Review 6

General

R6.C1: Spatial smoothing of high-resolution surface DEM. The high-resolution LiDAR DEM has to be smoothed for the purpose of estimating bed elevation and hydraulic potential at an appropriate scale. Given the assumptions underpinning subglacial hydraulic potential analysis, I would imagine one should smooth the ice-surface DEM using a kernel large enough to eliminate features that do not influence the hydrostatic stress at the bed. I assume such a kernel would equal or exceed the ice thickness, whereas 10% of the local ice thickness was used here. A more thorough justification would be appreciated here, but even better, a demonstration of the sensitivity of the results (if any) to this kernel size (i.e., include this as an uncertain parameter in the larger uncertainty analysis, if interesting). This could be interesting and useful for other studies, now that high-resolution DEMs are increasingly available, and surface features (e.g. crevasses, rocks, moraines) that do and do not geometrically influence subglacial water flow must be distinguished.

A: This is a good remark. Following the reviewer's suggestion, we extended the sensitivity analysis of surface elevation by exploring a wider range of smoothing windows, from 0 to 100 % of the local ice thickness

In addition, we revised the Methods to incorporate this smoothing parameter directly into the uncertainty propagation framework. We wrote: "*Unlike zb and f , the uncertainty in zs is not represented through random perturbations, as it arises from the choice of a moving-average boxcar filter applied to the surface DEM. This filter is intended to remove small-scale roughness not expected to influence the subglacial hydraulic head, and its window size, k, is expressed as a fraction of the local ice thickness (set to 10 % in the deterministic case). We account for surface uncertainties by generating an ensemble of surface DEMs, zs,k, with k ranging from 0 % to 100 % in increments of 10 %. For each realization, the value of k is sampled uniformly from this discrete set*."

This sensitivity test is now included in the Discussion (Fig. 5 and Table 1):

"*The total volume of water pockets within the surface uncertainties is* $123 \cdot 10^3 \pm 32 \cdot 10^3\ m^3$*. This variability reflects the range of smoothing windows applied to the surface DEM (from* $161 \cdot 10^3\ m^3$ *without smoothing to* $67 \cdot 10^3\ m^3$ *for a smoothing window equal to 100 % of the local ice thickness). Increasing the smoothing window progressively reduces the resulting total water-pocket volume, as this smooths surface depressions and reduces local variations in the subglacial hydraulic head field. The strong sensitivity of the hydraulic head field to surface smoothing highlights a potential source of error for other studies, where surface DEMs may not be reconstructed as accurately as in this study (i.e., from high-*

resolution LiDAR), or in general when surface smoothing is applied based on arbitrary choices”

Note that the mean water-pocket height of the stochastic ensemble in Fig 4 is lower than our previous computations, as it now includes smoother surfaces.

R6.C2: Range of f and l_f in uncertainty evaluation. Since flotation fraction f and correlation lengthscale l_f impart much greater uncertainty in water-pocket volume than the bedrock and surface elevations as examined, I am curious about the choices of ranges of each. What are the results for, e.g., f=[0.65,1.05] instead of f=[0.9,1.1]? The former seems more realistic to me than the narrow range (symmetric about 1) tested. Similarly, what are the results for l_f=10m as a minimum instead of 50m? Does this choice matter? 10m seems like a more reasonable lower bound from borehole studies, as was cited in the paper. Related, I found some of the statements on page 15, lines 361-369, surprising: “f=0.9 reflects relatively low water pressure...” and “f=1.1 represents a pressurized drainage system...”. The latter is certainly true, but this would only, to my knowledge, occur in isolated pockets, rather than characterize a reasonable assumption about the drainage system itself.

A: Thanks for this comment. As stated in the manuscript, we agree that the chosen range of *f* is somewhat arbitrary. Because *f* is not constrained by observations in our case, we deliberately chose a relatively narrow range symmetric around 1, i.e, centered around the Shreve assumption used in the deterministic case. In general, the goal was to show that even small perturbations in *f* already have a strong impact on the resulting water-pocket volumes. We believe that this goal is achieved with the selected values.

Regarding the spatial correlation length *l_f*, we confirm that smaller values lead to even larger total volumes. This is because shorter correlation lengths generate more localized anomalies in the hydraulic head field, increasing the number of hydraulic barriers. This behaviour is already discussed in the section “*Uncertainties in subglacial water routing and total water-pocket volume*”.

That said, we agree that values of *f* > 1 are likely to occur only locally rather than represent the drainage system as a whole, and we clarified this point in the revised text (see below). We also recalled that the physical quantity of interest for the routing model is the hydraulic gradient, not the potential as such:

“ *Here, spatial variability in f is used to capture the contribution to the hydraulic gradient due to the unknown spatial variability in basal water pressure (see Eq. (2)). The arbitrary range of f values chosen in this study spans a limited spectrum of the values encountered in reality: f = 0.9 reflects relatively low water pressure where the subglacial system likely channelizes, and f = 1.1 represents a pressurized drainage system with water pressure larger than ice* *overburden pressure, which may occur when subglacial water input exceeds outflow. Such conditions are considered realistic prior to and during the June 2024 flood at La Bérarde – at least locally – given the high water input from rainfall and snowmelt in the glacier catchment. Values of f > 1 have been locally observed for short periods in borehole water-level measurements (Harper et al., 2005; Huss et al., 2007; De Fleurian et al., 2016; Rada and Schoof, 2018) and have been reproduced in physically-based models of the subglacial drainage system (Werder et al., 2013). Under these overpressurized conditions, water could spread across the bed through linked subglacial cavities (e.g., Kamb, 1987) and/or flow upward through moulins or crevasses connected to the subglacial drainage system, eventually causing overflow at the surface. However, note that the quantity of interest to the routing model is the gradient of the hydraulic head, and therefore a value of f > 1 does not necessarily translate into a statement that the water pressure is greater than the ice pressure; rather, it states that the gradient downstream of a f > 1 area is larger than what the Shreve potential (f = 1) would predict. "*

Minor:

R6.C3: I find it bizarre that an outburst from a subglacial water body formed from high geothermal flux is a GLOF, but an outburst from a subglacial water body formed without high geothermal flux is called a WPOF. Am I reading this right?

A: this is correct: we initially wanted to distinguish subglacial lakes under ice caps (e.g. in Iceland) to water pockets in alpine mountain glaciers, as their mechanism of formation and filling are different. This definition was presented in a paper we published last year (Ogier et al, 2025: https://doi.org/10.1017/jog.2025.43).

R6.C4: Would be nice to state here that it was not possible to close the water budget (so sad!) as is stated later in the paper (not til 13.292). At this point the reader may wonder why the water budget, or at least the total water volume of the event, is not used as a clue.

A: Thanks for this comment. We added in the section "The June 2024 La Bérarde flood": "*However, the simulated flood volume cannot be directly compared with the observed flood volume, as the water budget cannot be closed because of large uncertainties in discharge estimates, which are caused by both the limitations described above and the failure of the gauging station at Les Étages during the flood. Nevertheless, the model provides useful information on the timing of the discharge response, indicating that the abrupt rise observed at Les Étages during the night of 20–21 June (before the station failed) cannot be explained by precipitation and snowmelt alone*".

R6.C5: Rather than, or in addition to, reporting 50 MHz as GPR antenna frequency, report wavelength in ice (which will be different than in air). It would be nice for the reader to have this and basic antenna characteristics reported here (antenna type, dipole lengths if applicable) even though the system is described elsewhere.

A: We added this precision: "*The GPR system (PulseEkko, Sensors & Software) was operated with two orthogonal bistatic dipole antenna pairs at a central frequency of 50 MHz. This frequency corresponds to an electromagnetic wavelength of ≈3.4 m in ice and represents a compromise between the targeted ice-depth reflections and spatial resolution..*"

R6.C6: Moving average filter on surface elevation (see also query above). What is the kernel shape: boxcar, Gaussian? What is the spatial scale that defines the "local" ice thickness here?

A: We use a boxcar kernel shape. Local ice thickness means the thickness at that pixel. We added these two precisions in the text: "*We applied a moving-average boxcar filter to the surface elevation that is equal to 10 % of the local ice thickness at each grid cell*".

R6.C7: \psi_S = \psi? Not sure what subscript S means here

A: thanks for pointing out this mistake. Indeed, we write \psi (The "S" initially stood for "Shreve" but we had abandoned this notation).

R6.C8: Fig 2. Hard to see the difference between blue and black lines in (c). Please change/enhance attribute difference between the two.

A: We thickened the blue lines in Figure 2c (note also the new colorscale for pannel d).

R6.C9: Two closely spaced reflectors. Here it would be relevant to know the GPR wavelength in ice and the typical separation of these reflectors. I would also suggest giving these features a descriptive rather than genetic name (e.g. "double/multiple basal reflectors" vs "subglacial water reflection") given the difficulty of interpreting such features and the expectation that the bed is fully temperate (i.e. likely to have some amount of water everywhere).

A: Thanks for the suggestion. We added in the Method the wavelength in ice (following the comment above). We also revised the text as follow: "*Figure 2c shows the locations where the radargrams display double basal reflections, i.e., two basal reflectors on top of each other that are vertically separated by typically a few meters (after time–depth conversion using a constant electromagnetic wave velocity in ice). We interpret these reflectors as the ice–water and water–bedrock interfaces of subglacial channels (e.g., Church et al., 2021)*".

We changed in Figure 2 and 3 "*subglacial water reflection*" to "*double basal reflections*"

We also added a precision on why we do not expect ubiquous reflection from the bed: "*Note, however, that the absence of strong reflections in the radargrams does not necessarily imply* *the absence of subglacial water, water pockets or empty cavities, as the signal could be substantially attenuated by water inclusions (Ogier et al., 2023) and/or debris (Santin et al., 2024). This likely explains the large absence of specular reflections in the GPR profiles (grey lines in Fig. 1c)."*

R6.C10: This is because the modeled… derived from hydraulic head". I think the narrow nature of the suggested pathways is also a consequence of the D8 algorithm (steepest gradient) chosen. Alternatives that partition flow between all down-gradient cells (e.g. D-Inf) would produce less concentrated/narrow pathways.

A: Thanks for the addition. We modified accordingly: "*This is because the modeled paths represent preferential flow directions derived from the hydraulic head field using a steepest-gradient (D8) algorithm, rather than actual subglacial channel geometry*".

R6.C11: I appreciated the presentation of the two scenarios. That was thought provoking. It made me wonder about the observations of supraglacial lake drainage in Greenland (Stevens et al., 2015) and whether nonlocal drainage (e.g. through a crevasse that suddenly connects to the bed) could induce supraglacial lake drainage via the flexure caused by the antecedent drainage producing crevasses in/near the lake basin.

A: Thanks for the comment and for the reference. We used it to illustrate the possible connection between the supraglacial lake and the bed in "Scenario one":

"*For instance, such a hydraulic connection can form through hydrofracturing triggered by increased basal water pressure (Stevens et al., 2015), which induces basal slip and/or uplift and promotes tensile stresses within the ice".*

R6.C12: "zones of low hydraulic gradient" AND low hydraulic potential (local potential highs can have low hydraulic gradients).

A: Here we refer to the "hydraulic gradient" rather than the "hydraulic potential", as it is the gradient that controls water routing. Shallow gradients in the hydraulic potential field—whether from high to moderately high values or from low to very low values—generally have a limited capacity to drain excess basal water compared to steep gradients.

R6.C13: "linked cavity drainage system". I think a connected drainage system is all that is required for this argument. Is there some reason the morphology of the system must be linked cavities instead of whatever other inefficient drainage system?

A: Thanks for this comment. We used "linked cavity" as a classic view for the inefficient drainage system, but indeed any others inefficient components (such as small conduits) make this point valid. We change "linked-cavity drainage system" to "*connected drainage system*"