The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
https://doi.org/10.5194/tc-2020-372-RC2, 2021

Comment on tc-2020-372

Anonymous Referee #2

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-RC2, 2021

The manuscript "Surface composition of debris-covered glaciers across the Himalaya using spectral unmixing and multi-sensor imagery" by Racoviteanu et al. presents a very method-focused study that aims to distinguish different surface components of debris-covered glacier tongues in the Himalaya from readily available satellite imagery using a spectral unmixing approach. Although spectral unmixing is a well-developed technique, it has thus far not been extensively applied to debris-covered glaciers. The method is first implemented and evaluated for small part of the entire study area, i.e. the upper Khumbu region, using high resolution satellite imagery as reference. Defined spectral endmembers are subsequently used to apply spectral unmixing to the entire domain, which spans most of the Himalayan arc. Although there are various surface classes detected using the approach, from a geographic perspective the main focus of the paper lies on supraglacial ponds and (to a smaller extent) vegetation.

Given the increased attention in recent years to debris-covered glaciers and their cryospheric and hydrological importance, particularly in the High Mountain Asia region, the study presented is certainly of relevance and would be a valuable contribution to The Cryosphere. Although distributed modelling of debris-covered glaciers is still in its infancy, an improved understanding of the surface composition and its spatiotemporal dynamics will be crucial for accurate modelling of these glaciers at larger scale in the coming years.

Despite the clear merits of the work, the manuscript displays several major technical, structural, and interpretational issues in its present form that will require major revision before I would be able to recommend publication in The Cryosphere. I outline the most important ones below, and identify many other (also important) issues in the line-by-line comments.

In terms of structure, the manuscript does not always follow a logical flow. There are parts of the results that are more fitting for the methods, and complete new analyses and

several figures introduced in the discussion. I therefore suggest the authors to restructure quite substantially. I also feel there is often a mismatch in the distribution of details among different components, especially in the methods. Some parts are described overly detailed, while other (often important) parts of the methodology are dismissed with a single sentence. Please refer to my line-by-line comments below where I identify several of these issues. I would, however, primarily suggest the authors to carefully reread their manuscript with this in mind.

The methods are both developed and validated for a single and relatively small subset of the entire domain over which they are applied. This is of course not ideal, particularly since the full domain is roughly 2000 km wide and considerable differences are to be expected over this large area. This could, for instance, be differences in lithological and morphological composition of the debris due to differences in geology and climate, atmospheric differences that could affect image corrections, differences in overpass time (i.e. solar zenith angles) etc. It would be very strongly recommended to seek further validation of the upscaling performed in the paper using additional high-resolution imagery outside the Khumbu region. Preferably far away, e.g. in Spiti Lahaul. Since acquisition of RapidEye by Planet Labs, academic access to the images is free. Additionally, almost all high-res satellite data (i.e. SPOT, WorldView, GeoEye, Quickbird and Pléiades) is accessible to European/Canadian researchers directly from archive (or even for tasking by submitting a small project proposal).

As mentioned above, the paper is method-focused and as such presents only (very) limited process-related analysis. Particularly for publication in The Cryosphere, I think it is important to include a more advanced analysis, and provide a better and more elaborate discussion in this regard. This would improve the paper and more clearly indicate to the readers the potential of the method as a basis or input for subsequent cryospheric/hydrological analyses. Currently the main focus lies with supraglacial ponds, and in principle this is fine, but the current analysis using a simple linear regression of glacier-wide aggregates is very limited and certainly not state-of-the-art. I am also uncertain about the validity of using linear regression in this case, and if the authors were to continue using this method they should assess and clearly indicate the assumptions that are made about the data and its distribution when applying this technique. I suspect there is considerable non-linearity in the relations between pond/vegetation and glacier characteristics, and other machine learning techniques could therefore be better suited here, for example Random Forest Regression. Furthermore, past studies have shown different elevation bands to have very different concentrations and distributions of supraglacial ponds (e.g. Kraaijenbrink et al., 2016; Miles et al., 2017; Ragettli et al., 2016), and the analysis at the glacier scale cannot incorporate these important specifics. I would therefore strongly suggest the authors to, instead of looking at entire glaciers, perform a lumped or distributed analysis of some sorts. I also think there are several additional variables that are worth exploring. Topographic ones, such as aspect, but there is also data about individual glacier change that would be valuable to link to (Brun et al., 2017; Dehecq et al., 2019; Shean et al., 2020). Finally, it would also be interesting and relatively straightforward to employ a more quantitative approach to the climate arguments presented by the paper, for example by including climatologies derived from ERA5 reanalysis data to the supraglacial pond analysis. Implementing things would allow to quantify many of the now qualitative statements, which would greatly benefit the message and value of the paper.

To summarise. I believe the manuscript displays an interesting, largely unexplored approach that could provide a valuable contribution. However, (i) the structure of the manuscript requires some reworking, (ii) validation outside the Khumbu region is necessary, (iii) a more rigorous analysis is required with respect to the supraglacial ponds.

Line-by-line comments:

L13. The presented study does not encompass the Hindu-Kush, so I would suggest to remove it.

L13. “cover mantle” -> remove either cover or mantle. I would suggest mantle.

L18. Landsat -> Landsat 8 OLI

L20. “We develop”, this implies that you developed the spectral unmixing technique yourself. Rephrase.

L22, L26. Use “classifications” instead of “maps”

L22. “finer classification maps”, how fine?

L22-26. Also mention more clearly in the abstract that you focus on the debris-covered part (as classified by Dirk Scherler) only.

L24. What does negligible mean here exactly, and if it is negligible, why were all these classes included?

L35. Again, suggest removal of “mantle”

L36. Would be good to include (Evatt et al., 2015) here

L39-41. No reference for this? (e.g. Nicholson and Benn, 2006; Østrem, 1959)?

L45. Pro- glacial -> pro-glacial. Also, why supraglacial without hyphen and pro-glacial with hyphen? Please be consistent.

L47. Pro-and -> pro- and

L58. Intraregional and regional differences and variability in rates of glacier change have become reasonably clear over the last years (e.g. Brun et al., 2017; Dehecq et al., 2019; Shean et al., 2020)

L63-67. Include (Herreid and Pellicciotti, 2020; Scherler et al., 2018) here.

L68. "Object-oriented" à "object-based". Object-oriented image analysis (OOIA). Object-based image analysis (OBIA).

L73-74. Second part of sentence need to be rephrased.

L89. "Planet" is not a satellite, but a company. Pléiades is written with an accent aigu on the e. There is also SPOT, Worldview, GeoEye.

L92. (Kraaijenbrink et al., 2016) already showed big differences between UAV-derived ponds and RapidEye-derived ponds.

L92. "archive Landsat series" -> "the Landsat archive"

L93. still?

L94. The Landsat archive indeed spans five decades, but the 30 m data (TM, ETM, OLI) only four. Landsat 4 was launched in '82 if I recall correctly.

L94. I would not necessarily call this a drawback, as it can be advantageous for some applications

L95-96. “which…sensor”. This is not a discriminating factor between full-pixel vs sub-pixel techniques, as they both utilize the same data picked up by the sensor.

L96-100. I cannot follow the logic here. First the authors mention little emphasis on spatial variation of pixel values and pixel neighborhoods, i.e. suprapixel, but provide examples that focus on the pixel internals, i.e. subpixel. Rephrase and/or explain better.

L105. Exploited -> explored

L112. “allow” -> could allow

L124-127. This is a bit out of place here, and should be expanded and moved to discussion.

L126. If the goal is to transfer the method to open source software, why has the procedure been built in ENVI in the first place? Throughout the methods there are a lot of (proprietary) ENVI algorithms and tools involved, which counters this statement.

L134-137. David Shean’s work should be added here (Shean et al., 2020)

L139-140. Rates of change of what exactly? Area, volume, debris-cover? Clearly specify this

L139. Use SI throughout. % per year -> % a^{-1}

L151, L153. Quotes for A, B, C are not necessary

L160-L161. Reads as if Landsat is considerably worse than Sentinel-2, and clearly not the first choice. Remove or rephrase.

L165. Verb should be plural

L163-164. Although I understand this choice, it is rather tricky to assume that the debris surface is similar from year to year around the same time. This should be better acknowledged.

L171. Is “Pléiades 1A” the name of the satellite or the sensor?

L181. Mentioning “Planet satellite” is a bit odd here. Furthermore, RapidEye is a constellation of five satellites.

L182. What is the geodetic accuracy of this L3 ortho tile? These preprocessed products often have orthorectification issues in high relief terrain. How was this solved/accounted for?

L183-185. I would stick to pure data description here and not hint at the methods already using this sentence.

L186. Did you also consider the high-resolution HMA dem? Why, why not? (Shean, 2017)

L193-194. Here a whole analysis (which is introduced in the discussion) is dismissed with a single sentence. It should be properly outlined here in a separate section. Also see comments for the discussion.

L199. Remove “easily” and replace “high-mountains” with “study area”

L217-218. It is very tricky to assume that these parameters can simply be transferred to the other scenes that are thousands of km away and from different times of day, dates and/or years. It is, as the authors write in L208, a procedure that should be performed on an image basis. This should be better acknowledged here, and potential limitations should be clearly indicated. To my opinion, this also strongly endorses the importance of additional validation of the applied spectral unmixing results for areas outside of Khumbu (see main comments).

L221. Remove “basic”

L229-231. Italics are not necessary here

L240. which -> that

L252. I happen to know what a MNF transform does, but the large majority of readers of TC probably do not. It should be better explained and also discussed why this is necessary. It is also not clear to me whether it was used just to determine the dimensionality, or also to reduce noise by discarding MNF bands and/or to decorrelate the OLI bands (Meer and Jong, 2000). Proper references for this procedure are also necessary.

L253. "Pixel purity routine", "the n-D visualizer" are very much ENVI terms and will not ring a bell with the readers. Since the endmember selection procedure is crucial for the entire analysis it should really be explained in full detail. Why were these tools used, to what effect, and what are the pros and cons. Also, it should somewhere be stated which version of ENVI was used, and whether it was ENVI classic or not.

L259-L263. I do not understand the flow and logic between these sentences. Please restructure.

L264. Not really "areas" if it is only one pixel. Also, picking one pixel does not mean it is not a mixel. Picking one pixel "reduces the chance of a mixed spectral signature in the region of interest of each endmember".

L265. How do you account for spatial discrepancies between the OLI, Pléiades and RapidEye data? I have not read anything with respect to co-registration of the different scenes. In such a multi-sensor study co-registration is a crucial component of preprocessing, since otherwise it is not guaranteed that the images line up correctly. This would greatly impact the endmember selection and validation procedures and could undermine the entire study. Even after co-registration there will be errors that should be considered and acknowledged.

L265. "false colour composites". Also, the band numbers are used often for all sensors, but they are not defined anywhere. Please add the bands, their no. and their spectral characteristics, e.g. wavelength and bandwidth/FWHM, to the dedicated table (Table 1).

L271-272. From my experience, turbid water can (at least in VIS) still look quite different from pond to pond, depending on the type of suspended sediment. From blueish (glacial silt) to reddish. How is that accounted for?

L287. “area” -> “an area”

L310. What is meant by “finer classification map”? What resolution, how was this done, to what purpose, how does this affect the analysis? This is crucial information that should be explained in detail. Also I think “map” can be removed as just “finer classification” suffices. For me, map has the connotation of being a spatial display of something with the primary purpose being presentation.

L313. “from the Khumbu” -> “that were derived for the Khumbu domain”

L313. I am not sure whether this strictly falls under upscaling, since the spatial support (i.e. Landsat pixel scale) remains the same. What about applying/extending/extrapolating/inferring?

L313. Composition does not seem the right word here. Classification?

L316-317. I find this too much detail. When something can be reproduced similarly in a plethora of ways and different software packages, it is not about the tools for the job, but purely about the method and approach. Also, the Python module of ArcGIS is called ArcPy, not ArcPython. And strictly speaking it would be simply Python scripting using the ArcPy module to invoke ArcGIS functionality.

L323. How is this iterative procedure performed exactly? How do you select new endmembers. Using the n-dimensional visualizer, or the PPI, or something else?

L324. These are not a lot of ground truthing points, to be honest. It is also important to know how these points were determined. It is somewhat vaguely stated that these are “well-distributed on several tongues”, but it is not clear how the points were generated/identified. To obtain a fair classification accuracy measure it is crucial that the validation points are not manually digitized, but randomly selected within the entire domain of the Pléiades/RapidEye images. To get a (more) even number of points among classes that strongly vary in size, a stratified random sample should be taken. This section requires more clarity about the exact procedures used to perform the accuracy analysis.

L328. OBIA is mentioned before, but never properly referenced using for example (Blaschke et al., 2014). I also find the description of the OBIA procedure to be quite lacking. What settings were used exactly? How did the image segmentation work? Was there any postprocessing done on the objects, e.g. splitting/merging? How were the lakes classified, manually or automatically using a decision tree approach? What was the accuracy of the OBIA classification? Without this information it is impossible for the reader

to estimate the validity of the derived data for validation purposes.

L333. Remove “might have occurred”

L336-353. It is not completely clear to me why the SAM procedure was included, since the remainder of the manuscript focuses almost solely on LMM results. This paragraph mentions that the SAM results were used to test endmember choices, but it is not clear how this is done. (And this should be included in the methods, not the results section). I would suggest to expand this section and clearly describe to what purpose it was implemented, or remove the SAM entirely from the manuscript if that does not compromise other parts of the study.

L351. Is that is -> is that it

L362. Abbreviation for root mean square error should be RMSE, not RMS.

L361-363. Why did it have lower average RMSE? Was this due to a specific class mainly, or overall. How was the class-by-class performance difference? Maybe the average worse performer, performed better in more ‘important’ classes? Please elaborate.

L363-365. Two times roughly same sentence here.

L372-376. I find most of this to be more fitting for the methods section. Also, how were these seemingly arbitrary threshold values determined?

L372-376. Since multiple classes can be attributed to the same pixel using this multi-step thresholding of the fractional results, the order in which these threshold classifications are combined into a final product matter. That is, what will be the final class of a pixel when it falls within the thresholds for multiple fractional layers? It is not clear to me how this is done exactly.

L380. A remote sensing classification accuracy of 75% is frankly quite low (see e.g. Foody, 2008; Foody and Atkinson, 2002). For me, it really gives rise to the thought how other classification procedures might fare on the same data. Would a simple minimum distance

supervised classification perform better or worse? Is it really beneficial to use this technique? Since the accuracies are low, particularly for the debris classes, I expect a very thorough and complete discussion about the limitations and capabilities of the method in comparison with possible other classification approaches.

L385-386. I do understand the argument that there is a link between the occurrence of a class and the classification accuracy.

L387. If it is heavily dusted, then it is not clear ice, right? That is, exposed ice != clean ice

L400-404. It might be good to add uncertainty ranges to these percentages, given the moderate classification accuracies.

L415-416. “5.6% of the debris area”. This is quite arbitrary because this number completely depends on the quality of the SDC dataset.

L420-421. If clouds are not present in the validation region, how were you able to assess the accuracy and confidently extend that to the other landsat scenes?

L424-426. There are several of these climatic ‘speculations’ in the manuscript, which could be substantiated by including some climate data (see also main comments)

L425. Of course it has to do with climate to some degree, but satellite images are snapshots and there is just a degree of luck involved regarding cloud cover. I would suggest not to over-analyze this.

L444. “latter two” is a bit odd here, since after the two that is being referred to there are other things still mentioned. Suggest rephrase.

L449. “OBIA image segmentation”. I would use either OBIA or image segmentation. This depends on whether you manually assigned the object to the water class or performed an automated procedure (an OBIA), which is not clear from the methods.

L454-455. Not sure whether it is fair to compare a water classification to a snow classification.

L459. Overestimated is one word.

L460. I am a bit puzzled by the binary pond area. Wouldn't one of the benefits of having fractional subpixel information be that one could do analysis using those fractional values. First converting them to binary information seems to undo that. Is this then really better/different than a supervised classification or NDWI thresholding approach?

L461. OBIA analysis = object-based image analysis analysis

L467. "Good agreement" is subjective, needs to be quantified.

L478. Maybe add a line or two that helps to substantiate this presumption?

L488. I have seen snow patches on the debris in spring in the field and on satellite imagery, but not in the early post-monsoon period. I am not saying it is impossible, I only find it quite unlikely. Isn't it clear from the rest of the Landsat scenes whether there are snow patches or not?

L500-512. As mentioned before, it would be a great addition to the manuscript to include climate data to really quantify this climate dependency instead of providing only speculation.

L511-512. As mentioned before, it would be a great addition to the manuscript to include data of glacier mass balance (Brun et al., 2017; Shean, 2017) and velocity (Dehecq et al., 2019) to substantiate these hypotheses.

L507. "Less glacier shrinkage" over what time period?

L511. Reference?

L522. Reference?

L524-566. I find it very odd to only introduce this analysis here, in the discussion. Although it is not part of the remote sensing and unmixing methodology, the methods

used here should be added to a dedicated methods section and the results to a dedicated results section. I am not completely opposed to introducing figures in the discussion section, but introducing three new figures with results there is a bit odd. I would suggest to carefully reconsider the discussion and put any methods/results related parts in the correct sections.

L531. What constitutes a debris cover glacier tongue in this case? How does removing small tongues help to remove bare land patches. This part requires clarification. Also, 1 km2 is not big, but certainly not very small: 79566 of 95537 glaciers in Asia are smaller than 1 km2.

L532. So larger glacier tongues have more turbid supraglacial ponds?

L538. "For ex."? Why not just the broadly accepted "e.g."

L542. I am not very surprised that average glacier values do not show strong correlations since the supraglacial pond density is highly variable over a single glacier. It would probably be better to look at elevation bands, as other studies have also done (e.g. Ragettli et al., 2016).

L551. Quantify "in general"

L551. Seems more than 20% on the figure.

L556. Again, what is meant by "in general"

L559-561. I do not find this surprising: (i) looking at the scatter plots I highly doubt whether the assumptions that are made for linear regression are valid here, (ii) the signal is strongly subdued by looking at glacier-average values. Other machine learning approaches that can robustly deal with non-linearity might work better here, e.g. Random Forest.

L562-566. I am not sure whether Figure 12 and this small description add much to the analysis in its current state.

L524-566. Overall, I find this analysis quite lacking in rigour and novelty. With a few

adaptations I think a much more interesting and valuable analysis can be performed (see main comments).

L574-575. It should be acknowledged here that the lake turbidity is temporally highly variable and, also given the uncertainties of the classification method, the satellite snapshots might therefore be difficult to use for this purpose. Spatial accuracy of the Landsat OLI data will also be a concern, as from acquisition to acquisition the pixels will be slightly misaligned, resulting in potentially very different 'mixel' compositions and unmixing results. This effect will be particularly strong for the relatively small ponds that are almost always adjacent to the spectrally very different debris pixels. This argument of course not only applies in this case, but also for the applicability of the entire approach with respect to multitemporal analyses. These limitations should be clearly stated and discussed in the discussion section.

L586. "outperforming". I do not think that purely based on visual inspection of a 5 x 5 km subset of one of the major glacier tongues in the validation region of this study, which is a minute subset of the entire dataset, one can draw the conclusion that this method outperforms the other approaches. To make such claims there has to be some level of quantification and an assessment of much larger area.

L599. To my opinion, automated scalability to large regions is also an important limitation to consider.

L603-604. This gives the impression that it would be simple to transfer the unmixing parameters to the entire Landsat archive. This is not true because of differences that exist between sensors and bands, even though sometimes these are small: MSS != TM != ETM+ != OLI. For each sensor separate endmember selection will have to be performed and for older images this will not be trivial, given the lack of high-res calibration/validation data. I am not saying it is impossible, but these lines should be honest about the ease of transferability and the application of the method to historical imagery.

L608. As mentioned before, I would like to see this confidence validated for a region outside the Khumbu with additional high-res imagery.

L611. What is meant by "some post-classification corrections"? How will these be determined without validation?

L626. I have not read this before and couldn't find it. I was under the impression that only turbid water was considered as endmember. Again, be strict about separating methods, results and discussion and do not introduce new methods in the discussion.

L632-635. I cannot follow the logic here. Please rephrase.

L636. reference for the bad performance?

L650-652. Successfully applied but not validated on accuracy.

L654. Important to mention, though, is that commercial high-res imagery was required for proper endmember selection. Also, I think detail alone is not the sole criterion on which performance should be assessed. Usability, scalability, ease of use, speed of implementation are all factors to consider.

L657-659. I don't think this was confidently demonstrated in this study. Also what is meant by historical and more recent here? All images that were used are from ~2015.

L658 "imagers" -> "images"

L660-662. Yes, this seem to be true. But would just calculating a long-term NDVI composite and thresholding based on that not results in a much simpler approach that is as effective?

L665-666. Rephrase sentence, grammar incorrect.

L675. "other python-based routines". Remove "other", the ENVI approach was not a Python one. Capitalize "Python", it is a name. Why just Python-based routines, it can probably be achieved using various programming languages? I would suggest to change this to "routines using open source software"

L685. Complement -> to complement

L687-688. I find this an odd last sentence. Would fit better somewhere in the introduction.

L697. "in ArcPython" -> "using the Python module ArcPy from ESRI ArcGIS"

Fig. 3. It would be good to map use the actual wavelengths on the x-axis for panel A.

Fig. 5. Both my printout and zoomed-in PDF have too low resolution of the grids, and details are not visible. Font size on the legend is also very small. Would be better to convert it into a full page 3x2 format.

Fig. 6. Similar comment as for fig 5. The small details are not discernable due to resolution/size issues.

Fig. 7a. What are the white blobs on the eastern moraine?

Fig. 9. The legend mentions transparent Pléiades outlines, but these details are not visible without zooming in a few 100%. Illegible on my (not bad) printout.

Fig. 9, L1169-1170. This is something for the results section, not for a figure legend.

Fig. 10. This figure does not add much to the analysis, in my opinion, and could easily be combined with figure 11.

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