

## ***Interactive comment on “Solar Wind and Kinetic Heliophysics” by Eckart Marsch***

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Received and published: 17 September 2018

This is an enjoyable review which is a nice summary of the work Eckart presented in his Alfvén medal talk at the EGU in 2017.

My comments are minor, and I am making myself known as my comments are more in the vein of a constructive discussion than a criticism. As a researcher who has been in this field and has followed the field, and specifically the issue of Alfvén waves and turbulence, since the early 90s, I was a little bit surprised by your choice of citations and one of the papers you have defined to be "seminal". Clearly Parker's paper deserves the adjective, while I am a bit less convinced by Horbury's paper on turbulence anisotropy and definitely disagree on this word being used for the Cranmer Van Ballegooyin paper of 2005.

And the reason is really quite simple. The analysis of CR and VB is more in an "as-C1

trophysics" style, i.e. it is not rigorous and indeed ends up mixing different concepts a little bit like a french chef mixes ingredients, with no guarantees that the different ingredients are consistent with each other. That is why it can be so comprehensive, though at the strong risk of CONFUSING the issues. Let me explain: Figure 6 from CR and VB illustrated here shows velocity fluctuations BUT they are in completely different frequency regimes at different distances.

This is not addressed by CR and VB who are happy to drive their models at 5 minutes and then fit nonlinear cascade and turbulent energies at HOUR periods (Helios data): the claims in that paper of consistency are incorrect and their "perturbation technique" is not at all a consistent turbulence theory. It is not even clear that their iterative scheme converges. I have written definitely fewer papers on the topic but have attempted to be consistent in hypotheses and conclusions: I believe the Velli 1993, Velli et al. 1992, - where the discussion of linear vs nonlinear cascade is discussed and the linear theory rederived with extreme rigour - and Verdini et al. 2010 papers would be definitely worthy of citation here even with the caveat that not all works by other authors can be cited. Specifically Verdini et al. 2010 is a much more rigorously self-consistent theory.

I am fine with the work of CR and VB being cited and the figure quoted, but at least a clarifying remark on the different frequencies of the measurements involved should be present.

On the kinetics, I think a wider range of citations should also have been included again even within the bounds of a personal review: for example, on parametric decay, the paper of Malara and Velli 2006 and the works including the expanding box, and oblique parametric decay, by Hellinger et al. and Matteini et al. (preceding the Verscharen paper) should be included.

I am including the references to the papers I have mentioned here. Naturally I am not requesting that Eckart cite all of these papers. I hope he might take a look though and consider at least a few of them :). And I insist the caption of figure 6, and possibly

the text, explain that the data is not uniform but covers completely different frequency domains in terms of dominant energy contribution. I also think the adjective "seminal" should be removed from the citation of the CR and VB paper.

I would like to end by thanking Eckart for the many wonderful discussions we have had and his fundamental contributions to our field.

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