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Interactive comment on “Temperature thresholds for Arctic plankton community metabolism: an experimental assessment” by J. M. Holding et al.

Anonymous Referee #2

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This study examined net community production (NCP) and community respiration (both measured from changes in oxygen) in mesocosms subjected to seven temperatures ranging from 1 to 10 C. The authors found that both NCP and respiration changed when the water was taken from the Barents Sea which had an initial temperature of about -1 C but not when the water came from a fjord with an initial temperature of 6 C. The authors used these data to argue that a warming of about 5 C will cause the Arctic to pass a “tipping point”, pushing it into a heterotrophic state. The paper makes several interesting points about an important question, about how Arctic microbial communities will respond to global warming.

The paper is now marred by several problems, although most of these can be fixed. A serious limitation, which cannot be fixed, is that the authors apparently did not do

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a control mesocosm held at the in situ temperatures. Some of the changes they observed were undoubtedly due to simply putting water in the mesocosms, perhaps most importantly, the probable change in the light environment (see below). Other problems, noted below, can be solved.

The authors make this very interesting observation that the “tipping point” when the system turns heterotrophic is about 5 C. The specific comments below have several technical complaints about this temperature. A more general criticism is that it reduces a very complex system to a single number. I’m all for reducing complexity, but not when doing so is misleading. Perhaps the biggest problem with the paper is that it implies that the direct effect of temperature on metabolic rates will have the biggest impact on these microbial communities. This paper cannot review the entire literature on the problem, but it certainly has space for a couple paragraphs about how warming and the loss of sea ice will affect mixing and thus nutrient supplies and the light environment. Other studies have argued that these indirect effects are likely to be greater than the direct effects.

Specific comments

p11290, line 16: What was the light intensity for the mesocosms, especially relative to the depth where the water for the mesocosms was taken? Any change in this light intensity could affect chlorophyll concentrations (photoadaptation), independent of changes in biomass, and phytoplankton production.

p11290, line 18: Was the gradual warming over three days done before the first measurements were taken? This warming period is not in time course data, such as those presented in Figure 3?

The authors argued that it was not necessary to gradually raise the temperature of the fjord water because it was already at 6 C. But fjord communities did experience large changes in temperature in going from the in situ temperature of 6 C to as low as 1 and as high as 10 C. The authors should have had an acclimation period with these waters

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as they did for the Barents Sea experiment.

P11291, line 17: What was the light intensity for the NCP experiments (oxygen changes in light bottles)? Was only one intensity examined?

p11292, line 5: The slope of chlorophyll versus time must also have units of time, i.e. per day.

p11293: The authors calculated NCP and CR (community respiration) per chlorophyll because they wanted to look at changes in rates independent of changes in biomass. The problem is, many non-chlorophyll-containing organisms contribute to respiration. Chlorophyll most likely does not track these other organisms (e.g. heterotrophic bacteria) very well. So, there is not much value in these per chlorophyll rates. In fact, the patterns for the per volume rates in Figure 4 look the same as the per chl rates in Figures 5 and 6. The authors could mention that they did the calculations, say the pattern is similar, without showing the results. They could do the same curve fitting thing in Figure 4 as they did in Figures 5 and 6.

P11293, line 21: The authors here used the GP and NCP rates per chlorophyll to talk about the autotrophic/heterotrophic balance. This is incorrect. This balance is defined by per volume rates or better, rates integrated over a water column, not the biomass-normalized rates. They should use the data in Figure 4C for the discussion here.

P11293, line 27: Here the authors give the “mean (\pm SE) threshold temperature at 4.78 ± 1.26 C. They need to explain how this threshold was calculated. My guess is that they set NCP per chlorophyll to zero and solved for temperature with the logistic equation.

But the logistic equation may be misleading and not the right way to analyze the data. The data seem to follow a step function. The values are roughly the same (within the errors) below 4C and then decrease abruptly to another level of values at the next tested temperature (a bit below than 6 C). There are no values in between the two

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temperatures following the curve of the logistic equation.

So, the authors should consider other analyses which would find objectively the inflection point in their data.

p11295, line 18: The authors can't use the Arrhenius equation to calculate activation energies because their data of rates versus temperature clearly don't follow that equation very well at all. If the logistic equation describes changes in rates versus temperature (which I don't think it really does), how could the Arrhenius equation? If that equation is not applicable, they can't calculate activation energies with these data.

p11297, line 2: I don't buy this argument that Arctic "communities" (the relevant organisms are bacteria, which may be helpful to say) are more sensitive to change because DOC pools are large in the Arctic. DOC pools are large because of the input of refractory organic carbon from terrestrial sources. The authors have to also argue that this DOC somehow becomes more labile with increasing temperatures.

Table 1 and 2: It seems that only one light intensity was examined, so it's not clear how the authors can calculate an integrated rate and present those values here in these tables. Regardless, nothing new is learned with those rates. They just make the table more complicated than necessary. My reservations about the per chl values were mentioned already.

Figure 1: This figure should be deleted. It's not needed for this mesocosm experiment. The authors can give the lat-lon in the Methods for readers who need to find out where the water was taken.

Figure 3: The data in this figure are difficult to see, unless I blow it up on my screen by 200%. The data points, not just lines, should be given.

Figure 5 and 6: These should be deleted and replaced with the per volume equivalents for reasons discussed above. The vertical dashed lines need to be explained.

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