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Interactive comment on "Importance of basal processes in simulations of a surging Svalbard outlet glacier" by R. Gladstone et al.

Anonymous Referee #2

This paper puts forward the importance of evolving basal properties in numerical simulations of ice flow. Sensitivity experiments are applied to the Austfonna ice cap, a well-suited study site, which displays both seasonal, and annual flow variations. The manuscript is generally well written. However, although I fully support the overall aim of the paper, the connection between the various analysis presented is not always obvious. A more careful description of the reasoning and use of model output would help strengthen the manuscripts 'conclusions. Below are my suggestions and comments.

-(l.28, p. 5828) It is not completely clear, how the present study can guide till model development - Probably best to stay in line with the conclusions as stated on lines 24-27, p.5839).

- I would suggest clarifying the goals and approach at the end of the intro. There is

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currently a bit of an imbalance, with a strong motivation given for the transient (basal stress) experiments, and virtually no mention of the steady state (temperature) runs. The link between the two sets of sensitivity experiments is not obvious as it stands.

-p.5833: How different are the SS temperature calculated for 1995 and 2011? If they are similar, then perhaps add a sentence to make this clear. Otherwise, aren't you possibly using fields that are not consistent with each other? For instance, during the temperature inversion, I would expect the amount of basal drag to directly affect basal sliding and thus the temperature distribution in the ice. I see your point (trying to isolate the basal drag effect), but still think that it would have been cleaner to run the experiments with the "complete set" of initial conditions obtained respectively for 1995 and 2011. I suspect your conclusions would remain the same. Alternately, you could show a timeseries, of thickness/vel. . ., averaged over B3, to support the inference that the "mismatch" between temp field and basal conditions do not influence significantly the model output (e.g., are there any "jumps" in variables early in the experiment??).

-l. 26-27, p.5838: I do not see how you can draw conclusions from the steady state simulations to explain the theory of bed reaching T_{pmp} and trigger for the surge phase. Moreover, it is not clear from Figure 7 bottom right, that the inclusion of sliding/advection reduces the temperature throughout the fast flow area – The text needs clarifications, and/or use different color scale on the Figure.

-Discussion on till mechanics (section 4): If the till has been deforming for some time, you may assume that is it in a critical state. In that case, dilatant hardening would not be expected to play a key role (Iverson, 2010, page 1107 for example). Consider removing the discussion related to dilatant hardening.

-l. 18-19, p. 5839: The statement that "water drains in efficient channels without penetrating significantly into the sediment" seems in contradiction with earlier statement of evolving till properties (l. 15-19, p.5837), and needs to be rephrased.

-l.18, p.5827: "In this respect", capital "I"

-l.28, p.5827: Clarke 1987 and Tulaczyk, JGR 2000a should also be cited here.

-p. 5829 / p. 5832: inconsistency in variable name for the basal drag coefficient (C/beta)

-l. 13-14, p.5833: It is not clear to me, what you mean by "Forcing is approximately present day". Climate forcing?? Or do you mean that initial conditions corresponds approx. to present day conditions?

Figures:

-Fig.1 and 2: use km for your UTM x/y coord.

-Fig.1: Those are presumable the winter velocities?

-Fig.2: could you add contour lines of the modeled velocities, for comparison?

-Fig.3: the colorbar is tiny!

-Fig.4, 5, 6: for added clarity, split the panels; enlarge the colorbar and move outside the colored map.

-Fig.7: move the colorbar (as above), and consider changing the colorscale for improved clarity on temp distribution for RHS.

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