Seasonal dynamics of megafauna on the deep West Antarctic Peninsula shelf in response to variable phytodetrital influx

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This article was transferred in from another journal without peer review.

Review History

RSOS-140092.R0 (Original submission)

Review form: Reviewer 1

Is the manuscript scientifically sound in its present form?
No

Are the interpretations and conclusions justified by the results?
No

Is the language acceptable?
No

Is it clear how to access all supporting data?
I did not notice any direction to the data or any supplementary material.

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
Yes
Recommendation
Major revision is needed (please make suggestions in comments)

Comments to the Author(s)
This paper examines the variations seen in time-lapse photographs of the seafloor in the WAP region. The authors use results to suggest the density, activity and impact of these fauna varies seasonally. They also argue that a particular species is a “keystone” species in part because of its observed dominance in the variations. While this paper presents interesting results I cannot recommend it for publishing without substantial revision. In particular the paper needs a better description of sensitivity of the methods to possible altered findings as well as a clearer definition of keystone attributes (and/or hypothetical framework to really test for the presence of a keystone). I expect that these can be addressed.

Line 81: Thistle et al. had suggested that megafauna can increase bioturbation mixing of food from the sediment surface to deeper layers, so I don’t think it’s simply a case of megafauna limit (reduce) food to deeper layers as the authors seem to imply.

Line 88: The paper would benefit from a rewrite of the objectives paragraph indicating what is to be tested/questioned. The use of the phrase ‘keystone’ is particularly troubling because it is not defined and yet also features in the abstract and conclusions. Nor are tested attributes of keystone species given. For example, a keystone definition might suggest that if the species was gone the system would fundamentally change. Elasiod species are known to change in dominance over longer time periods than examined here. Thus indicating that “keystone” species might not mean the same thing here as, for example, that specific kelp species form kelp forests are a keystone. The results re: the food bank concept is interesting. The introduction does not however give much a priori set of expectations about food bank mechanisms in way that later supports specific food back theory.

Line 130-132: This sentence is not clear and the first phrase could even be deleted.

Line 134: The authors might show a figure indicating how much of the original images was cropped to illustrate the lighting issue etc.

Line 145: It would be much more straightforward to use units of individuals per m2. Also it should be clarified somewhere that the density measure is subject to activity bias. That is, the more active the animals are, the more often they will be seen by the camera. Also I did not notice any statements confirming that the visibly of the fauna was assumed to be equal at the front of the image vs. the back or side.

Line 150: One could delete the word “likely” here, right? It would also be worthwhile to point out that these fauna can have irregular movement patterns but there method assumes only straight line motion from one frame to the next...another key source of under estimation.

Line 155: The formulas for the volume of the fecal measurements should be clearer, perhaps also showing on the example figure how the measurements were made. There are multiple shapes shown and I’m not sure how they were translated to volume, or how much it matters how this calculation was done to the final outcome.

Line 161: Here too I’m not sure of the importance of this assumption or how the authors came to believe it to be a good assumption. It’s also unclear to me how much the results rely on this assumption. In other works that have looked at such behaviour in time-lapse imagery time, a megafauna tracking was used that did not rely on such an assumption (sensu Bet et al. 2001). Here, the authors go on to describe a ‘rewrorking’ formula framework which is then used to later say that the reworking time is ~287 days for P murrayi and then that is used as evidence to suggest it is a keystone species. This seems to be a potentially confusing metric as it will not
actually take 287 days, unless P murrayi is the only species, which it is not. Would it not be sufficient to simply calculate the % contribution of that species to fecal cast volume? And, if I have this right, they determined the species that left each fecal cast? How good an assumption is that? There is no mention of methodology for the POC data that is later presented.

Lin 317: I’m not sure how the reworking calculation might be affected by different thickness of phytodetritus, if at all.

Line 199: How much of this increased density observation might be related to activity bias?

Figure 3: One might make it clearer when there are zeros vs no data.

Some/all of the many ANOVA stat results might be consolidated to a table. SD and SE are given in tables/figs.

Figure 5 The y axes should be labelled. The phytodetritus score points seem to be binned in a way that I was not expecting. Gridlines are usually not used in such figures for publication. The scale for the scoring should have major tick marks as whole numbers, right?

Line 304: Given that the paper is really about seasonality and only uses 15 months of data, the authors might reduce the amount of speculative interannual discussion to focus more on the core results. How unique are the findings of seasonal variation in activity, feeding etc.?

Line 206: I’m not really sure what ‘high variability’ is meant to say here b/c there was not really a good explanation of what high or low means in this context.

Line 359: See previous comment re: megafauna mixing sediments vs. consuming detritus from line 81

Line 368: Might just say here that the fauna likely go back over old tracks and fecal casts, ‘retracking’ might be overly confusing jargon.

Line 381: The authors mention that the species “should be included in models of biogeochemistry and climate change”. While I appreciate that the authors would like to make a point about the importance of their finding to how models work, they are far from making a convincing argument that such models should/could have this species or even holothurians/echinoderms per se in such models. I don’t think such a statement will foster serious dialogue without any kind of statements as to what might actually be modelled and if it is feasible.

Review form: Reviewer 2 (Laura Grange)

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes

Is it clear how to access all supporting data?
No supplementary data are provided. Access to additional data would provide support for the authors’ case in certain sections of the manuscript - please see comments in attached document.
Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
Yes

Recommendation
Accept with minor revision (please list in comments)

Comments to the Author(s)
I am happy to identify myself as a reviewer of the article. Please see the attached for my comments (Appendix A.). Dr Laura Grange School of Ocean and Earth Science Southampton University.

Decision letter
28-Jul-2014

Dear Dr Sumida,

Manuscript ID RSOS-140092 entitled “Seasonal dynamics of epibenthic megafauna on the deep West Antarctic Peninsula in response to variable phytodetrital influx” which you submitted to Royal Society Open Science, has been reviewed. The comments from reviewers are included at the bottom of this letter. In view of the criticisms of the reviewers, the manuscript has been rejected in its current form. However, a new manuscript may be submitted which takes into consideration these comments. Please note that resubmitting your manuscript does not guarantee eventual acceptance, and that your resubmission will be subject to peer review before a decision is made. You will be unable to make your revisions on the originally submitted version of your manuscript. Instead, revise your manuscript and upload the files via your author centre.

Once you have revised your manuscript, go to http://mc.manuscriptcentral.com/rsos and login to your Author Center. Click on "Manuscripts with Decisions," and then click on "Create a Resubmission" located next to the manuscript number. Then, follow the steps for resubmitting your manuscript. Your manuscript should be submitted within 6 months, but preferably sooner. If you are unable to submit by this date please contact the Editorial Office. I look forward to a resubmission.

Sincerely,
Charlotte Wray
Senior Publishing Editor,
Royal Society Open Science
openscience@royalsociety.org

Author’s Response to Decision Letter for (RSOS-140092)

Please see Appendix B.

RSOS-140294.R0

Review form: Reviewer 1

Is the manuscript scientifically sound in its present form?
Yes
Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes

Is it clear how to access all supporting data?
I did not find this.

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
No

Recommendation
Accept with minor revision (please list in comments)

Comments to the Author(s)
Most of the responses are fine with minor remarks below. Some fonts in figure 1 are obviously too small I’m still confused as to why the authors resist using units of ind. m-2, which is not an extrapolation, and is standard practice and would not change the observed variance or density. It’s really a style point that the editors can decide on as far as I’m concerned. Also, in the case where fauna remain in the scene for more than one consecutive image, it is often the case that one can determine if it is the same animal by its trail marks or some other feature (and thus avoid double counting, which the authors surely know). That being said, the key thing is that it can very well be true that activity can sometimes bias such results and it should not be treated as trivia. Line 166: The bottom line is that the calculated reworking time is indeed sensitive to this assumption. Somewhere they could acknowledge that this sensitivity exists and that while it may not change the relative reworking time, it could affect the absolute value of the estimate. From what is written, the best evidence that the result might be robust comes from the fact that the calculated times vary by a factor of about ten, and that there is no evidence to suggest that thickness might vary anywhere near that much, if at all. The response to comment is incomplete missing replies the last ~1/3 of review 2 comments. The issue of if or how to include a single species in a biogeochemical model is a matter of considerable contention and I do not find the current assertion that it ‘should’ be done compelling as there is no context for what tradeoffs might be done in terms of model complexity to account for that or what the purpose of such a model might be. Biogeochemical models, generally speaking, rely on functional groups. Is the natural evolution of their suggestion that we have species specific models that change depending on the dominate species from place to place (or from time to time as fauna can change dominance)? Could they at least please attempt to describe what such a model might estimate?

Review form: Reviewer 2 (Laura Grange)

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes
Is it clear how to access all supporting data?
No supplementary data are provided. Access to additional data would provide support for the authors’ case in certain sections of the manuscript - please see comments in attached document (Appendix C).

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
Yes

Recommendation
Accept with minor revision (please list in comments)

Comments to the Author(s)
I have indicated that I feel the manuscript is scientifically sound and that the authors’ interpretations and conclusions are justified by their results. I do however still have some concerns regarding the manuscript, which I believe need addressing. I have listed these concerns in the attached document. As before, most of these concerns are relatively minor, and therefore I do not feel it necessary to answer “no” to any of the above statements regarding scientific quality. My main reservations are still with the authors’ statistical approach. I am not an accomplished statistician, however the significant findings they present are not justified by the ANOVA/Kruskal-Wallis test results alone. The outcome of post hoc testing needs to be included so as specific pairwise differences can be supported. I have included detailed comments on this in the attached document. I originally queried the notable disparity between the data and associated error bars displayed in Figure 6, and the strongly significant result between one of the sampling months and all other sampling intervals. The authors’ response that “Despite the look of the graph, the results show significant differences between the data” may be true, however that is not justification for the approach used or an explanation of the disparity between the overlapping data distributions presented in Figure 6 and the apparent statistical significance. This result might be in error if the wrong test has been used or the data do not satisfy the test assumptions. I think this needs further thought and attention (see attached document). This revised version is an improvement on the original submission. Once again, I expect the authors to be able to address all of my comments and concerns, and therefore anticipate the paper will be of a high standard after revision and suitable for publication (after the statistics are addressed). I hope my comments have been useful.

Second decision letter

14-Oct-2014

Dear Dr Sumida

On behalf of the Editor, I am pleased to inform you that your Manuscript RSOS-140294 entitled “Seasonal dynamics of epibenthic megafauna on the deep West Antarctic Peninsula in response to variable phytdetrital influx” has been accepted for publication in Royal Society Open Science subject to minor revision in accordance with the referee suggestions. Please find the referees' comments below. The reviewers and Subject Editor have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the comments and revise your manuscript. Because the schedule for publication is very tight, it is a condition of publication that you submit the revised version of your manuscript within 7 days. If you do not think you will be able to meet this date please let me know immediately.

To revise your manuscript, log into http://mc.manuscriptcentral.com/rsos and enter your Author Centre, where you will find your manuscript title listed under “Manuscripts with
Decisions”. Under “Actions,” click on “Create a Revision.” You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre. When submitting your revised manuscript, you will be able to respond to the comments made by the referees and upload a file “Response to Referees” in “Section 6 - File Upload”. You can use this to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the referees. When uploading your revised files please make sure that you have:
1) A text file of the manuscript (tex, txt, rtf, docx or doc), references, tables (including captions) and figure captions. Do not upload a PDF as your “Main Document”
2) A separate electronic file of each figure (EPS or print-quality PDF preferred (either format should be produced directly from original creation package), or original software format)
3) Included a 100 word media summary of your paper when requested at submission. Please ensure you have entered correct contact details (email, institution and telephone) in your user account
4) Included the raw data to support the claims made in your paper. You can either include your data as electronic supplementary material or upload to a repository and include the relevant doi within your manuscript
5) Included your supplementary files in a format you are happy with (no line numbers, vancouver referencing, track changes removed etc) as these files will NOT be edited in production

Once again, thank you for submitting your manuscript to Royal Society Open Science and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes
Charlotte Wray
Senior Publishing Editor
openscience@royalsociety.org

Author’s Response to Decision Letter for (RSOS-140294)

See Appendix D.
Currently, little is known about the response of elasipod holothurians to the seasonal deposition of phytodetritus on the Antarctic continental shelf. This article attempts to address the question of whether the deposit-feeding megabenthos play an integral role in the recycling of organic carbon through their processing of Antarctic shelf sediments. In an attempt to answer this question, the authors investigate seasonality in megafaunal abundance, individual and population fecal production rates, and sediment reworking time using time-lapse photography at a soft sediment, midshelf site from 5 consecutive, 3-month sampling periods on the West Antarctic Peninsula (WAP). The elasipod holothurians, Protelpidia murrayi and Peniagone vignoni, comprised the majority of the epibenthic megafauna observed; the highest number of individuals being recorded in the austral summer. The authors suggest an increased frequency of fecal casts over the same time period as indicative of intensified feeding activity in direct response to increasing amounts of phytodetritus and/or bio-available organic material at the seafloor. The time needed to rework a square meter of sediment is used as an indicator of turnover, and the capacity of P. murrayi to process high volumes of sediment. The authors conclude that these holothurians act as a “filter”, modulating the distribution and assimilation of organic matter at continental shelf depths on the WAP.

This paper represents an interesting study that adds to the limited knowledge base regarding holothurian responses to seasonal phytodetrital flux on the Antarctic continental shelf. The authors quite rightly point out the implications of such a study, especially given the global abundance of elasipod holothurians at bathyal depths and their likely role in the recycling of organic carbon and modulation of biogeochemical cycles. If the authors are correct, publication of this manuscript is warranted, especially given the fact that the WAP is currently undergoing a period of rapid warming, which is likely to impact the relationship between productivity in the surface ocean and ecological processes at the seafloor.

I do however suggest there are some sections of the manuscript that warrant improvement, notably the figures (specifically the authors’ attention to detail), and the discussion section, where a broader comment on holothurian fecal production rates versus POC flux applicable to the shelf as a whole (as opposed to a single site) would be useful. Please see my detailed comments below. I also suggest the authors consider their statistical approach. Results from ANOVA and Kruskal-Wallis tests are used by the authors to identify pairwise differences between sampling periods as opposed to highlighting any overall differences in the entire data set. Pairwise comparisons (e.g. Tukey comparisons and Mann Whitney) should be used to identify where differences between individual sampling periods occur. In addition, based on my interpretations there appears to be disparity between the statistically significant results shown and the variance/overlap between data for different sampling periods (e.g. see Figure 6 and Kruskal-Wallis results for population fecal production rates; see comments below).
With revision, this study will be of interest to marine invertebrate biologists, marine benthic ecologists and should be of interest to specialists concerned with carbon cycling and the role of deposit-feeding megabenthos in biogeochemical processes in polar regions.

**The following information may be useful in preparing a revised version of the manuscript.**

(Line numbers according to formatting in original Word document unless otherwise stated, e.g. increasing chronologically throughout the document)

**Page 2, Lines 35-36**
Mention is made of seafloor images indicating “variability in the abundance and size of depositing phytodetrital particles”. There is no quantitative data however to support this statement. The amount of phytodetritus coverage is only qualitatively described through seafloor photographs using an ordinal scale, i.e. no phytodetritus visible (score 0), diffuse phytodetritus (score 1), 50-90% seafloor covered by phytodetritus (score 2) and > 90% of seafloor covered by phytodetritus (score 3). There is no mention in the manuscript of phytodetritus “abundance” or “particle size”. I suggest rewording this to “variability in the coverage of depositing phytodetritus material” or similar.

**Page 2, Lines 47-48**
Mention is made of *Protelpidia murrayi* being a “keystone species for organic-carbon recycling on the deep WAP shelf”. Data however are only presented from one site in a single location on the shelf. I do not necessarily disagree with this statement, however I think this argument needs prefacing and could be strengthened by additional information being provided in the discussion. For example, data exists from several sites both across and along the WAP shelf detailing elasipod holothurian abundance and POC flux. Assuming fecal production rates are similar across the shelf for different rates of POC flux for this species, the authors could comment on what this might imply for other areas on the shelf based on these data. This additional information, although speculative, would add context.

**Page 4, Lines 86 and 90**
Mention is made of “foraging rates” and “feeding rates”, however this study does not strictly measure foraging or feeding rate. The data collected are for defecation or fecal production rate only, and therefore “feeding rates” can only really be inferred. Given this, it might be more appropriate that discussion be limited to “fecal production” rates where possible.

**Page 4, Line 108**
Remove “(during FB-II)”.

**Page 5, Line 115**
Sample month/period information would be useful and could be provided in Table 1, e.g. Dec 99 – Mar 00 etc.
Mention is made of “appropriate light conditions”. This statement could be elaborated on to make Table 1 more standalone. What does “appropriate” mean? Does this statement refer to ambient “light” conditions or is this statement in reference to the area of each photograph that is well lit?

Were animals on both soft and hard substrates identified, or were animals only counted and described on soft sediment? A statement to this effect would be useful.

Add details of station A-C into figure legend. The authors also, need to specify what the green symbol (inverted triangle) is in Figure 1 in either the figure legend and/ or key. Text on main map could also be bigger/ more legible.

Mention is made of measuring fecal cast numerical densities and epifaunal densities. It is however very likely that the authors were unable to capture the “true” number (total) of megafauna in the field of view as images/ frames were only recorded once every 12 or 24 hours depending on the sampling period. Consequently, fecal casts tracked and counted in each frame will not have been necessarily produced by the holothurians captured on film. As a result individual fecal production rates are likely to be overestimated (if we are taking these rates to be representative of the megafauna actually inhabiting the patch of seafloor observed). I appreciate this “error” is inherent given the sampling restrictions etc, however some acknowledgement and discussion of this issue in the text would be useful.

Testing was done to assess for “homogeneity of variance”. What tests? Results of these tests could be provided in supplementary material. Was heterogeneity also assessed using graphical techniques, e.g. a graphical inspection of the residuals as recommended by Zuur et al., (2009)? It might be useful to do this, as some tests for heterogeneity can be quite sensitive to non-normality.

A brief explanation of how individual fecal production rate and population fecal production rate are calculated would be useful here (before “sediment-reworking time” is explained).

Replace “Therefore, it...” with “Therefore, this measurement represents......” or something similar.

I am not overly convinced by the statistical approach used. See later comments.
Figure 2; scale bars needed in seafloor photographs and inset panels E and F could be larger. Whole animal photographs of the holothurians responsible for the fecal casts illustrated in panels E and F would be useful here.

Page 8, Line 172
“Note the presence of a very small, greenish phytodetritus at...”, needs to be changed to “Note the presence of a thin layer of phytodetritus material at ...” or something similar.

Page 9 (line 18 on pdf)
Table 2; “Total” should read “Megafauna Total” for consistency.

Page 9, Lines 201-202
Is “24 ind. m$^{-2}$” in parentheses the standard deviation? If yes, then the use of this notation to indicate SD should be pointed out in the text. I appreciate the use of parentheses as indicative of SD is already explained in the header row of Table 2, however it would be useful to have this information in the main text as opposed to having it referenced in Table 2 only.

Page 10
Figure 3; There are no axes labels and the horizontal gridlines are not needed. I would also personally prefer to see a plot of mean and variance as opposed to total megabenthic density. Is there any reason why the total data were plotted as opposed to a measure of the central tendency and variance? I think the latter would demonstrate the distribution of the data more effectively.

Page 10, Line 212
The range and mean of \textit{P. murrayi} density would be useful here.

Page 10, Line 213
I am not sure of the statistics here. According to the authors, ANOVA is used to test the difference between the density of \textit{P. murrayi} observed during Jun 00-Oct 00 and the other sampling periods. The difference between sampling periods is claimed to be significant; the density being significantly lower in Jun 00–Oct 00 compared to the other sampling periods. I would however assume that an ANOVA can only be used to highlight that there is a statistically significant difference somewhere in the entire model and that Turkey (pairwise) comparisons then need to be undertaken to highlight where those differences are. Similarly for Kruskal-Wallis, I would assume a KW test can only be used to test the entire model, after which pairwise differences need to be tested using multiple Mann Whitney U tests accounting for Bonferroni correction. There are several places in the manuscript where this approach is used.

Page 10, Line 216
“densities in Nov 99 – Mar 00”; should this read “densities in Dec 99 – Mar 00” as in Table 1 and 2?

Page 11, Line 230
What does the “N” number represent? Number of frames?
Page 11, Line 233
ANOVA results should be in italics for consistency.

Page 11, Lines 229-235
Did the authors look at movement rates in relation to holothurian size? Could body size be a confounding factor?

Page 12 (line 23 on pdf)
“No00-Mar01” should read “Nov 00-Mar 01”.

Page 12 Line 242
More detailed information on the fecal cast volumes recorded for both holothurians would be useful (include possibly as a table similar to Tables 2 and 3 in either the main manuscript or as supplementary material).

Page 13
Figure 5; There are no axes labels and the horizontal gridlines are not needed. If the amount of phytodetritus is being scored according to a scale between 0 and 3, why are there data points at 1.5 and 2.5? I assume these data points correspond to halfway points on the ordinal scale. Do these categorizations need to be clarified in the figure legend?

Page 14
Figure 6A and 6B; the x-axis reads “Nov 99-Mar 00”. Should the x-axis instead read “Dec 99-Mar 00” as in Figures 3 and 4, and in Tables 1 and 2?

Page 14, Line 293
In addition to my comments on the statistics used elsewhere in the manuscript, I am not 100% convinced by the statistical interpretation of data on Page 14. The authors report a highly significant difference in the Nov 00-Mar 01 population fecal production rates compared to all previous sampling periods. I find this result however quite unusual given the appreciable error bar evident in Figure 6B (Nov 00-Mar 01). There is clearly high variance in these data. Could the high statistical significance be the result of a large sample size and/or an inappropriate statistical approach? A similar pattern is seen in Figure 6A, i.e. extremely large error bars. The heterogeneity in these samples is ecologically interesting and therefore needs to be retained, however a more appropriate statistical approach might resolve the apparent disparity between the statistical results and the error bars in Figure 6.

Page 15
Figure 7; Why is there no measure of mean or variance associated with these data (reworking time)?

Page 16, Line 338
“This is indicated...” should read “This response is indicated...” or something similar.
Page 15-18
Some general discussion on what these data mean for the Antarctic continental shelf as an entire area would be useful, and would provide context for the statement that “P. murrayi is a keystone species for organic carbon cycling”. Refer to earlier comments (Page 2, Lines 47-48).

All in-text citations are included in the reference list.
Response to RSOS Review MS ID RSOS-140092

Dear Editor,

Please find below the answer to the reviewers comments. I hope the ms is suitable for publication in RSOS now.

The authors.

Reviewer 1 comments:

Page 2, Lines 35-36 Mention is made of seafloor images indicating “variability in the abundance and size of depositing phytodetrital particles”. There is no quantitative data however to support this statement. The amount of phytodetritus coverage is only qualitatively described through seafloor photographs using an ordinal scale, i.e. no phytodetritus visible (score 0), diffuse phytodetritus (score 1), 50-90% seafloor covered by phytodetritus (score 2) and > 90% of seafloor covered by phytodetritus (score 3). There is no mention in the manuscript of phytodetritus “abundance” or “particle size”. I suggest rewording this to “variability in the coverage of depositing phytodetritus material” or similar.

Authors: OK. Changed.

Page 2, Lines 47-48 Mention is made of Protelpidia murrayi being a “keystone species for organic-carbon recycling on the deep WAP shelf”. Data however are only presented from one site in a single location on the shelf. I do not necessarily disagree with this statement, however I think this argument needs prefacing and could be strengthened by additional information being provided in the discussion. For example, data exists from several sites both across and along the WAP shelf detailing elasipod holothurian abundance and POC flux. Assuming fecal production rates are similar across the shelf for different rates of POC flux for this species, the authors could comment on what this might imply for other areas on the shelf based on these data. This additional information, although speculative, would add context.

Authors: OK. We agree with the reviewer and we removed the “keystone” concept throughout the text.

Page 4, Lines 86 and 90 Mention is made of “foraging rates” and “feeding rates”, however this study does not strictly measure foraging or feeding rate. The data
collected are for defecation or fecal production rate only, and therefore “feeding rates” can only really be inferred. Given this, it might be more appropriate that discussion be limited to “fecal production” rates where possible.

**Authors:** We have changed the text and include that we have measured the fecal production as a proxy for foraging rates in the WAP shelf.

**Page 4, Line 108** Remove “(during FB-II)”.

**Authors:** Removed.

**Page 5, Line 115** Sample month/period information would be useful and could be provided in Table 1, e.g. Dec 99 – Mar 00 etc.

**Authors:** OK. Table changed accordingly.

**Page 5, Line 117** Mention is made of “appropriate light conditions”. This statement could be elaborated on to make Table 1 more standalone. What does “appropriate” mean? Does this statement refer to ambient “light” conditions or is this statement in reference to the area of each photograph that is well lit?

**Authors:** These were periods when the flash unit functioned properly. We have included that information in the table footnote.

**Page 6, Line 136** Were animals on both soft and hard substrates identified, or were animals only counted and described on soft sediment? A statement to this effect would be useful.

**Authors:** We have clarified this question on the text:

“…The quantitative part of the study focused on the epibenthic megafauna living on soft sediments. During the course of the study the camera never faced hard bottoms. Due…”

**Page 6, Line 140** Add details of station A-C into figure legend. The authors also, need to specify what the green symbol (inverted triangle) is in Figure 1 in either the figure legend and/ or key. Text on main map could also be bigger/ more legible.
**Authors:** We have made a new map.

**Page 6, Line 152** Mention is made of measuring fecal cast numerical densities and epifaunal densities. It is however very likely that the authors were unable to capture the “true” number (total) of megafauna in the field of view as images/frames were only recorded once every 12 or 24 hours depending on the sampling period. Consequently, fecal casts tracked and counted in each frame will not have been necessarily produced by the holothurians captured on film. As a result individual fecal production rates are likely to be overestimated (if we are taking these rates to be representative of the megafauna actually inhabiting the patch of seafloor observed). I appreciate this “error” is inherent given the sampling restrictions etc, however some acknowledgement and discussion of this issue in the text would be useful.

**Authors:** While we appreciate the comments of the reviewer, we actually did know which fecal cast belong to which individual holothurian in most cases. In fact, Individual fecal production was calculated only for individuals we did know the fecal casts.

**Page 7, Line 158** Testing was done to assess for “homogeneity of variance”. What tests? Results of these tests could be provided in supplementary material. Was heterogeneity also assessed using graphical techniques, e.g. a graphical inspection of the residuals as recommended by Zuur et al., (2009)? It might be useful to do this, as some tests for heterogeneity can be quite sensitive to non-normality.

**Authors:** For ANOVA we used the post-hoc test of Tukey and for Kruskall, we applied the Dunn test, in order to identify which data differed significantly. We have included the information in the text.

**Page 7, Lines 159-160** A brief explanation of how individual fecal production rate and population fecal production rate are calculated would be useful here (before “sediment- reworking time” is explained).

**Authors:** OK. Included.

**Page 7, Line 165** Replace “Therefore, it...” with “Therefore, this measurement represents......” or something similar.

**Authors:** OK. Changed.
Page 7, Line 166 I am not overly convinced by the statistical approach used. See later comments.

Authors: See above.

Page 7, Line 169

Figure 2; scale bars needed in seafloor photographs and inset panels E and F could be larger. Whole animal photographs of the holothurians responsible for the fecal casts illustrated in panels E and F would be useful here.

Authors: OK. Included.

Page 8, Line 172 “Note the presence of a very small, greenish phytodetritus at...”, needs to be changed to “Note the presence of a thin layer of phytodetritus material at ...” or something similar.

Authors: OK. Changed.

Page 9 (line 18 on pdf) Table 2; "Total" should read “Megafauna Total” for consistency.

Authors: OK. Changed.

Page 9, Lines 201-202 Is “24 ind. m⁻²” in parentheses the standard deviation? If yes, then the use of this notation to indicate SD should be pointed out in the text. I appreciate the use of parentheses as indicative of SD is already explained in the header row of Table 2, however it would be useful to have this information in the main text as opposed to having it referenced in Table 2 only.

Authors: OK. Changed.

Page 10 Figure 3; There are no axes labels and the horizontal gridlines are not needed. I would also personally prefer to see a plot of mean and variance as opposed to total megabenthic density. Is there any reason why the total data were plotted as opposed to a measure of the central tendency and variance? I
think the latter would demonstrate the distribution of the data more effectively.

Authors: We accepted the comment and modified figures 3 and 5

Page 10, Line 212 The range and mean of P. murrayi density would be useful here.

Authors: OK. Included.

Page 10, Line 213 I am not sure of the statistics here. According to the authors, ANOVA is used to test the difference between the density of P. murrayi observed during Jun 00-Oct 00 and the other sampling periods. The difference between sampling periods is claimed to be significant; the density being significantly lower in Jun 00–Oct 00 compared to the other sampling periods. I would however assume that an ANOVA can only be used to highlight that there is a statistically significant difference somewhere in the entire model and that Turkey (pairwise) comparisons then need to be undertaken to highlight where those differences are. Similarly for Kruskal-Wallis, I would assume a KW test can only be used to test the entire model, after which pairwise differences need to be tested using multiple Mann Whitney U tests accounting for Bonferroni correction. There are several places in the manuscript where this approach is used.

Authors: We revised statistics throughout the text to adequate to the reviewers comments when applicable. However, our original analyses had no problem. Reviewer 1 was mainly concerned with the post-hoc tests, which we did but forgot to formally mention in the text.

Page 10, Line 216 “densities in Nov 99 – Mar 00”; should this read “densities in Dec 99 – Mar 00” as in Table 1 and 2?

Authors: In reality it is Nov 99-Mar 00. We have corrected this in the text and Figs/Tables.

Page 11, Line 230 What does the “N” number represent? Number of frames?

Authors: It refers to the number of individual holothurians measured.

Page 11, Line 233 ANOVA results should be in italics for consistency.

Authors: OK.
Page 11, Lines 229-235 Did the authors look at movement rates in relation to holothurian size? Could body size be a confounding factor?

Authors: The data on holothurian movement rates is very restricted since we did not have many individuals that we could track in the videos. Body size could be a factor and we did not look at this. However, we observed both small and large individuals apparently moving fast in frames.

Page 12 (line 23 on pdf) “No00-Mar01” should read “Nov 00-Mar 01”.

Authors: OK.

Page 12 Line 242 More detailed information on the fecal cast volumes recorded for both holothurians would be useful (include possibly as a table similar to Tables 2 and 3 in either the main manuscript or as supplementary material).

Authors: We do not think it is necessary to include yet another Table. Besides, we only have fecal cast data for one species of holothurian.

Page 13 Figure 5: There are no axes labels and the horizontal gridlines are not needed. If the amount of phytodetritus is being scored according to a scale between 0 and 3, why are there data points at 1.5 and 2.5? I assume these data points correspond to halfway points on the ordinal scale. Do these categorizations need to be clarified in the figure legend?

Authors: Figure changed.

Page 14 Figure 6A and 6B: the x-axis reads “Nov 99-Mar 00”. Should the x-axis instead read “Dec99-Mar00” as in Figures 3 and 4, and in Tables 1 and 2?

Authors: In reality it is Nov 99-Mar 00. We have corrected this in the text and Figs/Tables.

Page 14, Line 293 In addition to my comments on the statistics used elsewhere in the manuscript, I am not 100% convinced by the statistical interpretation of data on Page 14. The authors report a highly significant difference in the Nov00-Mar01 population fecal production rates compared to all previous sampling
periods. I find this result however quite unusual given the appreciable error bar evident in Figure 6B (Nov 00-Mar 01). There is clearly high variance in these data. Could the high statistical significance be the result of a large sample size and/or an inappropriate statistical approach? A similar pattern is seen in Figure 6A, i.e. extremely large error bars. The heterogeneity in these samples is ecologically interesting and therefore needs to be retained, however a more appropriate statistical approach might resolve the apparent disparity between the statistical results and the error bars in Figure 6.

**Authors**: Despite the look of the graph, the results show significant differences between the data.

**Page 15 Figure 7**; Why is there no measure of mean or variance associated with these data (reworking time)?

**Authors**: There are no replicates for this measurement.

**Page 16, Line 338** “This is indicated...” should read “This response is indicated...” or something similar.

**Authors**: OK.

Page 15-18 Some general discussion on what these data mean for the Antarctic continental shelf as an entire area would be useful, and would provide context for the statement that “*P. murrayi* is a keystone species for organic carbon cycling”. Refer to earlier comments (Page 2, Lines 47-48).

**Authors**: We changed this concept throughout the text.

Reviewer 2 comments:

**Line 81**: Thistle et al. had suggested that megafauna can increase bioturbation mixing of food from the sediment surface to deeper layers, so I don’t think it’s simply a case of megafauna limit (reduce) food to deeper layers as the authors seem to imply.

**Authors**: The reviewer is right, since there are other processes involved. However, the elasipods dealt in the present work very carefully eat the top
millimeter of the sediment surface and are likely to have little influence in sediment bioturbation and the burying of organic matter to deeper layers.

**Line 88:** The paper would benefit from a rewrite of the objectives paragraph indicating what is to be tested/questioned. The use of the phrase 'keystone' is particularly troubling because it is not defined and yet also features in the abstract and conclusions. Nor are tested attributes of keystone species given. For example, a keystone definition might suggest that if the species was gone the system would fundamentally change. Elasiod species are known to change in dominance over longer time periods than examined here. Thus indicating that “keystone” species might not mean the same thing here as, for example, that specific kelp species form kelp forests are a keystone. The results re: the food bank concept is interesting. The introduction does not however give much a priori set of expectations about food bank mechanisms in way that later supports specific food back theory.

**Authors:** We agree the keystone species is too strong a term and was then removed from the whole text.

**Line 130-132:** This sentence is not clear and the first phrase could even be deleted.

**Authors:** We did nod remove the sentence since we consider it important to understand how images were treated.

**Line 134:** The authors might show a figure indicating how much of the original images was cropped to illustrate the lighting issue etc.

**Authors:** Despite we could include a figure here, we believe it would be a waste of space and would not much information to the paper.

**Line 145:** It would be much more straightforward to use units of individuals per m2. Also it should be clarified somewhere that the density measure is subject to activity bias. That is, the more active the animals are, the more often they will be seen by the camera. Also I did not notice any statements confirming that the visibly of the fauna was assumed to be equal at the front of the image vs. the back or side.

**Authors:** We prefer to use the numbers we actually counted since extrapolations may not represent the reality. In relation to the density and animals activity, this is not always true. In fact, it may be opposite, since inactive animals will appear in consecutive frames, whereas active ones will come and go very fast.

**Line 150:** One could delete the word “likely” here, right? It would also be worthwhile to point out that these fauna can have irregular movement patterns but there method assumes only straight line motion from one frame to the next...another key source of under estimation.

**Authors:** OK. Deleted.
Line 155: The formulas for the volume of the fecal measurements should be clearer, perhaps also showing on the example figure how the measurements were made. There are multiple shapes shown and I’m not sure how they were translated to volume, or how much it matters how this calculation was done to the final outcome.

Authors: *Protelpidia murrayi* fecal casts are coiled. If you uncoil them, they may be represented as cylinders. What we have done was to measure the width of the fecal cast (i.e., the diameter of the cylinder) and its length (i.e., the height of the cylinder). We included the explanation in the text.

Line 161: Here too I’m not sure of the importance of this assumption or how the authors came to believe it to be a good assumption. It’s also unclear to me how much the results rely on this assumption. In other works that have looked at such behaviour in time-lapse imagery time, a megafauna tracking was used that did not rely on such an assumption (sensu Bet et al. 2001). Here, the authors go on to describe a ‘reworking’ formula framework which is then used to later say that the reworking time is ~287 days for *P. murrayi* and then that is used as evidence to suggest it is a keystone species. This seems to be a potentially confusing metric as it will not actually take 287 days, unless *P. murrayi* is the only species, which it is not. Would it not be sufficient to simply calculate the % contribution of that species to fecal cast volume? And, if I have this right, they determined the species that left each fecal cast? How good an assumption is that? There is no mention of methodology for the POC data that is later presented.

Authors: We think this is a perfectly good assumption. It is obvious other species consume organic matter in the WAP shelf. However, we wanted to show the potential of this particular holothurian, since we were able to track individual fecal casts.

Lin 317: I’m not sure how the reworking calculation might be affected by different thickness of phytodetritus, if at all.

Authors: We observed that holothurians modulate their feeding rates according to the availability of labile organic material. The thick carpets reported here were observed and measures from intact megacorer samples during the study period.

Line 199: How much of this increased density observation might be related to activity bias?

Authors: As it was explained above, one should observe the opposite.
Review of RSOS – 140294

I still have the opinion that this paper represents an interesting study that warrants publication. The evidence presented adds to the limited knowledge base regarding holothurian responses to seasonal phytodetrital flux on the Antarctic continental shelf, and addresses the potential importance of these animals at bathyal depths in biogeochemical and organic carbon cycling. The amendments undertaken by the authors have improved the manuscript, strengthened their arguments of interpretation and addressed many of my previous concerns/comments.

I am still however somewhat concerned with the presented statistical approach and the omission of details regarding the statistical tests used. The addition of information regarding post hoc testing is useful, e.g. Tukey and Dunn pairwise comparisons, however the results of Kruskal-Wallis and ANOVA tests are still being presented as evidence for specific pairwise differences between sampling months. A significant ANOVA/Kruskal-Wallis outcome cannot be used alone to confirm the results the authors present. A significant result from this type of test means you can only conclude that there is a significant difference between some of the sampling months, however not which months are different, i.e. the significant difference could be between any or all of the sampling months. To determine where the significance is, post hoc testing is needed. I assume this is why the authors undertook post hoc testing (as stated now in their Materials and Methods; page 165, line 7). If post hoc testing was undertaken why are the results of these tests not presented or at the very least referred to? I am not suggesting that the ANOVA/Kruskal-Wallis test results are invalid, however these data need to be followed by reference to the post hoc test that identified the significant pairwise difference or differences the authors highlight. In addition, all ANOVA/Kruskal-Wallis test results need to report degrees of freedom, alongside the test statistic and p-value, and sample size (N values) should be included for all analyses in the Materials and Methods section. For example, an N value of 452 epifaunal organisms and N = 2 to 14 individuals used for movement rate calculations are mentioned in the main text, however the N values used to determine fecal cast volume (Page 7, lines 156-157), individual fecal cast production rates (Page 7, lines 159-160) etc need to be clarified. Sample size information may also help clarify why there is a highly significant difference in the Nov00-Mar01 population fecal production rates compared to all previous sampling periods, despite the appreciable standard error bars presented in Figure 6B. This apparent disparity is something I still think needs reconciling.

After a thorough read through of the resubmitted manuscript I have the following suggestions for minor edits and improvement.

Page 6, line 142
Station locations in parentheses should read A-C, not A-B.

Page 6, line 153
I would agree with Reviewer 2 that some mention of this fauna’s tendency to move irregularly is needed, as this is likely to cause underestimation of movement rates. It is true that the authors acknowledge these distances represent minimal movement rates, however an additional sentence of clarification would be useful.

Page 7, line 163
Testing was done to assess for “homogeneity of variance”. What tests? Results of these tests could be provided in supplementary material. The authors responded to this query previously with information regarding post hoc testing (this information is needed so I am glad the authors have added this information to the manuscript). The authors however do not comment on the tests they used to test homogeneity of variance, e.g. Levene’s test?

Page 10, lines 206-209
A significant difference between Nov00-Mar01 and all other sampling months is reported. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Pages 11, lines 220-227
Significant differences in holothurian abundance and size are reported between specific sampling months. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Pages 11-12, lines 235-242
I asked the authors whether holothurian body size could be a confounding factor in movement rates. They responded positively, explained sample size was restricted and that both small and large individuals were observed moving quickly within frames. Maybe a couple of lines of text to that effect could be added to the manuscript to acknowledge the authors have considered body size as a factor, however are limited by sample number, but observations suggest a range of body sizes were sampled.

Page 12, lines 240-242
Significant differences in movement rates are reported between specific sampling months. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Page 12, lines 247-255
Significant differences in fecal cast volume are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).

Page 14, lines 273-282
Significant differences in fecal cast production are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).
Page 14, line 291
The source of the POC data is mentioned in the legend of Figures 6 and 7. It may be beneficial however to include a brief description of how these data were collected and/or reference to Smith et al., [9] in the Materials and Methods section.

Page 15, line 297
Significant differences in population fecal cast production rates are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).

Page 18, line 375
I would delete the word “extremely” here.
Response to RSOS Second Review MS ID RSOS-140092

Dear Editor,

Please find below the answer to the reviewers further comments.

The authors.

Reviewer 1 comments:

Concerning the statistics:

Authors: We included results of post hoc tests for all results cited in the text and requested by the reviewer.

Page 6, Line 142 Station locations in parentheses should read A-C, not A-B. Authors: Corrected.

Page 6, Line 153 I would agree with Reviewer 2 that some mention of this fauna’s tendency to move irregularly is needed, as this is likely to cause underestimation of movement rates. It is true that the authors acknowledge these distances represent minimal movement rates, however an additional sentence of clarification would be useful.

Authors: We have changed the text to: “Since these animals move irregularly, distances represent minimal movement rates and are likely to be underestimated, as we could not estimate the distance traveled (or direction of movement) from individuals that only appeared once in photographs.”

Page 7, Line 163 Testing was done to assess for “homogeneity of variance”. What tests? Results of these tests could be provided in supplementary material. The authors responded to this query previously with information regarding post hoc testing (this information is needed so I am glad the authors have added this information to the manuscript). The authors however do not comment on the tests they used to test homogeneity of variance, e.g. Levene’s test?
Authors: We used Bartlett’s test and included that information in the text.

Page 10, Lines 206-209 A significant difference between Nov00-Mar01 and all other sampling months is reported. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Authors: See Statistics comments above.

Page 11, Lines 220-227 Significant differences in holothurian abundance and size are reported between specific sampling months. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Authors: See Statistics comments above.

Page 11-12, Lines 235-242 I asked the authors whether holothurian body size could be a confounding factor in movement rates. They responded positively, explained sample size was restricted and that both small and large individuals were observed moving quickly within frames. Maybe a couple of lines of text to that effect could be added to the manuscript to acknowledge the authors have considered body size as a factor, however are limited by sample number, but observations suggest a range of body sizes were sampled.

Authors: We included the phrase: “We observed no influence from animal size on movement rates, but note that our sample size was small.”

Page 12, Lines 240-242 Significant differences in movement rates are reported between specific sampling months. Results of post hoc testing need to be reported in addition to ANOVA (see comments above).

Authors: See Statistics comments above.

Page 12, Lines 247-255 Significant differences in fecal cast volume are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).

Authors: See Statistics comments above.
Significant differences in fecal cast production are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).

Authors: See Statistics comments above.

The source of the POC data is mentioned in the legend of Figures 6 and 7. It may be beneficial however to include a brief description of how these data were collected and/or reference to Smith et al., [9] in the Materials and Methods section.

Authors: We included the text in the Material and Methods section: “Data on fecal casts and reworking times were compared with POC fluxes obtained by Smith et al. [9] during the same period.”

Significant differences in population fecal cast production rates are reported between specific sampling months. Results of post hoc testing need to be reported in addition to Kruskal-Wallis (see comments above).

Authors: See Statistics comments above.

I would delete the word “extremely” here.

Authors: Agreed.

Reviewer 2 comments:

Comment: Some fonts in figure 1 are obviously too small

Authors: Fonts increased.

Comment: I’m still confused as to why the authors resist using units of ind. m-2, which is not an extrapolation, and is standard practice and would not change the observed variance or density. It’s really a style point that the editors can decide on as far as I’m concerned.
Authors: We have made a thorough revision of figures, tables and text, and changed all to ind. m$^{-2}$ as suggested by the reviewer.

Comment: Fonts Also, in the case where fauna remain in the scene for more than one consecutive image, it is often the case that one can determine if it is the same animal by its trail marks or some other feature (and thus avoid double counting, which the authors surely know). That being said, the key thing is that it can very well be true that activity can sometimes bias such results and it should not be treated as trivia.

Authors: This was a common concern with reviewer 1 and we have put an explanation in the text (see response to reviewer 1 comments).

Comment: Line 166: The bottom line is that the calculated reworking time is indeed sensitive to this assumption. Somewhere they could acknowledge that this sensitivity exists and that while it may not change the relative reworking time, it could affect the absolute value of the estimate. From what is written, the best evidence that the result might be robust comes from the fact that the calculated times vary by a factor of about ten, and that there is no evidence to suggest that thickness might vary anywhere near that much, if at all.

Authors: We do not think our data on reworking times are super precise, but they show a ball park figure with is a good estimate of what is going on in the WAP shelf. Besides, it is very unlike precise numbers can be obtained given the complexity of the system. We think our data is a good estimate for global carbon cycling models.

Comment: The response to comment is incomplete missing replies the last ~1/3 of review 2 comments.

Authors: OK. Concerning the former comments missing a reply from us:

Reviewer 2: Figure 3: One might make it clearer when there are zeros vs no data.

Authors: We have already changed this figure by suggestion of reviewer 1 (pooled the data) and this is no longer a problem.

Reviewer 2: Some/all of the many ANOVA stat results might be consolidated to a table. SD and SE are given in tables/figs.

Authors: We did not want to increase the number of tables, and since the amount of stats numbers cited in the text is small, we decided to skip a
table. We shall include it on the supplementary material.

**Reviewer 2:** Figure 5 The y axes should be labelled. The phytodetritus score points seem to be binned in a way that I was not expecting. Gridlines are usually not used in such figures for publication. The scale for the scoring should have major tick marks as whole numbers, right?

**Authors:** This figure was also changed by suggestion of reviewer 1 (same reason) and these issues are no longer there.

**Reviewer 2:** Line 304: Given that the paper is really about seasonality and only uses 15 months of data, the authors might reduce the amount of speculative interannual discussion to focus more on the core results. How unique are the findings of seasonal variation in activity, feeding etc.?

**Authors:** We disagree with the reviewer, since interannual variability is an important issue in Antarctica as showed by Dave Karl and collaborators and other authors. Luckily we collected data with strong variability in two consecutive summers and the response in feeding rates was different. Therefore, we choose to keep this in the discussion.

**Reviewer 2:** Line 206: I’m not really sure what ‘high variability’ is meant to say here b/c there was not really a good explanation of what high or low means in this context.

**Authors:** We could not find this issue in Line 206. Perhaps the reviewer mistyped the line number.

**Reviewer 2:** Line 359: See previous comment re: megafauna mixing sediments vs. consuming detritus from line 81

**Authors:** On the Line 81 the reviewer comments that Thistle et al. suggest that megafauna can increase bioturbation mixing food from the sediment surface to deeper layers and challenges our view that activity of the studied holothurian might limit food to deeper areas. This is because the elasipod holothurian *Protelpidia murrayi* feeds very carefully on the top millimeter of the sediment causing little vertical mixing. By doing this, it limits the total amount of food other bioturbators (e.g. head-down maldanid polychaetes) may bury in the sediment.

**Reviewer 2:** Line 368: Might just say here that the fauna likely go back over old tracks and fecal casts, ‘retracking’ might be overly confusing jargon.

**Authors:** That is a commonly used word and that is also why we used it between parentheses.
Reviewer 2: Line 381: The authors mention that the species “should be included in models of biogeochemistry and climate change”. While I appreciate that the authors would like to make a point about the importance of their finding to how models work, they are far from making a convincing argument that such models should/could have this species or even holothurians/echinoderms per se in such models. I don’t think such a statement will foster serious dialogue without any kind of statements as to what might actually be modelled and if it is feasible.

Authors: See below.

Comment: The issue of if or how to include a single species in a biogeochemical model is a matter of considerable contention and I do not find the current assertion that it 'should' be done compelling as there is no context for what tradeoffs might be done in terms of model complexity to account for that or what the purpose of such a model might be. Biogeochemical models, generally speaking, rely on functional groups. Is the natural evolution of their suggestion that we have species specific models that change depending on the dominate species from place to place (or from time to time as fauna can change dominance)? Could they at least please attempt to describe what such a model might estimate?

Authors: We did not mean to give this topic a large attention since it is not the main topic of the manuscript. However, we do believe that important species that have a key role on the turnover of carbon in the ocean should be considered in these models. If not as single species, as major components of the surface deposit feeders in the region, contribution to the processing and recycling of carbon in the benthos.
Seasonal dynamics of megafauna on the deep West Antarctic Peninsula shelf in response to variable phytodetrital influx

P. Y. G. Sumida, C. R. Smith, A. F. Bernardino, P. S. Polito and D. R. Vieira

R. Soc. open sci. 1, 140294. (Published online 19 November 2014). (doi:10.1098/rsos.140294)

Following publication of our article [1], it has come to our attention that there is an error in the second to last sentence of the abstract.

The sentence in the published article reads:

‘Assuming that this species feeds on the top millimetre of the sediment, we estimate that, during periods of high phytodetrital flux, the *Pr. murrayi* population reworks one square metre of sediment surface in approximately 287 days’.

The sentence should read as follows:

‘Assuming that this species feeds on the top millimetre of the sediment, we estimate that, during periods of high phytodetrital flux, the *Pr. murrayi* population reworks the entire sediment surface in approximately 287 days’.

This error was introduced by the authors during the proofing stage. We apologize for any inconvenience caused.

Reference