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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Local impact of solar variation on NO_2 in the lower mesosphere and upper stratosphere from 2007–2011" *by* F. Friederich et al.

Anonymous Referee #1

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This article is very interesting and timely considering the recent research on energetic electron precipitation impacts on the middle atmosphere. I do find that improvement is needed before the article should be published and therefore recommend revision. Specifically, I think that the authors need to clarify and address their justification for the data selection criteria (spatial and temporal), as at the moment this does not come across clearly and it is very unclear how it affects the results. There are also some statements, which are not correct, e.g. that the magnetic latitudes would be more difficult to determine in the Southern Hemisphere. These need to be corrected. I have discussed these in detail in my comments below. I also have a few other comments I

would like the authors to address before the article is considered for publication.

General comments:

- Why have you used just NO2? MIPAS observed both NO and NO2 so the analysis could be done for NOx, instead of just NO2. Surely the conclusions made on impact on NOx and NOx-production rates would be more robust if total NOx was used?

- Why limit the study to solar minimum times? You are focusing on seasons when the dynamical impact (or indirect-NOx) is minimised so why not include the whole MIPAS time series? This would provide more data for the analysis leading to more robust conclusions - this would also be interesting for the wider audience. Also, I debate weather 2011 could be considered solar minimum, what were the Lyman-alpha levels like, for example, in 2006 (Fig. 1)?

- I think the seasonal & latitude selection needs to be clarified. The abstract suggest that the study focuses on the spring/summer/autumn seasons, but the selected time periods include the southern hemisphere mid-winter. The potential effect of descending NOx rich air in the Antarctic polar vortex is said to be taken into account by restricting data to latitudes from 50S-80N, but in the upper stratosphere the Antarctic polar vortex can easily stretch to 50S and further equator wards so this does not necessarily exclude the downwelling NOx impact, particularly not in mid-winter. Why not just separate the NH and SH analysis so local seasons can be looked at completely separately? Also, it is simply not true that the magnetic latitudes are more difficult to determine in the SH. The data selection criteria do not seem to be robust and this needs to be addressed.

Specific comments:

Abstract:

- Last sentence: This is not the first study showing impact on trace gases, as you write in the introduction, effects on hydroxyl, down to 50km were discussed previously,

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e.g. by Andersson et al., 2012 who covered all seasons. Hydroxyl is a trace gas, so perhaps this should be changed to NO2 instead. - Same sentence: “local impact” could be interpreted as local in latitude and longitude, but the data used was averaged over all longitudes in the latitude band, I suggest changing the wording accordingly.

Introduction:

- Page 32328, line 20 “relativistic energies”, It would be good to add the electron energy range in question here.

Chapter 2:

- Page 32330, lines 21-23: This will not be clear to most readers, please clarify or provide a reference where this is data selection method is explained.

Chapter 3.1:

- Page 32332, line 2-3: This is simply not true.

- Why the long time interval +-30 days? CIR, which are common during solar minimum times, are expected to repeat in the epoch analysis as events at +-27 and 0 days. You will end up counting several events in each epoch period, and counting them again as individual day = 0 events. This should be discussed.

- Why Delta Ap > 3.5? How is this limit determined?

Chapter 3.2:

- Epoch type 1: What do you mean by “out-of-phase UV radiation having non-linear influence on deltaNO2”?

- Epoch type 2: Since you are using the Ap index the more accurate statement here would be that the NO2 enhancements are linked to the Ap peaks, not that they are cause by pure electron precipitation. After all, Ap is not a measure of electron precipitation, just used as a proxy for it, but also affected by protons. Last sentence: It

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looks like this peak is centred at day -15, which does not clearly correspond to the Lyman-alpha. Please clarify.

- Epoch type 3: The correlation is also very good with Ap, based on Fig. 2.

- Epoch type 4: “due to noisy Ap signal”, the goal is to control for the Ap signal by restricting to DeltaAp < 1, so I’m not sure I understand what is meant by noisy Ap signal?

- Figure 3: What happens if you include the total NOx, instead of just NO2 in the analysis? One would expect to see the correlation extending towards higher altitudes. I think this would be an important test.

- Page: 32335, lines 13-15. Yes, this would be expected as you are looking at the in-situ (1 day, i.e. no delay from vertical transport point of view) instantaneous impact, for particle precipitation this should indeed be tied to the magnetic field. The high correlation extends to geomagnetic latitudes >70N, is this in line with the expected impact region of radiation belt electron precipitation?

- Figure 5: This makes me worry about the Ap index filtering criteria for the UV impact. How is the Ap criteria selected? What is the significance of the p-values for this type of correlation?

- Figure 6: Why is the UV correlation not at all symmetrical wrt the equator?

- Page 32336, lines 1-4: The meaning of this sentence is very difficult to understand, could you please clarify it.

Chapter 3.2:

-Page 32337, lines 14 and 16: Do you mean NO2 lifetime here as only NO2 observations were used? The NOx lifetime should probably be different from the NO2 lifetime, particularly at the higher altitudes?

- Figure 8: I’m not sure I understand this figure, it should be discussed in more detail.

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(Currently only mentioned on one line).

Typos:

- Abstract, line 16: “at that altitudes” should be “at those altitudes”

- Lyman-alpha with a hyphen. Across the text.

- Page 32336, line 13 and later, Should “pr” be in italics as on line 11, or should the italics be removed on line 11?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32327, 2013.

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