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# *Interactive comment on* "Distribution and C / N stoichiometry of dissolved organic matter in the North Sea in summer 2011–12" *by* Saisiri Chaichana et al.

**Anonymous Referee #1**

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General Comments

This paper details a multi-year high-resolution sampling of DOM dynamics in the North Sea. The main conclusions of the work suggest that there is high spatial and temporal variability in the total concentrations and C:N ratio of OM over the sampled periods, and that this inter-annual variability has strong implications for overall carbon budgets in the region. The implications of this work for carbon cycling in the region are important, and I found the paper to be generally well written. However, I struggle with the chosen focus on C:N ratios for elucidating DOM dynamics. I felt that novel portion of this work, that is, the elucidation of the impact temporal variability has on the overall carbon budget(s),



was not emphasized enough. In particular, there are a few main facets of the work I feel need further development if they are to be included in the final manuscript:

1) The objective that the C:N ratio is presented to address- and how this is interpreted and discussed. As mentioned in the work, many factors can affect this ratio! If this is the only metric you use to assess DOM dynamics, you need to be really careful. Can you really answer the larger objectives you outline with the hypotheses that you pose? How does the C:N ratio compare to the chla concentrations, nutrients, and previous work in the region on tracking allochthonous vs autochthonous sources of OM? How does your end-member calculations compare to actual end-members from the literature, and OM composition from other work?

2) Tie the POM work in better. How does this compare to the DOM, and what is this impact on the overall carbon budget?

The discussion and conclusions regarding the relationship of DOM with salinity. This relationship has been found to be conservative with mixing of the major water masses in the North-Baltic Seas transition, however major non-conservative processing on DOM does occur in the region (see eg. Osburn and Stedmon, 2011 Marine Chemistry DOI: 10.1016/j.marchem.2011.06.007). I would like to see further development of this, including tying the current work into previous analyses of DOM composition, etc. What are the implications of local variability, in this context?

3) This is perhaps the most important- I feel the discussion on C inventory should take center stage. How does this work advocate for or against high-resolution measurements? How does it revise or promote our understanding of DOC cycling in the region? How does this compare to other regions and/or the global budget? What are the uncertainties with C budgets, and does this paper help to narrow these?

Specific Comments

1 Introduction-

The first paragraph starting on line 23, to me, is the motivation for this work. Clearly relate the following discussions, and set-up to the goals of the study relative to this. Be explicit upfront- what do you hope to find with this work, and how is it novel (e.g. lines 25-29)? Some of this is outlined in later the introduction, but the narrative back to the main objectives of the work is lost throughout the rest of the paper. The authors attempt this tie by stating hypotheses and referring to them throughout. This causes the prose to be a bit awkward- and I suggest instead outlining the overlying research outcomes the authors hope the study will answer. As is, the hypotheses are too narrow to the stoichiometry work, and don't really address our gaps in understanding of the temporal variability of the C cycling in the region. Perhaps a figure would help with synthesizing what we know, and what gaps this study addresses? How has this sort of "high-resolution" work refined carbon budgets in other regions? What are the processes affecting atmospheric CO2 draw down in regions such as shelf seas? How does this relate to the global carbon budget? Why is the North Sea an ideal system to study in this regard?

Page 2, Line 6- Is DIC really the only place for long term carbon storage in the ocean? Better tie in why you are looking at DOC, not DIC.

Page 2, Paragraph starting line 13- I would argue that C:N stoichiometry is a very small part of understanding allochthonous vs autochthonous OM sources, and especially reactivity of DOM. You acknowledge some caveats here, but how does more compound-specific work (such as isotopes, biomarkers, etc) compare to C:N ratios (e.g. Kaiser and Benner, 2012 JGR-Oceans DOI: 10.1029/2011JC007141)? Convince the reader that the stoichiometry is an adequate tool for the objective you are outlining, ie. using C:N ratios to understand DOM source and reactivity. As is, I feel that the discussion and implications of the work rely too heavily on this.

Page 3 line 15- Add "climate" in front of cycles

2 Methods-



In general, I feel the methods are well explained and analytically sound. An interesting paper recently came out in EST Letters that I feel the authors could benefit from regarding "DON" calculations- Saunders et al., 2017 DOI: 10.1021/acs.estlett.7b00416

Page 4, lines 21-22- Why did you exclude the riverine-influenced sites? How does this impact your further discussion of sources and end-members and your hypotheses above?

3 Results- The results section includes a bit of interpretation in it (e.g. see paragraph on page 11 lines 11-17) and should be reworked to include only observations of the data.

Do you have the TS profiles? What about other property/property plots?

Page 7 line 21- What do you mean by noisy?

Page 8, lines 8-12- This is confusing, but is an important distinction. Be clear with your comparisons here, and throughout the rest of the manuscript! Perhaps delineating the water bodies by type for comparisons of measurements over time (eg. Open Atlantic water)?

Page 9, lines 10-11- How does the spatial subset data compare?

Page 9, paragraph lines 20-29- I don't understand the point you are trying to make here. Additionally, the DOM-Salinity relationships, while significant, are not very strong ($R2 < 0.5$ for all water bodies). Further discussion of conservative vs any potential non-conservative behavior is needed. Your salinity gradient is not that large- how does this impact your interpretations?

Page 10, line 9-14- I think this discussion would be better supported if depth were included on the figure. As is, I see no real linear relationship in the Winter 2012 samples and this discussion is not really supported by Figure 4- these relationships don't look particularly linear with salinity.

Page 10, line 28- What are the percent differences between these observations (i.e. interannual vs depth)? Is this statistically significant?

Page 11, line 20- “interesting differences”- what are these differences? Be explicit. This paragraph is confusing, perhaps by splitting up the observations into difference sentences would help for a more succinct narrative.

4 Discussion- I feel that much of the discussion should be reworked- I have a hard time following the structure of many of the arguments in the discussion, in particular the DOM-salinity (section 4.1) and the DOM variability (section 4.3) discussions. Are the end-member data robust enough you could perform an actual mixing analysis (similar to the approach in Perdue and Koprivnjak, 2007 Estuarine, Coastal and Shelf Science doi:10.1016/j.ecss.2006.12.021 ; See also the caveats outlined in using C:N ratio to determine terrestrial vs aquatic sources of OM outlined in this work) ? How, specifically, does the nutrient data tie into this? Section 4.1- I am missing the connection between the topic sentence and the following discussion. How does the lack of relationship between chla and POM support or refute your hypotheses?

Section 4.2 – This is your most interesting and novel finding. Do you see large spatial variations that might weaken the budget extrapolation? Are there any physical oceanographic work that support the shifts in exchange of water masses that you discuss? I think this section could be split and both paragraphs expanded upon significantly.

Section 4.3- The discussion of potential benthic inputs of OM must be further expanded upon- while this is an interesting hypothesis, the current arguments do not convince me. Do your turbidity or POC data support the nepheloid hypothesis?

5 Conclusions-

Again, I feel the focus here should be on the C budgets more than the DOM dynamics.

Technical Corrections

Comments for throughout the manuscript:



Make sure super and subscripts are correct (e.g. page 3 line 5, page 5 line 17). Check sentence structure for flow, spelling, and punctuation. Below are a few (non-exhaustive) examples: Page 3, line 16 a comma is missing after “2007)” as on page 6, line 9 a comma is missing after “(LOD)”. Page 12, line 14 is missing a period. Page 16 line 16 missing a “t” in “this”.

Check paragraphs for run-on sentences, which confusing the meaning. Eg. Page 11 lines 19-23.

I feel many of the connecting sentences are awkward and should be reworded to flow better. E.g. page 12 lines 2-4: “In this discussion, we consider...” Check that the citations are imported to the text properly (e.g. page 12 line 9). Make sure the nomenclature is used consistently- ie. DOM, DOC, DON.

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