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Interactive comment on "Ice content and interannual water storage changes of an active rock glacier in the dry Andes of Argentina" *by* Christian Halla et al.

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First we would like to thank M. Hayashi for his constructive and specific comments to improve our manuscript. Our responses to the referee comments (RC) are inserted below.

RC Line 22. Please spell 'groundwater' in one word, following the standard practice in the contemporary literature.

We will revise the spelling of 'groundwater' throughout the manuscript.

RC Line 51. Please spell out RCP8.5 and briefly indicate what it represents.

We will revise this sentence accordingly by spelling out the scenario RCP8.5 (climate scenario with the representative concentration pathway based on an additional radiative forcing of 8.5 watts per square meter by the year 2100). In current climate model simulations RCP8.5 may lead to a globally averaged temperature increase of $\sim$4°C relative to pre-industrial temperatures.

RC Line 294. Estimates of ice and liquid water fraction must be also sensitive to other poorly constrained parameters such as the p-wave velocity of rock and the three Archie parameters. Please comment on the model sensitivity and uncertainty concerning these parameters, and briefly explain how the values were 'taken from literature'.

From the Archie parameters, "a" should be set to 1 anyway as it just multiplies (the unknown) pore water resistivities (rho_w) with another unknown factor and there is no physical representation of this factor (e.g. Mollaret et al. 2020). We will change this in a revised version. The sensitivities of the other Archie parameter rho_w, m and n were already discussed in Hauck et al. (2011) and it was found that rho_w has by far the strongest influence (if not known through in-situ measurements). In our study, the in-situ measurements of rho_w from the thermokarst pond and surface snow were used to cover ranges of this sensitive parameter. We will discuss these dependencies in more detail in our revised version. The uncertainty of an unknown P-wave velocity of the rock material has of course an influence, but less than porosity and it can also easier be constrained through literature (rock type and p-wave velocity tables are generally available).

RC Line 296. I feel that 70% is a large number for a talus rock glacier. How were these values selected? Based on the literature? I am not sure how much information is in the literature, but Merz et al. (2016, Geophysics, 81, WA147-WA157) used 30% as a rough estimate, and pointed out the need for 'dependable estimates of porosity across the rock glacier' as a future challenge. As estimated ice and water volumes are highly sensitive to porosity (Fig. 7), I think that the choice of these values need to be critically examined and justified in light of the existing body of literature.

The values were selected based on observations from other studies (e.g. Arenson et al., 2002, Haeberli et al., 1998, Krainer et al., 2017) and reasoning about the potential ice-saturations for active or inactive rock glaciers. Active, i.e. presently moving rock glacier, are ice-supersatured in most parts, which would mean porosities >40% and up to 100% in case of larger ground ice occurrences (e.g. observed at Murtel-Corvatsch rock glacier). Without ice-rich, i.e. ice-supersaturated conditions the creep of frozen soils is restricted by interlocking particles (Arenson et al 2005, Arenson and Springman 2005). Observations from drill cores have shown that heterogeneous material compositions and ice content (0-100%) exist in ice-free, ice-poor, and ice-rich layers, in rock glaciers at the point scale. Therefore 30-70% porosity represent the lower and upper bounds in the present modelling case, to assess potential min/max ranges of the ice content. The 30 and 50% porosity models allow just ice saturation (< 40% absolute ice content) below or close to saturation, given that the voids in rock glaciers and the 4PM (the dominant grain size of Dos Lenguas are sands ($\sim$ 40% porosity)) contain also water and air. Therefore those scenarios and porosity assumptions would be suitable for an inactive or intact rock glacier, which does presently not move. The porosity model with 70% allow for ice supersaturated conditions larger than 40% ice content, which are necessary to assess potential ice content using the 4PM for active rock glaciers. We expected from geomorphological proxies (e.g, oversteepened front slopes) that Dos Lenguas is an active rock glacier. Now that we have evidence that Dos Lenguas is moving at rates of up to 2 m/yr, the 70% porosity is a plausible assumption for the start model to allow for potential ice-supersaturation. And yet, the model can be still regarded as conservative for ice-rich permafrost as ice saturations > 70% and pure ground ice bodies were not permitted by our porosity assumption. Porosity assumptions for geophysical models are further discussed in the new paper by Mollaret et al. (2020), where porosity is jointly inverted for and values of 30-70% are found there as well. A corresponding paragraph will be added in our revised manuscript.

RC Line 316. Mean fw and mean fi. Mean of what?

Fraction of water (fw) and fraction of ice (fi) is the terminology used for the parameter of the 4PM and is synonymously used for the (volumetric) ice and water content. The mean fraction was calculated from all 4PM model cells located below the rock glacier surface (i.e. model cells below side slopes and outside the rock glacier were excluded at profile C1 and C2). The standard deviation of the mean fraction was used for the error estimation and error propagation. The manuscript will be adapted accordingly.

RC: Please explain Line 340.

'Paired positive and negative changes' should mean that positive changes at the front of the transverse ridges and negative changes at the back of transvers ridges also show the advance of the ridge and furrow topography. The wording of the sentence will be revised.

RC: Please annotate 'central area' etc. in Fig. 4, so the reader can clearly understand which region is referred to here. Alternatively, this sentence can refer the reader to Fig. 1b.Table 5. This table is a bit difficult to understand at a first glance. It will be helpful to add vertical lines across the two tables, so the reader can relate the top table to the bottom table.

We will additionally refer the reader to fig. 1b and table 5 and add vertical lines in the table to improve the readability as suggested. Line 357. How are these average values calculated? Simple average of all grid cells shown in Fig. 4? Please briefly explain it here.

These values are calculated as the sum of vertical changes from the DEMs multiplied by the surface area of the rock glacier yielding the assumed volumetric net change of ice (density of 0.9 g cmˆ-3) per year. The ice volume is then given as water equivalent. We will adapt the text accordingly.

RC Figure 5. 'Depth' is used for the vertical axis in this figure, but it is a bit odd because depth is always referenced to the ground surface of a particular location. Please use elevation instead of depth.

The vertical axis will be changed to elevation.

RC Figure 6. The delineation of permafrost and ice-rich permafrost in SRT images are not consistent with that in ERT images (Fig. 5). Please comment on the differences in this paragraph, and discuss it again in the last paragraph of this section.

Restrictions of the resolution capacities of the two different tomographic data sets for the 4PM are discussed in the methodological discussion concerning the geophysical approach. However, we will follow the reviewers' suggestion by including a short comment on the inconsistency between ERT and SRT in the specific paragraph of the result section.

RC Line 440. Figure 7 shows the mean values of ice fraction and liquid water fraction over 'model depth', but I am not sure what the model depth refers to. Please explain how the model depth is defined.

The model area of the 4PM (see fig. 8, ice and water contents per porosity) is defined by the overlapping part of the two model grids of ERT and SRT for each profile and corresponds to the maximal depth penetration of the seismic waves due to their smaller investigation depth than ERT in our case. The model depth relates therefore to the vertical extent ('model depth' below surface) of the 4PM cells along the profile (x-axis/distance). This explanation will be added in the revised version.

RC Line 445-446. High porosity values are used for the mixed porosity model. Please my comment on Line 296.

See response to RC Line 296.

RC Line 482-484. Liquid water saturation and ice saturation are used in Fig. 8, instead of water content and ice content. This way of presenting the spatial distribution of water and ice is a bit misleading, because the reader cannot actually see the amounts of water in 'aquifers'. For example, 'aquifers from adjacent talus slopes' are in the

bedrock, not in talus sediments. In the bedrock, water saturation is high due to low porosity, but water content is small. For the discussion of water storage, it is more meaningful to show water and ice contents. Please revise the figures.

We agree that for the discussion of water storage the absolute quantities matter most. However, absolute values derived from the 4PM have still a quite high potential error due to the unknown porosity. Therefore, we used the different scenarios to model and evaluate reasonable ranges of saturations. All absolute values (given as mean along the profile) based on all porosity scenarios and all profiles are shown in Fig.7. The mean values of the scenarios that were evaluated as most reasonable were used to estimate the ice storage capacities and the amount of water and calculate absolute values of water and ice storage (table 6), which was potentially present during the measurements towards the end of the thaw season. In Fig. 8, we showed content per porosity (and in all plots of the supplements) to better display the heterogeneous material compositions using the same colour bar (scaling) for all fractions and all 4PM plots. Fig. 8 is used here only to visually indicate potential hydrologic structures. To compare water storage values of the different scenarios the reader can use fig. 7 (or is referred to the supplements). Showing all profiles as both, absolute and saturation based images of the fractions would take up too much space. We will explain the difference between the two types of plots and the associated uncertainties more clearly in the revised manuscript.

RC Line 490. The lines delineating aquifers and aquitards are similar, and not easily distinguishable. Please use more distinguishable line types. These diagrams show aquitards that look like vertical chimneys. It is hard to imagine how such vertical aquitards could form in rock glacier. A more plausible explanation for the presence of shallow perched aquifers (e.g. x = 150 m in C1) is the horizontal aquitard (i.e. permafrost) underneath the aquifer.

We will revise the line types to better distinguish the interpreted structures. Regarding the second part of this comment: You are of course right, that the interpretation of these vertical features in Fig. 8 is highly speculative, given also the inherent uncertainties of the underlying inversions and corresponding 4PM results. However, if we assume these features to be reliable, the formation of vertical water pathways could be related to the long-term evolution of the ridge and furrow structures and to the percolation of water in voids, channels, or crevasses/fractures between ice-rich permafrost. At first, different source areas of different talus slopes developing into single flow lobes might play a crucial role in the long-term evolution of the extensional ridge and furrow topography since ice-rich permafrost bodies of the developing lobes are separated by ice-free or ice poor outer edges before the different lobes flow together (if there is enough compression) or the lobes creep side by side. So, water could percolate into the ice-free or ice-poor voids between ice-rich permafrost. Second, if not separated from the beginning, the formation of surface crevasses and the separation of the two tongues shows that different creep velocities could also produce vertical fractures (if tensile stress is sufficient to cause brittle failure) and split off ice-rich permafrost in transverse ridges. The voids or filled (ice-free) fractures between ice-rich permafrost could then also function as 'vertical' flow path for water. Yet, given the speculative nature of the above, we will revise the figure and consider the rock glacier permafrost generally as aquitard during the thawing season and just mark areas were increased water saturations indicate percolation or water pathways between ice-rich permafrost (intra-permafrost flow). In the specific location (x = 150 m in C1) that would be below a perched aquifers, like it is marked (local aquitarde below an aquifer) in the figure.

RC Line 547-550. Please note that high water saturation does not necessarily indicate an aquifer. To qualify as an aquifer, the material needs to have high enough porosity and permeability. Please see my comment on Line 482-484 as well.

The reviewer is right that our use of the terminology has been somewhat misleading. In order to prevent jumping to speculative conclusions we will use the alternative terminology of water pathways with potential supra- , intra-, and sub-permafrost flow, (as suggested by Jones et al. 2019 for hydrological flowpaths in rock glaciers) in our

revised manuscript

RC Line 503. Gruben rock glacier abruptly appears here. Please provide the context. The same applies to the Galena Creek rock glacier in Line 505.

The context will be provided in the revised manuscript. To our best knowledge, the only studies that estimated mass balances for rock glaciers were done at those sites.

RC Line 673. How is this number calculated from Table 6? Please explain the steps and assumptions. It seems that the range of uncertainty in this number is unrealistically small in light of all the uncertainties in model parameters, as well as the geophysical data inversion. Please provide an explanation as to why the number can be so well constrained.

The number gives the range of the two scenarios that were qualitatively evaluated as most reasonable (L462-466). The range is based on the mean ice content along the profile that were extrapolated to the surface area and the depth of ice-rich permafrost bodies (see also section starting L306). The Uncertainties of the numbers are given in Table 6 and the result section, but can also be included in the text of the discussion (1.71($\pm$42%)-2.00($\pm$44%)x 10ˆ9 kg). The idea was to use only the mean values without the uncertainty ranges to compare the different water and ice storage estimations and thereby avoid too many comparisons and numbers of different estimates in the discussion. Of course, the mean values of each scenario can be extended to min and max values to show the larger ranges of the estimates. We will make this reasoning more clear in the revised version.

RC Line 681. I cannot follow the conversion between mm d-1 and kg. Also, I cannot understand why 36:28 is not equal to 19.8:14.7. Please explain how these numbers are calculated.

Thank you very much for spotting this error, which is due to a calculation error of the total surface area. The correct amounts of water in kg for the surface area of 247802 mˆ2 are: -36 mm yr-1 (-8.92x10ˆ6 kg) for the first period 2016-17 and 27mm yrˆ-1 (6.64 x 10ˆ6 kg) for the second period 2017-18. Due to different volumetric changes between the first period 2016-17 and the second period 2017-18, the ratio of the two different observations periods should not and cannot be equal. The sum of volumetric changes (in m3 yr-1) was hereby multiplied by the density of ice (900kg m-3) and divided by the surface area. We used mm yr-1, which is common for net balances (1mm represents 1kg mˆ-2 or 1l mˆ-2). This paragraph will be updated accordingly.

RC Line 688-690. The difference in meteorological conditions between the two seasons is casually discussed here, referring to a supplemental figure. I feel that this topic is central to the main objective of the paper and deserves more attention. For example, what was the difference in precipitation? What was the actual difference in mean air temperature during the two thawing seasons? Please expand the discussion and demonstrate a clear link between the meteorological condition and estimated storage change (negative in 2016-2017 and positive in 2017-2018).

Thank you for this of course very relevant comment! Unfortunately, there are no reliable precipitation data (rainfall and snow) available for the mentioned relevant parameters in the observation period 2016-2018. Mean monthly air temperatures of the meteorological stations are available and were used for the mean annual temperatures given in the study site section (see table 1). However, as we recorded ground surface temperature directly at Dos Lenguas, we think the trend of the temperature regime is better displayed by the in-situ ground surface temperature than by interpolated air temperatures from meteorological stations located in different topo-climatic positions. Therefore, it is not possible to demonstrate a clear link between the meteorological condition and estimated storage change based on the available data. Based on the ground surface temperature it is only possible to make a guess that negative net changes 2016-17 are related to the earlier and warmer spring (Sep 2016) and a later and higher temperature maximum during summer (Jan 2017) compared to the thawing period 2017-18. The forced warming during spring and early summer could have caused a deeper pene-

tration of the thaw depth in summer 2016-17 and yielded the negative net changes. The positive net balance in the following year (2017-18) indicates the ice loss 2016-17 could have been refilled and the active layer did not reach the depth of the year before at the dates of the measurements. Yet, these interpretations are speculative without precipitation and runoff data. Therefore, we kept the discussion on meteorological conditions short and related the potential water and ice content from the geophysical surveys to interannual storage changes from photogrammetry and potential amounts of discharge from the spring. These comparisons are of course also associated with large uncertainties, but at least they give an indication based on in situ measurements to interrelate the hydrological storages and processes.

RC Line 691. Please show the location of the spring in Fig. 1b.

The location of the spring will be added to Fig. 1b.

RC Line 691-695. I feel that the discussion on the water balance needs a bit more care, again because this is central to the main objective of the paper. For example, if the area of the rock glacier is 0.36 km2 (Line 127), then 104 x 10ËĘ6 kg is equivalent to 290 mm of water averaged over the rock glacier area. This is a large magnitude compared to annual precipitation of 50-150 mm (Line 106). What is the source of this water?

Thank you very much for your careful reading of our manuscript and the very useful comments regarding the discussion of our results. We will revise the discussion of the water balance with more care to the interpretations and uncertainties of the estimations. According to Schrott (1996) 104 x 10ˆ6 kg would be the upper bound if the spring flow is constant for 5 months with 8 l sˆ-1. You are right in saying that the corresponding 290 mm would be quite a large magnitude in this respect. Of course the data from Schrott (1996) relate to an observation period ∼30 years ago and have therefore to be used with care when comparing it to present day conditions. The major additional water source (to precipitation) could be the contribution area of the rock glacier, which delivers water, snow and debris to the rock glacier. The calculated surface area feeding into the root zone of the rock glacier is 1.01 km^2. The contribution area of the surface area of the rock glaciers is $\sim$ 2.7 km^2 (including the surface area of the rock glacier). A second source could be seasonal melt water from the active layer and permafrost within the rock glacier and the contribution area. A third source could be sub-permafrost groundwater. If we consider the total contribution area and the area of the rock glacier as the catchment area of the spring the potential discharge would corresponds to 35 mm (104 x 10^6 kg / 2.7 km^2). If we consider the total contribution area of the root zone (1.01km^2) and the area of the rock glacier (0.36 km^2) as the catchment area of the spring, the potential discharge would correspond to 75 mm (104 x 10^6 kg / 1.37 km^2). Both values, 35 mm and-75 mm could be plausible for the dry mountain environment and are partly in the range of the regional estimates of 50-150 mm based on TRMM data. So these numbers indicate that a large portion of amounts of annual precipitation could be released during the thaw period. So, internal storage change due to freeze and thaw could buffer the groundwater release and add or reduce groundwater flow during the course of the year.

RC: Does the spring flows only for five months (Line 692), or does it flow all year? What was the actual precipitation amounts in 2016-2017 and 2017-2018? How are 14-30% and 70-86% calculated?

During the field investigations in the 90ies the spring flow was observed from January until End of April with 5-8 l s^-1(Schrott 1996). There have been no observations during other times of the year. So it remains unknown if there could be discharge in the winter season. The ground surface temperature regime shows median temperatures >5°C for five months during summer and <-5°C during winter (S17). The cold temperatures make it unlikely that there could be discharge in winter. Unfortunately, there are no observations as to when exactly the discharge starts with the beginning of the thawing season. The large amplitudes of the ground surface temperatures indicate no continuous and/or only a thin snow cover during the winter season (Fig. S17). Sublimation

and partly infiltrating meltwater due to the melt of a thin snow cover (< 5cm) by strong solar radiation was observed a few times during the field work the day after precipitation events (snow and/or sleet) during summer. The drainage of meltwater from snow during winter due to strong solar radiation seems very unlikely given the ground surface temperate regime. It is rather likely that meltwater will refreeze during infiltration in winter.

Still, we don't have data for the amount of precipitation during the whole observation period.

14-30% gives the ratio of potential discharge at the spring to the amount of storage changes. Due to the calculation error of the storage change (see reply to RC Line 681), these ratios are incorrect, which is why the ratios given must be corrected. This whole section of the discussion and the corresponding numbers will be revised and the following information could be included: The mass loss in 2016-17 (8.92 x 10^6 kg, 95% confidence interval) would be 9-14% of the potential discharge at the spring (65-104 x 10^6 kg). The potential discharge (65-104 x 10^6 kg) divided by total liquid water content (min/max water content of model scenarios are 244-570 x 10^6 kg) indicates that 11-42% of the liquid water content could become discharged at the spring. Both comparisons indicate that a large portion of the groundwater and melt water could remain in the system, or could be exchanged, or could leave the hydrological system of the rock glacier through other groundwater pathways. However, it should be noted that the total liquid water content of the rock glacier may be overestimated due to the site and weather conditions during the field measurements and the sensitivity of the 4PM.

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