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Reply on RC2

Noora Partamies et al.

Author comment on "Magnetic local time (MLT) dependence of auroral peak emission height and morphology" by Noora Partamies et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2022-6-AC2, 2022

Thank you very much for helpful comments.

Motivation: I agree with the other reviewer that the authors could be a bit more explicit on the motivation of the study in the introduction. What useful physical information can be derived from the emission height?

The motivation for this type of studies is to understand the climatology of the auroral occurrence and behaviour in general. But it is the peak emission height of the aurora that would most closely relate to the precipitation energy of the electrons, and is thus interesting.

This has been phrased in the new version of the manuscript as: "Our motivation is to understand the climatology of the auroral emission height and structure evolution. Furthermore, as the auroral peak emission height carries information on the energy of the precipitating electrons, the MLT distribution of emission heights in this large dataset will contribute to the knowledge of electron energy distribution as well. Furthermore, as the auroral peak emission height carries information on the energy of the precipitating electrons, the MLT distribution of emission heights in this large dataset will contribute to the knowledge of electron acceleration mechanisms as well."

Time averaging: If I am not mistaken, the statistical connection between solar wind parameters and emission heights is done by comparing simultaneous one-minute values. This is OK for solar wind speed, which has a long autocorrelation time, but is questionable for IMF Bz, which can chance its polarity quite rapidly. This may be also significant for the conclusions drawn from the statistical results. I would like to see similar figures as in the current manuscript but using hourly means (rather than 1-minute means) of solar wind parameters in binning. This is a commonly used averaging in solar wind-magnetosphere coupling studies. See, e.g., Borovsky (2013) https://doi.org/10.1002/jgra.50110

This is correct. It is the 1-minute values that have been compared in this study. There would be many ways to deal with the time delay in the comparison. We decided to go for the simplest, no averaged way. IMF variability is surely a good reason to average IMF data for the comparison. Below are the IMF Bz plots for Lapland without averaging (as in the original manuscript) and with 1h-averaged IMF data (as suggested).

In the 1h-averaged version the averaged IMF Bz is required to be negative or positive respectively. The nightside height results from about 20MLT to about 03 MLT do not change. On the dusk side before 20 MLT the height distribution during negative IMF changes very little while that during the positive IMF becomes more variable when the IMF values are being averaged. Rather than physics the height fluctuations in the 1h-averaged version are due to the fact that during positive IMF conditions the number of data points becomes less evenly distributed between the bins. The number of data points is generally lower during positive IMF than during negative IMF, as well as towards dusk and dawn than around the midnight. On the dawnside the difference between positive and negative IMF shows similar behaviour with and without averaging. Here, the averaging actually distributes the data points more evenly and the end result is smoother and clearer.

Same set of figures are shown below for Svalbard: original and 1h-averaged. Again the data points of the height distribution during positive IMF become a bit less evenly distributed between the bin when we average the IMF Bz, but the differences between the two plots are very minor. If something was to be brought up, it was the larger difference between positive and negative IMF in the post-midnight sector when averaged IMF has been used.

Both figure pairs suggest that averaging describes the dawn sector precipitation better but matters less for the auroral precipitation energy in the other sectors. In the essence, the differences between the versions are subtle enough that we would rather describe in the text that this has also been tested without adding new figures or changing the current ones.

I think the authors should cite papers showing that solar wind speed (and more accurately high-speed solar wind streams and the embedded Alfvenic Bz fluctuations) dominates the occurrence of substorms. The fact that different emission height distributions are found for low and high solar wind speed probably reflects the fact that substorm activity is frequent during fast solar wind, but less frequent during slow solar wind.

E.g., Tanskanen et al., 2005, https://doi.org/10.1029/2005GL023318

This is a very likely scenario indeed. The suggested sample reference and the substorm occurrence rate paper (Tanskanen, 2009) have been added to the revised version of the manuscript. However, we think that it may be unnecessary to discuss the Alfvenic fluctuations as the solar wind driving process but would rather leave it to making the connection between high-speed solar wind and high substorm occurrence, high substorm intensity and high substorm energy dissipation rate.