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 4^{th} 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.