Reply to anonymous referee #2

Referee: This manuscript presents a valuable data set encompassing a range of complex processes and interactions. The data are unique in their breadth and should improve our understanding of biogeochemical cycling in an Arctic marine shelf environment that is likely to experience (or has already experienced) significant changes due to the effects of climate change. The stated goals of the study were (1) to understand "how net community production (NCP) responds to changes and modulates air-sea CO2 fluxes" and (2) identify "indices of ecosystem response to environmental changes." The authors conclude (1) that climate change is impacting the biological gradient across Arctic shelf breaks, (2) that the Mackenzie shelf acts as a CO2 sink despite net heterotrophy, and (3) that CO2 outgassing due to upwelling at the shelf break may be balanced by enhanced primary productivity due to concurrent nutrient to the surface mixed layer. While the underlying data are intriguing and provide a unique synoptic view of the Beaufort margin from a variety of biogeochemical angles, I find that the authors' conclusions are not entirely supported by the data as presented in the manuscript. It seems as if there was such a quantity and diversity of data available that assimilation of all components into a coherent picture became difficult and the strength of the authors' findings suffered as a result. My main difficulty with the manuscript stems from the lack of a clear temporal component to the sampling strategy and the fact that the conclusions rely heavily on calculations, estimates, and correlations that were not treated to a sensitivity analysis. The expense involved in a study with such large spatial coverage and range of samples understandably precludes a long-term time-series component. Yet, I am uncomfortable with some of the interpretations that require large assumptions about the past or future state of the ecosystem. Because of the complexity and scale of the system being analyzed, it seems natural that estimates would be required to successfully build a model of NCP and CO2 flux, yet I was left wondering how the main results would change if estimates were varied within expected ranges. This issue was especially important given the complexity of the system and the variety of additional processes and timescales that could not be measured directly. I would suggest that the authors conduct a sensitivity analysis and present the results in the discussion section in order to allow the reader to judge the robustness of their calculations, assumptions, and conclusions. In addition, I would recommend backing away from some of the more speculative conclusions, especially those with an unknown temporal component (e.g., the effect of prior phytoplankton blooms and sea-ice melting on the disconnect between CO2 influx and net heterotrophy) or those regarding the future state of the system under climate change. In order to evaluate potential changes to the Beaufort system in the future, I would also recommend including as much data as possible in an appendix or supplement so that effective comparisons might be made. As a general note, I found the syntax and sentence structure to be awkward in several places as noted below in my specific comments. Overall I think that this study is an extremely valuable baseline for evaluating future changes, but I would not support publication until the major issues raised above (and in the specific comments below) were addressed.

Reply: We would like to thank Referee #2 for his/her valuable and relevant comments. We are greatly indebted for the extensive review he/she has conducted. All comments and issues were addressed as suggested by the referee. In particular, the manuscript has been revised to include:

 A "sensitivity analysis" of the interplay between NCP and CO2 fluxes as based on error propagation of the various estimates, where available and appropriate. This analysis is presented in the new Appendix A to leave the main text concise and focused on key results. It should be noted that our work represents primarily a synthesis of field measurements and related properties as based on empirical equations or statistical models. In this context, we cannot conduct a sensitivity analysis in the same manner as a numerical model would be able to do, for example (i.e. to change one parameter after the other). The goal of our study is to provide a synoptic snapshot of the ecosystem metabolism and to stay close (as much as possible) to the actual dataset collected during Malina. This results in a sensitivity analysis that can be defined as an error estimate on the final results. In addition, we should mention that all the different budgets presented in this work were already showing the appropriate errors and uncertainties (see e.g. Budgets in Figures 5 and 9).

- 2) Revision of the speculative sentences addressing the temporal component or a perspective on future status of the Beaufort ecosystem. We understand that some sentences were going a bit far and we have revised all of them as suggested. However, some sentences were constructed/formulated on the basis of real results (e.g. temporal component of the bloom was estimated through remote sensing prior to the campaign; inference on the disconnect between CO2 fluxes and respiration is the result of our comprehension of the ecosystem as based on multiple previous campaigns in the region such as CASES, CFL, and ArcticNet; and our discussion of the evolution of the Beaufort Sea ecosystem with respect to climate change makes use of our results, but also of results coming from other papers published as part of the Malina special issue in Biogeosciences and other journals).
- 3) A presentation of the main data used in the present study as a supplement to the manuscript. It would not have been appropriate to include such a long Table as an appendix, but we do it as a supplementary document to provide a legacy for future studies. Here, we present the core data at the origin of the NCP and CO2 flux estimates because all the Malina data that were used in our work are in fact already available in a public database that can be found at the following address (see left column where the whole database can be downloaded): <a href="http://www.obs-

vlfr.fr/proof/php/malina/x_datalist_1.php?xxop=malina&xxcamp=malina.

4) Revision of all awkward sentences as suggested. Most sentences looked awkward because too long. We shortened and revised all sentences identified by the referee (and more).

Referee: Specific Comments:

Title (and throughout): suggest adding the word "the" before "Beaufort Sea"

Reply: Corrected throughout the manuscript.

Referee: P15643 L3: What does a "more dynamic atmosphere" mean?

Reply: We changed the sentence to "more energetic atmospheric and ocean forcings", which is later described in the text as intensified winds (storms) and increased current speeds.

Referee: L12-14: Reads awkwardly.

Reply: Sentence revised, broken up into two smaller sentences.

Referee: L18: Can you say that -2.0 \pm 3.3 is significantly different that zero? Is the uncertainty 1 σ ?

Reply: This range reflects the spatial variability across the study area and not the error on each discrete calculation per se. The mean value presented in the abstract is significantly different than

zero (it is the mean) and the associated high standard deviation simply represents the large spatial variability. The sentence has been clarified accordingly.

Referee: L22: Mismatch in units. Can you use the same throughout (mmol or mg)?

Reply: This is a debatable issue. After some discussion with the co-authors, we decided to keep CO2 flux units in mmol C, whereas all the other measurements are presented as mg C. This is just to keep coherency with existing studies that described CO2 fluxes in the region (e.g. Else et al. 2013) and primary production/respiration (e.g. Forest et al., 2011). Results are then more directly comparable without the need to over-discuss or document each number. We assumed that the typical reader of such a manuscript could easily convert mmol C into mg C.

Referee: L23-25: Reads awkwardly ("...cumulated to a..." and "...twice higher...")

Reply: Sentence revised, broken up into two small sentences.

Referee: P15644 L3-4: Use mg as unit?

Reply: It is more convenient to use g C instead of mg C for such large numbers in biomass.

Referee: L8: Conclusion (1) would seem to be difficult to support with data from this synoptic study.

Reply: Revised to be more oriented onto our local study. This conclusion is reached following our results, but also the results of other studies from the area.

Referee: L9: Conclusion (2) contradicts the finding of net heterotrophy in the Beaufort Sea.

Reply: We revised the sentence to soften this conclusion. It should be noted that this conclusion is specific to the surface layer, is apparently in contradiction with other findings, but is actually a key element of our study. We found that some areas characterized by net heterotrophy were in fact also characterized by a net CO2 sink. This seems contradictory, but raises the question on which surface processes are driving the CO2 sink. Net heterotrophy is a result of all the processes occurring through the water column; whereas CO2 fluxes reflect the processes occurring near the surface. This suggests that respiration is higher at depth than near the surface, a mechanism than can contribute to the biological pumping of CO2. Consequently, PP in the surface might exceed respiration of terrigenous and marine organic matter.

Referee: L9-15: It's unclear how these conclusions will be supported by a synoptic sampling scheme.

Reply: We think that a synoptic sampling scheme is actually the only way to reach the conclusions listed here. When looking around, many studies use only a few measurements to speculate on climate change and ecosystem function (...). Here, we use all the data available from a multi-disciplinary campaign to develop a strong argumentation, which is tied to many complementary sampling conducted within a synoptic scheme. Given this context, we are able to propose some conclusions, which of course need to further investigated and monitored (as mentioned).

Referee: P15646 L24: Change "has been" to "was"?

Reply: Done.

Referee: L29: Reads awkwardly ("...form the crucible for concluding on the potential...").

Reply: Revised to a simpler form.

Referee: P15647 L2: Change "to which extent..." to "to what extent...is net ecosystem metabolism coupled to..."?

Reply: Done.

Referee: L2-5: The abstracts suggests the following answers: (1) uncoupled (2) unknown (3) unknown

Reply: This is partly not accurate. (1) yes, uncoupled; (2) primary production was twice higher than organic carbon river inputs, nutrients brought by upwelling compensated CO2 outgassing, low productivity in areas covered by persistent ice cover; (3) high benthic biomass on the shelf because of higher vertical C fluxes, high zooplankton biomass in regions affected by upwelling and higher PP, respiration mainly at depth, not impacting CO2 fluxes, but rather exported C to depth.

Referee: L8: Rectangular shelf? L9 and L10: Change "on" to "to"?

Reply: Done.

Referee: P15648 L1-4: This sentence reads awkwardly, and I'm confused by the standard deviation at L4.

Reply: Revised, simply mention that discharge was near normal in August.

Referee: P15649 L8-9: Reads awkwardly.

Reply: Revised, broken up into 2 smaller sentences.

Referee: L29: Change "on" to "in"?

Reply: Done.

Referee: P15650 L4: Would it be necessary to include the relevant contents of Appendix B in Forest et al (2011) in an Appendix to this manuscript? The equations linking pathways included in the model seem to be key elements of this paper.

Reply: Yes, we included the equations.

Referee: P15651 L1-3: How does the algorithm correct for CDOM and non-algal particles? Later in the manuscript it is claimed that CDOM results in a 2.3x - 2.7x overestimate of PP due to CDOM. How does this fit with the CDOM correction mentioned here? What are the expected or calculated uncertainties in these PP estimates based on MODIS?

Reply: The algorithm is fully detailed in Belanger et al. (2013) as mentioned in the text. This algorithm takes into account CDOM and non-algal particles and it would not be appropriate to insert in our manuscript the full detailed methodology related to their approach. In brief, we used GSM01 algorithm, which was found to perform better than standard empirical algorithms (e.g. NASA's OC4v6) in Arctic waters dominated by CDOM. Still, CDOM remains an issue and we do not know exactly the uncertainties that arise from this issue (more investigations are needed, see Ben Mustapha et al. 2013). But our study provides an estimate of the error that can be associated with respect to the magnitude. The 2.3x-2.7x can actually be seen as the uncertainty range deduced and expected from our results.

Referee: L6: Why was 30 mg m-3 chosen as a threshold?

Reply: Because this is the threshold that Ardyna et al. 2013 found to be a good threshold, as mentioned in the text. This study investigated satellite chl-a at a Pan-Arctic scale.

Referee: L12: UNESCO in capital letters?

Reply: Done.

Referee: L21: Change "analyses" to "analyzes"?

Reply: Done.

Referee: P15652 L11: What thermodynamic effects were corrected for, and how were corrections made? L13: How were the underway and bottle data "merged"?

Reply: The rationale behind and the correction for thermal difference between the underway system and in situ ocean temperature are fully detailed in Else et al. 2013a as mentioned in the text. Underway data and bottle data were merged by assembling a unique database. The sentence has been revised.

Referee: L15: Is "virial" correct?

Reply: Yes, this is correct.

Referee: Eqn (1): The dashes and minus signs result in confusing syntax. Suggest using dashes only for minus signs.

Reply: This has been changed. We use capitalized letters instead of dashes.

Referee: P15653 Eqn (2): Why was this relationship chosen? How sensitive are the results to the choice of a different parameterization?

Reply: This relationship has been chosen to keep coherency with the same method and same equation of previous CO2 flux studies in the region (e.g. Mucci et al. 2010; Else et al. 2012). The sensitivity of the relationship has been fully discussed in Mucci et al. (2010) as now mentioned in the text. Mucci et al (2010) wrote that: *"The impact of different gas exchange formulations and wind speed on global air-sea CO2 fluxes has been reviewed by Wanninkhof (2007). The review concludes that*

the global fluxes are sensitive to estimates of gas transfer rate and the parameterization of gas transfer with wind. Parameterizations of gas exchange with wind differ in functional form and magnitude but the difference between the most used quadratic relationships is about 15%. Based on current estimates of uncertainty of the air-sear fCO2 differences, the winds, and the gas exchange-wind speed parameterization, each parameter contributes similarly to the overall uncertainty in the flux that is estimated at 25%. The Sweeney et al. (2007) parameterization used in our study is based on a global inventory of bomb-produced radiocarbon in the oceans and, importantly, compares exceptionally well with direct measurements of ks at low to moderate (McGillis et al., 2001) as well as at high wind speeds (Ho et al., 2006). We undertook separate flux calculations using other commonly used transfer velocity formulations (including Wanninkhof (1992), Liss and Merlivat (1986), and Wanninkhof and McGillis (1999)) to facilitate comparisons with other studies. However, the uncertainty associated with the application of the bulk parameterization has not been defined in marginal ice environments and this remains a subject of inquiry." (Mucci et al. 2010, doi:10.1029/2009JC005330)

Referee: L8-9: Why were these data removed?

Reply: This is a standard quality control process in order to remove wind data that could be influenced by the ship itself.

Referee: L19-23: These seem like large assumptions. How sensitive are results to these assumptions?

Reply: This assumption is based on the exhaustive sediment transport budget constructed by O'Brien et al. 2006. This is simply the unique budget that exists for the region and it does not provide any insights on the uncertainty (see p. 25 in O'Brien et al. 2006). In any case, our own results are not sensitive to this budget because it has been used only to discriminate the regional differences in river discharge variability (east vs. west of the delta). The river input was actually calculated from measurements by Water Survey of Canada. Together, this enabled us to provide a non-steady state carbon model ('snapshot' of the region during Malina, no mass balance).

Referee: P15654 L1: Is it possible to include actual measurements of DOC and other similar values in an Appendix?

Reply: All the discrete DOC measurements (as well as all the other data of this study) are now available in the public domain at this address. See the left column where the whole database can be downloaded:

http://www.obs-vlfr.fr/proof/php/malina/x_datalist_1.php?xxop=malina&xxcamp=malina

Referee: L3-6: Why were two methods used? Did the methods give comparable results? How did they compare? Can these data be included in an Appendix? What volumes of water were passed through the POC filters? Were POC values corrected for blanks?

Reply: In our study, we made use of two POC datasets collected by 2 teams during Malina. This was just to get as much as possible data to show best estimates of standing stocks of POC in our non-steady state carbon budget. The two methods provided comparable, with some variability (inevitable), which was taken into account for error propagation. All the data are available for download at the address above. And yes, POC values were all corrected for blanks.

Referee: L17: Does the persulfate wet-oxidation method miss some portion of occluded particulate OC? Would this lead to an underestimate of calculated DOC?

Reply: We are aware of the potential underestimations of DOC by the persulfate-oxidation method (WCO). This issue has been deeply debated and information on the controversy can be found in Raimbault et al. (1999, http://dx.doi.org/10.1016/S0304-4203(99)00038-9), a paper published by one of the co-author of this study. In brief: "the DOC/DON workshop of Seattle (Williams, 1993) showed that differences between methods were as not as large as previously suggested but has given insufficient information to explain it (...). Several recent papers have confirmed the good efficiency of the WCO for the digestion of organic carbon dissolved in sea-water". This Raimbault et al' study concluded that the WCO method is highly suitable for routine analysis and especially appropriate for ship board work.

Referee: L21: I don't understand what is meant by "the calibration of DOC."

Reply: Changed for "to verify the accuracy of DOC measurements".

Referee: L24: Were the sediment trap stations representative of the region as a whole? Where were these stations?

Reply: Yes, they were representative of the region, spanning from west to east along the shelfbreak. A full paper just on sediment trap and vertical fluxes is available here as mentioned in our manuscript (Forest et al. 2013, doi:10.5194/bg-10-2833-2013).

Referee: L25: If 80 μ m particles were the smallest recorded, how are these measurements connected to POC values (> 0.7 μ m) or actual sinking fluxes that include particles less than 80 μ m?

Reply: The 80μ m threshold is linked to the video profiler capacity. There is no link between the potentially small particles in the trap (0.7-80 µm) and the 80μ m threshold. The calibration of the algorithm is based on the slope of the particle size distribution, and not on distinct measurements of particle abundance in discrete size classes (see Forest et al., 2013 and references therein).

Referee: P15655 L10: Recommend providing a quantitative measure of "reproducibility."

Reply: Done. It is 0.1%.

Referee: L13-27: What are the uncertainties in these bottle-based estimates? How would they bias the calculated GPP values?

Reply: Carbon fixation assays using bottle incubations were performed with on deck incubators, but in situ incubations were also conducted to provide a measure of the uncertainty (see figure at the right). This process revealed that a very good relationship (m=1.02, r2=0.77) prevailed between the two method and the residuals can be used to propagate the error in our computation of GPP.



Referee: P15656 L4-6: This seems to be an important step in your process, so I think it needs clarification. What is a net particle rate and where are these numbers from? I would recommend justifying the assumption that they represent 10 and 15% of GPP. How does light penetration fit into estimates of GPP?

Reply: These sentences were revised to clarify what were the rates and from where the numbers came from (primarily a reference to Raimbault and Garcia 2008). Within our study, some assumptions needed to be made on the most comprehensible data available for the region (so our best "educated guesses"). The assumption regarding respiration and exudation by phytoplankton was not a trivial one. Such processes depend on community composition, the physiology of the cells, their activity, etc. We chose the mean rates found by Forest et al. (2011) in their study of biogenic carbon flows in the same region as the present study, which were also coherent with previous studies in the Arctic Ocean (see references therein). Of course, there are some uncertainties about these percentages, but in the context of a late summer situation and low productivity period, these uncertainties have never been really assessed. This would require specific fieldwork to investigate phytoplankton exudation and respiration in our study area, something that remains to be done. Light does not have anything to do with these assumptions.

Referee: L21: Does the cellular biovolume estimate (0.040 μ m3) match previous observations in the Beaufort?

Reply: These cellular biovolume estimates for bacteria are the first one for the region. See Ortega-Returta et al. 2012 (doi:10.5194/bg-9-3679-2012) for further details.

Referee: P15657 L2: Does "surface or subsurface" correspond to the water column or the sediments? L7: What is chl *a* correlated to in the given equation (I assume it is BGE)?

Reply: Surface/subsurface refers to the water column, as mentioned several times in the manuscript. And yes, it is to BGE, we corrected the sentence.

Referee: L13: Reads awkwardly.

Reply: Sentence was revised and shortened.

Referee: L21: Are the biomass values wet weight or dry weight? Can these data be presented in an Appendix?

Reply: They are in carbon. We will insert the biomass data in our supplemental material.

Referee: L24: What is the value of the mean ratio?

Reply: It is simply the proportion. The sentence was revised.

Referee: P15659 L8-19: Good section.

Reply: Thanks.

Referee: P15660 L25: Reads awkwardly ("...consequential of the disruption...").

Reply: Sentence revised (as a result of).

Referee: P15661 L3: Does this mean that MODIS-based PP estimates are underestimates? How does this impact GPP and CO2 flux interpretations made later in the manuscript?

Reply: Yes and no, this is complicated. We know that MODIS-based PP will be overestimated because of CDOM contamination; but it can be also underestimated because of the subsurface chlorophyll maximum (if it is a subsurface productivity maximum) or if PP occurs under the ice for which the satellite is blind. As mentioned by Referee #1, we decided in fact to mask the area where sea ice occurred. This sentence has been erased.

Referee: P15662 L13-15: The CO2 sink doesn't appear to be significantly different than zero, yet this uncertainty is not adequately addressed in the discussion sections.

Reply: As mentioned previously, this mean value of -2.2 is different than zero. It is the mean for the entire region and the associated standard deviation (3.3) reflects the large spatial variability (and not the error associated with value per se). The regional variability is fully discussed. This strong spatial variability for the Beaufort Sea also is something that previous studies (e.g. Mucci et al., 2010). The error range on the CO2 flux value itself is more on the side of $\pm 65\%$ (Mucci et al., 2010; Else et al., 2012; Else et al., 2013b), which provides an error range of ± 1.4 mmol C m-2 d-1 for the mean -2.2 value. This means that the area is "at worst" a very weak sink (-0.8 mmol C m-2 d-1), but still a sink (as we mention in the revised manuscript).

Referee: P15663 L3-4: The authors' data can't support this statement, which suggests that the DOC is of similar chemical character across the study area, without more information about spectral or compound-specific characteristics of DOC.

Reply: The sentence has been revised. It was not our intention to propose that DOC is chemically uniform across the region. It was mainly an affirmation to underscore that background DOC concentrations are relatively high and do not vary much regionally.

Referee: L16: Missing a word after "magnitude"?

Reply: Thanks, the word "greater" was missing.

Referee: L13-15: It is unclear how the data presented lead to this statement. More detail, reasoning, and clarification would be needed.

Reply: This sentence is the result of our observations: vertical POC fluxes are high near the coast despite low POC inventories (overall). We made the sentence clearer to illustrate that the residence time of POC near the coast is probably very short or that the area might be affected by resuspension.

Referee: L18-22: How does the nitrogen data compare to previous studies in the Beaufort Sea? Can these data be included in an Appendix?

Reply: For an exhaustive discussion on nitrogen cycling during Malina and how it compares with previous studies, we refer the reader to Tremblay et al. 2013 submitted as part of the Malina special issue. All the data are available in the public domain at the address mentioned above.

Referee: L27: Is it possible to show the sample locations in the accompanying figure (6)?

Reply: Since this figure is already complex (3-D with different data) it would make the figure much more difficult to understand if we add the sample locations as well. The sampling locations correspond in fact to the temperature-salinity sampling locations as shown in figure 4a-b.

Referee: P15664 L1: It is unclear how nitrate data alone provide information about cross-shelf flow?

Reply: The sentence has been revised (structure instead of flow).

Referee: L2: What does "oblique expansion" mean?

Reply: This is just because the nitrate concentrations were following the isopycnals. Sentence corrected.

Referee: L7: Are these GPP rates from the bottle incubation experiments or the MODIS data?

Reply: These are from bottle incubations. Sentence revised.

Referee: L19: How does the spring freshet, which delivers most of the flux of OC to the study area, factor into this assumption? Is the riverine DOC delivered from July 20 to August 24 different in character (and concentration) from the riverine DOC in the basin (delivered weeks earlier)?

Reply: Our study does not aim at describing the full annual cycle in POC and DOC inputs into the study area. This sentence is specific to our study period. Our synthesis is based on the data we collected at sea during the Malina campaign and we use only extra material to document the relevant processes related to plankton metabolism and CO2 fluxes. The spring freshet of the Mackenzie River is expected to deliver a large quantity of OC to the region – this is true. But we were not on the field prior to late July, so we cannot comment on the quality of the OC during this period. We simply don't have the necessary data, except for remote sensing (that we indeed use to document and discuss PP prior and during the campaign). Nevertheless, we can also discuss DOC concentrations in the basin because we measured them. In the basin, we measured similar quantities of DOC everywhere and the Mackenzie River input represents most likely a very small fraction of the overall DOC concentrations that can be found offshore (when integrated over the whole water column). This is because ambient DOC concentrations (background) are high and that riverine DOC is confined to the very near-surface layer. To discriminate exactly the contribution of the Mackenzie River DOC to other types of offshore DOC would require a full further study devoted only to this issue (for example by using lignin or amino acids).

Referee: P15666 L18-19: What data are these estimates based on and how are the estimates made? L10-14 and L23-28: This text may fit better in the discussion section.

Reply: These estimates are based on data from Link et al. (2013) as mentioned in section 2.6. Our goal here was simply to remind the reader about how we calculated the different properties related to the benthos. This section has been revised and shortened.

Referee: P15668 L2: Change "To which..." to "To what..."?

Reply: Done.

Referee: L15-21: All estimates suggest a sink of CO2. How do these estimates square with the NCP estimates presented in Fig 10a and on P15667? Are they statistically different from one another?

Reply: As mentioned previously, an interesting result of our study was to see that NCP rates were disconnected from CO2 fluxes. They do not square at all together, the comparison between figure 4f and figure 10a is very convincing, which lead to our construction of Figure 12. All the different estimates of CO2 fluxes (except Shadwick et al. 2008) are statistically similar. This reveals that our study is consistent with previous studies. The results of Shadwick are different because of the distinct factors that characterized their study as explained in the text (not the same exact region/timing, undersampling).

Referee: P15670 L9: Missing words?

Reply: Sentence corrected. More pronouns added.

Referee: L14-18: Could these patterns be related to issues with estimates? Perhaps a sensitivity analysis could clarify the potential for biases inherent in making estimates.

Reply: Yes, this "sensitive analysis" (or rather error propagation estimate) is presented in the new Appendix A. In brief, the imbalance between CO2 fluxes and NCP remains (and even exacerbated in some situations) when using different scenarios reflecting the range of possible values.

Referee: L20-24: This may be true, but I don't see strong evidence for this claim in the data.

Reply: This is because this sentence is directly related to the study of Else et al. 2013a who sampled the study region a few months after Malina. We need to take into account the full context to understand this sentence. We adjusted the sentence accordingly.

Referee: P15671 L1-28: Many of the claims made in this section are speculative. Each point requires much more in-depth development of the line of thinking and/or quantitative evidence. If this were not possible, I would recommend paring back the definitive claims made in this section.

Reply: This section has been thoroughly revised, but we kept the most robust facts and suggestions such as: (1) it is known that the Canada Basin is oligotrophic; (2) the region close to Cape Bathurst is very well known for upwelling and periodic outgassing (e.g. Mucci et al., 2010); (2) the same region is also known for enhanced diatom productivity as a result of upwpelling of nutrient-rich deep waters (e.g. Tremblay et al., 2011; present study); (3) the balance between the two processes will dictate if the region acts as a sink or source of CO2.

Referee: P15673 L1-3: What is the timescale of transport from the near-shore to the outer-shelf region, and how does this timescale compare to the sampling timescale? Is it possible that the spatial trends seen throughout this study are in anyway artifacts of a sampling timescale bias?

Reply: Very good question and difficult to answer. The transport varies from a few weeks to months depending on environmental conditions. But statistical tools can help to understand the

artifact linked to timescale in a given study period. This was in fact one of the goal in a precedent paper (Forest et al., 2013) within which the sampling date was explicitly included in a statistical examination (variation partitioning) of carbon fluxes during Malina. The conclusion is very clear, only 4.7 % of the variability is explained by a pure temporal trend (see the Figure 16).

Referee: L11-13: I don't find this connection to be strong enough to support the claim that "autochthonous processes govern the overall particulate carbon cycling over the Mackenzie Shelf…"

Reply: Our results are embedded in the context of previous studies to make a strong case. For example, the semantic paper of Macdonald 1998 provides estimates of river POC input in the range of 2.1 Mt POC yr-1, whereas autochthonous production (shelf only) is about 3.0 Mt POC yr -1 in their review. Clearly, both processes are important, but in terms of cycling and food web response, local production is still more important. In fact, the residual signal of marine POC in shelf sediment is very low, implying that cycling of marine POC is very efficient in the region. Cycling is all about fluxes, not reservoirs.

Referee: L22: What kind of unpublished data? L29: Change "sink" to "sinks"?

Reply: This is a manuscript in preparation by Lansard. All data are available on the web site mentioned above. Done for sinks.

Referee: P15674: L16: "...it appears obvious..." is based on what data?

Reply: Data from the study referred at the end of the sentence. The words "appears obvious" were erased.

Referee: P15675 L10: New section here?

Reply: This is the paragraph that describes processes in the basin. It fits in the current section.

Referee: L23-24: Reads awkwardly ("biogeochemical gradient in carbon and nitrogen cycling...").

Reply: We erased "carbon and nitrogen".

Referee: L22-25: It is certainly possible that carbon and nitrogen cycles will change over time, but I think that the authors' statement here is out of line with the data in this study, especially given the lack of a temporal component to the analysis.

Reply: Despite having a restricted temporal component (one season), we can insert our results into a long-term context given the many studies conducted over the years in the region. Our work is just another piece in the puzzle of the Beaufort Sea. Still, all the different pieces point toward the same direction: the shelf productivity is increasing because of intensified upwelling events; while the basin productivity is apparently decreasing or could decrease because of freshwater accumulation (stronger stratification). This is why the gradient is exacerbated.

Referee: P15676 L11-16: I am not convinced that this is the best method. Clarifying the specifics of the method, the details of the calculations, and the uncertainties involved in the estimations would go a long ways towards convincing the reader of the validity of the method and lead to more credible interpretations.

Reply: We do not pretend to have the best approach. But the goal of our work is to deliver a more complete picture of the ecosystem by documenting the different compartments at play. Following the recommendations of the referee, we have augmented the details of the calculations and assumptions (e.g. equations) and conducted an error propagation estimate, which is now presented in Appendix A. There are many ways to calculate NCP, and the approach needs to be justified by the objectives and questions of a given study. Hence, for the type of work we conducted, we are confident that we chose the best approach.

Referee: P15677 L18-20: How can you distinguish between DIC delivered via processes such as respiration versus advection of water masses?

Reply: This is just a suggestion. The spatial match between increased DIC and increased respiration is so "great" that we cannot avoid mentioning this observation. But we revised this sentence to propose that water mass advection is also a plausible cause.

Referee: P15678 L24: Reads awkwardly ("supporting previous evidences...").

Reply: We

Referee: P15679 L28+: How does the negative slope indicate an "evolving" system, and how is climate change implicated? This sentence is confusing and speculative. I would recommend removing it.

Reply: Agreed. We rephrased the sentence to make it more focused on the present study. It is indeed true that the Canada Basin ecosystem we sampled was highly oligotrophic. The revised sentence reads as "In particular, the negative slope computed for the western off-shelf zone represents a patent indication of the low-productive regime that prevailed in the Canada basin at the time of our study."

Referee: P15680 L15-24: This seems speculative and inappropriate for the summary section. Perhaps it can be folded into the appropriate introductory section?

Reply: This section describes our understanding of upwelling variability and coupling as a function of atmospheric forcings. It fits well into the summary section that we intend to provide here. Nevertheless, the sentences have been revised and shortened. In particular, the long sentence on sea ice decline was erased.

Referee: P15681 L6: "monitoring the upwelling flow" as a "key issue" seems to be slightly at a tangent to the main points of this manuscript.

Reply: We rather think that this is a very important point if we want to understand the potential long-term trend and balance between CO2 outgassing and increased PP potentially sustained by upwelling.

Referee: L13: Consider word choice ("rebarbative").

Reply: We changed this sentence for "more investigation is needed to conclude on the actual food web response to terrigenous carbon:

Referee: P15682 L5: Reads awkwardly ("Concurrently to the...").

Reply: Changed for concomitantly.

Referee: L11: Is the word "unicellulars" a standard term in biogeochemistry?

Reply: Changed for single-celled organisms.

Referee: L11-12: I would suggest flushing out the sea-ice melting effect on CO2 dynamics earlier in the text.

Reply: This is an important effect that we cannot avoid in our study (sea ice is so important). Discussion about it is done carefully by referencing the correct papers and results (e.g. such as in section 4.1, last paragraph).

Referee: L19-22: This final statement concerns cross-shelf OC flux, which seems to be a tangential to the main findings of the study.

Reply: The summary section aims at presenting key ideas for future studies in relationship with ecosystem metabolism and carbon fluxes. Hence, cross-shelf flow is nothing less than the most important variable to monitor given the rate of change and distinct changes that occur either over the shelf or in the basin.

Referee: L23: Change "on" to "in."

Reply: Done.

Referee: P15701 Fig4: Perhaps include a short explanation of the Cape Bathurst hotspot in the legend?

Reply: Yes. Done.

Referee: P15702 Fig5: Change "brow" to "brown."

Reply: Done.

Referee: Fig5: Unclear to what "POC in total" corresponds. Do you mean total OC (POC+DOC)?

Reply: Revised. This is the fraction of POC in total organic carbon.