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Interactive comment

Interactive comment on "Satellite ice extent, sea surface temperature, and atmospheric methane trends in the Barents and Kara Seas" *by* Ira Leifer et al.

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I believe that the manuscript does not meet the necessary criteria for publication and in my opinion, should be rejected. Below I elucidate few fundamental flaws in the proposed methodology and in the use and interpretation of the data that substantiate my decision.

On the proper use of the term "likely" in a scientific publication. The IPCC Fourth Assessment Report provides a clear definition guidance when it comes to key scientific uncertainty terminology, such as the term likely. The document draws a careful distinction between levels of confidence in scientific understanding and the likelihoods of

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specific results. Specifically, the report clearly defines the term likely as a 66% and higher likelihood of an outcome or result, where this can be estimated probabilistically. The authors' repeated use of the term likely throughout the manuscript does not comply with the standard definition provided by the IPCC Fourth Assessment Report. One occurrence, for example, is in the abstract, line 18, stating that the sources of methane underneath the claimed trends are likely attributable to methane seepage from subsea permafrost, hydrate thawing and petroleum reservoirs. They also use the same terminology - likely – and similar terms –i.e. possibly - when it comes to quantify the presence of subsea permafrost in the Kara Sea and Barents Sea (line 61). The manuscript does not provide any probabilistic estimate of the traceability of methane trends to the aforementioned sources, nor a probabilistic estimate of the extension of subsea permafrost in the Kara and Barents Sea. Rather, the term likely is inappropriately used in support of what appears to be instead, an unsubstantiated personal speculation made by the authors.

On the generation of figure 1: data source. Data traceability is the number one requirement to ensure traceability and reproducibility of a scientific study. No data source of the IASI methane retrievals (agency and retrieval version number) used to produce this figure has been specified, nor its accuracy, precision and applied quality control.

Statistical significance of the data set used in this study. For clarity, the authors need to provide an explanation about the filtering technique used in their study. The Yurganov and Leifer 2016a reference is written in Russian. This makes hardly possible for a non-Russian reader to understand this filtering technique and the need for its application. However, an English version of the abstract of this paper is available on ResearchGate. Here a brief description of this filtering technique is provided by the following statement: "The paper analyzes the data for cases of temperature contrast (the difference between the temperatures at the surface and at a height of 4 km) in excess of 10° C. ". There are fundamental objections that need to be made about the applicability of this filtering technique. Thermal contrast increases the signal to noise ratio in a satellite radiance

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measurement, enhancing the degrees of freedoms in all atmospheric retrievals, including methane. A temperature lapse rate of 5.5C/km (moist adiabat) is typically found in the tropics, with a corresponding ~1.3 degrees of freedom in methane. Differently from the seasonally stable tropical temperature lapse rate, the arctic temperature lapse rate is observed to undergo a large diurnal and seasonal variation. A study by Gardner et al. (https://doi.org/10.1175/2009JCLI2845.1) used a vast domain of station data (58,000 samples collected during the period 1988 - 2007) over the Canadian arctic to measure a mean near surface lapse rate of 4.9C/km. Methane degrees of freedom over the arctic have been observed to range between 0.1 and 0.6. A thermal contrast of 10C/4km roughly corresponds to a lapse rate of 2.5C/km. This, in turn, would correspond to a much lower number of degrees of freedom than those typically found in the arctic. In other words, the technique's thresholds appears too lose unless additional filtering or manipulation of the data are applied. The authors need to provide a full description of the technique and its impact on the final yield of the data. Near surface inversions are also fairly common over the arctic. The process leading to the formation of arctic inversions have been investigated extensively. Wexler (https://doi.org/10.1175/1520-0493(1936)64<122:CITLAA>2.0.CO;2) showed a radiosonde based observation of near surface temperature inversions of up to -20C/km in the northern Alaska region. Do the authors consider also negative thermal contrast? In summary, the complexity of the arctic temperature lapse rate calls for a more in depth description of this 10C/4km filtering technique and its impact on the spatial and temporal sub-sampling of the data set used in this study. If the goal was to only filter accurate methane retrievals based on their degrees of freedom, then a scatter plot of the sampled thermal contrast versus the methane retrieval degrees of freedom would demonstrate the applicability and utility of the technique. Is the data sample statistically significant? What is the average degree of freedom in the sampled data set used for this study in the end? Is it good enough for the type of climate applications pursued in this study? Is actually the thermal contrast a reliable metric to screen for "climate quality" data as opposed to more mathematically sound metrics such as the methane retrieval error estimate?

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On the thermal contrast and cloud free pixel filtering techniques used in the manuscript and their impact in skewing the results shown herein. Thermal contrast and clear sky conditions enhance the degree of freedom in the satellite based infrared methane retrievals. Hence, filtering the satellite methane retrieval profiles based on the correlative lapse rate and cloud occurrence is a way to filter methane retrievals with the highest degrees of freedom and improve the quality of the data set used in this study, as mentioned in the paper referenced in the caption of figure 1, Yurganov and Leifer 2016a. Nonetheless, methane emission is still present under low temperature lapse rate scenes and cloudy conditions. Incidentally, both conditions dominate the climatology of the Arctic region. These cases are not accounted for in the data set used in this manuscript and its derived conclusions. One would argue then, that upon applying this filtering technique, the data set has been altered to only represent a limited ensemble of methane retrievals. The full representativeness of the focus areas appears to have been lost. Hence the derived conclusions of this manuscript appear overall insubstantial. The above considerations are based on the assumption for which a valid and accurate correlative temperature thermal contrast data set was used. The authors never specified what data source was used to actually measure the 0-4km temperature lapse rate. Assuming that both temperature and methane retrievals are derived from the same source, the IASI (figure 1) and AIRS (figure 3 and onward) instruments, what's the uncertainty in the IASI and AIRS retrieved temperature lapse rate? Radiance measurements lack in information content from the lower troposphere, due to its higher opacity. As a result, any retrieval product – temperature, water vapor, methane, etc. - suffers by an increased uncertainty in the lower troposphere and a degraded vertical resolution. This makes it very difficult to ascertain the quality of the temperature profile in the bottom 4 km of the atmosphere. The authors did not specify the uncertainty, vertical resolution and quality control of the temperature thermal contrast used to filter the methane retrieval ensemble. A note of concern is the fact that temperature and methane retrievals from the same source are not mathematically independent from each other. Errors in the thermal contrast will inherently propagate in errors in the

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methane retrievals. If thermal contrast is not accurate, the correlative methane retrieval will inherit and amplify that inaccuracy (reference: Xiong et al., 2008).

On the gridding technique used in the manuscript. Caption in Figure 1 mentions a 0.5 gridding technique. Infrared based methane retrievals are notoriously affected by high rejection over cloudy and high uncertain surface emissiviity scenes, like those characterizing the Arctic region. Was the retrieval quality control properly taken into account and if so, was a minimum acceptance yield threshold factored in during the gridding process? As a result, was the gridding performed uniformly, both spatially and temporally? The points mentioned above are important aspects that need to be taken into account during the generation of a yearly average of satellite based infrared retrieved methane, like the one in Figure 1 and the trends of figures 3 and onward. None of them have been discussed in the manuscript. This aspect raises a huge concern about the uniformity and statistically significance of the data set used in this manuscript.

On the coastal features in the 2016 methane average shown in figure 1. Coastal areas suffer from high scene dis-homogeneity at a sub-pixel scale and, for this reason, are particularly difficult scenes for a satellite based infrared methane retrieval algorithm. Extended sensitivity experiments have shown that poor knowledge of sub-pixel land and ocean emissivity properties, along with poor knowledge of sub-pixel cloud and water vapor distributions, can result in high instability in the methane retrieval (Xiong et al., 2008). The high methane retrieval values observed along the coastal areas of Norway, Greenland and Canada might be suffering from these retrieval artifacts. The authors should pay high attention in not confusing these features with real methane signals when making the conclusions stated in the manuscript pertaining the highest values of methane observed in figure 1 (lines 70-73) for example. It is imperative that the authors specify the source of the IASI data used in this study, and discuss the associated level of uncertainty and quality controls.

On the IASI and AIRS sensitivity to surface methane emissions. Points 1 – 5 set

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aside, the manuscript fundamental flaw consists in the attribution of the observed trends to lower tropospheric emission of methane. To begin with, to what pressure level do the methane value in figure 1 refer exactly? Is this a total column, an averaged methane quantity, or a surface value derived figure? Lastly but more importantly, satellite thermal Infrared based methane retrievals – like those used in this manuscript - have highest sensitivity in the mid tropospheric portion of the atmosphere over polar regions. A plot of the AIRS vertical sensitivity functions can be found in Xiong et al. 2008 (https://doi.org/10.1029/2007JG000500). IASI has higher spectral sensitivity than AIRS in the methane region, nonetheless, its instrumental noise is higher. For this reason, the overall signals to noise between the two instruments, are comparable. Hence Xiong et al. 2008 paper can legitimately be used as an indication of IASI's sensitivity to methane. Figure 1 in Xiong's paper clearly indicates almost zero sensitivity to methane in the lower troposphere, the focus of this manuscript. For the reasons mentioned above, the claims made in the manuscript, that the trends in methane are ascribable to lower troposphere methane seeped from the ocean bottom, are substantiated by no reliable observation of lower tropospheric methane.

On the authors' SST trends speculations. The authors' use of SST as a proxy for seabed temperature is based on the assumption that other mechanisms, such as meteorology, solar insolation/log-wave downwelling radiation are negligible. The authors are ignoring an important factor affecting SST over the arctic. That is the surface albedo feedback. Also in this case, the authors make no reference to the actual uncertainty in the SST data. This is important to determine the range of confidence in the derived trend. A trend with no error bar is meaningless. The authors mention the use of cloud free pixels. Later (lines 236 – 237) they specify that clouds would alter SST. One would argue that SST increases induced by warm ocean current and methane emissions due to ocean seepage occur under all sky conditions though. How did then this cloud free filtering process affect the yield of the data and skew the results of the derived trends in SST and CH_4? There is an element of confusion between the use of cloud free pixels and cloud cleared pixels. At lines 254 the authors refer to the cloud cleared pixels as

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the valid ones. Cloud free and cloud cleared pixels are two very different things and should not be confused with each other.

On section 2.2.1 "Satellite data". AIRS methane data are not derived using the NUCAPS algorithm, the authors have made a wrong statement. AIRS version 6 relies on a different upstream first guess of temperature and water vapor. Both elements affect the downstream products significantly, including methane. Furthermore, the quality control methodology is significantly different. The methodology used to derive the trends in SST and CH4 is not scientifically sound. It suffers from the same problems described above on the generation of figure 1: the dubiously reliable thermal contrast based filtering technique, the unspecified uncertainty of temperature and methane and the limited vertical sensitivity of methane retrievals. The statement at lines 231 – 232, "CH4 retrievals are accurate over both ice and seawater" is inherently wrong. Uncertainties in surface emissivity used in the AIRS retrievals can vary considerably between ice and seawater. A thorough validation of it and how it propagates down to the uncertainty in methane retrievals is missing in the literature. Until these aspects are thorough examined, this impairs the applicability of thermal infrared retrieved methane over the arctic for climate trend studies.

On Section 3.2 In situ measurements over the Barents sea compared to AIRS methane accuracy. The magnitude of the anomalies measured by the in situ measurements of CH4 depicted in figure 5 corresponds on average to $\sim$20ppb. This quantity falls grossly beyond AIRS methane retrieval accuracy. Different sources of errors in the AIRS retrievals are enumerated in Xiong et al. 2008: noise equivalent delta temperature in the radiance observations, temperature, water vapor and SST retrieval errors. The resulting errors in methane retrievals as shown in figure 3 in Xiong et al., 2008. They all exceed the magnitude of the anomalies measured by the in situ CH4 observations. The conclusion to be derived is that, at this stage, AIRS retrieval precision and signal to noise might not be sufficient to detect the in situ anomalies allegedly attributed in this manuscript to methane seepage from subsea permafrost thawing. Furthermore,

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point source emissions are hardly detectable by a 45km spatial resolution retrieval like the AIRS methane retrieval product. Significant spatial and temporal averaging of the AIRS retrievals would be needed to increase the signal to noise. This poses an important question. What is the temporal scale of this methane plume anomalies? Can a sufficient number of AIRS retrievals be aggregated to reach significant signal to noise that would capture the plume? Finally, no error bar is shown for the SST and CH4 values used in figure 7, 8 and 10. No error analysis is computed to indicate a range of confidence that can prove any statistical significance in the trends derived in this study.

Authors' affiliation. A final note about one of the authors' affiliation. Leonid Yurganov has formally retired from the University of Maryland Baltimore County. The affiliation reported on this manuscript is no longer valid.

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