Response to referee reports on

"**A model study of Abrahamsenbreen, a surging glacier in northern Spitsbergen**", by J. Oerlemans and W.J.J. van Pelt.

GENERAL

Referee #1 is very positive, and advises publication after minor adjustments. We are grateful for the careful reading and the suggestions for improving the clarity of the manuscript.

Referee #2 is very negative, and advises rejection. We feel that his/her judgement is mainly based on an intuitive disagreement with the approach we take, rather than on solid arguments.
In our view there should always be an opportunity to present different approaches, as otherwise our science becomes one-dimensional. The task of a reviewer is first of all to detect mistakes and flaws, and to make sure that a paper has a good style and the reader can follow what is going on. Our paper provides a different approach, in which the assumptions and limitations are clearly stated (as emphasized by *referee#1*). We therefore think that our paper deserves to be published.
Below we elaborate more on why we think that it makes sense to apply a simple model to a glacier with limited data.

MORE SPECIFIC

Response to referee #1
Response to comments:
• (p 5695) The reason why Kongsvegen was used to determine a value for α_m indeed is that it also is a surge-type glacier in its quiescent phase, only about 40 km away from Abrahamsenbreen.
• (p 5697) This is a mistake in the text: "northwesterly direction" should be "northeasterly direction". With respect to the balance gradients: Austre Brøggerbreen, Midtre Lovénbreen and Kongsvegen are within 60 km west of Abrahamsenbreen, Hansbreen 220 km to the south. However, the balance gradients measured on these glacier are quite similar (Fig. 5). It is thus reasonable to take the average value and apply it to Abrahamsenbreen.
• (p 5698) The increase in E when going in northeasterly direction is taken directly from the map of E over Svalbard provided in Hagen et al (1993, Fig. 8).
• (p 5688) Good suggestion to improve clarity. The increase in the ablation area is indeed the consequence of the surface lowering during the surge.
• (p 5692) Calculation of the ice velocities: the referee is correct. In the initial calculation a time interval of 20 years was used, because the dates of the maps were not precisely known. So 219 and 290 m/yr are the appropriate values.
• (p 5699) E in eqs (15)-(16) is corrected for each individual basin by the numbers in the 7th column of Table 1.
• (p 5708) The contribution from the tributaries becomes zero because they have a negative net surface balance even before the front of the main glacier has passed.
• All the other comments refer to typo's or clarifications which can easily be made.

Response to referee #2

Glaciers are open and strongly damped systems. A lot of mass comes in, a lot of mass goes out. The size of a glacier is determined by the constraint that the ablation zone should be large enough to make melting of all the mass collected in the accumulation zone possible.

Where do ice mechanics come in? Of course the ice has to flow downwards and gravity does the job. The details of the velocity field do not matter, simply because conservation of mass dictates the size of a glacier. However, the *thickness* of a glacier is related to the ice mechanics. When the ice is stiff and / or sliding is more difficult, a glacier will be thicker to produce the necessary larger driving pressure gradient. A thicker glacier implies a higher surface elevation and therefore a more positive surface balance rate. When the balance rate depends *linearly* on altitude, the *mean* ice thickness is the relevant quantity. So when studying the sensitivity of a glacier to climate change, the role of ice mechanics can be taken into account by invoking a relation between the geometry / size of a glacier and the *mean* ice thickness. This is why it makes sense to use minimal glacier models. More generally speaking, we believe that minimal glacier models are useful in regions where measurements are very scarce and the goal is to identify the strength of the basic physical mechanisms.

For calving glaciers, the situation is different, and ice mechanics at the glacier front may play a more important role in the evolution of the glacier. However, Abrahamsenbreen is a land-terminating glacier.

Abrahamsenbreen has a well-defined main trunk, but otherwise the geometry is complicated. A large number of basins feed the main stream at mass-flow rates that vary in time. With the approach taken in our paper this can be dealt with. To model glaciers like Abrahamsenbreen with a comprehensive 3-dimensional model is very complicated, and will probably not be done for a very long time to come. The amount of input data needed is huge: e.g. who is going to map the ice thickness of the tributary basins? And, in the end, lots of assumptions will still be needed to formulate boundary conditions at the required level of detail.

During the past decades there have been many field campaigns to collect glaciological data on some of the glaciers in Svalbard. This has resulted in a long series of basically descriptive papers, in which observed geometric characteristics and velocity fields are summarized. However, efforts to use this information to tackle the basic question, namely how Svalbard glaciers may react to current and future climate change, have been very limited (except perhaps for Austfonna). In our paper we study this basic question, and we can only regret that referee #2 does not appreciate our effort.

Again, in our paper we are open and honest about assumptions and limitations of our approach. The results can easily be reproduced by anyone willing to spend a few days in writing the code. We therefore think that our paper deserves to be published and exposed to the scientific community rather than being rejected on the basis of intuitive arguments.