

Interactive comment on “Geophysical and geochemical controls on the megafaunal community of a high Arctic cold seep” by A. Sen et al.

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General Comments

This is a well-written manuscript describing the geochemistry, physical habitat and fauna at a series of methane-seeps in the Arctic. The authors relate changes in faunal distribution, from photographic images, to the local chemical and physical environment and discuss the micro-habitats available at such seeps. The manuscript would be improved if it were possible to make comparisons with the background fauna away from seep influence. There are a number of problems with the analytical procedures, presentation and interpretation of results as described below.

Response: Thank you for appreciating the premise of the manuscript. Unfortunately, comparisons with background fauna away from seep influence is only possible qualitatively, and this is the approach we took in the manuscript. More details on this are below.

Specific Comments

Methane

Since the study is based on methane seepage it is essential to have some reliable measurements of methane concentrations available to the biota. Unfortunately the method described for methane analysis does not measure “dissolved pore water methane”, as stated, but a mixture of free methane plus methane sorbed to the sediment and released by the sodium hydroxide addition (Ertefai et al. 2010). Since authigenic carbonate is present, the concentration of the sorbed methane can be up to two orders of magnitude higher than the dissolved methane (Ijir et al. 2009). It is unclear why pore water obtained from the rhizons was not used for on-board methane analysis. There is no information available, to my knowledge, on the extent to which sorbed methane is available to the biota. Thus comparisons between sites based on methane availability are thus not valid.

Response: We have to disagree with the reviewer saying that what we measured is “...a mixture of free methane plus methane sorbed to the sediment...” therefore claiming absence of any dissolved gas in our samples. Methane is soluble in water and is always present in pore water samples of marine sediments typically demonstrating 50-80 % porosity (Abrams et al., 2017).

It is possible that some amount of free and sorbed gas is also present in the bottom sediments in situ and some occasional desorption occurred due to NaOH in our samples. Portions of methane purposefully extracted with alkaline technique from sediments at active thermogenic seepage locations are essentially unknown because (1) petroleum exploration prioritizes acidic extraction and vacuum desorption methods, and (2) existing works on alkaline extraction focus on “..environments that are not influenced by thermogenic processes.” (Ertefai et al., 2010) and not related to visible seabed seepage. Moreover, Ertefai et al., 2010 report inaccuracy of their sorbed gas analyses from non-seep sites of as much as 65%, implying difficulties during even targeted extraction efforts. During the analytical stage of our work we did not aim to extract any sorbed gases, therefore, we did not apply any long-duration techniques for mechanical disintegration of clayey assemblages (orbital shakers, mills, etc.).

The publication by Ijir et al., suggested by Dr. Dando states that content of adsorbed gas is twice higher in carbonate concretions compared to surrounding sediments. In our work we collected and analyzed the samples that are by all means equal to what Ijir et al., 2009 call surrounding sediments with absence of any macroscopically observed carbonates. Therefore, the ratio of adsorbed and extractable methane to bulk methane in even non-seep environment is poorly understood and lacks firm quantifications due to substantial analytical errors. In our study area where free gas escapes the seabed, the contribution of occasional desorption of gas strongly-bound to clay minerals is deemed to be negligible compared to abundance of its more labile forms (dissolved and free gas).

The issue of mixing free gas and dissolved gas in headspace samples is inevitable and, we believe, has become a condition well known in marine geoscience. Headspace gas analysis shows concentration of only dissolved gas if this concentration is lower than the solubility limit under P and T conditions of a laboratory where the samples are collected. Concentrations measured in our shallow sediments are below this critical value, thus representing dissolved gas only.

Analyses of pore water collected with rhizons are not optimal due to long exposure (at least an hour) of water drops to the air in the syringes that always have some dead volume. During such sampling dissolved gas gets equilibrated with atmospheric gas causing loss of methane in analyte that is hard to trace and account for.

Despite this, even if our method did result in some amount of free gas and sorbed gas getting included in our measurements, the same method was used for all the pingos and the point was to compare between them. Therefore, even if our method overestimates dissolved methane concentrations, it does so equally for pingo 5 and the other pingos. And our measurements, which were lower at pingo 5 compared to the other pingos, likely indicates that dissolved methane is also lower at pingo 5.

Sulphide

Dissolved reduced sulphur species are utilised by chemoautotrophic free-living and symbiotic bacteria as an energy source. The concentration of dissolved “sulphide” ($\text{H}_2\text{S} + \text{HS}^- + \text{S}^{2-}$) and thiosulphate is thus an important measurement. The authors state that sulphide was below the detection limit in the bottom water and in the upper “few” cm of the sediment of all the cores. However, sulphide must have been present to support the bacterial mats visible on the surface. The detection limit is not given but since the lowest standard used in the assay was $40 \mu\text{M}$ (Hong et al., 2017) the method described may not have been able to detect concentrations of a few μM . Many thiotrophic symbiotic associations exist in sediments with dissolved sulphide concentrations of $< 1 \mu\text{M}$. It is very difficult to prevent oxidation of low concentrations of sulphide in pore water and since the samples were not analysed immediately, it is probable that oxidation occurred during preservation and storage. H_2S would have been carried into the upper sediment and the water column in the methane bubbles as well as in the associated water plume (Reeburgh 2009, Dando et al. 1994a). In addition, the drawdown of seawater induced by the rising methane bubbles (O’Hara et al. 1995, Zimmermann et al. 1997) would have locally generated reduced sulphur species from iron sulphides within the sediment (Dando et al. 1994b) as well as producing a halo of less reducing areas surrounding the bubble outlets.

Response: Dr. Dando is correct, the detection limit was $40 \mu\text{M}$ and we agree that thiotrophic symbioses can exist with much lower concentrations of dissolved sulphide.

We also agree that the presence of bacterial mats indicate the presence of dissolved sulphide at the water-sediment interface (although the mats are very patchy and usually quite small). We can therefore remove the sentence on sulfide toxicity with respect to non-seep fauna.

However, the main points we were trying to make were with respect to comparing sulphide concentrations between the different pingos and they are: 1) sulphide concentrations likely do not differ between the pingos and 2) we might have been unable to detect sulphide in the bottom water, but that is the case for both pingo 5 and the other pingos. We believe that these points are still valid. However, we can emphasize our inability to detect low concentrations of sulphide, both within the sediment and in the bottom water, but doing this would not alter the conclusion we came to, that 'GHP5 is not deficient in this regard (sulphide) either, in comparison to the other pingos'. We also would like to emphasize that, even though we did not measure sulphide concentration immediately, the porewater samples were collected by rhizons which has been shown to prevent oxidation (Seeberg-Elverfeldt Jens et al., 2005) of the samples. We also preserved the samples by adding saturated Ac(OAc)₂ < 30 minutes after the rhizons were disconnected from the syringes. Such preservation measure is widely used in the literature and can prevent the oxidation of sulphide.

With respect to the downward seawater irrigation due to rising bubbles in the sediments, Hong et al. (2017) have shown that it is unlikely to occur. Briefly, if seawater indeed intruded from the bottom seawater to the surficial sediments, one would expect seawater concentrations for all porewater species. Hong et al. (2017) have shown that this is not the case from the 100 fold higher ammonium detected in the first 5-10 cm of the sediments. We can therefore confidently exclude the impact on sulfide concentration by such so-called bubble irrigation mechanism.

Identification of biota

The statements "Visible fauna (at least a few centimeters across) were identified" (p5) and the statement in the following paragraph that "Numerous individuals of siboglinid worms were seen", appear contradictory. *Oligobrachia haakonmosbiensis* is large for a frenulate, with a tube diameter of 0.6-0.9 mm (Smirnov, 2014). It would be useful to have a high resolution image, perhaps as a supplementary file, to show how these individual siboglinids were visible in the photographs, since a lot of the Discussion is based on their presence or absence.

Response: We can add a supplementary figure, to show what the worms look like in the images. It should be kept in mind that individuals of *O. haakonmosbiensis* were not seen or marked. Clumps or mats of them were seen and these aggregations were outlined (Methods, pages 5-6). Since this point was not clear, we can change this portion of the Methods section to clarify this.

Most of the identifications relied on interpretations of images taken by a towed camera with a resolution of 16 million pixels and with stereo cameras mounted on a ROV with a resolution of 1.4 million pixels. We are not told the respective field of views photographed by these cameras so it is not possible to estimate the respective resolutions. It would help interpretation if the authors would calculate the sizes that the respective pixels represent. Rough calculations, from the dimensions given in Figure 2, suggest that the pixel size in images from

the stereo cameras may have been inadequate to resolve smaller organisms, such as *Oligobrachia* tubes, 0.6-0.9 mm in diameter (Smirnov, 2014) unless they occurred in clumps.

Response: As stated above, the worms did appear in clumps and these clumps were outlined as polygons in a GIS as opposed to every individual being marked. It is absolutely true that single individuals would not be visible in the images. Figure 2 does have scales in the mosaics/transects, but we can add scales in the individual images as well to give a better idea of the fields of view.

The core samples at GHP 5 were taken around the periphery of the pingo (Figure 1) so that it is not possible to deduce from these that *Oligobrachia* was absent from pingo 5.

Response: The deduction that *Oligobrachia* was absent from pingo 5 is not based only on the core samples. This deduction is based on the images and mosaics from the site. It is also based on our extensive surveys of the pingo before imaging for mosaicking purposes was conducted. Therefore, we did not conclude that *Oligobrachia* was absent from pingo 5 just because we did not recover them in the cores (and 1069 at least is not from the periphery). We concluded that they were absent because we spent a considerable amount of time surveying the pingo with the ROV's HD video camera, and did not see the clumps that are so abundant on the other pingos. We are quite confident that the worms are more or less absent at pingo 5. It is certainly possible that a few, scattered individuals are present and these solitary individuals would not be visible in any kind of imagery. However, this is one of the limitations that always exist with image-based studies and is the standard shortcoming that has to be taken into consideration when using an image based approach. We can add this caveat to the discussion. Nonetheless, mats of *Oligobrachia* are not present at pingo 5, and this is an important difference between pingo 5 and the other pingos, and the fundamental statement that we address in this manuscript.

Another problem in comparing tow cam and ROV pictures is that the ROV imaging was always from fairly discrete areas on pingo 5 while the tow cams were transects covering from the outside into the centre of the pingo (Figure 1). This might explain why *Nothria*, for example, was identified in all the tow cams but not in the ROV pictures. The tow cam epifaunal data, presented in Table 1, should therefore be divided into "on pingo" and "off pingo" sections. The reason that TC25 GHP3 clusters with the GHP5 ROV camera tracks is probably because tow cam 25 has the greatest proportion of offpingo track of any of the tow-cams.

Response: It is true that the tow cam images are transects whereas the ROV images cover more discrete areas (although ROV mosaic 3 is more a series of transects than a mosaic). That is why we chose to neither discuss the overall community structure, nor to make comparisons of overall community between pingo 5 and the other pingos, except very briefly, and to mention the diversity indices. We included a figure of the results of the community analyses in order for these results to be available for everyone, but we refrained from discussing these results, other than very briefly because we agree that the different ways in which the pingos were imaged could be a factor that muddles the overall community characteristics. This is why we instead chose to focus on the presence or absence of *Oligobrachia* since this is a trend that we feel confident of comparing between the different pingos, as stated above.

It is more likely that the absence of *Nothria* from the ROV images is due to the imaging capabilities, and not because the ROV images were discrete mosaics. This is because we could not see *Nothria* in ROV images from pingo 3, i.e., the ROV images over pingo 3 that could not be used because the navigation data was not useable. If the absence of *Nothria* from pingo 5 was simply due to the locations in which the images were taken, then they would be visible in the ROV images taken over pingo 3. Their absence from these images suggests that they are not visible in the ROV camera. Regardless, however, *Nothria* was not used in the community analysis, nor in comparisons between the pingos.

All of the images and faunal data from them is more or less 'on-pingo.' We did not use any 'off-pingo' images for our quantitative analyses, therefore the data presented in Table 1 cannot be divided into these two categories. There is no clear boundary that distinguishes the seafloor as being part of a pingo versus not. Therefore, we only used images that appear to be part of the different pingos. We know that our navigation data was not perfect for this, but we also looked at the images and the presence of obvious signs of seepage (carbonate crusts, bacterial mats, worm tufts, etc.) as evidence of the images being taken from the seeping pingos. It is entirely possible that some images included in our study are in fact, slightly off the pingo in question, but this error would exist for all the pingos from which the tow cam images were taken. It is true that this error would not occur for the ROV images over pingo 5 but, as stated above, that is why we did not go into any detailed discussions about comparing overall communities.

The only time that we did consider 'off-pingo' images was in a transect to the west of pingo 3 (at least 1 km away), which we discuss only qualitatively, where we introduce the idea that seepage areas might have higher species diversity than background, non seep affected seafloor.

In short, all the tow cam images are, to the best of our ability, with the given constraints of the study, taken over the pingos themselves. It would therefore be inappropriate to classify any of them as being 'off-pingo.' But we agree that off-pingo areas could have been included in the tow cam images, which makes comparisons with the discrete mosaics on pingo 5 difficult. However, we acknowledge this shortcoming and as a result, do not discuss community differences between pingo 5 and the other pingos. We stick only to the main difference, ie, the presence or absence of frenulates, which we do feel confident is a real difference between pingo 5 compared to the others.

The frenulates observed were identified from specimens in the core samples and density estimates for them were calculated from the densities observed in the cores. It would have helped the interpretations if information had been provided on the depth they reached in the sediment. This may be site specific, since the penetration depth of a species has been shown to vary between cores (Dando et al. 2008). *Oligobrachia haakonmosbiensis* were reported penetrating the sediment to a depth of 55 cm at the Hikon Mosby mud volcano (Lösekann et al. 2007).

Response: We did not include this because we did not conduct any good, exhaustive measurements on how far the tubes penetrated (and we had different types of cores,

which all affect the animals differently when they are retrieved). Roughly, we can say that the tubes reached 50-60 into the sediment, which is in the same range as what was seen at the Håkon Mosby mud volcano by Lösekann et al., (2008) and Gebruk et al. (2003). We can add this information to the text.

It would also be helpful to know whether other macrofauna were recovered from the cores, since most faunal species with chemoautotrophic bacteria at shallower seeps are infaunal and would not show on surface photographs. An example is at a methane seep at 170 m depth in the N. Sea where 3 such species were found living within the sediment and shells of a fourth, the bivalve *Lucinoma borealis*, were recovered (Dando et al. 1991, Dando 2001): no epifauna with chemoautotrophic symbionts were observed. Many frenulates have tubes completely buried within the sediment; thus chemosynthesis is probably more common at the pingo sites than this study of mainly epifauna suggests.

Response: We agree that there might be infaunal species that are chemosymbiotic, and we mention this in the text. We also tried to emphasize that we are only considering animals that are visible in images in this study. However, this might not have always been clear, especially when we talk about *Oligobrachia* being the only confirmed chemosymbiotic species. We can change the text to make sure that every time we talk about a trend like that, we specify that we are referring only to larger animals, visible in images and that smaller, infaunal animals are not taken into account in this study.

Discussion

On discussing the distribution of the frenulate *Oligobrachia*, the authors wrote: “the image transects containing siboglinid frenulates were much less even in terms of species abundances compared to the transect and mosaics which did not contain any frenulates”. This would be expected since it has been shown that, on the Rockall slope, frenulate distribution did not cluster with most other taxa and there was an inverse relationship between frenulate density and the density of other benthos (Dando et al. 2008). This was explained because sediment disturbance by other organisms would increase sulphide oxidation and displace, or bury, the thin tubes of the frenulates. It should be noted that in the latter study most of the frenulates had a low abundance and none of the tubes projected from the sediment, if they did at all, as far as those of *Oligobrachia haakonmosbiensis* and thus would not provide an epifaunal habitat. In the one obligate, methane seep frenulate species that occurred in high densities, *Siboglinum poseidoni* (Dando et al. 1994c), no epifauna were noted between or above the projecting tubes.

The authors consider that chemoautotrophic primary production at the pingos might exceed the photosynthetic primary production reaching the sea floor. The examples they cite are from deeper water, where less photosynthetic production reaches the sea floor. This is unlikely to be true at 400 m where much more photosynthetic production will reach the seabed. As shown in a comparative study (Bernadino et al. 2012), the isotopic difference between background and seep fauna was much lower at the Eel River seeps (250-500m) than at deeper seeps at 770 m depth and deeper. Isotopic evidence of food inputs is needed to support the authors' hypothesis. Since methane solubility increases with pressure there will, potentially, also be more methane available to the biota at deeper sites.

Response: We do have isotopic evidence of chemosynthetic food input at the pingo site, but these results are part of a separate article and we are not really at liberty to discuss them yet. Our goal was not to show that chemoautotrophic production exceeds photosynthetic production per se, we just wanted to include the notion that local primary production, in the form of chemoautotrophy occurs at the site. Instead of saying that ‘autochthonous chemoautotrophic primary production tends to exceed photosynthesis derived detrital food supply (page 12), we can change the text to say that both photosynthetically derived and chemosynthesis based organic matter is likely available at the pingos. The point here was to try to explain why certain animals were seen to appear to show a preference for seep based habitats such as the frenulates, carbonates or bacterial mats. With respect to nutrition, we mentioned that some animals might be grazing on bacterial mats, which could in turn affect higher order consumers and explain their distribution among bacterial mats. Therefore, changing the text so that it does not imply that chemoautotrophic input exceeds photosynthetic input will not change the overall message and so we can easily modify the text to avoid this confusion.

Regarding sulphide in the upper sediment, p.14 line 11: only 1 measurement at 5 cm depth is shown in Fig. 4 (for core 1045, off the edge of pingo 3). The exact value for this sample is not shown. However, a single measurement does not justify the statement that “sulphide was not detectable - - - even in the upper 5 cm of the sediment at the pingo site”. Should this read “sites”?

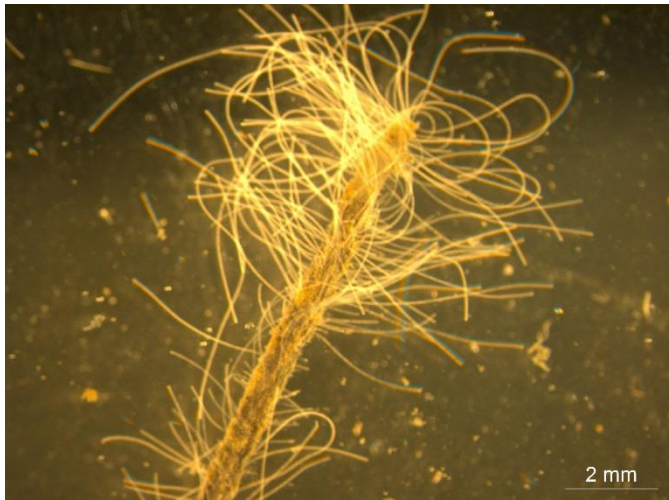
Response: The value is 0. The 0 values for the other cores should also be shown in this figure and we apologize that they are not there. Even at 10 cm, we often could not detect sulphide in the cores, which is the reason for the statement on page 14, line 11, that sulphide was not detectable in the bottom water or even in the first 5 cms. That is, this statement was not based on a single measurement from one core at all, but we agree that the 0 values for the other cores should be shown on this figure and we will rectify that.

As mentioned above, it is probable that sulphide was present at significant concentrations for the biota, including the bacterial mats at the surface, but was not detected using the stated analytical procedure. Serov et al. (2017 Fig. S2C) shows a picture from one of the pingos with a white bacteria mat, presumably of sulphur-oxidising bacteria, on top of “tubeworms” that project approximately 4 cm above the sediment, if the scale on the photograph is correct. If these are sulphur-oxidising bacteria then sulphide or thiosulphate must be present in the water column. The “tubeworms” are approximately 10 mm across, measured against the scale on the photograph, and thus cannot be *Oligobrachia*.

Response: As acknowledged above, we will change the text so that we clarify that we did not detect sulphide, but there could be concentrations lower than what we detected, including using bacterial mats as evidence for this.

With respect to the image in Serov et al., based on previous descriptions of *Oligobrachia*, it would appear that the worms with filamentous bacteria on them are too large to be *Oligobrachia*. However, we have sampled these worms and they are, in fact, *Oligobrachia* (manuscript in preparation). The bacteria can form large, dense colonies on the tubes of the worms, so that they appear much larger than the tubes themselves. Below is an image that shows this. This was also seen in nearby sites such

as in Storfjordrenna (Åström et al., 2016) and a site of pingo-crater complexes in Bjronøyrenna (publication in preparation).



P14, line 21 and subsequent text: “this particular image transect did not contain frenulates.” *Oligobrachia haakonmosbiensis* has large tubes for a frenulate, many species have tubes 100 μM or less across and would not be visible if they did project above the surface, although many species are completely buried within the sediment. More than one species frequently occur in the same core sample (Dando et al. 2008) so that it is not possible to state that frenulates were absent. To be correct you should write that “this transect did not show any ‘visible’ frenulates”.

Response: We can change this as suggested. As we mentioned before, we were trying to emphasize that only animals visible in images are considered in this study, but obviously, we have not emphasized this enough and we will make sure that this point is very clear.

GHP5 gas release: in many submarine seeps gas is only released at low tide when slight differences in bottom pressure cause the sub-surface gas volume to increase. At other frequently visited methane seep sites, such as the Scanner pockmark, continuous gas release may, or may not, be present during any given cruise. In the absence of data on the tidal conditions when observations were made over GHP5 it is not possible to state that gas was not released from this pingo.

Response: It is indeed true that gas seepage can be induced by tidal effects. We however do not think this can explain the contrast in gas flare activity between pingo 5 and the other pingos as tidal changes should pose the influence on all pingos that have almost the same water depth and are within an area of only about 2km^2 . Gas flare data was acquired in 2016 together with the 3D seismic survey, and all in all, the survey took two days. During this time-span, we detected no gas flares from GHP5, and thus we can rule out a potential tide-controlled leakage. Furthermore, as is mentioned in the manuscript, several cruises were conducted, over three years and during different seasons, all of which consistently documented the absence of any flare activity over pingo 5.

The enhanced reflectors below GHP5 indicate subsurface gas and, on enlargement of Figure 6, it is possible to see a small gas “chimney” towards the edge of the pingo (see Fig 6 section), although this is considerably smaller than the chimneys below the other pingos. Core 920 on the edge of GHP 5 contains methane of thermogenic origin (Serov 2017, Table S1), implying a deep source for the gas.

Response: The enhanced reflectors below GHP5 may indicate pockets of subsurface gas, buried carbonates or gas hydrate (but not necessarily active gas migration feeding the GHP). The seismic data show lower amplitude dipping reflectors underneath the unconformity below GHP5. Active seismic chimneys are normally represented by distinct acoustically masked areas (such as under pingos 1-3) or a vertical pipe structure often accompanied by velocity-related anomalies, which we don't observe below GHP5. A narrow zone of weak acoustic blanking under the margin of pingo 5 may indicate either very low to negligible fluid migration, or a fault zone. On the neighboring inlines and crosslines in our seismic volume, this feature appears even less prominent; therefore, we conclude that there is no significant fluid/gas migration underneath GHP5. On the other hand, underneath the other pingos, prominent seismic chimneys occur. We can change the text so that instead of writing “lack of chimney..” in the manuscript we can write “no prominent seismic chimney” underneath GHP5.

Active release of methane from the sediment will channel the methane into the higher porosity release channels. The sediment at the sides of these channels will have a low methane concentration, due to the down-flow of seawater from the sediment surface (Dando et al. 1994a, O'Hara et al. 1995). Thus it is not correct to argue that methane release will stimulate overall sulphate reduction and methanotrophy in a seeping pingo when compared to a non-seeping pingo with a high sediment methane concentration (p17 first paragraph). Microbes may also be removed from the system by the rising fluids.

Response: As our reply for the previous comments and the results from Hong et al. (2017), from the concentration of ammonium in the porewater, we can confidently exclude the possibility of downward seawater flow into the sediments. We therefore do not think such argument is relevant in our study sites.

Furthermore, sulfate reduction rates were measured independently of methane. Therefore, our argument that sulfate reduction rates are different at pingo 5 is still valid. Overall, the point is that pingo 5 is different from the other pingos. There is lower methane concentration in the sediment (whether this is strictly dissolved methane or not), there were no hydrates recovered from pingo 5, there are no prominent seismic chimneys below pingo 5, sulfate flux rates were lower at pingo 5, and no gas flares rising into the water column were seen at pingo 5. Together, these seem to indicate that the geochemical conditions at pingo 5 are different, which could account for the absence of frenulates. We even have preliminary results indicating that the microbial community at pingo 5 is different, including that ANMEs make up less of the total bacterial/archaeal community at pingo 5 (Klasek et al., in prep, which is referred to in the manuscript), which further supports our argument.

In short, we believe that our overall conclusion, of different geochemical conditions at pingo 5, compared to the other pingos, is nonetheless valid, and could account for the absence of frenulates from pingo 5.

Sulphate reduction (p16): “In most cores, the ratio of inorganic carbon to sulfate consumption was found to be close to 1:1 regardless of depth (both GHP5 and GHP3). The one exception was core 1048 from GHP5, for which, almost all values were closer to the 2:1 ratio.” Core 1048 is shown in Figure 1 to be the furthest away from any pingo, i.e. it is in background sediment. Thus it should be no surprise that in this core sulphate reduction is not dependent upon the presence of methane.

Response: This is correct.

“The dual need for inorganic and organic carbon sources (plus thiotrophic chemoautotrophy) likely results in frenulates **in general**, and, *O. haakonmosbiensis* specifically, relying heavily on a highly active sediment methanotrophic microbial community” (p16 line 31). This is not true as a general statement. Dando et al. (2011), in a study of the relationship between 10 species of frenulates and the chemistry of their habitat, found that, with the exception of 1 obligate methane seep species, all occupied sediments where the dissolved methane concentration was $< 1 \mu\text{M}$.

Response: We did not phrase this correctly. The idea was to introduce a hypothesis, that *O. haakonmosbiensis* relies on an active microbial community because they might be obtaining their nutrition from both their symbionts and the surrounding sediment. We can rephrase this.

“On the other hand, at GHP5, seepage of methane is low due to the lack of a sub-surface gas chimney. Methane is still present in the sediment, but in lower concentrations and as a result, methanotrophic microbes are likely less abundant and methanotrophic activity is considerably lower, as evidenced by lower AOM linked sulfate flux rates” p17. As mentioned earlier the authors do not know the concentration of available methane in the sediment and hence cannot make such comparisons regarding different methane concentrations at different sites. The values in Figure 4 may just equate to the amount of authigenic carbonate in the samples. A small gas chimney appears to be visible below GHP5 in Figure 6.

Response: As stated above, we disagree that we did not measure dissolved methane. Additionally, we must emphasize that the presence of gas hydrates was observed in most of the sediment cores recovered from pingos 1-3, while no gas hydrate was recovered from any of the sediment cores in pingo 5. Dissolved methane concentration must be high enough (i.e., at saturation level) to allow for the presence of gas hydrates, which is the case for most the sediment cores, but not pingo 5. This is quite solid evidence to support our inference and therefore contrasting methane concentrations between pingo 5 and the other pingos.

We do not understand the rationale behind “methane concentration may just equate the amount of authigenic carbonates in the sediments”. Precipitation of authigenic carbonates depends on the saturation state of carbonate minerals, which is a function of the availability of DIC, calcium, and magnesium in the porewater. The supply of calcium and magnesium in the porewater is independent of the supply of methane in the sediments. Of course a faster turnover of methane through AOM can accelerate the precipitation of authigenic carbonate precipitation but there is no sign showing the absolute amount of methane and authigenic carbonate should be in any way be related.

As discussed above, despite there being some blanking under pingo 5, the seismic data indicates that there are pockets of gas, but not necessarily active gas migration. Furthermore, no hydrates were recovered from pingo 5, which also suggests less seepage there. Combined, we believe that this suggests that dissolved methane concentrations are lower in the sediment at pingo 5, irrespective of whether one is convinced we measured dissolved methane in our samples or not. In any case, sulfate flux rates were measured independently of methane, and they suggest lower methanotrophic activity at pingo 5, which is the main crux of our argument.

The discussion regarding hydrothermal vents is not very appropriate for this paper with respect to *O. haakonmosbiensis*. This is a cold-water species that penetrates approximately 0.5 m into the sediment. At vent sites the temperature within the sediment would, almost certainly, be lethal for the species.

Response: We were referring to the ‘lower temperature’ zone of seeps where *Sclerolinum* is found, but we agree, we can remove the discussion related to hydrothermal vents.

Although *O. haakonmosbiensis* was the only metazoan with chemoautotrophic symbionts found, it does not mean that it was the only one present, since the infauna, where, for example, other frenulates and thyasirid and lucinid bivalves might be expected, was not studied. It is therefore also not correct to state that “the community at the pingos does not contain specialized seep endemics” (p22 line 13) since the infauna were not studied and *O. haakonmosbiensis*, if distinct from *O. webbi*, is probably a seep obligate species. “Endemic” is incorrect in this context since it refers to geographic regions, not habitats.

Response: We can change to say seep specific or seep obligate. And yes, we agree that there might be infaunal community members that are seep obligate, and we do mention this in the text (thyasirids). Once again, we will have to make sure that we clearly state that we are talking about larger, visible fauna. We acknowledge that the frenulates at the site might be seep obligate (e.g., page 19, page 12). But we can also change any discussions about seep obligates in the overall community and make sure that we do not say that they are absent or completely lacking, but rather, that only one species has so far been seen (and again, make sure that the scale we are referring to is large animals visible in images).

Figure 1

It would aid interpretation if the positions of the observed gas flares were pinpointed in figures b-e.

Response: We can add these.

Figure 2, 8 & 9

These would benefit from scales in the camera pictures, since the laser spots, when present, are difficult to see.

Response: We can add them.

Figure 3

Figure 3b has TC21 and TC18GHP3 plotted on top of each other, including the labels, so it is not clear what this point represents.

Response: This is because they are so similar, that they end up being right on top of each other. We can include an explanation in the figure caption.

Figure 4

The lines after the final points appear to be extrapolated randomly. If this is because the graphs are part of larger ones and have been cut off at 60 cm then it would be sensible to give the depth and values of the next points in parenthesis at the end of the lines. The coloured open circles are not well differentiated at the scale of the Figure and should be replaced by coloured filled circles to differentiate the cores.

Response: We can make these changes.

Figure 6

I think the vertical scale is m depth below the sea surface and not sediment depth. Fig, 6b is too small to be useful without enlargement.

Response:We can make 6b larger. Yes, the vertical scale should be meters below sea level and we can change this.

Discussion

The term “megafaunal taxa” is used in the Discussion. Megafauna are large animals such as cetaceans and large fish. The correct term for the taxa observed is “macrofauna”

Response: The distinction between megafauna and macrofauna is somewhat subjective and different people have different opinions on how to use the two terms. We use megafauna for this manuscript since we refer to animals large enough to be seen easily with the naked eye. We consider macrofauna to be smaller animals that are retained on a 0.3 mm or 0.5 mm sieve (this cut off seems to vary between studies) and are not easy to see through imagery. This definition is certainly subjective as well, but it is in accordance with many other similar seep and vent studies and we chose to use this terminology to be consistent with other studies with similar methodologies (Amon et al., 2017; Baco et al., 2010; Bowden et al., 2013; Hessler et al., 1988; Lessard-Pilon et al., 2010; Marcon et al., 2014; Podowski et al., 2009, 2010; Rybakova (Goroslavskaya) et al., 2013; Sellanes et al., 2008).

P14 lines 20 & 21, Fig. 2 should read Fig. 3

Response: Thanks for pointing this out, we can change this.

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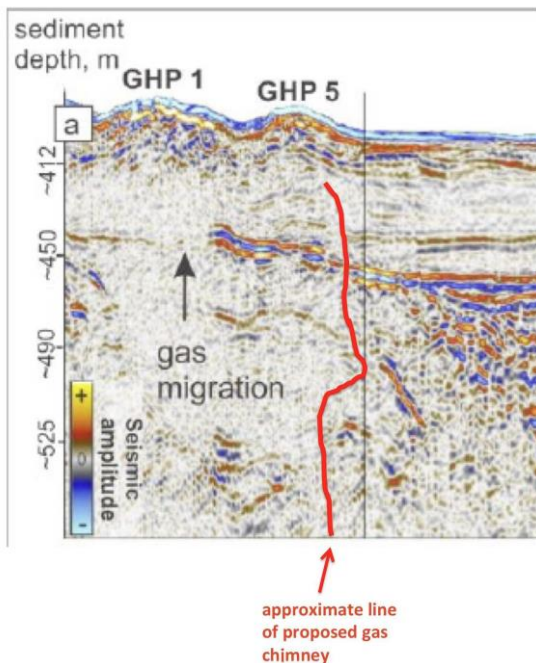
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Figure 6 section



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