

Interactive comment on “Scaling laws in Hall-inertial range turbulence” by Yasuhito Narita et al.

Anonymous Referee #1

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General comments

The paper, "Scaling laws in Hall-inertial range turbulence", discusses the behavior of the Hall term from generalized Ohm's law in the context of turbulent systems. The authors focus on two-dimensional turbulence and develop phenomenological scaling laws for the wavenumber spectra of the magnetic field, electric field and particle number density between ion and electron scales. The authors find differences in the spectral behavior associated with the parallel (compressive) and perpendicular (incompressive) components of the magnetic fluctuations with respect to the mean magnetic field and qualitative similarities between the presented model and previous observations are noted. I find the paper to be well written and believe the analysis provides important insight into the role of the Hall term in turbulent plasmas, which may be useful for

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interpreting future high-resolution measurements of space plasma turbulence. My main comment is that further discussion of the differences between the presented model and previous observations could be provided, which would give useful insight on the limitations of the presented model. Additionally, I have noted some statements which could use clarification and additional references that may be relevant.

Specific Comments

- Pg. 3, line 19 – 22: I am unclear how the compressible and incompressible components of the magnetic field are obtained from $\delta B_H \cdot \delta J_H$ and $\delta B_H \times (\delta E_H \times B)$. Additionally, how is it possible for the incompressible component of B to be anti-parallel to δE_H if δE_H is in the k_{\perp} direction? Wouldn't this imply that the magnetic field has a divergence?
- Section 4: Some further discussion of differences between the presented theory and previous observations and the possible explanations for these differences may be useful.

For example:

-While the presented model is qualitatively similar to previous observations in that the magnetic energy spectra become steeper in the kinetic range, observed slopes are often steeper than $-7/3$ [for example as in Stawarz et al. JGR, 121, 11021, 2016; Chen et al. ApJ, 842, 122, 2017; Breuillard et al. ApJ, 859, 127, 2018].

-Would the presented theory predict that the fluctuations in the parallel component of the magnetic field should dominate the spectrum? Some observations [for example Stawarz et al. JGR, 121, 11021, 2016 and Chen et al. ApJ, 842, 122, 2017] seem to show the perpendicular component dominating into the kinetic range.

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-How does the predicted increasing density fluctuation spectra mesh with the observed decreasing density spectra in the kinetic range, for example as recently observed with MMS by Breuillard et al. [ApJ, 859, 127, 2018] and Chen et al. [ApJ, 842, 122, 2017]?

- Matteini et al. [MNRAS, 466, 945, 2017] and Franci et al. [ApJ, 812, 21, 2015] may also be relevant references to discuss, as they also consider the role of the Hall term in generating a linear ratio between the electric and magnetic field.

Technical Corrections

- Abstract and Section 2: While the 2D nature of the turbulence in the solar wind is mentioned in the introduction, I think it would be useful to explicitly state in the Abstract and in Section 2 that a 2D geometry (i.e. no parallel component of the wavevectors) is being considered in this paper.
- Pg. 1, line 21: For a 500 km/s flow speed, shouldn't the frequency range be 0.5 to 5 Hz?
- Pg. 4, line 5: I think the right-hand-side of eq. 5 should be positive instead of negative.
- Pg. 5, line 15: A citation for the author's previous publication which is being referred to would be helpful.
- Pg. 7, line 10: I think that "revoke" should be "invoke".
- Pg. 7, line 19-21: Can the authors provide a reference for the statement in the last sentence in Section 3.1.

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