

Interactive comment on “The Potential of using Remote Sensing data to estimate Air–Sea CO₂ exchange in the Baltic Sea” by Gaëlle Parard et al.

Anonymous Referee #1

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The manuscript by Parard et al. is on a very interesting topic – the role of coastal waters (the whole Baltic Sea belongs to them) in the carbon cycle and using remote sensing in determining the role. It is obvious that most of the carbon processing is taking place in coastal waters (where the amount of carbon in different forms is the highest). On the other hand this is also the area where remote sensing has the biggest problems due to optical complexity of the waters, atmospheric correction issues (the assumptions used in ocean remote sensing are not valid in coastal waters) as well as the adjacency effects present close to the shores. The Baltic Sea is a particularly complicated study object due to its low reflectance (high concentration of CDOM) and low sun angles during most of the year. Therefore, the remote sensing part is the weakest link in this study.

First of all the remote sensing methodology part is not well described in the manuscript in order to be able to understand the potential errors of the methodology used. It is understandable from the Authors point of view that if they have published already two similar studies where the methodology was described in more detail then they kind of assume that the methodology works. Moreover, plagiarism detection software picks it up very easily if the methods description is repeated in several papers. On the other hand each manuscript has to be self-consistent. It is important from the readers perspective to understand what has been done without digging into databases, downloading relevant papers, and learning what kind of methodology was used to produce the results.

We appreciate the closeness with which the reviewer examined our work. As noted, the pCO₂ used in this paper to compute the air-sea CO₂ flux is the one described in the paper Parard et al, 2016. We initially developed the methodology more but the plagiarism software did not allow us to submit until we had removed too much of that part to keep any of it. In an updated version of the manuscript we would add enough information of the method to allow the paper to be self-consistent.

Digging into the databases and reading the previous papers by Parard et al on the same topic revealed that there are serious issues with the remote sensing products used. It is said for Chl-a that SeaWiFS and MODIS monthly means were used. It is not said which algorithm was used as the reference added there is about AVHRR not these two satellites. One can assume that OC4 type blue-green band ratio was used as this kind of algorithms are standard for these sensors. It has been known for many decades that blue-green ratios do not work in coastal and inland waters, especially in CDOM-rich waters like the Baltic Sea. This has been demonstrated by Darecki and Stramski 2004, Reinart and Kutser 2006, Ligi et al. 2017 and many others. The latest study used mainly modelled data. Meaning perfect reflectance values in that sense that there were no atmospheric correction errors. Still, the latest version of OC4 tuned for the Balti Sea gave correlations that were close to zero. The Copernicus Marine Environment Monitoring Service (CMEMS) validated the MODIS Chl-a algorithm for the Baltic Sea and got correlations $r^2=0.2$. The new CMEMS product is based on a neural

network approach but still their validation results show the correlation with in situ data is $r^2=0.2$. This means that one of the main input products used by the Authors has very little to do with actual chlorophyll-a values in the Baltic Sea.

The second product used by the Authors is CDOM. Again, an open ocean algorithm (Morel and Gentili 2009) was used to create the CDOM product. It is known not to work in coastal waters, especially in waters with high-CDOM like the Baltic Sea. There are several papers by Kowalczyk et al., Kratzer et al. and others where CDOM algorithms that produce realistic CDOM estimates for the Baltic Sea have been proposed. Probably the CMEMS previous version used the same algorithm as the Authors in their study, but CMEMS did not provide CDOM validation result for the Baltic Sea. Most likely, because the correlation with Baltic Sea CDOM was far lower than for Chl-a. The new (neural network based) CMEMS CDOM product has not been validated at all. Thus, the Authors used another remote sensing product that does not work in the Baltic Sea (and I cannot provide a recommendation where to download a reasonable product).

The third remote sensing product used is primary production. First of all, it is a Chl-a based calculation and the Chl-a product used by the Authors has very little to do with the actual chlorophyll in the Baltic Sea, as was mentioned above. The NPP model used is also for oceanic not Baltic Sea type waters. I am not sure how much does this affect the results, but it is sure that using a model not designed for the Baltic Sea with input product that is useless for the Baltic Sea should not provide very realistic results. Baltic Sea specific primary production models were published also more than 20 years ago (Wozniak et al. 1995 and several other papers by the same authors). So, better and more relevant NPP models exist. Without proper validation I do not trust the currently used NPP model.

As a remote sensing scientist I would like to see the remote sensing methods used in as many applications as possible. On the other hand, it hurts to see that people use different remote sensing product in their studies without checking are these products realistic or not. Huge amount of work has been done, but maybe only the spatial patterns found in the study have some connections with the real pCO_2 fields in the Baltic Sea. I definitely do not trust in any numbers currently shown in the manuscript as at least three of the input products that cannot be used in the Baltic Sea were used in this study.

We agree about the inconsistency of the numerical values of our input database but in our case, we needed the physical variability in space in time to be coherent more than the exact numerical value. The neural method is capable of learning to estimate the values wanted from this underlying physics independently on the correctness of their numerical value, provided the sources represent the underlying dynamics in a coherent way and we take the same inputs as the ones the algorithm was trained with.

Furthermore, for the inference of pCO_2 , the correlation of each input element to it is taken into account, so in this case of the coastal region, the impact of Chl-A will be lessened. It is going to play a role in the central part of the basin, however.

Unfortunately, the concerns raised by the reviewer, who is clearly an expert in remote sensing data, cannot be discussed without a better explanation of the details of the previous papers, and it seems the critiques are mostly on our previous work. Furthermore, even if the quality of the remote sensing data is poor, it does not mean that our results are not coherent. We will however add a part in the discussion to clearly discuss the fact

that the values obtain are dependant of the hypothesis that the observations representing at least the dynamic physic of the basin and the problems of the accuracy of the numerical values of the input data.