The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2022-9-RC1, 2022

Comment on tc-2022-9

Anonymous Referee #1

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-RC1, 2022

General Comments:

The authors propose a combination of InSAR time-series data from one viewing direction with one ML approach (Random Forest Method) to map the permafrost deformation in the Tibetan Plateau, emphasizing the area where radar visibility problems take place.

As InSAR delivers ground displacements along the slant-looking direction, visibility problems such as layover and shadow often arise in mountainous areas; it occurs in the descending track in this study. In such a case, InSAR users will usually take advantage of other data imaged from another direction that is the ascending track in this study. However, instead of using the ascending InSAR data, the authors employ the ML method to infer a permafrost stability map even at unmeasured areas. In other words, it appears as if the authors derived some signals from virtually nothing. In terms of the overall design of research work, I am not willing to recommend the authors' approach to my friends.

Furthermore, there are a couple of serious issues in the authors' interpretation of InSAR data and time-series analysis. They do not mention anything about the corrections of the tropospheric errors/artifacts. Even if they employ the time-series analysis approach with plenty of SAR images, it is impossible to ignore the tropospheric errors particularly when they use the entire image frame; the larger the imaged area, the larger the tropospheric errors. Although the authors attribute the apparent seasonal signals to the subsidence and uplift due to the freeze-thaw cycle of the active layer, we should first eliminate or minimize the tropospheric errors. Secondly, while it is related to the previous comment, Figure 14 derived from ascending track clearly indicates that the "deformation rates" are closely correlated with the local topography. Those are called topography (elevation)-correlated noise, which is again caused by tropospheric delays. They can be corrected, by fitting with the DEM. Thirdly, while InSAR tells us the surface displacements relative to non-deformed point(s), it is not clear where the reference pixels are located; the reference pixels should be stable not only in one InSAR image but also over the entire observation period. I, therefore, recommend reanalyzing the InSAR time-series data based on the ascending track, considering the points above. It is not clear why they must use the Random Forest Method; permafrost stability and landslide susceptibility follow totally different physical mechanisms. The authors should show both descending and ascending data over flat areas as verification of deformation signals as they are mostly vertical.

Specific and technical comments:

L30: As the focus is now on Tibet, those papers outside Tibet and/or Global should be removed.

L48: Delete "that"

L51: Unclear sentence

L58: The two references are not related to permafrost.

L109: Delete "in which"

L135: Replace "spatial" with "temporal"

L176: "relatively flat and homogeneous" conflicts with L115, "mountainous terrain"

L276: Is 0.8 true? There is a big deviation near the end.

Figure 6: When are the periods in the four years? Show month and date.

L319: Disagree with "in general agreement"

L333: Michaelides et al (2019) examined the post-fire area, where there occurred a big change in surface vegetation. But the authors are now examining unburned areas. If we follow the suggestion by Michaelides et al (2019), we expect significant deformation signals over "Bare lands" as there would be no insulation effects. The authors should check if there exist such signals.

Figure 12: Leveling route by Wu et al (2018) should be clarified, whereas only one leveling data was shown.