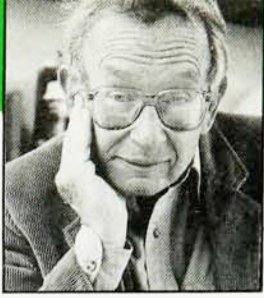

SPIN GLASS II: IS THERE A PHASE TRANSITION?

Philip W. Anderson

In the late 1950s and early 1960s Jim Kouvel, then at GE, and Paul Beck's group at the University of Illinois spent a lot of time exploring a phenomenon Paul called mictomagnetism. This phenomenon took place in dilute solutions of Mn atoms in Cu (and of other magnetic atoms in other nonmagnetic metals); I discussed these materials in my column in the January issue (page 9). These solutions, as I remarked, seemed to have small linear magnetic susceptibilities of typically paramagnetic magnitude (a few times 10^{-4} in dimensionless units). But Kouvel and Beck showed that the solutions exhibit, at a tiny scale and at very low temperatures, and in addition to the linear susceptibility, many of the phenomena typical of ferromagnetism: hysteresis, remanence and so on. In some ways these solutions are *more* hysteretic than ferromagnets, in that they can remember the sign and direction of the field they were cooled in, even when one applies an opposing field large enough to polarize them in the opposite direction.

Meanwhile Bernd Matthias and the rest of us at Bell Labs were very interested in the possibility that magnetism and superconductivity might coexist. Within the BCS theory the two should be quite incompatible, and in many cases they are; but (some 30 years too soon!) Bernd was determined to show that in at least some cases there would be a close relationship. In some of his dilute solutions of magnetic ions in superconductors (like Gd in $CeRu_2$) he noted the presence of the vague susceptibility peaks and remanences characteristic of spin glasses, and so he said: "Aha! Ferromagnetism and superconductivity are *not* incompatible!"

I always tried to listen more carefully to what Bernd's *results* said than what *he* said, since he had little regard for fine distinctions in statistical physics (like that between ferro- and antiferromagnetism, for instance, or between these and some vague bump in the susceptibility), but this is a case where he got to me. I was so certain that the transitions he was talking about were not true ferro- or antiferromagnetism that I failed to note what he had noted, that the transitions seemed remarkably sharp. I was particularly certain that a magnetic transition would involve a significant change in entropy and hence would certainly dominate the tiny energies and entropies of the superconducting state. (This was almost a decade before 1972, when Michael Kosterlitz and David Thouless, following my work on peculiar one-dimensional models, first showed that a phase transition could show no specific heat singularity at all.) Yet these bumps didn't seem to disturb the superconducting transition very much, which I felt meant that they were not phase transitions.

It is a bit ironic that only two or three years later, in 1965, an obscure journal called *Physics*, edited by none other than Bernd and myself, published the first evidence that there really was a spin glass transition, without either of us (or possibly even John C. Wheatley, the author) noticing. Wheatley was interested in testing his then new SQUID magnetometers in an interesting system and chose these same dilute solutions of Mn in Cu. His susceptibilities (measured, perforce, in a tiny magnetic field) followed a very precise Curie law C/T for each solution down to a temperature T_c, which was very exactly proportional to concentration, and then, as abruptly as he could measure, stopped changing with T and became constant. (Note that unlike the older measurements, Wheatley's did not exhibit a peak, because he cooled in a fixed magnetic field; a constant value of the susceptibility is characteristic of spin glasses when they are cooled in such a field.)

It was not until 1970 that the key measurements that woke the rest of us up to this peculiar transition were made—by Vincent D. Canella, John A. Mydosh and Joseph I. Budnick. This group measured ac magnetic susceptibilities with sensitive, but more conventional, methods, and discovered that the key variable is the magnitude of the measuring field. At 1000 gauss, there is only the conventional vague hump; at 1 gauss, a sharp, cusp-like peak appears whose width is less than 1% of T_c. Yet 1 gauss is 10^{-5} the magnitude of the internal field, since T_c is approximately 10 K. This tremendous nonlinearity is the appropriate characterization of the transition; later measurements, by P. Monod and Hélène Bouchiat, for instance, showed that the nonlinear susceptibility $\partial^2\chi/\partial H^2$ diverges as $(T-T_c)^{-P}$, where P is greater than 1. Thus, experimentally there is no doubt that the transition exists and is an equilibrium transition, since the nonlinearity can be measured above the transition point, where no one doubts that equilibrium is established in the system—after all, its natural relaxation frequency should be about 10^{12}–10^{13} sec^{-1}. Nonetheless, no measurement has ever revealed a specific heat singularity at $H=0$. As we shall see in the next column, the theoretical acceptance of a true phase transition, as well as an understanding of its nature, was much slower to come; and the most striking feature, the nonlinearity, is yet to be calculated, even roughly. ■

Philip Anderson is a condensed matter theorist whose work has also had impact on field theory, astrophysics, computer science and biology. He is Joseph Henry Professor of Physics at Princeton University.