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Interactive comment on "Climate change in a conceptual atmosphere–plankton model" *by* György Károlyi et al.

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Reply to reviewer 2

We thank the reviewer for a thorough reading of our paper and for the constructive comments and suggestions that, we believe, improved the presentation of our results. We reply all comments and indicate the changes made in the manuscript.

This manuscript describes a conceptual modelling study of how phytoplankton respond to changes in temperature, atmospheric CO2, and ocean mixing. In this study the authors attempt to use a relatively simple model that contains many assumptions to simulate phytoplankton growth as a function of changes in anthropogenic forcing. While I am not opposed to simple conceptual models that focus on key processes or dynam-

ics, I feel that the conceptual model presented here makes many assumptions that are not well justified.

We thank the reviewer that he is also in favor of conceptual models like the one we present here. We agree that our model only grasps the most important processes only in the simplest possible way, and neglects many effects that need to be addressed in an extended model. We believe, however, that the processes we include in the model are well justified. To better support this statement, we added a new schematic drawing (new Fig. 1, also attached to the reply) to illustrate the processes and feedbacks we include in our model. We believe that it is possible to extend our model by sequentially adding more and more details into it, as we now describe this in the Conclusions. We believe that applying this approach allows us to identify how individual components contribute to the overall behavior of complex models. We also explain this more explicitly in the Introduction.

To give more explanation why we think our model is well justified as a conceptual model, we added the following new text into the Introduction:

`In spite of the current trend to include biogeochemistry in climate models (see e.g. Schlunegger et al, 2019), a basic understanding of such processes is still limited. ... The situation appears to be similar to the understanding of thermal or fluid dynamical concepts decades ago. The study of e.g. the energy balance Ghil (1976) or of the thermohalin ciculation Stommel (1961) started with elementary conceptual models which later evolved into more complex ones, and are by now decisive components of cutting-edge climate models. We therefore propose here to study a conceptual atmosphere-plankton model where emphasis is on a proper choice of couplings (feedbacks).`

We also added the following new text into the Conclusions:

As far as we know, our work is the first step in the direction of studying the feedbacks between the atmosphere and the biosphere by a simple conceptual model. As such, both the biological and climate models are highly simplified. However, one can consider it as a starting modul of an extendable model system.

And a bit later we emphasize:

We think that mutual interactions and iterations between conceptual models and detailed Earth System Models (ESM) help to reveal the disctinction between relevant and less relevant mechanisms and feedbacks behind climate change. We expect deeper insight into these feedbacks by studying conceptual and ESMs parallelly in the future.

In addition, many relevant dynamics seem to be left out, e.g., other bottom-up controls on phytoplankton growth such as light and nutrients like N, P, and Fe, as well as top-down controls like grazing and mortality. While, it may be possible that global phytoplankton-climate dynamics and responses to forcing can be explained without such factors (although I doubt it), in order for me to have confidence in their model I would need to see better evidence that justifies the model simplifications and model parameterizations. Many other conceptual studies of phytoplankton use laboratory studies or observations to justify their model structure and parameterizations, surely this can be done here as well.

We agree with the reviewer that our treatment of phytoplankton neglects many important factors, this is why we call it a "conceptual" model already in the title of the paper. We point out that our choice for the ecological component in the form of a very simple logistic equation is motivated by the similarly simple representation of the full atmosphere by merely three ordinary differential equations of the Lorenz'84 model. We feel that the coupling of this simple atmospheric model to a detailed ecological model would be inconsistent. By taking such a simple set-up, we intend to strengthen the conclusions of more complex models by obtaining exact results in a simple transparent conceptual model. We prefer to work with a well understood, but at the same time paradigmatic description which enables one to explore the mechanism of basic feedbacks. Thus, for example, our approach makes possible to study the feedback of the primary producer on the temperature contrast which, in turn, drives the atmospheric dynamics. This might remain hidden in the complexity of current GCMs.

In the new closing section of the Conclusions we formulate that the present approach is considered to be a starting project on which a full hierarchy of models can be built. The next step in this hierarchy can be a still conceptual, combined atmosphere-ocean model to which a simple ecology model with more trophic levels and inorganic resources can be coupled.

These points are now added to the text in the Conclusions:

`On the one hand, trophical levels and inorganic resources can be easily added to the biological side of our model, on the other hand, simple ocean circulation models can extend the climate side of our model in order to make a first step to build more complex coupled models (Daron and Stainforth, 2013).`

In addition, and perhaps more importantly, for me to have confidence in the model there needs to be some validation, i.e., comparisons to actual data. I know that the goal is to simulate climate change and obviously one cannot validate future projections. However, it should still be possible to come up with a clever way to validate the model (or key underlying equations) using observations. There is also no real attempt to contrast the results of this study with other phytoplankton focused climate studies that have used ocean-only or Earth system models (e.g., see citations listed below). Without satisfactory justification and validation of the model and the results this is just *a mathematical exercise that while interesting, leaves the reader wondering if it has meaning. Therefore, I must recommend that the manuscript be rejected.*

We thank the reviewer for the citations, we found them immensely useful to put our work into context, and we added all of them to the list of references. Papers like these give the most important motivation for conceptual models like the one we present in our paper. These papers shed light on the fact that it is still under debate whether net primary production is increasing or decreasing in coupled carbon–climate models as a consequence of warming induces production increase and stronger nutrient limitations induced by increased stratification. We also compare now our findings with the conclusions of these papers.

To sum this up, we added the following new text to the Introduction:

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In spite of the current trend to include biogeochemistry in
climate models (see e.g.  Schlunegger et al, 2019), a basic
understanding of such processes is still limited.  It is still
under debate whether net primary production is increasing or
decreasing in coupled carbon-climate models as a consequence
of warming induces production increase and stronger nutrient
limitations induced by increased stratification (Laufkötter et
al, 2015).
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And:

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The direct effect of increased CO2 concentration on
phytoplankton dynamics can be stimulating or inhibiting, we
study both scenarios.
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We added to the Conclusions the following new text:

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One of our main results is that an increase in the global
temperature reduces mixing intensity, which is the leading
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factor in decreasing the total biomass of primary producers.
Interestingly, this result is in concordance with numerous
studies applying Earth System Models with vastly more
detailed plankton models (Bopp et al, 2013; Fu et al, 2016;
Kwiatkowski et al, 2019), although other works report different
observations (Laufkötter et al, 2015; Flombaum et al, 2020).
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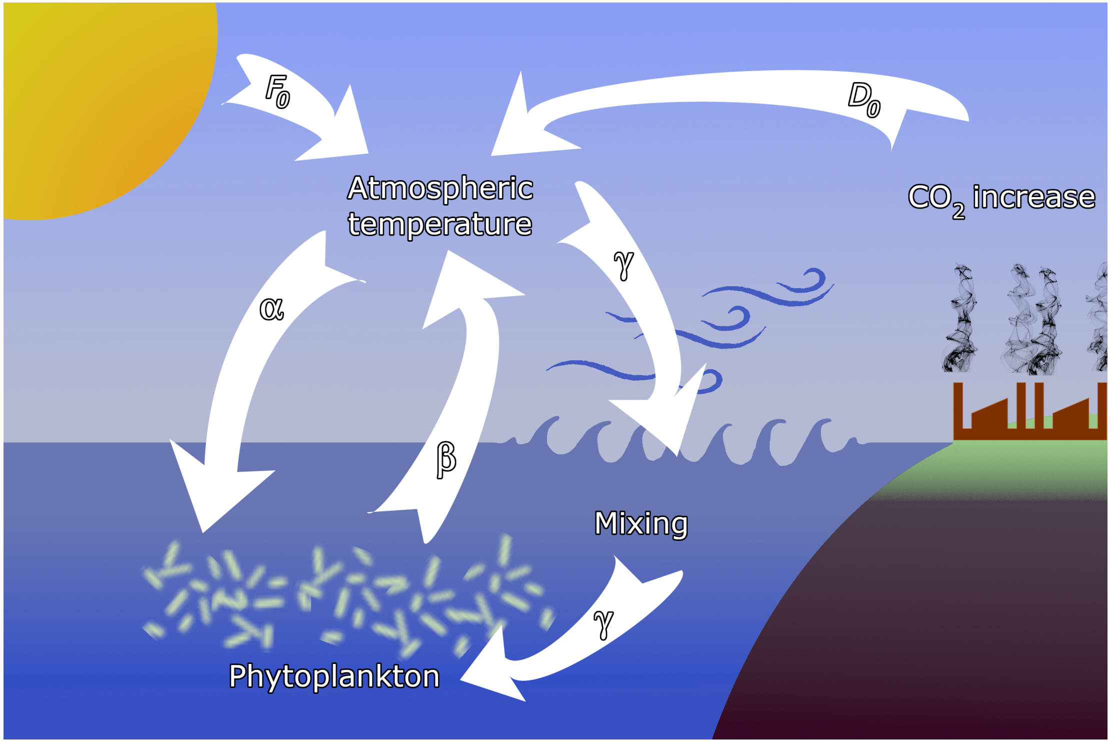
We thank the reviewer again for his insightful comments, and especially for the very useful citations that support our conclusions that global warming is expected to reduce the total biomass of primary producers. We hope that with the changes we made to the text, our paper can be accepted for publication in ESD.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-75, 2019.



F_0
D_0
Atmospheric temperature
CO_2 increase
γ
α
β
Mixing
γ
Phytoplankton

Fig. 1.