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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 #2

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The authors present a modelling study aimed at examining the cause of DIC drawdown, in the absence of nutrients, in the Baltic Sea in summer. The subject of C/N imbalances, and whether biogeochemical cycling in marine systems conforms to the Redfield ratio, is certainly topical. Unfortunately, however, I found the methods and results of this particular modelling study wholly unconvincing, the manuscript suffering major deficiencies.

(1) The standard version of the model fails to capture the apparent (see (2) below) DIC drawdown seen in the data during summer. The authors admit that there is a "large discrepancy between model simulation and observations" (page 617, line 17), and moreover that "it cannot simulate the CO2 undersaturation of the midsummer surface water in the eastern Gotland Sea (page 617, line 19). The authors solve this problem by increasing N2-fixation giving a rate of 167 mmol N m-2 yr-1 which they say is similar

to some previous estimates. This is the main point of the manuscript. This model run should be presented in the Results (not the Discussion) and should be the main focus, rather than the standard model run. Let's see detailed results (Figures) for pCO2, nitrate, primary production and chlorophyll, all compared to data. And comment should be made on other variables as well, particularly how well the model simulates DOC. The Discussion section should then be used for genuine discussion of the whole approach and reliability of the results. The authors need to do a whole lot more to convince me that their results are meaningful, and that the high N2-fixation rates are meaningful. Validation is wholly inadequate at the moment.

(2) I'm suspicious about the reliability of the data for pCO2. There is only a single data point on Fig. 3 for midsummer pCO2, this single point providing the foundation for the whole study in DIC drawdown. This point therefore needs detailed justification. Without such justification, the whole study and conclusions are suspect. I'm surprised that there is only a single point because the authors state that there are continuous measurements of pCO2 in the Gotland Sea (page 615, line 10). To make matters worse, the pCO2 data are a collage over different years. A further important point is the issue of evaporation and precipitation on pCO2, which the authors must address. For example, a freshening due to net precipitation may cause a large drawdown of pCO2 (but not nitrate) quite independently of biogeochemical processes. This net freshening is for example an important component of the DIC drawdown observed at BATS (Anderson and Pondaven, 2003). Have the pCO2 data been normalised to constant salinity (which is one way of getting around the problem)? The authors must address issues of both data and model reliability at length in the Discussion.

(3) The model description, and particularly regarding processes contributing to non-Redfield dynamics, needs significant improvement. Is the C/N/P in biomass different between for example phytoplankton, zooplankton and bacteria (as might be expected)? What processes in the model contribute to elevated C/N in DOM? Are different elements remineralised at different rates? Etcetera. And crucially, little detail is given on

the parameterisation of N2-fixation. We are told that this group is able to “outcompete other phytoplankton groups when nitrate and ammonium concentrations are low” (page 613, line 7), and a temperature function is used (Equation 2). Many more details are needed, along with a discussion of where parameter values come from and how reliable they are. And it is no use simply referring to Neumann (2000): although equations are provided in that reference, there is no critical discussion of the parameterisations involved there either.

(4) I find it surprising that the authors are able to ignore the effect of alkalinity on pCO2 (section 2.3). They should specify just what the observed alkalinity range is so that the reader can check this for themselves. And to make the reader’s job easy, they should specify the pCO2 range that would correspond to the observed range in alkalinity. Vague statements such as “it is almost independent” (page 614, line 17) will not do.

(5) The Introduction is poorly written and does not properly cover the previous work in this area. The authors claim that “measurements in various oceanic regions have shown that DIC concentrations in the upper water column continue to decrease after depletion of dissolved inorganic nitrogen” (page 610, line 25), citing several references. However I think that many of these citations in fact show an elevated C/N of drawdown relative to the Redfield ratio, which is not necessarily the same as a continued DIC drawdown after exhaustion of nutrients. Further, the description of factors contributing to C/N imbalance is weak. For example, production of DOM per se does not cause imbalance, only if its C/N differs from Redfield. And how does lateral transport (page 611, line 7) contribute to the imbalance? I’m not saying that it doesn’t, only that the text is poorly articulated. Next, the authors state that “previously, N2-fixation was considered not to be a candidate process” (page 611, line 9). That is not so. The authors should mention pervious studies by Hood et al. (2001) and Anderson and Pondaven (2003) in this context. Hood et al. achieved a model solution for the DIC drawdown at BATS that displays remarkable parallels with the study of the Baltic Sea presented here, in each case requiring apparently very high N2-fixation rates. However, Hood et al.’s modelling

work was superseded by Anderson and Pondaven, who showed that the DIC draw-down at BATS could be explained by a combination of factors without having to invoke high N2-fixation. Nevertheless N2-fixation was one of those factors and did contribute to the overall drawdown. The authors state here that “N2-fixation has been proposed as a possible mechanism in this [the BATS] region as well (Schneider et al., 2003), but this attempt is equivocal in how far N2-fixation can be held responsible to explain the observed summertime drawdown of CO2” (page 611, line 21). This statement simply does not do justice to the excellent progress that the Hood et al and Anderson and Pondaven studies have made. The authors should acknowledge the value of these studies and make clear that they are building on that work, not starting afresh with some new N2-fixation hypothesis which is the impression their text gives.

(6) The Discussion, and indeed the whole manuscript, needs a complete overhaul in view of my comments made above.

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