Biogeosciences

Discussions

Open Access

EGU

BGD

Interactive
comment

Interactive comment on "Causes of simulated, longterm changes in chlorophyll concentrations in the Baltic Sea" *by* Jenny Hieronymus et al.

Jenny Hieronymus et al.

jenny.hieronymus@gmail.com

Received and published: 2 August 2017

Response to Referee #3

We thank the referee for the valuable comments that will significantly improve the manuscript. Answers to comments are listed below. Referee comments in italics.

Reworking and shortening section 2.2 has been suggested by both referees. We agree and will review this section accordingly.

- *Already in the abstract combination of words "mixed layer 'parameter' concentrations" appears as solid term. However I did not find in the text how it was defined.*

Is it mean value of horizontal mean 'parameter' in horizontal mean mixed layer? or it is integrated characteristic?

The definition of mixed layer concentrations is

$$\frac{\int_{ML} C\, dV}{\int_{ML} dV}$$

. We will clarify in the manuscript.

- *Salinity in the Baltic Sea and in the Baltic Proper have strong lateral gradient. However, mixed layer depth (MLD) was defined as constant density difference. Could it be that with decrease of salinity MLD will increase? Could it be that seasonal variability in surface effects MLD and at the end all results ? The part with mixed layer definition should be extended and some how emphasized. Maybe it makes sense to include it as additional subsection.*

Horizontal gradients in S should not matter but weaker stratification might. However, we have carefully checked that it appears to find the pycnocline everywhere in the domain. The problem might have been bigger if the whole Baltic had been used, rather than just the Baltic proper though.

- *The "basin integrated approach" was used here (line 61). Would be good to see in the text why this is acceptable (preferably in more than one sentence, line 6*

This approach is basically more robust than a point value, as it takes away some small scale variability that we are not really interested in here. A 1d dataset is also required for the wavelet analysis.

- *While SCOBI model is 1D model (line 67), I would suggest to show results of wavelet analysis for idealized 1D cases. So it could be seen how certain changes are reflected in final results of wavelet analysis. For my opinion such sensitivity*

test could enhance conclusions. Otherwise, section 2.4 should be extended with some aspects of wavelet coherence.

All our wavelets are 1d. As stated above, the wavelet approach uses a 1d dataset. The only dimension we have is time. The spatial dimensions are removed by taking spatial averages.

- *Analysis focuses mainly on river loads and its changes. Other nutrient sources like atmospheric deposition, exchange with other Baltic Sea regions and there possible effect should be mention somehow*

  This is a good point. We will elaborate more on this in the revised version

- *It could be considered to include wavelet analysis in to the title – to my opinion application of this method is among the most interesting aspects of this manuscript*

  We will add this to the title.

- *Line 75: eq. 1. NFIX is nitrogen fixation term, in all phytoplankton groups it looks strange. Is it a misprint?*

  NFIX is zero in non-nitrogen fixing phytoplankton groups. We will put a note in the revised manuscript.

- *Line 78: SINKIphy / SINKOphy is it sinking of phytoplankton?*

  Yes, in/out of the gridbox.

- *Lines 177 - 181: Paragraph is confusing. It starts with sentence about open boundary, but last two sentences are probably about river loads. Please specify in more details: what these assumption were applied to*

  We will rewrite.

C3