Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1208-RC2, 2020

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Influences of hydroxyl radicals (OH) on top-down estimates of the global and regional methane budgets" *by* Yuanhong Zhao et al.

Anonymous Referee #2

Received and published: 8 April 2020

1 Overview:

Review of "*Influences of hydroxyl radicals (OH) on top-down estimates of the global and regional methane budgets*" by Zhao *et al.*

This review slipped through the cracks as the COVID-19 situation evolved here. My sincere apologies for any hold ups.

Zhao *et al.* present an analysis of a set of methane inversions using a set of 10 different OH fields from the CCMI experiment. They find the magnitude of methane emissions differs by roughly 30% (518-757 Tg/yr) depending on what OH fields they use. Over-

all, the study is useful in quantifying some of the uncertainties in methane emission estimates due to uncertain OH concentrations. The main shortcoming is the lack of discussion of what actually causes some of the differences (or really any discussion of OH). The figures are high quality but the text is very hard to follow because it's filled with many acronyms and parenthetical expressions. I would recommend major revisions.

2 Comments:

2.1 What processes actually drive these differences?

The main issue I feel is totally missing from the manuscript is any discussion of what processes are actually driving some of these differences. I believe there was only a single paragraph (Lines 70-79) even mentioning anything about what might affect OH. For example, do some of the CCMI models or inversions show consistent patterns with known climate oscillations? This was surprising given that this is a paper focused on how OH impacts methane. The obvious question is what leads to these differences/similarities in OH. The authors seem to treat the CCMI models as a black box which makes it hard to gain any understanding of what's happening. Given this, the only major take-away I had from the paper is that *"OH can lead to big differences in methane estimates"*, but this was already demonstrated by the box modeling papers (and others) that Zhao et al. are highly critical of. For example, the Rigby paper had error bars on their OH fields that bounded zero and the Turner paper had a case where OH didn't change. Both of these led to radically different methane emissions. Back to my point, I would find this manuscript much more useful and compelling if the authors actually highlighted processes and phenomena that lead to similar methane inversion responses. From Holmes et al., ACP (2013; https://doi.org/10.5194/acp-13-285-2013) we know some of the major processes that influence OH and Turner et al., PNAS (2018; https://doi.org/10.1073/pnas.1807532115) showed how this can co-vary with things like ENSO, do the authors see ENSO signals in the methane inversions? A recent paper from Nguyen et al., GRL (2020) tried to look at these feedbacks in a simple model. The authors should at least touch on the processes that influence OH, particularly those that could also influence methane.

2.2 Oversight of previous work and faith in the CCMI models

The authors seem to have quite a bit of faith in the CCMI models, more than this reviewer finds to be justified. There are quite a few known shortcomings of the models. For example, the models don't even get the ratio of the N/S gradient in OH correct. Yet the authors are quick to criticize MCF-constrained [OH] fields with seemingly no validation of their own OH fields (e.g., Lines 595-600). Is their analysis consistent with MCF? The authors seem to be arguing that these model-derived forward simulations of OH are more reliable than reconstructions.

The strongest claims made in this paper seem to be those that are critical of previous work estimating OH (e.g., Rigby and Turner). For example, Lines 595-600, the abstract is dismissive of box modeling results: 'previous research mostly relied on box modeling inversions with a very simplified atmospheric transport". The latter line in the abstract isn't even correct as there has been quite a bit of non-box model work the authors seem to discount or miss: McNorton et al., ACP (2016; https://doi.org/10.5194/acp-16-7943-2016), Gaubert et al., GRL (2017; https://doi.org/10.1002/2017GL074987), Rigby et al., PNAS (2017; https://doi.org/10.1073/pnas.1616426114), Turner et al., PNAS (2017; https://doi.org/10.1073/pnas.1616020114), McNorton et al., ACP (2018; https://doi.org/10.5194/acp-18-18149-2018), Maasakkers et al., ACP (2019; https://doi.org/10.5194/acp-19-7859-2019), Naus et al., ACP (2018; https://doi.org/10.5194/acp-19-407-2019), Nguyen et al., (2020; https://doi.org/10.1029/2019GL085706), and He et al., ACP (2020; https://doi.org/10.5194/acp-20-805-2020). About half of these papers use 3-D

atmospheric transport models and some even include fully-coupled chemistry (e.g., He et al., 2020), which is more comprehensive than the models used by the authors. The authors should do a more complete reading of the literature as they don't cite Holmes et al., ACP (2013), Murray et al., ACP (2014), or any of Michael Prather's papers.

2.3 Very difficult to follow

The paper is filled with jargon and abbreviations. For example, nearly half of the text in Lines 440-452 are acronyms or parenthetical expressions interjecting things. This was very hard to follow as a reader.

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