*(reviewer comment,* author response, change in manuscript)

*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.*

We recommend strongly to develop a specific fit for any new application. The fit coefficients are expected to change with using different products of driving data. Additionally, the driving data products are updated and improved regularly. As these annual updates often also involve changes in the historic data a new fit should be done for any new combination of driver data.

*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.*

We changed the use of the very general term coasts to coastal seas or continental shelves. The definition for coastal seas as used in this work can be found under Methods: Study area. A limiting factor for the extension of the maps to land is the availability of driver data. Intertidal areas for example are not represented in the driver data. We also added a sentence in the section 'Methods: Study area' to clarify what we mean when we use the term coastal seas in this manuscript.

> Please note, that this study concentrates on the continental shelf area. the near coastal zones (e.g. intertidal zones) are not included due to the limited availability of driver data in these regions.

*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)?*

This is a good question. One could easily argue to remove this part. We decided to include it to give a flux estimate for the entire Baltic Sea. Within ICOS, there was a new underway pCO2 system installed on a commercial vessel sailing through the Gulf of Bothnia in 2019. We therefore expect a much better data coverage in the region from 2019 on. It will be very interesting to compare the maps we show here with an updated version that include a full annual cycle in the Gulf of Bothnia.

*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.*

We think that in depth testing of underlying drivers, such as NAO, is exceeding the aim of this manuscript. Here, we primarily want to present the maps. That being said, we did of course have a look at potential driving factors, but we did not find evidence for the NAO to be a key driver in any of the regions. When looking into detail there are a few features that seem to be related (such as for example the large disequilibrium in the Norwegian Coast region in 2010, a year with a very negative NAO index).

*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.*

We changed this sentence to:

> The observation that large subareas (the Baltic Sea, along the shore of the western North Sea) did not show a significant trend can be explained by the fact, that coastal sea systems, especially enclosed areas as the Baltic Sea, experience a high anthropogenic pressure.

*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?*

Added the information about the boron-salinity ratio. We are working on including the error propagation into our scripts and this will be included in a future, updated release of the maps.

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.*

The possibility to compare was one point and the other was the time that past when preparing and analyzing the maps. We added the following sentence to the 'data handling' section:

> A newer version of the SOCAT database (SOCATv2019) was used for validating the maps against independent data.

*Why do the different panels in Figure 3 (in particular the second panel) show different subsets of SOCAT data points?*

changed

*Figure 4: colour bar should be labelled $fCO_2$, not _$fCO_2$.*

changed

*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?*

These lines are an artifact stemming from the open ocean pCO2 maps that were used as a driver. You can see here the remains of the 4x5 ˚ grid of the original Rödenbeck et al product.

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

Significant increase/decrease of temperature with time. This is described both in the figure itself, as in the figure description.

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

Changed the color code in Figure 11 and 12

*p9 line 2: missing citation.*

Added reference

*p10 line 3: MLR, not MLD.*

changed

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

The unit of all rates shown in this manuscript is per year. It is the unit of the fluxes is per day. We do not see the need for clarification here.

*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.*