

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

**Anonymous Referee #1**

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### **General comments**

This paper, while not introducing dramatically new scientific results, does provide a useful review of the concept of oxidising capacity of the troposphere. In particular it points out a need for more care by modellers when asserting that their “globally averaged OH is in agreement with other models”. The use of different but realistic OH distributions to illustrate the authors’ points gives useful weight to their arguments.

### **Specific comments**

The paper is slightly unclear in the all-important definition of the oxidising efficiency of the atmosphere. The authors initially use the uncontroversial definition of the removal rate of a gas. They then implicitly assume that this gas is either methane or methylchloroform with little discussion of why these gases were chosen. There are ob-

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vious advantages in these gases. There are some disadvantages too, particularly the temperature dependence of the OH rate coefficients. This temperature dependence seems to be regarded as an advantage in the paper since it emphasises the tropics, but it might overemphasise the tropics when considering the oxidation of other compounds, such as those mentioned by the authors in the first paragraph of section 1 (CO, NMHCs, SO<sub>2</sub>, NO<sub>2</sub>), that have lower or no temperature dependence of the OH rate coefficients and have their highest concentrations in the northern mid-latitudes.

To avoid confusion the authors should make it clear that when they refer to oxidising efficiency they often mean the efficiency to oxidise specifically methane or methylchloroform. For instance the phrase in the middle of the last paragraph of section 4 “Based on Figure 2b we conclude that  $[\text{OH}]_{GM}(\text{M})$  and  $[\text{OH}]_{GM}(\text{V})$  are not very good indicators of the atmospheric oxidizing efficiency” has not been demonstrated to be necessarily true for species other than the two tested. Similarly the last statement of section 6 needs to be qualified to the effect that it might only apply to methane and methylchloroform.

Last paragraph of section 2: The Collins *et al.* reference shows only OH mixing ratios increasing with height. No indication of the vertical variation of OH molecular densities is provided in this paper.

To summarise: This paper addresses a serious point and I hope it will influence the reporting of global [OH] values in future modelling studies. I suggest that the authors need to mention the oxidising efficiency of other species (CO is the largest sink of OH in the troposphere). They should discuss whether their recommendation that  $[\text{OH}]_{GM}(\text{CH}_4)$  is a better indicator of model oxidising efficiency than  $[\text{OH}]_{GM}(\text{M})$  is applicable generally to this wider range of species.

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Interactive comment on Atmos. Chem. Phys. Discuss., 1, 43, 2001.

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