Useful is the "Standard Model" of the Electronic Theory of Metals?

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The first decades of the 20th century caused such an overturning of our view of the physical world, with the discoveries of relativity and of quantum mechanics, that it has tempted many laymen, historians of physics, and even physicists to assume that everything since has been merely the working-out of the conseuences. There is, in this view, no possibility of a "continuous revolution" a la Chairman Mao. I disagree with this view rather strongly. I take as my motto the American sports dictum: "It isn't over till it's over." Surely the conceptual structure of the standard model of elementary particles, with its hidden and broken symmetries, its asymptotic freedom, etc., is bewilderingly different from the view of theoretical physics from 1930. All of its parts were bitterly contested by many of the original creators of quantum mechanics, in fact. The Feynman-Dyson revolution of "renormalizing" field theory which many of the "classical" quantum theorists were reluctant to accept, was followed by a series of further extraordinary conceptual flip-flops before we arrived where we are now.

In its less glamorous way, condensed matter physics has also followed the path of continuous revolution. In fact, broken gauge symmetry and the Higgs mechanism originated in condensed matter physics, for an example; and the most successful renormalized field theory may be the Landau Fermi liquid theory of metals and ,*He*. Nonetheless, this and the accompanying article will thry to persuade you that yet another revolutionary phase in this field is just beginning.

For almost four decades it has seemed to solid-state physicists that one of their crowning achievements is a generally accepted understanding of the physics of metals. This rests on three major developments of the '50s and '60s:

- Perturbative many-body theory based on the renormalization methods developed by Dyson et al. for quantum electrodynamics. These were adapted to the "Fermi liquid" of mobile electrons in metals by many theorists in Russia, Japan, and the West. Landau in particular and also Nozieres emphasized the concept of the renormalized electron or quasi-particle, an excitation, exact in the low-energy limit, which controls the entire dynamics and the transport properties of the "Fermi liquid."
- The BCS theory of superconductivity, its reformulation by Nambu, Eliashberg and Schrieffer to fit neatly into the above framework, and its adaptation to allow precise first principles calculations for simple metals.
- Developments in band theory, including the pseudopotential theory of Philips, Heine, etc., and the local density approximation of Slater, et al., which were used ably by Cohen and many others to provide quantitative bands in which the quasi-particles move.

It has been known since the work of Tomonaga and Luttinger in the '50s that one-dimensional metals are an exception (even aside from the discovery by Mott and myself that one-dimensional electrons localize). Note quasi-particles but collective objects, best thought of as density waves or solitons, are characteristic of one-dimensional systems, even though they often have Fermi "surfaces"---points of finte momentum at which the excitation energy vanishes. An extraordinarily full theoretical literature containing many exact results (most notable being the solution of the one-dimensional Hubbard model by Lieb and Wu, and the development of the "bosonization" method by Luther et al.) has grown up, although its correlation with real experimental data is in a primitive state (which was recently surveyed in PW by Jerome). A happy exception was polyacetylene, analyzed by Schrieffer and Heeger some 10-15 years ago.

Another exceptional case is the two-dimensional Quantum Hall effects, dominated by the energy gaps between Landau levels which, as Laughlin showed, completely alter the nature of the spectrum and invalidate the standard perturbative methods, but allow treatment in terms of fractionally-charged solitions.

But "ordinary" metals under "ordinary" conditions seemed, until recently, safe enough. There were even some unexpected triumphs for the Fermi Liquid idea. One was the realization by Nozieres that the Kondo effect essentially renormalizes magnetic impurities to a Fermi liquid ground state (interpreting the solutions of Anderson et al. and Wilson). This seemed apparently, also to hold good for the quantum valence fluctuating metals in which *f*-shell electrons at low enough temperatures become mobile, if very heavy, and contribute to the volume of the Fermi surface, which is often that calculated by LDA, even when the mass is enormously renormalized.

The discovery of the cuprate High *Tc* superconductors has stimulated a reappraisal of this "standard model." I have emphasized from the start in 1987 that the "normal" metal in the *CuO[sub 2]* planes above *Tc* does not behave like a Fermi liquid. This has stimulated a rash of speculative papers on possible alternatives, for example the "anyon" liquid of Laughlin and the "marginal Fermi liquid" theory of Varma, Littlewood et al. Experimental evidence overwhelmingly has come to favor my own theory, in which the two-dimensional gas is seen as resembling a manifold of one-dimensional "Luttinger liquids," one for each point on the Fermi surface, each very similar to the corresonding one-dimensional non-Fermi liquid. The main success of this theory is its explanation of the absence of coherent transport in the normal metal perpendicular to the *CuO[sub 2]* planes, a phenomenon unexplained in the "marginal" theory and various alternatives. That is, experimentally these are seen to have no three-dimensional character and no Fermi velocity in the "*c*" direction. Two recent results in this direction are:

- As angle-resolved photoemission measurements become more accurate, increasingly it has become clear that the expected splitting of the energy bands in BISCO, which as two *CuO[sub 2]* layers in close contact, is not visible. Many other details of the measurements are contraversial among the three main groups (Lausanne, Stanford, and Argonne) who reported results at the Stanford conference in March '95, but this was not, and it seems explicable only by a failure of the interlayer coupling to be coherent.

- Infrared spectroscopy with polorization perpendicular to the planes in 214 was also reported in that conference by several groups, again showing no coherent interplanar electronic motion in the normal state.

If this highly visible example of a two-dimensional metal is NFL (non-Fermi liquid), then how much can we rely on conventional theory in other cases? This is the question to which I have been addressing myself since 1991.

There are two chinks in the armor of Fermi liquid theroy. One is experimental: the persistent failure, for over two decades, of theory to account reasonably for a wide spectrum of experimental facts about metals in which, for one reason or another, the interactions between electrons are expected to be strong. These metals include, among others, the layer-structure dichalcogenides such as *NbSe2* and *TaS2*, the "bad-actor" superconductors such as V[sub 3]Si and PbMo[sub 6]S[sub 8], the "heavy-electron" superconductors such as *UBe[sub 13], CeCu[sub 2]*, and *Upt[sub 3]*, and the "organic superconductors" such as the Bechgaard salts. The unsolved puzzles for theorists include, often, the mere fact of superconductivity, but equally seriously the bewildering variety of "charge-density wave" and "spin-density wave" instabilities interleaving with superconductivity and occurring at a similar and unexpected temperature scale; and various transport anomalities. Much of this evidence is discussed in a lively way in the accompanying article by Piers Coleman.

The chink in the theoretical armor appeared first in a phenomenon discovered by G. Mahan in the early '60s, called variously the "Fermi surface anomaly," the "x-ray edge anomaly," or the "infrared catastrophe."

The spectrum of x-rays emitted by a metal when an electron falls from the conduction band into an inner-shell level shows an anomalous power-law dependence on frequency at the Fermi level rather than the sharp edge predicted by Fermi statistics. This phenomenon was explained formally by me, very briefly, and shortly thereafter in all generality, by Nozieres and de Dominicis. It involves an "infrared divergence" in the number of very soft electron-hole pairs emited in such a transition, and although these treatments were perturbative the phenomenon lies outside the "standard model" methods which are discussed above. (The simplest way to see this is that the coefficient of ln[omega] is (1/2)(eta/pi)• where eta is the scattering phase shift by the inner-shell electron, not (sin eta) or (tan eta)• which could follow from standard perturbation theory.)

If an inner-shell electron causes an infrared divergence when it interacts with a Fermi surface, one might ask, then why doesn't the same phenomenon occur for the conduction band electrons themselves? Exactly this kind of divergence is what causes all of the complications of the various models of one-dimensional metals. But in two or three dimensions, it has been thought since the '70s that the finite mass of the free electrons allows them to recoil upon collision with the Fermi sea electrons, which reduces the relevant phase shift eta to similar (system size)[super -1]. It was argued that the scattering could be described by an "effective range" *a* rather than by a finite phase shift eta, and so long as *a* remains finite there is indeed no difficulty. But in recent work I have shown that *a* does not remain finite in two dimensions, except in the limit of a low-density gas, because the "soft" recoils on which effective range theory depends are blocked by the exclusion principle. More generally, even in three dimensions, strong interactions seem likely to cause the same effect.

The effect of these diverenges is quite subtle, and in many ways a non-Fermi liquid could be mistaken for the corresponding Fermi liquid. It retains a Fermi surface, and like a Fermi liquid its transport and thermal properties are those of a "critical point" at *T*= *0*, as Haldane has described it: they are power laws in *T*, such as the familiar Fermi liquid specific heat linear in T, the Korringa law (T[sub 1] T)[super -1] = constant and rho rightarrow constant as T rightarrow 0. But in the more general non-Fermi liquid these power laws may have more general, non-integral exponents. In the normal state of the cuprates, one observes non-Korringa power law dependence of some nuclear magnetic relaxation times T[sub 1], and Bontemps' group has shown that sigma(omega), the infrared in-plane conductivity, seems to behave as omega[super -.7 ± .05]. The failure of c-axis motion can be thought of as *c*-axis conductivity obeying another power law, omega prompto (omega)[super .3].

The excitations which form the Fermi surface are no longer quasi-particles but solition-like objects which we have named "spinons" and "holons," with separate velocities for spin and charge excitations or even, in some one-dimensional cases, energy gaps for the one and not the other. The spin-spin correlation functions associated with spanning vectors "2k[sub F]" of the spinon Fermi surface can be much enhanced over those of a Fermi liquid and provide a theoretically sounder basis for the "antiferromagnetic spin fluctuations" observed by neutron scattering and NMR probes in some of these metals than does the Fermi liquid.

These results are derived by using an idea proposed by Alan Luther over 15 years ago which he called "tomographic bosonization," and which has been recently revived and generalized by Haldane. The idea is to describe the system in terms of collective fluctuations of the Fermi surface rather than by motions of individual particles; these fluctuations, which may be labelled by points on the Fermi surface, are the "bosons." In the non-Fermi liquid, as I have recently shown, the algebra of these fluctuations, is modified by interactions and the eigenmodes are bosons carrying charge and spin, not independent spin up or down. This concept of "charge-spin separation" is characteristic of this type of NFL model.

It has taken eight years for us to have reached the point with the cuprates where we can, at least in some examples such as BISCO 2212 and (la -Sr)[sub 2] CuO[sub 4], feel reasonably sure of the physics of the NFL state. (Even in these materials the superconducting state retains many somewhat mysterious features). No other material in the general class of strongly-interacting metals has been studied so intensively, from so many points of view, so it is not surprising that NFL theory has nothing but qualitative suggestions to make for other systems. A start on one other system is the interpretation by Clarke, Strong, and myself of certain anomalies in the "organic superconductor" (TMTSF)[sub 2]PF[sub 6]. What is clear is that two decades or more of efforts to fit all of these phenomena into a FL description are a catalogue of failure, and it is time we opened our minds to new ways of thinking. Many theorists seem to have teken the opposite point of view, that perturbation theory and the quasi-particle concept must be defended at whatever cost in terms of multiplication of arbitrary parameters and ad hoc assumptions, but in my opinion that is a Ptolemaic exercise in futility. If a revolution is bound to happen, we should follow the well-known advice in such situations, "relax and enjoy it."