

## ***Interactive comment on “Denitrification, dehydration and ozone loss during the Arctic winter 2015/2016” by Farahnaz Khosrawi et al.***

**Farahnaz Khosrawi et al.**

[farahnaz.khosrawi@kit.edu](mailto:farahnaz.khosrawi@kit.edu)

Received and published: 14 September 2017

We thank reviewer 1 for the constructive, helpful criticism and the suggestion for revision. We followed the suggestions of reviewer 1 and revised the manuscript accordingly.

*General statement: This is a fine paper on an important topic that merits publication after revision. I do have several comments for the authors, delineated below. Important ones are marked with \*.*

*\*1) The question of how denitrification and dehydration as such, versus a longer duration of cold temperatures into later parts of the spring season, have not been examined*

[Printer-friendly version](#)

[Discussion paper](#)



Interactive  
comment

quantitatively here. The authors should therefore avoid trying to make statements about how important denitrification and dehydration were (or would be) for the ozone loss. I suggest that the authors consider this point carefully in revision. I point out one place to make a change in text but I think there could well be others.

We hope that with the changes we made in the frame of the revision all misleading sentences have been corrected.

2) page 2, line 15. Please change ‘ice’ to ‘water ice’ here since some literature speaks of nitric acid ices. With this change, I don’t think you need to say ‘water ice’ later in the text; doing it once is sufficient.

We have changed “ice” to “water ice” as suggested.

\*3) page 2, line 27. This statement makes a lot of assumptions that I don’t think are merited. First, it ignores the literature on “denoxification”, much of which suggests that denoxification later in the spring, when there is more sunlight, can be as important or more so in prolonging ozone loss provided temperatures are cold enough. Second (and related), I would argue that prolonging the ozone loss depends more on vortex stability and dynamics than it does on the degree of denitrification. Please add a discussion of these issues here, with appropriate references.

The importance of denitrification for ozone loss was shown by e.g. Salawitch et al. 1993 and Rex et al. (1997). We added the missing references. For an additional discussion of denoxification and the importance of vortex stability we added the following paragraph in the introduction: *Another factor contributing to the severity of ozone destruction is the reduction of nitrogen ( $\text{NO}_x = \text{NO} + \text{NO}_2$ ) via the conversion of  $\text{NO}_x$  into  $\text{HNO}_3$  on the surfaces of PSCs, the so-called denoxification. Denoxification becomes important if temperatures are continuously low during the course of the winter as is the case in the Antarctic (e.g. Waibel et al. 1999). It has been shown that polar vortex stability, chlorine activation and ozone loss tend to be greater with lower vortex temperatures (e.g. von Hobe et al., 2013). Therefore, it is not surprising that the most severe ozone loss ever observed in the*

[Printer-friendly version](#)

[Discussion paper](#)



---

Interactive comment

4) page 4, line 28-29. I don't think these accuracy claims are true for MLS below 100 mb. Please check.

We have checked this. In Livesey et al. (2017) useful range of Aura-MLS O<sub>3</sub> for scientific studies is given from 261-0.02 hPa. As stated in Livesey et al. (2017) there had been a high MLS v2.2 bias at 215 hPa observed in some comparisons versus certain ozonesonde and satellite datasets. These high biases, however, were reduced in versions v3.3x and v3.4x, with additional smaller reductions in the ozone values in v4.2x, the version that has been used in the present study. In addition, substantial oscillations that were present in the ozone profiles in previous versions have been ameliorated in v4.2x.

5) page 5, line 2. Missing a word. Lowest retrieval level?

Thanks for pointing this out. It indeed should read "lowest retrieval levels". This has been corrected.

6) page 5, line 30. Reader needs a pointer ahead to indicate that you will define what you mean by 'unprecedented'. Add 'leading to unprecedented formation of ice PSCs (defined quantitatively below)' . . .

The sentence reads now: Temperatures dropped during the first cold period (December to end of January) below the ice formation threshold temperatures (Manney et al., 2016) leading to unprecedented formation of ice PSCs as will be discussed in more detail below (see Fig. 2).

7) page 7, line 10.  $2\text{CH}_4 + \text{H}_2\text{O}$  isn't quite total hydrogen. I don't think it matters much for your purposes, but please have a look at LeTexier et al. (QJRMS, 1988) on this.

This is correct, total hydrogen is properly defined as  $2\text{CH}_4 + \text{H}_2\text{O} + \text{H}_2$ , but in the lower and middle stratosphere H<sub>2</sub> is constant and thus total hydrogen can in the lower/middle

[Printer-friendly version](#)

[Discussion paper](#)



stratosphere be derived from  $2\text{CH}_4 + \text{H}_2\text{O}$ . We changed the text as follows to be more precise: *Dehydration from the EMAC simulation is derived by using total “stratospheric” hydrogen ( $2\text{CH}_4 + \text{H}_2\text{O}$ ) as substitute for a passive  $\text{H}_2\text{O}$  tracer (e.g., Rinsland et al., 1996; Schiller et al., 1996). Molecular hydrogen ( $\text{H}_2$ ) is nearly constant in the lower and middle stratosphere and can therefore be neglected in the calculation of total hydrogen. The quantity  $2\text{CH}_4 + \text{H}_2\text{O}$  is generally constant in the stratosphere. However, slight deviations from this quasi-conserved quantity can be found at high latitudes during winter where transport of mesospheric air rich in molecular hydrogen and poor in water vapour and methane is brought into the upper stratosphere (e.g., Le Texier 1988, Engel et al. 1996).*

\*8) page 7, line 18, 19. Need to be more careful here. You could say something like ‘The Arctic winter 2015/2016 had the greatest potential yet seen for record Arctic ozone loss if the vortex had remained stable (and temperatures had therefore remained cold) through late March’.

We refer here to the results by Manney and Lawrence (2016) and changed the paragraph as follows to make this more clear: *The Arctic winter 2015/2016 appeared to have the greatest potential yet seen for record Arctic ozone loss (Manney and Lawrence, 2016). Temperatures in the Arctic lower stratosphere were at record lows from December 2015 to early February 2016 (Manney and Lawrence, 2016; Matthias et al., 2016). As was shown by Manney et al. (2016) ozone destruction began earlier and proceeded more rapidly than in 2010/2011, the winter that so far has been the one with the strongest observed ozone loss in the Arctic (Manney et al., 2011). That lower-stratospheric ozone loss did not reach the extent of that in spring 2011 was primarily due to a major final stratospheric warming in early March 2016 that led to a vortex split and a full breakdown of the vortex by early April (Manney et al., 2016)*

\*9) page 9, line 30. Interesting – can you say something more about which other  $\text{ClOx}$  species are likely to be holding too much active chlorine?  $\text{Cl}_2\text{O}_2$ ?  $\text{ClONO}_2$ ? Also, I don’t think you can rule out that the activation is at the right time but just too weak? What is your

Printer-friendly version

Discussion paper



justification for ruling that out? Please clarify this here, as well as in other places where it is mentioned.

It is correct that a possible explanation could also be that chlorine activation is just too weak. We unfortunately cannot rule out for sure what the cause of this discrepancy is. We know from other comparisons that there are also differences between the simulated and measured HCl and ClONO<sub>2</sub>. Further, comparisons between different photolysis schemes performed by our colleagues at KIT (M. Sinnhuber and S. Versick) have revealed that the EMAC photolysis rates are too low at high solar zenith angles (>90°). The sentences have been changed as follows: *However, the enhancement of ClO<sub>x</sub> (ClO<sub>x</sub>=Cl+HOCl+2·Cl<sub>2</sub>+2·Cl<sub>2</sub>O<sub>2</sub>) in the EMAC simulation is found at the same time as in the Aura/MLS ClO observation, thus indicating that the later increase in ClO is not necessarily caused by the activation of chlorine being too late in the model simulation but could also be caused by the partitioning between the active chlorine species. In EMAC the photolysis rates are calculated with the submodel JVAL (Section 2.1). JVAL is part of the standard configuration of EMAC that was also used in the EMAC simulations contributing to the Chemistry Climate Initiative (CCMI, Jöckel et al., 2016) (note a similar configuration is used here apart from the resolution). An intercomparison of several photolysis scheme has shown that JVAL provides lower photolysis rates at very high solar zenith angles (>90°) for e.g. Cl<sub>2</sub>O<sub>2</sub> than other schemes. Thus, the partitioning of chlorine containing species may be shifted for high solar zenith angles and thus could be the cause for the delay in the activation of ClO in the model simulation. However, to entirely rule out the cause for this difference further studies are necessary which however are beyond the scope of this study.* The sentence in the conclusion has been changed as follows: *Since the enhancement in modelled ClO<sub>x</sub> is found roughly at the same time as the increase in ClO observed by MLS, the disparity in the modelled and measured ClO may arise from chlorine activation being delayed in the model due to inaccuracies in the partitioning between chlorine species at high solar zenith angles.*

Printer-friendly version

Discussion paper



Interactive  
comment

[Printer-friendly version](#)

[Discussion paper](#)

