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Interactive comment on “Dynamics of air–sea CO₂ fluxes in the North-West European Shelf based on Voluntary Observing Ship (VOS) and satellite observations” by P. Marrec et al.

Anonymous Referee #4

Received and published: 25 June 2015

Dear Editor,

The manuscript ‘Dynamics of air-sea CO₂ fluxes in the NW European Shelf based on Voluntary Observing Ship (VOS) and satellite observations’ by Marrec et al. provides important insights into ocean carbon dynamics and air-sea CO₂ exchange in the North-West European Shelf (NWES). The work first subjectively defines 5 geographical regions in the NWES based on seasonal water column characteristics, and then uses these regions to partition VOS and discrete pCO₂ measurements into the north and south Western English Channel (WEC) regions (samples collected between 2011 through to 2014). Multiple linear regressions were then derived using the pCO₂ measurements in combination with satellite/model derived SST, Chl-a, PAR, MLD, K and a

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time variable TI. The empirical algorithms were tested using SOCATv2 data and finally applied to the greater NWES region to predict monthly ocean surface pCO₂ concentrations and air-sea CO₂ fluxes from 2003 through to 2014.

Overall, this manuscript is well written and presents interesting results that are important for a wide scientific community. Their methodology is well thought out, both in deriving and testing their MLR algorithms. I am however slightly concerned about the importance of time (TI) in their regression and their extrapolated pCO₂ predictions.

I recommend the manuscript be published in biogeosciences after addressing the following minor issues.

General comments:

Section 1 (Introduction):

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.

Section 3.1 (FerryBox dataset):

Page 5646, line 14: Please provide more details on your stated uncertainty in calculated pCO₂ via DIC/TA.

Section 3.3 (Development of pCO₂ algorithms):

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 pCO₂ discontinuity at the region boundaries.

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Page 5652, line 4: Please state whether this increase in ocean surface pCO₂ (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.

Section 4.1 (Performance of MLR):

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 pCO₂ variability.

Please include a sentence discussing why K is used as a predictor for ocean surface pCO₂. 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/pCO₂ relationship.

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).

Section 4.2 (Spatial and temporal extrapolation):

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) pCO₂ measurements from which to justify your pCO₂ predictions are accurate. You should state that you have no way of quantifying uncertainties in your pCO₂ 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 pCO₂ concentrations could be predicted from your WEC model in these regions.

Was the anthropogenic increase factor (eq 4) included in these pCO₂ 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).

Specific comments:

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...'

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.

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.'

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

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

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

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

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

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

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