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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Halogens and the chemistry of the free troposphere" *by* D. J. Lary

D. J. Lary

Received and published: 5 November 2004

Thank you so much for your prompt response. I appreciate your time and effort.

Reviewer comment:

> '*Thanks for clarifying my questions. I did read the detailed description of the analysis procedure in your previous papers (otherwise I wouldn't have been able to understand the paper), however I think that at least a few paragraphs should be spent repeating the main points of the methods because otherwise it is impossible to assess the results which might appear to come "out of the blue" which is clearly not the case. In my opinion the reader should be able to get at least an idea of the methods from the current paper and should only have to go back to previous papers for the details but not the basic principle of the analysis. I don't think it's acceptable to refer to unreviewed webpages for the main parts of the methods description, this - certainly very powerful medium - should be used only for supplemental*

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material, not for "core" material.'

Reply: Thanks, you have convinced me! I was hoping to keep the paper short, but I do see your valid point. So I have now included a full description of the system so that the reader does not have to worry about looking up the other references. Thanks!

Reviewer comment:

'*Sources of Cl and Br: the referenced web pages only show results of CYCLING between the different species, I couldn't find a discussion of the actual sources. What was the model initialized with, short-lived organic halogens, CFC-breakdown products, seasalt, ...? Again: crucial explanations of the technique cannot be "outsourced" to web pages.'*

Reply: The assimilation technique has been cast in Lagrangian coordinates with each analysis window being one day long. So there is no implicit description of the general circulation. If all the NOy, ClOy, and BrOy species were being accurately observed this would not be a limitation. Since they are not, we need to get realistic fields of NOy, ClOy, and BrOy from somewhere. So we use the fields of total NOy, ClOy, and BrOy in our assimilation from a 81 year GSFC 2D model (Fleming et al. 1999). The 2D model transport captures much of the qualitative structure and seasonal variability observed in stratospheric long lived tracers, such as isolation of the tropics and the southern hemisphere winter polar vortex, the well-mixed surf-zone region of the winter subtropics and midlatitudes, and the latitudinal and seasonal variations of total ozone. The generally good model-measurement agreement of the 2D tracer simulations demonstrate that a successful formulation of zonal mean transport processes can be constructed from currently available atmospheric data sets (Fleming et al., 1999). This model run was used for international assessments of ozone depletion and is constrained by the recommended emission inventories of the various source gases. During the period 1992 to 2000 it is constrained with the observed residual circulation and gives realistic NOy,

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Interactive Comment

ClOy, and BrOy distributions.

Reviewer comment:

> '*Months other than those presented in the paper: apparently a lot more data is available than presented in the paper - I would have prefered to read about that in the paper and have comments on the variability in the paper and not on a webpage. In my opinion a paper is meant to summarize the work done and present conclusions that are valid for all the data analyzed, not simply to show the "tip of the iceberg" and then refer to webpages for the bulk of the data. Why didn't you include more general conclusions in the paper (ie not only refering to 2 months) - especially when you have the data to draw these conclusions? Please also include (if you haven't done already) the main points of your replies into the revised version, esp your explanations of p. 2338 and 2339. Again: all this doesn't diminish the overall value of the paper, which I think is definitely good work*'

Reply: Thanks. My examination of other months did not really add any understanding for me. The basic features are persistent. That is why for simplicity I chose just two months. The detailed response actually come from my investigation after submission of this paper, and are now part of a later paper currently in review so i am unsure that they should be included here for fear that I will be publishing the same material twice. I would appreciate the editors advice on this.

General Point: I have made the figures larger.

Thank you again for your prompt response. I appreciate your time and effort.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5367, 2004.