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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 #1

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The Dynamics of air-sea CO2 fluxes in the N-W European Shelf features very thorough and good observational science, but some issues in the extrapolation of these observations to the greater European Shelf, it is for this reason I recommend that the article is revised prior to being accepted.

Sections between the introduction and 3.2 (Satellite and other environmental data) sufficiently described the research. I found that the methodology of Ferrybox sampling, and subsequent bi-monthly calibration more robust that the majority of observations contained within the LDEO and SOCAT database, and this is borne out in the small magnitude of measurement uncertainty stated by the authors. 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. The choice of satellite products is good, the NEODAAS OC3 Chl a product is well known, and usually accurate, and the issues of case 1 vs case 2 is, (as the author states) unavoidable in these on-shelf environments. I also have no issues regarding the SST product. I am not accustomed to the MLD product, as I would usually derive MLD from Argo or CTD profiles. However, I appreciate that these data sources may not be available in the nWEC and sWEC. 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. The choice of wind speed data puzzled me, the NCEP 2.5 degree product is very coarse for this small study region. 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. The issue with using a 2.5 degree product is clearly seen in the striations seen in most of the air-sea flux figures. 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. The very common problem of sampling heterogeneity with the SOCAT database is shown in Figure 4. 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 pCO2 data derived from the MLR fully describes the errors that occur from extrapolating so far north (into CL, NCS and IS). I also note in figures 9,11 and 12, the MLR technique results in sharp boundaries between the biogeochemical regions, that likely do not exist, with these boundaries most noticeable in the CL, NCS and IS. I would suggest removing the CL, NCS and IS regions entirely from this study, and focusing on the WEC and sCS regions (where sufficient data exists). I appreciate that this removes

one of the aims of the paper- to quantify the entire SW shelf, but based on the in-situ data used in the paper, I do not believe this is possible. Perhaps more data for the CL, NCS and IS can be found in the LDEO '14 database, or by contacting groups working in this region? The issues of the sharp boundaries between 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. At present, I would like to see this a) lack of data in the CL,NCS and IS; b) sharp boundary problem; resolved before conclusions on the total air-sea fluxes are stated. For this reason, I will not go into further discussion on the conclusions. If no more in-situ data can be found for the CL, NCS and IS, then I'd either like to see additional arguments put forward as to the validity of extrapolating the technique this far North, or the removal of these regions from the study.

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