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# *Interactive comment on* "Multiscale estimation of the field-aligned current density" *by* Costel Bunescu et al.

**Anonymous Referee #1**

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Summary This manuscript presents a novel technique for assessing field-aligned currents across a range of scales, extending upon previous work present by Bunescu et al. [2015]. That previous work applied minimum variance analysis in sliding windows across a range of scales to determine the planarity and orientation of field-aligned currents. Using this technique, the field-aligned current density for each scale is determined by calculating the current from the gradient of the maximal variance magnetic field perturbation. While there is merit in this idea, the described technique does not sufficiently address the non-orthogonality of the scales used, which limits this techniques usefulness and how the results may be interpreted. As such, it is my recommendation that the authors revise this.

I would note that the manuscript does provide a good level of detail in relatively acces-



sible language and notation, which is a credit to the authors.

Specific comments There is much potential merit in the analysis technique described. However, in my opinion there is a potential underlying flaw that drastically limits the usefulness of this technique; the scales examined are not independent. As described, the minimum variance technique is applied to a collection of increasing scales by simply varying the window length over which the analysis is performed. As such, this does not isolate fluctuations on these scales and there is potential for the scales to 'bleed' into one another. The manuscript does discuss this in a limited fashion, noting that the scales are not orthogonal, however this does not go beyond a discussion. As a result, the calculated FACs at each scale are comparable to the total FAC, particularly at the smaller scales and the sum of the FACs across all scales is not the total FAC.

It is not clear to me exactly how to address this. The technique described in Bunescu et al. [2015] potentially enables different scales to be determined at different times by determining local maxima in delta_w Lambda_max, so some iterative process which identifies the relevant scales, filters the data at those scales, then runs the minimum variance analysis on those may be appropriate. Alternatively, filtering at a select number of scales will remove some of the 'bleed' between scales. These additions will not remove the non-orthogonality problem (discrete wavelet analysis or similar would be needed for that), band-pass filtering to attempt to isolate given scales should improve the results of the current calculation and remove the need to apply the weighting functions.

I note that in order to attempt to correct for the issue of the total multi-scale FAC, the manuscript describes three ways to weight the data: either taking the mean of the FACs across all scales; or multiplying by either the window width or one over the window width. These are somewhat contradictory to the aims of the paper as they either equally weight all scales or weight the to the larger or smaller scales. However, the principle of this analysis is to determine the most important scales. I believe that by applying the appropriate filtering, the need for these weightings will be removed.

Technical comments Figures should have panels labelled. While the panels are described in the captions, none are actually labelled.

The figures all appear to be fairly low resolution. For multi-panel figures, this makes them hard to examine in detail. Please provide higher resolution figures.

U1, 2 etc. are not labelled in Figure 4.

Figures 5, 9 and 13 all have a mis-labelled Y-axis in the top left plot (this should be "Magnetic Field (nT)" or similar)

The caption for Figure 6 does not described the coloured traces. Furthermore, the dashed lines only appear to be in two panels.

Each hodogram is missing the label for the Y-axis

In general, the description of the MSMVA panels in the text should be improved – it is somewhat hard to follow e.g. panel 9e4 etc. I would recommend unique letters for each panel.

P1. Line 11 - the abstract notes that the multiscale FAC is compared with input data and Swarm data, but gives no indication of how good or bad the comparison is.

P1. Line 17 – while I agree that solar wind-magnetosphere coupling is a key driver, there is an element of ionospheric feedback into the system which should not be ignored.

P1. Line 24 – above the ionosphere, one tends to measure magnetic perturbations due to the in-situ field-aligned currents rather than the ionospheric Pedersen currents

P2. Line11 – I suggest you reword this – it reads as though the maximum width was around 400-500 m but the average was greater than that. I believe you mean the peak of the distribution was 400-500 m

P2. Lines 21-32 – please be clear as to whether these scales are in-situ, in which case



the height they were measured is important, or mapped to some common altitude

P.18 Line 8 – You suggest that your technique is useful for comparing SwA and SwC data, but do not then go on to make this comparison. It would be interesting to see that (or remove this comment).

P.19 Line 14 – is this event a unipolar or multi-polar event from Wu et al. [2017]

The authors may also be interested in a study by Peria et al [2000, doi: 10.1029/GM118p0181] who examined used MVA to statistically examine auroral zone crossings by FAST.

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