Characterization of glacier debris cover via in situ and optical remote sensing methods: a case study in the Khumbu Himalaya, Nepal, by K. A. Casey, A. Kääb and D.I. Benn

Author Response:

We kindly thank all reviewers for their time and energy in reviewing the manuscript. We are grateful for the suggestions, and have addressed the points raised in the following response.

1st Review, T. Bolch:

General remarks: The manuscript contains analyses and novel results of high interest. In addition, this paper introduces some methods (esp. mineralogical analysis) and data (esp. Hyperion, EO-1 ALI) which are uncommon but promising for the glaciological research. However, the manuscript is currently too long and misses the focus. The main message of the paper should be presented more concisely. The authors are sometimes very detailed (e.g. description of sensors) and refer on the other hand sometimes only to the cited literature while some more information would be of interest for the less informed readers. This especially the case when the authors refer to own work (e.g. for the flow velocity calculations or debris cover determination). The authors should stay in similar level of detail, do not present background knowledge and focus on the really important issues. Repetitions should be avoided. Some of the presented techniques are well known and confirm previous results (e.g. velocity estimates, surface temperature). The authors present many different interesting methods and techniques and in most cases one or two examples for application. However, in order to clearly show the suitability for a special purpose the accuracy of the results remain vague (e.g. with TIR emissivity for debris-cover mapping, spectral angle mapper classification etc.) and the usefulness of the analysis and techniques need to be addressed more in detail. Sound numbers and uncertainty estimates should be included in several cases. The authors mention which important results could be obtained when combining the different methods but do not include really convincing results and remain descriptive. The content would be in my mind much stronger and convincing when the authors really apply the promising combination of the different techniques to obtain new promising results instead of just naming the possibilities. In this respect additional analysis and data needs to be integrated. The authors may then think to split the manuscript into two papers, e.g. one containing the mineralogical part of this paper which fits to a more mineralogical or geological related journal and the more applied and cryospheric related topics should remain then in The Cyrosphere. Or the content may be splitted in a Part I and Part II paper in The Cryopshere if the editors agree. However, this is the author's decision. The included references are comprehensive regarding the remote sensing and cryospheric related topics and also for the mineralogical part as far as I can evaluate it as a non-expert in mineralogy.

The contents of the manuscript should ultimately be published but the content needs to be consolidated and presented more precisely.

Reply:

The authors are very grateful for Dr. Bolch's careful review and detailed recommendations. With some distance from the manuscript and full background, mineral, geochemical, and in situ spectral data published in the discussion paper, we have decided to condense the manuscript considerably. The condensed manuscript focuses on the key methods recommended for supraglacial dust and debris composition characterization by satellite remote sensing.

We are very thankful for Dr. Bolch's line by line comments, and these points were addressed in full (or removed from discussion in the revised, more focused manuscript) . Responses to Dr. Bolch's specific questions are included below.

Dr. Bolch's line by line suggestions:

Data and Methods Please include some information about the co-registration of the data and also a short statement of the accuracy of the terrain corrected data.

Reply:

The only terrain corrections applied were that in use toward velocity mapping. However, velocity mapping was removed as a main method addressed in the revised manuscript. For reflectance analysis, radiometric corrections are applied as discussed in the 'Optical Satellite Data Acquisition and Methods' section. Reference to higher level satellite data products are given.

P522 L20f: TIR emissivity to map silica abundance --How was the threshold of 60% obtained? By visual checking? P523L7f: I agree that the thematic map shown in fig. 11 provides a hint about the extend of debris-covered ice. However, if you are familiar with this area you can immediately see several misclassifications. Hence, the authors should make this statement with more caution.

Reply:

As described in the paper revision, the threshold of 60% was selected by the geology of the region. Different glacier study areas are recommended to be tuned to the desired silica weight percent typical of the geology in the analyzed region. In our excitement with seeing the initial results, we agree that the discussion paper was not as cautious as needed in discussing this technique with regard to mapping debris covered glacier extent. This has been addressed in the revised manuscript.

5.3.2 Shortwave and thermal false colour composites P518L23f: Be a bit more precise. How can glaciers with debris-cover detected with thermal data? What are the drawbacks? You have already mentioned

the debris-cover glacier mapping with thermal data in the Introduction. Hence, provide the required information there which also avoid duplication.

Reply:

The point with the shortwave and thermal false color composites is to show geologic differences in supraglacial debris. It was not intended to be presented in itself as an extent mapping tool. We have tried to make this more clear in the manuscript revision.

5.3.4 Land surface temperature The section confirms mainly previous measurements. The authors should provide some more information about the suitability of the thermal information for glacier mapping. Could a "cooling effect" due to the underlying ice or exposed ice cliffs be detected? You may also consider the relation of the temperature to the colour.

5.3.5 Glacier velocity, streamlines Also this section more or less repeats (and confirms) the result of several previous research as correctly stated in the manuscript. The presented results are a bit vague. More details are needed so that it would make sense to present one more data on surface velocity for this area. . L6: What threshold was used? What does "some remaining spurious" mean exactly? How was the possible uncertainty estimated? L7. I do not understand why the authors use the from 2005 and 2009 to "ensure that the glacier surface velocities did not change significantly between 2000–2002 and the time of our in situ sampling". What does it mean that no "significant trend were found". It would be interesting to know if there are no significant trends throughout the glacier or if a decrease in velocity as suggested by Quicey et al. (2009) is found for parts of the glacier.

Reply:

For the above two points, we agree that glacier surface temperature and velocity could both be analyzed and presented in more detail. Due to the limit of a single publication, the mineral mapping techniques were chosen to be the focus. Glacier surface temperature and velocity map information was kept and included in mapping analysis, though removed from primary methods and results. These topics could be detailed further in forthcoming publications.

2nd Review, M. Abrams:

This is a good attempt to survey a large body of research results describing the use of optical remote sensing for glaciological studies. I am mostly in agreement with the thorough comments by Dr. Bolch, so I won't repeat what has already been offered. The authors should respond to suggestions to make more uniform discussions in various sections. Perhaps less detail about, for instance, the ASD spectrometer could be provided. Scanner information might be better summarized in a table

Reply:

The authors are thankful for Dr. Abrams time and energy in reading the TCD manuscript. The ASD spectrometer section was condensed as suggested. As recommended, Dr. Bolch's recommendations were implemented.

3rd Review, Anonymous:

Regardless its high scientific level and despite a certain value of a very comprehensive description mentioning all nittygritty details, the paper seems to be a bit too overloaden. For a basically glaciological journal it contains a lot (too much?) of litho-spectral information.

Overlong papers tend to be either not or not well-read by the community and hence to be a loss to the scientific community despite all the valuable information conveyed.

Thus, the solutions I suggest are: either "slim down" the text wherever there are portions which allow to refer to other papers without repeating their contents; or even break it down into two papers, one of which might be published in a journal more oriented towards spectral aspects of remote sensing.

Honestly, many scientists interested in glaciers might not even possess the necessary background for all the spectral details given in this article. In this sense the above given comment has to be seen.

Reply:

We are thankful for these points. The condensed manuscript focuses less on minerology and more on glaciology. As mentioned previously, the suggestion to streamline the manuscript is appreciated and was followed.

4th Review, A. Racoviteanu:

General comments. This paper focuses on characterizing the debris cover on two glaciers in the Nepal Himalaya using field spectrometry. This aspect is an important contribution, since there are very few measurements of spectral reflectance on debris-covered glaciers in this region. Furthermore, quantifying role of debris cover on glaciers and mapping of debris-covered glaciers remain significant challenges. So, the motivation of this study is appropriate and timely. The analysis is very detailed and thorough, based on extensive fieldwork and remote sensing data analysis. Some of the methods are novel.

The authors' main goal seems to be to provide background material for satellite image analysis techniques.

However, the manuscript in its current form is very dense and hard to follow. The section on geology is quite technical and in my opinion too detailed for a glaciologic audience. I find it hard to extract the relevant information, especially related to the remote sensing techniques that were explored in this study.

Concern 1. Goal of the paper should be better defined. What remote sensing applications is the study targeting? Is the goal is to validate remote sensing methods for estimating melt under the debris cover, debris cover mapping, debris temperature, or all of these? It is unclear of the focus is on the field results, or on the remote sensing techniques.

Concern 2. Content: There is a wealth of good material, and thorough analysis here. Some of it, though, it too detailed – the technical details on mineralogy and sensors could be put in an appendix to make the paper more concise.

Concern 3. Organization of the paper: A major re-organization is needed. A clearer distinction of methods, results and discussion would greatly improve the manuscript. In particular, the results sections contain a large amount of background and methodology, which diverge from the main points being made.

Concern 4. Writing: While the use of the English language is appropriate, and mostly correct, in many cases the phrases are very long, with various ideas in the same phrase. It would help the reader a lot to revise such sentences and make them more concise.

Concern 5. Length: The paper is quite long. It would help removing some of the material in the sections as suggested in the specific comments (for example, the discussion on sensors). Some of the material is redundant.

Concern 6. Glaciologic application: This is the key part of the paper, but it is not emphasized in the results and discussion. In particular, debris cover temperature, thickness and velocity would need to be discussed in more detail in light of the findings. It is of great interest to the glaciologic community to use these spectral reflectance measurements to explain the different behavior of debris covered glaciers in the same area, for example, Khumbu vs Imja Glacier. While these are referred to in the text, this link is not being made.

Concern 7. Overall, the results presented here provide a good basis for discussion- this discussion needs to be taken a step further, and the results should be thinned to the key ones which would help this goal.

My recommendation is a major review of the paper with emphasis on tailoring the results presented to the goal of the paper, and taking into consideration the comments below.

Reply:

The authors are very grateful for Ms. Racoviteanu's comments and thorough editing. The goals of the paper -- to provide VNIR-TIR satellite methods for characterizing supraglacial debris geochemical composition were focused (Concern 1). Addressing the second concern, the content was minimized significantly as recommended. The mineralogy, trace element geochemical results, sensor history, and field spectra sections were reduced or removed entirely. Further, the entire manuscript was reorganized with careful attention was paid to rewriting in a more clear, concise, glaciologically relevant fashion (Concerns 3-6). Results and discussion were strengthened where possible, however, the scope of this initial manuscript is presentation of field collected spectral and geochemical data and a first-order comparison with satellite remote sensing data and methods (Concerns 6,7). A foundation is provided for using the methods toward further analysis.

Additionally, we are immensely thankful for Ms. Racoviteanu's thorough line by line comments. All suggestions were heeded to in the manuscript revision. A specific responses to one of her points is included below.

Ms. Racoviteanu's line by line specific comment:

Fig 8: This is misleading. You show Imja glacier, which is a fast retreating glacier with the pro-glacier lake, but the text refers mostly to the mineral composition. There is no mention that I see on how the mineral composition might inform the behavior of Imja glacier (ie. maybe helping to confirm the hypothesis that Imja has a thin debris cover, which enhances ablation). This deserves some further thoughts.

Reply:

We restated the reason for using this scene, namely, that Hyperion spatial coverage is not available for Khumbu or Ngozumpa glacier study regions. Hyperion spatial coverage of nearby Imja and Lhotse provides an analog for clearly spaceborne detectable large mineral class differences.