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Interactive comment on "On heating of solar wind protons by the breaking of large amplitude Alfvén waves" *by* Horia Comişel et al.

Anonymous Referee #2

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The manuscript #angeo-2018-14 by Comişel et al. discusses heating of protons during the course of nonlinear evolution of a large amplitude Alfven wave in the solar wind. The author used a hybrid code to conduct 1D, 2D, and 3D simulations all started with a circularly polarized pump Alfven wave imposed at the initial condition. The simulation results showed that an efficient proton heating in the perpendicular direction occurs in 3D, but not in 1D and 2D. I think the finding itself is interesting and the paper is potentially worth for publication. However, the authors' discussion does not explain how the discrepancy arises between the different simulations. Also, there is a plenty of room for improvement in the quality of presentation. Therefore, I believe that substantial revisions need to be made on the manuscript before the publication.

Major Issues

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The simulation results suggested that pitch-angle scatterings by both the pump and daughter Alfven waves are more important than the heating in the parallel direction via damping of ion sound waves in 3D. In contrast, 1D and 2D results suggested that the pitch-angle scatterings are less important compared to 3D. It is not entirely clear how the difference arises. If you look at Figure 1, you can clearly see that the ion sound wave amplitudes do not change much between 2D and 3D. On the other hand, the amplitude of the daughter Alfven wave in 3D is much less than that in 2D. Apparently, this contradicts with the behavior seen in the distribution function.

The 3D simulation presented in the paper is quite large and should contain a lot of information. My impression is that the authors did not make use of the benefits of the large-scale simulation. I would encourage the authors to conduct more detailed analysis and draw more physically sound and grounded conclusions based on the data.

Minor Issues

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* The authors used beta = 0.01, which is very small in comparison with the solar wind at 1 AU. Please consider to state the motivation for adopting this particular value. Also, the author may think it better to include discussion on the dependence of the plasma beta.

* P.2, L. 32: "the conditions of beta plasmas" does not make sense.

* P.3, L. 29: The sentence ending with "due to most probably the small value of electron beta used in simulation" needs more clarification.

* P.4, L. 25: What the authors meant by the sentence "We see that these arcs..." is not clear. I can guess what you wanted to mention, but in general, it is not a good idea to let the readers guess the meanings.

* Figure 2: Are the color scales linear or logarithmic? Are they the same or different between the four panels? It is difficult for me to see behaviors in the low-energy part of the distribution function. Is this the authors' intention?

* Figure 3: It would be better to add a description for the dashed lines in the caption. The authors' definition of v_perp should never become negative. You need a more clear description what the distribution functions in Figure 3 represent.

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