

Interactive comment on “HEPPA-II model-measurement intercomparison project: EPP indirect effects during the dynamically perturbed NH winter 2008–2009” by Bernd Funke et al.

Anonymous Referee #1

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This paper is an important and substantive contribution that, subject to my comments below, certainly merits publication in ACP. Its conclusions are credible and will be valuable for modelers. I do have lots of comments; their overall intent is to improve the paper and make it more useful to the community. A important editorial comment is that the paper is simply too long. There is a tradeoff that must be made between comprehensiveness and readability. These two parameters often anti-correlate. In this case, I believe the authors have leaned too far in the direction of comprehensiveness at the expense of readability. I also think there are some references that they should add-these are noted in the text below.

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For example, Section 2 is too long by probably a factor of two. These datasets are mostly all mature and have been used for this problem in the past. For example, we do not need to be reminded that (line 31 on page 8) that SABER temperatures require non-LTE calculations in the retrievals or the details of when SABER is yawed north or that SABER's duty cycle is still nearly 100% (line 24-25). Rather, there is a rich literature of observational studies of these elevated stratopause events that could and should easily substitute for much of the text in Section 2. For example, although the paragraph on MLS is not too long, I am quite surprised that none of Gloria Manney's work was cited- for instance, her 2009 ACP paper which used the MLS data for the 2006 event much in the same fashion as presently done. Similarly, is there any usage here of ACE-FTS data that differs significantly from Randall et al., GRL, 2009 (also missing from the reference list)? And given that the Funke et al., 2014 JGR papers are cited, the usage of the MIPAS data should just cite those works; again, it is not necessary to tell us how non-LTE vibrational distributions were modeled (line 25, page 7). Finally, there must be dozens of papers which discuss SABER temperatures; two that might be useful to cite here are Siskind et al., GRL, 2007 for the 2006 event and Yamashita et al, JGR, 2013 (see their Figure 7 which shows 4 winters compared with MERRA). A similar comment concerns the models, although the situation is not as bad and Table 1 is useful. Nonetheless,, given the recent detailed discussions of HAMMONIA by Meraner et al and the discussions of WACCM by Randall et al. (2015), they should probably cut back their descriptions. For example, the citation to Meraner et al is sufficient to discuss HAMMONIA's gravity waves; all the information presented here on the source spectrum etc is superfluous and detracts from readability.

Other comments

1. I simply do not understand Figure 1. I don't see any symbol in Figure 1 which says "UBC". So how can the figure be showing it when none of the symbols do? What do the arrows show? I don't understand what the deviations are. Is this variance, standard deviation? What is the mathematic expression they are using? They should have two

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panels- one for absolute values, and one for whatever these deviations are. Then, the text needs to discuss this carefully, not simply with some parenthetical reference.

2. Figure 3 should be deleted. It adds no new information that is not already clearly shown in Figure 2. I realize that the intent is to illustrate something about timing after the SSW- all I see is a jumble of points. Certainly the spread increases after Feb 1, but I cannot discern anything else.

3. One misunderstanding that I think I have concerns when exactly during the season does EPP-IE couple most strongly with the stratosphere? From reading Funke et al's two 2014 papers, it appears that significant NO_x flux can penetrate into the stratosphere early in the winter, for example, November or December. Indeed in Funke et al., 2014, figure 10, one can see a tongue of EPP-NO_y down dipping down to below 40 km on January 1st, much lower in altitude than the descending tongue in the post-SSW period. But in the present paper (page 30), it states that descending NO_x can only be distinguished down to 0.3-0.5 hPa. This seems inconsistent and I think bears some explanation. Furthermore, if the pre-SSW NO_x is more important for its contribution to the stratospheric NO_x budget, then isn't the implication of the 2014 papers that the present focus on the post SSW descent is misplaced and of less relevance?

4. Section 7.1: It would be useful to discuss and justify the selection of CO as a tracer more. While I realize this is popular because of the low stratospheric values (page 27, line 5), I think it should also be stated that CH₄ might be easier to simulate since it would not require the details of CO₂ dissociation or reaction with OH to be handled so carefully. Indeed the present author has used CH₄ (cf. his 2014 paper) as did Siskind in 2015 and Randall has used this for SH studies (her 2007 paper). Furthermore, with the selection of both CO and NO_x we have two tracers that are being transported downgradient. Thus how can we know whether the transport we see is advective or diffusive? Is diffusion important in any of the simulations or in any of the model-model differences?

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5. There is some discussion of the upper boundary that is used for the medium-top models. But the 2016 Funke paper states that one of its main objectives was to construct such a boundary. How do the values adopted here compare with what is presented in that paper?

6. Page 39: lines 12-13. I do not see where the consideration of the sampling patterns has been so important. Figure 19 shows that the temperatures are pretty similar. The only way this sentence can be justified if there were a figure which show a case where the sampling patten was not considered vs. a case where it was. I don't think they've done this. It would not detract from any of their conclusions if this sentence were deleted.

Editorial comments

1. Line 18, on page 7 seems strange. "MIPAS passes the equator in a southerly direction at 10:00 AM. . . .observing the atmosphere day and night". Presumably the night time data are acquired when MIPAS passes the equator in a northerly direction? This is all phrased more tersely and more clearly in their 2014 JGR paper.

2. Page 25, line 1. The proper reference should be Siskind et al., JGR 2010 (not GRL, 2015), which discussed non-orographic drag in great depth. Likewise, consideration should be given to citing Chandran et al., GRL, 2011 who make this point as well.

3. Page 36, line 10, more grammar: Encountered is a verb and not an adjective and thus does not appear before the noun. It should read: "cold bias encountered at 1 hPa". Likewise page 38, line 3 "spread of the encountered below 0.1 hPa". And again on page 40, line 3: ". . . encountered during the perturbed. . ." And finally on line 11, page 40.

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