

# ***Interactive comment on “Case study of ozone anomalies over northern Russia in the 2015/2016 winter: Measurements and numerical modeling” by Y. M. Timofeyev et al.***

## **Anonymous Referee #2**

Received and published: 26 April 2018

The manuscript is aimed at the study of low total column ozone (TCO) episodes over the northern Russia during the winter 2015/2016. The authors analyzed the ground based and satellite TCO observations and compare them with the output of two models (RSHU and EMAC) exploited in specified dynamics modes. The subject of the manuscript is appropriate for AnGeo. The manuscript is well written and structured, the figures and explanations are clear. Despite of many similar studies have been described in the literature the manuscript provides some new information about the available ozone observing systems. The analysis of the observed data is extended by the comparison with numerical simulations and attempt to attribute the causes behind the appearance of low TCO events. There are, however, some issues which should be

[Printer-friendly version](#)

[Discussion paper](#)



clarified before the publication of the manuscript.

### Major issue

The conclusion about the role played by chemical and transport processes is not strongly supported by model results. It is mostly based on general knowledge of the different ozone time scales and not strongly supported by model results. The authors show that the RSHU model simulates some enhancement of PSC occurrence and ozone loss rate in the low TCO regions, but do not use it to support or reject the importance of chemical ozone loss. I think it makes sense to run specially designed model experiment (say the run without heterogeneous chemistry) to support purely dynamical nature of the low TCO events or explain why such runs cannot be performed.

Minor issues: 1. Page 1, Line 15: “unlikely” sounds too weak for the abstract.

2. Page 1, Lines 23-24: I would avoid using the same sentences in the introduction and conclusions. Potential readers could wonder if the minor role of heterogeneous chemistry is well known than why to tackle this issue again?

3. Page 1, Line 28: 2015/2016

4. Introduction: The motivation for the presented study should be stronger. The authors should emphasize the necessity to analyze new instruments and exploit two different models.

5. Ozone depletion: I understand this as chemical processes. However, low TCO events can be explained by the transport of low ozone to the considered location from the area inside polar vortex, where the ozone is small because of suppressed influx from the ozone production area. The authors concluded that the contribution of chemical destruction is small. Maybe than the ozone depletion term is not perfectly correct?

6. PSC: The ozone depletion via heterogeneous chemistry strongly depends on the availability of liquid sulfate aerosols. How they are treated in the models? The authors show PSC area. Does it include all kind of PSCs or just NAT?

7. Section 3: The exploited models use different meteorological reanalysis, therefore the difference in the results can be attributed to either model or reanalysis features. Would it be possible to attribute more precisely the difference between model results?
8. Page 5, lines 11-13: I am not completely agree with this statement. Does it mean that low TCO inside polar vortices will not take place without heterogeneous chemistry and chlorine activation. I think the role of transport is more important.
9. Page 5, last paragraph: In the present form it is not instructive. See my major comment.
10. Figures 4,5: The numbers are too small and hardly visible.
11. Figure 5: My impression is that the ozone loss about  $10^8$  molecules per second is too large. Please, check.

---

Interactive comment on Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2018-15>, 2018.

Printer-friendly version

Discussion paper

