

Interactive comment on “Low methane concentrations in sediment along the continental slope north of Siberia: Inference from pore water geochemistry” by Clint M. Miller et al.

Clint M. Miller et al.

cmm10@rice.edu

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We thank the referee for the time and energy to write a lengthy review. However, while some comments seem reasonable, we find the overall review largely unfair and non-constructive. In Sweden, a well-known adage is “som fan läser bibeln”, which describes how someone reads and misinterprets text so as to fit his/her views or intentions. We state this because, for the most part, the review is a string of comments that are either not germane to the primary subject of the manuscript or misguided in thought.

We note that there is no criticism on the importance of the topic, no criticism on the writing, no criticism on the data generated, and no criticism on the novelty of the work. Instead the criticisms, in a general sense, are:

(1) The manuscript does not contain additional data, even though it is one of the most comprehensive pore water chemistry data sets within a single effort generated to date in any region, let alone from a previously virtually unexplored section of the Arctic Ocean;

(2) The pore water results cannot be used to understand methane abundance in shallow sediment, although numerous papers in multiple regions, including in the Arctic offshore Alaska, convincingly demonstrate the opposite (eg. Borowski et al., 1996; Jørgensen et al., 2001; Torres and Kastner, 2009; Treude et al., 2014; etc.)

(3) The primary interpretation and conclusion conflicts with previous speculations in the region, although no pertinent information to the problem exists beyond the current work.

We also note that at least one criticism comes from “out of nowhere.” Page C4 the reviewer berates the authors for “vigorous” referencing of a study (Nauhaus et al., 2002) that we neither cite nor discuss in the MS.

We thus suggest that such a review arrives because the results and interpretations disagree with pre-conceived but wholly unconstrained concepts concerning methane on the continental SLOPE north of Siberia. We actually chased the research, in a very similar style to that done offshore Alaska, fully expecting strong pore water evidence for high methane concentrations across this region. However, the data absolutely do not support this.

We elaborate on the above with point-by-point responses below. We can and will correct certain portions of the manuscript as necessary. In the end, however, we largely disagree with the contents and tone of this review, and trust that the editors will also realize these issues.

We welcome additional clarifications from the referee, should this person wish to expound. We put considerable time and energy into generating the data and writing the

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manuscript, and stand by the main body of work, including the general interpretations.

BGD

Sincerely,

Clint Miller, and co-authors

Referee Comments (#) with Direct Responses (**).

#However, this ms rather demonstrates that the current state of knowledge of pore water biogeochemistry in particular areas of the ocean is very incomplete; a great deal of effort will be required in order to improve our understanding of the relationship between sulfur and carbon cycling in the Arctic.

** We do not follow this comment. We strongly suggest that the community knows a great deal about pore water chemistry, especially in regions that contain gas hydrate (Borowski et al., 2000; Torres et al., 2004; Treude et al., 2005; Dickens and Snyder, 2009; Coffin et al., 2013) albeit some of the details remain incomplete. This is stressed in the MS.

#The authors of the ms come to the following conclusions: 1) Based on interpretation of the pore water profiles, they found no evidence for upwardly diffusing CH4. 2) Based on these data, they strongly suggested that gas hydrates do not occur on the slopes of the ESM. 3) They claimed that previous investigators who suggested that hydrate deposits exist in the Arctic shelf/slope based on results of their investigations were simply wrong.

** This can and should be clarified further in the text. We strongly suggest that WIDESPREAD gas hydrates do not occur on the slopes of the ESM as speculated by previous papers. Here, it is important to stress, as discussed in the manuscript, that prior to our work, there is essentially no information on the topic from the studied area, and all has been conjectural. Our results and interpretations DO NOT CONFLICT with any previous data or direct results from the region that we are aware of.

#First of all, I do not understand why, when reporting low CH4 concentrations and

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comment

the relationship between CH4 and sulfate dynamics in the pore water, the authors did not measure the concentrations of either parameter. Is it not logical to measure CH4 and sulfate in pore water if one is going to report “low methane concentrations in the sediments”? These are rather routine measurements.

** Not all measurements could be generated given the limitations of the expedition and subsequent funding. However, we did measure the S concentrations of pore water, and know these are representative of sulfate, because we checked for hydrogen sulfide as well as measured dissolved barium. Moreover, as the referee almost assuredly realizes, there are problems with generating quantitative dissolved methane profiles in marine sediment because of degassing associated with changes in pressure and temperature.

** Here, it is absolutely crucial to realize that, as stressed in the text and at least to our knowledge, no region with significant methane in moderately shallow sediment (< 500 m) has high dissolved S/dissolved sulfate in pore waters near the seafloor, as well as other certain chemistry documented here. While not stated in the MS, this also includes localized areas of high advection and methane venting.

#The authors referred to other researchers in their ms to present supportive arguments, but none of these referenced studies avoided taking measurements.

** In fact, a significant fraction of the research referenced was partially generated by one of the authors (Dickens). It should be noted that the measurements were not avoided in some means to hide information, but rather that we know how methane exists and cycles in many regions, and the most prudent means of tackling the problem at a first-order level over an immense area from an ice-breaker is to generate numerous detailed pore water profiles (Borowski et al., 1999; Snyder et al., 2007; Hu et al., 2015).

#In addition, the authors of this ms speculate about the particulate organic carbon (POC) and OC content of sediment, but did not measure either parameter. OC content of sediments should be reported as a number of different carbon stocks, not just POC.

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** This is an odd comment because, as abundantly clear from numerous studies (Borowski et al., 1996; Dickens, 2000; Hensen et al., 2003; Geprags et al., 2016), the abundance of methane in shallow sediment on continental slopes does not depend on the current supply of POC, but rather on the integrated input of POC over long time intervals (e.g., hundreds of thousands to million year time scales), which we cannot assess without drilling. We are not sure what the referee means by different stocks. Basically, the data could be generated, but it is irrelevant.

#I do not understand why the presence or absence of CH4, either in the sediments or in the water column in this area, should be necessarily connected to the existence or non-existence of hydrates. Are hydrates the only possible source of CH4 in the Arctic shelf/slope? I believe not; hydrates could be only a tiny fraction of the source, because the hydrate stability zone (HSZ) created by P/T conditions could compose only a small fraction of the sedimentary drape (a few hundred meters), while the sedimentary drape could be a few kilometers thick.

** We do not understand this comment. We think that it belies some misunderstanding on how methane occurs on continental slopes in general, as well as misreading of the MS. First off, gas hydrates are not a source of methane, but one phase of methane in open and dynamic systems, where methane carbon can exist as dissolved gas, free gas, and gas hydrate.

** We cannot link shallow water chemistry profiles to methane abundance at truly deep depths, and we did not do so in the MS. However, truly deep methane cannot exist as gas hydrates, for reasons of P/T conditions.

** On the other hand, the presence of gas hydrate in the upper few hundreds of meters of sediment is absolutely related to total methane concentrations in pore space, which are linked to shallow sediment through diffusion or in some cases advection.

** We thought that these points were clear in the literature as well as in the MS, but could rephrase things to clarify with some guidance.

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[Discussion paper](#)



Interactive
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#Third, the purpose of this massive manuscript is not clear to me. This paper is flooded with equations and details devoted to methods, but mathematics, first of all should be applicable; then, the accuracy of mathematics does not aid in interpreting the inconclusive data.

** We do not understand this comment. We thought the purpose was very clear: we know how pore water chemistry profiles look above gas hydrate systems at numerous locations around the world, and we know how to interpret them at a basic level; we generated such profiles in the region of interest; the pore water profiles do not conform to those at any region where significant methane occurs in shallow sediment nor our understanding as to why such profiles arise.

#The methodology chosen by the authors of this ms and their level of understanding of the processes they were trying to investigate are my greatest concern. Biogeochemists working in the marine ecosystems have already gained some understanding of the fact that biogeochemical processes associated with diagenetic transformation of organic matter under anaerobic conditions in marine sediments are very complex microbe-mediated processes. These processes involve microorganisms from various physiological groups: aerobic and anaerobic saprophytic and cellulose- degrading bacteria, sulfate reducers, methanogens, denitrifiers, and methylotrophs. Transformation of organic matter is a multi-stage process: primary anaerobes decompose polymeric compounds to monomers, which, in turn, serve as a substrate for fermentation agents and gas-producing bacteria. A general conclusion is that the major fraction of OC preserved in the sediments is oxidized to CO₂ by the sulfate-reducing bacteria (SRB) and that 2 moles of OC are oxidized for every mole of sulfate reduced: $4H(CH_2)_n COO^- + (3n + 1)SO_4^{2-} + H_2O \rightarrow (4n + 4)HCO_3^- + (3n + 1)HS^- + OH^- + nH^+$. When acetate is oxidized completely, the atomic ratio of OC oxidized to sulfate-S reduced is 2 : 1. However, as 'n' increases, the C: S ratio changes; the ratio between the reactants could be different because it depends on the varying nature of the organic matter (Lerman 1982). This is because most of the photosynthate is not immediately available

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for oxidation; only the low molecular weight (LMW) fraction of dissolved OC (DOC) is rapidly oxidized by SRB, while the high molecular weight (HMW) fraction of POC, which usually increases with depth, is refractory. There are severe restrictions on microbial activity other than substrate availability, including that SR as a biotic process may be more strongly coupled to mineralogy (Ivanov et al., 1989). The knowledge that has been accumulated by scientists so far is very limited and only applicable to those particular ecosystems which were investigated beyond the Arctic.

** We do not know how to respond to this comment, as it mostly does not pertain to our MS, and also belies faulty logic. At a root level, and as best as we can ascertain, the comment suggests that biogeochemical processes are so complex that the community cannot obtain overall net chemical reactions and flow of carbon from pore water chemistry. If we correctly understand the comment, we then return an obvious question: how and why can the community measure similar pore water chemistry profiles at myriad locations and see basic commonalities (e.g., the absence of sulfate above sites with the presence of significant methane below), irrespective of the specifics and microbiology involved?

#The most reliable method to trace the course of sulfate reduction in sediments uses radioactive sulfate (^{35}S). By the use of this method it was shown that most reduced ^{35}S -sulfate was in pyrite and organic sulfur (Lein et al., 1982). The relationship between sulfur and carbon cycling in the Arctic marine systems is even more complicated, because the relationships between the sites of primary production and the sites to which organic matter is translocated and deposited, including organic matter delivered to the shelf/slope from surrounding land, are difficult to establish both qualitatively and quantitatively.

** We do not know how to respond to this comment, as it mostly does not pertain to our MS, it begins with a statement for which we disagree, and it does not make sense in its entirety.

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#A recently published review of CH4 emissions from the seafloor in the Arctic Ocean underscored that the role of SRB in the anaerobic oxidation of methane (AOM) is unclear and the ecology of AOM communities, particularly for high-latitude environments, is not well understood. For that reason, predicting CH4 fluxes, especially those related to hydrate dissociation, remains highly speculative (James et al., 2016). This is because CH4 is transported within the sediments in two different ways: as a dissolved phase (by diffusion or advection) or as free gas (ebullition). Free gas is inaccessible to microbes, which depend on a diffusive transmembrane gas transport. This means that release of free gas through the sediments might not leave any traces in the pore water (see Fig.5 in James et al., 2016).

** We do not know how to respond to this comment, as it mostly does not pertain to our MS, but we know the topics very well.

** The referenced MS (James et al., 2016), which we were unaware of when submitting our MS but have read since, by no means conflicts with our interpretations. These authors clearly indicate that AOM is a dominant process above methane-charged systems at steady-state conditions, and should impact pore water S/sulfate gradients (e.g., the very Fig. 5 that the referee emphasizes).

** Far more crucially, we are not concerned in our MS as to how methane would escape the seafloor via ENHANCED gas hydrate dissociation in the future, but whether significant methane exists in shallow sediment on the SLOPE of the ESM in the first place (i.e., at present-day), especially in the form of gas hydrate.

** We entirely agree with the comment that predicting the fate of gas hydrate dissociation on this margin (or indeed, any margin) in the future (or past) is highly speculative, as it depends on several factors, as also stressed by, for example Dickens (EPSL, 2003) or Stranne et al. (G3, 2016). But our MS does not discuss this aspect.

** We absolutely disagree with the comment that passage of free gas through sediment does not leave traces in the pore water. The paper and figures by James et al.

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(2006) by no means suggest this concept, and rightfully so. Pore waters in areas where methane advects from below at high rates, such as along faults and fractures (their Fig. 5), have truly different chemistry than seawater and anything in our results.

** All the above stated, we are more than happy to include and reference the paper by James et al. (2016).

#Moreover, recently published observational data show that in the Arctic environment, for example in the Alaskan Beaufort Sea continental margin sediments, substantial (30-500 μM) concentrations of sulfate can remain below the sulfate-methane transition zone (SMTZ) although mass balance cannot explain the source of sulfate below the SMTZ.

** This is an odd comment. First, 30 to 500 μM ($=0.03$ to 0.5 mM) is not substantial, compared to the ~ 28 mM in typical seawater. Second, there are at least three known reasons for this occurrence. (1) Pore water contamination, (2) hydrogen sulfide oxidation, and (3) barite dissolution. Third, mass balance always applies.

#In addition, sulfate reduction and anaerobic oxidation of CH₄ can occur throughout the methanogenic zone. Experimental data indicated decoupling of sulfate reduction and AOM and competition between sulfate reducers and methanogens for substrates, suggesting that the classical redox cascade of electron acceptor utilization based on Gibbs energy yields does not always hold even in diffusion-dominated systems (Treude et al., 2014).

** Yes, this may be true at a detailed and microscopic level, as pointed out by numerous authors, but not at any macroscopic level, at least that we are aware of, excepting odd environments (e.g., brines). The true beauty of pore water chemistry in the deep-sea marine environment is the remarkable consistency of multiple constituents linked to an array of environments. To restate from above, and in the text, all pore waters in methane-charged systems on continental slopes that we are aware of have certain commonalities – none that are seen in any of the pore waters generated in this study.

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[Discussion paper](#)



Interactive
comment

#Although they vigorously referred to Nauhaus et al. (2002) as a proxy-establishing experiment, the authors did not give this work any critical assessment. If they had done so, they would have definitely questioned the claim that methanotrophic communities associated with SRB oxidize CH₄ anaerobically in a 1:1 ratio to sulfate reduction. How that could be possible if the reported 4-5-fold increase in H₂S production (accumulated over 80 days!) was accompanied by an increase in CH₄ concentration of 3 orders of magnitude (from 0.01 to 15.8 mM)? Besides, rates of SR were so small (0.5-3.0 μ M/d-1) compared to the concentrations of sulfate (103-1.55 μ mol/L) that the question arises: How could this little change be reliably measured (without using the ³⁵S method, which they did not) and related to AOM?

** We have absolutely no clue from where this comment derives. We neither cite nor discuss the paper by Nauhaus et al. (2002). (Seriously, at this point and given previous comments, we are wondering if the referee even read our MS!) The nominal 1:1 ratio of methane and sulfate consumption across the SMT comes from numerous studies of pore water chemistry gradients at numerous locations once the chemistry gradients are placed into a flux domain (rather than simply concentration gradients). By no means does this argument hinge on the experimental results of Nauhaus et al. (2002), which incidentally claim such a 1:1 relationship. Should the referee have fault with the Nauhaus et al. (2002) paper, one might question the design of a laboratory experiment where the net process is examined over a very short time interval in a modified environment after collecting sediment from depth and under different P/T conditions.

#Not to mention that this effect has no applicability to the Arctic Ocean.

** We are not sure how to address. The referee seems to have a view that physical chemistry and biochemistry in the Arctic Ocean are somehow special, so that basics and inferences gained from elsewhere around the world do not apply.

** Here, it is especially important to note the paper by Coffin et al. (2013), as already highlighted in our MS. These authors characterized pore water chemistry in short sedi-

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Interactive
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ment cores above sequences with known gas hydrates along the shelf and slope of the Beaufort Sea (Arctic), very much as done in our MS. As predicted, they observed shallow SMTs indicative of a strong diffusive methane flux. As very much obvious in our work, their pore water profiles contrast with those from the slopes off northern Siberia.

#Another concern is this: How representative of the area are these data? Only four short transects consisting of 16 stations are presented; each transect is based on data from 2-6 stations. Data from only 2-4 stations represent all core depths. Core lengths vary from 1.95 to 8.43 m (mean length 5.25 m). Eight of the 16 stations are only represented by the very uppermost layers (from 0.16 to 0.39 m) of sediment collected by the multi-corer. These shortest parts are the most valuable as they represent the least disturbed environment, but they are too short to constitute any sort of conclusive data regarding CH₄ cycling in the sediments. I can only guess at how the authors succeeded in dividing these tiny cores into numerous parts, each 0.2-0.3 m in length, and accumulated enough data to compare these cores with one of two idealistic schemes to characterize the specific dynamics of processes occurring over a sediment depth of 100 m (Fig.1). Data obtained by other types of sampling (piston/gravity coring) should be treated and interpreted very cautiously as the cores are not only severely disturbed during the coring process, but also chemically altered as they are extracted from the sea floor and lifted onto the ship.

** This comment does not make sense. First, the fact that the pore water profiles give nice, detailed gradients in multiple species, demonstrably indicates that the cores have minimal disturbance. We can add core photos to our already long paper, if desired, to further emphasize this point.

** Second, the proposition that the uppermost part of a core is the least disturbed and most important to understanding processes is flat out wrong. This is because of the nature of coring, which tends to disturb (or in many cases not recover) the top few cms, and because of bioturbation and reoxidation.

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[Discussion paper](#)



** Third, the methodology for how the cores were sampled is detailed at length in the manuscript.

** Fourth, the link between shallow pore water chemistry and moderately deep methane abundance is well established, as we thought clearly articulated in the literature for almost 20 years and in the MS. Here, again, we stress the differences in pore water chemistry between the Beaufort Sea (Coffin et al., 2013) and the ESM. The pore waters of shallow sediment in the Beaufort Sea predictably support a high upward methane flux.

#Finally, the authors plotted water concentration profiles along each transect collectively (!) using colors and symbol types which make it virtually impossible to distinguish between these symbols, making interpretation of the data sets very difficult.

** On this matter, we welcome commentary, because we remain unsure how else to express the huge data set. As explained in the text, it seemed to us somewhat overwhelming to plot the chemistry at every site independently, or alternatively every species analyzed at multiple sites independently.

#From this, it follows that the authors assumed complete uniformity of processes occurring not only in the observed settings located tens of kilometers apart from each other, but also over the entire slope area! This is despite the fact that CH₄ fluxes on the East Siberian Arctic Shelf (ESAS), which could be associated with CH₄ releases from decaying hydrates, have been reported to vary by orders of magnitude within much smaller scales (Shakhova et al., 2015).

** We did not assume complete uniformity of processes. In fact, the MS goes into great detail explaining the range of processes that relate to the pore water chemistry – processes that have been well documented along many continental slopes.

#I see a clear discrepancy between the basic assumptions made by the authors and the methodology used to test these assumptions. The authors assumed CH₄ was

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being released from destabilizing hydrates, most likely via bubbles and the convective flow of geofluids.

BGD

** We do not understand this comment. The referee has leapt well beyond anything discussed in the MS. As stated above, and clearly in the MS, we discuss the lack of evidence for methane in shallow sediment and, by inference, gas hydrates on the slopes north of Siberia. Our MS has little bearing on how gas hydrates would be destabilized and how methane would be released.

** However, this comment seemingly belies a misunderstanding as to how carbon and methane cycle in sediment on continental slopes. In moderately shallow (< 500 m) sediment sequences with gas hydrate, there should be, at steady-state conditions, gas hydrate formation, gas hydrate dissociation and gas hydrate dissolution all co-occurring (see, for example, Dickens, EPSL, 2003). The pore water gradients between the top occurrence of gas hydrate in sediment and the seafloor arise, at a basic level, because of gas hydrate formation and gas hydrate dissolution; more specifically, where methane concentration gradients intersect the 2-phase gas hydrate-dissolved gas equilibrium curve.

#Despite that, all equations used for estimates refer to the diffusive transport of CH4 and other substances in the sediments. This is understandable; they used what was available. The problem is that the mathematics associated with diffusive transport cannot be used to describe the release of free gas from decaying hydrates. When assuming CH4 release from gas hydrates, one should realize that hydrates convert to free gas; the released gas travels upward much faster than diffusion occurs, through very efficient gas migration paths (chimneys etc.). In most cases, ascending CH4 can avoid oxidation in a few ways. 1) Because free gas resulting from hydrate decay is over pressured, it builds up a gas front; this disturbs sediment layering, creating the characteristic marks of gas re-lease (pockmarks etc.). 2) Only CH4 dissolved in pore water is reachable by microbial communities; CH4 released as free gas (ebullition) is not consumable by microbes. 3) AOM rates are only remarkable as compared to rates

Interactive comment

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[Discussion paper](#)



of modern methanogenesis, because all synergistic processes should be energetically efficient for all members of the microbial community, including SRB, methanogens and methanotrophs, etc.

** As noted above, this belies a wholesale misunderstanding as to how carbon and methane cycle in sediment on continental slopes. In most locations with gas hydrate, the vast majority of methane generated in the sediment ultimately (i.e., long time scales) escapes back to the ocean through diffusion. The assumption that methane-carbon above gas hydrates only returns to the ocean as free gas is entirely incorrect.

#Finally, the authors of the ms used three assumptions to explain their findings. Their first assumption is that bottom seawater on the slope north of Siberia is warming, leading to hydrate destabilization. There are no reports of increased bottom water temperatures along the slope of the Arctic during either the last glacial cycle (Cronin et al., 2012) or the Holocene (Biastoch et al., 2011; Dmitrenko et al., 2011; James et al., 2016). All papers published so far project the response of the hydrate inventory to possible future climate change in the Arctic.

**This comment is false. Nowhere in our MS do we assume that bottom seawater is warming. Rather, we point out that the region is of great interest because bottom water might warm in the future.

#The paper of Stranne et al., (2016) the authors refer to assumes a linear rise in assumes a linear rise in ocean bottom water temperatures of 3°C over the coming 100 years. This speculative warming of the Arctic is intentionally set higher than in other studies (<2°C by Biastoch et al., 2011; <1°C by Kretschmer et al., 2015) while modeling assumptions contradict the existing hydrological data (Biastoch et al., 2011; Dmitrenko et al., 2011; James et al., 2016).

** Following from our commentary above, none of this is assumed in current MS.

#Their second assumption is the quintessential statement that “Implicit of this finding

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[Discussion paper](#)



is that sediments sequences along the ESM lack gas hydrates" following the authors' speculations about why predictions of hydrates on the ESM are so markedly wrong.

** This is not an assumption, but rather a direct consequence of our results. The pore water chemistry profiles strongly indicate the lack of significant methane concentrations in the upper few hundred meters of sediment; given P/T conditions for gas hydrate, this absolutely implies an absence of gas hydrate.

** We can reword this if needed.

#The authors then suggest that: 1) the significant sea-ice concentration on the ESM diminishes net primary production (NPP); 2) the extremely broad continental shelf prevents accumulation of terrestrial organic-rich sediments; and 3) sediment accumulation is highly variable, so organic matter can be consumed during intervals of low deposition. None of these explanations is true. It was recently shown that the total OC (TOC) content in the ESAS/ESM sediments measured along the transect spanning more than 800 km from the Lena River mouth to the shelf (2000–3000 m water depth) varied between $\pm 1.2\%$ at shallow water depths and 0.8% in deeper water (Broder et al., 2016). In addition, TOC values and general patterns, which reflect fractions of terrigenous OC reaching the slope (based on biomarkers), were within the same range as those measured for the North American Arctic margin (Stein and Fahl, 2000, 2004; Goni et al., 2013). For comparison, an average value for the continental slope of the Gulf of Mexico, where large storage of CH₄ hydrates has been proven to exist, is 0.8% ± 0.2 (Gulf of Mexico Hydrate Research Consortium). Moreover, according to Arrigo and van Dijken (2011), the total annual NPP over the Arctic Ocean exhibited a statistically significant increase by 20% between 1998 and 2009, due mainly to increases in both the extent of open water (+27%) and the duration of the open water season (+45 days). Most importantly, increases in NPP over the 12 year study period were largest in the eastern Arctic Ocean, most notably in the Siberian (+135%) sector.

** While interesting, none of this is relevant. This is because, for high methane con-

[Printer-friendly version](#)

[Discussion paper](#)



Interactive
comment

centrations to exist in the upper few hundreds of meters on the slope, it is past carbon burial (i.e., not recent) that matters.

** We fully admit that we are somewhat perplexed by our findings, given our pre-conceived notions and past speculations. Hence, this portion of the manuscript is speculative. We can rewrite if guidance is given.

#It is interesting that the authors themselves confirmed that: 1) environmental conditions on the ESM are highly conducive for gas hydrates; 2) hydrate occurrence in the other areas of the Arctic, where hydrates were predicted, was confirmed by hydrate recovery; and 3) all the models developed by generations of geologists to predict hydrates in the Arctic used the same assumptions.

** We would agree with this statement, if logical qualifiers were added. The environmental conditions on the ESM are highly conducive for gas hydrates IF AND ONLY IF THERE IS SUFFICIENT METHANE; hydrate occurrence in the other areas of the Arctic, where hydrates were predicted, was confirmed by hydrate recovery AND BY PORE WATER CHEMISTRY IN SHALLOW CORES; all the models developed by generations of geologists to predict hydrates in the Arctic used the same assumptions WHICH ENTIRELY INFER A SOURCE OF CARBON TO PRODUCE METHANE.

** The referee is purposely ignoring two crucial facts, both discussed at length in the MS: (1) all previous works hinge on an assumption of significant methane in shallow sediment; and (2) NO pertinent data to the problem exists beyond our current work.

#If the authors agree that these statements are true, they failed to be critical of their own work, which is based on a handful of inconclusive data obtained on a single expedition, groundless methodology, and a few erroneous assumptions. Instead of casting doubt on the results of others, I would suggest that the authors question their own results and make a greater effort to accumulate clear, interpretable data. I believe I have made it quite clear that there is a huge discrepancy between the results presented by the authors and the far-reaching conclusions they are trying to support with these data. I

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[Discussion paper](#)



see no way to support publication of this manuscript in its current state.

** On this matter, and as should be even more clear, we entirely disagree.

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Interactive
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