Review of Parard et al: "Remote sensing the sea surface CO2 of the Baltic Sea using the SOMLO methodology"

Parard et al. provide a revised version of their previously submitted manuscript. Having read and reviewed both versions, I am glad to say that the quality of the revised version immensely improved. The authors managed to sort out almost all language and editorial issues (besides a few typo's listed below) and now provide a very well structured introduction and method section. Furthermore, the authors added some additional results regarding the seasonal cycle of the sea surface pCO2 in the Baltic Sea – Overall, a great improvement.

There are however a few more issues, most of them raised in my first reviewers comment, which have not or only partly been addressed. Hence, I do still think that some major and minor comments need to be addressed before the manuscript can be accepted for final publication.

Major Comments:

.)While the authors have done some great effort to describe why time is used as an additional parameter, it is still unclear to the reader why. You provide a perfect example yourself: Even though in November and May the predictor data (SST, etc.) do have the same value, the pCO2 itself does not have the same value. Time itself does not change the pCO2. Imagine a steady Baltic Sea system where all potential and known pCO2 drivers are constant. In this system pCO2 would be constant in time as well. Its rather other processes that vary in time that are not considered as driver in your SOMLO model. However, this or these process(es) are not known (I assume) hence time replaces them. I am not suggesting here that you include more drivers, however, I do believe it needs to be clarified why time is important in your case.

.) In my last review I mentioned that figure captions are very short. More information in the figure caption is needed to understand the figures without constantly going back to the text to find the description. That interrupts the flow. Furthermore, please improve the resolution of figures 5, 13 and 15 as they are very difficult to read.

.) I very much appreciate your effort in providing a PCA analysis, but I am not quite sure about the point you try to make. First of all, Figure 3 is very difficult to understand with the information you provide (although BG is a high quality journal, I am sure not every reader is familiar with a PCA analysis). The same counts for table 1. Besides that, table 1 illustrates that 76% of the variability can be explained by the first 3 loadings (or axes). Why not perform a PCA analysis a-priory and reduce the number of drivers to e.g. the first 3 loadings. This would strongly reduce your degrees of freedom while explaining the majority of all the variance in your system?

Minor comments:

.) in Section 2.2. you have 3 pCO2 sources, however, source 3 is not measured directly but calculated from pH and TALK. Due to the uncertainty in the calculation, have these data been considered more uncertain or have they been treated equally?

.) Abstract line 6: Sasse et al 2013 (final version has been published in 2013)

.) Abstract line 9: change “Sea” to “sea”

.) Abstract line 10: change “Depth” to “depth”

.) Introduction lines 49-50: Some other, recent basin scale studies that could be mentioned are Schuster et al 2013 (BG), Nakaoka et al 2013 (BG) and Landschützer et al 2013 (BG)

.) section 2.3 line 147: I assume the +- sign at the MD estimate should be removed

.) section 2.3 line 156-157: “as it is taken into account when classifying and calculatin the MLR parameters” - How?

.) section 2.3 line 199: I would argue that “quite good” can be changed to “good” if you consider that previously you already calculated R values of 0.9 and more.

.) section 2.3. line 203: This sentence is a repeat and can be deleted

.) section 2.4. line 240: change “PCO2” to “pCO2”

.) section 3.2.1. line 368: You mention validation and testing data. Please provide a short description on their difference and how they are used later on

.) Figure 7 and Figure 8: I am a bit puzzled why there is such a big change in NPP, from a maximum of about 70 mgC/m2/yr in Figure 7 to 400 mgC/m2/yr in Figure 8. Please clarify

.) Figure 12: there are a few “outliers” left with differences of more than 100 muatm. Have you considered a outlier removal criterion, e.g. Chavenets criterion?

.) section 3.2.3 line 458: you compare the model output to your estimates from 1998-2011 in Figure 14. In the text you say 1998-2009

.) section 3.2.3 line 461 the word “strongly” is overconfident given an R value of 0.6

.) section 4 line 507 and reference list: Landschützer et al 2013 has been published in BG (in the reference it still refers to the discussion paper – same as with Sasse 2013)