

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

Anonymous Referee #2

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Narita et al. propose a phenomenological model of turbulence for ion kinetic scales on the basis of Hall-MHD. The paper is clearly written and coherent and appropriate for *Annales Geophysicae*. However, several relevant references are missing, including some that arrive to similar results using the same formalism (Hall-MHD). I would highly encourage the authors to write a more detailed discussion and introduction incorporating theoretical and observational results relevant to their work and to describe limitations of their model.

1. Contrary to what the authors are saying on L5, there are now strong indication of kinetic Alfvén waves in the ion kinetic range, e.g. Roberts et al. *GRL*, 45, 2018. The authors should include this reference.

2. Regarding the limits of Hall-MHD to describe ion kinetic scale turbulence, there is

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plenty in the literature available. Hall MHD is valid in the limit where the electron temperature is much greater than the ions temperature and when the inverse of the linear transit time for an ion is much smaller than the turbulent frequency and the inverse of the linear transit time for an electron, respectively. Thus, in the instance where the temperature of the ion is finite, phase-mixing and damping of modes ought to be taken into account. Perhaps a good recent reference that can be added and discussed is that of Howes et al., *Nonlinear Processes of Geophysics*, 16, 2009.

3. Schekochihin et al., *ApJ Supplement*, (2007) provides a detailed description of ion-scale turbulence for weakly collisional plasmas through the use of gyro kinetic. Gyrokinetic is a reduced anisotropic limit of Hall-MHD with comparable results to that of the authors. However, Gyrokinetic, unlike Hall-MHD, incorporates phase-mixing due to Landau damping (not cyclotron-resonance). Can the authors incorporate in their discussion a comparison of their results with that of Schekochihin et al..

4. Chen et al., *ApJ*, 122, 2017, among many others, report magnetic energy spectra that are steeper for ion kinetic range. Can the authors incorporate a more detailed discussion incorporating observational evidence that are quantitatively different from their theory? Perhaps differences between Hall-MHD turbulent estimates and observations can be used to quantify the contributions of kinetic physics at the ion scale?

5. Alexandrova et al. [Small scale energy cascade of the solar wind turbulence] arrive to a similar scaling as that of the authors using Hall-MHD. Can the authors differentiate their work from that of Alexandrova et al.

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