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In preparing this response I have relied heavily on the written version of a review talk presented at the La Jolla Conference on Strongly Correlated Electron Systems,[15] to which the interested reader is referred for further details.

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SCALAPINO REPLIES: I believe that there are sound theoretical and experimental reasons for considering the possibility that the pairing mechanism in the cuprate superconductors is associated with antiferromagnetic spin fluctuation that leads to a $d_{x^2-y^2}$ gap.

While the initial theoretical suggestions for this mechanism were based upon perturbation theory for a two-dimensional Hubbard model doped near half-filling, subsequent Monte Carlo calculations[1] have shown that there is an attractive pairing interaction in the $d_{x^2-y^2}$ channel. Furthermore, exact diagonalization studies[2] of the strong-coupling t–J limit of the Hubbard model provide evidence that two holes form a $d_{x^2-y^2}$ bound state when the ratio of the exchange coupling J to the hopping t is greater than a critical value. Although none of these numerical calculations has provided definitive evidence for a $d_{x^2-y^2}$ superconducting state, the fact that quite different numerical approaches, ranging from conserving diagrammatic approximations to Monte Carlo and Lanczos numerical studies, all find evidence for $d_{x^2-y^2}$ pairing correlations is significant.

With respect to experiment, a variety of results have been compared with the spin-fluctuation $d_{x^2-y^2}$ pairing ideas, because detailed, albeit approximate, calculations based on those ideas have been carried out. Again, these phenomenological calculations certainly don't provide a unique interpretation, but the range of phenomena that have been fit within this framework is striking. For example, in addition to the nmr longitudinal-relaxation-time measurements mentioned in the Search and Discovery story, the temperature dependence of the transverse relaxation time[3] supports a $d_{x^2-y^2}$ scenario. In addition, the same form for the spin susceptibility that was used in calculating these nmr relaxation times provides a $d_{x^2-y^2}$-based explanation[4] for the isotropic but incomplete suppression of the neutron scattering intensity experimentally observed[5] in $La_{1.86}Sr_{0.14}CuO_4$ below T_c.

Recently an analysis of the temperature dependence of the microwave penetration depth and its dependence on impurities[6] has shown that a $d_{x^2-y^2}$ gap also provides a plausible explanation for why the temperature dependence of the penetration depth observed at low temperatures in clean YBCO is linear, while a quadratic dependence appears when impurities are added. A similar analysis that has been carried out for the real part of the microwave conductivity[7] will test whether the dynamic and impurity scattering lifetime effects are consistent with the $d_{x^2-y^2}$ and spin-fluctuation ideas.

Thus the question of whether the cuprate superconductors are in a $d_{x^2-y^2}$ pairing state induced by the underlying short-range antiferromagnetic correlations will be decided experimentally.

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SHEN REPLIES: The figure to which Philip W. Anderson refers was poorly reproduced in PHYSICS TODAY. Readers should refer to our original figure and discussion[1] for experimental details. We discussed the gap anisotropy in the context of the position of the midpoint of the leading edge of the photoemission intensity peak. At point A, the midpoint of the leading edge is shifted to higher binding energy below T_c, indicating a gap opening. At B, the midpoint is not shifted within the uncertainty, reflecting a much smaller (or null) gap. We need more theoretical input to understand the change of the photoemission line shape as a function of temperature.

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A Record-Breaking Superconductor, Missed

I read with interest the news story [by Barbara Goss Levi] in the July 1993 issue (page 20) entitled "Critical Temperature Nears 135 K in a Mercury-Based Superconductor." I was surprised by the opening sentences: "No superconductor has broken the record for the highest critical temperature since 1988, when a thallium-bearing compound, exhibiting resistanceless conduction at temperatures as high as 125 K, laid claim to the title. . . . But in early May a group from ETH in Zurich reported measuring a superconducting transition temperature several degrees above 130 K in a compound containing mercury together with barium, calcium, copper and oxygen."

I suspect that the author missed our Rapid Communication in *Physical Review* this March.[1] We measured a T_c of 131 ± 0.5 K in $Tl_2Ba_2Ca_2Cu_3O_{10-x}$—a value we believed to be the highest reported to date, as we stated in the abstract.

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Was Nuclear Collision Article Off Target?

In the article "Probing Dense Nuclear Matter in the Laboratory" (May 1993, page 34) Subal Das Gupta and Gary D. Westfall give one view of the recent history of nuclear collision transport models. For the record, I would like to present an alternative viewpoint and some corrections of basic facts.

The words "dense" and "hot" are often used in articles about nuclear collisions. Das Gupta and Westfall mention temperatures of "50–100 MeV" and densities of "3 to 4 times" the saturation density ρ_0 of nuclear matter as well as "2 to 3" ρ_0. Readers should understand that any attempt to extract central densities and temperatures of nuclear collisions is extremely model dependent. The nuclear collision computer models (such as the BUU model, discussed by Das Gupta and Westfall) have a great many ingredients, some of which are neither very well understood theoretically nor treated consistently. For example, the nucleon–nucleon cross sections, mean field and momentum-dependent interactions should all be related consistently rather than being independent inputs to the computer code. Also, the quantum content of the BUU method is rather limited, being restricted to the phase-space Pauli blocking. Further, the ground state nuclei in the VUU–BUU code come apart[1] in times on the order of 100 fm/c. (This is better than in the old cascade model, but perhaps not good enough.)

The idea of "understanding the nuclear equation of state" has been referred to by Das Gupta and Westfall as well as many others. Noting the absence of any definitive nontrivial knowledge about the phase diagram of nuclear matter, I wonder if one has honestly gained much understanding of the nuclear equation of state over the past decade. Certainly, the in-plane squeeze-out, out-of-plane squeeze-out and bounce-off effect are novel discoveries,[2] but so far these have only provided rather limited information about the nuclear equation of state.

Das Gupta and Westfall claim that an "incompressibility value of $K \approx 215$ MeV" has been obtained from heavy-ion collisions. Do they really believe we can extract K to a precision of ±1 MeV? Others would disagree,[3] finding in fact that the current complete data set is not adequate to limit the range of K to better than about a factor of 1.7 (200–350 MeV).

The BUU code referred to by Das Gupta and Westfall was developed by Jörg Aichelin and patterned after a code developed by Hans Kruse and coworkers. The code was first given the name VUU (Vlasov–Uehling–Uhlenbeck), in a paper that Westfall coauthored.[4] Why do Das Gupta and Westfall also not mention the important theoretical work of C. Wong[5] in the 1970s and early 1980s? Wong's ideas made the computational development of the VUU code a reality.

The complete absence of references to others[6] in the international physics community who played a prominent role in a decade of nuclear transport equation simulations is striking. The computational work of George Bertsch, Das Gupta and Wolfgang Bauer was not done in a vacuum but rather in the context of work on similar ideas by many others.

The idea that the transverse momentum spectrum $p_x(y_p)$ (where y_p is rapidity in the direction parallel to the beam) rises from negative values at bombarding energies less than 100 MeV/nucleon to positive values at higher energies is clearly demonstrated in the context of the VUU model in reference 7, I believe for the first time.

Das Gupta and Westfall assert that "the number of pions produced is small" between 100 and 1000 MeV/nucleon; in fact the number of pions increases dramatically from low values below the threshold energy of a few hundred MeV/nucleon to substantial numbers at high energies. I assume that Das Gupta and Westfall are also aware that the flow angle is measured in degrees and not MeV/c per nucleon, as in their figure 3.