

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-171

Jonathan Sharp (Referee)

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Referee comment on "Derivation of seawater  $p\text{CO}_2$  from net community production identifies the South Atlantic Ocean as a  $\text{CO}_2$  source" by Daniel J. Ford et al.,  
Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-171-RC1>, 2021

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### General Comments

Daniel Ford and coauthors use a feed-forward neural network (FNN) to estimate surface ocean partial pressure of  $\text{CO}_2$  ( $p\text{CO}_{2(\text{sw})}$ ) in the South Atlantic Ocean. The authors test satellite chlorophyll *a* (Chl *a*), satellite-derived net primary production (NPP), and satellite-derived net community production (NCP) as biological predictors in the neural network to determine which produces the most accurate  $p\text{CO}_{2(\text{sw})}$ . They find that using satellite-derived NCP as a predictor in the FNN scheme produces the most reliable  $p\text{CO}_{2(\text{sw})}$  reconstructions for the Amazon River plume and upwelling regions. They also show that, among the neural networks examined, the NCP-based FNN (SA-FNN<sub>NCP</sub>) has the highest capacity for improved performance under scenarios of reduced uncertainty. For these reasons, the authors suggest that using satellite-derived NCP as a proxy for biological effects in surface reconstructions of  $p\text{CO}_{2(\text{sw})}$  may be desirable going forward. Finally, Ford et al. find that SA-FNN<sub>NCP</sub> indicates that the South Atlantic Ocean is a source of  $\text{CO}_2$  to the atmosphere, whereas the FNNs with Chl *a* or NPP as a biological proxy or with no biological proxy all indicate that the South Atlantic Ocean is a  $\text{CO}_2$  sink.

This manuscript fits well within the scope of *Biogeosciences*: it explores the implications of choosing different biological predictor variables in estimation schemes for sea surface  $p\text{CO}_2$  and demonstrates the consequences of those choices for carbon cycling calculations. It is based on the very logical assumption that NCP, which captures all biological processes that modulate  $\text{CO}_2$  concentrations in the surface ocean, should serve as a better biological predictor than Chl *a* for estimates of  $p\text{CO}_{2(\text{sw})}$ . This work has the potential to shift the way in which studies of this nature are typically performed. That shift could result in better representations of sea surface  $p\text{CO}_2$  in regions that are highly influenced by biological processes, regions that may contribute a disproportionately large fraction of global  $\text{CO}_2$  flux across the air–sea interface.

In general, the manuscript is well-written and the figures and tables are effective in communicating the results. The manuscript addresses an important aspect of the global carbon cycle and is forward-thinking in its assessment of algorithm performance in

response to reduced uncertainties. A couple concerns of mine, however, include the lack of quantitative or graphical support for the conclusion that SA-FNN<sub>NCP</sub> produces the best representations of pCO<sub>2(sw)</sub> compared to the other FNNs (section 4.2) and the shortage of further investigation into one of the manuscript's major conclusions: that SA-FNN<sub>NCP</sub> flips the South Atlantic from a CO<sub>2</sub> sink to a CO<sub>2</sub> source. More discussion of these concerns as well as some minor comments can be found in the following sections.

## **Specific Comments**

### ***Performance of SA-FNN<sub>NCP</sub>:***

I mainly would like to see some quantitative or graphical evidence supporting the assertion that SA-FNN<sub>NCP</sub> outperforms the other FNNs in the Amazon River plume and upwelling regions. Figure 3 shows differences between the mean climatologies given by different FNNs at different stations and Figure 4 shows that some of these differences are statistically significant (in comparison to SA-FNN<sub>NCP</sub>), but neither says anything about the performance of any one FNN. That is left to the more qualitative discussion in section 4.2 that compares general patterns in pCO<sub>2(sw)</sub> from previous studies to those indicated by the FNNs.

The points made in that qualitative discussion are compelling and certainly do appear to indicate superior performance of SA-FNN<sub>NCP</sub> in the Amazon River plume, Benguela upwelling system, and equatorial regions. However, following along with the discussion takes some effort from the reader, and a lot of flipping back and forth between the text and Figure 3. A new figure comparing FNN results to some pCO<sub>2(sw)</sub> observations or a brief presentation of some relevant statistics would be more compelling. In particular, for example, pCO<sub>2(sw)</sub> data from the moorings at 6° S 10° W and/or 8° N 38° W could be plotted along with the SA-FNNs results to demonstrate the superior performance of SA-FNN<sub>NCP</sub>.

I think this area is especially important to improve upon given that the bulk error statistics (Figures 2, A1, and A2) indicate SA-FNN<sub>NCP</sub> to be the least accurate of the three FNNs that have biological predictors.

### ***Sink to source transition:***

The change in the cumulative regional sink from -7 Tg C yr<sup>-1</sup> with the NPP-based FNN (SA-FNN<sub>NPP</sub>) to +14 Tg C yr<sup>-1</sup> with SA-FNN<sub>NCP</sub> seems rather drastic, and I'm curious to know

more about why such a significant change occurs. The reason is not obvious from Figure 5 alone. If indeed the transition occurs because high outgassing events in biologically-controlled regions with relatively limited geographic extent are captured by SA-FNN<sub>NCP</sub> but not the other FNNs, as is suggested in lines 399–412, that point should be demonstrated and emphasized more explicitly.

This could perhaps be explored by breaking down the annual fluxes into different sub-regions (e.g., the biogeochemical provinces from Figure 1) and/or into average monthly fluxes to clearly show the spatial and/or temporal differences that lead to the significant discrepancy between SA-FNN<sub>NPP</sub> and SA-FNN<sub>NCP</sub>. This information could be presented in a table, figure, or even just in the body of the manuscript (like the geographic comparison in lines 419–420 between SA-FNN<sub>NCP</sub> and the Watson et al. [2020] product).

### ***Callbacks in Discussion section:***

In general, because the work is presented in separate Results and Discussion sections, I'd make sure to refer specifically to figures, tables, and statements from the Results section when commenting on them in the Discussion section. This will make it easier for the reader to follow what exactly is being discussed without having to determine for themselves where those results are presented. I mention a couple specific instances of this in the following section.

### **Minor Comments and Technical Corrections**

Line 17: There shouldn't be a comma after pCO<sub>2(sw)</sub>.

Lines 45–48: I'd split this sentence into two; there's a lot of information here and it's a bit difficult to follow as written.

Figure 1: Do the colors and/or symbols represent different things in this figure? If so, I would mention it in the caption. If not, they could all be the same color/symbol since they're labeled with letters anyway. Additionally, can you add the AMT stations or the transect lines to this figure?

Table 1: What is the reference associated with the estimated uncertainty of 1 uatm in atmospheric pCO<sub>2</sub>? Also, is it correct that the other uncertainty estimates all come from

Ford et al. (2021)?

Line 189: I'd rephrase this as "A non-parametric Kruskal-Wallis was used to test for..."

Line 232: Should the accuracy here for SA-FNN<sub>NCP</sub> be 21.68 matm, like in line 235 and Figure 2?

Line 238: Based on Table 2, should these numbers be 36%, 36%, and 20%?

Lines 259–260 (and elsewhere): Should be "minimum" instead of "minima" and "maximum" instead of "maxima", or remove the article "a". Minimum/maximum are singular whereas minima/maxima are plural.

Line 283: Add the accuracy of the SA-FNN<sub>NCP</sub> after you mention it here, so it can be easily compared with Landschützer et al. (2013) and Landschützer et al. (2014).

Lines 285–286: You've already mentioned in the previous sentence that the SA-FNN<sub>NCP</sub> approach has a similar accuracy to other approaches in the literature, so I'd say here that training the SA-FNN with Chl *a* or NPP gave comparable broad-scale accuracy to training it with NCP.

Line 295: "Satellite NCP is reliant on NPP as input" This has already been implied in line 288, so I'd remove the statement here or move it to the previous paragraph. The point is well made, it's just that the writing is a bit repetitive here.

Line 298–299: "This showed that reducing *in situ* NCP uncertainties provided the greatest reduction in pCO<sub>2(sw)</sub> RMSD, which was three times the reduction achievable using Chl *a*." I'd make sure to refer the reader to Tables 2 and 3 so they're not searching for this result.

Lines 328–329: "The stations (Fig. 1) represent locations of previous studies into *in situ* pCO<sub>2(sw)</sub> variability in the South Atlantic Ocean and allow comparisons with literature values." This point should be made earlier, in the Methods section, perhaps around line 182.

Lines 338–339: "Valerio et al. (2021) indicated pCO<sub>2(sw)</sub> varied above and below pCO<sub>2(atm)</sub> at 4° N 50° W consistent with the SA-FNN<sub>NCP</sub>." It looks to me in Figure 3 like pCO<sub>2(sw)</sub> from

SA-FNN<sub>NCP</sub> remains almost exclusively below pCO<sub>2(atm)</sub> at this site?

Line 396: Should be "may be" instead of "maybe"