Rebuttal to Referee #2

General comments

This article presents an analysis of the cryospheric components of the Copiapó watershed. This watershed is located in arid region of Chile, this region is considered one of the richest in ice-rich features, hence understanding the spatial distribution of cryo-landforms and their potential significance for local communities relying on water resources is very relevant. However, the overall aim of the study is not clearly articulated. In the abstract, the authors state that their goal is to quantify the water volume contributed by distinct cryoforms to the regional watershed. At the same time, they propose to categorize cryospheric reservoirs within sub-watersheds, while also introducing the concept of a Normalized Cryospheric Index (NCI) as a novel framework for "cryospheric watershed classification." Given these multiple objectives, the authors should more explicitly define the central purpose of the study or better articulate how these components are connected.

We appreciate the reviewers advice and have refined both the Abstract and Introduction to better articulate the overall aim of the study. The revised manuscript now states that our primary objective is twofold: (i) to quantify cryospheric water storage and short-term resources across the main cryoform classes (snow, debris-free and debris-covered glaciers, rock glaciers, protalus lobes, and gelifluction slopes), and (ii) to synthesize these components within a unified Normalized Cryospheric Index (NCI) framework that enables comparison and prioritization of sub-watersheds in arid mountain regions.

Additionally, the revised version integrates new results derived from surface velocity analyses of rock glaciers, protalus lobes, and gelifluction slopes, which serve as a dynamic and physically consistent magnitude for evaluating the activity and hydrological potential of periglacial landforms. These velocity fields, obtained from DInSAR and Offset Tracking, complement the ice volume estimations and provide an additional quantitative dimension for assessing cryospheric water reserves.

By combining ice-volume estimations with kinematic indicators, the NCI now incorporates both static (storage) and dynamic (flow/activity) cryospheric parameters. This integrative approach allows identifying sub-watersheds where active permafrost-related landforms may exert a stronger hydrological influence, refining the prioritization of cryospheric watersheds in arid Andean basins.

In general, the manuscript presents some interesting and potentially valuable ideas; however, it lacks organization and important clarifications, and some of the applied methodologies require re-evaluation. in order to strengthen the coherence and focus of the manuscript. For these reasons, I recommend that the paper be considered for publication only after major revisions. I provide further details in my specific comments below.

We have reorganized the manuscript for clarity: (1) Methods are expanded and ordered by cryospheric class; (2) Results present volumes/thicknesses before spatial distributions; (3) Discussion now separates storage vs. resource roles and integrates hydrological evidence. We also re-evaluated and justified all methods, adding sensitivity/uncertainty ranges for each class.

Methodology and results

In general, the use of the terms "reserve" and "resources" is not always straightforward, and at times the distinction is difficult to follow. Nevertheless, I find the introduction of the NCI to be a very interesting and innovative contribution. The manuscript applies several techniques and methodologies to estimate water volumes; however, these approaches require more careful evaluation and justification, as many of them rely on assumptions and overlook relevant previous work.

We now define "reserves" (long-term frozen storage: glacier ice, permafrost-related landforms) and "resources" (short-term/seasonal snow and melt) at the start of Section 2. We have added a consolidated methodological table linking each cryospheric class to its corresponding model or dataset (Farinotti, 2017, 2019; Gärtner-Roer et al., 2014; Cicoira et al., 2020; Jones et al., 2018), together with the assumptions and uncertainty ranges. For rock glaciers, we combined the empirical approach of Jones et al. (2018) with the physically based viscoplastic model of Cicoira et al. (2020) using DInSAR-derived surface velocities. The Cicoira model was applied under different temperature scenarios (−2 °C, −5 °C, and −8 °C) to evaluate the sensitivity of permafrost creep and ice-thickness estimations, with −5 °C selected as representative of Andean permafrost conditions (Monnier & Kinnard, 2015). For protalus lobes and gelifluction slopes, we used an adapted form of Cicoira's equation constrained by field-based thickness ranges (10–20 m and 5–10 m, respectively) and ice-content fractions (25–49%) from Hilbich (2022) and Schrott (1996), calibrated with DInSAR velocities to improve the physical consistency of the ice-volume estimates. These methodological refinements allow for a more robust and spatially consistent quantification of cryospheric ice volumes and associated uncertainties within the Copiapó watershed.

The periglacial and glacial landform inventory is primarily based on the work of García et al. (2017). However, this earlier inventory could be improved by incorporating more recent guidelines, such as those developed by the IPA Action Group on Rock Glacier Inventories and Kinematics. Re-evaluating García's inventory using the updated techniques and methods proposed by this initiative would be highly desirable. Furthermore, regarding the debris-covered glacier inventory, it is not entirely clear which conceptual framework was applied to define this glacier type. Was Kirkbride's definition adopted, or another classification scheme? In addition, the uncertainty of the inventories has not been addressed. There are well-recognized studies that provide methodologies for estimating such uncertainties (e.g., Paul et al., 2013; Braun et al., 2019).

We agree and have aligned our inventory to the IPA Action Group guidelines (terminology, attributes, and kinematics flags for rock glaciers). For debris-covered glaciers, we explicitly state the conceptual framework (Kirkbride-type debris mantling over glacier ice) and mapping rules. We also added an uncertainty assessment following Paul et al. (2013) for outlines (buffer-based error propagation) and Braun et al. (2019) for elevation/mass-change context; these uncertainties are propagated to volume estimates.

The quantification of ice volume (reserves) is somewhat unclear. On the one hand, in Section 3.2 (Survey of cryospheric water reserves), the authors state that ice thickness for debris-covered glaciers was estimated following Farinotti et al. (2019). However, in Section

3.5.2 they indicate that area–volume scaling was applied, while in Section 3.5.1 you mention the use of the physically based model proposed by Farinotti et al. (2017). It is therefore not evident which method was ultimately employed in your analysis. In any case, I would strongly recommend relying on the physically based model, or even using existing outputs from that approach, rather than area–volume scaling, which has been shown to systematically over- or underestimate ice volume.

Thank you — this has now been clarified and made fully consistent throughout the manuscript. The quantification of cryospheric ice volumes follows a unified methodological framework that integrates both physically based and empirical models according to the type of landform. For debris-free glaciers, we use the physically based model of Farinotti et al. (2017), validated against the consensus ice-thickness dataset of Farinotti et al. (2019). For debris-covered glaciers, we did not apply area–volume scaling. Instead, we extracted the mean ice thickness along the frontal and lateral margins of each debris-covered glacier directly from the Farinotti et al. (2019) raster and multiplied it by the debris-covered area to estimate total ice volume. This approach remains anchored to a physically based field while conservatively representing the debris-covered portion, avoiding the biases of scaling relationships. The associated uncertainty is estimated at ±20–30%, reflecting spatial variability in the Farinotti raster and sensitivity to the selected margin window.

For rock glaciers, we combine the empirical relationship of Jones et al. (2018) with the physically based viscoplastic model of Cicoira et al. (2020), calibrated using DInSAR-derived surface velocities to capture realistic creep behavior. The Cicoira model was applied under three temperature scenarios (−2 °C, −5 °C, and −8 °C) to quantify the thermal sensitivity of permafrost deformation, adopting −5 °C as representative of Andean permafrost conditions (Monnier & Kinnard, 2015).

For protalus lobes and gelifluction slopes, we implemented an adapted form of Cicoira's viscoplastic equation, constrained by thickness ranges of 10–20 m and 5–10 m, respectively, and ice-content fractions of 25–49% derived from Hilbich (2022) and Schrott (1996). DInSAR velocities were also used to calibrate permafrost creeping rates, improving the physical realism of the ice-volume estimates for these landforms.

All conflicting statements were removed, and the revised manuscript now presents a single, transparent methodological pipeline linking each cryospheric class to its corresponding model, dataset, and uncertainty range. This unified approach enhances the reproducibility and hydrological interpretability of the cryospheric ice-volume estimates for the Copiapó watershed.

Why did you not use the airborne GPR measurements collected over glaciers in the study area, specifically Del Potro and Tronquitos glaciers? These data were obtained during a joint Chile–Germany field campaign funded by the Dirección General de Aguas in 2013, where ice thickness measurements were acquired (DGA, 2014). Incorporating these observations would significantly strengthen your analysis, as they could be used either to constrain the model parameters or to validate the modeled ice thickness and volume estimates.

We agree and now incorporate the DGA (2014) airborne GPR profiles over Del Potro

and Tronquitos glaciers as independent validation datasets. Modeled ice thicknesses were compared with along-track GPR means. They provide an additional benchmark for parameter calibration in physically based models and further support the spatial consistency of ice volume estimates across cryospheric units.

We fully agree that the airborne GPR measurements collected by DGA (2014) are a valuable source of independent observations. We did not use them to constrain the model itself, but we actually used them as an external validation point for the resulting volumes.

Glacier	RGI ID	Farinotti model volume (km^3)	DGA 2014 GPR volume (km^3)
Del Potro	1.715.087	0.362	0.293
Tronquitos	1.715.038	0.157	0.092

These DGA (2014) volumes were used to provide a magnitude check. At Del Potro, the modeled volume is ~24% larger than the GPR-based estimate; at Tronquitos, the difference is larger (~70%). This spread is within the expected uncertainty range for individual glacier inversion products in Farinotti et al. (2019), particularly for small glaciers with limited thickness constraints.

Estimating rock glacier ice volumes is highly challenging, as values can vary between 10% and 90%. Accurate estimates generally require the use of geophysical inversion models (e.g., 4Phase or similar; Halla et al., 2021). This important consideration should be discussed in detail, which is currently missing from the manuscript. Similarly, the assumed ice thickness in gelifluction and protalus lobes (5–10 m) may be over- or underestimated if not supported by prior evidence; in fact, the results presented suggest higher ice content (up to 15 m). Ice-rich mountain permafrost can also occur in other, less typical cryospheric landforms, such as block and talus slopes or terraces, which may likewise be underestimated in the current analysis (Köhler et al., 2025).

We now expand the discussion on rock-glacier ice-content uncertainty, referencing geophysical inversion approaches (e.g., 4Phase; Halla et al., 2021). Our estimates combine Cicoira's viscoplastic model (with DInSAR-derived velocities) and the empirical approach of Jones et al. (2018), and we report ±25% uncertainty for rock glaciers. For protalus lobes and gelifluction slopes, we applied an adapted form of Cicoira's viscoplastic equation constrained by thickness ranges (10–20 m for protalus lobes and 5–10 m for gelifluction slopes) and ice-content fractions (25–49%) from Hilbich (2022) and Schrott (1996). DInSAR velocities were used to calibrate

permafrost creeping rates, improving the physical consistency of ice-volume estimates for these landforms.

Regarding snow (resource): If the authors aim to evaluate the water volume of the cryospheric components, a key aspect to consider is snow depth. Without estimates of snow depth and its spatial distribution, the assessment of total water reserves remains incomplete. There are two initiatives currently working with similar datasets in the Andes, and previous work has already addressed this topic (e.g., Saavedra et al., 2018). Please review these initiatives and earlier studies for comparison, as they also include methodologies for estimating uncertainties. Moreover, previous glacier mass balance estimates should be considered, as they provide insights into potential contributions to runoff. This addition would strengthen the manuscript analysis and discussion, especially since earlier studies have reported neutral or slightly negative rates (e.g., Braun et al., 2019; Dussaillant et al., 2019).

We have added a subsection on snow depth and spatial distribution, discussing available Andean approaches (e.g., Saavedra et al., 2018) and associated uncertainties. We complement snow cover/persistence with depth-related estimates where feasible and propagate uncertainty.

Another important aspect missing from the manuscript is the inclusion of runoff data to validate the assumptions presented. Again, if the stated goal is to quantify the water volume contributed by distinct cryospheric landforms to regional watersheds (lines 11–12), a more comprehensive description of the hydrology is necessary. Some of the sub-watersheds are well equipped with gauging instruments (Water directorate of Chile), which could help assess potential contributions. But, without including groundwater analysis, the link between cryospheric components and their contribution remains incomplete. Once more, the manuscript leaves many open questions and unresolved aspects because the overall purpose of the article is not clearly defined.

Thank you — this issue has been fully addressed in the revised version of the manuscript. We now include runoff analyses from DGA gauging stations (2008–2020) at the sub-basin scale, as well as a detailed assessment of the hydrological behavior at the Pastillo gauging station, located at the confluence of the three main upper sub-watersheds. The Pastillo station provides the most representative record of the basin's integrated discharge, reflecting the cumulative contribution from snow, glacier, and permafrost sources. By comparing these runoff records with high-altitude meteorological data from the Iglesia Colorada station, we show that several flow peaks occur in the absence of liquid precipitation, confirming a cryospheric origin of baseflow sustained by meltwater from snow, glaciers, and permafrost.

Additionally, we have incorporated an analysis of the hypsometric curves of the cryospheric sub-watersheds, which helps illustrate the topographic control on the distribution of frozen storage and meltwater generation areas. The hypsometric characterization strengthens the connection between elevation-dependent cryospheric processes and observed discharge responses at Pastillo.

This new hydrological and geomorphometric evidence is explicitly linked to the Normalized Cryospheric Index (NCI), allowing a clearer distinction between quick

(snow and glacier melt) and delayed (permafrost-related) hydrological responses. We also include a discussion of groundwater–permafrost interactions as a mechanism explaining sustained baseflow during dry periods. The revised aim and structure now clarify the hydrological context of the Copiapó basin and directly connect cryospheric metrics to observed flow dynamics and topographic controls, resolving the ambiguity noted in the original version.

Line-specific comments:

106: The concept of Cryospheric reserves and resources is interesting. However, sometimes less is more. My original suggestion was to retain the term cryospheric components and avoid introducing additional terminologies. After reading the manuscript, I notice these definitions are used throughout the text, and while generally acceptable, some instances may be unnecessary. For example, on line 129, I would simply use cryospheric components.
We accept this suggestion: in line 129 and similar instances we now use "cryospheric components"; definitions of reserves/resources remain only where essential (Methods, Discussion).

133: It is unclear what you mean by "in this review." Please clarify.
We have reviewed the literature on exposed glaciers and rock glaciers and provided a summary here. We have removed that part from the phrase, which now reads: 'Existing studies predominantly concentrate on exposed glaciers and rock glaciers.'

139: Be aware that Peña and Nazarala (1987) observed the driest year on record, which explains why 67% of the total discharge was reported.
Ok, we have added this notion to the phrase:
'A comparable investigation was conducted for the Maipo River, revealing that in the absence of snowfall, exposed glaciers may contribute as much as 67% of the total discharge (Peña and Nazarala, 1987), a percentage that was recorded in the driest year on record.'

142-143: There are several more recent studies relevant to the Alps that should be cited (e.g., Ciccoria et al., 2019; 2020).
We have added the suggested Alpine literature (Cicoira et al., 2019; 2020 and related works) to strengthen the broader context of creep mechanics and periglacial dynamics.

146 / Table 2: The table caption is not correct. I suggest including an additional column indicating which cryospheric component(s) or landform were evaluated in each study. Also, note that Ayala et al. (2016) was conducted during the Megadrought (Garreaud et al., 2017), and Ayala et al. (2020) provides a long-term estimation, offering a more comprehensive glacio-hydrological perspective. Peña and Nazarala (1987) focused on a single extremely dry year, so be cautious when presenting numbers without climate context.
We corrected the caption, added a column specifying the evaluated landform(s), and annotated climate context (Megadrought period; long-term vs. single-year studies). Values are now discussed with proper climatological framing.

151-156: This paragraph is somewhat confusing, as the different cryospheric components appear mixed. Consider reorganizing for clarity.
The paragraph was rewritten for clarity, grouping components by response time (snow → glaciers → permafrost) and aligning with the NCI logic.

156: Please provide the reference for the study mentioned; it is not clear which work you are citing.
We added the missing citation and checked the entire section for complete references.

161: Standardize the punctuation between periods and commas for consistency.
We have reorganized this phrase to:
'For the Dos Lenguas glacier, Halla et al. (2021) concluded that it has an ice content of 1.71 (± 42%) - 2 (± 44%) × 10^9 kg with an interannual water exchanges of -36 mm yr-1 (-8.92 × 106 kg) and 28 mm yr-1 (6., 64 x 106 kg).'

166-170: Use debris instead of detritus for clarity and consistency with cryospheric terminology.
Ok, we have changed 'detritus' to 'debris' throughout this part of the text.

211: It is unclear which cryoform was measured here. Were only gelifluction slopes measured, or were rock glaciers also included? Please clarify.
We clarified the field targets and specify which landforms were measured at each site; where relevant, we distinguish gelifluction lobes from adjacent rock-glacier bodies.

368-398: Why is this section titled Glacier and Periglacial Environment Inventory Results if these results were obtained previously? Were they already published? Please clarify. If this is a new presentation, justify it.
The reviewer is correct, in that these are not inventory results, but results of our mapping. We have changed the name of the title to:
'Glacier and Periglacial Environment Mapping'