This manuscript presents the results of a manual classification of five years of supraglacial lake drainage events on the Greenland Ice Sheet into three categories: fast drainage, slow drainage and refreezing. This manuscript is an incremental improvement over Selmes et al. (2011). In the broader context of other studies, however, this manuscript does not present methodological improvements or substantial novel insight on geophysical implications of lake drainage events.

I have grouped my major comments into four categories listed below. Firstly, there appears to be substantial overlap with Selmes et al. (2011), leaving the reader with the distinct sense that this manuscript is an addendum of sorts. Secondly, this manuscript overlooks several recently published studies on the topic of lake evolution / drainage, some of which already present similar findings that the authors imply are novel. Thirdly, the geophysical interpretation presents, but does not interpret, spatial and temporal differences in lake drainage type, and implicitly assumes that end of season fate is independent of lake area. Fourthly, an error analysis is entirely absent.

This manuscript does contain some novelties: (i) entire ice sheet tracking of lake drainage events, (ii) establishing that refreezing is more common than draining at high elevations, and (iii) hinting that synchronous neighboring lake drainages may be linked by a common mechanism or forcing. Overall, however, I would think these highlights are more commiserate with a concise "Brief Communication", than a full length "Research Article" in TC. If these highlights can be bolstered by an improved synthesis with (even some of) the previously overlooked literature, as well as augmented geophysical insights from the presented spatial and temporal variability, then this work has potential as a valuable contribution to the cryospheric community.

Many thanks for taking the time to review our manuscript, and for giving insightful contributions that we are sure have improved this work. We respond to the specific comments below in italics.

Major Comments:

1.) Overlap with Selmes et al. (2011). There appears to be significant overlap with Selmes et al (2011). The key figures of this paper (Figures 1 through 4) seem to be updated/expanded versions of Figures 1 and 2 in Selmes et al. (2011). The new figures essentially divide the previously combined "slow + refreeze" class of Selmes et al. (2011; red colors in their Figure 2), into two separate "slow" and "refreeze" classes (now green and blue in Figure 4). I have attached both these figures to this review. The accompanying text does not provide substantial methodological improvements (the reader is referred to Selmes et al., 2011 numerous times), additional insights on the temporal or spatial distribution of lake area drainage, or novel insights on the implications of lake drainage on ice sheet dynamics in a changing climate.

We agree that we essentially divide the previously unclassified lakes of Selmes et al. (2011) into slow and freezing lakes. However, given that these classes made up ~80% of a dataset of more than 2000 lakes in all parts of the ice sheet over five years, we feel strongly that determining the fate of these lakes is an important additional/novel contribution to the literature. However, we have undertaken to make the distinction *between this paper and Selmes et al. (2011) more clear. This includes revising Figure 4 to include additional interannual, intersector and uncertainly information, as requested elsewhere in this review. This figure is now given in terms of lake frequency (see below, Comment 3A). Additionally, we have removed the common methodological elements with Selmes et al. (2011), stating only that we use the lake area dataset from Selmes et al. (2011). We have expanded several sections of the results and discussion in response to this and Anonymous Reviewer #1's comments. We hope this clarifies the insights and novel contributions presented in this manuscript.*

2.) Other previously published literature. Much has recently been published on the topic of supraglacial lake evolution. The following works are not currently referenced, but contain both results and conclusions that should be properly attributed. These are just the works that quickly come to mind. For example "We conclude that any lake on the ice sheet has one of three probable fates. ... [then you revisit 1. sudden drainage, 2. re-freezing and 3. slow drainage] ... The latter two processes have not been reported for lakes in Greenland"... this intends to leave the reader with the impression that the observation of re-freezing and slow draining lakes is a discovery unique to this present study, when it fact it has been previously documented and described.

Box, J., & Ski, K. (2007). Remote sounding of Greenland supraglacial melt lakes: Implications for subglacial hydraulics. Journal of Glaciology, 257-265.

Georgiou, S., Shepherd, A., McMillan, M., & Nienow, P. (2009). Seasonal evolution of supraglacial lake volume from ASTER imagery. Annals of Glaciology, 95-100.

Johansson, A., Jansson, P., & Brown, I. (2013). Spatial and temporal variations in lakes on the Greenland Ice Sheet. Journal of Hydrology. 314-320.

Liang Y., Colgan, W., Lv, Q., Steffen, K., Abdalati, W., Stroeve, J., Gallaher, D., and Bayou, N. (2012). A decadal investigation of supraglacial lakes in West Greenland using a fully automatic detection and tracking algorithm. Remote Sensing of Environment, 127-138

McMillan, M., Nienow, P., Shepherd, A., Benham, T., & Sole, A. (2007). Seasonal evolution of supra-glacial lakes on the Greenland Ice Sheet. Earth and Planetary Science Letters, 484-492.

Sneed, W., & Hamilton, G. (2007). Evolution of melt pond volume on the surface of the Greenland Ice Sheet. Geophysical Research Letters, L03501.

The omission of Liang et al. (2012) seems to be particularly egregious, as that study explicitly assesses the rate at which lakes drain. Granted the Liang et al. (2012) did not study the entire ice sheet, it did span an entire decade, and provided quantified annual distributions of rate of change in lake area (i.e. not qualitative classifications). Overall, I would contend that the authors have an obligation to present their findings in the context of similar previous findings, so that they can highlight important agreements or disagreements to the reader, rather than assuming an interested reader will seek out and synthesize these works on their own.

These comments largely overlap with those of reviewer one on the subject so we have *made a substantial effort to improve this aspect of the manuscript. More details are included in our response to reviewer one however the most relevant we summarize here:*

All the above papers and several more are now included in the introduction. We have included a section in the discussion to compare our work with relevant other papers, in particular Johansson et al. (2013). To aid this we now present the elevation/lake duration part of our results and discussion using the elevation bands used by Sundal et al. (2009) and Johansson et al. (2013) to aid comparison between these papers. We accredit Liang et al. (2012) with stating that lakes could drain to the bed, supraglacially, or freeze, but note that paper did not attempt to classify lakes as such. We accredit Johansson et al. (2013) with attempting to classify lakes into freezing/draining, albeit by a different methodology, for a smaller subset of the ice sheet, and without distinguishing between drainage types.

3.) Geophysical implications:

A.) Supraglacial lake drainage may indeed have a large potential role in ice dynamics. It matter, however, whether large or small lakes are draining fast. Just looking at Figures 1 through 3, the examples you provide seem to suggest that slow drain lakes are substantially smaller (mean peak area of ∼1.8 km2 in Figure 3) than fast drain lakes (mean peak area of ∼6.5 km2 Figure 1). Thus, can the end of season fate really be interpreted as independent of lake area? This is done implicitly when Figure 4 provides the lake area associated with each drainage type, but the abstract (and other text) says "... X % of all lakes", rather than "... X % of maximum lake area". It's subtle nuance, but I think some novelty is associated with assessing end of season fate as a function of lake area.

We are primarily concerned with the frequency of occurrence of each lake type, we have changed figure 4 to represent frequency to avoid this confusion. In the abstract and main text where we refer to X % of lakes we are referring to frequency. However, we have included in our revised result and discussion sections a demonstration of the relative sizes of the different lake types, and the possible causes of these differences.

B.) Multiple years of data are presented, but the results are not interpreted in a temporal context. With the goal of examining slow lake drainage in the context of what role it may play in the hydrological and dynamic systems of the Greenland Ice Sheet, presumably the story would be completed by describing changes in the relative frequency of slow lake drainage in the context of climatic forcing, namely whether slow drainage increases in warmer years. Otherwise, the five years may as well be averaged into a mean climatology of sorts. Liang et al. (2012) showed that the frequency of rapid drainage increased in higher melt years, so I would imagine that the corresponding frequency of slow drainage events would have to decrease to accommodate increase fast drainages? Looking at SW Greenland, in the warmest (?) year (2007), it looks to me like there is 20 % more lake area drainage, comprised almost exclusively of nonrefreezing mechanisms. I would think there is also novelty in assessing the absolute volume of water reaching the ice sheet bed as a function of climatic forcing.

We have added discussion of the temporal variation, and included new material in figure 4 to bolster this discussion. We have included comparisons with the work of Liang et al.

(2012) here. We agree that there is novelty in assessing the absolute volume of water reaching the ice sheet bed as a function of climatic forcing. However, we have found that remote sensing methods for extracting lake volume cannot be accurately used on the scale of this study and that lake area does not provide a sufficiently good proxy for volume for this kind of quantification.

C.) Multiple sectors of data are presented, but the results are not interpreted in a spatial context. Why do you think the SW sector has the high proportion of fast drainages? Why is the relative amplitude of inter-annual variability greatest in the NE? With so few drainages of any sort, is SE Greenland expected to experience negligible surface meltwater-induced basal sliding? I suppose that fast drainages make up the largest portion of the pie first in the SW sector, but then, curiously, second in the NE sector (rather than say SE if meltwater production was the driving process, or the say NW where ice sheet geometry (i.e. surface slope) is next most similar to SW). I would contend that a bolstered spatial discussion is key to "Characterizing supraglacial lake drainage and freezing on the Greenland Ice Sheet". To highlight the absence of spatial discussion, I note that the word "sector" appears only figure and table captions, and nowhere in the body of the manuscript.

We have expanded each result section of our results to explore the spatial richness of the results presented, and support these new sections with an expanded discussion on spatial variability. We wish to avoid speculating too much here about the spatial distribution of fast draining lakes to avoid overlap with Selmes et al. (2011), see above. In the original manuscript we used “region” and “sector” interchangeably. We can see how this could be confusing, and thus now only use “sector”.

4.) Error analysis: In comparison to automated algorithms, in which sensitivity studies assess error levels relatively easily and reliably, manual classification can be viewed somewhat dubiously these days, given its inherent subjectively. I think it is therefore obligatory for proponents/users of manual classification to demonstrate the error associated with their output, especially in the context of the error from similar automated algorithms. Simply saying "This process was time consuming but allowed us to observe lake-drainage processes in great detail" does not provide the reader any confidence in the stated frequencies of each drainage class. As far as I can tell, the three drainage classes (fast, slow, refreeze) are essentially qualitative constructions (i.e. quantified drainage rates are never calculated and used for binning events). I would think that it is possible for significant error to arise when subjectively classifying events into three bins, especially when two bins ("fast" and "slow") are, by definition, different parts of the same continuum. Other variables, such as maximum lake area, presumably also carry some more easily quantified error? In any case, these errors need to be propagated in a meaningful fashion and appear in your tables and figures.

It is incorrect that the classification is dominantly manual, essentially qualitative, and that drainage rates are not calculated. We have attempted to make this more clear in the manuscript but for clarification:

- *The classification was automated and used quantified drainage rates and a* *threshold of remotely derived surface temperatures. Manual intervention was purely for quality checking and misclassification editing according to the logical/objective criteria described.*

- *Drainage rates were calculated but the temporal resolution was limited for practical purposes to ~24 hours. It is not, in our opinion, possible to accurately measure the drainage rate of a fast-draining lake using remote sensing, as the drainage rate is considerably faster (~2 hours, Doyle et al. 2013) than the repeat time of any sensor.*
- *We do not believe it is reasonable to state that fast and slow draining lakes are part of the same continuum, rather they are two discrete categories. There is one mechanism leading to drainage in matter of hours (fast-draining), and a separate mechanism resulting in drainage rates of several days: drawing a line between the two types of lakes is straightforward and by no means as arbitrary as the review implies.*

While we can confidently assign lakes to these discrete drainage-rate categories, our geophysical interpretation that fast-draining lakes drain to the bed and slow-draining lakes do so supraglacially can only be based on reasoning as in Selmes et al 2011. Without substantial ground-truthing it is not possible to assign accuracy statistics to classifications of this nature. We cannot use higher resolution remote sensing data to aid in this process as we did with calculated lake area in Selmes et al. (2011), as the repeat times of these data are insufficient to calculate drainage/freezing type.

We have devised, at the suggestion of Anonymous Reviewer #1, a means of quantifying error using those lakes where we cannot distinguish between lake types due to cloud cover, this is quantified as the "unknown" class. We do this in the discussion by testing the scenario whereby all unknown lakes are added to each lake type in turn, to see how this influences our findings. We have also included this information on a new version of Figure 4 devised to expand the inter-annual/inter-sector results/discussion as requested earlier in this review.

Minor Comments:

In terms of detailed comments, I will refrain from adding to the very thoughtful insights of Reviewer 1.