Theory and Experiment in High-Temperature Superconductivity

P. W. Anderson (*1*) states that his theory of high-temperature superconductivity is consistent with a large number of experimental constraints. It should be pointed out, however, that most if not all of his theoretical explanations of experimental observations have been produced *after* the experimental results had been obtained. It may be that the inherent complication of this theory makes a priori predictions difficult, but predictive power has traditionally been the hallmark of successful scientific theories.

There is one important set of experiments that Anderson did not address in his article (or in his other papers) that may offer an opportunity for prediction: the dependence of critical temperature on uniaxial pressure. Because an essential component of Anderson's theory is that the superconducting transition temperature, T_c, is controlled by interlayer interactions, it seems that the theory would predict an increase in T_c under compression in the *c* direction and a decrease in T_c under compression in the *ab* plane (the latter because the intraplane exchange, which would increase under compression in the *ab* plane, enters in the denominator of Anderson's T_c equation). Although the experimental situation in this respect has not been established, some existing experimental evidence (*2*) appears to contradict this expectation.

It would be helpful if Anderson would clarify the prediction of his theory with respect to these experiments (*2*) and describe an experimental test that could prove his theory wrong.

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Response: The main points of my article were that Fermi liquid theory is not acceptable in the cuprates, the one-band planar Hubbard model represents the basic physics, and the third dimension controls T_c. Hirsch's comment is directed to a peripheral remark in my article and opens an entirely new subject. I did not expand on my theory, as this would have required difficult theoretical arguments.

That my theory has predictive power is shown by three notable post-theory confirmations: (i) the $(a + bT^2)^{-1}$ temperature dependence of the Hall angle in the normal state; (ii) the rapid disappearance of the relaxation rate proportional to temperature or frequency below T_c (leading to large peaks in thermal and quasiparticle conductivity below T_c without a corresponding Hebel-Slichter $1/T_1$ peak); and (iii) the appearance of a plasma edge below T_c in $(La–Sr)_2CuO_4$ *c*-axis infrared conductivity in a region behaving like a simple dielectric above T_c [recently measured by S. Uchida and his colleagues (*1*)]. Also, my interpretation of angle-resolved photoemission, while it postdated the data, was in complete disagreement with the experimentalists' interpretations. There are several other less straightforward cases. With respect to the specific point to which Hirsch refers, the situation is made complex by the fact that increasing the interlayer tunneling matrix element $t_\perp$ lowers the crossover temperature to Fermi-liquid–like, three-dimensional behavior, as observed by Batlogg *et al.* (*2*) in $(La–Sr)_2CuO_4$. This seems to be the limit on T_c as δ is increased (oxygen overdoping), and in this region dTc/dp is negative, where p is pressure. For a small δ it is positive. Another problem is that T_c is dependent on $t_\parallel$ (intralayer bandwidth) and the Mott-Hubbard U, which are also dependent on pressure.

With regard to Hirsch's last point, as to what would disprove my theory, I have been searching for crucial experiments, but it has passed the test of all those I know of. This is not the case for any of the hundreds of alternative theories that have been put forward, almost all of which cannot explain many observations. Two excellent attempts were based on a solid foundation of experiment but had little theoretical input and did not account for experimental results outside their base. Spin-fluctuation theories do not account for the detailed results on transport phenomena and photoemission, and the "marginal Fermi liquid" theory is an empirical "half-way house" on the way to my conclusions; when tested, it seems to have no consequences that differ strongly from mine, but it does not explain, for instance, nuclear magnetic resonance phenomena or *c*-axis conductivity behavior.

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