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Geoscientific Model Development Discussions

Interactive comment on "Simulation of atmospheric N_2O with GEOS-Chem and its adjoint: evaluation of observational constraints" *by* K. C. Wells et al.

Anonymous Referee #2

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Wells et al. provide a timely and relevant analysis of atmospheric N2O sources and sinks using a global inversion system with the GEOS-Chem CTM and its adjoint. The paper is well written throughout with clear explanations of the technical details and their findings. The manuscript is appropriate for Geoscientific Model Development and I recommend that it is published subject to addressing the following comments.

P. 5370, lines 2-4: the sentence about averaging kernels seems a bit technical and out of place in the abstract, I think it would be beneficial for the reader to change this description to say something about the inversion sensitivity to the emissions. lines 12-15: I think the first two sentences would read better if merged into a single sentence.

P. 5371, line28: suggest changing "scale on which" to "scale over which".

P. 5372, line 25: it would be useful to have a brief description of CARIBIC, including a definition of the acronym, on the previous page as is done for HIPPO. Would it also be useful to reference any previous studies of the CARIBIC N2O?

P. 5373, line 11: suggest moving GEOS-5 to go after the definition. line 12: clarify longitude and latitude for horizontal resolution? line 16: suggest using "anthropogenic" rather than "anthropogenic sources".

P. 5374, line 9: define what MERRA means. Section 4: it might be useful to include a short paragraph on satellite observations of N2O either at the end of this section or in the introduction, especially as this is very briefly touched on at the end of the summary section.

P. 5379, line 7: suggest changing "based on" to "using".

P. 5381, lines 1-7: it would be useful if the authors could comment on the sensitivity of the inversion to the vertical profile of the measurements here – it looks to me as though the HIPPO observations provide a stronger constraint because they extend throughout the troposphere to the surface, therefore, do the authors have any sense as to the altitude range at which the constraint breaks down? This would be especially useful in the context of Section 5.5 and would maybe make a strong statement on the importance of in situ profiles from aircraft as part of the global observing system. line 23: should "sources as well as sinks" be "sinks as well as sources"? the previous subsection has dealt with sources and this one talks more about the sink.

P. 5382, line 7: stating that the second year is the final year seems a bit unnecessary, also the statement that "the inversion does not significantly affect the observations" seems to be the wrong way around to me – isn't it the impact of the observations on the inversion which is being assessed?

P 5383, line 1: it's hard to tell from the Figure that there is any significant change in the

a posteriori compared to the a priori. lines 24-26: it would be useful if the authors could comment here on any vertical correlations that might help to address this, or which limit the impact of aircraft observations measured at cruise altitude.

P. 5385, lines 1-2: it might be useful if the authors could comment briefly on how the box model results relate to transport across the tropopause on different timescales (e.g. tropical vertical mixing vs. isentropic mixing in the extra-tropics).

P. 5386, line 2 and P. 5387, line 2: please check the consistent use or a priori or prior and a posteriori and posterior.

P. 5391, line 25: clarify that the surface observations are both in situ and flask measurements.

P. 5392, line 19: is this statement on model transport specific to GEOS-5? It might be useful to add a comment on model transport issues based on other studies using relatively long-lived constituents (e.g. CO2 or CO).

P. 5393, lines 5-15: I think that it would be of benefit to the reader if the authors could rephrase the description of the averaging kernels to what it means in terms of the sensitivity of the inversion to the emissions (similar to my comment for the abstract) – by all means this should then reference the averaging kernel values but would be easier to understand the broader significance. line 13: is it an underconstraint or no constraint in the tropics? lines 21-23: could this also be linked to requirements for similar targeted measurements for other atmospheric constituents, and greenhouse gases in particular? How do the findings here compare to similar studies for CO2 and CH4?

P. 5394, line 1: it would be useful to have a brief sentence on satellite observations in the introduction.

P. 5409: clarify that HIPPO and CARIBIC are aircraft measurements.

Figures 6 and 7 would benefit from some clearer labelling of the plots and reference in

the captions.

Interactive comment on Geosci. Model Dev. Discuss., 8, 5367, 2015.