REFERENCE FRAME

SPIN GLASS III: THEORY RAISES ITS HEAD

Philip W. Anderson

Sam Edwards was finishing out his term as head of the SRC, the British equivalent of the NSF, during the winter of 1974–75, after being appointed to a professorship at Cambridge. (He is now Sir Sam, the Cavendish Professor, successor in that chair to James Clerk Maxwell and four or five Nobel laureates, including Sir Nevill Mott.) Being Sam, he was unfazed by the full-time SRC job and needed research to do on the train back and forth to London, so he dropped in every Saturday at the Cavendish Laboratory for coffee and a chat with me and the theory group. I made a point that year of being there on Saturdays as well as during the week, and we did a lot of talking about localization and the "Fermi glass" (that is, the problem of electrons frozen in place by localization and interactions), the theory of liquids and the glass transition, and other problems of mutual interest.

One of these problems was that of dilute magnetic alloys, which seems to have acquired the name "spin glass" in a 1970 paper I wrote with Wai-Chao Kok (now at Singapore University) for a 65th-birthday festschrift for Mott in the *Materials Research Bulletin*. (See my columns in the January and March issues of PHYSICS TODAY.) I described to Sam the old mystery of continuous, disordered freezing in these alloys, and the new mystery of the sharp cusps and nonlinear behavior that John A. Mydosh had reported. Sam's ears pricked up. He had a notebook full of methods he had been trying on gelation, the glass transition and various polymer questions, but had been frustrated because these are not clean, well-posed problems. (He had also tried the methods on localization; and later, in the hands of Franz Wegner and Shinobu Hikami, they did work reasonably well on that problem—but some extra frills were needed.) I was convinced that the random Heisenberg Hamiltonian

$$\mathsf{H} = \sum_{(ij)} J_{ij} \mathbf{S}_i \cdot \mathbf{S}_j$$

where $\mathbf{S}_i$ is a classical spin vector at site i and J_{ij} is the interaction between the spins on sites i and j, was almost certainly the proper statement of the spin glass problem. Sam was overjoyed when he learned this, because here was a nice clean problem to work his methods on.

The methodology of the resulting, justifiably famous paper was almost entirely his. (That's why I can make such an immodest statement.) But the basic physical concept we worked out together. We decided that the thing to do was to ignore the spatial ordering, that is, to neglect the long-range ordering of spins in space, if any, and instead to look for long-range order in *time*. Richard Palmer later named this concept "nonergodicity" because it means, when present, that the system does not explore all possible states in the course of time. As the measure of long-range order in time, we introduced q, which is the average correlation between a spin $\mathbf{S}_i$ measured at one time and the same spin measured a macroscopic time t later. The equations are

$$q(t) = \langle \mathbf{S}_i(0) \cdot \mathbf{S}_i(t) \rangle_{\text{ave over } i}$$

$$q = \lim_{t \to \infty} q(t)$$

First we did a little physical calculation of the transition temperature below which q became nonzero. One separates out one of the sites, say i, and assumes that all the neighboring sites have a finite value q_0 of the "spin glass order parameter" q. Then one calculates the correlation enforced on site i by the effective fields due to the other sites, and finally one averages it over all sites i in the sample. For self-consistency, the order parameter thus calculated must have the value q_0 initially assumed for sites that are neighbors of i. The order parameter we calculated,

$$q_0 = \langle \langle \mathbf{S}_i(0) \cdot \mathbf{S}_i(\infty) \rangle_{\text{ave}} \rangle_{\text{ave over } i}$$

had a nonzero solution below a certain T_c. (The subscript "ave" stands for the statistical mechanics average over thermal fluctuations.)

Much more devious is Sam's so-called replica method of calculating thermodynamic properties. To do thermodynamics properly, one must average extensive quantities such as free energy and entropy. These are all derivable from the logarithm of the partition function

$$Z = \operatorname{Tr} \exp(e^{-\beta \mathsf{H}})$$

which grows exponentially with the size of the system. It is dangerous—in fact wrong—to average the partition function in random systems like the spin glass. This is because the partition function fluctuates too much: Special configurations, such as regions where all the J_{ij}'s are accidentally positive, will dominate its average value.

The key point of principle that makes studies of the spin glass and similar systems difficult problems is this: They are "quenched" random systems, with the values of J_{ij} fixed for all time by the conditions of preparation of the sample. But we want to average over macroscopic samples, in which many different configurations of J_{ij}'s occur, in such a way that the average represents the behavior of a *typical* system and is the proper "extensive" or "intensive" thermodynamic quantity that varies sensibly with the size of the system. Sam recalled the obvious identity

$$\ln Z = \lim_{m \to 0} \frac{(Z)^m - 1}{m}$$

The necessary average is then that of the m-th power of Z, not of its log; but when m is small this is no easier.

Now we do an outrageous thing: We note that the average of Z^m for $m = 1,2,3,4, \ldots$ is calculable because it is the average of an exponential containing J. For m an integer,

$$\langle Z^m \rangle = \operatorname*{Tr}_{(\mathbf{S}^\alpha)} \int \exp\left(-\beta \sum_{\alpha=1}^{m} \sum_{ij} J_{ij} \mathbf{S}_i^\alpha \cdot \mathbf{S}_j^\alpha \right)$$
$$\times P(J_{ij}) \mathrm{d}(J_{ij})$$

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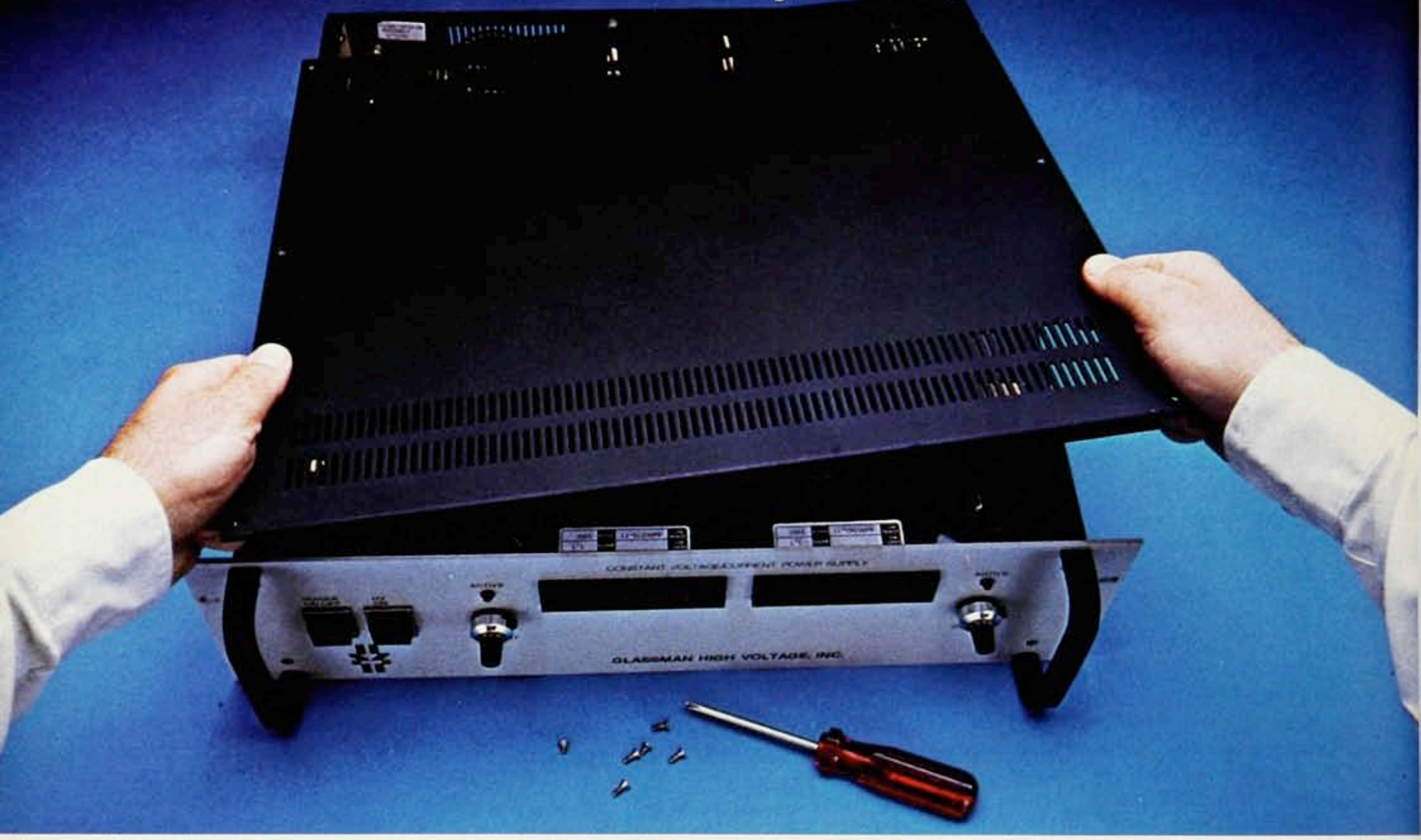

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which is the average over m identical "replicas" of the system. If

$$P(J_{ij}) \propto \exp(-J_{ij}^{\,2}/2J^2)$$

this integration is easily done. It gives rise to the following type of statistical problem:

$$\langle Z^m \rangle = \operatorname*{Tr}_{(\mathbf{S}^\alpha)} \exp\left[\frac{\beta^2 J^2}{2}\left(\sum_{\alpha=1}^{m} \mathbf{S}_i^\alpha \cdot \mathbf{S}_j^\alpha\right)^2\right]$$

This is no longer a random problem, but a regular one. It is more difficult, however, because it is biquadratic in spins. It can be solved in mean-field theory—assuming that $\mathbf{S}_i^\alpha$ and $\mathbf{S}_j^\alpha$ are uncorrelated (correlation only gives terms of order m^2) but that $\mathbf{S}_i^\alpha$ and $\mathbf{S}_i^\beta$ *are* correlated. It can also occasionally even be solved by renormalization-group methods. But one has the awful problem of extrapolating from all positive integral values of m to small m. In principle it is not rigorous to take the limit as m goes to 0 when the function is known only for integral values of m. In practice, however, it turns out to be easy since one keeps only terms of order m. What is more, so far none of the real difficulties encountered in the spin glass theory seem to have come from failure of the mathematical extension to $m \to 0$! Recently Haim Sompolinsky (*Phys. Rev. B* **25**, 6860, 1982) and Miguel Virasoro (*Europhys. Lett.* **1**, 77, 1986) have given us some ideas about why that is true.

In the mean-field solution the "Edwards–Anderson order parameter" q reappears in a new guise, as a replica–replica correlation function

$$q_{\alpha\beta} = \langle \mathbf{S}_i^\alpha \cdot \mathbf{S}_i^\beta \rangle$$

Thus in some real sense the different replicas represent very widely separated instants in time at which we choose to look at the same system.

Sure enough, the mean-field theory we worked out showed a nice sharp cusp in the susceptibility, in qualitative agreement with experiment, and weakly nonlinear behavior, qualitatively correct but too small. Unfortunately it also gives a cusp in the specific heat, which to this day has never been seen, and which is surely unphysical for real, finite-dimensional spin glasses. Nonetheless the result, giving a sharp freezing transition and describing a true nonergodicity, seemed sufficiently promising that we felt that the replica methodology was the doorway into the problem and that final solutions were just around the corner.

Little did we know! See next time, when I reveal the Negative-Entropy Catastrophe. ■