



Interactive
Comment

Interactive comment on “Climatic drivers of seasonal glacier mass balances: an analysis of 6 decades at Glacier de Sarennes (French Alps)” by E. Thibert et al.

Anonymous Referee #2

Received and published: 25 July 2012

General comments

The paper statistically quantifies the long-term link between seasonal mass balance data of Glacier de Sarennes with local meteorolgical data (represented by temperature and precipitation of close weather stations) and with larger-scale atmospheric circulation (represented by the NAO-index).

Low frequency signals, change points and random fluctuations in the time series are detected, based on a methodology published in Eckert et al. (2011, JoG), before the time series and trends are related to each other. Various temporal means and smoothing windows of the NAO-index are tested in terms of their correlation with local meteo-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



rological data and seasonal glacier mass balances.

Regarding the periods before and after the detected change points in winter mass balance in 1976 and in summer mass balance in 1982 the authors analyse changes in snow and ice ablation, in terms of duration and intensity. This is done based on frequent readings of one stake, which has been shown before to be representative for the glacier-wide mass balance. Also, sensitivities of snow and ice ablation to positive temperatures and the sensitivity of the ELA to temperature are quantified.

The paper is of high quality, it is well written, clear and comprehensive. The analysis is described in a very careful and transparent way so that the reader can fully understand and reconstruct how it was carried out. The presentation is well structured and particularly the figures are very intuitive to the reader. The study provides a lot of detailed quantitative results based on field data, which are interesting and highly relevant within the scope of *The Cryosphere*.

In my opinion, the value of the study lies

- (1) in the long term seasonal mass balance data of Sarennes glacier, which had been carefully checked for systematic errors in a previous study by the same authors
- (2) the careful analysis of the mass balance data and its relation to local meteorological data and
- (3) the high temporal resolution of the mass balance data which allows attribution of changes in summer mass balance to changes in snow and ice ablation and to ablation duration and intensity.

The part of the paper which deals with the link of glacier mass balance and local meteorological data to several smoothed versions of the arbitrarily chosen NAO-index is a bit unsatisfactory to me, mainly because it is not motivated by a scientific question or hypothesis and the conclusions reached on that issue are not substantial.

In conclusion, I recommend the paper to be published in *The Cryosphere* after the
C1067

TCD

6, C1066–C1070, 2012

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



authors have addressed some comments given below.

Specific comments

It is not clear from the paper, whether you used homogenised time series of the stations Lyon-Bron and Besse. Please give the source of the data source and state that the data are homogenised. If you did not use homogenised data, please do so, because otherwise you cannot be sure, that the detected changepoints are real climate signals rather than inhomogeneities in the dataseries.

Concerning the part of the paper which links mass balance to NAO: Please consider motivating more clearly, why you carry out that analysis or/and shorten that part of the paper. It reads as if the aim was obtaining optimal correlation between NAO and mass balance. What is the gain in knowledge of correlating the NAO-index with glacier mass balance?

The NAO is an index for larger-scale atmospheric circulation, but a weak predictor for the synoptic situation. If your goal is to show the linkage of glacier mass balance with synoptic patterns, I think you should rather use an index of weather-type classifications. I don't agree with your use of the word "synoptic". Synoptic systems have typical time scales of some days. An averaged NAO over several months is even less an index to describe "synoptic" conditions. Depending on what you want to show, maybe "larger-scale atmospheric pattern" is a more adequate term (as you use it in p 2118 line 23), rather than "synoptic".

I think it is problematic to calculate correlations of smoothed timeseries. You discuss that (p. 2125, line 8), but then you do it anyway and justify it by citing other studies. I don't understand or cannot find a plausible physical reason, why you have to smooth the NAO before comparing it to local data. Just to increase correlation?

On p. 2123, line 25: you justify smoothing NAO by "..seasonal synoptic patterns can influence local climate for longer than they actually last (van Loon and Williams, 1976)".

TCD

6, C1066–C1070, 2012

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

That is not convincing, because related to your work, it means, that the mean NAO-index of one year (or season) has an influence on the following years (or seasons) of glacier mass balance. I think if you argue like that you should propose a physical mechanism how this should work.

p. 2135 lines 14-19: you write "The physical explanation proposed by Beniston and Jungo (2002) for the positive correlation between NAO winter anomalies and high altitude summer temperatures..." I don't find any statements concerning winter NAO and summer temperatures there and I don't understand why that should be connected.

p. 2135 lines 14-19: "... inducing vertical atmospheric circulation, decreasing cloudiness, and thus persistent warming". Consider writing "inducing downward atmospheric motion.." or "subsidence" instead to make clear that it is not ascending motion that is induced.

p. 2138, lines 1-4: "...the NAO index... corresponds to the influence of blocking events in winter or spring on summer balance." Isn't that too speculative to have that in the conclusion section? As far as I know, this has been observed in the last 2 decades only, so it might not be representative for the whole mass balance period of Sarennes?

You show that temperature sensitivity of stake 4 has no trend over 6 decades. That is interesting as it is contrary to results of other studies. Is that likewise true for the other stakes? I think the paper would benefit being more focussed on results on the relationship of mass balance data to local meteorological data, because here you can come up with a lot of quantitative results.

Did you correct the stake elevation for surface lowering during the 6 decades? Could you comment on that?

Title: I think the title might be somehow misleading. Reading only the title I would expect an analysis which covers all possible "drivers" of glacier mass balance to determine their specific importance at Sarennes glacier, but your analysis is based only

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

on precipitation and temperature. In p 2122 line 1 you write "To infer potential drivers, we relate..." This also leads to the expectation that you have included at least radiation in your analysis, because radiation is the main "driver" of ablation in a physical sense, whereas temperature is just a good "predictor" of the summer mass balance, because it is a good proxy for all related processes.

p 2124, line 10: "However, ...NAO series cannot be statistically analysed in the same way as the Sarennes data and local covariates." Why not? As I understand, you do the same analysis, but just the outcome is different in the sense, that you get two change points instead of only one.

Tables 2 and 3: Lyon temperature means May-October daily means temperatures as in Fig. 4? And Besse precipitation means Besse winter precipitation as in Fig.4?

p 2116 , lines 1-2: That has been done in the previous paper by Eckert et al. and I think that should be made clear.

p 2119, line 19: Is there a reason why you didn't update the dataset to 2011?

Technical corrections

p 2120, eq (2): please specify the meaning of N

p 2122, line 10: here you cite (Böhm et al., 2001), but it is not included in the references

p 2134, line 2: replace "snow" with "ice" before "sensitivity"

Interactive comment on The Cryosphere Discuss., 6, 2115, 2012.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

