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Interactive Comment

Interactive comment on "On the wintertime low bias of Northern Hemisphere carbon monoxide in global model studies" *by* O. Stein et al.

Anonymous Referee #3

Received and published: 12 June 2014

Review: "On the wintertime low bias of Northern Hemisphere Carbon monoxide in global model studies." Stein et al.

This is an interesting manuscript that evaluates the simulated seasonal CO distribution from the MOZART chemistry transport model for 2008 against a variety of data. Sensitivity studies are performed to assess the seasonality of different emissions sources and their likely contribution to winter biases in Northern Hemisphere (NH) CO. The manuscript also examines the sensitivity of this model bias to two different dry deposition schemes. An optimised simulation is constructed to match the observations on the basis of perturbed winter traffic emissions and altered dry deposition fluxes. Figure 1 displaying the different CO budget terms is very informative. The paper is generally extremely well-written if somewhat lengthy. My greatest concern is the optimised ap-

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Interactive Discussion

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proach as it is based on constructing a missing winter source of emissions. Although the general idea is well-argued the derivation of these specific scaling factors and within season variation in Table 4 are not outlined. With some improved description here this concern would be addressed.

The following general major and specific comments are given below in chronological order.

Major comments:

1) Adding the word "found" before "in global models studies" may be appropriate.

2) The introduction is rather long –possibly not all the numerical values given are essential and the text could be more concise. In particular the text on anthropogenic emissions and MACC could be shortened. The methods also could be shortened where it is repetitive in a few places as outlined in specific comments. The evaluation data section again contains many instrument details that could perhaps be shortened.

3) The dry deposition velocities simulated with the two schemes seem to differ greatly both in terms of magnitude and variability. It would be useful to comment further on this.

4) Evaluation is performed often over three large continental regions. However these regions cover almost equivalent areas of land and ocean. For North America and Europe the sampling stations are mostly over land so it would seem appropriate to apply a land mask or to evaluate over land and ocean separately. Due to the long lifetime of CO, this may only be an issue for the lower model levels but still worth clarifying.

5) It would be helpful to have more than one year of simulations to compare to observations and to draw conclusions from sensitivity studies. Although the anthropogenic emissions may not change dramatically in recent years, biomass burning and natural emissions may. It would be useful to at least comment on this aspect.

6) Since simulated NH low biases are the key feature of this paper it would be helpful to either replace the current panels of maps in Figure 6 (since this is one of the few figures that is a map) with anomalies plots relative to one of the satellites or to add extra panels depicting anomalies to Figure 6.

7) Initial background text in Section 4.1.3 on the optimised approach and Figure 10 although interesting appear somewhat tangential to this study and could be condensed. This would allow for less general background and more specific background to the origin of the scaling factors in Table 4 and their variation. With regards to these scaling factors was some preliminary work done to derive the "optimised" concentrations? Optimized suggests the emissions were derived to match the concentrations but there is no description of the optimisation process and Table 4 appears somewhat out of the blue.

8) I'm find the text describing Fig 13 rather misleading in a number of aspects especially in having a comparison for July. The first sentence in the text describing Figure 13 results refers to differences of 170 ppb in December between MI-OPT and MI-DEP. December is not shown in this figure and the differences in January do not look like they exceed 100 ppb at most. Area average values in Figure 9 for Europe look to be < 50 ppb. Moreover, is there any merit in showing any panels for July since the scaling factor was 1, hence we would expect identical(?) results for MI-OPT and MI-DEP? Lastly, the zonal-mean OH results are illegible in the Figure and confusing displayed as is. Could these be new panels in fig 13?

9) "Overestimations attributed to natural emissions". This conclusion is important and could be phrased more clearly when discussing Figure 6 so it can be referred to in section 4.1.3. However, the argument constructed can also be applied to a certain extent to the MI+AN simulations for East Asia.

Specific Comments:

Page 247, line 5 "finally CO concentrations" – this text would be better placed earlier in

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the abstract where emission perturbation results are described.

Page 249 Line 18: Text discusses considerable uncertainties about the global budget of CO and refers to Table 1, but the relationship between Table 1 and uncertainties is not given. Further text should explain the connection.

Page 249: Line 7 add “of CO” after “methane oxidation”.

Page 250: line 17 remove “however”.

Page 251: line 16 remove “also”

Page 253: line 22 remove “Inspired by MACC . . .set-up”

Page 255: beginning of page- line 15 can the anthropogenic, biomass and biogenic emissions data used in this study be referenced to Tables 1 and 2?

Page 266: Line 21-: is it correct to say “differ in the underlying emission inventories” or rather is it that the emissions in these same inventories have been perturbed?

Page 266: lines 22-24 and page 257 lines 17-18- this text is repeats text in earlier sections- is it necessary?

Page 258: line 2 – remove “In total”.

Page 258: line 8 “Data was averaged over continental areas”- were these the blue boxes in Figure 4? If so these span both land and ocean hence averaging may not be appropriate.

Page 258: line 25- it would be clearer to discuss the European stations and the USA stations in a separate sentences as the latter only are then referred to in the following sentence.

Page 259: line 1: “interpolated, single profiles combined to monthly means” this is rather unclear. What temporal resolution data is interpolated? What is the relevance of “single”- which usually refers to location rather than time?

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Page 262: line 5 “Model data interpolated to the station location”. This sentence is too vague as is. Either remove as the interpolation is discussed earlier or expand.

Page: 262 line 11: boreal summer- the word boreal is unnecessary when describing North America and Europe.

Page 262: line 15 please explain more clearly why the seasonal distribution for the NH in Figure 6 shows a greater photochemical sink in summer? I don’t see this as an obvious conclusion from the selected figure panel.

Page 262: line 18 re-phrase “relative compliance” for clarity.

Page 263: line 1- is 9% significant?

Page 263: line 20 re-phrase “this summer overestimate in lasts until winter 08/09 (Fig 8). Figure 8 does not show a time series rather a seasonal cycle for 2008.

Page 263 line 24 refer to Figure 7 for clarity.

Page 264 line 2- the MI-BB and BIO simulations do seem to marginally improve the NH bias in wintertime in the top left panel in Figure 7. So the sentence “This constrains ..” could be re-phrased for clarity.

Page 264, line 5-8, it seems the important point to be made here is that although the improvement is only marginal with MI-VOC it does not lead to an overestimate in JJA.

Page 265: line 27: I don’t think global warming potential is a function of the RCP scenario- rather the GHG species?

Page 266: line 21, 19Tg yr-1 seems rather low. Is the contribution from North America much higher than from Europe?

Page 267: line 10 re-phrase “not enough to catch-up”.

Page 268: lines 1-11- this text reads more like discussion than results- perhaps it would be more relevant in the conclusions section.

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Page 269: line 17 add “this” before “cannot be addressed”.

Page 269: line 25. Give the literature ranges for the CO burden. Does the MI-OPT value for the CO burden of 369 Tg still lie well within the range of the reported values in the literature?- this seems the more important point to make.

Page 270: line 13. remove from “of which ... project”. The MACC project is rather heavily emphasised in the manuscript when it is not the basis of this study, and hence appears as an advertisement rather than added scientific value.

Page 271: line 3 – remove “rough”.

Page 272: line 14 “(iii) a poorly established seasonality”- in which type of emissions?

Fig 3: the scale could be improved both in terms of readability and the number of significant figures displayed.

Fig 7 and 9: the y axes could be re-scaled for each panel to show more clearly the differences between the sensitivity simulations. As is the features of the different simulations are hard to distinguish.

Fig 12: again the x-axis scale could be improved for clarity. Explain what is meant by the range of model results and how these are derived?

Fig 13: please insert the panel for July for CO even if the values are small as the figure looks most odd without. The “zonal mean” plots in the lower panels have illegible axes.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 245, 2014.

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