

Review of “Future Arctic . . .”

BY BEDNARTZ ET AL.

General

The paper addresses an important scientific question, namely projections of future ozone levels in the Arctic. Overall the paper is well written and the discussions and arguments are clear (see below for some exceptions). The study uses a well established and well described model (the UM-UKCA model). The results on the timing of future Arctic ozone recovery are very relevant for the readership of ACP and also for future scientific ozone assessments. A particular strength of the paper is the use of ensembles and the analysis of relevant chemical and dynamical processes with a focus on the case of the simulation for winter 2063.

One major question to the models projecting the future is how well they simulate present day chemical Arctic ozone loss. Of course, this is a prerequisite for the assessment of future recovery. And it is not obvious that state-of-the-art models do a good job in all respects at present day chemical Arctic ozone loss. For example, Brakebusch et al. (2013) find a systematic high bias in ozone in the model of 18% in the lowermost stratosphere in March. They attribute most of this ozone bias to too little heterogeneous processing of halogens late in the winter and suggest that the model underpredicts ClONO_2 early in the winter and has too little activated chlorine. How is the UM-UKCA model doing in this respect? How well does the model simulate denitrification (which is important for Arctic ozone loss)? Further, as far as I understand the UM-UKCA model uses an equilibrium NAT scheme, where NAT is formed at the NAT equilibrium temperature, which likely overestimates the onset of heterogeneous reactivity in the model. Are there earlier studies, where these points have been addressed?

In the model heterogeneous reactivity is driven by NAT and ice particles, i.e., what regards the Arctic largely by NAT. This is likely not realistic, as there are extensive observations of liquid particles in the polar regions (e.g., Pitts et al., 2013). Nonetheless, Keeble et al. (2014) obtain a reasonable simulation of the Antarctic ozone hole assuming heterogeneous reactions on NAT and ice using the model employed here. Similarly, Grooß et al. (2011) used a set-up with a NAT dominated heterogeneous chemistry (likely not realistic) but were able to reproduce the observed extremely low ozone values in the Antarctic. Possibly, it is not necessary to get every detail of PSC formation right to obtain a reasonable representation of chlorine activation and ozone loss in the model (see also Kirner et al., 2015; Solomon et al., 2015, and references therein). But I suggest that the issue of heterogeneous reactivity is discussed in more detail in the paper (see also detailed comments below).

Moreover, an important theme of the paper is halogen induced ozone loss due

to heterogeneous reactions and chlorine activation (and the relative role of dynamics). As sufficiently cold conditions develop almost exclusively in the polar vortex, halogen induced ozone loss is only expected to occur in the vortex. However the analysis in the paper is mostly based on geographical latitude thereby neglecting the distinction between inside and outside of the vortex (in Fig. 7a however, a vortex average is presented, see also comments below). For example, how different would Figure 1 look, if equivalent latitude would be used rather than geographic latitude? Further, in the discussions on the ozone anomaly simulated for the year 2063, the distinction between vortex processes and out of vortex processes is not always brought across clearly (see detailed comments below). From my reading of the discussion in the paper (top of page 14), in the model, a significant fraction of the 2063 anomaly is driven by chemistry *outside* of the vortex – is this correct? I suggest improving the discussion and carefully quantify the contributions of chemistry and dynamics inside and outside of the polar vortex to the simulated ozone anomaly in 2063.

In summary, I think with respect to several issues raised in this review, the paper needs to be revised and improved. Nonetheless, I believe that this is a potentially very good paper, which could make an important contribution to improved projections of future Arctic ozone levels and in particular regarding the various processes impacting polar ozone. The paper will also be very relevant to the upcoming new WMO ozone assessment.

Detailed comments

- p 1, l 15: This statement is confusing: to me it implies that present day spring Arctic ozone is 50-100 DU below the values expected after recovery in 2060. Is this what you want to say here? Is this true for your model simulations presented here?
- p 1, l 20: why does an increase in downwelling lead to less consistency?
- p 2, l 2: The use of CFCs did not lead to the ‘suggestion...’
- p. 2, l 4: One should distinguish the issue raised by Molina and Rowland (1974) (upper stratospheric ozone, globally) from the ozone hole issue pointed out by Farman et al. (1985).
- p 2., l 19: a citation from 1997 does not really allow to say ‘soon’ with reference to Farman et al. (1985).
- p 3, l 1: the impact is also on ecosystems not only on human populations.
- p 3, l 14: change ‘sulphate’ to ‘cold sulphate’
- p. 3, l 25: ‘controlling’ is perhaps to strong

- p 4, l 30: it might be worth pointing out that the mean age of the UM-UKCA model is in relatively good agreement with the observations in the high latitudes of the Northern hemisphere (according to Chipperfield et al., 2014), which is the most important region for this study.
- I am assuming that not only this reaction is not taken into account on liquid aerosol but all five reactions listed in on page 13707 of Keeble et al. (2014). While this is likely not realistic (there are extensive observations of liquid particles in the polar regions, e.g., Pitts et al., 2013) it should not affect the quality of the ozone loss simulations too much. Assuming that the details of heterogeneous reactivity are not essential for a good representation of polar ozone loss; see also discussion below.
- p. 5, l 10-12: I do not think it is correct to say that Keeble et al. (2014) showed that ozone depletion can be attributed to heterogeneous reactions on NAT and ice. They obtain a reasonable simulation of the Antarctic ozone hole making this assumption. In the real world, for a long time, the heterogeneous reactivity will be dominated by ice particles. On the other hand, neglecting ice particles (and indeed NAT) does not result in a substantial change of the simulated ozone loss (Kirner et al., 2015; Solomon et al., 2015). So I think the wording should be more careful here.
- p 5, l 29: chemical formulas should not be in italics
- Section 2.3: I would suggest some more discussion of the relevance of the cycles discussed here. Could you roughly quantify what is meant with “lesser importance”. It could be close to negligible for some of the cycles I think. On the other hand close to the tropopause in the non activated region natural (e.g. HO_x driven cycles might be important for ozone loss.
- Figure 1: how different would this figure look, if equivalent latitude would be used rather than geographic latitude. Would this not be the better choice? What is the reason for preferring geographic latitude vs. equivalent latitude?
- p 7, l 15: change ‘atmospheric’ to ‘stratospheric’ – Cl_y is not defined in the troposphere
- p 7, l. 20: I do not agree. The paper by Haigh and Pyle (1982) does not discuss the relevant ozone loss cycles in the polar regions, in particular the ClO-dimer cycle, which does not slow down with decreasing temperature. I think you are discussing polar ozone loss in the lower stratosphere here.
- p 7, l 31: If I understand correctly, this value is computed by determining the minimum ozone value poleward of 65°N each day and then computing the mean value over a month. Have you ensured that all these values are within the polar vortex? Or could some of these values stem from (dynamically caused) so-called mini-holes?

- p 8, l 5: What is the implication of this statement? This sentence could be interpreted as stating that under present day conditions routinely strongly depleted ozone values are found. Please clarify.
- P. 8, l 25-28: Difference to the results of Langematz et al. (2014); if the reason is ‘differences in the representation’, do you mean chemical or dynamical effects? If you agree with me that the difference is very likely not due to chemistry, you could state this point more clearly.
- p 9, l 6: This is a bit misleading – are there more, even less important halogen cycles? I suggest stating which of the six cycles are dominant, which play a minor role and which are negligible.
- p. 9, l 21: do you really mean ‘halogen losses’ here?
- p 9, l 26: this formulation is a bit awkward; I think you never applied the 11-year running mean rather than removing it.
- p 10, l 3: not only the amount of PSCs also the length of the cold period. This is also important (Manney et al., 2011). Further below you also make this point.
- p 10, l 18: change ‘insignificant’ to ‘not significant’
- p 10, l. 19: “Similar is true” – reformulate
- p 10, l 27-29: Actually, the ozone levels in Antarctica are also strongly influenced by the BD-circulation; it is just that the dynamical variability is lower – correct?
- p 11, line 1: provide a citation and/or explanation for \bar{w}^*
- p 11, l 15: ‘relatively’ to what?
- p. 11, l 21-22: A central issue here is also the continued presence of PSCs and thus the continued activation.
- p 11, l 32: will continue to occur in the future ...
- p 12, l 10: “long-term minimum of ensemble mean” – this is not quite clear? Which period is exactly considered? And what is meant is the lowest value for March mean ozone in the ensemble?
- p 12, l 16: This effect could be reduced by considering equivalent latitude.
- p 12, l 17-19: Why did you choose 850 K to define the vortex? This is above the altitude where most halogen induced ozone loss occurs. Also how is the PV value defined (citation?)?
- p 12, l 20: I suggest showing the vortex average data.

- p 13, l 9: this is an important point that should also be brought across clearly in the abstract.
- p 13, l 18: as stated before, heterogeneous reactivity in general should be more important than NAT formation in particular. Also, is there any formation of ice particles in the model for the year 2063?
- p 14, l 2-4: it is interesting to note that only part of the effect of the anomaly has its origin in processes in the polar vortex. Doesn't this mean that in the model only part of the chemistry driven effect is caused by halogen chemistry? Again I suggest to bring this message more clearly across the the abstract.
- p 14, l 9: here you state that the halogen effect is 40 DU but above you state that the polar vortex effect is only 25 DU. Does this mean that in the model, a significant fraction of the 2063 anomaly is driven by halogen chemistry *outside* of the vortex? Is there chlorine activation outside of the vortex in the model? This discussion at this stage and the attribution of ozone loss to processes needs to be improved.
- p 14, l 21: citation for 'other studies'
- p 15, l 31: it is not clear to me where the number of 20% is coming from. on p. 14, you report that the halogen induced loss in 2063 is twice that of 2060, which might be the first order information of interest here. However, to me the question is still open of how much the difference between 2060 and 2063 is a polar vortex effect and in how far is is influenced substantially by out of vortex processes.

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