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Comment

Comment on "Finite-size scaling of the 5D Ising model"

E. LUIJTEN(*)

Department of Physics, Delft University of Technology
P.O. Box 5046, 2600 GA Delft, The Netherlands

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In a recent letter [1], Mon addresses the disagreement between a renormalization prediction [2] and Monte Carlo (MC) analyses of the critical value of the Binder cumulant in the five-dimensional Ising model. Whereas there is firm evidence supporting his overall conclusion, namely that the discrepancy can be explained by strong finite-size corrections [3], there is no such evidence for Mon's identification of the nature of this correction. This is highly relevant for the discussion of this problem, because neglecting the dominant correction to scaling can evidently explain the incorrect conclusions drawn from previous simulations.

The discrepancy concerns the value of the dimensionless amplitude ratio $Q \equiv \langle m^2 \rangle^2 / \langle m^4 \rangle$, or, equivalently, the so-called Binder cumulant $g \equiv -3 + 1/Q$. Several Monte Carlo simulations have yielded estimates for g which differ significantly from the renormalization prediction. In order to determine whether finite-size corrections may be responsible for this discrepancy, Mon calculates Monte Carlo estimates for two other universal amplitude ratios, *viz.* the first and third absolute magnetization moments divided by the appropriate power of the second magnetization moment, and compares these quantities with their respective renormalization predictions. Renormalization theory predicts a leading finite-size correction proportional to $L^{-1/2}$ (L is the linear system size), which originates from the so-called shift in the critical temperature. Mon analyzes the difference between the MC estimates and the respective predicted thermodynamic limits and concludes that these differences can indeed be described by an inverse square-root law. Subsequently he analyzes the Binder cumulant in a similar way and concludes that also in this case $L^{-1/2}$ corrections may account for the existing discrepancy between the renormalization prediction and MC calculations.

We find this explanation unconvincing. It is correct, as Mon claims, that an $L^{-1/2}$ correction was not taken into account in ref. [4], but the very reason for this was that such a correction could not be observed in the finite-size data, although the maximum available system size was larger than in ref. [1]. This is in agreement with the results presented in ref. [3], where the Binder cumulant was analyzed for a more general class of systems above the upper critical dimensionality. These systems are described by essentially the same renormalization equations, and their Binder cumulants were shown to coincide with the renormalization prediction within

(*) E-mail: erik@tntnhb3.tn.tudelft.nl

very small error margins. The amplitude of the $L^{-1/2}$ term, or more generally the $L^{y_\mathrm{i}/2}$ term (where y_i denotes the leading irrelevant exponent), turned out to be *negligible* for these systems. In fact, also Mon finds for the first and third absolute moment correction amplitudes which are equal to zero within one standard deviation. Furthermore, the finite-size corrections for these quantities are small compared to the error bars and the range of system sizes is limited. Hence, it seems well possible that a different power law can be fitted to them as well. However, the dominant contribution in ref. [3] came from the next-to-leading finite-size correction. This correction, which originates from a nonlinear term in the renormalization equations, is proportional to L^{-1} in the five-dimensional Ising model. Hence, instead of fitting the finite-size corrections to a square-root curve, it would be more clarifying if they were fitted to a general expression $\propto L^x$. Indeed, it is obvious from fig. 3 in ref. [1] that the $L^{-1/2}$ expression does not describe the corrections very well at all. Only three out of seven data points lie within one standard deviation from the curve. Mon remarks that this might indicate that the renormalization theory does not provide a good description of the tails of the distribution function for small L. Figure 3 in [1] shows that the finite-size corrections for the system sizes $5 \leq L \leq 11$ lie on a curve which is definitely steeper than $L^{-1/2}$. Data points for $L = 3, 4$ and $L = 12, 13, 14$—for which the first and third absolute moments are available in [1]—could help to clarify the L dependence of the finite-size correction, but are missing in fig. 3.

Figure 1 below shows the data points as extracted from fig. 3 in [1] along with Mon's $L^{-1/2}$ curve and a L^{-1} curve fitted to it. Naturally, if sufficient (accurate) data were available, one would use a curve containing both (or even more) terms. The L^{-1} curve clearly constitutes a much better description of the data, covering *all* available data points instead of only those for the three largest system sizes. Furthermore, we have tried to determine the exponent of the correction from the data: $x = -1.2 \pm 0.3$, *i.e.* within one standard deviation from -1. If we restrict this fit to the three largest system sizes, *i.e.* those used in ref. [1], we find that the exponent is completely undetermined, $x = -1.4 \pm 3.0$. Any conclusion based upon this

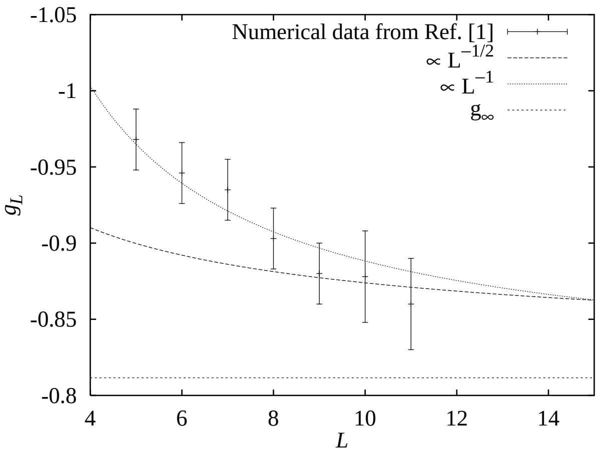

Fig. 1. – The critical fourth-order cumulant in the five-dimensional Ising model as a function of the linear system size L. The dashed curve indicates the $L^{-1/2}$ curve from ref. [1], fitted to the data for the three largest system sizes. The dotted curve denotes the next-to-leading finite-size correction, which is proportional to L^{-1}, fitted to all data points. The horizontal line marks the value of the fourth-order cumulant in the thermodynamic limit.

restricted data set thus seems premature. We believe that all this provides evidence that in the five-dimensional Ising model the L^{y_i} term constitutes the dominant finite-size correction for the Binder cumulant, just as it does for lower-dimensional systems with algebraically decaying (long-range) interactions [3], and that the above-mentioned discrepancies can be attributed to ignoring this correction. We see no reason to doubt the validity of the renormalization theory for the smaller system sizes, because it predicts adequate finite-size corrections.

Finally we remark that a solid basis for the "alternative theory" mentioned at the end of ref. [1] has not been given, while numerical evidence, *i.e.* a value for the Binder cumulant which differs significantly from the renormalization prediction, is absent. Furthermore it was noted in [3] that this alternative shift of the critical temperature is not well compatible with the renormalization theory. It would imply the presence of an additional term of unknown origin in one of the basic renormalization equations.

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