Summary

A 1D ecosystem model parameterized for the Baltic Sea is used to assess impacts of viral infection on biogeochemical processes (primary productivity, carbon export). A set of experiments are performed, with and without a combination of viruses and warming conditions. The study finds an interactive effect of warming and temperature on primary productivity, and carbon export.

Main comments

The study is generally clearly written, and the results are novel and interesting. The findings have clear implications for our understanding of viral impacts on ocean biogeochemistry in a future ocean. I did find a few areas where results were presented confusingly. There are also some formatting issues that may need to be addressed, and one minor query about model structure. These are all outlined below.

- We thank the reviewer for the positive comments on our study and suggestions to improve the manuscript. Below, we address all the specific comments.

Specific comments

General comment: the manuscript says ‘code available on request’. It would be nice to make this available in such a way that others might be able to reproduce the findings.

- We fully agree and we will provide the link to the data repository in the revised version of the manuscript.

Page 2 line 50: “to our best knowledge…” a recent study assessed viral impacts on ocean biogeochemistry in a 1D setting at two ocean sites (Xie and Zhang 2022). I recommend *including this citation and outlining how the present study differs.*

- Thank you for pointing us at this study, which we have missed. We will include this study in our citations and set it in context with ours.

Page 6, equation 19: it looks like this is linear w.r.t. P? So, equivalent to Holling I, aka mass action? This implies the rate of grazing is unbounded, such that at very high P concentration, it can become quite large. It’s a little unusual not to bound the rate of grazing with Holling II, Michaelis-Menten, or similar (e.g., Gentleman et al. 2003). Would be nice to see if including this makes a big difference to the results.

- We decided to model both the viral lysis and grazing as contact-based rates. We chose this design following the study by Talmy et al., 2019. We chose this description to model both viral lysis and grazing dynamics in the same manner. While we agree that the unbounded grazing could potentially overestimate grazing under high phytoplankton and zooplankton concentrations, we do not see any sign for this in our results. Even if we exclude viral presence, zooplankton grazing does not lead to an inhibition or a rapid termination of the phytoplankton bloom. In fact, the phytoplankton mortality caused by zooplankton grazing in our simulations is in the same order of magnitude as laboratory experiments, as described in L168. However, we are happy to discuss the potential

overestimation of zooplankton grazing if the model is applied to other systems in the revised version of the manuscript.

Page 7 line 159-161: "…qualitatively matches…" I'm fine with this sort of qualitative comparison, but am I right in saying that for me to evaluate the consistency, I need to access Hjerne et al. 2019, and determine which of their data is being referred to? This seems like a heavy lift, and I suspect most readers will not make the effort. Can these data be recreated here, as you have with the Mojica 2016 data (Figure S7).

- We do not have the raw data from Hjerne et al. 2019, so we are not able to recreate their results. We compared our simulations to the visualization in their study (Fig. 2). However, we can plot our supplementary data (S6) in a similar way to the data by Hjerne et al to allow for a better comparison with our model simulations. For clarification, the data in Fig S7 is our own collected data on phytoplankton mortalities caused by viral lysis and grazing, not data from Mojica et al. 2016.

Page 7 line 166: "The maximum mortality…shorter than that caused by grazers". I was curious as to why this is. I couldn't find it explained in the discussion. My apologies if I missed it. If an explanation hasn't been provided, please consider including one.

- Thank you for pointing us at this, we are happy to discuss this in more detail in the revised version.

- These dynamics are caused by the contact-based description of our viral lysis and grazing. While viruses in our simulations with a very small diameter of 43 nm have a higher maximal clearance rate, zooplankton with their large size are more competitive at low zooplankton and virus abundances.

- In early spring, as long as zooplankton and viruses are rare, zooplankton graze more effectively due to their larger size resulting in a higher contact-based grazing rate. For this reason, zooplankton abundances (and their grazing mortality) increase earlier in the year. Once viruses become more abundant though, their rapid increase in numbers outcompetes zooplankton, leading to a drastic increase in virus-mediated mortality and a rapid decline of the phytoplankton spring bloom.

Page 7 line 168: "(Fig S7)". Slightly pedantic on my part, but I expect the figure numbers to appear sequentially in the manuscript. This is the first supplemental figure and it goes straight to figure S7. Where are figures S1-S6 discussed? Are these discussed in the main text? Please make sure that all supplemental figures are discussed in sequence in the main text.

- We will make sure figures S1-S6 are discussed in the revised version of the manuscript and that all figures are mentioned in chronological order.

Page 8 line 176-180: "depth resolved … detritus production" I don't understand the reasoning here. What does it mean to say that "higher temperatures seem to play a bigger role than stratification"? Surely temperature is mechanistically linked with stratification? Do you mean to say that, the effect of temperature on biological rates has a stronger impact than the effect of temperature on stratification? If so, how can you conclude this? It's not at all clear to me what is being said here.

- Thanks for raising this question, we will clarify this in the revised version. Warming can have two contradicting effects on phytoplankton. On the one hand, it can increase primary production due to the warming-driven stimulation of growth. One the other hand, warming can cause a stronger stratification of the water column, prohibiting the supply of nutrients from deeper water layers, thereby limiting primary production. Because the net effect of warming on phytoplankton biomass is positive in our simulations, we conclude that the stimulation of growth by temperature increase plays a larger role than the nutrient limitation resulting from increased stratification.

Page 8 line 184-185: "The earlier onset …. Causes an earlier increase of virus biomass". I can't seem to find an explanation here or in the discussion for why this is the case. Apologies if I missed it. If an explanation isn't included, please provide one.

- The earlier onset of the spring bloom is caused by the earlier increase in water temperature in the "Future" and "Future+Viruses" scenarios. We will clarify this in the revised version.

Page 8 line 196: "to our best knowledge" as above, there is one study with viruses in 1D (Xie and Zhang 2022). Please cite and explain how the present study differs.

- As mentioned above, we will set our study in context to this study in the revised version.

References

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- Hjerne, O., Hajdu, S., Larsson, U., Downing, A. S., & Winder, M. (2019). Climate driven changes in timing, composition and magnitude of the Baltic Sea phytoplankton spring bloom. Frontiers in Marine Science, 6, 482.

- Talmy, D., Beckett, S. J., Taniguchi, D. A., Brussaard, C. P., Weitz, J. S., & Follows, M. J. (2019). An empirical model of carbon flow through marine viruses and microzooplankton grazers. Environmental Microbiology, 21(6), 2171-2181.