Mapping the bathymetry of supraglacial lakes and streams on the Greenland Ice Sheet using field measurements and high resolution satellite images[1]

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Reviewer comments and responses
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We appreciate the thoughtful, constructive comments offered by Allen Pope, Gordon Hamilton and an anonymous reviewer. We have carefully considered all of the feedback we received and our efforts to incorporate their suggestions have resulted in an improved paper that we believe merits publication in *The Cryosphere.* This document lists the comments, articulates our responses, and summarizes the changes made to the revised manuscript.

Short Comment C1961: 'Landsat terminology' by Dr. Allen Pope

1) Overall, a very interesting paper which addresses an important problem. Although wide applicability may not be attained yet, this paper does provide an important step.

   Thank you for this positive assessment of our work. We believe remote sensing of supraglacial lakes and streams has significant potential to advance our understanding of the hydrology of the Greenland Ice Sheet and are proud to have made a contribution toward this objective. We fully realize, and openly acknowledge in the manuscript, that our work represents only an initial step toward the goal of a flexible, generally applicable approach to bathymetric mapping of the supraglacial environment.

2) One important - but minor - correction is the terminology used to refer to "Landsat." With the recent launch of Landsat 8, it is important to make clear that the paper is emulating Landsat 7 (ETM+).

   Our original use of the term "Landsat" was not sufficiently specific, and we agree that this is an important clarification. The sensor considered in our paper was in fact Landsat 7 (ETM+) and we have replaced all instances of "Landsat" with "Landsat 7" in the text and added (ETM+) in parentheses where the sensor name first occurs. We have also changed the label for Figure 4b to read "Landsat 7," not just "Landsat," and added a similar clarifying footnote to Table 2.

3) In addition, for the figures which reference wavelengths, I think it would make it more easily readable if band numbers were included for all sensors.

   We considered this comment at length but have to disagree with Dr. Pope in this case. Given that we are considering three different sensors, presenting

band numbers for each instrument would be unnecessarily confusing. Band numbers in and of themselves are just labels with no real meaning, whereas wavelength is the actual physical quantity describing the range of electromagnetic radiation measured by each "band." By presenting our figures in units of wavelengths, the various sensors we considered can be compared more readily. In addition, presentation in terms of wavelengths also makes the satellite-based data more comparable to our field spectra and provides greater flexibility for future sensor design as well. In any case, we believe that band numbers would confuse rather than clarify matters; wavelength is the relevant physical quantity that can be compared across sensors, whereas band numbers are really just names.

Short Comment C1963: 'Ln vs Log' by Dr. Allen Pope

1) I noticed that although the equations used "ln" to calculate X, the figures (e.g. Figure 5a) plots against log (assumed to be base 10). My guess is that the figure was produced in Matlab, were "log" actually is the command for "ln" (as opposed to log10) - but it would be good to clarify this ambiguity for future studies to be clear on what was actually done/reported.

   Good eye, thanks for catching this ambiguity. The mathematical operation used in this study was in fact the natural logarithm, ln, not the base 10 logarithm. The axis label for Figure 5a has been modified to read ln rather than log, which should alleviate any potential confusion.

Referee Comment C2462: 'Anonymous Review' by Anonymous Referee #1

1) In this paper, the authors investigate the potential for using optimised ratios of spectral bands of multi-spectral satellite imagery (specifically WorldView2 imagery) to derive new, and better, estimates of supraglacial lake and stream bathymetry on the Greenland ice sheet. The authors build on their previous work by applying techniques devised for use in land-based fluvial remote sensing to the ice sheet environment, for which accurate estimates of water storage remain elusive. The authors conclude that depth estimates derived from band ratios optimised on a location-by-location basis are very accurate, however optimal band ratios are found to differ quite significantly between images and locations.

   The paper is well written and easy to read. The method is robust and the analysis comprehensive. The authors offer a critical discussion of their results which nicely highlights the limitations and potential benefits of their study, and provide sensible directions for further work. The work described in the manuscript offers a valuable contribution to knowledge in the fields of cryospheric remote-sensing and ice-sheet hydrology.

   Thank you, we appreciate this positive assessment of our work. We hope that this study will stimulate further research on remote sensing of supraglacial lakes and streams and address some of the limitations identified by this initial investigation.

2) Can you compare depth retrieved using your method to depth retrieved using

empirical methods e.g. Fitzpatrick et al. 2013?

In many respects, this study builds upon that of Fitzpatrick et al. (2013), whom we cited in our original manuscript. These authors used a depth-reflectance relation based on a single MODIS band to estimate depths in relatively large supraglacial lakes and reported an observed vs. predicted R^2 value of 0.79, comparable to the results we obtained using WV2 images. We have added a sentence to the first paragraph of Section 3.2 acknowledging this earlier work. We also mention the Fitzpatrick et al. study in the first paragraph of the discussion, where we point out that the use of higher-resolution satellite images in our investigation allowed us to map the bathymetry of smaller water bodies than would be possible with MODIS. A direct comparison of various bathymetric algorithms was not an objective of our study, so we did not attempt to apply Equation (2) of Fitzpatrick et al. to our data. Because this empirical expression was derived for a different sensor with a distinct set of bands, such a calculation would not have been straightforward, beyond the scope of the current investigation.

3) What were the R^2 values for each individual site, when depth was estimated using the optimum band ratio devised using all data?

This inquiry regarding the performance of the merged quadratic OBRA relation presented in Figure 4b when applied to the individual sites is a fair question. We applied this equation to the validation data for each individual site and performed regressions of observed vs. predicted (OP) depths. The results of this analysis are now reported in Section 3.2 (pg 15, line 5) of the revised manuscript: low OP R^2 values were obtained for the Olsen River (0.25) and Cold Creek (0.14), but much higher for Lake Napoli (0.90). We attribute the poor performance of the merged OBRA relation in retrieving depth from the two streams to their location on separate images and the fact that the majority of the merged data set was drawn from the deeper waters of Lake Napoli. We also revisit this point in the Discussion (pg. 18, line 14), where we acknowledge that a depth-reflectance relation based on data from several sites on different images might not yield reliable bathymetric information on a site-by-site basis and that site-specific calibration might be necessary. This caveat is an important conclusion of our study that we had reported in our original manuscript. Adding the OP R^2 values resulting from application of our merged relation to individual sites only helps to illustrate this point. Developing a more general, robust depth-reflectance relationship is an important objective of our ongoing research in Greenland.

4) On page 4743, Lines 20-23, when you refer to supraglacial lake volume estimates inferred from remote sensing, shouldn't you also refer to estimates from modelling studies such as Luthje et al. 2009 and Leeson et al. 2012?

A sentence has been added to the end of the paragraph in question to acknowledge that modeling studies also can provide estimates of water storage volumes, with citations to the two papers mentioned. We have also added a new citation to a field-based study by Tedesco et al. (2012) that examines the effect of ablation of the bottom of supraglacial lakes on volume estimates. These references were added to the bibliography as well.

5) In Figure 1, it is hard to see on map (a) where each of the study sites is located.

Figure 1a has been updated. The map is now focused more closely on the study sites, while retaining enough of the outline of the Greenland coast to provide some frame of reference. Each site is now identified with a unique symbol, with a legend provided as well.

Referee Comment C2734: 'Review of Legleiter et al., "Mapping the bathymetry of supraglacial lakes and streams . . . ", TCD' by Gordon Hamilton

1) This manuscript makes a nice addition to the literature on quantitative studies of supraglacial meltwater in Greenland. Remote sensing of melt pond bathymetry is a well-established method, based largely on radiative transfer theory developed for mapping coastal marine environments. This paper takes a slightly different approach by implementing a band ratioing technique developed for terrestrial rivers. The authors apply it to very high spatial resolution images of surface ponds and rivers in Greenland collected by the WorldView-2 satellite.

The paper is well written and easy to follow, and the results are promising. I have a couple of minor comments that should be addressed, but am otherwise looking forward to seeing this work published.

Thank you for this positive evaluation of our work. We agree that the initial results reported in this manuscript are encouraging and look forward to applying these methods to better understand the hydrology of the Greenland Ice Sheet. We have given careful consideration to the comments provided by Dr. Hamilton and his feedback has resulted in a much improved manuscript, as articulated in the responses below.

2) In applying the band ratio method to supraglacial melt ponds and streams, the authors make the assumption that radiative transfer processes are similar to gravel-bed rivers where the method was originally developed (see P10 L5). How safe is this assumption, given that the inherent optical properties of water play a large role in the transfer of light?

This is an insightful comment but we believe that the encouraging results reported in our manuscript validate the assumption that similar radiative transfer processes operate in supraglacial lakes and streams as in terrestrial rivers. The inherent optical properties of the water column are an important control on the propagation of electromagnetic radiation through any water body and thus influence the feasibility of estimating depth via passive optical remote sensing. While true that the optical properties of terrestrial rivers and supraglacial lakes and streams are different in many respects (i.e., due to a near-absence of sediment in organic matter in the supraglacial setting), ratio-based bathymetric mapping proved effective in both environments. As explained in the manuscript, one of the primary advantages of Optimal Band Ratio Analysis (OBRA) is the ability of this technique to provide reliable depth estimates despite variations in water column optical properties, bottom reflectance, water surface roughness, and other factors that influence the

upwelling spectral radiance. The high accuracies obtained via OBRA indicate that this approach is just as appropriate for water bodies on the surface of the Greenland Ice Sheet as for the terrestrial rivers for which the method was developed originally.

3) One of the curious results is that, although the band ratio method seems to work very well in general, the optimal combination of bands for extracting depth is variable from location to location, and maybe also from image to image. The authors attribute some of this variability to adjacency effects which seems reasonable. But how would one deal with those effects in practice?? I'd encourage the authors to provide a set of objective rules for selecting a priori which band combination should be ratioed for any given feature.

   Although the OBRA method provided consistently accurate depth retrieval results across all three sites and for both field and image spectra, the finding that the optimal band ratio varied so markedly was an unexpected result for us as well. Our manuscript openly acknowledges that the bands most useful for bathymetric mapping differed on a site-by-site and image-by-image basis and that at present we are not able to provide a general, flexible depth-reflectance relation. In the presence of such complexities, however, the OBRA technique provides a robust empirical approach that can provide reliable depth information given sufficient calibration data. At this point, proposing a general set of rules for a priori selection of an appropriate band combination would not be justified so we have made no such recommendation. The strength of the OBRA method is the ability to take as input paired observations of depth and reflectance and derive an effective depth-reflectance relation on a case-by-case basis. Ultimately, we hope to establish a more general relationship that would not require field data for calibration, but that objective remains a goal for future research and is beyond the scope of the current manuscript.

4) The figures are clear and appropriate, but I would have liked to have seen a figure comparing bathymetry as measured in the field and as extracted from WV2 imagery. A figure showing representative transects across a melt pond and a stream would be very instructive.

   We have added a new Figure 8 to the revised manuscript to present transects across the Olsen River and Lake Napoli that compare field surveys to image-derived depths. A discussion of these transects and their implications has been added to the last paragraph of the Results section as well.

5) P2 L23: delete "the"

   Thanks, done as requested. That mistake was not a good way to start the paper.

6) P3 L9: change "detained" to "retained"

   Done as requested.

7) P3 L15: passive voice, change "has become" to "is"

   Done as requested.

8) P4 L3-25: many of these issues were addressed in Sneed and Hamilton (2011).

   We somehow overlooked the work of Sneed and Hamilton and so thank the reviewer for bringing this publication to our attention, as the study is

closely related to our investigation. We have read the 2011 paper with great interest and have rewritten the paragraph in question to appropriately acknowledge earlier work by these authors. The following paragraph also cites the field observations of Sneed and Hamilton.

9) P4 L19: passive voice, change "could" to "can"

Done as requested.

10) P5 L5: two significant digits? Really??

The important point we were trying to make here is that our field observations were made close in time to the image acquisition, but the use of two significant digits for stating the time difference in hours was probably overkill. We know precisely when the images were acquired but our field data collection spanned a period of several hours each day, so stating a time difference down to the minute was misleading. The times are now reported as 3-72 h, with no significant digits.

11) P7 L5: what about the potential for shadowing as the boat carried out its surveys? Or is it unimportant because incoming radiance measurements were not co-located with the upwelling radiance measurements?

This is a valid point and although we attempted to avoid configurations that would lead to self-shadowing by the boat, a small number of our field spectra were probably affected by shadows. We have added a sentence acknowledging the possibility of self-shadowing by the boat to the paragraph in question.

12) P8 L1: strictly speaking, you probably need to account for differences in solar zenith angle between the lake-shore and boat locations, although the differences will likely be insignificant on sub-km length scales.

A good point, but the shore-based instrument was never more than 800 m from the boat, so differences in solar zenith angle between the two sensors were negligible. A sentence to this effect has been added to the paragraph in question.

13) P8 L21: one significant digit is probably fine.

Agreed, as explained in our response to a previous comment. The time difference between field measurements and image acquisition is now reported as 3-72 h.

14) P9 L19: delete comma.

Done as requested.

15) P10 L5-20: did you collect any meltwater samples to quantify the IOP? Might be worth reading Sneed and Hamilton (2011) for some support to these ideas.

For logistical reasons, we did not collect any water samples during our investigation and thus do not have any direct measurements of the inherent optical properties of the water column. However, we have read the work of Sneed and Hamilton (2011) and cite their analysis of water samples from a melt pond in East Greenland as evidence to support our assumption that sediment and organic matter are negligible in the supraglacial environment, implying that radiative transfer is dominated by pure water absorption. Referring to the IOP data from this earlier study thus helped to strengthen the

argument presented in our manuscript.

16) P14 L19: passive voice, change "could" to "can"

Done as requested.

17) P14 L25: Rephrase. Earlier studies have already shown that spectrally-based methods are valuable for mapping supraglacial bathymetry.

The sentence in question has been rewritten to emphasize that the field measurements of depth and reflectance from our study provided direct, on-the-ground evidence to confirm the feasibility of mapping supraglacial bathymetry via remote sensing.

18) P15 L4: no need for two significant digits.

Corrected, as in response to earlier comments.

19) P15 L13 and L15: "fairly" and "quite" are vague.

These qualifiers were unclear and have been deleted as requested.

20) P18 L6: passive voice, change "could" to "can"

The words "could indicate" have been replaced by "suggests".

21) P18 L22: the ability to map bathymetry of supraglacial streams is very valuable, but keep in mind you need to measure current speed in order to convert to discharge flux.

An excellent point of which we are well aware. Our ongoing work focuses on developing methods for estimating not only depth but also flow velocity using a combination of remotely sensed data and hydraulic relationships. Only if velocity can also be inferred reliably will be able to calculate discharge through meltwater channels, but that is the goal toward which we are currently working. For the present manuscript, we wanted to mention the possibility of estimating discharge and characterizing meltwater flux but any further discussion of this topic would be beyond the scope of the current paper.

22) P19 L6: many of these effects were investigated by Sneed and Hamilton (2011). Reference: Sneed, W.A. and G.S. Hamilton. 2011. Validation of a method for determining the depth of glacial melt ponds using satellite imagery. Annals of Glaciology, 59, 15-22.

The sentence in question has been rewritten to acknowledge the prior investigation by Sneed and Hamilton (2011) into the effects of variable substrates, etc. Thanks again for bringing this study to our attention.