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Biogeosciences
Discussions

Interactive comment on “Factors influencing the stable carbon isotopic composition of suspended and sinking organic matter in the coastal Antarctic sea ice environment” *by* S. F. Henley et al.

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We would like to thank both anonymous referees for their constructive feedback. Their comments were helpful and have been addressed in order to improve the original manuscript. Page and line numbers detailed here in the Authors’ responses refer to the revised manuscript in Microsoft Word format, not the interactive discussion paper.

Anonymous referee #1

Comment: In general the described methods are appropriate and relevant to the manuscript’s specific objectives and are described in detail, showing the complexity of the analysis and the used instrumentation. However, it not describes the statistical analyses applied to obtained data in this investigation. These statistical analyses (correlations or regressions; multiple regressions; Model I or model II regressions; linear or non linear models) should be described and incorporated into the text.

Response: All statistical analyses have now been clarified in the text (e.g. page 18, lines 505 to 507) and the appropriate input values and results given in a summary table (page 38).

Comment: I suggest also, indicate in the text the used statistical software and/or graphics software.

Response: A section has now been added to the materials and methods section describing the statistical software (page 10, lines 262 to 265).

Comment: The figures are appropriate and pertinent with the presented results; however the graphics seem to concentrate a lot of information. In this sense, it might be advisable to use some summary tables (e.g.- regressions results: independent and dependent variable, intercept, slope, r2, p-value, etc)

Response: As recommended, a summary table has now been added (as for the first comment). Slope and intercept of regression lines have not been given, as they are not relevant to the arguments made in the discussion.

Anonymous referee #2

Comment: I recommend that the authors provide details on how they measured pH and at what level of precision.

Response: More detail has now been added explaining how we measured pH (page 5, line 117 to page 6, line 122 – sea ice sample collection; page 7, lines 155 to 158 – surface water sample collection; page 8, lines 200 to 203 – pH measurement and precision). We have described that samples for pH were taken into 250 ml glass biological oxygen demand (BOD) bottles that were immersed in the sea ice sack hole or were overflowing when being filled from the Niskin bottle. This ensured that no air bubbles were included and the bottle was sealed immediately with a ground glass stopper. On return to the laboratory, samples were stored, unfiltered, in the dark overnight to allow them to reach room temperature for analysis on the following day. pH measurements were performed on the day following sample collection using a bench-top pH meter calibrated to buffer solutions of pH 4.01, 7.00 and 10.01. We have pointed out that the maximum error on triplicate pH measurements across all samples was ± 0.02. The CO2SYS programme used to calculate [CO2 (aq)] corrects measured pH and alkalinity at the temperature of the sample during analysis to ambient temperature in the surface ocean at the time of collection and is thus representative of in situ conditions.

Comment: Error bars should also be provided for their estimates of [CO2].

Response: Maximum error on [CO2 (aq)] calculations, taking into account the maximum error on all input parameters (pH, alkalinity, temperature during analysis and corresponding ocean temperature, salinity), is 11.0 % (as explained at page 8, lines 206 to 208). Appropriate error bars have been added to figures that display [CO2 (aq)] data (Figs 3 and 8). An explanation of these error bars has also been added to the relevant figure captions and error estimates have been added to the text where [CO2 (aq)] values are quoted (e.g. page 11, lines 291 to 293). Importantly, 11 % error does not change the scientific story that [CO2 (aq)] cannot account for the observed changes in δ13CPOC.

Comment: Paragraphs between 11059/line 6 and 11060/line3 Here the authors are speculating on why d13C is so much lower in P. inermis. I find this discussion difficult to follow. First we are told that CCM could be important. But then we are told that CCM produces heavy d13C and therefore P. inermis is not using CCM. Instead, the mixed assemblages that came before did use CCM accounting for their higher d13C. OK so far, although I am not sure on what basis the latter suggestion was made. But then we are told that any of the known CCM cannot explain the d13C of these mixed assemblages. The following paragraph seems to say that we don't know why P. inermis is so different, which is fair enough and I would leave it at that. I think the two previous

paragraphs should be removed or clarified.

Response: We understand that the discussion of possible reasons for low δ13CPOC in P. inermis was difficult to follow. We have now restructured the paragraphs to explain that P. inermis has a low δ13CPOC signature because this species does not employ a CCM and perhaps due to a contribution from isotopically light "Proboscia lipids" (Sinninghe Damsté et al., 2003) (page 19, lines 545 to 558). Then we have gone on to discuss the potential of CCMs and direct bicarbonate uptake to increase δ13CPOC and have thus speculated that some diatom species in the mixed assemblage may employ CCMs (page 19, line 560 to page 20, line 571). Discussion of alternative CCMs that cannot account for higher δ13CPOC has been removed to avoid confusion.

Comment: Last conclusion (11067 line 15-16): "This study [. . .] highlights the need for parallel analysis of diatoms assemblages to reliably interpret sedimentary d13C POC records" I disagree. I think that the fundamental observation of this study for paleoceanographers is not just that shifts in diatom assemblages can have a profound effect on d13C-POC, but also that d13C-POC can be changed by diatom species whose frustules are not preserved in sediment. Therefore, combining d13C-POC with diatom assemblages will not help. The conclusion, which I think should be highlighted in this paper, is that the only way to stand a chance at interpreting correctly the d13C-OM record is to look at diatombound OM. Then, we would know what diatom species we are dealing with for a more accurate interpretation of the d13C record.

Response: We accept the reviewer's comment about parallel analysis of diatom assemblage not being helpful in the context of this study and have now incorporated the suggestion of the utility of diatom bound δ13CPOC. We have added that species-specific δ13CPOC may also be useful for the interpretation of sedimentary paleoarchives (page 27, lines 807 to 809).

Comment: 11043 line 24: I don't understand why Kienast et al. (2001) is the reference of choice for ice core record of pCO2.

Response: We agree with the reviewer that Kienast et al. is not the best reference for ice-core pCO2. This has now been removed as the reference of choice, and replaced with Berner et al., 1980, Barnola et al., 1987 and Masson-Delmotte et al., 2010 (page 3, lines 72 to 73).

Comment: 11045 line 13: what is a BOD bottle?

Response: The definition of a BOD (biological oxygen demand) bottle has now been clarified in the text (page 5, line 118).

Comment: 11049 line 11: which method was used to remove the swimmers?

Response: Swimmers were removed from sediment trap cups manually using HCl-cleaned plastic forceps and a binocular microscope. This has now been added to the text (page 9, lines 250 to 252).

Comment: 11058 line 14: I don't understand the meaning of "generic effect" of changes in diatom species composition

Response: We understand that the term "generic effect" of diatom species composition is confusing. We have now clarified that we are arguing that species-specific effects on δ13CPOC are not exerted by all diatom species in Ryder Bay. Instead, we argue that it is the unusual biochemistry of P. inermis that drives distinct negative shifts to δ13CPOC values as low as -29 ‰ in this study (page 18, lines 517 to 522).

Comment: 11060 line 21: Show how the contribution of ice algae was calculated (I guess two end members mixing, but how do they establish the end-members?)

Response: As recommended, we have now included an explanation of the calculation for estimates of sea-ice contribution to POM (page 20, line 590 to page 21, line 601). In the interest of keeping this explanation brief, we have used one value for seawater ($\sim$ minimum across both seasons) and have amended the estimates accordingly. This widens the range of our estimates of the proportion of sea-ice material, supporting our further discussion of the factors affecting the δ13CPOC signature of sea-ice and its

importance for δ13CPOC in surface waters and underlying sediments.

Interactive comment on Biogeosciences Discuss., 8, 11041, 2011.