Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2017-33-RC2, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 3.0 License.

Interactive comment on "The Potential of using Remote Sensing data to estimate Air–Sea CO_2 exchange in the Baltic Sea" *by* Gaëlle Parard et al.

Anonymous Referee #2

Received and published: 21 June 2017

The study by Parard et al. focuses on the very important and interesting aspect of the present day oceanography, namely on the role of coastal and marginal seas in the global carbon cycle. There is an ongoing debate in the scientific literature if these regions act as sink or source of CO2. Parard et el. propose to use for the studies on CO2 fluxes in coastal regions remote sensing tools. In the revised manuscript they present results from the Baltic Sea. The worldwide context (though poorly presented in the paper) and importance of the problem raised by the authors places, in my opinion, the manuscript within the scope of interests of Earth System Dynamics. However, the manuscript should be first improved in several aspects mentioned below and thus requires further revision.

General comments: 1). The goal of the presented manuscript is ambiguous. It is unclear what is the novelty in the presented research especially in the context of previous publications of the authors in the field. Please specify clearly what is the added value of the presented study. 2). The importance of the study could be better presented in the worldwide context of carbon cycling and role of the coastal and marginal seas. 3). The manuscript should contain better review on the pCO2 fields and CO2 fluxes reported for the Baltic Sea in the recent years. There were several papers published on that recently. Important contribution to that issues are also regular measurements of pCO2 made on the VOS line operated by IOW between Germany and Finland. This comment refers to the entire manuscript but especially to the introduction section where only the paper by Wesslander et al. (2010) is mentioned in that context. 4). The methods used in the study are not well described and documented. It is relatively clear how the winds data were established. However it is unclear how the remote sensing data are transferred into pCO2. I am aware of the ongoing debate on the obstacles with the application of remote sensing in the Baltic Sea. Since I am not an expert on remote sensing I do not want to judge on that. However, at least the limitations of the remote sensing methods should be discussed in the manuscript in the context of pCO2 calculations. 5). The CO2 flux across the air/sea interface is a function of the wind speed and pCO2 difference between seawater and the atmosphere. Both these parameters are critical for accurate CO2 flux estimations. It would be meaningful to demonstrate that the pCO2 fields obtained from the remote sensing data are correct. This could be done by comparison with the available pCO2 measurements. 6). Experimental data suggest that there are two minima in seasonality of pCO2 in the Eastern Gotland Basin, which are related to the spring bloom and mid-summer N2 fixation. Why this is not seen in the modelled pCO2 (Fig. 2)? Please comment on that. 7). How the accuracy in the determination of pCO2 fields influence the calculated CO2 fluxes? The latter, as it appears from Fig.8, are burdened with a relatively high uncertainty. 8). Presenting the results as annual means is not very informative. Fig. 3b gives the impression that seawater is permanently undersaturated with CO2 (seawater pCO2 lower from the atmospheric one). This is misleading. 9). The entire manuscript requires careful editing.

CC BY

Now it contains number of technical defects. As a part of this work English could be also improved. However I leave this as a suggestion only as English is not my mother tongue.

Minor comments: 10). It would be meaningful to add a map of the Baltic Sea showing the places mentioned in the manuscript. 11). Page 2, line 25. Not the best choice of references – paper by Omstedt et al. 2009 does not refer to the global scale 12). Please add how big the river runoff is (page 2, line 32) 13). Page 3, line12. Mixed layer depth is not always on 60m. 14). Section 3.2.1. The discussion on seasonal and annual means are mixed up in the text. This causes that it is difficult for the reader to follow the text. 15). Page 6, line22. I think it should be Fig. 3. 16). Page 6, line 30. Fig. 3 does not show seasonality 17). Page 6, line 30. Outgassing can happened only when seawater pCO2 is higher from the atmospheric one. It is impossible in summer in open sea. 18). Page 7, line 7. Please name these different satellite products. 19). Page 7, line 12. “flux from the coastal region” – this suggests flux in only one direction – please rephrase. 20). Page 7, line34. What data this refers to? Fig. 3 shows data for GF also for the period before 2008. 21). Page 8, line 2. Should be these 22). Page 8, line 15. Wrong unit of the wind speed 23). Page 8, line 16. “in function of the basin” – unclear. 24). Page 8, line 26. Please rephrase 25). Page 9, line 1. Over or in the marginal seas 26). Page 9, line 7. Please reduce the number of figures after comma. 27). Page 9, line 11. Please correct citation. 28). The abbreviations of the different water basins (GB, CB, GF, SB, BS) should be explained when first time used in the paper 29). Fig. 3a, name data 1, data 2 etc.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2017-33, 2017.