

Thank you for giving me the opportunity to read the paper “Interactions between two functionally distinct aquatic invertebrate herbivores complicate ecosystem- and population-level resilience” by Werba et al. In this paper, the authors report on the results of an experimental study to examine the differential effects of *Physa* sp. and *Daphnia magna* on microcosm recovery that follows a eutrophication event. I appreciate the time and effort that went into this study. Even so, it is my professional opinion that the paper is not publishable in its current form or context. The main reason I feel the paper is not publishable is that it needs clarification in a number of places, and I do not completely agree with all aspects of the conclusions of the paper, which in my opinion are not supported by the data and analyses. Moreover, the manuscript is not well organized and clear, and it is quite long for the amount of data presented. Finally, some critical data and context is missing, including the phytoplankton composition in the study, and many classic references are missing. Overall, these limitations reduce the logical flow, obscure the essence and relevance of the conclusions, and diminish the potential impact for a wider range of readers. Provided below are general and specific suggestions which I believe would improve the quality and impact of the work.

## General Issues

**Comment 1:** The Methods section of a manuscript should be sufficiently detailed so that the reviewer and the readership have a clear understanding of: (1) how the data were collected or obtained, (2) how the key variables were defined and measured, (3) the study design, and (4) the statistical methods used for data analysis. In my professional opinion, the authors failed in points 1, 2, and 3. To be more clear, the experimental set-up subsection (2.1) should be divided into more subsections (2.1.1, 2.1.2, 2.1.3, etc.) and each step should be better described. The authors did in the "Analysis" section. I had a hard time trying to understand the experiment, and even after a while, I do not know if a fully understood. I strongly suggest a figure showing the experimental design.

**Comment 2:** The results should be presented with a neutral description and without questions, explanation, or interpretation, as the authors did in lines 164-168 and in line

183. Research questions and explanations about the analyses carried out are to be given in the Introduction and in the Methods section, respectively, whereas interpretations as to the importance and meaning of the results are to be given in the Discussion section.

**Comment 3:** In my opinion, one weakness of the results is that the authors did not provide the composition of the phytoplankton community. Cyanobacteria is commonly found in phytoplankton communities during a eutrophication event, and its presence can shift the composition of the herbivore community, and even the importance of functional traits (<https://doi.org/10.1007/s10750-018-3710-0>). Could the authors provide a list of phytoplankton species found in the experiment?

**Comment 4:** Furthermore, cyanobacterial dominance may also regulate the herbivores via bottom-up effects. The authors only consider the top-down effect of herbivores on phytoplankton, but not the other way around. What do the authors think about this aspect and whether it should be incorporated as a possible explanation of the results?

**Comment 5:** According to Docherty and Smith (1999; DOI: 10.1136/bmj.318.7193.1224), a discussion should be prepared by organizing information in the following order: (a) statement of principal findings; (b) strengths and weaknesses of the study; (c) strengths and weaknesses in relation to other studies, discussing particularly any differences in results; (d) meaning of the study: possible mechanisms and implications; (e) unanswered questions and future research. In my professional opinion, the authors failed to provide all the information mentioned above, especially the strengths and weaknesses of their study and in relation to other studies, as well as possible mechanisms and implications of their findings.

**Comment 6:** There are significant errors in some of the Figures and also a lot of missing information including axis labels, units, etc.

**Comment 7:** The authors choose a new style of figure caption, which include the following elements: a declarative title that summarizes the result or major finding of the data that are being presented in the figure, a brief description of the methods necessary to understand the figure without having to refer to the main text, and statistical information.

However, not all figures contain all these elements, which makes it difficult for readers to understand the style. As it is an unusual style, I recommend presenting all the elements in all captions in the right order or choosing the "traditional" way, in which the captions did not overtly state the main finding of the data being presented.

**Comment 8:** The captions should be presented in the same way as described above.

**Comment 9:** The authors referenced figures in the entire Discussion section. The most common convention is that tables and figures are not referenced from the Discussion section of a paper. I strongly recommend the authors do not repeat detailed results that can be found in the Results section. In general, specific figure numbers do not need to be re-stated in the Discussion unless you feel that doing so would substantially enhance your argument or discussion point. In my opinion, this is not the case.

### Specific comments

1. **Lines 25-26:** Biodiversity is a generic name and it can also be evaluated using functional groups. I think what the authors really want to say here is that most research has a strong focus on species richness and taxonomic identity to represent biodiversity, and they are proposing that using functional groups to evaluate biodiversity is the best approach. I strongly suggest reformulating and developing this paragraph. First, the focus here should be on functional traits and not functional groups. Second, functional groups are not the opposite of biodiversity and it is just another way to measure biodiversity. Finally, there are many classic references on this subject that are missing here. Some references: <https://doi.org/10.1111/fwb.13051>, <https://doi.org/10.1111/j.1365-2664.2011.02048.x>, <https://doi.org/10.1002/ece3.601>
2. **Lines 45-46:** First, the authors specify the specific goal in the 5th paragraph, which is to gain insights into the role of functional traits in the ability of an ecosystem to recover from perturbation. Then, in the last paragraph, the authors say the focus of the experiment is to examine the differential effects of *Physa* sp. and *D. magna* on microcosm recovery that follows a eutrophication event. In my opinion, this makes it difficult for the readers to understand the purpose of the work. I recommend that the authors state the objective of the work at the end of the introduction. Moreover, it is important to remember that only one functional trait is evaluated, which is the feeding mode. So, the authors cannot gain insight into the role of functional traits in ecosystem recovery.
3. **Lines 67-68:** Put everything in the same parentheses, like: (Mackay and Elser, 1998, but see Paterson et al., 2002).

4. **Line 76:** Please, put a comma after first.
5. **Lines 84-89:** The authors stated their predictions but did not show their hypotheses. The purpose of the study, that is, what the study hopes to accomplish, should be stated, along with specific study hypotheses.
6. **Lines 86-87:** This prediction is not fully explained in the Introduction. Why do the authors expect this?
7. **Lines 87-89:** I agree with that, but in this case, the work cannot discuss this since the authors did not identify the algae species, they identified only two genera (*Chlorella* and *Scenedesmus*).
8. **Line 92:** How long does the experiment last? I read this information below. However, this information must be in the first paragraph of the Experimental set-up.
9. **Lines 94-95:** But it was a population of algae or a mix of many species of algae? This is not clear here. Reading your text, sometimes I think it is a culture of *Chlorella* spp., but sometimes I think it is a mix of many species of algae. Please, the authors should clarify this.
10. **Line 100:** The tank's capacity was 1L?
11. **Lines 102-103:** And these numbers that the authors choose to start the experiment were based on what criteria?
12. **Lines 106-107:** Again, why did the authors choose this amount of nutrients to characterize as a eutrophication disturbance?
13. **Line 111:** Is not this amount too low? If the tank capacity is 1L, this amount represent 1% of the total and in my opinion, it is a poor representation.
14. **Lines 116-117:** Only here we now how long the experiment last!
15. **Line 118:** Please, correct to 5 mL instead of five mL
16. **Lines 119-123:** I am not familiar with this method, and probably many readers are not. Therefore, this must be better explained.
17. **Line 156:** Please, correct to (Bates et al., 2015).
18. **Line 158:** Could the authors provide a list of the species found in the experiment?
19. **Lines 164-168:** The results should be presented with a neutral description and without questions, explanation, or interpretation.
20. **Line 183:** The results should be presented with a neutral description and without questions, explanation, or interpretation.
21. **Lines 202-204:** The results suggest that herbivore treatment ( $R^2=10\%$ ) and time ( $R^2=6\%$ ) contribute to explaining the low amount of variation, so it may not even be an ecologically relevant factor. What do the authors think about this aspect? What variable is missing here? Such discussion must be better elaborated in the Discussion section.
22. **Lines 207-208:** I did not understand why there are so many unknown species in the plot. Didn't the authors identify the phytoplankton species? Moreover, why is there an amoeba in the plot? Amoebas are single-celled protozoans of the order Amoebozoa and not algae, so they do not belong to algal communities.
23. **Lines 209-210:** In my opinion, it is of huge importance to know which species are. We see an increase, but only two species have been identified.
24. **Lines 2015-216:** Figures should not be referenced from the Discussion section of a paper.
25. **Line 217:** Where are the references for this statement?

26. **Lines 218-219:** Many studies have shown that not only traits but also the number of species (species richness) are important for ecosystem functioning and stability. Moreover, the authors considered only one trait, which is feeding mode. But, as the authors state right in the abstract, different functional traits have differential effects on system-level resilience. Here I miss important discussions about theory and limitations of the present study.
27. **Line 220:** Figures should not be referenced from the Discussion section of a paper.
28. **Lines 222-223:** Where are the references for this statement?
29. **Lines 224:** Where are the references for this statement?
30. **Line 226:** Figures should not be referenced from the Discussion section of a paper.
31. **Line 229:** Figures should not be referenced from the Discussion section of a paper.
32. **Line 230:** Figures should not be referenced from the Discussion section of a paper.
33. **Line 233-234:** After 3 paragraphs, this was the first reference that I found in the Discussion.
34. **Line 237:** Figures should not be referenced from the Discussion section of a paper.
35. **Line 243:** Figures should not be referenced from the Discussion section of a paper.
36. **Lines 245-246:** The concept of selection effects should be clearly defined here. Why did the authors bring this to light here?
37. **Lines 247:** Figures should not be referenced from the Discussion section of a paper.
38. **Line 256-257:** Figures should not be referenced from the Discussion section of a paper.
39. **Line 267:** Figures should not be referenced from the Discussion section of a paper.
40. **Lines 282-284:** There has been a lot of recent work on this. Please, also cite recent references.
41. **Line 287:** This result was not clear since the authors did not identify the phytoplankton community (at least, the readers do not have access to this result). Given this, they could not state anything about changes in phytoplankton composition.
42. **Line 289:** Remove ')’.
43. **Line 293-294:** Figures should not be referenced from the Discussion section of a paper.
44. **Lines 296-297:** This is an understatement. The authors should note that this is an experiment with many limitations. One of them is the absence of predators, such as fish, that can vanish zooplankton like *Daphnia magna* from the environment of zooplankton such as *Daphnia magna*. This in turn will prevent the recovery of phytoplankton control by zooplankton grazing (DOI: <https://doi.org/10.1371/journal.pone.0212351>). I suggest to tone down or at least clearly define the limitations of the present study to improve the communication of concepts to readers.
45. **Lines 307-308:** This is a bit misleading. First, the authors did not actually consider multiple functional traits. Therefore, the results do not prove what the authors claim, but rather indicate that feeding mode was not the best functional trait to predict changes in ecosystem resilience.