Reply to A. Patrascioiu’s and E. Seiler’s comment on our paper

Percolation properties of the 2D Heisenberg model

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The most of the problems raised by the authors of the comment \cite{1} about Ref. \cite{2} are based on claims which have not been written in \cite{2}, for instance almost all the introduction and the point (1) in \cite{1} are based on such non-existent claims.

Instead in Ref. \cite{2} we avoid to make claims not based on well-founded results. For instance in the abstract we write “... This result indicates how the model can avoid a previously conjectured Kosterlitz–Thouless [KT] phase transition...” and in the conclusive part we notice that “Our results exclude this massless phase for $T > 0.5$”. Therefore it seems to us that the opening sentence in the Comment \cite{1} “In a recent letter Allés et al. claim to show that the two dimensional classical Heisenberg model does not have a massless phase.” is strongly inadequate.

As for the points that appear in the Comment:

• (1) The purpose of the paper \cite{2} is to fill a gap in the research about the critical properties of the Heisenberg model. This gap is the following one: in Ref. \cite{3} a scenario was proposed where the 2D Heisenberg model should undergo a KT phase transition at a finite temperature. This scenario is based mainly on three hypotheses, the third one (which states the non-percolation of the $S$–type or equatorial clusters) being left in \cite{3} without a plausible justification. To back up that hypothesis a numerical
test was cited in [8] but the details of the numerics (temperature, size of
the lattice, etc.) and several data concerning the percolation properties
of the system, were completely skipped. The only quoted result was (see
beginning of section 4 in [8]) “We also tested numerically for \( \epsilon = 1/3 \),
There is no indication of percolation...”. On the contrary, such inter-
esting results about the critical properties should be put forward with a
thorough description of the hypotheses involved. Moreover, one would
like to understand how was possible to use the small value of epsilon
mentioned in Ref. [8], because that value implies a really tiny temper-

ature \( T \) and consequently it requires a huge lattice size. If “Everybody
agrees that at \( \beta = 2.0 \) the standard action model has a finite correla-
tion length”, see [1], also everybody would like to know details about the
numerics and the computer used to simulate the model at such a small

- (2) There is a statement in [8] which is repeated several times: all results
are valid for any versor \( \vec{n} \) of the internal symmetry space \( O(3) \). In par-
ticular, a percolating equatorial cluster is found for every \( \vec{n} \). Under these
conditions, we do not see how the percolation of the equatorial cluster
may lead to a breaking of the \( O(3) \) symmetry.

On the other hand, the fractal properties of a cluster are very sensitive
to the choice of parameters. By varying \( \epsilon \) around the value \( \epsilon = 1 \) (for \( T = 0.5 \)), one can make the data for \( \langle M_S \rangle / L^2 \) in Table 1 of [8] to change rather
dramatically. It is important (even in the case of a high temperature
regime, like \( T = 0.5 \)) to study this dependence. It is sensible to expect
that the fractal properties of the cluster show up at the threshold of
percolation. Again in [8] we do not claim that the cluster is a fractal, but
just write “... [the equatorial clusters] present a high degree of roughness
recalling a fractal structure”. To state any firmer claim, a deep analysis
of the errors and better statistics in Table 1 should be performed. All
these problems are currently investigated.

- (3) It is true that not all flimsy clusters can avoid a KT transition.
However this trivial truth proves nothing. Other kinds of lattices can
hold versions of the \( XY \) model with no transition (see for instance [9]).

On the other hand, the statement “... there should be no doubt that on
such a lattice [square holes of side length \( L \)] the \( O(2) \) model has a KT
phase transition for any finite \( L \)” is surprising. In Ref. [8] it is shown
that for any finite \( L \) the KT transition is still present but it approaches
\( T = 0 \) as \( L \) becomes larger. The idea of a fractal as the limit of some
kind of cluster should not be forgotten.

- (4) We agree with one of the sentences of this point: “It would be in-
teresting to verify this \textit{the existence of a KT transition for XY models on a fractal lattice}”. Yet we do not see the relevance of such an obvious claim.

We disagree however with the authors of [1] when they say “our argument does not depend on the existence of such a transition on that particular percolating cluster”. Instead, after the conclusions of Ref. [2], we think that the \textit{non–rigorous} proof proposed in [3] for the case when the equatorial cluster does percolate, heavily lies on whether or not such a transition is realized.

References

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