The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-173-RC1, 2019

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "GPS Interferometric Reflectometry measurements of ground surface elevation changes in permafrost areas in northern Canada" *by* Jiahua Zhang et al.

Anonymous Referee #1

Received and published: 15 October 2019

GPS-IR is interesting for permafrost studies. New determinations of GPS-IR surface elevation changes can be useful. Unfortunately, I found this manuscript unacceptable and it has to be improved strongly before being published in this journal. The findings of the submitted paper do not add any value to the existing literatures as they are already presented by Liu and Larson (2018) and Hu at al. (2018). Please highlight what is really new and what is the outcome and applicability of this approach in a more prominent way. The following points should be answered: It is already proved by Larson et al. (2008) that the reflector height (H) and the phase of SNR observations are highly correlated. Therefore, some of the H variations should come from the phase variations. SNR observations also are a function of GPS receiving antenna and

Printer-friendly version

Discussion paper

GPS signals. The authors have not discussed what types of antennas and signals are investigated, and how their impacts have been moved from the estimated heights. In addition, the penetration depth of microwave signals should be physically estimated to correct the estimated heights. The authors just used low elevation angle observations as SNR oscillations are clearer. However, the tropospheric impact is not negligible for low elevation angle observations even for 2-3 m antenna heights, so its impact should be studied in the paper. The tropospheric refractions seem to have a seasonal impact on GPS-IR (Williams and Nievinski, 2017). The authors have not corrected their GPS-IR solutions. Therefore, the reported slopes are tropospheric-contaminated. In addition, the authors used different antenna monuments (Figure 4), but they did not study the thermal expansion of monuments. The authors used the mean value of each year after the outliers are rejected to estimate and report the slopes, while the results presented in Figure 5 are somehow confusing. For example, at Alert what is the mean for 2016? It seems it is very different from the other 5 years and 2016 cannot represent the reported slope. There are other examples like 2014 at Iqaluit, 2004 and 2011 at Resolute Bay. In addition, it is more complex at Bakes Lake as it seems just a linear fit doesn't represent the GPS station and higher-order polynomials should be used. It would be easier if the median reflector heights for each year were also plotted together with the time series in Figure 5. That would help us to understand if the linear fit is good enough to report surface elevation changes.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-173, 2019.

Printer-friendly version

Discussion paper