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

Anonymous Referee #3

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The manuscript present interpretations of long-term ecosystem model results by wavelet analysis. The method to understand the model behaviour applied here is relatively new and promising. Generally, the manuscript is well written.

Some open questions and suggestions:

Original SCOBI model equations were published in Eilola et al. 2009. To my opinion introduction to SCOBI model is too long and could be easily shortened in a new publication. Authors reproduce already published part of the model equations and description. However, it takes more space than in the original Eilola et al. 2009 paper. Moreover, text and equations are slightly different from original publication, that is confusing. Previews publication contains number of reference to sources of SCOBI model equations (see Eilola 2009, Table A.4.). These references are not listed in the current

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version of model description. Without these references it is hard to follow why particular formulation is relevant.

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 ?

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. May be it makes sense to include it as additional subsection.

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 62).

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.

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.

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.

Line 75: eq. 1. NFIX is nitrogen fixation term, in all phytoplankton groups it looks strange. Is it a misprint? Line 78: SINKIphy / SINKOphy is it sinking of phytoplankton

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? Line 148: eq. 14. : “frac” is a misprint. 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?

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