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Biogeosciences Discussions

Interactive comment on "The oceanic cycle of carbon monoxide and its emissions to the atmosphere" *by* Ludivine Conte et al.

Anonymous Referee #2

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General comments

In their study the Conte et al present a novel modeling scheme for marine carbon monoxide source and sink processes implemented into a state-of the art ocean biogeochemistry model. They successfully evaluate the model against available observations and present important novel insights into the spatial distribution of open ocean emissions of CO. This work will be useful to readers of BG due to the relevance of CO for a number of atmospheric chemical cycles, including tropospheric chemistry of OH, CH4, O3. I recomment publishing the paper after addressing the minor revisions below that primarily aim at helping to improve the presentation of results and methods used manuscript.

Specific comments

The short lifetime of CO in water implies large spatial and temporal variability. Related to this I have a few questions/ comments:

âĂć If I get it correctly, for simulating CO you decided to choose a model setup that uses climatological forcing that does not resolve diurnal light cycle. Forcing data like NCEP or ERA interim however do resolve SW radiation fluxes on a 6 or 3 hourly basis. Given that you are using a sophisticated wave-length dependent CO production mechanism omitting on the other hand this feature seems like an odd decision on complexity versus simplification. As production and decay rates are highly uncertain, does resolving wave length dependency and omitting the day night cycle (thus also dark production, as described in Day and Faloona 2009, JGR Oceans 114) imply you expect the former to propagation stronger into the presented mean solution than the latter, i.e. more important to resolve in a realistic CO production scheme? âĂć How does the seasonal cycle of CO look like in the model? Is the quality of the model solution different for different seasons, i.e. how does it relate to the models' ability to represent the seasonal cycle of Chl-a? âĂć In the evaluation of the concentrations you a using model data collocated to observational data. Does this mean you are using individual grid cells? If so, are these representative for a larger surrounding area – did you consider averaging several grid cells, as physical features such as the extent of subtropical gyres, location of fronts etc are not geo-referenced, i.e. collocated with real world conditions during the ship cruises? In particular for the vertical profiles it could be useful instead of showing one profile adding its variability taking into account several neighboring cells (and eventually temporal standard deviations for within the averaging period).

Given that the compilation of observational data is presented to be unique and its averaging methods are very diverse I suggest to extend the section observations. E.g. it would be interesting to learn, also in light of the large temporal variability of CO concentrations, if certain months/seasons are better resolved in the observations than others. Now I can deduce this only from the tables, but do not get any direct information in the main text. Also, it would be important to know on how many values per averaging

period the temporal means are based, as e.g. the observed diurnal cycle is very strong and party not symmetric (afternoon maxima). Furthermore, I am do not know if CO measurement techniques are comparable across the different observational sources wrt the limit of quantification/ detection, or if there was development in the methodology from the 1970s to now. Are all of the published observational data equally reliable, in particular wrt to low CO values?

I struggled with the structure of the manuscript, presenting first simulation results and evaluation of the "standard" experiments followed by a separate discussion of the sensitivity experiments.

Statements as " indicating again a possible bias in the production process " in the section on the standard experiment could be easily complemented by the results of the sensitivity studies on process parametrization, instead of having to collect this information later in the manuscript. Also this separation of the evaluation of surface concentrations leads to inconsistent level of discussion of potential sources of discrepancies: Whereas in the standard experiment it was argued that missing processes related to sea ice or a missing spatial variability of the decomposition rate might be causes of discrepancies in polar regions, only later in the section of CO production the authors state that also missing terrestrial CDOM sources might be a source of model data discrepancies. It is not clear to me how it was decided which of the parameterizations are chosen to be "standard" vs "alternative". For example, I understand that choosing the Launois et al. 2015 CDOM parameterization leads to high CO production and in combination with a consumption rate of 0.2 d-1 CO concentrations get too high compared to observations. On the other hand using the "standard" parameterization together with a consumption rate of 1 d-1 lead to very low CO concentrations. The combination of the Launois et al. 2015 CDOM parameterization and a fast consumption rate was however not tested or presented without commenting on the reasons for this. If instead the authors would present all of the tested parameterizations in the methods part equally, present first an evaluation of the model results wrt to the range of parameters chosen

and process parameterizations, and resulting from this discuss the source and sink budget, and emissions only for the most successful configuration, decisions taken and it's consequences could become clearer.

The model evaluation is lacking discussion of the simulated physical ocean solution, e.g. whereas contributions of MLD are mentioned in possible causes of discrepancies of modelled and observed CO concentrations these are not compared to the ship cruise or climatological T,S, or MLD data. I guess it would be possible to get the CTD data of the ship cruises and compare them. It would be useful to see in particular in the analysis of the vertical profiles, but also for the surface data whether how NEMO performs in regions with CO data. Furthermore the discussion of the evaluation could be more detailed in discussion the quality of the Chl-a solution, which influences CO production.

The authors assume a constant homogeneous atmospheric mixing ratio of CO in their emission calculation. As major sources of CO (fossil fuel combustion, biomass burning) are on land and a major sink is reaction with of a large hemispheric and seasonal variation of CO mixing ratios in air due to the continental distribution and OH seasonality is expected. Is the over-saturation of the ocean indeed that strong that these variations can be omitted in the emission calculation?

p8l23 .. same forcing fields as the ones in Aumont ea 2015.. Please help the reader to easily understand implications of your model setup into the results by repeating main characteristics of that forcing (source, spatial and temporal resolution). p10l10: .. all vertically integrated over the upper 1000m. The vertical profiles suggest that below the euphotic zone there is not much CO left, why do you choose to integrate over 1000m?

P12l24 .. which can be related to differences in the light penetration and mixed layer depth. Please be more specific.. is the simulated MLD generally too low/high? Do you indicate a different mixing scheme would improve the profiles? How does the model's vertical resolution in the upper ocean affect the vertical profiles?

P13 l17ff: .. those minimums are best represented by the relation of Morel since the ones in Launois .. give too high and too low values. Could you please comment on the simulated Chl field here, so that it get clear that these CDOM parameterizations are responsible for the discrepancies rather than the simulated Chl. Furthermore, satellite derived observations are based on a number of assumptions (e.g. also wrt to light penetration depth in turbid and non-turbid waters) and models (bio-optical, atmospheric correction..), in particular in derived product as CDM.

Conclusions ii) .. the model does not consider the inter annual variability of ocean physics and biogeochemistry.. Even with a climatological forcing both ocean physics and biogeochemistry solutions will show inter annual variability due to e.g. fluid dynamics (wave propagation) and different plankton over-wintering stocks. Clearly this variability will be lower than in simulations using transient forced boundary conditions or coupled atmosphere-ocean simulations. I suggest to rephrase this statement slightly.

Technical correction

Fig.7 : I find the color coding of the lines confusing.. is it time or location, what does it show in the subplot 'Swinnerton and Lamontagne 1973' ?

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