

Manuscript Revisions and Response – Reviewer 1

We thank the reviewer for their thoughtful and constructive review. These comments substantially strengthened the manuscript by helping us clarify the methodological framework, expand the discussion of model assumptions, and better integrate the multiple lines of evidence used in the reconstruction.

Major comments –

1. Framework and independence of evidence: We thank the reviewer for this helpful suggestion. We substantially revised the methodology (Sections 2.0–2.4) to explicitly clarify the roles of (i) witness testimony used for model parameterization, (ii) witness testimony retained only as contextual interpretation of the event, and (iii) independent geomorphic observations (sediment deposits and dendrogeomorphic evidence). We now explicitly state (Section 2.1; lines 105–110), that witness accounts were not treated as an independent validation dataset because they informed model parameterization, thereby avoiding circular reasoning. We also clarify that field sampling criteria were established prior to examining the model results, ensuring that the geomorphic observations constitute an independent line of evidence. We also implemented the suggested conceptual workflow figure to illustrate how the different datasets contribute to the reconstruction and how each line of evidence informs the modelling framework.

2. Pre-event topography and modeling: We thank the reviewer for this thoughtful comment and for the constructive suggestion regarding historical imagery. We agree that fluvio-glacial plains are prone to channel migration and morphological reorganization, and that the Copernicus DEM postdates the 1989 event. We considered the reviewer's suggestion to approximate pre-event topography using georeferenced historical aerial imagery (e.g. trimetrogon photography), but ultimately chose not to attempt to reconstruct channel-scale evolution for several reasons that we believe also indicate the limited impact of DEM currency on our results.

First, the flood routing in this reconstruction is governed by valley-scale topographic confinement rather than by channel-scale geometry. Witness accounts and field evidence indicate wall-to-wall inundation with modelled flow depths of approximately 4–10 m, controlled by the large-scale features of the valley like valley constrictions/widenings. These first-order controls are not sensitive to the post-1989 channel migration and sediment redistribution that a pre-event reconstruction would aim to recover.

Second, the scale of any plausible channel adjustment is small relative to the 30 m resolution of the Copernicus DEM. We also considered historical Trimetrogon imagery, but found that the channel-scale adjustments visible in these images are considerably smaller than the 30 m resolution of the Copernicus DEM and therefore would not materially alter the model terrain. Adjacent Patagonian river systems have, moreover, experienced only moderate natural channel adjustment over recent decades (Bañales-Seguel et al., 2020), consistent with the assumption that valley-scale controls have remained largely stable.

Third, because the DEM reflects post-flood terrain, any erosional widening of the channel by the 1989 event would tend to reduce, rather than increase, confinement in the model relative to pre-event conditions. Our modelled depths are therefore likely conservative in this respect.

Finally, we note that both hydrodynamic models use the same DEM (Section 2.2.1), so the model-to-model comparison that forms a central part of our analysis is unaffected by DEM currency in any case. We have retained and slightly expanded the discussion of DEM uncertainty in Section 2.2.1 to make these points explicit, in line with the reviewer's alternative suggestion, rather than attempting a pre-event topographic reconstruction.

3. "Vanishing Evidence" and Proxy Limitations: We thank the reviewer; these comments help us clarify the dendrogeomorphic sampling and better integrate it with the modelled reconstruction.

- Locations relative to flood extent. Sampling targeted trees adjacent to Río Soler and front-row stands perpendicular to the valley (Table 2), i.e. within the active flood track. Figure 2 identifies all sampling locations, while Figures 7 and 8 illustrate representative field evidence. We chose not to add a separate inundation overlay because these figures collectively provide the spatial context requested. We have revised the text to direct readers explicitly to these figures.

- Strategy and sample size. Sites were selected a priori from Google Earth and Landsat imagery using the criteria in Table 2, following established dendrogeomorphic sampling protocols [citations in text]. The sample size ($n = 50$) was not picked in advance for ease of collecting and extracting a representative sample given the limited time and demanding field logistics. We state this in Section 2.4.2.

- Independence from model results. Sites were selected before the simulations were run, from a priori expected-impact reasoning [citations in text] rather than modelled outputs. Thus, the evidence was designed as an independent test of modelled flood reach. We make this explicit in Section 2.4.2.

- Scar height and orientation. These were recorded in the field but omitted from the original manuscript after we set aside the dendrogeomorphic dataset. As physical measurements independent of the ring-count statistics that proved unreliable, they retain interpretive value. We added to the report the five measured scar/debris heights and orientations, annotated Figures 7 and 8 accordingly, and compare them with modelled water-surface elevations at each location as approximate minimum-stage constraints on modelled flow depth (Section 3.4 / revised Table 3).

4. We thank the reviewer for highlighting this important issue. Section 4.1 has been substantially expanded to discuss the implications of applying shallow-water hydrodynamic models to an event that likely behaved as a dense, debris-rich flow in its upper reaches. We now explain how the evolving flood rheology influences reconstructed flow depths and peak discharge, why this effect is spatially variable, and why the reconstructed peak discharge should be interpreted as an upper-bound estimate.

5. We substantially revised Section 4.1 to place the reconstructed peak discharge in the context of other documented Patagonian GLOFs through the addition of Table 4 and accompanying discussion. This comparison demonstrates that Laguna del Cerro Largo represents one of the largest documented moraine-dammed GLOFs in the region and highlights the limitations of applying empirical peak-discharge relationships beyond the range of events from which they were derived. The discussion has also been expanded to explain why physically based hydrodynamic reconstruction may be more appropriate for exceptionally large moraine-dammed lakes.

Minor points:

- All tree species names are italicized
- Following the insertion of a flowchart to illustrate the conceptual contribution and usage of each dataset, we think that an additional table as optionally suggest by reviewer 1 would be redundant but will defer to the editor's decision if they think it necessary.

Manuscript Revisions and Response – Reviewer 2

We thank the reviewer for their thoughtful insights, positive comments, and constructive review. These insights substantially strengthened the manuscript by helping us clarify the methodological framework, expand the discussion of model assumptions, and better integrate the multiple lines of evidence used in the reconstruction.

Major Comments

1 We appreciate the observation of our oversight in giving a more detailed reporting on how the UAV data was used. After consideration, we think that presenting this dataset in its own stand-alone paper will better allow us to demonstrate the utility of this UAV derived DEM and the insights it offers. While we did use it in our mapping of geomorphic features, ultimately the inclusion of this dataset was less quantitative analytically, and therefore, we have omitted mention of this dataset for this publication.

We have additionally refined our discussion to be more data driven. Specifically, we added a ~3 paragraphs (lines 549-567) which contextualize the use of our data and how it supports our conclusions about the characterization of dam breach processes, flood wave propagation, and the respective modeling. We characterized the evolution of flood rheology, drawing attention to the how that process was captured in the sediment samples. We further discuss the topographic controls which drive those sedimentary depositional patterns.

2) Writing Style: We appreciate the reviewer's suggestion that we consider rewording some of the passages to a more appropriate writing style as is more customary to the physical sciences. We have removed unnecessary adjectives that detracted from the scientific methodology and results.

From the original manuscripts, L78-80 these lines were removed entirely in the current draft.

L168-169) wording was revised to make more accessible for diverse audiences

L255-258) The witness account was shorted to highlight the scientific contribution to our methods, and supplemental interview cited to direct interested readers to his account.

We would also respectfully like to push-back on the comment of editorializing our study. As an interdisciplinary study, we want to bring attention to the valuable contribution that non-scientific community contributors and collaborators bring to the scientific process, and there has been much work in the social sciences on the value and politics of representation of these communities in the sciences. We feel it important to not mediate between these accounts and the results drawn from them, which is why we drew from the interviews explicitly. For this reason, we hope the editors can see the value in our inclusion of quotes where appropriate from these accounts in our manuscript.

Other Comments:

L 80 – We appreciate the feedback to make this manuscript more approachable for diverse audiences. We have removed the citation and references of epistemological blind-spots (even though this is an excellent read!) and have introduced this concept more directly in our manuscript)

L233-237: We deleted this paragraph and – as suggested by the reviewer – addressed the our sediment results in the discussion to give more depth to the depositional environment and conditions.

L284 – We clarified the methodology of C14 sampling and included the limitations of the singular dating, softening wording about the take-aways.

L289-294: The relationship between grainsize and depositional environments / distance downstream was clarified (L 445)

L347-349: We thank the reviewer for pointing out the awkward wording of this portion. We clarified that this is down-valley (not down core) and added the Sincavage reference in the bibliography.

L354: We thank the reviewer for this comment but chose to not address this comment as suggested in figure 6 as none of these samples were used for the C14 dating. We added a more detailed description of the core in revised manuscript lines 560-565

L373-374: We provided three citations to similar events triggered by ice avalanche. I softened language to reflect that indeed, there may be other mechanisms which caused the outburst flood, but this seemed most supported by our evidence.

L416–420: We absolutely agree with the reviewer, which is why we have written another publication that treats this issue more directly that is currently under review. To adequately go into the depth of the policy impacts, I don't think the little space here will do justice to the complexity of the issue. I added a personal communication citation to better substantiate these claims, citing emails, and interviews with relevant authorities. I don't, however, believe we overstate the implications as we merely report what we were told by local decision-makers which is that they were unaware of larger-magnitude peak discharges and shared that there was inadequate planning and policy to mitigate risk, especially in light of the larger magnitude discharge estimates. This was a personal communication from the relevant government professional, which I cite in the body of the text (L 679).

Fig 4 caption – Thank you for pointing this out. I accidentally mixed up the models in the description and fixed it as Loic pointed out.

Figure 6: This figure has been modified to include a scale to enhance readability.

Technical Suggestions:

L49: Thank you for this suggestion, we have implemented this correction.

L80: Nightingale 2016 is no longer cited in the paper.

L237: Sincavage et al., 2023 added to bibliography

L256: An explanation of the *Nothofagus* genus is given (line 333).

L289: Campesinos are translated (L437)

L371: The in-text reference to the original report was reworded and includes a proper citation to the report (L588).