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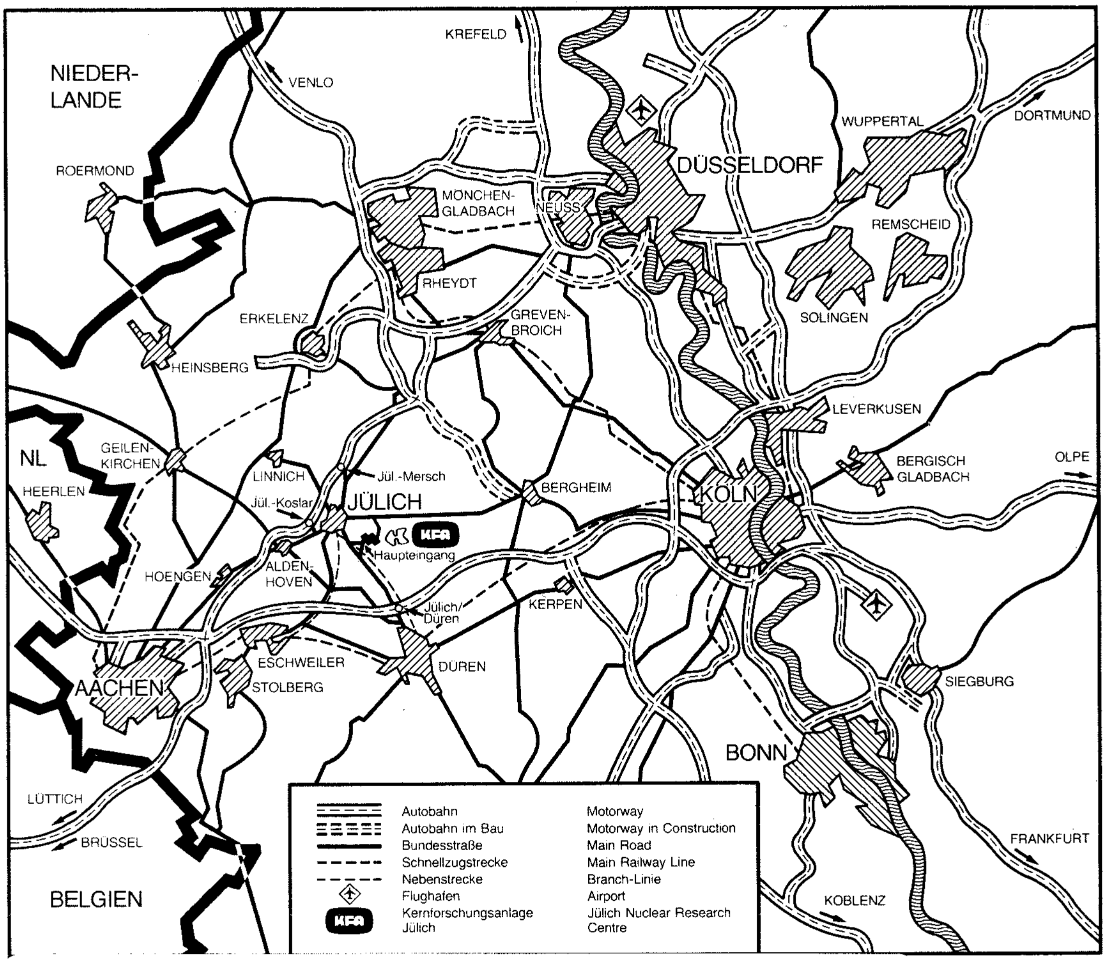

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ABSTRACT

Three physicists discuss some of the recent ($t > 1978$) contributions of statistical mechanics to magnetism. Critical phenomena, the random field Ising model and spin glasses are mainly addressed. STOCHAS is a specialist of statistical mechanics, in which he has a profound knowledge. I hope I have been able to transmit his thinking correctly. SIMPLICIO asks simple-minded questions and occasionnally makes impertinent comments. MAGNES is an experimentator and tries (not always successfully) to keep the conversation within three-dimensional magnetism. The fact that Simplicio sometimes presents my own point of view, while in Galileo's book [1] he represents the Pope, should not be given any special meaning.

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Magnes – Since we are here gathered to celebrate Magnetism, I thought it appropriate to celebrate also the privileged role of magnetism in statistical physics. Is it not true that statistical physicists introduced Ising model, XY models, Hubbard models, Heisenberg models everywhere in Physics, even though these models were created for magnetism? And those people, if they want to describe ferroelectricity [1,2], superfluidity [3], superconductivity [4,5], polymers [6] or ... brain [7,8], most often start with introducing spins, even if those are purely fictive, a mere language to describe two-level system.

Stoches – Indeed a number of magnetic problems have been a prey for statistical mechanics, and we have no time here to discuss them all. We have to leave aside, for instance, one-dimensional systems (solitons [9,10], Haldane effect [11], etc.), incommensurate phases [12], ANNNI models [12,13,14]. But we should speak in some detail of phase transitions in systems with frozen disorder (random fields and spin glasses). Before that, we should also briefly discuss critical phenomena in clean systems, including not very recent results which should be recalled in order to make recent achievements understandable.

Simplicio – And, this year, it would be improper not to say something on superconductivity, even briefly.

Stochas – Obviously we have very much to say in little time and we cannot avoid to be superficial.

Simplicio – One can wonder whether magnetism is really the best field to check statistical mechanics. A great advantage is that the symmetry with respect to magnetisation reversal (or time reversal to be pedantic) is total in the absence of a magnetic field and can be violated at will. On the other hand, if you want to measure critical exponents for instance, magnetic data are most often blurred by crossovers [15] from Ising to XY and then to magnetically isotropic (Heisenberg) behaviour.

Stochas – In principle, crossover functions can also be calculated, not only the exponents [16].

Magnes – Moreover, the crossovers which discontent Simplicio are not always present in magnetism. In fact, the exponent $\gamma = 1.3$ of the Heisenberg model was already hidden, but present in susceptibility measurements on Ni made by Weiss and Forrer [16] in the 30's, and which could only be understood more than 30 years later ! [17]

Stochas – Indeed, if they had used log-log paper, Weiss and Forrer would have been indeed the first to publish a non-trivial critical exponent. Instead, the first was L. Onsager in 1944 [18], who incidentally was not particularly interested in magnetism. The critical exponents he obtained for the two-dimensional Ising model have been experimentally checked in adsorbed layers [19], in reconstructed surfaces [20], and only occasionnally in magnetism [21].

Simplicio – Of course, magnetism is most often three-dimensional. and the exact method of Onsager cannot be generalized in 3 dimensions.

Stochas – It is indeed remarkable that so few exact results are available in 3 dimensions, and so many in 2 dimensions.

Simplicio – A great success of the last few years seems to be the use of conformal invariance, which yields a number of exact results in 2 dimensions. What is the resonance of such methods in magnetism ?

Stochas – Firstly, conformal invariance yields certain results in 3 dimensions. For instance it yields very general informations on the long distance behaviour of three-point and four-point correlation functions [22,23]. More precise informations are only available in two-dimensions. For instance, in the (two-dimensional) Ising model, which is of particular interest in magnetism, one finds at T_c

$$< S(\vec{r}_1)S(\vec{r}_2)S(\vec{r}_3)S(\vec{r}_4) >= \frac{1}{2}\left|\frac{z_{13}z_{24}}{z_{12}z_{23}z_{34}z_{41}}\right|^{1/4} \sum_{\sigma=\pm 1} f_\sigma(\zeta)f_\sigma(\zeta^*)$$

where $z_{jm} = x_{jm} + iy_{jm}$ if $\vec{r}_j - \vec{r}_m = (x_{jm}, y_{jm})$; $\zeta = z_{12}z_{23}/z_{34}z_{41}$ and $f_o(\zeta) = \sqrt{1+\sigma\sqrt{1-\zeta}}$.

Magnes – It is not easy for me to measure 4-spin correlation functions. I prefer the pair-correlation, which I can measure by neutron scattering.

Stochas – The methods also yields informations on finite-size effects on the pair-correlation, but in the Ising model all that is known since Onsager.

Simplicio – I would like to come back to 3 dimensions. There, if you want to calculate critical exponents, approximate renormalisation group methods seem to be unavoidable. Unfortunately, I am unable to follow them. The expansion in powers of $\epsilon = 4 - d$ is nicely explained by Wilson up to order 1 [17], but I failed in understanding any more advanced calculation.

Stochas – Indeed, Wilson's original method is not adequate for higher orders, and should be replaced by field-theoretical methods [24,25].

Magnes – What does it exactly means ?

Simplicio – One has sometimes the feeling, it just means a jargon unaccessible to common people. In Wilson's treatment [17], the motivation was clear: eliminate the most microscopic degrees of freedom, which are not universal, and get the free energy in terms of somewhat, less microscopic variables. A miracle was that the corresponding transformation could be handled by perturbation theory.

Stochas – Unfortunately, you get in trouble if you want to go beyond order 1 in $\epsilon = (4 - d)$. The trouble is that a simple-minded rescaling changes the form of the free-energy. Beyond order 2 there is really no other possibility than using so-called field-theoretic methods.

Magnes – Well you do that, and if I do not understand what you do, it is no tragedy. Anyway, you provide me with an expansion; for instance the exponent γ of the susceptibility $\chi \sim (T - T_c)^{-\gamma}$ is for the three-dimensional Ising model

$$\gamma = 1 + 0,167 \quad \epsilon + 0,077 \quad \epsilon^2 + 0.040 \quad \epsilon^3 + \cdots \tag{1}$$

and then I replace ϵ by 1 and compare with my experimental results in 3 dimensions.

Stochas – No, Magnes, do not do that ! The expansion (1) is a so-called asymptotic expansion. That is, it is divergent for any $\epsilon \neq 0$. In fact, if you do what you say, you get a better result to order 2 than to order 3, and order 4 would be catastrophic (the correct result is $\gamma \simeq 1,241$ [26]).

Magnes – This means that I cannot have a better approximation than order 2, and that it is useless to calculate the next order ?

Stochas – Fortunately not ! One knows how the high order terms behave, and one can use this knowledge to apply a certain transformation [26], due to Emile Borel, which makes the series convergent. The divergence of perturbation series already appears in much simpler problems, e.g. for <u>one</u> anharmonic oscillator [27,6].

Magnes – That is fine, thus you can provide me with good values of the critical exponents ?

Stochas – Absolutely. The following table gives the exponents η and ν for the most important magnetic models in 3 dimensions. $\gamma - (2-\eta)\nu, \alpha = 2 - d\nu, \beta = 2 - \alpha - \gamma$ are easily deduced.

	Ising Model	XY-Model	Heisenberg Model
η	0,031	0,033	0,033
ν	0,630	0,669	0,705

Magnes – Stochas, I am glad to have got so complete informations about the critical exponents. I know you are also able to say a lot about amplitudes, scaling functions, crossover functions, etc. However, you have been talking about clean systems. I have been told that Statistical mechanics is not in such a good position about impure systems. And, you know, my chemists do not give me always very clean samples.

Stochas – Frozen disorder does change the universality class and, for instance, critical exponents. These can sometimes be calculated simply by including the degree of disorder into the renormalization process. You just get one more equation which takes care of this additional variable to be renormalized. For instance the critical exponents of ferromagnets with weak bond disorder were calculated a long time ago [28] using the ϵ-expansion, which in this case turns out to be a $\sqrt{\epsilon}$ expansion. In dimension 2, where this expansion cannot be applied, the specific heat C was more recently [29a] found to diverge as $C \sim \ell n\ [\ell n|T-T_c|^{-1}]$ and very recently it was found that [29b], at T_c

$$\overline{<(S_o \cdot S_{\vec{R}})>^2} \sim (\ell n\ R)^{1/4}/\sqrt{R}\ .$$

This is in contrast with a previous result [29a].

Simplicio – I have been impressed by the technical difficulties which have been overcome in these works. However, the fact that even specialists find contradictory results discourages me to try to understand these things. Perhaps I shall do better with random field models. I know that the upper critical dimension d_{cu} (above which the mean field theory yields good exponents) is moved from 4 to 6, so that if you want to make an expansion in powers of $\epsilon = d_{cu} - d$, you have $\epsilon = 3$ in d=3 dimensions, and the series should not be easy to use [30,31].

Stochas – We do not need them in many cases, because random fields also change the lower critical dimension, which turns out to be 4 for continuous spins with full rotation symmetry in 2 or 3 dimensions (of the spins) [29]. Only for spins with a discrete symmetry, e.g. the Ising model, magnetic long range order is not destroyed in 3 dimensions. [32,33,31]

Simplicio – Then, we should mainly discuss the random field Ising model. Another reason for to do so is that strict continuous symmetry does not exist in magnetism since there is always some anisotropy.

Magnes – And one more reason is that it allows me to make publicity for all the beautiful experiments done by magnetic people on the random field Ising model [34,35,36,37,38]. The beauty of magnetism is that you can change the random field in an antiferromagnet at will, just by changing the external, uniform field [39,40]. Thus, starting from a paramagnet, you can either cool it down in constant field or cool it down in zero field (Fig. 1) and switch the field on afterwards. And neutron scattering tells you that you do not get the same state ! [34,35,36] The zero field cooled (ZFC) sample is long range ordered while the field cooled (FC) one is not, and is probably made of domains. And both states seem to be equally stable.

Simplicio – This is rather well understood now [41], at least in the case of a Heisenberg system with a weak anisotropy like $Mn_xZn_{1-x}F_2$. Then the FC system does consist of domains, and is in principle metastable. However the decay rate has been estimated [41] to be unobservable on the lifetime of the Universe.

Magnes – But then how do you explain that the domains can reach a rather big size, say 500 Å, and then do not move any more ?

Simplicio – These 500 Å are a minimum size below which no domains can exist. Such a size might be, for instance, the domain wall thickness. In fact it is appreciably bigger, but related to the wall thickness [41].

Magnes – It is really sure that the stable structure is the long range ordered one, and that the lower critical dimension is not 3 as claimed a few years ago by several theorists ? [42,43]

Stochas – Yes, there is now an exact proof [44] that ferromagnetic order is not destroyed at low temperatures by weak random fields in three dimensions.

Simplicio – The method is a rather classical one: a lower bound is established for the magnetisation. Although the mathematical language is too complicated for me, I believe the argument is rather natural. On the other hand its result is purely qualitative: it state that a transition to conventional long range order occurs. Is it first order or continuous? In the latter case, what are the critical exponents ? Nobody knows. Quantitative methods of modern statistical mechanics fail. This surprise has a simple explanation: the free energy has a very large number of minima, which are in fact metastable states [45,46].

Magnes – These metastable states are in practice very important and useful. My permanent magnets, for instance, should absolutely stay in their metastable state. However, I understand that metastable states make a theorist's task difficult.

Stochas – The relevance of multiple states is clear in spin glasses, which I would like to discuss later. It is not so, however, in the random field Ising model, where one state, the one with long range order, has a much lower free energy than the other states, as shown by Imbrie [47] at T = 0, and by Bricmont and Kupiainen [44] at low temperature.

Simplicio – And this is also in agreement with the simple argument of Imry and Ma [29]: in d dimensions a domain of size R costs a surface energy of order JR^{d-1}, and the energy gain due to the random field H is typically $HR^{d/2}$, which is bigger if $d > d_{cl} = 2$. So the ferromagnetic state is stable at <u>low</u> temperature with respect to domain formation. On the other hand, if you want to treat the critical properties by standard renormalisation group methods, you find that no second order transition is possible except above a lower critical dimension $d_{cl} = 3$ instead of 2. This value ($d_{cl} = 3$) is now believed to be wrong. The method fails because, near T_c, many minima of the Landau free energy functional are relevant, and cannot easily weighted by the correct Boltzmann factor [45,48,49]. In charge density waves too, multiple minima can be responsible for mistakes ... [50,51].

Magnes – Let us rather stay in magnetism. You mentioned it is not known, whether the transition of the random field Ising model is first order or not. As a matter of fact, certain experiments suggest it is first order [38].

Simplicio – It is not necessarily convincing, since this model is subject to metastability and hysteresis, which appear already above the transition, even if it is continuous [45,52,53]. Thus, discontinuities of the staggered magnetisation may be observed, which are related to the disappearance of metastable states rather than a first order transition.

Magnes – It should be said also that some experimentalists [34,35,36] claim that the transition is continuous.

Stochas – On the other hand, numerical evaluations, and very approximate analytical arguments, suggest that, if the transition is continuous, the magnetisation exponent β is 0 ± 0.1 [31]. A negative value would be inconsistent, with a continuous transition, i.e. it would imply a first order transition. In short, the theory is not able to decide.

Simplicio – Since $\beta = 0$ for d = 2 and $\beta = 1/2$ for d = 6, it is tempting to assume a monotonous increase, and $\beta > 0$ for d = 3. Then the transition would be continuous.

Stochas – It has also been suggested on the basis of mean field theory and high temperature expansions [58] that it is continuous for weak fields and first order for high fields. Now, let me come back to the multiple free energy minima. I wish to convince Simplicio that they are no insuperable obstacle to quantitative many body methods. There are at least two examples. One is a domain wall in a two-dimensional random <u>bond</u> Ising model [54,55]. The other example is the spin-glass with infinite range interactions, or Sherrington-Kirkpatrick (SK) spin-glass [56,57]. Both models have been solved exactly.

Magnes – My samples have unfortunately neither 2 dimensions nor an infinity, but 3.

Stochas – Two-dimensional problems are unfortunately easier to solve theretically and can give informations about three-dimensional physics. However, I do not want to bother you and we shall discuss the two-dimensional, random-bond-Ising model another day [58]. However, I would like to discuss the $\underline{\text{SK spin} - \text{glass}}$. In principle, infinite-ranged models can be treated by the mean-field approximation.

Simplicio – However, the mean-field equations have probably an infinite number of solutions, many of them are stable or metastable equilibrium states, and we wish to weight them with the appropriate Boltzmann factor and to calculate the relevant averages. This is a formidable task.

Stochas – However, De Dominicis and Young [59] claim to have done that. The equilibrium states are indeed those which minimize the so-called TAP-free energy of Thouless, Anderson and Palmer [60]

$$\mathcal{F}_{TAP} = -\frac{1}{2}\sum_{i,j} J_{ij}m_i m_j - \sum_j H_i m_i + \frac{T}{2}\sum_i \Big\{ (1+m_i)log\left[\frac{1}{2}(1-m_i)\right] + (1-m_i)log\left[\frac{1}{2}(1-m_i)\right] \Big\} - \frac{1}{4T}\sum_{i,j}(1-m_i^2)(1-m_j^2)J_{ij}^2 \,. \qquad (2)$$

Simplicio – I do recognize the first two-terms, which correspond to the Bragg-Williams or mean-field approximation [51]. The last term worries me because, at low temperature, it favours the value $m_i = 0$.

Stochas – TAP did notice that and remarked [60] that (2) is only correct if certain conditions "to be discussed elsewhere" are satisfied. Thus, as they say, $\mathcal{F}_{TAP}$ "is not a genuine free energy". However, Mézard, Parisi and Virasoro [62] have been able to derive the thermodynamics of the SK spin-glass from assumptions concerning in particular the distribution of free energy minima. Mézard et al proved that these assumptions (suggested by 10 years of discussions on the SK spin-glass) are at least self-consistent.

Simplicio – Thus, the SK spin glass is now accessible without replicas, Grassman variables, etc. ?

Stochas – In principle yes [9]. In particular, the order parameter can be defined without replicas. Defining an order parameter is already a problem if you have many solutions.

Simplicio – Why not to choose, as Edwards and Anderson [63]

$$q_{EA} = \frac{1}{N}\sum_i \overline{< S_i^2 >}$$

Stochas – It is not possible to obtain an exact self-consistent equation for q_{EA} alone. This is not surprising since q_{EA} gives a very partial information on a very complex system. Similarly, you cannot write a self-consistent equation for the pair correlation function in a fluid because it is a too partial information. A wonderful fact is that one can, in spin glasses, write (presumably exact) self-consistent equations for a slightly more complicated order parameter, which turns out to be a function q(x) of a single real variable. dx/dq is the probability that there are states having overlap q. The overlap between two states l, m is defined as [64]

$$q_{lm} = \frac{1}{N}\sum_i < S_i >_l < S_i >_m$$

Simplicio – You told me that there are self-consistent-equations for q(x). Can you write them ?

Stochas – I do not think we can do that here. The equations due to Parisi [65] and to Sompolinski [66], are complicated to write and to explain. The reader should look at the review by Binder and Young [64], p. 881.

Simplicio – You said these equations are "presumably" exact. Why ?

Stochas – There are other possible solutions of the problem. From the solutions which have been found, the Parisi-Sompolinski solution is the only one which has no inconsistency. With its order parameter which is a function, it represents a novel type of phase transition, in contrast with that of the random field Ising model, which is a tedious ferro- or antiferromagnetic transition.

Magnes – Unfortunately, this model with long range interactions is unphysical.

Stochas – In fact, d-dimensional models which short range interactions are expected to behave like SK spin glasses for $d > 6$ [64].

Magnes – But what can you say about d = 3 ?

Simplicio – The many metastable states are probably a feature of real spin glasses as well.

Stochas – Strictly speaking, the "thermodynamic states" of the SK model should be separated by barriers which diverge in the infinite volume limit. Thus they are not metastable, but stable. This might be a difference with real spin glasses [67].

Simplicio – However, even in short range, spin-glasses one can imagine a hierarchy of potential barriers with a maximum height which diverges in the infinite volume limit. I like to call the corresponding states "metastable". However, the properties of realistic spin glasses should be rather different from those of the SK model.

Magnes – A basic difference might be, the spinglass transition is destroyed by an external field [70a].

Simplicio – This has, however, been questioned [70b].

Magnes – Some properties of the SK model, such as the divergence [68] at T_c of the "nonlinear susceptibility" $\partial M/\partial H$ have been experimentally observed in real spin glasses [69].

Simplicio – This can be explained, in fact, by realistic, though phenomenological models [70].

Stochas – Yes, but the long range model did give the idea to experimentalists. Another instance is the coexistence of ferromagnetic order in one spin direction with spin glass order. It was established for long-range interactions [71,72], and checked experimentally [73], though not very often.

Simplicio – It had already been proved earlier for realistic spin-glasses [74].

Stochas – Yes, but his was published in an "obscure European journal" [75], and not in Physical Review Letters as required for a decent paper.

Magnes – Anyway, no theory explains the reentrance phenomenon observed more frequently, for instance [76] in the particularly popular system $Sr_{1-x}Eu_xS$ for x = 0. With decreasing temperature a ferromagnetic phase first appear, then the spin-glass phase, but the latter event is accompanied by the disappearance of ferromagnetism.

Stochas – The interest of the SK model goes far beyond magnetism. It can be related to optimisation problems [77,78] (e.g. in electronic technology) and neural networks [79,80,81,82,83].

Magnes – Are we not going too far away from magnetism ?

Stochas – I wanted to delight you with the idea that magnetism gave rise to concepts which can be important for understanding how our brain works. The basic ideas are [9]: i) each neuron is linked by synapses to many others, so there are long range interactions. ii) each neuron is subject to an electric current which can roughly take two possible values, so it is a two-level system or a spin 1/2.

Simplicio – Now, to get a spin glass, we need a Hamiltonian – here an Ising Hamiltonian since we have Ising spins. Is our brain subject to a Hamiltonian ?

Stochas – No, and indeed a hamiltonian model like Hopfield's [7] is less realistic than cellular automata [31]. However, the minimisation of an Ising Hamiltonian gives a simple prescription to define a memorized pattern, and since there are many minima in the spin-glass case, the system is able to memorize many patterns, i.e., many spin configurations. And then, it can recognize them. Even a sample with mistakes will be identified as the pattern which has, essentially the maximal overlap with it.

Simplicio – I am not sure it is a good method. If the machines at the post-office were to use such a method, they would work slowly and badly. If your Hopfield system has stored a small A and a big O, it will identify a small O as an A, because the overlap is better. If you want to identify a certain tree, say an oak, you will not count the leaves as the Hopfield model would do, but analyse the shape of one leaf or one fruit. I even wonder whether it would really be possible to store all configurations of all possible trees with all possible numbers of leaves and branches and fruits in our brain. Tell me how many neurons I have in my brain ?

Stochas – About 10^{12} [8].

Simplicio – Then I can store a huge number of informations, namely $2^{10^{12}}$. And you, my dear Stochas, probably more since you know a lot more than I.

Stochas – No Simplicio, a neural network of the Hopfield type with N neurons cannot store more than 2N patterns [108] if you want it to be able to learn any arbitrary new pattern. Following Montaigne, *"à une tête bien pleine je préfère une tête bien faite"*, thus a brain able to learn and not only to store.

Magnes – Dear colleagues, please, come back to the English language and magnetism! And first to spin glasses, real spin glasses with short range interactions. Is statistical physics now able to tell us whether three-dimensional spin-glasses have a transition or not ?

Stochas – There are few analytical result, e.g. the exact proof by Nishimori [84] of the absence of thermodynamic singularity on a certain line. The dominant belief among specialists at the moment (mainly motivated by simulations [85,86] and series [87]) but also by phenomenological arguments [88] is: i) Ising spin-glasses with short range interactions have a transition in three dimensions and not in two. ii) 3 is the lower critical dimension of Heisenberg spin glasses with

Rudermann-Kittel interactions [89]. iii) Heisenberg or XY spin-glasses with short range interactions do not order [90,91,92].

Simplicio – However, there is a simple argument [93a] to state that XY (and presumably Heisenberg) spin glasses have a lower critical dimension which is not higher than that of an Ising spin glass. Namely, you can define Ising spins with two XY spins: $(\vec{S}_1 \times \vec{S}_2)/|\vec{S}_1 \times \vec{S}_2|$ – and with 3 Heisenberg spins: $\vec{S}_1 \cdot (\vec{S}_2 \times \vec{S}_3)/|\vec{S}_1 \cdot (\vec{S}_2 \cdot \vec{S}_3)|$. Now, these fictive Ising spins interact, so we have transformed the system into an Ising model where, however, the interactions are not short-ranged, at least in the XY case. Monte Carlo simulations [93b] suggest that such an Ising order can occur in XY magnets even, surprisingly, in two dimensions.

Magnes – I doubt I could see experimentally such fictitious spins. I would like better you tell me something about amorphous ferromagnets. I have heard you claim that they do not exist, although I have quite a few of them in my lab.

Simplicio – In principle, a big amorphous ferromagnet at equilibrium is unstable with respect to domain formation [29]. Because of the random anisotropy A you gain an energy proportional to $AR^{d/2}$ when you create a domain of size R. Now the energy cost due to the domain wall is proportional to the surface R^{d-1}, multiplied by the thickness l, multiplied by the energy density $J(\vec{\nabla} M)^2 \approx J(M/l)^2$. For $l \approx R$ domain formation turns out to be favourable below 4 dimensions if the radius R is larger than some threshold $R_c(\sim J^2/A^2$ in 3 dimensions). However, even if your sample is bigger than R_c, the ferromagnetic state may be metastable. At equilibrium, an amorphous ferromagnet would presumably be an Ising spin-glass since each spin has two equivalent easy directions in the absence of an external field. But the equilibrium magnetisation correlation function $< \vec{M}(0) \cdot \vec{M}(r) >$ in zero-field should vanish, presumably as exp($-\kappa$r).

Stochas – There was some controversy about this point [94,50]. In the XY model in the absence of vortices, a weaker decay has been proposed [95]. Since vortices are always possible, the decay of the correlation function should be exponential, but κ might be smaller than $1/R_c$.

Magnes – I wonder if it is not easier to observe <u>non – equilibrium properties.</u> What you said about the huge metastability times in the random field Ising model is presumably general to all systems with frozen disorder. Then, if I want to observe a transition, the best method might be to study how the metastability sets in, i.e. how a typical relaxation diverges ?

Simplicio – Indeed, a modified Vogel-Fulcher law has been suggested. This means that the typical relaxation time τ should diverge as $\tau \sim exp\left[(T-T_c)/T_1\right]^{-p}$ instead of a power law. The original Vogel-Fulcher law, observed in glasses, corresponds to p = 1 and has been first suggested to hold in spin-glasses by an experimentalist [96a] who later recognized [96b] the difficulty to distinguish experimentally a Vogel-Fulcher law from a power. Values $p \neq 1$ were proposed by theorists in spin glasses [97] and in the random field Ising model [52,53]. The same argument [53] yields a frequency-dependent susceptibility $\chi''(\omega) \sim (\ell n\ \omega)^{\mu}$, where χ'' denotes the imaginary part.

Magnes – And such a law might well be in agreement with experiment on spin-glasses near the transition [98].

Simplicio – This would be a mechanism for "1/f noise", since $\chi''(\omega)/\omega \sim \ell n^{\mu}(1/\omega)/\omega$. True 1/f noise would correspond to $\mu = 0$, but would violate the rule that $\int_o \chi''(\omega)\frac{d\omega}{\omega}$ is finite. Thus, $\mu > 1$. Experimentally, it is hard to distinguish $\ell n^{\mu}(1/\omega)/\omega$ from $1/\omega$. The logarithm arises from the Arrhenius law, which states that the time $t = 1/\omega$ necessary to jump over a free energy barrier of height W is proportional to $exp(W/K_BT)$, so that $W \approx \ell n(1/\omega)$. Then, hypotheses on the distribution of barrier heights, which we have no time to explain, yield χ'' [53].

Stochas – It should be recalled that this mechanism is neither the universal explanation of the 1/f noise, nor the most general one.

Magnes – The mechanism you describe is related to metastability. The idea to observe metastability is not new. For instance, experimentalists have been measuring remanent magnetisation in spin-glasses. As in ferromagnets [99] it is well-known that the short-time decay is [100]

$$M(t) \simeq A(1 - q\ell nt/\tau) \tag{3}$$

Since we would not like M to go to $-\infty$, it has been suspected that the correct law is [101]

$$M(t) \simeq A(t/\tau)^{-q} \tag{4}$$

On the other hand, good fits have been obtained with a stretched exponential possibly corrected by a power law [102]

$$M(t) \simeq A exp\left[-(t/\tau)^{\lambda}\right] \tag{5}$$

What says the theory ?

Simplicio – Phenomenology [52] would predict, for Ising models with short range interactions,

$$M(t) \sim (\ell n\ t/\tau)^{-\mu} \tag{6}$$

but it might be that the demagnetising field (i.e. long range interactions) plays an important part in the decay of M.

Stochas – There are also theoretical justifications for a stretched exponential [64].

Magnes – I do not think many experimentalists are ready to accept (6). Anyway, we should now conclude. Perhaps we could summarize the situation in disordered systems as follows: statistical mechanics had beautiful successes, but also big failures. Even some points which seem well understood theoretically are difficult to check experimentally. I think I shall follow the example of many colleagues and convert myself to superconductivity, if it is not too late.

Stochas – It might be indeed that modern magnetism is non-magnetism, i.e. one should understood how magnetism can disappear and be replaced, for instance, for superconductivity. P.W. Anderson did a lot to help magnetic people in converting themselves into superconducting people, since he diverted the Hubbard model [103], originally invented for magnetic metals, into a model for superconductivity (Fig.). He even made the provocative suggestion that the spin 1/2 Heisenberg model with antiferromagnetic interactions might well be non-magnetic. Instead it would have a so-called resonating valence bond (RVB) state, where spins are paired, but not in a well-defined way. Instead, many different pairings are represented in the ground state.

Simplicio – I think this is analogous to benzene, which resonates between the 5 possible pairings. Now, is really spin 1/2 antiferromagnetism unstable ?

Stochas – Probably yes for the triangular lattice [104]. Probably not for other lattices. However, it might have an energy very close to that of the magnetic state, and be stabilized by impurities, which are anyway necessary for superconductivity. It can also be that additional complications present in high temperature superconductors play an important part in superconductivity, even if the RVB theory already contains most of the essential ingredients, as suggested by variational methods [105] numerical calculations [106] and perturbation theory [107].

Magnes – We shall probably know that in a few years. To reach that goal, statistical physics and magnetism will hopefully have a fruitful collaboration.

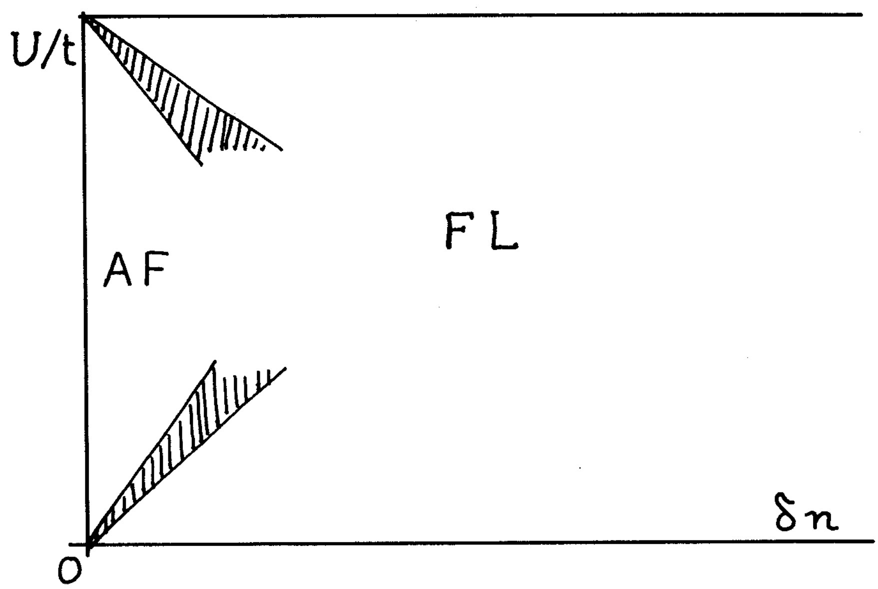

Fig. Competition between superconductivity (dashed area) and antiferromagnetism in the Hubbard model (H. Schulz, private commun.). δn is the excess of electrons per site ($\delta n = 0$ correspond to one electron per site, and $-1 < \delta n < 1$ for S electrons). U is the Coulomb repulsion and t the band width. "FL" (for Fermi liquids) designates the nonmagnetic, non superconducting state. A narrow superconducting region is expected theoretically both in the weak coupling and in the strong coupling case.

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