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*".