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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Recent trends in atmospheric methyl bromide: analysis of post-Montreal Protocol variability" *by* S. A. Yvon-Lewis and E. S. Saltzman

Anonymous Referee #1

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Recent trends in atmospheric methyl bromide: analysis of post-Montreal Protocol variability by Yvon-Lewis and Saltzman

General Comments

This is a very nice paper, which takes advantage of the decline in atmospheric concentrations of methyl bromide resulting from post-Montreal Protocol phase out to re-examine the atmospheric budget of this important trace gas. The study employs a model that has already been used for a similar type of study so the focus of this paper is the interpretation of the model results. The paper is well written and easy to follow,

although the figures are less clear (see specific comments below).

I was struck by the model derived seasonal cycle in the missing source, but more importantly concerned by the use of the lack of seasonality in the concentrations generated from the runs with a longer lifetime as a factor to support one of the main conclusions of this paper. I believe there is a flaw in the model scenarios with the longer lifetime that needs to be addressed.

When this is addressed, along with the clarity of the figures, then I would recommend publication.

Specific Comments

I was really interested by the seasonal cycle of the missing source as illustrated in Figure 3. It got me thinking "What source(s) would have a similar seasonal cycle in both hemispheres? i.e. both reduced in the boreal summer. What northern hemispheric source would not exist at all during the boreal summer? What southern hemispheric source would be weak (and not exist for 2 months) during the austral winter?". This led me to thinking about the impact of tropical sources which may come from a particular hemisphere, but be emitted in to the atmosphere of the other hemisphere if equatorward of the ITCZ and how this might effect the results of this simple 2-box atmospheric model.

I think it would be useful for the authors to discuss the implications of this model derived seasonality of the missing source as it can give important clues to the nature of this source. They should also further discuss the limitations of the model for tropical sources and thus on the derived-seasonality of both the missing source and the atmospheric concentrations. This is touched on at the bottom of page 6522, but I believe that this is an important point given that the lack of seasonality in the concentrations generated from the runs with a longer lifetime is a factor used to support one of the main conclusions of this paper.

In this respect, I find it very odd that the authors do not appear to have adjusted the seasonality of the missing source for the scenarios with the increased lifetime. I realise that for scenario 5 the whole of the missing source term is treated as agricultural emissions, but for scenarios 6-8, I would have thought it essential that the remaining missing source is tuned to give the observed 1995-8 seasonality. This seems to me to be a flaw in this analysis. If this is not done, you cannot conclude that the lack of seasonality in the concentrations generated from the runs with a longer lifetime is evidence that the estimate of a 0.7 year lifetime is correct. If anything the model overestimates the seasonal cycle of the concentrations in the southern hemisphere when a lifetime of 0.7 years is used.

A 2-box atmospheric model may struggle to reproduce the seasonal cycle of concentrations due to the simplification of the transport times. I think this paper should focus on the interannual variability which I believe this model can adequately deal with and for which the more recent observational data provides a new constraint.

The 3 factors for which the interannual variability is examined are the biomass burning and non-QPS anthropogenic emissions, and the OH concentration. Figures 1 and 2 show this variability for the two emission sources. It would be good to have an additional figure for the OH. I appreciate that some of the change in the OH sink (Gg/y) will be a function of the changes in atmospheric concentration and thus the particular run. However the authors could calculate the sink due to OH as a function of the observed OH and methyl bromide concentrations, or could at least provide a figure of the changing OH concentration to give an indication of the magnitude of its variability.

I actually found Figures 1 and 2 rather difficult to read as bar charts. Having the bars overlaid on top of each other means that many of the bars are hidden. Couldn't the same information be illustrated by using line graphs instead?

I appreciate that the scales on Figures 1 and 2 are the same to allow comparison. I would suggest the numbers of tick marks are also the same.

In Figure 3, I presume that the bars are stacked on top of each other. This makes it harder to see the seasonal variability in the SH missing source. Again a line graph might make this easier to visualise.

Figure 4 is far too small. I couldn't make out the different symbols although it was intuitive which was which. The caption is also confusing in terms of the make up of each run. E.g. "(a) biomass burning trend with no anthropogenic phaseout". It is not clear if the OH varies interannual in this run. (b) is more explicit. I would suggest that the runs are either described as in Table 2 or that you simply refer to them as scenario 1 to 8 as described in Table 2.

Figure 5 is again rather small – text is very hard to read. Once again I find the overlaid bars confusing as you cannot see all of them. Line graph?

It would be helpful if the caption or footnotes for Table 1 indicated what the column "1996 (60% Ag)" refers to. The text at the top of page 6526 states that the 2007 budget shown in Table 1 is from scenario 8. Until that point I had assumed that the column labelled 2007 was for scenario 4. If it does refer to scenario 8, then shouldn't it be labelled "2007 (60% Ag)"?

Note also that the footnotes marked by asterisks for Table 1 appear to be incorrectly labelled. I believe they should be: * Net ocean flux = -14.0 Gg/y ** Net ocean flux = -6.6 Gg/y *** Assumed to be a natural missing source

Technical Corrections

Page 6515, line 26 "of the annual budget"

Page 6521, line 20, "is a larger fraction"

Page 6525, line 22, "adjusted to match"

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6515, 2009.