

Interactive comment on “Diatom flux reflects water-mass conditions on the southern Northwind Abyssal Plain, Arctic Ocean” by J. Onodera et al.

Anonymous Referee #2

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General comments

The authors present the result from shallow and deep sediment traps deployed between 2010 and 2012 at a unique station NAP in the Chukchi Borderland. Times-series of bulk composition, diatoms frustules fluxes and POC fluxes were evaluated. The seasonality in the properties of sedimentation is related to the physical conditions of the water masses. Shift between advection of shelf waters and Canada basin waters were shown to greatly influence the quality of the sinking flux. The authors demonstrate high sinking fluxes of diatoms are mainly due to northward advection of phytoplankton-rich shelf waters. It is also mentioned that a part of the flux is due to autochthonous production. Offshore displacement of cold eddies is emphasized as an explanation for the maximum diatoms sinking rates observed in winter. I greatly enjoyed reading

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the manuscript especially the discussion section. This paper presents very interesting information about poorly documented winter process. The authors provide interesting and original demonstration by relying the sinking rates and bulk composition with the circulation patterns as the Beaufort Gyre. Nevertheless, I pointed out some weakening mainly in the introduction and results sections that could be largely related to English writing mistake and wrong formulation. I underline some questions and comments that should be answer and corrected before considering for a publication in Biogeoscience. I think the authors could address all this comment without much of the difficulty.

I join a PDF with inserted minor comments that should be addressed.

Specific comments

Introduction:

The information and references presented in the introduction are relevant but not well organized. Some sentence cut the flow of the text as L12 p15217, which link with the text before and after is not clear.

I would expect to have the proportions of diatoms in the total carbon fluxes over the shelves and basin. Such information would help to understand the importance to monitor the diatoms flux offshore where picoplankton actually dominates the production.

You cite a previous work of Watanabe et al., (2014). The main result of its studies should be presented in the introduction. Same for the Zernova et al. (2000), what is their main finding? There is few information about sedimentation rates offshore so you need to present them.

L9 L13 p15218: Be cautious, the results observed at a unique station cannot be extrapolated to the whole western Arctic Ocean. For example, the Canada basin exhibits different hydrography and communities than the Chukchi borderland and sedimentation dynamics are certainly different there.

Material and Methods:

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There is some useless information presented in this section, which make the reading difficult. I underline some of them in the specific comments. I'm not familiar with models and I would like to have a more clear explanation of the models used and its parameterization. I don't really understand how the initial conditions are chosen and how these conditions affect the model. Why changing to COCO 3.4 and NCEP1?

The end of the section is imprecise. I don't understand which "seasonal experiments" and which "major variability" you talking about. Please precise the parameters and experiments you describe.

Results:

I found the result clearly presented. However, the description of the Figure 3c and 3d are difficult to follow. The results referring to the shallow traps should be more clearly differentiate from the results associated to the deep traps. To increase the clarity of section 3.3, I suggest to present first the upper trap and then depict the difference and similarity observed in the deep trap like the author has done in the first paragraph of the 3.3 sections. I like the idea to present a temporal succession of species but the authors should clearly keep the timeline when describing the figure.

The tables A1a, A1b are far too long. I suggest a table with average values of the parameters for relevant time period/seasons and move the full table as a supplementary material.

End of 3.1: How currents could deepens the trap. I expect the opposite effect; currents should incline the mooring and thus decrease the depth.

L10-11 p.15221: Please mentioned the exceptionally low fluxes and bulk content in the entire years 2012 and provide some values to compare with 2010 and 2011. What kind of particle is represented by the white color in 2012 (figure 2e)?

L10-11 p.15222: I don't agree. There is interesting difference between shallow and deep traps. The summer peak is significantly higher than the winter peak at deep

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traps, the summer material seems more preserved than the winter material. You should present and discuss these facts in the discussion section.

L4-6 p.15224: Explain why the fact you just find the needle-like valve rather than the intact cells indicate a high diatom POC flux from Rhizosolenia and Proboscia.

Discussion:

In section 4.1, the beginning of the paragraph should be better presented. I suggest to first present your hypothesis of the advection of shelf waters. After, you could detail the different findings and observations that drive you to such conclusion.

I not convinced with the last sentence of the section. All along you explain diatoms are probably advected from the shelf in 2011 while oligotrophic waters are advected in 2012. Then you conclude a highest primary production in 2011 but you don't have any measures of primary production or nutrient. Moreover, if the diatoms are advected, they don't support local primary production. Please provide more clues to support such conclusions.

I enjoy reading the section 4.2 and 4.3 that are well written and very interesting. I pointed out the term "unique" in L18 p.15227. Maximum winter diatom fluxes were observed both in 2010 and 2011 and not at a unique occasion. Are the cold-eddies mechanisms responsible for these two maxima? Is there evidence than cold-eddies propagates mainly in autumn-early winter?

L13-17 p.15228: I'm not sure about the relevance of this comparison, the Honjo et al., (2010) trap was deployed largely deeper (3067) which could easily explain the lower fluxes.

Technical corrections

L2 p15216: replace "through" by "to"

L7 p15216: 98 taxa are plural and should be "98 taxas".

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L 21 p15216: I don't think temperature is the main factor of increasing primary production over the shelf. What about light? Nutrients?

L23 p15216: I suggest "dominant phytoplankton"

L1 p15217: "has been quite low". Why use the past, it is not low anymore?

L3 to L5 p15217: I suggest to merge these two sentences and reformulates by using "zooplankton fecal pellets" and "shell-bearing microplankton" as the subjects of the sentence.

L8 p15217: deepening of the nutricline. The reference to McLaughlin and Carmack 2010 should be added.

L17 p15217: Bad tense used. I suggest begin the sentence by "While the shelf has been substantially monitored, the year round studies. . .over the basins. . ."

L23 p15217: "whereas" wrong term.

L29 p15217: replace "among" by "between".

L12 p.15218: removed "twice" and add "Two" at the beginning of the sentence.

L15-16 p.15218: unclear, it look like you sample each 10-15 days? Specify if it's an automatized system? If it's automatized why not choose the same time lag between each sampling? Please provide more information about the sampling method here.

L16 p.15218: Remove "The record . . .show that"

L17-18 p.15218: By reading this sentence I first understand the trap depths vary from 60m to 80m along the experiment. Then I understand two traps were deployed by depth. Please clearly indicate there are two traps at shallow depths (180m and 260m) and two traps at deep depth (1300m and 1360m).

L19 p.15218: Indicate quickly what is the purpose of the neutralized formalin.

L20 p.15218: change "all of the . . . traps" by "the samples from both traps except the

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one. . .”

L21 p.15218: Why some traps have very low volume? Have you a technical reason to support the fact you discard them from the analysis? If not you will bias the quantitative measurements by removing them from the study.

L24 p.15218: What is the difference between the pore size and the grid size?

L7-9 p.15222: Remove “in contrast” because you start a new idea here. To highlight the fact it’s the highest values I suggest to write “The maximum fluxes reached . . . and . . . in winter 2010 and 2011, respectively.

L24 p.15222: How dominance can be low?

L29 p.15222 to L3 p.15223: These sentences are repetitive to express just one idea. It can be reduce to “ The biogenic materials collected in this study were primarily of marine origin. Åž. By the way, such general observation should be at the beginning of the paragraph about species composition.

L19-22 p.15223: The sentence is unclear. Please reformulate maybe split in two sentences.

L26 p.15223: Chaetoceros appear very low on the Fig. 4. So I would not consider this group as a dominant one for POC flux. Conversely, Thalassiosira appear an important group to consider for POC flux.

L2 p.15224: The name “Fossula arctica” doesn’t appear on the graph 4 so I suggest to write “The ice-related algae F. arctica. . .”.

L17-18 p.15224: It’s more precise to say the presence of F.Arctica suggest the presence of sea-ice transported from the Chukchi shelf.

L23-26 p.15224: Please write the full name Proboscia eumorpha to facilitate the understanding.

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L6 p.15225: “suppress” must be change by “absence of” in the whole section.

L20 p.15225: Unclear, what did you compare with 2011: the position or the height of the gyre.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C7085/2014/bgd-11-C7085-2014-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 11, 15215, 2014.

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