

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

Received and published: 19 May 2017

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

Printer-friendly version

Discussion paper



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.

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

[Printer-friendly version](#)[Discussion paper](#)

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 pCO₂ 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.

[Printer-friendly version](#)[Discussion paper](#)

Printer-friendly version

Discussion paper

