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Biogeosciences
Discussions

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

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Dr. Gerhard Herndl Associated Editor Biogeosciences

Dear Dr. Herndl

Please find attached a revised version of our manuscript (bg-2012-211) "Seasonal patterns in Arctic planktonic metabolism (Fram Strait - Svalbard region)". We have carefully considered the constructive comments by reviewers in preparing the revised version of the manuscript and have made, accordingly, extensive changes. We have revised the original version to address all of the comments raised by the reviewers

In particular, we now include a new figure (new figure 5) where we explore the relationship between metabolic rates and water temperature. We have also modified two

figures (figures 1 and 3) and have deleted a deleted figure (previous figure 5). We have also corrected all grammatical mistakes and typos included in the previous version of the manuscript. We now include a new table in the supplementary material (table S2) where we report the metabolic rates integrated to 30m depth and note that there are not significant differences between metabolic rates integrated to 20m from that integrated to 30m. We have elaborated further the possible consequences of global warming and ice melting in the Arctic Ocean on the metabolic rates in this area. The changes made are described in detail in the sections that follow below.

We believe, that as a result of these changes, the manuscript is now much improved relative to that originally one submitted, and hope that you will find it now acceptable for publication in Biogeosciences.

Sincerely,

Raquel Vaquer-Sunyer

Reviewer#1: The paper reports measurements of GPP, NCP and CR rates in the European Arctic – the work itself must have been a major undertaking and the data is potentially very valuable. The paper makes the point in the introduction that whereas there is work on the rates in the Antarctic (authors please note there is also UK and US work, as well as that from your laboratory) there is a paucity of work in the Arctic.

Comment: We agree we should not be parochial in our choice of references. Action: We now have provided more references on polar metabolic rates. Particularly we now also refer to Dickson and Orchardo 2001, Lefèvre et al. 2008 and Robinson et al., 1999.

Reviewer#1: Its presentation leaves a lot to be desired – it contains more than its share of often quite silly mistakes for example it's glaringly obvious that the line "fitted" to the data in Figure 5 is wrong, it certainly does not match the equation given in the text.

Comment: We apologize for the many errors, and the mistake in this figure. We have now deleted this figure following the suggestion by another reviewer and carefully checked the revised manuscript to avoid mistakes

Reviewer#1: There are quite a number of matters the authors need to give attention to – I have numbered the important specific points to make them clear. The paper is accompanied with Supplementary material in the form of a Table containing the data in the rates and standard errors. This is valuable to the reader but a mystery to me is that in the GPP and CR column of rates there are blank values with errors. I fail to understand how nonexistent value can have an error.

Comment: We apologize for these errors. Action: We have deleted the values entered in blank cells by error.

Reviewer#1: In a paragraph (p.7710, l. 21 et seq) they discuss their errors. Most of the numbers in this short paragraph appear to be incorrectly reported:

1) The report that "The experimental standard errors (SE) among replicate samples varied between 0.04 and 6.27 mmol O2 m-3, with a mean of 0.66 $\pm$0.03 mmol O2 m-3." From the supplementary material, the smallest SE value of the rate measurements I can find is 0.12, the largest is 54.85 and the mean is 1.3 mmol O2 m-3, all very different to what they report.

Comment: The standard errors reported here correspond to standard errors associated to the oxygen measurements. These errors do not refer to the standard errors associated to metabolic rates that have been calculated using error propagation. The initial oxygen measurements and their associated errors are not reported because it is a large amount of data, and only calculated metabolic rates and their associated errors are reported here. Standard errors associated to the metabolic rates are higher than associated to oxygen measurements replicates, but the highest value previously reported was 35.43 and not 54.85, as indicated by the reviewer. However, this high error was one of those corresponding to blank cells, which have been removed.

Action: We have edited the text to better explain that the standard errors reported here refer to the errors associated with the oxygen measurement and not the errors associated with metabolic rates and we have moved this paragraph to the methods section as suggested by another reviewer. The text now reads: "The experimental standard errors (SE) of O2 determinations among replicate bottles varied between. . ."

Reviewer#1: 2) They note that: "These errors represent a mean of 0.19 % of the total value of the measurement". This is incorrect; it must be 19%.

Comment: As explained above these standard errors refer to errors between bottles replicates and indeed represent a mean of 0.19% of the total oxygen content measured, ranging from 0.0004% and 2.29%.

Reviewer#1: 3) They then go on to claim "These errors are very close to the limit of analytical detection, reported to vary between 0.06 and 1 mmol O2 m3 (Robinson and Williams, 2005)". Robinson and Williams (Table 9.1) reported a range of 0.06 to 0.1 mmol O2 m3 – regardless of which the means you adopt (0.66 or 1.3) the upper end of the range of the author's values is 6-10 times greater that reported by Robinson and Williams, not by my measure "very close".

Comment: We agree with the reviewer that the higher SE is 6 times greater than that reported by Robinson and Williams. Action: We have changed the text to acknowledge that the lower range of our errors are close to the analytical detection limit but that we also have higher errors. The text now reads: "Although the lower range of these errors is close to the limit of analytical detection, reported to vary between 0.06 and 0.1 mmol O2 m3 (Robinson and Williams, 2005), the upper range of these errors is considerably higher"

Reviewer#1: They then go on to discuss their findings, which is useful. Their rates are summarised in Table 3 and Figure 3. I have two questions relating to Figure 3.

4) Why, if it's an analysis of seasonality, present the data in chronological order and not

seasonal

5) NCP must be presented as a linear plot and there is a logic in plotting GPP/CR logarithmically, but why plot GPP and CR on a log basis – doing so gains nothing and prevents easy comparison with NCP. Table 3 reports volumetric and integrated rates.

Comment: The suggestion of the reviewer, to group the data by season, is most useful. These figure are a greatly improvement and we thank Dr. Williams for the suggestion.

Action: We have redrawn the figure grouping the data seasonally and using liner scales for GPP and CR (New Figure 3).

Reviewer#1: The depth-integrated value for the ARTICOS study in based on a single point. The depth of integration used is 20m – whereas the reported mixed layer is given in the text as 67.7m (p. 7710, l. 15) during the dark period

6) What are the grounds for using 20m - the justification is given (p. 7707, l. 13 onwards) is not convincing. Author note: that if the euphotic zone is defined by light penetration this is independent of the irradiance level, i.e. it is the same in the dark as the light.

Comment: The selection of an integration depth in the high Arctic is rather cumbersome. Two criteria are used in the literature, mixed layer and a light reference (e.g. 1 % PAR). Regarding the photic layer, the integration depth during the winter period should be 0, as it is dark around the day and 0 light penetrates to any depth; this rules out the light penetration as a criteria. The mixed layer is also cumbersome, as ice melting in spring and summer leads to very shallow pycnoclines and, correspondingly, the mixed layer in only of 2-3 m depth, much shallower than the photic depth, and the water column can be mixed to considerable depths (> 100 m) in the winter due to convective mixing. Our choice of 20 m is, thus, based on the need to have a common integration depth and a compromise among the extremes discussed above. We decided to integrate down to 20m because this depth is close to both the chlorophyll a maximum layer

(23.5m) and to the mixed layer depth (17m) located below the shallow thermocline. To assess the impact of this choice on the calculations, we have also integrated to 30m depth. We didn't found significant differences between the metabolic rates integrated down to 30m with those integrated down to 20m. However, as some stations did not allow integration to 30m because of lack of data at that depth, we maintain the integration depth in 20m and we now include a new table in the supplementary material reporting metabolic rates integrated to 30m whenever this was possible (New table S2).

Action: We now discuss the nuances of settling on an integration depth for a year-round study in the Arctic. The text now reads: "The selection of an integration depth in the high Arctic is rather cumbersome. The two criteria most widely used in the literature, mixed layer and a light reference (e.g. 1 % PAR) are difficult to apply. Regarding the photic layer, the integration depth during the winter period should be 0, as it is dark around the day and 0 light penetrates to any depth; this rules out the light penetration as a criteria. The mixed layer is also cumbersome, as ice melting in spring and summer leads to very shallow pycnoclines and, correspondingly, the mixed layer in only of 2-3 m depth, much shallower than the photic depth, and the water column can be mixed to considerable depths (> 100 m) in the winter due to convective mixing. We chose to integrate down to 20 m across all cruises because this depth is close to both the chlorophyll a maximum layer (23.5m) and to the mixed layer depth (17m) located below the shallow thermocline in the summer. We assessed the sensitivity of our estimates this choice of integration depth by also calculating metabolic rates integrated down to 30 m depth. This exercise showed integrated metabolic rates to be rather insensitive to the choice of either 20 m or 30 m as integration depth (cf. table S2)."

Reviewer#1: 7) The standard error reported in the table I calculate to be the derived from the variance of the values used for the mean. This is quite in order but it should be made clear in the Table caption.

Action: We have made clear in the caption that the standard error corresponds to the variance of the values used to calculate the mean.

The authors then (p. 7711 & 7712) engage in a useful discussion of their work. They note correctly that although NCP is related to the f-ratio they cannot derive a value from their work –that is a sound conclusion but to be expected – f-ratio is in effect a time-averaged value.

Comment: We thank the reviewer for this comment.

Reviewer#1: They then (p.7713, l. 8 onwards) discuss the relationship between GPP and CR, as log(GPP/CR) v log (GPP) plots. I have noted that the line shown is incorrect and not consistent with the relationship (p.7713, l.19). This caused me to re-run the regression and was unable to obtain the same value are reported in the text. The difference was quite significant when it came to calculating the "threshold" value (see discussion bottom of p.7717) – I obtained a value of 1.7 against their value of 3.8. I spent some time trying to locate the cause and eventually found that data appear to have been omitted from the plot given in Figure 5, notably the "fliers" such as the 549 value for ATP 2010 6, 15m, but also several others.

Comment: We did not omit any value from the data set. The relationship reported here was made using base-10 logarithm. We have now transformed the data before fitting the regression line and we have obtained the same slope but a different intercept (-0.43 versus -0.19), yielding a threshold value of 1.79, as that obtained by the reviewer.

Reviewer#1: 8) If I am correct, the point one would make to the authors is that, whereas there may be good reason to omit particular values, this is a dangerous area in science and one needs to tread carefully. The justification needs to be spelt out - and an objective procedure needs to be adopted and declared. The convention is the deemed "filers" are shown on the plots, singled out in parenthesis (or some form of identification).

Comment: We agree, and have not omitted any value from our calculations.

Reviewer#1: The research group lays importance on the production rate at which P=R

– viewed as a "threshold" value, which they seem to regard as potentially a universal biological ecological property – akin to (but obviously not the same) as the compensation point. This might be the case in a closed ecosystem, however in a different context - when endeavouring to account for net heterotrophy – the group argues that the system is receiving significant external subsidies. This organic import will be embedded in the calculated "threshold", so the resultant value will have a "geochemical" component (net organic import, which supplements in situ photosynthesis) as well as a biological component. An in depth discussion of this is probably beyond the present paper, but there are a couple of elementary practical problems that need addressing in the present paper. The principle issue is what form of regression analysis to be used – an ordinary least squares or a Model II regression. The present paper uses an OLS regression (as far as I can determine – it is not specified), whereas the two Regaudie-de-Goux papers from the same group used Model II regressions. If I make an OLS and a Model II (MRA) analysis of a single dataset, I get a 2 to 3-fold spread of values, in some cases much greater spreads. Which one should we use?

Reviewer#1: 9) The question one asks is, if it is possible to get a range of values from such simple alternative forms of data processing, what, if any meaning, do you give to the numbers you obtain. I think an in depth discussion of these and other matters (e.g. what, other than some empirical property, are we measuring) relating to the "threshold" notion is long overdue –without clarification we are just generating numbers with no idea what they mean, if anything.

Reviewer#1: 10) Authors please make clear the form of regression used if OLS, why not the same as in the two Regaudie-de-Goux papers, and please check the consequence on the obtained "threshold' value.

Reviewer#1: A second issue is whether you should include non-significant values in regression analyses – they account for quite a significant fraction (30%) of the values in the present work. Whereas I can see that you can use non-significant values to derive a mean (as the SE can be incorporated into the overall SE) this is not available in the case of regression. The matter is made worse if log-log plot are used, as the non-significant values are characteristically the lower values in the data set and as log values they pull the line with as strong a moment as the higher, significant values. This needs some consideration.

Comment: We thank the reviewer for noting this puzzling result, which we had not detected. We have thoroughly explored the impact of the choice of least squares fitting mode, data transformation and the exclusion of non-significant results on the outcome of the threshold calculations. Action: We have now removed the metabolic rates non-significant (i.e. < 2*SE) to calculate the GPP threshold for metabolic balance. We have used both types of regression (OLS and model II) and we compared the results obtained. The GPP threshold for metabolic balance in Arctic communities should be encompassed by the range of results, between 3.01 and 5.22 mmol O2 m-3 d-1. The text now reads: "The GPP/CR ratio increased with increasing GPP, as observed elsewhere in the ocean (see Duarte and Agusti 1998, Duarte and Regaudie-de-Gioux 2009), implying that unproductive Arctic communities tend to have a low GPP/CR, thus tending to be heterotrophic. The fitted regression equation implies that the average GPP required to balance Arctic planktonic metabolism is 3.01 mmol O2 m-3 d-1, when using ordinary least squares (OLS) regression and of 3.82 mmol O2 m-3 d-1 when using model II regression. Fitting the relationship between Log CR and Log GPP using a logistic regression yields exactly the same result 3.01 mmol O2 m-3 d-1 as that obtained using ordinary least squares regression. However, use of the relationship between NCP and GPP to derive the GPP required to metabolic balance (i.e. GPP at NCP = 0) yields a higher value of 4.78 mmol O2 m-3 d-1, when using OLS regression and of 5.22 mmol O2 m-3 d-1when using model II regression. These rates are higher than average rates for oceanic communities (1.07 mmol O2 m-3 d-1), but lower than a previously reported value for the Arctic Ocean based on a more limited data set collected in summer (5.45 mmol O2 m-3 d-1, Duarte and Regaudie-de-Gioux 2009)."

Reviewer#1: In the section on Metabolic rates they raise the very pertinent problem of

the supply of organic material to the heterotrophic population during the dark period. I think it should be pointed out that of the 7 reported values, only 2 are significant (>2*SE), so we're dealing with a very thin data base. As noted earlier the SE they report in Table 3 appears to be derived from variance of the mean, if only 2 out of 7 values are significant then it is a pretty dubious value. More appropriate in this case is the error of this mean value – derived from the errors of the individual measurements – this by my calculation is $\pm$1.12 mmol/m3 d, greater than the absolute mean rate (0.84 mmol/m3 d).

Comment: We agree with the reviewer that the use of standard error derived form the error of the individual measurements would be more appropriate. CR values in winter were very low in most stations. These very low rates have associated a relative high SE. Action: We now include the SE derived from the SE of the individual measurements.

Reviewer#1: They argue that the winter respiration is sustained by the DOC prior to the onset of the dark period, i.e. that the calculated requirement (75.3 mmol/m3 d - presumably 90 dark days – authors please give us the basic details/justification of the calculation) is less than the mean DOC for the area (89 mmol/m3) thus there is sufficient DOC to sustain this demand. The implication is that there would be 89-75 = 14 mmol/m3 remaining by the end of the winter. This is wholly at variance with our understanding of the biogeochemistry DOC: there are several thousand DOC analyses and you simply do not encounter values below 35 mmol/m3 in the oceans – even in the deep ocean where there have been c.1,500 years for decomposition to occur. So, the proposition does not stand up to the most elementary analysis.

Comment: We agree that further details on the calculations are needed.

Action: We have included some text in the Methods section to give details in how the calculation was made. The text now reads: "An estimate of the DOC needed to sustain community respiration during the dark period was derived using the mean volumetric community metabolism integrated during that period (112 days). Conversion from oxygen to carbon was made assuming a 1.25 molar stoichiometry between O2 and C (Williams et al. 1979)."

11) The authors need to give more careful consideration to this analysis, as I feel it is flawed.

Comment: We agree that DOC at the end of the dark period cannot be 14 μg C L-1, and we have modified the text accordingly.

Action: We have acknowledged in the text that an important part of this DOC could be refractory and have identified different possible sources of DOC to the area during the dark period. The text now reads: "(…) suggesting that the large DOC pool in Arctic waters would suffice to maintain significant respiration rates in the plankton community across the dark period if all this DOC was entirely labile. However, the resulting DOC concentration would be below that ever recorded in the ocean. Hence, respiration rates in the plankton community across the dark period must be supported by allochthonous DOC inputs. During the dark period the West Spitsbergen Current transports warm Atlantic Water (AW) northward that melts ice and maintains open waters west of Svalbard. This Atlantic water transports important amounts of DOC that can be used to support bacterial respiration during the dark period. (…) Use of terrestrial DOM by marine bacterial communities will largely depend on its chemical composition and lability (Sondergaard et al. 2003)."

In summary, the authors present a useful set of data for a region of the oceans where next to nothing exists; in that respect the work is potentially valuable and welcome. The presentation and aspects of the analysis need attention. The authors need to go though the paper checking the details and correcting the errors. Clearly at this stage the paper is nowhere near suitable for publication, but the authors should be given the opportunity to res resubmit as the data it contains is useful.

Comment: We have carefully considered the constructive comments by yourself and

other two reviewers in preparing the revised version of the manuscript and have made, accordingly, extensive changes. We believe, that as a result of these changes, the manuscript is now much improved relative to that originally submitted, and hope that you will find it now acceptable for publication.

Reviewer#2: 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.

Reviewer#2: 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.

Comment: The selection of an integration depth in the high Arctic is rather cumbersome. Two criteria are used in the literature, mixed layer and a light reference (e.g. 1 % PAR). Regarding the photic layer, the integration depth during the winter period should be 0, as it is dark around the day and 0 light penetrates to any depth; this rules out the light penetration as a criteria. The mixed layer is also cumbersome, as ice melting in the winter leads to very shallow pycnoclines and, correspondingly, the mixed layer in only of 2-3 m depth, much shallower than the photic depth, and the water column can be mixed to considerable depths (> 100 m) in the winter due to convective mixing. Our choice of 20 m is, thus, based on the need to have a common integration depth and a compromise among the extremes discussed above. We decided to integrate down to 20m because this depth is close to both the chlorophyll a maximum layer (23.5m) and to the mixed layer depth (17m) located below the shallow thermocline. To assess the impact of this choice on the calculations, we have also integrated to 30 m depth. We didn't found significant differences between the metabolic rates integrated down to 30m with those integrated down to 20m. However, as some stations did not allow integration to 30m because of lack of data at that depth, we maintain the integration depth in 20m and we now include a new table in the supplementary material reporting metabolic rates integrated to 30m whenever this was possible (New table S2). We also now discuss the nuances of settling on an integration depth for a year-round study in the Arctic.

Action: We have explained better the reason to select 20 m as integration depth. The text now reads: "The selection of an integration depth in the high Arctic is rather cumbersome. The two criteria most widely used in the literature, mixed layer and a light reference (e.g. 1 % PAR) are difficult to apply. Regarding the photic layer, the integration depth during the winter period should be 0, as it is dark around the day and 0 light penetrates to any depth; this rules out the light penetration as a criteria. The mixed layer is also cumbersome, as ice melting in spring and summer leads to very shallow pycnoclines and, correspondingly, the mixed layer in only of 2-3 m depth, much shallower than the photic depth, and the water column can be mixed to considerable depths (> 100 m) in the winter due to convective mixing. We chose to integrate down to 20 m across all cruises because this depth is close to both the chlorophyll a maximum layer (23.5m) and to the mixed layer depth (17m) located below the shallow thermocline in the summer. We assessed the sensitivity of our estimates this choice of integration depth by also calculating metabolic rates integrated down to 30 m depth. This exercise showed integrated metabolic rates to be rather insensitive to the choice of either 20 m or 30 m as integration depth (cf. table S2)."

Reviewer#2: 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.

Comment: We agree with the reviewer that the manuscript will be improved exploring variation of metabolic rates with temperature. Regrettably, Chl a concentrations were not available for all cruises, so this relationship could not be sufficiently explored. Action: We have included a new figure showing the relationship between volumetric and integrated metabolic rates with temperature (new figure 5). We have explored the relationship between metabolic rates and temperature using quartile regression. We have added an explanation on the methods section: "Quantile regression was used to describe the temperature-dependence of the volumetric and integrated metabolic rates. The relationship between metabolic rates and temperature was described by fitting the relationship between the 90%, 50% (median) and 10% quantiles of the distribution of metabolic rates and water temperature. Quantile regression estimates multiple rates of change (slopes), from the minimum to maximum response, providing a more thorough description of the relationships between variables, which are missed by other regression methods focused on prediction of the mean value (Cade and Noon, 2003). Quantile regression can be considered as an extension of classical least squares estimation of conditional mean models to the estimation of a compilation of models for several conditional quantile functions, considering the median as the central parameter (Koenker, 2005).", as well as in the results section: "Both volumetric and integrated NCP and GPP tended to decrease with increasing temperature. Examination of the relationship between production rates (both NCP and GPP) and temperature showed that the range of production rates become narrower with increasing temperature, with most production rates being low at higher temperatures (Fig. 5). Conversely, volumetric and integrated CR tended to increase with increasing temperatures, with the range of respiration rates becoming wider with increasing temperature (Fig. 5).", and in the discussion section: "NCP and GPP tended to decrease with increasing temperatures, concurrent with recent experimental work (Holding et al. 2012). At low temperatures high GPP and NCP are reached during the spring bloom, and low GPP and NCP at stages previous to the development of the bloom. Thus, at low temperatures we found a high variability of NCP and GPP data (Figure 5), whereas at higher temperatures these metabolic rates tended to decrease and variability is lower. This suggests that the NCP and GPP are related to the stage of the bloom at lower temperatures, while at higher temperatures temperature dependence controls the relationship."

Reviewer#2: Please see below the comment about Figure 5 and the mistake of doing statistical analysis of gross production vs. respiration.

Action: we have now deleted this figure and calculated the GPP threshold for metabolic balance using different approaches (see comments to reviewer#1).

Reviewer#2: 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).

Comment: We apologize for grammatical mistakes and typos. We have revised and corrected the use of English language throughout the manuscript.

Specific comments Reveiwer#2: Abstract: "Net", "primary", "gross", "respiration", and "community" should not be capitalized. These aren't proper nouns.

Action: We have made the change requested by the reviewer

Reviewer#2: 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.

Comment: We agree with the reviewer that the word "must" is too strong. Action: We

have replaced the word "must" by "should"

Reviewer#2: 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.

Action: We have deleted the paragraph.

Reviewer#2: 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.

Comment: In next paragraphs we point that these data are not only important to gain additional understanding on the functioning of these communities and their role in the regional carbon budget, but they are also essential to provide baseline data to detect changes in Arctic planktonic metabolism with climate change.

Reviewer#2: 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.

Comment: We agree with the reviewer that a description of what previous studies found is useful. However, we note that this description is made in the discussion section, where we compare our results with that found in previous studies.

Reviewer#2: 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.

Action: We have re-write the sentence. The text now reads: "This last study is included here to provide a more complete assessment of the metabolism in this area, as it was conducted in the same area using the same methods."

Reviewer#2: P7704: The first line (clause) on this page is not a complete sentence, and the one that follows doesn't make sense.

Action: We have rewritten these sentences. The text now reads: "However, as integration depths vary between studies, these studies are not included here. Whereas the previous observational data were insufficient, the set of estimates reported here provides the first empirical basis with which to establish patterns in the seasonal variability in planktonic metabolism in the European Arctic Ocean. Additionally it allows us to provide a first approximation at the annual balance between gross primary production and plankton respiration in these communities."

Reviewer#2: P7708, line 16: "Chlorophyll", which begins this sentence, should be capitalized. There is the same problem in the Results on page 7710.

Comment: We agree with the reviewer. This was a typo.

Reviewer#2: Page 7710: The Results section needs a few subheadings.

Action: We have added subheadings to the results section.

Reviewer#2: P7710, line 21: This paragraph about precision can be moved to the Methods. Its current location disrupts the flow of discussing the real results.

Action: We have moved the paragraph to the Methods section.

Reviewer#2: 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.

Comment: The production/respiration ratio has been used extensively in scientific literature, not only for marine systems (e.g. Cota et al. 1996) but also for reservoirs (e.g. Forbes et al. 2012), rivers and streams (e.g. Mulholland et al. 2001) and lakes (e.g.

del Giorgio and Peters 1994). We believe that using the GGP to CR ratio is a valid approximation to investigate net heterotrophy and the extensive use of it in the literature supports our statement. We believe that the information provided by the CR to GPP ratio is not redundant with that of NCP, as NCP conveys no information on the relative difference in magnitude.

Reviewer#2: 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.

Comment: Quiñones and Platt (1991), state that new production can be equated to NCP under certain assumptions and conditions, and that the denominator of the f-ratio (New production plus regenerated production) can be equated to gross primary production. So, under certain conditions the ratio of NCP to GPP has been argued to be equated to f-ratio. Eppley and Peterson state that new production can be equated to export production. Concretely they say: "New production (. . .) is quantitatively equivalent to the organic matter than can be exported (. . .)".

Action: We have changed the text to acknowledge that NCP can be considered equal to export production as suggested by Eppley and Peterson. The text now reads: "The ratio of NCP to GPP (NCP/GPP) can be considered an estimate of f-ratios, the fraction of total primary production supported by nitrate (Quinones and Platt 1991). On a long-term basis and with the assumption of steady state, NCP can be considered equal to export production (Eppley and Peterson 1979), as the storage in the upper water column is small relative to the production rates."

Reviewer#2: P7714, line 1: The authors say that negative NCP has to be supported by allochthonous 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.

Comment: We agree with the reviewer that surplus production from recent time periods or advected OM from neighbor waters can also sustain negative NCP values. However, we would like to note that we are talking of time scales of one day (so negative NCP at night should be supported by production during the daytime of the same day).

Action: We have added some text to acknowledge that negative NCP can be also supported by production in a recent time period or in neighboring waters. The text now reads: "(...) supported by organic matter produced in a recent time period, advected from neighboring waters or allochthonous inputs."

Reviewer#2: 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.

Comment: The problem of using ratios in regressions with one of the variables is not that it is improper, is that the null hypothesis is not that the slope = 0. The relationship between GPP/CR and CR includes GPP is in both dependent and independent variables; in this case the null hypothesis of this relationship is not that the slope equals zero, but that it equals one. GPP is calculated using both NCP and CR. However, to be able to determine a GPP threshold for metabolic balance, the relationship between one of this parameters and GPP should be determined.

Action: We have deleted the figure 5, and calculated the GPP threshold for metabolic balance using different approaches. Specifically, we have used a logistic regression between Log CR and Log GPP, as well as explore the relationship between NCP and

GPP and the ratio GPP/CR using ordinary least squares regression and model II regression.

Reviewer#2: Discussion: Most Discussion sections are better without subheadings. It flows better without them, assuming the writer works at the transition between the big topics. Comment: We thank the reviewer for her/his positive comment and leave the discussion without subheadings as in the previous version of the manuscript.

Reviewer#2: 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.

Action: We have reduced this section by 25%.

Reviewer#2: P7717: The authors say that there is enough DOC because total DOC concentrations (around 80 uM) 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 uM, maybe more) of that DOC is refractory with a large fraction having turnover times exceeding thousands of years.

Comment: We agree with the reviewer that the refractory condition of a part of the DOC pool should be taken into account here. Indeed, although the DOC pools could be enough to satisfy the DOC requirements for bacterial respiration, we should also note that during winter advenction of Atlantic water could also supply organic mater to sustain bacterial respiration. Action: We have added some sentences to acknowledge that an important part of the DOC pool could be refractory and that allochthonous organic matter can also be advected with Atlantic water flowing into the Fram Strait during the dark period. The text now reads: "(...) suggesting that the large DOC pool in Arctic waters would suffice to maintain significant respiration rates in the plankton community across the dark period if all this DOC was entirely labile. However, the resulting DOC concentration would be below that ever recorded in the ocean. Hence, respiration rates in the plankton community across the dark period must be supported by allochthonous DOC inputs. During the dark period the West Spitsbergen Current transports warm Atlantic Water (AW) northward that melts ice and maintains open waters west of Svalbard. This Atlantic water transport important amounts of DOC that can be use for marine bacterial respiration."

Reviewer#2: 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."

Comment: We base our comment in the paper by Seuthe et al. 2011. In this paper Seuthe et al. investigated the microbial community and processes in the same stations where we measured planktonic metabolism during early spring 2008. During the study bacterial production was very low ($\leq$0.63 μg C l$-$1 d$-$1) and phototrophic protists biomass dominated over that of heterotrophic protists in the stations with autotrophic metabolism, suggesting that protists greatly contribute to community metabolism. We base our statement on low bacterial respiration in the very low bacterial production rates, however, we cannot separate bacterial respiration from that of the rest of the community, and, unfortunately, we cannot report the fraction of total respiration by bacteria. Action: We now include the reference to the paper of Seuthe et al. 2011.

Reviewer#2: 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.

Comment: We agree with the reviewer that his paragraph would be better closer to the discussion about negative NCP.

Action: We have moved this paragraph close to the discussion about negative NCP. We have also added some text about lability of the DOM. The text now reads: "Use of terrestrial DOM by marine bacterial communities will largely depend on its chemical composition and lability (Sondergaard et al. 2003). Glaciers can be a considerable source of labile organic matter to the marine environment in the Gulf of Alaska, with 66% of the total DOC being bioavailable (Hood et al. 2009). This study reported bioavailable DOC to range between the 23 and 66% in different watersheds of the Gulf of Alaska."

Reviewer#2: 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.

Action: We have deleted this table and presented more extensive description of DOC results. The text now reads: "DOC concentration were comparable in Atlantic waters (mean $\pm$ SE = 93.24 $\pm$ 5.20 μmol C L-1), than in warmed Polar waters (91.12 $\pm$ 3.55 μmol C L-1), and were lower in Polar waters (78.71 $\pm$ 2.26 μmol C L-1), although this difference was not significant ($p > 0.05$). The average DOC concentration (mean $\pm$ SE = 89.01 $\pm$ 2.46 μmol C L-1) was comparable to that previously reported in the same area, 104 $\pm$ 25.7 (Kritzberg et al. 2010) and 93.95 $\pm$ 54.526 μmol C L-126 μmol C L-1 (Tovar-Sánchez et al. 2010)."

Reviewer#2: 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.

Action: We have now deleted this figure.

References:

Cota, G. F., Pomeroy, L. R., Harrison, W. G., Jones, E. P., Peters, F., Sheldon, W. M., and Weingartner, T. R.: Nutrients, primary production and microbial heterotrophy in the southeastern Chukchi Sea: Arctic summer nutrient depletion and heterotrophy, Mar. Ecol-Prog. Ser., 135, 247-258, 1996.

del Giorgio, P.A., and Peters, R.H.: Patterns in planktonic P:R ratios in lakes: Influence of lake trophy and dissolved organic Carbon, Limnol. Oceanogr., 39, 772-787, 1994.

Forbes, M.G., Doyle, R. D., Scott, J. T., Stanley, J. K., Huang, H., Fulton, B. A., and Brooks, B. W.: Carbon sink to source: longitudinal gradients of planktonic P:R ratios in subtropical reservoirs, Biogeochemistry, 107, 81-93, 2012

Mulholland, P. J., Fellows, C. S., Tank, J. L., Grimm, N. B., Webster, J. R., Hamilton, S. K., Marti, E., Ashkenas, L., Bowden, W. B., Dodds, W. K., McDowell, W. H., Paul, M. J., and Peterson, B. J.: Inter-biome comparison of factors controlling stream metabolism, Freshwater biology, 46, 1503-1517, 2001.

Reviewer#3: 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.

Comment: We would like to thank the reviewer for her/his positive comments.

Specific comments:

Reviewer#3: 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 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.

Comment: We have included the 2 references suggested by reviewer.

Reviewer#3: Page 7703, line 27: please reformulate the following sentence: “the area studied is in the same.”

Action: We have rewritten the sentence. The text now reads: “(. . .) as it was conducted in the same area using the same methods.”

Reviewer#3: Page 7704, lines 1-2: please reformulate the following sentence: “However, as integration depths vary between studies are not included”

Action: We have rewritten the sentence. The text now reads: “However, as integration depths vary between studies, these studies are not included here.”

Reviewer#3: 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.

Action: We have added some discussion on possible influence of increasing water temperature on metabolic rates. The text now reads: “Warming is also expected to directly affect metabolic rates, as temperature plays an important role in regulating metabolic processes (Iriberri et al., 1985; White et al., 1991), and metabolic rates are expected to increase exponentially with water temperature (Brown et al., 2004)."

Reviewer#3: 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.

Action: We have included additional information in the map: Svalbard and the Barents Sea are now indicated in the map, as well as some arrows to indicate the currents directions. However, because of the lack of space in the map we have not included Kongsfjorden-Krossfjorden. We believe that the symbols of the stations should provide enough guidance to identify it in the map.

Reviewer#3: 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.

Comment: Five periods are listed in the text: (1) the dark period in the late fall- early winter, (2) early spring, (3) spring, (4) late spring-early summer, and (5) summer. However, we agree with the reviewer that there was a mistake in the list of cuises. Action: We have corrected the list of cruises and in figure 3 we have grouped the results by sampled period.

Reviewer#3: Pages 7706-7707, Ships were specified for the two early spring cruises (KV Svalbard icebreaker) and for the December 2006 cruise (R/V Jan Mayen) but not for the other cruises (Hesperides in the acknowledgement section?).

Action: We now include the names of the ships. The text now reads: "Seven stations

were sampled in December 2006 on board R/V Jan Mayen (Fig. 1, Table 1). Our two early-spring cruises (2007 and 2008) were conducted in a pre-bloom situation, in heavily ice-covered waters on board the icebreaker KV Svalbard. Twenty-two stations were sampled in July 2007 on board R/V Hespérides; seven in summer 2008, eight in June 2009, seven in spring 2010 and twelve in spring 2011, all on board R/V Jan Mayen (Fig. 1, Table 1)."

Reviewer#3: 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.

Comment: Incubations onboard were performed to emulate conditions in situ, i.e. receiving the same amount of light and temperature conditions. We believe that both types of incubations (performed "in situ" and onboard) are comparable for the Arctic Ocean, where almost no differences in water temperature were found in the upper 20m. Action: We have added a sentence to state that both methods are comparable. The text now reads: "As incubation conditions mimic environment conditions the results are comparable with incubations performed in situ."

Reviewer#3: 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.

Action: We have acknowledged in the text the conditions needed to assume that new production is equivalent to export production. The text now reads: "On a long-term basis and with the assumption of steady state, NCP can be considered equal to export production (Eppley and Peterson 1979), as the storage in the upper water column is small relative to the production rates."

Reviewer#3: 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.

Action: We have removed the paragraph on page 7714.

Reviewer#3: 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.

Action: We have added the methods used.

Reviewer#3: 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.

Comment: In this paragraph we report results about the autotrophy/heterotrophy communities in the stations. We believe that report the percent of stations that support heterotrophic communities are results and do not belong to the discussion section.

Reviewer#3: Page 7716, line 1: please replace "that" by "than".

Action: we have made the change requested by the reviewer.

Reviewer#3: 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.

Action: We have deleted this sentence as suggested by the reviewer.

Reviewer#3: 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 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.

Comment: We have mentioned f-ratios before in the result's section so we do not agree that the text comes out of nowhere. We believe that this paragraph adds useful discussion of the results. Another reviewer also founds this text is quite relevant and a useful discussion of the work.

Reviewer#3: 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.

Comment: We agree with the reviewer that exploring the correlations between metabolic rates and water temperature will improve the manuscript.

Action: We have explored the relationships between both volumetric and integrated metabolic rates with water temperature. However, we couldn't normalize to chlorophyll a as we lack the needed data for most cruises. We found significant negative relationships between production rates and water temperature. However, NCP and GPP showed a much higher range of values at the lower end of water temperature and we consider more appropriate to fit quantile regressions than ordinary least square regressions. We have now added a new figure (new figure 5) showing the relationships between metabolic rates and water temperature and the fitted 90, 10 and 50% quantile regressions. We have added an explanation on the methods section: "Quantile regression was used to describe the temperature-dependence of the volumetric and integrated metabolic rates. The relationship between metabolic rates and temperature was described by fitting the relationship between the 90%, 50% (median) and 10% quantiles of the distribution of metabolic rates and water temperature. Quantile regression estimates multiple rates of change (slopes), from the minimum to maximum response, providing a more thorough description of the relationships between variables, which are missed by other regression methods focused on prediction of the mean value (Cade and Noon, 2003). Quantile regression can be considered as an extension of classical least squares estimation of conditional mean models to the estimation of a compilation of models for several conditional quantile functions, considering the median as the central parameter (Koenker, 2005).", as well as in the results section: "Both volumetric and integrated NCP and GPP tended to decrease with increasing temperature. Examination of the relationship between production rates (both NCP and GPP) and temperature showed that the range of production rates become narrower with increasing temperature, with most production rates being low at higher temperatures (Fig. 5). Conversely, volumetric and integrated CR tended to increase with increasing temperatures, with the range of respiration rates becoming wider with increasing temperature (Fig. 5)." and in the discussion section: "NCP and GPP tended to decrease with increasing temperatures, concurrent with recent experimental work (Holding et al. 2012). At low temperatures high GPP and NCP are reached during the spring bloom, and low GPP and NCP at stages previous to the development of the bloom. Thus, at low temperatures we found a high variability of NCP and GPP data (Figure 5), whereas at higher temperatures these metabolic rates tended to decrease and variability is lower. This suggests that the NCP and GPP are related to the stage of the bloom at lower temperatures, while at higher temperatures temperature dependence controls the relationship.", and in the discussion section: "NCP and GPP tended to decrease with increasing temperatures, concurrent with recent experimental work (Holding et al. 2012). At low temperatures high GPP and NCP are reached during the spring bloom, and low GPP and NCP at stages previous to the development of

the bloom. Thus, at low temperatures we found a high variability of NCP and GPP data (Figure 5), whereas at higher temperatures these metabolic rates tended to decrease and variability is lower. This suggests that the NCP and GPP are related to the stage of the bloom at lower temperatures, while at higher temperatures temperature dependence controls the relationship."

Reviewer#3: 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.

Comment: We agree with the reviewer that including zooplankton respiration in the calculations would be useful to include in the discussion. However, as the data on zooplankton respiration are integrated over 200m and our metabolic rates over 20m this calculation cannot be done, as zooplankton respiration is not expected to be homogeneous over 200m. Action: We have specified that zooplankton respiration rates were referred to the cruise conducted in summer 2007.

Reviewer#3: 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.

Action: We have made the changes requested by the reviewer

The following references are missing from the reference list: 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.

Action: We have revised throughout the reference list and we have included the references that were missing to the reference list

Reviewer#3: 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?

Comment: Standard errors are quite low, ranging from 0 to 0.4 (table 1), and they are smaller than the symbols used for the mean.

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

Action: We have made the change requested by the reviewer. We have also modified this figure as a request of Reviewer#1, and now we present the data grouped by seasons instead of using chronological order.

Reviewer#3: Figure 4: same comment than for Fig. 3.

Action: We have made the changes requested

Reviewer#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.

Comment: we apologize for the mistake in the draw of the regression line. We have deleted this figure from the manuscript as a request of another reviewer. We have also recalculated the GPP threshold for metabolic balance using different approaches (please see responses to reviewer#1 for details)

Reviewer#3: 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.

Comment: The two papers used for comparison were conducted during 2 of the cruises reported here (summer 2007 and summer 2008). We have deleted this table as a request of other reviewer.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C4165/2012/bgd-9-C4165-2012-supplement.pdf

Interactive comment on Biogeosciences Discuss., 9, 7701, 2012.

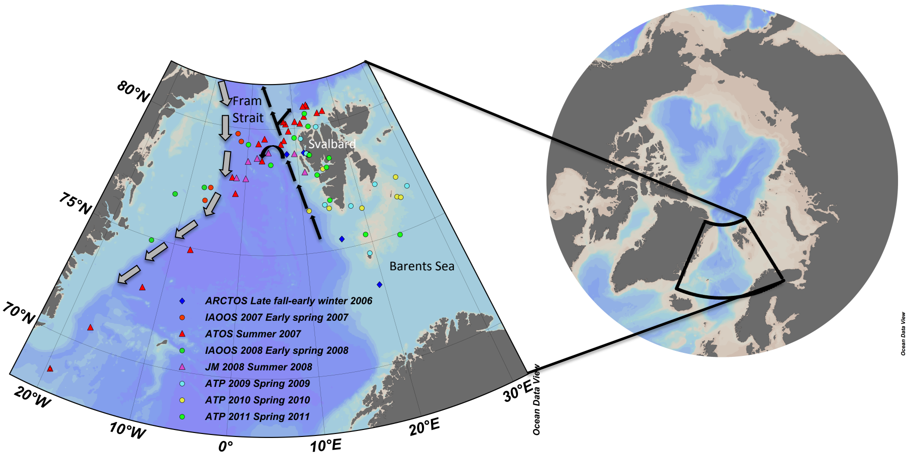

Fig. 1. New Figure 1

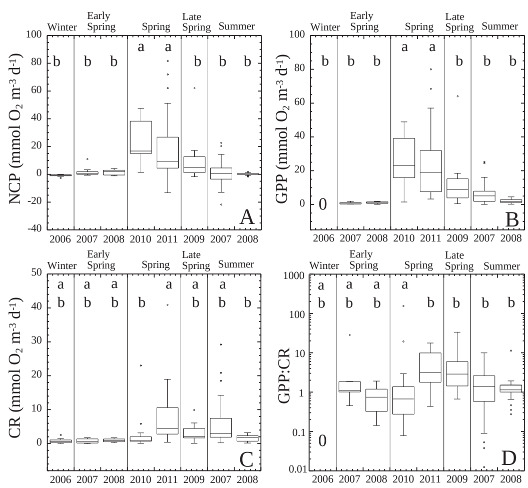

Fig. 2. New Figure 3

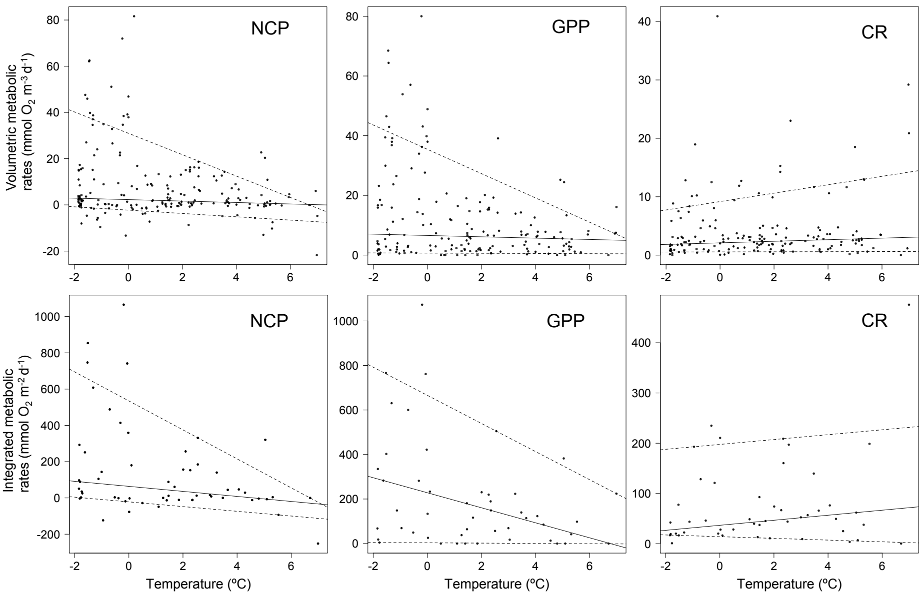

Fig. 3. New figure 5