\dagger = \sum_{<jk>} \Psi_j^\dagger \hat{t}_{jk} \Psi_k \quad , \quad (A12) \]

where

\[ \hat{t}_{jk} = \tau_3 \exp \left\{ i\tau_3 \int_j^k \vec{A} \cdot d\vec{s} \right\} \]

\[ \times \left[ \begin{array}{c} t \text{ j and k near neighbors} \\ |\Delta'| \text{ j and k second neighbors} \end{array} \right] \quad (A13) \]

with \(\vec{A} = \pi y \hat{x}\). This is the Hamiltonian of electrons moving in a magnetic field of flux \(\pi\) per plaquette, i.e. a quantum hall problem.

The two kinds of ordering along the \(\delta = 0\) line we have discussed correspond to the two distinct ways a mass can be added to the Dirac spectrum without destroying relativistic invariance. The eigenvalue spectrum of \(\mathcal{H}\) described above expressed either as a \(d_{x^2-y^2} + i\epsilon d_{xy}\) superconductor or as a lattice Landau level is

\[ E_q = \pm 4t \sqrt{\cos^2(q_x) + \cos^2(q_y) + \epsilon^2 \sin^2(q_x) \sin^2(q_y)} \quad . \quad (A14) \]

The parameter \(\epsilon\), which measures the amount of P and T violation is thus one kind of mass. The other kind corresponds to a staggered potential. Thus taking \(\epsilon = 0\) we find that the eigenvalues of

\[ \mathcal{H} = \sum_{<jk>} \Psi_j^\dagger \hat{t}_{jk} \Psi_k + \sum_j \Psi_j^\dagger V_j \Psi_j \quad , \quad (A15) \]

where \(V_j = \pm V_0\), depending on whether \(j\) is even or odd, are

\[ E_q = \pm 4t \sqrt{\cos^2(q_x) + \cos^2(q_y) + m^2} \quad (A16) \]

with \(m = (V_0/4t)\).

![FIG. 4. Spinon dispersion relation calculated variationally using the t-J Hamiltonian and projected particle and hole excitations of the \(d_{x^2-y^2}\) superconductor at half-filling. The dashed line shows the opening of the mass gap associated with chiral or antiferromagnetic ordering.](image)
\[ E_{\text{spinon}}^{\text{spinon}} = 1.6J \sqrt{\cos^2(q_x) + \cos^2(q_y)} \]  
(A18)

for the spinon dispersion relation. This is plotted in Fig. 4. Only one branch is present because the particle and hole become equivalent under projection. Similar considerations applied to the holon give

\[ E_{\text{holon}}^{\text{holon}} = \pm 2t \sqrt{\cos^2(q_x) + \cos^2(q_y)}. \]  
(A19)

However, if one uses the correct relaxed vacuum for the t-J model, which has \( \epsilon = 0 \) and \( m \simeq 0.25 \), one finds that the spinons and holons are no longer free but bind with string potential. If a Hamiltonian that stabilizes the chiral state is used, then one finds the potential to be a logarithm. In any case attractive interactions between these particles grow with the onset of order and result in their being bound at low energies except when the order vanishes. The functional forms of these potentials are consistent with the physics of a U(1) gauge theory undergoing a confinement transition.

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