Dear Dr. Dallimore

We sincerely appreciate the considerable time you have taken to review our manuscript. We are also grateful for the comments in the manuscript attachment which will undoubtedly improve the manuscript. While we cannot fix the data we have, I hope we can provide some answers and comments below. Your comments are in normal text and our responses are in bold. We have also attached a version with our responses in red to help distinguish comments from replies.

Many thanks and best regards

On behalf of the authors

Tom Birchall

Please note that our overarching response is at the end of this document. However, we have addressed some of the more specific comments below.

Comments from manuscript attachment

The comments within the supplement are extremely useful and we will make those changes as they will improve the manuscript. The only specific comment we address here is:

P 15 - There is no convincing evidence presented indicating presence of pf in nearly all these well presentations.

We completely agree – perhaps we were unclear with this but it was more to show that the petrophysics here is extremely challenged (or simply impossible)in identifying permafrost directly. The more important thing is to note the lithology (these wells were cored) and where gas influxes happened (e.g. in the middle of a sandstone with no other explanation for a top seal – including cementation etc. Cores were generally more useful than petrophysical data).

Unfortunately, as a permafrost scientist I was disappointed that the available data presented in the paper to demonstrate permafrost occurrence, ice bonding and associated permeability reduction is in my view rather weak and not adequate for publicatiion as submitted. I have studied well log interpretations of permafrost occurrence in hydrocarbon wells in the North Slope of Alaska, the Mackenzie Delta and the Beaufort Sea. As described in the paper there are many published studies from these settings using industry data to delineate well-defined occurrences of permafrost based using mainly well log resistivity and seismic velocity anomalies as well as in situ temperature measurements. Many of these wells have additional well log data and core samples that reveal that the permafrost occurs in mainly unconsolidated high porosity sediments, rather than in cemented bedrock. Based on the presentations in the Birchall et al paper it seems that the shallow sections of the hydrocarbon wells and scientific wells from Svalbard were not fully characterized by industry with the same rigor that we see in some other settings. This deficit in hard data is a challenge for the authors to overcome if they wish to present convincing and strong arguments in support of their conclusions. Indeed in the various well log sections presented in the paper, I did not see any examples that in my opinion yielded a high confidence assessment of ice bonded permafrost occurrence with multiple lines of evidence such as documented in other settings. Without well log indicators, the authors rely heavily on assumed variations in permeability expected from ice bonded permafrost to non-ice bonded sediments resulting in water influx into well bores or simply the occurrence of free gas.

We will provide information from the DH8 wellbore from the Longyearbyen CO2 Lab that was drilled and fully cored to characterise permafrost (but did not penetrate its base). We have provided temperature where it is available but see below for a more clear example from Beka et al. (2017). Note that we are very aware that we cannot directly see permafrost evidence in the petrophysical data (we have three experienced petrophysicsists who have tried), we elaborate on that later in this document.

424 T.I. Beka et al. / Journal of Applied Geophysics 136 (2017) 417–430

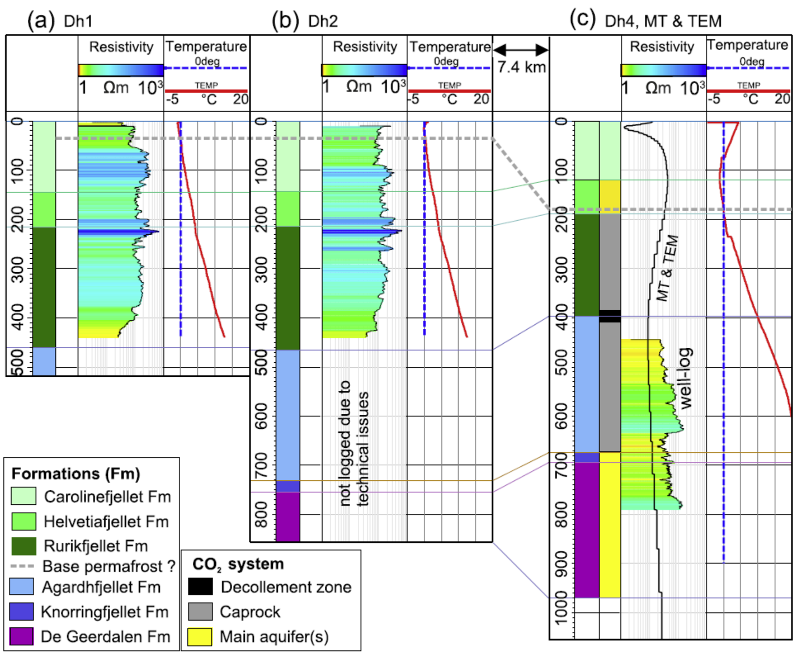
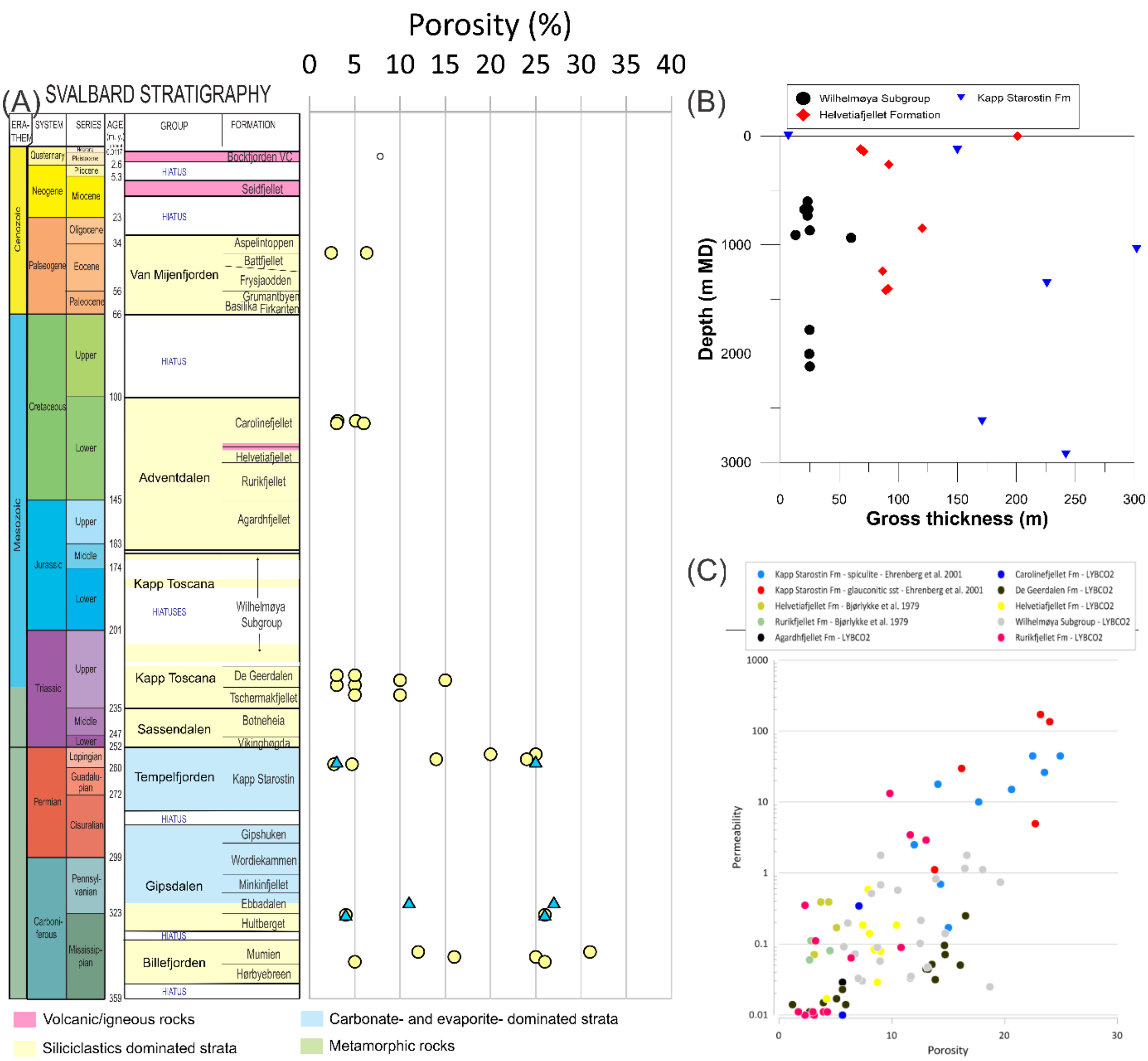


Fig. 8. Resistivity logs of Dh1 (a), Dh2 (b), Dh4 (c) boreholes (see Fig. 1b for locations) compared with the final 2D resistivity model averaged between the sites T08–s09 and presented as a 1D curve in (c). In addition, temperature log and lithology are displayed for each borehole. As the TEM and MT result, the shallow resistivity log from Dh1 and Dh2 indicate lower resistivity for the upper ca. 50 m depth and suggest a decreasing tendency below ca. 350 m in a similar way as the modelled resistivity result towards the décollement zone (the black section of Dh4"s stratigraphy). For more details on the wireline logging refer to Elvebakk (2010).

This association in itself is not an unreasonable expectation, however the authors have not given the reader the basic information they need to appraise these indicators. While there are some vague statements suggesting that the study wells penetrate low porosity bedrock rather than unconsolidated sediments, no data is presented on the petrophysics of bedrock occurrence in terms of porosity and permeability and only a few examples of pore water geochemistry.

We agree on this – we can provide more information on this from CO2 Lab data and publications (see below for an example of figure – made by K. Senger, one of the authors here). and we can add information on the poro-perm and fractures from Adventdalen Note that most data from the economic wellbores throughout Svalbard were not drilled by permafrost scientists, thus shallow pore water geochemistry data is not collected (however it is something that has been studied at pingo sites (e.g. Hodson et al., 2019 & 2020).



All of these factors are critical for the assessment of the manifestation of ice within the porosity of the bedrock setting or occurrence of ice in fractures limiting fracture permeability. I have not rejected the paper as it is my hope that the authors in a revision of the paper can provide an expanded assessment of the petrophysics of the setting they are studying. Hopefully bedrock mappers or industry scientists have appraised the porosity, permeability and fracture characteristics of bedrock occurrences. If these observations are available they should be described for the reader and interpreted within the context of the goals of the paper.

In addition, it is important to understand if there are any observations of ground ice form and occurrence that have been complied by surface mapping or in laboratory studies on core samples. Another concern is the confidence in assessment of the base of permafrost. No in situ ground temperature data are presented on permafrost occurrence and for the most part the authors have had to rely mainly on permafrost modelling by others or very weak estimates based on drill mud temperatures. I note that on Figure 3 for instance that the authors acknowledge considerable uncertainty in their estimates of base of permafrost.

We agree, the base permafrost depths are uncertain. However, we are more concerned with what is happening below the permafrost and not characterising it. This is very true, the base permafrost

estimates are highly uncertain and we address this later. We have also noticed that temperature data have become obscured in Figure 5 and needs to be fixed.

Gas and water anomalies in many wells are considerable distance below their high confidence estimates (see my notes on the pdf copy of the manuscript). This does not lend confidence to the assertions made in the paper as it may be that the indicators are not related to variation in ice bonding. Concerns related to the geothermal setting, pore fluid chemistry and petrophysics also in my view render the gas hydrate discussion in the paper as even more speculative than the permafrost discussion. Indeed in several places in the text the authors suggest that if gas occurred in hydrate form the volume of stored gas could be much larger than if the gas was free gas. This suggestion is in my view unsupported, leading to my suggestion that reference to gas hydrate occurrence in the paper be significantly scaled back.

We very much agree, and the permafrost and hydrate modelling parts will be significantly scaled back and only kept where we have reasonable calibration points in Adventdalen.

I close with a note of encouragement to the authors - the topic they are studying is important and certianly of interest to the readers of the journall, thus I hope that they can continue their study and advance this paper to publication. I have made some minor comments on the attached PDF that I hope will help point out to the authors my concerns with the present manuscript. These include a need for considerable effort to improve nearly all of the figures and the captions for the figures and tables. I have not dwelled on the writing however there is also a need to stream line the writing (perhaps by simplifying some of the extended discusison on the petroleum setting) and improve the consistency in terminologies referring to permafrost terminology.

We will cut back on the petroleum systems part and improve the figures as suggested.

Finally, I recommend that the authors more carefully assess references from the Mackenzie Delta. This could be done simply by using Googlescholar with a search for ‘permafrost occurrence Mackenzie Delta’. In particular perhaps the Geochemistry paper by Collett and Dallimore and Geology paper by Dallimore and Collett would be helpful as they provide some insights and observations of gas within and below ice bonded permafrost

Perhaps we were unclear in the aim of our manuscript and this is something we will rectify. The focus of this paper was intended to be to highlight gas accumulations and fluids below the permafrost in Svalbard, something that has not been documented before.

Perhaps we were not clear enough in the aims, and why it is important we utilise this vintage data - the petroleum wells were drilled between the 1960s and 1990s and coal boreholes sometimes predate that. Very few recent wellbores penetrate the entire permafrost interval, and even fewer are likely to do so in future due to their high cost and the cessation of economic drilling in the archipelago. Although they were not focussed on characterising the base permafrost, they made many important observations, most notably large pockets of shallow gas (even blowouts) that could not be explained by conventional trapping methods. Because these data have not been previously looked and the topic of permafrost trapped gas has not been studied in Svalbard, which

is warming faster than anywhere in the Arctic. We feel it is important to disseminate it to the scientific community as a team of authors with a variety of geoscientific backgrounds.

While we agree that the details in the data are not heavily focussed on permafrost, it is to be expected when the vast majority of boreholes that penetrate to the base of permafrost were in no way targeting it. Only petroleum wellbores have collected petrophysical data, and rarely a full suite of logs over the shallow intervals. The coal exploration wellbores do not collect any petrophysical data, but do fully core them and record depths where gas influxes occur.

We should also be very clear that Svalbard's geology and geological history is very different to that of the McKenzie Delta or North Slope of Alaska. In Svalbard the base permafrost is invariably in bedrock that has previously been buried to several kilometres depth and subsequently uplifted. Therefore, the already highly lithified, low porosity and heterogenous rocks show very little petrophysical contrast to fluid type or phase changes, though still provide good indicators to lithology. It is not the same as ice-bonded hard vs unconsolidated sediment that we see in other parts of the world (and also provides challenges for geophysical methods). I can assure you we did look closely at the petrophysical data (we actually used your very nice McKenzie work to see if it could be applied here) – three of our authors have worked extensively with petrophysical data in our careers (one has just returned as a petrophysicist on the latest IODP campaign). Ultimately, we found that petrophysical data is only useful at determining lithology, not at identifying permafrost (but we can make a figure to demonstrate this if it is useful).

It is because of the differences in geomorphology and geological histories between Svalbard and the well-studied parts of North America that make providing these data and observations so important to the scientific community. Svalbard is representative of the high relief, rapidly uplifting, glaciated parts of the Arctic and this is the first such investigation on the subject of sub-permafrost trapped methane in Svalbard. As a scientific community we need insights into this gas in the literature, not just because it is a hazard, but because of its important link with better understanding glacial recharge and the geological controls that occur over longer timescales than glaciologists often consider (e.g. uplift, erosion, fracturing, abnormal fluid pressures). These are systems that we cannot develop an understanding of from areas with better and recent datasets such as the McKenzie Delta, Alaska's north slope, Siberian continental shelf environments.

By far the biggest strength of this drilling-focused dataset is in its diversity and sheer number of wells that have encountered shallow gas. When combining this with well reports, gas influx data (even blowouts), and the geology, we can eliminate alternative hypotheses (e.g., lithological seals). So, while there certainly is no individual piece of data that provides a smoking gun, the evidence is there, and we should probably be clearer in describing that.