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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.

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1 Referee 1

1.1 Major improvement requests

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

   - 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.

Reviewer is right, the increase of the parallel anisotropy is weak and the main effect is a change (with the parallel electric field magnitude) of energy range of electrons with the parallel anisotropy. Variations of the anisotropy *intensity* with parallel electric fields are not so evident, what could be an effect of the thermal electron feedback (via anisotropy-induced currents to KAWs dispersion, see KAWs' model in Damiano et al. (2015)). High energies are supposed to be dominated by electrons transversly heated by the betatron mechanism associated with dipolarization fronts. Moreover, as was shown in some previous papers (e.g., Artemyev et al. (2014)) the parallel anisotropy is usually observed for thermal electrons so we are mainly interested in variation of energies of anisotropic electron population. To show how well statistics cover different ranges of electric field amplitudes, we have added additional panels to figure 11 which show the total number of measurements in each electric field bin.

   - 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.

We agree, formation of anisotropic electron population would take a certain time. However, we think waves interact with electrons much longer than we observed them moving across the spacecraft with the plasma flows, i.e. we consider wave amplitude as a instantaneous measure of the general wave intensity (that is expected to correlate with the electron anisotropy) within a spatially localized region transported by plasma flow. Regarding the Reviewer suggestion to use wave scalar potential instead of wave field: figure 1 in supplement show dependence on scalar potential magnitude $|\phi_{\|}|$ but since we need to apply KAW model to reconstruct $\phi_{\|}$ this approach can introduce some uncertainties because we can't correctly determine the wave frequency in the plasma frame.

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.

   In a case-study section we didn't use any normalization to show dimensional magnitude of electric fields, but we indeed used such normalization on figure 10. In the revised revision we use only electric field magnitude without normalization (figure 2 in supplement).

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

   Due to the Doppler shift $\omega_{sc} = \omega_{pf} + k \cdot v \gg \omega_{pf}$ and for KAWs we expect $k_{\|} \ll k_{\perp}$ we estimate $k_{perp} \approx \omega_{sc}/v_{i\perp}$ (see details of this approximation in Chaston et al. (2012)). Note, in our statistics we usually have $v_{i\|} \leq v_{i\perp}$. We use ion velocity as a proxy of the plasma reference frame speed, i.e. although electron velocity can follow better $E \times B$ drift than ions, ion motion consists with the motion of the

KAWs source region as KAWs are driven by ions.

1.2 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 $aE_{\perp}/B_{\perp}$ 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.

   Reviewer is right, the third criterion is that the observed spectra ratios follow the theoretical equation. We rewrote the corresponding sentence.

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

   Reviewer is right, ion distribution may contains a lot of different popualtions with the various anisotropies; and this picture would be quite far from the simple isotropic approximation. We have clarify this point in the text.

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

   Thanks! We have rephrased this one.

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.

   Reviewer is right, this assumption is an oversimplification, that is due to absence of information about actual wave frequencies within strong plasma flows. We have corrected the corresponding phrase.

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

   Thanks for question! We have included detailed explanation of $k_{\|}$ estimation (see line 114 in the revised version):

   knowing the transverse component $k_{\perp}$ as function of wave frequency in the spacecraft frame ω_{sc}, we estimate the parallel $k_{\|}$ from equation (?):

$$k_{\|} = \frac{\omega_{pf}}{v_A\sqrt{1 + k_{\perp}^2(\rho_i^2 + \rho_s^2) - \frac{\omega_{pf}^2}{\omega_{ci}^2}(1 + k_{\perp}^2\rho_i^2)}} \quad (1)$$

   where ω_{pf} is a wave frequency in the plasma rest frame and ω_{ci} is a local ion cyclotron frequency. Because we can't estimate ω_{pf} we choose two typical values: $\omega_{pf} = 0.05\omega_{ci}$ and $\omega_{pf} = 0.5\omega_{ci}$ and define $\phi_{\|05} = \phi_{\|}(0.5\omega_{ci})$ and $\phi_{\|005} = \phi_{\|}(0.05\omega_{ci})$.

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.

   As we use 0.01 Hz for background magnetic field, we need a long enough time interval, but dipolarization fronts sometimes occur within such interval and exclusion of all dipolarization fronts would significantly decrease our statistics. The duration of the front crossing is sufficiently small, so thermal electron population associated to the front does not change statistical properties of electrons. We also assume that the main effects associated with dipolarization fronts will be on more energetic particles than considered thermal electrons. This suggests that there is no strong effect of the dipolarization fronts occurrence on our statistical results.

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.

   Good point. We have added more details to the text (see line 178 in the revised version):

   To reveal the relation between KAWs' electric fields and electron anisotropy, we consider 2D distributions of flux anisotropy in energy and electric field space. To do this, we firstly interpolate electric fields to the FPI time grid so each particular time point in the considered interval could be associated with flux anisotropy in all energy channels and electric field measurements. Then we bin a field amplitude range and average all energy distributions over the selected time interval.

8. In evaluating the wave scalar potential, perhaps only the parallel component of the electric field should be considered, and the potential designated $\phi_{\|}$. Also, I would suggest (if not already done) to only account for electric field compo-

nents with frequencies 0.1 Hz in order to exclude contamination from non-KAW sources.

Reviewer is right, we use only the parallel electric field component. We consider only electric field perturbations with frequencies $\omega_{sc} < \omega_{ci}$ here to exclude non-KAW sources but due to the Doppler shift it is difficult to chose the reasonable upper frequency limit.

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.

   Thanks for suggestion! We have revised this figure to shows some sort of box-and-whiskers plots (figure 3 in supplement).

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.

    Reviewer is right, here different lines show averages for different plasma bulk velocities and there is no strong dependence on this parameter. We have changed this figure (see our response to the minor point 9).

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.

    In fact we use wider time intervals (about 5-10 min) than shown to access low frequency field fluctuations and than consider only bounded part in our analysis.

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.

    We have revised both statement and statistical figures; e.g. new Fig. 11&8 show such correlation.

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

    MMS electric field dataset provides an uncertainties of DC electric field for the shortest antenna, and if we would consider these uncertainties for electric field error estimates, we would get $\sim 1-2$ mV/m for both parallel and perpendicular electric field components. However, KAW electric field consists of both DC (low frequency) and higher frequency part, what complicates determination of electric field errors. We have indicated in the text that results of < 1 mV/m should be taken with a certain caution, as these results may be affected by uncertainties of DC field measurements.

1.2.1 Additional minor comments about text and figures

- In the abstract, the name Alfvén has been misspelt

  Thank you, we have fixed this mistake!

- Line 80: "rest reference frame" is ambiguous (although the latter part of the sentence clarifies it as plasma frame)

We rephrased this sentence.

- Figure 2: What is the definition of Bt here? Please indicate the properties of low-pass filtering in the caption as well as the spacecraft (which MMS satellite?) and the date.

  We added more details about used filter to the figure caption and replaced B_t with $|B|$.

- Figure 9 has ν as the X-axis, whilst figures 5 and 6 have ω

  We changed all labels to ν

- 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)

  We added additional legend.

- 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 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 137: (f,g) and (d,e) are swapped

  We replaced figures 3 and 4 with single figure for each event for simplicity (figures 4 and 5 in supplement).

- Line 94: Theoretical dispersion predictions cannot "deviate", however they may be in disagreement with observations. Please rephrase.

  We rephrased this sentence.

- 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.

  We changed label for all panels with anisotropy.

- Line 139: Which value of vA are you using? Based on instantaneous plasma parameters, or averaged over the whole interval?

  We use velocity value averaged over considered 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.

  We changed figure labels.

- Line 222: What do you mean with "perspective candidate"?

  This phrase has been rewritten!

- 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.

  Done. Thanks!

References

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