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Interactive Comment

Interactive comment on “The role of N_2-fixation to simulate the pCO$_2$ observations from the Baltic Sea” *by* A. Leinweber et al.

A. Leinweber et al.

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Review 2:

We would like to thank referee #2 for his/her time to review our manuscript and would address the points in his/her review.

1.) To this point we are not quite sure if we will be able to meet the referee’s standards regarding his request in restructuring the paper. However, there are several points in the review that we would like to address in a revised paper.

2.) In "2.4 Data availability..." we are telling the reader that: "In order to generate representative values for the eastern Gotland Sea, continuous measurements of sea surface pCO2 and deep profiles for salinity, temperature, and DIC concentrations were averaged within a box of the eastern Gotland Sea (Fig.2)." We did did not use only a few data points to compare to the model results. Each "single data point" is derived from continuous spacial measurements of sea surface pCO2 over the entire eastern

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Gotland Sea (see Fig. 2 in our manuscript). These values have been carefully weighted and averaged over the entire Gotland Sea for each month, resulting in a representative mean pCO2 value for the entire region. Hence, this "data point" is an ideal observation to compare to 1D-model results. However, we have noticed that this should be written more precisely in this chapter, to avoid misinterpretations. Also, due to numerous data points behind the averaged values, we assume that our observations, although taken from different years, still represent the average annual pCO2 cycle. This observations have been approved by now (Schneider et al., 2003; data from FINNPARTNER). In a revised paper, we would like to add additional pCO2 data from other years into our figures to show that the discussed pCO2 minimum in summer is not a single time event. Regarding precipitation/evaporation, a drawdown in salinity of about 0.5 due to precipitation the pCO2 decrease would account for about 20 uatm, i.e. far less of what we need to explain. pCO2 is not a conservative parameter. Hence, to our understanding, a salinity normalized pCO2 does not help.

3.) All three functional groups of phytoplankton (diatoms, flagellates, cyanobacteria), zooplankton, and detritus have, in our standard model simulation, the same constant stoichiometric N:P and hence C ratio. As described in the discussion, we indirectly change the C:P ratio in cyanobacteria later to investigate, how high the C:P ratio in cyanobacteria has to be, to explain our observations. As can be seen in Fig. 1 of our manuscript, there is no process in the ecosystem model to simulate DON. We are going to explain under 5.), how we have accounted for a seasonal DOM sink for inorganic carbon. The parameterization of N2-fixation in the standard model has been derived from observations, published in Wasmund et al (1997). They observed the accumulation of surface blue-green algae mats under conditions of e.g. low wind speed, sea surface water temperatures of 16 C and higher, and low surface nitrate concentrations. Allthough a lot of work has been done in this field, still there is no standard parameterization available for N2 fixation. In a revised paper, we would like to add the kind of observations published in Wasmund et al. (1997).

4.) To add alkalinity as an additional state variable into a model for the Gotland Sea is, unfortunately, not as simple as it is in other regions of the worlds ocean. This is due to the fact, that the salinity/alkalinity dependency is not linear in the Gotland Sea. Our calculations have shown, that observed changes in alkalinity of about 150 umol/kg and concomitant changes in DIC of about 70 umol/kg result in an error of less than 20uatm in pCO2. Hence, we decided to hold alkalinity as constant in the model, and decided to use simulated pCO2 rather than DIC to compare to observations. However, we see that this part raises lots of questions. In a revised paper, we would like to include an extra chapter to explain, why we can assume for our model simulation that alkalinity can be kept as constant.

5.) We admire the work done by others and have mentioning them well in the introduction, as can be read in the last two sentences of the first paragraph of the introduction. As for his/hers statement: " However, I think that many of these citations in fact show an elevated C/N drawdown relative to the Redfield ratio, which is not necessarily the same as a continued drawdown after exhaustion of nutrients" we account for an elevated C/N drawdown due to a higher C:N ratio in POM as well as in DOM. However, even elevated C:N ratios in OM does not resolve the large drawdown that has been observed. Since we do not simulate DON in the model, we account for a seasonal storage term of CO2 in DOC derived from available DOC and DON data. This is described at the end of the result section. Here, we are also mentioning, that we can only account for about 60% of the semi-labile DOM pool as an additional seasonal sink for inorganic carbon. I.e., only that fraction of the DOM acts as an additional sink term, that is above the Redfield ratio. However, as also seen in comments of referee#1, in a revised paper we would like to address this point in more detail, to avoid misunderstanding. To explain the midsummer pCO2 minimum, we assume lateral processes to be negligible as a first approximation. This assumptions is based on a comparison of pCO2 and salinity data during summer, where no correlation can be found. We could add this additional information in a revised paper.

To our statement in the introduction(p. 611, line 9) we refer to the references cited at this point. We admire the excellent work done e.g. by Hood et al. (2001) and others, as mentioned in the following two sentences in the introduction. We have to apologize that the referee was mislead when we were citing Schneider et al. (2003). Here, we are not talking about the BATS site, but about the eastern Gotland Sea and we would change this in a revised manuscript.

Interactive comment on Biogeosciences Discussions, 2, 609, 2005.