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Interactive comment on "Three-in-one: GPS-IR measurements of ground surface elevation changes, soil moisture, and snow depth at a permafrost site in the northeastern Qinghai-Tibet Plateau" *by* Jiahua Zhang et al.

Anonymous Referee #3

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This paper introduces GPS interferometric reflectometry technique as a tool for remote sensing of surface elevation changes, soil moisture contents, and snow depth at a single permafrost site. In addition, authors proposed an improved method for soil moisture estimation by modeling surface vertical movements and removing its bias on reflected GPS SNR phase shifts.

Overall, the objectives and approaches are clear and the proposed solution for soil moisture estimation at permafrost areas with vertical displacements is a genius idea. However, there are some concerns which convince me to call for a major revision for

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Discussion paper

this paper.

1. The inconsistency between GPS-IR-driven and in-situ-measured snow depth values is out of acceptable range. GPS-IR capability of snow depth measurement has been examined several times in many studies, and strong agreements have been achieved. Although the correlation in this paper looks promising and reflects the general patterns of snow accumulation, the bias is not acceptable as previous studies have reached to better agreements. In addition, the way that the authors explained this "systematic" inconsistency does not make sense. The reflectivity difference between snow and the underlying frozen ground is not so much for GPS L-band signals. Moreover, this reflectivity difference, even if we consider it as a potential source of error, would not affect neither the amplitude nor the polarization because signals are assumed to be reflected off the "top" of the snowpack. Furthermore, "possible penetration into the soil when manually probing the rod", if happened, would reduce the bias as it would cause an overestimation in in-situ snow depth measurements. I would seek for either a better explanation or a reconsideration in the snow depth retrieval method. Looking into "higher-order frequencies" can be a solution for this issue as proposed by Cardellach, Fabra, et al. (2012) and Ghiasi (2020).

2. The authors have used Stefan's equation for modeling the surface elevation changes as they believe GPS-IR elevation retrievals are not accurate enough because their uncertainties are in the order of few centimetres. I would say that "a few centimetres" is an acceptable accuracy for this purpose since Stefan's equation has not shown a better accuracy in literature. I would suggest conduct the same validation using surface elevations directly obtained by GPS-IR. Besides, the term "uncertainty" used by the authors does not look very exact because it is driven based on the standard deviation of the mean values which are not necessarily to be normally distributed.

3. Although the paper appears in a very clear and accurate English writing, some sentences are too short, e.g., line 221, and some sentences start with "And" which looks somehow inappropriate in academic English writing, e.g., lines 236 and 266.

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