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Interactive comment on “On the ion-inertial range density power spectra in solar wind turbulence” *by* Rudolf A. Treumann et al.

Anonymous Referee #1

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The manuscript "On the ion-inertial range density power spectra in solar wind turbulence" by Treumann et al. describes a possible explanation for the occasional "bumps" seen in the power spectra density in solar wind measurements around the ion scale. This interpretation is based on the electric ion response due to quasi-neutrality and pressure balance with the magnetic power spectra. The idea is interesting and worth to be published. The derivations seems to be logical from the basic equations and (sometimes) strong assumptions, but my main concern (in General Comment 1) is that the organization of the paper makes it hard to read (for me and maybe other readers), in the sense that is easy to lose focus: many times I did not understand the purpose of some paragraphs or derivations for backing up the main conclusion of this paper. I maybe wrong, but I would appreciate at least the opinion of the authors about this

issue.

The rest of my comments, including some suggestions for improvement, are as follows.

GENERAL COMMENTS:

1) It is not clear to me (and maybe to some future readers of this manuscript) what part of all this discussion of this manuscript (mostly the equations) can be traced back to previous references and what part can be considered original as to the knowledge of the authors. It would be nice to add some references to the equations where appropriate. I understand that they propose a different interpretation for the bump in the density spectrum, but many of the equations seem (to me) to be widely known while others not that much (probably due to some special assumptions). I am not sure where to draw the line in order to say that from this point on it is mostly different from the standard theory of turbulence. It might be due to the organization of this manuscript, it is not that easy to follow, there are some digressions halfway that make difficult to see what is exactly the focus and what is the purpose of each section/paragraph toward the final goal, there is lack of "roadmap". In summary, the presentation and organization of the ideas could be improved.

2) The explanation of the flattening or bump in the density spectra of turbulent fluctuations due to Kinetic Alfvén waves is very popular (see, e.g, [Howes2011] and later reviews by the same author, [Harmon2005], etc). Why is not properly discussed here?. It is fine to put forward an additional explanation, but I do not find fair to neglect a more common one without good arguments or comparison of their respective advantages/disadvantages (KAWs are mentioned only once in this paragraph in page8, "to propagate in the kinetic Alfvén mode, or attributed to dissipation. Though this is neither impossible nor can it be excluded here, the rather more convincing conclusion is that we are dealing with de-magnetised ions in the ion-inertial domain which respond to the turbulent induction electric field and become swept over the spacecraft by the fast solar wind flow" ...and that explanations given there is not actually convincing. And even

more so taking into account the large scatter in the data.)

3) In-situ measurements are not the only way to get density power spectra in the solar wind. Scintillation observations of radio wave propagation can also provide complementary insights and cross-comparisons. That technique is often used to get density spectra in the interstellar medium ISM (mostly through diagnostics of pulsar radio-emission), since scintillation is dominated by small-scale density fluctuations [Armstrong1981]. Interestingly, scintillation observations in the solar wind have also revealed a kind of "bump" (or at least a flattening) in the power density spectra at ion scales [Coles1989, Spangler1995, Harmon2005], while their counterpart in the ISM seems to be missing [Haverkorn2013 and references therein]. It would be interesting to add at least a comment about this in this manuscript.

SPECIFIC COMMENTS:

- page2, lines3-4: "....K or I-K spectra respectively their anisotropic generalisation"—-> This is not clear, is there may be a missing word? (respectively with respect to what?)

- page2, line9: Please define the angular brackets in Eq 1 (those are mentioned much later).

- page2, line30: What is "some unknown strongly damped virtual evanescent mode..."? A wave mode with all those adjectives is very vague, it can mean anything and confuse readers (it confused me, specially with the phrase after that says "...contribute to both density and temperature"). Please be more specific and/or provide references.

- page2, line31: "in the higher frequency range their existence"—> please specify to what is being referred by "their"

- page3, Fig1, and page6, Figure2. How do exactly the data of those figures was obtained from the data of Safrankova2016? Please provide more details for the sake of clarity.

- page3, line1: "Above frequencies > 10ˆ Hz.....those frequencies exceeding the ion cy-

clotron frequency"—-> Please specify the local ion cyclotron frequency for those in-situ measurements (otherwise it is hard to compare frequencies in Hertz with characteristic plasma frequencies in dimensionless units).

- page4, lines4-5: "...electromotive force terms..... do not vary and, in considering the effects of the electric field on turbulence, can be dropped"—> This does not seem straightforward for me. Could you please be more clear on how that conclusion was reached? (to neglect those terms?)

- page7, lines20-21: - "This makes the inclusion of the turbulent Hall electric field Eq. (4) in the general case difficult (not speaking about the additional effect introduced by the Hall term)." –> This sentence is hard to understand. What does it mean? In what sense the inclusion of that terms is "difficult" and what is the relation with the phrase in parenthesis? ("additional effect" with respect to what?)

- page8, line19: "This assumption corresponds to the complete neglect of the turbulent dispersion relation" —> what would be the difference with the inclusion of the "turbulent dispersion relation". I am missing the link because the concept of turbulent dispersion relation is non-standard or widely known.

- page8, line28: "indicate either the presence of new waves" —> what does "new waves" mean? New with respect to what? Most of the standard theory of plasma waves is known since the 60's (Stix, etc) so I am confused about the adjective "new" here.

- page9, line5: "related to the turbulent velocity fluctuations whose spectrum under weak conditions" —-> what is exactly "weak conditions" does it mean "weak turbulence conditions". Or some other assumption that is relaxed ?

-page10, line3: Please define BMSW (I think it is an instrument on board a spacecraft different from WIND, which is not mentioned here).

-page11, line 9-10: "(We may note at this place, that concerning IK spectra it seems improbable that they would be realised in the scale range of the ion-inertial domain.)" –> Why? why is that improbable? Please clarify?

-page11, lines9-10: it is mentioned that the occasional presence of a bump in the turbulent density power spectra can be due to the transient features in the solar wind, or local conditions such as vthi>VA. But it would be good to also discuss related possibilities. For example, the last inequality is related to the dependence of the spectral break on the ion plasma beta (already discussed in some of the references of this manuscript but not mentioned here), but it could be related to the heliocentric distance of the observations since the local plasma conditions will be different (which is of course related to the plasma beta). This has also been pointed out before (see also General Comment 3)

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