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Interactive comment on "Mapping potential signs of gas emissions in ice of lake Neyto, Yamal, Russia using synthetic aperture radar and multispectral remote sensing data" *by* Georg Pointner et al.

Anonymous Referee #2

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Review: Mapping potential signs of gas emissions in ice of lake Neyto, Yamal, Russia using synthetic aperture radar and multispectral remote sensing data

General Comments:

The manuscript presented is a detailed study of a single lake (Lake Neyto) in the Yamal Peninsula, which if the hypotheses are correct, present a method that could be used to monitor multiple lakes across a much larger area of both the Yamal and likely Siberian region. The methods presented in the study are technically sound, but the results are

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Discussion paper

presented and interpreted to fit the narrative and at times, are cavalier by placing too much weight on hypotheses that do not have in-situ observations to back them up. The narrative of the paper hinges on the fact that methane ebullition is creating hotspots that are tens to hundreds of metres in area. The hot spots are spatially co-located with areas of open water that are observed in high resolution spring time optical acquisitions (WorldView 2), and when overlaid on SAR imagery, are also collocated with larger regions of lower backscatter.

The authors propose that the regions of lower backscatter are the result of methane ebullition that is creating large cavities in ice thickness, creating a specular reflection away from the sensor. This is difficult to agree with for a few reasons: First, as evidence in the Discussion section where the authors present evidence of surface slushing as a result of a hole being augered into the ice, the water level went approximately 40cm over the ice surface. This is significant, because if methane ebullition is creating holes or areas of thin ice, then the surface will undoubtedly become wet as the ice is depressed passed the hydrostatic water level. The slushy snow will either absorb incoming microwave radiation, or it will refreeze as snow ice (and become a greater backscatter). Since most of the lower backscatter areas increase throughout the winter season, it is more likely that the surface is becoming wetter as the ice is depressed by the increasing weight of the snowpack and water floods the ice surface. This is consistent with Figures 7, 8, and 9, as the area impacted by the hole is nearly always a concentric circle, consistent with water spreading on a (relatively) flat surface. Second, if cavities that are present in the ice are large enough to act as a spectral reflector as opposed to roughness, then based on scattering theory the radar cross section from the target would be consistent regardless of incidence angle. The authors have normalized the incidence angles in this study, and it would be interesting to see if the NRCS is consistent across the incidence angle range observed. Third, there are no in-situ observations. The authors rightly mention that this area is extremely difficult to get to, and that direct personal observation of the holes are not safe due to thin ice. This acknowledgement of the limitation needs to also bring with it a lessening of the claims/assumptions that the source of the hotspots is definitely methane ebullition, and the mechanism that influence the SAR scenes.

That being said, there is considerable scientific merit to this paper in the methods, statistical tests, and results that it shows. In my opinion I believe that the paper will become acceptable after significant revision to ensure that interpretation of imagery lacking in-situ data remains inquisitive as opposed to prescriptive.

Specific Comments:

I will list line #s in this section, but firstly – this manuscript did not have a Study Site section. This is integral to this paper as it consistently references the surround region, and studies that have been done on other lakes. Please include.

The Introduction section is very detailed but extremely long. Paragraphs between lines 55 and 95 can be further summarized to provide key points to the reader.

Page 1, Line 20: "Methane (CH4) is a powerful greenhouse gas and the global trend of its atmospheric concentration has shown significant changes over the last decades (Nisbet et al., 2014)." What changes" The concentration of Methane, or its effects? Please be specific.

Page 2, Line 38: "150 thousand" Please write as 150 000

Page 2 Line 48: ". . . that gained a lot of attention in the scientific community recently." What sort of attention? Newspaper? Scientific studies? Please provide references, and if they were the references earlier in the sentence, please provide at the end.

Page 3 Line 62: "Low radar return is observed from ground-fast lake ice due to low dielectric contrast between ice and the lake sediments (Duguay et al., 2002). On the other hand, strong reflection of the radar signal occurs at the ice-water interface of floating lake ice because of high dielectric contrast between ice and liquid water (Duguay et al., 2002; Engram et al., 2013)." Provide the actual real and imaginary values of the relative permittivity so the reader can understand what a high and low dielectric contrast are.

Page 3 Line 68: “Coming back to gas emissions”,

Remove – this is unnecessary.

Page 5 Lines 119 – 128: Understanding such phenomena can be important for numerous reasons, such as climate modelling, where global models 120 currently incorporate methane release from permafrost environments only poorly (Turetsky et al., 2020) and only consider ebullition from superficial seeps, or the understanding of sub-lake permafrost dynamics (Pointner et al., 2019). Another important point is that gas emissions can pose serious threads to humans, e.g. people working in the gas industry or local indigenous people. The Yamal-Nenets are reindeer herders that travel across the Peninsula throughout each year. They frequently crossfrozen lakes in winter. In June 2017, a powerful explosion from a gas-inflated mound that formed under a riverbed near Seyakha 125 on the Yamal Peninsula has been documented by Bogoyavlensky et al. (2019c), scattering debris over a radius of a few hundred metres. For lake Otkrytie, an eruption that seems to have been capable of breaking lake ice of 1.5 m thickness was described by Bogoyavlensky et al. (2019a). Understanding where different forms of gas release happen may be favorable for identifying areas of increased risk for humans.”

This paragraph is out of place here. It should be moved to the beginning of the Intro or in the Discussion section to provide information about the impact of the study.

Page 6 Line 129: The Data section should have a table of the acquisitions that were used in this analysis for reproducibility. It’s also important to list the relevant metadata about those acquisitions, specifically the local time of acquisition and the incidence angle. For example, you have several scenes that were acquired during days in which the temperature exceeded 0C. A daytime/nighttime acquisition time becomes quite crucial to your study then.

Page 7 Line 154: 1236.5 MHz and 1278.5 MHz

Use GHz or MHz – be consistent.

Page 7 Line 180: "closest to lake Neyto and located on the east coast of the Yamal Peninsula at a distance of approximately 80 km, to assess potential temporal relationships between backscatter anomalies and air temperature"

80km is a significant distance when considering air temperature, and the fact that the Seyakha station is located on the coast and lake Neyto is located in land of the Yamal Peninsula. Is it possible that a gridded reanalysis product would be better representative?

Page 8 Lines 183 – 187: "2.7 ArcticDEM digital elevation model V3.0 The ArcticDEM is a high-resolution, high quality, digital surface model (DSM) of the Arctic created by the Polar Geospatial Center (PGC) at the University of Minnesota from optical stereo imagery acquired by the WorldView-1, WorldView-2, 185 WorldView-3 and GeoEye-1 satellites using photogrammetric methods (Porter et al., 2018). Its spatial resolution of 2 m is unprecedented for digital elevation models (DEMs) with a pan-Arctic extent. The ArcticDEM was used for the terrain-correction of all SAR data presented in this study."

This just doesn't need to be in here. The mention of ArcticDEM can be provided in Section 3.1.1., but is not necessary to the level of detail.

Page 10 Lines 265-266: "We used the green band as the input as it showed the highest contrast between the holes and areas of surrounding ice"

This is surprising. Not the NIR band? It would be good to see a breakdown with a profile of reflectance, for instance.

Page 11: Table 1

This needs to be in the Data section.

Page 11 Lines 295-296: "We estimate the total number of pixels in the negative class (regular floating lake ice) to be about one order of magnitude larger than the total number of pixels in the positive class (anomalies) in the validation dataset (Table 1)"

Where is this assumption coming from? Please provide.

Page 12 Lines 311 – 318: In order to compare levels of σ 0 from anomalies when lake ice was present to those of open water on lake Neyto, we used all available Sentinel-1 EW and IW scenes acquired in July and August from 2015 to 2019, when the lake can be assumed to be largely ice-free. We masked the images using the same lake masks as described in Sect. 3.2.1 and calculated the mean σ 0 315 for the whole lake on single dates and averaged it over time, similarly to the calculations described in Sect. 3.3 above. We calculated the difference between this temporal mean of assumed open-water backscatter and the temporal mean of the positive (anomaly) class backscatter (see last paragraph in Sect. 3.3). Again, all calculations were performed separately for each polarisation channel."

This method has some pretty important flaws. As mentioned later in this article, open water backscatter is likely to be influenced by Bragg scatter due to waves, and slight waves on the order of 3cm can cause considerable bbackscatter of the signal. Holes in the ice would not exhibit this same kind of wave action. How can it be certain that we're comparing apples to apples here? Figure 2: The workflow is not referenced anywhere in the paper. Also, it's confusing. The input data and actions are the same colour/shape, and the other symbols don't follow a similar structure. Please revise to be consistent. It also needs a legend to delineate input/output/method.

Page 19 Lines 345-346: "The majority of holes is characterised by an area smaller than 5 m2 , the median is 4.25 m2 . Few holes with areas larger than 100 m2 were identified."

How is it that we can detect holes that are smaller than 5 square metres? Also, that would mean that you're assuming that the cavities in the ice are much, much greater than 5 square metres based on the area of low backscatter surrounding each hole.

This does not seem practical compared to the likelihood that the surface snow is being wetted, and is absorbing the incoming microwave signal.

Page 20 Line 354: "Figure 8 shows the same locations of detected holes deduced from the WorldView-2 image acquired on 22 May 2016 as in Fig. 7 on top of a Sentinel-1 EW HH-polarised acquisition from 7 April 2016, taken more 355 than a month earlier than the image in Fig. 7."

What was the temperature on 22 May 2016?

Page 21 Line 359: "A steady increase of area of backscatter anomalies in late winter and spring is evident. The maximum extent of backscatter 360 anomalies was especially high in 2019, where on the last useful acquisition date, its area was approximately half of the whole lake area (Fig. 9, compare also to Fig. 3 (a)). "

Its evident that the intersection also increases when the air temperature is close to 0C or higher. This is very important, because slushy snow would be present during the same period, especially if they are located next to holes that are 40cm below the hydrostatic water level.

Page 21 Line 361 – 362: "The total lake area is approximately 200 km2 . Maximum air temperature is often approaching or slightly exceeding 0 âŮęC throughout the analysis periods"

Seyatha station is also coastal, which is in contrast to the region surrounding the lake. I'm not confident that a direct comparison is appropriate.

Page 25 Lines 378-382: Potential signs of gas emissions might also be seen in Sentinel-2 optical acquisitions of the lake during melt and lake ice break-up. In 2019, a comparably high number of cloud-free Sentinel-2 acquisitions were taken during these time periods. 380 Figures 11 (a)-(e) show Sentinel-2 true-color composites for a section in the Northern part of the lake during melt and lake ice break-up in 2019. Irregularities in snow cover on top of the lake ice may be seen in Fig. 11 (a) and (b), while diminishing patterns of bright ice and dark spots not much larger than the pixel resolution are likely depicted in Fig. 11 (c)-(e).”

This is a leap, as the pattern in these images is very consistent with breakup of lakes with no methane ebullition.

Page 27 Lines 394 – 396: “This result appears especially 395 significant when considering that the holes were mapped at 0.5 m pixel-spacing and anomaly regions from Sentinel-1 at 40 m pixel-spacing.”

Why could this be? Sentinel 1 acquisitions with a 40m pixel spacing could not resolve the holes, no. And it’s unlikely that the cavities will be over 200m in diameter. You have also presented that when augering into the ice that the ice is so depressed that the surface is wetted up to 40cm above the ice level. This evidence makes me invoke Occam’s razor that the most likely result here is that the hole is influencing flooding of the ice surface and slushing events.

Page 27 Lines 397 – 400: “As snow seems to have melted earlier in zones where anomalously low backscatter was observed before and the blob-detector algorithm was especially used to detect holes characterised by high contrast to surrounding bright ice, there could be more seeps that either do not form holes in the ice, are characterised by lower 400 contrast in zones with more snow, or both.”

This is less likely than ice pushed below the hydrostatic water level with a hold nearby.

Page 27 Lines 404-406: “However, we are not aware of any 405 studies reporting such causes for shallow Arctic lakes and based on studies by Bogoyavlensky et al. (2019a, 2018, 2016) and Kazantsev et al. (in review), we consider gas emissions as the most likely explanation.”

This line is carrying a lot of weight, and needs to be validated.

Page 27 Lines 411 – 414: “Continuous seeping with durations of at least weeks to months, associated with continuously expanding cavities might be an explanation. On the other hand, it seems surprising that the strongest expansions occur in spring, where the largest ice thicknesses can be assumed."

See snow slushing example

Page 28 Lines 418 – 421: "In case of cavity formation, it could be that the backscatter level of many pixels in the anomaly regions in the Sentinel1 EW imagery (40 m pixel-spacing) is caused by a combination of lower backscatter from cavity regions (due to increased 420 specular reflection from the gas/water-interface) and higher backscatter from zones of regular floating lake ice, as the resolution is comparably coarse."

This sentence is hyperbole – Can you support this with other references or studies? If not, I suggest its removal.

Page 28 Lines 422 – 429: "In 2016 in late April and early May, very low backscatter from the entire lake surface was observed, which suggests wetting or melting of snow on top of the ice took place during that period and backscatter was mainly governed by interaction with the wet snow (Duguay and Pietroniro, 2005). Consequently, images acquired during that time were excluded from the analysis 425 (Fig. 9 (b)). One ALOS PALSAR-2 fully polarised scene in 2016 was available, which was unfortunately acquired during this period and was thus also not used for the analysis of scattering mechanisms. However, ALOS PALSAR-2 fully polarised data from 2015, one year earlier than the WorldView-2 scene was acquired, were available. The shape and locations of backscatter anomaly regions vary significantly between different years (Bogoyavlensky et al., 2018; Pointner and Bartsch, 2020) (compare also to Fig. 1, Fig. 3 and Fig. 10), but the characteristic expansion is similar in all years analysed, as discussed above."

I'm not sure what we as the reader get out of this paragraph because you're discussing data that you did not analyze.

Page 28 Lines 439 – 440: "At L-band, backscatter from anomaly regions is higher than from regular floating lake ice (Fig. 10 (c)), which is the opposite 440 effect as for C-band (Fig. 10 (a) and (b)).”

That is not what you presented in Figure 10 though, you presented the T11 parameter which is not “the backscatter”.

Page 28 Lines 450-451: “Another obvious difference between C-band and L-band is that backscatter from anomaly regions is higher at L-band (Fig. 10 (a), (b) and (c)).”

This was already stated above.

Page 29 Lines 458 – 462: “As a consequence of slowed ice growth, the cavities are filled by water, partly filled by gas or completely filled by gas (Engram et al., 2020). Resulting rough surfaces are the ice-water interface or the gas-water interface (Engram et al., 2020). For lake Neyto, formation of potential cavities (anomaly regions) could start in late winter or 460 spring and then the cavities may successively expand over time (compare to Fig. 9). Bogoyavlensky et al. (2018) and Pointner and Bartsch (2020) showed that locations of potential cavity zones (backscatter anomalies) vary significantly between years for lake Neyto.”

It would make sense that the location of ebullition would remain consistent based on the source of ebullition. What biogeochemical process is there that you can justify the movement of the methane source? This needs to be addressed.

Page 29 Lines 463 – 465: “Features related to ebullition responsible for increased L-band backscatter in PALSAR-1 SAR imagery in Engram et al. (2020) are of much smaller spatial scale than features that are expected to be responsible for 465 anomalies in SAR imagery of lake Neyto.”

What are the features responsible in Engram et al., 2020?

Page 29 Lines 483-485: “Ice metamorphism processes related to increased solar radiation and air temperatures in spring such as the the formation of bubbles and air channels on the ice surface or the formation of ice needles 485 (Kouraev et al., 2015) may play a role, but this could not be assessed."

Slushing of the ice would happen during the winter season as well, not just the spring

Page 30 Lines 490-491: "During lake ice drilling on Yamal in April 2019, several lakes were found to have water level up to 40 cm higher than the level of lake ice. In situ observations of the lake ice of lake Neyto in winter or spring would be required to understand the cause of the anomalously low backscatter in detail."

YES. This really provides evidence of what you're seeing in the SAR scenes. Based on the location of the holes and the area of low backscatter, the interaction has much less to do with the under-ice roughness/cavity, and much more to do with the absorption. Keep in mind that absorbed signals generally also show that they are the result of surface roughness in polarimetric decomposition (see target decomposition of first year sea ice, for instance). This sentence above supports the slushing hypothesis with in-situ observations of the snow/ice dynamics in the region.

Page 30: Figure 12 In the caption, please provide the exact date of the observation, and the lake name (with coordinates). Page 31 Lines 503 – 510: "A steady increase of area of backscatter anomalies in late winter and spring can be seen in Fig. 9 for all years analysed. Especially high is the fraction of lake area covered by areas of anomalously low backscatter in 2019 (compare also to Fig. 3). 505 Also in 2019, a comparably high fraction of cloud-free Sentinel-2 observations were acquired during lake ice break-up. These acquisitions may show additional signs of degassing (Fig. 11, northern part of the lake). Regions that seem to have become snow-free earlier in Fig. 11 (a) and (b) partially match regions with increased frequency of dark spots in Fig. 11 (c), (d) and (e). Especially noticeable are diminishing patterns of apparently bright ice in Fig. 11 (c), (d) and (e). These bright patterns may show similar features as the WorldView-2 image acquired on 22 May 2016, but the limited spatial resolution of Sentinel-2 does 510 not allow to draw firm conclusions"

Based on the discussion about this study, I believe that this paragraph is really too inconclusive to make any assumptions, and suggest its removal.

Page 32 Line 539: “We do not claim that anomalies on these lakes are necessarily caused by gas emissions.”

It appears that you have the same amount of evidence for these lakes as you do for Lake Neyto. It would be appropriate for you to state that the patterns are consistent with methane ebullition, but needs to be verified throughout the paper.

Page 33 Line 550: “anomalies are indeed likely caused by gas emissions through the lake sediments.”

Consider rewriting to read “anomalies are consistent with previous studies that quantify gas emissions. . .”

Page 551 – 553: “. The successive expansion of anomaly regions observable mainly during late winter and spring in all of the analysed years (2015 to 2019) might be explained by cavities formed by the gas emissions that successively hollow out the lake ice around seep locations over time.”

I disagree with this based on the evidence I have seen for the wetting of the snowpack due to overflow or through holes in the ice.

Page 33 Line 560: “to the chosen validation strategy and could allow to monitor gas emissions on lake Neyto also in the future.”

Consider adding “also in the future upon the verification of this hypothesis.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-226/tc-2020-226-RC2-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-226, 2020.