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Biogeosciences
Discussions

Interactive comment on "Stable isotopic composition of top consumers in Arctic cryoconite holes: revealing different position in supraglacial trophic network" *by* Tereza Jaroměřská et al.

Anonymous Referee #2

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Dear authors, dear editor,

My apologies for the long delay in reviewing your manuscript!

I have now read your manuscript and provide my comments. First, it is important to state that I am less familiar with gletsjer ecology and geology, and that it is the first paper I read about cryoconite holes.

Having said that, while I feel that the paper presents interesting and novel data, I also strongly feel that it lacks conclusiveness to warrant publication in a top journal like Biogeosciences. My main reason for this somewhat harsh conclusion is that the results presented on stable isotope signatures of tardigrades, rotifers and cryoconite 'sedi-

ments+OM' of three gletsjer systems can only be described and compared, but that virtually all data needed to explain the observed patterns are lacking. I perfectly understand that the authors set out to do a pioneering study on the trophic ecology of the most abundant consumers in these holes, but their explanations for the obtained results remain very speculative for lack of measurements of both resources and subsidies. I would not object to the data being published in a more specialised journal, but in Biogeosciences, I expect to read papers which provide mechanistic understanding of patterns and processes, and while there is relevant discussion about this, the obvious absence of the necessary data for this prohibits any substantial conclusions. Isotopic signatures of nutrient and OM subsidies are missing, and so are isotopic signatures of primary producers vs detritus vs heterotrophic bacteria. I perfectly understand that these are not easy to obtain, but it was foreseeable that such data would be required to explain observed patterns, and methods exist for determining each of the above factors.

Apart from this general appreciation, I also have some more specific contents. In the introduction, I feel the authors should do an extra effort to make this manuscript better accessible and therefore more interesting for readers who are not familiar with the cryoconite holes. Aspects I wondered about, not knowing these specific systems, is what size and depth range such cryoconite holes have and how common they are, or better still, what % of a gletsjer's surface they make up. It is one element I would need to answer the "so what" question: what could be the quantitative importance of these 'miniature' ecosystems in the ecology and biogeochemistry of gletsjers?

I would also have liked to read more clearly structured information about the different organic matter resources – both autochthonous and allochthonous – that tend to be present in cryoconite holes. Particularly on the primary producers, info is rather minimal.

And as you were comparing three different gletsjer systems, I would have hoped that you had some clear rationale for choosing precisely these three systems, which would

then translate in one or more clear hypotheses about what (different) patterns to expect. Now, there are no very clear goals , questions or hypotheses about such expected patterns, and I am left with the question: since you sampled glaciers in three different settings, how did you expect their food webs and fuelling resources to differ?

In lines 44-47, you are mentioning a correlation between the size distribution of primary producers and the community composition of consumers. In what way, and what is the relevance here?

The materials and methods section is generally well described, but I was rather surprised to read the following final sentence of the conclusions section: “Another outcome of this study is the introduction of a modified technique of sample preparation avoiding procedures such as sugar centrifugation or oven drying.” While reading M&M, I did notice some minor differences in sample elutriation and sorting procedures compared to common practices, but nowhere did I see a clear statement about an important novel methodological approach to treating samples.

Given the subtitle ‘community structure’, I had somehow hoped to read a species or genus-level analysis of consumer communities, yet in the end, rotifers were just treated as rotifers, and tardigrades were largely left without a name and assigned to feeding groups, which are well known to be of very limited relevance to describe the actual feeding behaviours of tardigrades, particularly with respect to their potential to feed on microalgae. Similarly, whilst the authors explain that they identified cyanobacteria and algae, we later learn that they looked at roughly 10% of these primary producers, because sample preservation could have strongly biased actual community composition. So there is no info on community composition, only some more anecdotal statements as to certain abundantly present Cyanobacteria and algae. This is probably also why there is no statistical analysis of differences in community composition of either consumers or primary producers. Incidentally, I wondered why both a Spearman rank and a Pearson product-moment correlation were tested. I would expect that if the data fit the assumptions for parametric tests, one would choose the latter, and if not, the

former.

The results are generally well presented. I have only some specific suggestions. 1) I would have preferred to see absolute abundances of tardigrades and rotifers in table 1 instead of a general dominance-indication. That dominance indication is not very scientific, and it does not provide any relevant info on potential differences in abundance of consumer groups among gletsjer systems. Actually, since you sampled sufficient replicates in 2 out of 3 gletsjers, you could statistically underpin whether tardigrades were more or less abundant than rotifers in a specific gletsjer system. 2) Figure only shows the correlation between cryoconite delta13C and rotifer delta13C. That of tardigrades is not shown because it was not statistically significant. I would then at least like to see the r and actual p-value for the tardigrade correlation, as well as an estimate of the slope of the regression for both rotifers and tardigrades. That would allow me a clearer picture on whether tardigrades had a somewhat less clear but still generally very similar pattern as rotifers, or whether their patterns differed importantly. 3) I would have started the results section with what is now 3.2. Cryoconite composition. 4) Section 3.3: as mentioned above, table 1 should provide absolute abundances of consumers, and it should also provide info on the proportion of dormant consumers! 5) I would be interested to read in one or two sentences to what extent other meiofauna or small invertebrates than tardigrades and rotifers were present (any Nematoda, Copepoda, Ostracoda, . . .?).

Discussion: At the end of the first discussion §, you finally provide some ‘expectation’, but it is not clear what this expectation is based upon.

In the relatively lengthy and speculative discussion, I read virtually nothing about temperature effects on isotopic fractionation between consumers and resources. I believe that there is some relevant literature on that, and it should have been included here.

In section 4.2., dissolved organic matter suddenly becomes an important candidate food for these consumers. That may well be, but what results is this based upon here?

Finally, there are a few instances where isotope 'terminology' is used in a somewhat sloppy way: 1) Line 71: "preferential excretion of δ14N" should be "preferential excretion of (light) 14N". 2) Line 250: "rotifers revealed higher values of 15N isotope" should be "rotifers had higher δ15N" or "rotifers had heavier stable nitrogen isotopic signatures'. 3) Line 251: "potential differences in δ15N composition" should be "potential differences in N isotope ratios/signatures".

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