

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-249

Anonymous Referee #1

Referee comment on "Modeling of the large-scale nutrient biogeochemical cycles in Lake Onego" by Oleg P. Savchuk et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2021-249-RC1>, 2021

General comments

In the paper, the authors use a 3D thermo-hydrological and biogeochemical model to simulate the nutrient cycles in Lake Onego.

They reconstruct 3 decades and made a lot of comments and conclusions on the simulated results.

The most important problem they have to face is that there are very few data available to validate their model. The authors are fully aware of this and justify their work and the use of the 3D model on this basis. The knowledge gained and integrated into the model should be able to compensate in some way for the lack of data. The authors go so far as to say that the hindcast results can be used as a form of re-analysis.

According to me, there are several problems with the approach:

1. calibration: models outputs are very sensitive to the parameters values which differs from one lake to the other. The authors have not performed any kind of calibration. They have used the parameters set calibrated on data of the Baltic Sea which is very different from the lake Onego. Adding to this the lack of validation data, the simulations used cannot be considered reliable.

2. validation: there is really too little data for the model to be properly validated. Comparing a few simulated values on Lake Onego with those measured on other "similar" lakes is not sufficient for this. Yet, the authors could have considered some remote sensing measurements issued from satellite images that would have help them a lot for this validation process.

3. simulation: the authors made only one simulation instead of performing a model exploration that could have provided some estimation of the uncertainties on the model outputs. Indeed, the author says that the simulation results are plausible but nowhere

they give an estimation of this "plausibility" (and so the uncertainties) of the results.

3. conclusions: the authors made a lot of comments and conclusions, as if the simulations they performed were reliable. Moreover, they argue that the simulated results can be used as a form of re-analysis when there is almost no data available.

Finally, a lot of comments and information are given but sometimes the most essential ones are missing. In particular, with regard to the available data, which is of importance here, details are not always given. The comparison are not well explained and the value of the errors between simulated and observed data are not given.

I understand that this case study is complicated, because of the lack of observation data. Models are obviously interesting tool that we must use, but in combination with observation data. Without them, it is impossible to validate the model outputs and to make conclusions. According to me, satellite images should be the first thing to work with when direct measurement data are not available. In the case of lake Onego, which is moreover large, this will be all the easier. The second thing is to do model exploration to draw conclusions from a set of simulations rather than one. Finally, if so little data are available, considering a 1d vertical model could be a first interesting step.

Specific comments

section 2.1: in the section "model presentation", the author said that the model they consider is the SPBEM. They give a reference (Isaev et al. (2020)) to the reader in which, according to them, all the equations, parameters, constants, etc are given. And they explain what adaptation they made to apply the model on the case of Lake Onego. If I well understand, they change a little bit the structure of the biogeochemical model, but they keep the same parameters values than those used (and calibrated) to simulate the biogeochemical functioning of Baltic Sea. How can the authors justify that? We know that the models can be very sensitive to parameter values, that the parameters values can be different from one ecosystem to the other, which is the reason why the calibration step is important. I understand that the author do not have a lot of available measurements, but this is not a sufficient reason not to pay attention to the parameter set used in the model.

l 90-91: in the modified version of the model, only two variables were considered for the autotrophs: diatoms and non-diatoms. How the authors have chosen the values of the parameters corresponding to the non-diatoms group that gather several variables of the original SPBEM ?

l 113-115: for the 40-year spin-up simulation, did the authors consider some nutrients inputs from the river? If so, it could have lead to some accumulation of the nutrients in the sediments that is really slow, no?

I 150, section 3: which observation data are available exactly? A table that summarizes all the available data that have been used for this study would be helpful.

I 164: what do the authors mean by "we omitted the analysis"? Have the authors access to some other measurement data that they did not consider? Or did the authors only show the ice cover and the water temperature because it is the only measurement data they have?

I 165: the authors says that "surface water temperature" is an "important integral indicator of the hydro-thermodynamics" which I do not agree with. Surface water temperature is highly influenced by external meteorological inputs and does not reflect the complex thermal structure of lakes, in particular the stratification periods that play a key role in ecosystem dynamics.

I 170: why the authors did not use the entire satellite images for comparison with the simulated field? This is one interesting advantage of using a 3D model? Moreover it gives access to additional data that are of particular interest in a case such as this one, where only a few measurements data are available.

I 173, figure 4. Were the data of this figure used for a calibration procedure or not?

I 195: the authors should explain briefly somewhere in the manuscript what is the concept of "biological summer" because they refer to it several times.

I 204-206: The authors mention a difference (based on field data) in the BSD in the shallow areas and in open deep waters. Did they retrieve this difference in the simulations? Can they give some estimations based on the simulations that can be compared with the observed BSD?

paragraph 3.1.2 The authors show some simulation results but there is no data available for comparison. How can we trust such results? The only comparison that are made is on the annual integrals of the simulated phytoplankton primary production that is compared with values of other lakes...

I 223-224: the simulations show, for each year, a strong phytoplankton spring bloom and a minor autumn bloom. Was that also observed in reality? Here again satellite data could be useful.

I 252-254: do the authors speak about simulated results or observations? it is not clear.

I 256: can we get more information about these direct measurements?

I 257-258: why the author compare a range of observed values ("from 413 mg C m⁻² day⁻¹ at the top of Petrozavodsk Bay to 122 mg C m⁻² day⁻¹ in the open areas of the Bay") with an average simulated value on the entire limnic area?

I 276: what does "in good agreement with the phenology of the Lake Onego ecosystem deduced from field observations" mean? the author should explain which field observation they are talking about and make a detailed comparison.

I 284: here again give details about the available measurements? is some measurement at one day? several times? what was measured, etc...

Table 1: why did the authors give a range of values for the measurements and a mean and standard deviation for the simulated results? what does this range of values represent for the measurements?

I 341: which reported maximum? is it a field observation?

paragraph 3.2: here again, the authors make a lot of comments based on the simulations whereas there is no data for comparison. Therefore, the conclusions they made (I 355-360) are unreliable.

paragraph 3.3: same remark than for paragraph 3.2.

technical correction

line 61: reproduces instead of reproduces

line 66-68: there is something missing in this sentence. "can be used" but for what?

lin 85: there are two references Isaev et al (2020). Add a a and b to distinguish the two papers.

line 86: "those our formulations"? put "these formulations" instead

line 89: lakes instead of lake

line 94: "and thus never became limiting" instead of "that is never became limiting"

line 164: "we omitted the analysis" instead of "we omitted analysis"