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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A discussion on the determination of atmospheric OH and its trends" *by* P. Jöckel et al.

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Cumulative reply to all comments

We thank all the contributors to the discussion for their truely constructive comments. In the following, we answer their main points:

1. The temperature dependence of the rate coefficient: We completely agree that the temperature dependence of the rate coefficient for the reaction with OH represents an additional weighting in calculating $< k(T)(OH) >$. On page 3 in our manuscript, we indicated that the $< k(OH) >$ of different tracers can only be scaled, if the temperature dependencies of the rate coefficients are similar. To make this issue even clearer, we put a special note in the revised version (see new section "Discussion"). Further systematic modeling exercises focusing on the sensitivity of the tracer information to the

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temperature dependence of the rate coefficient are clearly required, but are going far beyond the scope of this paper. Such sensitivity studies must also include a sensitivity analysis of independently varying OH and temperature distributions.

2. The average lifetime of the tracer: In our studies we have arbitrarily chosen a tracer with the approximate lifetime of 5 years. Obviously, the results are dependent on the tracer's reactivity and therefore on its average lifetime. Short lived tracers exhibit a high variability and a high sensitivity to local and temporal properties of the OH distribution. In contrast, long lived tracers spread out around the globe, show smaller spatial gradients, exhibit less variability, and are less sensitive to short term changes in OH. The choice of the approximate tracer lifetime depends on the characteristics of the OH distribution that should be probed. For instance, the detection of long term trends ($>$ 1 year) of the global OH distribution, requires a tracer lifetime which is longer than the longest (tropospheric) mixing time. In the revised version of our paper we show the relation between tracer lifetime, tracer variability, and the information carried by the tracer (see new section "Discussion").

3. Alternative sinks (ocean uptake, photolysis, etc.): We are aware that the possibility of additional sinks must be considered for any potential tracer in a real experiment. The intention behind the present study, however, is to show some of the fundamental problems which already occur in an 'ideal' world with OH as the only sink. Nevertheless, we put a note on this in our revised paper (see new section "Discussion").

4. The use of two tracers with different lifetimes: The idea behind using two tracers with different reaction coefficients (but same temperature dependence), is the cancellation of the 'dynamical variability' in the time series, if the ratio of these two tracer mixing ratios is observed. Also a higher accuracy can be obtained in comparison to absolute tracer concentration measurements. However, our model results showed that this 'cancellation of the dynamical variability' does not occur or is convoluted by other effects. In an additional appendix (C) in the revised version of the paper, we analyze this counter-intuitive result.

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5. The use of more sophisticated inversion techniques: We completely agree with Referee # 2 that "*there are a myriad modeling studies that can be performed*", especially those involving sophisticated inversion techniques. The inversion of the incomplete information (inherent in tracer time series at a limited number of sites), by including additional probably incomplete, arbitrary, and/or uncertain information necessarily introduces new uncertainties and systematic errors. The robustness and self-consistency of inversion methods have to be thoroughly checked, before they can be applied to a real tracer experiment. Especially, the question to what extent the inversion results depend on the choice of additional information (i.e., the applied atmospheric model, the applied meteorological data, etc.) can only be answered by systematic model and inversion method inter-comparison studies. This is exactly the key point of our proposal. (One should, for instance, not forget that also the reanalyzed meteorological fields are to a large extent model results, and that e.g., the NCEP and ECMWF fields differ quite a lot.)

The CAGE suggested by M. Krol focuses on this issue and is a good starting point which should be considered in the IGAC initiative. For the present paper, we want to stress that we intentionally avoided the application of sophisticated inversion methods, in order to highlight the most fundamental problems. We performed the first step by applying the simplest case, namely using only the information which is available in the (pseudo-)observations and showed how far one can get with this. Furthermore, we want to point out that for an estimation of the influences of the processes listed by M. Krol, inversion techniques are not necessarily required. The sensitivity of the tracer distributions to model parameterizations, the choice of boundary conditions, etc., can also be performed in 'forward' mode. In summary, we consider the 'inversion issue' (inversion of incomplete information) as one further important aspect of the main problem (determining global OH), which should be examined systematically in future. A detailed discussion of this topic, however, is far beyond the scope of the present paper (in the revised version, we briefly comment on this in the new section "Discussion") and we thank especially M. Krol for his contribution.

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6. Detection of trends: We also performed experiments with varying global OH abundance (e.g., linear trend, step function) in order to examine if these changes are detectable from the tracer pseudo-observations. The result was rather trivial: It was possible to detect the changes, by comparing the model simulation with change in OH with that without change in OH. In reality, however, the respective time series with 'unchanged' OH during the same time interval does not exist. Comparison of tracer observations from different time intervals can yield relevant results. However, differences can be caused by both, different meteorology and chemistry. To distinguish between both contributions is difficult. Detection of trends smaller than those caused by the 'meteorological noise' is therefore not possible, whereby the extent of 'meteorological noise' depends on the approximate tracer lifetime.

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