(reviewer comment, author response, change in manuscript)

The authors use a MLR approach applied to the SOCAT CO2 data-base to reconstruct a spatially and temporally resolved data-set from 1998 to 2018 in the European continental shelf. From this data-set the authors analyze the temporal trends of pCO2 during winter in different regions (North Sea, Baltic Sea, Norwegian coast & Barents seas) that are compared to the increase of atmospheric CO2.
A more detailed and in-depth analysis could be made.

For instance, the authors compute the trends based on winter-only data. However, since they have a fully seasonally resolved reconstructed data-set, they could also analyze the temporal trends using summer-only data. Are the trends the same ?
In addition, they could compute the trends using the full annual average, which in principle should provide the most robust estimate of inter-annual variations since it integrates all components of seasonal variations. Are the results for the full annual average the same as the winter-only or the summer-only trends?

We do not use winter-only data for estimating the trends. The trends shown in this manuscript are computed over the entire year. We did compute also winter only and summer only trends for comparison to the other studies. Trends in summer were generally less significant than the all-year or winter-only trends. We agree with Referee#1 that the contribution of the different seasons to the overall trend is an interesting feature that can be investigated further. We therefore added a paragraph addressing the trend in pCO2 for every month.

> The hypothesis, that an earlier or more intense bloom onset is responsible for the relatively low trends in the North Sea is supported by looking at the contributions of the different months to the overall trend. Figure 10 show the trend for each month in the four different regions.

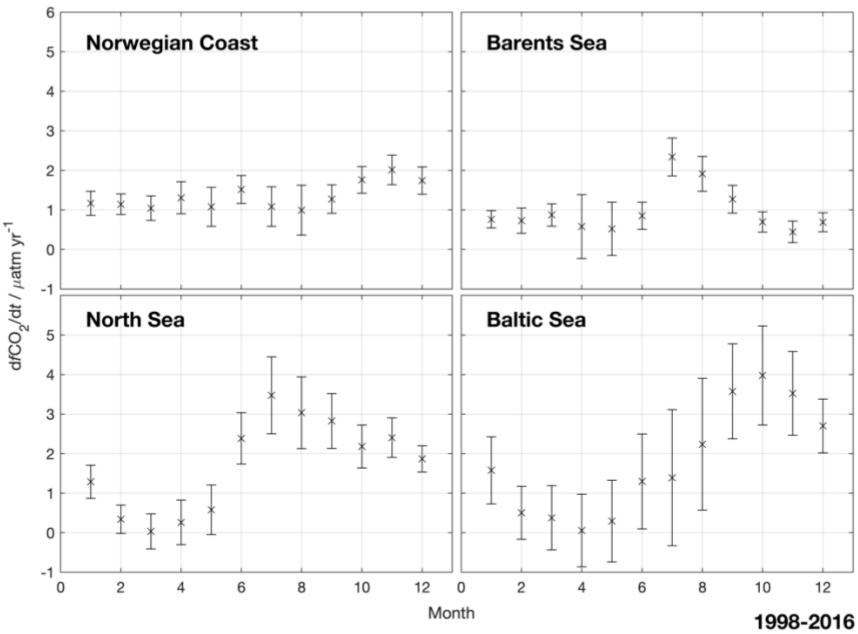

Figure 1 The trend in surface ocean fCO_2 estimated resolved per month (1998 to 2016).

The other question that the authors could attempt to address is how useful is this MRL approach compared the raw SOCAT data-set to compute temporal trends. So, would the analysis of temporal trends of the raw SOCAT data give the same results as the MRL expanded data-set ? Of course this would require to aggregate the raw data into larger boxes (for instance 3 large boxes for the North Sea:

southern bight of the North Sea, Central North Sea and Northern North Sea) to overcome the lower coverage of the raw SOCAT data. This question is motivated by the fact that the European Shelf is one of the areas which is most dense in CO2 data, so that you need to address the question of the usefulness of using a complex MRL approach to reconstruct and gap-fill for an original data-set that is one of the most dense for continental shelves.

Reviewer 1 is right, when they state, that the European shelfis one of the coastal regions in the world with highest density in CO2 data, especially when looking at the northern North Sea and parts of the Baltic Sea. However, that is not similarly true for all European shelf regions. In the North Sea it would definitely be worthwhile to perform a proper data-based, high resolution trend analysis for the entire basin and then comparing the results to ours. We think, that our manuscript here is not the right place to do so. For the northern part of the North Sea, there is a recent study by (Omar et al., 2019) focusing on winter trends. They find the same trends as we: no significant trend east of about 5E and a trend close to the atmospheric trend west of 5E. We followed also the suggestion of RW1 and performed a quick-and-dirty trend analysis for 9 large boxes (based on deseasonalized gridded SOCAT data from SOCAT v5). The results of this analysis support the results from our maps. We added a table with the SOCAT-based trends to the fCO2 trend section of manuscript and a picture showing the regression analysis to the supplement.
Principally, we do think, that is there is a large value in developing gap filling methods also in regions with a high data density. The major application of gap filled pCO2 products lays not the estimation of trends in pCO2, but in air-sea CO2 fluxes and estimating the ocean carbon sink. For this pCO2 data covering all months, years and regions is crucial.

> The northern European shelf is a region with a high data density. In order to validate the general patterns of fCO_2 trends we estimated the fCO_2 trends also from the SOCAT v5 observations, that was used to produce the MLR (Table 6). We gridded and deseasonalized the SOCAT v5 data and divided the entire region into 9 subregions. A figure showing the fits and the data coverage can be found in Appendix A.
> These directly observation based trends show similar general patterns as those based on our maps (Figure 8, 1998-2016): (1) largest trends in the southern North Sea, (2) decreasing towards the North with trends around the atmospheric trend in the northern North Sea and trends around 1 μatm yr^{-1} in the Barents Sea, (3) close to atmospheric trends in the Baltic Sea.

Region	Latitude / °N	Trend / μatm yr^{-1}
North Sea, South	51 - 54.5	3.2 ± 1.3
North Sea, Center	54.5 - 58	1.43 ± 0.21
North Sea, North	58 - 62	2.320 ± 0.089
Norwegian Coast, South	62 - 68	2.12 ± 0.19
Norwegian Coast, North	68 - 73	1.426 ± 0.099
Barents Sea, South	69 - 74	1.31 ± 0.30
Barents Sea, North	74 - 85	1.01 ± 0.22
Baltic Sea, South	54 - 56	2.05 ± 0.12
Baltic Sea, North	56 - 61	1.84 ± 0.21

Table 1 fCO_2 trend calculated from gridded, deseasonalized SOCAT v5 observations.

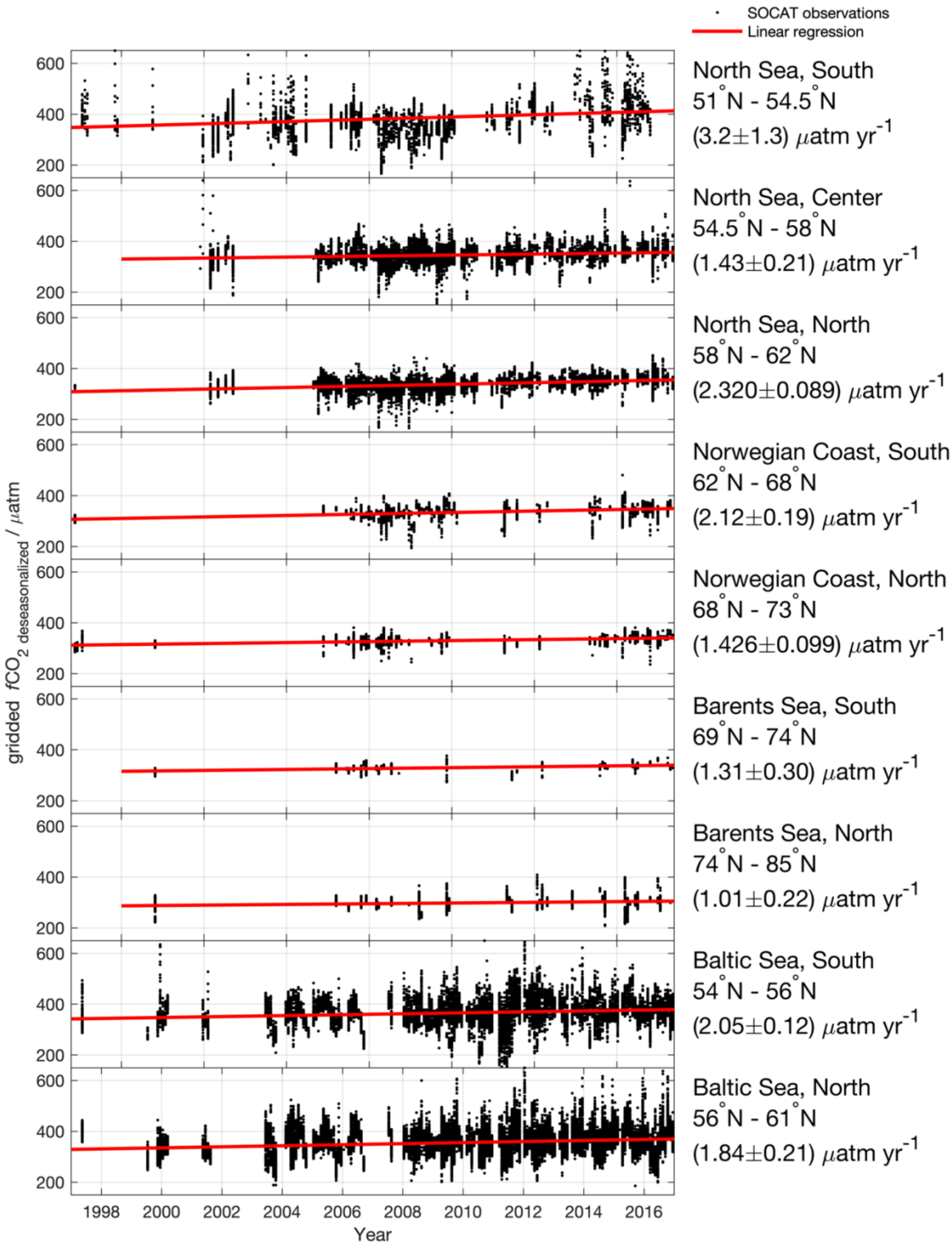

Figure 2: Trend in surface ocean fCO_2 in deseasonalized, gridded observation data (SOCAT v5).

Figure 9 shows that in the Southern bight of the North Sea (<53_N) there's a very strong difference between the part along the UK coast (red color = strong increase of pCO2 in time) and the part along the Dutch coast (blue color = very low increase of pCO2 in time). The two regions are clearly separated along a line that seems to correspond approximately to the 2_ meridian. This line seems to also separate the Central and Northern North Sea although the differences in pCO2 trends are not as marked. But this is really strange as the spatial pCO2 distributions in the Southern Bight of the North Sea are relatively homogeneous horizontally (Thomas et al. 2004; Schiettecatte et al. 2007) so it's really odd that the temporal trends should be so different. This seems to be related to the way the MRL was implemented in the North Sea that seems to have been divided into East and West regions (along the 2_ meridian) in the computation scheme (I guess). Anyway this needs to be addressed, either change the computation scheme to avoid this spatial artefact, or if this is "real" then please provide an explanation for this odd looking spatial difference.

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).

> As most the driver data has a smaller resolution than the final maps (see Table3) the grid of the driver data is still visible in the final maps. This is specifically the case for the used open ocean pCO$_2$ maps. Residuals of the original open ocean Rödenbeck map (resolution 5 x 4) are clearly visible in the MLR 1 maps as well as the trends and fluxes calculated from these.

MODERATE COMMENTS

P2 L9 : "small currents caused by the topography" does not cover the full spectrum and complexity of physical processes in continental shelves. In continental shelves there are difference buoyancy sources (thermal and haline stratification) and mixing processes (tides, upwelling, internal waves) that lead to contrasted physical settings. Please refer to classical paper by Blanton (1991).

We agree that in coastal regions more diverse physical processes involved. The processes we named was meant as examples. However, we changed the sentence and added a reference to Blanton (1991).:

> Small scale circulation patterns governed by topographic features, thermal and haline stratification, or mixing though tidal cycles, upwelling or internal waves result in a need for more complex maps with a higher resolution (Bricheno et al., 2014; Lima et al., 2012; Blanton, 1991)

P2 L5-14 : The introduction on the differences between coastal and open ocean waters seems to miss some important elements. CO2 patterns in costal environments are more complex that in the open ocean because overall coastal waters are more productive than open ocean, because there are several sources of nutrients such as mixing processes at continental margins (upwelling and internal wave mixing) and riverine estuarine inputs. In addition shallow areas are vertically mixed while deeper areas are seasonally stratified. Please refer to classical paper of Wollast (1998). Overall this leads to important spatial heterogeneity and strong horizontal gradients of productivity that are reflected in equivalent gradients in surface CO2.

We agree completely with RW1 that the description of differences between open ocean and coastal regions was lacking some of the biogeochemical characteristics. We changed the paragraph accordingly:

> Generally, coastal regions show a larger productivity than open ocean regions due to different additional sources of nutrients (e.g. mixing at continental margins, river runoff). While deeper regions are seasonally stratified, shallow regions are vertically mixed allowing for exchange between the benthic and pelagic parts of the ecosystem (Griffiths et al.,2017, Wollast,1998). Together with strong gradients of productivity this leads to spatial and temporal heterogeneity in surface CO_2 content.

P2 L 15 : Please briefly explain why methods for open ocean are inadequate for coastal waters and provide references if available.

We do not state that the methods are not suitable for coastal oceans. The work of (Laruelle et al., 2017) for example is based on the SOM-FFN method of (Landschützer et al., 2017). Our point is that the currently existing open ocean maps have a too scarce resolution and therefore cannot be used in coastal regions (This is stated in the text). Recently, many reanalysis products that are used as driver variables became available in a higher resolution. In addition to that, the computers get stronger and stronger. This now enables the production of maps with a higher resolution. (At least in regions with sufficient pCO2 observations)

P 3 L 20 : define "winter season" in the southern north sea diatom blooms can start as early as February.

We added the information, which months were used in the respective literature studies to Table 1.
We discuss at different places throughout the manuscript that the variability of spring bloom start, especially in coastal regions, is one of the major limitations of using winter-only trend estimates in coastal regions. In the new version of the manuscript there will be a new paragraph in the discussion section focusing the influence of the season on the trend estimate.

P8 L 13 Nondal et al. (2009) report a TA-salinity relation for the Northern North Atlantic Ocean that should be applicable for Norwegian coast and Barents sea but it could useful to check if it is applicable in the North Sea (e.g. Salt et al. 2013), and in particular in the Southern North Sea (Hoppema et al. 1990).

We agree with RW1 that using the (Nondal et al., 2009) equation results in larger uncertainties in the North Sea, especially the southern North Sea than at the Norwegian Coast or in the Barents Sea. Using Nondal et al equation in the North Sea will most likely result in underestimating the alkalinity at low salinities. In the North Sea the low salinity water usually come either from the Baltic Sea of riverine input. The regressions shown in (Salt et al., 2013) and (Omar et al., 2019) show a larger intercept than the equation we used. Both regressions are focusing on the Skagerrak region and the Northern North Sea The work of (Hoppema, 1990) in the Southern North Sea and the Wadden Sea shows in general very high alkalinity vs salinity ratios compared to the other two studies.
Besides the Skagerrak, regions with low salinity can be found close to shore and at the big river mouths. When looking on the GLODAP dataset alkalinity and salinity in these regions is not correlated at all. Therefore, the pH in these regions should be handled with care. However, for the majority of the North Sea where the salinity is varying between 34 and 35, the equation of Nondal et al describes the salinity-alkalinity correlation better than the equations of Salt et al and Omar et al, which are based on the Baltic Sea inflow. We agree, that using different equations for the different regions could be a good way to improve pH maps based on fCO2 maps in the future.

P 13 L 9-10 : Calling this comparison "validation" is a bit surprising. The authors used the SocatV5 data to generate a fCO2 data using MLR and then compare it again to the original SocatV5 data. This is not a real validation.

We do compare our maps against independent data. We predict our maps for the years 2017 and 2018 and compare these against SOCAT data from these years (SOCATv2019). However, as this obviously is not stated clearly enough, we changed the introduction of this section.

> The prediction of the maps into the years 2017 and 2018 will be compared with data from the newest SOCAT release (SOCATv2019) to have a comparison with an independent dataset.

We added also a sentence to the data handling section:

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

We acknowledge that the naming of the section 'Validation' caused confusion and changed its name to 'Performance'.

P14L8 you discuss data in 2017 and 2018 but at the end of the introduction (P4L3) you say that you look at trends from 1998 to 2016.

We use the data from 2017 and 2018 for validating our maps. The trends are calculated only until 2016. We did not calculate trends for the latter years, as these have a higher uncertainty due data from these years not being included in the fits. For extending the fits to 2018, we recommend obtaining a new fit equation that includes data from 2017 and 2018. The general goal of these maps is an annual release of the maps based on the latest SOCAT version.

P18L4: Paper of Sharples covers the period 1974 and 2003, so it's a stretch to assume that the trend for the 1974-2003 was continued over the period of 1998 to 2016. There are several other papers that have addressed recent changes of phytoplankton phenology in the North Sea.

We added (Desmit et al., 2020) as a reference. The paragraph was changed to:_

> The bloom timing and onset in the North Sea after the 1990s has been shown to be mainly triggered by the spring-neap tidal cycle and the air temperature (Sharples et al., 2006). The bloom timing and onset was found to be significantly earlier in the 2010s compared to the previous decades (Desmit et al., 2019).

P20L6: "The lower trend stems most likely from an earlier onset of spring bloom" The authors have the data to test this, since they have reconstructed a temporally resolved data-set. If the onset of the bloom is earlier in the year, then so should the peak of the bloom. The seasonal CO2 minimum is a good proxy for peak spring phytoplankton, so the authors can check if this has changed in time and occurred earlier in the year.

We tested this and found the low trends to come mainly from spring (see Figure above). When plotting the seasonal cycles for pCO2 in the early part of the time series in comparison to the later part of the time series, there is a shift to an earlier decrease in pCO2 during spring visible. We think that this is a very interesting topic and it certainly holds the potential for further, more detailed investigation. However, we also think that this will go beyond the aim of this manuscript.

P20L24: "The sea-air CO2 fluxes (Figure 12) show that most regions are a net and increasing sink for CO2. The only source net regions are the southern North Sea and the Baltic Sea. The two different regimes in the North Sea with the southern, nonstratified part being a source and the northern temporarily stratified part a sink for CO2, are well described in the literature (Thomas et al., 2004)." Thomas et al. (2004) only sampled the North Sea during 4 cruises, and their "spring" cruise was in mid-May, when the spring phytoplankton in the Southern Bight of the North Sea is over. So Thomas et al. (2004) missed the peak of the spring bloom (and minimum of CO2) that occurs in April, as clearly shown by the work of Schiettecatte et al. (2007) and Omar et al. (2010). This is why Thomas et al. (2004) reported the Southern Bight of the North Sea as a source of CO2 to the atmosphere, since their data-set does not represent the period of strong CO2 under-saturation during spring. The better seasonally resolved data-set of Schiettecatte et al. (2007) shows in fact that the Southern Bight of the North Sea is a small sink of atmospheric CO2, although admittedly lower than the Northern North Sea.

We do not agree that different timing is the reason why (Schiettecatte et al., 2007) reports the southern North Sea as a sink for CO2, while (Thomas et al., 2004) find it being a source. All spring cruises in (Schiettecatte et al., 2007) were very late in the month. 11BE20040329 and 11BA20040524 are in the SOCAT database. As the paper states that there was a time difference of 28 days between the March,

April and May cruises, respectively, we can assume that the April cruise also took place during the last days of April. That means that the May cruise in (Thomas et al., 2004) (64PE2002506) started only a week after Schiettecatte et al.'s April cruise. (Thomas et al., 2004) might have missed the minimum, but we doubt that this effect is large enough. We think the difference in the various flux estimates is largely driven by interannual variability. As you can see from the data presented in (Omar et al., 2010) bloom timing and intensity can vary rapidly from year to year. Another large factor in comparing these flux estimates is the used wind velocities. Both studies (Schiettecatte et al., 2007 and Thomas et al., 2004) use wind velocity during the time of the cruise. This means the in Schiettecatte et al., (2007) wind data from a few days in the end of the month is used for reporting a monthly flux.
We extended the discussion about this point:

> The sea-air CO_2 fluxes show that most regions are a net and increasing sink for CO_2. The only source net regions are the southern North Sea and the Baltic Sea. The two different regimes in the North Sea with the southern, nonstratified part being a source and the northern temporarily stratified part a sink for CO_2, have been described in the literature before (Thomas et al., 2004). However, there is a large interannual variability in the f CO_2 disequilibrium (Omar et al., 2010). This is reflected in the fact that studies based on different years find conflicting results regarding the direction of the flux (Schiettecatte et al, 2007, Thomas et al., 2004). This large interannual variability can also be found in our maps. During some years larger parts of the North Sea were a net source, while during other years also the southern North Sea acted as net sink.

MINOR COMMENTS

The text contains several typos and inadequate terminology.

We carefully read through the text again and corrected it.

P 2 L 5 : terms like coastal seas, coastal seas or continental shelves would be more adequate than "coasts"

We went through the text and changed the general term coasts to coastal seas or continental shelves

P8 L 12 : "calculating ocean acidification" is an awkward expression. You calculated pH from which you compute a trend. This trend is not necessarily negative (acidification). In some coastal areas an increase of pH has been reported, in other areas there is no trend (Duarte et al. 2013).

We changed 'calculating ocean acidification' to 'calculating pH'.

P 8 L16: "river moths" => river mouths

changed

P19L4: "eutrification" => eutrophication

changed

Legend of Figure 4. Is incorrect. The figures show deltafCO2 not fCO2

changed

P17L8 : "to validate this to validate this"

corrected

P19L5 : Can you provide a reference showing the effect of eutrophication on CO2 ?

Added references here

References:

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