

Interactive comment on “Modelling annual mass balances of eight Scandinavian glaciers using statistical models” by M. Trachsel and A. Nesje

Anonymous Referee #2

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Review of Trachsel and Nesje: Modelling annual mass balances of eight Scandinavian glaciers using statistical models

General comments

In this paper the authors model annual mass balance of eighth Scandinavian glaciers using statistical models. They discuss the relative importance of precipitation and temperature for the annual balances and the influence of AMO and NAO over time. They derive climate sensitivities and estimate mass balance for 2050 and 2100. The topics mass balance modelling and relationships between mass balance and climate indices are not new in science and a series of works on the matter for the study glaciers already exists. The authors refer to several of them in this paper, but more could be added. Moreover, a clear objective and purpose of the work in line with existing work is

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missing. Reading the paper I am not sure what is new compared to previous studies, what is the motivation of their study, how does their modelling give new insights or can be used in future studies. The abstract and conclusion chapters do not reveal much new information. I also find the authors use of terms somewhat confusing, both their description and division of models and their use of mass balance terms. I suggest to avoid using ablation-season temperature and accumulation-season precipitation (it is heavy to read for one thing) and use standard terms instead. The mass balance glossary is a good source and should be referred to (Cogley et al., 2011). The authors could also discuss their choices more: e.g. why were NAO and AMO selected? There are other indices that has been studied for the same glaciers, see Rasmussen & Conway (2005) already referred to in the paper, and see Rasmussen (2007) (not as easy available, but still a very good work on the topic) in addition to the paper by Mernild (2014) mentioned in the interactive discussion .

To sum up, the authors need to address better what is done so far, what is investigated in this study and what is the new outcome.

Specific comments

P 385. The introduction needs to be rewritten. The authors uses the term physically based mass balance models in contrast to statistical models. They here divide statistical models in i) temperature-index-models and (ii) models that use seasonal and monthly mean T and P. This description in the introduction is a bit unclear and division is not necessarily appropriate for the papers they refer to. There is a whole range of mass balance models from sophisticated energy balance models to simplified temperature-index models and regression models (e.g. see overview by Hock, 2005). Furthermore, mass balance models can be for a point, a profile or they can be spatially distributed, and they may or may not be coupled with a dynamical model. For example an energy balance model can be just for a point, whereas a degree day model can be coupled to a dynamical model. Moreover, temperature-index models also needs calibration.

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P 385/386 Aims i-iii. What is the purpose of these aims? In particular iii) seems not to be a good approach based on current knowledge (e.g. a model need to take into account the changes in ablation and accumulation season as well as dynamical changes). Furthermore, why are these years chosen and more importantly, what is the advantage of this approach compared to already published studies?

Line 23. Meteorological can be deleted before data.

Could add that Liestøl (1967) also used Bergen to model and reconstruct the mass balance of Storbreen.

Could here or rather in the introduction mention other data sources, e.g. seNorge.no, reanalysis data etc. In the discussion could be interesting to have a comparison of the results using other data sources. e.g. that mass balance have been modelled for all of Norway using temperature-index models (Engelhardt et al., 2013)

P387. Objectives are usually given in the introduction. Again, it is not very clear why it is done and why this approach has been used. Move and rewrite first paragraph. Linear and additive models could be explained here. Many different terms are used and not all are familiar with these terms. The authors may help the reader by putting it in context with previous work in the introduction (the terms are not mentioned there now).

P 393. Line 9. Are the results then directly comparable when different (the most parsimonious) models are used for each glacier?

Chapter 3. Line 20->The cumulative mass balance records are not a result of this paper and does not strictly belong here.

P394. Line 3-7. This is not a new result, has been stated in many previous papers. Wording should be rewritten.

P 397 Line 5 This is the first time fig 5 is mentioned, but it ends abruptly with a new paragraph, rewrite. Please explain a bit what you mean and what is new here compared

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to this rather well known results of cumulative balances and mass surpluses for certain periods. Why just AMO and NAO, since there are other indices too that has been studied? The chapter 4 discussion has one subheading for the last ~third of the text, suggest to add 1-2 subheading(s) also for the first part.

P398 Line 18. Incorrect to write that glacier melt started in the early 2000s in Scandinavia. Some of the continental glaciers have in general lost mass throughout since measurements began in 1940s or 1960s, except for the period with transient mass surplus 1989-2005. That glaciers in the alps and Scandinavia correlate differently to the NAO-index is not new, here more references to previously studies should be added, e.g. Marzeion & Nesje (2012) and see also Rasmussen (2007) for more references.

P400. Line 9. Might is a weak Word here, it will. Line 24. Here you refer to previous studies, then use present tense. Then you refer to your own results, a bit abrupt, rewrite.

P401. Line 4-5. I find this way of using references with your results confusing. Use 'as shown by' or similar.

Conclusion: should be rewritten to focus on the new outcomes of your study.

Table 2. The first sentence is repeated in the next sentence. Giesen is written with one s.

Figure 1. The mid panel is cut in a strange way. It should be possible to show it in three full panels, using the same orientation and a different location map than the left panel. The source of the P and T data should be given, e.g. seNorge.no? It looks like three maps, not a location map with inserts (they are the same size), thus, rewrite text.

Figure 2. This is the same as Fig 5a. As this is not a result by the authors I suggest to remove it here and add the data source to the figure text in 5a.

Figure 3. The first two sentences are nearly identical. Change Bn to Ba as Ba is standard term shortening for annual balance now in common use (Cogley et al., 2011).

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Bn is usually used as shortening for net balance.

Figure 4. could specify the confidence bounds in figure text.

Figure 5. See comment to Figure 2. a)-d) are missing from the figure. c) Jones et al., 1997 are referred to, but data spans to 2010, could add an updated ref or write Jones et al., 1997 with updates.

Figure 6. Very small font compared to other figs.

Figure 7. Annual balance = Ba

Figure 8. Not sure if it is worthwhile to show this result due to the limitations of the model. See previous comments.

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Cogley, J. G., Hock, R., Rasmussen, L. A., Arendt, A. A., Bauder, A., Braithwaite, R. J., Jansson, P., Kaser, G., Möller, M., Nicholson, L., and Zemp, M.: Glossary of Glacier Mass Balance and Related Terms, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, Paris, UNESCO-IHP, 114 pp., 2011.

Engelhardt, M., T.V. Schuler, T.V. and L.M. Andreassen. Glacier mass balance of Norway from 1961–2010 calculated by a temperature-index model. *Annals of Glaciology*, 54(63), 32-40. 2013.

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Rasmussen LA. Spatial extent of influence on glacier mass balance of North Atlantic circulation indices. *Terra Glacialis* 11, 43–58. 2007.

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