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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.

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Thank you very much for your insightful and helpful comments. We will respond to each comment in the same order as Daout and Dini.

I. We agree with your statement, that the lag time between maximum air temperature and maximum subsidence is a widely accepted fact, rather than a new statement made by Daout et al. (2017). We will change our manuscript accordingly, to make sure that this is clear to the reader.

II. The approach of Li et al., (2015) to determine the active layer thickness (ALT) from the lag time between maximum air temperature and maximum subsidence is very simplistic, especially as it does not consider variations in the ground moisture content. We will therefore remove the section about the calculation of the ALT in the next draft of our manuscript.

III. We do not agree with the statement that we misinterpreted the tropospheric delays as freeze-thaw related processes. The reasoning of Daout and Dini to assume this to be the case is (1) the seasonal patterns shown by us follow topographic structures and (2) the correlation between the amplitude of the seasonal patterns and topography. They also ask for clarification on our linear spatial trend correction, which we will answer in (3).

(1) In our opinion Daout and Dini do not take into account, that we separated seasonal freeze-thaw related processes into two different models: the freeze-thaw model in flat areas / valleys and the seasonal sliding model on slopes (described in sections 4.4 and 4.5 respectively). It may therefore seem like our seasonal freeze-thaw related processes follow topographic structures when looking at only one of these models.

(2) If the seasonal pattern we observe in our data was caused by tropospheric delay and not ground deformation, then we would expect to see a correlation between the strength (i.e. the amplitude) of this seasonal signal and the elevation. This has been shown for example by Dong et al. (2019) or Dini et al. (2019). This is not the case in our data (Fig. 1 below). We selected not only one reference point but instead 50 – 90 (Section 4.2), which should also help to reduce the effect of tropospheric delay on our results.

(3) For linear spatial trend correction of the Qugaqie basin we used only regions we expect to be relatively stable on a multiannual scale (i.e. flat and not in immediate contact with water bodies or glaciers). We then determined the linear correlation of their multiannual surface velocity and their elevation. The resulting linear trend (R^2 = 0.12 for ascending and R^2 = 0.38 for descending) was then removed from all ascending

and descending data points.

IV. We will change the section in question, as Daout and Dini point out correctly, that the comparison drawn in our manuscript is not appropriate here.

V. We agree that the publication Dini et al. (2019) in “Remote Sensing Environment” is a more suitable citation and we will change the sections in question accordingly.

References:

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Fig. 1: Diagram of 1000 randomly selected data points (normalized for the lower frequency at higher elevations) showing the relationship between the amplitude of the seasonal signal and elevation.

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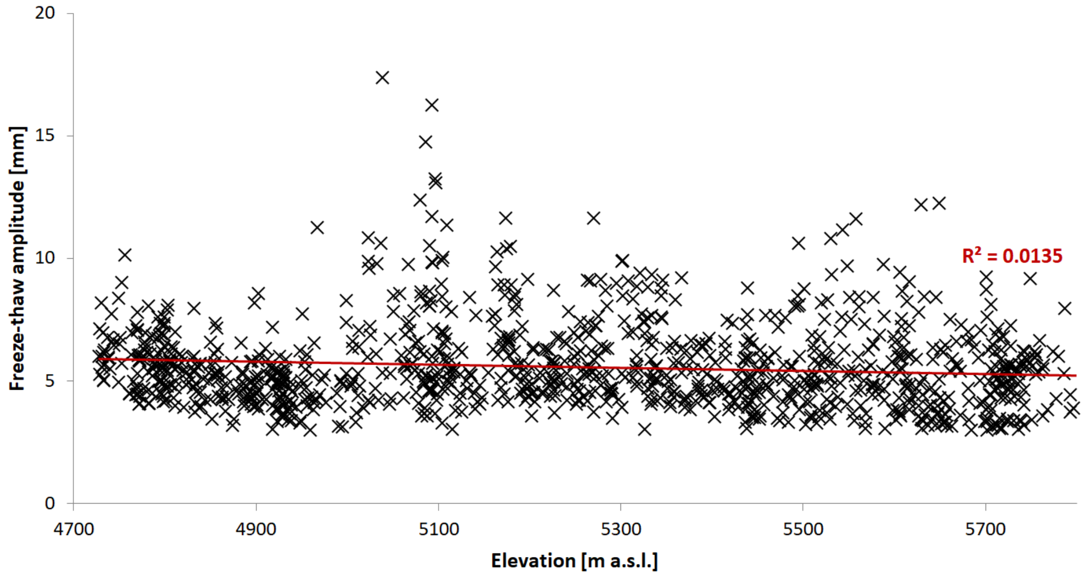

Fig. 1.

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