Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-514-RC2, 2017

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "NO_y production, ozone loss and changes in net radiative heating due to energetic particle precipitation in 2002–2010" *by* Miriam Sinnhuber et al.

Anonymous Referee #2

Received and published: 14 July 2017

Overall this is an interesting, well-conceived and well written paper that should be of solid interest for ACP readers. I have one general concern. Assuming it can be addressed, along with several more minor concerns, the paper should be suitable for publication.

The main concern is that one can't help but note that the authors put much more effort into validating their NOy calculations by comparing continuously with MIPAS. Section 3.3 is quite good in this regard. They do not do this with ozone; rather, they reference other work. However, this is unsatisfactory and the net effect is that their modeled ozone changes are less robust than their modeled NOy changes. Particularly for some

of the strongest years, like 2003-2004, it would be extremely helpful and much more convincing just to show their calculated ozone for with and without EPP/SPE compared with some observations. Even simple comparisons- like for example, the ozone reduction discussed by Natarajan et al [2004, GRL] (and which remains, in my opinion, the most compelling case of stratospheric ozone reductions in response to these events, and which unfortunately, is not cited) would be better than nothing. Although it would be hoped that they could do more. Many previous works (Siskind, Funke, Jackman) gave the various contributions to NOy in both absolute numbers but also percent. Given the differing transport amongst the 3 models, it would be useful to get percent contributions. This would be especially helpful for the ozone and heating rate calculations. I had a very hard time deciding how significant these effects were. Continuing on the above thread, for Figure 11- the post SSW changes look interesting. Again, along the lines of my comments above, it would be much more compelling if they could do some comparison with observations. For example, show an average of the 3 post-SSW year temperatures compared with non-SSW years and then with their model. Given how comprehensive their NOy model-data comparisons were, the lack of such comparisons for ozone and heating/temperature changes are more apparent. Ultimately, with the uncertainty both in relative contribution and as well as validation, I find the last several lines (12-15 on page 32) to be too speculative in my opinion and should be removed. I only see a significant blob of color on the EMAC plot and only for one spring. Almost nothing in 2006 and 2009.

Minor Intro: I believe SPEs have been known to be sufficiently energetic to directly ionize the lower stratosphere. The text keeps saying "upper". English grammar: line 28 page 5 "prevents .. from propagating" Figure 2- they should zoom in on the vertical axis- there is no reason to show 5 orders of magnitude when 3 will cover the range. Line 9, page 6; line 1 page 10 and many places elsewhere. Could the authors please use hPa rather than Pa? Unless ACP has a preference, I believe more people in aeronomy intuitively think in terms of hPa. Related to the above- line 29, page 12. Doesn't > 100 pa refer to the stratosphere? The text says mesosphere. Figure 8-

why are there apparent vertical discontinuities in the ozone change? For example, top panel, beginning of 2005 where the colors go from dark green to blue instantly over a wide range of values.

Referencing: I believe Randall et al 2007 first coined the phrase “EPP-IE”. They should be cited on line 8 of page 3. They already are cited elsewhere.

Siskind references get the first initials wrong. In one place its DR in another its DW. Should be DE

Same problem with Fleimng- its EK in the reference with Jackman. EL in other places.

Matthes paper is 2017 as it has appeared now.

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