The Cryosphere Discuss., 3, C275–C295, 2009
www.the-cryosphere-discuss.net/3/C275/2009/

The Cryosphere Discussions

Interactive comment on "Mapping and morphometric analysis of glaciers in Jotunheimen, South Norway, during the "Little Ice Age" maximum" *by* S. Baumann and S. Winkler

S. Baumann and S. Winkler

sabine.baumann@uni-wuerzburg.de

Received and published: 16 October 2009

Authors' response

First, we want to express our thanks to the three reviewers and Mauri Pelto, and Frank Paul for their constructive and useful comments. Below, we describe the general changes we will apply to the manuscript to improve the paper, followed by more detailed replies to the individual points raised by the reviewers.

Overall remarks

Based on the reviews and the short comments, we would like to make the following

changes to the manuscript:

We will include four new figures: a) a colour-coded map showing the aerial reduction between LIA maximum and 2003, b) a scatter plot showing minimum and maximum glacier altitude vs. LIA glacier area, c) a scatter plot showing area change between LIA maximum and 2003 vs. LIA glacier area, d) an area elevation distribution in 100m intervals for LIA maximum and 2003.

We will revise Tables 1 and 2 as requested.

We will extend and discuss the analysis of glacier length analyses and results changes in more detail. Furthermore, we will extend the comparison to other glacier areas (especially from the European Alps). A new section about analysis of mapping results will be included in Sect. 6.

The text will be revised and several sections be rewritten to clarify the message and improve the English language. The revised manuscript will be checked by a native English speaker.

M. Hoelze (Referee comment)

'Although, the mean glacier length as a variable is proposed in the guidelines for the preparation of glacier inventories by Paul (2009), the implication of the mean length of the glacier flow lines for the presented analysis should be elaborated? Mean glacier length is highly correlated with area and adds no additional information, which is even evident in the results shown in this paper (area change 35% and mean length change 34%). Therefore I posed myself the following questions: a) How many flow lines did the authors use for the determination of the mean value? b) How strong is the mean value influenced by the selection of the flow line? c) What are the criteria for the selection process of the individual flow lines, i.e. when does a glacier branch obtain an individual flow line?'

To a) We used 1-3 flowlines to determine the length of each glacier. To b) We did

not map the flowline several times on the same glacier to study the sensitivity of the length to the flowline selection. This has been done in a previous study resulting in all flowlines lying in the 95%-confidence interval (Baumann, 2006). To c) If the glaciers were very wide in the upper or lower part, the flowline was split in the respective area. On some glaciers, different individual flowlines were mapped. In those cases, we did not split these areas into several glaciers owing to the pre-defined glacier basins.

'In many cases glacier length and length change strongly depends on glacier dynamics. Taking mean values of glacier length and length changes of a large glacier sample suppresses the important individual signal of the glaciers. How do the authors interpret the value of the mean glacier length of about 1.6 km (value only mentioned in the abstract)? What is the meaning of this value? Why not using the maximum glacier length instead and why not analysing and comparing individual classes of glacier length and glacier length changes (the authors have already given some results in table 2, but it is not mentioned nor discussed in the text)?'

The mean value of all the glacier lengths was given to compare it to the corresponding value of 2003. In the revised version we will use glacier length intervals as suggested. We will also lengthen the discussion about the length determination. As MH suggested, we will extend Table 2 and add the values of the 2003 glacier flowline length. Additionally, we will add some results of this comparison in Sect. 5. They will be discussed in a new section.

'In the discussion section (6.4) the results of area change in the investigated region is compared with other mountain ranges by the authors. I would expect a more detailed discussion about the question: why is the relative area reduction in Jotunheimen different from the other regions?'

The discussion will be extended in the revised version. A comparison to other areas in Norway is not possible, however, due to lack of other regional studies.

'Page 359, line 5: The differences between field data and mapping should be specified

with a value.'

We will take up this suggestion and specify the differences in the revised paper.

'Page 360, line 16: If satellite images and aerial photographs are compared to maps in order to assess their accuracy, the data basis (and the accuracy!) of the maps should be known as well. The maps are probably based on the same aerial photographs as the authors used in their study and therefore not independent. This would result in an interpretation of the interpretation quality, only due to different investigators.'

Three different sources were used for the geomorphological maps, but none of the studies specifies the accuracy of the maps. Erikstad and Sollid (1986) made a lichenometric research on the glacier moraines. The authors give no information whether they primarily used aerial photos for the mapping. The scale ranges between 1:8333 and 1:12500. Matthews (2005) used enlarged aerial photos to map moraines in the field. Winkler (2001) used aerial photos the same way, but drew maps on enlarged topographic maps using the results of his aerial photo analysis. The possibility that some mappings base on the same aerial photos cannot be excluded. However, all those references were based on extensive field research. The evidence for a simple double interpretation is estimated as rather small. We do not intend to include additional comments on this point in a revised paper.

'Page 361, line 16: Why do the authors determine the coefficient of determination between min. altitude and total area? Please explain.'

The coefficient of determination is based on the scatter plot of minimum glacier altitude vs. glacier area. The coefficient of determination shows the goodness of its fit according to the best fitting straight line through all points. We removed the plot from the original manuscript, but will include it again in a revised version, combined with a plot of maximum glacier altitude vs. glacier area.

'Page 362, line 5: It would be useful to have a map like in Andreassen et al. (2008, Fig.

5) with a colour coded map of the relative area change per glacier.'

This kind of figure will be included.

'Page 365, line 24: The authors write that 'the glacier surface during LIA maximum is not known . . .' Please clarify and explain, why the surface of the LIA glaciers could not be reconstructed? In the European Alps several reconstructions of LIA maximum glacier surfaces and volumes have been performed (e.g. Maisch, 2000).'

The glacier surface at LIA maximum is not known. A reconstruction could be done, but will only be an estimate based on empirical studies and statistical or physical relationships (Paul, 2007), in any case with a certain degree of uncertainty. The main problem is usually the mapping of the LIA trimline in the upper reaches of a glacier (accumulation area), whereas the LIA snout position is much easier to detect. Reconstructing the LIA surface was not the goal of this paper and has not been done.

I. Brown (referee)

'However, I find the assumption that the outermost moraines are LIA moraines, somewhat unconvincing. I note that the authors accept that certain high altitude sites may be exceptions. Nevertheless, the authors need to provide more evidence to backup the fundamental assertion. Recent studies by, amongst others, Atle Nesje and Richard Shakesby suggest that outer moraines might be older than the LIA. Whilst I accepted that Jotunheim might be different than Jostedalsbreen and its' surroundings this needs to be properly validated.'

A recent study show that the outermost moraines in the Jotunheimen region indeed date from the LIA with very few exceptions (Shakesby et al., 2008). The same is also shown for the Breheimen and the Jostedalsbreen region. Timing and expansion of pre-LIA glacier stages in Jotunheimen is mainly made by analysis of drilling in glaciofluvial sediment and radiocarbon dating of soil sediments (e.g. Matthews (2005); Shakesby et al., (2007); Matthews and Dresser (2008); Nesje et al. (2008); Nesje (in press)).

In Breheimen, an extensive research project (with one of the authors (SW) heavily involved) did only show possible pre-LIA moraines at some very few high-altitude, ice-cored moraines (Winkler et al., 2003; Shakesby et al., 2004). Overall, in Breheimen as in Jotunheimen, only very few high-altitude sites have outermost moraines with a potential pre-LIA origin – and those can relatively easily be detected by their different morphology.

'The authors could further improve their paper by explaining the orthorectification methods used. From reading between the lines I assume a polynomial fit of some sort was used. Please provide more information on this. '

The orthorectification of the satellite image was already done by Norsk Satellittdataarkivet and checked by NVE (Andreassen et al., 2008). The aerial photos were orthorectified by the authors using ERDAS IMAGINE 9.1. We followed the guidelines for orthorectification in the manual ERDAS IMAGINE Tour Guides (ERDAS IMAGINE (2006), pp. 287-304). First, we performed a geometric correction by using principal point (measured on image), focal length (given), the N50, fiducials (measured on image), projection UTM WGS 1984 Zone 32N, and four to eight GC points (depending on fitting of image). As resampling method we used Cubic Convolution. After this process, the aerial photos were combined as mosaic. Probably, the detailed description of the orthorectfication is only interesting for people familiar with this kind of processes. We will include in Sect. 4.2 a short comment that we followed the ERDAS manual.

'I would ask the authors to consider changing the use of the term Central Flowlines to Centrelines as we are unsure of the morphology of the historical glaciers and their flow regimes.'

This will be done.

'Section 4.2, p. 358, line 23. I am unclear as to whether the map was always used as the basis for glacier mapping? Is this the case?'

We wanted to state that the glacier outlines deduced from the geomorphological maps were used as final outlines when they were available for a glacier. They were not altered even if outlines deduced from the satellite image or the aerial photos were different. To avoid misunderstanding the sentence will be rewritten.

'Section 5 (Results) p. 361 line 13. Why are the maximum and minimum altitudes of interest? How can you reconstruct the maximum altitude without attempting to reconstruct glacier volume?'

True, we do not know the real maximum altitude as the glacier surface is unknown. The maximum altitude is in most cases similar to 2003. We included it as the minimum and maximum altitude is part of the basic inventory data of glaciers, and can give insight into the glaciological regime and climate setting. The uncertainty in reconstruction of the maximum altitude is quite small compared to the uncertainty in reconstructing the glacier volume. Therefore, we did not want to estimate the volume in this study. See also comment to M. Hoelzle.

'Section 5 (Results) p. 361 line 28 (and line 1 p. 362). This is repeated from Section 4.2.'

Will be deleted.

'Section 6.4 p. 367 and 368. I don't believe that it is relevant to compare Jotunheimen with Baffin Island and the Southern Alps. I would rather you search the literature for more data from Scandinavia and Europe.'

A few remarks on this point have already been made above. We do not know of other comparable studies from Scandinavia, but we have included additional studies from Europe in our comparison as suggested. We do, however, disagree that comparison to the Southern Alps of New Zealand is irrelevant. Chinn et al. (2005) clearly demonstrated a high degree of simultaneous glacier behaviour in maritime Norway and New Zealand in the 20th century. By including LIA maximum as far as data is available, a

comparison between maritime Norway appears to be much more justified than the traditional comparison between the Southern and the European Alps (Winkler, 2009; Winkler, Matthews & Nesje in preparation). Owing the lack of data from Jostedalsbreen, the Southern Alps of New Zealand act as substitute in order to retain all potentials with the paleaoclimatic interpretation of the results. The glaciers of Baffin Island are located in a different climate and landscape, and the glaciers are larger than in Jotunheimen. A comparison to this area is thus interesting because of the differences. Therefore, we will take up the suggestion to include additional studies from Europe in our comparison, but keep the other glacier areas.

'As a general comment I think this paper would benefit from an extensive revision with particular attention paid to the language and grammar. The English is rather poor and should be reviewed by a native speaker.'

We agree and will do so.

A. Kääb (referee)

'(1) The English language is not bad, but needs thorough revision by a native English speaker. It contains a number of errors. Also, a number of scientific terms are wrong and complicate the understanding (e.g. 'shot' for acquisition; is 'foreland' a correct term for forefield?)'

According to the some of the most prominent native-speaking experts on the related field (e.g. John A. Matthews), glacier foreland is the appropriate English translation for the German 'Gletschervorfeld' (introduced by Kinzl). Glacier 'forefield' is regarded as wrong and inappropriate. We thus keep foreland.

'(2) It is confusing to me which data sets the authors actually compiled by themselves and which they got readily and by whom. They authors have to clearly state which data they got readily, where from, modified, or constructed by themselves. Best place to do that might be the data sources list under 4.1. Some of the data you used might

be copyright protected and you might want to refer to the permission under which you used it.'

The list of the study material will be revised. We will also extend Sect. 4.2 and acknowledgments.

'(3) It would be good to have references to similar works done elsewhere, e.g. in the Alps (e.g. Maisch et al). What was done similar, what different. Different or similar analyses, etc.?'

See overall remarks and reply to I. Brown.

'page 352, line 7: flow length: correct term? Is that the length of the central flow line?'

Term will be changed.

'P352 2nd paragraph: Too simplistic. Be more specific in what is relevant for your study.'

The order of the paragraphs in Sect. 1 will be changed in order to clarify the impact on our study.

'P354 L14: ØYEN. Why capital letters?'

Typing error.

'P356 L6: why referring to Bindschadler et al. 2001? This paper is about Landsat7. You used Landsat5. The paper is a large review. What exactly do you refer to?'

Bindschadler et al. (2001) was chosen to address the need of having little seasonal snow remaining when mapping glacier outlines from satellite, the type of Landsat was unimportant. Maybe, the reference is misleading. Instead, we will refer to Paul (2009).

'Section 4.2.: It became not clear to me if you digitized the entire LIA outlines, or made a connection with LIA outlines in the lower parts and the present day outlines for the upper part. How did you reconstruct the LIA extent in the upper glacier parts. It is often

not well visible.'

We did not digitize the entire LIA outlines, but used the outlines of the glaciers from the 1980s and 2003 for the upper parts. These outlines represent the minimum glacier extent. AK is right, that changes in the upper part are often not well visible, especially compared to changes in the lower part. We only digitised the upper parts when trimlines were clearly visible.

'Section 4.3: the influence of the N50 DEM not reflecting the LIA glacier surface should be mentioned already here and discussed carefully.'

Will be done.

'P362 L23: I don't understand well why/how you re-did the orthorectification.'

The description of redoing the orthorectification was a little bit misleading. In the 'normal' orthorectification we used the DEM as elevation file and the N50 as x/y-reference. One third of the aerial photos fitted poorly with the glacier outlines from the 1980s. We extracted the altitude lines from the DEM, and a difference was visible between N50 and the altitude lines of the DEM. We also orthorectified several aerial photos with the DEM as elevation file and as x/y-reference. Therefore, we wanted to test the impact of this difference on the mapping of the LIA outlines, especially on the aerial extent. We mapped the LIA areas on these orthophotos and compared both obtained glacier extents. To will make this process more clear in the text and change the mentioned sentences.

'P363 L5: Orthorectification of maps? A contradiction in itself.'

AK is right that the formulation was a contradiction. As mentioned above and in Sect. 4.1, the geomorphological maps are not constructed as topographical maps, but more like a sketch. This means, that they were prepared on non-orthorectified aerial photos. Therefore, they had to be georeferenced. We made a geographical correction of the maps and used the aerial photos in addition to identify possible GCPs. We will

change ‘orthorectification’ (363, 5) to ‘georeferncing’ and add that non-orthorectified aerial photos were used as basis for the geomorphological maps.

‘P363 end and P364 beginning: I don’t understand what you mean. Please be more specific.’

We wanted to point out two different ways of handling sources of different resolutions. An error is included if a comparison is made (363, 17-26). If sources of different resolution are used as complements of each other errors in combining the results or making analysis are possible. But we did not find any reference pointing to this problem. Therefore, we will delete this last paragraph of Sect. 6.1 because it is not necessary for our study.

‘Section 6.3; P366 L 11: Why did you have to orientate all airphotos individually, not combine many to an image block? That would presumably have given much better results.’

We used the ERDAS manual (see comment to I. Brown). We would like to use an image block orthorectification process to enhance the fitting quality, but have not done so. Additional advises about this method by AK are very welcome.

‘Acknowledgements: Usually, the reader finds here some information on the data providers. See major above recommendation (2)’

Additional information will be added in the acknowledgements about data providers.

‘References: Nesje ... xxx ?’

Will be done.

‘Figures: I would very much like to see the usual scatter plot of all glaciers and their area changes (i.e. glacier size vs. glacier area change between 2003 and LIA).’

Will be done.

M. Pelto

‘1. In Andreassen et al., (2008) the map showing the percent change in glacier area over a more recent period is provided. This map illustrates the spatial differences in the changes around Jotunheimen, highlighting the greatest changes in areal extent occurring on the northern side and eastern side of the district. This same map should be included for the percent change in areal extent from the LIA maximum to 2003 in this paper. Is this same pattern of change evident? This comparison will indicate whether the spatial pattern of retreat from the LIA maximum to the mid-20th century has been different than the spatial pattern since. This will differentiate spatial variation in glacier response to post Little Ice Age climate both in the shorter term and longer term.’

See overall remarks, a map will be included. A comparison of the aerial change (between LIA and 2003 and between the 1980s and 2003 (Andreassen et al. (2008))) shows that the area reduction varies within the region. The pronounced retreat in the Northeast and East of Jotunheimen in the recent period (1980s – 2003) is not visible over the longer time span. No spatial pattern at all is visible for the area change between LIA maximum to 2003. We will add more information about this comparison in the revised version.

‘2. The minimum, maximum and mean glacier altitude were determined, only the mean minimum and mean maximum altitude are briefly reported, without any spatial analysis. In particular it would be quite important to report the change in these three mean altitudes from LIA maximum to the present. This would provide a useful measure of mean ELA change. As Raper and Braithwaite (2009) recently noted the median and mid-point altitude provide useful measures of both the ELA and through comparison the mass balance gradient. In particular the spatial pattern of midpoint or median altitude change would address the objective of identifying spatial differentiation of glacier behavior. The change in mid point altitude can if it is informative be related to aspect or glacier size.’

The importance of these variables, especially the spatial variation was not emphasized in the TCD-manuscript. To gain a spatial overview of these variables, four colour-coded glacier maps were made showing the relative changes of them between LIA maximum and 2003. Additionally, the relative altitude change vs. LIA area was plotted in four graphs. None of these maps and plots did show any differentiated spatial pattern and was also not related to glacier size. We therefore did not include any of them. But we will describe these results in a revised version (Sect. 5). The mean and median altitude at LIA maximum, and not so significantly the minimum and maximum altitude show a spatial differentiation and increase from West to East. This increase is seen as a change of climate regime in between Jotunheimen with a more maritime western and Central and a more continental eastern part. Connection to glacier size was not visible, only for the minimum value, as already described in the manuscript. Not any pattern was visible between the changes of the four altitude values and aspect or slope. We will address these issues in the discussion (new section).

'3. Why is a value of 0.01 km2 used for minimum glacier size? This is quite small. The overall impact on glacier area is insignificant of course. However, does inclusion of these small ice masses significantly alter the conclusion as to glacier disappearance? If 0.1 km2 is used does the number of glaciers that disappeared change? Andreassen et al., (2008) indicate 35 glaciers in the less than 0.1 km2 size class versus 26 noted for the LIA maximum. How many of these are the same glaciers? Of the 13 that disappeared what was their LIA maximum size? Recent papers from the North Cascades and Switzerland have noted greater percentage losses for smaller glaciers in the last 50 years.'

As already mentioned in the text, the size of 0.01 km^2 is chosen according to the pixel size of the satellite image (larger than nine pixels) (Andreassen et al., 2008). A larger minimum size was not chosen because we wanted to keep the same initial conditions as for the glaciers in 2003 in Jotunheimen. A direct comparison to Andreassen et al. (2008) only from the publication is not possible, because they included the glacier

area Breheimen that is excluded in our study. The number of 35 glaciers < 0.1 km^2 MP mentioned is indicating the number of glaciers on the N50 (topographic map of the 1980s) and not the number of glaciers in 2003. In Jotunheimen, there are 75 glaciers smaller than 0.1 km^2 in 2003 and 26 at LIA maximum. 22 of these glaciers are the same. Therefore, four of these 26 glaciers disappeared between LIA maximum and 2003. If we exclude all glaciers < 0.1 km^2 at LIA maximum, hence nine glaciers disappeared between these two dates. These nine glaciers ranged between 0.11 and 0.25 km^2 at LIA maximum. Because of the small number of disappeared glaciers, an exclusion of the smallest area interval would alter the results. 5.6% of the glaciers disappeared with the original dataset, 4.3% with LIA area > 0.1 km^2, and 5.0% with LIA and 2003 area > 0.1 km^2. Therefore, we will not change the initial setting, but we will add a comment on it in the discussion (Sect. 6.2).

'Minor Points: The timing of maximum LIA extent is discussed at length. The focus is on changes from the LIA maximum regardless of timing. Given that no rates of change are calculated the determination of a specific date is not vital. This section could be shortened.'

This minor point by MP had its relevance to the original manuscript. As we will show a spatial pattern of Jotunheimen (see comment above) in a revised version, Sect. 3 now serves as basis for the results and discussion of this topic and will not be shortened.

'A more appropriate regional comparison in terms of glacier size and climate setting is Garibaldi BC. A recent paper Koch et al., (2009) undertakes a similar study and notes a 51 decline in glacier area from the late 18th century to 2005.'

See overall remarks and reply to M. Hoelzle.

'Section 6.2 should be before the results.'

Section 4.5 (Sensitivity analysis) is before the results. Therefore, we will keep Sect. 6.2 (Uncertainties of mapping) at this place because uncertainties in general are also

discussed there.

'A more detailed image illustrating a moraine wall in satellite imagery would be useful.'

Fig. 5 is showing three detailed satellite images of three different glaciers. We assume that it is difficult to see the moraine wall on these images because of the overlaying LIA glacier outlines, but decided not to add any further image. Landsat images can be downloaded for free on the NASA web-side (https://zulu.ssc.nasa.gov/mrsid/mrsid.pl), and, according to our given coordinates, it is possible to locate Jotunheimen and have a look at detailed images. Furthermore, the visibility of LIA extent is an important point but good enough as described in Sect. 4.2 that it will not need more emphasis in a revised paper.

F. Paul

'Apart from some smaller points that have also been mentioned by the other reviewers (english grammar, used terminology, averaging of length changes), my major objection is the missing presentation of the derived inventory data (e.g. mean, max., min elevation, slope, aspect, etc.). Where can I find them? The title also mentioned 'morphometric analysis' of the glaciers. What does this mean? I strongly encourage the authors to add at least a few of these derived data sets and their changes since the LIA (see details below) to make the study more comprehensible. Otherwise, the most interesting part of the study (in my opinion) would be missing. The results section is currently rather thin and only aggregate figures are given for the observed changes. I see a large potential to improve the paper by adding more of the obtained results of the glacier inventory ('morphometric analysis') and the associated changes since the LIA. Please consider to include the following graphs (e.g. using scatter plots) with a discussion: - mean aspect vs. mean or median elevation - min/max elevation vs. glacier size - relative change in size vs size (maybe also in a table per size class) - change in length vs length - area elevation distribution in 100 m bins (LIA and 2003) Also, change in min or mean elevation since LIA would be interesting as this is a good proxy for the

change in ELA0.'

See overall remarks, reply to M. Hoelzle, and reply to M. Pelto. We will add a selection of the suggested figures. Morphometric analysis means in this study the analysis of glacier variables based on topography.

'The statistical averaging of size or length dependent values like changes in length or area, should be avoided or at least commented.

See reply to M. Hoelzle.

'The differences to other mountain ranges might be more related to a different size/length class distribution rather than due to differences in climate forcing. Please compare only the values for similar size/length classes.'

See overall remarks and reply to M. Hoelzle.

'In part, this paper reads like a review of my own publications. However, it is not only me who have published papers in this field. A large number of other studies on glacier mapping and change assessment in different parts of the world exists as well and I would suggest to add a few of them.'

See overall remarks and reply to I. Brown. We will also add some other studies.

'The english grammar and wording needs improvement at several places. Please use the help of a native speaker to polish the english (e.g. in most cases 'larger' should be used for 'bigger')'

Will be done.

'The terminology is sometimes not consistent with the standards (e.g. mean glacier flow length).'

Will be changed.

'When something has been done for this study, please write it. On page 353, L19 you

write ‘inventory data ... can be determined’ (cf. also P 368, L18). I assume you have calculated them? On page 357, L11 you write ‘All material has to be orthorectified ...’., I assume you have done this?’

Will be done.

‘Paul 2003 is only published electronically, please replace with Paul 2007 which is a book.’

Will be done.

P 354, L2: ‘... with decreasing mass turnover ...’

Will be done.

P 358, L9: Please use ‘separated’ instead of disintegrated (which is close to disappearance).

It is 359, 9. Will be done.

P 361, L16: I would rephrase ‘altitude difference’ to elevation range

Will be done.

P 362, L6: Length changes should not be averaged as there is generally a strong dependence of the change on the original length.

OK. See reply to M. Hoelzle.

P 366, L20: Please change to ‘Sidjak and Wheate, 1999’ (this is also wrong in references P 372, L23)

Will be done.

P 367/8: The comparison with glacier changes in the Alps, on Baffin Island or New Zealand should be justified somehow. Please only compare changes in the same size classes or give a comment on how size class distributions differ in the respective

mountain ranges.

See reply to I. Brown.

P369-373: It seems that the list of authors is not in the correct order for publications with the same first author (one author, two authors, three or more authors).

Will be done.

P 375, Table 2: The row ‘> 10 km’ can be omitted when there is no glacier longer than this.

We put this row in the table to emphasize that there are no glaciers longer than this. A last row ‘$\geq$ 5 km’ would not show this circumstance. Because of the mention in the text (Sect. 5), we will change Table 2.

P378, Fig. 3: In the Fig. caption you write ‘glacier outlines 1980s’, but in the legend you show glacier area covered (shaded grey). I would suggest to only overlay the outlines using a colour that is different from grey (maybe light blue?).

We wrote glacier outlines in the source references, but glaciers in the figure caption. The outlines were from Statens Kartverk and we modified them by ourselves to glacier areas. Therefore, we will not change this figure.

P379, Fig. 4: The glacier with the red number 119 shows a straight line as a flowline which seems to be not perpendicular to the elevation contour lines. Please check. Can you add elevation contour lines also on the 2003 glacier surfaces?

Will be done. Contour lines are already on the 2003 glacier surfaces, but maybe they were a little bit too bright.

References:

Andreassen, L. M., Paul, F., Kääb, A., and Hausberg, J. E.: Landsat-derived glacier inventory for Jotunheimen, Norway, and deduced glacier changes since the 1930s,

The Cryosphere 2, 131-145, 2008.

Baumann, S.: Aufbau eines Gletscherinventars für Kolumbien und Abschätzung glaziologischer Parameter, Unpublished masters' thesis, Ökoklimatologie, Technische Universität München, München, 2006.

Bindschadler, R., Dowdeswell, J., Hall, D., and Winther, J.-G.: Glaciological applications with Landsat-7 imagery: Early assessments, Remote Sens. Environ., 78, 163-179, 2001.

Chinn, T., Winkler, S., Salinger, M. J., and Haakensen, N.: Recent glacier advances in Norway and New Zealand: A comparison of their glaciological and meteorological causes, Geogr. Ann. A, 87, 141-157, 2005.

ERDAS IMAGINE: ERDAS IMAGINE Tour Guides, 730 pp., 2006.

Erikstad, L., and Sollid, J. L.: Neoglaciation in South Norway using lichenometric methods, Norsk geogr. tidsskr., 40, 85-105, 1986.

Matthews, J. A.: 'Little Ice Age' glacier variations in Jotunheimen, southern Norway: a study in regionally controlled lichenometric dating of recessional moraines with implications for climate and lichen growth rates, The Holocene, 15, 1-19, 2005.

Matthews, J. A., and Dresser, P. Q.: Holocene glacier variation chronology of the Smørstabbtindan massif, Jotunheimen, southern Norway, and the recognition of century- to millennial-scale European Neoglacial Events, The Holocene, 18, 181-201, 2008.

Nesje, A.: Latest Pleistocene and Holocene alpine glacier fluctuations in Scandinavia, Quaternary Sci. Rev., 1-18, in press.

Nesje, A., Bakke, J., Dahl, S. O., Lie, Ø., and Matthews, J. A.: Norwegian mountain glaciers in the past, present and future, Global Planet. Change, 60, 10-27, 2008.

Paul, F.: The New Swiss Glacier Inventory 2000 - Application of Remote Sensing

and GIS, Schriftenreihe Physische Geographie, Glaziologie und Geomorphodynamik, edited by:

Haeberli, W., and Maisch, M., Geographisches Institut der Universität Zürich, Zurich, 210 pp., 2007.

Paul, F.: Guidelines for the compilation of glacier inventory data from digital sources, GLIMS, 2009.

Shakesby, R. A., Matthews, J. A., and Winkler, S.: Glacier variations in Breheimen, southern Norway: relative-age dating of Holocene moraine complexes at six high-altitude glaciers, The Holocene, 14, 899-910, 2004.

Shakesby, R., Smith, J., Matthews, J., Winkler, S., Dresser, P., Bakke, J., Dahl, S.-O., Lie, Ø., and Nesje, A.: Reconstruction of Holocene glacier history from distal sources: glaciofluvial stream-bank mires and a glaciolacustrine sediment core near Sota Sæter, Breheimen, southern Norway, The Holocene, 17, 729-745, 2007.

Shakesby, R. A., Matthews, J. A., and Schnabel, C.: Cosmogenic 10Be and 26Al ages of Holocene moraines in southern Norway II: evidence for individualistic responses of high-altitude glaciers to millennial-scale climatic fluctuations, The Holocene, 18, 1165-1177, doi: 10.1177/0959683608096592, 2008.

Winkler, S.: Untersuchungen zur Klima- und Morphodynamik in skandinavischen Gebirgsregionen während des Holozän – ein Vergleich ihrer Wechselwirkungen und Prozesssysteme im überregionalen Kontext kaltgemäßigter maritimer Gebirgsregionen, Habilitation thesis, Fachbereich Geographie / Geowissenschaften, Universität Trier, Trier, 2001.

Winkler, S.: Gletscher und ihre Landschaften. Eine illustrierte Einführung, PRIMUS Verlag, Darmstadt, 183 pp., 2009.

Winkler, S., Matthews, J. A., Shakesby, R. A., and Dresser, P. Q.: Glacier variation in Breheimen, southern Norway: dating Little Ice Age moraine sequences at seven

low-altitude glaciers, J. Quaternary Sci., 18, 395-413, 2003.

Interactive comment on The Cryosphere Discuss., 3, 351, 2009.