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The Cryosphere Discussions

Interactive comment on "Comparison of the meteorology and surface energy balance at Storbreen and Midtdalsbreen, two glaciers in southern Norway" *by* R. H. Giesen et al.

Anonymous Referee #3

Received and published: 27 January 2009

The paper 'Comparison of the methodology and surface energy balance at Storbreen and Midtdalsbreen, two glaciers in southern Norway' submitted by Giessen et al. presents an impressive 5 years data set from automatic weather stations from two glaciers in south-west Norway.

The paper is very well organised, nicely written and the findings are of great interest. I highly recommend this paper for publication within TC after some minor revisions have been performed. In especially I suggest the following items to be addressed prior to acceptance of the paper:

1. It would be good to have some information on the mean equilibrium line altitude

(ELA) in recent years and the vertical distance of the AWS sites from the mean ELA, in order to compare both data sets. Moreover, an idea of recent accumulation area ration (AAR) would be good to learn something about the state of imbalance of the mass balance during recent years.

2. It is tentative to perceive Midtdalsbreen (Mi) as the more maritime and Storbreen (St) as the more continentally influenced glacier, regarding climate forcing. At least the higher specific humidity and higher wind speed at Mi point towards that. However, optical depth and cloudiness are higher on St. The latter that does not fit into this probably too much simplified image. Also, I am surprised that specific humidity is larger an Mi but optical depth and cloud coverage are higher at St. I would expect that more humidity in the air should increase cloud coverage and optical depth and not vice versa. Maybe, the authors could comment on that and try to include some general assessment of differences in climate setting a the two different sites.

3. It would be very interesting to see in which way large-scale climate forcing does individually affect energy and mass exchange of the glaciers. The authors present some of these aspects in a more general approach. However, it would be interesting to evaluate what are the synoptic situations that produce large and striking differences between the two glaciers in some variables or fluxes and to discuss this. I acknowledge that this kind of assessment requires some kind of synoptic type weather pattern classification or the like and this is way beyond the scope of this paper. Nevertheless, I find this very interesting and if it can't be done within the current paper it should be considered as a topic in future research using that data set.

4. I find the assessment of errors not concise enough (p. 889f). Even if the authors do not provide a fully mathematical error propagation analysis for all processing steps - which I understand is difficult to do - I would expect that reasonable error estimates based on assumptions are given for all variables. These values should be included in Table 2.

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Smaller items and technical points:

a) P 877, line 21: 'to the surface. a second sonic ranger ...': The 'a' should be a capital letter.

b) P 880, line 12: '(linear correlation of 0.081)': Is this r or rˆˆ2?

c) P 881, line 18ff: Can you provide a reference for the statement that using the wind speed measurement from the upper level would suppress turbulence too strongly. Otherwise please state the reasons for this behaviour.

d) P881, line 26: The variable 'Zov' is used for the first time and its meaning should be given in words as well.

e) P 882, line 24: Beginning of the sentence with a capital letter: 'a comparison ...'

f) P 884, line 18: '... near the relatively cold glacier surface, ...': I would omit the word 'relatively'. It is not really needed.

g) P 884, line 23: '... slightly higher. a similar ratio ...': Start the sentence with a capital letter 'A similar ration ...'

h) P 885, line 20ff: To me the details in this paragraph sound more like a working hypothesis than a finding. The details presented are reasonable but there is no real prove that it is like this. It is more an interpretation of data. I would prefer this to be stated like a hypothesis rather than like a conclusion.

i) P 888, line 15: Start the sentence with a capital letter: '... on Storbreen. Over the entire'

j) P 889, line 21: Start the sentence with a capital letter: 'A possible source ...'

k) P 890f: In this section I recommend that all references of any other cited studies in this section are included in the text even though they are listed in the accompanying table.

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l) P 891, line 2 and 3: I would move the reference '(by Liestol (1967))' from the 3rd line to the 2nd line after the words 'than those reported', because it is not straightforward that measurements from 1955 are only published in 1967.

m) P 891, line 8 (end of line): Begin the new sentence with capital letter

n) P 898, line 8: It should be a capital letter starting the title: 'A glacio-meteorological'

o) P 898, line 16: 'Kuipers et al.' is only submitted. It cannot be cited in case it is not accepted for publication by the time that this paper will be published.

p) Heading to Table 1: Can you provide reference or website for 'Statens Kartverk' and include that also in the list or references?

q) Figure 1/2: Can you include the locations of the AWSs in Fig. 1. Doing so Fig. 2 could be omitted. It would save space and it would make it easier to perceive the differences in climate setting at the two sites as provided in Fig. 1.

Interactive comment on The Cryosphere Discuss., 2, 873, 2008.