

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

Anonymous Referee #1

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Review of ‘The oceanic cycle of carbon monoxide and its emissions to the atmosphere’, ms by Conte et al. submitted to Biogeosci Discuss, ms# bg-2018-410.

General comments Carbon monoxide (CO) is an important atmospheric trace gas. The oceans (incl. open ocean and coastal oceans) are sources of atmospheric CO, however measurements of diss. CO are analytically challenging and thus only a few data sets have been published so far. To this end it is not a big surprise that its consumption and production pathways in the ocean are not well known and the oceanic CO emission estimates are associated with a very large degree of uncertainty. The ms under review presents the results of a new modelling study of the oceanic CO pathways and its emissions to the atmosphere. Despite the fact that the model approach and the chosen input parameters are reasonable, I have some major concerns (see below) which

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should be carefully addressed in a revised version.

Major comments

- Coastal oceans (incl shelf areas, estuaries etc) are important sources of CDOM which, in turn, is the prerequisite of the photochemical production of CO. I am wondering why the role of coastal oceans is not discussed in the article. It is only mentioned briefly on page 17, lines 30-33. I understand that the model is not suitable to simulate coastal oceans (shelf areas, estuaries etc.). To this end, the authors should modify the ms title and the discussion by stating that their results are only valid for the open ocean or include a discussion of CO in coastal areas (i.e. contribution to CO emissions etc).

- Important literature has been ignored:

Kawagucci, S., et al. (2014). "Molecular hydrogen and carbon monoxide in seawater in an area adjacent to Kuroshio and Honshu Island in Japan." *Mar. Chem.* 164: 75-83.

Park, K. and T. S. Rhee (2016). "Oceanic source strength of carbon monoxide on the basis of basin-wide observations in the Atlantic." *Environmental Science-Processes & Impacts* 18(1): 104-114.

Xie, H. X. and O. C. Zafiriou (2009). "Evidence for significant photochemical production of carbon monoxide by particles in coastal and oligotrophic marine waters." *Geophys. Res. Lett.* 36.

Yang, G. P., et al. (2010). "Distribution, flux and biological consumption of carbon monoxide in the Southern Yellow Sea and the East China Sea." *Mar. Chem.* 122(1-4): 74-82.

- The dark production (DP), which was shown by Zhang et al., (2008) to be a significant additional source of CO, has been ignored in the model approach (see equation (1)). However, in the conclusions (page 18, line 12-18) it is stated that ‘[...] analyses of the collected vertical profiles did not seem to clearly support the importance of such a mechanism to explain the differences with our simulated profiles.’ This is too vague

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and not acceptable. I think that the correct scientific approach to tackle this 'problem' would be to include the DP (I guess you can use the parameterization given by Zhang et al., 2008) in equation (1) and show the results of model runs with DP/without DP. Only based on these model results you will be able to assess the role of the DP.

Minor points - Page 3, line 21: please give the correct chemical formulas for nitrate, ammonium, phosphate, and iron.

- Page 6, section 2.1.4: please note that f_{CO} is a '(dry) mole fraction' (it is not correct to call it a 'mixing ratio' or a 'concentration').

- Page 6, line 19: In view of the pronounced spatial and temporal variability of atm CO I am wondering why the atm CO was set to fixed global mean. Please discuss.

- Page 7, wind speed: Please state whether you used a global mean wind speed (which value? ref?) or whether a global wind field (ref?) was used for the computation of the air/sea gas exchange. -

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