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Interactive comment on "Extraordinary runoff from the Greenland Ice Sheet in 2012 amplified by hypsometry and depleted firn-retention" *by* A. B. Mikkelsen et al.

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This study analyses the extreme 2012 discharge observed at the Watson River near Kangerlussuaq, southwestern Greenland in ice sheet surface melt and runoff context. An interesting comparison of summer 2012 with the similarly warm summer 2010 reveals different timing and magnitudes in discharge, as well as ice sheet melt between the two years. The implications are that due to lack of water retention in the lower accumulation area, runoff in 2012 occurred at elevations even higher than the long-term ELA. The study contains valuable information, for example it illustrates the shorter runoff time further in compared to the beginning of the 2012 melt season due to the al-

ready developed surface drainage network and the saturation of part of the snow cover or its complete depletion in the lower parts of the accumulation area.

While I agree with the general analysis, methodology and implications that the authors attempted, I believe that there might be over-interpretation of the observations. In detail, the authors suggest that the melt in 2012 was higher than in 2010 by 106 % above ELA up until 1840 m a.s.l, and by 231 % between 1840–2050 m a.s.l. In a recently accepted study, in which I have been the lead author (Charalampidis et al., in press), and is available as Discussion Paper (Charalampidis et al., 2015), the estimated melt at 1840 m a.s.l. in 2012 was higher by 65 % with respect to 2010. It is understandable that a different 2010 vs 2012 melt ratio can be found, as it is area vs point study, however the high discrepancy between our own estimates based exclusively on in situ observations and this study makes me skeptical over the accuracy of the calculated melt by the authors.

Secondly, it is suggested by Figure 2E that in 2012, melt at elevations 1850–2049 m a.s.l. approximated melt at lower elevations (1550–1849 m a.s.l.) of the accumulation area throughout the melt season. While I agree that melt was prominent in 2012 in the lower accumulation area, I find this result somewhat unrealistic, which points out to the need for more careful melt estimate. For instance, at these elevations during the equally warm summer of 2010 (Charalampidis et al., 2015), Figure 2D suggest very limited melt at elevations 1850–2049 m a.s.l. I cannot think of the factor in terms of atmospheric conditions that induced the difference between the two years, since melt-albedo feedback due to saturation/surface runoff is doubtful that it was active until 2050 m a.s.l. even during the 2012 melt season.

I also find hard to believe, and it is strongly suggested by this study, that the discharge cannot be explained without this – in my opinion – unrealistic estimate for the uppermost elevation interval. Part of the 2012 melt at elevations higher than ∼1850 and as high as 2050 m a.s.l. likely percolated and refroze or run off to lower elevations and refroze, thus contributing to the upglacier extension of the thick ice lenses observed at

lower elevations (Charalampidis et al., 2015). Without denying that runoff occurred in 2012 from well into the accumulation area, I believe that the contribution to total runoff was much lower than what the authors suggest, and should not have been more than 10 %. Considering the increasing complexity of drainage systems with increasing meltwater abundance, I find that it is more likely that during the extreme 2012 melt, water piracy among neighboring catchments was more influential to the observed 2012 discharge at Watson River (Lindbäck et al., 2015). I note that it is also implied by Figure 2d that the late July 2010 peak discharge is also due to runoff from the accumulation area, which is not in my opinion a realistic result.

The above suggest a somewhat speculative closure of the hydrological budget, which is insufficient to substantiate the selected title for the study. I think that the authors should take advantage of the Cryosphere article format, and describe in detail their methodology, in order to clarify the discrepancies with other available melt estimates. The selection of daily time step for the model as well as the utilized model itself should be discussed, perhaps by showing sensitivity tests with smaller time steps to prove the accuracy of the melt calculation. Also, the largest uncertainty in the study, as the authors suggest, is introduced by the delineation of the catchment. I think the study can benefit by the use of the most recent and detailed dataset available (Lindbäck et al., 2014) for this purpose.

I believe that substantial rewriting is in general necessary throughout this Discussion Paper. This can be easily understood by the very long abstract and introduction that contain large amounts of text that belong in other sections, and tends to be repetitive with prime example the very frequent mention of the collapse of the bridge and the minor role of supraglacial lakes. Youtube links are generally to be avoided in scientific literature, while the supplementary figure could have easily been part of the manuscript. Exaggerations along the text should be avoided, e.g. I don't think a 0.3 to 0.5 m thick ice layer can either cause large-scale runoff from the accumulation area or be characterized as "extensive" or "massive". Furthermore, dense ice layer is by definition dense.

Specific syntax errors need to be corrected throughout, e.g. there can be no flattest ice surface, and uncertainty can be either greater or the greatest. Care should be put into accurate mention of facts and figures from the literature, e.g. the baseline ELA is not 1524, but 1553 (van de Wal et al., 2012). Finally, I believe that time series used in this paper, for reasons of consistency, should be mentioned with their established name in the literature, e.g. AWS_U should be KAN_U (Van As et al., 2012; Charalampidis et al., 2015).

To summarize, I welcome the publication of this study in the form of a Discussion Paper. I am looking forward to the more thorough, realistic and significantly improved peer-reviewed form. I could potentially provide more specific comments and detailed inline edits, should the authors feel the need for further input.

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