Comment on bg-2017-540 Geophysical and geochemical controls on the megafaunal community of a high Arctic cold seep

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This manuscript presents data on the megafaunal community structure associated with cold seep sites in the western Barents Sea inferred from high resolution seabed imagery, and relates these data to available geochemical information. The manuscript is well-written, and as information on the ecology and biogeochemistry of cold seeps in the Arctic is still quite rare, it makes an important contribution to the field.

Response: Thank you for appreciating the manuscript.

At the same time, there are some methodological constraints that limit the interpretability of the data. In particular, the conclusions drawn with regard to microhabitats are in my view not fully supported by the available data and the respective part of the discussion could be shortened.

Response: We hope that we have addressed your specific concerns adequately below. Please note that we were not referring to or studying microhabitats in this manuscript. We were mainly looking at larger scale patterns and processes, as opposed to small scale variability in habitats and conditions.

Seepage rates/ volumes at cold seep sites often vary strongly over small spatial scales, and the seafloor pictures shown in this ms indicate that the same seems to be true true for the GHPs under study, with patches of bacterial mats, for example, probably indicating higher than average methane/ sulphide availability. In fact colonisation by bacterial mats and certain megafauna species (Calyptogena, Acharax; siboglinids etc), are often linked to relative flux rates (e.g. at HMMV or Hydrate Ridge) and can even serve as (crude) indicators for seepage intensity. The limited number of sediment cores taken in this study (and I fully accept the limits that can be placed on sampling in Arctic and deep waters) will likely not have been sufficient to resolve this spatial pattern which is likely to impact on microhabitat preferences.

Response: We completely agree that seepage and concentrations/fluxes of different compounds demonstrate heterogeneity over very small spatial scales at chemosynthesis based habitats such as seeps. We also agree that animal and microbial distributions can therefore serve as crude indicators of seepage patterns. However, as mentioned in our response to Dr. Dando, we did not attempt to use the images or the cores to examine patterns over small spatial scales. Our aim was to compare the different gas hydrate pingos, specifically to compare pingo 5 to the other three pingos, which are on the scale of several hundred meters in diameter. Similar numbers of cores were taken from pingo 5 as well as from the others, and we believe that these cores are representative of conditions at those pingos as a whole, which is what we examine and discuss. Furthermore, we used additional lines of evidence, in addition to geochemical measurements, to support our conclusions of inter-pingo differences, such as presence/absence of gas flares and seismic data that demonstrates different types of sub-surface gas/gas hydrate reservoirs.

An analogy can perhaps explain this issue. Mountain tops and valleys have very different kinds of vegetation because they experience different environmental conditions overall, and this difference corresponds to what we were demonstrating, i.e. that pingo 5 has a different animal community from the other pingos that corresponds with overall different geochemical conditions. For example, high altitude trees, say, pines, might be absent in the valleys, which is similar to the siboglinids being absent from pingo 5. Within a mountain top or a valley, there are additionally small scale differences in species distributions related to various factors, such as soil type and quality, the presence of streams or rivers, steepness of slope, etc. Similarly, within a pingo that contains siboglinid worms, small scale differences can determine if the worms are present in a location or absent a few centimeters away. We would like to emphasize that we are not tackling this second question and we agree that our methodology is insufficient for doing so.

In addition, temporal variability of seepage - even at sites considerably deeper than those under investigation here - is often strongly linked to tidal rhythms and longer term observations are likely to be necessary to conclude with certainty whether active seepage occurs at a specific site or not.

Response: It is true that we cannot assess temporal variability in seepage. However, as addressed in Dr. Dando's review, in terms of gas flares, we are confident that pingo 5 does not show any flare activity because this data was collected over multiple years, across different seasons, and each time, over long periods of time that well encompassed tidal rhythms. Furthermore, we do not say that pingo 5 exhibits no active seepage in the sediment, quite the contrary, in fact, we argue that pingo 5 does show enhanced flux of methane and sulfide , which could even account for the bacterial mats seen there (page 11, lines 18-19). Instead, what we argue is that methane flux, sulfate flux and sulfide flux and therefore AOM rates, are lower at pingo 5. In addition, we did not recover any gas hydrates from the +5 gravity cores at pingo5 whereas we recovered gas hydrates from the gravity cores from the other pingos. Such difference in gas hydrate abundance also clearly demonstrates the contrast in methane supply from pingo 5 compared to the other pingo features.

In this context I was also surprised to see that all pictures taken along each of the specific transects seem to have been 'lumped together' for analysis and no attempt was made to distinguish between fauna at more or less active seepage sites.

Response: In fact, this is what we did and was the aim of the manuscript: to compare the 'less active' pingo 5 to the 'more active' pingos 1-3.

Judging from fig. 1, the photographic transects seem to have included (or at least could have been extended to include) reference areas without seepage and I was disappointed to see no comparison between fauna at seep and reference sites. Is there a reason for this ? This would add a very valuable and relevant dimension to the manuscript.

Response: We did compare with images that were far enough away from the pingos (at least 1 km away) to be considered more representative of 'reference' sites. However, bacterial mats were nonetheless seen in this area and since we are unaware of the extent to which lateral diffusion takes place, we cannot confidently state that any of our image transects included true reference, non-seep impacted areas. That is why even though we did make some comparisons to 'non-seep' areas, we were very cautious with these comparisons.

While it is likely that AOM consortia provide the sulphide utilised by chemoautotrophic bacteria, it seems unlikely that sulphide can be both abundant enough to support the bacterial mats visible in the photographs while being removed so efficiently from the substrate that concentrations in the upper sediment and bottom water were below detection limit. Could this be an analytical error ?

Response: As we clarified for Dr. Dando, the detection limit was 40 µM. Bacteria can survive on lower concentrations, therefore, they might be present even when we are unable to quantify sulfide through our measurements. As we agreed to do, based on Dr. Dando's discussion of this topic, we will remove the section that says that sulfide flux to the bottom water is so low as to allow non-seep specific fauna from colonizing the area without getting poisoned by sulfide, since it is true that sulfide might be reaching the bottom water despite our inability to detect it.