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Prof John Cardy
Department of Physics
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Dear John Cardy,

I am writing to you regarding a small detail of your book Scaling and Renormalization in statistical Physics (Cambridge University Press, 1996), which I rely on for part of my teaching duties. I indeed have a small critical comment to present. A footnote down page 18 drew my attention to your chapter 2 on mean-field theory. It echoed a pedagogical note[1] that a colleague of mine, Emmanuel Trizac, and I, wrote a while ago on this subject.

My remarks are concerned with your pages 17 and 18, and more specifically with equation (2.7). You use $f_{MF}(M)$ as a Landau free energy of a freely varying parameter M. In spite of having defined on page 17, after (2.2), M as the average magnetization, you write after (2.7) that M is yet unspecified and that the minima of f_{MF} yield the magnetization M. There is a footnote that states that f_{MF} can be proved to give an upper bound on the exact f.

In the present case, and because the final result is of course sensible, one does not notice that the method could, when extended to less trivial cases, somehow prove hazardous. After all, f_{MF} is only defined at M equal to the self-consistent

[1] http://www.msc.univ-paris7.fr/˜vanwijland/EJP-27-407-2006.pdf

magnetization and there is no reason to study its variations with M. Let me explain what I mean in more detail.

Suppose that I do not *a priori* know the physics underlying the ferromagnetic Ising model. I imagine that, say in two dimensions, the system may order differently on two square sublattices a and b (as it would indeed do for an antiferromagnetic coupling). I therefore introduce the two sublattice magnetizations M_a and M_b and construct f_{MF} in exactly the same way as you did, with the result,

$$\beta f_{MF}(M_a, M_b)/N = -\ln 2 + \frac{1}{2}\beta J M_a M_b - \frac{1}{2}\ln[\cosh \beta J M_a \cosh \beta J M_b]$$

At $M_a = M_b = M$ one of course recovers your expression.

But the "surprise" comes from the fact that the physical point, obtained by searching M_a and M_b such that

$$\frac{\partial f_{MF}}{\partial M_a} = \frac{\partial f_{MF}}{\partial M_b} = 0 \qquad (1)$$

does not minimize f_{MF}. Thus this mean-field free energy is not good for drawing free energy landscapes. In fact, below T_c, at the nontrivial physical solution of (1), f_{MF} has a saddle point! And the global minimum of f_{MF}, which lies at the boundaries of the (M_a, M_b) domain, is reached, still at $T < T_c$, for the antiferromagnetic state...

As you write in your footnote on page 18, there are other "less physically transparent" ways to carry out a mean-field analysis. My feeling is that it is a bit dangerous to introduce a variational Landau like free energy following the otherwise physically explicit approach presented in your book. Of course, the equation of state is insensitive to all this nitpicking!

With best regards,

Frédéric.