Biogeosciences
Discussions

Interactive comment on "Global high resolution monthly pCO$_2$ climatology for the coastal ocean derived from neural network interpolation" *by* Goulven G. Laruelle et al.

Anonymous Referee #2

Received and published: 14 April 2017

Review of bg-2017-64 "Global high-resolution monthly pCO2 climatology for the coastal ocean derived from neural network interpolation" by Laruelle et al.

This manuscript proposed a modified two-step artificial neural network method for deriving pCO2 (SOM-FFN, Landschützer et al., 2013), and focused on shelf seas. The most important modification are (1) much higher resolution as 0.25 degree; (2) inclusion of sea-ice as a predictor of pCO2. From this effort, the authors may present a fine scale coastal sea pCO2 globally, as Fig. 2 in the manuscript shown. This is certainly of value. However, there are some major issues. The method is not new, rather an interpolation of the open ocean model. It was said that all data were converted to 0.25 degree from their original resolution. Then please indicate clearly original resolution

of each data, for example, SSS, SST and depth. At least for SST and SSS from the World Ocean Atlas, I wonder if the resolution is fine in the shelf seas (sorry I do not check, my memory is 1 degree). If it is true, I do not think such an interpolation of SST and SSS would help in deriving really high resolution pCO2 (i.e. the final result might be close to a simple interpolation of modeling pCO2 of 1 degree resolution). SOCAT was used for tuning the model and LDEO was used for validation, while the two dataset was largely overlapped. This is not allowed for developing a sound and solid approach. Randomly picking data from SOCAT for calibration, and then removing those data at the same location when picking the LDEO data for validation, would not be too hard to do. The target of this manuscript is not clear. Based on the title, it looks that it is talking about a new product. As to the text, methods and validation are vague, while the authors are still eager to describe the seasonality and spatial distribution, but with no way to go into depth. And maybe because of no full confidence in the results, they frequently warned "considered with caution". I would suggest the authors focusing on method and validation, teasing each detail carefully, which would raise the merit of this study. Because one of the most important changes is to include ice, the authors need to show that by including ice, what was improved? What more was acquired/learned?

Specific comments: Abstract- Writing of the abstract needs to be improved. A very clear point should be delivered. People want to know by modifying an established algorithm, what has been acquired/improved and how good it is. Now the authors just say it is assessed using two datasets. Meridional distribution is confirmed. And then talking about seasonality produced from this dataset, which people do not know if it is true or not. If spatial and temporal variability are what the authors concerned, the title should be changed correspondingly. Line 36-39, "Overall, the seasonality in shelf pCO2 cannot solely be explained by temperature-induced changes in solubility, but are also the result of seasonal changes in circulation, mixing, and biological productivity." This should be well known by everybody. I wonder what it adds to place this sentence in the abstract. It is not clear if it is to explain the seasonality the model produced is not satisfied, or simply to explain the seasonality. One may guess that in the model

only temperature was included, so the modeling seasonality can't be explained. But in fact salinity, chlorophyll and sea-ice were all included as predictors in the model, with circulation, mixing, and biological productivity all considered in addition to temperature-induced changes in solubility.

Line 118, it is Landschützer et al. 2015? Should it be 2014?

Line 141-144, "This approach facilitates future integration with existing global ocean data products (e.g., Landschützer et al., 2016; Rödenbeck et al., 2015) and model outputs, which typically struggle to represent the shallowest parts of the ocean (Bourgeois et al., 2016)". Can you explain what the inner boundary of the global ocean data products is, where they are still confident? I do not think 500 m depth would still be too shallow to struggle. I would think that using 500 m depth as the outer boundary of shelf model would be more than enough (You used 1000 m depth as the outer boundary).

Line 152-156, chlorophyll was not included to define biogeochemical provinces using SOM?

Line 185-189, SeaWiFS extends to 2014? Please double-check. To my knowledge, it ends in 2010. By the way, normally people write it as SeaWiFS, not SeaWIFS.

Line 186, should it be "one of the environmental drivers"?

Section 2.2, it would be better if to appear before the model. Then no need to ask readers to "see below" in Line 164 and 168.

Line 198, why ice was recalculated? And what kind of recalculation?

Line 211-222 is not evaluation. It is the model training.

Line 216, do you mean you used chlorophyll in FFN but not in SOM? Why?

I would say that the entire data and method section is really confusing. A cartoon, with input and out clearly indicated, and calibration (training) and validation clearly separated, would help. Also, why twice FFN? The rationale to do this is not clear.

C3

Line 353-359, this explanation is confusing. There is no reason why results from the global open ocean model can be so different from the coastal model in the overlapped cells. The only critical changes are higher resolution (actually it is an interpolation) and sea ice. Have you tried giving up ice, let other conditions be the same, see what it will be?

Fig. 2, suggest to use other color, say brown for lands. It is now not easy to tell ice cover from the land.