Response to "Comment on 'Supersymmetric Phase Transition'"

It is well known that many of the global and gauge symmetries which are broken spontaneously at low temperatures may be restored at sufficiently high energies.[1] In our Letter,[2] we have shown that in a particular globally supersymmetric model with a conserved fermion number, there exists a nonzero critical temperature, at which supersymmetry (SUSY) is broken. This occurs via a second-order phase transition which is akin to a Bose-Einstein condensation (BEC). We have introduced a chemical potential corresponding to the fermion number of the supermultiplet and solved the model exactly in the weak-coupling limit. No other approximations have been used.

The author of the Comment[3] claims that on "broad thermodynamic grounds," the model would always give BEC, irrespective of the details of the SUSY interactions or the Lagrangian density. The author may be right about his phenomenological considerations and broad thermodynamic grounds. But we feel that they are too broad and too phenomenological. For example, there is no sign of SUSY in his phenomenological free energy. Now, SUSY is not just a system of bosons and fermions. It is one of the relativistic symmetries aiming to unify particles with different intrinsic spin within a single multiplet. We do not see how SUSY can be implemented at the level of thermodynamics and we believe that the author has not done so. This is clear from one of the conclusions reached by the author that "bosons and fermions in the same multiplet obey Bose-Einstein statistics." To our knowledge nobody has ever suggested that, even in the context of supersymmetric theories. Thus his remarks are relevant to our paper only to the extent that the model considered by us also has fermions and bosons in it.

The author's remark about the introduction of μ by us is due to a misconception about the role of μ in our paper (see Ref. 4 in the Comment). It is true that the use of μ's for noncommutative supercharges is questionable. But it is fully justified to introduce a μ for an overall conserved quantity, namely fermion number, which is an ordinary "charge" and *not* a supercharge. As far as the author's comparison of our work with that of Paranjape, Taormina, and Wijewardhana[4] is concerned, we offer the following comments. Their work differs from ours in at least three aspects. They try to get SUSY breaking via interactions, while we work in the weak-coupling limit. They are unable to introduce a μ, but we do. They find no SUSY breaking in the four-dimensional model of Fayet (see the references in Ref. 4); we find broken SUSY in our model for $d > 2$. These differences make a comparison of their work with ours difficult.

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[1]See, e.g., S. Weinberg, Phys. Rev. D **9**, 3357 (1974).
[2]P. N. Pandita and S. Singh, Phys. Rev. Lett. **50**, 1550 (1983).
[3]P. Shukla, Phys. Rev. Lett. **52**, 236 (1984).
[4]M. B. Paranjape, T. Taormina, and L. C. R. Wijewardhana, Phys. Rev. Lett. **50**, 1350 (1983).

