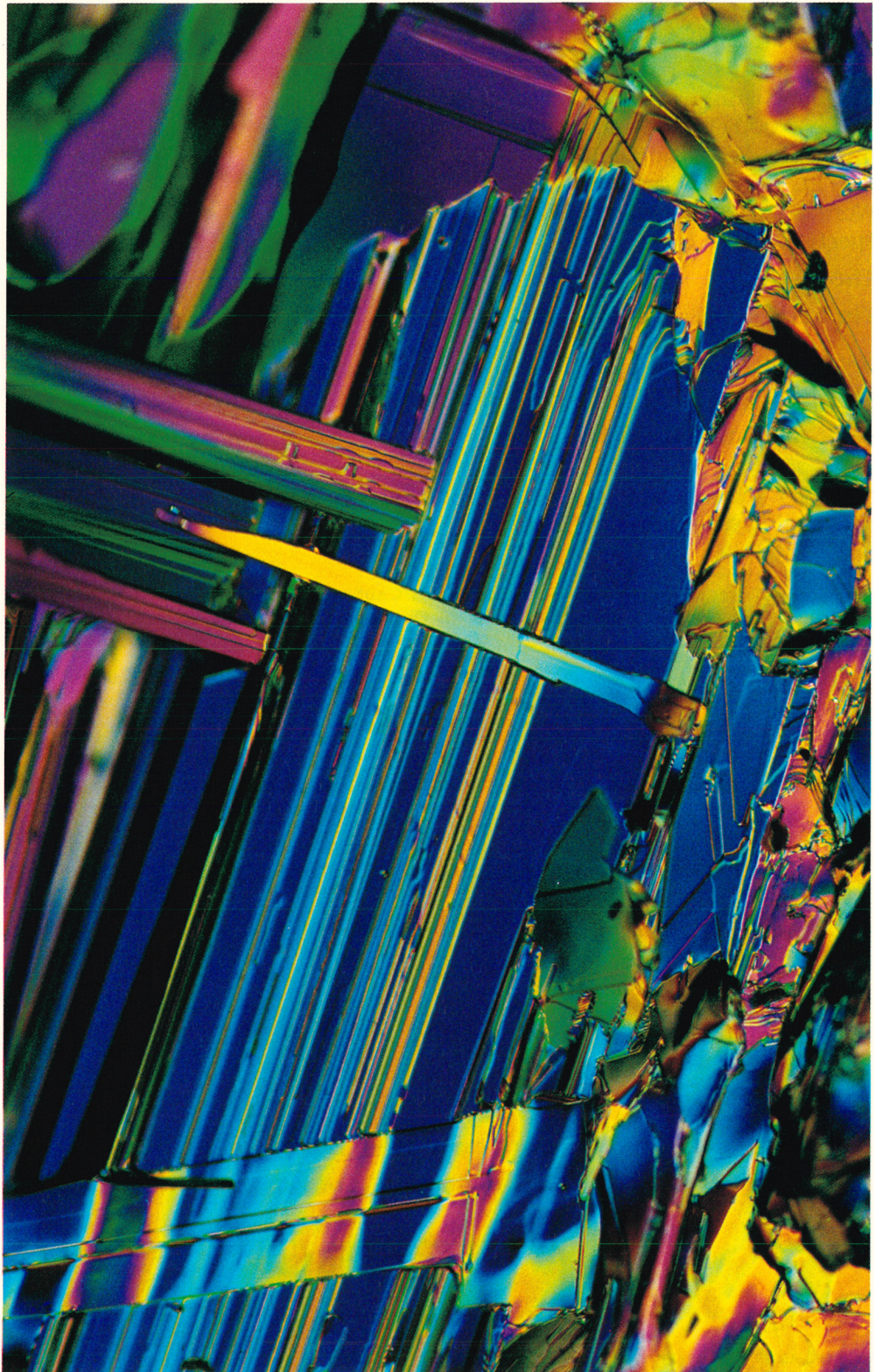


A DIALOGUE ON THE THEORY OF HIGH T_c



Superconducting
Bi₂Sr₂CaCu₂O_x, as
seen with reflected
differential
interference contrast
microscopy. This
view of the *ab* plane
surface of a platelet
of the ceramic
material shows this
high-temperature
superconductor's
strong planar
structure.
(Photomicrograph by
Michael W.
Davidson, W. Jack
Rink and Joseph B.
Schlenoff, Florida
State University.)
Figure 1

The give-and-take between two solid-state theorists offers insight into materials with high superconducting transition temperatures and illustrates the kind of thinking that goes into developing a new theory.

Philip W. Anderson and Robert Schrieffer

Although ideas that would explain the behavior of the high-temperature superconducting materials have been offered almost since their discovery, high- T_c theory is still very much in flux. Two of the leading figures in condensed matter theory are Philip Anderson, the Joseph Henry Professor of Physics at Princeton University, and Robert Schrieffer, Chancellor's Professor at the University of California, Santa Barbara. Anderson's ideas have focused, in his own words, "on a non-Fermi-liquid normal state with separate spin and charge excitations, and deconfinement by interlayer Josephson tunneling as the driving force for superconductivity." Schrieffer, for his part, has pursued "the interplay between antiferromagnetism and superconductivity, extending the pairing theory beyond the Fermi-liquid regime in terms of spin polarons or 'bags.'" PHYSICS TODAY asked the two to discuss the state of high- T_c theory today; their conversation took place via telephone, fax, electronic mail and in person.

Anderson: The consensus is that there is absolutely no consensus on the theory of high- T_c superconductivity. I suppose that, in a way, this is true. One can find groups working with some level of conviction on almost every hypothesis, and there are groups from very diverse backgrounds (such as quantum chemistry, electronic structure and many-body physics) working on the problem, groups that have very little meaningful communication with each other. Looked at relative to this scientific Tower of Babel, I suspect you and I are practically speaking the same language. Would you agree?

Schrieffer: The problem of understanding high- T_c superconductivity has indeed attracted a large number of talented theorists from a wide variety of fields. Each theorist brings his own set of techniques—and, more importantly, prejudices—to the party, with most new ideas really being an outgrowth of past work. Since our backgrounds are similar, it is no surprise that our ideas on high T_c begin in similar soil (the land of spins), supported by experimental facts. What is remarkable is how far apart, in some respects, we appear to wind up by using "scientific deduction" from a common point of departure. This divergence is a tribute to the richness of man's imagination. One hopes that ideas that are ultimately found to be inappropriate for high T_c will be important elsewhere. As figures 1 and 2 illustrate, Nature is wonderfully imaginative in the high- T_c materials, exhibiting regions of antiferromagnetism, semiconduction, superconductivity, strongly correlated electrons and metallic properties when plotted as a function of the doping. This richness indicates that the high- T_c problem is quite complex.

Anderson: Even among many-body physicists working on the problem, it might be possible to find a majority voting for "conventional" Bardeen-Cooper-Schrieffer theory with electron-phonon interactions—or at least electron-"something-on" interactions—as "the mechanism," and most of them would use G. M. (Sima) Eliashberg's

formalism to do their calculations.¹ I believe they are wrong. I'd like to hear your opinion, but first let me say a couple of things that bear on this question. In the first place, I think few people realize that we now know of at least six different classes of electron superconductors, and two other BCS fluids as well. Out of these only *one* obeys the so-called conventional theory—that is, BCS with phonons that fit unmodified versions of Eliashberg's equations. There are:

▷ Free-electron-like (s-p and lower d-band) metals. These all fit the theory and can be predicted.

▷ Strong-coupling, "bad actor," old-fashioned "high T_c " materials such as Nb_3Sn and $Pb(Mo_6S_8)$. These seem to have phonons, but they have many unusual properties in both their normal and superconducting states, and it would be rash to assume they fit simple theory. They have, for instance, peculiar magnetic properties in the normal states.

▷ Organic superconductors. These are still almost a complete mystery.

▷ Heavy-electron superconductors. These are now proven to be BCS-like but anisotropic—so-called d-wave superconductors, perhaps. No phonon mechanism is proposed.

▷ $BaBiO_3$ -based superconductors. These have phonons but cannot fit simple theory because their electron density is too low, Coulomb repulsion seems nearly absent and they have their highest T_c 's at dopings where conventional superconductors become normal, that is, at the metal-insulator transition.

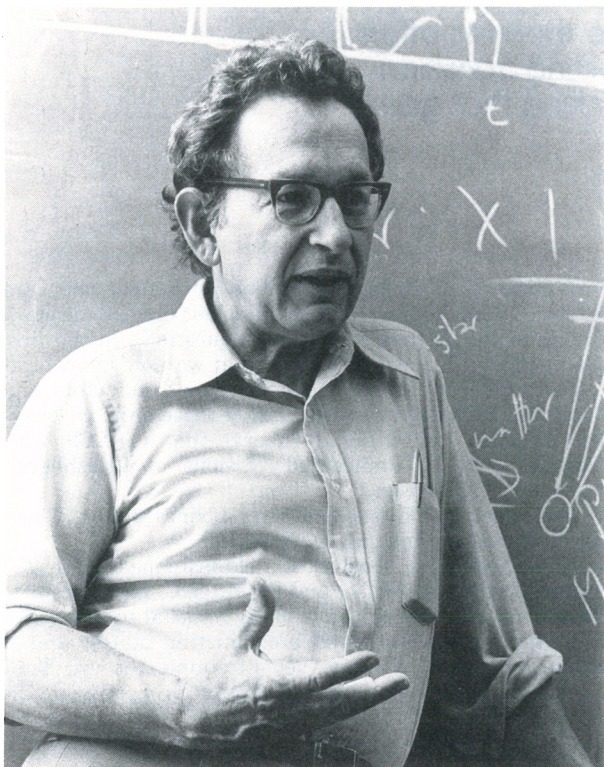
▷ High- T_c cuprates. In addition to their abnormal T_c 's, these materials have very abnormal normal-state properties.

And of course there are the two other BCS fluids: helium-3 and neutron-star matter, both of which are paired superfluids.

My point is that it seems crazy to suppose that the high- T_c cuprates, the oddest of all the electron superconductors, fit back into the first class, the free-electron-like metals, when we haven't been able to fit any of the others into that mold. Back in the 1960s we may have created that abomination, a theory that has become "nonfalsifiable" in the Popperian sense in that people insist on inventing more and more ingenious ways to make it fit any anomaly!

A second fundamental point is that if Mother Nature were to sneak up to my bedside tonight and whisper in my ear, "It's phonons" or "It's anyons" or "It's spin fluctuations," I would, even if I believed her, still be at a loss to understand anything about the cuprates. That is because, from the first, I have seen the problem as only *minimally* about the mechanism for superconductivity. The mechanism should follow relatively easily once we have worked out the much harder problem of the state of the nonsuperconducting metal *above* T_c . I wonder if you agree with all this.

Schrieffer: There is a long-standing confusion about precisely what constitutes the "BCS theory" as opposed to



Philip Anderson

the “BCS model,” the latter having been constructed to apply only to conventional (low temperature) superconductors. The BCS theory is a microscopic field theoretic framework for treating a fermion system in which an effective attractive interaction brings about a phase-coherent pair condensate, with strong spatial overlap of fermion pairs. The energy of a single pair drifting relative to the condensate is discontinuously increased by the action of the Pauli principle.

Notice, I did not refer to phonons as the source of the attraction, nor to momentum eigenstates $(\mathbf{k}, -\mathbf{k})$, nor to the pair’s total spin of zero. A BCS-type condensation can certainly occur in systems having none or only some of these conventional quantum numbers, such as you mentioned—helium-3, the actinides, atomic nuclei and neutron stars, for example. The quark condensate was the first-born pairing condensate, one second after the Big Bang. Admittedly, these examples are an important extension of the BCS model, I but the basic BCS theory remains unaltered in each of the developments. I would submit that while many BCS models are required to account for these widely different systems, in fact a *single* BCS theory underlies the physics of all the apparently distinct phenomena.

One added point is the question of whether Lev Landau’s theory of a Fermi liquid is absolutely essential to the pairing theory.² Based on the field theoretic description of the pairing condensate developed soon after BCS by Lev P. Gor’kov, by Yoichiro Nambu and by Eliashberg, it became possible to systematically include strong damping of the normal-state excitations in treating the pairing condensation.³ While the traditional Fermi-liquid theory certainly cannot describe the normal phase of the high- T_c materials, it is possible that analogs of the field theoretic approach will provide a rich enough framework to treat high- T_c superconductivity. You and William Brinkman

generalized the BCS model to treat superfluid helium-3 within the pairing context.⁴ The interaction there is spin fluctuations.

My belief is that the pairing condensation is what Mother Nature had in mind when she created these fascinating high- T_c systems. Thus I believe the high- T_c compounds are nicely nestled in the family of “pairing superconductors,” which extends from helium-3 with $T_c \sim 10^3$ K to atomic nuclei with $T_c \sim 10^{10}$ K, to say nothing of the quark condensate. It would be marvelous if there were an island of “exotica” from 23 K to 125 K, but I believe this is unlikely. Therefore, while you and I are close on a number of topics, we may well differ on whether a field theoretic version of the pairing condensation can basically do the job.

Anderson: No, I too am a firm believer that we have pairs, certainly singlet and possibly “extended” s wave, but closer to BCS than, say, helium-3. I see the superconducting state as in many ways *more* “normal” than the normal state.

Schrieffer: I fully agree that a proper understanding of the normal phase is a prerequisite to understanding high T_c , and that this is a formidable problem, probably far more difficult and interesting than the supercondensation itself—but this remains to be seen.

Finally, as for phonons, they must certainly play some role in setting T_c , although I believe their role is that of a supporting actor, except in materials like $\text{Ba}(\text{K,Pb})\text{BiO}_3$, where charge-density fluctuations coupled to phonons may dominate the attraction. Nevertheless, I believe theories based on phonons, charge fluctuations and variants of these should be pursued, because they may be relevant to such systems as the bismuthates.

Anderson: A final point on which I feel we have no substantive differences is the appropriate physical model that must serve as a basis for any further progress on the theory of high-temperature superconductivity. I think that, in the end, we would both come down to a simple one-band Hubbard model. I might emphasize further that I feel it essential to focus on the two-dimensional case, and I would feel that the self-exchange energy U is relatively large, but qualitatively any not-too-small value of U will behave in much the same way.

My original reason for choosing this model was a rough estimate of the relevant parts of the band structure. This estimate was later confirmed in considerable detail by the cluster calculations of Michael Schluter and his coworkers. This work shows that the electronic energy levels of fairly sizable clusters in the Cu-O planes can be fitted very accurately with a “ $t - t' - U$ ” Hubbard model. Nuclear magnetic resonance data confirm, via the beautiful interpretative work of the Illinois and Zurich groups, that the nature of the electron states does not vary with doping. Then the fact that the materials with Cu^{2+} stoichiometry are all antiferromagnetic Mott insulators seems to me to pin down the model many of us—certainly you and I—assumed to be correct from the beginning.

What is essential about the model—and also really, really difficult—is that one must use as carriers the same electrons that are participating in making an antiferromagnetically correlated spin structure. This dual role is a classic problem that has been bothering theorists for at least 30 years. Perhaps you could comment on the model,

why you believe in it and what version you prefer, and also explain it a bit more thoroughly than I have.

Schrieffer: Here we are in complete agreement. I believe you are correct that there is a single Fermi surface of primary importance to high- T_c superconductivity and that it arises from a strongly hybridized admixture of copper 3d wavefunctions of $x^2 - y^2$ symmetry and oxygen $p\sigma$ orbitals. Of course, optical excitations can involve transitions to other bands, but such excitations do not play an essential role in low-energy states mixed by the correlations responsible for superconductivity. The initial proposal I made in 1987, that the normal-state excitations are "spin bags," reflects the fact that the holes and spins belong to the *same* band, unlike the situation of the conventional spin polaron, where the hole is in band A and the spins are in band B. In the latter case, the pairing interaction arises from the Heisenberg exchange energy J between different orbitals, while in the former case the exchange interaction arises from the much larger self-exchange energy U , a concept you pointed out many years ago in your impurity-problem paper. It seems to me that the nmr and photoemission data strongly support this framework. Concerning the magnitude of the self-exchange energy, I agree that U is sizable, probably comparable to the bandwidth—that is, the intermediate coupling domain. Thus, one has the hope of getting the right physics from either the strong-coupling or weak-coupling limit, unless an important phase transition intervenes, which I doubt.

Anderson: I'd like to add a comment on the "two band" Hubbard model that many authors have proposed. I feel this is inconsistent with the chemistry of the copper oxides. The one consistent feature of their chemistry is the rigid, strong bond between the copper atom and the four planar oxygens. This requires that the bonding combination of Cu and O orbitals be quite deep below the Fermi level, so that no dynamical freedom is left to treat d and p orbitals separately.

Where we differ, I would guess, is in our vision of what happens in the process of doping and how mobile carriers are actually created. In fact, as I understand your present version of "spin bags," they resemble the soliton-like objects that we initially visualized, which have themselves metamorphosed toward the "dipole" and "spirals" of Boris I. Schraiman and Eric D. Siggia.⁵ I have to agree that these are highly respectable candidates for mobile objects at low doping levels. Perhaps you could explain how you see this rather complex picture at this time.

Schrieffer: The issue we have been exploring is whether or not the dominant elementary excitations in a single-band model behave as solitons (that is, spin bags⁶) having the traditional quantum numbers of a hole—charge $+e$ and spin $1/2$ —but having very strong dressing effects. Here the dressing takes the form of suppressing the amplitude and twisting the quantization axis of the local antiferromagnetic order that would have occurred in the absence of the hole. In the antiferromagnetic insulator with long-range spin order, the spin bag represents a local decrease in the spin-density wave gap $2\Delta_{\text{SDW}}$ over a region of size ξ , which in the weak-coupling case is large compared with the lattice spacing a . In addition, the bag causes a twist of the spin quantization axis. In the case of large self-exchange energy U , the spin bag corresponds to



Robert Schrieffer

a decrease in the near-neighbor spin order $\langle \mathbf{S}_i \cdot \mathbf{S}_{i+j} \rangle$ around the hole, with S_i^2 being nearly fixed. This region of decreased spin order and spin twist is comoving with the hole and forms a "bag" inside of which the hole lives. One can qualitatively view the situation as analogous to an electron bubble in helium-3, in that in the present case the outward uncertainty-principle pressure of the hole is balanced by the inward pressure of antiferromagnetism. The attraction between spin bags arises primarily from the lower amount of exchange energy required to produce a commonly shared bag as opposed to two separate bags.

In the case of large self-exchange energy U , the spin-bag effects arise from the stirring up of the location of the spins as a hole moves. This stirring up can be described as a gauge-potential effect that arises from the phase of the overlap of the spin wavefunctions as a hole hops—that is, a spin Franck-Condon factor. It is this combination of spin amplitude (longitudinal) and spin twist (transverse) dynamics that leads to bags and their attractive interaction. Such effects have been found in numerical calculations on the two-dimensional Hubbard model.⁷ Thus spin bags correspond to "nontopological solitons." (See James A. Krumhansl's typology of solitons, in his article in *PHYSICS TODAY*, March, page 33.)

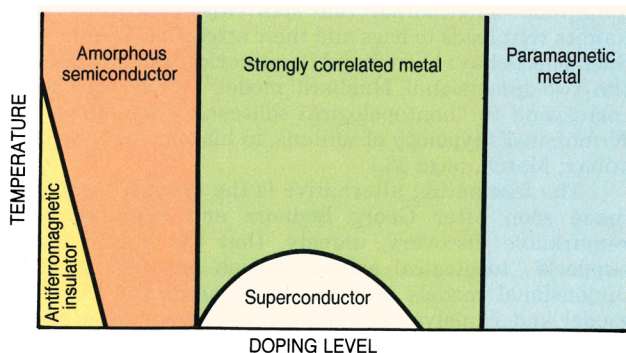
The fascinating alternative is the proposal that you made soon after Georg Bednorz and Alex Müller's remarkable discovery, namely, that the ground state supports "topological solitons," such as occur in one-dimensional models like the one-dimensional Hubbard model and in polyacetylene.⁸ In the latter, there is an explicit symmetry breaking corresponding to an alternating bond length along the $(\text{CH})_x$ chain. This gives rise to fractionalization of quantum numbers, in that the charge of an added hole is carried away by a positively charged soliton of spin zero, while the spin is independently carried by another soliton of charge zero. A similar effect occurs

in the one-dimensional Hubbard model.

Your proposal of holons and spinons for high- T_c materials is consistent with the anomalous, or exotic, excitations that occur in these one-dimensional cases. A central difference between the spin bags and the holons and spinons is that the former attract primarily within the two-dimensional Cu_2 plane, while as I understand it the latter are predicted to attract only if hole-pair hopping between neighboring planes occurs. Perhaps you could explain this difference and how you account for superconductivity in terms of the topological excitations.

Anderson: If there is a central controversy among serious theorists, it is about different versions of theories of the normal state, none of which can properly be described as undiluted Fermi-liquid theories. Some, like spin bags and David Pines's "antiferromagnetic Fermi liquid,"⁹ are closer to having genuine electron-like quasiparticles. The rest—the "marginal Fermi liquid" of Chandra Varma, Elihu Abrahams and their coworkers¹⁰; the gauge theories of Patrick Lee and Paul Wiegmann¹¹; the flux phase and anyon theories deriving from Robert Laughlin's work; and my own theory, which I shall discuss later—are frankly not Fermi liquids. There are many experimental reasons for abandoning Fermi-liquid theory. The frequency dependence of the relaxation rate $1/\tau$, which implies a vanishing quasiparticle amplitude, is the most quoted. But I see as also conclusive the low c -axis conductivity and the failure of sum rules for the photoemission data. Fermi-liquid theory can be indefinitely modified, but fortunately it has enough content that it can make sharp predictions, and *many* of these are strongly violated, not just one or two.

I have serious problems with the two "hyphenated"



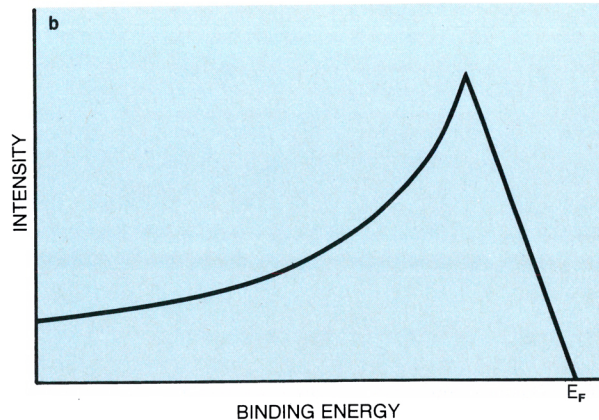
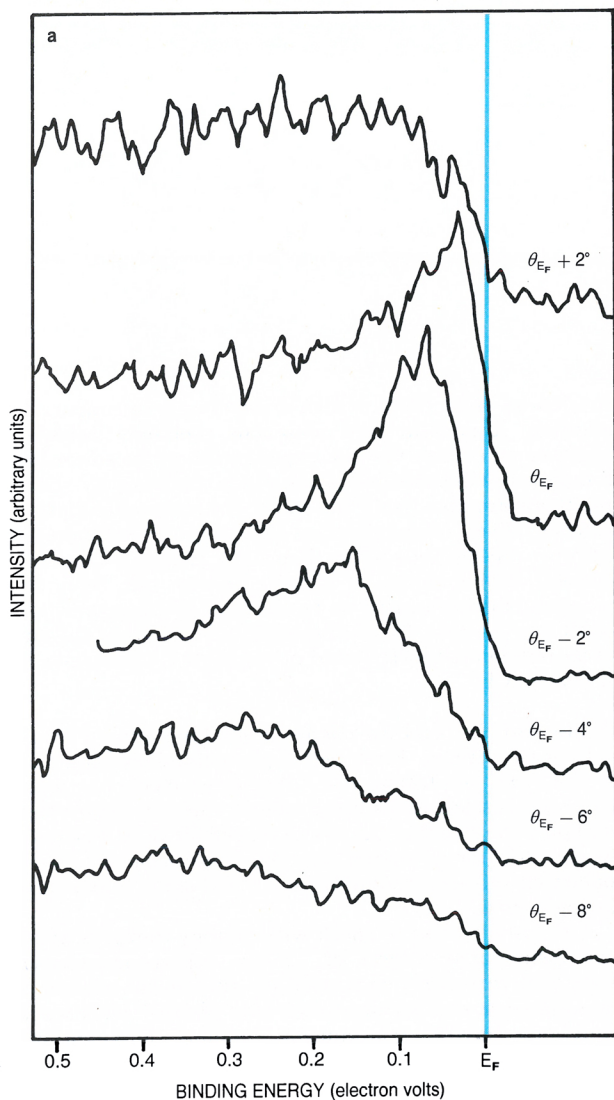
Generic phase diagram of the cuprate superconductors. The doping level is measured relative to the insulating parent compound. **Figure 2**

Fermi-liquid theories—the marginal and the antiferromagnetic—because I think they both have inconsistencies. But they have the great advantage that they are experiment based, even though they are a bit "myopic" in that each looks at only a fraction of the experimental data. What do you think?

Schrieffer: It is clear from experiment that the strict interpretation of a Landau Fermi liquid is not correct for the cuprates. While photoemission studies give strong support for the existence of excitations that have the gross features of quasiparticles, the observed line is asymmetric and the linewidth Γ appears to grow with energy E as $\Gamma \sim E^\alpha$ with $\alpha \sim 1$ rather than 2, as in the Landau theory. Unfortunately, there is considerable controversy about whether or not there really is a sum-rule violation in the photoemission-determined BCS density of states. Time will tell. Also, the temperature dependence of the electrical resistivity in the ab plane in the normal phase varies essentially as T rather than as the universal T^2 of the Landau theory. The question is how to account correctly for these differences from the properties of familiar metallic solids.

Some workers have proposed that the difference is largely on a low-energy scale, with the higher-energy excitations being essentially as in the Landau theory. In the spin-bag approach, the excitations are very different from Landau quasiparticles, in that the overlap Z between bare and dressed hole states is extremely small, vanishing in the long-range ordered antiferromagnet and being of order 0.1 or smaller in the doped superconducting material. I agree that at present no theory can account for all the normal-phase data, including the marginal-Fermi-liquid approach of Varma and his coworkers¹⁰ and the spin-fluctuation scheme of Pines's group.⁹ Deducing a consistent approach to the normal-state properties from a first-principles microscopic theory will still require much work, although I believe excellent progress is being made in this direction. A central question is whether spin and charge are deconfined, as in your approach and the anyon approach, or whether the excitations carry spin and charge together. This question was definitively answered for $(\text{CH})_x$ by nmr, transport and magnetic measurements.¹² I am betting that the deconfinement does not occur in the high- T_c materials.

Anderson: Finally, of course, we have to agree to disagree, because I believe our final assessments are considerably different. I see no reason to hedge on my opinion that the problem of high T_c is solved in principle, in the sense that I know the basic physics¹³ of the metallic state above T_c and the mechanism¹⁴ that causes the high T_c . I also believe that the number of crucial experiments testing this theory is already sufficient to preclude the



Photoemission spectra. **a:** Angle-resolved photoemission for a $\text{Bi}_2\text{Sr}_2\text{CaCu}_2\text{O}_8$ sample in the normal state at 90 K. The angle of the photoemitted electrons measured is indicated for each curve (where θ_{E_F} is a reference angle at which the hole state is at the Fermi energy). From bottom to top the values of the hole momentum $\mathbf{k}$ are moving through a predicted Fermi energy E_F . Note the broad tail and sharp cusp feature dispersing as $\mathbf{k}$ passes through the Fermi level. These data have been confirmed in detail by a number of groups. **b:** Calculated spectrum for the two-dimensional "Luttinger liquid" theory of the normal state. No attempt has been made to include thermal smearing or the background; a weak logarithmic singularity at the cusp will not show in the experiment, and so is not plotted. Adding the experimentally estimated background leads to a fairly good fit to the experimental results in **a**. (Experimental results from C. G. Olson, R. Liu, D. W. Lynch, R. S. List, A. J. Arko, B. W. Veal, Y. C. Chang, P. Z. Jiang, A. P. Paulikas, *Phys. Rev. B* **42**, 381, 1990.) **Figure 3**

possibility of going back to conventional theories.¹⁵

My own picture of the normal state is both more conservative and more radical than spin bags and perhaps even than "anyons." It is more conservative in that I find that there is still a Fermi surface satisfying Luttinger's theorem. For this reason Yong Ren and I, following Duncan Haldane, call it a "Luttinger liquid," a generalization of the Landau liquid of normal metals.

The Fermi surface is, however, not the surface of a Fermi sea of quasielectrons; rather it is the surface of neutral, spin-carrying fermions that we call "spinons." One of the main motivations for introducing these neutral, spin- $1/2$ excitations is that they exist in the one-dimensional Hubbard model, which can be solved exactly. Oddly enough, they have a *longer* mean free time than the transport time τ determined from $\sigma = ne^2\tau/m$, which is proportional to $1/T$ in good, pure cuprate single crystals. Thus they have a very sharp Fermi surface.

The spinons do not carry charge, and so we have to invent a second branch of the excitation spectrum: "holons," which are charged, spinless objects. It is not obvious that there is any meaning to assigning statistics for spinons and holons. Holons are a limit of collective excitations near the spanning vectors $2\mathbf{k}_F$ of the Fermi surface and—again—are clearly present in one dimension. What has

thus happened is what is called "separation of charge and spin": There are two *different* Fermi velocities for charge and spin fluctuations.

When we remove an electron from this metal, it leaves behind not a simple hole quasiparticle with a definite energy, as in normal Landau-liquid metals, but at least one spinon excitation and one holon excitation, and also a small shower of soft collective excitations. The breadth of this spectrum, into which the hole can be thought of as decaying, is equal to its original energy—hence the observed transport relaxation rate, which is just the decay rate. But the spectrum has a sharp feature, a cusp at the spinon energy. Figure 3b is a sketch of this spectrum, which I feel explains¹⁶ the strange shape and sharp cusp feature of the observed angle-resolved photoemission (figure 3a). As $\mathbf{k}$ approaches $\mathbf{k}_F$, the spectrum peaks more sharply at a Fermi surface in momentum space—behavior that satisfies Luttinger's theorem.

This holon and spinon liquid has two very unusual properties. First, transport current is very weakly scattered by charge fluctuations such as phonons or impurities because it is carried by a collective displacement of the whole spinon Fermi surface. Spin impurities, on the other hand, cause residual resistivity—a crucial and quite striking experimental fact. In a sense, this liquid is a

" $T_c = 0$ superconductor" in the absence of spin scattering.

Second, the liquid is strictly "confined" to the two-dimensional CuO_2 planes, in almost the same sense that quarks are confined to nuclei. The only objects that can move coherently from one plane to another are real electrons, but these break up incoherently into holons and spinons when they arrive. Thus we get no coherent three-dimensional motion in the third direction, along the c axis.

The absence of c -axis motion even between the very close planes in $\text{YBa}_2\text{Cu}_3\text{O}_{6+x}$ and $\text{Bi}_2\text{Sr}_2\text{CaCu}_2\text{O}_8$ is also attested to by, for instance, the absence of strong infrared absorption of c -axis-polarized photons. This absence represents a rather large increase in kinetic energy caused by the confinement. This energy provides the motivation for T_c —namely, that *pairs* of electrons can tunnel coherently from plane to plane, even if single electrons cannot, and this begins to occur at a $T_c \propto t_1^2/J$, where t_1 is the interlayer matrix element and J the width of the spinon band. The superconducting T_c is thus a *crossover* from two- to three-dimensional behavior. This is confirmed by various measurements, such as the remarkable observation of large splittings in the photoemission below T_c by William E. Spicer's group at Stanford.¹⁶ I can calculate T_c , but I find the behavior below T_c hard to fathom; I think the "gap" Δ may be very dependent on $|\mathbf{k} - \mathbf{k}_F|$, rather than on the angle $\hat{\mathbf{k}}$ as in anisotropic superconductivity.

Unfortunately, I don't have space here to go in detail into the many theoretical and experimental arguments that support the above picture. If you can see any crucial defects or have questions, I'd surely like to hear about them. The biggest problem I have had with this theory seems to be that everyone simply talks about his own theory and never examines anyone else's—for instance, mine—critically.

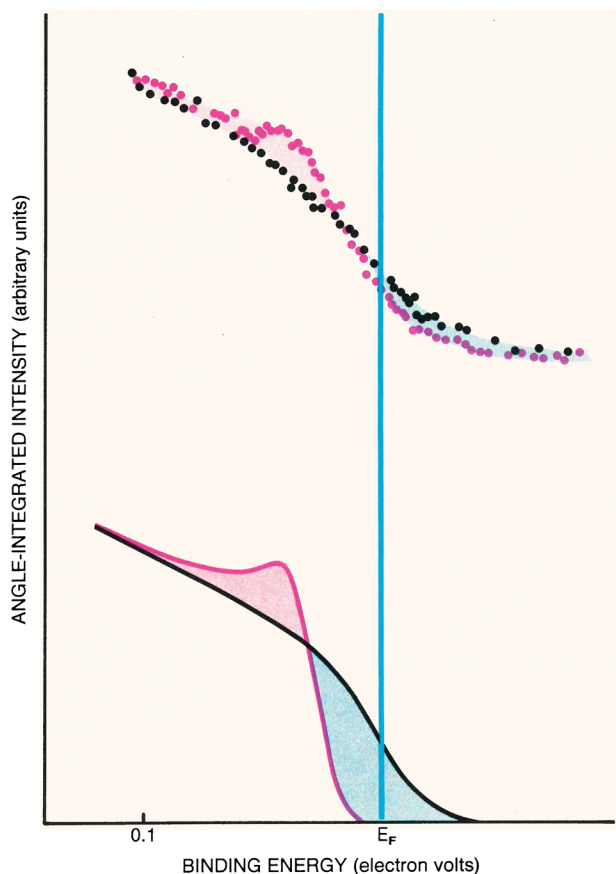
Schrieffer: I would be delighted if such a scenario were in fact played out in nature, particularly in view of the fact that Wu-Pei Su, Alan J. Heeger and I predicted a decade ago that such excitations occur in conducting linear polymers like polyacetylene, as has since been established experimentally.¹² Unfortunately, no "smoking gun" has yet been found for such excitations in high- T_c materials. While indirect evidence can be cited, such as you have mentioned above, history has cautioned us about concluding that an approach is valid based on agreement with certain data, particularly when the critical features of the data are in serious question. Such features include sharp peaks and the lack of sum rules observed in angle-resolved photoemission spectroscopy, as illustrated in figures 3 and 4. In the case of sharp peaks, one obtains an equally good fit to the data using a decay rate $\Gamma \propto E^\alpha$, with $\alpha \sim 1$ and no explicit cusp. And Yves Petroff at LURE in Orsay has shown that the apparent sum-rule violations most likely arise from stray electron emission associated with surface

roughness. This effect is seen dramatically in copper. In addition, it is clear that materials problems influence whether one observes an exponential or a power-law temperature dependence of the c -axis conductivity. Such problems are also likely the cause of the disputed optical rotation effects often quoted as critical for the existence of anyons. (See *PHYSICS TODAY*, February, page 17.) One very hopeful development in high- T_c theory has been the quieting of drumbeating so we can hear the true whispers of Mother Nature. I would be delighted if the smoking gun of deconfinement were found in the high- T_c materials, as it was in $(\text{CH})_x$.

Anderson: If I had my choice of smoking guns, I would ask for two things. First, better photoemission data, both sample and resolution wise. Angle-resolved photoemission spectroscopy is, for this problem, the experiment that will play the role that tunneling played for BCS. Second, I would ask for a search for the absorption of c -axis-polarized infrared radiation in normal-state, c -axis-insulating crystals.

Finally, I'd like to check with you my thoughts about the fashionable subject of "anyon superconductivity." Laughlin very early convinced me that in an exchange-dominated model, spontaneous "flux phases" with wavefunctions having spontaneous Landau diamagnetism could arise. (The most likely physics, however, is dominated not by the nearest-neighbor exchange energy J but by the nearest-neighbor hopping energy t .) Such states will have a "spin gap" or pseudogap for spin excitations. The scale of such gaps is likely to be very large, because J is large here, and I would suppose they occur in the normal state—as we both agree, the normal state is the key problem. But all kinds of strong experimental data tell us that no such gaps exist and that we have a Fermi surface in the normal state. Thus the "anyon" theory of these flux phases is not, so far, a theory of *our* superconductors, and must somehow be extended in as yet uncharted directions to become one. This theory makes no contact with the observed phenomena of superconductivity and seems unlikely to lead to properties resembling those of a conventional BCS superconductor even as much as those of the cuprates do.

On the experimental side, it is not generally appreciated how much more accurate and careful the chiral light scattering measurements by Kenneth Lyons at Bell Laboratories¹⁷ are than those of Hans Weber's group at the University of Dortmund in Germany,¹⁸ and that they give an effect that is 100 times smaller and physically very different. Aaron Kapitulnik, on the same crystals, sees nothing at all, with a superior setup.¹⁹ It is disturbing to find the most careful experiments showing the smallest effects. Given the serious doubts about the experiment of Weber and his coworkers, Lyons is only one positive result against at least two strong negative ones, if we include the



Angle-averaged photoemission above and below T_c for $\text{Bi}_2\text{Sr}_2\text{CaCu}_2\text{O}_8$. Upper plots are experimental data; lower curves are theoretical. Colored points represent data taken at 15 K; black points, at 105 K (in the normal state). Colored curve represents BCS theory at 15 K; black curve, at 105 K. The authors disagree as to whether background effects shift the positions of the curves and undo the apparent violation of the sum rule, according to which the differently shaded regions should have equal areas. (Adapted from J.-M. Imer, F. Patthey, B. Dardel, W.-D. Schneider, Y. Baer, Y. Petroff, A. Zettl, *Phys. Rev. Lett.* **62**, 336, 1989.) **Figure 4**

References

1. G. M. Eliashberg, *Sov. Phys. JETP* **11**, 696 (1960).
2. L. D. Landau, *Sov. Phys. JETP* **3**, 920 (1956); **5**, 11 (1957).
3. See J. R. Schrieffer, *Theory of Superconductivity*, W. A. Benjamin, New York (1964), chapter 7.
4. See G. D. Mahan, *Many Particle Physics*, Plenum, New York (1990), chapter 10.
5. B. I. Shraiman, E. D. Siggia, *Phys. Rev. B* **40**, 9162 (1990).
6. J. R. Schrieffer, X.-G. Wen, S.-C. Zhang, *Phys. Rev. B* **39**, 11663 (1989). A. Kampf, J. R. Schrieffer, *Phys. Rev. B* **41**, 6399 (1990); **42**, 7967 (1990). D. M. Frenkel, W. Hanke, *Phys. Rev. B* **42**, 6711 (1990).
7. See D. J. Scalapino, *High Temperature Superconductivity, The Los Alamos Symposium—1989*, Addison-Wesley, Redwood City, Calif. (1990), p. 314.
8. P. W. Anderson, in *Frontiers and Borderlines in Many-Particle Physics*, R. A. Broglia, J. R. Schrieffer, eds., North-Holland, New York (1987).
9. A. J. Millis, H. Monien, D. Pines, *Phys. Rev. B* **42**, 167 (1990).
10. C. M. Varma, P. B. Littlewood, S. Schmitt-Rink, E. Abrahams, A. E. Ruckenstein, *Phys. Rev. Lett.* **63**, 1996 (1989).
11. N. Nagaosa, P. A. Lee, *Phys. Rev. Lett.* **64**, 2450 (1990). L. B. Yoffe, P. B. Wiegmann, *Phys. Rev. Lett.* **65**, 653 (1990).
12. S. Kivelson, W. P. Su, A. J. Heeger, J. R. Schrieffer, *Rev. Mod. Phys.* **60**, 781 (1988).
13. P. W. Anderson, forthcoming book on high- T_c superconductivity, Princeton U. P., chapters 2, 3 and 6.
14. P. W. Anderson, in *High Temperature Superconductivity*, K. Bedell, D. Coffey, D. E. Meltzer, D. Pines, R. Schrieffer, eds., Addison-Wesley, Redwood City, Calif. (1990), p. 3.
15. P. W. Anderson, *Phys. Rev. B* **42**, 2624 (1990).
16. D. S. Dessau, B. O. Wells, Z.-X. Shen, W. E. Spicer, A. J. Arko, R. S. List, D. B. Mitzi, A. Kapitulnik, *Phys. Rev. Lett.* **66**, 2160 (1991).
17. K. B. Lyons, J. Kwo, J. F. Dillon Jr, G. P. Espinosa, M. McGlashan-Powell, A. P. Ramirez, L. F. Schneemeyer, *Phys. Rev. Lett.* **64**, 2949 (1990).
18. H. J. Weber, D. Weitbrecht, D. Brach, A. L. Shalankov, H. Keitar, W. Weber, T. Wolf, J. Geerk, G. Linker, G. Roth, P. C. Splitterger-Hünnekes, G. Güntherodt, *Solid State Comm.* **76**, 511 (1990).
19. S. Spielman, K. Fesler, C. B. Eom, T. H. Geballe, M. M. Fejer, A. Kapitulnik, *Phys. Rev. Lett.* **65**, 123 (1990).
20. R. F. Kiefl, J. H. Brewer, I. Affleck, J. F. Carolan, P. Dosanjh, W. N. Hardy, T. Hsu, R. Kadono, J. R. Kempton, S. R. Kreitzman, Q. Liu, A. H. O'Reilly, T. M. Riseman, P. Schleger, P. C. E. Stamp, H. Zhou, L. P. Le, G. M. Luke, B. Sternlieb, Y. J. Uemura, H. R. Hart, K. W. Lay, *Phys. Rev. Lett.* **64**, 2082 (1990). ■

negative result of muon experiments.²⁰ I'd like to hear your opinion, and I'll leave you the overall summary.

Schrieffer: I also have doubts concerning the origin of the positive results observed by Lyons and by Weber. Hopefully, this experimental issue will be soon resolved.

More generally, the first impression one gets of the theoretical developments on high T_c over the past four years is that theorists do not know what is really going on. While this is partly true, Bednorz and Müller's 1986 discovery did mark the beginning of a remarkable period of development in condensed matter physics. Before that time, strongly correlated fermion systems were an interesting byway of the field, but most serious many-body theorists believed Fermi-liquid theory could cover the most interesting materials. We are now rewriting the condensed matter textbooks of the future by adding volume II, in which interactions must be included in zero order, on an equal footing with one-body kinetic effects. At issue here is not so much which particular ground or excited states are physically realized, but rather how to develop concepts and methods to handle such systems in general. While one or possibly a small set of closely related approaches will fully explain high T_c , as BCS did for low T_c , it is inevitable that quite new phases of strongly correlated matter will be discovered, whether superconducting or not, and our understanding of these will benefit from this intense period of theoretical activity. As Laughlin has said, these exotic excitations are too tempting for Nature to ignore. Just as BCS was the dawning of a new type of physics now extending over 13 orders of magnitude in temperature, so we are perhaps witnessing the beginning of a major advance in our understanding of systems most of which are yet to be discovered.