

Interactive comment on “The role of N₂-fixation to simulate the *p*CO₂ observations from the Baltic Sea” by A. Leinweber et al.

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Review 1:

We would like to thank the anonymous referee for his/her time and effort to put together a lot of helpful comments and suggestions to our manuscript. Hopefully, our comments can answer most of the points, especially the referee's general opinion that the model's 'validation' (here, we prefer talking about "comparing model simulations to observations") is insufficient. We found, that this is in great part probably due to keeping relevant parts extremely short with explanations, and in a revised paper we would like to change this. First, we respond to the referee's general comments, followed by answering the specific comments.

For the biological part of the model, the referee is missing comparison of the different phytoplankton simulations with observation in Neumann (2000). Thanks to observations over the past decades, the seasonal cycles of different phytoplankton groups is

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well documented for the Baltic Sea (i.e. Lenz, 1996; Wasmund, 1997). In Neumann ((2000), Figure 4), data have been extracted from 3-D model simulations for the Baltic Proper as well as for the Pomeranian Bight, to show that the simulated seasonal cycle of different phytoplankton groups mirrors the general observations. I.e., diatoms dominate the spring bloom, and flagellate grow later and dominate in summer, a very common abundance in the mid-latitudes. The occurrence of visible (both from ship or space) surface blue-green algae blooms have been observed under conditions of low wind speed, sea surface water temperatures of 16 C and higher, and low surface nitrate concentrations (Wasmund, 1997), i.e. usually in midsummer. In our manuscript, the 1-D model results also represent this seasonal cycle (Fig. 3b), hence we assume that the ecosystem model, by the way one of the still few ecosystem models that already has implemented more than just one single phytoplankton group, is representing the observations well.

The referee sees problems in the CO₂ submodel, because in his opinion it is poorly validated, due to i) only a few observations for pCO₂ and ii) for the fact that the alkalinity is kept constant in this model. Since these problems are also mentioned by referee #2, this clearly indicates that we have to be more precise when explaining these things in the manuscript. First, we did not compare our model results using only six single data points. Each "single data point" is derived from continuous, spacial measurements of sea surface pCO₂ in the eastern Gotland Sea (see Fig. 2 in our manuscript). These values have been carefully weighted and averaged over the investigated area, resulting in a representative mean pCO₂ value for the eastern Gotland Sea. Hence, this mean monthly value is an ideal observation to compare to 1D-model results. With regards to alkalinity, we agree that in most 1D-models it should not be a problem at all to simulate alkalinity. There is one advantage in the Baltic Sea, i.e. that we don't have to account for the impact of calcium carbonate production. However, in the eastern Gotland Sea we have a nonlinear relationship between salinity and alkalinity. In a revised manuscript, we would like to address the steps more precise that lead to our decision to keep alkalinity constant in our model simulations. Please note also our

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explanations listed under point 6). Third, referee #1 does not agree in the way the model parameters have been changed. Modifying the gas exchange parameterization, changes in a C:N ratio and even the introduction of a seasonal DOC sink only effects the carbon cycle, nothing else, since the available amount of N stays the same in the model. That is, until we are carefully changing the parameterizations of N₂ fixation to explain the discrepancies of model results and pCO₂ observations. We hope that by responding to the specific comments, we can answer most of the questionable points.

To the specific comments: 1.) 'Introduction': we do not say that the internal C:N ratio of phytoplankton can not be considered as constant. But observations have shown that the changes of inorganic nutrients not necessarily follow the rule, that Dugdale and Goering (1967) have imposed from results by Redfield et al. (1963). The C:N ratio between single cells or phytoplankton species can vary significantly, however Redfield et al. found in general a constant stoichiometric ratio in freshly formed particulate organic matter (POM). This concept is used in biogeochemical models and gives the advantage, that simulating complex or unknown biological processes can be avoided. To our knowledge, most of the existing NPDZ models are based on this concept. Hence, model simulations for regions showing a continuous seasonal decrease in DIC despite nitrate depletion, can not reflect this observation. In our opinion, comparing ecosystem model results to CO₂ data rather than to e.g. nitrate data helps revealing discrepancies using the above concept. 2.) p. 612: We agree with referee #1, that there is no shut down of photosynthesis after nitrate is depleted, and elevated C:N ratios in POM and semi-labile DOM is common under nitrate depleted condition. There are e.g. two ways to solve this problem in a model: One way would be, like the referee suggests, to change the metabolic parameterization in the model for each phytoplankton group. Another way to address this problem is to approach it from a biogeochemical perspective. I.e., we know about the processes mentioned above, and they are reflected in i) slightly higher C:N ratio in POM compared to the Redfield ratio, and ii) in high C:N ratios of labile and semi-labile DOM. We might not know the exact physiological steps that are leading do higher C:N ratios, but looking from a biogeochemical perspective

we can easily take advantage of these results and incorporate those into the model simulations. 3.) p. 612, parag. 2.1: We have forgotten to point out to Figure 3c in the manuscript, where simulated SST have been compared to observations. 4.) p. 612, line 21: Diatoms are the most abundant phytoplankton group during spring. I.e., as soon as there is enough light and nitrate available, they start to grow. To reproduce this behaviour of this functional group in a model, no temperature dependency is necessary. 5.) p. 613: equ. 1 and 2: The description for N₂ fixation in the ecosystem model part (2.2) is based on the original description of how N₂-fixation used to be modeled in ERGOM (Neumann, 2000). Observations for i) organisms that are capable to fix N₂ and ii) N₂ fixation rates are still rare, hence finding parameterizations to put into a model is quite challenging. Our model parameterization used for N₂ fixation is based on observations (Wasmund, 1997). They observed visible (both from ship but also from space) surface blue-green algae blooms under conditions of low wind speed, sea surface water temperatures of 16 C and higher, and low surface nitrate concentrations. However, based on the comparison of CO₂ data and model simulations, we suggest an improvement for N₂ fixation parameterization. This is, when we change the temperature dependency later in the discussion. This change is based on observations, as mentioned in the manuscript (4.1 Chronology, where we are pointing out that Aphanizomenon is generally distributed in the upper 10 to 20 m of the water column (Larsson et al., 2001)). 6.) We are thankful to referee#1 comments on the alkalinity in our model. As mentioned earlier as well as in the manuscript, to add alkalinity as an additional state variable into a 1D-model for the eastern 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 seasonal salinity/alkalinity dependency is not linear in the eastern 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 keep alkalinity constant in the model simulations, and decided to use simulated pCO₂ rather than simulated DIC to compare to the available observations. However, we see that this part raises lots of questions to the reader. In a

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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. 7.)p 614, line 20. Here, we wanted to emphasize the "change" in DIC. 8.)p.614, line 23: To calculate the start alkalinity value, we have chosen the definition of alkalinity from Dickson (1981). In that definition NO₃ is not included. However, when phytoplankton is consuming NO₃, there is a concomitant increase in OH⁻ ions, part of the definition for alkalinity. 9.) p.616, line 19: In our model, regenerated productions means, that for every regenerated N (i.e. ammonium), C is also regenerated with the same constant stoichiometric ratio as we are using it for production of organic matter. That means, since there is no actual process in the model that can simulate an increase of C:N ratios in organic matter, a continuous net uptake of C is not possible. However, we know from observations, that there are elevated C:N ratios in OM, leading to additional net uptake of DIC. To include this fact in the model, we are using elevated C:N ratio for POM and DOM, both taken from measurements in the eastern Gotland Sea. This should mirror the processes the referee is addressing to. Since there is no DON state variable in the model, we have estimated the summer DOM pool from seven years of available DOC and DON data. These data show a seasonal DOM cycle with elevated C:N ratios during summer. Here, only that part of DOM can act as an additional sink for DIC, that is above the constant stoichiometric ratio. Our investigation results in an additional seasonal inorganic carbon sink of about 60% of the observed increase in DOM concentration during summer. Since we were looking for an explanation for the strong pCO₂ minimum during summer, this is in our opinion a reliable way to account for the processes referee#1 is pointing out. However, like the problem with the alkalinity, we would like to add more information on how we have calculated the additional seasonal carbon sink in DOM in a revised paper. 10.)p.618, line 17: This value is a mean value published for the eastern Gotland Sea by Nagel (1999). To our knowledge, standard measurements for POC, PON, and POP can not distinguish between dead or alive POM. 11.)p. 619: As mentioned earlier, we used a biogeochemical rather than a physiological attempt to address the problem. 12.)p. 620, line 7: We apologize here for being imprecise. We

wanted to point out that the N₂ fixation rates from our standard model simulations are much smaller than observations, but have forgotten to add this result (13.14 mmol N /m a) to table 1. 13.) p. 620: The ecosystem model used here is one of the first NPDZ models, that actually has included N₂ fixation. Hence, since knowledge is still sparse about N₂ fixation, it is not easy at all to compare model results with observations. This is one of the strength that we wanted to point out: using CO₂ data to compare to model simulations with observations can reveal unaccounted processes. To solve this problem, we have checked in greater detail available observations for N₂ fixation. We used these additional information to receive a revised parameterization for N₂ fixation in the eastern Gotland Sea. 14.)p. 620: We have checked the impact on other state variables, and the necessary adjustments are well within justified ranges. In a revised paper we could add another figure like Fig. 3, showing the differences compared to the standard model output. 15.) p. 622, line 21: It is known, that C:P ratios up to 420 have been reported for blue-green algae in the Baltic Sea. Hence, using a constant stoichiometric ratio close to Redfield et al. (1963) leads to a total uptake of phosphate in the model simulation, what does not coincide with observations. That is one reason, why e.g. Hood et al. (2001) at station BATS do not assume a phosphate limitation at all for N₂ fixing organisms in their model. Here, we wanted to know how much phosphate would be needed to fit the pCO₂ observations, and if the amount would be in the range that has been observed. We have found, that we would only need twice as much phosphate, that is about a doubling of the C:P ratio in the N₂ fixing organisms. This modification has no impact on diatoms, since they are not limited by phosphate (see 4.4 Efficiency, third paragraph). Flagellates do get an advantage by adding new N and P into the system. However, reducing their growth rate within justified ranges (Fennel, 1996), gives reasonable results for flagellates and N. In a revised paper, this would be shown in an additional figure like Fig. 3. 16.) The sinking speed has changed well within a range of published sinking speeds, so we do not see a problem here that it is interfering with our main message of the paper. However, we agree that export production is changing, and that is what we should think of as a general result. We add

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new nutrients into the system and this will have an additional effect on export production, denitrification etc. However, in this model study, we concentrated on processes in the surface water. Investigating the processes in the deeper water column would be a very good topic for additional model studies. In addition, elevated sinking rates of POM would favor the observations made by Struck et al. (2004). They could identify fixation of atmospheric nitrogen by pelagic cyanobacteria as the major source for export production during the summer season.

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