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Peer Review File

Spin pumping effect in non-Fermi liquid metals

Corresponding Author: Dr Xiao-Tian Zhang

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Communications Physics.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
In the manuscript, the authors proposed using spin pumping effect as a probe for non-Fermi liquid metals. The author considered a system of ferromagnetic insulator coupled to a non-Fermi liquid system via spin exchange, and the NFL metal is modeled using a Fermi surface near the Ising-Nematic quantum critical point. The authors calculates the magnon self-energy due to coupling to the quantum critical fermions, and proposes that it can be measured through the ferromagnetic resonance. I think the result of the manuscript is interesting and appropriate for nature communications physics. However, I am concerned with the exposition of the manuscript and the computation results that the authors presented, which should be addressed before publication.

1. In the paragraph at the end of page 4, the exposition about NFL field theory techniques is confusing and misleading. Various formalisms are mentioned: Large-Nf expansion, Migdal-Eliashberg theories, SYK models and anomaly-based approaches. The author does not make clear explanations of what different techniques achieve and what are the differences. Furthermore, many comments about the previous literature are incorrect. In the end, the authors seem to be saying that the ME framework does not work, but then why do they proceed with this approach? Some more detailed comments:
a) From Ref.41, I guess the authors define the large-Nf approach as the patch formulation pioneered by Sung-Sik Lee. However, none of the following Refs. [42-44] falls into this category.
b) The authors seem to define Migdal-Eliashberg equation as always neglecting the vertex corrections. However, there is a known caveat that this procedure is only justified for v_F q>>\Omega, and it has been acknowledged that for v_F q<<\Omega vertex corrections must be included. Therefore, the authors are not actually going beyond Eliashberg.
c) The authors write that Ref. [47], which they classify as Migdal-Eliashberg, contradicts Refs. [42-44]. This statement is incorrect. (a) The 1/3 exponent that the authors quote from [47] explicitly involves vertex correction, and therefore according to the authors' standard it should not be classified as "Eliashberg". (b) Refs. [42-44] are computing the conductivity of the NFL, whose associated polarization bubble contains odd-parity form factors. The result is therefore very different from what the authors are considering, a bosonic bubble with even-parity form factor. The results are not comparable, and no contradiction exists. (c) The same comments apply to Ref. [52] regarding the anomaly approaches.
2. In the following paragraph, the authors cited [33] and invoke coupling to localized spins to justify dropping the AL diagrams. The authors should address the self-consistency of this approximation. Would this introduce an additional term in the fermion self-energy that may modify the critical behavior?
3. In Eqs.(23)-(28), the authors computed the magnon self-energy at finite temperature. Certain manipulations in this step are not clearly justified neither in the main text nor in the supplement. Given that this is the main result that the authors present, the validity should be double checked. Detail comments:
a) While the zero-temperature computation before incorporated vertex correction. This part does not. Why is this justified? It seems that small-angle scattering is dominant both at T=0 and finite T.
b) In Eq.(23), there is potential typo: n_F(\omega) should be n_F(\omega')
c) Why is it legitimate to restrict the range of \omega' integration in [-T,T]? Furthermore, the authors are using the term low-temperature limit, but the expansion written above (25) is for T->\infty.
d) Same comment apply to the Kramers-Kronig integral in Eq.(27). Why is the integral range restricted to [-T,T]?
e) The result that the authors obtain in Eq. (25) is different from Ref. [33], which should scale as \omega/\gamma(T). It seems that the difference is due to the d-wave form factor of the self-energy, but I suggest the authors clarify this further in the

manuscript. In particular, if there is some isotropic background in the fermion self-energy, such as a disorder self-energy or a FL self-energy, would this make the result weaker or not? The authors should comment how these modify the experimental predictions.

Reviewer #3

(Remarks to the Author)
The authors compute the ferromagnetic resonance (FMR) of magnons in a 2D magnet coupled to a 2D non-Fermi liquid (nFL). The internal critical bosonic fluctuations of the nFL are assumed to be of the Ising-nematic kind. The magnon self-energy that governs the FMR is related to the spin susceptibility of the nFL, and is computed via calculation of the nFL spin susceptibility.

The calculation of the spin susceptibility involves some approximations whose validity isn't clear and which need to be clarified. The authors neglect Aslamazov-Larkin (AL) contributions of the nFL critical boson to the nFL spin vertex correction. Is this because these contributions somehow vanish for the Ising-nematic nFL? These contributions are typically required for spin conservation. The authors correctly mention that the spin conservation in the nFL is broken by coupling it to the 2D magnet, but I would think that the correct way to deal with this non-conservation is by including the nFL-magnet interactions (Eq. (3)) into the computation of the nFL spin susceptibility instead of dropping the AL contributions from the nFL's internal critical bosons. The contribution of \chi_{loc} in Eq. (8) could also be interesting especially because it is not affected by conservation laws, but it doesn't seem to have been discussed.

If the authors can address the above issues then I would say that the paper is publishable in Communications Physics.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
In the second version, the authors has made major revisions to the manuscript. The authors have directly addressed the issue of spin conservation of the itinerant fermion + nematic critical fluctuation system. The authors clarified that it is the magnon-fermion coupling that leads to a nonzero magnon susceptiblity which is later feeded back to compute the magnon self-energy and the FMR modulations. In summary, I belived the authors have made satisfactory revisions to the manuscript and I recommend for publication.

Some minor remarks:
1. In Eq.(9), the exponent is opposite to the results presented later. Is it a typo?

2. Both the fermion self-energy and the magnon self-energy are denoted by \Sigma. I recommend the authors change the notation for better readability.

3. The symbol `a' is used heavily in the computations but defined only in a distant inline equation. I suggest the authors remind the readers the meaning of 'a' near the computations.

Reviewer #3

(Remarks to the Author)
The authors have made corrections based on my concerns, and their results are now sensible and may be published in Communications Physics.

Reply letter for comments on "Spin pumping effect in non-Fermi liquid metals"

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I. LIST OF CHANGES

According to the critical comments of the two reviewers, we made substantial revisions to the maintext and supplementary information of the manuscript, including the equations, narratives and the figures. To reduce the labor for reviewers, we list all the major and minor revisions below. We are grateful for the reviewers' efforts which has significantly improved the quality of the manuscript.

A. Major revisions

1. We add a new Sec. "Non-Fermi liquid at the Ising nematic QCP" to specify the NFL properties at Ising nematic QCP before coupling to the adjacent FI. In this section, the spin conservation and vanishing of dynamic spin susceptibility are discussed.

2. We replace most of the contents in the Sec. "Divergent FMR modulation at $T = 0$" and Sec. "FMR modulations at finite temperatures". In these two revised sections, we carry out perturbative calculations on the dynamic spin susceptibility with respect to the interfacial spin exchange interaction.

3. The Feynman diagrams for the perturbative calculations are plotted in FIG. 2(c-g).

4. The main results of these two sections are in Eq.(22) [and text below] and Eq.(30), respectively. These results are qualitatively different from previous version which are illustrated in the FIG. 4.

5. We have corrected the calculations in the "Method" section which is moved to the Supplementary Information Sec. S2.

6. We modified the narratives below Eq.(10) where we explain: (1) $\chi_{\rm uni}(\omega)$ can only be non-zero upon evoking spin exchange coupling at magnetic heterostructure interface; (2) $\chi_{\rm loc}(\omega)$ is free from the restriction imposed by spin conservation.

7. The remarks below Eq.(11) in the previous version are replaced by narratives and equations from Eq.(11) to Eq.(15).

B. Minor revisions

1. We add Supplementary Information Sec. S3 and Sec. S5 to support the conclusions drawn in the maintext Sec. "Divergent FMR modulation at $T = 0$" and Sec. "FMR modulations at finite temperatures", respectively.

2. We move the panels in FIG. 4 of previous version to Supplementary Information Sec.S2.

3. We modified the narratives below Eq.(5) on the origin of the critical bosons.

4. We modified the narratives in the Abstract, Sec. Introduction (the paragraph "In this work") and Sec. Summary and Discussion according to the major changes;

II. RESPONSE TO REVIEWERS' COMMENTS

A. Reviewer #2

In the manuscript, the authors proposed using spin pumping effect as a probe for non-Fermi liquid metals. The author considered a system of ferromagnetic insulator coupled to a non-Fermi liquid system via spin exchange, and the NFL metal is modeled using a Fermi surface near the Ising-Nematic quantum critical point. The authors calculates the magnon self-energy due to coupling to the quantum critical fermions, and proposes that it can be measured through the ferromagnetic resonance. I think the result of the manuscript is interesting and appropriate for nature communications physics. However, I am concerned with the exposition of the manuscript and the computation results that the authors presented, which should be addressed before publication.

Response:

Thank you for providing a thoughtful summary of our results and acknowledging the value of the paper, despite certain issue remain problematic in previous calculations.

1. In the paragraph at the end of page 4, the exposition about NFL field theory techniques is confusing and misleading. Various formalisms are mentioned: Large-Nf expansion, Migdal-Eliashberg theories, SYK models and anomaly-based approaches. The author does not make clear explanations of what different techniques achieve and what are the differences. Furthermore, many comments about the previous literature are incorrect. In the end, the authors seem to be saying that the ME framework does not work, but then why do they proceed with this approach? Some more detailed comments: a) From Ref.41, I guess the authors define the large-Nf approach as the patch formulation pioneered by Sung-Sik Lee. However, none of the following Refs. [42-44] falls into this category. b) The authors seem to define Migdal-Eliashberg equation as always neglecting the vertex corrections. However, there is a known caveat that this procedure is only justified for $v_F q \gg \Omega$, *and it has been acknowledged that for* $v_F q \ll \Omega$ *vertex corrections must be included. Therefore, the authors are not actually going beyond Eliashberg. c) The authors write that Ref. [47], which they classify as Migdal-Eliashberg, contradicts Refs. [42-44]. This statement is incorrect. (a) The 1/3 exponent that the authors quote from [47] explicitly involves vertex correction, and therefore according to the authors' standard it should not be classified as "Eliashberg". (b) Refs. [42-44] are computing the conductivity of the NFL, whose associated polarization bubble contains odd-parity form factors. The result is therefore very different from what the authors are considering, a bosonic bubble with even-parity form factor. The results are not comparable, and no contradiction exists. (c) The same comments apply to Ref. [52] regarding the anomaly approaches.*

Response:

We are very grateful in receiving such detailed comments and suggestions form which we benefit a lot. Actually, we find that the miscellaneous discussions in "the paragraph at the end of page 4" of the previous version is no longer relevant and we delete this part. In the revised version, we make clear explanation of what techniques are being used in a newly added Sec. "Non-Fermi liquid at the Ising nematic QCP". This section is devoted to clarify the spin susceptibility at Ising nematic QCP without evoking the coupling to adjacent FI.

We came to realize that the uniform component of the dynamic spin susceptibility $\chi_{\rm uni}(\omega)$ of the NFL metal has to be zero for an isolated itinerant electron system. The $\boldsymbol{q} = 0$ component of the spin susceptibility is nothing but the spin density which is a conserved quantity. And, the conservation has nothing to do with the form factor (no matter even or odd) in the fermion-boson interaction at Ising nematic QCP. Actually, the itinerant spin is conserved near any charge-type Pomeranchuk instability. From a technical point of view, the spin conservation is related to the Ward-Takahashi identity which requires a careful accounting of the self-energy and vertex diagrams on equal-footing. Moreover, as pointed by the Referee, we are interested in the dynamic limit $v_F q \ll \Omega$ when evaluating the spin susceptibility (or equivalently the charge polarization). In this limit the Migdal approximation is not applicable. Namely, the external boson momentums are in the dynamic limit; while, the internal bosons are self-consistently solved in the opposite static limit. This is because the Landau damping term is obtained in the static limit $v_F q \gg \Omega$ which fundamentally changed the low-energy scalings and leads to the NFL behavior for the fermions. This strategy is commonly used, for instance see the calculation on nematic correlation function on Page 3 of *Klein et al.,* PRB 97, 155115 (2018).

Given the general physical argument above, the non-zero dynamic spin susceptibility obtained in Eq.(13,20) in previous version is wrong. The non-zero dynamic spin susceptibility can only be non-zero when the relaxation mechanism provided by magnons is considered which is not the case in previous calculation scheme. We have located a mistake in Eq.(47) in the previous version. After correcting this mistake, we draw the conclusion in Eq.(13) and paragraphs nearby in the revised maintext, where we indeed have a vanishing dynamic spin susceptibility $\chi_{\rm uni}(\omega) = 0$. Derivation details are moved to Supplementary Information Sec. 2b and 2c. We show that Ward identity remains valid for arbitrary deformation of the 2D Fermi surface at the charge-type Pomeranchuk instability.

2. In the following paragraph, the authors cited [33] and invoke coupling to localized spins to justify dropping the AL diagrams. The authors should address the self-consistency of this approximation. Would this introduce an additional term in the fermion self-energy that may modify the critical behavior?

Response:

Then, we evoke the interfacial spin exchange coupling between the itinerant electrons in NFL metal and the magnetic moments in FI. Conclusively, this coupling provides a spin relaxation channel for the NFL metal rendering $\chi_{\rm uni}(\omega) \neq 0$. In this way, the non-quasiparticle nature is brought out, in the meantime, this can be detected in the spin pumping experimental setup. Specifically, we perturbatively include all the leading order Feynman diagrams and evaluate the spin dynamics in $\chi_{\rm uni}(\omega)$ at $T = 0$ in Sec. "Divergent FMR modulation at $T = 0$" and at $T \neq 0$ in Sec. "FMR modulations at finite temperatures." To summarize, we first include the critical bosons near Ising nematic QCP self-consistently, then, we treat the coupling to magnons in a perturbative manner. And, we are not concerned with the stability of the NFL state after evoking the interfacial coupling since the magnons are gapped Goldstone modes which has been proven [see *Watanabe and Vishwanath*, PNAS 111, 16314 (2014)] to be unable to quantitatively modify the properties of itinerant electrons.

In the perturbative calculation scheme, we consider the self-energy (SE) and vertex (V) diagrams at the leading order and also analyze the potential relevant next order AL diagrams. The seemingly higher order AL diagrams are actually at same order as SE+V at the Pomeranchuk instability (in this case Ising nematic QCP). which is pointed out by *Chubukov et al.*, PRL 112, 037202 (2014). The crucial point in their proof lies in the fact that the effective four-fermion interaction is mediated by magnetic fluctuation where the dynamics in the spin susceptibility comes from the same fermions under consideration. However, the situation at the magnetic heterostructure interface is quite different in the sense that the effective interaction between fermions [NFL metal in green region of FIG. 1] is now mediated by the magnetic fluctuation with a different origin [FI in pink region of FIG. 1]. Importantly, the magnons in FI is gapped and has its own spin dynamics. In this regard, we show in Eq.(14) and maintext nearby in the revised version that the AL diagrams are officially irrelevant. Therefore, we focus on the SE+V diagrams defined in Eq.(15) which are illustrated in newly added FIG. 2 in the revised version.

3. In Eqs.(23)-(28), the authors computed the magnon self-energy at finite temperature. Certain manipulations in this step are not clearly justified neither in the main text nor in the supplement. Given that this is the main result that the authors present, the validity should be double checked. Detail comments: a) While the zero-temperature computation before incorporated vertex correction. This part does not. Why is this justified? It seems that small-angle scattering is dominant both at $T = 0$ and finite T.

Response:

In the revised version, we consider both the self-energy and vertex diagrams due to interaction with magnons at both $T = 0$ and finite T. While, when deriving the fermion and boson Green functions self-consistently, we ignore the vertex corrections at both $T = 0$ and finite T. These are justified results for the NFL at Ising nematic QCP: i) at $T = 0$ see Ref. 36 in the revised version; ii)at $T \neq 0$ see Sec. V of Ref. 42. We didn't specify this point in the manuscript, rather, we properly cite these two references around Eq.(11) and Eq.(23).

b) In Eq.(23), there is potential typo: $n_F(\omega)$ should be $n_F(\omega')$

Response:

Indeed. Thanks for the carefulness despite this equation has been removed in the revised version where similar equations using the Kramer-Kronig relations can be found in Eq.(24).

c) Why is it legitimate to restrict the range of ω' integration in $[-T, T]$? Furthermore, the authors are using the term low-temperature limit, but the expansion written above (25) is for $T \to \infty$. d) Same comment apply to the Kramers-Kronig integral in Eq.(27). Why is the integral range restricted to $[-T, T]$?

Response:

Thank you for asking this question. We are sorry for the unclearness and confusion. Actually, we have integrated over the entire interval bounded by a UV cutoff ω_c when using the Kramers-Kronig relations. In the revised version, please find the expressions in Eq.(25) and Eq.(S74). Restricting to the frequency interval to $[-T, T]$ is actually a justified approximation. The entire integration interval can be divided into the low-temperature regime $|\omega| \in [T, \omega_c]$ and the high-temperature regime $|\omega| \in [0, T]$. In these two regimes, the self-energy takes a distinct for as dictated in Eq.(S73) [see supplementary information Sec. S6b in the revised version]. In the end, we extract leading order expression for the finite-temperature regime under consideration. This temperature regime is finite yet low, in the sense that: i) "finite" means that there's a lower bound $|\omega| \ll T$; ii) "low" means that there's an upper bound $T \ll T_0$ below which the solution obtained for fermion and boson Green functions are legitimate [see narratives below Eq.(23)].

e) The result that the authors obtain in Eq. (25) is different from Ref. [33], which should scale as $\omega/\gamma(T)$. It seems that the difference is due to the d-wave form factor of the self-energy, but I suggest the authors clarify this further in the manuscript. In particular, if there is some isotropic background in the fermion self-energy, such as a disorder self-energy or a FL self-energy, would this make the result weaker or not? The authors should comment how these modify the experimental predictions.

Response:

This is a very good point! The expression obtained in Eq.(8) (or below Eq.(3)) in Ref. [33] is for the renormalized spin susceptibility, which is nothing but the charge polarization function. This form $\omega/\gamma(T)$ is the finite version of Landau damping term originated from particle-hole bubble of the gapless fermions. Let me first explain the difference between this result and our results in previous version and then in the revised version, respectively.

In the previous version [all references and equations in this paragraph are from previous old version], the quantity calculated in Eq.(26) is charge polarization bubble which is not exactly the boson polarization function at Ising nematic QCP. The boson polarization bubble at finite temperature has been calculated in Eq.(17) of *Punk*, PRB 94,195113 (2016). This expression is indeed of the form $\omega/\gamma(T)$, and this is consistent with the result from Ref. [33]. This form is nothing but the damping term for critical bosons at finite temperature. On the other hand, Eq.(26) differs from the boson polarization by the two external vertices which then yields a different form in Eq.(27).

In the revised version [all references and equations in this paragraph are from the revised version], the charge polarization bubble (or equivalent the spin susceptibility) is evaluated in Eq.(14,26,28) for finite temperatures. The result is of the form $\omega T^2/\gamma^2(T)$ which is also different from the boson polarization bubble by definition. As suggested by the Referee, we further remark this result around Eq.(30): i) linear-in-ω indicates that the Gilbert damping form is restored opposed to the zero-temperature results in Eq.(17,19,20); ii) the Gilbert damping coefficient now conceives an important information of the quantum critical region, namely the inverse correlation length or the boson mass $T^2/\gamma^2(T) \sim \xi^{-2}(T)$.

B. Reviewer #3

The authors compute the ferromagnetic resonance (FMR) of magnons in a 2D magnet coupled to a 2D non-Fermi liquid (nFL). The internal critical bosonic fluctuations of the nFL are assumed to be of the Ising-nematic kind. The magnon self-energy that governs the FMR is related to the spin susceptibility of the nFL, and is computed via calculation of the nFL spin susceptibility.

The calculation of the spin susceptibility involves some approximations whose validity isn't clear and which need to be clarified. The authors neglect Aslamazov-Larkin (AL) contributions of the nFL critical boson to the nFL spin vertex correction. Is this because these contributions somehow vanish for the Ising-nematic nFL? These contributions are typically required for spin conservation. The authors correctly mention that the spin conservation in the nFL is broken by coupling it to the 2D magnet, but I would think that the correct way to deal with this non-conservation is by including the nFL-magnet interactions (Eq. (3)) into the computation of the nFL spin susceptibility instead of dropping the AL contributions from the nFL's internal critical bosons.

Response:

We are indebted to this comment based on which we substantially changed the content of the manuscript that includes the calculation scheme, equations, figures and narratives. We follow the advise and carefully analyze the role of AL diagrams due to internal critical bosons and more importantly the magnons in the adjacent FI.

Firstly, we came to realize that the uniform component of the dynamic spin susceptibility $\chi_{\mathrm{uni}}(\omega)$ of the NFL metal has to be zero for an isolated itinerant electron system. The $\boldsymbol{q}=0$ component of the spin susceptibility is nothing but the spin density which is a conserved quantity. And, the conservation has nothing to do with the d-form factor in the fermion-boson interaction at Ising nematic QCP. Namely, the itinerant spin is conserved near any charge-type Pomeranchuk instability. Bearing this in mind, we carefully go through the calculations in the Method Section of the original version where we discovered a mistake in Eq.(47). The appropriate accounting of self-energy and vertex corrections from critical bosons on equal-footing successfully results in the Ward identity. This then leads to the conclusion in Eq.(13) [and paragraphs nearby in the revised maintext] dictating a vanishing dynamic spin susceptibility $\chi_{\mathrm{uni}}(\omega)=0$. Since the derivation merely generates a vanishing result, we enclose the corrected derivations in Supplementary Information Sec. 2b and 2c.

Secondly, we deal with the spin non-conservation due to the interfacial coupling to magnetic moments in a perturbative way. This is because the magnons in FI is a Goldstone mode which is unable to induce any NFL behaviors for the itinerant electrons [see *Watanabe and Vishwanath*, PNAS 111, 16314 (2014)] . Therefore, we don't take the magnons of the NFL metal into consideration self-consistently, rather, we regard the spin exchange coupling as a perturbative. We calculate all the Feynman diagrams at the leading order, namely the self-energy (SE), vertex (V) diagrams. Moreover, the seemingly higher order AL diagrams has been proven [*Chubukov et al.*, PRL 112, 037202 (2014)] to be on the same order as SE+V near a Pomeranchuk instability and important in restoring the spin conservation for the itinerant spins. The crucial point in their proof lies in the fact that the effective four-fermion interaction is mediated by magnetic fluctuation where the dynamics in the spin susceptibility comes from the same fermions under consideration. Using this fact, *Chubukov et al.* prove that AL diagrams is on the same order as SE+V. However, the situation at the magnetic heterostructure interface is quite different in the sense that the effective interaction between fermions [NFL metal in green region of FIG. 1] is now mediated by the magnetic fluctuation with a different origin [FI in pink region of FIG. 1]. Importantly, the magnons in FI is gapped and has its own spin dynamics. In this regard, we show in Eq.(14) and maintext nearby in the revised version that the AL diagrams are officially irrelevant. In the revised version, we delete most of previous calculations and replace by new calculations for the SE+V diagrams [see definition Eq.(15) in the revised version] at $T=0$ in Sec. "Divergent FMR modulation at $T=0$" and at $T\neq 0$ in Sec. "FMR modulations at finite temperatures."

The contribution of χ_{loc} in Eq. (8) could also be interesting especially because it is not affected by conservation laws, but it doesn't seem to have been discussed.

Response:

Yes! χ_{loc} is free of the restriction imposed by conservation rule and can be very interesting and fundamentally different from the uniform component studied in the present paper. Also from the technical aspect, the calculations on the diagrams are very different which we are studying in an on-going project [see Ref. 47].

If the authors can address the above issues then I would say that the paper is publishable in Communications Physics.

Response:

Thank you for your critical comments and acknowledging the value of the paper.

Before addressing the reviewers' comments point by point, we list the major revisions as below:

- We have changed the notation for the magnon self-energy from Σ to Π to distinguish between the self-energies of fermions and bosons.
- The definition of 'a' is indeed far from its first appearance in the calculation. We have restated the meaning of 'a' at its first occurrence in the calculation and highlighted it in blue.

Response to Reviewer #2

In the second version, the authors has made major revisions to the manuscript. The authors have directly addressed the issue of spin conservation of the itinerant fermion + nematic critical fluctuation system. The authors clarified that it is the magnon-fermion coupling that leads to a nonzero magnon susceptiblity which is later feeded back to compute the magnon self-energy and the FMR modulations. In summary, I belived the authors have made satisfactory revisions to the manuscript and I recommend for publication.

Reply—We sincerely thank the reviewer for the insightful and precise summary of our manuscript. We also thank the reviewer for strongly positive evaluation on the novelty and advantages of our work.

Some minor remarks:
1. In Eq.(9), the exponent is opposite to the results presented later. Is it a typo?

Reply—We would like to point out that there is no typographical error in the exponents. The exponents in Eqs. (9) and (10) appear opposite because one represents the scaling of frequency, while the other corresponds to the scaling of the correlation length.

2. Both the fermion self-energy and the magnon self-energy are denoted by \Sigma. I recommend the authors change the notation for better readability.

Reply—We have changed the notation for the magnon self-energy from Σ to Π to distinguish between the self-energies of fermions and bosons.

3. The symbol `a' is used heavily in the computations but defined only in a distant inline equation. I suggest the authors remind the readers the meaning of 'a' near the computations.

Reply—The definition of ‘a’ is indeed far from its first appearance in the calculation. We have restated the meaning of ‘a’ at its first occurrence in the calculation and highlighted it in blue.

We appreciate the reviewer’s suggestions for these modifications. We found them very helpful and have incorporated the changes into the manuscript.

Response to Reviewer #3

The authors have made corrections based on my concerns, and their results are now sensible and may be published in Communications Physics.

Reply—We sincerely appreciate your positive evaluation on our work.