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2
3 **Reply to the interactive comment by Referee #2 on “Diatom flux reflects water-mass**
4 **conditions on the southern Northwind Abyssal Plain, Arctic Ocean” by J. Onodera et al.**

5
6 General comments

7 The authors present the result from shallow and deep sediment traps deployed
8 between 2010 and 2012 at a unique station NAP in the Chukchi Borderland.
9 Times-series of bulk composition, diatoms frustules fluxes and POC fluxes were
10 evaluated. The seasonality in the properties of sedimentation is related to the physical
11 conditions of the water masses. Shift between advection of shelf waters and Canada
12 basin waters were shown to greatly influence the quality of the sinking flux. The
13 authors demonstrate high sinking fluxes of diatoms are mainly due to northward
14 advection of phytoplankton rich shelf waters. It is also mentioned than a part of the flux
15 is due to autochthonous production. Offshore displacement of cold eddies is emphasizing
16 as an explanation for the maximum diatoms sinking rates observed in winter. I greatly
17 enjoyed reading the manuscript especially the discussion section. This paper presents
18 very interesting information about poorly documented winter process. The authors
19 provide interesting and original demonstration by relying the sinking rates and bulk
20 composition with the circulation patterns as the Beaufort Gyre. Nevertheless, I pointed
21 out some weakening mainly in the introduction and results sections that could be
22 largely related to English writing mistake and wrong formulation. I underline some
23 questions and comments that should be answer and corrected before considering for a
24 publication in Biogeoscience.

25 I think the authors could address all this comment without much of the difficulty.

26
27 I join a PDF with inserted minor comments that should be addressed.

28
29 **Author’s reply**

30 We appreciate Referee #2 on the positive review of this manuscript and many
31 suggestions to improve the manuscript. All comments by referee #2 were helpful to
32 revise the manuscript. Our reply to each specific comment is listed as follows. We hope
33 this revised manuscript is acceptable.

37

38 Specific comments

39 Introduction:

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

43 [The sentences and some references in the introduction was re-organized.](#)

44

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

49 [With the reference by Ardyna et al \(2011\), difference of dominant phytotplankton in
50 eutrophic and oligotrophic waters were shortly mentioned in the introduction.](#)

51

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

56 [The main results of Watanabe et al. \(2014\) and Zernova et al. \(2000\) were presented.
57 The difference of this study from Watanabe et al. \(2014\) was also written in the revised
58 introduction.](#)

59

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

64 [As you mentioned, there are differences in hydrography and communities. In the
65 sentence describing objectives of this paper, target area was corrected as “the
66 Northwind Abyssal Plain” from “the Western Arctic Ocean”.](#)

67

68 Material and Methods:

69 There is some useless information presented in this section, which make the reading
70 difficult. I underline some of them in the specific comments. I’m not familiar with
71 models and I would like to have a more clear explanation of the models used and its
72 parameterization. I don’t really understand how the initial conditions are chosen and

73 how these conditions affect the model. Why changing to COCO 3.4 and NCEP1?
74 The methods for model study were rewritten. All the specific comments were applied to
75 correct the text. We hope the revised method section is easier to understand.
76
77 The end of the section is imprecise. I don't understand which "seasonal experiments"
78 and which "major variability" you talking about. Please precise the parameters and
79 experiments you describe.
80 The sentences in the end of this section were rewritten.
81
82 Results:
83 I found the result clearly presented. However, the description of the Figure 3c and 3d
84 are difficult to follow. The results referring to the shallow traps should be more clearly
85 differentiate from the results associated to the deep traps. To increase the clarity of
86 section 3.3, I suggest to present first the upper trap and then depict the difference and
87 similarity observed in the deep trap like the author has done in the first paragraph of
88 the 3.3 sections. I like the idea to present a temporal succession of species but the
89 authors should clearly keep the timeline when describing the figure.
90 The description of Figure 3c and 3d were revised. The time-series succession of major
91 diatom species were described. In addition maximum value of relative abundances for
92 dominant species, and difference in settling diatom flora between shallow and deep
93 traps were written.
94
95 The tables A1a, A1b are far too long. I suggest a table with average values of the
96 parameters for relevant time period/seasons and move the full table as a supplementary
97 material.
98 We ask the editor to move these tables to supplementary material.
99
100 End of 3.1: How currents could deepens the trap. I expect the opposite effect: currents
101 should incline the mooring and thus decrease the depth.
102 As you expected, the temporal deepening of sediment trap moored-depth is due to the
103 incline of the bottom-tethered mooring by intensified currents. We slightly modified the
104 sentence.
105
106 L10-11 p.15221: Please mentioned the exceptionally low fluxes and bulk content in the
107 entire years 2012 and provide some values to compare with 2010 and 2011. What kind
108 of particle is represented by the white color in 2012 (figure 2e)?

109 The white area of bulk component in Fig 2e represents that no bulk component
110 analysis was fully/partially conducted because of limited sample volume. This is shortly
111 mentioned in the last sentence of figure caption for Fig 2e.

112

113 L10-11 p.15222: I don't agree. There is interesting difference between shallow and deep
114 traps. The summer peak is significantly higher than the winter peak at deep traps, the
115 summer material seems more preserved than the winter material. You should present
116 and discuss these facts in the discussion section.

117 The description was revised based on the comment as follows. "The high diatom flux
118 season at the deep trap depth was similar to that at the shallow trap depths (Fig. 3a, b).
119 However, there was different from shallow trap data that total diatom flux at deep trap
120 in summer 2011 was higher than that in winter maxima." Although I do not have the
121 certain evidence, the possible reason of minor winter flux maximum at deep trap is not
122 only decomposition of biogenic particles but also the horizontal diffusion effect of
123 settling particles in deep sea under the eddy as simulated by Siegel et al (1990). This
124 was shortly included in the last paragraph of Discussion section 4.2.

125

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

128 Because I did not distinguish the intact cells from all encountered diatoms during the
129 counting work, diatom POC flux derived by diatom cell size and count data (including
130 empty cells) sometime become overestimate and exceed total POC flux. The genera
131 *Rhizosolenia* and *Proboscia* have a large carbon content per cell while there occurrences
132 in November 2011 were as usually needle-like end part of empty *Rhizosolenia* and
133 *Probosocia* cells.

134

135 Discussion:

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

139 The one sentence was added as follows. "Because the phytoplankton productivity and
140 phytoplankton assemblage is clearly different between the Chukchi Sea shelf and the Canada
141 Basin, the settling diatom flux at Station NAP should reflect the times-series hydrographic
142 variations."

143

144 I not convinced with the last sentence of the section. All along you explain diatoms are

145 probably advected from the shelf in 2011 while oligotrophic waters are advected in 2012.
146 Then you conclude a highest primary production in 2011 but you don't have any
147 measures of primary production or nutrient. Moreover, if the diatoms are advected, they
148 don't support local primary production. Please provide more clues to support such
149 conclusions.

150 The term of primary productivity in the section 4.1 had to be replaced to diatom flux
151 because we have no time-series observation data regarding primary productivity. We
152 just suggested the influence of shelf waters rather than variation of primary
153 productivity at Station NAP.

154

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

160 We removed the word "unique" from the sentence. Although the model experiment for
161 eddy advection at Station NAP in November-December 2011 was not conducted, eddy
162 occurrence and westward advection is usually figured in the southwestern Canada
163 Basin. As the cause of particle flux maxima in November-December of 2010 and 2011,
164 westward advection of eddies originated from off the Barrow Canyon are the strongest
165 candidate to explain the results.

166

167 L13-17 p.15228: I'm not sure about the relevance of this comparison, the Honjo et al.,
168 (2010) trap was deployed largely deeper (3067) which could easily explain the lower
169 fluxes.

170 According to schematic diagram in Honjo et al. (2010), lower POC flux in subsurface of
171 Canada Basin is estimated. The POC flux at ~120 m depth at 75°N and ~200 m depth at
172 80°N in the Canada Basin is about 10 and 7 mmol m⁻² yr⁻¹, respectively. The annual
173 POC flux at Station NAP for the first deployment period is about 27 and 20 mmol m⁻²
174 yr⁻¹ at shallow and deep traps, respectively.

175

176 Technical corrections

177 L2 p15216: replace "through" by "to"

178 The word "through" was replaced by "to".

179

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

181 The word “taxa” is plural form of “taxon” as far as I know. We did not change this word.

182

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

185 As far as I refer the paper by Wang et al. (2013), temperature was the main factor for
186 increasing primary production in future. However, the biological reaction to
187 environmental change is various in the Arctic Ocean. The description “such as
188 temperature” was removed from the sentence.

189

190 L23 p15216: I suggest “dominant phytoplankton”

191 The words “major phytoplankton” was changed to “dominant phytoplankton”.

192

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

194 We used the words as a present participle. The words “has been quite low” were simply
195 rewritten as “is quite low”.

196

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

200 The sentences were merged and rewritten as follows. “The low productivity of
201 shell-bearing microplankton and zooplankton fecal pellets, which have a role as ballast
202 for settling organic matter, limits the function of biological pump in the oligotrophic
203 cryopelagic Canada Basin (Honjo et al., 2010).”

204

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

207 The reference “McLaughlin and Carmack 2010” was added.

208

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

211 The sentence was revised. “While the shelf and shelf slope areas of the Arctic Ocean
212 where there have been substantially monitored (i.e., Hargrave et al., 1989; Fukuchi et
213 al., 1993; Wassmann et al., 2004; Forest et al., 2007, 2011; Gaye et al., 2007; Sampei et
214 al., 2011), the year-round study of sinking biogenic particles over the basins is still
215 limited, except for a few studies (Fahl and Nöthig, 2007; Lallande et al., 2009; Honjo et
216 al., 2010; O’Brien et al., 2013).

217

218 L23 p15217: “whereas” wrong term.

219 The sentence “...(Honjo et al., 2010) whereas there has been...” was revised as “...(Honjo
220 et al., 2010). However, there has been...”.

221

222 L29 p15217: replace “among” by “between”.

223 The word “among” was replaced to “between”.

224

225 L12 p.15218: removed “twice” and add “Two” at the beginning of the sentence.

226 The sentence was collected as suggested.

227

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

231 The sampling schedule was manually decided. High resolution sampling (10days
232 interval) was set for late spring - summer, instead sampling resolution became low
233 (15days) for fall-winter.

234

235 L16 p.15218: Remove “The record . . .show that”

236 Deleted.

237

238 L17-18 p.15218: By reading this sentence I first understand the trap depths vary from
239 60m to 80m along the experiment. Then I understand two traps were deployed by depth.
240 Please clearly indicate there are two traps at shallow depths (180m and 260m) and two
241 traps at deep depth (1300m and 1360m).

242 In order to avoid the misleading, the sentence was revised.

243

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

245 The words “as an antiseptic (pH~8.2)” were added.

246

247 L20 p.15218: change “all of the . . . traps” by “the samples from both traps except the
248 one. . .”

249 The sentence was changed based on the suggestion.

250

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

253 quantitative measurements by removing them from the study.

254 Some samples with very low particle volume are essentially reflecting low flux of
255 settling particles rather than technical problem of trapping settling particles. The
256 temporal deepening of moored sediment trap in July 2012 might affect the trapping
257 efficiency as mentioned in text. The sample volume of those samples were too low to
258 analyze the bulk component and diatom analysis. In this study, quantitative
259 measurements for annual flux was not conducted.

260

261 L24 p.15218: What is the difference between the pore size and the grid size?
262 The pore size determines the particle size remained on the filter. The grid size means
263 the interval of printed grid lines on the filter. The microscopic observation was
264 conducted along the grid lines.

265

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

270

271 L24 p.15222: How dominance can be low?
272 The sentence was corrected as follows. “The observed relative abundance of sea
273 ice-related diatoms in total diatoms was less than 23% in summer 2012.”

274

275 L29 p.15222 to L3 p.15223: These sentences are repetitive to express just one idea. It
276 can be reduce to “ The biogenic materials collected in this study were primarily of
277 marine origin. Å’z. By the way, such general observation should be at the beginning of
278 the paragraph about species composition.
279 Based on the suggestion, the sentence “The diatoms encountered ... species.” was
280 deleted. The following sentence “Because diatom species usually observed ... were
281 primarily of marine origin.” was moved to the upper part of paragraph on diatom
282 species composition.

283

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

287

288 L26 p.15223: *Chaetoceros* appear very low on the Fig. 4. So I would not consider this

289 group as a dominant one for POC flux. Conversely, *Thalassiosira* appear an important
290 group to consider for POC flux.

291 There was mistake in the legend of Fig. 4. The legends of *Chaetoceros* and *Thalassiosira*
292 must be swapped. In addition, the graph data for *Chaetoceros* contains the data both
293 vegetative cells and resting spores. *Chaetoceros* vegetative cells rather than spores were
294 important for POC flux. The text “(resting spores)” was deleted.

295

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

298 We changed the sentence as referee #2 suggested.

299

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

302 The sentence was changed based on the suggestion. “The high dominance of *Fossula*
303 *arctica* at Station NAP in summer 2011 suggests the presence of sea-ice transported
304 from the Chukchi Sea shelf.”

305

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

308 The words “*P. eumorpha*” in the sentence was replaced by “*Proboscia eumorpha*”.

309

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

311 Instead of the word “suppress”, “lower”, “reduce” or “absence of” were used in the
312 section.

313

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

316 The sentence was rewritten as follows. “The COCO model demonstrated that the
317 sea-surface height was higher over the entire western Arctic basin and the maximum
318 height was located more to the western side of the basin in summer 2012 than those in
319 summer 2011.”