Dear Anonymous Referee #1,

We very much appreciate your quick and constructive comments, which allowed us to improve the overall quality of our manuscript.

Anonymous Referee #1 Comment (AC): *I hope that the authors upload this data to either (or preferably both) of these databases, as it is a useful dataset for the community.*

Author Reply (AR): Be assured that we will upload this dataset to both SOCAT and LDEO databases.

AC: *I would expect more profiles to be available in the CS regions, have the authors looked for research cruises or glider based observations of MLD in the CS region? More in-situ comparisons (outside of E1 station) against the modelled MLD would be welcome, as this is a complex region for MLD approximation.*

AR: We considered your remark and made 2 additional figures (in supplement material, Fig. S1) to compare the observed MLD in the Celtic Sea and at E1 fixed station with Armor-3D L4 Analysis observation products provided by the Copernicus Marine Environment Monitoring Service (ex-MyOcean, http://marine.copernicus.eu/). The later are combined products from satellite observations (Sea Level Anomalies, Mean Dynamic Topography and Sea Surface Temperature) and in-situ (Temperature and Salinity profiles) on a ¼ degree regular grid in our study area, and modelled MLD computed from the MARS3D model. These figures clearly show the robust approximation of MLD by the model, particularly concerning the start and the end of stratification, despite a small overestimation of the modelled MLD compared to the observed MLD from the Armor-3D L4 Analysis products. In the revised manuscript, we give these details at the end of Section 3.2. and refer to the supplement material for the new figures supporting our modelled MLD.

AC: *I would recommend using a finer scaled wind product, such as those available from the ASCAT sensor (KNMI have a 25 km coastal product that may be of interest.) Or alternatively, a modelled wind speed from ECMWF or equivalent.*

AR: We followed your recommendation and now use 0.125° by 0.125° monthly average wind speed data from ERA-interim reanalysis as described in Section 3.2. of the revised manuscript.

AC: *Additionally, I am confused as to whether a correction has been made to account for the variability of monthly wind speed data. The air-sea gas exchange parametrisation used requires either high resolution data, or the intrinsic variability of monthly data to be accounted for.*

AR: We agree and now considered the intrinsic variability of monthly wind speed data in the revised manuscript using the four-time daily wind speed data from ERA-interim reanalysis. We applied the formulation given by Jiang et al. (2008) based on Wanninkhof et al. (2002) as explained in Section 3.5. of the revised manuscript.

AC: *There is very little data in the CL, NCS and IS, and an abundance of data in SCS and WEC. As the Ferry box measurements are also based in this region rich in SOCAT data, I am not convinced by the extrapolation of the MLR outside of the SCS and WEC, nor am I convinced that the low stated RMSE of the synthetic pCO_2 data derived from the MLR fully describes the errors that occur from extrapolating so far north (into CL, NCS and IS).*

AR: We acknowledge the referee #1 for suggesting the use of pCO_2 data from the LDEO database. Thanks to this suggestion we now have access to new pCO_2 data, particularly in IS and nCS, which consolidate our assumptions for the pCO_2 by MLR. These new in-situ pCO_2 data are represented by yellow dots on the updated Figure 8 of the revised manuscript. These new results support our extrapolation in these poorly studied areas and therefore support the main purpose of this study, which is to have access for the first time to pCO_2 estimates in this area where only few pCO_2 data are currently available. Figures 8, Figure 4 and Table 1 have been revised in the manuscript to include these new sources of in-situ pCO_2 data.

In addition, as suggested by reviewers #2 and #3, we used the methods from Lauvset et al. (2013) and Omar et al. (2007) to calculate our uncertainties on air-sea CO_2 fluxes, which greatly strengthen our findings. We can now argue strongly on the role of different provinces as significant sink or source of CO_2 over a full seasonal cycle. We are very grateful for this tremendous improvement of our manuscript. We now give an explanation of the method in Section 3.5. based on the work of Lauvset et al. (2013) and Omar et al. (2007). All fluxes in the revised manuscript and figures are now given with their respective calculated uncertainties.

AC: *The issues of the sharp boundaries between systems regions in figures 9, 11 and 12 are also problematic, perhaps another reviewer has come up with a solution for this? For* *example, I am surprised in the strength of the gradient between the nWEC and sWEC, in figure 11 between August and October.*

AR: We based our separation of the different provinces on a 10 year dataset of SST covering the entire shelf (Fig. 2), which provides robust estimates of the mean location of thermal fronts. We feel that the use of fixed boundaries allow a clear discussion of our datasets and direct comparison between the representative provinces. The sharp boundaries between permanently well-mixed and seasonally stratified systems can appear as surprising, especially between August and October. However, these sharp boundaries are a fact that we observed every years between sWEC and nWEC waters. To support this we made 2 new figures (Figure S3 in supplement material) showing a comparison between in-situ pCO_2 data acquired during 2 crossing performed in August and September 2014 between Roscoff and Cork (Ireland) (from a newly exploited Voluntary Observing Ship, the ferry *Pont-Aven*) and mean pCO_2 data along the ferry tracks calculated from our MLR from 2003 to 2013. We did not have access yet to the requested satellite and modeled products in 2014, which explained the choice of using monthly mean pCO_2 estimates instead of newly computed pCO_2 estimates from remotely sensed and modelled data. These two figures and the new in-situ pCO_2 data between Roscoff and Cork clearly show the presence of these sharp boundaries and we hope that these new data sufficiently support and illustrate this phenomenon. In the revised manuscript, we added an explanation and reference to these figures to support our choice of fixed boundaries at the end of Section 4.3.2.

References:

Jiang, L.-Q., Cai, W.-J., Wanninkhof, R., Wang, Y., Hüger, H.: Air–sea CO_2 fluxes on the US South Atlantic Bight: spatial and seasonal variability. J. Geophys. Res., 113, C07019, doi:10.1029/2007JC004366, 2008.

Lauvset, S. K., Chierici, M., Counillon, F., Omar, A., Nondal, G., Johannessen, T., and Olsen, A.: Annual and seasonal fCO_2 and air–sea CO_2 fluxes in the Barents Sea, Journal of Marine Systems, 113–114, 62-74, 2013.

Omar, A. M., Johannessen, T., Olsen, A., Kaltin, S., and Rey, F.: Seasonal and interannual variability of the air-sea CO_2 flux in the Atlantic sector of the Barents Sea, Marine Chemistry, 104, 203-213, 2007.

Wanninkhof, R., Doney, S. C., Takahashi, T. and McGillis, W. R.: The Effect of Using Time-Averaged Winds on Regional Air-Sea CO_2 Fluxes, in Gas Transfer At Water Surfaces, AGU Monogr. Ser., vol. 127, edited by M. A. Donelan et al., pp. 351–356, AGU, Washington, D. C., 2002.

Dear Anonymous Referee #2,

We very much appreciate your constructive comments, which allowed us to improve the overall quality of our manuscript.

Major issues:

Anonymous Referee #2 Comment (AC): *1. You have used a wind speed product from the NCEP/NCAR reanalysis, which has a spatial resolution of 2.5◦ by 2.5◦ . Your entire study area is quite small (5◦ latitude by 6◦ longitude), and the area where your data are collected and your algorithms are defined is much smaller than that. The wind speeds used thus have a very low resolution compared to the observations, which are all inside one NCEP/NCAR grid cell. I would urge the authors to check the effect on the results from using a higher wind speed product, for example the 0.125◦ by 0.125◦ monthly average data from ERA-interim. Using a higher resolution wind speed product may not affect the MLR results very much, but I suspect that it will matter quite a lot for the calculations of air-sea CO_2 fluxes.*

Author Reply (AR): We followed your recommendation and now use 0.125° by 0.125° monthly average wind speed data from ERA-interim reanalysis as described in section 3.2 of the revised manuscript.

AC: *2. You do not present any uncertainties for your flux estimates. Have you calculated uncertainties? If not, you will have to do this as otherwise you have no basis for your comparison of different provinces nor the comparison of different studies. See for example Lauvset et al. (2013) or Omar et al. (2007) for examples of such a calculation and evaluation. It would strengthen your results tremendously if you can show that your study area is a significant carbon sink over a full seasonal cycle.*

AR: As you suggested, we used the methods from Lauvset et al. (2013) and Omar et al. (2007) to calculate our uncertainties on air-sea CO_2 fluxes, which greatly strengthen our findings. We can now argue strongly on the role of different provinces as significant sink or source of CO_2 over a full seasonal cycle. We are very grateful for this tremendous improvement of our manuscript. We now give an explanation of the method in section 3.5. based on the work of Lauvset et al. (2013) and Omar et al. (2007). All fluxes in the revised manuscript and figures are now given with their respective calculated uncertainties.

AC: 3. *The manuscript is, as mentioned, well structured but is at times difficult to read and it is repetitive. The language would probably benefit from a thorough editing. The authors should carefully revise the entire manuscript for clarity and flow, and remove the many repetitions of certain findings/results/conclusions.*

AR: Based on the remarks of all four reviewers, we have clarified several findings/results/conclusions, which resulted in the removing of several repetitions. Our manuscript has also been corrected by a native English speaker for general improvement of the phrasing.

Minor issues:

AC: *Page 5642, Line 7: The gas transfer velocity coefficient is calculated, the wind speed is the remotely sensed data.*

AR: As recommended by other referees, we now use the wind speed instead of the gas transfer velocity in our algorithms.

AC: *Page 5642, Line 10: "relative uncertainties of 17 and 16 uatm". Relative to what?*

AR: We deleted the term "relative", which made no sense here.

AC: *Page 5647, Lines 13-15: Did you calculate the uncertainty yourself (using known uncertainties in your input data), or did you use the number given by Zeebe and Wolf-Gladrow? If the latter, then the ±5.8 is likely to be the lower end of the uncertainty estimate.*

AR: We used the number given by Zeebe and Wolf-Gladrow (2001). We acknowledge that this is likely the lower end of the uncertainty estimate and we mentioned it in the revised version of the manuscript and gave more details on the DIC/TA accuracies as follow: "The methods used for the analytical determinations of DIC and TA are described in details in Marrec et al. (2014) and gave accuracies of ± 2 and 3 µmol kg^{-1}, respectively. Thus, the computed values of pCO_2 from DIC and TA have uncertainties at the lower end of ± 6 µatm (Zeebe and Wolf-Galdrow, 2001)."

AC: *Page 5647, Lines18-19: "we estimated uncertainties relative to high-frequency pCO_2 measurements of ±5.2" This sentence is not very clear. Do you mean that the underway pCO_2 measurements have this uncertainty? Do you base this on comparison with the discrete data only, or have you also done some form of error analysis for the underway measurements? If* *the former, then 5.2 is probably not different from 5.8 and it would be more correct to say that the discrete and underway measurements give the same pCO_2 values.*

AR: We estimated the accuracy of the underway pCO_2 measurements based on comparison with approximately 300 pCO_2 data computed from DIC/TA as detailed in Marrec et al. (2014). In the revised manuscript, we followed your advice and clarified this section as follow: "Sensors were calibrated and/or adjusted based on these bimonthly discrete measurements as described in Marrec et al. (2014). Based on the comparison between high-frequency pCO_2 data obtained with a Contros HydroC/CO_2 FT sensor and bimonthly pCO_2 data calculated from DIC/TA, we estimated high-frequency pCO_2 measurements uncertainties at the lower end ± 6 µatm (Marrec et al., 2014), in the same range as computed values of pCO_2 from DIC and TA."

AC: *Page 5648, Line 26: It is not really binning when you regrid high-resolution data onto a coarser grid, but that is semantics. This terminology is used in other places in the manuscript also.*

AR: We now use the term grid instead of bin, excepted when we spoke about binning the SOCAT/LDEO data (true meaning in this case).

AC: *Page 5652, Line 4-5: It is not necessary to inform the reader that 1.7 times 10 is 17.*

AR: Agree, deleted.

AC: *Page 5652, Line 22: SOCATv2, which has data until 2011, contains 10.1 million measurements from more than 2660 cruises. The information you state here is for SOCATv1.5 which contains data only up to 2007.*

AR: Corrected to SOCATv2.

AC: *Page 5653, Line 8-9: "averaged over each defined province (Fig. 2, Sect. 2)". This needs more explanation, it sounds like you compared one average data point in each province to the algorithm based estimate.*

AR: We included a new figure in the supplement material (Fig. S2) and a short explanation on our methods in Section 3.4.: "We binned the SOCAT and LDEO data into 0.05°*0.05° grid and computed the mean monthly value in each grid cell. The performance of the model was obtained by comparing the mean observed and predicted monthly value in each cell (see

Figure S2 in supplement material and Fig. 2). For each province, the observed and predicted monthly mean based on this 0.05°*0.05° grid cells were plotted on Fig. 8.”

AC: *Page 5654, Line 7-8: Like for wind speed, there are higher resolution sea level pressure data products available.*

AR: We now use sea level pressure data products from the ERA-interim re-analysis for higher resolution and mentioned it in Section 3.5.

AC: *Page 5654, Line 22: use smaller than rather than inferior to*

AR: Corrected.

AC: *Page 5655, Line 1-5: These two sentences repeat the same information and are awkwardly written. Please revise.*

AR: We revised these sentences in the revised manuscript in the second paragraph of Section 4.1.: “The seasonal pCO_2 signal followed an average dynamic closed to a sinusoidal signal, with maximal values in fall and minimal values in spring, with transitional values in winter and summer. Therefore, the time variable TI, which is a sinusoidal function, contributed to more than half of the variability of the pCO_2 signal, highlighting the strong seasonality observed on this signal (Fig. 5a). “

AC: *Page 5655, Line 6: Have you checked what the result is if you do not include Chl a in your MLR? It would be interesting to see.*

AR: We made the computation and found that if we do not include Chl-a in our MLR, we obtained R^2 of 0.77 and 0.78 with RMSE of 17.3 µatm and 18.7 µatm, in sWEC and nWEC, respectively. We added the following sentence in the revised manuscript in Section 4.1.: “It is worth noting that when excluding Chl-a in the MLR, R^2 decreased by 0.03 and 0.05 and RMSE increased by 1.4 µatm and 2.1 µatm in sWEC and nWEC, respectively.”

AC: *Page 5656, Line 1: do you mean intra-annual?*

AR: We meant inter-annual variability, but we acknowledge that the sentence was not at the good place, which could be misleading. We moved this sentence 4 lines further.

AC: *Section 4.2: You are inconsistent in how you compare the observations (i.e. SOCAT) and the model (i.e. pCO_2 calculated using the algorithms). Always compare the model to the* *observations, not the other way around. In addition, it would be worthwhile to use some of the statistical tools for data-model comparison outlined in (Stow et al., 2009).*

AR: We agree and now compare the model prediction to the observations explicitly in section 4.2. We also included a new figure in supplement material (Fig. S2) for statistical data-model comparison as outlined in Stow et al. (2009).

AC*: Section 4.3.1 and 4.3.2: These subheadings do not adequately relate to what is presented in the sections. Both sections mostly discuss variabilities in Chl a, and not* pCO_2 *or air-sea* CO_2 *fluxes. Please revise such that the presented Chl a data is better related to the* pCO_2 *and flux results.*

AR: These subheadings are now called: "Seasonal and biogeochemical controls of pCO_2 in stratified / permanently well-mixed systems". We clarified the discussion and deleted repetitions.

AC: *Figures: Some of the figures, especially Figure 3, have quite poor resolution which makes them difficult to read. Please increase the resolution.*

AR: We increased the resolution as suggested.

References:

Lauvset, S. K., Chierici, M., Counillon, F., Omar, A., Nondal, G., Johannessen, T., and Olsen, A.: Annual and seasonal fCO_2 and air–sea CO_2 fluxes in the Barents Sea, Journal of Marine Systems, 113–114, 62-74, 2013.

Marrec, P., Cariou, T., Latimier, M., Macé, E., Morin, P., Vernet, M., Bozec, Y.: Spatio-temporal dynamics of biogeochemical processes and air–sea CO_2 fluxes in the Western English Channel based on two years of FerryBox deployment, J. Marine Syst., doi:10.1016/j.jmarsys.2014.05.010, 2014.

Omar, A. M., Johannessen, T., Olsen, A., Kaltin, S., and Rey, F.: Seasonal and interannual variability of the air-sea CO_2 flux in the Atlantic sector of the Barents Sea, Marine Chemistry, 104, 203-213, 2007.

Stow, C. A., Jolliff, J., McGillicuddy, D. J., Jr., Doney, S. C., Allen, J. I., Friedrichs, M. A. M., Rose, K. A., and Wallheadg, P.: Skill assessment for coupled biological/physical models of marine systems, Journal of Marine Systems, 76, 4-15, 2009.

Dear Anonymous Referee #3,

We very much appreciate your constructive comments, which allowed us to improve the overall quality of our manuscript.

Major comments:

Anonymous Referee #3 Comment (AC): *The extrapolation from a very specific area in the English Channel up to the Celtic and Irish Sea seems to me not straight forward. I would like to see more and/or better arguments that allow the extrapolation from the English Channel to the other regions. Alternatively I would suggest concentrating on the Channel area.*

Author Reply (AR): This issue was also raised by reviewer #1 and he advised us to use pCO_2 data from the LDEO database. Thanks to his suggestion we now have access to new pCO_2 data, particularly in IS and nCS, which consolidate our comparison between observed and modeled pCO_2 in other region than the WEC. These new in-situ pCO_2 data are represented by yellow dots on the updated Fig. 8 of the revised manuscript. These new results greatly enhance our extrapolation in these poorly studied areas and therefore support the main purpose of this study, which is to have access for the first time to pCO_2 estimates in this area where only few pCO_2 data are currently available. Table 1 and Figure 4 in the revised manuscript have also been updated to include these new sources of in-situ pCO_2 data.

AC: *There is no discussion of uncertainties. Especially when using an MLR based algorithm to calculate fluxes and compare them to other studies, an estimation of uncertainties would be very helpful. I guess the uncertainty is quite substantial, what can be already seen in Figure 5C where deviations between measurements and estimated pCO_2 reach values of +- 50 µatm. This makes it hard to resolve interannual variability.*

AR: As also suggested by reviewer #2, we have now performed a thorough error estimation based on the work by Lauvset et al. (2013) and Omar et al. (2007) to calculate our uncertainties on air-sea CO_2 fluxes, which greatly strengthen our findings. We acknowledge both reviewer #2 and #3 for this suggestion. We can now argue strongly on the role of different provinces as significant sink or source of CO_2 over a full seasonal cycle. We now give an explanation of the method in Section 3.5. based on the work of Lauvset et al. (2013) and Omar et al. (2007). As you pointed out, there can still be large differences between the estimated and observed pCO_2 values in the case of extreme events from one year to the other as observed on Fig. 5c for the well-mixed sWEC. We discuss further this issue in the revised manuscript in Section 4.1. However, as mentioned above, the computed uncertainties for air-sea CO_2 fluxes plotted on Fig. 12 allowed us to assess the inter-annual variability in stratified regions. All fluxes in the revised manuscript and figures are now given with their respective calculated uncertainties.

Minor comments:

P. 5646

AC: *l. 4ff: the authors write "five key regions" but in Fig. 2 one can see 6.*

AR: It was "six key regions", we modified it in the revised version.

AC: *l. 6ff: I was wondering if this should go to the methods part*

AR: We deleted this sentence, as we previously mentioned the same topic at the end of the introduction. As noticed by reviewer #2, some repetitions occurred in the manuscript and we deleted this sentence for clarification.

AC: *l. 22/Fig 1.: the shelf break is mentioned in the text but not shown in Fig.1*

AR: The location of the shelf break is materialized by the 200m isobaths shown on Fig. 1. We added: "(Fig. 1, south-western area)", in Section 2. of the revised manuscript.

AC: *P. 5647 l. 1-24: First I would like to see a little bit more information about the pCO_2 data. There is no information about the performance of the Contros sensor. And without careful calibrations uncertainties in pCO_2 better than 5 µatm are hard to achieve and even for 5.8 µatm one have to spend a lot of effort. Furthermore there is no uncertainty given of DIC and alkalinity measurements. To my knowledge the uncertainty of 5.8 µatm comes just from the calculation and one have to take the measurements in to account, too.*

AR: A detailed description of the Contros sensor performance based on a comparison with approximately 300 DIC/TA discrete samples is discussed in Marrec et al. (2014) as well as the analytical methods used for DIC/TA determinations and their respective accuracies. We now added these accuracies and some details on the Contros performance in the revised manuscript and refer to Marrec et al. (2014) for more details as follow: "The methods used for the analytical determinations of DIC and TA are described in details in Marrec et al. (2014) and gave accuracies of ± 2 and 3 µmol kg-1, respectively. Thus, the computed values of pCO_2 from DIC and TA have uncertainties at the lower end of ± 6 µatm (Zeebe and Wolf-Galdrow, 2001). Sensors were calibrated and/or adjusted based on these bimonthly discrete measurements as described in Marrec et al. (2014). Based on the comparison between high-frequency pCO_2 data obtained with a Contros HydroC/CO_2 FT sensor and bimonthly pCO_2 data calculated from DIC/TA, we estimated high-frequency pCO_2 measurements uncertainties at the lower end ± 6 µatm (Marrec et al., 2014), in the same range as computed values of pCO_2 from DIC and TA."

AC: *Furthermore the authors state that the pCO_2 sensor was only used since April 2012. Before that date the pCO_2 data are based on a bimonthly sampling program, but in Fig. 5 there are data for every month of the year. Did the authors interpolate between the sampling campaigns?*

AR: We apologize for the misunderstanding; "bimonthly" meant that we performed samplings twice a month. We added this clarification in the revised manuscript.

AC: *P. 5649 l. 4-6: the authors state that they validated the satellite SST with the measured one. Please state the uncertainty and not only the r2.*

AR: We agree, in the revised manuscript we have now: "A validation between monthly in-situ SST and associated satellite SST showed a robust correlation (R^2=0.97, N=448, $p<0.001$ and RMSE=0.43°C)".

AC: *P. 5650 l. 11-15: A 2.5◦ x 2.5◦ grid seems very coarse. There are other products with a finer resolution: Modern-Era Retrospective Analysis for Research and Applications (MERRA), it comes at a resolution of 2/3◦ x 1/2◦ x 1 h.*

AR: We agree, we now use wind speed products from the ERA-interim re-analysis project produced by the European Center for Medium-Range Weather Forecasts (ECMWF) with a resolution of 0.125° by 0.125° as suggested by reviewer #2. See Section 3.2.

AC: *The wind speed is used to calculate the transfer coefficient k (after Nightingale, 2000). Since there is a clear relationship between k and wind speed I'm wondering if one could just use the wind speed instead of k. In this case one is independent from the parameterization.*

AR: Agree. We now use the wind speed instead of the gas transfer coefficient k.

AC: *P. 5652 l. 3ff: To calculate the atmospheric increase one could use the data record from Mace Head Observatory. The data are used in the manuscript anyway. When I plot their data over the last 10 years I got an increase of 2 µatm/year.*

AR: We acknowledge that calculating the atmospheric increase using pCO_2 data from Mace Head observatory is a better approach. When plotting xCO_2 (ppm) from Mace Head Observatory from 2003 to 2014, we also obtained an increase of 2.0 ppm/year in the atmosphere. When we consider pCO_2 (µatm) growth rate computed from water pressure vapor (computed with SST and a default salinity of 35.3) and atmospheric pressure, we obtained an increase of 1.8 µatm/year. We used this value as explained in Section 3.3. of the revised manuscript.

AC: *P. 5653 l. 22ff: The Wanninkhof (1992) k-parameterization is known to overestimate the fluxes. The authors use it to show the range of the flux estimates. As mentioned above I would prefer a thorough error estimate than using different parameterizations of k.*

AR: As mentioned above and suggested by both reviewer #2 and #3, we have now performed a thorough error estimation based on the work by Lauvset et al. (2013) and Omar et al. (2007) to calculate our uncertainties on air-sea CO_2 fluxes, which greatly strengthen our findings. We acknowledge both reviewer #2 and #3 for this suggestion. We can now argue strongly on the role of different provinces as significant sink or source of CO_2 over a full seasonal cycle. We now give an explanation of the method in Section 3.5. based on the work of Lauvset et al. (2013) and Omar et al. (2007). All fluxes in the main text and figures of the revised manuscript are now given with these newly computed uncertainties. However, in Table 4, we chose to keep results from other parametrization, which, we think, might be useful for the scientific community that needs these other parametrizations. This is explained in the caption of Tables 3 and 4.

AC: *P. 5654, l. 26 – P. 5655, The authors draw the conclusion that TI has the highest contribution because the system is biologically driven. Even if this might be right, but every parameter that is variable on seasonal time scales can be the driver (e.g SST, MLD). I think this is actually the challenging task (of algorithms/models) to identify the parameter that drive the observed patterns. I suggest not drawing this conclusion. (this topic appears more often in the rest of the manuscript)*

AR: We agree and we rephrased in Section 4.1. as also suggested by reviewer #2.

AC: *P. 5658 l.13/14: ". . . SOCAT data fitted well with computed pCO_2. . ." is not a quantitative description. Please add a number.*

AR: Following reviewer #2 recommendation we included new figures in supplement materials to strengthen the validation of our algorithms and provide a qualitative comparison between observed and modeled data (see Fig. S2 in supplement materials for more details). In the revised manuscript we refer to this supplement materials for more qualitative comparison in Section 4.2.: "For the nWEC (Fig. 8d), the modelled data followed the main features of the seasonal cycle described by the observed data and are in relatively good quantitative agreement (see Fig. S2 in supplements for more details)."

AC: *P. 5663 l. 6/7: Following my concerns from above I'm not convinced that the presented MLR really resolves the inter-annual and decadal variability*

AR: As explained above, there can still be large differences between the observed and estimated pCO_2 values in case of extreme events from one year to the other as observed on Fig. 5c for the well-mixed sWEC. We pointed out this issue in the revised manuscript at the end of the 4th paragraph of Section 4.1. However, as mentioned above, the computed uncertainties for air-sea CO_2 fluxes plotted on Fig. 12 allowed us to compare the inter-annual variability in other regions. All fluxes in the revised manuscript and figures are now given with their respective calculated uncertainties.

AC: *Table 1 and 2 should be switched, because Table 2 is mentioned before Table 1.*

AR: Done.

AC: *Table 1: What has happened to a4? I guess te authors have a good reason for it, but would be nice to know.*

AR: We apologize, this was just a typing mistake, Table 1 (now 2) has been corrected with a4.

AC: *Figure 3: The figure captions should be a bigger.*

AR: Corrected.

AC: *Figure 6: In panel F the residuals look like increasing. Can this be due to a wrongly estimated atmospheric increase?*

AR: We do not observe a significant increase of the residuals. As mentioned above, the atmospheric pCO_2 increase of 1.8 µatm/year is included in our computations as explained in Section 3.3 of the revised manuscript.

AC: *Figure 7: Having dashed lines every 6 month would make the figure easier to read. However in the panel for the sWEC during spring 2012 the divergence between observed and estimated CO_2 flux is big! What is the reason for it?*

AR: We agree and added dashed lines every 6 months on Fig. 7. We agree that there is a divergence in 2012 and this is in line with the difficulty to model extreme events occurring one year (2011) in the sWEC but not the others (2012 and 2013) (Fig. 5). As mentioned above, in the sWEC the model is not as robust as in other provinces. We agree that this is a limitation of our model and discussed it in Section 4.1. of the revised manuscript. However, the new computations of modeled air-sea CO_2 fluxes uncertainties are robust and we now feel confident we can discuss inter-annual variability in all other regions.

AC: *Figure 9, 10: I think they could go to the appendix.*

AR: We chose to put the physical parameters as SST (Fig. S4) and MLD (Fig. S1) in supplement. We kept Chl-a as a main figure (Fig. 9 now) since reviewer #2 recommended to relate the seasonal variability of Chl-a and pCO_2 in Section 4.3.1. and 4.3.2.

AC: *Figure 10, 11, 12: Why are there no data for December?*

AR: Because no monthly satellite Chl-a data were available in December. This is now mentioned in the caption of Fig. 10.

Technical comments:

AC: *P. 5651, l. 20: for consistency change "adjusted-R^2" to "adjusted R^2"*

AR: Done.

AC: *P. 5653, l. 3: I guess the authors meant Fig. 4 instead of Fig. 5.*

AR: Agree, we modified in the revised manuscript.

AC: *P. 5655, l. 2: close instead of closed*

AR: Corrected.

AC: *P. 5664, l. 21: there is a word missing or an "and" too much*

AR: Agree, we rephrased.

AC: *Table 4, l. 2: add the word "mixed" between permanently and provinces; l. 4: k or K for the transfer coefficient*

AR: Corrected.

References:

Lauvset, S. K., Chierici, M., Counillon, F., Omar, A., Nondal, G., Johannessen, T., and Olsen, A.: Annual and seasonal fCO_2 and air–sea CO_2 fluxes in the Barents Sea, Journal of Marine Systems, 113–114, 62-74, 2013.

Omar, A. M., Johannessen, T., Olsen, A., Kaltin, S., and Rey, F.: Seasonal and interannual variability of the air-sea CO_2 flux in the Atlantic sector of the Barents Sea, Marine Chemistry, 104, 203-213, 2007.

Dear Anonymous Referee #4,

We very much appreciate your constructive comments, which will allow us to improve the overall quality of our manuscript.

General comments:

Section 1 (Introduction):

Anonymous Referee #4 Comment (AC): *Please include a few sentences outlining any previous data-based work that has focused on advancing our understanding of ocean carbon variability in the NWES. Also include a sentence on how this study builds on previous work in the NWES, or if this is the first, clearly state it.*

Please include a sentence outlining the rest of the paper.

Author Reply (AR): We included all your suggestions, see Section 1.

Section 3.1. (FerryBox dataset):

AC: *Page 5646, line 14: Please provide more details on your stated uncertainty in calculated pCO_2 via DIC/TA.*

AR: We added the following explanation in the revised manuscript and refer to Marrec et al. (2014) for more details on the analytical methods used to measure DIC/TA: "The methods used for the analytical determinations of DIC and TA are described in details in Marrec et al. (2014) and gave accuracies of ± 2 and 3 µmol kg^{-1}, respectively. Thus, the computed values of pCO_2 from DIC and TA have uncertainties at the lower end of ± 6 µatm (Zeebe and Wolf-Galdrow, 2001)."

Section 3.3. (Development of pCO_2 algorithms):

AC: *In section 2 (study region), you state the frontal zone separating the sWEC/nWEC oscillates and could be precisely locate from data. I suggest you use this ability to create a dynamically shifting front (via SST) from which measurements are partitioned prior to the MLR. This might also reduce your current pCO_2 discontinuity at the region boundaries.*

AR: We based our separation of the different provinces on a 10 year dataset of SST covering the entire shelf (Fig. 2), which provides robust estimates of the mean location of thermal fronts. We feel that the use of fixed boundaries allow a clear discussion of our datasets and direct comparison between the representative provinces. The sharp boundaries between permanently well-mixed and seasonally stratified systems can appear as surprising, especially between August and October. However, these sharp boundaries are a fact that we observed every year between sWEC and nWEC waters and that can occur elsewhere on the shelf. To support this we made 2 new figures (Fig. S3 in the supplement material) showing a comparison between in-situ pCO_2 data acquired during 2 crossing performed in August and September 2014 between Roscoff and Cork (Ireland) (from a newly exploited Voluntary Observing Ship, the ferry Pont-Aven) and mean pCO_2 data along the ferry tracks calculated from our MLR from 2003 to 2013. We did not have access yet to the requested satellite and modeled products in 2014, which explained the choice of using monthly mean pCO_2 estimates instead of newly computed pCO_2 estimates from remotely sensed and modeled data. These two figures and the new in-situ data between Roscoff and Cork clearly show the presence of these sharp boundaries. Moreover, region boundaries represent the shifting area of thermal fronts. As we could not estimate pCO_2 using our algorithms in frontal zones, such an approach appeared suitable. We hope that we now provide enough evidence for the choice of our different provinces. We added a short statement at the end of Section 2 for the choice of fix boundaries and added the discussion above with reference to the new figures of the supplement material at the end of Section 4.3.2. in the revised manuscript.

AC: *Page 5652, line 4: Please state whether this increase in ocean surface pCO_2 (1.7 uatm/yr) is a global or regional estimate. Also please include a sentence discussing if this estimate is representative of your study region.*

AR: Following reviewer #3 suggestion, we computed an atmospheric pCO_2 increase of 1.8 µatm/year based on our Mace Head (Ireland) xCO_2 dataset. Based on the paper by Signorini et al. (2013), we assumed that ocean surface pCO_2 increase is trending at the same pace as the atmosphere pCO_2. Thus, we consider an increase of seawater pCO_2 of 1.8 µatm/year representative of our study region. This is now explained in the revised manuscript at the end of the Section 3.4.

Section 4.1. (Performance of MLR):

AC: *I'm concerned about the significance of time (TI) in your regression model. This is a non-physical parameter which captures up to 50% of the observed variability, thus indicating* *key physical information is missing from your MLR (be it salinity, nutrients, ect). Could you please include in table 1 a regression model where TI was not included as a predictor parameter, and discuss what it means if temperature and biological indicators can only capture ~50% of the pCO_2 variability.*

AR: We included the variable TI at the end of Table 2 in order to observe the relative importance of each physical and biological variable in the MLR without TI. We acknowledge that it allowed the reader to better understand the respective role of each of these variables. We now explain why the variable TI captures up to 50% of the observed variability in the second paragraph of Section 4.1.

AC: *Please include a sentence discussing why K is used as a predictor for ocean surface pCO_2. Also remember that K is calculated via temperature, so K and T are not independent variables. This may in part explain the observed difference between your Temp co-efficient and the Takahashi SST/pCO_2 relationship.*

AR: We now use wind speeds instead of K in the MLR and only in sWEC following suggestion of other reviewers.

AC: *Page 5656 line 17: I disagree that the distribution of residuals in the nWEC looks more homogenous. To strengthen your claim you could colour the points in Fig 6D to indicate sample year (or Latitude).*

AR: We followed your advice and colored the points in Fig. 6c and 6d to indicate sample years. Our statement is now well supported by the figures since the residuals during year 2011 in the nWEC were clearly more homogenous than in the sWEC. We modified the text specifically for year 2011 in the revised manuscript in the 3rd paragraph of Section 4.1.

Section 4.2.:

AC: *While I believe empirical approaches are extremely valuable in predicting ocean carbon variability in data-limited regions, they do have limitations. From Figure 8, it seems your MLR predictions compare well to the SOCATv2 measurements in the sCS. In the IS and nCS however, you have no (or very few) pCO_2 measurements from which to justify your pCO_2 predictions are accurate. You should state that you have no way of quantifying uncertainties in your pCO_2 predictions beyond the two WEC regions. One possibility to strengthen your approach would be to look at correlation length scales in you predictor variables. If your* *predictor variables are highly correlated between the WEC and the IS and nCS, it suggests pCO_2 concentrations could be predicted from your WEC model in these regions.*

AR: This issue was also raised by reviewer #1 and he advised us to use pCO_2 data from the LDEO database. Thanks to his suggestion we now have access to new pCO_2 data, particularly in IS and nCS, which consolidate our comparison between observed and modeled pCO_2 in other regions than the English Channel. These new in-situ pCO_2 data are represented by yellow dots on the updated Fig. 8 of the revised manuscript. These new results greatly enhance our extrapolation in these poorly studied regions and therefore support the main purpose of this study, which is to have access for the first time to pCO_2 estimates in this area where only few pCO_2 data are currently available. Table 1 in the revised manuscript has also been updated to include these new sources of in-situ pCO_2 data. We also included a new figure in supplement material (Fig. S2) for statistical data-model comparison as recommended by reviewer #2.

AC: *Was the anthropogenic increase factor (eq 4) included in these pCO_2 predictions? If so, please state these estimates are representative of the year 2012, and discuss why you observe any trends (as is evident in the nWEC).*

AR: For a comparison of the observed and modeled pCO_2 over three years in the WEC, we used July 2012 as a reference month. This allowed a direct estimation of the performance of our model without any impact of the anthropogenic increase factor. However, for the ten years estimates in other regions of the shelf, the regional pCO_2 anthropogenic increase of 1.8 µatm $year^{-1}$ was included in the data using July 2012 as the reference year. This explains why on Fig. 8 the estimated regional anthropogenic signal might be visible over a decade in several provinces. We clarified the revised manuscript at the end of Section 3.3.

Specific comments :

AC: *Page 5643, line 1: Please rephrase for easier reading. I suggest, 'Continental shelf seas form a complex interplay between the land, ocean and atmosphere, hosting a multitude. . .'*

AR: Done.

AC: *Page 5644, line 6: Neural network is the name given to the family of statistical learning models of which the self-organising map is one of. Please correct this sentence.*

AR: We agree and we corrected the sentence as follow: "More complex neural networks techniques using self-organizing map have also given promising results."

AC: *Page 5646, line 1: I suggest rephrasing this sentence for easier reading. Perhaps 'The WEC forms part of the North-West European continental shelf - one of the world's largest margins.'*

AR: Done

AC: *Page 5646, line 7: I suggest removing 'by' from 'depths and by intense'*

AR: Done

AC: *Page 5652, line 13/14: Please remove 'the' from 'the SD', and include 'the' in 'of p over study period'.*

AR: Done

AC: *Page 5656, line 9/10: Suggest changing 'on' to 'in'*

AR: Done

AC: *Page 5663, line 5: Please change 'confirm' to 'further supports'*

AR: Done

AC: *Page 5664, line 20: Please include time (TI) in the list of predictor variables.*

AR: Done

AC: *Fig 10,11,12: What happened in December?*

AR: Because no monthly Chl-a data available in December. This is now mentioned in the caption of Figure 10.