Biogeosciences Discussions

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Comment on bg-2022-249

Anonymous Referee #1

Referee comment on "Modelling the interactive effects of viral presence and global warming on Baltic Sea ecosystem dynamics" by Shubham Krishna et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-249-RC1, 2023

GENERAL COMMENTS

This work tackles parts of an important subject of relevance to the journal. However, it does so in such a simplistic way that the conclusions are either obvious (viruses impact plankton production) or unbelievable (results are based on a model describing only one phytoplankton type, so there is no competition between phytoplankton that are more or less impacted by their own species-specific virus and/or by the likely selection zooplankton grazing). For some reason the authors do not seem to be aware of Flynn et al. 2022, which goes into various of the matters considered here and shows the critical importance of using a multi-species model. Here, the authors have actually used a 2-phytoplankton variant of their approach, but this is mentioned rather in passing in Discussion. If the whole work had been conducted using that more complex model then the work would have been on a much firmer grounding.

DETAILED COMMENTS

L12 Virus-host dynamics are highly specific; the specificity of this interaction here needs to be made very clear in the abstract.

L16 How did this warming interaction come about?

L22 Is there a specific reason for not referencing Flynn et al. 2022 - it seems to have

rather a lot in common with this submission.

L26 Such an increase in primary production is not assured, and depends on the timing of events; these are matters for which models can help.

L28 It is very important to indicate early on that virus induced mortality is very different to that induced by zooplankton.

L69 It is very important to make it clear how many phytoplankton-virus couples are considered here - from what I can see there is just the one, implying that the Baltic has only one phytoplankton species with its virus and zooplankton. That is surely too much of a simplification. When a virus attacks its host, we must expect other phytoplankton to come to dominance. Whether they are suitable prey for the zooplankton is another important matter.

L83 Cell size is affected by factors other than temperature, and certainly the species composition (and thence the specificity of any virus attacks) will be affected during successions.

L134 I really do not see how such runs can possibly be related to reality. What happens depends as much on how uninfected species behave as it depends on that of virus-affected species.

L153 Most of what is released when phytoplankton burst would contribute to the DOM pool (as per L203), not to detritus. This does not appear to have been modelled, and neither is the activity of bacteria (and their grazers) that would be stimulated by such an event.

L213 Virus presence alone cannot lead to a regeneration of nutrients (by which I assume you mean inorganic nutrient). I do not see how, at least in the system modelled, virus attack could ever promote primary production. Can it?

L220 This model really cannot support such a claim; to do so it needs to describe the biodiversity of the plankton, and the allied specificity of viruses on components of the community.

L231 What does this 'interact actively' term mean? Viruses cannot do anything alone; they reply on the success of their host, and thence on many factors. This statement seems rather exaggerated.

L246 While this paragraph is interesting, and begs additional questions, I fear that the model is far too simple to make generalised claims like this.

L261 How was the zooplankton configured to handle this additional prey item?

L268 This is incorrect. They only impact their specific host, and the ramifications from the different host-virus interactions with competition appears (from Flynn et al. 2022) to be complex and profound.

Fig.1 That this is operated within a 1D scenario based on a real hydrodynamic scenario makes it no more representative than models operated in theoretical scenarios. The problem here is that the trophic setup is far, far, too simplistic. Viruses would only impact their own host; the idea that all phytoplankton would be impacted simultaneously in nature is not plausible. There is no bacterial activity simulated here (with or without their own viruses). Excretion of DIN by phytoplankton? What types of zooplankton are these (I assume from the 'sloppy feeding' term they are metazoan?).

Fig.5 These 'future' plots carry even more caveats than does the control. All of these appears rather too much like a ‘first try’ rather than a comprehensive attempt to explore the dynamics.