Responses to reviewers for Sasaki et al, submitted to *The Cryosphere*

We'd like to thank both reviewers for their very valuable inputs and thorough reviews. Our answers to each comment are indicated in blue below.

Reviewer 2:

The article, *Contrasting patterns of change in snowline altitude across five Himalayan catchments*, represents a tremendous effort, deriving snowline line altitudes from the Landsat and Sentinel-2 satellite missions for the period of 1999–2019. The findings are important and will likely set the stage for future High Mountain Asia studies. I found the article to be informative and well-written, but there were several areas throughout the text that I believe should be addressed before publication. I have provided thoughts, questions, and comments below.

Thank you to the reviewer for the positive and constructive comments and detailed suggestions, which have greatly helped to improve the manuscript's rigor and presentation. Our answers to each comment are indicated in blue below.

Major Comments

1. The Introduction seems to lack important details about previous findings of the SLA or snow covered area that should frame the findings of this study. For example, the discussion of MODIS in lines 31–40 focuses on the limitations of the technique and thus largely seems to dismiss the findings of previous studies. I agree that coarser resolutions make snow covered area and snowline altitudes more challenging to interpret, but there is much that can be learned from these studies. I suggest adding in a paragraph to inform readers of the current knowledge of HMA snow cover and changing the language around the MODIS studies to better reflect their importance. Consider adding in references to Hammond et al. (2018; https://doi.org/10.1002/joc.5674), Lund et al. (2020; https://doi.org/10.3389/feart.2019.00318), and other related studies.

   Thank you for this helpful perspective. We agree that MODIS (and SAR methods, as you note) has been an incredible tool for understanding regional snow changes and snowline altitudes, setting a fantastic foundation of knowledge about snow phenology and surface processes, and that this tool offers some incredible opportunities that compensate for some of the limitations of higher-resolution sensors. We agree that our introduction may have focused too much on the limitations of MODIS and previous studies, and we will reframe this in the revised manuscript to focus on the opportunity that we embrace to build upon this foundation. A very basic summary of our perspective:

MODIS has provided a suite of systematic regional (and global) snow and surface albedo observations for over 20 years, and its daily acquisitions maximize the likelihood of observations despite seasonal variations in cloud cover and illumination (Hall et al., 2010). Due to the sensor's observation strengths, the data have proven essential to identify trends in snow phenology, including snow cover duration and extent (most recently e.g. (Johnston et al., 2023; Notarnicola, 2022; Roessler & Dietz, 2023)). Within High Mountain Asia, daily MODIS snow products have been further analyzed to examine dynamics of glacier snowline altitudes (Tang et al., 2020), and snow-cover and snowline phenology (Tang et al., 2022). These data have further been essential for the constraint of snow reanalyses (Kraaijenbrink et al., 2021; Liu et al., 2021). Nevertheless, the standard MODIS snow products typically overestimate snow cover in HMA and usually need additional filtering or processing (Muhammad & Thapa, 2020). More refined spectral unmixing techniques can be very successful (Painter et al., 2009; Rittger et al., 2021). However, the spatial resolution of MODIS products limit their ability to identify fine topographic controls on snow phenology (Girona-Mata et al, 2019), necessitating analyses with finer spatial resolution to understand snow dynamics at the catchment scale.

Outcome: We will modify the introduction text to integrate some of these points and balance our discussion of the MODIS sensor and associated analyses.

2. The Abstract and Conclusion of the manuscript seemingly present an overtly positive perspective of the presented methodology and its potential for global applications. While I take no issue with the positive tone and rhetoric, I think a thorough discussion of the uncertainties is lacking, but is well-warranted. In particular, there are three areas that I see as problematic: (1) the inter-satellite disagreement, (2) cloud and shadow impacts on SLA accuracy, and perhaps most critically, (3) the impact of image availability upon the calculated time series trends.

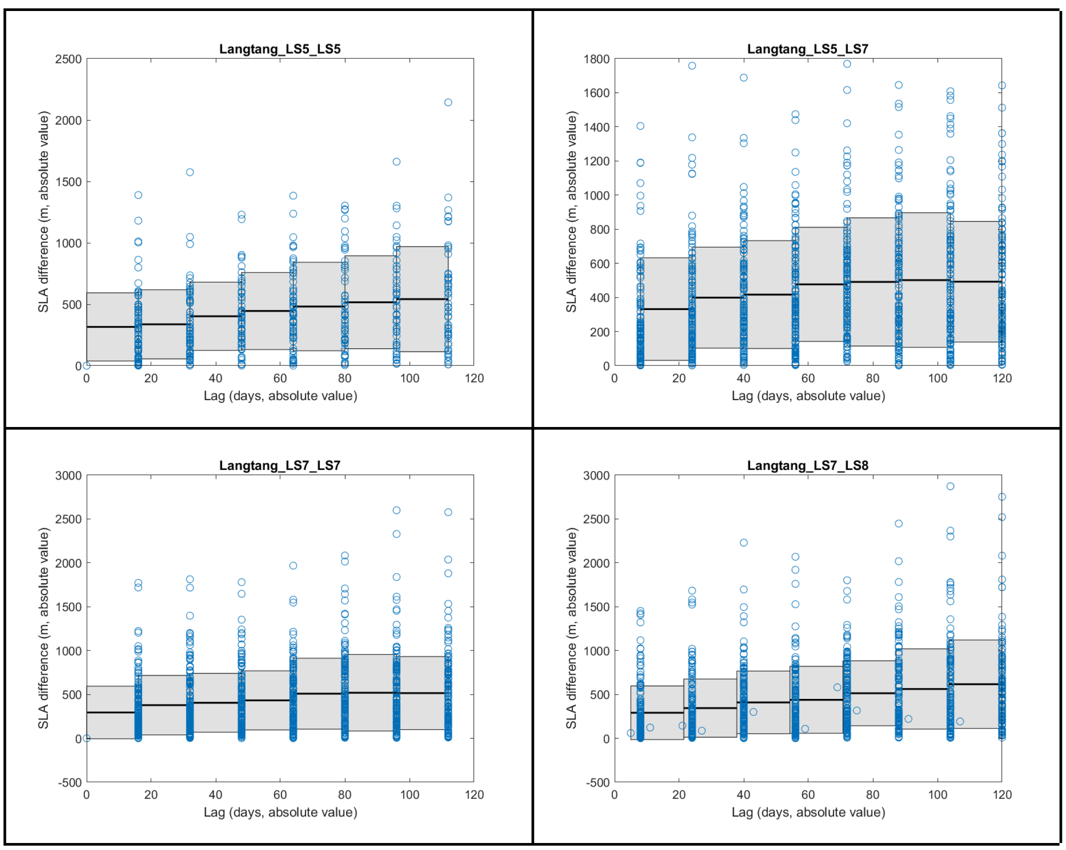
   We thank the reviewer for their constructively critical thoughts on this topic, and agree to better quantify and discuss the methodological uncertainties.

   1. Section 4.1 presents the inter-satellite comparison (Lines 178-187). I found this paragraph difficult to follow, particularly because there weren't any figures/tables to aid in the presentation of the numbers. I think one or two supplemental figures would aid the reader here. I suggest including several nearest temporal neighbor pairs for the Landsat-5 and Landsat-7 era (I believe the orbits are offset by ~8 days?) and several pairs for the Landsat-7/8 and Sentinel-2 era.

      Thank you to the reviewer for this suggestion. Reviewer 1 also expressed concerns about the comparability of the results from these different sensors, and we agree to show temporal correspondence between snowlines of varying lag times.

We have performed such a comparison between sensors for each site, as well as for each sensor against itself at each site. Our results show that 1) snowlines can be moderately variable, even over short timeframes and 2) this behaviour is irrespective of sensor. Specifically, at all sites and for all sensors, snowlines typically differ by 200-250m (median values) within 20 days, and this SLA difference increases with increasing time periods. However, we find similar correspondence between Landsat sensors (between sensors or comparing to themselves) as for comparisons with Sentinel-2. We do note that S-2 data agrees more closely with itself for shorter lag-times. However, as noted in the response to Reviewer 1, we have revised our evaluation of the detected snowlines and find that Sentinel-2 and Landsat-8 perform equally well in identifying the elevation of the snowline boundary.

Examples for Langtang are displayed in Figure R8. Note that y-axis scales differ between the panels. This suggests that the snowline data derived from distinct sensors exhibits similar precision. From this analysis itself, it is difficult to ascribe a number to this precision, and we prefer to evaluate this against the manual snowlines. However, it is encouraging that the distinct sensors all show a similar behaviour compared to themselves and one another. We will include a more thorough description of this analysis in the manuscript, along with the full set of figures in the supplementary material.



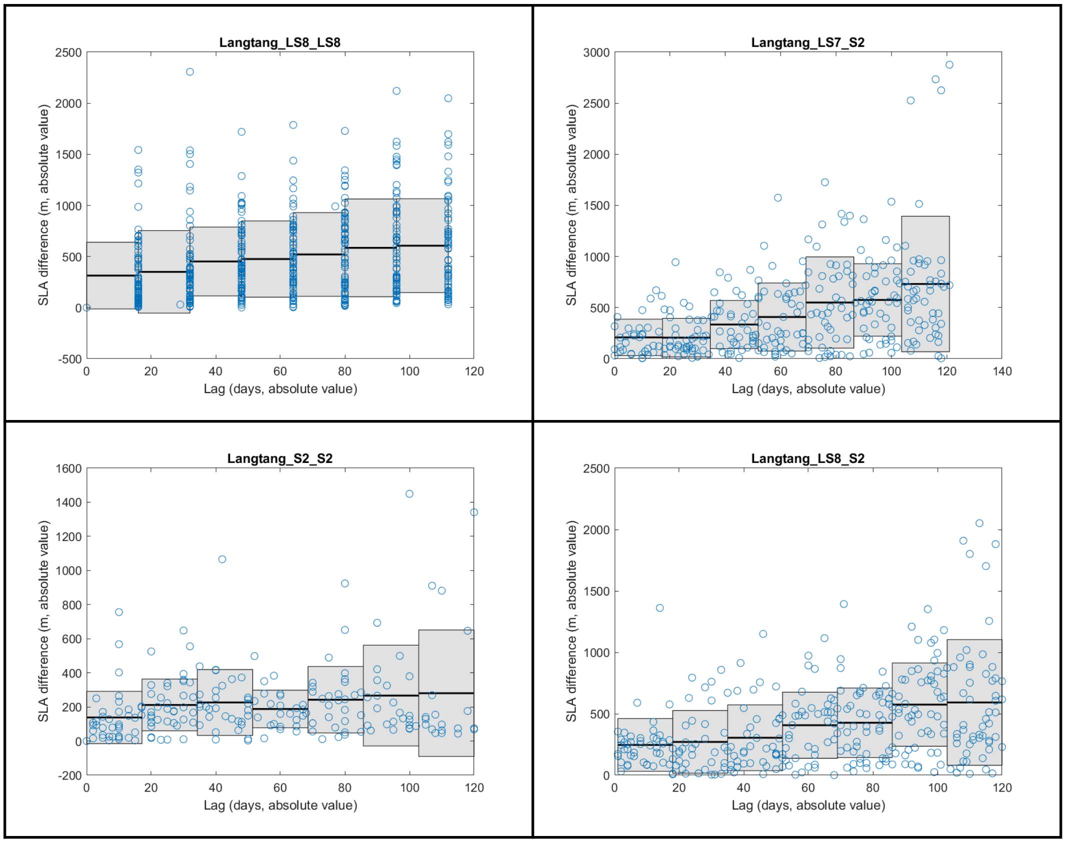


Figure R8. Boxplots of snow line altitude difference for scene-pairs with varying time lags, for different sensor combinations. For all sensors, snowlines typically vary by ~200m within 10-20 days. The variance increases, but not so dramatically, for longer lag times. Note that y-axis scales differ between panels.

Outcome: We will discuss the association of the agreement between sensors in the Discussion section.

2. The impact of shadows and clouds are discussed in Section 5.3, but an analysis of clouds/shadows is not presented in the results. Additionally, I felt that the methods were unclear regarding how clouds in images were handled. I suggest including a few figures in the supplement of cloudy and shadowy imagery and presenting a formal analysis in the results that shows readers how these image artifacts can affect SLAs.

   Thank you; this is an entirely reasonable suggestion. The masking of clouds and shadows was discussed in more detail in Girona-Mata et al (2019) but reviewer 1 also requested that some additional methodological details be included, as the Google Earth Engine workflow differs slightly, although the approach is the same.

Outcome: We will include a revised version of Figure S2 in the main text to depict our workflow. We will also provide some examples of the workflow and classified results in the supplementary information.

3. Critically, the number of available images per basin changed after the Sentinel-2 constellation was launched. The 1999-2009 interval only saw two satellites (Landsat-5/7) each with repeat orbits of 16-days, whereas the 2009-2019 interval saw five satellites (Landsat-5/7/8 and Sentinel-2A/2B). The trends presented in the results are important and will likely be highly cited in the future. But these trends should be presented in the context of the image availability limitations, particularly given how cloudy these regions can be. I suggest that the authors include a supplemental table or figure that shows how many images were used in this analysis across time for each of the regions. I also think a simple analysis on how the image availability may affect the calculated trends is warranted.

   We agree with the reviewer that this is a major challenge for the trend analysis, and that we could have been clearer about this in the original manuscript. In addition to the issues already mentioned by the reviewer, much of Landsat-7 ETM+ observation period was marred by a malfunction of the Scan-Line Corrector mechanism, further limiting the observable area with this sensor after 2003.

   We have produced figures to highlight the image availability, which will be included in the revised Supplementary Material (Figure R9-R13). All sites show reduced data availability for the earlier decade compared to the later decade, and reduced image availability in the summer (due to the monsoon), as has been found by diverse previous studies. Crucially, though, all sites experience a similar pattern of monthly data availability, which is important for constraining the harmonic regressions to a comparable degree, and for reducing any seasonal bias in the trend analysis.

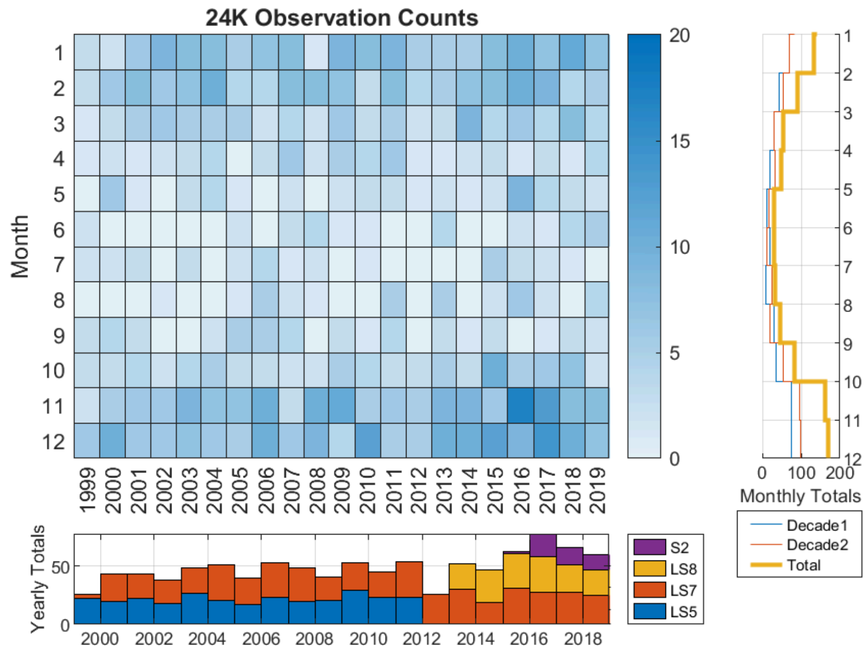


Figure R9. Depiction of monthly and annual snowline retrievals at 24K, including interannual breakdown for different sensors and seasonal breakdown between decades.

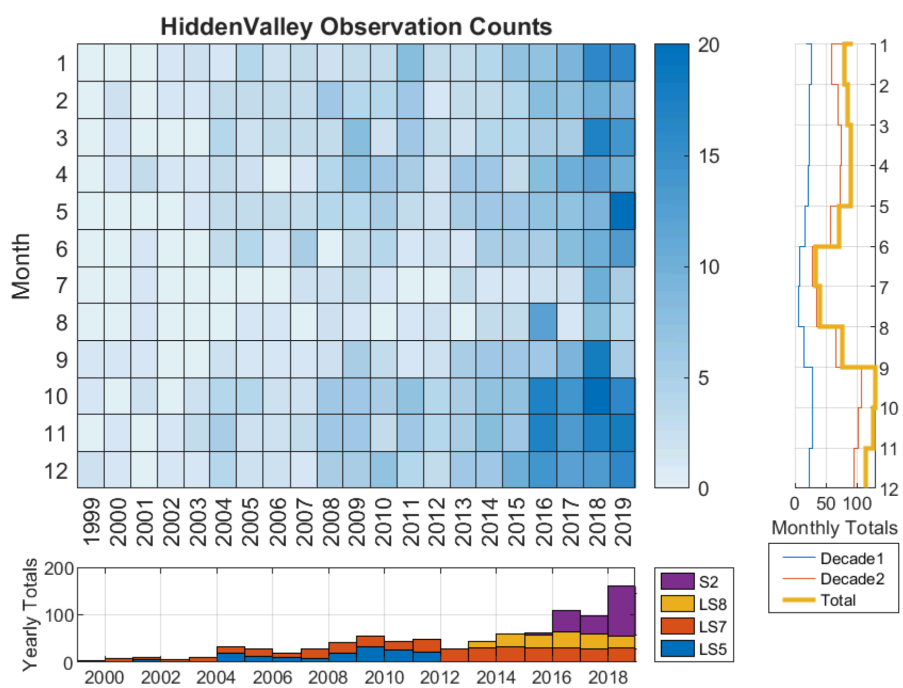


Figure R10. Depiction of monthly and annual snowline retrievals at Hidden Valley, including interannual breakdown for different sensors and seasonal breakdown between decades.

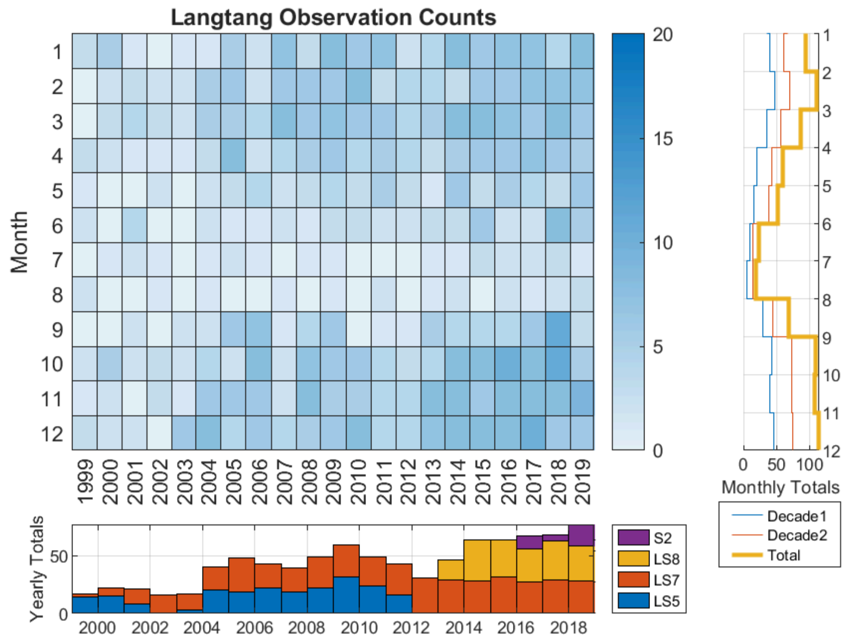


Figure R11. Depiction of monthly and annual snowline retrievals at Langtang, including interannual breakdown for different sensors and seasonal breakdown between decades.

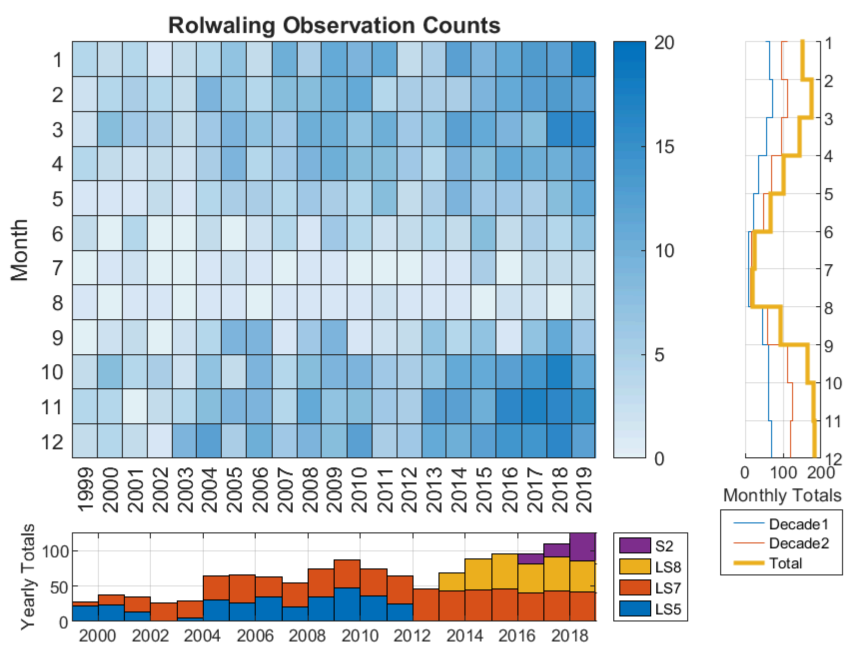


Figure R12. Depiction of monthly and annual snowline retrievals at Rolwaling, including interannual breakdown for different sensors and seasonal breakdown between decades.

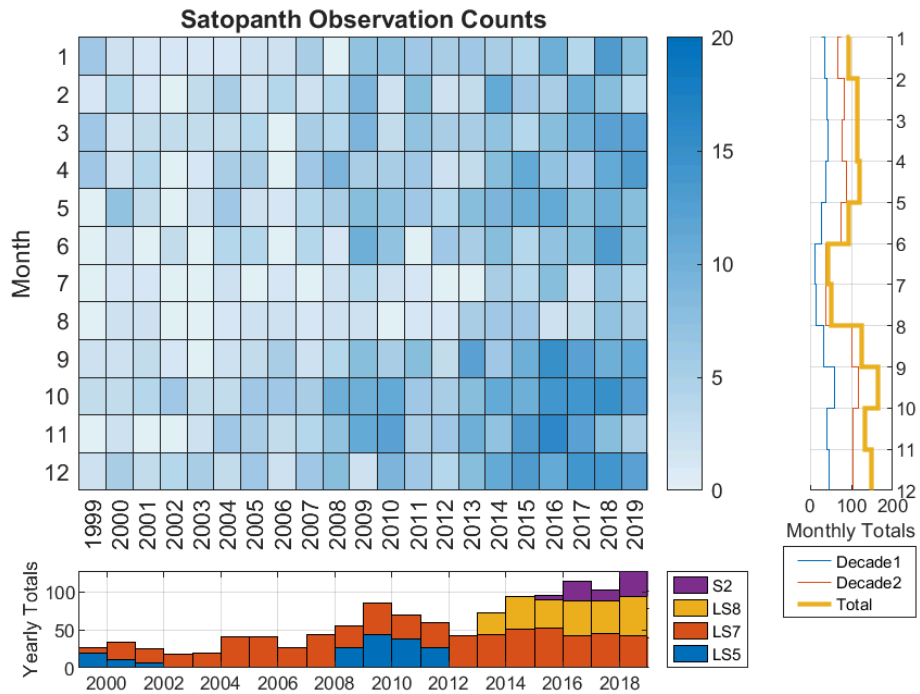


Figure R13. Depiction of monthly and annual snowline retrievals at Satopanth, including interannual breakdown for different sensors and seasonal breakdown between decades.

We also thank the reviewer for the question with regards to the impact of image availability on our trend derivation. To address this, we performed experiments using our trend retrieval code with synthetic datasets. We started with a harmonic snowline variation sampled to a weekly interval, then progressively added trends, noise, and seasonal and interannual biases in sampling to reproduce the patterns evident in our observations (Figures R14-18).

As you may recall from the text, our approach differs considerably from a basic trend approach. It aggregates observations to monthly data, determines the running mean of homogenized monthly data, then determines a seasonal pattern of the deviations from the running mean. Basically, this entails a seasonal decomposition of the observations, with the trend determined from the seasonally-corrected variations.

Overall, the code does an excellent job of retrieving the underlying trend in the original data. Importantly, it does much better than a regression through the raw (not undersampled) signal itself, which is very sensitive to the starting/ending data. The code generally retrieves the underlying trend to within 2m/a, but struggles if the random noise is too high (>200m noise between scenes). Some examples for different settings are attached.

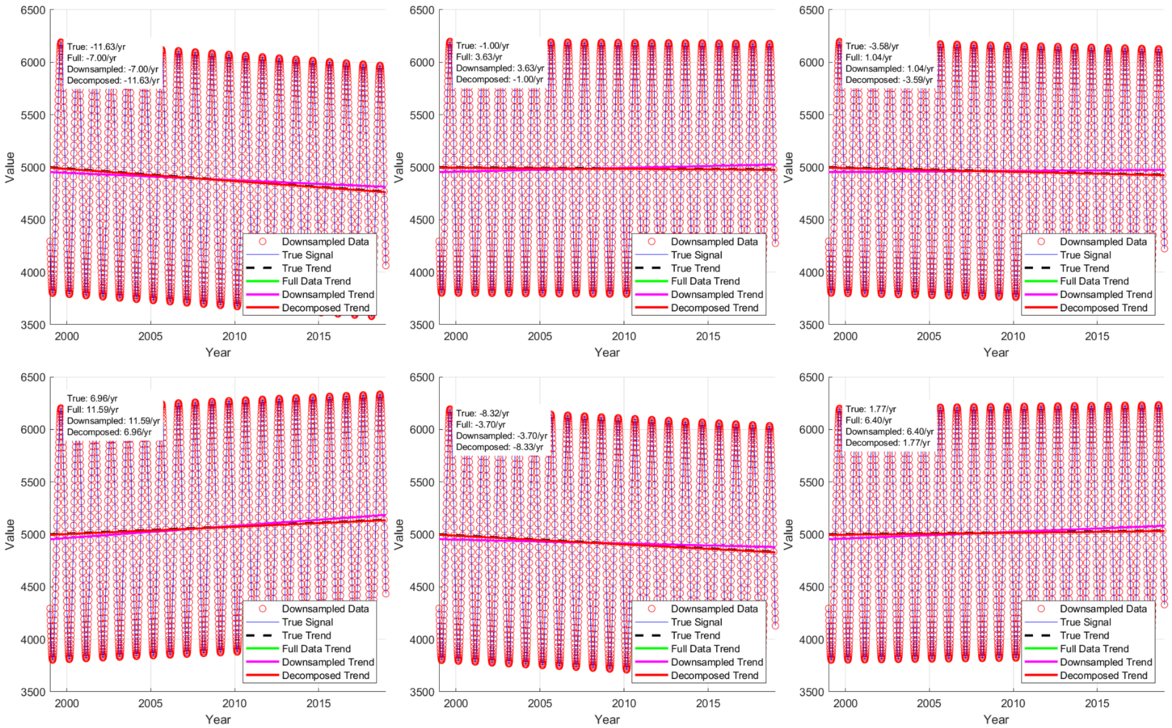


Fig R14. Trend retrieval experiment for 6 synthetic datasets. 'True Trend' is the trend built into the timeseries, 'Full Data Trend' is the regression of the full timeseries (positive bias!), 'Downsampled' is just a regression of the downsampled data, and 'Decomposed' is based on our approach.

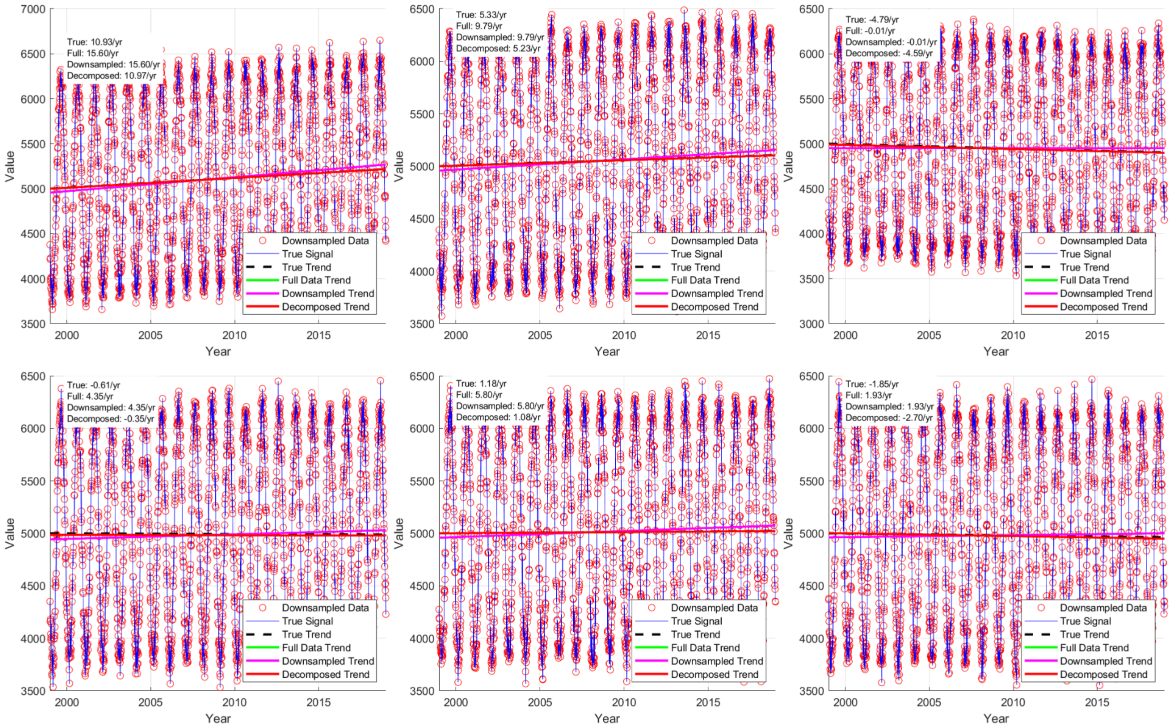


Fig R15. Trend retrieval experiment for 6 synthetic datasets including a 100m random variation. 'True Trend' is the trend built into the timeseries, 'Full Data Trend' is the regression of the full timeseries (positive bias!), 'Downsampled' is just a regression of the downsampled data, and 'Decomposed' is based on our approach.

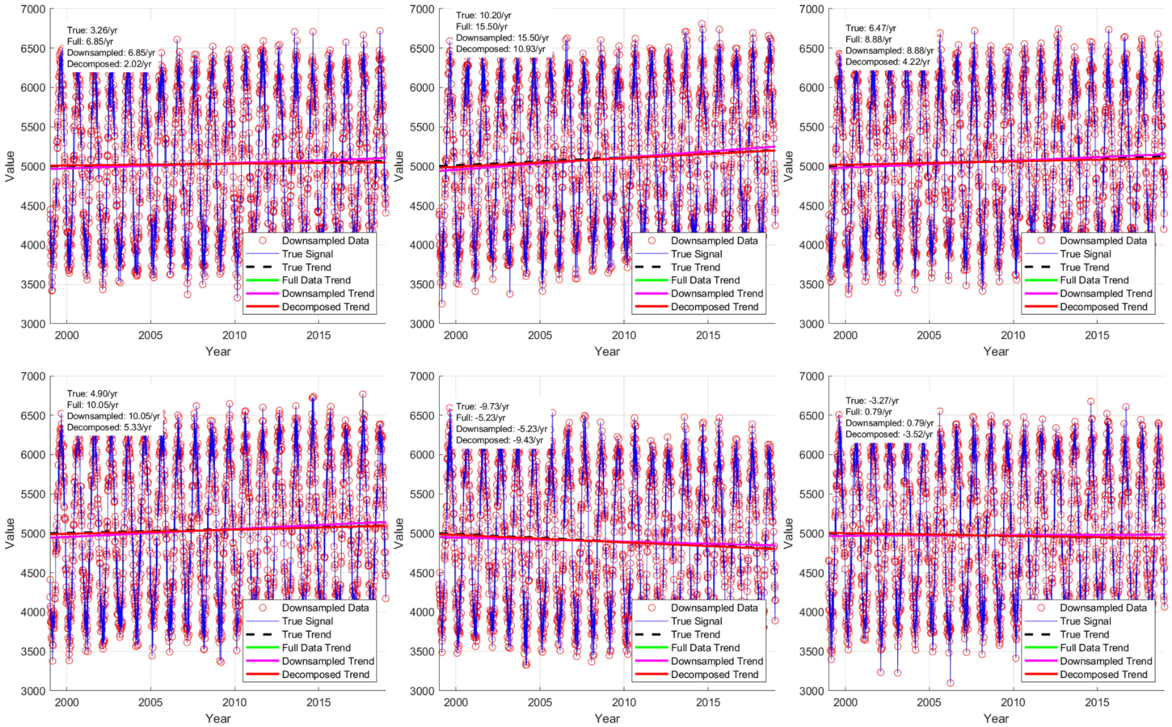


Fig R16. Trend retrieval experiment for 6 synthetic datasets including a 200m random variation. 'True Trend' is the trend built into the timeseries, 'Full Data Trend' is the regression of the full timeseries (positive bias!), 'Downsampled' is just a regression of the downsampled data, and 'Decomposed' is based on our approach.

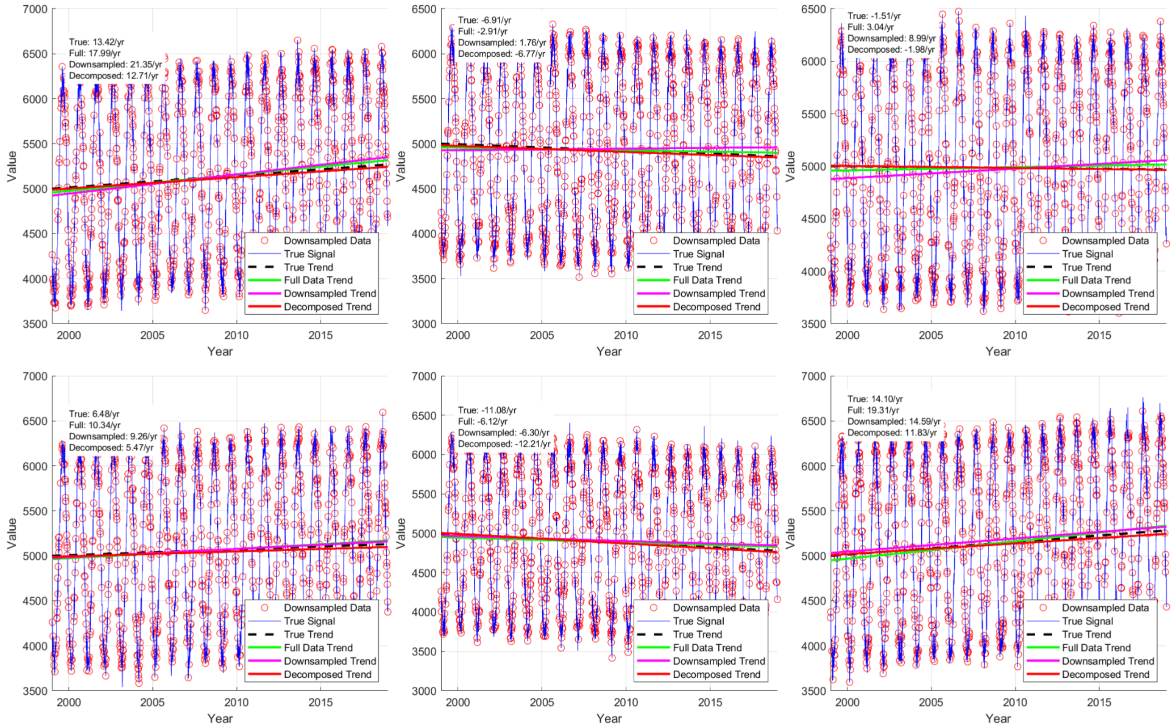


Fig R17. Trend retrieval experiment for 6 synthetic datasets including a 200m random variation and seasonal reductions in image availability (focused in the summer monsoon). 'True Trend' is the trend built into the timeseries, 'Full Data Trend' is the regression of the full timeseries (positive bias!), 'Downsampled' is just a regression of the downsampled data, and 'Decomposed' is based on our approach.

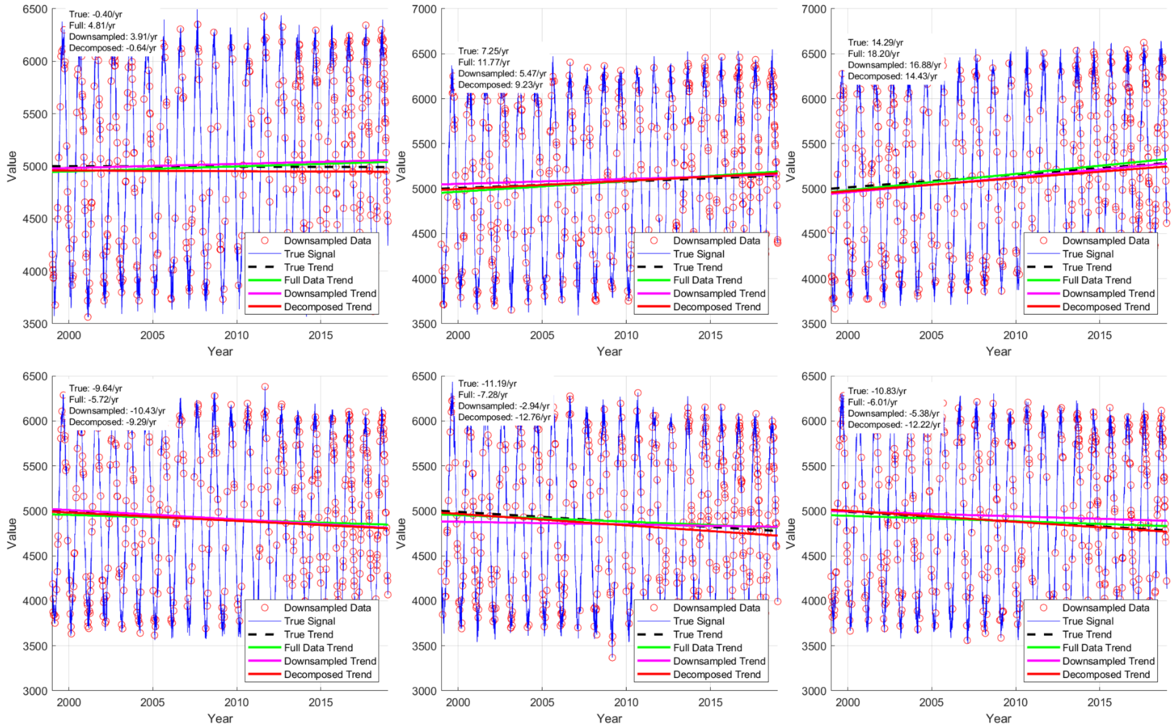


Fig R18. Trend retrieval experiment for 6 synthetic datasets including a 200m random variation, seasonal reductions in image availability (focused in the summer monsoon), and interanual variations in image availability (increased progressively at L8 and S2 launch years). 'True Trend' is the trend built into the timeseries, 'Full Data Trend' is the regression of the full timeseries (positive bias!), 'Downsampled' is just a regression of the downsampled data, and 'Decomposed' is based on our approach.

Outcome: We will include Discussion of the limitation of image availability in the main text, and will include depiction of the seasonal/annual available images for each site in the Supplementary Material. We will also introduce Discussion of the approach’s ability to retrieve trends from undersampled, noisy data in the main text, and the example synthetic tests in the Supplementary Material.

3. Throughout the manuscript, I found the language that described the relative location of the SLAs to be inconsistent and confusing. Terms describing the SLAs included increasing vs. decreasing, maximum vs. minimum, peak vs. low peak, etc. Inherently, SLAs are more complicated to discuss than snow covered area, but I think the manuscript could still be improved. I suggest that the authors stick to one or two pairs of descriptors and apply them consistently throughout the text.

Thank you for this helpful comment. In the revised manuscript, we will endeavour to simplify and homogenize our choice of descriptive language. We will, for example, eliminate ‘peak’ and ‘low-peak’ in favor of ‘maximum’ and ‘minimum’.

Minor Comments

Line 10 – As stated in the methods, the algorithm closely follows the Girona-Mata et al. (2019) algorithm. The primary difference appears to be a few parameters that were changed and the implementation within Google Earth Engine. To me, “propose an algorithm” implies originality. Consider using a different word or phrase.

Thank you. This is a fair criticism, and we will adjust this to ‘adapt’.

Lines 11-12 – The study reveals significant uncertainties with this method that are not fully discussed in terms of the global implementation of this method. I think the uncertainties are worth mentioning in the abstract.

Thank you, we will adapt the manuscript to explicitly identify the challenges of satellite sensor compatibility and spatiotemporal inconsistency of observations due to cloud cover, deep shadow, and satellite status.

Line 13 – Perhaps mention that the “meteorological factors” are defined by climate models.

Thank you, we will make this explicit in the revised manuscript.

Lines 17-19 – The phrasing here is a bit convoluted. Perhaps move the regions earlier in the phrases. For example, “We suggest that the increase in SLA in Nepal was caused by…”

Thank you, we will adapt the sentences as suggested.

Line 33 –The reference to Lievens et al. (2019) is perhaps a bit misleading. Lievens et al. (2019) used high resolution Sentinel-1 datasets to derive a coarser resolution snow depth product. Since the study did not emphasize snow covered area or snowline altitudes, I suggest removing. Additionally, the study was presented as more of a proof-of-concept and its accuracy is still highly debated in the literature.

Thank you, we will remove this reference. We agree that the study of Lievens et al (2019) is a very interesting concept, but is not important in this context.

Line 43 – For cloudy images, SLA may be less biased than snow covered area, but there can still be significant biases. I suggest revising.

Agreed. We will rephrase this to ‘can be less biased’.

Lines 46-48 – Is there anything that can be learned from these previous SLA studies that can better inform the interpretation of the results?

Thank you for this comment. We agree that we could have provided a clearer summary of these studies' results. Related to the reviewer's comment about the results from MODIS, we will expand the snowline derivation and results discussion, considering some of the aspects below. (McFadden et al., 2011) highlighted the suitability of Landsat-scale multispectral sensors to identify snowlines on glaciers, and to determine trends in snowlines (their study area was in the Cordilleras Blanca, so the results themselves are less relevant). (Mernild et al., 2013) used transient snowlines on glaciers determined from Landsat data to effectively constrain their seasonal mass balance modelling results for two Arctic glaciers. (Krajčí et al., 2014) proposed an effective method for statistically determining basin snowlines and demonstrated its suitability for MODIS data. (Tang et al., 2020) determined end-of-summer snowlines on glaciers from MODIS, and found increases in the central and eastern Himalaya, with insignificant trends elsewhere, but did not characterize off glacier transient snowlines. (Tang et al., 2022) used MODIS to examine snowcover duration and onset/cessation changes, and highlighted three distinct characteristic patterns of snowcover in High Mountain Asia, corresponding to distinct climatic domains (westerly, monsoon, and transitional). Broadly, the results indicated trends towards shorter periods of snowcover, excepting the western Himalaya, part of eastern Tibet, and part of the eastern Tien Shan. Girona-Mata et al (2019) identified a new method to determine catchment snowlines from Landsat data in the Langtang basin of the Himalayas and identified a clear dual-peak seasonal pattern. Despite the challenge of working with these data, the high-resolution snowline observations (1000s of values rather than an integrated catchment value) further enabled the authors to identify the aspect-dependence of seasonal snowlines (with deviations of several hundred meters) and to elucidate the seasonal meteorological factors controlling snowline changes. That study proved the value of going beyond MODIS data for the examination of snow phenology at small scales, but did not examine trends in snowlines and only examined one site in a very broad region. It is thus essential to develop and apply similar workflows at a broader set of sites, in order to provide refined insights into snowline dynamics, as well as for constraint of advanced models (e.g. Buri et al, 2023; 2024). We note that similar advances are being put forth for identifying snowlines on glaciers (Loibl et al., 2025).

Line 53 – Given that this paper was submitted in 2024, I think it is misleading to use "past 20 years" when the period of study was 1999–2019.

Thank you, we will adjust this.

Figure 1 – While I think the flow and presentation of the figure is excellent, I find it difficult to see the subplot labels a–e and the labels for the hypsometry in each of the subplots.

Line 71 – There is no mention of the hypsometry presented in the figure. Also, please state the source for the elevation data.

Line 73 – Please state the sources for precipitation and air temperature.

Thank you. Referring to the three comments above together: we have reworked this figure somewhat to make the captions and subpanels clearer, and to reference the source data in these locations. The revised figure and caption appear below:

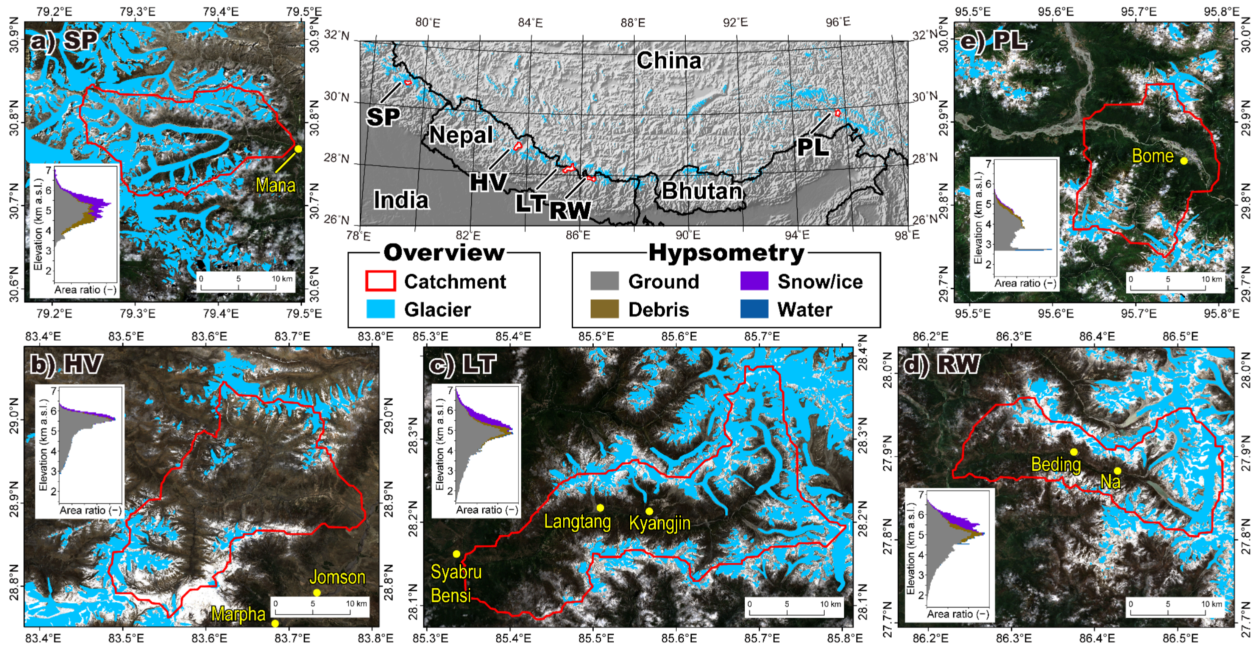


Figure 1 (revised).Figure 1: Upper center figure shows the location of target catchments (from west to east; Satopanth (SP), Hidden Valley (HV), Langtang (LT), Rolwaling (RW), Parlung (PL)). Enlarged views of target catchments are shown in surrounding figures; (a) Satopanth (SP), (b) Hidden Valley (HV), (c) Langtang (LT), (d) Rolwaling (RW), and (e) Parlung (PL). Catchment outlines and glaciers are indicated by red and light blue polygons, which are sourced from HydroSHEDS (Lehner and Grill, 2013) and GAMDAM (Sakai, 2019) databases, respectively. Yellow dots denote representative villages. The background images of (a)-(e) are composite images created using the Sentinel-2 images acquired between 2017 to 2020.

Line 78 – What is the significance of choosing watershed boundaries that have comparable size to Girona-Mata et al. (2019)? Perhaps a short clause or sentence is warranted to explain this significance.

This is a good question. In fact, this is an arbitrary decision, whereas the study of Girona-Mata et al (2019) was focused on a particular catchment, which therefore defined the size. For our purposes, it is more important that the catchment sizes be comparable to one another, but it was practical for us to use the previous study's approximate domain size.

Line 79 – Please provide an explanation for why TOA was used instead of the surface reflectance. Most publicly available snow covered or fractional snow covered area products use surface reflectance. I think the choice to use TOA is defensible, but it would be helpful for readers to understand the reasoning.

Thank you for your question. The use of surface reflectance products is essential for estimation of fractional snow-covered area, but robust processing of TOA data to surface reflectance requires good knowledge or estimation of atmospheric conditions. This can be problematic in the vicinity of extensive cloud coverage, in some cases introducing extra uncertainty. Furthermore, operational processing to surface reflectances was at an early stage when we completed the primary analyses for this work, so SR data archives were poorly populated. We see that these archives are now (nearly) complete, including the harmonised S-2/Landsat archive, with very little data dropout between processing levels, so an updated version of the tool could leverage these data with slightly better radiometric consistency.

For our purposes, though, the objective is simply a discrimination between snow and no-snow conditions at the pixel level (a binary classification), and we seek to make use of scenes with considerable cloud contamination. An NDSI threshold is imperfect, but an established, robust and accessible method for this in a variety of illumination and atmospheric conditions, and is nearly equally applicable to both TOA and SR values. There is an extensive precedent for NDSI as a practical method for identifying snow, although the precise threshold values are not always transferable between settings or even seasonally (e.g. Burns & Nolin, 2014; Dozier, 1989; Gascoin et al., 2019; Härer et al., 2018). As the evaluation of the snowlines shows (see response to reviewer 1), the method succeeds to resolve the snowline that an independent observer would identify with higher resolution data.

Outcome: We will more clearly justify the use of TOA data in the study, and indicate the future extension to SR data as an opportunity.

Lines 80-81 – Was resolution rescaling performed for either the Landsat or Sentinel-2 datasets to make the datasets more comparable? If so, please state how this was done. If not, please state why this choice was made.

Thank you for this comment. We did not perform any resolution rescaling, but derived snowline estimates independently for Sentinel-2 and each Landsat sensor. As is shown in the performance evaluation, these sensors have a comparable ability to resolve the snowline seen in higher-resolution data, despite the slight resolution difference. However, we preserve the resulting SLA measurements distinctly for each sensor.

Lines 82-83 – Given my major comment, I think a supplemental table or figure that shows how many images for each study site were obtained per year or season would be helpful for readers to better understand how image availability could bias the results.

We will be happy to include this information in a Figure in the supplementary material.

Line 84 – Perhaps state what the DEM is used for. From the methods, it seems that the DEM was used for SLA extraction and calculation of the aspect.

Good suggestion, you are correct and we will include this information in the revised text.

Line 109 – Were clouds masked or removed from the images?

Yes, clouds were masked, but this was not clearly stated in the manuscript. We used a combination of the methods: the cloud mask based on metadata provided by UGUS for Landsat sensors and by Copernics for Sentinel-2. We will indicate this in the revised Methods section.

Line 112 – Perhaps a citation for NDSI is warranted.

Yes, we are disappointed that we failed to include a reference to (Dozier, 1989) in the first manuscript, and will be sure to credit this manuscript in the revised text.

Line 113 – Was 0.45 used as the threshold for both Landsat and Sentinel-2 NDSI products? As mentioned in line 145, the Landsat satellites and Sentinel-2 satellites have different radiometric resolutions. How might this choice have influenced the results presented in Section 4.1?

Based on the revised evaluation of the SLA results at our specific study sites, we will completely overhaul section 4.1 in the revised manuscript. Our results show very clearly that for all test scenes, the automatically-derived snowlines closely corresponded in space and elevation to those derived from manual inspection of higher-resolution satellite images. Specifically, we determine NMAD values of 8.6m and 10.2m for the comparison against manually-digitzed snowlines. We do find slightly improved performance with Sentinel-2 data, showing NMAD values of 5.8m and 7.2m as compared to 11.0 m and 12.1 m for Landsat data, but this performance difference is nearly negligible for the detection of changes over time.

Nevertheless, the reviewer raises an excellent point, as the spectral responses of Sentinel-2 sensors differ (both in terms of wavelength bandwidth and actual sensor sensitivity) from Landsat sensors. Strictly-speaking, this is also true for the differences between Landsat sensors. There has been considerable work to make Landsat and Sentinel-2 data more comparable to one another have advanced considerably in recent years, and a harmonized Landsat-Sentinel product is now available (Feng et al., 2024). This would be a better product for future trend analyses, but did not include all relevant datasets over our study areas at the time of our analysis. This is relevant because the slight band-specific divergence between Landsat-8 and Sentinel-2 could lead to differentiation between NDSI values between sensors, and therefore a different ‘optimal’ threshold to discriminate between snow and non-snow. In fact, the differentiation of spectral responses varies regionally and by land cover, which further complicates the issue. Still, past studies have leveraged the two satellites as near-equivalent for operational snow-cover detection (e.g. Gascoin et al, 2019), showing similar snowcover detection performance, as in our results (above). We also note that the sensors agree very closely in terms of the seasonal cycle that they resolve (as shown by the harmonic regressions). The key difference, from our investigations, is that Sentinel-2 provides considerably more observations, allowing a better overall constraint of the snowline elevation seasonal cycle. However, these additional observations may induce bias for the later period, relative to the early period, so trend analyses should examine the effect of their inclusion.

Outcome: We will include additional discussion in the text regarding the harmonization of Landsat and Sentinel-2 datasets for future work, and additional figures in the manuscript examining the effect of Sentinel-2 data inclusion on our seasonal cycle and interannual trend results.

Line 116 – Perhaps a citation for improvements in Landsat-8 and Sentinel-2 is warranted.

Both Landsat-8 and Sentinel-2 show dramatic improvements in snow and ice mapping capabilities as compared to Landsat TM and ETM+ sensors, in terms of radiometric resolution, sensor stability, and acquisition schedule, but can use very similar snow/ice mapping algorithms (Paul et al., 2016). We will happily add this reference in the revised manuscript.

Line 120 – please describe a bit more about this multistage process. What are the key stages in the process?

This comment refers to the Canny Edge detection, which is a commonly-used filtering procedure for edge detection based on gradient thresholding. This involves first using a Gaussian filter to reduce noise in an image. In a second step, the gradient of the pixel values is computed. There is then an elimination of pixels whose gradient is lower than their neighbours (ie focusing on the locally-high values to isolate the edge position). Next, a double-thresholding based on the remaining data values is used to identify strong and weak (intermediate) edges. Finally, the whole edge detection algorithm eliminates weak edges that are not connected to strong edges, producing the result. This entire processing is adaptable, but typically automated within most applications. As Canny Edge Detection is a very established and accessible method in image processing and computer vision, we do not think it is necessary to detail all these steps, and instead simplify the description.

Line 166 – Why were scenes from Landsat 5 and 7 not evaluated? I would expect Landsat 7 to be particularly important to evaluate, given the scan line artifacts.

Thank you for raising this question. In the earlier study by Girona-Mata et al (2019), upon which our method was based, an evaluation of Landsat-7 results (both areas with and without SLC-error problems) was performed against high-resolution images. Those results showed very similar performance to that which we determine in this study, although the evaluation is performed only against Landsat 8 and Sentinel-2 data in this study.

Outcome: We will present this evaluation more clearly, as well as the reporting of the Girona-Mata results, which we match very closely.

Line 185 – Seasonal snow, by definition, has large variability by season. Thus, this statement does not seem particularly meaningful to me and seems to minimize the importance of the inter-satellite variability in snowline detection. Perhaps it is sufficient to state the inter-satellite variability. This is a common problem for most studies that use multiple satellite platforms.

Thank you for this comment. Our results show that the satellites actually have nearly the same ability to retrieve the snowlines, and in fact the seasonal variations retrieved by different sensors are nearly identical. The major change that we see by including multiple satellites is the variation in seasonal image availability, particularly after the launch of Sentinel-2, which retrieves images with the same seasonal pattern and biases, but has a much higher likelihood of successful acquisition for any given month than any of the Landsat sensors. We can add some of this discussion to the manuscript.

Lines 197-199 – The use of the word "peak" is a bit confusing. I understand that the "peak" refers to the period when snow covered area is at its greatest extent/SLA is at its lowest elevation, but this is not intuitive when looking at the plots on Figure 2, where peak conventionally refers to the time period when SLA is at its highest elevation. Please see major comments for more details.

Thank you for this comment. We will revise and homogenize our language to describe the snowline variability, in response to your main comment.

Figure 2 – I am struck by the variability of the derived snow line altitude within the months. For example, the range in January for Figure 2d is nearly 2000 m. I think that Hammond et al. (2018) may be worth reviewing and discussing – they suggest an average snowline of ~3000 m for these catchments. Their methods and definition of snowline also differs from yours, but may be worth a brief mention/discussion. In these study regions, Hammond et al. (2018) seems to suggest relatively low snow persistence, so I am wondering if much of the variability in these derived snowlines can be attributed to snow storms that deposit snow at lower elevations but then melt out shortly after.

Thank you for the comments. The study of (Hammond et al., 2018) is an excellent point of comparison. We believe that the reviewer may have misunderstood their continental average snowlines (Figure 3), however, which correspond to the lower limit of observed snow (given in their study by a snow persistence or frequency of coverage of 7%). In fact, in their Figure 2, it is clear that the approximate average snowline, indicated by a 50% snow persistence (Krajci et al, 2014) for these catchments (located between 28N and 31N), is approximately 4500-5500m. Indeed, that study suggests that snow is occasionally evident as low as 3000m, and more often (20-30% persistence) at 4000m elevation (values for 31N). We absolutely agree that the strong variability of snowlines in the winter months (although please note that this plot combines the full 20 years) is due to occasional storms that deposit relatively light snow to very low elevations, which then ablate to the seasonal freezing line. The cumulative likelihood of such a storm having occurred increases throughout the winter, such that the seasonal snowline eventually converges to approximately the freezing line before the monsoon. These processes were discussed in Girona-Mata et al, 2019 for the Langtang catchment, and we expect that similar overall seasonal dynamics play out in the five study catchments, as they are all somewhat influenced by the monsoon. We note, as well, than Hammond also points at increased snow persistence in the Indian Himalaya (Satopanth site). However, while the Hammond et al (2019) study provides a good general assessment of snowcover persistence and change (and at the global scale), our study provides much more precise detail for the study catchments by identifying the dominant seasonal patterns with high-precision data. We also highlight that MODIS snowcover seasonal patterns can be problematic due to the large pixel sizes (e.g. Figure S6) for studies of smaller domains.

Outcome: We have adapted some of this discussion into the main text.

Line 217 – Please see major comments. Could this trend be influenced by image availability?

Thank you, we have replied to this following your main comment.

Line 224 – I did not see these trends presented in any of the tables or Table S1.

Thank you for highlighting this surprising oversight, which we have now rectified by including the regression information directly into Figure 6.

Figure 4f – I presume the rose diagram intervals are by 25%?

These are 45 degree increments; we will indicate this in the figure caption.

Line 242 – Per major comments, it would be helpful to share some metric for readers to better understand how image availability may be at play here.

Thank you, we have replied to this following your main comment.

Line 249 – the use of “lowering” and “decrease” to describe SLA in the same sentence is a bit confusing. Please see major comments.

Thank you for this comment. We will revise and homogenize our language to describe the snowline variability, in response to your main comment.

Lines 249-262 – Much of this paragraph reads as discussion rather than results to me, mostly due to the connection to the snow/rain transition zone. In particular, the snow/rain transition zone was not defined in this study and its discussion here seems a bit unsupported to me. Further, the monsoon period is when the least number of images were available (figure 2), thus any firm claims about patterns during the monsoon season seem difficult to make without including a brief discussion of the uncertainties.

Thank you for your comment. We have relocated this discussion and make a more cautious discussion to interpret the results.

Line 274 – Since air temperature is not statistically significant, does this mean that the air temperatures tend to be below 0°C or are they poorly modeled?

Referring to the case of Satopanth glacier, as this is simply a statistical correlation. As can be seen in Figure 6, temperatures are below zero, but they are below zero at all sites; this is simply a temperature index from the reanalysis data, representing the variations of temperature over the study period, rather than the true temperature itself. Indeed, as the catchments span several 1000m elevation, one would expect positive and negative temperatures somewhere in each catchment at nearly all points in the study period.

The lack of correlation could be due to poor reanalysis modelling or because the 12-month temperature changes are not the major driver for snowline change. Without a detailed investigation using in-situ data, it is not technically possible to disentangle these aspects. We can make an interpretation based on the reanalysis data, but there is certainly the possibility that the reanalysis data are wrong. As Satopanth is susceptible to winter westerly precipitation (unlike the other study sites), and past studies have highlighted a stronger advection and deposition of snow as a cause of the Karakoram Anomaly (references in (Farinotti et al., 2020)), leading also to increased glacier albedo over this period (Ren et al., 2024), we expect that the changes in snowlines over this period are attributable to changes in solid precipitation, rather than temperature. The statistics support this interpretation, with the important caveat about reanalysis data.

Figure 6 – I suggest keeping consistent y-axes for each of the variables. For example, it is difficult to compare solar radiation across the different sites.

The purpose of the figure is not to compare between sites, but to visualize and substantiate the correlations of distinct variables with the snowline altitudes. Changing the y-axes to the same range obscures this. We have therefore retained the original axes (which have the same scale) but drawn readers' attention to this in the figure caption.

Lines 294-295 – Given the relatively low number of images obtained during the monsoon period, this statement seems difficult to support.

We agree with the reviewer that there are relatively fewer images available during the monsoon, and we will be careful not to overinterpret the dynamics during the period most affected by cloud cover. However, for all sites, all observations during this period are in the top percentiles of observed snowlines (contrasting with the large spread of values observed in all other seasons, nearly all of which are also below the monsoon values). Consequently we have very high confidence that the maximum SLA is consistently reached during the monsoon. For the interpretations, we will make it clearer that we rely on the interpretation of Girona-Mata et al (2019) as applying to all five sites during this period.

Line 302 – what does it mean for SLA to have a low peak – the rest of the sentence seems to indicate that this was when the elevation line was lowest? But this is a confusing way to describe it.

We appreciate that this was confusing nomenclature, and have adjusted the text to 'seasonal minimum'.

Line 304 – I'm not sure that the solar radiation from Figure 5a or Figure 6a supports this statement. Even Figure 4f seems to suggest approximately equal distribution between N and S aspects and NE and SE aspects.

This statement is not related to Figure 5a or 6a, both of which are indicative of the multiannual or decadal changes in meteorology and SLA, but does relate to Figure 4, where there is, indeed, very little aspect differentiation of SLA for Satopanth, as compared to all other sites. This is due to the heavier winter snowfalls, which deposit more snow than can be easily ablated by shortwave radiation. We appreciate that this was not clear from the sentence and have revised it accordingly.

Line 305-306 – Please provide a citation for the westerly winds and the snow storms. Many readers are likely not as familiar with this area as the authors and could benefit from the citations.

The studies of (Cannon et al., 2015; Maussion et al., 2014) are a very good introduction to these dynamics, and we will be happy to refer to them in the text.

Line 313 – "Therefore" makes it seem as though this is a continuation of the previous sentence discussing the Satopanth Catchment, but I am not sure that another paragraph is warranted.

Thank you, we will modify this in the revision.

Line 333 – What does declining SLA mean? Does it mean that the SLA is moving up or down in elevation?

Per your main comment, we have homogenized and simplified our terminology around SLA changes.

Lines 347-348 – I think this statement deserves a bit more analysis due to the cloudy nature of HMA. Please see major comments.

As is highlighted in the new evaluation section and results, this was the worst-performing scene out of 15 test scenes, and we think it exemplifies the worst case scenario. We are happy to add some discussion of this challenge to the text.

This particular comparison is made exceptionally difficult because of the cloud cover, and (unlike the other evaluation scenes) it is readily apparent that the snow-free rock outcrops identified automatically had, only 1 day prior, been covered by a thin veneer of snow. The distinct scene extents also pose a problem for this site, and are exacerbated by the variable cloud cover. Despite these difficulties, the automatic SLAs showed median absolute deviations of 82m and 16m relative to the SLA derived by operator 1 and 2, respectively. The relatively large apparent disagreement in catchment-wide values is thus due to different undersampling problems with both the Landsat and PlanetScope scene. The Landsat scene has much of the lower-bound of snowcover obscured by clouds, rendering the normally-negligible rock outcrops to significantly impact the statistical snowline altitude. The PlanetScope scene, by contrast, misses much less of the lower-bound snowline due to clouds (but is still somewhat affected) but does not cover the entire catchment, including some of the upper rock outcrops which the automatic method detects as a snowline from the Landsat image. We highlight that this is a worst-case scenario, as the scenes exhibit relatively few clouds, as controlled by our scene selection filter, yet the clouds occur directly over the snowline. We have some ideas to mitigate this in future analyses, but note that this is a severe minority of scenes.

Line 351 – But the study does not cover the globe, only a portion of HMA. Surely this would be an issue in almost every mountainous region. Thus, would each region need to have a unique elevation masking parameter?

To use this approach for the entire region or globally, yes, a solution would be needed for this particular challenge. One option would be to use a different definition of the statistical snowline, following e.g. Krajci et al (2014) as the lowest elevation with a 50% snowcover likelihood. There are difficulties with this approach, as well (e.g. a bimodal snowline elevation distribution) but this eliminates the challenge of high altitude rock outcrops. Other options to better homogenize data would be to leverage temporal stacking of snowcover maps (say, with a 2-week window, similar to Ren et al (2024)), or a data fusion approach.

Lines 354-355 – The usefulness of this technique is also dependent on image availability, which has changed markedly with time and is affected by cloud cover.

Indeed, and we will indicate this explicitly in the text, including a depiction of the number of images available per site over time.

Line 360 – The description of MODIS SLAs vs. derived SLAs should first be presented in the results. Please include a description of the MODIS SLA calculation and comparison to derived SLAs in the Methods.

We rather feel that this is simply a point of discussion and interest with regards to the strengths and weaknesses of our method as compared to that derived from coarser data products. However, we agree that a fuller description of the text here should be included. We will therefore revise this text.

Line 375 – This is only the second mention of the term, “arid”. Please describe for readers why arid regions are important in your study. i.e., Are the study areas considered arid?

We have rephrased this sentence to indicate that it would be worth extending these investigations to regions with different climate dynamics in terms of seasonality and quantity of precipitation.

Lines 388-392 – I’d like to see a few conclusions drawn on the limitations of the technique. As presented, the conclusions are overtly positive, but the authors identified several key uncertainties in the methods that should be considered before applying this algorithm to other regions.

Thank you for your comment. We agree that we could be more explicit about the limitations of this method, and will adapt the following text into the discussion:

There are three major limitations of importance, while there are some additional challenges that have relatively little effect on our results. A major consideration is image availability, which is improving due to the increased number of operational imagers, but could have a strong impact on both the characterization of seasonal snow dynamics, especially for earlier periods, as well as the robust detection of a trend. A second major limitation is the prevalence of cloud cover, which further limits the usable area of affected images, and can, in some cases, lead to biases in the detected snowline due to undersampling. This can be mitigated with more stringent cloud coverage criteria, but will further reduce image availability for severely cloud-affected regions. The combination of multiple datasets with differing footprints, compounded by variable cloud cover, can lead scenes with short temporal baselines to appear to differ in snowline due to spatial sampling biases (Figure S5). Our cloud masking was largely successful for the evaluation scenes, but is likely to fail in some situations, leading to false snowline detections. Furthermore, this study used top-of-atmosphere radiances from multiple sensors to determine snowcover based on a fixed NDSI threshold; the algorithm could, in the future, be applied to homogenized surface reflectances with a fractional snow cover algorithm (Rittger et al., 2021). However, our tests showed close correspondence with independent evaluation datasets, and the previous study of Girona-Mata et al (2019) showed that a similar method produced reliable snow cover at the Langtang site. Further adaptations to the method, to enable application more broadly, could include temporal stacking, data fusion, or a different statistical definition of the snowline, in order to further control for spatial sampling challenges.

Figure S3 – What do the vertical lines represent? Are those the basin-wide average? If so, please include in legend and discuss in caption.

These lines correspond to the median value (the 50th centile from the cdf). However, this figure will be removed from the manuscript as we have revised our evaluation of results.

Technical Corrections

Line 17 – Replace “while” with “whereas”.

Done, thank you.

Line 18 – Per the previous phrase in the sentence, “the decrease in SLA” is singular. Please replace “were” with “was”.

Correct, thank you.

Line 23 – delete “(as Heading 1)”.

Done, thank you.

Line 73 – I believe this should be Table 1, not “Table 2”.

Corrected, thank you.

Line 150 – “t-tests” instead of “T-tests”?

Corrected, thank you.

Line 200-201 – Having a colon and a semi-colon in the same sentence seems unnecessarily complex. Consider revising to two sentences.

Thank you, we will split this sentence into two.

Line 209 – “snow line” is used here. Previously, “snowline”. Please check for consistency.

We will ensure that this is consistent throughout the revised manuscript.

Line 228 – “aspect dependence” instead of “SLA dependence”?

Yes, the suggested phrase is what was intended, and this will be adopted in the revised manuscript.

Line 266 – Replace with “first half (1999-2009 in blue) and the second half (2010-2019 in red) of the study period.”?

Corrected, thank you.

Line 305 – SLA has already been defined.

Corrected, thank you.

Line 307 – "…with more snow cover on the west-facing…" As it currently reads, "snow" is arbitrary and could imply snow mass, which was not studied here.

Good point, we will indicate 'snow cover'.

Line 320 – Replace "nearby regions" with "study catchments"? Nearby regions is fairly arbitrary.

Thank you, we will take your suggestion.

Line 384 – "HMA", not "HAM"? Also, HMA is previously defined on line 24.

Corrected, thank you.

Lines 400-401 – This is a link to the Landsat portion of the GEE catalogue. Perhaps update to the more inclusive reference, or provide links for each of the HydroSHEDS data, Landsat data, and AW3D30.

Great point and suggestion. We now provide…

Figure S4 – "Monsoon" instead of "Mnsoon" for figure labels? Also why are no data plotted for Sentinel-2 in Satopanth and Langtang in pre-monsoon?
We will correct this plot, thank you.

References in revision statements:

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