Review of the study by Zhang et al. in TCD

General comments

The study by Zhang et al. aims at presenting changes in glacier extent over a 40-year period in the Geladandong mountains using satellite data. Though such studies are generally welcome, this one has a major flaw and several other severe shortcomings that need to be addressed before this study can be accepted. I have listed them below and give some advice on potential improvements.

The major flaw is related to the glacier area derived from the MSS scene. A large portion of what has been classified as being a glacier and already disappeared by 1992 (see Fig. 7) is actually ground ice located in river beds. Of course, this cannot be calculated to the glacier class despite having the same spectral properties. In consequence, the entire study needs to be performed again (please also consider the specific comments below for this). I further recommend to the authors to get more familiar with the interpretation of multispectral satellite imagery and the numerous forms ice can have without being a glacier.

Specific comments

Introduction

The introduction should provide important background to a study and give a motivation for the research objectives. Motivating area change assessment with potential future shortening of fresh water once the glaciers have disappeared does not really work. The link between changes in runoff and changes in area is very poor and might even be reverse with increased glacier shrinkage (when surface elevation decreases). I suggest to have a focus on the remote sensing methods and include for example a comparison between manual delineation of glacier outlines and those generated by unsupervised mapping from MSS and CBERS which both lack a shortwave infrared band that is usually used for automated glacier mapping.

Repetitions should be avoided and the topics presented should have a logic flow. On P509 it is written that: “Mountain glaciers are ... considered to be sensitive indicators of climate change” (L17/8) and in the next sentence: “Alpine glaciers, ... are also regarded as one of the best natural indicators of climate change”. Further “... an increasing number of researchers are beginning to study glacier change.” sounds like this is a new topic but it has already a 25 year history for remote sensing based studies. The next sentence “Climate in the ...” has no relation to the context before and should be introduced properly. It also interrupts the general thematic flow as on page 510 “Developments in remote sensing ...” continue with the remote sensing topic. Please be aware that (L8/9) neither Hall et al. (1987) nor Paul (2002) “quantify long-term trends of glacial extent” (these are methodological papers). Also the study by Silverio and Jaquet (2005) only covers a 10-year period.

The following context about remote sensing is very general, but the statement on L10 about the unsupervised classification is highly specific and comes from no-where. As far as I know, automated mapping with Landsat MSS is a rather challenging issue with lots of problems (e.g. Svoboda et al., 2009). Statements like these require better explanation (and this might be better done in the Methods section). The details about the study region (L13-17) might be more appropriate in section 2 where they are currently largely repeated.

Sentences like (L23) “... the findings allow inferences to be drawn on the future of frozen water resources in northwest China.” require two points to be clarified: (1) It should be stated how this can be done (deriving from annual area change rates the future water resources and concluding from a small subregion to what will happen in northwest China), and (2) in the current writing it reads like if glaciers are frozen water rather than made of compressed snow.

Previous works: when only one study is cited (Ye et al. 2006) it must be “but this study ...” in L 26. As this study uses exactly the same MSS and TM image, a more critical look at their results should be

provided before the work is repeated. In their Fig. 2 the here wrongly mapped ground ice is not visible. It must also be explained what the present study adds upon previous work. Just adding one more year to the same dataset might not be sufficient. In my opinion the Table 4 in Ye et al. 2006 is providing more information than Table 2 in this ms.

The statement (L2/3 on P511) "Ice thickness information is unavailable, so these results do not allow us to assess the trend in ice volume loss." is also interesting. Why is ice thickness information required to derive volume change? And why is it already possible to expect a specific trend (volume loss) when changes have not been determined (maybe also considering the surging glaciers in the region)? Please be aware that discussion of climate change impacts on runoff requires determination of volume changes or mass balance. Area changes (by a few percent) do not really impact on runoff as the larger ablation due to surface lowering can easily offset the reduction resulting from a smaller area.

In summary, the introduction need to be restructured, provide stronger arguments about the motivation, and clarify what has been done here compared to earlier works. In addition, some wrongly stated glaciological concepts need to be corrected.

Study area

To advance the science somewhat over what has already been done by Ye et al. (2006), I would suggest that this time all glacier changes are analysed in respect to their individual sizes, i.e. with proper drainage divides for each glacier. Values for the contiguous ice masses can be provided as well, but maybe for more than only the three selected here. In this regard, the "includes over 40 glaciers" (L23/24) is "includes over 40 contiguous ice masses" (or glacier complexes) as individual glaciers are not analysed. Please be also aware that this is a rather small sample compared to earlier studies and to what (the freely available) satellite data provide. Change assessment for a few hundred glaciers (should be easy after separating them) and a somewhat larger region should be considered at minimum.

Another important reason for looking at individual glaciers rather than glacier complexes is the high number of surging glaciers in the region. Their area gains and losses should be treated separately from all other glaciers (e.g. Yde et al. 2010).

Providing climate data for the investigated region is not really required, as they are not used further in this study (e.g. for a climatic characterization of the glaciers). Wikipedia is not the most reliable source for such information.

In view of the applied (but unfortunately undefined) unsupervised classification technique it would be important to mention that neither shadow nor debris cover is a problem for glacier mapping in this region and that only bare rock is surrounding the glaciers which allows discrimination of glaciers from other terrain also with VIS/NIR bands (due to their high reflection). Of course, it is expected that the difference between ice on the ground and glaciers is known and it should be mentioned that the former is present in the MSS scene.

When images with adverse snow conditions are used (CBERS) they should be shown and it should be illustrated how the manual corrections have resulted in a reasonable outline. It might also be a good idea to compare the CBERS results in this case with a more recent Landsat scene (e.g. there is one from 2009) to be clear that the CBERS outlines are accurate. Otherwise results are not trustworthy.

Data and methodology

This section needs to be restructured. Currently it is a little bit chaotic in my opinion. It jumps from a short characterization of all Landsat sensors, to a historic reference, to a description of CBERS, to data sources, to CBERS geometric correction, to spectral characterization of glaciers and an overview of methods (have all of them been applied here?), to error terms for each sensor. Thereby, well

established glacier mapping methods (e.g. band ratios) are not mentioned (cf. Racoviteanu et al., 2009), an undefined unsupervised classification method is applied (it needs to be described how this method is working) but glacier area is extracted by visual interpretation (P514, L2/3), and changes in area extent is derived using overlay analysis. Though tempting, this method should not be used for this purpose as it is too sensitive to geolocation errors and noise in the classification). Moreover, rather historic online references appear (P512, L19 & P513, L24) that do not really explain what has been done in this study (and should be replaced with journal papers).

My first suggestion to improve this section is to separate the data from the methods section. There is no need to repeat details from a table in the main text (resolution, band numbers) when they are not further used to illustrate something. The important point here would be to mention that neither MSS nor CBERS has a SWIR band that could be used for automatic mapping so that manual digitizing is required. Secondly, the unsupervised classification method needs to be described in full and thirdly, please do not assess area changes from overlay analysis. The area of each glacier needs to be determined for each image individually (after careful correction of errors) as otherwise area changes might result from co-registration errors. Though the overlays in Fig. 7 are too small to see it clearly, they indicate changes that are related to co-registration errors and adverse snow conditions (e.g. the grow of glaciers from 2004 to 2011). Indeed, there is no way to derive any meaningful numbers from such a comparison. I would also suggest illustrating the snow conditions in the various scenes (showing a close-up), in particular where they are poor. But given the large overlap with the study by Ye et al. (2006), I also suggest investigating another (and much larger) region. Finally, when manual digitizing is applied, a proper accuracy assessment should be provided to determine whether the observed changes are significant.

Results

Apart from the fact that the results are wrong due to the wrong interpretation of the MSS scene and likely too much snow being mapped as glaciers in the CBERS scene, I have the following recommendations for improving the results section:

- Please use SI units (km^2) for glacier area rather than hectares.
- The area and area change values should be reported in a table and only the most important points highlighted or summarized in the text.
- Absolute area change values are difficult to compare (they could be listed in the table), please show and discuss relative area changes.
- The too small overlays of raster grids in Fig. 7 could be replaced with one large grid overlay using different colours for all points in time (after proper correction) and a close-up showing vector outlines in an interesting region.
- The interpretation/speculation about the causes of the observed changes (e.g. P515, L24) should be provided in the discussion section (which is currently missing).
- All glacier complexes should be split into entities using drainage divides derived from a DEM and changes should be assessed for each individual glacier.
- Relative area changes per glacier vs size could be shown in a scatter plot and obviously surging glaciers should be marked.
- It might be interesting to analyze if non-surging glaciers have advanced as well.
- When calculating area changes for the entire region, the surging glaciers should be removed from the sample as their area change might have different causes.
- As seasonal snow is an issue, it should also be demonstrated (overlay of outlines) that seasonal snow has no impact on mapping quality (please provide an accuracy estimate).
- It should be checked if presenting changes for glacier complexes makes sense (as in Table 2). If yes, more complexes should be listed and relative area changes should be added (in the Table). The text should focus on interesting issues rather than repeating the table contents.
- A more detailed analysis of the changes could be made, for example if there is a spatial pattern of the relative changes or in which elevation bin and/or aspect sector do they occur (see also previous studies on that topic

Discussion
The discussion section is missing.

Conclusions
When the satellite images are interpreted correctly, the conclusions would be completely different and the results of this study would likely be very similar to Ye et al. (2006). So I suggest that the authors either investigate another and larger region, or present a more comprehensive analysis of the changes (see above).

Please also explain why disappeared ice masses should return by 2011 (P517, L2)?
I would avoid the sudden link back to ice volume changes (P517, L11ff), as this is not the topic of the present study.

Tables and Figures

Table 1: Abbreviations in the table should be explained in the table caption (VIS, NIR, NE, CCD, MIR should be SWIR).
Table 2: If it makes sense, add more glacier complexes and provide also relative area changes

- Data for Figs. 3-6 should be shown in a table, add a scatterplot with relative area change values for individual glaciers
- Figs. 2 and 7 are much too small, please increase the size, maybe focus on one plot and show subsets of interesting comparisons (CBERS close-up, overlay of outlines, CBERS vs. TM, ...)
- The "glacier" specific changes in Figs. 8 and 9 do not carry much information (-> table). Consider to show mean values per size class instead (e.g. integrated in the scatterplot). There are many examples in the current literature.

References

Racoviteanu, A.E, Paul, F., Raup, B., Khalsa, S.J.S. and Armstrong, R. (2009): Challenges in glacier mapping from space: recommendations from the Global Land Ice Measurements from Space (GLIMS) initiative. Annals of Glaciology, 50 (53), 53-69.

Svoboda, F. and Paul, F. (2009): A new glacier inventory on southern Baffin Island, Canada, from ASTER data: I. Applied methods, challenges and solutions. Annals of Glaciology, 50 (53), 11-21.

Yde, J.C. and Paasche, Ø. (2010): Reconstructing climate changes: Not all glaciers suitable. EOS, 91(21), 189-190.