

Interactive comment on “Monitoring of glacier albedo from optical remote-sensing data: application to seasonal and annual surface mass balances quantification in the French Alps for the 2000–2015 period” by Lucas Davaze et al.

Anonymous Referee #1

Received and published: 23 May 2017

This manuscript is about a method to estimate annual and seasonal mass balances from relations received by using optical satellites on board of the platform TERRA with the sensor MODIS.

General comments:

This study was elaborated very carefully and it presents an interesting contribution to the estimation of surface mass balances for unmeasured glaciers. The paper is in general well written with a clear and logical structure. However, there are some basic points, which have to be discussed. Albedo is mainly a surface specific individual site

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characteristic and it influences strongly the surface energy balance mainly through the shortwave incoming radiation. Therefore, it is clear that process based relations exist, which influence the melt behaviour of a glacier. This is clearly shown by the authors in the paper through the high correlations between summer albedo observations and summer surface mass balance. This part of the paper is excellent and should be kept as it is. However, the relations for winter mass balances the authors try to look for are not really obvious as they are not based on the same processes. Winter balance is mainly influenced by accumulation of solid precipitation and it is in whole paper not clearly stated why winter albedo should have a correlation to accumulation. With the MODIS sensor, it is possible to detect the albedo but never the amount of accumulation, which is the most important variable for winter surface mass balance. It is also clear and the authors mention that correctly in their paper that for larger glaciers (such as Mer de Glace and Glacier d'Argentière) the correlation of integrated albedo is increasing. This is obvious as these glaciers in general have a different behaviour of accumulation than smaller glaciers. Especially in maritime environments one have sometimes even during the accumulation period strongly varying albedo because lower parts of the glaciers show more variability concerning the change between melt and accumulation. Therefore, it can be assumed that the method developed by the authors probably work better in maritime areas than in continental ones. For the small glaciers they introduced a threshold albedo, which varies with each glacier. Thanks to this threshold, they found higher correlations for two of the small glaciers like Saint-Sorlin and Talefère. However, as they state by themselves in the conclusion section: '... but albedo thresholds seem to maximize artificially the correlation between winter SMB and winter integrated surface albedo'. Therefore, the approach is not really suited for the winter mass balance and it is strongly recommended that the authors remove the part related to the winter mass balance in their paper as it is not really related to any substantial processes, which they can capture with their method. It would make the paper much more concise, shorter and more reader friendly.

Specific comments:

Introduction: General: the authors are citing a lot of their own work. It would be appropriate to acknowledge also the international literature about albedo such as the work done by Klok, Knap, Painter, Pope and Takeuchi.

Line 47: add before the references e.g.

Line 49: this is not a very representative list of literature, please add more relevant literature here!

Line 51: Stocker 2013 is not a very good reference at this place of the introduction section. Please add relevant literature which is more specific for the content of your sentence. Line 55: Sort countries in brackets alphabetically

Line 57: replace 'little' by 'small'

Line 64: insert 'To reach this aim (maybe replace by objective) . . .

Line 81: However, this method is still the best one can do and your method is also based on calibration -> therefore this is a very weak reasoning for your method! Please delete this sentence!

Study area and data:

Line 115: but this is in fact a very strong reduction in the number of available glaciers and it reduces the representativeness of the application of the method. Therefore, it is not a very good argument against the study of Drolon et al 2016 mentioned on line 80

Line 154: why not using the individual values to estimate better the uncertainties?

Methods:

Line 177: Sentence misses a verb!

Line 230: Sentence is not clear, please clarify!

Line 237: What happens if you have strong summer snow fall events?

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Line 276-277: is good but contradicts a little bit the objective mentioned in abstract and introduction at the beginning! You want to do it simple but then you agree that it is laborious?

Results:

Line 333: and MODIS does not see these areas?

Line 341: why not selecting only one or two MODIS Pixels in flat parts and observing and comparing these with the measurements and using then for evaluation?

Line 381: Two main observations stand . . .

Line 401: use m as unit instead of cm!

Line 430: However, this is important or not! I do not understand the justification within this chapter?

Discussion:

Line 626: such as instead of such has

Line 632: see study of Machguth, H., Haeberli, W., and Paul, F., 2012, Mass-balance parameters derived from a synthetic network of mass-balance glaciers: *Journal of Glaciology*, v. 58, no. 211, p. 965-979.

[Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2017-49, 2017.](#)

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