Response to referee #3

We thank the referee for its positive opinion on our work. We have addressed all points raised by the referee as detailed below.

> 1. It would be nice to announce already in the title that this is a work based on field data.

The title of the manuscript was changed to “Formation of glacier tables caused by differential ice melting:field observations and modeling”.

> 2. The introduction is nice and well documented. To complete the picture of sublimation-induced patterns, it may be interesting to mention also blue ice ripples, observed in Antarctica (Bintanja et al., J. Glaciol., 2001) an more recently on Mars (Bordiec et al, Earth-Science Rev., 2020).

Thank you for the suggestion. These patterns are now mentioned in the introduction of the revised version of the manuscript.

> 3. Speaking of extra-terrestrial conditions, it would be interesting, to broaden the discussion, to make some predictions associated with planetary environnemental conditions. For example, shouldn't we expect these tables to be also present on Mars, and if so what is the expected critical rock size?

To our knowledge no glacier table has yet been observed on the surface of Mars, though the limited resolution of orbiter images could prevent such observations. However, the environmental conditions necessary to their formation would be hard to come by on Mars: first, both CO2 and water ice present at the surface do not undergo melting but sublimation (which can cause another type of ablation feature (Taberlet & Plihon, 2001)). Besides, the mechanism of differential ablation requires the glacier surface to be only partially covered with large solid blocks, which is rarely the case on Mars: the polar ice caps are not surrounded by steep slopes that would provide such boulders. On the contrary, mid-latitude glacial features (such as Lobate Debris Aprons or Lineated Valley Fills) are entirely covered by a thick layer of debris (and could even be rock glaciers).

> 4. Single column figures appear too small.

The figures were made bigger following the referee’s suggestion

> 5. Fig. 4a: the y-axis label is \Delta z, but shouldn't it be H?

Δz is a variation of altitude which is negative as the glacier surface lowers due to melting. $H(t) = -(z_{\mathrm{ice}}(t) - z_{\mathrm{ice}}(0)) = -\Delta z_{\mathrm{ice}}$ is a positive quantity corresponding to the ice ablated thickness. The distinction was made clearer in the revised version of the article.

> 6. Wind speed is discussed in different places. Being a profile, it depends on the altitude at which that speed is recorded. Or the authors may refer to the wind shear velocity u_*. Please be more precise.

The referee is right. The wind speed was measured at an altitude z_{m}=5 m. This was mentioned in the observation section of the revised version of the article.

> 7. Coefficient h_eff: I find this notation a bit misleading as it looks like an effective height, whereas it has the dimension of W/K/m^2.

Due to modifications in the formulation of the model, this notation does not appear anymore in the revised version of the manuscript.

References :

Taberlet, N. and Plihon, N.: Sublimation-driven morphogenesis of Zen stones on ice surfaces, Proceedings of the National Academy of Sciences, 118, https://doi.org/10.1073/pnas.2109107118, 2021.