

Interactive comment on “A decade of supraglacial lake volume estimates across a land-terminating margin of the Greenland Ice Sheet” by A. A. W. Fitzpatrick et al.

Anonymous Referee #1

Received and published: 27 April 2013

The paper by Fitzpatrick et al. reports results concerning the spatio-temporal evolution of supraglacial lakes over a western region of the Greenland ice sheet. Lakes volume and extent are derived from MODIS data for a period of 11 years. Lake extent is derived using a band ratio approach where volume is estimated using an empirically-derived depth vs. reflectance relationship from in-situ measurements.

Supraglacial lakes have been attracting researchers over the past recent years and, though much is still unknown, the recent studies are contributing to the understanding of the basic processes and mechanisms connected to the formation and disappearance of such lakes, as well as their potential impact on ice dynamics. In this regard,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

understanding the spatio-temporal evolution of the lakes is important and deserves attention. The topic is, therefore, of interest for the cryosphere community at large and can impact ongoing and future research.

The style of the paper is correct and the structure allows the reader to follow the different steps without problems and with clarity. The authors mainly focus on reporting their results and avoid speculating on potential consequences of their findings for ice dynamics (with the exception of a few cases that I personally find the most interesting part of the paper). The paper is a simple, communicative report of the analysis of a complicated data set to manage (MOD09). It could very well be a section of a PhD thesis work translated into a paper.

I would recommend the authors to revise the paper and re-submit it considering the following suggestions.

I feel that the section concerning the remote sensing methodology could and should be broadened. Not particularly the section concerning the extent (which is a much easier job compared to the volume estimates) but the one concerning lake volume. Though the authors show an independent validation of the formula in Eq. 2, that still remains an empirically-derived formula using a few measurements. There have been several studies focusing on using theoretical tools to estimate lake depth from spectral measurements but these are ignored by the authors. In particular, the most recent work by Hamilton and Sneed (2012) and Tedesco and Steiner (2012). These studies should be considered for understanding and discussing the uncertainty related to the estimates of lake depth from spectral measurements. It also would have been helpful to compare the formula derived in Box and Ski (2007) with the one here derived. The authors should have made a better job on the development of the remote sensing tool. Given the data set that they were able to process, this would have made the paper a more interesting contribution. In this context, the section discussing the limitations is weak and it does not provide anything specific to the technique here used but just an overview of well known problems.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

One major problem is the impact of clouds on the final results. How do we actually know the impact of the spatio-temporal distribution of clouds in the MODIS images on the final results ? For example, in one year the clouds could have been covering the lakes for longer periods and for another there could have been longer periods with clear skies. The authors briefly mention this point in their discussion concerning the 2012 season but I consider this to be a problem that propagates through the entire paper and never addressed. Beside the overall results in terms of statistics, extent and volume distribution , etc., this might very much impact the results concerning the rapid draining analysis. I strongly and cordially invite the authors to address this point in a revised version of the manuscript and consider a major point that could preclude publication.

Figure 7 is mentioned before figs. 4-5 and 6.

There is no mention of the potential effect on the lake estimates of snow/ice-covered lakes. These lakes appear to be a considerable number (at least from the helicopter vantage point). I am not sure there is a way to really address this point using visible data but the authors should mention this.

The title should be changed to actually reflect the fact that the focus of the study is on the Russell Glacier Catchment.

Interactive comment on The Cryosphere Discuss., 7, 1383, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)