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Atmospheric
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Discussions

Interactive comment on “What does the global mean OH concentration tell us?” *by* “M. G. Lawrence et al.”

M. G. Lawrence et al.

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We appreciate the comments and recommendations of both referees, and have taken most of them into account in revising the paper.

We thank Dr. Krol for the general compliments towards the study, and in particular for his suggestion that “the recommendations should be followed by modelers.” We are in agreement that a shorter paper might be preferable, since it would be more accessible to a larger audience. However, we also feel it is necessary to treat the topic with at least enough detail that a clear recommendation can be made and justified. In revising the manuscript we have attempted to shorten the discussion where possible; in particular, we have removed a few of the supporting arguments and recommendations where we deemed them to make only a marginal contribution to the overall paper. We have also decided to maintain the focus on CH_4 and CH_3CCl_3, rather than add additional detailed discussions of other gases (such as CO). Finally, we have provided

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a more detailed abstract, which should hopefully make the point clearly available to a wider audience.

Like the “global mean OH concentration,” the terms “lifetime” and “turnover time” are also not consistently defined in the atmospheric sciences community. Dr. Krol indicates that the lifetime can be defined as the total content divided by the loss rate. This is the typical definition used in texts such as the recent one by Brasseur and colleagues, and is the definition which we adhere to, as is made clear in our Equation 4 (3 in the submitted version). Note, however, that one could argue that this is really only an accurate definition for one-box systems undergoing exponential decay, and not for more complex systems.

With regards to the turnover time, Dr. Krol refers to Prather (1997), and indicates that the turnover time is related to the loss frequency only, and thus differs from the lifetime. Prather (1997) in turn refers to Bolin and Rodhe (1973), who in their seminal paper wrote that for a gas presumed to be in steady state, “the turnover time, τ_0, is usually expressed as the ratio of the total mass in the reservoir to the total flux.” Brasseur’s text gives a similar definition: “The turnover time associated with the reservoir is provided by the ratio of its burden at steady state to the sum of its sinks (or its sources).” In our case, where the sink (or flux) is due to reaction with OH, and CH_4 is close to being in steady-state, then the turnover time is indeed equivalent to the lifetime, which would justify interchanging the terms. It is clear that this is another inconsistency in definitions which needs to be dealt with by the atmospheric sciences community. For the sake of clarity in this paper, we have decided to exclusively use the term “lifetime,” with the exception of indicating once that this is equivalent to the definition of turnover time as given by Bolin and Rodhe (1973).

The temporal averaging was already mentioned in Section 4 in the submitted paper (not only in Section 8). We now include an estimate of the effect of using monthly means versus using output which resolves the diurnal cycle. Based on a 1-month run at a reduced resolution (T21), we computed a difference of 0.4%. This is so small that we find it highly unlikely that the effect would be significant at a higher resolution or

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over a longer period. A brief explanation of what we meant about the excessive storage requirements, and why we did not do such a longer, high-resolution run: assuming 8-byte words, a single 3D output field at the high resolution used in the paper (192x96x28 grid points) consumes over 4 MByte. One year's worth of [OH] and T output every half-hour would then be about 150 GByte. Also, since the three control runs which were used here each had a turnaround time of about a month, rerunning the computations with an online computation of $[OH]_{GM}$ is not really reasonable in this case. It would be interesting, though, to see if the result from our short test run is confirmed in other model settings.

We now mention that the modeled MCF emissions are the same as in Krol et al. (1998), and refer to Jöckel (2000) for further details, which is available via http://www.ub.uni-heidelberg.de/archiv/1426. The CH_3CCl_3 field used in these computations is for 1993. We indicate in the text that using this distribution gives a value of $[OH]_{GM}$(MCF) which is only slightly different (1-5%) than when a uniform distribution is assumed.

With regards to the first paragraph in the comment about our use of the term "oxidizing efficiency", we have tried to make this more clear; please see the response to referee 1.

The next paragraph in Dr. Krol's comment suggests that "the oxidizing capacity is more properly defined as the rate of OH production (in molecules/year)." We find that this may be an appropriate measure of the "capacity of the atmosphere to oxidize pollutants," but that it needs more discussion and consideration. This proposed definition treats all OH molecules alike, whether they are produced at the poles or equator, or near the surface or in the upper troposphere. However, for the practical consideration of the actual removal rate of various gases, it is very important to know where OH molecules are being produced, and not just how many. How is this dealt with? Furthermore, "the rate of OH production" is ill-defined; does this mean only primary production (mainly $O(^1D)+H_2O$), or does it include various kinds of secondary production (e.g., release from reservoirs such as CH_3OOH, or null cycles such as HO_2+O_3

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followed by $OH+O_3$)? It seems that both definitions would have drawbacks: the former ignores the capacity of some of the secondary OH to cause the loss of gases (e.g., CO does not care whether it reacts with a primary or secondary OH molecule); the latter overestimates the real oxidizing capacity by including the OH which is produced and then quickly lost again in null cycles. Once such issues are clarified, the definition proposed by Dr. Krol could potentially be a good one to adopt as a standard for the "oxidizing capacity." For the sake of this study, we will maintain our definition of the "oxidizing efficiency", which we interpret differently from the "oxidizing capacity", since our main interest is knowing how rapidly (or efficiently) various key trace gases will be removed from the atmosphere on average. It would be valuable if IGAC or another community-wide organization would discuss and adopt standard definitions for these terms.

We now include the suggested reference to Krol and van Weele (1997) and have reworded the text accordingly.

The role of the ITCZ location in the OH asymmetry is now mentioned with reference to Montzka et al. (2000).

We find the suggestion of dividing the atmosphere vertically at 750, 500, and 250 hPa so that the domains have (approximately) equal air mass to be excellent and have adopted this. Note that for models with terrain-following coordinates, the air masses in the lowermost boxes are not exactly equal, but they are within 10%, as now mentioned in the text.

We have modified the comment about the differences in the trends to reflect the comment.

"1,1,1 tricholoroethane" is now stated.

"Sect." (also elsewhere) came from the EGS typesetting and has been changed back to Section.

References:

Bolin, B. and Rodhe, H., A note on the concepts of age distribution and transit time in natural reservoirs, *Tellus*, *25*, 58–62, 1973.

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