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Response to referee #1: p. 1-7
Response to referee #2: p. 8-9
Response to referee #3: p. 10-11

Response to referee #1

We thank referee #1 for his careful reading of our manuscript and for his suggestions that helped us substantially improve the formulation of the model and the discussion on the physical interpretation of the mechanisms leading to the formation of glacier tables.
The main changes can be summarized as follows: as suggested we have considered a more detailed energy balance model both at the ice and rock surfaces (including latent turbulent flux, and using open data to estimate the incoming longwave radiation). This allowed us to compare more clearly the relative importance of the different fluxes and was exploited in the discussion section. We now discuss in more detail the importance of taking into account the fluxes exchanged on the sides of the rocks. We chose to remove the linearization of the problem and the analytical formula from the main text (after the modification of the model, a linearization is still possible but the final expression does not simplify and does not present much interest). We have also modified the organization of the manuscript to make a clearer separation between the observation, the model, the results and the discussion.

We have addressed all points raised by the referee as detailed below.

> **Improve the energy balance calculation at the different interfaces**
> You could use LW from Safran reanalysis (Vernay et al., 2021) available at https://t.co/h0UYFkwlML. The incoming longwave radiation is strongly depending on humidity and cloudiness so you could get a better estimation of this variable from this reanalysis.

Following this suggestion, we used the Safran reanalysis to obtain the time resolved incoming longwave radiation and we used it as an input in our model.

> Humidity data from the same reanalysis can be used to estimate latent heat flux on the ice surface

Indeed, we did not take into account the latent heat flux in our previous model. We have used the humidity data measured at the Requin AWS (which were very close to that of the Safran reanalysis) to compute at each time the latent heat flux.

> You could use a more detailed estimation of the turbulent flux as a function of the surface roughness that can be your tuning parameter. See for example Wagnon et al. (2003).

Following this suggestion, we used a more detailed modelisation of the turbulent flux with a surface roughness z_0 of the ice as tuning parameter both for the sensible and latent heat fluxes. The value best fitting our data is z_0= 0.34 mm which is in the (wide) range of what is reported in the literature. We chose to cite Brock et al. 2006 instead of Wagnon et al. 2003 who studied a glacier covered by a layer of snow.

> You could solve the transient 1D heat diffusion in the rock with a Neuman surface boundary condition given by the surface energy balance and a Dirichlet bottom boundary condition given by the ice temperature (273.15K) to estimate the heat flux at

the ice/rock interface. This model could be validated by comparison between modeled and observed rock surface temperature.

Indeed we chose to neglect the transient effect in our model. We wanted to keep our model as simple as possible with the goal of presenting a minimum model able to quantitatively describe the glacier table formation process. For this reason, we decided to keep this simple assumption as we observed that transient effects do not have a significant effect on the daily averaged glacier table formation dynamics.
For the thinner (h<0.25 m) rocks studied (1, 2 and 4) the transient effects are not perceptible on the time resolved formation dynamics (fig. 4) or on the surface temperature measured for rock 4 (fig. 6). They are however visible for rock 3 (h=1.7 m) for which we showed in the supplementary material data of the measured surface temperature and compared it to the model (fig. S5). Nonetheless, even in this case, the averaged (over the formation period) surface temperature predicted by the model is very close to the measured value (within 1°C in this case). The non linearity of the heat flux (with T_{rock}) being weak, the daily averaged ablation rate under the rock is predicted accurately by our model despite the transient effect. We have addressed the validity and importance of this assumption more clearly in the main text and with more detail in the supplementary materials.

A figure summarizing the intensity of the different flux at the different interface (air/ice, air/rock, ice/rock) would then provide a nice material to discuss the physical processes leading to table formation:
Air/Ice : Latent flux, Sensible flux, net shortwave radiation flux, net longwave radiation flux, total surface energy flux balance
Air/Rock : Sensible flux, net shortwave radiation flux, net longwave radiation flux, surface heat flux toward the rock, total surface energy flux balance
Ice/Rock : Heat flux
From this you could clearly identify what is playing a role in the difference between ice/rock flux and air/ice flux. I think this is missing in the study before developing an analytical approach.
We thank you for this suggestion. We have added such a figure including a schematic of the different heat flux at the different interfaces (fig. 5a) and average values (over the formation period) of these heat fluxes for the 4 rocks (fig. 5b and c). This was then used to discuss the physical effect controlling the table formation.

The "geometrical effect"
This effect assumes that the sensible and longwave radiation net flux at the air/rock interface is the same on the horizontal and vertical faces. I don't think this is true for incoming long wave radiation. Is this effect really needed to explain your data? If yes, you should show it by comparing your results with and without this assumption. Otherwise its existence is not really shown by the study.

The geometrical effect is an important part of our modelisation as it takes into account the 3D geometry of the problem. However the way we considered the longwave flux in the previous version of the manuscript was not well justified. In particular, as you pointed out, the longwave flux coming from the atmosphere should be received only by the top surface of the rock. However the whole external surface of the rock, including the sides (assumed at temperature T_{rock}) should emit a longwave flux. For rock 3 in particular the side surface is twice as big as the top surface and this is not neglectable. This flux has to be balanced by a longwave flux emitted by all the surfaces "seen" by the rock sides (including the ice surface, other rocks and the terrain surrounding the glacier, which is far away but represents an important solid angle). As we could not estimate this quantity in a simple manner, we chose to keep this received flux on the side as an adjustable parameter in our model. The best fitting value was 340 W/m² corresponding to an environment mean surface temperature (with emissivity 1) of 5°C which

seems physically acceptable. The turbulent flux also exchanges heat on the whole external surface of the rock (in a complex manner: strictly the flux depends on the orientation of the faces with respect to the wind and more generally on the exact shape of the rock). But as the turbulent flux is characterized in our model by a millimetric roughness length, it seems justified as a first approximation to consider a homogeneous turbulent flux on the external surface of the rock.
In order to illustrate the importance of this assumption, it is indeed useful to test our model with and without it. In fact due to error compensation, the ice ablation rate under the rock is not very sensible to this assumption but this is not the case of T_{rock}. We have added in the supplementary materials a figure showing a comparison for rock 3 between the measured T_{rock} and the two versions of the model (fig. S5). The purely 1D version of the model leads to an overestimation of 13°C on average and even reaching 150 to 200°C on sunny days which is unphysical.

Effect of wind

I think you overestimate the effect of wind on the energy balance by neglecting the latent flux which is also proportional to wind speed but of opposite sign to the sensible flux. The latent flux should be estimated (see my first general comment).

As suggested we took into account the latent heat flux in our model. For the Mer de Glace during period A and B, the latent flux had the same sign than the sensible flux (condensation resulting from a high specific humidity). We found in the literature (Hock 2005) that depending on the glacier location and time period this flux can indeed be either positive or negative.
For the rocks surface, we found the predicted latent flux to be quasi systematically negative (corresponding to evaporation). As the surface of the rocks would then quickly dry we added the condition $Q_{\mathrm{L}}^{\mathrm{Rock}} = 0 \text{ if } Q_{\mathrm{L}}^{\mathrm{Rock}} < 0$.

Comparing model and data

You should compare observed and modeled cumulated melt and not reset the comparison every day. This is especially true in this case where this is the cumulated melt that matter to form tables. The performance of the melt model cannot be assessed like this.

We followed your suggestion and plotted the cumulated melt predicted by the model on the duration of the observation.

Manuscript organization

My last general comment is that the manuscript is not well organized with mixing observation, model, results and discussion. It makes it very complicated to read and to understand all your findings. The manuscript should be re-organized with a clear separation between observation, model and results/discussion.

We have reorganized the manuscript as suggested.

Specific comments (embedded in the annotated PDF)

I would be surprise that [the sensible flux] is the same as the one for ice since is depend strongly on surface roughness that is likely different

Indeed the surface roughness of the rock and of the ice surface are likely different but in similar order of magnitude (between 0.1 mm and 1 cm). The agreement with our model was not improved in a perceptible maner by letting this parameter be free on this range (the sensible flux is linked to $\log z_0^2$). For the sake of simplicity we have made the choice to limit the number of adjustable parameters and we took the same value of z_0 for the ice and for the rock.
We have made this assumption to appear clearer in the manuscript.

What is the link between the Requin weather station and Nadeau et al. ?

This reference does not describe specifically the Requin station but used the exact same measure solution, communication technique, interface and data gestion. We thought this reference could be useful for readers interested in technical detail. We have made this clearer in the manuscript.

> The model tend to overestimate the rock surface temperature and quite a lot. The agreement with data on figure 4 is hard to evaluate, what can be considered as good ? The validation of your assumption is really subjective depending of what you consider "very good".

The new version of the model leads to a better agreement with the observation.
We have made our comparison between the observation and modelisation less subjective by discussing the error amplitude.

> Timeserie of 2019 and 2021 look different [in fig. 4]. The time resolution is not the same no?

Indeed, the time resolution of the Requin AWS was enhanced between the two time periods. This was mentioned in the revised version of the manuscript.

Updated figures

The figures updated following the comments of referee #1 are shown below.

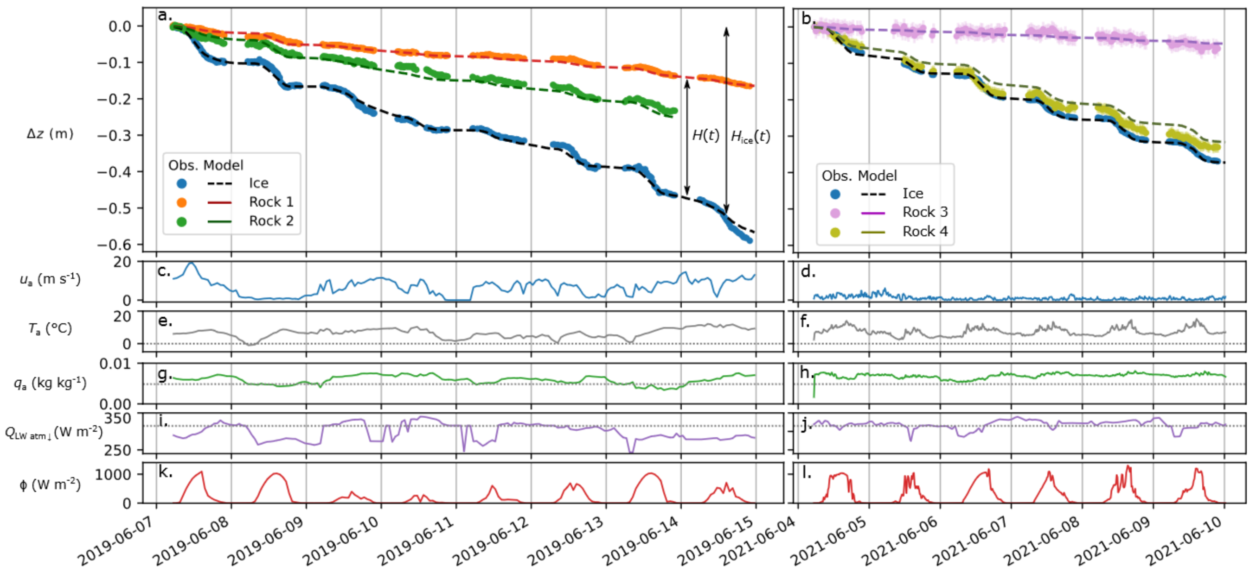

Figure 4. (a, b) Vertical position of the ice surface (blue markers), and of rocks 1 to 4 (colored markers, see table 1). The black dashed line corresponds to the model for ice ablation (see section 2.3). The colored solid lines correspond to the model for rocks (see section 2.3). These models use as input meteorological data measured at the Requin AWS: wind speed u_a (c, d), air temperature T_a (e, f), air specific humidity q_a (g, h) and solar radiation Φ (k, l) as well as the longwave radiation coming from the atmosphere $Q_{LW\ atm\downarrow}$ obtained from the S2M reanalysis (i, j). The dotted lines correspond to the ice surface values.

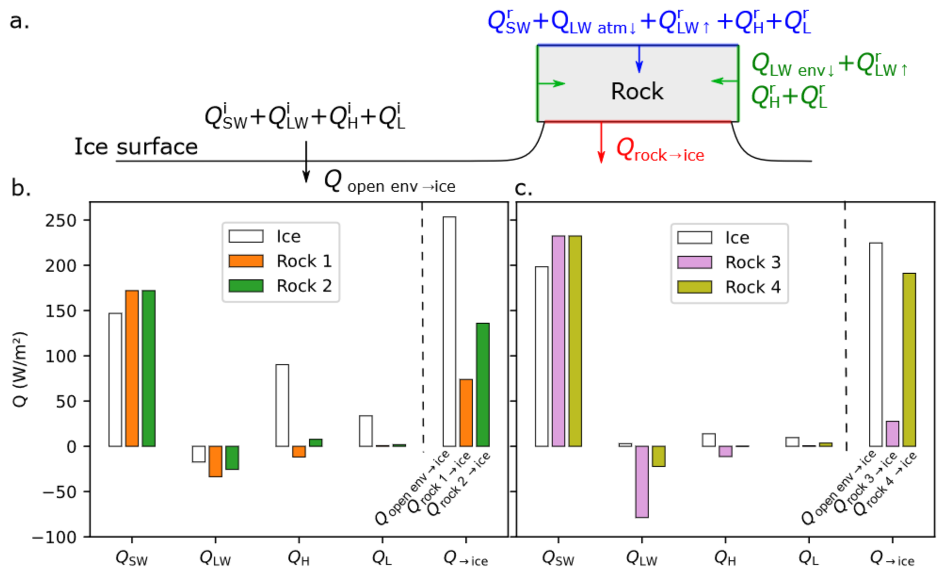

Figure 5. (a) Schematics of the heat fluxes considered in the models for the ice surface melting (left) and glacier table formation. The colors denotes the surfaces on which the fluxes are received. (b, c) Distribution of the heat fluxes averaged over the duration of observation as predicted by the time resolved model for the ice surface (white) and for the 4 rocks studied (colored).

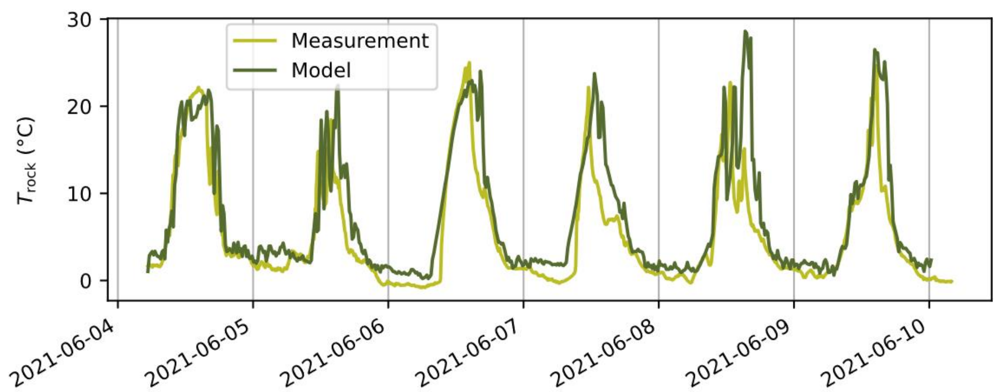

Figure 6. Temperature T_{rock} of the top surface of rock 4 measured using a thermocouple (light green) and predicted by the model using the meteorological data of period B (dark line).

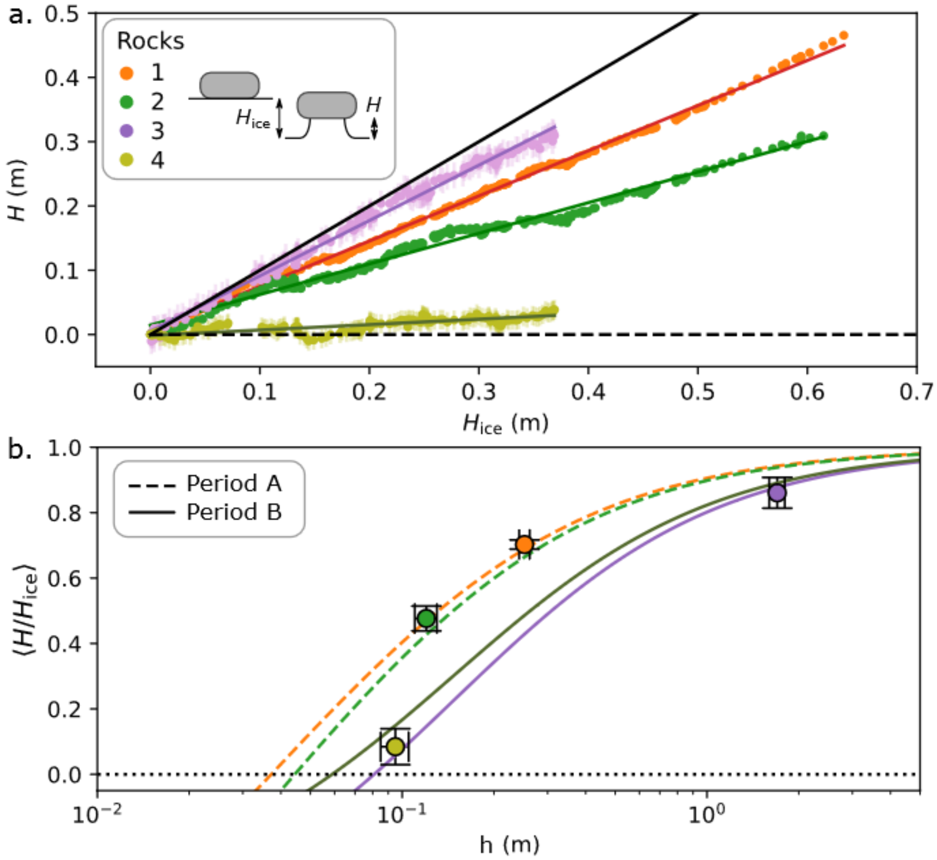

Figure 7. (a) Evolution of the height H of the ice foot under rocks 1-4 as a function of the ice ablated thickness H_{ice}. The colored lines correspond to linear adjustments of the data. The solid black line has a slope 1. (b) Mean ratio of the height H of the ice foot under a rock and of the total ablated ice thickness H_{ice} since the beginning of the table formation, as a function of the thickness h of the rock. The markers correspond to the slopes of fig. a for each rocks. The lines correspond to the application of the model for the meteorological values measured in period A (dashed lines) and B (solid lines) and the aspect ratio β of each rock given in table 1.

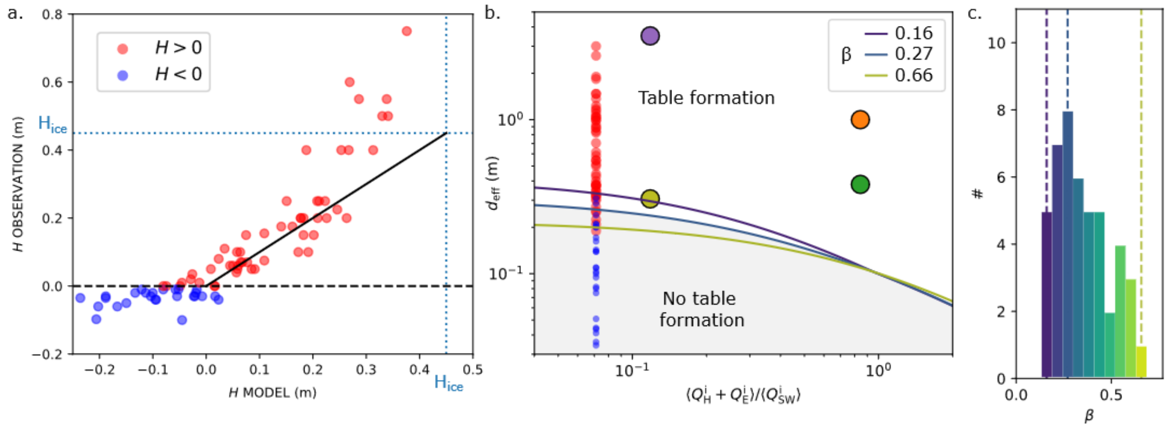

Figure 8. (a) Observation versus model for the distance H between the bottom of rocks and the ice surface for the data measured on June 3rd 2021 (see fig. 3). The data are represented in red if a table is observed ($H > 0$) and in blue otherwise. The solid black line has a slope 1 and the dotted lines correspond to the ablated ice thickness H_{ice} since the melting of the snow layer (see the supplementary material). (b) Diagram of the ability for a rock to form a table as a function of its typical width d_{eff} and the ratio $\langle Q^{\mathrm{i}}_{\mathrm{H}} + Q^{\mathrm{i}}_{\mathrm{E}}\rangle / \langle Q^{\mathrm{i}}_{\mathrm{SW}}\rangle$. The solid lines corresponds, for three values of the aspect ratio β to the delimitation predicted by the model with averaged meteorological data, between a rock forming a table and a rock sinking into the ice surface. The small circles correspond to the data of (a) with the same color code. The larger circles correspond to rocks 1-4. (c) Distribution of the aspect ratio β of the rocks close to the transition between the two regimes ($|H| < 0.2$ m). The vertical dashed lines correspond to the limits plotted in fig. b. with the same color code.

New figure in the supplementary materials

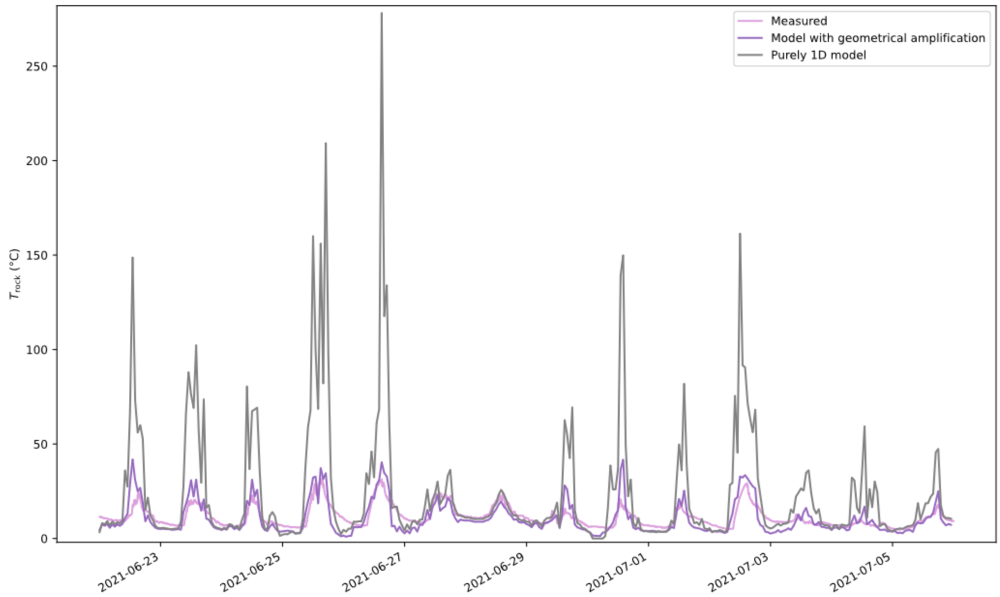

FIG. S5. Surface temperature T_{rock} of rock 3 measured using a thermocouple between the 22 June and the 6 July 2021 (light purple). The dark purple line corresponds to the prediction of the model described in section 3.2 of the main text (taking into account a geometrical amplification effect) while the grey line corresponds to the prediction of a purely 1D model ($d_{1\mathrm{D}} = h$ and all the fluxes are received on the same surface area).

References

Brock, B. W., Willis, I. C., and Sharp, M. J.: Measurement and parameterization of aerodynamic roughness length variations at Haut Glacier d'Arolla, Switzerland, Journal of Glaciology, 52, 281–297, https://doi.org/10.3189/172756506781828746, 2006.

Hock, R.: Glacier melt: a review of processes and their modelling, Progress in Physical Geography: Earth and Environment, 29, 362–391, https://doi.org/10.1191/0309133305pp453ra, number: 3, 2005.

Response to referee #2

We thank referee #2 for its careful reading and for its positive comments on our work. We have addressed all points raised by the referee as detailed below.

> The measurements show an interesting relation between boulder size and height of the supporting ice column. I suppose that the spread in the data is at least partly due to the fact that the height H will grow with time, and its maximum represents the height when the boulder topples; if that is the case, it is worth pointing out, presumably in the caption to figure 3.

The data of fig. 3 shows the height of 80 structures at one point in time when the rocks did not appear to have already fallen from their ice foot, even for the highest structures. This was made clearer in the caption of the figure. To our interpretation, the spread of the data is likely due to the influence of the aspect ratio of the rock on the formation dynamic.

> Obviously a one-dimensional theory misses the lateral melting of the ice column and thus the cause of rock collapse, and it might be interesting in future work to add a lateral melt component (which can be done in the same fashion as here)

Indeed we concentrate our study on the vertical melt of the ice under the rock and we did not consider the lateral melt of the ice foot which ultimately controls the maximum height reached by the glacier table. This was made clearer in the model description and a qualitative discussion on the effect of the lateral melt of the ice column was added at the end of the revised version of the manuscrit.

> Although the model is validated with reference to the four principal rocks, the statement at 199 that there is 'excellent agreement' of the theory with the measurements for the sample of 80 tables is disingenuous: it is obviously not. What can be said is that the agreement is good for small ice columns, but it is quite inadequate for large ice columns and for holes ($H < 0$). Indeed one of the features of figure 3 is that the data follows a fairly well-defined curve (with spread perhaps due to the comment above (?)) where on the face of it H asymptotes to ~ -0.5 m for small d or h (use h not e), and appears to become infinite (well, large) for $h \sim 1$ m or $d \sim 5$ m. It seems to me that this latter behaviour is associated with a lack of lateral melt of the column (and thus H can become very large as toppling will not occur). For a large boulder, radiative melting will disappear due to complete shading. Using the numbers in the paper, I find the short wave radiation to be ~ 220 W m^{-2} and the effective (LW and turbulent) heat flux to range from 35–126 W m^{-2} , depending on wind speed.
> Under a large boulder, only the turbulent heat transfer will provide much melting, and I suppose the column narrows more slowly, allowing growth to greater heights. Similarly for holes, for example the left most data point in figure 3a, a 1 cm pebble in a 5 cm hole will be effectively shielded both from wind and incident radiation. So in my view the correct statement is to highlight the agreement at small H, but also highlight the disagreement at large or negative H, and then comment accordingly, perhaps as above.

We agree that claiming that the agreement was excellent was too strong of a statement. Indeed the agreement and discrepancy of these data were not commented on in a clear manner. As you mention, our model is not expected to stay valid for holes (H<0) and does not take into account lateral melting which controls the maximum height reached by the ice columns. In the revised version of the manuscript we have commented in the result section (4.3) more clearly the two regimes (H<0 and H>0) and where our model is expected to be valid. We have also mentioned in the conclusion the lateral melting of the ice foot that likely controls the maximum height reached by the structure.

We have taken into account the smaller points mentioned: we have rewritten the second half of the abstract, mentioned tafoni and ice sails in the introduction and used h to denote the rock thickness instead of e.

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.