**Atmospheric
Chemistry
and Physics**
Discussions

ACPD

Interactive
comment

Interactive comment on "Technical Note: Intermittent reduction of the stratospheric ozone over Northern Europe caused by a storm in Atlantic Ocean" *by* Mikhail Sofiev et al.

Mikhail Sofiev et al.

mikhail.sofiev@fmi.fi

Received and published: 3 December 2019

Author response to the referee comments to Sofiev et al,

First of all, we would like to express our deep gratitude to the referees for their thorough work and detailed comments, which helped us improving the manuscript. We closely followed the recommendations while preparing the revised version of the paper. The reviewer's comments and our responses are presented below.

Referee 2

My principal concern is the limited discussion on the quantitative comparison between

the model and the observations of OMI and, even more importantly, MLS. For example, a figure showing the differences (or percentage differences) between SILAM and MLS ozone profiles is missing. This will add to the value of this work, showing the capabilities of the model to capture the variations of ozone horizontally (using comparisons to OMI, which are already included in the manuscript) and vertically (using comparisons to MLS profiles).

A. The figure comparing the MLS and SILAM profiles has been added and the evaluation discussion expanded, also accounting for the request of the Reviewer 3.

Throughout the paper, total ozone is expressed in Dobson Units, μmole m-3 and mole m-3. It is very important that the authors conclude in one of these units and change the figures and manuscript according to it. I would suggest using Dobson Units.

A. The units have been harmonised: column load is now in Dobson Units whereas concentration is in μmoles m-3.

2. The methodology of the comparisons should be briefly mentioned in the abstract.

A. Added

3. There is a significant issue about the quantitative evaluation of the SILAM model. In Figure 3 and Figures S8-S12, the differences between model and satellite are always spanning from +10 DU to -30DU. Why is the scale of these figures so large? These figures need to be produced again with a scale e.g. +10 to âĹij-50 DU and with a finer analysis, so that the reader will be able to easily see the areas with high differences.

A. The figures have been redrawn

4. Section 2.2: This section should be enriched with information concerning the algorithm that was used for the retrieval of ozone from the satellite measurements and on the ways this could this affect the difference found between the model estimations and the satellite measurements. Some more information on the collocation methodology are also missing. Remember that the reader must be able to reproduce your scientific

methodology, or at least understand it.

A. We expanded the outline of processing of the ozone products of OMI used in the comparison. More details are also provided on the colocation method.

Figures S1 – S7 show a latitude belt between âĹij10-30 degrees N with total ozone below 210 DU. This is a very low estimation, since in that latitude belt total ozone values are rarely below 220 DU during November, usually ranging between 220 and 280 DU (based on OMI and TROPOMI/S5P total ozone measurements). This underestimation and its effect on the model's overall output should concern the authors. Please discuss and correct if possible. Moreover, all figures S1-S7 have the same legend.

A. Yes, it was our concern indeed. This effect has just been traced to the problems with the lightning emission and some missing chemical reactions in the troposphere. Together with sub-optimal scavenging in tropics, they were causing a significant under-estimation of the tropospheric ozone in tropical regions. A new version of SILAM is gradually emerging with better skills in the tropical troposphere. However, the issue has little effect on the stratospheric concentrations and is fading out outside the trop-ics. Therefore, it does not affect the results of the current paper. We added a brief discussion in the Annex, next to the figures S1-S7, which captions were also corrected.

6. Page 4, lines 6-7: Some references or an explanation (in case that the OMI data are used to extract this conclusion) to support the "usual level of 300-350 DU", are needed.

A. Clarification added

7. Page 4, line 18: Does this mean that you actually corrected the SILAM model itself or its output for the bias? This is not clear here.

A. No, the model results are presented as they were. The sentence has been clarified

8. Page 4, line 19: Where did this "âĹij310-320 DU" result from?

A. This is just a "typical" level of ozone load in the 60-70ïĆř outside the region affected

by the depletion – the yellow-shaded areas in Figure 2.

9. Most of the "Discussion" section is just a second introduction. This is not a discussion of the work done and its outcomes and it should be moved to the respective section. To my opinion, Section 4 is the "discussion" section and it should be renamed.

A. The corresponding part of section 4 has been moved to Introduction, also accounting for the restructuring request of the Referee 3.

10. The "Conclusions" section is very short, it looks like it is written in the form of bullet points and lacks coherence. It needs to be restructured and should focus on the temporal and spatial quantitative accuracy of the prediction of such phenomena by the model. The differences and the bias between model and satellite measurements should be summarized and commented here. This is also the section to point out the novelties and significance of this work and its contribution to our knowledge about incidents like this.

A. The conclusion section has been reviewed, also following the restructuring request.

Minor Issues 1. page 1, line 16: "The high accuracy . . .." 2. Page 2, line 16: The second sentence of this paragraph should be rephrased. It is not clear what this means 3. page 3, line 17: Please rephrase as follows "The current study used three sets of satellite data: from Ozone Monitoring Instrument OMI. . ." 4. Page 4, line 14: Please rephrase, e.g. as follows "The model predictions, namely the shape. . ..". 5. Page 4, line 27: Please give the number of the section you are referring to. 6. Figure 1a shows the "Meteorological situation" (please rephrase that) for 2.11.2018 and the figure legend states that this figure refers to 3.11.2018. Please correct this. 7. Page 5, lines 5-7: The sentence "Its strength. . . ozone recovery" discusses the episode under study, while the rest of the paragraph describes the historical record of total ozone during November in the latitude belt above 60 degrees N. This sentence should be slightly rephrased (e.g. "The episode of November 2018 was a result. . .") and placed at the end of the paragraph. 8. Page 5, lines 29: The first sentence of the paragraph

should be rephrased, e.g. "The bulk impact of the episode under study. . .". 9. The word "bulk" is too frequently used. Please use another synonym, instead

A. Thank you for the detailed editions! We introduced the corrections

Referee 3 Structure of the manuscript: The structure of the manuscript needs to be changed in my opinion. The discussion section is disappointing. It contains mainly descriptions about other events and an outlook of what might or might not happen due to climate change in the future. A discussion of the model forecast results and evaluation is missing in this section. I would thus suggest a re-structuring of the manuscript: 1. Introduction, 2. Model and observations 3. Results, 4. Conclusions. Sections 3 and 4 would then go into the results section. I would shift the first part of the discussions section (about the general character of TST events) to the introduction. The second paragraph could move to the conclusion section (in a somewhat condensed way).

A. The paper structure has been reviewed, also accounting for the comments 9 and 10 of the Referee 2. Namely, part of discussion has been moved to introduction whereas section 3 and 4 became the new Results and discussion section.

2. Section 4: Evaluation of the SILAM predictions: In section 3 the evolution of the event is nicely described according to the forecasts. I think the validation results should be presented in a similar, more detailed way. It is merely a few sentences that describe the total of the results. However, this is the most interesting part! It would be good to know whether the underestimation of total ozone in the model was present before and after the event as well or just during the event. Is there an explanation for this underestimation in the model? Also, more quantification of errors of some kind (e.g. table of biases) would be nice.

A. The evaluation has been extended, also following the request of Referee 2. We expanded the MLS comparison of the vertical ozone profile and highlighted that the model skills (in particular, its bias) were not related to the episode but rather reflecting the somewhat too low oxidation capacity of the current SILAM chemistry scheme,

especially in the troposphere.

Printer-friendly version

Discussion paper