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

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Reviewer comments to manuscript by J. Hieronymus, K. Eilola, M. Hieronymus, H.E.M. Meier, and S. Saraiva "Causes of simulated, longterm changes in chlorophyll concentrations in the Baltic Sea" submitted to "Biogeosciences"

The study deals with the application of statistical wavelet analysis to the results of numerical simulation of multi-decadal ecosystem dynamics performed on a 3D biogeochemical model. This combination of empirical and theoretical approaches is rather novel in both methodological and, especially geographical aspects and could be interesting not only to the readers of, say, "Ecological modelling" but also to a much wider audience of "Biogeosciences". However, in order to reach this audience the scientific presentation and analysis should be significantly streamlined, deepened, and made



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much more relevant to the Baltic Sea realities. To my mind, the necessary efforts would amount to a moderate or even major revision.

1. General comments and suggestions 1.1 Objectives of the study As can be understood from the title, the major goal of this study is to find (reveal, explain) the "causes" that determine the simulated long-term dynamics of phytoplankton in the Baltic Sea. Meanwhile, all the causal relationships driving the variations of variables had already been assumed and explicitly parameterized in the model formulation and algorithms, including prescription of the initial and boundary conditions. Correspondingly, the simulated variations is merely a result of the dynamical balance between positive ("sources") and negative ("sinks") terms ("fluxes") in a system of differential equations. In that sense, the causes are already implicitly known and all that is needed is just a clever quantitative analysis of the fluxes and balances that determine the dynamics. Such kind of analysis has already been successfully performed and published in numerous papers, including those co-authored by the authors of both this manuscript and this review. Therefore, the necessity in empirical approach to deterministic causal relationships needs a specific motivation and justification that must be given already in the Introduction.

1.2 Interpretation of results To my mind, there are two major challenges to this study and presentation of its results. Firstly, it should always be remembered by authors themselves and made clear for the reader where and when you are discussing the results of simulation vs. where and when – the real Baltic data and conditions. As it seems to me, the text is often written in such a way as if this distinction is almost forgotten (neglected). Just a few, by far non-comprehensive, examples: "The co-variation of key variables with modeled phytoplankton... (line 1)", "...the effect of nutrient loads, nutrient concentration, temperature, irradiance and mixed layer depth on the modeled phytoplankton community (lines 46-47)", whereas part of these "variables" is prescribed, while another part is MODELED similarly to phytoplankton; at lines 341-342 "the coherence of the mixed layer concentrations of phytoplankton with key variables affecting the primary production has been examined for the Baltic Proper" at this start of Summary and conclusions you must explicitly indicate that the entire analysis was made on results of modelling. From that perspective, the entire text should be carefully read and appropriately edited.

Secondly, interpretations of the wavelet spectra in time-frequency domain, especially interpretation of the wavelet coherence must be explained already in Methods in more detail in respect to what does it show – periodicities and their coincidences, time lags, phase shifts, correlation and its strength, what is a wavelet power, what are the AR1 and global power spectra, etc.? Particularly important are considerations and interpretations involving the "coincidence" vs. "causality", i.e. "simultaneously (coherently) occurring" (by the same or even different reasons) vs. "because of". The text is often written as if you imply the latter. It could be very helpful if you would illustrate your explanations with the wavelet analysis of water temperature as more reliably simulated.

1.3 Chlorophyll as a measure of the phytoplankton biomass As is well known, the C:Chl ratio in the phytoplankton of temperate latitudes varies from 10 – 20 in the winter to over 100 in the summer but the exact seasonal patterns and ranges of these variations are different between both the algae species (functional types) and sub-basins. Therefore, the use of Chl as a model variable with the constant C:CHL=50 may be considered just as a some nominal measure of the phytoplankton biomass, which comparability to real measurements has a large inherent uncertainty. Apparently, the authors understand this conventionality very well as, for instance there is no mentioning of chlorophyll in the Abstract at all and quite a few in the Summary and conclusions. Such understanding should be made clear for the reader, while the word "chlorophyll" has to be replaced with "biomass" or "phytoplankton biomass" wherever it is possible.

1.4 Targeted scales As the seasonal cycle is only expected for both the functioning of the Baltic Sea ecosystem and variations of the prescribed boundary conditions, the authors have to focus much more on a longer, interannual to decadal time scales, reverting to the annual and shorter scales if only absolutely necessary for important discoveries.

1.5 Relation to the real world There are almost no comparisons to the data and estimates based on measurements and experiments. Then, it should be explained why an exception was made for chlorophyll (Section 3.1), in contrast, for instance, to other, also simulated variables like temperature, salinity, and nutrients. The revision choice could be between either repeating similar comparisons and wavelet analysis for other measured variables, thus expanding the entire study and shifting it towards model validation, or excluding observed chlorophyll from Section 3.1 entirely, thus confining the analysis to merely simulated time-series. Taking into account my comment 1.3, I would recommend the latter, while a relation to the real world could rest solely on the literature references.

1.6 Mode of presentation To my mind, there are two major flaws in how the manuscript is written and results are presented. In addition to a lack of comparison to the real world data, there are almost no references to published studies and conclusions that are pertinent to findings and features that are presented and discussed in the manuscript. Moreover, in its present form the manuscript looks rather as a technical report, kind of monotonically listing some results calculated just because there is a novel tool and there are computed variables. As I have already started indicating above, the manuscript should be made more conforming to the usual scientific standards, that is highlighting: WHY (which yet unsolved problems, justification of the approach to solve them), HOW (pros and contras of the tool and processed material, including plausibility of simulation), WHAT (the novel conclusions, how realistically and reliably they are comparing to existing knowledge and views, uncertainty of results, unsolved remnants).

2. Specific comments and suggestions 2.1 Title According to my comments above, an every word in the title should be carefully reconsidered starting from “causes” (are you revealing deterministic causes or just interesting co-variations?) to “chlorophyll” (phytoplankton biomass) to the Baltic Sea (without Arkona and Bornholm basins your





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area is not even the Baltic Proper). An explicit indication already in the title at the implemented wavelet analysis or even wavelet coherence would be appropriate as well.

2.2 Abstract The "internal loads" here and elsewhere is a bad term for a reversible phosphate exchange between the water body and sediments where the total pool have been accumulating for decades if not for centuries, because of external loads. Please, reconsider everywhere.

2.3 Introduction Besides the general description of the scene, it needs a better emphasis on the yet unanswered scientific questions as a prelude to better motivation and justification of the necessity in empirical wavelet analysis.

2.4 Methods Within a basin-integrated approach you are actually dealing not with the "horizontally integrated" values (that must then be in thousand tonnes) but the "horizontally averaged" concentrations, biomasses, depths, etc. or basin-averaged as at line 332. Correct, please, at lines 61, 205, 211, 220, 221, Fig. 3, and elsewhere, wherever I could have missed it.

2.4.1 Model – I understand your reasoning at lines 72-73 but suggest to carefully reconsider which explanations you want to give already here, in Methods, i.e. pretty far away from their subsequent usage in Results and Discussion and which reminding would be enough to make directly there. Just as example,"The model value for diatom sinking rate is five times higher than that for flagellates while cyanobacteria is assumed to have no sinking rate" at lines 336-337 is quite enough and even more informative than "Furthermore, the sinking rate of diatoms is five times larger than that for flagellates" at lines 90-91 and about blue-greens at line 97. – Also, I am not sure how necessary is Eq.1 without indication of conversions between Chl and C, explanations on sinking terms and on absence of NFIX in equations for diatoms and flagellates. Perhaps, more important is the explanation about Chl as a measure of phytoplankton biomass and a constant C:Chl and C:N:P ratios. The decision could depend on whether you need to refer to Eq.1 in subsequent analysis. – In Eq. 3, NF is not defined anywhere. I guess, it could be a product of Eq. 13 and some function of temperature but it is not presented. – "...higher salinity means more phosphate is retained in the benthic layer..." (lines 102-103). In my parameterization, borrowed also by Eilola et al. (2009, p. 168) it is the other way round – less retention (higher release) at higher salinity. Correspondingly, check, please, at lines 251-253 and elsewhere. – Your parameterization of the nitrogen limitation (Eq. 5) as a sum of separate/independent (!?) ammonium and nitrate limitations (Eqs. 6 and 7) under certain conditions is higher than 1, hence, contradicts to the basic assumption $0 < NLIM < 1$ and amplifies the growth rate rather than limits it. As can be calculated by these equations with constants (9, 10, 12) and the real Baltic Proper monthly averages of ammonium and oxidized nitrogen concentrations, the value of NLIM for flagellates and cyanobacteria is higher than 1 during half a year, from November to April. For example, the average (2005-2015) March concentrations of NH4=0.19 and NO23=3.02 uM estimated from monitoring data in the Gotland Deep (BY15) would result in NLIM=0.92 for diatoms but in NLIM=1.13 for others. Consequently, the consistency of interpretations in Section 3.3 must be checked out and corrected as necessary. On the other hand, this entire Section 3.3 should be reconsidered anyway (see below). – In addition to or even instead of Eqs. 14-18 in Section 2.2.2, a comparison between the simulated depth where LTLIM is less than 0.5 (or 0.25, or both) and the mixed layer depth could be more important for the subsequent analysis in Sections 3.4 and 3.5.

2.4.2 Forcing Please, clarify either in the text or in Fig.2's legend: 1) what is shown in Fig.2 – total loads to the entire Baltic Sea or only to the study area as in Fig.1, 2) had the direct point sources been included in the prescribed "river loads" and, thus, assumed seasonally variable as well, 3) see also suggestion to Fig. 2 below.

2.5 Results and discussion 2.5.1 Regardless of whether you'll retain the (very poor and dissatisfying because of variable C:Chl) comparison to observation or will just stay with simulation, the entire Section 3.1 suffers from almost total lack of discussion on the "re-



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distribution” of local maxima, supported by the references to, e.g. Kahru et al., (2016), look also for Wasmund from IOW, Winder, Griffith and their colleagues from Stockholm University, results of AlgaLine, HELCOM, etc.

2.5.2 Nutrient loads. To my mind, you phrasing in the entire Section 3.2 reads as if you imply a casual and almost immediate effect of river inputs at the surface concentrations already at the annual and shorter scale as, for instance, at lines 282-283. Please, consider, at least, two important features of the Baltic Sea: a) long nutrient residence times caused by an order of magnitude difference between residing pools and nutrient amounts annually put into the Sea, b) nutrient exchange of your study area with the south-western Baltic, where the vegetation season starts earlier, and with the northern gulfs with delayed seasonal development. So, never mind the seasonal scales. Perhaps, my further confusion with the longer scales is triggered by the lack of proper explanations on interpretation of “coherence”, because I read the entire Section as if it means causal relationship. Adjust your text accordingly to these considerations with appropriate references. A hint – your considerations here could be related to, at least Conley et al. (2002, 2009), Vahtera et al., (2007) and Savchuk (2010). But then the question may arise – how novel are your results and why they are important? Also, what new and important could be in consideration of coherence between river loads and phytoplankton functional types? If nothing substantial, then Figs 6-7 can be painlessly cut off together with corresponding considerations.

2.5.3 Nutrient limitation. Here I have several comments, perhaps, somewhat contradicting one another. – To start with, the entire approach to analysis of simulated variables can be questioned in several aspects, since the analysis and conclusions are based on: a) a specific combination of prescribed constants and could change even with a minor recalibration, b) an inconsistent parameterization of the nitrogen limitation (see above), and c) simulated seasonal dynamics of vertical nutrient distribution that are pretty far away from the observed dynamics (see, e.g. Fig. 5 at p.2120 in Liu et al., 2017). – Furthermore, the implementation of your limiting functions (Eqs. 5-12) instead of a common N:P ratio that directly indicates a deficient/excessive amounts of nutrients gives misleading results. For instance, your finding about persistent winter phosphorus limitation found in the model contradicts, at least, to the situation during recent decades. Note, that for the example given above in 2.4.1, March phosphate concentration was 0.60 uM, which results in DIN:DIP ratio of 5.4., indicating clear N limitation, whereas your Eq. 8 will give PLIM=0.86 for diatoms and PLIM=0.92 for others, which led you to claim the P limitation. – On the other hand, the higher values of N:P ratio (but still well below 16) indicating similar relaxation of the nitrogen limitation relatively to contemporary conditions have also been simulated for the beginning of the XX century, e.g. by Schernewski and Neumann (2005), Savchuk et al., (2008), and Gustafsson et al., (2012). So, I would recommend to repeat your analysis with N:P ratio as more conventional and less questionable indicator of nutrient limitation. – According to Eq. 4, i.e. the minimum law, only P or N must singularly limit at any specific moment. That means that there is simply no place for simultaneous limitation by both P and N together, as shown in Fig. 8. Find, please, less confusing form of presentation, perhaps, with different colors or even contour plots. – Despite my general recommendation about references, the entire paragraph at lines 270-273 does not look as especially necessary here. Instead, in the following considerations at lines 274-280 you would better recall, at least Conley et al. (2002, 2009), Vahtera et al., (2007) and Savchuk (2010). – Consider also, please, moving considerations about coherence between salinity and nutrients from Section 3.2 to Section 3.2 as an explanation of mechanisms transporting results of nutrient redox alterations from deep to surface layers. – On the other hand, there are too many trivial if not erroneous (e.g. occurrence of nitrogen limitation only since the 1980s) and confusing (one periodicity preceding another, how NUTLIM for cyanobacteria accounts for nitrogen fixation) considerations in the entire Section including Figs. 8-17 that should be streamlined and put in the context of existing knowledge as some novel proven findings. – Finally, I would insist on the total re-working of your study on nutrient limitation avoiding dubious and rather inconsistent NUTLIM concept, especially as been applied to seasonally variable mixed layer and the layer underneath it, down to 150 m.

2.5.4 Perhaps, it would be expedient to put effects of the light limitation and mixed layer depth into some kind of the "critical depth" concept that is studying the period when phytoplankton is not removed from the suitable light conditions for too long

2.5.5 The entire Section 4 must be somewhat shortened and significantly re-written paying attention to: a) avoiding explicit repetitions about simulated results both within Sect 4 and with preceding Sections, b) clearly indicating the temporal scale of every conclusion, c) cardinally reconsidered Section 3.3, especially about unheard-of phosphate limitation of the spring bloom, d) clarification of confusing statements about maxima and phenological terminology (spring, summer, autumn, late summer, etc.).

3. Minor things, technical corrections and language cosmetics Title – Do you need comma after "simulated"? Lines: 12 – Consider, please, replacing "observed" with "found"; 18-19 & 54 – "...functioning of the biology and the biogeochemistry" – vague and slang-like wording, please, find appropriate formulations, preferably helpful in further considerations; 25 – is it "intensification" (hinting at almost immediate response) or rather long-term "accumulation"?; 28-29 – "...the boundary between anoxic and oxic sediments where denitrification occurs also increases." What increases – the length, the area, why only sediments? Consider, please, "the area of interface between oxic and anoxic zones (opt. – i. e. hypoxic zone)"; 36 – consider "rate" or "velocity" instead of "speed"; 64-68 – this mixture of 3D and 1D here is confusing. Since the details as at p. 165 in Eilola et al. (2009) are not necessary here, just simplify appropriately, something like "biogeochemical interactions are described by the SCOBI model"; 87 – "...described IN sections..."; 124 – accordingly to mimicked mechanism, it rather "accounts for inhibition of the nitrate uptake" than "represents preferential ammonium uptake"; 130-131 – Please, explicitly list the order of phytoplankton types in (9-12) 140 – If you think Eq.13 is necessary here, then, please, check the spelling and explain why do you need multiplication of two constant instead of one constant equal to their product; 159-160 – Is the word "matlab" a universally known term, like salinity of ni-





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trate? Is it important to indicate here, or just "... using the algorithms from Jackett et al. (2006)" would do? 223 – “pre-industrial” - something is needed after this adjective: conditions, situation, trophic state, whatever... 245 – which “scarce observations” if you are dealing with the prescribed regular time-series? Do you really use river time series integrated along the entire Baltic Sea coast for studying a coherence with surface nutrients restricted to your study area, regardless of the all shifts and delays in seasonality? 260-261 – why unnecessary “Furthermore, as described above, . . .”, where it is important to start from the reminding: “In the model, the effect of nutrients on the primary production. . .” 273 – please, consider carefully “. . .increased deep water respiration but also due to increased temperatures resulting in reduced oxygen solubility.” Deep water respiration increases not by itself but due to increased PP and sedimentation. Hypoxic area is delimited by an absolute value of 2 ml/l that has little to do with the relative solubility. 280 – “. . .increased riverine loads. . .” Even according to your Fig.2, they have been decreasing since the 1980s! I think, “the accumulated terrestrial inputs” would be better description; 282-283 – here you go again “. . .since the mixed layer is directly affected by riverine input. . .” On which scale?! Please, estimate pools and compare to inputs! 283 – “The mixed layer also comprises a smaller volume of water.” So, what? Besides, 4,000 (volume of layer 0-27 m) out 12,000 km3 doesn’t look small to me. Fig. 2 – Here and everywhere with the curves I recommend using pretty common convention about color and chemical elements (https://en.wikipedia.org/wiki/CPK_coloring), at least, strictly using blue for N and then red or purple for P. Besides, it could be more logical showing all loads on one graph and concentrations on another. Fig. 4 – why different definitions – is “surface DIN concentration” also a mixed layer concentration similar to phosphate?

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