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Comment

## ***Interactive comment on “Seasonal patterns in Arctic planktonic metabolism (Fram Strait – Svalbard region)” by R. Vaquer-Sunyer et al.***

### **Anonymous Referee #2**

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The authors examined net community production (NCP) and community respiration (CR), along with a few standard biogeochemical properties (chlorophyll, temperature, and DOC) in the Fram Strait of the Arctic. The main argument, and it's a sufficient one, for publishing this paper is the paucity of especially respiration data in the Arctic (and all oceans, for that matter). The paper has some interesting points about negative NCP.

The paper could be greatly improved on several fronts. There is lots of discussion about irrelevant things (see below) while other important points are missed. In addition to the points mentioned below, the authors don't say enough about light and mixing. Their rate measurements are from 3-4 depths and they integrated down to 20 m. The choice of 20 m is kinda weak. But it's admirable and great to see that they did in situ incubations, even when the temperature was -13 C.

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Do the authors have any information about the thickness of the mixed layer? Of the euphotic zone? They should discuss this, giving any available data. Did they capture all of primary production in the water column in spite of stopping at 20 m? This has a huge impact on the NCP discussion.

The authors could do a bit more to explore the variation in their rate measurements. Right now they show and discuss only how gross primary production varies with chlorophyll. What about respiration and NCP? Perhaps most importantly, how do these rate measurements vary as function of temperature? The paper has lots about temperature and climate change. The authors have the data to actually address this.

Please see below the comment about Figure 5 and the mistake of doing statistical analysis of gross production vs. respiration.

Finally, the writing is rough in places, with some simple mistakes in the English (many of which the grammar and spell check of Word would find, if turned on).

#### Specific comments

Abstract: “Net”, “primary”, “gross”, “respiration”, and “community” should not be capitalized. These aren’t proper nouns.

P7702, First paragraph of Intro: “Must” shouldn’t be used here. I don’t disagree with the generalizations here, but the “must” is too strong of language. In fact, the authors’ own data indicate the complexities of the real world and why something so dogmatic sounding as “must” is nearly always inappropriate in papers such as this one.

P7703: This paragraph about variability in the Arctic exceeding the Antarctic is irrelevant and it may not be true; my guess is that we just don’t have enough data to say for sure. The paragraph should be deleted.

P7703, line 13: This paragraph just lists the previous studies of respiration in the Arctic, making the argument that more are needed. That is true, but you could say that for just about anything in the Arctic, even in other oceans. They are big. The authors should

think about a stronger argument for why we need more data or identify unresolved issues either raised by previous studies or not examined by previous studies.

Also, the authors' list of previous studies could be done more succinctly (readers can count the number of studies by just looking at the references). But it would be better if they say something about what the previous studies found.

P7703, line 26-27: I think the authors are trying to say that the Regaudie-de-Gioux and Duarte, 2010 study is in the same area and used the same methods as the authors' study, but this isn't clear.

P7704: The first line (clause) on this page is not a complete sentence, and the one that follows doesn't make sense.

P7708, line 16: "Chlorophyll", which begins this sentence, should be capitalized. There is the same problem in the Results on page 7710.

Page 7710: The Results section needs a few subheadings.

P7710, line 21: This paragraph about precision can be moved to the Methods. Its current location disrupts the flow of discussing the real results.

P7713, line 13: The authors use, incorrectly, the ratio of GPP to CR as an index of net heterotrophy or net autotrophy. This paragraph should be deleted. The main reason is that the most appropriate index is NCP, which was already discussed in the Results section.

P7713, line 24: The following is a bit picky about language and terms, but these are tied to some important concepts. It's better to say that the ratio of NCP to GPP is a measure of new production, not the f-ratio (which are related, but not the same). Eppley and Peterson didn't "assume" this to be equal to export, but rather it was an hypothesis, which has been tested and examined extensively through the years.

P7714, line 1: The authors say that negative NCP has to be supported by al-

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lochthonous organic carbon. That's true for large time and space scales, but it's not necessarily the case for short ones. To talk about "allochthonous" here is misleading and perhaps even wrong. For example, NCP is negative at night everywhere, yet we don't talk about these systems needing external organic carbon. I think the Arctic is variable with patches and times of negative NCP not requiring "allochthonous" organic carbon because of excess organic carbon build up in a recent time period or in neighboring waters.

P7714, line 8: This paragraph here has to be deleted. It describes a regression analysis of GCP versus CR, which is improper to do because GCP depends on CR. Statistics cannot be done on two variables when one is calculated from the other.

Discussion: Most Discussion sections are better without subheadings. It flows better without them, assuming the writer works at the transition between the big topics.

P7715: The first section on Methods in the Discussion should be deleted or at least minimized to a most one paragraph. The authors are stuck with their bottles and this discussion doesn't help. It would be appropriate and necessary only if they were trying to compare their bottle rates with some bottle-less rate.

P7717: The authors say that there is enough DOC because total DOC concentrations (around 80  $\mu\text{M}$ ) are around the total organic C required for the "dark period". This comparison must (oops! I used that bad word) take into account the fact that about half (40  $\mu\text{M}$ , maybe more) of that DOC is refractory with a large fraction having turnover times exceeding thousands of years.

P7720: The authors should give a rough number for the fraction of total respiration by bacteria. What is "small"? They say "protists are believed to greatly contribute to community metabolism", implying the authors have evidence or some reason for saying "are believed". If they have the some relevant data, they should state it, as this is an intriguing observation. If not, they should say something like "we hypothesize that protists contribute greatly to community metabolism."

P7721, line 8: This paragraph about terrestrial organic carbon should be connected and be closer to the discussion of negative NCP. Also, the authors can be more quantitative and use the previous estimates to say a bit more about whether terrestrial sources solve their problem of negative NCP. They also need to comment on the lability of this organic carbon. Although its turnover is faster than the really refractory DOC in the oceans, being only hundred years or so, it's still long, probably too long to really help with the negative NCP problem. Even if the authors don't agree, these topics need to be discussed.

Table 2: This table should be deleted. The comparison between their study and others can be done in the text. Instead, the authors should present more of their data, not just the summary statistics. They observed a two-fold variation in DOC concentrations, so at a minimum they should present averages for the various water masses they sampled.

Figure 5: This must be deleted. Not only is CR in the GPP calculation, but the graph compares GPP/CR vs. GPP, i.e. it's almost meaningless.

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