Response letter

Thank you for arranging the review of this manuscript. We are pleased that all three referees appreciated the manuscript. We also thank the referees for their numerous but minor suggestions for improvement. A detailed response to the referees is below. We trust the manuscript will now be judged suitable for publication.

Reply to referee report 1

- 1. p2: The points (1)-(4) emphasized by the authors seem uncontroversial to me. (1) is required by Noether's theorem, (2) is required by equivalence of ensembles. Point (3) I found to be a succinct statement that the generic hydrodynamic behaviour is that of a "Type II" Goldstone mode with diffusive broadening, complementing the recent study of Ref. [41]. Point (4) is also to be expected, as internal symmetries differ from "mixed" symmetries in several basic respects.

  I think it would be helpful to clarify, even in the introduction, why Points (1)-(4) are worth emphasizing, and how some of the more trivial confusions arose in the first place.

Response: As one example of a reference that said contradictory things to our claims, we note that in Ref. [5], it is stated that:

> "nonAbelian hydrodynamics in the conventional sense of describing the collective flow of quantum numbers in the classical liquid does not even exist".

There are many other references that appear to present slightly misleading statements about hydrodynamics in theories with global vs. gauge symmetries, etc., so we think it is good to be crystal clear about these 4 points.

- 2. p2: I suspect that the repeated emphasis on Point (1) throughout the manuscript is based on a misconstruction by the authors of the motivation behind various works in the condensed matter literature, which study the hydrodynamics of systems with non-Abelian symmetries in physically realistic, constrained regimes, in which the number of dynamical degrees of freedom can differ from rk(G) = r. Whether or not such regimes are meaningful at asymptotically large scales certainly merits discussion, but there is not much engagement with this subtlety in the paper (beyond footnote 2).

Two concrete and well-studied examples with Abelian U(1) symmetry are the lattice Gross-Pitaevskii model (Kulkarni, Lamacraft, PRA '13 and Kulkarni, Huse, Spohn, PRA '15) and the easy-plane classical XXZ model (Ref. 62). Both of these models exhibit an emergent, low-temperature "conservation of phase difference", which leads to robust signatures of anomalous (KPZ) broadening on accessible timescales.

For this reason, I find the remark that there are r hydrodynamic modes "...at all orders in the hydrodynamic derivative expansion..." meaningless, without further qualification. If additional slow modes need to be added by hand, then there is an error in this expansion at some finite order. I realize that there is a distinction between studying some effective theory at all orders and describing a given lattice model at all orders, but this does not seem to be acknowledged clearly in the paper (e.g. the claim of validity "to all orders" is repeated in Secs. 2.2 and 2.3.)

Response: Typically, if there is an additional approximate slow mode, then it will decay with some long but finite lifetime. This is what one of us called "quasihydrodynamics" in Ref. [31]. Ref. [32] (as one example) shows that such quasihydrodynamic regimes can indeed occur, but usually at finite temperature where there is a qualitatively wide range of time scales where quasihydrodynamics takes place. Indeed, this physics would be beyond the approach of this mansucript, which is focused only on the genuine effective theory that persists at the very longest length/time scales: this is what we call hydrodynamics.

- 3. p4: The claim is repeated that "We will see that all charges Q_A represent independent hydrodynamic degrees of freedom, and that, in a domain of length L, this conclusion will be robust to all orders in the perturbative hydrodynamic expansion in L^{-1} . This conclusion can be found in [12, 13], but appears to conflict with other literature [5]"

  Could the authors clarify how, precisely, this claim conflicts with other literature? Are the earlier approaches simply incorrect, or applicable to a particular regime of times and temperatures?

  As far as I understand, the claim of accuracy to "all orders" is based Eq. (2.12). While this is an appealing result, it states simply that the Hilbert space on a fluid cell may be stratified into sectors of distinct n_A, up to exponentially small corrections, i.e. is a statement about thermodynamic ensembles (see also Point 16.) The extrapolation to "hydrodynamic degrees of freedom" seems too hasty: dynamical constraints routinely emerge in low-temperature and quantum coherent settings, and such constraints need not contradict Eq. (2.12).

Response: We noted in point 1 of our response what the conflict was with Ref. [5], and have correspondingly addressed this point in the main text of the paper.

We would not generally expect that the long time dynamics of an ergodic, thermalizing system at finite energy density is governed by emergent degrees of freedom that cannot be predicted from symmetries alone. While this can happpen towards zero temperature, typically at finite temperature the hydrodynamic description is always valid (assuming that any spontaneously broken symmetries, etc., have been accounted for). A simple example of this is a Fermi liquid, which at finite temperature is described by conventional hydrodynamics; at zero temperature in contrast, there are infinitely many emergent conservation laws. This emergence changes the scalings of hydrodynamic transport coefficients, but not hydrodynamics itself (at finite temperature).

- 4. p6: Section 3 begins "In this Section we present a systematic effective field theory of hydrodynamics. One important feature of this formulation is that it captures in full generality the effects of hydrodynamic stochastic fluctuations. We will use this in later Sections to rule out any anomalous hydrodynamic transport behavior of spin diffusion for non-integrable SU(2)-invariant spin chains."

  As the authors are presumably aware, the idea of including stochastic fluctuations in nonlinear hydrodynamics dates to the 1970s, and the most thoroughly tested incarnation of the theory in this decade is the version due to Spohn and van Beijeren, in Ref. 57. I think it would be appropriate to mention these bodies of work at the start of Section 3, to allow the reader to place Refs. 32 and 33 in their proper context. Since the theory of Ref. 57 is also stochastic and effective, it would be helpful to clarify what precisely is gained by the more recent approaches. For example, is there a situation in which the approach of Ref. 57 leads to demonstrably incorrect conclusions?

  I raise this point because the statement in the abstract, that the "low energy theory is a set of coupled noisy diffusion equations", is simply what is expected on general grounds of symmetry and linear response theory. It is therefore consistent with the theory of Ref. 57 (as noted explicitly around Eq. 2.10 of Ref. 62). I would argue that anomalous transport in non-integrable, isotropic chains was already "ruled out" in Ref. 62: what remained to be explained is why it is seen at all!

Response: As we note to another referee, one can in principle recover our results using more standard nonlinear fluctuating hydro, but the purpose of this paper was to independently check this result using a more modern framework that is more adept at dealing with noise and nonlinearity. In particular, we do not need to make quite as many assumptions as are required in the MSR approach. The purpose of this was to make sure that the fluctuating hydrodynamic prediction that the reviewer might have expected based on the older work was not incorrect, and we indeed found that it is correct. But given the controversy the reviewer notes, we expect that everyone agrees it was worthwhile to double check the hydrodynamic predictions themselves.

- 5. p7: It is not clear to me what prediction the authors' effective theory makes for lattice systems with energy conservation and internal U(1) symmetry (presumably normal diffusion for both). I would like to see a discussion of this in relation to the above mentioned works on emergent KPZ physics in the lattice GPE and classical XXZ models.

Response: We added a comment below eq. (3.14) on the diffusive scaling for systems with energy conservation and U(1) symmetry. The discussion of this subsection is aimed at outlining the main aspects of the formalism that describes hydrodynamics in the simplest scenario of a single conserved charge. To reach KPZ one needs to have a very long lived momentum; see our response to point 2 above.

- 6. p9: It was not clear to me when the discussion began to specialize to SU(2). I was also curious as to how the counting in Eq. (3.29) and (3.30) generalizes, specifically whether its difficulty depends, in a simple way, on the rank and dimension of the underlying Lie algebra.

Response: Starting from after eq. (3.28), the discussion specializes to the SU(2) case. We added a sentence below eq. (3.28) to clarify this. At higher-rank, there will be additional tensor structures coming from the fact that $\mathrm{tr}(\Phi^3)$ does not vanish for Φ belonging to the adjoint representation of SU(N), with N$>$ 2. We are not aware of a thorough counting of transport coefficients at higher-rank, although it would be interesting to explore this more systematically.

- 7. p9: Eq. 3.36. I think it would be helpful to describe the physics of various terms in this equation, seeing as it aspires to a universal description of hydrodynamic spin currents in lattice systems, and is therefore a significant result of the paper.

Response: We added an explanation of each term appearing in the current constitutive relations below eq. (3.36). We thank the referee for this suggestion.

- 8. p10, top. It is stated that "and their coefficients can have unrelated values. This will not lead to qualitative changes in the predictions discussed below". However, suppose lambda is the dominant term. One qualitative change, peculiar to non-Abelian symmetry, occurs in all dimensions: the hydrodynamics of spin becomes approximately norm-preserving, and the number of hydrodynamic degrees of freedom is approximately reduced by one. Further, in $d = 1$, the hydrodynamics in this regime is near-integrable, regardless of the underlying microscopic dynamics. I think the existence of such unusual regimes within the leading-order effective theory merits comment, and is related to the subtleties arising in Section 6.

Response: This is an interesting suggestion raised by the referee. We did indeed think about this while working on this project. But in the end, we realized that the effective theory for the norm preserving hydrodynamics necessarily always has relevant operators, and so would be a fine tuned result. The stable hydrodynamic fixed point is the vanilla diffusion fixed point, which is what we described in this paper. In the absence of fine tuning, we do not expect a lattice model to be governed by the norm-preserving hydrodynamics. We added a comment below

(3.36) mentioning that if λ is the only nonvanishing coefficient, the dynamics would be integrable in one dimension.

- 9. p12: it is stated "This same singularity was found in Ref. [42], where high-temperature non-analyticities in transport of many-body chains were first discovered. In that case energy conservation was crucial in order to have this effect. Here, the nonlinearity associated to λ is present even in systems which break energy conservation; the crucial ingredient is the presence of multiple densities".

  This is slightly inaccurate: it is already clear from the treatment of Ref. [42] that multiple densities are the "crucial ingredient" to achieve this effect at relatively low order in mode-coupling theory.

Response: We slightly reworded this part to emphasize that it was already clear from [42] that the crucial ingredient is to have multiple densities.

- 10. Section 4: I found the arguments in this section elegant, but a little confusing in relation to the rest of the paper. In 4.1, the non-commutativity of the generators was only noted at the end of the subsection, and it was not clear what symmetry group one should keep in mind. In 4.2, the lack of commutativity between rotations and translations in continuous space was alluded to (apparently the lattice of the title had been discarded). But in the continuum, it is expected that vorticity conservation is a consequence of the Euler equation.

  A global aspect of the presentation I find peculiar is the overall neglect of simple dynamical constraints that generically emerge in realistic systems (e.g. phase coherence, spin coherence) and confound simple intuitions about the counting of conservation laws, at the expense of a detailed discussion of rather exotic dipole-type constraints.

Response: As emphasized above, the emergence phenomenon is typically destroyed at finite energy density, and therefore can be neglected in a hydrodynamic effective field theory. To help guiding the reader, we added a sentence at the beinning of sec. 4.1 mentioning that.

- 11. Section 6. Let me first say that I find the analysis in this Section 6 a thorough and valuable contribution to this old debate. At the same time, the discussion of others' recent studies of the same problem misses several important aspects of their motivation. I want to emphasize that on a qualitative level, there is little disagreement between the recent proposal that the torsional mode is responsible for the observation of anomalous transport at short times, and the argument in Sec. 6.4: both proposals ultimately attribute this anomalous behaviour to the reactive, norm-conserving piece of the spin current in Eq. 6.17.

  One subtlety that I think is missing from the discussion of Sec. 6 is the fact that the continuum limit of the classical Heisenberg model is integrable. The authors correctly mention "dangerous irrelevance", but quantifying the "danger" is precisely what previous studies were trying to achieve. It should be noted that this aspect of the debate is analogous to the decades of discussion on the Fermi-Pasta-Ulam-Tsingou chain, which similarly has an integrable continuum limit (the KdV equation), with subtle consequences for ergodicity of the FPUT dynamics.

Finally, I want to mention that the physics at issue is similar to the emergent constraints arising in the lattice Gross-Pitaevskii model mentioned above: in particular, the continuum Landau-Lifshitz equation maps exactly to the continuum NLSE, with the torsion variable mapping to the differential phase. The existence of a hydrodynamic regime for the torsional mode thus has an established precedent in the literature on non-linear fluctuating hydrodynamics.

Response: We thank the referee for the overall positive opinion of our contribution.

We agree that previous studies were trying to understand dangerous irrelevance. In the coarse-grained theory such effects lead to the generation of dissipation and noise fluctuations (as was previously found in the study of the non-zero temperature Gross-Pitaevskii model mentioned by the referee). For this reason, in fluctuating hydrodynamics there is no subtlety in the dimensional counting: the concept of dangerous irrelevance only exists from the point of view of the microscopic theory.

We agree that the continuum limit of the microscopic equations is integrable. This is why we had mentioned the importance of umklapp scattering.

- 12. p17: "Two specific possible flaws in this argument are that: (1) it appears to rely on a breakdown of ergodicity, due to the time dependence in (6.5), yet we are most interested in looking at diffusion in equilibrium correlators; (2) it has been emphasized in Ref. [62] that the continuum limit of the Heisenberg model does not apply at infinite temperature, as umklapp processes cannot be ignored."

  I think Point (1) could be better phrased. Ref [21], based on Ref [61], proposed a hydrodynamic equation that, if trusted for arbitrarily long times, implies a logarithmic divergence of D. There are multiple possibilities: a. The dynamics of the classical Heisenberg model is not fully ergodic. b. The description of Ref. [21] is approximately correct, up to some crossover time at which normal diffusion is restored. c. The description of Ref. [21] is incorrect, as tau is not a hydrodynamic mode.

  As far as I can tell, the numerics presented are not inconsistent with viewpoint (b) and the authors are in favour of viewpoint (c). My own view is that more work needs to be done to rule out even (a) conclusively (e.g. the model has non-trivial exact solutions), and that this will require techniques beyond hydrodynamics. Regarding point (2), I think it should be mentioned that for the spin-1/2 XXX model, the continuum limit of the Heisenberg model does seem to apply at infinite temperature. This is not an intuitive result, but its microscopic derivation was provided recently by de Nardis et al., PRL 125, 070601. From this perspective, the authors' Point (2) amounts to the claim that microscopic integrability suppresses Umklapp processes - why is this the case?

Response: We emphasize that the plots in fig. 8 show absence of any transient beyond the thermalization scale as far as the spin autocorrelation function is concerned. Of course, it could be that long cross-over times might be present in other sectors of the theory, but given the motivation of the paper we mostly focused on spin transport. We do advocate that τ is not a long lived degree of freedom, for the reasons we stated in the manuscript (and below). We also emphasize that Section 6.4 presents a quantitative explanation for the plots in fig. 8 based on the vanilla diffusion theory.

Regarding point (2), we note that the high-temperature Heisenberg model defined on the lattice cannot be described by the Heisenberg model in the continuum limit, as the latter is integrable. Again, the discrepancy with the continuum limit is due to Umklapp processes. In this paper, we are exploring the hydrodynamics in a generic chaotic system with SU(2) symmetry and we find that the classical Heisenberg model at high temperature, particularly the numerical behaviors observed in Ref. [21], are consistent with our theoretical prediction. Furthermore, the spin-$\frac{1}{2}$ XXX model is integrable and therefore the physics will be qualitatively different than the chaotic dynamics being studied here. Our point (2) does not make any logical implication regarding the integrable case.

- 13. p17: "From the perspective of our effective theory, we make two general comments: (1) SU(2) symmetry is not sufficient to render τ a hydrodynamic mode, and even if the effects of the lattice were small, they are expected to be "dangerously irrelevant", breaking conservation laws and qualitatively changing the character of hydrodynamics. (2) We can easily test for whether τ is hydrodynamic on the lattice in numerical simulations. As detailed below, we find no evidence that τ is long-lived."

  Again, I take issue with the wording here. Regarding Point (1): the proposal of tau as a hydrodynamic mode in Ref. [17] arose from considering fluctuating hydrodynamics about a frozen, classical, ferromagnetic spin-wave background. Existence of such stationary backgrounds is a much stronger condition than SU(2) symmetry, and was implicit in the treatments Refs. [17,21]. In particular, there is no expectation that the models studied in Eq. 6.10, and Figs. 4 and 5, exhibit a torsional mode: in the language of Ref. [21], the continuum limit of the Hamiltonian evolution is not even defined.

  Regarding Point (2), I disagree that one can "easily test" whether tau is hydrodynamic from the numerical simulations performed; please see below.

Response: We do not see the connection between our fully-fledged hydrodynamic framework and the approach of Ref. [17], which deals with an integrable system, see point above.

- 14. p.20 "there is an unambiguous numerical test: an emergent hydrodynamic mode must arise in the symmetry sector of τ , defined in (6.4)."

  I do not agree that this diagnostic is unambiguous. The derivation of tau suggests that it is a long-wavelength, highly extended degree of freedom, and I am sceptical that it could be probed by the naïve point splitting (6.11j).

  For example, consider making this claim in the context of the (quantum) spin-1/2 XXZ model. There (6.11j) is actually a conserved charge density of the model (the energy current), and therefore a hydrodynamic mode. However, no-one is claiming that the energy current in the XXZ chain should have a superdiffusive component. Yet in this case, superdiffusion has been conclusively traced to the validity of a large-scale description in terms of the Landau-Lifshitz equation (de Nardis et al., PRL 125, 070601).

  All this is to say that the analogous discussion for the spin-1/2 XXZ model implies that the emergence of a hydrodynamic torsional mode at long length scales need not have anything to do with the local operator in (6.11j). As a point of principle, this is unrelated to whether or not the underlying model is integrable, and contradicts the view that the proposed numerical test is "unambiguous".

Response: We believe that if there are emergent conservation laws that do not overlap at all with the simple local operators we have described, then those emergent degrees of freedom also should not overlap with the spin current, spin density, etc., and therefore should be invisible in hydrodynamic correlation functions.

Again, the XXZ model is integrable, so we do not want to compare in much detail to our chaotic model.

- 15. p.21 "This is distinct from the theoretical proposal in [21], which requires an emergent hydrodynamic mode for τ operator... However, we emphasize that there is no sign of any additional emergent hydrodynamic modes, in any channel."

  The proposal of Ref. [21] is simply not applicable in this case, see Point 13.

Response: We removed the first sentence above from the draft.

- 16. p.27, eq. A7. "we have approximated that $\lambda_{1,2}$ are small": why is this admissible? The integration over these variables is unbounded. And what is the precise "limit" being taken in A8? I raise these points because Eq. 2.12 is advertised as a non-perturbative result to "all orders", but the surrounding discussion is not correspondingly precise.

Response: We have clarified the discussion in the Appendix to emphasize the saddle point integral being done.

Reply to referee report 2

- page 2: "focus in on" (delete "in")

Response: We corrected the typo. We thank the referee for pointing this out.

- The manuscript quotes *most of* recent literature on anomalous transport in (integrable & non-integrable) classical and quantum spin chains with SU(2) symmetry: Ref. [16-30]. I do not understand the order of this quotation, it is not chronological, but also not in relevance or importance? (I would suggest chronological order though, which also suggests causal relations between references). I also believe that some important related references are missing in this list, e.g: PRL 106 , 220601 (2011): The very first reference which observed anomalous spin transport in SU(2) quantum Heisenberg spin chain, PRL 111 , 040602 (2013), JSP 179, 110 (2020), SciPost Phys. 9, 038 (2020), PRL 125, 070601 (2020)

Response: We added the above references to our list of citations.

- page 6: I find the wording "effective field theory of hydrodynamics" a bit strange, hydrodynamics is a field theory!?

Response: By "effective field theory" we want to emphasize that our formulation is based on an action principle together with basic symmetry requirements. Traditionally, hydrodynamics is based on a set of "effective" equations which are obtained from a combination of symmetry and phenomenological constraints (such as the second law of thermodynamics). We updated the beginning of sec. 3.1 and expanded on this point.

- Notation for time-and space component of charge/current vector, e.g. $J^\mu = (J^t, J^i)$ looks quite confusing at places. t and i often resemble running time-space variables. Wouldn't (J^0, J^1) be clearer?

Response: The current index convention is aimed at distinguishing between space and time, since the systems discussed do not in general possess relativistic invariance. Since this notation appears in many places of the rather long paper we prefer to keep it in its current form, but are happy to do otherwise if the referee has a strong preference.

- Introducing the fields $\varphi_{1,2}$ in (3.3) is not well explained. How do they relate to $\lambda_{1,2}$ in (3.2)? This part I find really difficult to read, maybe the presentation and be more streamlined and made more concise?

Response: We updated the discussion around eqs. (3.3)-(3.5) to clarify the introduction of $\varphi_{1,2}$, and how they are related to $\lambda_{1,2}$.

- After Eq. (3.8b), η and η_μ are introduced as "PT eigenvalues". It needs to be better explained, the eigenvalues are actually $(-1)^\eta$..., what values do η take?

Response: There was indeed a typo in the introduction of η, η_μ. We corrected the text below (3.8b) and mentioned explicitly that the values η, η_μ can take are ± 1.

- What is a "dissipative superfluid"?

Response: We thank the referee for pointing out the ambiguity of this wording. We removed "dissipative" as we understand that it could be confusing.

- After (3.33), what does the value 0 of the second index of EM field tensor F_{i0} mean, before this was designated as "t", see my comment above?

Response: This was a typo, we changed F_{i0} to F_{it}.

- After (3.34): It sounds nontrivial to me that you can annul three terms by varying 2 parameters (μ, β), is this correct? (of course there might be a nontrivial relation among these terms)

Response: We could not understand this comment. If the referee is referring to eq. (3.43), this condition is a standard result for which we remind to ref. [39] (eq. (5.29)) for a more explicit derivation.

- (3.46,3.47) and the text in between: I do not understand what indices 0 and 3 (which are sometimes superscript and sumetimes subscript) mean there? (has to do with flavor?)

Response: We added a more explicit explanation of the notation below eq. (3.46). In particular, we clarified that β_0 and $\hat{\mu}_0$ denote background values. To avoid confusion, we wrote all flavor indices as superscripts.

- Last eq. of (3.48), I guess the indices "I" and "J" need to be the same there?

Response: We agree with the referee. We corrected eq. (3.48).

- (4.1), I guess x_i as a space component of space-time point x, or? But why does the integration variable then read (only) "x"? Again, notation could be made much clearer in this part!

Response: The referee understood correctly the notation. We added a clarification on this notation below eq. (4.1).

- Subsection (4.3): The authors say that bringing dipole-moment (fractonic) constraints bring "nothing new" to the game. But they find that this implies the hydrodynamics to be "frozen", right?

  So this is a rather remarkable result to me!

Response: We are glad that the referee thinks our result is interesting! By nothing "non-trivial" here we meant that there are no propagating hydrodynamic degrees of freedom, but we have changed the wording to emphasize what we meant.

- In the same paragraph the authors say that G has to be "simple" and a few lines below that it needs to be "semisimple". Please clarify.

Response: The argument above eq. (4.14) indeed applies to simple Lie groups. For semisimple Lie groups the situation is more subtle and that argument does not directly apply. The point of the counterexample below eq. (4.14) is to furnish a simple case where the argument given above does not work. To avoid generating confusion, we changed "semisimple" to "simple".

- Eq. (5.9): I do not understand the meaning of of the first equation, I guess this is a low frequency dependence of D? but then writing $log(\omega)^{0.5}$ does not make much sense (we need to put absolute values twice at least).

Response: Yes, (5.9) indicates the frequency dependence of D. The purpose of that equation is to show the scaling at low-frequency rather than providing a precise quantitative answer. We have replaced $\omega \to \frac{1}{\omega}$ to emphasize that the diffusion constant is positive.

Reply to referee report 3

- I agree with many of the suggestions of the other 2 referees. One additional point: it would be useful to clarify in section II what parts could be obtained without the formalism of Refs. [32,33] . (Say using the Martin-Siggia-Rose framework, which I suspect will be more familiar to most readers.)

Response: We have added some comments to the start of Section 3. Given what was a controversy about whether standard fluctuating hydrodynamics could fail in SU(2) symmetric spin chains, we wanted to use this modern technique which is very systematic and does not start from an assumed form of the equations of motion, in order to check that nothing strange happens. In the end, our conclusions do agree with those predicted by MSR for the leading long-time tail $D \sim 1 + \sqrt{\omega}$. Besides being more systematic, the formalism of Section 3 can incorporate non-Gaussian contributions of the noise, as well as quantum fluctuations, although in this context they are more subleading.