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Interactive comment on "Thermal electron anisotropy driven by kinetic Alfven waves in the Earth's magnetotail" *by* Alexander Lukin et al.

Anonymous Referee #1

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Anonymous review of article Thermal electron anisotropy driven by kinetic Alfvén waves in the Earth's magnetotail by Lukin et al, submitted to Annales Geophysicae.

In this manuscript, the authors have performed a statistical study of spacecraft measurements related to bursty bulk flows detected in the Earth's magnetotail, and show that (in agreement with previous works) the field measurements for the selected time periods are indicative of kinetic Alfvén waves (KAWs). They present both sample cases and statistics over 81 events. They also present measurements of electron flux anisotropies over various energy ranges, and select a mid-range (thermal and subthermal) band for further study. They propose that the parallel and perpendicular electric field enhancements detected and connected with KAWs are responsible for enhancements of parallel electron anisotropy in the energy range most likely to respond to KAW

excitement.

The article is well written and presented and is thorough in introducing pre-existing literature on the topic. The combined use of statistics and sample cases is good, and the statistics are sufficient. For the most part, the language is good, though some clarifications and minor language-editing is called for. With some extra work, I'm convinced this will be a good addition to the field. However, there are aspects to the work which seem unfinished and thus require still major work. I have listed major and minor comments separately.

1 Major improvement requests:

1) Figures 7, 8 and 10 and the main result, several points:

a) A blue-red color map would need to show very strong correlation to convince readers of a connection. In their current form, these images are not proof of the effect you are searching for. At the right-hand side (large field amplitudes) there is some darkening to blue (enhanced parallel anisotropy), but only at low energies - at high energies for $E_\perp$ the effect is opposite. Also, you state you have poor statistics there. How poor? Line plots with uncertainties or error bars for select energy channels would be the way to convince the reader here.

b) Why did you decide to use simply the electric field magnitude? This neglects the sign and time-integrated effects. I acknowledge that electrons are fast, but evolution of the ensemble population and it's anisotropy should be considered as an effect taking time. I would suggest looking at e.g. the wave scalar potential as the X-axis variable here instead of the E-field magnitude.

2) Why did you use the convectional electric field for normalizing electric fields? Or did you? A larger electric field will have a stronger effect on electrons, so in this respect no

normalization is warranted. If there are large variations in electric field, then perhaps some other approach is needed. Smaller electric fields will have a significant impact only on anisotropy of low energy particles. You already use the electron temperature to scale energies in selecting different ranges - perhaps use the square root of temperature to scale the electric field? I'm also uncertain of what has exactly been done as despite the text talking about normalization, Figures 7, 8, and 10 show mV/m as the unit.

If you are able to convince me that electric field normalization with the convectional field is required, please also give reasoning why to use the ion bulk velocity in the convectional electric field instead of the electron bulk velocity.

3) Line 112: How do you acquire $k_\perp$ in the spacecraft frame? Please explain.

2 Minor improvement requests:

1) Line 66: There are three criteria listed for selecting an interval for analysis. The first two are clear enough, but the third criterion is not quantitative. If the requirement is e.g. that $E_\perp/B_\perp$ follows a power-law fit in respect to omega within a certain regime, as is presented as a property of KAWs, this would be a good time to actually introduce it. If it's a different criterion, it should be explained.

2) Line 16: You quote tail ions as being well scattered and isotropic. However, tail dynamics can also result in deformed ion VDFs. This has been predicted in simulations already in

- Nakamura et al. (1998) https://doi.org/10.1029/97JA01843

and shown in observations:

- Birn, Runov and Zhou (2017) https://doi.org/10.1002/2017JA024230
- Birn, Chandler, Moore, Runov (2017) https://doi.org/10.1002/2017JA024231
- Runov, Anvelopoulos, Zhoi (2012) https://doi.org/10.1029/2011JA017361
- Runov et al (2017) https://doi.org/10.1002/2017JA024010

3) Lines 17-18: Anisotropy can be indicative of currents, but not a direct source. Currents result from bulk motion. Please rephrase.

4) Lines 80-84: I believe the statement here should be clarified. In the plasma rest frame, KAWs indeed are found at frequencies below the ion gyrofrequency, so it is important to account for the doppler shift when assessing them in the spacecraft frame. However, the doppler shift effect dominates the frequency only at large wavenumbers k. I agree that it's important to assess this range, but to state that all spectrum properties are dominated by the doppler effect is oversimplification.

5) Line 112: Please briefly clarify the reasoning behind method from Chaston 2008 for evaluating $k_\parallel$.

6) Lines 164-165: Why did you not exclude the dipolarisation fronts from this analysis? For the first example, the front is right in the middle of your wave analysis region and for the second one, just at the start.

7) Lines 164-166: How do you perform time binning in order to classify plasma and field measurements? They have different time resolutions, after all. Please clarify this section. Although the figures show use of parallel and perpendicular fields separately, the text does not indicate this at this point.

8) In evaluating the wave scalar potential, perhaps only the parallel component of the electric field should be considered, and the potential designated $\phi_\parallel$. Also, I would suggest (if not already done) to only account for electric field components with frequencies 0.1 Hz in order to exclude contamination from non-KAW sources.

9) Figure 9 shows averages, but this is a lazy solution for presenting statistics, robbing the reader from understanding the details of the data set. I would recommend using box-and-whiskers plots to show the distribution of values inside the statistics. You will need to add extra panels to the plot to facilitate this, but it is surely doable.

10) Figure 9: There are three different categories of events shown in panels b and c but no explanation or discussion of this. Are they for different BBF bulk velocities? As there is no major deviation between these classifications, I would exclude them in favor of the box-and-whisker plots, and simply mention in the text that there was no systematic dependence on BBF velocity to be seen.

11) Figures 5, 6, and 9 show the frequency range going down to 0.01Hz which corresponds with a wave period of 100s. For example event 2, the analysis period is about three minutes, which is insufficient to accurately assess such low frequency fluctuations. Please Consider the cone of influence in evaluating your wave spectra. This is only a minor point as you don't draw much conclusions about this energy range.

12) Line 244: The statement is problematic due to the Figure 3f encompassing the energy range [200,2000] eV and it showing only very weak and sporadic parallel/antiparallel asymmetry. It is clearly visible in Figures 3e, though. If a statement is to be made for the energy range [200,1000] eV, it should be supported by a figure where the effect is visible.

13) Could you please attempt to provide an estimation of the error in determining the parallel and perpendicular components of the electric field?

3 Additional minor comments about text and figures:

- In the abstract, the name Alfvén has been misspelt
- Line 80: "rest reference frame" is ambiguous (although the latter part of the sen-

tence clarifies it as plasma frame)

- Figure 2: What is the definition of B_t here? Please indicate the properties of low-pass filtering in the caption as well as the spacecraft (which MMS satellite?) and the date.
- Figure 9 has ν as the X-axis, whilst figures 5 and 6 have ω
- Figures 5,6,9: as panels a and c have different legends, panels b should also have them (even if they are the same as panel c)
- Figures 3 and 4 should have their subpanels properly labelled. Now there are both sides a) and b) and two panels a) and two panels b) etc. Also, the figures are very small and hard to read.
- Line 94: Theoretical dispersion predictions cannot "deviate", however they may be in disagreement with observations. Please rephrase.
- Line 121: the energy range plots are in Figure 3, not 4.
- Line 122: I think Figure 4h would fit thematically better into Figure 3.
- Line 124: The perpendicular anisotropy formulated here is used in Figures 7, 8, and 10 as well as Figure 4h. I would recommend consolidating the way it is written in figures, and perhaps clarifying it in captions.
- Line 137: (f,g) and (d,e) are swapped
- Line 139: Which value of v_A are you using? Based on instantaneous plasma parameters, or averaged over the whole interval?
- Figures 7 and 8: Please refrain from using E both for kinetic energy and electric field in the same figure. This notation made it very difficult to understand the normalizations you applied.

- Line 222: What do you mean with "perspective candidate"?
- Figure 5: Please clarify the caption: "[the] red line shows results for observed plasma parameters" is not appropriate as it shows a theoretical prediction based on plasma paramaters averaged over the analysis interval [?], not a "result". Similar improvements can be made to other captions as well.

I look forward to reading the revised manuscript.

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