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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Implications of model bias in carbon monoxide for methane lifetime" *by* S. A. Strode et al.

Anonymous Referee #1

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This paper analyses CO simulations by a chemistry climate model in order to better understand possible reasons for reported low-biases in CO, especially on the NH. To this end, a rather complicated set-up in presented in which a kind of ad-hoc inverse modeling approach is combined with ad-hoc data-assimilation. Nevertheless, some nice insight is gained in the possible causes for model-data mismatches. I have two major comments, which I outline below. I think the authors should address these issues carefully.

1. Major comments:

Structure of the paper

The structure is rather messy. I think the introduction, method, and results should be

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better separated to improve readability. For instance, section 2.2.4 clearly presents results, but is presented in the method section. Section 3.4 presents a section that starts with "The primary source of OH" that I would classify as "Introduction". Section 3.3 suddenly ends with something about TCO, which seems more appropriate in section 3.4.1. The method description now centers around several "model options", which makes it hard to detect the strategy behind the paper. Also, the forward referencing to the sensitivity simulations (page 20311: "We conduct a sensitivity simulation, described in section 3.2") makes the paper very sketchy. I would rather prefer to start with table 1 and slowly describe all the simulations in sequential order. The problem is, I guess, that the sensitivity simulations follow from the first results. I that respect, it would also be an option to follow that line and first present and discuss the reference simulations, motivate the sensitivity simulations, and then present those. Anyhow, the authors should improve the readability by severely reshuffling the paper.

Unclear choice of presenting the results

One might question the rather ad-hoc way in which the sensitivity simulations are set up. As the authors know, there are more formal "inverse modeling" methods to optimize emissions and OH to improve the match between model and observations that are based on the minimization of a cost function. I agree that the selected ad-hoc inversion is an attractive alternative. However, what I find disturbing is the poor definition of the "cost function". Figure 3 (section 3.2.1) focuses on the latitudinal GMD CO observations between March and August, and for spring and summer individually. Why is the rest of the season left out? Figure 4 goes more quantitative, but now uses March through May (?). Figure 5 focuses on June through August, but again the rest of the year is left out from the analysis. It remains also unclear when the emission and OH changes are applied. Is this during the whole simulation (including Jan-March) or only in the months under investigation? This latter question is linked to section 3.2.3 in which emissions are changed in each season (table 3 (and not 2)). This analysis now focuses on April 2007 (why?), and shows strange results in table 3. The zero incre-

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ments in Sep-Dec are intriguing, as are the exact same increments that are required for Asian / EU emissions and Russian BB. These results look rather suspect to me. It becomes even more complicated when MOPITT results are further considered (section 3.2.4) when it turns out that some selected adjustments are incompatible with MOPITT (again focusing on April 2007 only). In summary, I can live with the fact that no formal inverse method is applied, but the lack of a clear cost function, target period, and optimization strategy makes the results non-robust and confusing. The authors therefore should work out a clear strategy and clear goals (is the focus on spring and summer biases only?). Having said this, I think the results that I see are quite interesting and provide a valuable contribution, once the strategy is better worked out.

2. Minor issues:

The title does not seem to cover the contents of the paper. The paper is only partly about the methane lifetime. Maybe: “Analysis of the carbon monoxide bias in chemistry climate models”.

Page 20306, line 16: CO only contributes to O3 formation at sufficiently high NOx concentrations. Please add the role of NOx.

Page 20307, line 25: “consistency of CO emission estimates with surface and satellite observations”: this now sounds as if emission estimates are measured at the surface and by satellites. Please make clear that there is a model in between.

Page 20311, line 17: What are these co-emitted NMHCs? It sounds rather misty that on page 20312, line 15 these co-emitted emissions are reduced. Is this a kind of fudge factor? Are these anthropogenic NMHCs or natural? Please clarify.

Page 20313, line 25: “during” NH spring.

Page 20316, line 28: table 3.

Page 20317, line 1: and b shows → and b show

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Page 20318, section 3.2.5: I find these results interesting. Has a reason been identified for the different transport? Which transport patterns have been identified as the best? Presumably vertical transport differs in MERRA, and this is an assimilation product, which is consistent with observations, hence better? Could this important observation be mentioned more clearly, e.g. in the abstract?

Page 20320, line 22: The increased OH → The decreased OH Page 20323, line 11 (and section 3.4.4 in general): Are there observational indications (e.g. from satellite data) that NOx emissions are too high (a known bias)? Also, reducing the NOx concentrations by 30% will affect not only the OH recycling (HO2 + NO), but also the O3 production. Do I understand correctly that these effects are ignored here, because O3 columns are also prescribed in the RefCO-OH simulations? Please mention this clearly. Do you proportionally adjust NO and NO2?

Page 20325, line 14: Linked to the above: "reducing the uncertainties in NOx emissions": this would call for a similar approach using e.g. NO2 column observations from OMI/GOMe-2, like Miyasaki, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20305, 2015.