

Response to Reviewer Letter

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Manuscript Title: Subauroral Crosstalk in POES/Metop TED Channels

We would like to thank the reviewers for their helpful comments. We hope that these answers and the revised manuscript meets the reviewers' approval. Note that the lines have been updated to match the revised version.

1 Reviewer 1

I'm mostly happy with the changes the authors have made. It would be nice if they had been able address the issue with contamination of the electron data by protons, but I accept their assurance that the effect is likely minimal. It would be nice if they could address this further should they get the code working. The colours of figure 4 are still not perfect – it is primarily the legend rather than the plot itself that is the issue – but it is readable enough.

Reply: *As our correction method is not based on the MEPED electrons but on the background counts of the TED detector, we think there is really no need to address this topic in the context of a paper discussing the TED detector. Nevertheless, we agree that in principle the MEPED electrons should be corrected in regard of proton contamination. We enlarged the legend and doubled its linewidth. Hopefully this solves the issue of Fig. 4.*

2 Reviewer 2

The paper investigates the contamination of particle measurements from the Total Energy Detector (TED) on POES and Metop satellites due to energetic electron contamination, particularly at subauroral latitudes. The study spans data from 2001 to 2018 and proposes correction methods to address these contamination issues. The authors have looked at the POES Metop TED channels and the contamination within them as well as comparing to other inferred data on electron and proton precipitation. Previous work has taken closer looks at the contamination in the MEPED channels, with some work looking at the TED channels. The paper could use some work to become more clear as well as showcase the results. This lessens the impact of this paper, though it still provides useful information to be included in the literature.

I applaud the authors on improving the paper. I do still have some questions and comments which I'm sure they will be able to address.

1 Comment:

Line 49 – I'm not sure why the authors state it's a "so-called" slot region. It is called the slot region.

Reply: *Corrected.*

2 Comment:

Line 54 – The inner boundary can extend lower – to the effective loss height of the energetic protons. In fact many energetic particles are lost at much lower altitudes (e.g. Fang et al. (2008) and more updated papers show the peak ionization height for a 10 keV electron around 100 km and a 1 MeV electron closer to 60 km).

Reply: Right, the fixed altitude may not be ideal here. Also, citing Fang et al. (2008) would not be very helpful as it deals with electrons only and thus does not fit to the inner radiation belt. The authors already published on the altitude-energy dependency of proton and electron interactions with the atmosphere, see Wissing and Kallenrode (Fig. 2 in 2009). However, we would like to keep the introduction simple without energy dependence. It now reads "...in regions such as the SAA, the inner boundary can extend to very low altitudes, where its downward extent is ultimately constrained by atmospheric losses."

3 Comment:

Foot notes in paragraph starting on line 114 – A figure for each dataset used and when it is available would be beneficial, perhaps instead of the footnotes.

Line 114. This sentence is a bit confusing. It's unclear how 2001 – 2018 will help with an inter-comparison in 2024.

Line 114 – With the data being analyzed on both a 16 second and annual or multi-annual averages are you saying you averaged all the data to a single value for a given year or even for a two+ year period? More clarity for this paragraph would be helpful as I'm not sure what they authors are trying to say beyond which satellites were used in the study.

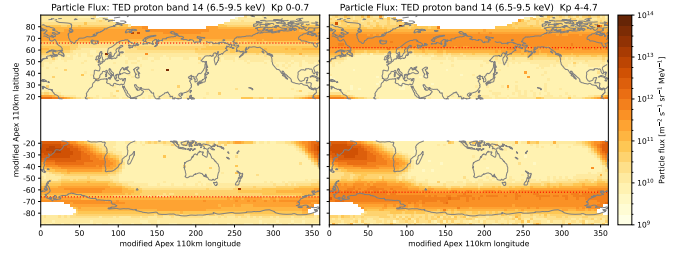
Reply: You are right, this is more confusing than it needs to be. In case that a data set is available it makes no sense to restrict the use. We now included the POES TED data during 2001–today (end of 2025) for the long-term plots, see Figures 1 and 2. This does not change any result.

However, some data sets are restricted, e.g. N16 TED data becomes a mess in the later phase, the background counts have been taken from the netCDF files, which are available starting in 2013 (and are investigated for that year), and one of the Metop satellites is launched later which affords a separate year of investigation. The comparison data sets SSUSI and S4 have temporal restrictions, too.

As the information on the used data periods and further averaging is not uniform for every section it just confuses the reader here. We removed the part and provide all necessary information on data period and how data is handled in the according sections. This also avoids any footnotes.

4 Comment:

Figure 1: This figure still does not seem accurate – at a minimum, including the continent outlines would help the reader immensely to ensure that the SAA and other details are indeed in the correct locations.



Reply: Figure 1 has been updated with continent outlines. Note that the continents are slightly deformed as this figure shows Apex coordinates. As clearly seen, the SAA is at the correct location.

5 Comment:

Line 203 – There doesn't seem to be a clear separation or at least not a consistent separation within the longitudinal regions of the SAA in the southern hemisphere. Perhaps a zoomed in look at this would help the reader see any separation that is there. E.g at about longitudes of 0 – 80, maybe a bit further and then again at longitudes of 350 – 360

Reply: We choose a new color map in Figure 1 to make it more visible. If further close-up is needed, Figure 2 provides a view on 60°E.

6 Comment:

Paragraph starting on line 215. Not everyone will be familiar with what you mean by polar horns. This is due to the weakened magnetic field, so I would suggest stating this explicitly.

Reply: This has been added.

7 Comment:

Line 297 – by low pitch angle diffusion do you mean slow pitch angle diffusion below the strong diffusion limit? So a weak diffusion regime which we assume is most often found during periods of quiet activity? That said – you can have strong diffusion even during quiet times depending on the source of the pitch angle diffusion, energy, etc.

Reply: In some occasions strong pitch angle diffusion might even occur during quiet times. We replaced the "equivalent to low Kp" by "most often occurring under low Kp conditions".

8 Comment:

Line 302 – I would suggest to specify the outer radiation belts as the electrons can be seen in both the inner and outer radiation belts.

Reply: The sentence now reads "...this region belongs to the outer (electron) radiation belt."

9 Comment:

Line 303 – the “impenetrable boundary” is actually very penetrable, so I would be careful with applying that as a hard cutoff.

Reply: *We are not sure what is meant by “applying that as a hard cutoff”. In case that the $L=0-2.5$ and $L=3-5$ region in Fig. 8 are meant, we do not consider “penetration events” an issue as they will not significantly alter the long-term average composition of the proton and electron radiation belts.*

10 Comment:

Line 307 – this can cause electron loss for those particles that are moved into regions that now intersect with the magnetopause. E.g. Kim et al. (2010), Shprits et al. (2006) or Tu et al. (2019).

Reply: *The paragraph has been rephrased in order to point this out. Kim et al. (2010) is now cited.*

11 Comment:

Line 321, I’m not sure what you are implying here. When you say “real short-term electron losses” do you mean the loss to the atmosphere or just a decrease in the flux of the trapped population?

Reply: *Well, we try to get from the processes to the particles and then to the contamination. Maybe it’s better this way: “In the context of our study, it is unnecessary to distinguish between the Dst-effect displacements and loss processes such as magnetopause shadowing or atmospheric interactions, as all lead to a (at least short-term) reduction in energetic electron populations, thereby resulting in reduced contamination during periods of high geomagnetic activity.”*

3 Additional changes

Some additional changes have been made in the paper, mainly to improve the clarity or to including all available satellites. The changes are just small without any impact on the results, but we want to mention them here.

1 Change:

The note on the TED proton data as given in Appendix B was not correct. We apologize for that. The structure is {a, -1, -1, -1, -1, -1, -1, -1, c, -1, -1, -1, d, -1, -1, -1}, or with other words there is no measurement during time-slot b. We removed the Appendix B. This has an impact on the data due to the different integration time of the first measurement. Thus, we updated all figures and fits. The changes are very small in most cases not even noticeable.

12 Comment:

Line 369 – it would be beneficial if the authors would define what they mean by night time. Is that 20 – 04 LT? 18 – 06 LT? I would assume that the LT/MLT would have an impact on this result as well. I believe it’s what you have in the figure panel titles, but it should be in the paragraph as well.

Reply: *It is 20-04 local time. This information has been added to the paragraph. LT has been chosen because the S_4 index was available in LT. A comparison in MLT would have looked a little different at $100^\circ E$, because it would not cross the central aurora, but the main difference, the non-existing peak at $(20^\circ E, 60^\circ S)$ would have been the same.*

13 Comment:

Line 375 – While the bulk of the SAA is equatorward of 50S, the weakened magnetic field still has an impact at those more poleward latitudes in the southern hemisphere. If you were to plot this with geomagnetic coordinates how does this change? Does the sinusoidal effect seem to disappear?

Reply: *Fig. 1 shows a similar data set (TED 0° p 14) in geomagnetic (APEX) coordinates. No sinusoidal effect is seen.*

14 Comment:

Line 420 – Again, I would not say “so-called slot region”. It is the slot region or where the slot region is typically observed.

Reply: *Corrected.*

2 Change:

We found the reason for the saturation effect. It is a bug in the handling of the background values in the netCDF files and should be noted. The proposed correction method is independent from this and will work with the recent netCDF-files but it can also be used for the old bin-format and in a (one day maybe) corrected netCDF-version. The only difference is that -physically- this is not a saturation but a handling issue.

We changed in Section 5.1:

”Different possible physical or technical sources of non-

linearity come to mind. First,... However, direct comparison with the old bin-format reveals that the saturation effect is caused by a different handling of the background data in the netCDF-files. In the netCDF-version all background data above an arbitrary level are cut and replaced by a linear interpolation of the closest surrounding background values. We probably can call this a bug as it unnecessarily hides the real (much higher) background values.”

In the summary we added:

”We should note that our proposed correction method is not limited to the recent netCDF-files. It should also work with the older bin-files or a corrected netCDF algorithm without high-background cutting - even though a user should think about setting a maximum background flux (like the saturation threshold) that still allows for a reasonable subtractive correction, a certain proportion maybe. In case of the “type D” contamination a different background handling in the netCDF-files would be clearly beneficial.”

In fact the netCDF files are the only source for the more recent POES/Metop satellite data. Thus a correction is needed anyway.

3 Change:

Every single background channel belongs to two particle channels. Thus the according saturation threshold should be the same. In most cases this was the case already. However, there are channels which allow an easier threshold detection than others, e.g. the p 4 threshold

can typically be detected better than the p 8 threshold. The data set has been updated accordingly. Note that the threshold setting has an impact on the fit itself as it determines which points are included. Thus fits might slightly change in numbers - but as seen in the overview plot, the impact is practically negligible.

4 Change:

While the previous version listed all fits in Tables 2 and 3, the updated version lists the valid ones only. Thus no fits are given for type B, C1 proton contamination or C2. The same applies for the fit overview plot of Fig. 10. Additionally the contamination type is now listed in the Tables instead of the text. This should add clarity to the paper.

5 Change:

NOAA POES 16 has been included in the correction (e.g. Tables 2 and 3, Fig. 10 and the correction algorithm). For this we had to extract the background counts from the binary files. As the paper now includes all SEM-2 TED instruments that have been installed on satellites this adds a significant benefit to the paper. Some of the channels of N16 TED show an effect similar to the C1 type but with a stronger impact on the data. This probably was the reason why its TED instrument is considered to be defect starting from 2006. We would consider some of the channels as unusable (type C2) even before this date. As such type C2 has been added to Fig. 9.

References

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