

Interactive comment on “A ^{13}C labelling study on carbon fluxes in Arctic plankton communities under elevated CO_2 levels” by A. de Kluijver et al.

Anonymous Referee #1

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This study examined the effect of CO_2 on the flow of ^{13}C , added as bicarbonate, through various components of the plankton and into sinking material in mesocosm experiments. The authors explored these flows by measuring ^{13}C in lipids specific (more or less) to various types of plankton; the ^{13}C content of large zooplankton was assayed directly with animals picked out by hand. The authors emphasize that their experiment is novel because it was with Arctic waters, but in fact, it should interest readers working in all environments. This is an excellent, potentially very important study.

My main scientific concern is prompted by a stray comment on page 8583, “Heterotrophic (gram-positive) bacteria” (this should be “bacteria”, not the adjective, “bacterial”). As mentioned in the specific comments below, this implies that the lipids exam-

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ined by the authors are found only in Gram-positive bacteria (Gram should be capitalized), which are a small fraction (<10%) of the total bacterial community in the oceans. But the authors observed that a large fraction of ^{13}C was found in these bacterial lipids. How can this be? Are these lipids actually in more than just Gram-positive bacteria?

Another general comment is that the writing is a bit rough at times and often very dense with lots of numbers cited in the text. The Abstract is a disservice to the rest of the paper.

Specific comments:

Abstract: The abstract just ends without any “take home” message, without a summing up type conclusion about the authors’ most important finding. To make room for this, the first paragraph can be reduced by 50% by folding in some of the methods mentioned in this paragraph with the results.

The second paragraph, which describes how the mesocosms changed over time, can be replaced with a discussion of the model results, which sum up everything. The changes over time are not important enough to highlight in the Abstract.

P8572, line 9 and throughout the paper: The authors can just say “phytoplankton”, instead of phyto 1 and “mixotrophs” instead of phyto 2. They can define those terms for this paper. Especially “mixotrophs” is much more informative than “phyto 2”.

P8583, line 3: How do the authors explain why production by phytoplankton increased with pCO_2 but POC did not? Phytoplankton were not a small fraction of total POC, right?

P8583, line 15: This is the only place in paper that mentions “gram positive bacteria”. Are the lipids examined by the authors only found in gram positive bacteria? These bacteria, which are mostly in the Actinobacteria phylum, are not very abundant in the oceans. It is essential that the authors discuss the limitation of looking only at Actinobacteria, if in fact that’s what they did.

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P8587: This very long paragraph is packed with very interesting numbers about the fluxes calculated by the model based on the authors' results. All or at least most of these numbers should go into a table, and the text can be cut in half, focusing on making a few critical comparisons. A few numbers can be cited in the main text, but the large number of them cited here and elsewhere in the Results makes it difficult to read.

P8588: The Discussion opens up with a very long and dense paragraph. The authors should find some place to break up it up, making it easier for readers to follow things and to identify the most important points being made by the authors.

P8589, top of page: The authors found a very high ratio of bacterial production to primary production (BP:PP), an average of 34%. This high fraction implies an even higher amount of organic carbon being processed by bacteria, because the ratio does not include respiration. The best estimates of growth efficiency (BGE) average <20% and are often around 10%. If it's 10%, then >100% of all primary production would have to be routed through heterotrophic bacteria; Total consumption = BP:PP/BGE. Such imbalances and net heterotrophy are probably common in the Arctic.

The very old review by Cole et al. (1988) is not worth citing. The most recent review is by Fouilland and Mostajir (2010). I don't think the authors need to have much about this topic, but something needs to be said.

P8592, line 8: Rather than "dorming", the authors probably mean "dormant".

P8592, line 9: The authors should stick to their assumption about bacteria using only DOC and not complicate the discussion by raising the possibility of these microbes using POC. Earlier, the authors said that detritus was a small fraction of POC, implying that most of the POC is in living organisms, the carbon of which is unlikely to be used by bacteria. The ^{13}C in bacteria could be similar to that of POC because these microbes may make up a large fraction of total POC. Perhaps more likely, ^{13}C in POC reflects what's in phytoplankton which in turn have a huge impact on bacteria.

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P8592, line 19: Nearly all of the paragraph starting here should be deleted. The reader doesn't learn anything new, except perhaps the difficulty of these measurements.

P8954, line 14: Why would sedimentation lead to lower grazing? The authors need to make their logic clearer here.

Fouilland, E. and B. Mostajir. 2010. Revisited phytoplanktonic carbon dependency of heterotrophic bacteria in freshwaters, transitional, coastal and oceanic waters. *Fems Microbiology Ecology* 73:419-429.

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