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Comment on acp-2021-737

Anonymous Referee #2

Referee comment on "Exceptional middle latitude electron precipitation detected by balloon observations: implications for atmospheric composition" by Irina Mironova et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-737-RC2, 2021

The authors use a number of observations and simulations to investigate the atmospheric impact of an EEP event identified by a ballon borne experiment. The study is very interesting, but I find that some of the necessary supporting information is not included and it is not always clear why things were done the way they were. Detailed comments and questions are included below and I recommend that these are addressed in a revision before the paper can be accepted for publication.

Major comments:

- Why is some data not shown? For example, the VLF data is used to justify the extend and occurrence of the EEP event, but is not shown in the paper. Similarly, the POES data is only presented with a daily resolution. Both datasets are used to explain that the event lasted for a few hours and motivate the model simulations. This is even explicitly mentioned in the discussion so it seems important that you present more of these observations.
- With predicted ozone loss at 90% level, I was left wondering why are you not using MLS and MIPAS ozone observations to check for ozone impacts as predicted by the model simulations, as you already use other measurements from both instruments.
- It is not clear what the reason for using two separate models is. Why do you need to use the HAMMONIA model? The event is short-lived, and any model results are only presented for a period of four days. Why not use only the ExoTIC ion chemistry model so that the particle impact can be simulated as accurately as possible? This would also mean not needing to account for horizontal transport effects. The balloon measurements are from one point so most accurate ionisation rates are for one location only. Presently only the NOy partitioning from the ion chemistry model are shown. Could you show the density/VMR results from the ExoTIC ion chemistry model?
- The HAMMONIA model results in Figure 5 are presented in a format that is difficult to assess (spatial extreme statistics). If the impact is limited to within the polar vortex you could use a vortex average, or present the results as maps for example. Also, since you have observations to contrast the model results directly to, I suggest presenting the results in a way that allows the reader to make those comparisons.
- For the ionisation rates, balloon rates and AIMOS rates are used. AIMOS uses POES

observations and you show that POES measured enhanced EEP on the 14 December, as also observed by the balloon. What is not clear presently is how this does not lead to "double counting" EEP ionisation in the simulations where AIMOS + balloon rates are used. This need to be clarified. Can you include an example of representative AIMOS ionisation rated in Figure 4a) along with the balloon rates for comparison?

Further comments:

- Level of radiation belt information and referencing. For example in the Introduction, line 18: I would not expect most ACP readers to know about adiabatic invariants in the radiation belts. There are no references to help the reader, but, actually, this is not even needed to understand the study (from the atmospheric point of view, the precipitation side is a different story!). Similar situation happens again in the first paragraph of section 5, where you have a lot of detailed information about radiation belt processes that are not really needed in the context. For example, discussing inward radial diffusion does not seem necessary for the results presented in the manuscript, which are focused on the atmospheric impact. I suggest the authors look at these sections critically and revise the text so that it best supports the understanding needed for this work. Whether this is to dial the radiation belt side down, or provide much more context (in which case please include more references) I think is up to the authors.

- Since you show data from both, you need explain what the difference and significance between the two POES telescopes (0° and 90°) is.

- With the instrumentation relying on X-ray absorption, did you check for any changes in solar X-ray flux during the time of investigation?

- You state that the Apatity balloon didn't observe EEP 5h before the Moscow balloon, but at some point in the three overpasses over Apatity POES (0° telescope) did. Moscow balloon observed EEP, but POES 0° telescope did not, however, POES 90° telescope did. You should explain in the text what the significance of these is. It is not clear at present why the Apatity observations are included, or why the 0° and 90° telescopes provide such different results. The Apatity observations do not seem to be discussed any further in the manuscript.

- Section 3.1. Can you provide some context for the observed Dst and AE values representing moderate levels of geomagnetic activity? These seem rather low values to me. You should also include a reference for southward IMF being needed to enable electron precipitation.

- Section 3.2 the first paragraph should be moved/merged in the introduction section.

- What do you mean in section 3.2 about HNO_3 controlling stratospheric ozone depletion? Do you mean as a reservoir for NO_x, in PSCs, or its role in denitrification? It would be good to explain this already in the introduction as you go on to present both observations and simulation of HNO_3. Please include references. As you mention its importance for stratosphere, could you elaborate on the potential contribution of mesospheric HNO_3 (since these are presented) to stratospheric HNO_3?

- In the context of fast horizontal transport, rather than try to see a localised impact, why not utilise observations of tracers and/or include illustration of the edge of the polar vortex? I understand this could be possible from MIPAS observations.

- For the averaged MIPAS and MLS observations at latitudes 10-55 geomagnetic, geographically this includes the equatorial region. This seems concerning, why include

geomagnetic latitudes as low as 10°?

- Section 3.2, line 157. Substorm activity has not been mentioned before this. Some further explanation is necessary.

- line 164. What do you mean here by fast horizontal transport? Do you mean that the polar night jet extends to these low latitudes (what about horizontal mixing within the vortex)? If we assume a 70 m/s zonal wind speed, and take a simple zonally symmetric vortex, at latitudes of 40°-50° it would take 4-5 days to circle the Earth. I don't quite see how the hotspot would have move such a large distance within the same day, even if the vortex is likely not zonally symmetric. Perhaps this just need a little bit more clarification. If anything, the MIPAS maps seem to suggest to me that there could have been a precipitation hotspot over the North-America/North-Atlantic sector on 14th Dec (more analysis would be needed to show this of course)!

- Figure 3 caption: What is the rhombi mark? Diamond?

- Are the models forced with ionisation for 6h or 8h? There seem to be conflicting numbers in the manuscript. On this topic: the balloon was only up for a very short period of time: 13:26-13:45 UT. How does this justify 6h/8h of forcing? POES observations supporting the use of 6h/8h are not presented.

- Is constant ionisation used for the whole 6h/8h? If yes, you should provide some justification for this.

- Relating to one of the major comments: Why use different ionisation rates in the HAMMONIA model? How much do these differ from the balloon ionisation rates? From reading section 4.3, it really seems like there could be double counting of EEP, with AIMOS representing medium energy electron precipitation using POES and adding on the balloon ionisation rates (also representing medium energy electron precipitation!).

- I find Figure 5 confusing: These are simply NH maximum of minimum values, without any regional restriction. The contour labels are too small (one can not see the powers clearly).

- Figure 5 and section 4.3: There doesn't seem to be clear evidence for residual circulation in these figures. The timescale of 4 days is not really enough to see vertical transport clearly. I suggest carefully revising the text regarding residual circulation or showing more supporting evidence.

- Line 238: You are not presenting model simulations over Moscow, so you do not show ozone loss over Moscow during the event. Consider the use of the extreme statistics not tied to a specific location, or revise here.

- Section 5: "*Both POES and VLF data on 14 December, as well as NO_x and HNO_3 observations throughout December, suggest that events indeed lasted for a few hours…*" It seems critical that you present the POES and VLF observations in high enough a temporal resolution to support this statement.

- "*Complete destruction of ozone in the upper mesosphere over the region where high-energy electron precipitation occurred is also shown by HAMMONIA numerical experiments*." Statement like this is conflicting when you present the results not tied to a location. I can see why you many want to present the maximum impact possible (in which case the ExoTIC model may have been sufficient, rather than HAMMONIA). On the other hand as you state, horizontal transport is relative rapid and these effects may be mixed in quickly and in this context also zonal averaged may be justified. I strongly recommend

you consider presenting the HAMMONIA results in a different format (perhaps a time series of horizontal maps with a selected time resolution). I would also very much like to see the ExoTIC model results.

- Conclusions "*This conclusion inspires further studies involving a wider network of the balloon-based instruments*." I think for a commend such as this, it is important to show how different the balloon ionisation rate is to the AIMOS ionisation rates.

- Please pay attention to the varied used of "*middle latitudes*" and "*midlatitudes*". It is confusing, I suggest using just "midlatitudes".