Forest et al.
Biogeosciences submission
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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 CO_2 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 CO_2 sink despite net heterotrophy, and (3) that CO_2 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 CO_2 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 CO_2 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.

Specific Comments:

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

P15643
L3: What does a "more dynamic atmosphere" mean?
L12-14: Reads awkwardly.
L18: Can you say that -2.0 ± 3.3 is significantly different that zero? Is the uncertainty 1σ?
L22: Mismatch in units. Can you use the same throughout (mmol or mg)?
L23-25: Reads awkwardly ("…cumulated to a…" and "…twice higher…")

P15644
L3-4: Use mg as unit?
L8: Conclusion (1) would seem to be difficult to support with data from this synoptic study.
L9: Conclusion (2) contradicts the finding of net heterotrophy in the Beaufort Sea.
L9-15: It's unclear how these conclusions will be supported by a synoptic sampling scheme.

P15646
L24: Change "has been" to "was"?
L29: Reads awkwardly ("…form the crucible for concluding on the potential…").

P15647
L2: Change "to which extent…" to "to what extent…is net ecosystem metabolism coupled to…"?
L2-5: The abstracts suggests the following answers:
(1) uncoupled
(2) unknown
(3) unknown
L8: Rectangular shelf?
L9 and L10: Change "on" to "to"?

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

P15649
L8-9: Reads awkwardly.
L29: Change "on" to "in"?

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.

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?
L6: Why was 30 mg m^{-3} chosen as a threshold?
L12: UNESCO in capital letters?
L21: Change "analyses" to "analyzes"?

P15652
L11: What thermodynamic effects were corrected for, and how were corrections made?
L13: How were the underway and bottle data "merged"?
L15: Is "virial" correct?
Eqn (1): The dashes and minus signs result in confusing syntax. Suggest using dashes only for minus signs.

P15653
Eqn (2): Why was this relationship chosen? How sensitive are the results to the choice of a different parameterization?
L8-9: Why were these data removed?
L19-23: These seem like large assumptions. How sensitive are results to these assumptions?

P15654
L1: Is it possible to include actual measurements of DOC and other similar values in an Appendix?
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?
L17: Does the persulfate wet-oxidation method miss some portion of occluded particulate OC? Would this lead to an underestimate of calculated DOC?
L21: I don't understand what is meant by "the calibration of DOC."
L24: Were the sediment trap stations representative of the region as a whole? Where were these stations?
L25: If 80μm particles were the smallest recorded, how are these measurements connected to POC values (> 0.7μm) or actual sinking fluxes that include particles less than 80μm?

P15655
L10: Recommend providing a quantitative measure of "reproducibility."
L13-27: What are the uncertainties in these bottle-based estimates? How would they bias the calculated GPP values?

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?
L21: Does the cellular biovolume estimate (0.040 μm^3) match previous observations in the Beaufort?

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)?
L13: Reads awkwardly.
L21: Are the biomass values wet weight or dry weight? Can these data be presented in an Appendix?
L24: What is the value of the mean ratio?

P15659
L8-19: Good section.

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

P15661
L3: Does this mean that MODIS-based PP estimates are underestimates? How does this impact GPP and CO_2 flux interpretations made later in the manuscript?

P15662
L13-15: The CO_2 sink doesn't appear to be significantly different than zero, yet this uncertainty is not adequately addressed in the discussion sections.

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.
L16: Missing a word after "magnitude"?
L13-15: It is unclear how the data presented lead to this statement. More detail, reasoning, and clarification would be needed.
L18-22: How does the nitrogen data compare to previous studies in the Beaufort Sea? Can these data be included in an Appendix?
L27: Is it possible to show the sample locations in the accompanying figure (6)?

P15664
L1: It is unclear how nitrate data alone provide information about cross-shelf flow?
L2: What does "oblique expansion" mean?
L7: Are these GPP rates from the bottle incubation experiments or the MODIS data?

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)?

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.

P15668
L2: Change "To which…" to "To what…"?
L15-21: All estimates suggest a sink of CO_2. How do these estimates square with the NCP estimates presented in Fig 10a and on P15667? Are they statistically different from one another?

P15670
L9: Missing words?
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.
L20-24: This may be true, but I don't see strong evidence for this claim in the data.

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.

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

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

P15675
L10: New section here?
L23-24: Reads awkwardly ("biogeochemical gradient in carbon and nitrogen cycling…").
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.

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.

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

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

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.

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

P15681
L6: "monitoring the upwelling flow" as a "key issue" seems to be slightly at a tangent to the main points of this manuscript.
L13: Consider word choice ("rebarbative").

P15682
L5: Reads awkwardly ("Concurrently to the…").
L11: Is the word "unicellulars" a standard term in biogeochemistry?
L11-12: I would suggest flushing out the sea-ice melting effect on CO_2 dynamics earlier in the text.
L19-22: This final statement concerns cross-shelf OC flux, which seems to be a tangential to the main findings of the study.
L23: Change "on" to "in."

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

P15702
Fig5: Change "brow" to "brown."
Fig5: Unclear to what "POC in total" corresponds. Do you mean total OC (POC+DOC)?