We thank the reviewer for the extensive feedback on our manuscript.

Regarding the reviewer's first criticism: The model we presented is largely identical to the model already published in Hochfeld & Hinners (2024). This means that the simulated phytoplankton population dynamics are identical in the previously published version and in the version presented in this manuscript. However, to be able to analyze ecosystem functions, we had to modify the output parameters to be able to estimate carbon export, nitrogen fixation, and resource use efficiency (RUE). Regarding RUE, we moreover had to exclude the zooplankton and the cyanobacteria from the simulations, as their grazing and nitrogen input, respectively, would make a meaningful calculation impossible. Currently, this is described in lines 179−198. We propose not to generally refer to the model as “modified” or “extended” in a future version of this manuscript to avoid confusion, but to clearly describe the model modifications in comparison to the published model.

Regarding the second criticism of the reviewer: As described in the response to reviewer 1, we understand that our motivation to introduce the model to readers by summarizing the results of the published article (Hochfeld & Hinners, 2024) has led to confusion about what is new in this manuscript. We therefore suggest moving Fig. 2 (which, as reviewer 2 points out, overlaps with the results of Hochfeld & Hinners, 2024) to the appendix and focusing our manuscript more clearly on the new results on ecosystem functioning. Since our model, as described above, is largely identical to the previously published model, we consider it more appropriate to focus the model description in this manuscript on the modifications rather than, as the reviewer suggests, to describe the model again in detail, which we have already done extensively in the previously published version (12 pages in the supplementary material). Unfortunately, we cannot follow the reviewer's criticism of our lack of discussion of model limitations. We discuss the assumptions and limitations of our model very specifically for the different aspects examined in the discussion (lines 411−412, 466−469, 480−482, 502−513, 542−545, and 581−584). We can only assume that the reviewer did not find this clear enough. We suggest that in a revised version of the manuscript, we summarize the model biases more clearly in a separate paragraph in the discussion. If we are invited to submit a revised version of this manuscript, we would be thankful for a clear guidance by the editor how to deal with these criticisms regarding model description and limitations, whether to follow our suggested modifications or the modifications asked for by the reviewer.

Regarding the reviewer's third criticism: We thank the reviewer for the extensive list of articles that investigated phytoplankton diversity dynamics. As far as we understand, most of these models deal with diverse phytoplankton populations and thus allow for one aspect of evolution: selection from a diverse pool of individuals. The second important aspect of evolution, the possibility of new mutations, is not taken into account by almost all of the models mentioned. In our introduction, we focus on the ecosystem modeling studies that take both aspects of evolution into account. This is still a new approach that has only been implemented in a few studies so far (e.g., Beckmann et al., 2019; Hinners et al., 2019; Le Gland et al., 2021; Sauterey et al., 2017; Smith et al., 2016). In the revised version, we propose to include a more detailed description of models that investigate phytoplankton diversity dynamics mentioned by the reviewer. In previous review processes, we were advised to support each statement with a maximum of three references. We found no clear information on this for *Biogeosciences*. We would be grateful for a brief statement from the editor on the maximum number of references that should be given per statement.

Regarding the final criticism of the reviewer: The available long-term data for the Baltic Sea are unfortunately not sufficient for an extensive quantitative model calibration and validation. Data on species level are sparse and provide insufficient temporal resolution/coverage to calibrate or validate our model with respect to bloom timing and relative abundances of our focal species. Instead, we used data on functional group level from Hjerne et al. (2019) to validate the model qualitatively regarding bloom timing and relative abundances of phytoplankton groups. We suggest including a subsection on model validation into the results section and a discussion of the missing quantitative calibration and validation into our description of model limitations.

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