

The Cryosphere Discuss., author comment AC4
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Reply on CC2

W Brian Whalley

Author comment on "Comment on "Ice content and interannual water storage changes of an active rock glacier in the dry Andes of Argentina" by Halla et al. (2021)" by W. Brian Whalley, The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-88-AC4>, 2021

Dear Authors. Thank you for your comments

Regarding your first point, I appreciate that your detailed work refers to a single feature. By implication however, your findings refer to the general study of water storage in glaciers and rock glaciers. Thus, your study becomes a part of an overall appreciation of water content in South America and needs to accommodate a variety of findings under slightly different climatic conditions – as you are arguing for a zonal (or morphoclimatic) interpretation.

- I appreciate your view (third point) that, 'the assessment and discussion of the origin of a distinct rock glacier or landform should be based on on-site specific geomorphological characteristics (form, process, and material) of the landform. Indeed, I recently (Whalley, 2021a) I suggested that it was necessary (though geomorphological mapping) 'to recognise and link materials (M), 'processes' (P, that is mechanisms integrated over time) and visual categorization and geometrical information (G). In principle, this information, i.e. site metadata, can be collected and a database interrogated to maximise geomorphological knowledge'. I suggest above (points 6 and 9) that it is the rheological (dynamic) properties of a feature and related to the materials, that account for the forms seen. In this it is necessary to look at the connectivity of material movement downslope and the origin of both water/ice and solids. Further, that other examples in the literature, which can be seen on Google Earth, do show rheological properties that are consistent with a glacier ice core (for valley floor rock glaciers) or a substantial snow/ice mass that has been buried by copious debris supplies from above – which is the case at DL. As mentioned above (6) ice that collected in the vicinity of [-30.2429,-69.7747] has moved downslope and now lies buried under the debris in the snout lobes. That there are no 'glacial deposits, like moraines' as 'traces of a former glacier' is rather easily explained; the rock glacier deposits *are* the moraines. A transect {-30.24316,-69.77959,255} shows a distinct (right) lateral moraine of a former small debris-covered glacier, with its main ice collection area at about [-30.2429,-69.7784]. This small glacier was clearly overwhelmed by the ice and sediments of the ice rock glacier of DL.
- It is arguable whether science should be conducted according to inductive or deductive principles (see Ayala (2009) for basic discussion related to Darwin). Goudie and Viles (2010) argue for an abductive view in the construction of ideas and models but in order

to overcome 'prejudices and conditioning' the 'critical rationalist approach' of Karl Popper should be used to 'attempt to disprove rather than verify our hypotheses' (Schumm, 1991). In other words, and in this case, alternative viewpoints are not only acceptable but to be welcomed (12, supra). Thus, my observations of meltwater pools in a wide variety of instances in the literature, which show that ice melting is not 'iso-volumetric' supports a massive ice origin. A theory should make predictions that can be tested. I suggest that meltwater pools will be seen on DL around [-30.2479,-69.7850] in the next ten years to become like [-30.2413,-69.8542] to which it is topographically similar and functionally related.

- I shall not argue about your geophysical results – which was not my intention in the first place – and referee 2 (supra) has already commented on these. However, you state that DI should be considered as a 'talus rock glacier'. I have no difficulty with the terminology only that it must necessarily be 'creeping permafrost'. Some authors e. g. Evin et al. (1997) have argued for 'hybrid models' and Monnier and Kinnard (2015) have discussed 'glacier-rock glacier transitions' and Jones et al. (2019) present water content evidence from a variety of rock glacier models. More investigations are clearly required.
- With respect to 'surface texture, the geomorphological characteristics and spatial connection of the rock glacier to the upslope are recommended proxies for visual observations' (IPA, 2020) I have here outlined some reasons for considering the characteristics at DL (and elsewhere) as indicative of glacier flow. However, the IPA document presents a major misunderstanding of the nature of rock glaciers by concentrating on kinematics rather than dynamics (rheological properties). Any flow mechanisms, i.e. dynamics not just kinematics, needs to consider the full implications of the materials involved. In other words, the IPA statement follows the pure Urtext (12) with not even alternatives such as hybrid or equifinality possibilities.
- I do not have space to argue my case about the IPA (2020) publication but rather point out that in stating that 'rock glacier (or permafrost) creep has to be understand (sic) here as a generic term' (p. 6) and 'Rock glaciers, as landforms resulting from a permafrost creep process, should not be confused with debris-covered glaciers'. (p. 11) it follows the 'exclusive' approach (5 supra). In particular, by assuming the dogma associated with the permafrost Urtext (12) and by ignoring the glacial/glacigenic model for which there is good evidence, it has engendered 'belief perseverance' in some sectors of the geoscience community where there is also 'confirmation bias' that has not been assuaged by showing falsifiers (black swans). That I have generated some discussion is a good thing, although I return to my original quotation from Charles Darwin on observations. But thank you for your paper and its valuable measurements.

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