Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC3
https://doi.org/10.5194/bg-2022-31-RC3, 2022

Comment on bg-2022-31

Anonymous Referee #3

Referee comment on "Modelling the effects of benthic fauna on carbon, nitrogen and phosphorus dynamics in the Baltic Sea" by Eva Ehrnsten et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-31-RC3, 2022

In this paper, a new tool is presented that couples a low-resolution pelagic biogeochemical model with a low resolution benthic biological model for the Baltic sea. The (vertically integrated) benthic state variables are then used to calculate impacts on biogeochemistry using presumed effects of bioturbation and water-column conditions on denitrification and phosphorus dynamics. My main doubts with this paper are connected to the biological focus of the model.

Essentially there exist two schools of modelers: some modelers take a *biological* approach and ignore or strongly parameterize biogeochemistry. Their models disregard the small-scale vertical gradients of solutes in the sediment and often consider only surface-averaged concentrations of particulate substances (e.g. organic matter). Moreover, their models operate on seasonal time scales, as organisms usually react on these time scales. Opposed to this are the modelers that tackle sediment dynamics from a *biogeochemical* perspective and strongly parameterize biology. These modelers take into account the fine-scaled vertical gradients of solids and solutes that are observed in the sediment, and their dynamics includes reactions operating at very different timescales, from very short (< seconds) up to very long time scales (multi-years). In these models, the metabolism of the (higher) organisms is included as "oxic mineralization" of organic matter, while their bioturbation activity is included as a "coefficient". Thus, these models strongly parameterize the biology, and only explicitly account for the biogeochemistry. As long as the main conclusions of these models are stated in the area of the model focus, there is nothing wrong with any of these approaches. For instance, it is reasonable to assume that a biogeochemical model can rather faithfully reproduce the impacts of certain external conditions on sedimentary nitrogen or phosphorus removal rates, but it is questionable whether such models can also well represent the distribution of the benthic organisms that drive the biogeochemical cycles. Similarly, why would we put a lot of faith in biogeochemical conclusions that come from a model that focusses on biology and parameterizes the biogeochemistry? This is in a nutshell the doubts I have on this paper. While the conclusions seem logical, I am still to be convinced that the tool used to arrive at them is appropriate.

Because of the biological focus, there are quite some assumptions with respect to biogeochemistry that are not dealt with in the manuscript. For instance: the paper talks about the sediment pools of C, N and P, and Si. Biogeochemically one distinguishes between particulate and dissolved pools – here I had to guess that the pools refer only to particles (the 'food' of the organisms). Thus, the transient (within season) storage of dissolved components is ignored. Is this a reasonable assumption ? (I could not find any evidence for this). In addition, historical eutrophication in the Baltic may have caused significant storage of dissolved nutrients deep in the sediment (i.e. ammonia, phosphate, sulphide), which are not accounted for in the model. Can these be ignored – what is the effect of ignoring these on long-term simulations?

In addition, the dependencies of the biogeochemical processes on the model variables are so complex that it is very difficult to see how these processes are affected. For instance the formula (5), which essentially describes the dependency of denitrification on water-column oxygen and biota, has 4 "fitting" parameters – to what data have these been fitted? The P-sequestration formula (formula 7) has even 8 "fitting" parameters. On line 187, it is said that it is difficult to constrain the new parameters. Does this mean that these parameters have not been fitted at all – and if they have, on which data? And why would instead running sensitivity analyses by changing the Ebio parameter be a valid alternative? A little more effort in showing that these dependencies are realistic is required. (and where is formula 6?).

I also find the lack of any comparison of model output with biogeochemical sediment data worrisome. On L 313, the authors claim that they cannot "properly validate the simulated sediment stocks or fluxes due to a lack of large–scale data and insufficient understanding of the multitude of mechanisms underlying the biogeochemical transformations and fluxes". The first part (lack of data) does not do justice to the multiple biogeochemical studies in the Baltic that have recorded sediment-water exchange fluxes, and measured sediment concentration profiles in great detail. Also, I do not agree with the statement that there is "insufficient understanding" of biogeochemistry. As a quantitative science, biogeochemistry is at least as (and probably much more) advanced as biology! And even if it were true that we do not understand the biogeochemistry, why would we then trust the simple parameterisations that are used in this manuscript?

In summary, as much as I like the conclusions from this paper, the authors need to try a bit harder to convince that biogeochemistry in the Baltic can be predicted based on presumed effects of biological activity on N and P removal.