Atmos. Chem. Phys. Discuss., 12, C8861–C8867, 2012
www.atmos-chem-phys-discuss.net/12/C8861/2012/


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Analysis of present day and future OH and methane lifetime in the ACCMIP simulations" *by* A. Voulgarakis et al.

**Anonymous Referee #2**

Received and published: 31 October 2012

This paper covers some very interesting ground examining OH and methane liftetime variations in the various RCPs, present day variability in simulated methane lifetimes and preindustrial to present methane trends. It summarizes the results of 14 models, discusses various sensitivity studies and analyzes the reasons for differences between the models.

It is clear that this is a potentially important paper and should be valuable in informing the current IPCC. This type of paper represents an enormous amount of work involved in collating the individual results from many different models as well as running the simulations in the 1st place. I realize the authors were under a timeline to submit this paper and I commend them for getting it in on time. The paper should be published but it needs a considerable amount of work both in its presentation and the analysis. It



does not read as a finished product, as I'm sure the authors are aware.

Included here are some suggestions of how the paper could be improved.

Major Comments:

1. The organization of the paper does not seem quite right to me. In particular section 3.2 and 3.3 show the surface and regional changes in OH and speculate about the reasons for these changes. This seems premature in terms of the paper organization. The factors driving OH abundances are given later. I would suggest including these sections after the paper discusses the factors driving OH changes or simply show the changes but do not discuss what is causing them until later.

2. Much of the paper includes speculative comments. In some cases these were presented as fact (I give some examples below). I found section 6 to be probably the strongest in the paper as it really tried to quantify the causes in model differences. While I do not disagree with the author's interpretations, in many cases they came across as speculative. However, it is rather annoying when a paper is constantly saying "It could be due to this ....". .. "It could be due to that...." without backing up the statements. I think the paper could be improved and better structured if you have a set number of things you want to show and then conclusively show them. It is also important to distinguish between conclusions that are clearly shown and conclusions that are speculative. Maybe put the speculative comments in a discussion section.

3. I believe the paper can be significantly tightened up. At present it is rather repetitive and somewhat rambling in places.

4. While I realize it is somewhat controversial I think the authors need to exercise some discretion in what models they include in certain analysis. For example: 1) Some models did not include future changes in meteorology. As the paper shows these changes are clearly important when interpreting changings in OH. 2) Some models used their own CH4 emissions versus specifying CH4 in the lower layer. This results

in a rather different methane burden, at least in the GISS model. 3) JO1D in UM-CAM is dramatically different than in the models (less by a factor of ~2). This results in much different behavior. While it may be instructive to show these outlier models in the various plots and interpret their results relative to the other models this needs to be done judiciously. In the various plots the points for these models should be clearly defined as being anomalous in some way and should not be included in finding the model spread or in determining the mean model behavior. They should not be included in finding the mean model trend. They are, however, very useful in diagnosing the model behavior.

Minor Comments.

1. p22948, l27: “related underlying processes” is rather imprecise and somewhat awkward here.

2. Table of model components (A1). This table should be cleaned up. The two models with methane emissions should clearly reference how their emissions are diagnosed. For lightning emissions there are references to Price and Rind 1992, 1993, 1994 and as well as Price et al. 1997. Are all these schemes really different or is it just the references? Also I assume each scheme has been tuned to a global lightning value in a particular year – the lightning emissions for a particular timeslice should be included for each model (also discussion on p22953).

3. Since this paper is about methane, additional information should be given about the specified methane concentrations for the various RCPs and models much earlier in the paper At least this information should be presented prior to the point where the authors interpret the impact of the different methane burdens on OH. The methane burden in models with interactive emissions should be starred. Also please comment on the fact that methane emissions are specified on the lower boundary. How will this impact the simulations of methane burdens and the relationship between CH4 and OH?

4. Figure 2. There are no mean trend lines in some of the panels.



5. p22952, l8: “more or less linked”. Do you mean that these models used SSTs and sea ice from the appropriate RCP? Also for historical runs did models use observed SSTs or model generated SSTs? Please be more explicit here.

6. p22953, l7 “timeslices” are not yet defined.

7. p22953, l8 in what sense are NMVOCs nearly identical? Do the models have similar oxidation mechanisms? And is the difference in the NMVOCs discussed later in the paper due primarily to due to biogenic emissions?

8. p22953, l15, Are you implying that the Oslo model has no interannual variation in lightning emissions.

9. p22953, l21: What do you mean by significant underestimates and overestimates?

10. p22954, l26 The results of experiments prior to 2000 are also presented. In addition the models are not run from 2000-2100 as implied but for various timeslices. Also, please state explicitly whether the SST is held constant interannually.

11. p22956, l3. Discussing the range in the spread of methane lifetimes to show the state of knowledge makes little sense to me. This spread is probably not a robust statement of the error, but is determined by outliers. You should simply give the standard deviation after removing model’s with different simulation protocols or obvious outliers.

12. p22956, l25: “most of the ACCMIP”.

13. p22956, l24: is the estimate chemical lifetime derived from data comparable with the lifetime derived from the models. Is the same tropopause used, for example?

14. Last paragraph beginning on p22956. Many terms used here are not well quantified. Please quantify “most”. A little further down quantify “minimal”.

15. P22957, l9: “smallest mean global methane lifetime levels for 2100 relative to 2000.” – do you mean change?

16. Sections on surface changes and regional changes. Many aspects of these changes are ascribed definitely as due to a particular cause: increase in methane, changes in surface NOx emissions etc. I see no reason to doubt these ascriptions, but they have not been conclusively shown. The authors need to modify their language in ascribing these changes to a particular cause, or provide more evidence for what they are saying.

17. p22956: N/S OH ratio. This seems like a nice diagnostic but please explain its significance. Why is this shown? Maybe this figure is simply out of place. It seems like it could be used to support the importance of changes in CH4 and NOx, at least locally.

18. p22957, l3: “equivalent of RCP 2.6”. I assume you mean CAM 3.5 uses a climate forcing no really consistent with RCP 2.6, and not different emissions. In spite of this difference should CAM 3.5 be included in the RCP 2.6 statistics?

19. P22958, l7:“Furthermore, an equatorward redistribution of anthropogenic emissions in the future may partly shift OH production away from northern midlatitudes, reducing the fraction of global OH that exists in the Northern Hemisphere” - this does not seem to be consistent with the increasing ratio shown in the figure.

20. p22959, l12 “the dominant factor driving OH changes is methane”. Have you shown this, or is it a supposition? Please rephrase.

21. p22959, 1st and 2nd paragraphs. These paragraphs could be written more coherently. At present they seem somewhat repetitive and not very concise.

22. p22959-22960 “occurring due to NOx emission decreases” – have you shown this?

23. p22962, l3: “This trend could have been different had the vertical level at which the convective updraft mass flux was taken evolved with time.” Yes, but do you have any particular evidence this is the case? It is also possible the strength of convective mass fluxes will decrease in a future climate.

24. p22963, l3: Here you ascribe the changes in the methane burden to the fact that



methane in the GISS model responds more fully to OH. This change in burden could also be due to high methane emissions.

25. p22964, l4: ....despite the fact that its relative changes across timeslices are smaller compared to other drivers (e.g. emissions). This clause makes little sense to me. Water vapor increases exponentially with temperature.

26. p22964,l11 “and the inverse”. This clause is really unnecessary.

27. p22964l23-end: Please give mean and standard deviation of ozone changes for different scenarios.

28. p22964, l27: Have you shown stratospheric influx of ozone has increased in these simulations?

29. p22965, l15: It seems surprising to me that an ozone hole that extends SLIGHTLY too far equatorward and the persistence of the hole somewhat too long will make impact the difference of J1D between present and RCP8.5

30. Section 5.1: A number of points are made here that need to be better quantified: “In RCP 8.5, it is likely that methane changes are a major driver of OH and methane lifetime changes.”, “the relatively small increases cannot be the main driver of the sizeable OH and methane changes” etc. Plots of OH change versus NOx emissions, CH4 lower boundary conditions and NMVOC emissions for the various scenarios would help quantify these points.

31. p22967: A table of the different sensitivity experiments would be helpful.

32. p22968, l2-3: “It is methane abundance itself that actually drives the largest...” How about “We conclude it is....”. I'm not sure this point has been conclusively shown.

33. p22969, l14:please define “response time”

34. Table 5: Most of the results in this table are insignificant. Why show a table of insignificant results? In addition, this table needs to be carefully proofread. The table

suggests that response to changes in JO1D is not significant. Some of the significant values in the table are not highlighted in bold.

35. p22970, l23-24 – more significant than. . .Is this a robust statistical result?

36. Figure 10. Much of the variation in OH with VOC emissions and J(O1D) appears to be due to outliers. In as far as these points do not truly represent the model variability the outliers should be excluded from the statistical analysis. If one excludes these few models does on still get a statistically robust relationship?

37. p22972, l1-6 The paper concludes that the importance of the CO burden is because of the secondary CO produced. How do the authors know this? It could also be due to the fact that the CH4 and CO sink is both primarily due to OH oxidation.

38. Figure 11. The temperature and humidity response are clearly connected through water vapor. Why show both?

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22945, 2012.