Response for Referee #1

We thank the referee for his comments and suggestions. We will strive to address each specific concern in detail.

1) Major comments

Comment 1 — Coastal oceans (including 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).

Reply: we agree that coastal areas could be important for the oceanic CO cycle. In the next manuscript version, we propose to state in the title that our study mainly deals with the open ocean and to better discuss the implications of not resolving properly the coastal ocean in the discussion section. To be more precise, PISCES does include a crude representation of the coastal areas, as some specific processes are represented (riverine inputs, iron input by sediment re-suspension, or coastal upwellings). However, these areas are represented with large uncertainties, mainly due to the low horizontal resolution chosen. Indeed, a horizontal resolution of ~ 200 km does not allow to fully resolve some fine-scale coastal processes such as tides or mesoscale and submesoscale eddies and associated upwelling. Coastal bathymetry and complex coastal currents would be much better represented with the same model using much higher horizontal resolution (see Bourgeois et al. 2016 for a application of the global NEMO-PISCES model at higher horizontal resolution).

Comment 2 — 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.

Reply: we will consider the vertical CO profiles as well as the surface concentrations measured by Kawagucci et al. in our next manuscript version.

- 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.

Reply: we will consider the oceanic CO concentrations measured by Park and Rhee in our next manuscript version, as well as discuss our global CO emission estimate against their estimate (4-24 Tg CO yr^{-1}).

- 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.

Reply: we thank you for pointing out this article. We will discuss the potential CO photoproduction by organic particles as the article suggests this process to be of importance for both coastal and blue waters.

- 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.

Reply: we did consider Yang et al. oceanic CO measurements. However, due to the coarse model resolution and the very coastal location of the data, we are not able to include these data in our evaluation as there is no model grid cell associated to the location of their measurements.

Comment 3: 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 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.

Reply: we initially chose in our study to only represent the established sources and sinks of the oceanic CO, and the ones for which global or open ocean parameterizations exist. The dark production is an issue as the mechanism associated to this process is not yet totally established (consumption of the CDOM by heterotrophic process? or physicochemical process?). Furthermore,

there is no parameterization for the open ocean. Indeed, Zhang et al. (2008) developed one for absorption coefficients at 350 nm (a_{350}) of more than 0.23 m^{-1}. PISCES describes the global oceanic a_{350} but with an annual mean in the surface ocean of 0.06 m^{-1}(standard deviation is 0.04 m^{-1}, with a minimum value of 0.01 m^{-1} and a maximal value of 0.38 m^{-1} reached in coastal areas). Therefore, the parameterization might not be suitable for a use in a global, blue water model. Zhang et al. themselves also suggest that extrapolating the parameterization to the open ocean may lead to large uncertainties. Considering that neglecting the dark production is highly questionable, we propose in our next manuscript version to better discuss this process and to test the parameterization of Zhang et al. (2008) in PISCES in order to estimate the error we make by neglecting it in our best guess simulation.

2) <u>Minor comments:</u>

Comment 1: Page 3, line 21: please give the correct chemical formulas for nitrate, ammonium, phosphate, and iron.

Reply: Yes, $NO3$, $NH4$, $PO4$, Si and Fe, will be changed for NO_3^-, NH_4^+, PO_4^{3-}, $Si(OH)_4$ and dissolved Fe.

Comment 2: Page 6, section 2.1.4: please note that fCO is a '(dry) mole fraction' (it is not correct to call it a 'mixing ratio' or a 'concentration').

Reply: We will replace the term 'mixing ratio' by 'dry mole fraction'.

Comment 3: 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.

Reply: Tests had previously been performed about the atmospheric CO but were not shown in the manuscript version. They show that the value of the atmospheric CO dry mole fraction is of little influence on the oceanic emission. For example, using a homogeneous and constant CO dry mole fraction of 45 pptv leads to a global oceanic CO emission of 3.7 Tg C yr^{-1} (against 3.6 Tg C yr^{-1} using 90 pptv). We will specify these results in the next manuscript version.

Comment 4: 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.

Reply: As mentioned in Aumont et al. 2015, we are using a global climatological wind field based on European Remote-Sensing Satellite (ERS) satellite product and TAO observations (Menkes et al., 1998). We will better explain the origin of the different forcing fields in the next manuscript version.

Bibliography:

Aumont, O., Ethé, C., Tagliabue, A., Bopp, L., Gehlen, M.: PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies, Geosci Model Dev, 8, 2465–2513, doi:10.5194/gmd-8-2465-2015, 2015.

Bourgeois, T., Orr, J.C., Resplandy, L., Terhaar, J., Ethé, C., Gehlen, M., Bopp, L.: Coastal-ocean uptake of anthropogenic carbon, Biogeosciences, doi:10.5194/bg-13-4167-2016, 2016.

Menkes, C., Boulanger, J.-P., Busalacchi, A. J., J. Vialard, J., Delecluse, P., McPhaden, M. J., Hackert, E., and Grima, N.: Impact of TAO vs. ERS wind stresses onto simulations of the tropical Pacific Ocean during the 1993–1998 period by the OPA OGCM, in: Climatic Impact of Scale Interactions for the Tropical Ocean-Atmosphere System, EuroClivar Workshop Report, 46–48, 1998.

Zhang, Y., Xie, H., Fichot, C.G., Chen, G.: Dark production of carbon monoxide (CO) from dissolved organic matter in the St. Lawrence estuarine system: Implication for the global coastal and blue water CO budgets, J. Geophys. Res., 113, doi:10.1029/2008JC004811, 2008. Geophys. Res., 113, doi:10.1029/2008JC004811, 2008.