Response to comments by anonymous reviewers #1 and #2

General statement:

The paper aim was to study the long term trends in the climate parameters (average temperature, wind speed and solar irradiance) on phytoplankton and nutrient in the Baltic Sea was studied with an integrated three-dimensional coupled sea-ice ecological model. This is a scientifically important research topic especially in a situation where most of the future forcings (sometimes even the sign of future trends) are unknown.

At this point, we have two negative reviews on the table. However, practically all the comments in both reviews concentrate on one aspect only: the nutrient cycle. From our point of view it is only one of the poorly constrained input parameter and in our opinion the reviewers completely missed the point of the paper which was to check the range of possible future variability in primary productivity, not to predict it (which is at present impossible with future forcings in the region of the Baltic Sea so poorly constrained)

Response to reviewer #1

#1.0
This manuscript present results from three long term experimental simulations with a coupled hydrodynamical-ecological model in the Baltic Sea. The purpose of these simulations is to estimate the influence of altered atmospheric forcing on phytoplankton and nutrient concentrations. This is a highly relevant topic but unfortunately the overall quality of the manuscript is poor regarding both presentation and scientific content and can not be recommended for publication in Ocean Science.

We feel it disappointing that the reviewer did not feel the manuscript should be recommended for publication. The presentation quality obviously could be improved in the revised manuscript while scientific content is what the more detailed comments below are about and we do not agree with the most important one (lack of phosphorus cycle in the model) and only partly with the second one (lack of some processes in our nitrogen modeling) – see below.

The main concerns are stated below.

Validation of both physical and ecological model:

#1.1
One of the most important aspects of ecosystem modelling is the underlying physical model. The authors only present model-observation comparison of SST and only for the southern Baltic Sea and no reference is given to a more thoroughly validation with this model set up. The SST is mainly determined by the atmospheric forcing and this really does not give much information about the model performance.

The sea surface temperature (and more generally sea water temperature) is determined by short and long wave radiative fluxes as well as sensible and latent heat fluxes through the sea surface (this is because the geothermal flux at the sea bottom may be safely neglected as it is several orders of magnitude smaller). All the above fluxes may be parameterized using just three parameters: short wave downwelling irradiation, sea surface temperature (deemed to be practically equal to the near-surface atmosphere temperature) and the wind speed. This is exactly what is used in the model we ran in the study (and ther models as well). We explicitly described the data used for all three. More generally, the physical part of the model is well tested as it as Parallel Ocean Program and Community Ice CodE (POPCICE) from Los Alamos National Laboratory (as stated in the manuscript). We do not believe we need to provide additional validation tests in the scope of this study.

#1.2
For instance, how well is the vertical stratification resolved?

As stated in the manuscript the Baltic model had 18 depth levels. The first three layers were each 5 m deep. We believe that 18 levels for the shallow Baltic Sea is certainly a better resolution than 21 levels for the ocean in the standard POPCICE runs.

#1.3
Is 9km horizontal resolution in the Danish Straits enough to model the water exchange between the North Sea and the Baltic Sea?

No, but we do not know of any existing model which is able to represent realistically the historical major inflows of North Sea water. Therefore, expecting our model to correctly predict the future inflows which depend anyway on the unknown future atmospheric forcings would be unrealistic (by the way, the importance of this unknown for the future primary productivity in the Baltic is one of our main conclusions). We do plan (as stated in the manuscript) to increase the resolution to eddy resolving 2 km one as one of the next stages of the model improvement, but this is not part of the ECOOP work reported in this manuscript. We hope the reviewer will agree that waiting for an ideal model with result publishing would effectively eliminate all the modelers from the academic world. Especially as the progress of this model seems to be driven by paper reviewers (like in "an unending race with reviewers' demands").

#1.4
The validation of the ecological model is also far from sufficient. A comparison of modelled and observed Chlorophyll concentrations is seen in fig.6b but it does not give much information. DIN concentration is not even compared to observations. The model needs to be thoroughly and quantitatively assessed before this kind of sensitivity analysis can be made.

We believe this research included effort towards this goal. The model is constantly updated and new processes are added. However a proposition that we should not do research before the model is perfect would mean we should not publish anything until we die. And this is equally true about all modeling efforts.

We believe that the version of the 3D model we used was good enough to do this kind of study of possible range of variability in primary productivity. However we concede that the nitrogen cycle in this version of the program was not its strongest card (we agree that it was not validated as well as chlorophyll) and we are ready to modify the manuscript text and conclusions, giving the reader a caveat about the nitrogen cycle.

#1.5
The ecological model is too simple:
It is questionable if such simple model can be use in this kind of long term simulations. Neither phosphorus nor oxygen is included in the model. Increasing the wind 30% will definitely have a large impact on the bottom oxygen condition which in turn will have an effect on the sediment phosphorus release and the N:P ratio in the water. This change could affect the nitrogen fixation (which is also not included in the model) an in turn

the primary production and phytoplankton concentrations. The authors mention this at p.538 ": : :although cyanobacteria overcome N shortage by N-fixation, so primary production is limited by available phosphorus" there is no justification on how this process can be neglected.

The aim of this study is not to correctly model all biogeochemical cycles in the Baltic but to study the impact of climate forcings on Baltic primary production. There seems to be consensus (see for example Graneli et al 1990, Savchuk 2005 or Savchuk & Wulff 2009) that the limiting factor (apart of short wave irradiation) in the Baltic is nitrogen and where it is not the situation is close to the Redfield equilibrium suggesting co-limitation by N and P (the exceptions, namely the blooms of nitrogen deficit tolerant species like blue-green algae in the Baltic Proper in the summer or nitrogen fixing ones like and late summer cyanobacteria blooms confirm rather than deny the nitrogen limitation). This may change in the future but mostly because of factors that are beyond the scope of sea circulation models (namely anthropogenic load changes). Of course adding additional elemental cycles in the model could and should be done in future, but increasing the number of poorly constrained processes may not actually help understand whether the model works well for the most important ones. If one has doubts about the N limitation of Baltic primary production, what (s)he needs is more observations, not modeling, at this stage. In addition to that, wind forcing (mixing) works similarly for both nitrogen and phosphorous, increasing the supply of both with more upwelling so modeling only one of them gives a first order approximation of the other.

By the way the example given by the reviewer (more wind would mean more mixing, therefore more oxygenated deep waters, therefore less sediment phosphorus release, meaning nitrogen would be less limiting) is interesting but we do not believe the processes are constrained enough to enable this kind of modeling). Anyway the large observed export of phosphorus to Skagerrak (Savchuk 2005) seems to be a buffer for this kind of process and an additional evidence of nitrogen limitation in the Baltic. This line of argumentation explains also why oxygen is not a necessary element of the present study (oxygen is not limiting anywhere in the Baltic euphotic zone except maybe for some highly eutrophic coastal lagoons, to small for this kind of a model anyway).

#1.6
To my understanding the zooplankton biomass is prescribed by observation. What is the reason for not including it as a state variable? It is highly unlikely that the zooplankton biomass will stay constant while the phytoplankton biomass increases. At page 542 "the zooplankton biomass is prescribed as a force and it uses abundance data from the Mankowski (1978), Ciszewski (1983) and Mudrak (2004) for the southern Baltic Sea". But the model domain covers the whole Baltic Sea, the Kattegat and the Skagerrak. What data is used here?

The following text will be added to the manuscript if the editor "if she/he would encourage submission of a revised manuscript" (quoting a phrase from an email explaining the OS reviewing process):

Description of the method:

Any periodic variability can be represented by sum of harmonic functions. The main method of analysis of such signals is the Fourier transform. Assuming, that seasonal and long term variations of the zooplankton biomass are periodic, we can use the following expression:

$$Zoop = Z_o + Z_a \cos(\omega t + a) + Z_b \cos(2\omega t + b)$$

where:

$Zoop$ – zooplankton biomass

t – time in year
Z_o – mean annual value
a, b, Z_a, Z_b – initial phase and amplitude of the first and second harmonic
$\omega=2\pi/365$

The coefficients in the equation were obtained on the basis of experimental data from the southern Baltic Sea .

#1.6
What kind of data is use for the lateral boundary condition in Skagerrak?

Skagerrak lateral boundary condition is done using the restoring method. We add to the calculated value the difference between the model result and climatology
Tmodel = Tmodel + (Texperyment – Tmodel)/tau
where tau is a time constant (we use 30 days).
We use restoring for temperature and salinity in the North Sea and Skagerrak with a linear decrease towards the Baltic Straits.

Therefore the flows on the boundary are zero by definition.

#1.7
Conclusions are not supported by the presented results:
All conclusions are based on surface nutrient and chlorophyll data, what about the subsurface concentrations? A more accurate analysis would be to compare the vertical integrated primary production.

Considering that the paper deals with the Baltic, a Water 2 Case basin of high light attenuation values and low solar zenith angles, the euphotic zone is only a few meters deep. Therefore what the reviewer suggests is tantamount to using the first (surface) layer of the model we used. Which is exactly what we did.

#1.8
A fourth reference simulation would also be beneficial.

Possibly, but it is not clear to us what the run would involve.

P. 548 "The results show significant changes in phytoplankton biomass Phyt distributions, which take place in areas (open sea), where there is a considerable increasing in currents" - This change in currents is not presented at all.

We are ready to add (not shown) to the sentence.

P. 548: "It is the result of the rise in nutrient concentration Nutr (Fig. 11) in the upper layer caused by the increasing of the wind speed, i.e. by mixing deep."
- This increased mixing is not documented. At least some salinity profiles should be presented.

We are ready to add a figure presenting this to a revised manuscript.

Next sentence says P.548: "With the parameter values in scenario 2 and 3, for increasing turbulence (mixing) (30% increased wind speed and western component of wind *speed, : : :)". - I do not understand this at.*

This will be clarified in a revised manuscript if we are advised to submit one. The point of the sentence was to say that in the runs with increased winds, an increase of both mixing and phytoplankton mass were increased. By the way "mixing deep" will also be changed to "mixing depth".

P. 549: "The results are consistent with in situ observations for temperature and chlorophyll-a for five years (2000-2004)."
- Only SST have been compared to observations and it has a significant bias of 1.4 degrees C. From figure 6b I would not say that the observations are consistent with observations.

Well, we do agree that the term in question does have a qualitative nature. We promise to make it clearer in any revised version.

Other remarks:
The manuscript is sometimes difficult to follow. The language needs to be improved and the structure is sometimes confusing. This is especially true for the model description. Page 539 - 541 includes four model equation but only 3 of them are numbered. Integral limits are missing, misprints and new equations are presented in the section "parameters".

All this will be corrected in the revised manuscript, if any.

At page 540: "The state Eq. (2) for nutrient includes the first four terms on the right hand side (/: : :/) and the four processes nutrient uptake (UPT), dark respiratory release (RELE) : : :", but (RELE) and (UPT) is not visible in eq.(2). Instead one need to look in Table 1 to see that RELE=RESPdark. This section must be made clearer.

Agreed.

Presentation of the results and discussions are mix together which leads to confusion of what is actually modelled.

Also agreed. We will try to untangle this.

Response to reviewer #2

Because almost all of reviewer #2 comments in the "general comments" section repeat arguments of review #1 (available on-line 12 days before Review #2 was submitted which should help avoid repetition), we respond to them by referencing to the comments by reviewer #1 (numbered for this purpose as comments #1.x).

The aim of the submitted MS was to investigate the influence of long term trends in climate variability on temperature, nutrients (DIN) and phytoplankton dynamics in the Baltic Sea using a 3D coupled ecological model. However, the applied biogeochemical model does not consider phosphorous even though the N:P - ratio is important for the competition between species in the Baltic Sea and primary production.

This was explained in response to comment #1.5

Blue-green
algae are completely ignored although they make harmful blooms every summer.

This also has been explained answering comment #1.5. Blue-green algae will be added to the last but one sentence of section 2.1 if we are advised to prepare a revised manuscript.

Zoo-
plankton is also not dynamically described.

See the response to #1.6

#2.1
River loads were ignored in this set-up
although the Baltic Sea is highly eutrophicated due to run-off from a large catchment area.

This is a problem we already work to solve in the next version of the model. However future inflows of nutrients from both rivers and atmospheric deposition are very poorly constrained making the nutrients (including nitrogen) another parameter which variability effect on the future Baltic ecosystem should be studied. We have plans to do that using the next version of the model.

We did see this is a problem and we realize that this is the thing the future of this manuscript hinges on. Our nitrogen cycle is in the version of the model the calculations were done with was still rudimentary (no river inflow and deposition, no outflow through the Straits, the limiting factor is total biologically available nitrogen (NO_3+NO_2+NH_4) treated as one parameter, no nitrigen fixation, denitrification and nitrification). However because in the control run (no forcing parameters changed) the interannual nitrogen concentration drift was very small (not shown) meaning that the processes we missed in the model roughly balanced out on annual scales, we decided that the model is ready for this kind of work where nitrogen is meant mostly to limit primary productivity (which it did), in other words we have the processes that control the nitrogen cycle in short time scale . We plan to add a comment about this in a revised manuscript.

I believe that the applied model is too simple to make realistic long-term scenarios.

This also has been replied under comment #1.5 which first sentence is echoed above.

In addition, the model was only validated for the southern part of the model domain and for surface values of T and phytoplankton, and not for DIN. Model scenarios outside the validation area are therefore questionable.

This comment repeats comment #1.1 making our response below it valid also here.

I therefore suggest to reject
the MS for publication in Ocean Sci.

Our response for comment #1.0 is also valid here.

Specific comments:
p.1, line 1: a parameter is a constant. Don't you mean climate variables?

I know this is outside the scope of the article but because the review and response will be available on-line I will correct the above misstatement for the record.

The etymology of the word parameter supports its original meaning as a non-observable (it meant "outside measurement"). However the present usage is completely different. In physics (at first thermodynamics) parameter was used in the meaning of variable for over 100 years. What are "state parameters" we learned about in school? Usually temperature and pressure. Certainly no constants. Checking the exact phrase "parameter variability" (in quotation marks) in http://scholar.google.com/ returns over 5000 scientific papers which authors believe parameters can be varied, many of them use the phrase in the title.

The modern dictionaries support this unequivocally. Of the three that are available online only Merriam-Webster http://www.merriam-webster.com/dictionary/parameter
mentions the word "constant" but only in the context of mathematics (meaning #1) as "*an arbitrary constant whose value characterizes a member of a system (as a family of curves)*". An "arbitrary constant" which can have different values is certainly no physical constant. However the same dictionary gives another meaning (#2) of the word as "*any of a set of physical properties whose values determine the characteristics or behavior of something <parameters of the atmosphere such as temperature, pressure, and density>* ". The example (underscored) is exactly how we use the word.

Encarta http://encarta.msn.com/dictionary_/parameter.html gives five meaning, never mentioning the word "constant". Two most relevant for science both describe parameters as variables:
*2. **variable quantity determining outcome:** a measurable quantity, e.g. temperature, that determines the result of a scientific experiment and can be altered to vary the result*
*4. mathematics **variable mathematical value:** in a mathematical expression, a variable value that, when it changes, gives another different but related mathematical expression from a limited series of such expressions*
(the emphasis is in the original text). We clearly use parameters (actually including temperature) that determine the result of a scientific experiment and can be altered to vary the result.

Brittanica http://www.britannica.com/EBchecked/topic/442983/parameter gives only the meaning in mathematics as:
***parameter,** in mathematics, a variable for which the range of possible values identifies a collection of distinct cases in a problem [...]*
the word constant is used nowhere in the definition.

I believe no more evidence is needed that in modern usage of the word a *parameter* is a *variable*.

p.1, line5: 'A simple ecosystem model: : :'

OK. Thanks for catching this.

p.3, line 13: parameters or variables?

As shown above, parameters are variables.

p.3, line 16-19: did you mean that the nutrient loads from atm. and rivers were ignored or kept constant?

This has been explained above under the comment about river loads (#2.1).

p. 4, line19: the spring bloom is triggered by increasing light. Nutrients are not limiting this time of year.

Point taken. The sentence will be rephrased to say that in the Baltic nutrients are necessary for the spring blooms triggered by other parameters such as light availability (Wasmund 1998), increased temperature (Snoeijs 1990) and spreading of freshwater (Hordoir and Meier 2011).

However, the Sverdrup (1953) hypothesis saying that springs blooms start when euphotic zone becomes deeper than mixing zone, which was a paradigm for over a 50 years, has been recently questioned by Behrenfeld (2010) ["Abandoning Sverdrup's Critical Depth Hypothesis on phytoplankton blooms" Ecology 9(4), 977-989], at least for North Atlantic. Behrenfeld proposed in its stead a "Dilution– Recoupling Hypothesis" explaining the onset of spring blooms with the balance between phytoplankton growth and grazing, and the seasonally varying physical processes influencing this balance, the kind of processes which are a trump card of modeling group of our first author (actually the zooplankton – phytoplankton balance dynamics was the original reason why this modeling effort has been started).

p. 6, line 10-18: I find the model too simple since it does not contain P. The N:P ratio is very important for the outcome of the competition between phytoplankton species fx. diatoms and blue-green algae. Also, the model does not describe blue-green algae growth which is an important feature during summer in the Baltic Sea.

This has already been answered under comment #1.5

p. 10, line 7: River loads were ignored even though you stated on p.3, lines 5-10, that nutrient loads from rivers are important? I don't see how it makes any sense to run the model without river loads in a closed estuary receiving high amounts of nutrients from the catchment area?

Again, this has been commented and answered above (#2.1).

p.12, line 22-24: the comparison is only done for the southern Baltic Sea and the surface area. Also, DIN has not been validated at all. But results from the scenarios are shown for 9 stations all over the Baltic Sea – not validated by the model. I suggest that the authors focus on the southern part only, if the model has not been validated elsewhere.

Again, this has been replied under #2.1

Fig. 1. Delete the figure to the right and use real depths instead of model levels. The reader is probably more interested in depths than in model levels.

Agreed.

Fig. 2. maybe use 'predation mortality' and 'other sources of mortality'

Agreed. Again, thanks for catching it.

Fig. 5. There seems not to be any spring bloom and generally very low chl a concentrations in the Kattegat? Also, DIN concentrations are very low west of Bornholm.

Correct. However the Figure presents this are the results of the modeling, not observations. All we can do is to comment on the discrepancy in the text of a revised manuscript.

Fig. 6. Why not show the correlation for DIN? Does the data come from the stations in fig 7?

Because DIN is neither the real product of this paper nor a conclusion point. In case we do a revision, the text of the manuscript will be screened to remove anything that might suggest otherwise (as a part of the process of making the text more homogeneous and better flowing).

Is it monthly means or point-by-point comparisons?

The manuscript already explains that the "analysis of the modelled surface concentration of chlorophyll- a Chl mod (value for the first of 5 m layer) was carried out jointly for the entire experimental material, i.e. for 196 points [...]".

Jacek Piskozub
for all the authors