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

Anonymous Referee #3

Received and published: 17 August 2012

The data presented by Vacquer-Sunyer et al. offer the first seasonal/inter-annual perspective on the balance between production and respiration in the Atlantic sector of the Arctic Ocean. The authors do a very good job at evaluating the methods used and the potential caveats of in-vitro respiration assessments. I find the work presented to be useful in providing a basis of comparison for the future and extending further north work that has been done at lower latitudes. The paper reads well (abstract needs a little work though) and the figures and tables are pertinent. Several minor issues should be addressed.

Specific comments:

Page 7703: in the sentence reporting previous studies of primary production, it would be nice to see some papers from the present millenium. Here are some suggestions

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among many others:

Ardyna et al. 2011. Environmental forcing of phytoplankton community structure and function in the Canadian High Arctic: contrasting oligotrophic and eutrophic regions. *Marine Ecology-Progress Series* 442: 37-57.

Hodal and Kristiansen 2008. The importance of small-celled phytoplankton in spring blooms at the marginal ice zone in the northern Barents Sea. *Deep-Sea Research Part II-Topical Studies in Oceanography* 55: 2176-2185.

Page 7703, line 27: please reformulate the following sentence: “the area studied is in the same.”

Page 7704, lines 1-2: please reformulate the following sentence: “ However, as integration depths vary between studies are not included”

Page 7704: in the paragraph on climate change, I would suggest adding a sentence or two on the direct influence of rising seawater temperature on planktonic production and respiration processes (in addition to sea-ice melt) to better prepare for the contents of the paper.

Pages 7705-7706: authors refer to Fig. 1 to locate the Kongsfjorden-Krossfjorden fjord system and the west coast of Spitsbergen (Svalbard). Unfortunately there are no labels on the figure. Please add the necessary information to help unfamiliar readers refer to the regions mentioned in the manuscript (e.g., Kongsfjorden-Krossfjorden fjord, Svalbard, Barents Sea). Adding a few arrows would also help to visualize the different currents and water masses mentioned in the text.

Page 7706, lines 14-15: The text specifies that five periods were sampled during eight different cruises. However, six periods and nine cruises are listed in the next sentence. Please clarify and use coherent designations for each cruise and period throughout the text, figures and tables.

Pages 7706-7707, Ships were specified for the two early spring cruises (KV Svalbard

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icebreaker) and for the December 2006 cruise (R/V Jan Mayen) but not for the other cruises (Hesperides in the acknowledgement section?).

Pages 7706-7707: different methods were used to assess metabolic rates during the different cruises but the potential influence of such methodological variability on data comparability among cruises is not clear in the rest of the manuscript. Page 7713. Last paragraph: please note that the assumption that new production or NCP is equivalent to export production is valid only on a long-term basis (i.e. annual) and under the assumption of steady-state.

Page 7714, lines 3-7: this paragraph is redundant with the paragraph on page 7719 lines 5-8 in the discussion section. Please remove it from one of the two sections.

Page 7714, line 14: authors reported a relationship with bacterial abundance. This variable was never mentioned before. Please explain the method used to estimate bacterial abundance in the Material and Methods section or cite the paper that shows the actual data.

Some argumentative/qualifying sentences that appear in the Results section would be better placed in the discussion. See page 7715 lines 10-21 for example.

Page 7716, line 1: please replace “that” by “than”.

At the bottom of page 7716: It's not clear what the fact that a method based on changes in DIC was not available before the mid-1980's brings to the discussion. Given the time elapsed it certainly is not a justification for choosing to use the O₂ method 20 years later. I would recommend deleting this or reformulating it.

On page 7719 of the discussion, I'm not sure what the point of the paragraph discussing “the assumption that NCP/GPP is an estimate of f-ratio does not apply when respiration rates exceed production”. It is now well understood that agreement (or lack of) between these two quantities closely depends on the temporal scale considered (no one would expect it to work at daily time scales) and the C:N:O stoichiometry of

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respiration, production and recycling. It may work in some systems for some periods (especially when integrating estimates over the time course of a bloom and the few weeks after) but not in others. There are no previous mention or measurements of the f-ratio presented in the paper. It seems the authors are shooting at a straw-man (and for no obvious reasons), which distracts from the essence of the discussion.

The arguments presented in the section beginning at the bottom of page 7721 would benefit from a more detailed investigation of temperature effects on the rates presented here. The paper advocates an important role of future warming in shifting production/respiration ratios, but it's not clear whether the data actually presented in the paper provide a basis for this claim. It would be useful to see whether a correlation exist between temperature and CR or GPP rates normalized to chlorophyll a and temperature. I am left with the impression that production/respiration ratios are controlled by overall productivity (i.e. as a function of nutrient supply across different system) instead of temperature.

Page 7721, line 7: please specify that zooplankton respiration rates were estimated only during the ATOS cruise held in July 2007. It would also be useful in the discussion to assess (just a back-of-the-envelope calculation) whether including this additional respiration term (not captured in-vitro) would affect conclusions on net autotrophy/heterotrophy.

Page 7703, line 14: please specify to which paper Wassmann et al. (2006) is referring to (i.e., 2006a or b).

Page 7709, line 4: the publication Boyer Montegut et al. 2004 appears as Montegut et al. 2004 in the reference list.

Page 7719, line 18: please correct "Von Quillfeldt 1997, 2000"

Page 7728, line 9: add "2010" at the end.

The following references are missing from the reference list:

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Page 7703, lines 2-3: Sakshaug and Slagstad, 1991; Sakshaug et al., 1994. Page 7703, line 20 and Table 4: Apollonio 1980

The following references are in the reference list but not cited in the manuscript:

Page 7730, line 14: Reuer et al. 2007. Page 7730, line 17: Robinson and Williams 1993.

Figure 2 mentions standard errors but I don't see them. Is this because they are actually smaller than the symbols used for the mean?

Figure 3: Please change the order of the panels in the caption as follow: (B) GPP and (C) CR.

Figure 4: same comment than for Fig. 3.

Figure 5: the fitted line in Fig. 5 appears to be the 1:1 line instead of the regression line advertised in the legend. It certainly does not fit the data shown.

Table 2: Since the present study covers a vast sampling area and different seasons it would be useful to specify the area and period of the two papers used for comparison.

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