

Interactive comment on “Photosynthetic production in the Central Arctic during the record sea-ice minimum in 2012” by M. Fernández-Méndez et al.

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The authors would like to thank Referee #3 for the kind words and the useful comments that will improve the manuscript. Please find below the comment followed by our answer starting with an “A:”

General comments

The manuscript presents results from a cruise in the Eurasian Basin of the Arctic Ocean in summer 2012. The study focuses on primary productivity, both as measured net primary production (NPP), from 14CO_2 uptake, and as new production estimated from seasonal drawdown in nutrients in the mixed layer. Samples from water column, sea

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ice and melt ponds were analysed and summarised for their contribution to the total primary production in this part of the Arctic Ocean. From the data the authors suggest the importance of ice algae and sub-ice algae to the primary production in the Arctic Ocean. The study is interesting and focuses on an important subject, and geographical area, where we need to improve our knowledge to understand future changes. I recommend the manuscript to be published after some rather minor revision.

A: We would like to thank referee 3 for this positive comment.

The decision of the authors to put so many of the figures and tables in the Supplement is unfortunate, especially since they are referred to so frequently. I strongly recommend that all (or definitely most) of these to be included in the manuscript. (Figure S4 may be the least important.)

A: We hope the editor agrees to moving all supplementary figures to the main text.

I miss the word ‘Ocean’ (as in ‘central Arctic Ocean’) in the title, and generally throughout the manuscript. This is a more correct term, but also makes it clearer. Also, the use of ‘Central Arctic’ is a bit confusing since the study only focuses on the Eurasian Basin (and adjacent shelf seas). This should maybe be changed for clarity.

A: We added the word Ocean to the title and checked for consistency throughout the manuscript. Specific comments

P. 2899, L 20-28: When describing the area and general circulation it would be helpful to refer to a figure, with some of these features added. Figure 1 could partly do here, but then, for example, the Eurasian Basin should be noted.

A: The Eurasian Basin is now labeled in Fig. 1 and a red arrow indicating the entrance of Atlantic water has been added.

P. 2901, L 5-9: The statement ‘nutrient availability . . . is probably decreasing. . .’ together with the sentence after makes me a bit confused. From the second sentence, as I read it, one could get the impression that although it has been hypothesised that nutri-

ents may increase due to river runoff, this may not be enough to substantially increase primary production. However, could it partly counteract the potential decrease due to stronger stratification, so that the net effect may be no significant change? This should at least be clarified/rephrased. The word 'probably' (L 5) could be changed to 'possibly', or 'may', or something similar, but better is if the two sentences are rephrased.

A: The sentences have been rephrased as follows: "On the other hand, nutrient availability in the euphotic zone of the deep Central Arctic Ocean may decrease due to the stronger stratification caused by increased freshwater storage. An increase in nutrients from river runoff has been hypothesized, but a recent study by Le Fouest et al., (2012) indicates that the contribution of these nutrients will not be enough to increase primary production in the deep Central Arctic substantially, and since they will be consumed at the shelf seas."

P. 2902, L 6-7: FYI and MYI have not yet been defined. As far as I can see this is done first at P. 2904, and in Table 1, but needs to be defined here.

A: MYI is defined in P.2900 L24. FYI will be defined here.

P. 2902, L 9-10: Data sources are important information that should not be put away in a Supplement (stresses one of the General comment above).

A: Table S1 just contains the Pangaea dois, which are also in the references mentioned each time a dataset is introduced.

P. 2907, L 7: 'Only' duplicates of all treatments do not give strong statistics. The decision may have been due to available resources, but should possibly be commented on.

A: We agree that duplicates are not enough to do any kind of statistics. This is now clearly stated here.

P. 2907, L 10-11 (and throughout): To me the use of 'negative control' (when no nutrients are added) is confusing, since no nutrients are removed. Why not simply use the

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term 'control (C)', since this is what it is? A: We agree and the term has been modified throughout the manuscript.

P. 2908, L 14: What depth/layer is used as 'surface'?

A: This sentence has been improved for clarity: "We then derived the uptake since last winter by calculating the difference between the integrated nutrient profile at the end of the productive season (August-September) and the nutrient value at the temperature minimum depth, which represents the initial nutrient concentration available in winter in the mixed layer."

P. 2908, L 21: I'm missing a reference to Dugdale and Goering (1967).

A: Added.

P. 2909, 3.1: To me much of this section feels like 'Method' material. It may be fine in the Results, but may be better placed earlier, to 'set the scene'.

A: The paragraph is at the beginning of the results to set the scene.

P. 2912, L 17: The reference to Table 3 must be wrong. That table shows nutrient inventories. Should be Table 4/Table TS3. The different values presented on L 14-16, are they presented in any table/figure (I don't seem to find them)? There is also a very large uncertainty in the average value for the INPP in ice-covered waters. Any comment that could be added to this?

A: Yes, the reference should be to Table S3 (TableS2 in the revised manuscript). The values in L14-16 are presented in Figure 2 (Figure 4 in the revised manuscript). The average value for INPP in ice-covered waters is putting together a large area with high spatial variability.

P. 2913, L 20-23: These stations do also show the shallowest depth of the Euphotic zone, which could be worth mentioning/discussing.

A: We agree that this should be mentioned.

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P. 2914, L 13-14: If only comparing two periods one cannot use the term 'trend'. Should be rephrased.

A: Has been substituted by "We observed a decrease.."

P. 2916, L 7-13: Is there any estimated uncertainty in the INPP from sedimentation of sub-ice algae? The potential total new production mentioned on line 12 does not have any extra uncertainty from this addition, but certainly this may be very large.

A: The range of estimated values for sub-ice algal sedimentation range from 1-156 g C m⁻² d⁻¹. Since the aim of this calculations was to discuss the possible contribution of sub-ice algae to total new production, we used the mean. This information has been now included in the manuscript.

P. 2916, L 28-29 – P. 2917, L 1-2: These two sentences need some rephrasing. Suggestion:'. . ., they have their peak in production, and thus seem to adapt to higher light conditions. This would already have. . .'

A: Rephrased as suggested.

P. 2917, L 8-9: Sentence stands a bit disconnected. Could be rephrased and merged with next sentence: Our nutrient addition experiment suggests. . . their biomass, which is in agreement with previous findings that sea-ice diatoms can store. . . (Kamp et al.,. . .).

A: Rephrased as suggested.

P. 2917, L 23: The word 'normal' may not be the best choice, and would need to be explained. Maybe 'typical', or something related (during the last. . . time frame).

A: We substituted "normal" by "typical (previous to the current trend of sea-ice extent decrease)"

P. 2919, L 17-18: Are there any estimated uncertainty in these numbers?

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A: Yes, Table 4 includes the information of the minimum and maximum values.

P. 2919, 27-28 + P. 2920, L1-2: This is a repeat of what was said on page 2917.

A: We agree that the information is repeated, but the context and aim of mentioning it here is different. We rephrased it to avoid being repetitive. "This, together with the capability of ice algae to store nutrients (Kamp et al., 2011) might provide them with an advantage against phytoplankton."

P. 2920, L 17-19: This sentence is not very clear to me, but then the sentence after says the same thing, much better. Thus remove the first one, or do some merge.

A: The first sentence was removed and the second one modified as follows: "However, the carbon flux was mainly composed of debris and the few algae observed in the sediment traps using light microscopy were flagellates."

P. 2921, L 21-27: One should be careful when comparing only two years. Was the same method used to estimate NPP in these two studies? Did they cover the same area? Also, as far as I can see the Vetrov and Romankevich did not present any uncertainty estimate of their average, making it more difficult to compare. In addition, their average value for the period does not tell whether there was some trend during these years. It comes down to a question of the significance of the difference, which is hard to evaluate.

A: When comparing the Grossmann and Gleitz study with ours we made sure that the method and the area sampled were the same. The lack of estimate uncertainty in the Vetrov and Romankevich study makes assessing the significance of any difference complicated. This has been now explicitly acknowledged in the text.

P. 2924, L 24-27 + P. 2925, L 1: These sentences/statements are not very clear to me. In 'earlier sea-ice based NPP' you mean the timing? Some rephrasing could make this clearer.

A: Yes, we were referring to timing. The sentence has been modified to improve clarity.

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Technical comments

A: All technical comments have been implemented as suggested.

Overall: typically citations in text should be arranged chronologically, not alphabetically as is done consistently in this manuscript.

A: Thanks for the hint. There was a problem with the citation software.

P. 2902, L 16: Remove 'only'. P. 2902, L 22: Change 'the' to 'a' (CTD system). P. 2903, L 5, and P. 2906, L 20: Chl a. . . P. 2903, L 27: '. . . the amount of labelled. . .' P. 2905, L 3: '. . .those where. . .' P. 2905, L 4: NPP is already defined, on page 2903, so don't need to be written out here. P. 2905, L 23-26: INPP has not been defined yet, and first comes on P. 2912 and Table 4 (as far as I can see). P. 2912, L 8: (18 and 30, respectively). P. 2912, L 26-27: Remove '. . . sampled with the peristaltic pump from the ice floe, . . .' since this is described in the Methods. P. 2914, L 10-28: I don't find a reference to Fig. S7. This would fit in the last paragraph on the page, but then figures from S7 and onwards need to be renumbered. P. 2914, L 24: '. . . in 1982 as in 2012.' P. 2920, L14-17: Many 'However' here, the first one could probably be removed. P. 2924, L 27: '. . .Central Arctic Ocean Basins. . .' Figures: Fig. 2, Caption: Remove 'nitrate' after '> 3 μM '. Fig. 5: 'Central Arctic Ocean'. Add a notation about the different scales. Also, if this is the expected size of the figure in the printed version the text/values on the colour bar must be much larger. Now it's almost impossible to read them. Supplement (which I hope will be moved to the manuscript): C987 Table S2: Neither INPP nor MYI is explained/defined. It is of course in the manuscript, but would be helpful here. In addition the five geographical sections are the ones shown in Fig. S7, right? It could be helpful to refer to that here. Fig. S6: Change 'nitrogen (A)' to 'nitrate (A)'. Fig. S7: It would be helpful to see the sea-ice extent in 1982 depicted in (A). A: We tested this but since almost the entire area shown was ice covered, the few discontinuous lines depicting the ice extent were rather confusing. Fig. S8: Make a note about the different scales for the Melt ponds panels.

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Interactive comment on Biogeosciences Discuss., 12, 2897, 2015.

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