Review Link et al. (2012): Multivariate benthic ecosystem functioning in the Arctic - Benthic fluxes explained by environmental parameters in the southeastern Beaufort Sea, **bg-2012-473**

Authors' response to Referee #1, Dr Holtvoeth

We thank Dr Holtvoeth for his thorough review of our manuscript. We value the numerous constructive comments as well as technical and detailed corrections he proposed for improving our manuscript. We will integrate them into our revised manuscript. In the following we answer to Dr Holtvoeth's comments and questions point-by-point (Replies in italics).

JH:

General comments:

The manuscript by H. Link and co-authors presents a very fine data set of biogeochemical benthic fluxes data for the Canadian Arctic. It is well written, the title is appropriate and the scientific approach is sufficiently backed up by references to other studies. Regarding the scarcity of such data, it is definitely worth publishing and well placed in *Biogeosciences*. There are some weaknesses, mainly in the structure of the paper and in the way the data is presented, that I will explain in the following. I believe, however, that these weaknesses can be eliminated relatively easily since the data set and its interpretation are generally sound.

Scientific approach

With the current fundamental changes affecting the Artic environment in mind, the authors set out a clear target: using a statistical approach, can they identify environmental factors that allow predicting benthic ecosystem response to environmental change and associated changes in nutrient fluxes across the sediment-water interface. Such changes in benthic biogeochemical fluxes could inverse the role of the seafloor as sink or source for nutrients which, in turn, will change the nutrient budget of the water column and affect marine productivity.

The essential environmental factors controlling the functioning of the benthic ecosystem are the supply of marine and terrigenous organic and inorganic material (quantity and quality) and bottom water oxygen content. While the inorganic material is a major control of sediment properties such as porosity (permeability/oxygen penetration) the organic material is the food source of the benthic community. The level of oxygen controls the way and, to some extent, the efficiency of organic matter breakdown (aerobic *vs*. anaerobic organic matter degradation, turn-over rates). Fluxes of oxygen and of compounds released from the breakdown of organic and inorganic sedimentary material (nitrogen-containing compounds, silicic acid) could be monitored as well as Mackenzie River run-off and particle load, bottom water oxygen concentration and marine primary productivity (via surface water chlorophyll concentration, for example). So, if one of these fluxes indicative for benthic organic and/or inorganic matter remineralisation, or a combination thereof, would be found to strongly correlate one way or the other with one of the environmental factors one would be able to predict the benthic ecosystem response towards major environmental changes. So far, so good.

However, this fairly clear target gets lost to some extent at the point when a research question and three working hypothesis are introduced towards the end of the introduction chapter. For example, the research question ("*What is the spatial variation of benthic boundary fluxes [...]?*") is not exactly a research question, the way I understand it, at least, since the answer will be a mere observation rather than providing a new causal relation. A serious research question in this sense would be, for example: What *drives* the spatial variation of benthic boundary fluxes? This could actually be the subtitle for the whole of the discussion during which another question could be raised: "Is oxygen flux a suitable proxy for benthic activity?" skipping the first hypothesis. The remaining two work hypotheses greatly overlap in their focus and could be discussed in conjunction.

I think the authors have made their lives unnecessarily difficult by steering away from a pretty clear target and coming up with the research question and hypotheses, instead. I would therefore suggest skipping the research question and merging two of the hypotheses or, even better, to abandon them altogether. This would allow for a more focussed and straightforward discussion.

Reply:

Hypotheses are a common way to structure research manuscripts in ecology. In general, the main study question can only be tackled by breaking it down into several sub-questions, for each of which a hypothesis is formulated that can be tested by an appropriate statistical test. Without clear hypotheses, statistics cannot refute a hypothesis. An important point in our statistical approach is the use of regression models, which are cause-and-effect models, allowing for predicting benthic remineralisation from environmental parameters. This is different from using correlation models, which simply describe a relation between variables without direction of the effect.

About hypotheses and statistics: The philosopher Sir Karl Popper mentioned in his book of 1963 (Conjectures and Refutations: The Growth of Scientific Knowledge; Routledge) that the progress of knowledge demands that theories become more and more testable. It follows that the rational scientist should never defend any theory "beyond refutation." The more precise and the more comprehensive a hypothesis is, the greater is the "risk" it takes of being refuted.

Furthermore, as Chamberlin (1965) wrote: "Conscientiously followed, the method of the working hypothesis is a marked improvement upon the method of the ruling theory; but it has its defects which are perhaps best expressed by the ease with which the hypothesis becomes a controlling idea. To guard against this, the method of multiple working hypotheses is urged." *So, the advantage of subdividing the manuscript according to a number of testable hypotheses is that the reader can locate and relate data, statistical methods, results and interpretation according to each hypothesis. This favors transparent result presentation. We use the final conclusion to summarize how the results of the different hypotheses tested allow for answering our main study question.*

We agree that we can improve the structure of our manuscript.

1. We acknowledge that the use of one research question and three hypotheses is confusing. The first section of our results is indeed purely descriptive and provides an overview of the different flux data observed, which we consider relevant research because no other baseline data in the study region exist. Nevertheless, we think it may be adequate to combine the presentation and interpretation of these results with the first hypothesis. Thus, sink and source distribution will be described for each flux, before testing the hypothesis that "The classical proxy of benthic activity, oxygen flux, does not determine the overall spatial variation (i.e., relative sink and source distribution) in nutrient fluxes".

2. We agree that both hypotheses 3 and 4 refer to the influence of different environmental parameters on benthic remineralisation fluxes. However, there is a fundamental difference between them: Testing ***hypothesis 3*** *means to check separately whether different combinations of environmental parameters affect SINGLE fluxes. The rejection of this hypothesis would have meant that the same environmental forcing controls each of the different fluxes measured here. In that case, one predictive model of a flux could serve for all different fluxes and therefore the general benthic remineralisation function. The support of our hypothesis (each flux is determined by a different set of environmental parameters) emphasizes that it is necessary to find a combined approach for a simplified model to predict benthic remineralisation function. (Nevertheless, the insight from our results may facilitate modeling of single nutrient biogeochemical cycles by including broader environmental parameters.) For a simplified statistical model predicting all different fluxes with the same environmental parameters at the same time we test* ***hypothesis 4****.*

We agree that the difference between the two approaches has not been adequately emphasized in the manuscript and improve this in the revised version of the manuscript accordingly.

JH:

Data presentation

The presentation of the field data (Figure 2) could definitely be improved (see also detailed comments). The description of the results and the discussion were sometimes hard to follow as the data is not presented in a straightforward spatial context (Fig. 2 contains longitude, only). Some data is not presented in a figure or table, at all. I found myself comparing site numbers in the map (Fig. 1) with sinking particle flux and $^{13}C_{org}$ data from the supplementary file, for example. Contour plots might do a better job in presenting the data. At least the sinking particle flux should be presented this way since it would also illustrate the potential influence of the Mackenzie River plume and where terrestrial contributions could be expected.

<u>Reply:</u>
We agree that the data presentation needs to be improved. Fig. 2 was intended for a whole-page display (which we found out to be impossible with BGD publishing). Adding site numbers to Fig. 2 should help with its interpretation.
We consider the environmental data in our study as auxiliary data used to understand the variability in benthic boundary fluxes, and not our main results. Since these data are mostly unpublished, we feel they should be made available as supplementary information. However, we would like to avoid a lengthening of the manuscript and therefore keep this table as a supplement, since Biogeosciences calls for short and concise manuscripts opposed to other journals such as Deep Sea Research or Progress in Oceanography.
We acknowledge that contour plots are particularly suited to present spatially distributed data. However, they are only meaningful if minimum requirements in data quantity, distribution and resolution are met to allow for sound between-data point interpolations. As this is not the case for our study, we refrained from presenting contour plots in our manuscript.

JH:
Interpretation
The biogeochemical fluxes determined in this study, associated processes and principal causes are sufficiently explained. The value of the study lies in the fact that it contributes to explain the well-known but poorly understood patchiness of benthic life frequently observed at the seafloor and that it tries to link environmental factors and the state of the benthic ecosystem.
However, I felt a bit let down by the authors when the discussion turns on oxygen flux as a proxy for benthic remineralisation. So, if oxygen flux is not really representative of organic matter remineralisation, what is the alternative? Oxygen flux is a number that can be transferred into organic carbon turn-over rates. These calculated rates may correlate well or poorly with the real rates, which probably depends on the sedimentary setting. Of course, at the current stage, i.e., without a sound empirical data base, the authors are not in the position to present a formula for improved remineralisation rates based on combinations of biogeochemical fluxes. However, if they suggest their colleagues working on benthic biogeochemical processes should consider other fluxes in addition to oxygen, what should they do with these? What to look out for? At the very least, the authors should develop a concept for future work on the matter and give clues where a solution to improved benthic carbon turn-over rates might be found. It would be great if the authors could come up with a re-assessment of the suitability of oxygen flux as a proxy for benthic remineralisation based on their findings. Does oxygen flux still give a fairly good idea of organic matter remineralisation on the southeastern Beaufort shelf whereas it appears pretty unreliable in areas with high terrestrial input, for example?

<u>Reply:</u>
In our study we demonstrate statistically that oxygen fluxes provide only a partial measure of carbon-turnover rates. We do not intend to provide a mechanistic understanding of geochemical formulas. Instead, we want to contribute to a better understanding of how environmental factors may influence the release of nutrients from the benthic to the pelagic system. We suggest that (i) qualitative models to predict spatial variation of multivariate benthic remineralisation from environmental parameters should be improved (these may be relative values) and that (ii) easily measurable fluxes (e.g., oxygen) may be used to calibrate the relative output to absolute expected values. An improvement of qualitative models could be the integration of information on organismal presence. We think that Dr Holtvoeth's last question is highly interesting and should be considered in further studies. In our investigation, however, we think that we do not have sufficient data to address this issue, since it would require data from sites with high non-terrestrial input as well. Simple correlation tests,

omitting sites 390 and 690 do not indicate high correlation of oxygen with any other flux (data from Table S1). The PCA without sites 390 and 690 does not indicate oxygen as a good proxy elsewhere either (results are similar, if site 680 is also omitted):

Eigenvalues

PC	Eigenvalues	%Variation	Cum.%Variation
1	2.84	47.4	47.4
2	1.56	26.1	73.4
3	0.819	13.6	87.1
4	0.584	9.7	96.8
5	0.124	2.1	98.9

Eigenvectors

(Coefficients in the linear combinations of variables making up PC's)

Variable	PC1	PC2	PC3	PC4	PC5
O2 [mmol m-2 d-1]	0.415	0.532	0.176	0.111	-0.133
Si(OH)4 [µmol m-2 d-1]	-0.560	-0.061	0.204	-0.051	0.652
PO42- [µmol m-2 d-1]	-0.483	-0.139	0.495	0.314	-0.628
NH4+ [µmol m-2 d-1]	0.316	-0.473	-0.050	0.781	0.229
NO3- [µmol m-2 d-1]	-0.202	0.679	0.061	0.491	0.228
NO2- [µmol m-2 d-1]	0.375	-0.094	0.822	-0.190	0.242

JH:

The "Conclusions" are another weak section of the manuscript. Opening the conclusions with a question and answer that cast doubts whether or not the whole study was actually worth the effort is not great. Since many readers will skim through the manuscript and read the abstract and conclusions, only (sad and a bit unethical but we all do it sometimes), it is best to open the conclusions with a one-liner repeating the main target of the study ("In our study of benthic biogeochemical fluxes on Arctic shelves we tried to identify environmental factors that would allow to predict..." or something like that). This should then be followed by the key observation(s) and whatever could be achieved towards reaching the target. Even if the target was not hit 100%, there will always be improvements of current knowledge that should be highlighted and insights as towards which measures have been missing/would be required to reach the target (here: benthic faunal composition, for example).

This study also illustrates the general importance of the benthic ecosystem with regard to the role of the sea floor acting as either source or sink of nutrients in the overlying water column. Highlighting this could add a little more relevance and give the final paragraph(s) a twist, e.g., towards current debates on geo-engineering measures such as ocean fertilisation even though this, of course, is considered for very different oceanic settings. Nevertheless, an opportunity to point out the fine balance established in benthic ecosystems and their apparent vulnerability towards anthropogenic or natural environmental change should not go amiss.

Reply:

We agree with Dr Holtvoeth and the conclusion is thoroughly corrected in the revised manuscript.

JH:

Detailed comments:

Abstract

The abstract is currently fairly long but could easily be streamlined. For example, the hypotheses do not necessarily have to appear in the abstract. The entire section from "*to address the following question and hypotheses*" to "*... drive the overall spatial variation in benthic boundary fluxes*" (lines 10 – 17) could be replaced with something like "... aiming to identify the key controlling factors of these boundary fluxes through a statistical approach." This would save quite a few lines and make the abstract a more straightforward read.

Reply: *Agreed. The hypotheses will be summarized in the abstract.*

JH:
Page 16935, lines 27-28: Sediment pigments and $_{13}C_{org}$ levels do not (actively) "*explain*" fluxes of silicic acid as the fluxes do not result from these parameters - rather the opposite. One might say, e.g.: "Fluxes of silicic acid correlate best with ..." – and that is due to...? This relation presumably results from siliceous algae being the main primary producers of pigments and isotopically heavy organic matter?

Reply:
We agree with the incorrect expression. Sediment pigments explain the variation in fluxes of silicic acid. The used analysis (regression) is cause-and-effect, and therefore the word "explain" is used, but explaining the variation. $_{13}C_{org}$ will be omitted from the sentence, it is not the correct result.

JH:
Page 16936, lines 2-5: *"We conclude that it is necessary to consider long-term environmental variability in the prediction of the impact of ongoing short-term environmental changes on the flux of oxygen and nutrients in Arctic sediments."* So, is this meant to say that, in the long run, short-term variability of benthic boundary fluxes will change? Isn't this a long-term change in itself? "Short-term" as defined earlier by the authors means "seasonal to annual" variability. This is obviously "ongoing". Or does "ongoing" in this case refer to the current climate change and associated changes in nutrient and carbon fluxes in the Arctic? This would then be a long-term change (annual to decadal) according to the authors' definition. This sentence is somewhat confusing. Please, clarify.

Reply:
"We conclude that it is necessary to consider long-term environmental variability along with rapidly ongoing environmental changes to predict the flux of oxygen and nutrients Arctic sediments even at short time scales."

JH:
1 Introduction
Page 16937, lines 17-19: "*Thus, the quality of organic matter at the seafloor will influence the pattern of benthic nutrient remineralisation* [...]." In this context, the authors might also be interested in recent complementary findings from the Crozet Islands where both biomass and species distribution of the benthic macrofauna are determined by the amount and the quality of organic matter (unsaturated fatty acid content, in particular) arriving at the seafloor (Wolff et al., 2011).

Reply:
We thank the reviewer for this information. Complementary information on the quality of organic matter that reach the seafloor in our studied area have been presented by Rontani et al. (2012) and Tolosa et al. (2013) in the Malina special issue. Molecular lipid biomarkers (hydrocarbons, alcohols, sterols and fatty acids), compound-specific isotope analysis, lipid content and some products of phytoplancton and terrestrial higher plants have been measured in surface sediments of the Mackenzie Shelf and slope in summer 2009. This data allow to better constrain the sources of terrestrial and marine organic matter on the Mackenzie Shelf and Slope. Unfortunately, the absence of replication in these studies and the weak number of common sampling sites limited the integration of these data in our statistical approach. Nevertheless we agree, that the impact of changes in the pelagic polar ecosystem on benthic ecosystem (and remineralisation) functioning will amplify through its influence on benthic fauna.

JH:
Page 16938, lines 18-22: Should the authors want to keep working hypotheses I suggest merging hypotheses (3) and (4), which can be done without loss of meaning. For example: "(3) Different combinations of environmental conditions that vary either on a long-term (decadal) or short-term (seasonal to annual) scale determine individual fluxes as well as the spatial variation in benthic boundary fluxes."

Reply:
We thank Dr Holtvoeth for this suggestion. However, under the section of 'General comments', we

explained why we consider it a loss of meaning to join these two hypotheses. The aim is to show, that it is not the same combination of predicting variables if a single flux is considered OR if all fluxes are considered simultaneously. We emphasize this more clearly in the introduction.

JH:

2 Material and methods

This section appears generally okay, sampling methods and lab procedures are described in sufficient detail. As I am not into statistics to the same extent as the authors, I cannot reliably judge the appropriateness of the methods applied.

Page 16939/40, lines 28 and 1-3: "*Six additional sub-cores of 2.4 cm diameter and 8 cm and 1 cm length were taken for determining sediment pigment concentration and water content and sediment solid phase composition, three sub-cores each, respectively (Table 1). Samples from the sediment surface (0 to 1 cm sediment depth) of additional sub-cores were stored in...*" A bit confusing; better: "Six additional sub-cores of 2.4 cm diameter were taken, three of 8 cm and 1 cm length, respectively, for determining sediment pigment concentration, water content and sediment solid phase composition (Table 1). The surface samples (0 to 1 cm) were stored in..."

Reply: *We thank Dr Holtvoeth for his suggestion and reworded the phrase accordingly.*

JH:

Page 16940, lines 11-16: "*Chl a and phaeopigment concentrations were analysed fluorometrically ... after acidification. Chl a and total pigment concentration (Chl a + phaeopigments) were determined.*" Replace "*analysed*" with "determined" and delete the sentence: "*Chl a and total pigment ... were determined.*"

Reply: *We thank Dr Holtvoeth for his suggestion and correct the paragraph.*

JH:

Page 16940, lines 20-22: *"The dried solid fraction was homogenised and the water content used to correct the analyses for the presence of sea salt.*" - Which analyses were corrected for the presence of sea salt? I don't quite understand what was done, here. Does this simply mean that the weight difference between wet and dry sample was converted to seawater content for the calculation of porosity using an average seawater density? Please, clarify.

Reply:

We exactly performed what the reviewer described. A subsample of surficial sediment was sealed in a preweighed vial and frozen under inert atmosphere for later determination of porosity. We adapt the phrase:

"The dried solid fraction was homogenized. Porosity was determined from weight loss upon freeze-drying. The weight difference between wet and dry sample was corrected for salt from seawater content using an average seawater density (2.65 g cm^{-3}) and the value of salinity measured in the bottom waters."

JH:

Page 16942, line 1: *"... bottom water collected by the rosette ...*" – A rosette is not mentioned before, but supposedly the authors mean the water was collected by a rosette of Niskin bottles fitted to the CTD?

Reply:

We thank Dr Holtvoeth for his comment and adapt the phrase: "... bottom water collected by Niskin bottles of the CTD-Rosette."

JH:

Page 16942, lines 8-10: *"During incubations, oxygen concentration never decreased by more than 25% in order to avoid anoxic conditions and biogeochemical transformations.*" – I suppose this means that oxygen concentrations were not allowed to decrease by more than 25%? Did you top up the oxygen when concentrations dropped below a certain level?

Reply:
We clarify: "Incubations were stopped when oxygen levels had decreased by more than 20% to avoid anoxic conditions and biogeochemical transformations."

JH:
Page 16944, lines 7-9: *"Changes in porosity of sediments depends on the sedimentation rate, which is generally about 1 mm yr^{-1} in the study area (...) and can therefore also be considered long-term."* – The value for the average sedimentation rate alone does not allow concluding that porosity changes long-term, only. Various factors determine porosity: grain-size distribution, primarily, but also composition and compaction. The latter, of course, does depend on sedimentation rates to a large extent. However, I can easily imagine settings where porosity varies while the sedimentation rate remains the same and vice versa, and this may even occur on short time scales. Varved sediments show changing porosity with annual frequency, for example. Similarly, blooms of large diatoms may change porosity on a seasonal basis. In these cases, changes in porosity are due to changes in sediment composition, or source, rather than a change in sedimentation rate. I would think that in the given setting, with low sedimentation rates, compositional changes, i.e. changes in sediment sources, are more important a factor for sediment porosity than sedimentation rate. The sedimentation rate may vary synchronously; however, it is not the ultimate cause for a change in porosity.
What is the main sediment source? I would expect predominantly riverine supply at some of the sites studied (690, 680, 390). Since the Mackenzie River supposedly shows strong seasonal changes in run-off, like most arctic/subarctic rivers, the quality of the delivered sediment and, hence, porosity might also vary short-term. Having said all this, I do actually agree that changes in porosity do actually reflect rather long-term variability at least at the more distal sites. However, the authors cannot argue with the sedimentation rate to define porosity as a long-term changing proxy.
How about referring to the long-term trends in Mackenzie run-off, for example? (data available online, e.g., at http://www.eoearth.org/article/Freshwater_discharge_in_the_Arctic) Then, again: since river-controlled $^{13}C_{org}$ is categorised as "*other*" environmental factor, perhaps porosity should be seen as such, as well?
Reply:
We agree that sedimentation rate does not translate directly to porosity. We will clarify our definition based on the following explanation:
As Dr Holtvoeth explains, porosity and wet bulk density are typical bulk parameters, which are related to the sediment type – terrigeneous, calcareous and siliceous – and composition. In our studied area, most of the Mackenzie Shelf and Slope is covered by marine mud, predominantly silt and clay-sized (Hill et al., 1991;Conlan et al., 2008). Deeper sediments are composed of glacial sediments from the end of the Pleistocene, covered with deltaic deposits from the end of the Pleistocene and the Holocene (Blasco, 1991;Hill, 1996;Hill et al., 2001). Recent results on the organic fraction of the uppermost sediment layer also indicate that the gradient of terrestrial input from the shallow to deeper Beaufort Sea is not very pronounced, and that the input of marine derived matter is most important (Tolosa et al., 2013). Porosity profiles are very similar in the uppermost sediments layers of the studied area, for shallow and deeper sites, with values between 0.85 and 0.90 at the top of the sediment column and between 0.75 and 0.80 below 10 cm depth. The following figure presents porosity data obtained in sediments of the Mackenzie Shelf and slope in summer 2004 (Courtesy of C. Magen).

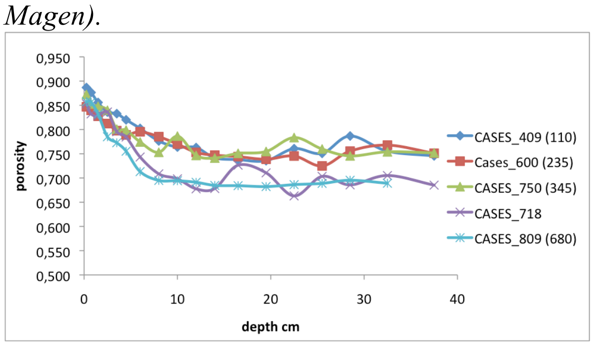


Figure: porosity profiles measured on sediment collected during CASES program in summer 2004. Numbers under comma refer to Malina sampling stations. (Courtesy of C. Magen)

Long-term variation:
Assuming an average sedimentation rate of ~1 mm yr^{-1} (as reported in Richerol et al. (2008) and Bringué and Rochon (2012) using ^{210}Pb in excess dates), the first 40 cm of sediment represents ~400 years of sedimentation and the surficial sediment we sampled represents ~5-10 years. Seasonal variations of surficial (< 1mm) porosity cannot be excluded. However, the surficial sediment slice sample (1 cm) we collected integrates these potential seasonal variations, and current data does not indicate high annual variation. Long-term trends in Mackenzie River run-off have not changed markedly within the last decades (Durantou et al., 2012). There is little to any change in sediment properties over the last centuries (see figure from Bringué and Rochon (2012)). The content in organic and inorganic carbon as well as the grain size fraction did not change significantly over holocene period.
Overall, these findings suggest that porosity is relatively constant over decadal and century scales and can be considered as a « long-term » variable as defined in our approach.

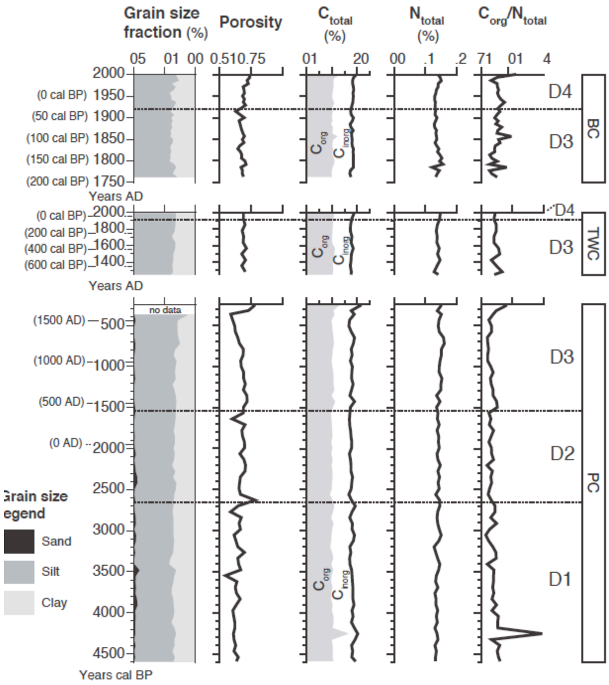


Figure:Grain size composition and geochemical parameters of Cases - station 803 on the Mackenzie slope; from Bringué and Rochon (2012).

JH:
Page 16944, lines 15/16: "*... are considered as "other" environmental factors.*" – The authors need to be consistent with their definition of "other" for $^{13}C_{org}$ and phaeopigment concentrations. I also found "*intermediate-term*" (Abstract, line 23) and "*medium*" (Table 3 incl. captions) associated to these factors.
Reply:
Agreed. We will use "other" as definition throughout the manuscript.

JH:
Results
This chapter is generally good apart from the visual presentation of the field data (see general comments on figures/data presentation above and below).

JH:
Discussion
There is no need for the headlines of the subchapters repeating each working hypothesis. They could be shortened to, for example:
4.1 Spatial variation of benthic boundary fluxes – and its causes,
4.2 Oxygen flux as proxy for benthic activity,
4.3 Combinations and variability of environmental factors controlling biogeochemical fluxes. (merged) As suggested in the general comments, restructuring of the discussion might make these chapters unnecessary, anyway.

Reply:
We thank Dr Holtvoeth for his suggestions and agree that shorter subheadings will provide more clarity. We modified them accordingly:
4.1 Spatial variation of benthic boundary fluxes: underlying factors
4.2 Oxygen flux alone is not a suitable predictor of benthic remineralisation
4.3 Distinct environmental forcings on different biogeochemical fluxes
4.4 Statistical modeling of benthic remineralisation using environmental parameters

JH:
Page 16949, line 1: "*Benthic activity is most often derived from sediment oxygen demand...*" – This translates into: the higher the oxygen demand in the sediment, the higher the benthic activity. I suppose, that's not exactly what the authors mean to say? How about: "*Benthic activity is* closely linked to bottom water oxygen concentration (...) *and assumed to decrease with increasing depth and distance from the continental source of* particles and carbon nutrients."
Reply:
We clarify: "In the literature, benthic activity is most often derived from sediment oxygen demand (Graf, 1992; Wenzhöfer and Glud, 2002; Link et al., 2011) and assumed to decrease with increasing depth and distance from the continental source of particles and carbon."

JH:
Page 16949, lines 4-7: "*... benthic remineralisation function is more complex than oxygen fluxes.*" – ?? I suppose, the authors mean that oxygen flux is not a simple function of benthic remineralisation (of organic matter).
Reply:
We clarify: We use the term "benthic remineralisation function" as the benthic ecosystem's function of multiple nutrient releases to the water column. While benthic ecosystem function is often measured as oxygen demand only, we want to emphasize that benthic ecosystem function is multivariate. In our study, benthic ecosystem function is measured as the multiple nutrient fluxes, the remineralisation function, which is more complex in its variation that simple oxygen fluxes.

JH:
Page 16950, line 1: Where is the "*Tuktoyaktut Peninsula*"? Please, add to map (Fig. 1).
Reply: *Agreed. We add 'Tuktoyaktuk Peninsula' to the map in Figure 1.*

Pages 16950/51, lines 29 and 1/2: "*... is probably explained by the presence/absence of efficient oxidative barriers at the top of the sedimentary column**,** such as oxygen and Mn-oxides (...).*" – How does oxygen work as an "*efficient oxidative barrier*"? (Delete comma after "*column*".)
Reply:
The "oxidative barrier" is the layer enriched in both Mn- and Fe-oxides and oxy-hydroxides which act as oxidants to reduced species (or metabolites) that diffused from deeper layers. A complex web of reactions and microbial pathways dominate heterotrophic dissimilatory reactions. Many of these reactions involved reactive Mn- and Fe-oxides and oxy-hydroxides that continuously precipitate / dissolve in the top few cm of sediment where oxygen penetrates. For example, it is thermodynamically possible that Mn(III, IV)-oxides support anaerobic oxidation of NH_4^+ to NO_3^- (Anschutz et al., 2005;Hulth et al., 1999;Luther and Popp, 2002, and others). Manganese oxides can also oxidize

Fe(II) and then promote the precipitation of fresh Fe(III) close to the interface (Hyacinthe et al., 2001;Myers and Nealson, 1988). The occurrence of these metal-oxides at the top of the sediment limits the exchange of reduced species and their loss to the overlying bottom-waters.

JH:
Page 16953, lines 10-13: "*Sampling sites in the Cape Bathurst Polynya and on the western Mackenzie slope were also distinct from all deeper sites with respect to silicic acid and ammonium release. Clearly, oxygen uptake alone cannot describe the spatial pattern of benthic ecosystem functioning in our region.*" – Is it possible that there is input of terrestrial biogenic silica? Or is there a difference in bottom water pH and/or salinity between the deeper and the shallower sites? These factors could affect silicate solubility (see, e.g., Loucaides et al., 2008).
Reply:
The input of terrestrial biogenic silica cannot be excluded, but this should lead to similar patterns at the Mackenzie Delta and slope sites (not only slope sites as is the case here). The same would apply for differences in bottom water salinity (data shown in Table 1; similar for all delta and the western slope site, but not the Cape Bathurst site), which does not indicate a relation with the different silicic acid release patterns in the western Mackenzie slope in Cape Bathurst site.
Data on bottom water pH is only available for a few sites (see table below, courtesy of B. Lansard). For the sites in question we did not see a trend of a relation between silicic acid fluxes and pH.

Station	***Station Depth (m)***	***Depth (m)***	***pH***
390	*58*	*28*	*7.91*
690	*54*	*36*	*7.88*
680	*121*	*100*	*7.89*
260	*59*	*41*	*8.07*
110	*407*	*150*	*7.85*
345	*586*	*225*	*7.99*
235	*661*	*655*	*8.06*
135	*227*	*224*	*7.90*

JH:
Page 16953, line 18: "*Such effects have been related to particular species*" of macrofauna? Holothurians?
Reply:
We clarify: "For example, Michaud et al. (2009) have demonstrated differences in benthic fluxes when sediments contained either the polychaete Alitta virens, the bivalves Mya arenaria or Macoma balthica, or a combination of these species, always keeping the total volume of macrofaunal organisms constant."

JH:
Page 16953, lines 25/26: "*... – whatever factors influence the spatial pattern of benthic nutrient remineralisation.*" – Now, I thought this is what this study is all about. This sounds as if the authors were not able to identify any factors. Delete or replace with something like: "... – independent of which factors are mainly controlling the spatial pattern of nutrient remineralisation."
Reply:
We thank Dr Holtvoeth for the suggestion and rephrase the sentence accordingly.

JH:
Page 16954, lines 17-20: "*The faunal composition, which has important effects on ammonium release by sediment oxygenation and bioturbation, might be one of these lacking measurements (...).*" – Further factors are likely to be sediment mineralogy and pore-water pH. Ammonium might be

adsorbed onto clay minerals, for example (Müller, 1977). Clay mineralogy and amounts certainly vary with distance from the Mackenzie delta. Since *"faunal composition"* is a bit vague, the authors might want to give an example such as holothurians selectively feeding on fresh phytodetritus or on more refractory sedimentary organic matter (FitzGeorge-Balfour et al., 2010). It might be worth pointing out that a change in benthic macrofauna as a response to modified organic matter supply represents an important feedback and would have to be considered in assessments of biogeochemical flux dynamics under future environmental change scenarios.

Reply:

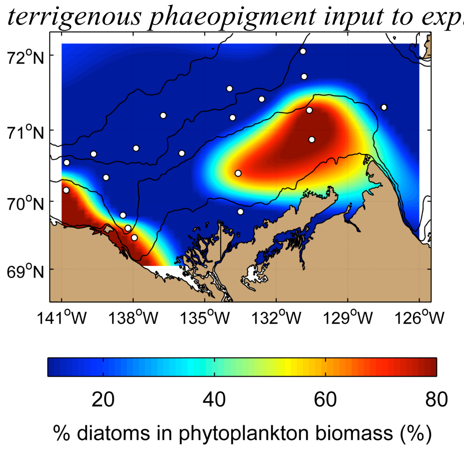
We thank Dr Holtvoeth for this valuable comment. We will integrate the possible influence of sediment mineralogy and pore-water pH, of which no data is available at this point.
The role of faunal composition will also be clarified using the example of polychaetes and bivalves mentioned above (see comment for Page 16953, line 18), since holothurians were not present in our samples.

JH:

Page 16955, lines 5-20: This section needs clearing up. Perhaps remind the reader first that Chl *a* concentrations *do* correlate with silicic acid whereas phaeopigment concentrations, other than expected, *do not*. Then discuss the reasons for the correlation or missing correlation, respectively.
"*Possibly, the input of terrigeneous phaeopigment-loaded material from the Mackenzie is higher towards the western part of the Mackenzie plume (...).* – Up to this point, I was not aware of the fact that phaeopigments might have a terrestrial source! Since potential decoupling of phaeopigment and marine-derived Chl *a* concentrations is quite an important issue, this should be introduced early on (in the introduction).

Reply:

We agree that our interpretation was a bit far-fetched. Following a discussion with some colleagues of the Malina program (e.g. Coupel et al., in prep.), we realized in fact that a recent diatom production had occurred in stations affected by high Chl a content (Stations 140, 260, 680, 690; no data for 390; see figure below) apparently due to a terrain-following nitrate flux from deeper waters (i.e. isopycnal slanting over the shelf). At other stations where phaeopigments dominated sediment pigment concentration, diatom biomass in the water column was very low (even absent) and primary production was low as well. This indicates, that detrital material at the seafloor may be composed of diatom-poor, silicate-poor material. In brief, we will remind the reader that Chl a concentrations do correlate with silicic acid fluxes whereas phaeopigment concentrations do not. We will then cite the work of our Malina colleagues that found high biomass of 'fresh' silica-rich diatoms at stations where we found high Chl-a at the seafloor. In this way, we refrain from speculating on any terrigenous phaeopigment input to explain the discrepancy between Chl a and phaeopigments.



Forest, A., Coupel, P., et al., in prep.

JH:

"Phaeopigment-enriched sediments could then represent diatom-poor organic matter input, and would therefore not lead to increased silicic acid release." Sediments cannot (actively) "*lead to increased [...] release*" and organic matter input is not diatom-poor. Suggestion: "Thus, sediments

may contain increased concentrations of partly terrigenous phaeopigments but low concentrations of diatom-derived silicic acid and Chl *a*."

Reply:

Agreed. We rephrase the sentence as following: "Thus, sediments containing increased concentrations of phaeopigments were the consequence of both past and present diatom-poor primary production at these locations, likely affected by low pelagic nitrate availability."

JH:

Page 16955, lines 21-24: "*In summary, ...*" – This paragraph can be removed from here. It does not exactly summarise the previous paragraphs (no mention of NO_x or phosphate, for example) but contains a conclusion which, furthermore, overlaps with the content of the following chapter.

Reply:

We agree that this paragraph is conclusive of this section. We think it can be helpful to distinguish the different intentions in testing hypotheses 3 and 4. However, we rephrase the paragraph to better emphasize the result that the single fluxes are not controlled by the same combination of environmental conditions.

JH:

Figures

Figure 1: It would be good to have the surface circulation (major currents) in the map so that one can see how terrestrial organic matter supplied by the Mackenzie River is potentially shifted around. Perhaps, highlighting the area where predominantly terrigenous material is deposited (including sites 680, 690 and 390) might be a good option, as well.

Reply:

Yes, we could add a rough contour of the Mackenzie river plume as based on Matsuoka et al. (2013) (see his map using CDOM). But this would yield an uncorrect picture of particulate matter transport from the River to our study sites. Most sediments sink directly (97%) within the 10-20 m isobath (Doxaran et al., 2012;O'Brien et al., 2006). We therefore think, that emphasizing terrigenous input in our study region map could lead the reader to misunderstandings of what we believe to be important environmental forcings.

Figure 2: These plots are very small; unless this figure covers nearly the full width of a printed page it will be really tough to read. The figure is supposed to illustrate where in the investigated area biogeochemical fluxes are positive (from water column to sediment) or negative. However, the plots are not easy to take in. Having water depth on a horizontal axis is quite unusual, for example, and the site numbers labelling each data triplet are missing. Although the range of the individual data sets at each site would drop out, I would think that contour plots overlying the map of the area would serve the purpose better.

Reply:

We agree that the data presentation needs to be improved. Fig. 2 is intended for a whole-page display (which we found out to be impossible with BGD publishing). Adding site numbers to Fig. 2 will help with its interpretation as well.

We acknowledge that contour plots are particularly suited to present spatially distributed data. However, they are only meaningful if minimum requirements in data quantity, distribution and resolution are met to allow for sound between-data point interpolations. As this is not the case for our study, we refrained from presenting contour plots in our manuscript.

JH:

Finally:

During some literature/web research for this review, I came across a paper by Scudlark and Church (1989) with a remarkably similar scientific approach albeit carried out in a salt marsh. Nevertheless, the authors might be interested to have a look at this paper.

Reply:

We thank Dr Holtvoeth for this information. The paper provides a valuable example that flux *measurements across the sediment-water interface cannot be replaced by fluxes calculated from pore-water profiles due to the important biogenic processes in the sediment surface. Although Scudlark and Church (1989) do not discuss how environmental conditions may influence the ensemble of all fluxes, they provide useful interpretations on the geochemical processes underlying net single fluxes.*

JH:
Technical comments:

Reply:
We thank Dr Holtvoeth for the technical comments and the thorough review. We include all corrections of technical comments mentioned here in the revised version of the manuscript.

Page 16935, line 15: "*And (4)* ***A*** *combination...*" – Should this text passage be kept delete "And" or replace capital "A" with small "a".
Page 16936, line 20: insert **"et al."** after "*Ebenhöh*"
Page 16936, line 24: replace "*But* ..." with "However, ...", delete comma after "increasing"
Page 16940, line 2: "... *sediment pigment concentration* ***and*** *water content and sediment solid phase composition* ..." **-** Replace first "and" with comma.
Page 16940, lines 19-20: "*Porosity was determined by comparison of weight of wet and dried sediment. Porosity was calculated using a dry sediment density of 2.65 g cm*$^{-2}$ *(Berner, 1980)."* **-** Density is given in g cm^{-3}!
Page 16940, line 23: "*For stable isotope composition analysis, ground****ed*** *sediments were acidified ..."* **-** Replace "*grounded*" with "ground".
Page 16940, line 24: "... *dilute HCl (1N) solution ..."* Replace "*(1N)*" with "(1M)" for 'molar' (**also on p. 16941, lines 7, 9, 13**); 'N' for 'normal' is a bit old-fashioned (and only valid for HCl). What's the actual concentration of the diluted solution, then?
Page 16940, line 25: Replace "*rinced*" with "rinsed".
Page 16941, line 1: Replace "*Spectrometry*" with "Spectrometer".
Page 16941, line 3: "*... with respect to* **the** *V-PDB* **standard** *for carbon.*"
Page 16941, lines 3/4: "*The analytical precision error ...*" – Delete "*precision*".
Page 16943, line 25: "*Gilbert et al.* ***2005***" – not in reference list!
Page 16943, lines 13/14: "*This is likely due to the seasonal and spatial dynamic of primary production and carbon fluxes ...*" – Delete "*likely*", replace "*dynamic*" with "dynamic**s**".
Page 16944, lines 3/4: "*Over a period of several decades, the upward migration of* **the** *sedimentary redox boundary can generate* **a** *surficial peak of metal-oxides ...*"
Page 16944, lines 19-23: "*Predicting variables allowed in the model were:* **s***ediment surface Chl a concentration, sediment surface phaeopigment concentration, sediment surface porosity, sediment surface manganese-oxides concentration, sediment surface ironoxides concentration, sediment surface* $^{13}C_{org}$*, bottom water oxygen concentration and vertical flux of POC.*" – How about: "Predicting variables allowed in the model were: concentrations of Chl *a*, phaeopigment, manganese oxide and iron oxide in the sediment surface, sediment surface porosity and $^{13}C_{org}$ as well as bottom water oxygen concentration and vertical flux of POC." That saves a line! Save another one **similarly on pages 16945/6, lines 29 and 1-4**, respectively.
Page 16949, line 4: replace "*But* ..." with "However, ...".
Page 16949, line 15: Insert comma after "*2009*".
Page 16949, lines 21-24: "*... the influence of the Mackenzie Delta increases interannual variability of benthic oxygen uptake at its plume*" – A delta does not have a plume, a river has. Suggestion: "This indicates increased interannual variability of benthic oxygen uptake in the realm of the Mackenzie River plume whereas the spatial distribution of benthic oxygen uptake as, e.g., in the Cape Bathurst Polynya is likely controlled by changes in marine primary productivity."
Page 16950, lines 4-7: "*Second, primary production in the Cape Bathurst Polynya area has a higher diatom contribution (Ardyna et al., 2011), which allows for an* **leading to** *increased fresh silicic shell export (Simpson et al., 2008). Indeed* **In fact***, Sampei et al. (2011) ...*" – Delete *"fresh"*. Silicate doesn't go off easily, anyway.

Page 16950, line 13: replace "(> *1 cm)*" with "(< 1 cm)".
Page 16950, lines 23/24: "*... more available fresh* **organic** *matter*".
Page 16950, line 25: "*The generally low* **flux of** *nitrite flux reflects its role ...*"
Page 16951, line 14: "*... can be explained by either a lost* **loss** *of the sediment capacity* **of the sediment** *to adsorb remobilised phosphate or ...*"
Page 16951, line 18: "*Sulack*" (text) or "*Sulaka*" (references)?
Page 16951, line 20: Replace "*sote*" with "site"; insert "organic" after "*fresh*".
Page 16952, line 3: Insert "our" before "*experiments*".
Page 16952, line 9: Swap "*benthic*" and "*polar*".
Page 16952, line 11: Delete "*a*" after "*accompanied by*".
Page 16952, lines 12-15: "*When considering all fluxes synchronously, site 390 can be well separated from 690, these two are different from the lower Mackenzie Shelf (site 260 and 680), which finally can be separated from the Cape Bathurst Polynya site (110 and 140) and the deeper Beaufort slope sites (235 and 345) in their remineralisation functioning (see also Fig. 3).*" – This definitely needs some serious rephrasing!
Page 16952, lines 16-18: "*This spatial pattern has also been found using* **is confirmed by** *lipid biomarker analyses conducted on sediment samples collected at some of the* **from some of our** *sites we studied (Rontani et al., 2012; Tolosa et al., 2012).*"
Page 16952, lines 12-15: "*Although sediment oxygen consumption is widely used to describe****d*** **as a proxy for** *benthic remineralisation function (...) our results confirm this hypothesis* and show that other important fluxes resulting from differences in benthic remineralisation including six major fluxes are not dominated by* **strictly related to** (?) **the** *oxygen flux.*" (*Note: The headline is not the first sentence of the text.)
Page 16953, lines 16-18: "*Recent experimental studies have shown that benthic fluxes other than oxygen, e.g. silicic acid or ammonium, respond to treatment* **change as a result** *of different organic matter input (...).*" – Fluxes do not (actively) respond. They are controlled by, result from...
Page 16954, line 28: "*... indicates* ***a*** *degradation of organic matter ...*" – Delete "*a*".
Page 16956, line 4: "*The similarity of the dbRDA plot and the PCA plot show****s,*** *that the environmental variables ...*" – Insert "s", delete comma.
Page 16956, lines 15/6: "*Assuming the importance of biological activity for phosphate (...), nitrogen derivates (...) and silicic acid (...) release, high Chl a concentrations at the seafloor not only provides the fresh matter for bacterial degradation,* ***but*** *it also stimulates ...*" – High chlorophyll concentrations do not provide anything, fresh marine organic matter (phytodetritus) high in chlorophyll does (provide food/energy for bacteria and macrofauna). Rephrase, delete "*but*".
Page 16956, line 23: "*...will show a distinct benthic ecosystem functioning.*" – Replace "*distinct*" with "distinctly different".
Page 16956, lines 27/28: Suggestion: "*..., and may therefore describe a***n underlying low-frequency variation**, *on top of which short-term environmental factors further* modify *benthic fluxes*."
Page 16957, line 1: "*About 40% of the total variation in benthic remineralisation* ***function*** *could not be explained ...*" – Delete "*function*".
Page 16959 (References), lines 6-13: correct order: shift Anschutz et al. (2000) to the top.
Page 16964 (References), lines 17-22: Wrong alphabetical order: swap Morata et al. (2008) and Michaud et al. (2009).
Page 16971, Table 3, caption, line 5: "*...,* $_{13}C$ *– isotopic* signature of **organic** *carbon; ...*"
Page 16972, Table 4 and caption, line 5: Replace "d13C" with "$_{13}$C ".
Page 16974, Figure 2, caption, line 6: "*(values above the plane represent release, below the plane uptake)*" can be deleted (repetition).

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