Reviewer 1

The revised manuscript is generally acceptable, but some grammatical mistakes and errors (mixing up carbonate and bicarbonate in lines 530 ff) need to be corrected. See details below.

We would like to thank Prof. Teske for taking the time to review our paper again. We agree with his suggestions to improve the manuscript. Below are our point-by-point responses to his comments. The original comments are in regular font, and our responses are in italics.

Figure 2:

Are you sure that panel 2f (showing actual profiles) will appear in clear resolution?

***Answer**: We agree that the resolution of Panel 2f, which displays the actual profiles, was insufficient in the submitted revision. To address this, we are now providing the figure in a higher quality, ensuring clearer and more detailed visuals.*

Line 530 ff:

One sentence defines alkalinity as bicarbonate (HCO3-) concentration; but the next sentence talks about carbonate (CO32-) as the main contributor to seawater alkalinity but from its context really means bicarbonate. Please correct this.

"We established ratios of sulfate to alkalinity (HCO3-) fluxes (Fig. 5). As about 90% of seawater alkalinity can be contributed to carbonate (89.8% HCO3-, 2.9 % CO32-) (Kerr et al., 2021) we set alkalinity synonymous with carbonate concentration."

***Answer:** We appreciate your attention to this and have corrected the mentioned section, and we also checked the remaining manuscript accordingly. The revised sentence reads:*

*"*We established ratios of sulfate to alkalinity (HCO_3^-) fluxes (Fig. 5). As about 90% of seawater alkalinity can be contributed to bicarbonate (89.8% HCO_3^-, 2.9 % CO_3^{2-}) (Kerr et al., 2021)we set alkalinity synonymous with bicarbonate concentration."

Line 755: "suggests is" does not fit here in this form; was a longer sentence intended?

***Answer:** You are right. We missed deleting a part of the former formulation, and we have now corrected it in the manuscript.*

Line 765: "scarce methane data" means that very limited information about methane is available (few data points and measurements). You probably want to say that only small amounts of methane were detected. It is possible to have plenty of methane data, but they indicate that only small amounts of methane exist.

***Answer:** We agree with your comment regarding the term 'scarce methane data. To better reflect the findings, we have revised the wording to indicate that only small amounts of methane were detected in the samples, rather than suggesting that limited data were available.*

Reviewer 3

General comments

The manuscript by Schnabel et al. presents an impressive set of pore-water data and sulfate reduction rates as well as metagenomic and metascriptomic data for 50 (40?) sediment cores/gravity cores retrieved from the SW Barents Sea shelf. The objectives of the study are to assess how the magnitude of upward methane/hydrocarbon flux impacts the geochemistry, biogeochemical processes – namely microbial sulfate reduction - and microbial communities in the surface and subsurface sediments.

The manuscript is definitely of interest for the readers of Biogeosciences and generally well-written. However, there are several issues that need to be specified and described much more precisely – in particular the terms „seepage“ and „HC reservoir“. There are also several statements and concepts presented in the manuscript that are not correct as given (e.g. statements about seismics). The authors should definitely define the term „seepage“ and say what they mean when they speak of „seepage“. What about the activity/episodicity of any potential seepage? Please, precisely specify and distinguish whether you speak of transport of methane by molecular diffusion (as is obviously the case at most of your sites) or fluid seepage – i.e. migration of fluids and or free gas bubbles through the pore space of the sediments at rates exceeding those of molecular diffusion. In other words, if upward methane transport occurs in the form of molecular diffusion – as seems to be the case at most of your study sites – I would not speak of seepage. I would therefore suggest to more generally speak of upward methane „fluxes“ throughout the manuscript. The different intensities/magnitudes of methane upward flux then determine the depth position of the SMT and the magnitude of SR as well as the type of microbial community/ies.

I also did not fully understand which type of „HC reservoir“ precisely you speak of. This is also not clear from Chapter 2. Do you mean free gas in the deeper subsurface? What about gas hydrates? The potential role of the presence of gas hydrates in the subsurface – as an intermediate methane reservouir - has not been mentioned and discussed at all. There are numerous studies that have demonstrated that during active seepage events methane is transported upwards from deeper sources (mostly in the form of free gas) and becomes trapped in the form of gas hydrates at shallower sediment depth (if positioned within the gas hydrate stability zone). After these gas hydrate deposits have formed they give off methane, which diffuses upward towards the sediment surface and leads to the establishment of an SMT where AOM consumes most of the upward diffusing methane (e.g., Dickens, 2001, GCA; Lapham et al., 2010, EPSL). The methane gradient – thus magnitude of upward flux – and depth position oft he SMT then depends on the depth position of the gas hydrates.

Moreover, the referencing to previous relevant studies is also not sufficient. In the past 20 to 25 years numerous studies have been performed to investigate the regional variability of upward methane fluxes. There are for example several studies by the group of Gerald Dickens that have investigated differences in upward methane fluxes – for example on Blake Ridge and in other ocean areas. Also the impact that upward methane fluxes and in particular of AOM on the geochemical composition of pore waters and sediments – including mineral dissolution (e.g. magnetite) and precipitation of authigenic minerals – including carbonates, barite, Fe sulfides/rock magnetic properties is mostly missing (see suggestions given below). There are also several previous studies that have correlated pore-water profiles with micobial communities (e.g. Oni et al., 2005, Frontiers

Microbiol.; Wunder et al., 2021, ISME; Schnakenberg et al., 2021, Frontiers Microbiol.).

It would also be good to have a zoom-in map of the study area in order to have an idea of the bathymetry and seafloor topography. The insert shown given in Fig. 1 b is not very informative. It would be good to see seafloor topography/bathymetry in ordert o assess whether there are typical seep seafloor features of methane seepage such as pockmarks and to find out at which water depth the study sites are located (also with respect to judging whether the sites lie within the gas hydrate stability or not).

Answer: *We thank the Reviewer for taking the time to revise our paper. We have agreed with most of the suggestions to improve the manuscript. Below, we provide point-by-point responses to the Reviewer's comments. The original comments are in regular font, and our responses are in italics.*

Specific comments

L. 24 and throughout the manuscript: The term „inconspicuous“ is rather unusual in this context. Do you mean „low“ upward fluxes?
Answer: *We rephrased the sentence for greater clarity and took out the term inconspicuous, by using “small and often unnoticed upwards HC fluxes”*

L. 30 Do you mean constant/no depletion instead of „linear“

Answer: *To clarify, by "linear profiles," we are referring to regression lines through measurement points that demonstrate a clear trend of either increasing or decreasing with depth. This explanation should address any confusion regarding the terminology. It’s important to note that "linear" does not imply "no concentration change with depth." A quick image search for "linear profile" shows many examples with values that increase or decrease, confirming that the term is appropriately used. As such, the text remains unchanged, as we believe the term is sufficiently clear.*

L. 40: What precisely do you mean with „inconspicuous HC seepage“ ? I would rather speak of low methane „fluxes“. i.e. diffusive flux.

Answer: *similar to L24, was changed and is now defined*

L. 41: „shallower“ than what precisely?!

Answer: *When we refer to "shallower" we specifically mean that sulfate depletion occurs at depths that are shallower than those typically observed in comparable, non-affected sites (i.e., sites not impacted by hydrocarbon (HC) seepage). This shift in depth is a direct consequence of the HC seepage, which influences the biogeochemical gradients in the shallow subsurface. To improve clarity, we have revised the wording at this point.*

L. 46: What precisely is a „minor“ seep ?

Answer: *In response, we have added clarifying terms to the sentence to better explain the nature of minor seeps. Specifically, we now describe these seeps as "characterized by low, primarily diffusive HC fluxes" to make the distinction clearer. We hope this improves the clarity of the statement.*

L. 48: The effect of upward diffusion of methane and resulting AOM on sediment geochemistry has been shown and reported by numerous studies: including Riedinger et al. – and Henkel et al. etc.

Answer: *some references were added*

Ls. 49 to 51: These sentences are unclear. What precisely is a „distal manifestation“? „seabed“? I guess you mean sediment surface, right?!

Answer: *In response to your comment, we have rephrased the entire paragraph to avoid any misunderstandings.*

L. 52: I guess you mean „geochemical“ instead of geological, right?!

Answer: *Y*es, we agree—it was a mistake, and we have changed 'geological' to 'geochemical' in the manuscript as suggested

L. 54 ff.: The statement – as it stands here – is not correct. It is not methane-containing fluids that produce seismic signals but the presence of free gas that induces an impedance contrast that is registered by seismics. Fluids of high dissolved methane concentrations are not detectable by seismic approaches. Please, rephrase this more precisely throughout the manuscript.

Answer: *Thank you for your valuable feedback. We agree with your comment that it is the presence of free gas, rather than methane-containing fluids, that produces the seismic signals. We have rephrased the manuscript accordingly to more precisely reflect this distinction.*

L. 58 ff.: Strictly speaking, it is not the flux of methane itself but the consumption of methane/HCs in the process of AOM that impacts the geochemical composition of pore-waters and sediments. This has already been demonstrated by numerous studies, some of which should definitely also be refered to/cited here. So, in addition to what you describe/refer to in this part of the introduction you should at leasdt also mention a few of the most prominent impacts of upward methane flux and the resulting oxidation of methane by sulfate (AOM) . namely the precipitation of authigenic carbonates and barite (e.g. studies by Bohrmann, Torres, etc.) and also the dissolution of magnetite producing distinct minima in magnetic susceptibility (e.g. Riedinger et al., 2005; März et al., 2008).

Answer: *We agree that metabolic reactions and their products are primarily responsible for the majority of changes, and we have updated the text accordingly. However, we did not delve into the prominent manifestations of HC seepage at this stage of the introduction because we believe that if seepage is low, the larger manifestations will also be minor. We discuss AOM and carbonate precipitation in later sections of the introduction and the discussion. Therefore, we made only minor adjustments to the wording at the beginning of the paragraph and have also decided to include some relevant references at this point.*

L. 62/63: I also do not agree with this statement …. Methane formation is extremely widespread in continental margin sediments – and in most cases is not associated with underlying HC reservoirs but rather formed in situ by biogenic processes.
Answer: *Unfortunately, we cannot follow your problem. We cannot see the connection of your argument (which is correct) to our text.*

We agree that the relative importance of sulfate reduction can vary depending on the specific environmental context. In response, we have clarified in the manuscript under which conditions sulfate reduction is the most important anaerobic organic matter degradation process. Additionally, we have incorporated several recent studies.

L. 70: This is not entirely true … rather depends where you are. There are also more recent studies on the role of sulfate reduction (e.g. Bowles et al., 2014).

Answer: *We agree that the relative importance of sulfate reduction can vary depending on the specific environmental context. In response, we have clarified in the manuscript under which conditions sulfate reduction is the most important anaerobic organic matter degradation process. Additionally, we have incorporated several recent studies.*

L. 90: Please, also give reference to other relevant previous studies – e.g. Niewöhner et al. (1998; GCA), Treude et al.; Riedinger et al. (2005, 2014, 2017), März et al. (2008), Henkel et al. (2012; GCA).

Answer: *Thank you for your helpful suggestion. As recommended, we have added additional references to strengthen the manuscript.*

L. 113: This statement contradicts that in the abstract. Here you speak of 40 gravity cores while in the Abstract you mention 50 gravity cores.

Answer: *We have adjusted the sentence. There are 50 cores in total, 40 from HC-affected sites and 10 from reference sites*

Figure 1: The zoom-in in Fig 1b is not informative at all. It would be good to have a map showing the bathymetry/seafloor topography. The map also does not indicate where the potential „HC reservoirs“ are found in the deeper subsurface.

Answer: *We added a bathymetric map to the figure.*

Figure 2 is very difficult to read and understand. What precisely is shown in Figs, 2a to 2d? These are definitely not measured pore-water profiles … are these modelled profiles or gradients? Please, specify and overhaul this figure as well as the figure caption.
The title of this figure says „sulfite“ ?! I guess you mean sulfide, correct?!

Answer: *You are correct that the figures in Figs. 2a to 2d do not represent measured pore-water profiles. These plots show the modeled profiles (or modeled gradients) based on the linear regression analysis applied to the measured data. For a better understanding we changed the figure caption. We also corrected the spelling mistake for sulfide.*