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# *Interactive comment on* "Microbial nutrient limitation in arctic lakes in a permafrost landscape of southwest Greenland" *by* B. Burpee et al.

**Anonymous Referee #1**

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Overview and context In this paper, the authors discuss nutrient limitation of 'bacteria' in western Greenland lakes. They used an exo-enzyme based approach to conclude that the microbes in these systems are primarily limited by the availability of P, but that some lakes were limited by N as well, particularly in July. The work is presented in the context of a rapidly changing arctic environment with rapid permafrost melting which is likely to affect nutrient limitation and productivity in these systems. Scientific significance (rating: 2) and scientific quality (rating: 2) I think the authors are studying an important issue. The arctic is rapidly changing due to climate change and the biogeochemical behavior of these systems is in flux. Furthermore, little work of this kind has been done in GreenlandâĂŤmuch more has been done in Alaska due to a LTER that is located



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there. Much of the changes in the biogeochemical behavior of aquatic systems in the arctic will revolve around how the composition and concentrations of dissolved organic matter (DOM) are affected by warming and changing plant composition, so this paper is very timely.

So, there is great potential for this work to help us understand this poorly-studied region of the arctic, but the actual data presented fall way short, in my opinion, both in terms of the approach used and the execution of the study. The authors use exo-enzymes to ascertain nutrient limitation. There is a reasonably well-developed field that presumes to be able to determine C, N and P limitation based on a suite of exoenzymes based to a significant extent on the work of Sinsabaugh and others. While it makes perfect sense that some enzymes should be up-regulated and down-regulated depending on what nutrients are most limiting, I feel that the implementation of these concepts in the exoenzyme-nutrient limitation literature is totally flawed. For instance, in the present work, the authors use one enzyme to characterize C-limitation and another enzyme to characterize P-limitation and two enzymes to characterize N-limitation. Recent transcriptomic work has shown that each of these different states can be quite complex with somewhere on the order of 200-600 proteins being up or down-regulated based on the nutritional state of the microbes. It seems that only looking at a couple of those proteins is likely to lead to idiosyncratic conclusions. I am not suggesting that the authors should necessarily be using a transcriptomic approach, but given this information, I think it is trivial to draw a line at a 45 degree angle and suggest that anything above the line is P-limited and anything below the line is N-limited. This kind of information just has not been substantiated enough to say whether that line should be 45 degrees, 48 degrees or 10 degrees.

I also have concerns about the execution of the studyâĂŤin particular, although I know it is problematic doing research in remote places, the authors froze the samples for transport from the sites back to the USA or NZ where analyses were conducted. No mention is made of any controls or quality control to determine if freezing had any effect



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on enzyme activity. This would be particularly problematic if it affected some enzymes differently than others which would certainly affect the conclusions of the study. It would also be useful to know that each of the enzymes was measured at the Vmax, providing a solid quantitative measure of enzyme activity.

Another concern here is that the authors refer to the organisms producing the exoenzymes as 'bacteria', but I doubt they looked to see if there were also archaea and/or eukaryotes in their samples. If so, they should mention it.

Presentation quality (rating: 2) The authors use an approach adopted from Moorhead et al. 2013 to determine nutrient limitation from enzyme data whereby a vector length and angle are calculated. I had to look at several papers before I found a decent description with the mathematics of this approach (Hill et al. 2014). The description should also be included in the present manuscript because most readers will not be very familiar with it. It is also complicated by the fact that several of the figure axes and captions in the paper seem to be mis-labeled or not labeled at al. Units in Fig. 3 are not given and the ratios in that figure for BG:NAG+LAP are on the order of 10-60. But then in Fig. 4, the axis for BG:NAG+LAP is in the range of 0-0.8. The caption says that what is plotted are the vector angles, i.e., not the activity, but in Table 2 the vector angle ranges are around -10 to +45. So it is really not clear what is being plotted in Fig. 4. Figure 4 also seems like a more convoluted plot than it needs to be. If BG is in the numerator for each axis, it cancels itself out and essentially they are plotting NAG+LAP against AP and therefore should be labeled that way. Figs. 5 and 6 also need units to be labeled.

More specific comments: p. 11873: Why did they use DIN: TP as an index of nutrient limitation? A more appropriate comparison would be DIN:DIP or TN:TP.

p. 11874 line 15: I don't think you can necessarily infer that the DOM supply was poor in P from this relationship. There can be (and likely are) other sources of N and P other than DOM. Also, it is the supply relative to the requirements of the organisms



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that would determine this relationship.

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