Dear Editor,

Thank you for sending us referees' reports on our manuscript BV13170. We have carefully read the reports and made considerable efforts to modify the manuscript following their suggestions and answer their questions.

The First Referee has made a ridiculous/inadequate judgement of our work. (1) He/she even misunderstood the title! The “Grassmann” means a manifold labeled by U(N)/U(m)U(N-m), but not the Grassmann number for fermion fields in his/her understanding. (2) Furthermore, he/she said fermionic systems are not deserved to study for spin problems. However, the truth is that spin can be carried by both bosons and fermions. (3) In his/her mind, large N study is of no sense. He/she even asked “why should the limiting case of an infinite N be of interest”. Large N technique is a standard technique in many areas of physics: condensed matter, field theory, superstring theory, etc. The infinite N limit is often a saddle point, corresponding to mean field solution. While the 1/N expansion is an approach to include the fluctuations around the saddle point solution. (4) He/she criticized our work “coming from the authors’ curiosity”. Wow, this is too difficult to refute. We are indeed curious about the problem. If not, scientific research is of no sense. From the above, we think the Referee is not suited to judge our work. Anyway, we have also written a reply for the Referee. See below.

The Second and the Third Referees are more fair. We thank them to appreciate the value of our work as shown in their words such as “interesting”, “timely” and “worth reading”. Their reports contain some technical details, which indeed help us to refine our expressions to make our manuscript clearer, more readable and more solid. We have answered all their questions in the replies which are listed below.

We have also included a list of main changes below. We hope the Editor would consider our manuscript again.

Thank you very much.

Best regards,

Shan-Yue Wang, Da Wang and Qiang-Hua Wang

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A list of main changes:

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1. The first sentence of the abstract. “Motivated by the numerical evidence of a continuous phase transition between antiferromagnetic and paramagnetic phases in the half-filled SU(N) Hubbbard model…” is changed to “Motivated by the realization of SU(N) antiferromagnetism with the multi-row representations in cold atom physics…”

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Reply to the First Referee:

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We first thank the Referee for his/her quick report. However, we do not agree with many of his/her judgement. Replies to his/her criticisms are listed below.

>>Referee: “It is not obvious to me why one should pay attention to these cases which "mimic ... fermionic systems", if basically Bose atoms and the implicit Bose version of the Hubbard model are investigated in the literature (including ref 42 mentioned in the text).

In this work, we are interested in the spin dynamics. In both solid state and cold atom physics, spin can be carried by both bosons and fermions, which means we can study spin dynamics in both systems. Furthermore, a bosonic model can be equivalent to a spin model only when the bosons are “hardcore” which in fact is difficult to achieve in experiments. However, a fermionic model is more natural to describe the spin dynamics due to the Pauli’s principle and interactions between different spins. Therefore, it is reasonable to pay attention to fermionic system.

>>Referee: “Anyway, since Grassmanian calculations are often employed in quantum optics and many-body physics, one should get a clear justification of why to come back to them, and next to the 1/N expansion techniques.”

The Referee has misunderstood the meaning of the term “Grassmann”. It means a manifold labeled by U(N)/U(m)U(N-m), but not the Grassmann number in fermionic path integrals. We never use Grassmann path integral in our study.

>>Referee: “I understand the N>6 could be of interest, but why should the limiting case of an infinite N be of interest on physical grounds, in the realm of cold atom physics?”

Large N technique is a standard technique in many areas of physics: condensed matter, field theory, superstring theory, etc. The infinite N limit is often a saddle point, corresponding to the mean field solution. While the 1/N expansion is an approach to include the fluctuations around the saddle point solution.

>>Referee: “In its present form, the paper may be regarded as a formal technical result coming out from the authors' curiosity and a possibility (that might be quite unique) to get analytical results.”

We agree that this work is from our curiosity. But which scientific research is not? Our work solves the critical exponents of a new university class using a standard perturbation technique and more importantly, the predictions can be checked in experiments. The most important aspects of a university class are its symmetry and critical exponents. Our work is a “natural step in the direction of determining critical exponents” (as pointed by Referee-II), “quite interesting, timely, and worth reading” (as pointed by Referee-III).

Above all, we hope the Referee can reconsider our manuscript carefully.

Thank you.

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Reply to the Second Referee:

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We first thank the Referee for his appreciation of the importance of this work. The replies to his/her questions are listed below.

>>Referee: “What determines the choice of m for a given physical system? The authors refer to QMC works [28,29], but even there it is not evident what dictates the choice m=N/2 apart from higher symmetry. Why the critical exponents should depend on m at all?”

The parameter m is the number of the atoms in A-sublattice and (N-m) ones in B-sublattice. We highlight such a physical realization in this version in two places: one is above Eq.2 and the other in the discussion section: “Our theoretical predictions can be checked in cold atom experiments by loading $m$ atoms with hyperfine spin $(N-1)/2$ in A-site and $(N-m)$ atoms in B-site to achieve such an AF critical point.”

The critical exponents do not depend on m in large N limit. But in the first order expansion, they do depend on m. The simplest way to explain such a dependence is to check to self energy diagram of the Z field. Summation of internal spin indices give a global factor m as seen in Eq.18. As a result, \eta\_z should depend on m because it is extracted from the the k^2log(k) term of the self energy directly. We have added related discussions in the manuscript below Eq.25: “Such a behavior can be understood by counting the internal spin components: the vacuum polarizations of $i\lambda$ and $A$ as shown in Fig.1(a,b) are of order $N$, and thus the self energy as shown in Fig.1(c) is of order $m/N$. Since $\eta\_z$ is extracted from the self energy directly, its first order correction must be of order $m/N$.”

>>Referee: “The obtained results for the critical exponents, Eqs. (22)-(24) of the paper depend on N/m only. Is that the property of first-order 1/N expansion only? Are there any qualitative arguments why does that happen?

This question has some overlap with the above one. The m/N-behavior can be obtained by analyzing the order of the diagrams. We have added related discussions in the manuscript below Eq.25: “Such a behavior can be understood by counting the internal spin components: the vacuum polarizations of $i\lambda$ and $A$ as shown in Fig.1(a,b) are of order $N$, and thus the self energy as shown in Fig.1(c) is of order $m/N$. Since $\eta\_z$ is extracted from the self energy directly, its first order correction must be of order $m/N$. The remaining exponents, especially the two particle ones can be analyzed by considering more diagrams as shown in Fig.2.”

>>Referee: “During the calculation the authors implicitly assume m<<N while in the Discussion they apply the results to m=N/2 case, clearly violating the mentioned assumption. What additional class of diagrams start to contribute in the first order of 1/N expansion if m is of order of N? Is there a possibility to sum this additional class of diagrams? If not, the authors can not extend their results to m=N/2 case.”

Our calculations are based on 1/N expansion but the results show m/N dependence. Therefore, applying to m=N/2 is not safe. Nevertheless, it deserves a try as seen in some well known examples: Anderson’s spin wave theory based on large S but S=1/2, large-N theory of QCD depends on small 1/N but N=3 in fact. However, here, the fact is we fail.

There are no additional diagrams in the first order of 1/N expansion. But going beyond the first order, more terms like (m^2/N^2), (m^3/N^3), (m/N^2), (m^2/N^3) appear. We have added related discussions in the manuscript at the end of the section “1/N expansion”: “Finally, we point out that the $m/N$ correction is the property of the first order expansion. Higher orders would lead to other terms like $m/N^2$, $m^2/N^2$, etc. See the discussion section for details.”

Besides that, we have also added a remark in the discussion section: “(3) In order to go beyond the first order perturbation, one need calculate more diagrams. For example, in the vacuum polarization diagrams, the bare $Z$-lines should be replaced by dressed ones and give corrections of order $o(m/N)$ (corresponding to the first order). Then the self energy is corrected to $o(m^2/N^2)$. Repeating the above steps (a self-consistent procedure) would generate all orders of $(m/N)^{1,2,3\cdots}$. Besides the above diagrams, one can further consider the vertex correction terms, which yield other terms such as $o(1/N^2)$, $o(m/N^2)$ and so on.”

>>Referee: “Are there (other) applications of the non-linear sigma model on U(N)/(U(m)U(N-m)) manifold, especially for 1<m<<N where the results of the authors could be applicable?”

We are motivated by cold atom physics, and right now we can only apply it in the cold atom system “by loading $m$ atoms with hyperfine spin $(N-1)/2$ in A-site and $(N-m)$ atoms in B-site to achieve such an AF critical point” as discussed in the final section.

Above all, we hope the Referee could reconsider our manuscript.

Thank you.

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Reply to the Third Referee:

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We first thank the Referee for his/her positive judgement of our work: “interesting, timely and worth reading” which encourages us so much. The replies to his/her questions/suggestions are listed below.

>>Referee: “State more clearly what precisely is the novel result in the paper. I suspect it is the derivation of the model, and the eq. 22, but am not quite sure. The paper must contain some new result to be publishable.” (DW: This is the most difficult to answer.)

The Referee’s understanding is quite correct. The two particle exponents of AF moment are gauge invariant and measurable in experiments directly. This is just one of the motivation of this work. The other “new” result is just hidden in the derivation of the model. It provides a physical realization of such a novel critical point “by loading $m$ atoms with hyperfine spin $(N-1)/2$ in A-site and $(N-m)$ atoms in B-site to achieve such an AF critical point” as highlighted in the final section. In previous works by Macfarlane, Hikami and others, they just conceived such a model by symmetry analysis without physical applications. In this work, we put it in the cold atom background and derive it from a physical configuration. We believe this is an important step to revive this old model and arouse the interests of cold atom physicists or more general readers.

>>Referee: “Correct eq. 7, in which Z and Z^\dagger, should presumably exchange places, and the last term has the whole trace squared. (Z^dagger Z is a m-dimensional matrix, whereas Z Z^\dagger is not, in my understanding)”

Z’Z is a m-dimensional matrix, while ZZ’ is a N-dimensional matrix. In eq.7, due to the trace operation, both ZZ’ and Z’Z are correct, i.e. Tr(ZZ’ZZ’)=Tr(Z’ZZ’Z). The second term is the square of the whole trace. We have changed it to [Tr(ZZ’)]^2 for clarity.

>>Referee: “For completeness and reader's benefit, discuss the transformation property of the field Z under U(m) U(N-m), and the local vs. global invariance of the eq. 10, for example. The paper is too sketchy on this important issue.”

We are sorry to neglect the discussion about the symmetry of the model as it has already been discussed by MacFarlane, Hikami, Duerksen and Maharana (Refs.40-43). However, in this version, we accept the Referee’s suggestion to add the discussion of the symmetries for the benefit of general readers. Below Eq.10, we add that “The action is invariant under a global $U(N)$ and a local $U(m)$ transformations: $Z\rightarrow U\_NZU\_m$ and $A\_\mu\rightarrow U\_m^\dag A\_\mu U\_m-iU^\dag\partial\_\mu U$.”

>>Referee: “I do not understand the discussion on the "renormalized classical region" after eq. 11. The quantum model should presumably be d+1 - dimensional version of the classical d-dimensional model. Is there anything more to it here than this?”

In fact, the term of “renormalized classical region” is borrowed from Chubukov, Sachdev & Ye (Ref.34), Starykh (Ref.35) and Irkhin, Katanian & Katsnelson (Ref.36). It means when we solve critical exponents for a classical thermodynamic phase transition, the quantum fluctuations play no role and can be safely neglected by keeping only zero frequency in Matsubara frequency summations. We add more explanations about it in the new version at the end of section II: “we will only consider the renormalized classical region (keeping only zero frequency in Matsubara summations by neglecting quantum fluctuations) since it is sufficient to obtain the critical exponents for a thermodynamic phase transition.”

>>Referee: “Ask a colleague to correct many subtle mistakes in English, for a better read.”

We did pay a lot of efforts to the English written. Some grammar mistakes are corrected and some expressions are polished for better understanding.

Above all, we hope the Referee could reconsider this manuscript.

Thank you.