The Cryosphere Discussions

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Comment on tc-2020-372

Marin Kneib (Referee)

Referee comment on "Surface composition of debris-covered glaciers across the Himalaya using linear spectral unmixing of Landsat 8 OLI imagery" by Adina E. Racoviteanu et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-372-RC1, 2021

Summary

The manuscript 'Surface composition of debris-covered glaciers across the Himalaya using spectral unmixing and multi-sensor imagery' by Racoviteanu et al. presents a linear spectral unmixing approach to categorize the surface of debris-covered glaciers in different surface types, including light and dark debris, ponds, vegetation and ice. The spectral endmembers are derived from the Khumbu area and the method is validated for this domain. The method is then applied to the whole Himalaya. The main output is a map of surface characteristics for all debris-covered glaciers across the Himalaya, including vegetation and ponds. The study finally discusses some of the controls of occurrence of vegetation and ponds.

The focus of this work is interesting and relevant. The ponds and vegetation maps especially give interesting information on the state of the debris-covered glaciers (a more advanced state being characterized by larger ponds and extensive vegetation), and are the first steps to monitor the variability of ponds at the large scale. This is of interest to understand the glacier system but also to monitor GLOF hazard. These features are also known to contribute significantly to the mass balance of debris-covered glaciers.

However, I have a number of major methodological comments and issues regarding the soundness, validation and transferability of the methods that are still unclear and that would need to be properly addressed for the paper results and conclusions to be solid and credible. I also think that the analysis of the pond and vegetation controls needs to be improved and should better account for past work (especially for supraglacial ponds), especially with regards to the Himalayan climate gradient and the glacier-scale pond variability, which would greatly improve the value of this manuscript. For these reasons I recommend that this work undergo major revisions. Finally, I have included a number of minor comments meant mostly to improve the general readability of the manuscript.

General comments

Choice of endmembers and validation: my understanding from the manuscript is that the Pléiades (and RapidEye) images used here were used as a quality control of the surface type of some Landsat pixels. For this to work, you would need to make sure that the Landsat pixels are composed only of one surface type, or at least quantify the different surface types. This can be done fairly easily with some manual delineation from the Pléiades images, however one thing to be careful with here is the coregistration of the Pléiades and Landsat 8. Somewhere in the text is mentioned that the position of Landsat 8 is accurate within 50 m, which may translate to significant surface type changes, i.e. the surface characterized using the Pléiades image may not be the same as the Landsat one if the images are not correctly aligned. Proper alignment of Pléiades/RapidEye and Landsat (using cross correlation techniques for instance) should be ensured and demonstrated in the paper.

Focus regions: the method is applied to the entirety of the Himalaya, with a focus on three regions meant to represent the climate variability across the domain. The Lahaul Spiti is very far to the West and the Khumbu and Bhutan are relatively close, in the eastern part of the range. To really represent the climatic gradient as described in the manuscript, I strongly recommend adding one or two study regions in between Khumbu and Lahaul Spiti domains.

Generalization of the method to all of the Himalaya: the method was calibrated and validated for one specific location of the Himalaya and only for one Landsat 8 image. Further checks are needed to demonstrate the transferability of the method to the whole Himalayan range. I recommend the authors to validate the surface composition maps they obtain for at least 1 (better 2 or more) other site and Landsat 8 image. Given the free availability of Rapid Eye imagery using a Planet academic licence, this would be easily achievable.

Controls on supraglacial ponds: this is an interesting point and one of the, if not the, main outcomes of this manuscript. However, the analysis conducted here is very simplistic, especially considering the work from past studies, and further analysis is needed here to show the significance of such results. It is difficult to see anything in the related figure 11. I would suggest conducting a more detailed analysis of the controls, especially by partitioning glaciers in elevation bands since the ponds are very variable already at the glacier-scale (this is obvious comparing Khumbu upper and lower sections for instance). The 'slope' derivation is unclear - does it relate to longitudinal surface gradient? (Quincey et al., 2007; Miles et al., 2017; King et al., 2020). Consideration of surface depressions/topography (Benn et al., 2017; Miles et al., 2017; King et al., 2020; Salerno et al., 2016) and velocity (Miles et al., 2017) would also be welcome.

Use of the SAM method: The use of the SAM in this manuscript raises a few questions:

- Why use it over the whole Landsat image if the focus is on debris-covered glaciers?
- Why use it only for the Khumbu domain if the aim is to provide maps at the scale of the Himalaya?
- The main advantage of this approach stated here is that 'it is relatively insensitive and albedo effects'. This is because this method looks at the relative differences between the spectra, which is also the case with linear spectral unmixing when it respects the 'sum-to-unity' constraint. The advantage of the SAM over the LMM is therefore not clear.
- In part 3.1, L 357, you say that 'The SAM method is presented here only as an additional verification on the endmembers chosen'. This is in contradiction with the presentation of the SAM in the methods.

Based on these different points, I feel that the SAM does not bring much added value to this manuscript, but instead is an additional method that adds confusion to the results. I therefore suggest removing it entirely, unless it was indeed used to select endmembers, in which case, two sentences about this in the methods should be largely enough (the 'endmember selection' part is already very detailed).

Line-by-line comments

Title

Title: specify '*linear* spectral unmixing'

Title: replace 'multi-sensor' with 'Landsat 8'. Pléiades & RE imagery are only used for calibration & validation at a very specific location. Not used for the unmixing.

Abstract

L 13: The study area corresponds to the Himalaya, remove 'Hindu Kush'

L 14: 'covered *with*'

L 14: specify '*rock* debris'

L 17: Add accent on Pléiades - change throughout text

L 18: specify Landsat 8 - add throughout text whenever required

L 22: The surface composition maps are still 30 m. What do you mean by 'finer classification'?

L 24: i would not qualify the missing 19.7% as negligible

L 25: This might need to be retoned - see comments on the results regarding the seasonal variability of supraglacial ponds.

Introduction

L 34: remove 'Hindu Kush'

L 35: suggest adding reference here - e.g. Sherler et al., 2018 and Herreid and Pellicciotti, 2020

L 36: rock falls - plural

L 42: add references to energy-balance descriptions of ice cliffs. E.g. Sakai et al., 2002; Reid and Brock, 2014; Steiner et al., 2015; Buri et al., 2016a & b

L 44-45: these references don't really fit here. You could keep Reid and Brock, 2010 for melt under debris. Add other references to melt under debris and melt of ice cliffs & ponds. The link with Foster et al., 2012 and Miles et al., 2020 is not clear here.

L 51: Shugar et al., 2020a -> 2020. Change also in references. Check throughout text.

L 54: 'properties of the supraglacial debris' - not clear. Replace with 'number/area of ponds'

L 55: i.e. -> and

L 57: There are many more studies looking at the evolution of supraglacial ponds, including some already cited in this manuscript (Miles et al., 2017; Watson et al., 2016; Liu et al., 2015 …). Add more references here or specify the statement.

L 60: debris cover -> debris-covered

L 60: the references for transition of dcgs to rock glaciers is largely incomplete. Other relevant references that could be added: Jones et al., 2019; Knight et al., 2019 and related literature

L 62: references?

L 63: Shugar 2020b -> 2020. Check throughout text.

L 63: Wangchuk and Bolch, 2020: this is more a methods paper than a regional inventory.

L 63: add Chen et al., 2020

L 64: ‘not consistent’ - this is very vague. Specify.

L 64-66: this is repeated from the previous sentence. Remove

L 67: methodologies. Use plural.

L 67: is the methodology really the problem here? My feeling is that the main issue with mapping these relatively small features comes from the resolution of the sensor used to map them more than the method - see Watson et al., 2018 for comparison of sensors to map ponds with NDWI. Also in Kneib et al., 2020, we decided to use a NDWI instead of an LSU approach to map the supraglacial ponds.

L 68: remove ‘only’

L 69: Google Earth is a tool, not a set of data. Usually it’s Landsat images that are shown there.

L 69: Other relevant references for mapping multispectral images: Miles et al., 2017; Steiner et al., 2019; Kneib et al., 2020, Kraaijenbrink et al., 2016, Anderson et al., 2019, among others.

L 70: Other relevant references for mapping with topographic models: Herreid and Pellicciotti, 2018; Westoby et al., 2020

L 73: add Steiner et al., 2019 for ice cliffs in Langtang. For ponds, see Liu et al., 2015.

L 85: Object Based … add capital letters

L 78-89: Incomplete. In this paragraph you need to cite Scherler et al., 2018 and Herreid and Pellicciotti, 2020 - the current 2 global datasets for on-glacier debris-cover extents, and where they fit in terms of methods.

L 90-114: this part lacks organization and the reader is lost. L 108-110 should go in the previous paragraph. L111-114 does not tie well here and should go higher with the description of applications of spectral unmixing.

L 101: define spectral unmixing here in a few words.

L 105: We also used spectral unmixing to map ice cliffs in a recently published study (Kneib et al., 2020)

L 111: The Xie et al. references do not quantify supraglacial features (cliffs or ponds) but are focused on the debris-covered area. Wangchuk and Bolch, 2020 use Sentinel imagery, not Landsat. L 111-114 is unclear and could be removed or at least rearranged.

L 118: teste -> test

L 118: on -> over

L 119: you need to demonstrate the transferability of the approach. See major comment.

L 127-128: Transferability to open-source software is not addressed in the manuscript.

Methods and data sources

L 141: add Brun et al., 2018; Kneib et al., 2020

L 141-142: Are these changes in glacier area? Specify if so

L 146: There is more variability than 7-8 %. See numbers from and refer to Watson et al., 2017, who looks at the full Khumbu region, and Kneib et al., 2020 for Khumbu glacier.

L 149: i do not think that the decimals are needed here - unless they correspond specifically to the bounds of the Landsat images that were used?

L 151: add Maurer et al., 2019

L 153-158: The difference between the different regions is a bit unclear. I would insist on this idea of monsoon gradient (Bookhagen and Burbank, 2010).

L 163, 165 & 171: specify that the Pléiades and RE data were only used for the Khumbu region

L 165-166: 'so we … Landsat data'. Not necessary, remove.

L 170: need-> needed

L 170: the images are not entirely cloud-free.

L 171-173: It is actually very important that all the images are from the post-monsoon and i would recommend insisting on this, since even for different years you would expect similar surface conditions. This is especially true for ponds (Miles et al., 2017)

L 176: images per acquisition -> images (there is only on Pléiades acquisition). Similarly, remove 'fall acquisition' (L 178)

L 179: specify snow-free in the debris-covered part

L 180: reference for ERDAS?

L 182: image parts -> scenes

L 182: using -> with

L 183: 4, 3 (space missing)

L 183-184: Have you considered correcting the Pléiades image to surface reflectance? This would give you an idea of what the spectral values are there for in an image for which you can determine the composition well. I am also surprised by the use of RapidEye image with Pléiades images, as if they were equivalent (the RE image is almost not mentioned in everything that follows). The spatial resolution is indeed very different (also the spectral), and the RE image is corrected to surface reflectance while the Pléiades are not. If you consider the RE image to be enough, using Pléiades sounds like an 'overkill' since the RE images are freely available on Planet.com with an academic licence.

L 187: the RapidEye images were resampled? Why and to what resolution?

L 192: elevation data used here is also remote sensing data - the tiles of 2.2 & 2.3 need to be clearer

L 192: This part is confusing and mostly unnecessary. Just specify what data was used, for what reason (vertical accuracy and less voids) and what you extracted from it.

L 193: against what did you evaluate the 2 datasets? A check could be to test against the Pléiades DEM. Also, for this study the absolute elevation values are not so much of interest, as long as they represent the topography well.

L 193: specify time period covered by these datasets.

L 196: If the goal is to make a thorough comparison of the 2 datasets (which i do not think is needed), you will want to compare quantitatively the data void area

L 202: remove 'of interest'

L 205-208: not needed here.

L 219: Why such low elevation?

L 202-203: 'which provided…'. Remove. Already stated before.

L 229: 'debris *and/or* ice cliffs, ponds…'

L 234-234 & 236: Shouldn't it be 'linear **un**mixing models'? Also, in the manuscript you use LMM but also spectral unmixing in an equivalent way. I would recommend sticking with one term and using it consistently.

L 243: 'for variable illumination conditions': explain better or remove whole sentence.

L 243-245: why?

L 245: 'fully-constrained' - remove space

L 249-253: See major comments.

L 256-270: Was the endmember selection done within the debris-covered area or using the whole scene? I suspect the whole image. How did you distinguish clean ice from snow? These two surface types can be difficult to distinguish from one another.

L 257: ROIs -> actually just single pixels.

L 257: MNF is only used once and therefore does not require an acronym. Same thing for PPI (L 259) and SMAAC (L 261)

L 258: What are then the results of this MNF? And this SMAAC?

L 265: remove 'ice'

L 267-268: this does not make sense, why take clean ice and cloud endmembers then?

L 270: i understand that you are using the Pléiades image to check qualitatively the surface type of the Landsat pixels. This could be made a bit clearer in the text (the exact use of the Pléiades image). Furthermore, this raises a few questions (see major comment):

- Did you coregister the Pléiades/RapidEye with the Landsat image?
- For this to be valid, you need to make sure that either the pixel has only one surface type, or to quantify the surface types within the pixel

L 256-277: endmembers may vary from scene to scene, depending on the spectral characteristics (not likely for Landsat), the illumination, the geology (for the debris pixels). Assessment of their transferability is required to validate mapping across all the Himalaya (see major comments).

L 269-270: how many pixels does this make then? 6? You could consider showing them in one of the figures (Fig. 1 for example. Same thing for the validation pixels, but maybe this will be too much)

L 275: The data from Casey et al., 2012 would be more relevant (also for the Khumbu area)

L 275-277: show the spectra from Matta et al., 2017; Naegeli et al., 2015; 2017; Casey et al., 2012 in Fig. 3b.

L 280-281: spectral unmixing -> linear spectral unmixing (or LMM) to be used consistently

L 282: this evolves through the manuscript, sometimes 'SDC v.1 dataset', sometimes 'SDC', sometimes 'SDC dataset'... i suggest using just SDC consistently.

L 282: Landsat 8 and Sentinel-2

L 283: There is only one Scherler et al., 2018. Correct throughout text and also in the references.

L 283-284: not particularly relevant, remove.

L 287: What about expanding debris-covered areas (Kamp et al., 2011; Thakuri et al., 2014; Xie et al., 2020; Bhambri et al., 2011...)?

L 290-291: How was this automated check and repair performed?

L 291: What about the other areas?

L 300-301: how was this evaluation performed?

L 302-308: this is mostly repeated from before. Remove/reorganize.

L 310-311: what happens if a pixel satisfies two different thresholds?

L 315: ‘finer classification map’: the maps are still 30 m resolution and only one surface type is selected in the end?

L 321: remove space

L 331-332: it would help to show these ‘validation’ pixels on a map

L 333-335: reference?

L 337: How? Confusion matrix? Final pond area?

L 339: Watson et al**.** (2018).

Results

L 341: see comments above. I suggest removing this whole part.

L 343: in figure 4, how were the shadows mapped?

L 343: ‘well’: qualitative statement, difficult to interpret

L 352-353 & 356: if this is the case, this should appear in the methods

L 359: this part is very descriptive and qualitative. Shorten and merge with 3.3.

L 362-365: move to discussion.

L 366: RMSE

L 367-368: remove sentence (repeated)

L 370: Is ‘normalized fractional maps’ not the equivalent of ‘partially constrained’? In both cases the coefficients of the unconstrained model were normalized? This would then be in contradiction with what is stated above?

L 376-377: The names of the glaciers do not appear in the figures, which makes it very difficult for anyone who’s not familiar with that area to follow.

L 384: define Kappa coefficient. It is the only occurrence of this coefficient, is it really relevant?

L 387-388: give an estimate of the cloud-covered area

L 388-390: discussion.

L 384-400: this section is repetitive, especially because the accuracy values are the same than the producer’s accuracy. I suggest using the Dice coefficient (Dice, 1945), which takes producer and user accuracy into account in one metric (add it to Table 2). This would make this part easier to read.

L 405: debris-covered

L 415: correctly -> usually

L 421-422: this is mostly discussion.

L 428: rather than climatic patterns, i would think this to be related to the local meteorology.

L 436: Specify Fig. 8b for Labeilong Gl.

L 438-441: discussion.

L 449: it is really difficult to make out the ponds in figure 6. Should it not be figure 9 instead?

L 455-457: it would make sense to use a confusion matrix with a Dice coefficient instead. Part of this paragraph would fit better in the methods.

L 457-459: Not relevant, this is for snow and this paragraph is about water. Plus, this belongs to the discussion.

L 473: It would be interesting to compare these results with the results you would obtain with an NDWI-based approach, following the same calibration-validation scheme than for the spectral unmixing

Discussion

L 481: debris-covered

L 484-486: you mention this distinction between light and dark debris as coming from the geology. Could this not also be related to the debris water content? Especially if the debris is very thin (as for the thinly debris-covered ice cliffs), i suspect that this could play a role?

L 492: the analysis

L 492-493: remove 'chosen … image'

L 494: reference? Mention that in the post-monsoon the snow-cover is usually minimal (unless there are early snowfalls, which can happen)

L494-497: Note that there can be, especially in the post-monsoon, very bright cliffs. This is true after a light snowfall when the snow sticks longer to the ice than to the rock, but i have also seen a lot of cliffs with clean ice, especially in the post monsoon. Your figure 2b is a good example, you can also have a look at Kneib et al., 2020, figure 1 - in this paper we used 2 thresholds to map ice cliffs: one for the clean ice and one for the dirty ice. Some of this clean ice could also come from ice sails (Evatt et al., 2017) - there are some of these in the upper part of Khumbu, and they are common on glaciers in the western part of the range. The main limitation for these two features is obviously the size, and the mapping will be limited to the largest features.

L 499-500: Mapping ice cliffs with a 30 m resolution sensor is not realistic, and the results would not be representative. I suggest removing this sentence.

L 503-504: more than the method or the spectral resolution, the limitation will be the spatial resolution. References to studies focused on ice cliff delineation would be welcome.

L 505-517: Ponds are difficult because they are very variable from one season to the other and from one year to the next. The area covered by ponds should be minimal in the post-monsoon (e.g. Miles et al., 2017). This point should be highlighted in the discussion, with references to studies looking at pond variability (Liu et al., 2015; Miles et al., 2017; Watson et al., 2016). It would also be interesting to compare the numbers you get for other regions with other studies (Liu et al., 2015; Miles et al., 2017; Watson et al., 2016; Kneib et al., 2020) focusing on other glaciers.

L 510-511: explain this statement.

L 514: in on -> in

L 515: suggest: angles -> gradient. Also, how is this calculated? Specify in methods.

L 516: reference?

L 517: reference?

L 520-521: it is not clear why the bright non-glacierized area would not be mapped as light debris instead of vegetation.

L 530: simplify sentence

L 532: acronym already introduced in introduction. Since only used twice, acronym is probably not necessary

L 535-545: this should go in the methods & results.

L 536: debris-covered

L 536-537: what does the size of the glacier have to do with the turbidity of the ponds?

L 540: How did you derive the slope? Explain. Seeing that the pond coverage is so variable even at the scale of one glacier, and so is probably the slope, my suggestion would be to look at the results in terms of elevation bands (or other glacier partitioning - possibly based on slope?)

L 529: A lot of this paragraph should go in the methods + results.

L 554: Have you looked at the relationship (for ponds and vegetation) with debris stage? (Herreid and Pellicciotti, 2020).

L 563: errors in the SDC

L 565: it would be interesting to look at the changes of ponds and vegetation from east to west more in details

L 566: 'cannot be examined here in detail' -> 'is beyond the scope of this study'. I disagree, i think the analysis can be taken a bit further with the available data. It would actually add a lot of value to this manuscript.

L 575-581: this is not convincing. One problem is that the fraction of water will also be lower at the pond margins - and since Landsat 8 has a relatively low resolution, this will be the case for most pond pixels. You also do not present any results on this topic. As such, this paragraph can be removed.

L 576: 'fraction of a *pond* pixel covered…'

L 584: this is not the focus here since the delineation was applied only to ponds within the debris-covered area. Also, the main difference noted between the different datasets is the mapping of the supraglacial ponds, while it is noted that there are no major differences for lakes outside the glacier areas. Therefore i would not mention the lakes outside the glacier boundaries but focus on the mapping of the supraglacial ponds, which is still a relevant discussion point. Finally, one problem that arises when applying your approach to off-glacier lakes will be the endmembers you used, since the turbidity of the lakes, but also their depth, will be quite different from those of the supraglacial ponds.

L 591: this ‘outperformance’ is only true for supraglacial ponds (at least in figure 13)

L 599: note that the outlines shown from Chen et al., 2020 have obviously been manually delineated.

L 600-601: you need to mention the pond variability, which could explain some of the differences here.

L 609: in -> to

L 613: this needs to be proven

L 615-616: what corrections do you have in mind? So this is not a final product?

L 630-631: Where are these results? Are they of any use? If yes, they should be discussed in more details

L 631-632: in Kneib et al., 2020 we also used a light and dark debris endmember

L 639: were they removed or were they not?

L 642-646: It seems that the use of this Scherler et al., 2018 SDC triggered a lot of small issues and it occupies a large part of the methods, results and discussion. No inventory will ever be perfect but do you think that your results could have been improved using the Herreid and Pellicciotti, 2020 dataset, that claims to be ‘better’ than the SDC you used?

The main drawback of this dataset being that they used updated glacier outlines…

L 651: The Pléiades and RapidEye were only used for endmember selection and validation of the method.

L 653-655: This has not been proven

L 656-659: my understanding is that Shugar et al., 2020 used a mosaic of Landsat images to map the lakes, which means that only the persistent large lakes would be mapped anyways. So this problem is not related to the NDWI. The NDWI approach may not be perfect, but some studies have demonstrated that it works fairly well (Miles et al., 2017; Watson et al., 2018). I am not convinced by this point and would recommend a comparison of your results with a NDWI-based approach (following the same calibration scheme as for the spectral unmixing).

L 668: remove 'is of interest'

L 689-690: remove last sentence.

L 702-703: this is a key item and one of the main results of this study. It will be useful to have a link in the article.

L 975: remove, appears twice.

Tables

Table 2: use same number of decimals in the whole table

Table 6: explain in caption what 'manual' and 'automated' spectral unmixing refer to. All these outlines are from this study - no need to specify.

Figures

Figure 1: caption: give description of images used for Himalaya map.

Figure 2: acknowledge pictures' photographer(s).

Figure 3: caption: for panel b, add references. Add titles for the 2 panels. Panel a: plot x axis as wavelengths and use the same x axis for panel a & b. Why are the values so low for the debris in panel b compared to panel a?

Figure 5: only one legend needed. Same thing for scale & N arrow. Increase visibility of legend

Figure 6: I cannot see any letters in the maps. Only 1 legend, N arrow & scale (if same scale) needed. Consider adding a panel where you zoom in on a glacier tongue to see the ponds/vegetation/ clean ice better. No clouds in this image then?

Figure 7: the Pléiades has a very 'red' appearance - i suggest adjusting band composition to make it look more like what the human eye would see and doing it consistently in the other figures. Usually supraglacial lakes do not have a crescent shape.

Figure 8: I cannot see the black arrows mentioned in the caption.

Figure 9: the comparison of the lakes is not clear. It would help to have one of the datasets fully transparent, with just the outlines. The debris-cover outlines box is hard to see in the legend.

Figure 10: sq.km. -> km2

Figure 11: Hard to see anything. Change colors. Plot in different panels ponds and vegetation. Try log scale (for panel b at least that could be useful). sq.km. -> km2.

Figure 12: describe what the background images are. Only one scale and one N arrow needed. It is difficult to make out anything. Try zooming in a bit? The red-green combination in panel b is not ideal.

Figure 13: Consider increasing background transparency to make the outlines stand out. It's difficult to see the purple outlines. Also for the light blue ones. Consider increasing line width.

References

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