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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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Response to referee O.P. Savchuk

We thank the referee for his extensive and insightful comments that will greatly improve the manuscript. Our responses are listed below.

1.1 As we understand it, the referee states that causes are already implicitly known since we know the governing equations together with boundary- and initial conditions and that what is needed is just a clever diagnostic of the fluxes. We disagree. In fact, we do not know of a single solution to the Navier-Stokes equations that govern the fluid dynamics of the problem. A consequence of this is that

there is not one single dynamical theory which is founded on the full equations of motion. It is thus by no means, in general, simple to tie some observable quantity, such as a chlorophyll, to a boundary condition. This is, in turn, why we use language such as small scale mixing caused by breaking internal waves even though breaking internal waves are solutions to the equations of motion rather than a boundary condition. The usage of the word "causes" in this way is very common, and we don't see anything wrong with it.

Furthermore, we are aware that this is not the only study of its kind. However, the coherence of different variables on inter-annual time-scales, as considered here, have to our knowledge not been scrutinized before in the Baltic. The referee suggests that our approach needs a specific motivation and justification. However, we look at co-variations between different variables on inter-annual time-scales, which is a very much a classical line of inquiry in many scientific disciplines, but using a relatively novel method and do not see what further justification that could be needed.

1.2 We agree and intend to remove the observational part as is suggested by the referee in a later point. We will also improve the methods section to include a more extensive description of wavelet coherence. Indeed, we will clarify the text so that it is clear that we do not mean to imply causality.

1.3 We agree and will change this according to the suggestions.

1.4 Our main focus is on longer time-scales. However, we have chosen to include seasonality where we see a shift from an earlier different pattern such as during the mid 1970s. We will review and remove the seasonality where we find that we can.

1.5 We will go with the suggestion and remove the observational part. The big problem with observations is that the datasets required for inter-annual coherences do not exist. Long time-series exist only in some spots, and even there, there are typically gaps in the data that makes them unfit for wavelet analysis. This is of course a problem that cannot be helped through literature references.

1.6 We agree with the reviewer and will improve the text and structuring to better highlight the Why?, How? and What?. There are not many references to works on the inter-annual time-scale in the manuscript because few exist. The lack of observations was discussed above.

2.1 A title is supposed to be both informative and to some extent catchy. I think very few people will be interested in having a very intricate division of the Baltic Sea in a title. We will change the title to: "Causes of simulated long-term changes in phytoplankton biomass in the Baltic Proper: a wavelet analysis". The usage of the word "causes" is discussed already in 1.1.

2.2 We will change internal loads to sedimentary release.

2.3 We will improve the introduction by emphasizing the questions and knowledge gap for inter-annual time-scales but we think that the justification for using wavelets is obvious. Classical spectral analysis cannot be used to study temporal changes in frequency, while wavelets can. That is why we use them.

2.4 This will be corrected.

2.4.1 We understand the objections and will rethink what equations we need to include. Indeed, the referee is right and the statement in lines 102-103 about the relation between salinity and the sedimentary phosphorus release is wrong. However, the statements in lines 251-253 do not mean to imply that an increase in sedimentary phosphorus release due to high salinity has anything to do with the observed pattern. Rather, salinity is here thought of as a proxy for water exchange. If mixed layer salinity shows in-phase coherence with mixed layer phosphate while

DIN shows anti-phase, it indicates that the phosphate is accompanied by high salinity water (perhaps upwelled from deeper layers) while DIN is accompanied by low salinity water. We will clarify this and search for any further problems relating to the mistake on lines 102-103. The questions about nutrient limitations are discussed in our answer to 2.5.3.

2.4.2 It is only around the study area. This will be clarified in the updated version.

2.5.1 Section 3.1 will be removed.

2.5.2 Riverine nutrient input is directly put into the mixed layer and the effect is obviously immediate in the vicinity of the river mouth. Further out it is likely set by an advective time-scale (L/V) for the Baltic Proper. This is not to say that exchange with other basins is not important (the reviewers point b)), but we have not quantified those exchanges in this manuscript.

Long residence times for nutrients in the Baltic are in point a) suggested to be a reason for why riverine input should be unimportant for annual and shorter time-scale biomass fluctuations. This is clearly incorrect. The mixed layer nutrient pool is depleted every year. And the fact that the residence time for nutrients in the Baltic as a whole is long appears to us to have very little to do with short time scale biomass fluctuations.

To conclude, we are not convinced by the referees suggestion that riverine input is unimportant on short time-scales. A clear coherence between riverine input and mixed layer concentrations on an annual time-scale in the wavelet analysis is also evident. This, of course, does not imply a causal relationship, but it is to our mind clearly worthy of note. About the use of the term coherence; it is standard mathematical lingo and it does not imply a causal relationship. Surely, this is not something that needs explaining in a science paper.

2.5.3 a) and c) All climate models are sensitive to parameter choices and analysis of simulated variables are standard scientific practise. We can't see any reason why these practices need defending here.

b) It is true that NLIM under certain conditions may obtain values >1 but this does not imply that this would amplify growth. PLIM <1 gives P limiting conditions if NLIM >1. NUTLIM is the quantity that the model cares about and it is always 0 <NUTLIM <1. No problem there apart from the cosmetics of having NLIM >1. This answers also the questions put forth in 2.4.1 about the limiting functions.

Regarding limiting functions. We are analysing modelling results and thus we need to use the rules that govern the models growth rate. Therefore, we disagree that it would be better to use N:P-ratios and believe that using some other limitation than what determines the growth in the model would be very confusing. Thus, the statement that the usage of the NUTLIM concept in our analysis leads to misleading results is incorrect, as it illustrates the actual workings of the model. However, we think that the criticism put forth by the reviewer that NUTLIM and N:P ratios may lead to different nutrient limitations is important, and we will highlight this in the revised manuscript. About fig. 8 it does not show simultaneous limitation by both nutrients. Rather it is the size of the rings that indicate the monthly values that give this appearance, we will add a note in the figure caption.

2.5.4 For some quantities this might be helpful but we prefer the mixed layer concept, as is much more straight forward when it comes to the physical quantities. The sharp pycnocline inhibits vertical transfer, and is therefore a more natural choice for studying variations in N and P concentrations

2.5.5 We agree with the referee and will rewrite section 3.3 in accordance with the review comments. The phosphate limitation of the spring bloom produced by the model will be discussed. This can also contribute to the new discussion about the NUTLIM vs N:P ratio concept that will be added

3. We have no objections to the minor comments, with the exception of the riverine

input where we have given our view in 2.5.2