Cloud effects on the surface energy and mass balance of Brewster Glacier, New Zealand

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General

This is a solid paper in the sense that the modelling work and subsequent analysis is done carefully and described in a systematic way, with clear figures and tables. The authors show that they have a good understanding of the processes they are studying.
However, the originality of the work is low - novelty is missing. There are long sections explaining processes and effects that have been known for a long time and described in many papers in the literature. Apparently there is a tandem-paper (*Cullen, N. J. and Conway, J. P.: Seasonal variability of surface climate and energy balance in the ablation zone of Brewster Glacier, New Zealand, J. Glaciol., submitted, 2015*), referred to frequently, and I wonder why the effect of clouds is not taken along in that manuscript.
I recommend rejection of the paper in its present form, because the scope is too limited and there is not enough substance.

Comments

Cloudiness as used in this paper is *not observed*, but inferred from the AWS measurements. Although central to the discussion, only a few lines are spent to describe the procedure (p. 991, lines 8-15). I cannot judge the quality of the reconstructed cloudiness. The paper referred to is not yet published. How is the discrimination between temperature, humidity and cloud actually done ? How large is the error in the cloudiness after removing the effects of temperature and humidity ? How does this work out on the later attempts to compare the effects of clouds with temperature and other parameters ?

There is no disscussion on the height at which the sensors are mounted on the AWS. Winter snowfall is large; does this cause any technical problems or issues that require corrections in the data ?

The discussion of scale is virtually avoided in the paper. It is known that the components of the SEB vary widely over a glacier surface, and one may wonder to what level of detail the analysis of the situation at a single point should be taken to remain meaningful in view of this spatial variability. In the end, the interest is in the total surface mass budget of a glacier, or at least in the distribution of the balance rate over the glacier. This is particularly relevant because the strength of the snow-albedo feedback on the SMB, an important factor in determining the climate sensitivity and discussed in detail, depends strongly on the altitude relative to the ELA. This paper would have been much more interesting if the calculations would

have been done in a spatially-distributed way (on a grid), or at least for some other points with different altitudes.

The description of results and model output analysis in section 3 is way too lengthy. It is more a listing of observations and thoughts than a clear presentation of the key results. In the text one should not describe in detail what is seen in the figures.

There is nothing special about clouds as compared to other meteorological variables. Clouds occur frequently and affect the SEB in a significant way, but they are just part of the meteorological forcing. The analysis performed here is interesting from an didactical point of view (although not very original), but does not help to improve existing models or projections of future glacier mass balance. One could do a similar analysis for days with low wind speed and days with high wind speed, and conclude that wind speed is important. A statement like *Efforts to characterise glacier–climate connections need to consider the effects of changing atmospheric moisture on melt rate as well as accumulation* is just too general. I would like to see ideas or attempts on how to do that. For instance, what about the use of high-resolution climate models, or just re-analaysis data, or weather station data, to hindcast or forecast the conditions at the glacier spot and drive the mass balance model for 20 years ?

It is state of the art now that data from AWS on glaciers are used to test and calibrate spatially-distributed mass balance models or even high resolution meteorological models that have SMB as an inherent 'product'. Testing and calibration implies a careful quantitative consideration of how processes in the model compare with those measured in the field. The authors have done this only for a single point, and therefore I find the scope too limited.

Altogether, although there is nothing really wrong with this paper in a technical sense, it just does not have enough meat to warrant publication in a journal like **The Cryosphere**. The authors may consider to

- run the calibrated SMB on a grid for the entire glacier;
- reconstruct with this model the SMB of Brewster Glacier for a longer period.

This would make the work much more interesting.

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