

Interactive comment on “Sources of carbon monoxide and formaldehyde in North America determined from high-resolution atmospheric data” by S. M. Miller et al.

Anonymous Referee #2

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This paper uses a Lagrangian transport model to analyze the carbon monoxide budget over North America and evaluates current emission estimates inverting simulations for CO and formaldehyde. The authors find that current emission inventories overestimate CO emissions from fossil fuel combustion and show the strong contribution of methane and VOC oxidation to CO levels in summertime.

The study is carried out in great detail and described in a clear and concise manner, including a complete description of the model itself and the inversion algorithm.

I believe this study makes a valuable contribution to the current literature both in regard to the technical and scientific aspects of the paper and I recommend publications once

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the authors have clarified the points specified below.

Specific Comments: Page 11369, Line 20: Is CO no longer a criterion pollutant?

Page 11397, Line 10: What does the latest IPCC report state about this issue?

Page 11397, Line 16-18: I assume this refers to the global scale?

Page 11398, Line 4: As before, has this estimate changed in the latest IPCC report?

Page 11398, Line 15: What about uncertainties in emission factors?

Page 11398, Line 25: "build up to very high levels" is somewhat awkward, as it implies there are no high CO levels. Please re-phrase.

Page 11399, Line 12: I suggest "The present paper describes"

Page 11403, Line 7: "with with"

Page 11403, Line 15: Is this mass violation purely an issue of the driving winds or a model issue?

Page 11404, Line 13: What are the uncertainties you expect by neglecting HCHO deposition? And do you consider deposition of CO?

Page 11404, Line 17: EPA has a newer emission inventory, do you know how this compares?

Page 11407, Line 15: Would you say this is the main advantage over CTMs?

Page 11409, Line 11: Do I understand correctly that you only consider an uncertainty of 30% for the MEGAN emissions, but do not consider uncertainties related to the simplified chemistry and coarser resolution of some of the input fields implemented in your VOC simulation? I think you would allow for a clearly larger uncertainty or can you justify the value you used?

Page 11415, Line 22: explain "95% CI"

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Page 11416: I found the three paragraphs after line 8 confusing to follow. In lines 10ff you state that WLEF finds higher scaling factors for spring, in line 5 you say that the inversion results do not reflect an overall increase in emissions from winter to summer. Do you mean that a decrease in fossil fuel emissions from winter to spring was offset by the higher fire emissions in summer?

Page 11416, Line 12; I think you want to refer to Figure 11?

Section 3.4: In your comparison the model does in one case compare better with URI data, in the other with the NCAR data. If the difference between the measurements is due to a calibration issue, this would indicate to me that HCHO simulations are all over the place. Related also to comments above I wonder about the degree of confidence in the simulated HCHO. It seems that the authors are using a mix and match of different input parameters to estimate HCHO fields and the evaluation presented here does not give much confidence in the simulations. Have you for example also compared your HCHO fields to those simulated by GEOS-Chem? This could be a good test for your chemistry.

Page 11421, Line 17: The relative contributions neglecting methane in your model are still lower compared to Hudman et al. - 33% vs 56%. I do not think it is very useful to compare the values for summertime North America to global values.

Page 11422: To what degree could your seasonal adjustment to NEI emissions be impacted by uncertainties in your VOC contributions?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11395, 2008.

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