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Interactive comment on "InSAR time series analysis of seasonal surface displacement dynamics on the Tibetan Plateau" *by* Eike Reinosch et al.

Anonymous Referee #2

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The article from Reinosch et al. presents a case study of InSAR-measured displacements patterns related to freeze/thaw processes in two basins of the Nam Co area (Tibetan Plateau). The authors aim to study the seasonal and interannual dynamics of the ground in a periglacial environment. The objectives of the paper are relevant in term of research in SAR remote sensing applied in geosciences, and for understanding the short and long-term changes of periglacial processes at landscape-scale. In this way, the topic appears to be suitable for The Cryosphere. However, to my opinion, four major issues have to be addressed by the authors before the paper could be accepted for publication (see thereafter). There are also several complementary elements that should be considered (listed at the end of the review).

Note that I decided not to read the others comments posted during the open review process in order to avoid being influenced. There are thus potentially repetitions with the reviewer 1 and other persons who previously commented on the article.

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—Major comments—

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—1. Confusion between environmental factors, processes and InSAR-measured effects—

The air temperature (environmental factor) transferred into the ground and varying under and over zero degree leads to phase change of water/ice (process), that leads to frost heave and thaw subsidence (effect). Of course, there is a link between these elements but it is misleading to present InSAR as a technique able to directly measure the freeze/thaw cycles and thermal properties of the ground. In addition, subsidence, even in periglacial environment, can be measured without being necessarily related to thaw. This confusion is present all along the manuscript. Here an non-exhaustive list of examples:

- l.17 and l.51: "surface displacement processes", "seasonal displacement processes": use "processes" or "surface displacements" separately. A surface displacement is the consequence of a process.

- l.31: "We observe a very clear seasonal freeze-thaw cycle": you do not observe the freeze-thaw cycle, you observe heave and subsidence.

- l.311: "freeze-thaw model"

- l.318: "freeze-thaw amplitude"

- Fig.5: "freeze-thaw amplitude" (or "freezing and thawing parameters" in the legend) is not the right terminology, you measure heave/subsidence amplitude. "Maximum freeze-thaw subsidence" should also be replaced by "maximum thaw subsidence"

- l.406-407: "The day of maximum subsidence (DMS) is the day in summer during which the soil has thawed to its maximum extent". For the same thaw depth, subsidence can vary depending on the ice content.

On slopes, it is important to think the same way: various processes can lead to an apparently similar effect (downslope movement). See major comment 2.

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—2. The three models, the downslope projection and the assumption of variable ice content to differentiate linear/seasonal patterns on slopes—

I would suggest to consider other names for the models and be more clear about their differences from the start (before 4.3). A summary comes at l.333-338, but it is a bit too late. It would be easier to follow if the overall idea is clearly explained just before l.257. FTM name could be changed to heave/subsidence for the reason explained in major comment 1. SSM name is not fully correct: the landforms may have seasonal acceleration/deceleration but do not fully stop creeping. MSM is in general not clear to me: in areas <10deg, what is the difference with FTM? Did you remove the seasonal trend to keep only the multi-annual trend? Is SSM also based on projected results (not clearly stated at l.322-328)? Overall the names are mixing displacement patterns and related processes: maybe easier to choose either process-based names: for ex "heave-subsidence model", "seasonal slope process model", "linear slope process model" or displacement-based names: "vertical cyclic model", "downslope cyclic model", "downslope linear model" (just as examples).

The assumption of projection along slopes, if mostly right for landslides and rock glaciers, can be problematic for processes including both downslope and heave/subsidence components (such as solifluction,with displacement normal to slope in winter and vertically down in summer). In addition, it has been documented that

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these processes can occur on slopes <10 deg (see Matsuoka, 2001). I understand the need to simply but this limitation should at least be acknowledged in the manuscript. Gravity-driven downslope pattern does not necessarily mean permafrost creep, even in periglacial environment. Have you considered the potential presence of rock slope instabilities in these areas? If it sounds possible, you could be a bit more modest in the assumption relating linearity with high ice content (as you state at l.457-462; l.540-543). If not likely in these areas, it has to be stated.

Overall, maybe consider to use "downslope-dominated" (or gravity-driven) vs "vertically-dominated" (or freeze/thaw-driven)Âż instead of speaking about linearity/seasonality. As you write at l.536-538 (too late and too little explained to my opinion), it can "have been misidentified as linearly moving, while actually featuring both the seasonal freeze-thaw cycle prevalent in the valleys and the seasonal sliding pattern of the slopes". A way to check this it to plot the velocity in addition to (or instead of) the cumulated displacement on Fig.6. Fig.6B may look linear but looking at the velocity, I think you may see variations as well.

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—3. SAR data, ISBAS processing, interpretation of uplift areas and DMS—

Information about several basic data properties and methodological information (important for interpreting the results) is missing: multilooking factor, final spatial resolution, LOS angles, spatial/temporal baseline thresholds, temporal distribution of the initial SAR scenes (baseline plot), map with coherence, map with location of reference areas (in supplementary material for ex).

About ISBAS processing: l.189-190: "... where the coherence is intermittently below the chosen threshold..." and l.198 "... near water bodies, where coherence is very low. We therefore decided to use a very low coherence threshold of 0.1 to increase...": I am not especially known with ISBAS approach, but this sounds quite dangerous to me, especially if you used a threshold of 0.1 in some areas (l.198). Does it mean that you have 25% of interferograms with <0.1 in these areas? Maximizing the coverage also to areas where the results cannot be reliable due e.g. to vegetation or moisture means that some of your interpretation can be based on wrong estimates. At least good to try to explain as much as possible the method, document the uncertainties and acknowledge the potential limitations (in methods and/or in discussion). A coherence map could also be a nice way to document the distribution of these Âńless reliableÂż areas.

Due to this lack of information, it is hard to fully understand the cause of the uplift detected in some flat valley bottoms (l.366 and Fig.3). Looking also at Fig.6A, if you subtract the last and the first acquisitions, you also get a positive trend. Is it really likely that all these locations are affected by sediment accumulation or can it be a bias? I wonder if this cannot be due to low reliability (low coherence) in these areas, especially during the "wet" periods when the ground is subsiding. Or a bias due to the temporal sampling of the initial interferograms? Or atmospheric effect (remaining stratified component)?

About DMS: at l.413-414, it is written that there is shift of 11-27 days between ascending/descending datasets. Why that? 11/27 days is quite a lot, considering that it should in theory document the same thing. Can it be due to a shift of velocity value (problem with the location of the reference points?) or the different LOS incidence angles (different sensitivity to the vertical)? Due to undocumented information about data properties, it is hard to understand the results and fully trust them.

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—4. Poor discussion, bold/vague statements—

DMS 9 days prior to the temperature peak in one of the AOI is presented as "no lag" (l.422, l.427): this is in fact an inverse lag (or lag in the "wrong" direction), which has to be discussed. I would guess this may be due to the distance to the meteorological station: NAMORS station is maybe not representative of this AOI considering that

Qugapie has a significantly higher elevation? Did you try to apply an altitude correction? Figure 3: The bottom of the graph E/F is too little explained/exploited in the manuscript. To my opinion, this is maybe the most interesting finding of the study. There is a lack of structure in the Discussion. Consider dividing the Section in three parts, for ex: "Uncertainties/Error source"; "Thaw subsidence / Frost heave cycles"; "Downslope processes".

The Discussion is also quite vague. Some examples thereafter:

- l.480-481: one major error source in periglacial environment (during summer) is the impact of ground moisture on the phase (moisture can lead to a biased detection of distance change, up to 10-20% of the wavelength). Good to discuss this. See e.g. references: De Zan et al., 2014; Zwieback et al., 2017.

- l.485-486: "it does not follow the trend of a sine curve perfectly. Nonetheless we consider it a valid if imperfect approach". Really vague.

- l.486-488: "not identify any significant difference in the freeze-thaw cycle between areas where permafrost is likely to be present and areas where the ground is only seasonal frozen": this has not been presented in the Results.

- l.488-489: "we therefore disagree with similar studies..." without explaining more about the results and differences with the other studies, it is a bit too bold to say this...

- l.489-491: "the amplitude, the day of maximum thaw subsidence and the active layer thickness... agree well": summarize the values and explain more.

- l.498: "the point density of their data within Qugaqie basin is too low to draw reliable conclusion": I would also say that the meteorological data you have available may also be too weak to draw reliable conclusions.

- l.499-500: "explained by the small size of the basin": why the size would have something to do with the lag time? Have you considered other explanations: topography/location/altitude/permafrost extent? Looking at Fig.1, it appears that half of

Qugaqie basin is in permafrost zone, while Niyaqu basin is mostly in seasonally-frozen ground. These differences (and the consequences on your results) could be discussed.

- l.524-525: I believe you write here a start of answer to my previous question.

- l.511-512: How would be carefully speaking about permafrost degradation with only 3 seasons.

- l.543: "This disagrees...". It is not because you did not find strong seasonal variations, that your study disagrees with the study you refer to. Not the same context, focus, method, etc. And written at l.534-538: it can have both a downslope and a seasonal pattern. As it is not based on 3D measurements and projection has been performed, we cannot know.

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—Complementary comments—

—Abstract—

It could follow a better structure (context, processes you aim to study, presentation of the data and methods, and main results/conclusions).

- l.19: "these processes": not clear which processes . Last lines of the abstract explaining the active layer freeze/thaw and related displacements could come earlier.

- l.21: "Sentinel-1 constellation" or "Sentinel-1 satellites"

- l.25: "monsoonal climate accelerates those movements". Confusing as you say just after that not all are affected by seasonal variations.

- l.30-31: "permafrost degradation": I would be careful drawing conclusions about permafrost degradation based on only 3 seasons.

—1. Introduction—

The introduction is overall a bit poor. It currently focuses a lot on the Tibetan Plateau,

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it could benefit for other references to similar kind of studies in others regions of the world. Here an non-exhausive list of ref. that could also contribute to go further with the discussion of your findings: in Alaska: Liu et al., 2010, 2012; Schaefer et al., 2015; in Canada: Short et al., 2014; Rudy et al., 2018; in Greenland: Strozzi et al., 2018; in Svalbard: Rouyet et al., 2019; in Siberia: Antanova et al., 2018.

- l.45: "...harder to quantify using optical remote sensing due to their debris cover". I do not see what is the problem of the debris cover. I would say that the major reasons that there are difficulties and lack of inventories are: 1) small features, difficult to document if the spatial resolution is poor, and potentially slower than glaciers (problematic for optical remote sensing techniques); 2) more research focus on glaciers due to size/impact. Mountain permafrost is a relatively new field. But some studies using remote sensing are available, see e.g.: Kellerer-Pirklbauer et al., 2012; Millar & Westfall, 2008, Rangecroft et al., 2014; Lilleøren & Etzelmüller, 2011; Barboux et al., 2015.

- l.46: "...despite their importance as water storages". I would add: the evidence of a link between rock glacier kinematics and climate change. See e.g.: Delaloye et al., 2010; Kääb et al., 2007; Kellerer-Pirklbauer & Kaufmann, 2012; Roer et al., 2005; Ikeda et al., 2008; Kenner & Magnusson, 2017.

- l.52: inverse the order -> there is no thaw subsidence before a previous frost heave.

- l.54: "InSAR results" instead of "models"

- l.57-58: Rephrase. Maybe: "SAR satellites are side-looking and observe the Earth obliquely."

- l.65-67: this is more for "Methods" than "Introduction". I would simplify here (for ex just say "lead to poor phase stability, so-called coherence"). And go further with it in Section 4.

- l.70: "This is not a problem in our study sites.... In fact, we found..." It does not really fit in "Introduction". Corresponds more to "Results". And without further explanation (dry-little precipitation -> little snow cover), it is a bit useless.

- l.76-77: "rising lake level": this has not much to do with your study. Could come as a general info about the context (first paragraph) but does not really fit here.

- l.82: As explained in major comment 2, the linearity is not in itself a proof for ice content. But it shows that the landform is gravity-driven (dominated by a downslope component). The assumption should at least be further discussed (in Section 6 for ex): What does "high ice content" mean? What about rockslides? What about solifluction processes (with ice content and both seasonal variability and downslope component)?

- l.85: "landforms without ice": without massive ice? Without significant ice content. Seasonal pattern does not mean "no ice".

—2. Study Area—

- l.95: rephrase "endorheic catchment borders on the catchments..."

- l.96-98: rephrase. For ex: "They have elevations up to 7162 m a.s.l. The highest parts are glaciated, while the other areas are considered to be in the periglacial zone."

- l.112 and l.114: "... with high vegetation, such as..."; "... lack of high vegetation..."

- l.117: "... due to significant change of physical surface characteristics..."

- l.129: mixing spatial/temporal limitations. For ex replace by: "... making continuous temporal coverage of..."

- l.128-129: rephrase "... different levels of glacial impact and the predominant landscapes and their related surface motion processes at Nam Co."

- l.138: "Other landforms were accumulated through ... processes". Material (sediment) accumulates and processes shape landforms. Not fully correct to say that a landform accumulates through a process.

- l.141: "virtually free of vegetation". What does "virtually" mean?

- l.140-143: What about the bedrock? Missing information about geological context.

—3. Data—

- l.156: what do you mean by unreliable? Or "stable" (l.158)?

- l.162-164: the part about the interferogramanalysis, baseline and coherence thresholds is already about the "Methods" (how the dataset has been processed). Move to Section 4 + important to document the chosen thresholds (max temporal/baseline baselines). The max temporal baseline determines the max. velocity you can detect.

- Table 1: I am missing info about the line-of-sight (important for ex. to know what is this incidence angle and thus the sensitivity to vertical/horizontal component for both geometries) + We don't know the temporal distribution of the SAR scenes (fully continuous? Some gaps?). A baseline plot (potentially in supplementary material) would be valuable.

—4. Methods—

- l.183-184: "with a short temporal baseline": the threshold (max. temporal baseline) is not documented. Important to add this and explain why it has been chosen (in relation with the detection capability and the max. expected velocity)

- l.189-190: "... where the coherence is intermittently below the chosen threshold..." and l.198: "... near water bodies, where coherence is very low. We therefore decided to use a very low coherence threshold of 0.1 to increase..." = 0.75 intermittent value with a 0.1 threshold applied (25% of interferograms with <0.1)? Sounds really liberal to me, good to discuss it (see major comment 3).

- l.209: "strong shift": shift or spatial trend?

- l.215-216: "the error range of the slope projection..." the sentence is not understandable here (before having explained how the projection is performed)

- l.233: "...must not have any significant velocity by themselves". Rephrase, no clear.

Don't you just mean that the points have to be in areas supposed to be stable?

- l.251: "LOS shifts": what does it mean? "velocity shifts along the LOS" maybe?

- l.273: "lateral sliding" is a bit counter-intuitive terminology. Horizontal component would be easier to understand.

- l.279-280: "exception for areas with a large east-west velocity". Be clear about how you selected these areas (which velocity threshold? Manual selection based on prior knowledge?). How to be sure you didn't miss any area <10 with a significant horizontal component.

- l.290-291: why smoothing? Without knowing the resolution of the final InSAR results, hard to understand.

- Table 2: MSM displacement type: not only along slope if I understood correctly, also vertical on flat areas. Related geomorphological processes: maybe be a bit more specific – permafrost creep can also have seasonal variations. FTM slope: what does Âńmostly <10degÂż means?

- l.320: here you speak about the resolution, it comes too late (as I guess it applies also for the other models) and there is no info about the multi-looked resolution before interpolation. You could also add an explanation about why it is important to interpolate (sounds unnecessary to me).

—5. Results—

- l.374: again here, prefer "horizontally" instead of "laterally"

- l.359-360: here some causes of temporal decorrelation are presented, but one is not explained clearly: too fast movement. This is also why it is important to document somewhere the max. temporal baseline used for InSAR processing.

- l.380-382: "most of the low coherence areas would likely be considered as unstable": I don't think it is right to say this: as written at l.359, the gaps in coverage can also

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be due to shadow/layover, and in addition, low coherence can also be due to ground moisture.

- Fig.3: Due to the chosen color scale, it is really hard to see the difference between areas with vertical assumption or those with downslope projection. Also hard to spot the areas affected by subsidence. + Maps C and D are only for Qugaqie basin. Why? + I may have missed sth, but I think there is no mention of the "seasonal sliding coefficient" threshold used to differentiate "linear velocity" and "faster in summer" in map C.

- l.406-407: see major comment 1: Thaw subsidence is related to temperature, but is not a thermal property.

- l.413: "DMS of the descending dataset occurs earlier": why? (see major comment 3).

- l.449: "while the latter arrests most sliding processes". Âńslows downÂż would be more correct.

- Fig.6 "areas throughout Qugaqie basin": what about the other basin? As for Figure 3, not clear why you let one basin out of the analysis. + The locations of the selected points could be shown somewhere, on a map.

—6/7. Discussion/Conclusion—

(Mostly developed in major comment 4)

- l.518: missing a reference here.

- l.530-531: "by clustering data point with a strong linear pattern and high slope velocities": not clear what it means? Maybe add a map and some examples (InSAR vs orthophoto) in supplementary?

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