

Reviewer comments in gray italics, my answer in black and the changes in the text in “...” and bold

Answer to referee 1

The authors have partly addressed my previous comments. However, two important points were eluded.

*First, the authors acknowledge that there is an **artifact in the interpolation scheme** that led to strange CO₂ patterns in the North Sea, and an artificial difference between the west and the east side of the North Sea in particular in the SBNS that has been shown in the past as relatively homogeneous. They acknowledge that “These lines are a remnant of the open ocean pCO₂ maps, which were used as a driver in the MLR (in this case Rödenbeck, 4x5° resolution).” However, they did little to try to correct this by adjusting/modifying the interpolation scheme. I do not see the added value of interpolating data and using fancy MRL approaches in an area where the data coverage is already very dense, to provide in the end clearly biased maps that the authors did not bother to try to correct.*

We regret that we have missed to address all the previously raised concerns by the referee. It was certainly not our intention to elude the important points raised. While we do understand and agree (hence our acknowledgement in the text) with the referee’s argument that the coarse resolution nature of the open ocean driver data (namely the Rödenbeck 4x5 degree pCO₂ field) and the resulting “patchy” pCO₂ reconstruction appears too heterogenous in contrast to other studies. The referee therefore suggests correcting our approach in order to improve performance, however there are some noteworthy complications that prevent us from doing so.

Firstly, the construction of the input data for our MLR is out of our hand. While we would benefit from a finer resolved Mixed Layer interpolation scheme by Rödenbeck et al, (and equally finer resolved physical proxies such as temperature and salinity, etc) this is not feasible considering the built-up of the method. That said, Rödenbeck et al have now created a finer resolved version of their scheme (with 2.5° x 2° resolution), however, even with this finer resolved version we still see this spatial gradient. Other products with higher resolution that extend to the coast and may serve as an alternative for the future are currently in review (see also Laruelle et al and the product of Landschützer et al 2020 and discussion below), but none of these alternative products offers a resolution attempted in our study. Our primary intention is to make use of the coarse resolution existing pCO₂ estimates to provide novel and fine-resolved coastal estimates, but not to improve the existing estimates. That said, we believe this still is valuable information.

Secondly, we believe it behooves us well to highlight shortcomings of our approach, even when they are outside our ability to change. That said, we agree that this deserves a more detailed discussion than previously provided.

Thirdly, we agree with the argument by the referee that direct measurements do not show these gradients as strongly and are therefore more reliable. Nevertheless, for many applications, such as model evaluation or the investigation of regional trends a high-resolution gap-filled pCO₂ product is required, desired or even inevitable. In addition, the amount of

available data is not such that they can be mapped with confidence every single year. We have first-hand experience, and gaps due to instrumental failure and funding issues, do occur. Here we offer a first, though not bias-free, estimate that aims to be applied to all coastal regions of the western Nordic Seas, discussing its shortcomings and offer ways forward to improve it in the future. One way forward would be to improve the resolution of the open ocean pCO₂ product. A second possible way forward would be to apply different drivers, as we potentially do not need pCO₂ as a driver for data rich regions. Illustrating and discussing ways forward is certainly something we have missed in our previous manuscript and our first response to the referee's concerns, however, working out the technical aspects is, as we still believe, beyond the scope of this study. In this study we focus on the best and most robust scheme for all regions combined.

Therefore, considering the point raised by the referee and our answers above, we have extended the discussion and added the following:

In the Results (p 10, l 11 – p 12, l 6):

“We notice that the gradients that exist between the grid cells in the Rödenbeck map, are still visible in our maps in some regions, for example the sharp gradient in the southern North Sea in February, or the east-west and north-south gradients in the entire North Sea in August. Such gradients are also evident in directly mapped pCO₂ data (Kitidis et al. 2009), however, here they are more strongly meridional and latitudinal in their extent. As such, while these gradients do reflect actual features of the pCO₂ distribution in the North Sea, their specific shape here, are also a consequence of the influence of the Rödenbeck maps on our estimates; from the use of these maps as a driver in the MLR and their importance in improving the statistical performance vs the MLR that did not use these values as a driver (MLR 1 vs MLR 3, Table 5). Also, they do reflect the uncertainty of - and our level of confidence in - the estimated pCO₂ values; being approximately similar to or slightly larger than the RMSE of MLR 1 (Table 5). Any smoothing would be completely artificial, and, while being more visually pleasing, would not better reflect the truth in any meaningfully quantifiable extent. We have therefore chosen to leave them untouched. These gradients are therefore also visible in subsequent pH and trend maps.”

In the Discussion (p 14, l 9 – l 7):

“One clear drawback of the here presented MLR 1 is the clearly visible grid-pattern of the open ocean pCO₂ product that was used as input data with its grid size of 5° x 4°. This artifact implies sharper gradients in fCO₂ than can be found in observations. There are two ways how one could get rid of this artifact in a future release. A finer resolution of the used open ocean maps will lead to a better representation of the actual gradients in our mapped product. Rödenbeck et al. just released a newer, finer resolution of their open ocean product that we intend to use in a future version of this data product. Additionally, running the MLR without an open ocean pCO₂ product can provide a coastal pCO₂ product without this artifact (given that all other driving parameters, such as temperature or mixed layer depth, also are available in the required resolution). While in principle it is preferential to have coastal maps that are independent of the open ocean

products, MLR 3, which is running without open ocean pCO₂ as driver, did not reach the same accuracy as MLR 1. New and better input fields or a different regression method could help improving the independent coastal maps in the future.”

Second, the authors acknowledge that the pCO₂ shows a shift in the spring bloom timing. This is a really interesting result that would strongly contribute to the future impact of this work. So I do not understand that the authors did not include this in the paper, I do not see how this could be “outside the scope of this work” as replied.

Again, it was certainly not our intention to elude the important points raised and agree that this observation should be included and explored to a larger extent in the discussion. We do believe, however, it is “out of scope” for us to quantify the reason why this shift has happened, as this would entail detailed examination of the atmospheric, oceanic and ecosystem conditions that can bring about such a change. We also note, that the monthly resolution of our maps somewhat restricts abilities to detect changes in the timing of the onset of the spring bloom, as such changes may be a matter of days to weeks. That being said we do see a significant shift the bloom timing in the western North Sea between the first and the second half of our time series. We added a panel showing the average pCO₂ seasonalities in the northwestern North Sea from 1998 to 2007 and 2007 to 2016. We also added extended discussion and added an additional panel to Figure 10.

(p 20, l 18 – p 21, l 6)

“Figure 10a shows the annual trends in fCO₂ in each month in the four regions considered. Particularly in the North Sea and Baltic, very low fCO₂ trends are observed in February – May, suggesting that changing timing of the spring bloom might be important here. Investigating the seasonal fCO₂ in more detail (Figure 10b), revealed an earlier and deeper fCO₂ drawdown in the second decade of our analysis (2007-2016) than in the first (1998-2007) in the northeastern North Sea (58 – 60°N, 3 -8°E). This strongly suggest that an earlier and stronger spring bloom is lowering the annual pCO₂ growth rates in this region, which is among the ones with the smallest fCO₂ trends ($X \mu\text{atm yr}^{-1}$, Fig. 9). In the other regions, no such changes could be established with confidence. Future investigations should aim at generating fCO₂ maps with higher temporal resolution, as changes in the timing of the spring bloom might be a matter of days or weeks, which would not be fully resolved by the monthly maps presented here.“

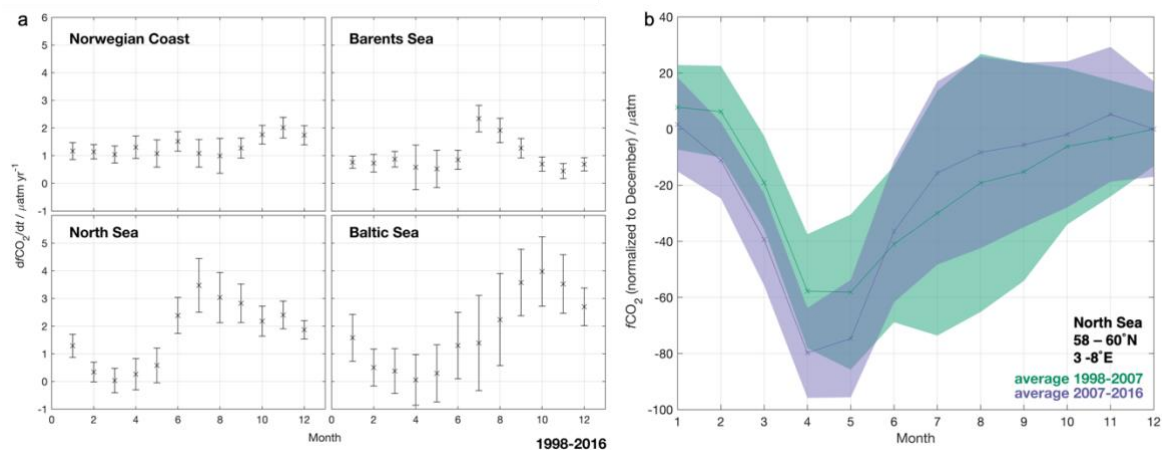


Figure 10: (a) The trend in surface ocean fCO₂ estimated resolved per month (1998 to 2016). (b) The average seasonality in fCO₂ for the periods 1998-2007 (green) and 2007-2016 (purple) in the northeastern North Sea (58 – 60°N, 3 -8°E), normalized to December. The standard deviation for each month is shown as shaded area.

Can the authors add to the discussion how their approach compares to the recent work of Landschützer et al. (2020) that seem to provide a consistent and uniform interpolation scheme for the open and coastal oceans.

At the time of initial submission, the study by Landschützer et al 2020 was not yet submitted and the submission of the 1st revision the study only existed as a pre-print (i.e. has not undergone peer review in its online form). To follow rigorous scientific standards, we try and avoid discussing grey literature. Fortunately, the study by Landschützer et al 2020 has now been accepted for publication. We understand the resemblance between these studies and the resulting need for clarification. There are several major differences between the study of Landschützer et al 2020 and this work:

Firstly, Landschützer et al 2020 do not provide a new estimate in our chosen study domain, but combines the open ocean estimate by Landschützer et al 2016 with the coastal ocean estimate by Laruelle et al 2017. Therefore, most regions that are both covered in our study and in the Landschützer et al 2020 estimate actually stem from Laruelle et al 2017, which we do discuss in our manuscript.

Secondly, Landschützer et al 2020 combine their estimates to provide a 0.25x0.25 degree climatology covering the global ocean. Here, we, on the one hand, provide a higher resolution local estimate, which on the other hand focuses on longer term signals rather than seasonal variations.

To acknowledge the existence of this climatology, and its potential to further improve our local high-resolution approach, we added to the text:

(p 2, l 22 – l 24)

“A global climatology covering both open ocean and coastal regions was recently

performed by combining this product with the open ocean product of Landschützer et al (2016) (Landschützer et al.,2020)."

Refs

Landschützer, P., Laruelle, G. G., Roobaert, A., and Regnier, P.: A uniform pCO₂ climatology combining open and coastal oceans, Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-90>, in review, 2020.