The Cryosphere Discussions

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Comment on tc-2022-9

Anonymous Referee #3

Referee comment on "Permafrost Stability Mapping on the Tibetan Plateau by Integrating Time-series InSAR and Random Forest Method" by Fumeng Zhao et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-9-RC3, 2022

This study proposes a new method that integrates InSAR time series and machine learning (random forest) for mapping permafrost stability in a selected region in central Tibet. This is probably one of the first efforts of such integration tailored towards estimating stability. However, the current framework relating InSAR-estimated surface deformation to permafrost stability is both conceptually flawed and poorly explained. The quality of the manuscript in its current form is further jeopardized by vague and even sometimes careless description and presentation. I raise a few major issues as detailed below and choose not to list minor editorial comments.

1. To permafrost scientists and engineers, 'permafrost stability' can be expressed from various perspectives, e.g., mechanical strength, permafrost thickness, temperature, active layer thickness, ground ice content, thaw settlement, engineering, hydrology, and even carbon stock and fluxes. In the abstract and introduction, the authors put forward a link between ground deformation (or should be ground *surface* deformation) as an observable for permafrost stability and the whole study has built on this concept. Since the authors didn't define what they mean by 'permafrost stability', I had to guess they were mainly concerned with thaw settlement.

2. There are a few fundamental and practical issues that need to be critically addressed if using thaw settlements to infer permafrost stability.

- (a) As pointed out by the authors, thaw-season subsidence is predominately caused by thawing of the active layer, not permafrost. The authors used an annual cycle + linear trend to separate seasonal and linear deformation (eq 1). Such a simple model is a reasonable choice and was adopted in many InSAR studies on permafrost. However, it is clear from Figure 5 (esp. P2) that InSAR time series often deviate from this simple time pattern. Such deviations may cause errors in the estimated trend, esp. given that the duration of the InSAR time series is only about 6 years.

- (b) Since the presented results include both seasonal and linear deformation and in many places the authors didn't explicitly state whether the deformation is seasonal or trend, I got completely lost and wasn't sure what kinds of deformation are presented and what were used as input into the random forest. E.g. it is unclear what kind(s) of deformation are shown in Figures 7 and 8. I can only guess from the units that they are thaw-season subsidence. But wouldn't you mainly use the trend?
- (c) Only till the result section 4.2.1, the authors stated the thresholds in deformation trends to classify stable vs unstable ground as "+/- 0.15 mm/year and -40 mm/year". Such important criteria need to be justified and introduced in the methodology. What are the bases of these thresholds? E.g., why trends larger than 40 mm/yr mean unstable permafrost, as it may seem small to different experts. Are these trends in the vertical direction or in line-of-sight (LOS) direction? 0.15 mm/year is extremely small compared to nominal uncertainties of InSAR measurements. Without estimating the uncertainties in the measured trend, is it still meaningful to set such a small threshold?
- (d) Not stated explicitly in the paper, but I suppose the authors converted LOS deformation trends to vertical by assuming the ground motion is purely vertical; and used vertical trends as input to random forest and all the InSAR results presented are in the vertical direction. Then another major flaw lies in the ignorance of lateral flow on slopes in periglacial landscapes. Depending on the geomorphic type and nature of the processes, lateral movement on certain such as landforms such as solifluction sheets, rock glaciers, and even fluvial fans can move (much) faster than 40 mm/year, yet the underlying permafrost could be stable. Whereas mass wasting associated with thermokarst processes such as active layer detachment slides and thaw slumps (Figure 16 shows one example) can show very fast movement due to degrading permafrost. Without differencing the nature of deformation on flat vs slope regions and knowing the surface geomorphology, the inferred 'permafrost stability' would be unreliable over slopes.
- (e) Yet, one of the selling points of this work is to use random forest to fill gaps in 'poor visibility areas' on slopes. This machine-learning-enabled advantage cannot solve the fundamental issue raised in (d).

3. The methodologic description of how integrating InSAR with random forest to infer permafrost stability is very vague to me. I raised a few concerns related to this methodology above and would summarize the key ones below.

(a) InSAR observations: seasonal or trend or both? No uncertainties.

(b) Classification of stable vs unstable: what are the bases?

(c) What are the exact inputs and outputs of the random forest?

(d) What are the reasons for selecting the topographic and climate variable? E.g. it makes little intuitive sense to include curvature and it turns out that curvature is the least important factor.

(e) What land surface temperature did you use? Annual ground surface temperature?

The authors should pay more attention to properly citing references and following scientific rigor. Here are a few examples:

Line 40: Schaefer et al., 2015 was about retrieving active layer thickness from InSAR, not about using the thickness as an index for permafrost stability.

Line 44-45: The sentence is about sparse field-based measurements. But the two papers cited are both based on remote sensing.

Line 48-49: Widhalm et al., 2017 mainly used SAR data, didn't involve 'many environmental factors'.

Line 62: Schaefer et al., 2015 was not a Tibet study.

Line 140: Ran et al. (2021) were concerned with permafrost temperature and thickness, not ground deformation.

(This list can be very long)

Two relevant and important papers were published recently and should provide some guidance and inspiration.

- Ran et al. "Biophysical permafrost map indicates ecosystem processes dominate permafrost stability in the Northern Hemisphere." Environmental Research Letters 16.9 (2021): 095010.
- Chen et al. "Magnitudes and patterns of large-scale permafrost ground deformation revealed by Sentinel-1 InSAR on the central Qinghai-Tibet Plateau." Remote Sensing of Environment 268 (2022): 112778.

In addition to the issues raised earlier, there are a few places of superficial or incorrect expressions of permafrost concept, such as:

Line 28: shrinking of ‘permafrost boundaries’ should be ‘permafrost extent.

Line 34: thickening active layer is not the root cause of carbon release from permafrost

Line 278: ground surface, not permafrost, heaves; ground (the active layer to be exact) is freezing, not frozen in September.

Line 519: deterioration should be degradation.