

# ***Interactive comment on “Multi-year surface velocities and sea-level rise contribution of the Basin-3 and Basin-2 surges, Austfonna, Svalbard”***

**by Thomas Schellenberger et al.**

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Having considered the reviews by Anonymous Reviewer 1 and Alex Gardner, I don't want to pile on too much more misery, because in the end this work presents a great deal of interesting data about the Austfonna surges, and has the potential to provide a useful update to the nature of these events. But a few further things need to be mentioned.

## GENERAL COMMENTS

In accordance with the other reviews, this paper seems to be rather sloppy in places

both in the way it is written, and in attention to detail. I hope that more careful internal review by co-authors during revisions will solve some of these problems. I will refrain from pointing out every small issue, and I look forward to reading an updated manuscript. However, some important issues seem to be recurrent and some not yet mentioned, and these are dealt with below.

## SPECIFIC COMMENTS

Several strong assertions are made about the location and volume of meltwater, and the nature of mechanisms occurring at the bed (e.g. p7, line19; p8, line19; p10, line11; several other places). No observations have been made of meltwater or the bed so it is important to clearly separate what is observed and what is inferred from the observations you have made.

The strongest claims of this paper are made about the frontal ablation rates and sea-level contribution, yet important aspects of these calculations and data are not presented or discussed. The reason that I am troubled by this is that you seem to reach very different conclusions to those we reached in Luckman et al. (Nature Comms., 2015) in which the frontal ablation rates did not change significantly between surge and pre-surge conditions of Aavatsmarkbreen (admittedly a much smaller glacier). Firstly (and I don't say this simply to gain a new citation) it is a bit of an omission not to have discussed your results in comparison to our paper, and I encourage you to check our reference list to be sure that there are no other comparisons to frontal ablation rate papers you have over-looked. Moving on, I am not yet convinced by your method because you give very little detail about the geometry of the glacier and how it is changing and in my experience, ice-front change normally dominates the calculated ablation rate. I would like to see the series of ice-front positions from which you calculate volume loss and, much more importantly, you need to discuss the potential impact of changing surface topography on the values you calculate. How can you justify constant thickness values in Page 6 line 5 (even though you do mention potential errors) during a surge? If so much ice is being lost, where is it going? Do you see large or small calved

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icebergs? How does the lost mass interact with sea-ice? What proportion of loss is through ocean-melt? It is plausible that, because Basin 3 is in a different setting to Aavatsmarkbreen, and the ice-front is diverging into deeper water, your frontal ablation values are reasonable, but you really need to present much more information, and discuss all of the confounding factors, to allow the reader to be able to understand and agree with your conclusions. This is a big claim and needs clearer evidence to support it.

Several seemingly novel compound terms are used as if they are well-accepted: e.g. "frontal ice plug", "sheer-tearing of the ice-sediment interface", "hydro-thermodynamic". In my view these terms obscure rather than clarify the discussions and it would be better to explain things more descriptively, and without the introduction of new jargon.

Page 4, line 21: The use of 'time-steps' does not seem to be helpful, is not consistent with the rest of the text, and, if this really is the best way to explain things, they need to be introduced before they are referred to.

The final paragraph of the conclusion is all about Sentinel-1, yet the paper does not use these data so I find this a strange and unhelpful way to end.

In summary, it will be great to see these observations published and I look forward to seeing the paper revisions, but the frontal ablation calculations (because they seem to be the result you are promoting) need to be presented more completely and more thoughtfully, and more attention to detail needs to be paid elsewhere.

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-5>, 2017.

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