

## Reply to anonymous reviewer

We would like to thank the anonymous reviewer for the constructive comments on our manuscript 'Deep-sea benthic communities and oxygen fluxes in the Arctic Fram Strait controlled by sea-ice cover and water depth'. Especially the recommended literature was of great help to improve our manuscript. We will first address the reviewer's 'specific comments' and secondly like to reply to 'Small corrections/comments' with stating the planned improvements.

In the following, author responses starts with the term 'Reply' and changes, that will be included in the manuscript, are given in blue.

Specific comments:

1. The introduction will benefit from turning the lists of which factors depend on which other factors into a narrative explaining how they influence each other. This change would necessarily make the introduction a bit longer, but improve the logic, flow and justification for the study. Also, the authors would help the reader by providing a bit of background why they estimate remineralization of new production rather than calculating it because they can.

Reply: We will follow the reviewers' suggestion and rewrite the introduction into a narrative explaining how factors influence each other by giving the information how they single factors are correlated which each other. Further, we will point out the link between the new production and the remineralization. It will be changed to 'Benthic deep-sea remineralisation depends on primary production and is as such closely linked with primary production patterns, known as pelagic–benthic coupling (Graf, 1989). The relationship, however, includes many and partly inter-dependent factors. Benthic deep-sea remineralisation is positively correlated with surface primary production (Graf et al., 1995; Wenzhöfer and Glud, 2002), which is on its turn controlled by light availability and nutrient supply (Kirk, 2011; Cherkasheva et al., 2014; Fernández-Méndez et al., 2015). Though, on an annual basis, only the new production leaves the euphotic zone (Platt et al., 1989), supplying the benthos with organic carbon. Benthic remineralisation is negatively correlated to water depth (Wenzhöfer and Glud, 2002), as it represents a loss of organic carbon by pelagic remineralisation (Rullkötter, 2006, Belcher et al., 2016) and thereby a loss of benthic food. After organic carbon reached the seafloor, it is ingested and remineralised by the benthic community. Benthic community parameters, e.g. biomass, density, structure, and functions of different fauna size classes, are controlled by food supply (and thus by primary production) and water depth (Piepenburg et al., 1997; Flach et al., 2002; Smith et al., 2008) but also by sediment properties (Wheatcroft, 1992; Vanreusel et al., 1995). Benthic remineralisation rates also depend on benthic community biomass (Glud et al., 1994). Furthermore, benthic remineralisation is enhanced if the benthic community intensifies oxygenation of the seafloor (Glud, 2008) and thus also depends on the benthic community structure. Therefore, the ecosystem processes primary production, pelagic remineralisation and benthic remineralisation, as well as the components benthic community biomass, density, and structure are controlled by abiotic and biotic factors and additionally create a cascade of dependencies from the ocean's surface zone of primary production to and within the deep-sea benthos.'

2. The discussion (especially 4.1 and 4.2) repeats the results to a large extent. Instead, it should place the results in the context of the extensive literature from the area and beyond. I recommend the authors summarize their findings more concisely and discuss their results in the context of, for example, the pan-Arctic scale Progress in Oceanography issue from 2015, primary productions model estimates covering the area, the series of three articles from Patrai – Codispoti 2013 etc.

Reply: Indeed, we repeated the results to a large extent, as we first needed to interpret our results before we could start to place the output in the context. However, we will reduce the repetition where ever possible and thereby follow the reviewer's suggestion. For example, the second paragraph of section 4.1 will be changed to 'The results of Pabi et al. (2008) showed that the annual primary production pattern follows the general sea-ice concentration pattern in the Fram Strait and is up to 10-times larger in the WS area compared to the EG area. Thus, the sea-ice concentration represents the general primary production pattern in the Fram Strait. As the sampling was performed in Mid/End of June 2014 and July/August 2015, it is very likely that the spring bloom, which usually starts in May (Cherkasheva et al., 2014), had finished. This is indicated by lower nutrient concentrations in water depth  $\leq 50$  m compared to the nutrient concentrations between  $>50$ – $300$  m water depths (Graeve and Ludwichowski, 2017a, b). The N:P ratio in the upper 50 m during the expeditions was six and seven in the EG and WG area, respectively (Graeve and Ludwichowski, 2017a, b), indicating that primary production was nitrate limited, similar to the permanently sea-ice covered central Arctic Ocean (Tremblay et al., 2012, Fernández-Méndez et al., 2015). Furthermore, the timing of our sampling suggests that the increased carbon supply by the spring bloom had already reached the seafloor and enhanced the benthic remineralisation (Graf, 1989) in both areas. The pattern of contrasts between the EG and WS area continued in the benthic food supply, which was also found by Boetius and Damm (1998) for areas with contrasting sea-ice cover at the continental margin of the Laptev Sea.'

We will further integrate the results from the suggested articles.

3. Water depth and vertical flux are well-documented highly influential factors structuring benthic communities both in terms of biodiversity and biomass/abundance anywhere in the ocean, in addition to sea ice cover. While these factors are mentioned in the discussion (without much literature support actually), it should also be noted more prominently that eastern Fram Strait receives constant inflow of particle rich Atlantic water, and this advective input adds to the vertical flux (see for example Wassmann et al. 2015 PiO for a summary). It is indeed complex to separate out the effects of water mass properties including particle content, and ice cover – a fact that should be acknowledged.

Reply: We will add information regarding the advective Atlantic input and acknowledge the complexity to track back the origin of organic matter resource. Indeed, there is quite some knowledge about the vertical carbon flux available. However, most of the data are from the more southerly and mainly sea-ice free locations in the Greenland Sea ("The Northern North Atlantic", edited by Schäfer, Schlüter and Thile). Owing to the complexity to separate out the effects of water mass properties, we only cited literature from very closed-by locations and thus, ensure a maximum of reliability of our comparison of remineralization data with the vertical carbon flux.

4. The authors said they struggled to find some relevant information (e.g. on primary production) for the western Fram Strait side, and therefore used values from the central Arctic. They might consider the results of the SFB313 that spent years investigating East Greenland including the slope, including carbon remineralization, primary production, benthic community structure etc., <http://www.springer.com/us/book/9783540672319>. Was the region never covered in any of the primary production models? Some additional useful information from eastern Fram Strait is also available, e.g. Wlodarska-K. et al. 2004 in DSRII.

Reply: We would like to excuse our unsuccessful literature research and thank the reviewer for the suggested literature. In the meantime, we found modeled primary production in the Arctic, which included estimates of primary production across Fram Strait. This source indicates the expected and

contrasting primary productivity between the EG and WS area (Pabi et al, 2008, doi:10.1029/2007JC004578). In addition, the suggested study of Codespoti et al. (2013, <http://dx.doi.org/10.1016/j.pocean.2012.11.006>) presents net community production values, which reflect new production and thus will also be used to give a more reliable insight into the relationship between primary production and benthic mineralization in the Fram Strait.

Small corrections/comments:

1. P4 l6 I would not call primary production and oxygen flux an ecosystem component, they are rate measurements of processes. The benthic community is an ecosystem component.

Reply: We will follow the reviewers' suggestion in the rewritten introduction (see reply to reviewers 2 'specific comments' no°1).

2. P4 Delete l6-8 (redundant to previous sentence).

Reply: We will delete the redundant sentence in the rewritten introduction (see reviewers 'specific comments' no°1).

3. L4 l9 Rather 'nutrient concentrations' (or which property of nutrients?)

Reply: Following the suggestions of the reviewer 1 Paul Renaud, all data regarding nutrients will be removed from the manuscript. However, we will add information regarding the nutrient state of the Fram Strait in the discussion.

4. P4l10 If this is to be general across the globe, add 'In general, benthic community ...'

Reply: We will add the term 'In general,' to the sentence.

5. P4l14 and elsewhere. I was taught 'therefore' never starts a sentence.

Reply: We have to disagree with the reviewer and refer to the following websites:

<http://grammarist.com/grammar/therefore/>

<https://www.iup.edu/writingcenter/writing-resources/grammar/common-problems-with-however,-therefore,-and-similar-words/>

6. P4l20 'Western' Arctic is a rather undefined term, since different nations use it in very different ways, rather give the region.

Reply: We will follow the reviewers' suggestion and change 'western Arctic' to 'Chukchi and Beaufort Sea'.

7. P4l21-22 Unclear how the 'better fit' works when one doesn't know what other factors were included.

Reply: We will change the sentence to 'A pan-arctic benthic remineralisation model showed a better fit when water depth and benthic chlorophyll data (representing food supply from primary production) were taken into account, compared to a model using only water depth as controlling factor (Bourgeois et al., 2017). This indicates that surface primary production patterns and water depth are both relevant factors controlling benthic remineralisation in the Arctic Ocean.'

8. P4I34 No need to repeat the three references for the same aspect since already given in I28 P5I17ff What time period is considered when talking about stable ice cover here? What time period is considered in the number of 0.6 years per decade? (And somewhere in the discussion the author talk about ice thinning, a bit of a contradiction.)

Reply: We will remove the repetitive references. The cited references only mentioned 'stable ice cover' without data support. Therefore, the dataset presented in our manuscript actually describes the sea-ice conditions for the first time in reliable, satellite-based numbers. The time-period for the sea-ice rejuvenation will be added. However, we have to disagree with the reviewer that a sea-ice rejuvenation is contradicting with a sea-ice thinning. Multi-year sea-ice is thicker than perennial, first-year sea-ice. Consequently, when sea-ice becomes younger, it is likely that it becomes thinner as well, which we pointed out in the introduction (P5L19).

9. P6I4 Why combine sea ice cover and nutrients under one sub-header? I suggest separating those sections.

Reply: As mentioned in the reviewers' small corrections / comments no°3, data regarding nutrients will be removed from the manuscript. Thereby, the identified issue will be solved.

10. P5I6 rather 'Study area and field sampling' or 'Study area and sample collection'. None of the sample preparation or processing is described here.

Reply: We will follow the reviewers' suggestion and use the term 'Study area and field sampling'

11. P6I9 Although both 'data are' and 'data is' is allowed per some dictionaries, it really should be 'data are' (one datum, several data).

Reply: We will change the term to 'data are' throughout the entire manuscript.

12. P6I15 Provide a reference for the nutrient measurement method.

Reply: As mentioned in the reviewers' small corrections / comments no°3, data regarding nutrients will be removed from the manuscript. However, we will add information regarding the nutrient state of the Fram Strait in the discussion.

13. P6I20 Which property of phospholipids and proteins and organic matter was measured – presumably concentrations?

Reply: We specify the measured property and change the sentence to 'Various biogenic sediment compounds including grain size, water content, chlorophyll a (Chl a) and phaeopigment concentrations (Phaeo), portion of total organic carbon (TOC), phospholipids concentrations, protein concentrations, portion of organic matter, and the bacterial enzymatic turnover rate (FDA) as bacterial activity proxy were determined from the sediments sampled by the MUC and chambers of the autonomous benthic lander system.'

14. P6 section 2.3 The methods description is extremely abbreviated, but it is an editor decision if this is sufficient.

Reply: We are aware of the intense use of abbreviations. However, all abbreviations are common and introduced before, as recommended by the manuscript guidelines of 'Biogeosciences'.

15. P7 2.4 What taxonomic resolution was aimed for?

Reply: We will add the aimed taxonomic resolution, which was at least class level for macrofauna and order level for meiofauna.

16. P12I6 It would be appropriate to include the nutrient profiles (at least upper water column) into the MS figures rather than the supplement given that the nutrient inventories provide the basis to the level of primary production possible (although measured after the bloom was done presumably). At the very least some concentration ranges should be mentioned. Define 'surface'.

Reply: As mentioned in the reviewers' small corrections / comments no°3, data regarding nutrients will be removed from the manuscript. However, we will add information regarding the nutrient state of the Fram Strait in the discussion.

17. P12I16 Why 'indicates'? Later you test this!

Reply: We will remove the sentence, as indeed we later test this.

18. P12I12 There are different opinions on this, but given that I would find at least a range of densities etc. presented (as is done in the next section 3.4). At the very least, table 2 should be referenced here so that the reader can find the results.

Reply: We will follow the reviewers' suggestion and present ranges for the parameter 'median grain size', 'portion of grain size >63  $\mu\text{m}$ ', 'water content' and 'porosity' in section 3.2. However, we will deviate from the pattern used in section 3.4 for the parameter Chl  $a$ , Phaeo, CPE, Chl  $a$ /CPE ratio, Chl  $a$ /Phaeo ratio, TOC, organic matter, proteins, lipids, FDA, as it would lead to an absolutely illegible paragraph. Therefore, we will present the minimum and maximum values across the entire Fram Strait only for Chl  $a$ , TOC and organic matter and will not distinguish between the EG and WS area. For the remaining parameter Phaeo, CPE, Chl  $a$ /CPE ratio, Chl  $a$ /Phaeo ratio, proteins, lipids, and FDA magnitudes will be given. In addition, we will refer the reader to Table 3 (former Table 2), Figure 3 and Supplement Table S4, which holds more detailed information. The text will be changed to 'The sediment bound Chl  $a$  concentration ranged between  $0.4 \pm 0.3 \mu\text{g ml}^{-1} \text{ sediment}^{-1}$  (n=15) at EG III and  $12.7 \pm 3.1 \mu\text{g ml}^{-1} \text{ sediment}^{-1}$  (n=15) at SV I (Table 3) and differed significantly between the EG and WS area (Figure 3, Supplement Table S4). A similar pattern was found for sediment bound Phaeo concentrations and CPE concentration with over 4-times higher median values in the WS area compared to the EG area (Figure 3). The Chl  $a$ /CPE and Chl  $a$ /Phaeo ratios did not differ between the EG and WS area (Supplement Table S4), which indicates that the benthic community in both areas fed on a similar food quality and received the spring bloom food supply at the same time, respectively. Sediment bound TOC ranged between  $0.44 \pm 0.04 \%$  (n=15) at EG II and  $1.58 \pm 0.27 \%$  (n=15) at SV I and differed between the EG and WS area, similar to organic matter, which ranged between  $3.45 \pm 0.6 \%$  (n=15) at EG II and  $12.0 \pm 4.2 \%$  (n=30) at HG III (Table 3, Figure 3, Supplement Table S4). Proteins, lipids and FDA also differed between the EG and WS area with 5.6-times, 2.3-times and 1.8-times higher median values in the WS area, respectively (Figure 3, Supplement Table S4).'

19. P13I31-32 add 'rather than an actual interannual difference'

Reply: We will remove the entire sentence 'These differences are probably a result of the different sampling periods (June in 2014 and end of July/beginning of August 2015), resulting in different Phaeo and CPE concentrations.'. For justification please look at small corrections / comments no°20.

20. P14I5 Just above you wrote the different is likely related to the months, while this line states it is a spatial difference. Both may be true, but as written the statements seem contradictory.

Reply: By removing the sentence in P13I31-32 (small corrections / comments no°19), the inconsistency identified by the reviewer will be solved.

21. P14I26 Significant indeed, but the authors should mention that the global R values are rather low, same with the macrofauna results.

Reply: We agree with the reviewer that the global R values are low and added this information to the text. For example, it will be changed to 'Regarding macrofauna communities based on density (Global R = 0.257, p = 0.007) and biomass (Global R = 0.238, p = 0.003), the ANOSIM revealed significant but weak differences between the HSC and LSC area.'

22. P15 I10 perhaps add 'marginally not significant'

Reply: We decided to omit the last part of the sentence. It now reads 'Further, the two-way crossed PERMANOVA revealed that the sea-ice coverage (LSC and HSC) explains a significant (p = 0.008) portion of the macrofauna density variability.' We reported that the result of the interaction effect of water depth and sea ice concentration on macrobenthic community biomass was significant. Therefore, it was pointless to look at the effects of the single factors, simply because the test just showed that their effect depends on the effect of the other factor.

23. P15I16 As phrased, this is not a question.

Reply: We will rewrite the sentence to 'The aim of this study was to link contrasting sea-ice conditions with...'

24. P15I23 Grammar. If there were a strong link ... we would expect .... (conditional)

Reply: We will change the sentence to 'If there were a strong link between sea-ice conditions and deep-sea benthic oxygen fluxes, we would expect contrasting primary production, benthic food supply, benthic community parameters and benthic oxygen fluxes between the EG and the WS area.'

25. P16I14/15 This is not the right place to mention this point, move to figure caption or results text.

Reply: We will remove this sentence, as the information is already implemented in the method description of the PCA.

26. P16I27 opposite to our expectations or in contrast to our expectations. The following PCA sentence is grammatically incorrect. The PCA only shows .. but does not test ...

Reply: We will change the sentences to 'This is in contrast to our expectations and to findings of Boetius and Damm (1998). However, a PCA only shows correlations but does not test for the significances of these relationships.'

27. P19I4 The Kortsch paper is on shallow nearshore hard bottom communities, not quite the right reference here.

Reply: Indeed, Kortsch et al is not an appropriate reference, as it deals with benthic changes in a fjord system. The reference will be removed and instead we added Harada (2015, doi: 10.1016/j.gloplacha.2015.11.005).

28. P19I10 In earlier sections the authors talk about 'stable ice conditions' in Fram Strait, while here they state that ice is thinning. Specifying by which metric the conditions are stable will relieve the contradiction.

Reply: We will specify, that the term 'stable conditions' is used in terms of the general pattern of the sea-ice concentration in the Fram Strait (west: high concentration/east: low concentration).

29. P19I14 My understanding of the Boetius et al. paper is that these authors discussed the high Melosira biomass to be generated on the shelf and maintained (but not produced) over the basin through constant resupply of –albeit low – nutrients during ice drift, not as a consequence of increasing algal biomass in the central Arctic. General: Someone should switch German to English comma rules throughout.

Reply: We are thankful for the additional perspective regarding the interpretation of the publication of Boetius et al. (2013). The aim of the sentence is to point out that the Fram Strait benthos did not receive any algae patches (as far as assessable), which would have had a dramatic impact on the microbial and therefore total remineralization and would indicate that our presented mineralization are underestimations. However, as this is not the case (no algae patches found), our results are reliable. However, in order to stress this, the sentence will be changed to 'However, fast sinking algae patches as reported by Boetius et al (2013) in the central Arctic, which would lead to increased benthic mineralization, were not observed during a video transect at EG IV in 2014 (pers. Comm. J. Taylor).'

We will apply English comma rules by using the free-ware version of gramma software grammaly.com. and we will let the manuscript be checked by a native speaker.

30. Table 2. Use same number of decimals within one parameter (e.g. days with sea ice has between zero and two decimals).

Reply: We will adjust the number of decimals to be consistent throughout one parameter.

31. Table 3. Spell out HSC and LSC.

Reply: We will spell out HSC and LSC in the table caption. It will be changed to 'The table shows that there are differences in the macrofauna community between the highly sea-ice covered area (HSC) and the low sea-ice covered area (LSC), while this is not the case for the meiofauna community.'

32. Figure 1. Specify time frame for 'general summer sea ice extent', by month and period.

Reply: We will specify the month and period for the 'general summer sea-ice extent', which is September 1981-2010 (<http://nsidc.org>)

33. Figure 3. Indicate if any of the differences between EG and WS were statistically significant. This and other figures explain abbreviations or say in caption where they are explained.

Reply: We will indicate significant differences between the stations in the figure and add the number of observations for each bar according to the reviewer's suggestion and the suggestions of the reviewer Paul Renaud. Further, will explain used abbreviations in the figure and table captions.

34. Figure 6. Typos: Arctic missing 'c'. Sauter et al. and Bourgeois et al. missing periods after al.

Reply: We will correct the typo's in figure 6.