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Biogeosciences Discussions

Interactive comment on "The northern European shelf as increasing net sink for CO_2" *by* Meike Becker et al.

Anonymous Referee #2

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This is an interesting manuscript that tackles an important problem: maps interpolating sparse observations of surface ocean pCO_2 (and related variables like pH) perform well in the open ocean but generally do not accurately reproduce the conditions seen in more complex shelf sea environments like the northwest European continental shelf, the focus of this study. The authors apply a long-established technique (MLR) but with the innovative step of using low-resolution open-ocean pCO_2 maps as one of the predictors. They tested two different open-ocean pCO_2 maps and also developed a 'traditional' MLR based only on other in situ variables. One of the open-ocean maps, which did project pCO_2 values across the shelf seas, performed slightly better than the traditional MLR but the other, which did not, performed better or worse depending on the metric considered, although the authors state it was better. The former

open-ocean-map-based MLR was therefore used to derive most of the results. The discussion is mostly a description of the trends in surface ocean pCO_2, air-sea CO_2 fluxes and pH in the relevant shelf seas.

There are a few issues I think the authors should consider revising before publication:

One of the strongest reasons to use an MLR instead of a neural network approach is the relative ease with which the predictive model can be shared and used by other researchers. Please would the authors therefore provide the actual fitted coefficients to their equation 1.

The word 'coasts' is used throughout to describe the study area but it is not clear how this is defined. For me 'coast' would refer to the very near coastal zone (e.g. intertidal areas) as opposed to 'shelf sea' which would go out to a depth contour of e.g. 200 m. The results do not also extend all the way to the coast, as can be seen from the white gaps between land and ocean on Figures 4, 5, and 9–12 and noted in the penultimate sentence of the Conclusions. Please explicitly define, and consider revising, the terminology used.

Is it valid to predict all the way up into the northern Baltic Sea given that there appears to be only one month of data there (Figure 2)?

The previous study results given in Table 1 for the North Sea show a range of different values (specifically, Thomas et al. (2007) vs Salt et al. (2013)) and also covering different time periods, with Salt et al. finding a different rate of change from 2001-2005 compared with 2005-2008. Salt et al. implicate the NAO as a key driver of this short-term variability, but this study does not mention the NAO explicitly. Do these new results provide any evidence for the NAO influencing air-sea CO_2 exchange here? On the other hand, Figure 9, upper left grid box panel for the North Sea, indicates that no significant trend can be found in the North Sea for these short periods reported by previous studies. Implicitly, this figure is therefore saying that the different trends reported in previous studies are in fact not significant. Is that a point the authors intend

to make? Either way it feels like there is some interesting discussion missing here.

p19, line 1 states the western North Sea did not show a significant trend, but this area does not have black dots in Figs 9 and 10. Are trends significant here or not? Also, this paragraph as a whole does not effectively justify or explain its opening sentence.

Please provide details of all CO2SYS options selected (e.g. borate:chlorinity). Consider using the newer CO2SYS v2 from Orr et al. (2018) and including error propagation from the equilibrium constants in your calculations?

Finally, a few minor points to consider:

It is noted several times that and old version of SOCAT (v5) was used for the fitting before the explanation on p8 that the reason for this was so that the newer version could be used to independently test the fits. It would be helpful to mention this the first time SOCAT is discussed. Why do the different panels in Figure 3 (in particular the second panel) show different subsets of SOCAT data points?

Figure 4: colour bar should be labelled fCO_2, not ΔfCO_2.

Figures 5, 9, etc.: maps contain a lot of straight lines and right angles, usually indicates boundaries between regions with different predictive equations but they don't entirely match with the regions shown in Figure 1, what is the cause?

Figure 9: what is the difference between a cross and a circle?

The colour scale on Figure 11 feels counterintuitive, as usually CO_2 source areas are shown in red and sinks in blue.

p9 line 2: missing citation.

p10 line 3: MLR, not MLD.

In units for rates please explicitly clarify whether d means decade or day.

There are a few issues with the English language throughout so this aspect should also

be carefully checked through.

I support the comments and suggestions made by the other reviewer.

0.1 References

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Salt, L. A., Thomas, H., Prowe, A. E. F., Borges, A. V., Bozec, Y. and de Baar, H. J. W.: Variability of North Sea pH and CO_2 in response to North Atlantic Oscillation forcing, J. Geophys. Res. Biogeosci., 118(4), 2013JG002306, doi:10.1002/2013JG002306, 2013.

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