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Discussions

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

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By Simon Daout and Benedetta Dini

We would like to make some comments on this publication, since we have been cited a few times. We believe that some interpretations have been discredited by the authors by providing incorrect statements. Moreover, we feel that some citation are made inappropriately (and incorrectly), denoting a superficial reading of exsisting literature.

l. 425. Other studies (Li et al., 2015; Daout et al., 2017) have stated that there is often a significant lag time between the day of maximum air temperature and the day

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of maximum subsidence.

The day of maximum subsidence cannot be associated with the day of maximum temperature as it is perfectly known with in-situ ground monitoring and permafrost models that the active layer temperature does not follow a diffusive model but is mainly controlled by the Stefan equations (Riseborough, 1996). In other words, the subsidence has been shown to continue, at lower rates, well after the day of maximum temperature, until the temperature falls below zero. In situ-measurements (eg. Gruber et al., 2019 and many others) image this seasonal pattern, which can differ slightly from the Stefan model prediction depending on the moisture content, the snow coverage, the vegetation cover... In addition, the thawing of the ice-rich layers, together with the thaw settlement, can be delayed by few months from the freezing onset. For instance, Liu et al. (2017) document changes in active-layer thickness for the Tien Shan and show with their detailed time/depth graphs that complete active-layer refreezing at depth commonly takes place around the end of the year. Therefore, the lag time between the day of maximum air temperature and the day of maximum subsidence is not a statement from Daout et al., 2017 but a fact.

l. 433. We used this lag time to determine the active layer thickness (ALT) by assuming the heat transfer to be one-dimensional and the soil to be homogeneous.

The freezing onset is at first order controlled by the time at which surface ground temperatures drop below zero. Amplitude and timing of deformation are then controlled by the water/ice availability and the amount of excess ice in the ground (e.g Daout et al., 2017, Dini et al., 2019). It is, therefore, wrong to draw direct links between the observed deformation and the active layer thickness because the active layer does not follow a purely diffusive model and its behaviour in response to freeze-thaw is associated with the ability of the soil to retain water (grain size, mineralogy..) and the soil thickness.

l. 494. We observe a significantly shorter lag time, with most areas in this basin reaching their maximum subsidence ahead of the maximum air temperature by a few

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days to weeks.

The absence of lag between the day of maximum air temperature and the day of maximum subsidence is most likely linked to a misinterpretation of uncorrected tropospheric delays which is instead attributed by the authors to freeze/thaw related processes. This is also supported by the clear correlation at high-frequency (i.e. well localised patterns following topography) and large scale between the seasonal amplitude and the topography (e.g Fig. 5). As Dini et al., 2019 (Remote Sensing of Environment) show, the attempt to remove atmospheric effects with the use of filters on the time series does not completely remove the layered atmosphere effects. For this reason, Dini et al. show that unless the interferograms are corrected before the time series generation, it is important to apply further corrections, such as those that use atmospheric models and/or empirical corrections generated by looking at the signal-topography correlation. In the aforementioned work, there are plenty of examples taken from a large scale study that indicate the important effects of such corrections and that show the atmospheric-dominated seasonal cycles before applying such corrections. The authors do say that they perform a linear spatial trend correction to mitigate for that, however it is not very clear what this involves and the homogenous timings of maximum subsidence look suspicious for non-atmospheric processes.

l. 486. We could 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 seasonally frozen. We therefore disagree with similar studies (Daout et al, 2017; Li et al., 2015) that associated this process with permafrost.

Frost heave/thaw settlement is primarily caused by the formation/thawing of excess ice (these depending on water content and porosity of the soil), especially through ice lenses formation (segregation ice) in frost-susceptible materials (silt, fine sand, loess) with high water content. Permafrost acts as an impermeable layer that retains the soil-moisture and isolates the active-layer from the deeper ground temperature gradient. Freeze/thaw cycles are therefore mainly detectable in permafrost regions, where the soil contains enough ice/water content to produce thaw settlements higher than 0.5-1cm. In addition, it is evident that the point of change from subsidence to heave around October/November shown in Daout et al., 2017 relates to delayed thawing at depth (see comment 1), followed by heave as a consequence of the freezing and increasingly cold temperatures penetrating at depth until complete refreezing causes a period of winter inactivity. Also, large-scale models (e.g Qin et al., 2017, Gruber et al., 2012) have described the north-western part of the Tibetan plateau, studied in Daout et al., 2017, as a cold and continuous permafrost region with mean annual ground temperature below -5°C. Daout et al., 2017 can only describe permafrost related process and it is, therefore, unreasonable to think that the observed thaw settlement effects could be associated to freeze/thaw processes in non-permafrost areas.

l. 549. However, Dini et al. (2019) did not project their data along the steepest slope, which explains the lower values, and neither study analyzed the seasonal displacement patterns of rock glaciers in their study area.

The article that the authors incorrectly cite (rock glaciers velocities in Bhutan were analysed in Dini et al. 2019 published in RSE, not Dini et al., 2019 published in Engineering Geology) does indeed talk about rock glaciers velocities as they are projected on the steepest slope gradient. The method of assuming that for slope processes (i.e. landslides and rock glaciers) the velocity can be approximated to the steepest gradient is, in fact, quite well established. The authors present in this article a method to calculate a coefficient (correctly citing Notti et al., 2012) which was in fact generated in full by Notti et al. (2012). This is what is also applied in Dini et al. (2019, RSE). Citing from Dini et al., 2019: "If the displacement vector is assumed to be oriented downslope along the maximum gradient, which is a generally acceptable first assumption for gravitational slope movements, then it is possible to estimate the percentage of displacement detectable in the LOS (Notti et al., 2012) and thus to estimate a downslope velocity closer to the true velocity." In addition to this, Dini et al. (2019) looked for decorrelation over rock glaciers in their SBAS results. As the velocities were projected on the maximum

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gradient and clear decorrelation corresponding to a rock glacier throughout the area of study was not found, it seems fair to state that the relatively slow movements observed over rock glaciers are real (at least over the observation period) and not an effect of misinterpretation of the InSAR results. In addition, the reason why Dini et al. 2019 have not analysed the potential of seasonal accelerations and deceleration of rock glacier movements is due to the temporal sampling of ENVISAT and ALOS, which is on average of 90 days, and therefore completely unsuitable to look at seasonal velocity variations.

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