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Interactive comment on "Dynamics of air–sea CO_2 fluxes in the North-West European Shelf based on Voluntary Observing Ship (VOS) and satellite observations" *by* P. Marrec et al.

Anonymous Referee #3

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The manuscript from Marrec et al presents an interesting dataset of pCO2 measurements obtained during 3 years onboard a ferry crossing the Western English Channel regularly. They used a three year dataset of measurements of both, direct pCO2 measurements and calculated pco2 measurements based on IDC and TA samples, to establish an MLR algorithm to estimate surface water pCO2 values in the western European continental shelf from satellite observations.

The three years of measurements between UK and France cover a highly variable area and the dataset will contribute to better understanding of the variability of the carbonate system in this continental system. The manuscript is well structured and it is easy to follow the authors through the manuscript. However, it has a high descriptive part and the scientific conclusions are mainly based on an algorithm that is extrapolated to regions where its validity is not clear. I would like to see this manuscript published in Biogeosciences, but it needs some revisions:

My major concerns are:

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.

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 pCO2 reach values of +- 50 μatm. This makes it hard to resolve interannual variability.

In addition to these major concerns I have some minor comments:

P. 5646

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

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

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

P. 5647

l. 1-24: First I would like to see a little bit more information about the pCO2 data. There is no information about the performance of the Contros sensor. And without careful calibrations uncertainties in pCO2 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.

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Furthermore the authors state that the pCO2 sensor was only used since April 2012. Before that date the pCO2 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?

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.

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.

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.

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.

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.

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)

P. 5658

l.13/14: "... SOCAT data fitted well with computed pCO2..." is not a quantitative description. Please add a number.

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.

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

Table 1:

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

Figure 3:

The figure captions should be a bigger.

Figure 6:

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

Figure 7:

Having dashed lines every 6 month would make the figure easier to read. However,

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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?

Figure 9, 10:

I think they could go to the appendix.

Figure 10, 11, 12:

Why are there no data for December?

Technical comments:

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

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

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

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

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

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