Experimental Constraints on the Theory of High-T_c Superconductivity

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Analysis of the many experiments on high-temperature superconductivity indicate several essential aspects of any theory. The conductivity and other transport properties as a function of disorder, temperature, and frequency point to a non–Fermi liquid–like behavior, whereas photoemission experiments and magnetic properties indicate the presence of a Fermi surface in momentum space. To reconcile this apparent contradiction, a new type of electron liquid, called a Luttinger liquid, has been postulated, and the present article aims to show the need for this postulate. Theory and experiment indicate that the suitable phenomenological electronic structure model of the CuO layers is that of the one-band Hubbard model. It is also argued that experiment clearly indicates that interlayer interactions strongly affect the superconducting transition temperature, T_c, consistent with the fact that no theoretical calculations on two-dimensional Hubbard models have resulted in the prediction of high transition temperatures, and that anyon models are not favored by experiment.

A common misapprehension is that the theory of high-temperature superconductivity is in disarray. It may be true that theorists are in disarray, perhaps understandably, in that the situation genuinely is complex, many of the most crucial arguments are quite subtle and involve many esoteric and unfamiliar branches of physics, and people have committed themselves to diverse points of view that seemed tenable at the outset. I have considerable understanding of some of these theories, having tried them myself over many years even before 1987 to explain puzzling phenomena in many of the older superconductors. I think it would be useful to summarize the reasons for discarding many of these kinds of ideas, for the sake of not only theorists but also bewildered experimentalists and, most important of all, the curious outsider.

It is possible to produce a reasonably complete and consistent theoretical viewpoint that does not encounter any serious theoretical or experimental problems (*1*). This theory involves many elements that are esoteric even to the average many-body theorist, and it might therefore best be discussed in a book or a series of specialized articles, and this is being done. A more generally understandable review, which does not require special technical expertise, on the other hand, may be written about a number of serious constraints on any viable theory, most of which come from simple experimental measurements and simple but quite rigorous theoretical interpretations.

In a widely circulated but unpublished paper about a year ago, I expressed some of the constraints (*2*). It is now possible to do so even more clearly.

There are four major areas in which one can give clean results. These are (i) deductions from transport of electrons in the *ab* plane, (ii) deductions from transport along the *c* axis, (iii) growing consensus on the model: results from electronic calculations and from optical and photoelectronic probes, and (iv) deductions from the heuristics of the transition temperature, T_c.

Many theoretical approaches start from the premise that the real problem is to find a novel mechanism leading to enhanced attractive interactions of some sort, pairing the electrons of the normal metal that is supposed to exist above T_c. As a first step theorists do conventional band calculations to derive the "electronic structures" of the materials. The experimental experts on the various kinds of probes for studying Fermi surfaces and band structures—angle-resolved photoemission, angle-resolved positron annihilation, and de Haas van Alphen studies—sometimes take such structures very literally and use them as a guide. That such structures must not be taken too literally is, for instance, shown by the fact that experimentally the *c*-axis mean free path is less than the unit cell size so that it is not physically meaningful to attempt to determine departures of the Fermi surface from precise two dimensionality, since *c*-axis momentum cannot be defined. It is of course natural to continue to use conventional tools in conventional ways for lack of anything else, and the experimentalists especially express themselves with some caution, but as in this instance data interpretation may be influenced by theoretical preconceptions.

In the third section on models I will sort out the evidence that the fundamental model must transcend band theory, a point of view that is indeed beginning to be a consensus among theorists and that was well covered in the popular article by Schrieffer and Anderson (*3*). In the first two sections, however, I want to focus on the deeper and more precise question that was stated but not answered in that article: Is the normal metal above T_c in the cuprates a Fermi liquid in the precise meaning of that term? This question is the clear focus of the present discussion. It is my view that the experimental answer is unequivocally negative. If the answer is that the normal metal above T_c is not a Fermi liquid, the conventional approach sketched above is not likely to be fruitful but must be supplemented.

Fermi Liquids

Let me explain what the Fermi liquid question means and why it is important. Not since Wigner's work of the 1930s have theorists felt they could ignore the strong Coulomb interactions among electrons and of electrons and lattice vibrations in metals, and use a pure free-electron Sommerfeld-Drude-Bloch model. But that model worked with great precision as to qualitative behaviors; the general understanding of the reasons, in the operation of the exclusion principle, dates to the early 1950s or even earlier; but it was the Russian group around Landau and Migdal who codified what is now known as the Landau Fermi liquid theory, which expresses the precise sense in which this is true. In this theory an exact meaning is given to the concept of "quasiparticle," an exact low-energy elementary excitation of the Fermi liquid that has all the properties of a real free electron but can have a modified velocity, mass, and so forth and contains all of the high-energy effects of the interactions. In perturbation theory the quasiparticle excitation is connected to the real bare particles by a finite "wave function renormalization factor," Z. Z is also the fraction of the amplitude in a photoemission or positron experiment, for instance, that appears in the peak associated with the quasiparticle; the rest is incoherent many-quasiparticle superpositions.

One should not get the impression that a

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small Z means that the quasiparticles are not the whole story: they are, in that the Landau theory shows that all sufficiently low-frequency dynamical properties follow from quasiparticle motions alone, because they are the only low-frequency electronic excitations.

When we find such a quasi-exact low-energy theory, we now know, from the work of Wilson (*4*) and others, that it is certainly what we call a "fixed point of a renormalization group." In this case this means that we start at high energy and successively eliminate (renormalize away) the interactions and states, leading to an effective theory at low energy and long wavelength that in general will be simpler and simpler as we renormalize: it will converge to a "fixed point." This means that, even though we may not have access to the lowest energies, the renormalization group will have come a long way from the high-energy phenomena where it started and the resulting description of the state will be very far from the various alternatives. Thus, although it may seem like nitpicking to argue, as we do, about whether Z is really exactly zero or only 1%, it makes a qualitative difference, because it means we are near a qualitatively different fixed point and have a qualitatively different set of elementary excitations.

In summary, if we find that many of the electronic properties have settled down to characteristic temperature dependences as we go to the lowest available temperatures (and it is a serious problem in some cases that T_c is so high that we do not really have very low temperatures available), we should assume that we have reached the neighborhood of a fixed point, whether Fermi liquid or some other new possibility.

Properties of High-T_c Materials

One of the obvious facts about the cuprates is how similar the "normal" metal properties are among dozens of chemically very different materials (see Table 1). For most of these materials, if we are reasonably close to the optimum doping for T_c, we have very similar resistivity per plane, for instance, in the direction parallel to the planes (*5*); the spin susceptibility (*6*), nuclear magnetic relaxation (*7*), Hall effects (*8*), photoelectric (*9*) and infrared spectra (*10*), and tunneling curves (*11*), just to give a few examples, each measured on some subset of the few materials that can be prepared in single-crystal or at least well-characterized form, always have striking resemblances to each other and striking differences from other metals. Most observers accept that such details as the "chains" in YBCO or the "apical oxygens" of La_2CuO_4 are not controlling the properties and that the electronic properties are dominated by the CuO_2 planes.

Here I must appeal to a point of logic. The common response, when one makes a firm statement that all of these materials are not Fermi liquids because of one or another observation, is to say that the observation encounters exceptions among these many materials. But that is not the point: if they are all at the same fixed point—and they clearly are—it will be non–Fermi liquid for all if it is not for any one: it is necessary only to prove the negative in one instance. Exceptions are logically irrelevant.

Characteristic of Fermi liquids are a number of typical behaviors, with well-understood modifications due to special effects such as strong random scattering or magnetic impurities. One strong invariant is the volume within the Fermi surface, a theorem attributed to Luttinger, which, it turns out, is even stronger than Fermi liquid theory (FLT) because it still holds in many non-FLT one-dimensional models and is likely to hold in the cuprates. One can sketch a general proof that, if there is no new broken symmetry, any surface of low-energy excitations in k-space will enclose the appropriate volume for the density of electrons and hence will have a "large," "Luttinger" Fermi surface (*12*). This is a point on which there is some confusion (in which at one time I shared): the one-dimensional solutions clearly show that the Fermi surface may exist in a non–Fermi liquid.

Transport and response functions for a Fermi liquid are as follows: a constant density of states is reflected in finite T-independent spin susceptibility, a specific heat of γT, and $KT_1(\mathrm{NMR})T$ = const. (where T_1 is the time constant for relaxation of spins). The resistivity is $\rho = \rho_{res} + AT^2 + \rho_{phonon}$; Matthieson's rule of additivity is good if ρ_{res} is not too high, and the deviations due to weak localization are well understood. Heat conductivity K = const. × T obeys the Wiedemann-Franz law of proportionality to σ approximately. The Hall effect ideally is independent of T both at very low T and anywhere above $1/4\ T^{Debye}$.

A second strong rule is that $\rho_{res}^{(T=\omega=0)}$ does not renormalize with Z (where ω is frequency): it is a purely geometrical, wave-function effect of scatterers on the phases of quasiparticle wave functions [Mott's theorem, an early version having been proved by Schrieffer (*13*)]. It does not participate in the dynamic effects that cause Z. This is also true (in Migdal theory) of phonon resistivity. This is why the strongest arguments against FLT are based on resistivity data. Let me sketch the best of these.

Resistivity in the ab *plane.* The characteristic observation is that ρ_{ab} is not exceptionally high and that it has two unusual

Table 1. Materials.

Nickname	Composition at (approximately) highest T_c	T_c (K)	Structural features	Single crystals?	Well-characterized
214	$La_{1.85}Sr_{0.15}CuO_4$ (also many analogous)	~40	Octahedron; all planes identical	Yes	Yes
$YBCO_7$	$YBa_2Cu_3O_7$	~95	Chains and pyramidal plane pairs	Yes	Yes
$YBCO_{6.6}$	$YBa_2Cu_7O_{6.6-0.7}$	~60	Chains (O-deficient) + plane pairs (pyramids)	Yes	Yes
BISCO					
2212 (*32*)	"$Bi_2Sr_2Ca_1Cu_2O_8$"	~80	Pyramidal plane pairs	Yes	Yes
2201	"$Bi_2Sr_2CuO_6$"	~10	Single tetrahedral planes	Yes	Yes?
2223	"$(Bi,Pb)_2Sr_2Ca_2Cu_3O_{10}$"	~110	Two pyramids + one simple plane	No	?
	All nonstoichiometric				
TICOs					
2212	"$Tl_2Ba_2Ca_1Cu_2O_8$"	100	Same as BISCOs	No	Yes
2201	"$Tl_2Ba_2CuO_6$"	10–70	Same as BISCOs	No	?
2223	"$Tl_2Ba_2Ca_2Cu_3O_{10}$"	125	Same as BISCOs	No	Yes
"Electron doped" (*33*)	$Nd_{2-x}Cl_xCuO_4$	38	Ce	Yes	?
YBCO "124" (*34*)	$Y_1Ba_2Cu_4O_8$	81	Two chain layer + two planes	No	Yes
YBCO "247" (*35*)	$Y_2Ba_4Cu_7O_{15}$	95	Alternate two + one chains	No	Yes
"∞-layer" *n* (*36*)	$Sr_{1-y}Nd_yCaO_2$	40	Only planes, no apices: electrons	No	No
"∞-layer" *p* (*37*)	$Sr_{1-x}Ba_xCuO_{2+y}$	60–90	Same, holes	No	No

characteristics: usually there is no residual resistivity, and the T dependence is roughly linear (5):

$$\rho_{ab} = 0 + AT^{1\pm\epsilon} \quad (1)$$

(The correction to linear is weak: $\epsilon \sim 0.1$, or the correction could be logarithmic.) The conductivity for planes doped to have maximum T_c (15 to 30% hole doping) is not very different per plane from one substance to another. There are exceptions to these rules but exceptions from a generic behavior cannot restore the validity of the Fermi liquid fixed point.

This behavior is particularly striking in that Eq. 1 holds both for the pure, stoichiometric single crystal Y $Ba_2Cu_3O_7$ and for the very nonstoichiometric materials BISCO (2212) and $(La_{1.84} - Sr_{0.16})CuO_4$. There is little effect, therefore, of random scattering by the doping ions. In BISCO, for instance, the best estimates are that ~25% hole doping occurs entirely through the presence of off-stoichiometric distributions of the various ions. One may calculate rather easily the residual resistance that would result from a 25% off-stoichiometric concentration of ionic charges near the planes.

Another theorem that does not renormalize with many-body effects is the "Friedel sum rule" (*14*), which states that the screening charge around an ion in a metal is related to the scattering phase shifts by (in two dimensions)

$$n = 2\sum_m \frac{\delta_m}{\pi} \quad (2)$$

where $m = 0, \pm1, \pm2$, and so forth (the rough approximation of circular symmetry is inessential; the numbers do not depend on it). The screened ion is not more than 4 Å from the plane, so it is screened by electrons in about one unit cell of the square lattice, which implies that $\delta_{m=2}$ is probably small. Setting $\delta_{m=0} = \delta_{m=1} = \pi/6$, I estimated that the transport cross section for a single ion is

$$S = \frac{1}{k_F}\frac{1}{2\pi}\int_0^{2\pi} d\theta \sum_m |(1 - e^{2i\delta m})\cos m\theta|^2 \simeq \frac{3}{k_F} \quad (3)$$

(where k_F is the Fermi wave vector and θ is an integration variable) and so the mean free path is

$$\ell^{-1} = nS \simeq 0.075k_F \quad (4)$$

If we use the simple, unrenormalizing formula

$$\sigma = \frac{e^2}{2\pi\hbar}(k_F\ell) \quad (5)$$

we get (taking a typical plane spacing of 6 Å) a residual resistivity in the neighborhood of 100 microohm-cm. This resistivity is characteristically comparable to that per plane of BISCO, YBCO, 124, or Tl2212 in the region of 100 to 200 K. In fact, when these are successfully doped in the planes with scatterers that are strong enough to cause residual resistivity, this is a fairly usual value of ρ_{res} (8). What is, however, strikingly characteristic is that this resistivity due to the off-stoichiometric doping is never seen, nor is there any resistivity visible that is clearly caused by conventional phonon scattering, a resistivity that can be calculated to be of a similar order of magnitude although with a more complicated temperature dependence.

In summary, residual resistivity can be induced by certain kinds of doping, and, when it exists, it is additive to the mysterious linear term AT (A depends very little on purity). But the easily calculable residual resistivity of the dopant ions and the phonon resistivity are missing.

There is a great deal of information on the infrared conductivity of the *ab* plane that is confirmatory of the conclusions reached here, especially that the phonons (which are clearly visible in infrared and Raman spectra) have surprisingly little influence on the electronic response, and that the strong linear T dependence precisely corresponds to a linear ω dependence of relaxation rate τ^{-1} which extends smoothly out to frequencies near and beyond 1000 cm^{-1}, where it is unthinkable that phonon scattering could be involved (*10*).

As I emphasized, the absence of residual resistivity means that FLT has failed. This is independently confirmed strongly by the less rigorous argument that the ω dependence implies a mean scattering rate τ^{-1} that would lead to a divergence to zero of Z, as calculated from the perturbation theoretic self-energy of the quasiparticles. [This is the "marginal Fermi liquid" argument (*15*).] It hardly needs to be said that the entire apparatus of the conventional theory of metals, including the phonon or other boson-coupling pairing mechanisms as embodied in the "Eliashberg equations" that we used so effectively for conventional superconductors, might not be usable in such a radically modified environment. Thus, the great bulk of the literature speculating on one or another "pairing mechanism" may be irrelevant because it does not confront these deeper questions. The best description of the normal state from a purely empirical point of view is that the observed conductivity is that of a material that is going to be a superconductor at $T = 0$, and that has a depairing mechanism which prevents that superconductivity from being manifest, yet conventional scattering mechanisms do not cause resistance. (A similar behavior, although with different T dependence, is that of σ_{xx} in the quantum Hall effect.)

Resistivity along the c *axis.* A second, equally rigorous argument against the validity of FLT can be constructed around the same set of theorems applied to the resistivity in the *c*-axis direction, perpendicular to the CuO_2 planes. Again relying on the basic Mott theorem (*13*) that conductivity is not renormalized by the dynamic quasiparticle renormalizations, we observe that for conventional metals, almost no matter how anisotropic the mass, a conductivity greater than the Mott minimum metallic conductivity

$$\sigma_{min} \simeq \frac{e^2}{h}k_F \quad (6)$$

can be expected in all directions. The factor k_F can in fact be anisotropic if the Fermi surface is very cylindrical, having low dispersion in the *c* direction; k_F then represents the average magnitude of this dispersion in *k*-space and might be an order of magnitude lower relative to the *ab* direction. A common misapprehension is that localization is possible in one direction only, not in all; this is untenable because localization is a coherent backscattering phenomenon that requires the electron to retain its local coherence in all three directions in order to observe any localization in one.

A more serious problem arises in the very anisotropic material BISCO and perhaps elsewhere. Here it is reasonable to suppose that the dispersion in the *c* direction, "$t_\perp$" in energy units, is less than the inelastic scattering rate $\hbar/\tau$. In this case we will have incoherent Giaever tunneling in the *c* direction, not metallic conduction. However, in all such materials there are close pairs or triplets of CuO_2 planes that would give large infrared conductivity owing to the infrared-active interplanar hybrids of odd and even symmetry, a conductivity, again, at worst an order of magnitude less than the conductivity along the *ab* plane.

The general observation on *c*-axis conductivity is that it (i) is usually lower than, and often much below, the minimum Mott conductivity and (ii) often decreases rapidly with decreasing T. Early measurements often showed a roughly inverse behavior to the *ab* plane conductivity, that is, $\sigma_c \propto T$, $\rho_c \propto 1/T$ (*16*), at least at low temperature. The key argument is a logical one: that if even a few of the cuprates are clearly violating Fermi liquid inequalities, the overwhelming implication is that all are; demonstrating that "pure enough" or "adequately oxygenated" crystals show metallic

behavior in the sense of a positive temperative coefficient fails to answer the question of what was going on in the original materials, which certainly comprise the great majority. In no sense does the Mott limit restrict itself to "pure" or "sufficiently oxygenated" materials. In view of the anisotropy ratios (*17*) from 300 to 10^4 observed in many single crystals, it is clear that defects of all sorts—spiral dislocations, for example, which are common, as well as *a*-axis twins, grain boundaries, and so forth—may account for some of the observed "normal" T dependences. Again, the logic is that, if any one cuprate is not a Fermi liquid, none is.

The linear T dependence, with a very large coefficient of resistance at high temperature, is sufficiently common that one comes to suspect that it may be a generic behavior of Giaever tunneling for these materials. No theory of Giaever tunneling for non–Fermi liquids has been developed. The fact that this linear resistivity often adds in series with a $1/T^P$ term (where P is the power law constant) is significant in showing that this is not a simple Drude or Giaever conductivity.

The infrared conductivity polarized along the *c* axis is quite hard to measure, but what measurements exist show no sign of a very strong σ-π absorption in the region from ~500 to 1000 cm^{-1} (*18*). These measurements seem to confirm another puzzling paradox: the conductivity that in conventional superconductors determines the square of the inverse penetration depth λ^{-2} via the relation derived from a sum rule:

$$\int_0^{\Delta} \sigma(\omega)d\omega = \frac{1}{\lambda^2}\frac{8}{c} \qquad (7)$$

(*c* is the speed of light) is not adequate to explain the observed penetration depth (*19*): that is, the normal metal is in some cases strictly two dimensional as a metal yet becomes a three-dimensional superconductor. Very few of the candidate theories can cope with this fact.

There are many other unconventional aspects of transport, as, for example, a strongly and anomalously T-dependent Hall effect (8), but I have focused here on rigorous, quantitative, inescapably logical deductions that force one to a non–Fermi liquid fixed point.

The One-Band Hubbard Model

A consensus has grown up among many theoretical students of the high-T_c problem as to the appropriate model that must be solved to understand all this anomalous behavior. A semipopular exposition on this subject was given by Schrieffer and Anderson in *Physics Today*, June 1991 (*3*).

Underlying this model are a number of quite solid deductions from optical and photoemission data, electronic calculations and simulations, and other observations. To give the answer first, the equivalent model Hamiltonian—not, now, at the "transport" scale of ~100 K but at the 1-eV scale of the fundamental electronic interactions—is a "one-band Hubbard model" (*20*).

There is only one band of electrons that plays a role in the low-energy properties (less than ~1 to 1.5 eV) of these substances on the CuO_2 planes. This band is a hybrid (antibonding in character) between $Cud_{x^2-y^2}$ orbitals and $O_{2p\sigma}$ orbitals, which overlap strongly and whose wave functions are even relative to the CuO_2 plane. There is only one "Wannier function," local orbital, per Cu site. Charge and spin polarization effects cause the apparent degree of hybridization to vary somewhat depending on which experiment we study, but not outside of reasonable limits. The basic strong interaction between these electrons is repulsive, opening up a "Hubbard gap" U between states containing, respectively, two holes or one hole per Cu.

One should understand that the above picture is a model, albeit a quite accurate one. The "upper Hubbard band" is really above a "charge transfer" gap, not a pure Coulomb gap; the added electron is to some extent in an *s* orbital in the surrounding Cu ions; and the band is a fairly complicated antibonding hybrid. But through careful photoemission analysis Sawatsky (*21*) has been able to demonstrate how the Hubbard band forms above the high density of states peak of the nonbonding O orbitals. Optical studies show the rather well developed Mott-Hubbard gap of about 1.5 eV in the antiferromagnetic, insulating cuprates. As holes are doped into the "lower Hubbard band," intensity disappears in both optical and BIS bands, which can be clearly identified as the upper Hubbard band. The rate of disappearance agrees reasonably well with that calculated for a one-dimensional Hubbard model (*22*).

This picture is strongly supported by various computations, which, on this energy scale, can be done quite accurately. Careful electronic calculations by Schluter and Hybertsen (*20*) show that energy levels of clusters of several CuO_2 units in the appropriate background can be closely matched to those of a one-band Hubbard model; also, a fairly good account of the overall spectra can be produced from direct simulations [as, for example, in Horsch's work (*23*)] from a one-band Hubbard. Finally, nuclear magnetic resonance (NMR) coupling constants have been shown by the Illinois, Los Alamos, and Zürich groups (*24*) to be compatible with this band picture and not with one in which the magnetic electrons are distinct from those carrying the charge. Moreover, neutron (*25*) as well as Raman and NMR studies (*7*) have shown that antiferromagnetic interactions and antiferromagnetic correlations persist well into the superconductivity, or "strange metallic," regime and exhibit the superconducting gap. These magnetic couplings follow only from a basic repulsive electron-electron interaction such as the Mott-Hubbard U, of fairly strong magnitude.

Two popular types of theories are excluded by this basic model information. Both rely on large phonon coupling: the "negative U" or bipolaron scheme and the "density of states peak" idea. In the negative U scheme it is supposed that preformed pairs of electrons are bound together by large phonon displacements and their Bose condensation is T_c. Aside from the well-known problem of such a theory that the Franck-Condon displacements make metallic conductivity impossible and the Bose temperature negligible, the clear evidence that the magnetic and superconducting electrons are the same—in the neutron data of Rossat-Mignod *et al.* (*25*) they show the same energy gap—rules this out. The fundamental interactions of the superconducting electrons are repulsive.

The "Van Hove peak" idea of a high density of states peak coincident with some strong phonon coupling again fails to account for the clear evidence of an upper Hubbard band (*26*). Such speculations go back to the older "high T_c's" of the A15 and Chevrel structures, where also they were not successful. It is not consistent to ignore the dominant effect of the inescapable repulsive interactions when dealing with narrow bands or narrow-band features, because both theoretically and experimentally such features enhance repulsive effects (*27*).

T_c Is Controlled by Interlayer Interactions

A great many attempts at parametrizing the T_c data have been made, with greater or less success, but, in general, there is a tendency for such studies to be somewhat selective as well as very uncritical of sample characterization issues. A rough outline of what is really there, as far as I can see, follows.

1) For every material there is an optimum (but not necessarily very sharply defined) degree of doping for high T_c: below this level of doping, the material tends to be too insulating; above this level, almost always there is a crossover in the normal state toward conventional Fermi liquid behavior. It is significant that Fermi liquid–

like behavior contraindicates high T_c.

2) At and near the optimum doping there is rather little difference in normal state properties, especially the linear T resistivity per plane, suggesting that these are controlled by the planes themselves. Unfortunately, only a restricted number of single-crystal samples are available to verify this estimate, but so far it holds.

Very large differences in T_c's are caused by the "reservoir" layers between the planes. Bismuth materials show this very clearly: single planes have $T_c < 10$ K; double planes, ~80 K; triple planes, 110 K; $(La - Sr)_2CuO_4$ has T_c near 40 K, but the per plane properties are practically identical to 95 K YBCO, or to 80 K BISCO. Single-plane Tl cuprates may be optimized to nearly 80 K but also can drop to 10 K without enormous change in planar properties, while double and triple planes go to 105 and 125 K (*28*).

By careful selection, some of these data points can be made to fit on various universal curves but not in a way that carries conviction. To me, it seems an inescapable conclusion from the overwhelming tendency of the data that T_c is not a single-plane property. Many theorists have been trying to find a T_c mechanism within the single-band, one-layer Hubbard model, yet even a casual glance at the experimental facts convinces one that superconductivity is caused by effects outside that simple model and does not occur with high T_c in an isolated cuprate plane. (It can and does occur in a single unit cell with more than one plane.)

Many other structural features have been postulated as vital, but fortunately all have now been excluded by the finding of high T_cs without them. A list of a few of these follows (see Table 1 for supporting evidence).

1) The "chain" layer of YBCO, missing in almost all other high-T_c materials.

2) The "apical oxygen," the oxygen completing either a bipyramid or square pyramid with the CuO_2 square planar group. Materials with T_c's of 40 to 90 K have been found with no apices at all.

3) Hole-particle asymmetry. Electron-doped materials have lower T_c's and do not show all the anomalies as clearly but are definitely part of the picture.

4) Order versus disorder. YBCO is a stoichiometric crystal, BISCO is as disordered as you can get, with T_c's within 15 K of each other. In fact, disordering YBCO can change its planar properties, anisotropy, for instance, sharply without changing T_c at all.

5) Tight groups of planes. $(La - Sr)_2CuO_4$ and the newer "∞-plane" materials show that, although tight groups (pairs or triplets) of planes are good for highest T_c's, they are not essential.

If there is a generalization that so far has not failed, it is that superconductivity is always a two- to three-dimensional crossover, experimentally; it is this generalization which our theory exploits (*1*).

Anyon Superconductivity?

One of the types of theories proposed for high-T_c superconductivity is vaguely described as "anyons": theories in which the superconducting or normal state or both are described by spontaneous time-reversal (or parity) breaking and an excitation spectrum consisting of vortex-like solitons. I have not specifically discussed these theories above but, in view of the interest they attract, they may deserve separate mention.

As a description of the normal state, they suffer primarily from the evidence that the normal fixed point has a true Fermi surface [evinced by photoemission data and the Korringa NMR relaxation of many nuclei, demonstrating a large constant density of states of Fermi-like spin fluctuations (*29*)]. Anyons are motivated by, and require, a gapped or pseudogapped spectrum.

If we, ignoring the evidence that the normal state is a distinct fixed point, confine ourselves to the superconducting state, one experiment seems to mitigate against anyon states. This is the very clean IBM demonstration of persistent currents in a loop composed partly of high-T_c and partly of ordinary superconducting material (*30*). This experiment shows that the phase of a singlet pair wave function is correlated macroscopically and seems only explicable with a conventional Bardeen-Cooper-Schrieffer pair order parameter. This, with many other less conclusive experiments, shows that conventional Ginsburg-Landau theory with a true order parameter describing singlet pairs is the phenomenology of high T_c. The highly sophisticated theoretical demonstrations of anyon superconductivity do not demonstrate the existence of a suitable order parameter and are not completely convincing as to Ginsburg-Landau behavior. The optical experiments, which seemed, momentarily, to support T or P noninvariance, seem now to be inconclusive at best, having been contradicted by more sensitive tests (*31*).

Finally, much is made in several papers of "spin gaps": actually, these are pseudogaps involving considerable loss of density of states for a few tens of degrees above T_c. Some materials with the highest T_c's do not show much hint of such gaps, which brings into play our "single fixed point" argument. Also, attempts to fit data with such a gap leave it relatively small even compared with the known superconducting gaps, and certainly out of scale with the "fixed point" physics that occurs throughout the range up to 1000 to 2000 K. It must be an additional, mostly irrelevant quirk of the very complex physics.

Summary

The theoretical picture of high T_c becomes very much less confusing when one examines experimental results critically with certain minimal theoretical results in mind. The careful reader may wonder if any candidate highly regarded theory survives, but he should not despair; in my opinion there is at least an existence proof that there is one theory (*1*) that is both internally consistent and compatible with all these experimental constraints. Whether there is another time will tell.

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Shock Waves in Stellar Atmospheres and Breaking Waves on an Ocean Beach

George Wallerstein* and Steve Elgar

The phenomenon of ocean waves breaking on a beach is analogous to shock waves in the atmosphere of a pulsating star. In both cases a velocity discontinuity is clearly present. In stars the upper, expanding layer halts and falls back so as to interact with the rising gas at a shock. Similarly, a bore on a beach reaches its maximum extension before sliding back onto the next incoming wave. Analogous quantities such as the surface gravity of the star and the beach gradient in the ocean have similar effects on the flows and the nature of the discontinuity between them. Phenomena that are not analogous include the thermodynamic properties of the two media. Ocean observations may help solve some problems in shock phenomena associated with stellar pulsation.

At the meeting of the International Astronomical Union in 1952, Sanford (*1*) reported an entirely new phenomenon in stellar spectroscopy. The 17-day variable star W Vir showed emission lines of hydrogen and doubled absorption lines during rising brightness. At the same meeting, Schwarzschild (*2*) pointed out that Sanford's observations could best be understood in terms of a shock wave separating the rising and falling gas layers in the star's atmosphere. Owing to the Doppler shift, the stellar absorption lines were separated in wavelength by an amount corresponding to about 55 km s^{-1}, which is Mach 8 for the largely hydrogen gas of the infalling layer whose temperature is near 5000 K. The hydrogen emission lines were emitted by the shock-heated gas that marked the boundary between the two layers.

Two flow regimes also exist ahead of and behind ocean surface gravity waves breaking on a beach (Fig. 1). The wind-generated waves arriving at the beach from the deep ocean steepen as they propagate into shallow water, and eventually break and form bores (discontinuities in water depth, distinguished by white foam from air entrainment, Fig. 1) that continue to propagate shoreward. Seaward of the bore the water travels toward the shore, whereas ahead of the bore there can, in addition, be water from the previous wave flowing seaward down the beach slope. A breaking wave is thus also a shock with a velocity discontinuity. In fact, the equations of motion describing long waves in shallow water are the same as those describing compressible gas dynamics in one-dimensional flows. What can be learned by comparing stellar shocks and ocean waves?

In addition to providing insights into the behavior of nonlinear hydrodynamic phenomena, the comparison between stellar shocks and ocean waves can be used as a study of the utility of a physical analogy (that is, of two apparently unrelated phenomena that show similar physical behavior). Such an analogy differs from the analogies derived from mechanics that were used to describe electromagnetic waves in the ether during the 19th century because the mechanical analogs were purely theoretical. In addition, a physical analogy differs from a mathematical analogy, such as that of vibrating electrical circuits and mechanical devices, in which the phenomena are not physically similar but are related only by the similarity of the differential equations that describe them (*3*).

Equations of Motion

The equations of motion for a surface gravity wave propagating in shallow water ($kh << 1$, where k is the wave number and h is the water depth) are directly analogous to the equations governing a compressible gas. Fol-

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