Dear Editor,

Thank you for handling our manuscript. We would also like to thank the reviewers for their prompt and constructive reviews. We provide a PDF in which all text modifications are highlighted (additions in blue, deletions in red).

Below, we respond to the reviewer (**R**) point by point and summarize the main changes. The letter **C** is used to refer to the reviewer's comment (e.g. **R1.C1** for the 1st comment of reviewer #1). In our answers (**A**), the revised text as implemented in the updated manuscript is written in blue italic. Unmodified text is also shown in italic, but in black.

Christophe Ogier, on behalf of all co-authors.

Reviewer 1.

R1.C1: Fig. 2: In the axis there aren't X and Y, there should be East and North

A: We changed x and y to East and North, respectively, for all figures with maps (Fig 2, 3 and 4)

Reviewer 2.

R2.C1: Pre-flood vs post-flood topography: The study relies on post-flood data to model pre-flood states, specifically using a June 28 DEM for surface elevation and an October 28 DEM to derive bedrock elevation. This approach assumes that the surface and bedrock conditions did not change enough between June and October to invalidate their hydraulic barrier models. However, a massive flood – including the drainage of an estimated 100,000 m^3 supraglacial lake – occurred in the interim. If the topography indeed remained largely changed (which would be rather curious), this justification should be explicitly stated in the text. If they indeed did change due to the event, that physical alteration should be accounted for. I would expect to see an estimate or discussion of this in the manuscript.

A: Thanks for this comment. The October DEM has been used to compute the bedrock elevation (which is assumed to remain unchanged over time), and we used the 28 June DEM to compute ice thickness and hydraulic head for the flood event. We acknowledge that the one week period between the flood (21 June) and the acquisition of the surface DEM (28 June) constitutes a potential limitation to accurately model the hydraulic potential *at the time* of the flood. However, we assume these changes in surface elevation to be negligible for our study. We explained this by adding the following paragraph in the subsection "Glacier surface topography and ice thickness measurements" (in Methods):

"Although the June DEM was acquired one week after the flood event, we expect negligible changes in glacier surface elevation during this short interval because surface melt was

small. Localized surface changes may have occurred in the vicinity of the supraglacial lake, for example due to crevasse formation associated with the lake's subglacial-drainage, although there was no clear evidence of such crevasses after the event. Filhol et al (2026) shows that between 2024 and 2021 , the largest surface elevation differences occurred at the exact location of the supraglacial lake, with a cumulative lowering of approximately 15 m. This ablation hotspot is likely explained by progressive melt related to the development of ice cliffs (i.e., increased melt where cliffs steepen and become debris-free). While a similar process might have occurred in June, we assume it to be negligible over the course of a week."

Note that we added the reference of the companion study that describes the flood in general terms (Filhol et al ,2026), since this is now a preprint in NHESS.

R2.C2: Limitations of the steady state model: The reliance on a steady-state model to simulate a highly dynamic event is a major limitation, which the authors also recognize. While a steady-state approach provides an effective way to estimate maximum potential storage geometry, it fundamentally falls short of simulating the dynamic, time-dependent processes of the actual flood (e.g. cavity opening, ice creep, or a rapid reorganization of the drainage system). A detailed explanation of this model selection – including its specific pros and cons – earlier in the manuscript (e.g., in the Methods or Introduction sections), rather than being reserved primarily for conclusion (or as a potential future step if I understood it correctly), would be beneficial to the manuscript.

A: We thank the reviewer for this suggestion: we agree on this point. The limitations of the steady-state approach and the justification for its choice are now discussed earlier in the manuscript: we moved (and slightly developed) the relevant paragraph from the Discussion section (formerly "Future work") to the "Methods overview" subsection. Here below is the paragraph added in Method Overview:

Our numerical approach intentionally represents a steady-state configuration of the subglacial drainage network and does not aim to reproduce the highly dynamic, time-dependant physical processes occurring during the flood event (e.g., enlargement of subglacial channels, ice creep, and transient reorganization of the drainage network). While physically-based models capable of representing some of these processes exist (see Flowers, 2015, for a review), their outcomes would be highly sensitive to stochastic, small-scale processes related to water routing that are neither directly observable nor straightforward to parameterize. The application of such models to real alpine glacier settings remains strongly limited by data availability (such as exact bedrock topography) and the difficulty of constraining initial and boundary conditions at the required spatial and temporal scales. Therefore, our approach focuses on estimating the maximum potential *storage capacity of hydraulic barriers given the geometric inputs available for Glacier de Bonne Pierre.*

Because of this restructuration, we removed the subsection *Future work* and we redistributed its content in the sub-subsection "*Outlook: observations needed to discriminate between the two scenarios*". This paragraph now gathers both the currently missing observations and the types of future observations that would help better understand the processes involved (see the revised manuscript version for its full extent). At the beginning of this paragraph, we recall the speculative aspect of our estimated water pocket volume:

"*In scenario 1, the volume of subglacial water (≈ 150×10^3 m^3) remains speculative because no direct observations confirm the long-term persistence of a large water pocket. […]*"

We also added "*steady-state*" in the following sentence at the end of the Introduction: "*Our approach is purely geometrical and steady-state: it relies on surface and bedrock topography, […] .*"

R2.C3: The hydrological budget: Because critical river discharge estimates became highly uncertain during the intense sediment transport of the flood, the study cannot successfully close the hydrological budget. This context is vital, as it renders the 160,000 m^3 calculated subglacial volume strictly a theoretical estimate of potential capacity, rather than a measurable "missing link" definitely proven to be in the flood waters. Ensuring that this distinction remains sharply in focus throughout the text will strengthen the paper's scientific rigor.

A: We agree that this is an important point to better clarify. We now explicitly state in the section "The June 2024 La Bérarde flood" that the hydrological budget could not be closed because of too large uncertainties in discharges estimation and because of the failure of the gauging station at Les Etages. The revised text reads as follows:

"*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.*"

We also added a new table (which summarizes the deterministic and stochastic water-pocket volumes, following a suggestion from R4), in which we clarify that *"[…] these volumes* *correspond to maximum potential water-pocket volumes for each configuration and cannot be confirmed due to the lack of in situ observations*".

Reviewer 3.

A: We thank the reviewer for their comments. The main concern was the need for additional references to 1) the literature on similar flood events and 2) the equations used in the study.

1) Regarding the literature on flood events, the manuscript already includes references to recent review papers on glacier lake outburst floods (GLOFs) and water pocket outburst floods (WPOFs)(Roberts, 2005; Björnsson, 2010; Carrivick and Tweed, 2016; Emmer et al., 2022; Zhang et al, 2024, Ogier et al, 2025). In addition, the Discussion section presents examples of documented outburst events in which subglacial water storage has been suggested to contribute to the total flood volume (Walder and Driedger, 1995; Huss et al, 2007). For this reason, we did not add additional references about glacial floods in these sections. Note that, however, we added a reference to illustrate the possible hydrofracturing at the bottom of the supraglacial lake caused by increased basal water pressure (Stevens et al., 2015).

2) Regarding the equations, we believe that the manuscript already explicitly references their origin: the classical hydraulic potential formulations come from Shreve (1972) and is found in textbooks such as The Physics of Glaciers by Cuffey and Patterson (2010). The equations used to compute the volume of water pockets come from the application of the D8 flow-routing algorithm (O'Callaghan and Mark, 1984) and our own implementation in the Julia-based code *WhereTheWaterFlowsSubglacially*. This code package will be published together with this study, and can thus be cited in the future. The equations used in the uncertainty analysis are summarized in Appendix A. Consequently, we did not add additional references for the equations.

Reviewer 4:

Major comments:

R4.C1: **Attribution of the flood mechanism**

The authors should clarify more explicitly that the study does not confirm the existence of a subglacial water pocket and that the results represent potential storage capacity. It would also be helpful to discuss whether the estimated volume could realistically account for the magnitude of the observed flood.

A: We thank the reviewer for this important comment. We agree that the distinction between potential storage capacity and the *actual* subglacial water storage (not known) at Bonne Pierre should be clearly made in our manuscript. We believe that this clarification was already stated in the Abstract (*"In the absence of direct observations of water*

Pockets, [...]"), Introduction ("*The resulting volumes therefore represent a first-order estimate of the potential water storage capacity of hydraulic barriers prior to the June 2024 flood event, rather than a reconstruction of the actual filling or rupture processes associated with the flood*") and Conclusions ("*However, the lack of direct observations of water pockets and the steady-state nature of our numerical approach prevent a definitive attribution of the flood mechanism*"), but we added a sentence in the Discussion, in the subsubsection "Outlook: observations needed to discriminate between the two scenarios": "*In scenario 1, the volume of subglacial water (≈ 150×10^3 m^3) remains speculative because no direct observations confirm the long-term persistence of a large water pocket. [...]".* This subsubsection is new and was added to better structure the Discussion (see also **R2.C3**).

We also added a new table (which summarizes the deterministic and stochastic water-pocket volumes, following a suggestion from R4, see minor comments), in which we clarify that *"[...] these volumes correspond to maximum potential water-pocket volumes for each configuration and cannot be confirmed due to the lack of in situ observations*".

R4.C2: Justification of the flotation fraction

The authors should provide stronger references supporting this assumption and discuss whether values exceeding the ice overburden pressure (f > 1) are realistic in the study area.

A: We acknowledge that the arbitrary choice of the flotation fraction *f* represents a major limitation in our study. In the initially submitted version, we explained our choice mainly in Appendix. We now moved these justifications in the Discussion, in order to explicitly discuss the likelihood of having f > 1 in a more relevant location of the manuscript. We also added a reference to the work by De Fleurian (2016), who shows f ≥ 1 from borehole observation in Greenland, and the capability of numerical models (e.g. Werder et al, 2013) to reproduce spatial fluctuation of *f* with values temporarily above 1. We also recalled that the physical quantity of interest for the routing model is the hydraulic gradient, not the potential as such. The relevant text in Discussion now reads as follows:

" [...] 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 is420 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."

Moreover, we added a forward reference at the end of Methods: "This arbitrary choice reflects the lack of direct measurements of basal water pressure and is discussed in Section 5.2." This makes it clearer that this important choice is discussed later in the manuscript.

R4.C3: Limitations of the steady-state hydrological mode

The authors should expand the discussion of the limitations associated with applying steady-state models to extreme flood events. In particular, it should be clarified that transient pressure changes and rapid drainage reorganization are not captured by the modelling framework used here.

A: We added such an explanation paragraph at the beginning of the Methods, in "methods overview", where we explicitly write that we don't capture transient pressure changes and rapid drainage reorganization (cf. answer to R2.C2):

Our numerical approach intentionally represents a steady-state configuration of the subglacial drainage network and does not aim to reproduce the highly dynamic, time-dependant physical processes occurring during the flood event (e.g., enlargement of subglacial channels, ice creep, and transient reorganization of the drainage network). While physically-based models capable of representing some of these processes exist (see Flowers, 2015, for a review), their outcomes would be highly sensitive to stochastic, small-scale processes related to water routing that are neither directly observable nor straightforward to parameterize. The application of such models to real alpine glacier settings remains strongly limited by data availability (such as exact bedrock topography) and the difficulty of constraining initial and boundary conditions at the required spatial and temporal scales. Therefore, our approach focuses on estimating the maximum potential storage capacity of hydraulic barriers given the geometric inputs available for Glacier de Bonne Pierre.

R4.C4: Interpretation of GPR observations

The authors should discuss the detection limits of GPR and whether large water pockets could remain undetected due to signal attenuation or resolution limitations. This discussion is important given that the modelling results suggest the possible presence of a relatively large water volume.

A: Thanks for pointing out this limitation: the GPR acquisition indeed allowed for relatively good bedrock mapping, but was not sufficient to determine precisely (or to rule out) the presence of water pockets. We added the airborne-GPR profiles in grey in Figure 2c. Together with the bedrock reflection in that same figure, it highlights where in the glacier GPR reflection couldn't be identified. We then added two new sentences in Results (+ two references):

"*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. 2c). A denser network of GPR profiles would likely be required to resolve three-dimensional features such as a water pocket (Vincent et al, 2012) or an empty subglacial cavity (Ruols et al, 2024).*

R4.C5: Interaction between supraglacial and subglacial water systems

The authors should clarify whether hydraulic connections between the supraglacial lake and the subglacial system are observed or inferred. It would also be useful to discuss whether similar lake drainages have occurred in previous years and why the 2024 event might have been different.

A: Thanks for this suggestion. There are observations of such a hydraulic connection *after* the flood, but not *during the flood*. We clarify this in the Discussion, in the subsection "Scenario 1: Long-term water pocket formation and sudden rupture", by adding: "*Crevasses at the base of the supraglacial lake at Glacier de Bonne Pierre were observed a few days after the lake had emptied in 2024. However, it remains unknown whether such a connection between the suggested water pocket and the supraglacial lakeexisted prior to, or during, the supraglacial lake drainage.*"

Note that we already state in the section "The June 2024 La Bérarde flood" that this is the first glacier outburst documented for Bonne Pierre since the supraglacial lake has developed and drained annually: "*Since 2016, this lake has been observed to slowly drain subglacially and annually, but without any reported downstream impacts*". The description of the two flood scenarios in the Discussion presents hypotheses for why the 2024 event was different.

Minor:

1. **R4**: Terminology such as “water pocket”, “subglacial reservoir”, and “cavity” should be defined clearly and used consistently throughout the manuscript.
   A: The use of the words “water pocket”, “subglacial reservoir”, and “cavity” have been re-considered to avoid confusion and to improve consistency through the manuscript (see changes in PDF). The distinction between cavity (i.e., a space at the bed-ice interface) and a water pocket (i.e., a relatively large water-filled cavity) is now more precise in the manuscript.

2. **R4**: Section 5 (Discussion) is relatively long and could benefit from clearer sub-structure, for example by separating interpretation of results, flood mechanism scenarios, and model limitations
   A: We removed the subsection “Future work” in the Discussion because some of the content about the justification of the choice of the method is now presented earlier, in the Methods Section (see **R2.C2**). To make the Discussion more digest, we created a new Table to summarize the volumes estimates discussed in the Section, and we created a new subsubsection in the Discussion titled “Outlook: observations needed to discriminate between the two scenarios”. This subsubsection better summarizes the available and missing field observations of the 2024 event.

3. **R4**: Some figures (particularly Figures 3 and 4) are visually dense and could be improved by increasing contrast between hydraulic contours, flow paths, and water storage areas.
   A: Thanks for the feedback. We improved contrast in Figure 3 and 4 by reversing the colorscale for the water pocket heights and by adding some transparency to the related layer. These changes make shallow water lighter and increase the visual contrast with the other elements. We also changed the contour color of water pockets from purple to black.

4. **R4**: The uncertainty analysis is valuable but somewhat difficult to follow. A summary table presenting deterministic estimates, ensemble means, and uncertainty ranges would improve readability.
   A: Thank you for this suggestion. We have added a summary table (Table 1) in the Discussion which presents the deterministic estimate together with the ensemble means and standard deviation for the different stochastic configurations. We believe that this addition improves the readability of the uncertainty analysis by providing a rapid overview of the results.

5. **R4**: Minor language editing would improve clarity in several sections where sentences are particularly long.
   A: Thanks for providing this feedback. See PDF with highlighted changes for small edits.

Review 5 (Martin Lüthi):

R5.C1: A large lake forming on a temperate glacier is a quite unusual occurrence. Usually, in temperate ice, channels and cracks do no heal (refreeze) easily, and once a channel has been enlarged it will likely persist for an extended period of time, especially at the low ice thicknesses of ca 100 m. Could it be, from similarity with Tete Rousse etc, that also this glacier features cold ice, at least in some areas?

A: This is a good point. Although the absence of distributed ice temperature observations at Bonne Pierre prevents us to be definitive, there is a number of reasons why we think it is temperate. These include: the glacier low altitude, the proglacial stream observed at the glacier portal, the annual drainage of the lake through a subglacial drainage network, the ubiquous presence of scatters in GPR- radargrams (which likely prove the presence of liquid water).

The question of the annual closure of the supraglacial lake in a temperate glacier is indeed puzzling. We interpreted this similarly than an ice-marginal lake in a temperate glacier: the lake drains when the ice dam floats, likely due to water pressure locally reaching the ice overburden pressure.

That said, the IGE group in Grenoble is planning boreholes temperature measurement in 2026 to investigate the possible presence of cold ice. For now, without in situ data, we decided to focus on hydraulic barriers and not thermal barriers.

R5.C2: It would be interesting to assess how the supraglacial lake change the hydraulic potential landscape. Ponding lake water could easily create an upstream dam/barrier (in the potential) within which a subglacial lake could form. Drainage of the surface lake would then simultaneously lead to a change of the potential barrier, and let the subglacial lake drain.

A: Yes this is exact. We indeed already computed the subglacial hydraulic head with the presence of a supraglacial lake of 100’000 m^3 (see Discussion, subsection “On the possibility of a WPOF at Glacier de Bonne Pierre in June 2024”). However, we did not observe a new water pocket upstream the lake. We added in the text: “*Note that, given the bedrock and surface topography, the presence of this supraglacial lake does not produce an additional water pocket impounded by a hydraulic barrier upstream.*”

R5.C3: If I understand correctly, large parts of the glacier were mapped with Lidar twice in 2024. Did you look at the differences of the DEMs? Are there any traces of subglacial collapse features, or lowering of the surface that cannot be explained with a melt model?

A: We indeed looked at these differences (not shown in the paper), but we did not identify any melt anomalies. This is what motivated us to write in the Discussion: "*In particular, the absence of surface deformation features - such as concentric crevasses following water pocket drainage (Gagliardini et al, 2011; Vincent et al, 2012) - does not support rapid emptying of a large water pocket, although it cannot entirely rule it out*".

R5.C4: It would be interesting and instructive to show the river discharge curves, as well as results from the hydraulic modeling.

A: Thanks for the suggestion. For the hydrological analysis, we now refer to the companion study which describes the overall flood event and which is now a preprint in NHESS:

Filhol et al., 2026: "Compound Drivers and Spatial Connectivity led to the Devastating Debris Flood in the Village of La Bérarde, June 2024, French Alps https://doi.org/10.5194/egusphere-2026-971).

In the section "The June 2024 La Bérarde flood" we wrote: "*The event is described in detail in a companion study that quantifies the hydrological and geomorphological drivers of the flood and the role of sediment connectivity between the source area and the village (Filhol et al., 2026)*". We thus decided to not explicitly show these data.

R5.C5: The conclusions should be considerably shortened, and just highlight the essential new conclusions. Now it reads like an extended abstract. Be to the point, and highlight the essence of the paper, the conclusions!

A: Thanks for this feedback. We shortened the conclusion, while keeping the main outcomes of the paper (see changes in the highlighted PDF attached to our response).

Minor: minor comments are directly implemented in the revised text. Below are the most important changes:

-We changed 10^5 to 10^3 in Figure 5.

-we applied consistency for the orthograph of "water-pocket volume(s) and height(s)".

-Figures 2,3,4 have been improved: thanks for suggestions. Caption of Fig.5 is now shorter.

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 $123\cdot10^3 \pm 32\cdot10^3$ m^3. This variability reflects the range of smoothing windows applied to the surface DEM (from $161\cdot10^3$ m^3 without smoothing to $67\cdot10^3$ 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*”