Authors' Response to Reviews of

Sediment aggradation rates in Himalayan rivers revealed through InSAR's differential residual topographic phase

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RC: Reviewers' Comment, AR: Authors' Response, □ Manuscript Text

AR: Dear Referee,

Thanks so much for your interest in our work and your valuable comments. We really appreciate the time you took to share your suggestions. Your insights have greatly helped improve our manuscript, and we're truly appreciate the time you devoted to this manuscript.

Kind regards,

Huang and Sinclair

RC: I am not an expert in remote sensing or interferometry, so my focus will be on asking questions which I feel the text could benefit from addressing (either to help non-specialists such as myself to understand the paper, or to address the question directly).

The authors use interferometric techniques on repeat SAR observations to estimate sediment aggradation rates of gravel bedded rivers at the base of the Himalaya in the Ganges plains. They compare these rates with subsidence estimates from the local floodplains in order to show a very interesting, and novel methodology. They use these results to explore flooding and avulsion risk in this area, and discuss ways in which the method could be further validated.

Major Comments:

I enjoyed the paper, and the methods developed here are very creative and interesting. I found the quality of the science to be good. The presentation quality is the biggest issue for me, as the paper is confusingly structured, has some grammatical issues which make reading it unclear at times, and has calls to figures and equations in an order which distract from the core message of the paper.

1. The goal of the paper appears to be to establish a methodology for using interferometric analysis of repeat SAR data to estimate high resolution aggradation/subsidence rates in an alluvial environment. However, the results presented are not compared with any alternative estimates of topographic change in the study area, and so the validity of the results are unclear. A comparison of the estimated aggradation rates for other dissimilar fluvial environments like the lower Ganga River and the Floodplain of the upper Yamuna Valley are presented as being on the same order of magnitude as their results, but this does little to support the methodology. This technique might provide an amazing new tool for assessing high-resolution topographic changes associated with fluvial processes using publicly available data (awesome!), but a lack of ground truthing or valid comparison with other methods for the results really hurts the papers ultimate impact. If no further validation is possible, I think the paper needs to highlight the preliminary nature of the results so as

not to be cited as a source for known aggradation/degradation rates, and soften statements such as "We successfully mapped millimeter-scale elevation changes in (four) river channel(s) over a ~15km stretch…"

AR: Since this is the first observation detecting millimeter-scale riverbed elevation changes, there are no existing methods to compare with. One approach is to use the subsidence rate in the adjacent cropland, mapped using the conventional InSAR method based on the deformation phase. Whether the conventional InSAR deformation phase or the residual topographic phase from our new approach is used, as long as the input phase data comprises high quality, the results should indicate the same rate. For instance, in Figure 17, point C, which is adjacent to river 3, the data shows a subsidence rate of -14 mm/year based on conventional InSAR deformation phase. In Figure 14, river 3 near point C, the data indicate a subsidence rate of -12 mm/year, with an uncertainty range of -12% to +8%, corresponding to rates between -13.4 mm/year and -11 mm/year based on the differential residual topographic phase.

This novel approach works with the residual topographic phase, making the quality of the topographic phase a fundamental factor. The topographic phase is caused by variations in terrain; hence, flat terrain does not exhibit a topographic phase. For mountainous terrain, the applicability needs further testing to determine the limits of slope regularity and angle. In this initial study, the four rivers exhibit consistent slopes, with a slope gradient of 0.008 (vertical: horizontal). This low-angle slope results in a low phase gradient, which contributes to a good quality of residual topographic phase, with no challenge in its unwrapping as well. To ensure the quality of the residual topographic phase, the phase profile along the river slope is an important metric to assess. If an area is relatively flat and small, it may not provide a clear topographic phase trend profile for quality control, making this novel approach potentially inapplicable in such cases.

From Figure 4(b), the coherence at a pixel located at the mountain front of river 2 shows distinct seasonal patterns. The highest coherence occurs around January each year, indicating the driest period with minimal disturbance on the riverbeds' surface. Conversely, the lowest coherence occurs around June, when the bumpy river water surface causes reduced coherence. Although there are slight variations between years from 2017 to 2021, the overall seasonal sinusoidal pattern and amplitude remain consistent. This highlights an important point: the coherence time-series reflects the regular cycle of river inundation and drying, which may suggest consistent sediment aggradation rates from these natural cycles. However, if this cycle is disrupted by an extreme flood event, the annual sediment aggradation rates would likely be affected by the abrupt sedimentation from such an extreme event.

2. The paper is challenging to read due to repetitious information and confusing narrative structure, grammatical issues, calls to figures that don't exist (e.g. figure 18), and calling the figures and equations in a loosely structured order.

AR: Thanks, now the numbers are corrected and we have worked through the manuscript to check for grammar and the narrative. We believe condensing the methodological context at the beginning helps.

Figures: To be clear, I don't believe all figures and equations must only ever be called in order, but the paper would benefit greatly from trying to streamline the readers experience somewhat. The current structure results in a lot of searching around trying to match up the text with the

figures/equations. Additionally, many figures include imbedded text for the axes and legends that are so small they are either challenging to read when printed, or are just unreadable even when zooming in on a computer.

AR: Figures 8 and 9 have been updated to remove the small embedded text. Their captions have also been revised to include explanations of what the small embedded text represents within the figures.

Grammar and consistency: There are many examples of bad grammar and sloppy editing which make the paper challenging to read. For example: using the acronym "LOS" on line 241 and in Figure 9, but defining it on line 328, in Figure 12, then again on line 383, and 467. An example of a grammatical issue is: "Azimuth is along satellite fly track direction, range is across satellite fly track direction". The paper would greatly benefit from a copy edit. I have included many examples of copy editing issues in my line edits, but they are concentrated at the start of the paper, and are not comprehensive.

AR: Now 'LOS' is defined on line 244.

AR: the DEM is outdated, it will include residual topographic phase mixed with the line-of-sight (LOS) motion phase as input for SBAS inversion, leading to higher uncertainty in the LOS displacement results (Berardino et al., 2002; Morishita et al., 2020).

Narrative structure: The methodological context followed by the authors specific methods results in some repetition, and there are concepts in the methodological context which do not seem to impact the core thrust of the paper. It seems to me that it could be shortened and focused, although this may just be a product of my lack of experience in the field of satellite based remote sensing.

AR: We have now explained in lines 161–166 why certain concepts in the methodological context impact the core of the paper. We have also put some of this methodological context into the Supplementary Material 1.

AR: ripples. Since the amplitude is average in dry season of year 2019, the effect of soil moisture of the sandy riverbed might be less significant. Because the SBAS-InSAR method relies on distributed backscatter, which is particularly effective in areas with diffuse scattering with stronger VV polarization, it is important to explain upfront the backscattering type and polarization characteristics of the dry gravel riverbeds. Additionally, applying this novel approach to more complex rivers, beyond ephemeral rivers, requires classifying dry gravel pixels based on SAR amplitude polarization characteristics and statistical metrics. It is important to use SAR amplitude for classification instead of optical or multi-spectral images, as the same SAR images' phase component is used in this novel approach to map sediment aggradation rates.

3. My understanding after reading the paper is that you do interferograms during the dry season, and across the monsoon season, but are unable to resolve interferograms during the monsoon season. However, I'm still not entirely sure how the authors deal with phase ambiguity if changes in elevation that occur across the monsoon season are quite large. Is my understanding correct? Is there data loss in the interannual interferograms associated with phase ambiguity? Can you provide a length scale of observable movement before the phase ambiguity is surpassed? Additionally, the authors see a fair amount of aggradation during the dry seasons in the river channel (fig 16), and explain this as "due to a combination of noise and varying perpendicular baselines caused topographic sensitivity variation." I don't understand that explanation, especially when the magnitude of the aggradation is so consistent, doesn't look like a noise signal, and sometimes looks

like it has a similar slope to your inter-annual aggradation rates. Can you clarify my misunderstanding of the work, or address this concern with the data?

AR: The relatively large gap during the monsoon season is not problematic because the phase information used to fill this gap combines phase ambiguity and sediment aggradation caused phase difference. The phase ambiguity is not removed and is represented as small jumps, as shown in Figure 16. These small jumps contribute to the uncertainty range of -12% to +8%.

For example, in the black points from the 2017 time-series in Figure 16, the ±100 m perpendicular baseline introduces a phase ambiguity of 0–5 mm/yr. Therefore, observable movement of 16 mm/yr cross the larger interferogram network gap, is required to exceed this 5 mm/yr phase ambiguity. In general, points of different colours within the same year should exhibit similar phase ambiguity caused small jumps because they share the same perpendicular baseline network, as illustrated in Figure 8. However, some variations in small jumps between different coloured points within the same year are observed. These discrepancies may be attributed to atmospheric noise and/or other factors. This might explain the irregular jumps between flow distances of 142,000–134,000 m in Figure 14. In this study, only the gaps between the two point clouds are considered as signal. The other possible factors and its mechanisms are worth to look at in the future studies.

Future studies will address the phase ambiguity issue. This study focuses on demonstrating a novel approach to using differential residual topographic phase to map river sediment aggradation rates. In our opinion, it is necessary to understand the original residual topographic phase history before applying further processes, such as flattening small jumps. Advancing this approach will be the immediate focus of future research. Figure 10(b) potentially illustrates the phase difference between two different years (black and blue), providing a more straightforward representation of phase differences caused by river aggradation.

Line edits:

L36: "where river channels"

L66: "and are labelled rivers 1-4"

Figure 1: "are approximately 15 km in length, and 300 m in width"

L107: "bars result in stronger"

L117: "Azimuth is along the satellite flight path, and range is perpendicular to the satellite flight path"

L121: "Amplitude is calculated following Eq. (5-8)"

AR: Updated now, thanks!

Figure 2: Why are the bar graphs in e-f descending rather than ascending. Not sure if this is some standard I'm unaware of, but it confused me.

AR: The SAR amplitude values in decibels typically range from about -25 dB to 0 dB for the most cases. This is why the bar graphs in e-f descending. We have now updated the figure caption to point out this information.

AR: **Figure 2**: Illustration of the SAR diffuse backscatter from a dry gravel riverbeds. The returned SAR backscatter energy intensity is usually very small because the radar signal loses strength as it travels to the target and back. The decibel (dB) scale is logarithmic, and the logarithm of a small number (less than 1) is negative. The SAR amplitude values in decibels typically range from about -25 dB to 0 dB for most cases (Flores-Anderson et al., 2019). This illustration demonstrates that each 20 m² InSAR mapping pixel contains tens of thousands of distributed scatters. The intensity of the SAR signal represents the sum of the distributed scatters

Figure 3: I recommend bigger text, hard to read.

Figure 4: bigger text

L241: Define "LOS"

Figure 5,6,7: bigger text

Figure 8,9: way bigger text for the legend!

L330: Figure 18?

Figure 10, 11, 12, 13: Bigger text

AR: Updated now, thanks!

Figure 14: it's interesting that you have larger uncertainty for river 1 and 2 which have more consistent data, and less uncertainty in river 3 and 4 which are much more noisy looking. Can you comment on this? Text size is ok here, but could still be bigger.

AR: In Figure 14, the shaded in grey spans from -12% to +8% is for every dots in the plot. Somehow when the dots are less scattered (e.g. river 1 and 2), the plot of shaded in grey shows the range really well. When the dots are scattered (e.g. river 3 and 4), the plot of shaded in grey not display well with the shaded range. However, the uncertainty range of -12% to +8% is the same at every dots in the elevation change rates plot in Figure 14.

L445: Figure 18!

Figure 16, 17: Bigger text. Also where is location E on the map?

L468: Figure 18!

AR: Updated now, thanks!

Section 8.3: This is a very interesting hypothesis about the relation between aggradation rates and avulsion timing. Can you find any evidence to support that in the sediment record near these rivers, or just another citation that might support this idea?

AR: We agree that this is a very interesting avenue for continued research. We have added three additional references that discuss rapid avulsion frequencies in rivers immediately east (the Kosi) and west (the Bagmati) of our study site in lines 499-503. The rates they quote are faster than the estimates we gave and that may be more closely linked to the behaviour at avulsion nodes rather than just a function of elevation contrast.

AR: avulsion every few hundreds of years (i.e. channel depth divided by aggradation rate). However, other mechanisms such as a sudden reduction in transport capacity near the avulsion node may cause the river to spill and avulse (Jones and Schumm, 1999). The Bagmati River which is just west of our study site in the Gangetic Plains has been described as 'hyper-avulsive' and has a record of channel avulsion on a decadal to century scale (Jain and Sinha, 2003; Sinha et al., 2005). Similar avulsion frequencies have also been recorded over the large Kosi River that drains east of our study area (Chakraborty et al., 2010).