Biogeosciences Discussions, 2, S271–S276, 2005
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European Geosciences Union

Biogeosciences Discussions

Interactive comment on "The role of N_2-fixation to simulate the pCO_2 observations from the Baltic Sea" *by* A. Leinweber et al.

Anonymous Referee #1

Received and published: 17 June 2005

Review of " The role of N2-fixation to simulate the pCO2 observations from the Baltic Sea " by Leinweber et al., 2005

General comments The authors used a 1D coupled physical biogeochemical model to explain the observed DIC strong drawdown (about 150 ppm lower than the atmospheric pressure) occurring after depletion of DIN from the end of spring bloom until late summer in the Eastern Gotland Sea. The biogeochemical model is an extension of the model used in Neumann (2000) (Journal of Marine Systems) obtained by adding a description of CO2 exchanges at the air sea interface. The authors introduce several modifications in the model in order to reproduce this decrease of DIC. There are not a lot of models dealing with cyanobacteria (a group that seems to be more and more important) and in this way, this paper is important. The paper is well organized, written in a comprehensive way and is totally in the scope of biogeosciences. However, I have

found that the validation of the model is insufficient. For the biological part, the authors refer to another paper (Neumann, 2000) but reading this paper I found very few comparisons of the simulated seasonal cycle of the different phytoplankton groups with the data. In the present paper, the CO2 submodel is very poorly validated. Keeping model alkalinity constant (although, the authors say that is not constant at all), they are not able to validate simulated DIC profiles. Only, the pCO2 produced by the model is compared with a few observations (about 6 values). Before using this type of model to investigate misunderstand process (this strong drawdown), the model should be thoroughly validated. It is not really difficult to simulate explicitly alkalinity in a 1D model. It will allow to compare the simulated and observed DIC as well as alkalinity (even if a 1D model has problem to take into account river input, these river inputs can be used as an external source, or to nudge the simulated and observed alkalinity). Of course, it will not explain this strong drawdown of DIC, but you will be sure that the tool that you use to understand a process is reliable. I do not agree with the way they investigate the strong drawdown. They made several modifications in the model (modifications that are not minor) and they are just looking at he pCO2 value trying to obtain the good decrease. Nothing is mentioned about the impact that these modifications have on the other modelled variables. When one changes the parmaeterization of a model, we should revalidate the model and not only to concentrate on one output. Besides, the way they are modifying the model is criticisable (see comments below).

Specific Comments Introduction : The authors largely explain that the internal CN ratio of phytoplankton can not be considered as constant and then impose it constant in their model. Is it the case? Please clarify! Page 612, please clarify how a high CN ratio in POM and the production of labile and semi-labile organic matter can continue to decrease the DIC concentration in the water. In fact, it is frequently observed that when nutrients are exhausted at the end of a bloom, phytoplankton continues to take up DIC through photosynthesis during a certain period. The phytoplankton at this time is living on the internal reserves of nutrients that were constituted during the period of nutrients availability. That is why we can not consider that the internal C:N ratio of phytoplankton

BGD

2, S271–S276, 2005

is constant, it changes by almost a factor four during the bloom An unbalanced growth model for phytoplankton growth where the nutrients uptake is uncoupled from photosynthesis is able to describe this feature. That is why the DIC concentration continues to decrease although there is no more nutrients.

Page 612, Parag. 2,1. please present briefly the results of the physical model and a comparison of its outputs with available temperature data (maybe, it has been done in another paper?). Page 612, line 21, Please explain why you have chosen a diatom growth rate independent of temperature. Page 613, equations 1 and 2: I am surprised by the temperature functions used in the model for flagellates and cyanobacteria. Cyanobacteria are always disadvantaged by the temperature because the temperature limitation of flagellates is always higher than 1 and lower than 1 for cyanobacteria. Besides, the authors said that they exclude the possibility of development of cyanobacteria below 16°C (see line 13, page 613). First, is it impossible for cyanobacteria to grow below 16°C (maybe they are not observed when temperature is lower than 16°C because nutrients conditions are not optimal for their growth and not due to the low temperatures, it has to be tested because this formulation is too restrictive). On the other hand, equation 2 that describes cyano growth does not suppress their growth below 16°C! Page 614, line 15: The authors say that the alkalinity exhibits important space and time variability due essentially to river fluxes. Are you sure that these variations of alkalinity will not have an impact on pCO2? Do you have estimated this impact? Please clarify and justify. Page 614, line 20: the authors say that alkalinity has an important impact on DIC values. So variations of alkalinity changes considerably DIC (and thus the simulated DIC profiles can not be validated with observations because alkalinity is kept constant in the model) but these changes in DIC does not influence pCO2? Why these changes are not important on pCO2? I think it will be nice to simulate alkalinity as a state variable (as it is usually done in CO2 model) and in this way, you can use available observations to validate your model. Otherwise you CO2 chemistry submodel can not be validated properly and so you can not be sure that its deficiencies are not due to a bad representation of these chemical processes. Page

BGD

2, S271–S276, 2005

614, line 20, the authors say that they use measured pCO2 to validate DIC, I would say to validate pCO2 of the model. Indeed, if simulated pCO2 is not in agreement with observations, it is not necessarily due to a bad representation of DIC (it can be pH, alkalinity,Ě). Page 614, line 23, the authors say : “internal changes in alkalinity in a given water mass may occur by nitrate consumption/release and calcium carbonate formation/dissolution Ě” If you use the definition of alkalinity given by Dickson (the one usually used), NO3 are not in the definition of alkalinity. Phosphate and silicate are in this definition. Which definition are you using? Page 616 line 19: “Flagellates benefit from “regenerated” nutrients, i.e; not net DIC uptake with no further DIC drawdown ..” Please clarify why there is not a net uptake of DIC when phytoplankton are living on regenerated nutrients. There is still photosynthesis that is just the source of nutrients that change. Page 618, line 17, based on measurements of a CN ratio of the POM of 8.1, the authors seems to change the CN ratio of all the compartments replacing the classic Redfield ratio by 8.1. Are these measurements of POM ratio representative of the situation throughout the year? Do they concern the living POM? Page 619, upper part, :.I think this is the contrary of what is said in the text : this is the excess CO2 uptake over nutrients at the end of the bloom that leads to rich carbon DOM production. Indeed, the DOM produced by phytoplankton exudation has a high CN ratio to compensate the increase of their internal ratio caused by the continuation of photosynthesis after nutrients exhaustion Page 619, first paragraph, to take into account the uncoupling between C and N in phytoplankton, the authors propose to remove a constant fraction of DIC at every time step during three months. First, the unbalanced between N and C does not occur during the whole season but at certain time. Then, if you think that the growth of phytoplankton is unbalanced you have to use an unbalanced growth model on order to estimate accurately the impact of uncoupling between photosynthesis and nutrient uptake on the DIC drawdown. IF it is not important, you can go back to the balanced growth. Page 620, line 7 : the authors say that “ our simulated N2-fixation rates for the period Nov 197 until October 1998 are much smaller than observations (Table 1)” However, when you look at table 1, it is not the case! Where is

BGD

2, S271–S276, 2005

Interactive Comment

the simulated rate? Page 620: I do not understand the demarche of the authors. At the beginning, we are told that the model has been validated. However, now they seem to doubt about the ability of the model to reproduce the bloom of cyanobacteria and they recalibrate the cyanobacteria module trying to obtain the DIC decrease. It means that the model they are using was not calibrated ? Page 621, the authors change the calibration of cyanobacteria and they obtain different results. They interpret the results just in terms of DIC drawdown. Nothing is mentioned about the representation of the cyano bloom compared to the data and the blooms of the other two groups due to the earlier occurrence of cyanobacteria. What about flagellates if cyano are blooming earlier? Please add figure showing the modifications of model results for the different groups when you change the representation of cyanobacteria. The other groups have to remain well represented as well as zooplankton, POMĚ. You have to look at the impact of your modifications on all the model variables and to check the reliability of the model to reproduce data (maybe the model simulates the DIC decrease but the other variables are wrong now!). Please also, specify the data that you have to validate the model. Page 622, line 21, the authors decide to increase the phosphorus availability in water. I do not agree. How are the phosphorus concentrations compared to observations now? What is the impact of this artificial increase on the other groups? Why do not you change the half saturation constant if you suspect that cyanobacteria are very efficient for phosphorus uptake? Page 623, line 7, the authors says that they have now an unrealistic increase of nitrate in the upper layer compared of observations. What is the impact of this increased nitrate on the other groups? Is it consumed? So, it means that the model is not able to reproduce what is happening in the system. The model simulates now the observed DIC drawdown but now other variables are wrong! So, maybe you have the right pCO2 cycle for wrong reasons! Page 623, to compensate the deficiency mentioned above they increase the sinking rate of detritus. What about the representation of the export? You change the dynamics of the system and what it is the impact on the other variables to increase the sinking speed of detritus which is a very sensitive parameter of biogeochemical model. Please clarify.

BGD

2, S271–S276, 2005

Interactive Comment

Minor comments Page 611, line 30, I would replace CO2 by pCO2 Page 612, line 9, 6 m corresponding to the deepest point in the eastern Gotland Sea, I suppose it is : Applied to the deepest area of the Gotland Sea. Page 616, the authors say " we will describe the simulated pCO2 and discuss the main underlying causes Ĕ" Please, specify which cases you will investigate

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