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Interactive Comment

Interactive comment on "Microzooplankton functional responses in the lab and in the field" *by* S. F. Sailley and E. T. Buitenhuis

Anonymous Referee #1

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I was excited to see the title of this work and looked forward to a solid review of microzooplankton functional responses, as this would be a great benefit to the field. However, I was disappointed. This study if made available to the scientific community will have one of three outcomes: 1) it will provide an initial guide to the literature for researchers who plan to do a rigorous review and synthesise (through meta-analysis) the vast amount of literature; 2) the most benign outcome will be that it is ignored; and 3) the most invidious outcome will be that researcher attempt to use the information provided as it stands and pay attention only to guidance provided in the manuscript portion of the publication. My strong recommendation is that this work be removed as a resource until it is fit for purpose. Reasons for this follow.

Major: 1. Possibly I have missed something, but I could not find any "methods".

How were the data temperature corrected? How were the data points obtained from sources, bith graphical and tabular? These and likely other issues MUST be outlined in detail, so that they can be repeated and checked.

2. The stated purpose of this work is to "To advance understanding of microzooplankton, its role in the ecosystem and its representation in models..." by presenting "a collection of data on microzooplankto processes". In fact, what this work does is: present a reasonable subset of the data on selected (admittedly key) processes associated with marine ciliates and heterotrophic dinoflagellates. These data if applied sensibly will lead to a better understanding of a range of marine ecosystems and may if used correctly improve model accuracy and precision. However this study does not do the latter two, and where it attempts to do so, it fails. Likewise, it does a cursory job of revealing gaps in our knowledge and data (see below). Therefore, the purpose of the work requires restating, or a substantial amount of intellectual and interpretive effort is required to fulfil the stated goals.

3. The terminology (and its application) within the entire report suggests a lack of knowledge of the subject, potentially reflecting other undisclosed examples of ignorance. Examples follow:

a. Let us be clear: "functional response" has a specific meaning in the ecological literature. It is the relation between ingestion (or grazing rate) and prey abundance. This is a well established ecological term, well beyond ocean sciences (see any good ecology text book). In contrast the response of predator specific growth rate to prey abundance is termed the "numerical response", although in its strictest sense it can also refer to the relationship between predator abundance and prey abundance. There are no similar terms for the response of predator GGE, assimilation efficiency, or egestion vs. prey abundance. Thus, from the start, the title of this work is misleading. Likewise, measurements of ingestion, growth, etc at a single prey abundance are not a "functional" or "numerical" response. On that note, I am concerned that the Authors are suggesting, through inference, that these single point measurements could somehow all be compiled to provide functional or numerical responses. I hope this is not so, as it will lead to a mess, even with the best scaling.

b. As an aside, presenting data on clearance rate is redundant and uncessary and it can lead to confusion. I recommend removing it from the data set..

c. Egestion and excretion seem to have been used interchangeably. They are fundamentally different, with the former being the removal of unused food stuffs and the latter being the removal of metabolic wastes (with osmoregulation often also associated with the latter). There is reference to a compilation on "excretion" (do you mean excretion?) by Nagata (2000) in the text, but there is no accompanying citation (another indication of sloppiness that makes me worry about other aspects of this work).

d. Microzooplankton should be defined using the well established and accepted terminology (see Seibruth et al. 1978 L&O, 23 1256-1263); i.e. plankton (meta- and protozoa) that are between 20-200 μm. If this study wishes to include nanoplankton (which it should as some of the ciliates and dinoflagellates fall into this category), then this should be clearly stated. If the study only plans to study hetero- mixotrophic protists, then this should be made clear too. In this respect the title is misleading and the text defining the study organisms is confusing. Likewise, and even more contradictory, although the Introduction provides information on HNF, there is no information on them later on. It does not seem like the Authors have tried to connect the dots up. I suggest revising the title to refer to 1) protozooplankton (if HNF are included) or 2) ciliates and dinoflagellates (which is what it does at the moment as far as I can tell!)

4. This study has ignored the entire freshwater literature. There is no reason to do this in a review, as the principles driving functional and numerical responses are not altered by salinity and the parameters obtained from freshwater studies would usefully inform marine systems. If, however, the Authors do limit themselves to marine work they must explain that they are doing so and, critically, justify why (because we only care about marine systems as they cover most of the world...would be a pathetic excuse). It is

a real shame that they have not assessed the freshwater literature, as an "aquatic" review would be so much more applicable to a data base on "Earth Sciences", which seems to be the focus of this venue.

5. This work is neither chalk nor cheese. If it is a data set that offers others the opportunity to pursue critical analysis of the data and then use those data to test hypotheses (i.e., meta-analyses), then that is potentially useful. If it attempts to synthesise the data into some trends (as illustrated by the few figures near the end), then it should present testable hypotheses (goals) in the Introduction and follow these through in the text. Likewise, like any good review of the literature that then applies those data, it must screen the data, using rigorous and clearly stated criteria to do so. At present, I cannot see any benefit to Figures 1-4 and suggest they are removed. I also do not see evidence in what I have read to indicate the Authors are sufficiently well informed in the field of microzooplankton biology to conduct the needed screening of the data.

6. Why were error estimates not provided with the parameters from the functional and numerical response (e.g. the half saturation constant is often accompanied by a SE).

7. Estimates of maximum growth and grazing rates, based on predictions from curves fit to the data are often misleading, as the responses do not always saturate. Presenting these in a table, without adequate cautions, is irresponsible.

8. Were specific growth rate estimates all determined based on natural logs, or were some based on log base 10 (or sometimes 2); were conversions made?

9. Also, I found the spreadsheet in the data base hard to follow. They are not well laid out.

10. If the Authors think that the ~15 year old study by Straile remains the definitive review on GGE, then they must defend this. Undoubtedly, they will not be able to do so; I know there have been substantial additions and revisions to his work. Therefore, either this section requires improvement, removal, or admission that it is inadequate.

11. Likewise, it is insufficient for the Authors to provide the caveat at the start that they did not mean to miss any data sets; they have done so. Some works/organisms/issues that come to mind are: the substantial work on the heterotrohic dinoflagellate Oxyrhis, the recent and important addition of Mesodinium as a grazer, the recognition that most oligotrichous ciliates are mixotrophic (choreotrichous ciliates are not), consideration and reference to recent and classic reviews on the subject (e.g., work by Capriulo, Dolan, Montagnes, Sherr & Sherr, and even Seibruth), work in the non-English (e.g. Russian, Chinese) literature.

12. The entire section in the Introduction on how “microzooplankton” feed is a simplification to the extent that it is misleading and in places wrong. As examples, most planktonic ciliates are not really filter feeders (they do not sieve water) and bacteria are likely the major food of HNF. It is clear that the Authors are not experts on this subject (and that they really have not attempted to develop a good understanding of the biology of the organisms). It would be much better if this section was reduced to one sentence “Microzooplankton feed in a range of ways”, with appropriate guidance to recent reviews provided.

13. The sentence in the last paragraph of the Introduction that starts “The impact will depend on...” is fatuous. What group would this not apply to?

14. In short, the Introduction should probably not try to introduce the importance of the group (anyone accessing the data set will be aware of this). Rather it should clearly state what this data set provides, how it should be used, and what its shortcomings are (e.g. it has not been critically edited).

15. Likewise there needs to be a solid Methods section, stating exactly what the Authors have done to the existing data, clearly indicating all assumptions and manipulations of data from the raw material.

16. The evaluation of the data (section 4) is obtuse at best. Examples follow.

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a. Of course different unit have been used. This is not an issue, if the dimensions are the same. When different dimensions have been used it is often because the research had different goals. To say that everyone should do the same thing is narrow minded thinking, assuming that all research was conducted to fit with the Author's goals.

b. The remaining portion of the first paragraph of section 4 is more of a whinge than a useful indication of how the data set might be approached. If nothing else, this paper should offer suggestions on how the assembled data might sensibly edited and synthesised to obtain their original goals (see above).

c. The second paragraph of section 4 suggests that the Authors have lost touch with reality, or at least it reveals their ignorance of the field and its challenges. The idea of adding all processes up to a sum value of ingestion is unrealistic in most cases, especially give the recognised non-linearity of several of the processes. The suggestion that protozoan rates for egg production (what are they thinking), sloppy feeding (good luck), and faecal pellet production (examined twice in the last 40 years and under special circumstances) reveals something about the quality of the Authors ability to conduct this review! The idealistic suggestion that work should be coordinated to measure multiple processes is commendable; it might be worth the Author's time to see who has attempted this since Caron suggested it 25 years ago. Likewise, it might be profitable for the Authors to review the literature on prey selection and nutritional quality if they really want to comment on it. However, if they plan to examine these issues, then one of the "goals" of this work has become to "reveal important gaps in the literature". If that is the case, then they should do it properly (i.e. include it as a goal in the Introduction and rigorously look for gaps), rather than playing lip service to the issues they raise. As an example, how likely is interference competition in planktonic systems where there are usually 1 to 10 ciliates (10 to 100 μm long) in 1 ml? (i.e., arguments made by Ardity and Ginsburg)? Is it likely that they will run into each other and compete for prey? I expect not, but a rigorous evaluation, empirical and theoretical, needs to be considered before this is pursued. Maybe the Authors wish to do this?

17. The Source Data references have an odd format. It would be useful if these included the original title, the date of publication and the journal.

18. The summary of data description (sections 3) could be substantially reduced and made easier to read if it was turned into a table.

19. See Fig. 4... Did Fenchel and Finlay work on protozoa as group or specifically on microzooplankton. Other protozoa likely have different rates than the micozooplantoinc ones.

20. Rather than exciting me, this review of the literature makes me concerned about conclusions that Buitenhuis et al have made in their modelling studies. I reiterate, it should not be made publically available in its present state.

Interactive comment on Earth Syst. Sci. Data Discuss., 7, 149, 2014.