

## ***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.***

### **Anonymous Referee #2**

Received and published: 30 September 2019

In this paper, the authors describe the output of the SILAM model, which forecasts 113 species 5 days ahead. The main focus of the work is total ozone and specifically an episode of a reduction of total ozone column during early November 2018 that was forecasted by the model. The forecast was evaluated by comparing to OMI total ozone measurements and MLS ozone profiles. This technical note concludes that the SILAM model is able of high accuracy short-term forecasting of the stratospheric composition.

The paper is interesting and generally well written, even though the reader gets the sense that it was written rather hastily. There are some major issues which I think need to be addressed before final publication. These issues are in the sense major

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that they are of high importance and require attention, but they are not extensive in terms of additional work required to address them. 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). To my opinion, the paper must be adequately revised before publication.

#### Major Issues

1. Throughout the paper, total ozone is expressed in Dobson Units,  $\mu\text{mole m}^{-3}$  and mole  $\text{m}^{-3}$ . 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.
2. The methodology of the comparisons should be briefly mentioned in the abstract.
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  $\sim$ -50 DU and with a finer analysis, so that the reader will be able to easily see the areas with high differences.
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 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.

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5. Figures S1 – S7 show a latitude belt between ~10-30 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.
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.
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.
8. Page 4, line 19: Where did this “~310-320 DU” result from?
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.
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.

#### 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.

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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.

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