Comments and responses to referees and authors.

ref 1

My response:

Thank you for your remarks.

1. My commentary is based on observations via Google Earth imagery. This makes it possible for any reader to look at the field evidence and surrounding areas. Charles Darwin noted, 'How odd it is that anyone should not see that observation must be for or against some view if it is to be of any service' (Ayala, 2009). This quotation highlights issues in the philosophy of science and the nature of evidence both of which I touch upon in my responses hereafter. I have numbered the main points sequentially for the benefit of the reader.

2. My original comments, and indeed my responses posed here, are intended to show readers the field evidence as I see it; 'it is essential to the scientific process that any hypothesis be ‘‘tested’’ by reference to the natural world that we experience with our senses' (Ayala, 2009).

3. Although it may not be 'possible to determine the origin of rock glaciers', the reviewer acknowledges that my argument is 'sufficiently convincing' to warrant using the glacigenic model for the Dos Lenguas (DL) rock glacier. My comments are based on observations from various glacier-rock glacier landsystems in the in the area. I chose to illustrate it with one specific example, but I fill in some more detail in my responses to others below.

4. In the responses I use the following convention to help readers identify locations on Google Earth (GE) by pasting in the numbers in the GE search bar between square parentheses. Thus, Dos Lenguas (DL) can be identified as decimal latitude and longitude [-30.24664,-69.78667]. A transect along the 'fall line' on the feature starts at the top with the last term (260) being a bearing in degrees from the preceding couplet as origin: {-30.24235,-69.76730,260}. This decimal degree convention is more useful to georeference features at various scales and transects for recording purposes than the traditional ˚ ' ". See Whalley (2021a, 2021b; collated references are at the end) for illustrations about the notation for studying rock glaciers elsewhere.

referee 2

Thank you for your comments. I fill in some detail here in direct response to your remarks (other information is provided below). I have tried to keep these succinct and directly related to what is particularly pertinent.

5. As my comments were primarily about field observations (see 1, supra), I only included two papers about the rheology of ice rock mixtures. It is the mechanical nature of the mixture model (rock/ice-snow/water/air) that determines the rheology. A thin glacier (<30m thick, slope angle ca 10˚, with an ablation-reducing debris cover) will flow at rock glacier velocities, < 1 ma^{-1}. However, talus (scree or rockfill) as an 'ice sparse' composite will not flow unless the ice content is high (perhaps >60%) and in thick (≈ 20 m) deformable bands or

lenses. The geophysical signature of a rock glacier at any location depends upon the field-mixture model, as well as the volume examined, given its inhomogeneity and anisotropy. The permafrost model correlates geophysical signatures to a formational mode for all rock glaciers (i.e. exclusively of non-glacier origin, see 17 below). My commentary suggests there is directly observable field evidence for a glacial origin for the deforming ice at DL. But, as Lliboutry noted (1990) of a comment by Haeberli (1989), 'I do not deny that many (not all) rock glaciers are below melting point at depth'.

6. Why don't all the slopes in the area show flow-features when, in a known permafrost area, there are plentiful scree slopes? The answer is that they will do so only if there is a thick enough body of ice, as a glacier in a conventional sense or with a thick snow/ice body covered with debris (5). On cliffed slopes with snow avalanching, this can be achieved if perennial snow accumulates (and is buried, perhaps sequentially, under debris). This is the point made by reference to rheology in Whalley and Azizi (2003) and the mixture model (see 5). It is the creep of massive ice, not rock debris – even if this is in a permafrost area. Permafrost is not necessary, but it is sufficient to keep creep rates lower than at ice pressure melting point. As an illustration, the transect, 1: {-30.2423,-69.7670,260} down the centre of DL rock glacier can be compared with a parallel transect, 2:{-30.24908,-69.76338,270}. The latter, some 700 m to the south of 1, is representative of much of that mountainside and must be under the same environmental conditions, temperature, snowfall and ablation, as the rock glacier, 1. However, transect 2 shows no signs of flow. The reason must lie in the 'mixture model', debris from the upper slopes has covered a perennial snowpack of a 'buried glacieret', 'buried debris-rich glacieret' or 'glacier enterré' (Lliboutry, 1961; Lliboutry, 1990). That there is no glacier/glacieret remnant showing at 1 is because the thick ice mass necessary for flow is covered with debris from above. The top of this original, small and confined, glacier would have been under the cliffs in the vicinity of Google Earth locality [-30.2429,-69.7747] and fed down gullies higher on the slope. Extant equivalents can be seen at the top of gullied south-facing slopes in the vicinity of [-30.23512,-69.83599]. The glacier and its protecting debris load have now crept downhill and formed the DL rock glacier. A short transect {-30.24318,-69.77858,160} for about 150 m, i.e. some 250 m east of the Halla et al. 'root zone' transect, is lower in the centre (by 5-10m) from the edges. This shows that ice had flowed out of this area and has not been replaced. This effect is similar to other rock glaciers with extending flow regimes (Whalley and Palmer, 1998, Whalley, 2021b).

7. Observations using GE brings to light further changes in surface topography of rock glaciers, notably the appearance of pools that show melting of ice below the surface debris. Recent coverage by GE shows meltwater pool exposures are becoming increasingly common. Ridges and furrows, piled up in lower (snout) regions are the result of basically compressive glacier flow with debris loads becoming increasingly thick near and at the snouts. This inhibits melting further from upstream amounts (where the debris load is thinner). Glaciers and rock glaciers may exhibit extending flow where, usually on steeper slopes and perhaps more restricted valley sections, transverse ridges and furrows are replaced by irregular or longitudinal features. Meltwater pools can form variously in them according to local topography and thickness of the debris cover.

8. These meltwater pools can be of considerable size, that shown in my Fig 1 at [-30.2413,-69.8542] has a water area of about 3 000 m^2 and has been in existence at least between 2006 – 2019 (from GE imagery). The total 'missing' volume of rock glacier is some 40 x 10^3 m^3, suggesting that the mixture model is predominantly of high percentage (massive) ice from a buried glacier tongue. This is commensurate with the sides of a 'thermokarst depression'

shown (Figure 4) of Trombotto-Liaudat and Bottegal (2020) at Morenas Coloradas debris-covered glacier [-32.9426,-69.3988] although the exact location is not given. Other long-lived meltwater pools can be seen up-valley to the exposed glacier at Morenas Coloradas, further examples can be seen in some of the images in Janke et al. (2015). Whether rock glaciers extend back into visible debris free and debris-covered versions (as suggested in the classification of Janke et al. (2015)) depends upon the relative inputs of glacier ice and weathered debris over time. The Colina Mountain example (Janke et al., 2015, Fig. 21B) [-34.3428,-70.0492] has a continuum of classes of debris-covered glacier/rock glacier with surface forms that include meltwater pools [-34.3437,-70.0486] & [-34.3494,-70.0583] and lateral erosion of pool with an exposed glacier ice cliff [-34.3571,-70.0718].

HAEBERLi

Thank you for your comments Wilfried.

9. Please note that I said, 'The geophysical data supplied by Milana and Güell (2008) and Halla et al. (2020) will be useful in the interpretation of these factors in glacier/rock glacier formation ...' In other words, evaluating the nature of the 'mixture model' that should be applied to the rheology (6, supra) will be helpful in establishing the geophysical properties and variability in rock glaciers. I am well aware of the range of geophysical results available from rock glaciers and why they can be so variable (acknowledged by Referee 2) and noted this in my original comment. This is also part of the review of the mixture models provided by Whalley and Azizi (1994) and I do not propose to discuss this variability here as my point was, and is, to look at visible forms and how they might inform us as to the origin of rock glaciers. The rheology gives the landform and its details, not the variable geophysical signature.

10. I am also aware of Gruben glacier/rock glacier and its ice-dammed lakes and the so-called 'periglacial part'. But readers should note that an interpretation of that rock glacier landsystem suggests that the rock glacier *does* have a glacier ice core (Whalley, 2020). It is no different from the observations of glacier ice cores in rock glaciers that have been recorded over the years from many parts of the world, for example; Kesseli (1941), Potter et al. (1998) and more recently Whalley (2021b). No amount of geophysical pleading can refute these observations. It is for time, as more meltwater pools are exposed, and readers to evaluate. A rough calculation (see 8, supra) shows that such meltwater pools are from the decay of massive glacier ice – which is what was the case at Gruben (Whalley, 2020).

11. It is certainly true that boreholes and exposures do show the complex nature of ice and debris in rock glaciers, see for example Janke et al. (2015) and Jones et al. (2019), especially near rock glacier snouts. Because of the increasing surface debris loads down-valley, ice exposures tend to be hidden by debris. However, some snout collapses can be seen in GE, such as at Glockturmferner (Austria) [46.89846,10.65058], compared with earlier views (Kerschner, 1983). Lliboutry described a section in the one of the four 'glaciers enterrés' below the west face of Cerro Negro (Andes of Santiago). The exact location is unknown but is in the vicinity of [-33.1484,-70.2367] (Lliboutry, 1961, Fig. 1). The section (Lliboutry, 1961 Fig. 4) and (Lliboutry, 1965 Fig.17.21) shows complex relationships between ice; young, old bubbly and bubble free ice together with silt and pebbled bands. This is more complex than the section shown by Trombotto-Liaudat and Bottegal (2020). Figure 8 of Janke et al. (2015) shows section of a meltwater pool showing banding, similar to Gruben rock glacier's drained lakes (Whalley, 2020). There is clearly much to be gained about the

structures of glaciers as they become exposed at the snouts of rock glaciers. This will help in matching geophysical attributes to structural glaciology and debris content.

12. Although there have been descriptions of rock glaciers since the early 20th C, the paper by Wahrhaftig and Cox (1959) has become particularly import in discussion about these features (Stine, 2013). Indeed, it has become the 'Urtext' for those believing the 'permafrost' origin of rock glaciers promoted by Wahrhaftig and Cox. The book by Barsch (1996) provides the stated dogma of the permafrost viewpoint. This text is followed by Barsch (1987) who denigrates many observations of glacier ice cores. Subsequently, sins of omission have followed by disregarding any other possibilities than the permafrost dogma, e.g. Swift et al. (2021). Please see Whalley (2021a) where some of these wrongs are addressed.

13. Professor Haeberli, as a true believer in the Urtext and permafrost dogma, has always maintained that rock glaciers cannot have glacier ice cores (i.e. be glacigenic). For him, this means that not only do glacier ice cores not exist but that any continuum or equifinality does not occur (pace Referee 2). Yet there are many reports of glacier ice in rock glaciers, as well as the well-established work of Potter at Galena Creek that cannot be denied (although I leave it to readers to adjudicate). Quoting many references that support a permafrost viewpoint amounts to 'affirming the consequent' (modus tollens). In terms of swans and rock glaciers, all swans are not white and at least some rock glacier swans are black and contain glacier ice cores. Thus, supposition and following a particular point of view is insufficient to replace valid contra-observations. In a Popperian sense therefore we might have to wait for contra-indications of permafrost, or affirmation of the appearance of glacier ice by meltwater ponds.

14. I have mentioned the work of the late Professor Louis Lliboutry in reporting 'glacier enterré' and in particular the complexities of snout stratigraphy. He also said (Lliboutry, 1990); 'I do not wish to enter into a public controversy with W. Haeberli about the origin of rock glaciers; he has always been deaf to my arguments. Nevertheless, the readers of his passionate assertions (Haeberli, 1989) must be aware that he intentionally omits to quote my detailed observations in the dry Andes (Lliboutry, 1955, 1965, 1986).' Further, 'Nevertheless, for the advancement of science, the essential point is not "must rock glaciers be left to scientists claiming to be permafrost specialists" but "what can we learn from the existence of rock glaciers in a given area"? I maintain that the geographical study of rock glaciers as an extreme case of glacier fluctuations, as an indicator of favourable mass balances in the past, or of past surges, would be much more rewarding than to consider them as a mere case of standard permafrost, or of creeping regolith.' (Lliboutry, 1990).

Halla et al

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.

15. 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.

16. 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.

17. 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 Dl 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.

18. 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.

19. 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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