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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Early unusual ozone loss during the Arctic winter 2002/2003 compare dto other winters" *by* F. Goutail et al.

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Review of Goutail et al.. "Early unusual ozone loss during the Arctic winter of 2002/2003 compared to other winters", manuscript ACPD-2004-si01004.

Overall, the paper presents a convincing case that chemical loss of Arctic ozone occurred earlier during the Arctic winter of 2002/03 than other winters, due to unusually cold conditions in early and mid-December 2002 compared to other winters. The study also shows overall excellent agreement between the observed ozone depletion and modeled depletion found using two different 3D CTMs (REPROBUS and SLIMCAT).

The paper will certainly be acceptable for publication in Atmospheric Chemistry and Physics Discussions after the following comments are addressed. Hopefully, these comments can be addressed by some improvements to the paper, rather than only

response (rebuttal) to this review.

Major Comments.

1. Observed and modeled ozone depletion are shown to be in good agreement, much discussion is given related to how the observed depletion was found, and a fair amount of discussion is given to description of the REPROBUS and SLIMCAT CTMs. Yet, the paper fails to describe critical details regarding the modeled ozone depletion.

For example, which rate constant was used for ClO+ClO+M ???

The description of REPROBUS refers to Sander et al. 2000, plus a host of additional references, but not the key Bloss et al.(J. Phys. Chem., 2001) paper that reported a faster rate constant for ClO+ClO+M.

The description of SLIMCAT gives no information about the rate constants.

I understand, from Martyn Chipperfield's presentation at Q.O.S., that SLIMCAT is now using the JPL 2002 rate constant for ClO+ClO+M (this is almost as fast as the Bloss et al. rate), plus is using the Burkholder et al. (J. Phys. Chem., 1990) cross section for ClOOCl. If this is the case, it should be stated in the paper, plus of course the Stimpfle et al. (JGR, 2004) paper that motivates the use of this combination of kinetics parameters should be described.

Also, polar ozone loss is quite sensitive to the abundance of bromine in the lower stratosphere. This quantity depends on assumptions of bromocarbon sources. For instance, the Schofield et al. JGR, 2004 paper presents SLIMCAT results for Bry_total = 21 ppt, based on an assumed ˜6 ppt contribution to Bry from VSL bromocarbons.

At a minimum, the following additional information is needed:

1a. description of the rate constant for ClO+ClO and cross section for ClOOCl used by each model for the 2002/03 simulation;

1b. description of how much Bry was present at 435, 475, and 550 K for each model,



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at a particular time of the 2002/03 run (these levels picked to correspond to data shown in Fig. 2; other levels would be OK).

If possible, it would be quite useful for many readers if a column for BrOx plots be added to Figure 6. It might make sense to break this figure up so that only 3 columns are shown per page, to allow each panel to appear larger than the present version, which is difficult to read.

Finally, it would also be useful, if possible, to show in the published paper a version of Figure 6 from SLIMCAT.

2. An oddity of the paper is that the observations are compared to modeled ozone loss using the version of the model available at the time of observation (e.g., page 5029, line 2). I suppose this is of some historical value. But, it does present problems in the interpretation of year-to-year differences in the model / measurement comparison.

Several items that should be addressed are:

2a. Clarify whether the SLIMCAT results shown in Figure 4 are for the version of the model available at the time of observation;

2b. Would be nice if somehow, the major changes to both models relevant to the comparison (e.g., photochemistry of ClOx; any changes in assumptions regarding levels of BrOx) were noted, perhaps in a table;

2c. Paragraph starting on line 14, page 5025, is confusing as written. First sentence says 1994/95 loss is not reproduced well. Third sentence says Goutail et al. (1999) found loss was "well captured". Very confusing which results are even shown in Figure 4; are they the results of Goutail et al. (1999) that according to the text compare well to observations? Clarification of this paragraph would be helpful.

3. I have some concern that the temporal evolution of ozone loss for a given winter might be influenced by the introduction of new stations, as they become illuminated. Since the stations rely on solar illumination, the early data period contains only



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southerly stations. As time progresses, data from more and more stations becomes available.

I suspect that the distribution of ozone loss within the vortex, combined with mixing, is such that this is not a major concern. However, it is not discussed at all in the paper. Might the "stabilization of ozone loss" in early February, discussed on page 5023, line 19, be due to this effect?

The final published paper would be improved if, to whatever degree possible, there was some discussion of possible bias in the temporal evolution of the observed ozone loss rate due to introduction of the northward stations over the course of winter.

4. The next comment follows point 3. For the ozone evolution over 11 winters shown in Figure 4, there are some stations always present, while data from a few appear when the station turns on, as indicated in Table 1. I suspect the introduction of these new stations has a minimal effect on the year-to-year comparisons, but this point is not addressed at all in the present paper.

The final published paper would be improved if, to whatever degree possible, there was some discussion of this point as well.

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Minor Points:

1. page 5021, line 20: what is meant by high? High altitude? or high density?

2. page 5023, line 3: need to say something like "relative percent reduction in ozone ..."

3. page 5023, lines 2 to 26: the tense of the verbs switches back and forth, from past to present. Would be better to use a single tense here, and throughout.

4. page 5023, line 9: "warming" should be "warmings"



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5. page 5024, line 20: "200" should be "2003". Also, I do not believe that OClO measurements for the winter of 2002/03 are discussed in the WMO 2003 (Orange Book) Report. Unless the authors can point to a specific page or figure, I think this material needs to be modified. Also, the Wagner et al. papers are missing form the reference list.

6. page 5025, line 20: some brief mention of what improvements to the models drove the changes would be useful (see major point #2 above).

7. page 5025, lines 23-25: better to state "the ozone reduction started"

8. page 5025, line 25: How about "The 10% loss of ozone by the end of December for the winter of 2002/03 is an extreme case" (e.g., it is the loss that is extreme).

9. page 5026, line 9: how about "REPROBUS capture loss at a rate ..."

10. page 5026, line 24: use of the adjective "abundant" might be confusing, since implies more ozone (e.g., "ozone is more abundant") rather than more loss. How about "more apparent"

11. page 5028, line 5: should "reactions rated" be "reaction rates"? This sentence is confusing.

12. page 5028, line 17: NAT should be defined first place it is used.

13. page 5041, Figure 4 caption: perhaps describe left hand side of figure first, which is more common.

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5019, 2004.