

Wang et al paper presents, compared and discusses a set of complementary remote sensing, contact and non-contact in-situ ground deformation measurements acquired at two sites on the Tibetan Plateau. A such diversity and long record of collocated observations is rare in permafrost terrain. I congratulate the authors on the effort to acquire, analyse and disseminate these results. I believe the dataset and the study are definitively valuable and that the article is worth publication after minor revisions. I have no major concern regarding the content but I suggest several ideas to enhance the value of the work and improve the readability of the paper.

- **Data availability:** l.694-695 only mention how to access the unprocessed radar images, which seems to indicate that the data record is not openly available yet. Given the uniqueness of the observations, I hope the authors have planned to provide the full data series in an open repository. This might be valuable for many other researchers on the way further!
- **Additional background information and processing details:** Sections 2 and 3 could benefit from additional details. In addition, some information is currently scattered/hidden in other sections (results and discussion). It would be easier for the reader to get them from the beginning. Some examples thereafter:
 - Section 2: A brief intro about the soil type, grain size, expected ice content/type at different depths, and the potential differences between the two sites would be valuable to understand some of our interpretation in the following. Such info comes very late in the manuscript (e.g., l. 612-616). Before that, we are missing a bit of context.
 - Section 3.1: Processing info about the applied filtering / potential outlier removal procedure (similar to Sections 3.2-3.3) are missing.
 - Section 3.2: At l.185, the number/proportion of outliers that were removed could be documented.
 - Section 3.4: Although indirectly mentioned at l. 217-218, it would be good to specific the temporal baseline (12-24d). The corresponding detection capability (max detectable displacement) could also be documented. The number of used interferograms (and potentially the number of those which have been discarded) could be reported. L.224: the moving window is therefore 36 days, right?
 - Section 3.5: At l.570, it is mentioned that the deepest sensors failed. Good to mention it in Section 3.5 as well, and specify which ones (which depth).
- **Better integration of the second site:** The title mentions one supersite and the start of Section 2 (l.103) focuses on the main monitoring station (SLC03). However, measurements have been acquired at a second site (SLC07) and are used in the analysis. Although it is clear that SLC07 is less instrumented, it would be easier to follow if the second station is better introduced from the start. Station SLC07 is first visible in Fig.1 but first mentioned in the text at l.137.

InSAR that can be processed at the regional scale has been processed only around SLC03 (Fig.3). Why? It could have been a fine way to further discuss the differences between the two sites.

To discuss the differences between the two sites, information about the surface conditions is mentioned, but not much else. What about the sediment type, the expected ice content, the climate?

- **Improvements of table/figure contents, legends and captions:** Most figures could be slightly improved to make it easier to read. It is sometimes necessary to dig into the text to find information necessary to understand. They are often not self-contained and standalone. Some examples thereafter:
 - Figure 1: Explain that panels b and c are at SLC03. c) what is the red dashed rectangle? The acronym SR50 in panel b) is not understandable (explained only at.175). Consider adding explanations of all acronyms in the caption.
 - Table 1: Why not including the similar info for InSAR? Even if not an in-situ technique, the same properties could be documented and help explain the differences.
 - Figure 3: Caption: 'Spatial distribution of long-term deformation trend (a) and seasonal deformation (b)'
 - Figure 4: Why is the SLC07 series stopping in summer 2024? If this is due to power-supply failures mentioned at l.289? I think it should be mentioned clearly before (methods section), because it is more than just a small data gap.
 - Figure 5: Orange (flag) = flagged and removed before averaging, right? Could be written more clearly in the legend or caption. The cause of the data gaps is not explained.
 - Table 2: The names of the stages do not correspond to the naming used in Fig.10 (AT1, LT1, etc). LF stages are referred to as 2022-23, 2023-24, 2024-25, although the dates only span one year (2023, 2024, 2025). Remove or explain why AF-2025-26 is lacking information.
 - Figure 7: In (a), none of the displacement time series start at zero. L.338 explains that the series are referenced to the first day. Which first day? Different for each technique? If yes, for sake of comparison in this graph, why not referencing everything to June 2022? The legend of the stages (AT, LT, AF, LF) could be added to the figure for better standalone understanding. The acronym could be explained in the caption. It might be a color contrast question, but it seems that the black line (thaw front) in (e) does not match 0 degree and therefore it is unclear how this line has been plotted.
 - Figure 9: References to the different freeze-thaw stage names could be added in the figure to help the readers.
 - Figure 11: Add a reference to Fig. 10g (full series).

- Figure A3: Somewhat at the same level as Fig. 11 (short-term analysis) and very much supporting what is described in Section 4.4.3. It would make sense to move it to the main manuscript.
- Figure 10: A lot of information in this figure. Ensure that all elements are well explained. For ex. what is the orange number at the bottom of the TDD dashed lines in c, e, i? And why missing in g? It could be mentioned in the caption that the displacement time series are based on LVDT only.
- **Discussion of the AL vs top-of-permafrost ice melt, and the pore ice vs excess ice controls:** A major point of the article is to discuss the differences of displacement patterns from ground thawing during the main spring-summer period (melt of ice in the active layer) and during the late-thaw-season (melt of ice at the top of the permafrost). Of course, the records are very interesting due to the high temporal resolution, and I overall agree with the interpretation, but the explanations are not always clear. The point is discussed several times (l.23-26; l.81-82; l.479-506; l.635-647) but remains at the same level along the paper, and somewhat mixes the question of the temporality (early/mid-season vs late-season) and the type of ice (pore ice vs excess ice) and the depth of the thaw front (AL, top-of-permafrost) without providing all information to make the links between these elements. At several locations, it seems to assume that excess ice is only in the deepest layers, at the top of the permafrost (section 4.4.2, l.591, l.639). Subsidence from active layer thaw is also due to ice melting and can also include a component from excess ice. That is shown in some references you are included at l.82-84. In your case, how do we know that the active layer has no excess ice? How do we know that the late-season subsidence corresponds to a thaw front reaching the top of the permafrost, considering that the ALT is estimated at 300 cm (l.118) but that the temperature sensors are not measuring below 240 cm (l.235). L.669 -670 list several contributions: ‘distinct deformation behaviours during active layer thaw, excess ground ice melt, active layer freezing, etc.’ and l.670-672 mention that ‘settlement during active layer thaw commonly occurred in a stepwise manner, whereas settlement by excess ground ice melting was more continuous’. Phrased this way, it sounds that movement from active layer thaw is not from ice melting. I guess you mean that it is due to pore ice melt only (by opposition to the deepest layers), but in that case, better to state it clearly. It is just a matter of phrasing and clearer explanations about the links between the thaw front depth, the ice type and content, and the resulting subsidence patterns.
- **Comparison between the different methods and discussion of each technique value/limitations:** Four methods are used to measure ground surface displacements. All have different properties and therefore advantages and limitations. The techniques are compared using correlation metrics, but in some cases without discussing much the observed differences. More weight is placed on LVDT, which is understandable due to the high temporal resolution. However, to

strengthen the clarity of the conclusions and better highlight the value of the multi-sensor supersite, I believe a more balanced treatment of the other techniques and more discussions of the differences would be valuable. Some examples thereafter:

- Figure 7: the InSAR and LVDT time series show a major shift of displacement patterns (several months of differences in the timing of the subsidence-heave cycle). Any idea why? It is mentioned several times in the text that InSAR and LVDT are well agreeing in term of correlation (e.g., l.362), but it is not necessarily the case temporally.
- L.345-349 and Figure 8: Methods measuring larger seasonal amplitudes are also those showing smaller long-term trend. Any idea why?
- L.355-356: LVDT-InSAR are more similar to each other, and GNSS-IR and ultrasonic ranging are also more similar to each other. Any idea why?
- Section 5: The in-situ techniques are quite well discussed but the value and limitations of InSAR are almost absent. At l.655, the regional scale is mentioned – this is only possible to get using remote sensing. Might be worth mentioning at the end of the discussion.
- Same in the conclusion: there is a conclusion for all techniques, except InSAR.

Additional details:

- Title : the « : » after multi-year is not necessary.
- L.32: remove « - » after seasonal.
- L.40-49: could mention in this paragraph that this is valid for flat terrain. On slopes, other elements must be accounted for.
- L.48: 'integrates' is probably not the right word. 'is controlled by'?
- L.52: not only the Tibetan Plateau, all cold-climate regions with permafrost (mountains at lower latitudes).
- L.52-53: 'To our knowledge, the work of Gruber (2020) remains...'
- L.61: space missing before 'Tilt-arm'. '...which are also contact in situ devices'
- L.69: 'larger than that of point sensors' could be removed (obvious in this sentence)
- L.73: 'limited to the work of Zhang et al. (2021)...'
- L.76: not clear what 'spatial monitoring' means. I believe it could be removed.
- L.76: InSAR acronym is never explained.
- L.92: 'permafrost-deformation' sounds a bit odd. It is not the permafrost itself that is deforming.
- L.95-96: the discrepancy between the start of the operation (2021) and the observation period used in the study (from 2022) is visible here but explained only later (l.141-142). You could consider simplifying here (for instance avoiding mentioning 2021) – it would be less confusing.
- L.96: SBAS-InSAR acronym not explained.

- L.110: 'monument' is not the right word. Simply 'structure' would fit better. Check in the whole manuscript: 'monument' is used several times in this context (e.g., l.366: possible structural movement instead, and at l.562).
- L.122: to make it clear, add the name SLC03 in this sentence and refer to Fig. 1d.
- L.137: to make it clear here, refer to Fig. 1d at the end of the sentence.
- L.125-127: refer to Fig. 1b-c at the end of the sentence
- L.131: missing space after 'Table 1'.
- L.146-158: half of this paragraph uses past tense, the other half uses present tense. I would suggest harmonizing to 'present' – lighter and the sensor is indeed still operating, I guess.
- L.198: reference to A/ϕ could be added in parentheses.
- L.199-200: revise this sentence. 'The reflector height h can be determined based on the relationship between the reflector height and the dominant frequency: equation'.
- L.242-249: partly a repetition compared to the method section (3.4). Some of the info here could be moved to methods, I think.
- L.251: 'Combined to Figure 1d, the spatial pattern shown in Figure 3a-b...' Very hard to relate cause the extent is not the same. A rectangle showing the extent of Fig.3 on Fig.1d would help. But ideally, it would be best to have the same extent as 1d on Fig.3, in order also to include the second site SLC07.
- L.283: specify when (May) the settlement begins.
- L.285: remove 'also' – not really comparable with the previous sentence.
- L.295: I don't think 'positive excursions' is a classical terminology: outliers instead? Check in the whole manuscript: 'excursion' is used several times in this context (e.g., l.307, l.547).
- L.311: The measured cumulative lowering is not mentioned systematically for all techniques, it would be nice to add it for the others (for sake of comparison).
- L.320: (iv) late-winter freezing
- L.326: 'early part of the AF stage' / 'settlement still persists'
- L.359: here short temporal baselines are mentioned but never explained before.
- L.394: bug of font size
- L.400: might consider weakening this statement a bit. It might look less obvious due to different scales (faster movement) but in reality the stepwise pattern is also clear in October.
- L.408: 'this layer froze during this period'
- L.408-409: 'freeze-back'
- L.415: 'freezing front advance'
- L.455: '...and, in some year, a lag is visible compared to the surface thaw onset.'
- L. 458: the Stefan equation is never introduced. Add a reference in Section 3.5?
- L.460: I don't think it is possible to say 'steepest proportionality'.

- L. 472: active layer is more usually written without a '-' Check the entire text, it is inconsistent.
- L.478: 'indicators of ground ice melt': early season subsidence is also an indicator of ground ice melt, just not at the same depth. Consider rephrasing to 'indicators of top-of-permafrost ice melt'
- L.487-488: 'shallower than in the other years': nice to include the values for the other years. Reference to figure 7e?
- L.490: good to also refer to the full time series (figure 10g).
- L.514: missing a mention to the year (2024) in first sentence. Or if this refers to all years, it should be modified as: 'Distinct slight heave events were observed in...'
- L.524: might try to find another word that 'dip' – unclear.
- L. 528: high accuracy, high temporal resolution'
- L.546-548: Long sentence. Consider splitting in 3 sentences: 'In the interior Tibetan Plateau, winter snow cover is often thin, and short-lived so snow does not permanently obscure the ground surface. In the ultrasonic record, snow accumulation appears as episodic positive outliers rather than as a persistent signal (as shown in Figure 5). With appropriate filtering, most of the underlying deformation signal can be recovered.'
- L.563: remove 'However'.
- L.570: 'failure of the deepest sensors': this info is missing in the methods section.
- L576: 'details'.
- L.583: 'soil temperature, moisture profiles and...'
- L597: start a new sentence after 'a few millimetre.'
- L.606: 'in overall process shape'? Not sure what it means.
- L.611: 'the timing of the thaw settlement'.
- L.630: 'smooth' instead of 'clean'?
- L.632: 'organic layer thickness?', without '-'
- L.641: when the thaw front reached...'
- L.644: 'signature of thaw front reaching the ice-rich permafrost'.
- L.646: might be wise to repeat that it is because 2023 is a cold year.
- L.654: 'the cumulative deformation legacy of ground ice loss'? Not sure to understand the phrasing. Simply 'indicator of the ground ice loss' would be clearer.