Response to Reviewer Letter

Jan Maik Wissing Olesya Yakovchuk Stefan Bender Christina Arras

September 23, 2025

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.

1 Reviewer 1

The authors have presented a work investigating the "cross-talk" (commonly referred to as "contamination" elsewhere in the literature) of the POES TED and MEPED instruments as part of the POES SEM-2 instrument suite. The authors provide qualitative evidence to support the conclusion that the TED instruments suffer from significant contamination, in the L<6 region of the radiation belts.
On the whole I do not have too many major issues with this paper.

1 Major comment:

My biggest gripe concerns the inclusion of the MEPED instruments in the authors' investigations. As far as I can tell, none of the authors conclusions regarding the MEPED instrument are new or novel. Indeed, most of them can be reached by a careful examination of Yando et al. (2011), and follow-up works such as Whittaker et al. (2014) and Peck et al. (2015). This is not to diminish the TED-related work, which I think has the potential to be a useful addition to the literature, however I think the MEPED instrument additions just distract from this and are unnecessary.
Reply: *The reviewer is right that the MEPED instrument should not be discussed more than necessary as this is not the main focus of this paper. We will remove these parts.*

2 Major comment:

I will also note that the authors should take care to carefully read through the paper for spelling and grammar issues, of which there are many. Not enough to decrease the readability of the paper, but enough to be distracting.
Reply: *We will carefully check spelling and grammar before the next submission.*

3 Minor issues

The following are a list of relatively minor issues with the paper, in addition to those noted above:

3.1 Minor issue:

Lns 80-81 - The MetOp program was not a successor to the POES constellation; POES was a NOAA initiative, MetOp was EUMetSat – they're complimentary (indeed, MetOP-A was launched before NOAA-19).
Reply: *Thanks, we removed the "successor".*

3.2 Minor issue:

Lns 83-84 - While it is true that, averaged over time, the POES satellites offer near-complete local time coverage, I think it is important to highlight the deficiencies in this; namely that there is a blind-spot around 12 MLT. Although not really too important for this paper, I think it is necessary to avoid misinformation regarding the coverage.
Reply: *We added the following information: . . . near-complete magnetic local time coverage. While we do not think that it notably impacts our results, the coverage has a blind spot at 12 h MLT limited to southern hemisphere subauroral latitudes and another at 0 h MLT on*

the northern hemisphere. For details we refer to Figure 1 (lower right) in Yakovchuk and Wissing (2019).

3.3 Minor issue:

Lns 96-97 - This claim regarding the 0-deg telescope measuring trapped particles should be cited, for instance Fig 1. of Rodger et al. (2010).
Reply: *This information has been added.*

3.4 Minor issue:

The authors attempt to create differential flux channels from the E1-3 integral channels is not particularly useful. The complex response of the electron telescopes to both electrons and protons make this qualitative at best, particularly for the "E2-E3" channel. The authors note this later on, but related to my point above, I do not [find] that this is particularly novel.
Reply: *Well, this wasn't meant to be something novel. But anyway, these channels will be removed from the paper as most other MEPED channels.*

3.5 Minor issue:

Related to the above, it is not clear from the instrumentation section if the authors are using decontaminated MEPED electron flux data or not. Fig 5. of Yando et al. (2011) shows that the E1 and E2 channels are strongly contaminated by roughly >200-300 keV protons, and the E3 by > 400 keV protons. Comparison with the P7 proton channel, which measures >35 MeV protons, is useful for removing solar proton events and the SAA, but not more general proton contamination. It should be noted that, depending on the version of data used, the older .CDF file data was typically roughly corrected for proton contamination, but using pre-Yando methods (.BIN files, from which the CDF data was derived, were not corrected). The newer .nc fluxes are not corrected for proton contamination: specifically, on page 32 of the MEPED Telescope ATBD, it is stated:
"Other factors not considered here that may significantly affect performance are the intercalibration of the satellites, the degradation of the sensors, and cross species contamination. These additional factors are discussed further in section 6.1 but accounting for these issues is beyond the scope of the current processing level. Users should consider how these limitations in the data accuracy might impact their use or interpretation of the data."
If the authors _have_ corrected the electron data, for instance using Ethan Peck's algorithm, they should mention this. Otherwise, any results derived from MEPED are suspect.
Reply: *Well, yes. It would be nice if the MEPED electrons are uncontaminated. Agreed. Unfortunatelly, we wasted more than a month on this comment. There is a Gitlab repository* `https://github.com/jpmailaddy/MPE` *from Joshua Pettit with about 10000 lines of IDL code (based on Ethan's findings). Given that we have no access to IDL we tried to translate it function-by-function to C++. While that mostly worked, but it also showed some bugs in the original code (you may check the repository for our comments) and missing data files that should be imported. We are in contact with Joshua but the issues are not solved yet.*
According to Pettit et al. (Fig. 6 in 2019) the corrected mep0e3 flux at L=4–6 may be factor 2 smaller than the uncorrected flux. However, this is for the 0 degree channel. The 90 degree channel is not presented. A similar correction approach is done by Asikainen and Mursula (2013). Their Fig. 5 (left) also shows a factor 2 for strong contamination events (note that their graph shows the reciprocal value). Considering the corrected 90 degree equivalent Asikainen and Mursula (2013)[their Fig. 5, right] gives a factor 1.2 to 1.45 more than the uncorrected flux. To be honest, the impact of a factor 1.2–1.45 on Figure 4 or 7 is practically neglegible. As this channel is not used for the correction, we will add a note and go on.

3.6 Minor issue:

The authors mention Figure 2 before Figure 3 – these should most likely be swapped.
Reply: *The order has been changed. Note that the figure numbering reflects the most recent paper version.*

3.7 Minor issue:

Figure 4 - in the top left panel, the authors have plotted MEPED 90-deg E3 – surely this should be the 0-deg channel?
Reply: *Honestly we choose the 90 deg channel with purpose. We do not expect the contaminating electrons to enter through the aperture, especially as the TED proton instrument is bend according to the proton motion in the applied electric field. If the electrons do not enter through the aperture it makes more sense to compare with the much higher fluxes in the 90 deg channel. However, the electron figure (below that) shows the 0 deg MEPED e3 channel. This information will be added to the paper.*

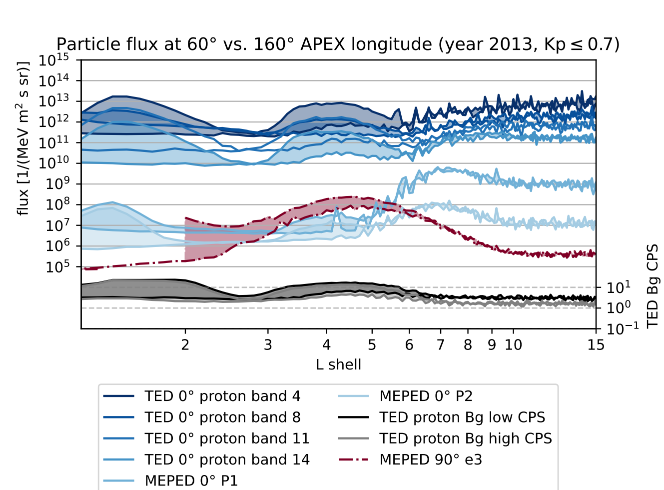
3.8 Minor issue:

Figure 4 - I would request that the authors choose a more colourblind-friendly colour scheme for their plots. Varying shades of blue and red can be very difficult for people

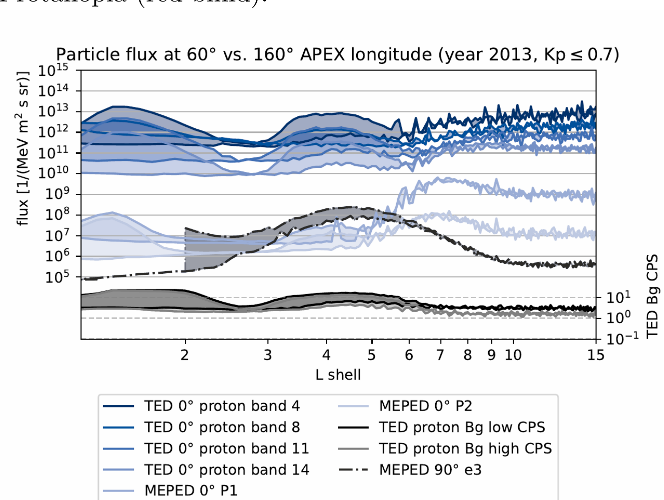
with different types of colourblindness to distinguish.

Reply: *Of course we want to provide color-friendly images. For this we simulated 100% green, red and blue colorblindness with python's daltonlens package (see below). The graph seem to be nicely readable for any kind of colorblindness. However, these are only simulations. If you are colorblind and separation of the lines is an issue (in contrast to the simulation results) do not hesitate to announce this again. Please note that these are the old images which still contain the MEPED channels. By removing them from the paper the color spread will increase and thus also the readability.*

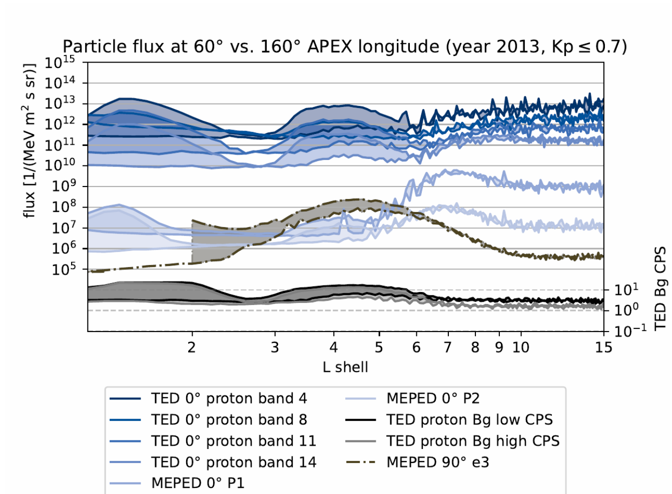
Normal:



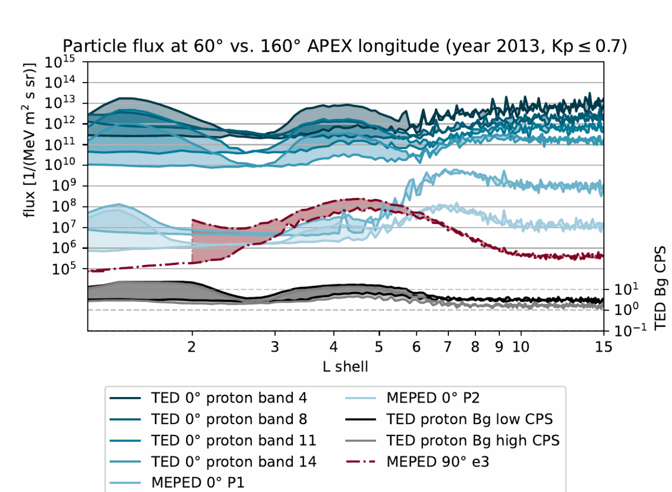
Protanopia (red-blind):



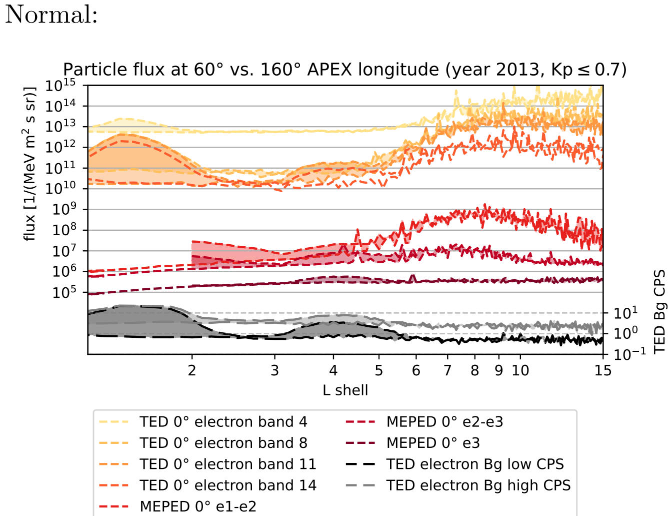
Deuteranopia (green-blind):



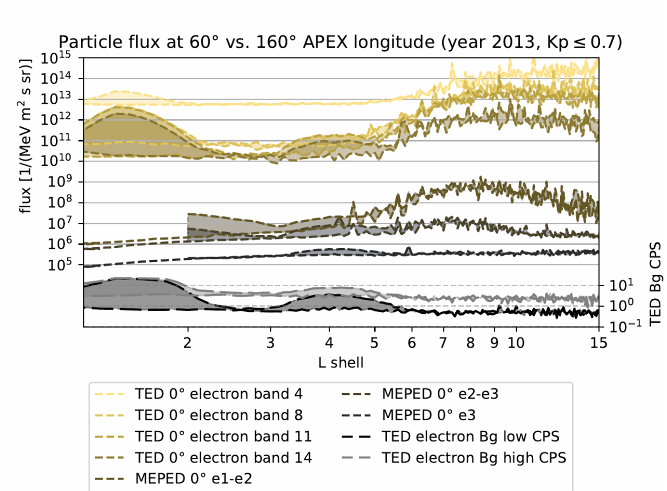
Tritanopia (blue-blind):



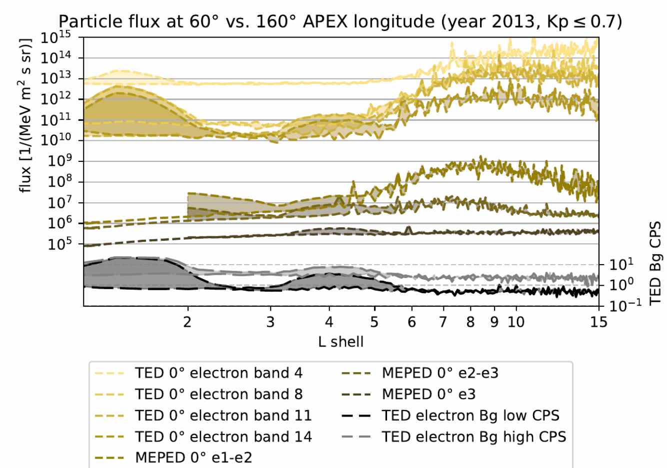
Normal:



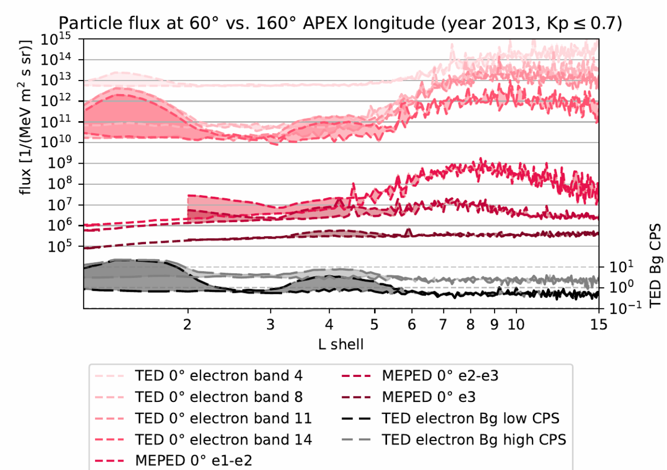
Protanopia (red-blind):



Deuteranopia (green-blind):



Tritanopia (blue-blind):



3.9 Minor issue:

Lns 241, 245, etc. The authors quite often state that values are "significantly" different – or instance, that the subauroral peak in MEPED 0-deg P1 and P2 is significantly smaller than in the TED protons. Unless the authors have statistical bases for these claims, they shouldn't use the term "significantly".

Reply: *Right, the term significantly has been replaced by more adequate expressions throughout the paper.*

3.10 Minor issue:

Lns 261-262 - (line in updated paper: 278) The authors should be careful attributing any particular measurements from POES as strictly trapped. As can be seen in Figure 1 of Crack et al. (2025), the vast majority of the time the POES 90 degree detectors are measuring at the very least the drift-loss cone, and often include the bounce-loss cone as well.

Reply: *Well, yes. That might be less generalized. Maybe like this: Within the subauroral area (which -in geographic coordinates- stretches from approx. 300°E, 70°S to 40°E, 55°S, see Fig. 1, top left, in Yakovchuk and Wissing, 2019) the 90° MEPED detector measures trapped, drift loss cone and bounce loss cone particles (compare Fig. 1, middle panel, in Crack et al., 2025). As the trapped population typically outnumbers the other populations, average fluxes are mostly reflecting the trapped population.*

3.11 Minor issue:

Ln 268-269 - while its true that geomagentic storms can result in decreases in flux, its not necessarily accurate to say definitively that this always occurs; as Reeves et al. (2003) noted, only roughly a quarter of storms resulted in decreases to radiation belt relativistic electron populations, while roughly half resulted in increases in fluxes.

Reply: *This seems to be a miss-understanding. A Kp dependence does not occur because of different flux levels before and after the storm (which both should be at rather low Kp) but because of low fluxes during the storm. Reeves et al. (2003) shows this on hisown, which has been added to the paper: "Energetic electron contamination might also explain the Kp dependence. Turner et al. (2012) point out that the radiation belt electron flux drops by orders of magnitude during geomagnetic storms. A quick radiation belt recovery, which was also noted by Turner et al. (2012), or increased radiation belt electrons fluxes after 53% of the investigated geomagnetic storms as observed by Reeves et al. (2003) do not contradict this statement as the increased fluxes are observed* *after the main event and thus during lower Kp-values (or Dst-values, as seen in Fig. 2 of Reeves et al., 2003).”*

3.12 Minor issue:

Ln 277 - Saying that any electrons have ”higher energy than ... E3” doesn’t really make sense – E3 is an integral channel, and measures all electrons in the >300 keV range. Similarly, Ethan’s E4/P6 channel is not the same energy range as E3 – typically the lower limit is considered to be 500-800 keV (it’s a relatively smooth gradient, so its up to interpretation exactly where it starts).

Reply: *Right, higher than e3 does not really make sense. We changed it to: “we can conclude that the contaminating electrons should have slightly higher center energy than MEPED e3. Observations from Peck et al. (2015, see their Fig.5) constructing a virtual electron channel e4 with a higher energy center of 800 keV (based on the MEPED P6 proton channel) show a flux maximum at L=* 4*, similar to Ross et al. (2021) for 4.2 MeV electrons.”.*

3.13 Minor issue:

Section 4.4 – This entire section is essentially just a rehashing of Yando et al. (2011), and is misleading in some parts as well. For instance, the authors make the argument that subtracting, for instance, E2 from E1 gives you a 30-100 keV channel, but this is not strictly true, as E1 and E2 respond differently to contaminating protons. Without removing this contamination (an imprecise art at best), E2-E1 does not give you anything particularly useful. I would recommend removing this section entirely, as it does not add anything novel to the paper.

Reply: *Will be removed.*

3.14 Comment:

”cross-talk” (commonly referred to as ”contamination” elsewhere in the literature)

Reply: *Corrected.*

To conclude, I think that the authors work regarding the TED portion of the POES SEM-2 instrument suite is useful, and a welcome addition to the literature. I do not think their MEPED work is particularly novel, however, and would recommend rewriting the paper to remove it. Following this, and the addressing of the above points, I think this work should be publishable.

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 infered data on electron and proton precipitaiton. 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.

1 Overall comment:

I’m not sure why the authors use the term “crosstalk”. Contamination seems to be a clearer word to describe what is happening. Crosstalk can potentially be confusing to the reader as it may mean that the crosstalk is intentional and intended – which I don’t think is the case for POES/MetOp.

Reply: *Sorry, this was indeed a language issue. It has been correced.*

2 Specific comment

Line 78 – while not very important, it would be nice to see more of an intro for this section.

Reply: *The Introduction of this section now reads: “This section describes the data sets utilized in this study and outlines the data processing methodology. The first subsection (2.1) introduces the data handling of the total energy detector (TED) particle measurements. After that the Modified Apex coordinate system is described, followed by the Kp-binning which has been applied to part of the paper’s data base. In the next subsections two independent data sets are introduced, which are compared to the particle fluxes. This is the backscattered UV, see Subsection 2.4 and the S4 scintillation index, see Subsection 2.5. The aim is to falsify or verify particle precipitation in certain regions or during special periods with self-contained data.”*

3 Specific comment

Line 80 – MetOp was not a successor to POES – POES was from NOAA and MetOp is from the UK. Though they are very complementary, I believe as planned.
Reply: *Thanks, we removed the "successor".*

4 Specific comment

Line 82 – The description of the orbital precession seems a bit misleading. What time period can all local times be covered by a single satellite? How does that compare to different types of events which may cause contamination? Also – was any intercalibration done on the different satellites? Can you use the precession from one spacecraft only to look at the contamination across all the sensors– or can you use data from only one satellite to determine if a sensor on that satellite is contaminated?
Reply: *The description has been updated. It now reads: "...their orbits have gradually drifted over time, enabling near-complete magnetic local time coverage (while combining multiple years and satellites)." That also implies that the length of a single event is much smaller than the orbital drift.*
No intercalibration has been made. But before an intercalibration can be made the contamination needs to be removed. In so far we opened the possibility to do so. According to the reviewer's comments we splitted the contamination analysis into a single satellite analysis. While this adds a significant benefit to the paper (thank you!) it also answers the reviewer's question if data from one satellite can be used to determine the contamination. A comment on intercalibration and the single satellite analysis has been added to the paper. In addition the single satellite results are compared.

5 Specific comment

Paragraph starting at line 89 – this needs references for the different look directions and the following paragraph as well could use references. The relevant ones are mostly already included in other locations, but the reader would be helped by having them here – or even a figure, or reference pointing to a figure, showing the geometry.
Reply: *The paragraph now includes references to the viewing directions (Fig. 2.1.1 of Evans and Greer, 2004) and the Green (Fig. 1 of 2013, showing a schematic of the TED configuration).*

6 Specific comment

Line 101 – I'm a bit confused. The whole point of the paper seems to be to provide new corrections, but then the authors state that the differential TED bands will not be corrected. I'm certain that I am just missing something here, so hopefully the authors can easily clarify.
Reply: *That's definitely not want we wanted to say. To clarify it: Janet Green supposed a correction for the TED total energy flux. The TED differential channels however have not been corrected before even though they are distributed in the "processed" data files. We clarified this in the text.*

7 Specific comment

Paragraph starting at line 113 – Another reference showing that the authors are using a more up to date version of IGRF than 1972 would be great. A figure showing the differences in the assumptions described in this and the following paragraphs would help to make it easier for the reader to follow. Along with this, a discussion about why getting the coordinate system right is important would be beneficial to add. Does this impact the pitch angles assumed to be within the loss cone and thus the likely fraction of the field of view inside or outside of the loss cone? Does this impact what cut offs and rigidities are determined for SEP events? If we're off by just a bit, how does that change the interpretation of the measurements?
Reply: *We added the reference of the recent IGRF description that is used in the APEX coordinates being Thébault et al. (2015). Also we added the references to the code van der Meeren et al. (2024) and Emmert et al. (2010). The authors do not think that it would make a big difference if the coordinate system is APEX, QD or AACGM, which should already be clear in stating that all these coordinate systems are more or less identical poleward of ±50 degrees. We choose APEX because it follows the magnetic field line and thus a particle stays on the same coordinates during a bounce motion, which is a nice aspect in particle modeling. However, that has already been described. No complex assumptions are made concerning the loss cone. The authors would guess that both viewing directions are contributing to the loss cone as they are relatively close together (nominally 0 and 30 degrees). However, this is just a guess and not part of the paper. In the sections comparing effects of particle precipitation we use the 0 degree instrument only which is noted in place.*

8 Specific comment

Line 130 – Is Kp the right index to use for this? More motivation for why using Kp instead of Sym-H, solar wind data, or another proxy would be highly beneficial here.
Reply: *Other solar activity indexes would probably show very similar results. We could mention that we used Kp because we want to the TED data in a larger model that*

is partly Kp-driven. But actually the main point is as follows: "It should be said that the Kp-binning is for illustration and discussion only while the correction algorithm is based on the background counts." This has been added.

9 Specific comment

Section 2.4 – Along with the discussions in this section about the derived flux and some of the discussion of its validation, it would be beneficial to include a discussion about the height that the derived fluxes are assumed to be at. The satellite is flying at 850 km, but the derived flux signal may be from 100km or above. The derived flux is likely from an integrated region, over the field of view – thus how does that then relate to the near point measurement of the satellite as it integrates its counts in a longitudinal sweep over the integration time in the detector. The SSUSI and EISCAT [does the reviewer mean EISCAT here?] images are including both integration within a longitudinal and latitudinal area whereas the particle detector is closer to a narrow cross section along a longitudinal path (≈ 120km in 16 seconds at about 800km altitude).

Reply: *We noted that the spatial information from SUSSI originates from 10 km x 10 km pixel resolution with later averaging on latitude bins (similar to the particle flux). Due to the high pixel resolution and the binning we do not expect a significant spatial difference between the measurements. Indeed the temporal variation between the measurement times of SSUSI and the particle flux measurement should be much more important - thus we are using a statistical approach instead of a one-to-one comparison.*

The altitude of the atmospheric ionization and the local particle fluxes are a very interesting aspect in this. For SSUSI this is described in Strickland et al. (1983). However, there is ongoing work in the SOLARIS-HEPPA community investigating the altitude of the SUSSI derived fluxes and it's comparison to EISCAT on the one hand and AISstorm ionization rates (which are based on the particle fluxes discussed in this paper) as well as other ionization rates. Two of the authors are involved. It is definitely out of scope of this paper to go into details here, but there will be a publication on this.

10 Specific comment

Line 158 – The sentence stating "Concerning this paper we may just conclude that..." It would be good to know if you did conclude this or not.

Reply: *This paragraph may easily be miss-understood. We clarified the sentences before arguing that proton precipitation may produce a stronger or similar signal than electron precipitation. Thus proton precipitation should be visible in SSUSI.*

11 Specific comment

Section 2.5 Thus far into the paper, there is not a discussion about S4. I would add something about comparing to S4 and SUSSI in the last paragraph of your introduction (around Line 70) as you do the Kp index. I would also discuss in here how the S4 index would be used for calibration or identification that there is proton precipitation (thus a more complicated period to determine if there was also electron contamination). As this is also an inferred response, a discussion about how to compare, or why you would compare the two datasets would be beneficial.

Reply: *Right, this section is not adequately represented in the introduction. We changed introduction to the next section which is following right away. The changes are given in the answer to Comment 2.*

12 Specific comment

Figure 1 – this figure would benefit from having the continents shown. It looks like what is being presented is the slot region and the filling of the slot region as geomagnetic activity becomes more active. It might help to compare how this looks to similar energies observed in the inner magnetosphere, e.g. from the HOPE, RBSPICE, MagEIS, and REPT instruments from the Van Allen Probes depending on the energy range you are focused on.

It would also help to have 0deg in the middle of the plot so that the SAA was not split along the edges. I would also double check the mapping as it seems different than previous studies (but difficult to tell based on how it is currently plotted). See for example Figure 1 in Girgis et al. (2023).

Reply: *The plot shows APEX coordinates, which slightly change the more common representation as known from geographic coordinates. As consequence of this we do not want to add the continents here. We checked the location of the SAA presented Fig. 1 of Girgis et al. (2023) against the SAA in the TED channels as e.g. shown in an earlier paper of us (Yakovchuk and Wissing, 2019, see Fig. 1). It nicely agrees. However we do not see a need to add that to the paper as it would just add the information that we are able to plot data correctly.*

The slot region typically is located between L=2–3. The slot region filling events may cover L=2.5–3.5. This does not coincide with an effect that stretches from L=3–6.

13 Specific comment

Line 190 – this is likely due to the SAA vs the region of the local bounce loss cone and there are references showing this that should be added to this sentence/section.

Reply: *We agree that it is likely that the SAA has an impact on the local bounce loss cone. But in this part of the paper we are just describing the observations. It is not clear in this part of the paper if the detected flux originates from the desired particle species and energy or if it is contamination. So it does not make sense to explain hemispheric differences by the SAA which finally turn out to be a completely different particle species which even does not precipitate but is mostly trapped.*

However we should add that the hemispheric asymmetry of the contamination is related to the altitude variation of the polar horns of the radiation belt which reaches lower altitudes in the southern hemisphere. The different radiation particle fluxes are shown e.g. in Winant et al. (2023) who compare the van Allen probe measurements on POES/Metop altitudes against the AE-8 model (Vette, 1991).

14 Specific comment

Line 201 – I would point the authors to studies for electrons and protons of similar energies observed in the geomagnetic equator related to the inner and outer radiation belts as well as the slot region and how those distributions change (or not) with geomagnetic activity (e.g. Li et al., 2023; Yue et al., 2017). For some of the energies, the local time is also likely important so averaging over local time in your geographic bins may impact how the data is viewed.

Reply: *We thank the reviewer for addressing these papers. Yue et al. (2017, their Fig 2) shows statistical flux maps of low energetic 11 keV protons measured by the HOPE instrument onboard the Van Allen Probes. During Kp≤1 the highest fluxes are observed between L=5 and the image boundary at L=6. It steadily decreases till L=2 or 3 where it reaches the minimum flux. During higher Kp the flux is increased from the outher boundary down to L=4 while L=4 does not represent a local maximum. This is in contrast to the (contaminated) flux in the TED channels that show a maximum at L=4, while it agrees with the SSUSI observations (Fig. 6) that show enhanced fluxes at L=4 (equivalent to -60° APEX) only during active periods. Li et al. (2023, their Fig. 1) adds proton fluxes at 55 keV from RBSP-A / RBSPICE to more comprehensive picture. Even though this is slighly higher than the TED fluxes (<10 keV), it shows a similar pattern.*

The reviewer is right in mentioning that MLT variation exists in the particle data. Two of the authors investigted this in detail (Yakovchuk and Wissing, 2019). However the impact on this paper is limited. Of course the MLT variation will affect Fig. 4 somehow. But e.g. the comparison to S4 is completely night-time which filters out MLT variations while the subauroral differences persist. The correction is based on background counts which should be independent from MLT and only synchronize with energetic particles. This information will be included.

15 Specific comment

Section 4.1 it might be useful for the authors to pull out the times when the satellites are flying through the SAA for the L-shell plots (using a geographic definition instead of just P6 filtering) to separate out those periods vs other periods where there may be contamination. It seems that the authors may have done some of this, but it's unclear to me if the authors removed the data from the SAA in Figure 4. If the SAA is removed from those figures, knowing how close the enhanced regions are to the SAA would provide an understanding of if the satellite was clipping that region or not and thus if the filtering was just inefficient.

Reply: *The P6 filtering has been applied to the MEPED electron channels only. When looking at Figure 4 (lower panel) this seems to work nicely as all electron data at 60°E and L<2 is removed. We rephrased the paragraph to point that out. It now reads: "The SAA is well known for intense particle contamination, particularly affecting MEPED electron channels (Peck et al., 2015). Therefore we applied a filtering method the the MEPED electron channels, which uses the P6 channel to effectively remove heavily contaminated MEPED electron data from the SAA (L≤2). Indeed, all MEPED electron data in the SAA-crossing 60°E longitudinal cross section is neglected." Concerning "other periods of contamination": we are not too much interested in the contamination of the MEPED electron channels as this has already been done. For the TED channels the area of the SAA and the subauroral area is investigated separately (without any P6 filtering).*

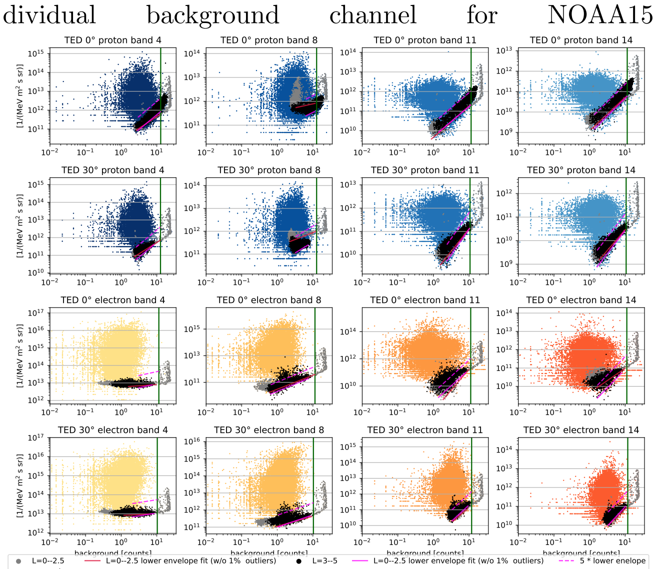
16 Specific comment

Figure 4 – How much of these differences is from the individual satellites flying through the radiation belts at different times? Are some of the differences due to some satellites sampling the peak in the belts due to a geomagnetic storm and others not quiet capturing that time frame? How much time is included in each of these plots? Are you taking both the northern and southern hemispheres together? Are some satellites having a bias between the conditions in the different hemispheres? E.g.

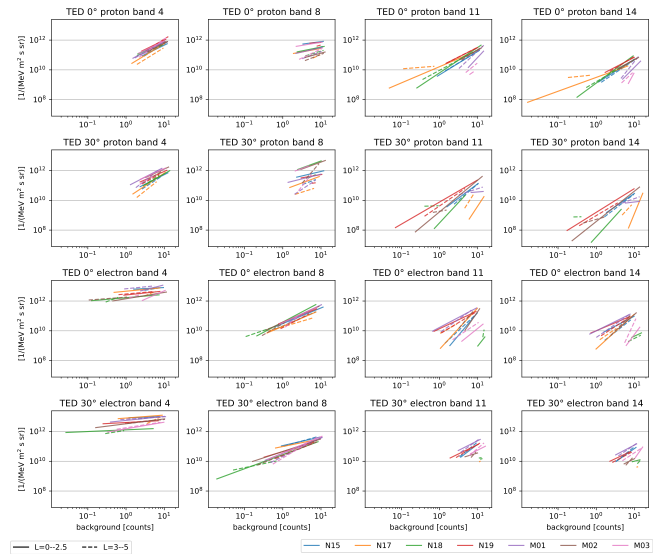
Is there more time at <0.7 Kp in the northern vs southern hemisphere or over the SAA for the different satellites? I'm assuming this is the average/mean flux value, what are the quartiles and median?

Reply: *The individual satellites are now shown in the updated Figure 8. The new Fig. 9 shows their individual differences. In most cases subauroral and SAA contamination are very similar between these satellites. Thus there is no significant sampling issue. Outliers are likely an indication for problems on a specific satellite's channel. Figure 4 contains 1098 Kp intervals, each of them containing 3 h of data filled with 16 s data points. This is slightly more than one third of the annual 2013 data. The plot is L vs. flux, thus both hemispheres are included according to their L-shell distribution. For a more detailed analysis we refer to the satellite specific contamination approach which uses the background counts as indication for the reliability of the flux.*

Correlation of TED channels and their individual background channel for NOAA15



Lower envelope of the correlation of TED channels and their individual background channel for all satellites.



17 Specific comment

Line 233 (and Line 244, and 249) – by (non-)Vertical TED proton (electrons) fluxes do the authors mean the integral channels or trapped distribution?

Reply: *Neighter nor. In the case of the TED instrument there are no integral channels, nor would we expect that the 30° instrument detects only trapped particles. Anyway, we added "differential" for clarity (in line 233). The same applies to the TED channels in line 244 and 249.*

18 Specific comment

241 – Given that the MEPED and TED channels are looking at different energies, would we expect the response to be the same? What is expected when comparing the different sensors?

Reply: *Will be removed according to comment of Reviewer 1, stating that the paper does not add anything new to the MEPED instrument and should be limited to the TED instrument.*

19 Specific comment

244, 249 – Please define Non-Vertical Fluxes. If this is the differential fluxes that weren't calibrated or had corrections – how does this impact the results here?

Reply: *We added to the particle data description that that whenever we are mentioning the TED particles in this paper, we refer to the differential channels and that these are always uncorrected (until now). This paragraph describes the particle uncorrected fluxes as they are provided. The correction is determined in Section 5.1 and applied to this data base in Fig. 10.*

20 Specific comment

Section 4.1 – How do these results compare to observations of trapped or lost populations from other missions/results.

Reply: *We refer to the answer to Comment 14.*

21 Specific comment

Line 254 – The authors state that the MEPED electron channels are not affected "as well", but it seems that in the previous paragraph the TED measurements are affected, but possibly not to the thresholds of other electron loss events, but the response is changed. Some clearer language here would be useful.

Reply: *This comment is unclear. The text in line 254 clearly tells that "MEPED electron channels* ***are affected as well****." There is no "not". Anyway, this will shortened due to Reviewer 1's comment about MEPED channels.*

22 Specific comment

Paragraph starting line 259 – Would you expect TED and MEPED electrons to respond similarly? They are different energies.

Reply: *No. But this channel is describing the contaminating particle population. This probably should be noted close by.*

23 Specific comment

Line 264 – I'm not sure which data set this sentence is discussing.

Reply: *We clarified this paragraph by adding "subauroral" to the title and referencing to Section 4.1 where the absence of energetic protons is described.*

24 Specific comment

Line 274 – Radiation belt losses are due to many phenomena beyond hiss and EMIC waves and are dependent on energy of particle they are interacting with, geomagnetic activity, local plasma conditions, the stretch of the tail, plasmapause and magnetopause location as well as dependent on L and MLT, among many other things. Since this study considers a statistical look, I would compare to statistical Van Allen Probe studies and not case studies.

Reply: *We will carefully go through Millan and Thorne (2007) and summarize the loss terms. Also we will mark the case studies as such.*

25 Specific comment

Section 4.3 – The discussion of shielding and what is observed by the TED instrument should go up in the data section as it is relevant to the entire process and any confidence in the data being returned. The paragraph starting on line 287 also makes it seem that a first step would be to work on the intercalibration of the sensors instead of combining them in some of the previous figures.

Reply: *On the one hand the shielding is crutial the whole discussion on the other hand it is also part of the results. And this is not limited to the impact of the different shielding on the fluxes but also the completely undocumented arrangement of the detector stack. We will add information about the detector stack and that it implies a different shielding to the data section. And we will refer to later sections for details.*

26 Specific comment

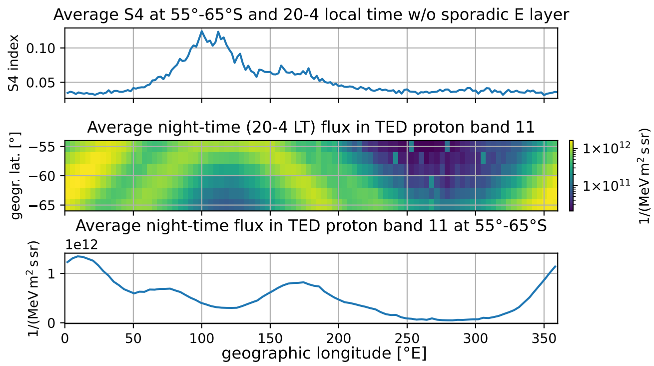
4.4 – It seems that previous studies have looked extensively at the contamination of the MEPED sensors, it would be beneficial at the start of this discussion if there could be a discussion about those previous results and if this studies results agree or disagree before going into the specifics from this study.

Reply: *According to Reviewer 1 this subsection does not add new aspects so we are going to remove most of the MEPED content and discussion.*

27 Specific comment

Figure 5. – are the TED proton band also limited to nighttime? I'm assuming S4 is an integrated signal/proxy that was also not filtered by Kp. It would likely be useful to then turn the TED proton band into a similar integrated measurement for a more direct comparison.

Reply: *Yes, we totally agree. We now enlarged the data base of the TED channel to match with the years of the S4 data set and put a filter for night-time in the same manner as done for the S4 data set. The result is shown here:*



We revised the text accordingly. However, it does not chamge the main statement that the subauroral maximum does not appear in S4.

28 Specific comment

I'm not sure what is meant by the wave like structure – this looks like it might just be the impact from the location of the SAA? It might help to plot this is geomagnetic coordinates instead of geographic.

Reply: *Exactly this was meant. However wave like structure might be missleading. It now reads: "The sinusoidal shape..."*

29 Specific comment

Paragraph starting in line 339 – This result makes it seem like a better color bar might help with the above result as the "wave structure" seems to greatly disappear. I would suggest finding a more linear color bar for the middle plot. Such as the cividis color bar in python `https://matplotlib.org/stable/users/explain/colors/colormaps.html`

Reply: *We new applied the linear and color-blind friendly color bar "viridis".*

30 Specific comment

Line 346 – I'm not sure if this would be a "detector artifact" or contamination from the SAA.

Reply: *We should clearly point out that this is not the SAA. The SAA affects geogeaphic latitudes equatorward of 50°S. In contrast, we are seeing a contamination by the polar horns of the outer radiation belt - which of course are a consequence of the lower local magnetic field just like the SAA. We used the term "detector artifact" in order to say that this is not the real TED proton flux. But you are right that we should use the term contamination here. This information has been added to the paragraph.*

31 Specific comment

Figure 6 and Section 4.5.2– the labels for the top three panels could be improved for clarity. I'm assuming those are the TED and MEPED. Are they from all the satellites? How does the lack of intercalibration impact this result? If this is all the instruments together, is this an impact from one instrument or one channel/data product? It seems that this is also averaged across longitude. I would also make all the x-axis in the same units. How comparable are the expected/inferred energy ranges between SSUSI and TED/MEPED? Are these sensors more sensitive to different energies so when combining them are all energies likely able to be combined in this fashion?

Reply: *The first sentence of Section 4.5.2 reads: "Figure 6 (top panel) presents the total precipitating proton energy flux derived from all 0° TED proton channels and MEPED 0° P1, covering an energy range of 154 eV to 80 keV." It is a composite from all satellites. The lack of inter-calibration will somehow affect the results but there is no (useful) inter-calibration before removing the contamination so this is probably the wrong question here. Anyhow, the main aspect in Fig. 6 is the falsification of the flux peak at L=4 (-60° APEX) and for this no inter-calibration is needed as all TED channels have it.*
The next question considers the longitudinal coverage. The intention was of course to compare similar things so both are centered at 60°±4° APEX longitude. However, the information that this also applies to the particle flux needs to be added to the paper. The figure will get the same x-axis.
We tried to select the same energy range for both. However, we will check these numbers again carefully. But we are not expecting the selection of the energy range to be very crutial. The auroral ionization is a local peak, so a few keV should not produce a major qualitative difference, especially as all energy channels in this energy range show a similar latitude-vs-Kp pattern. We should add that an in-depth comparision of particle fluxes, ionization rate models and SSUSI is an ongoing project as already discussed in answer to Comment 9.
The information will be added to the paper.

32 Specific comment

Section 5. More discussion could be used here. I'm not sure I follow how the conclusion is formed. I believe the authors may be correct that there is cross contamination between the channels, but a deeper discussion instead of pointing to sections would be helpful. What results from those sections are leading to this conclusion?

Reply: *We will add more discussion especially on the single-satellite correction and the intercomparison of the* *lower-envelope fits of the different satellites. It turns out that all of the channels have a certain value when the background-counts are in saturation. Above this saturation threshold no data should be used. In most channels the threshold is far above normal background values which means that a cut at the threshold is no problem for the data correction. In some channels however that threshold seems to be within the typical range of background values. Here a cut will also remove a big part of the flux values. Below the threshold most of the channels can be corrected. Some of the channels, in particular the proton band 8 (in both viewing directions) shows very strange response to energetic particles, being completely different in the SAA and the subauroral area. We will update Table 2 with the single satellite values (and a linear fit in log-log) that may be better extrapolated than the current polynomial. The Figure showing the corrected counts will also be updated. In addition we will provide a python routine for an easy application of the correction.*

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