The Cryosphere
Discussions

Interactive comment on "Summer valley-floor snowfall in Taylor Valley, Antarctica from 1995–2017" *by* Madeline E. Myers et al.

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Received and published: 5 January 2021

AUTHOR RESPONSES TO REVIEWER 2 COMMENTARY ON MYERS ET AL MANUSCRIPT

This paper addresses an important subject of the temporal and spatial patterns of snowfall in a polar desert in Antarctica, with implications for assessing climate change and relevance to local ecosystem processes. The paper essentially updates and expands the record of snowfall in Taylor Valley. It is a worthy effort and they add a new measure - persistence of snow cover.

While I heartedly endorse the publication of the report, it has many important flaws that need to be corrected. Overall, the science is fine, but the writing needs major major

improvement, as indicated by my extensive comments below.

In short, the entire manuscript has to be rewritten paying close attention to grammar, flow, and definitions. A number of broader issues stand out.

1. Precipitation and snowfall are used interchangeably. No rain falls in this region so why not just use snowfall? By including both words, sometimes in the same sentence, the reader gets the impression that rain is ignored.

2. Some care should be taken to distinguish between snowfall and snow accumulation because they are different. Also snow cover is a bit ambiguous and is typically used to refer to snow at a point rather than across a landscape, except at the end of the report then it's used to mean the latter. I think the use of snow cover can be avoided except for meaning across a landscape.

3. Often the authors refer to snowfall volume, yet they use one dimensional units of cm. Normal practice is to refer to precipitation amounts as depth in mm or cm. I suppose the authors could refer to specific volume, which also has units of mm or cm, but why complicate things. Unless they want to calculate volume of snow in a watershed, I'd stick with depth.

4. One important issue that is glossed over is uncertainty. If the data are compared then uncertainty needs to be included or the comparison has no context. Assessment of uncertainty, of both snowfall depth and duration, should be addressed in a separate paragraph.

5. Finally, I have a problem with the notion that one station can predict the snowfall at another station the following season. Do the authors think there is a teleconnection extending the 10-20 km between stations? By what physical process explains this phenomenon. Why aren't other station pairs predictors? How well are the stations correlated? How are we to know that this predictor is not a spurious correlation?

[M, D, & M] We are very grateful to Reviewer 2 for their extensive comments. The care

taken to point out the errors is immensely helpful to the success and impact of future publications. In revising this manuscript, we will give more attention to the language used to refer to precipitation versus snowfall, snow cover, and snow volume and call it out to make it more clear to the readers. We should have used snow depth rather than volume. Better care will be given to uncertainty. We incorrectly reported the uncertainty as error and that will be fixed. A section will be added to the discussion regarding uncertainty. The stations as predictors are discussed later in the reviewer responses. Another reviewer suggested a power analysis from Von Storch and Zwiers (2001) to analyze that relationship in more detail.

P1 L4: This is only part of the reason. The other part is the strong rain shadow exerted by the Transantarctic Mountains. See, Monaghan AJ, Bromwich DH, Powers JG and Manning KW (2005) The Climate of the McMurdo, Antarctica, Region as Represented by One Year of Forecasts from the Antarctic Mesoscale Prediction System*. Journal of Climate 18(8), 1174–1189 and see, Fountain et al., 2010

[M, D, & M] We propose updating this section to reflect that the MDVs lie within a rain shadow: "The Transantarctic Mountains buffer TV from the East Antarctic Ice Sheet (Chinn, 1990) and exert a rain shadow on the MDVs (Monaghan et al., 2005), thereby allowing them to remain ice-free and with little snow accumulation. Ephemeral streams supply melt from surrounding glaciers to . . ."

P2 L22: This reference is for sea ice, a more local reference, highlighting the effects on runoff from glaciers in the Dry Valleys, including the energy balance causes, is needed.

[M, D, & M] We agree, and the reference will be changed to Fountain et al., 1999 P3 L7: A more apt citation here, based on the physics of energy balance is Hoffman et al., 2016, already included in the reference list.

[M, D, & M] We agree. The citation will be changed from "Gooseff et al., 2011; Harpold and Brooks, 2018".

P3 L29: "They excluded..." It is not clear whether this study also excluded windy events or not.

[M, D, & M] Fountain et al. (2010) include accumulation during wind speeds > 5 m s-1 were as 'wind drift' and accumulation under lower wind speeds are termed 'direct precipitation'. This comment highlights one of the previously mentioned issues with this paper regarding the language used to discuss snowfall, snow cover, precipitation, etc. Because of the filtering above 5 m s-1, this paper only includes direct precipitation. The language will be changed to reflect more clearly when we discuss precipitation (i.e., rain and snow measured by the weighted gauge) and direct snowfall (i.e., accumulation measured by the ultrasonic sensors when wind speeds are less than 5 m s-1).

P4 L1: Abrupt and confusing transition from automatic measurements to a brief study of snow? It is unclear what you are trying to measure, not density, that is assumed to be 83 kg/m3, but then density was measured. Please clarify.

[M, D, & M] We are trying to measure density. To make this clearer, the paragraph will be updated beginning on P3 L29: "...0.5 mm water equivalent (w.e.). They converted ultrasonic distance ranger measurements of snow depth to w.e. using episodic measurements of density. A lack of published snow density records and logistical constraints limited snow density measurements to December of 2018 where we recorded a density of 83 kg m-3. Fountain et al. (2010) excluded precipitation events measured when daily average wind speeds exceed 5 m s-1 which could convey snow from the surrounding peaks to the valley floor. More details on station set up and data processing are described in detail by Fountain et al. (2010). Data are accessible from the MCM LTER website (http://mcm.lternet.edu/)." The MCM LTER reports snow densities measured on the glaciers, including the dates and thickness of the layers measured. These data will be analyzed and discussed to understand how a density of 83 kg m-3 impacts the results and perceived performance of the ultrasonic distance sensor.

P4 L9: "with Winter excluded for the same reason" Same as what? "Spring begins with

first light..." When is that?

[M, D, & M] We agree that this is vague and will update the sentence to: "Our study focuses on Spring through Fall, coincident with first and last light (September 1 and April 30; Acosta et al., 2020). This puts the seasonal and interannual variability of direct snowfall in the context of primary productivity and melt generation which are governed by available solar radiation. Summer falls from November 15 through February 15. Dates coincide with statistically distinct air temperature and solar radiation conditions (Obryk et al., 2020)."

P4 L10: "ends with final sunset" When is that? "Dates coincide with statistically distinct climate conditions" Climate is a very broad umbrella, which conditions, specifically?

[M, D, & M] It is based on the establishment of a statistically significant air temperature gradient with distance from the coast which is established by solar radiation. The atmosphere shifts from stable to unstable during the winter to summer transition as the increase in solar radiation warms the soil. Solar radiation also generates the onshore breeze which aids in establishing the gradient of increasing temperature with distance from the coast after temperature has been normalized to elevation. A more detailed discussion of this phenomena will be included in the discussion section 4.3 Implications for Hydrology and Ecology.

P4 L21: 'Instrumental' rather than "meteorological"?

[M, D, &M] Yes, thank you for pointing that out. This will be updated in the revised manuscript.

P4 L29: Why Commonwealth Glacier rather than Canada Glacier, which is adjacent to Lake Hoare? And why stake 23?

[M, D, & M] Commonwealth was chosen because it has stakes in the accumulation zone of the glacier. We regressed summer snow cover against all of the accumulation stakes on Commonwealth glacier and the strongest correlation was with stake 23,

which is why it was chosen. We will include a line about this in the manuscript.

P5 L5: "Precipitation" Is this a better descriptor or is snowfall? There is no rain and all precipitation is snow, so should the general term be used? I think not, it has a vagueness to it that is unnecessary.

[M, D, & M] It should be 'direct precipitation'. It includes measurements made by the ultrasonic and weighted gauge and the weighted gauge does not differentiate between rain and snow. Rain has been reported in the DVs, as recently as two years ago. Although it is very rare, we would rather not exclude it as a possibility in the weighted gauge measurements. This will be reflected in the manuscript. The usage of the terms direct precipitation, accumulation, snowfall, etc. will be described in better detail in the introduction and this section header will be updated to reflect those changes.

P5 L7: "Again"? When was it focused previously? Also considering that the seasons are partly defined by first and last light, if I understand the methods correctly, why is a 'light season' necessary? It's confusing. Also, this sentence underscores the confusing issue between precipitation and snowfall. Here you, 'focus on snow...', but the subheading is about precipitation, so the inference is that rain is being ignored.

[M, D, & M] These are all very good points and we struggled for a while with how to make it less confusing. We do include annual snowfall to compare our results with Fountain et al. (2010), but perhaps it would be best to discuss it in terms of snowfall and only specify when the snow is associated with a particular season. We agree with the confusion between snow and precipitation and will update the entire manuscript to highlight the differences in the terminology and use them properly because 'precipitation' and 'snow' are currently used interchangeably. Perhaps we could assume everything is snow to avoid confusion because rain is very rare.

P5 L13: 2007? Does this coincide with the results of Obryk on temporal break points?

[M, D, & M] Yes, Obryk sees a shift in temperature from increasing to no trend in

2007 at Lake Hoare and 2005 +/- 1 year at the other stations. The changepoint in the direct snowfall (because we are measuring with the ultrasonic) record is not statistically significant, but the trends on either side of it are. We don't discuss this very much, and a larger discussion section relating temperature to snowfall would improve the manuscript, particularly because the results of Obryk were a key motivator for this project.

P5 L18: This sentence doesn't seem logically connected. How is seasonal variability connected with differences in atmospheric influences? What influences are being considered?

[M, D, & M] The strength of Antarctic teleconnections is dependent on the season. By separating the analysis by season we may be able to isolate longer trends specific to and independent of these signals.

P5 L20: "spatial control" or spatial difference? Controls at this point in Results are unknown, but differences are known.

[M, D, & M] Yes, thank you for pointing that out. 'Spatial difference' is what is meant here and will be reflected in the manuscript.

P5 L21: You don't really mean 'volumes' right? Then it begs the question over what area are you measuring the volume. For precipitation, depth is the normal dimension used. Furthermore, the units of mm w.e. are not volume but a linear distance, so the dimensions of volume are wrong. Also, are these values averages? Please clarify. If yes, what is the standard deviation?

[M, D, & M] We mean depth here. These values are averages. The standard deviation will be reported in Table 1 with the seasonal means and the standard error of the mean.

P5 L23: If a third of w.e. snow occurs in spring and another third occurs in autumn (two thirds, not 'totaling over half', then one-third occurs in summer. If this is right, it doesn't square with the measurements at either the coastal or inland stations. Or am I

confused?

[M, D, & M] This sentence is confusing, but we agree that it is 'two thirds' rather than 'over half' and the sentence will be updated to: "Consequently, two thirds of the total August through May precipitation in TV occurs in the Lake Fryxell basin (FRLM and EXEM) during the Spring and Fall." We will also update Table 1 to include season totals, station totals, and a Taylor Valley total to make this more apparent.

P5 L25: "Bias". This brings up a couple of good points. First, do the authors mean 'bias' as in the measurements tend to be too high or too low? Second, do the authors mean 'uncertainty'. Considering they are comparing values, to make the comparison meaningful, they need to report uncertainty.

[M, D, & M] 'Bias' is not used properly here. We should have used 'uncertainty'. The uncertainties for each season, year, and station are reported in the supplemental file. The table, however, reports it as 'error' when it should be reported as 'uncertainty'. A careful examination of how we use 'error', 'uncertainty', and 'bias' will benefit the manuscript. Table 1 will be updated to show the uncertainty.

P5 L27: Looks like HOEM has a consistently lower seasonality than BOYM. Or is my interpretation due to missing data?

[M, D, & M] You are correct. We will update the paragraph to point toward HOEM as having the weakest seasonality. " BOYM (Figure 4d) has consistently low seasonality. Individual, large (> 2 mm w.e.) snow events can govern the season-scale fraction of snowfall for that year like we see in 2007 where Summer precipitation (9.5 mm w.e.) is nearly double that of Spring (5.0 mm w.e.). Low average precipitation and the occurrence of large snow events is likely responsible for its large interannual variability. We do not see any season-specific trend in snowfall at BOYM. HOEM has the weakest seasonality of all stations (Table 1; Figure 4c) and does not show any trends in snowfall for any season although data availability is limited to the last decade."

P5 L28: Revise, "where Summer precipitation (9.5 mm w.e.) is nearly" to "when Summer...was nearly".

[M, D, & M] This will be revised. The manuscript will be checked for this error throughout.

P5 L29: Again, another example of precipitation vs snowfall, "Low average precipitation and the occurrence of large snow events". So little rain and large snowfall?

[M, D, & M] This sentence is confusing and will be changed to: "Low average snowfall means that chance heavy snowfall events contribute to the interannual variability observed in the dataset."

P6 L1: Looks to me that EXEM has the greatest variability, not FRLM. Is that due to missing data at FRLM? It looks like prior to 2003 FRLM received no snowfall.

[M, D, & M] You are correct, EXEM does have the greatest variability. The supplemental table shows no data versus seasons with 0 mm w.e. accumulation. At FRLM, there was no data prior to 2002.

P6 L6: "stations are not predictors". I assume the authors do not mean the stations are correlated because in the following sentence Spring snowfall at FRLM predicts high summer snowfall at BOYM. I would have thought that a correlation matrix between stations would be included or perhaps referred to in supplementary data to support the notion that the station snowfall is not correlated between stations (is this right?). There is no physical reason for one station to predict snowfall at a later date at another station, unless it does so at the same station. Given the stations are only a few km apart the prediction is not based on the movement of air mass systems or a teleconnection. It's just persistence in the system.

[M, D, & M] We do mean they are correlated. We will include a correlation matrix as a supplemental file. I would argue that the reason we see heavy spring snowfall at FRLM indicative of heavy summer snowfall at BOYM due to the expansion of the

coastal climate further inland. That being said, we cannot explain why this relationship is only observed for FRLM and BOYM and not the other stations. This will be given more attention.

P6 L12: "snow cover heatmap"? Awkward, revise.

[M, D, & M] This sentence will be revised to: "Figure 5 suggests..."

P6 L14: "may inaccurately portray low snow cover for those seasons". It can't portray low snow cover, because like you say, the data is missing.

[M, D, & M] We intended for it to imply that there is some data missing for those years which may make the number of days with snow cover seem lower than it actually is. This will be revised beginning P6 L13: "...Spring and Fall, however a few days of data are missing during Spring and Fall of 2006, 2010, 2011, 2016, and 2017 snow years. The increase..."

P6 L15: "more gradual" than what? No rapid increase was identified.

[M, D, & M] This sentence will be removed.

P6 L17: replace 'high' with 'long'

[M, D, & M] Thank you for pointing that out. We will check for this error throughout the manuscript.

P6 L23: "snow cover at Lake Hoare is highly variable', clarify, snow cover persistence?

[M, D, & M] As was pointed out, snow cover is typically used to refer to an area rather than a point location. This distinction will be made when describing the language used regarding precipitation, snowfall, etc. Here, the sentence will be updated to "the fraction of days with snow on the ground".

P6 L25: Delete the last two sentences in the paragraph, they don't say anything.

[M, D, & M] We agree. These sentences will be removed.

P7 L29: Replace 'ground' with 'soil' or 'rock and soil'

[M, D, & M] We agree and will use 'rock and soil.'

P8 L1: Delete the sentence about sub-ice ecosystems, it doesn't go anywhere and is a distraction from the subject of the paragraph snowfall versus accumulation. If the ecosystem issue is important, develop in a separate paragraph.

[M, D, & M] We agree. This sentence will be moved to the last paragraph on P8 L30 just after "...snow accumulation at the valley-floor."

P8 L4: Delete 'necessary'. I'm sure one could figure a work-around if needed.

[M, D, & M] We agree. This will be removed.

P8 L6: Replace 'dissipation' with 'ablation', replace 'from' with 'based on'

[M, D, & M] Both of these changes will be made.

P8 L10: Replace 'pick up' with 'detect'

[M, D, & M] We will change this.

P8 L13: The last two sentences sort of repeat the last sentence in the previous paragraph. Can the influence of high winds be more fully addressed in a single paragraph rather than partly in two paragraphs?

[M, D, & M] Yes, these 3 sentences will be moved to the final paragraph on page 8 where wind is discussed again and is a reason for coupling measurement techniques.

P8 L18: " from cooling to no trend" Awkward. Rather than a 'shift', how about "a changing trend from cooling to no trend"?

[M, D, & M] We agree. The sentence will be updated to: "This shift coincides with a changing trend from cooling to no trend at Lake Hoare (Obryk et al., 2020)." We will still start the sentence with the word 'shift' in order to align with the previous sentence where we mention a 'shift' in interannual variability.

P8 L19: Delete the rest of the paragraph starting with, 'While the reconstructed...' The ablation and melt in the ablation zone vis a vis Hoffman et al., 2016 has nothing to do with snow cover. The text awkwardly summarizes the model and at the end of the paragraph, the authors back into a suggested process, reduced snow cover. Hoffman et al., do not argue that the lack of snow causes increased melt, they argue increased sediment on the ice surface. So, I don't know the purpose of these sentences.

[M, D, & M] We agree and the rest of that paragraph will be removed beginning with "While the reconstructed..."

P8 L27: Delete this paragraph, it doesn't make much sense. It starts to make an argument for snow cover vs snowfall relevance to local ecology. But that subject is dropped, and the subject shifts to the importance of winds again. It ends with an unsubstantiated statement about the best approach to measure snowfall and snow cover. Strangely, the ecosystem argument ignores an important aspect of snowfall and snow cover, its spatial distribution. In any case this paragraph doesn't really say anything important to the paper.

[M, D, & M] This paragraph will be replaced with one focused specifically on wind. Snowfall versus snow cover will be discussed in section 4.3 Implications for Hydrology and Ecology. This paper is more focused on the temporal characteristics rather than spatial characteristics of snow in TV.

P9 L3: "lowest relative loss of snow-covered area (72%)" What does this mean? And what is 72%? Does this mean that the area of Lake Hoare lost 72% of its snow cover? On average? Or is 72% of the Lake Hoare area covered in snow?

[M, D, & M] 72% of the area covered by snow in October 2009 completely ablated by January 2010 in the Lake Hoare basin. This is perhaps a better way to phrase this sentence and it will be rewritten as: "...2009-10 snow year. They found that 72% of the area covered by snow in October 2009 completely ablated by January 2010 in the Lake Hoare basin. Ninety-three percent and 97% of the snow-covered area in the Lake

Fryxell and Lake Bonney basins respectively ablated during the same time period. Lake Hoare lies..."

P9 L4: "least radiation" nuclear radiation?

[M, D, & M] This will be changed to solar radiation.

P9 L6: "may buffer reduced persistence associated with climatic conditions" Vague. What associated climate conditions?

[M, D, & M] This should say: "...buffer reduced persistence associated with higher RH, lower temperatures, and lower wind speeds."

P9 L7: volume, not depth? And the last sentence is very confused. How does snow at Lake Hoare inform on snow at the coast? "snow likely plays the larger role..." in what? And makes its monitoring increasingly important? Why not important, why is it increasing in importance? I might argue that it is more important to monitor snow up valley where there is less moisture.

[M, D, & M] We should have used depth here. There is a spatial gradient in snow depth described by Fountain. Near the coast there is more snow, so more light reduction for subnivean primary producers. This also depends on the photosynthetic efficiency of the communities. It is increasingly important because persistence is increasing and should be monitored at multiple locations to understand why. I agree that it would be great to monitor persistence along the valley (Fryxell, Hoare, and Bonney) to understand how climatic controls on persistence vary across the landscape.

P9 L9: Delete section 4.2? It doesn't come to any substantial conclusion. Given that the region is a desert and one large snow event can change the season of maximum snow fall, clearly the statistics will be very noisy and regressions and teleconnections will be insignificant. If the authors feel that this section is important, reduce it to one small tight paragraph.

[M, D, & M] We will delete this paragraph.

P9 L23: Delete 'sea ice extent' In the previous paragraph it was shown to be irrelevant to snowfall.

[M, D, & M] We agree it should be deleted

P10 L3: Delete section 4.3. This section lightly argues for the relevance of snow to the hydrology and ecology of Taylor Valley. Unfortunately, it's not particularly insightful and the topics have been better covered by the authors in the introduction. Also, there are several conceptual mistakes in 4.3 summarized below.

[M, D, & M] The discussion will be rewritten to focus more specifically on the dataset rather than hypothesize about potential causes of correlation.

P10 L7: "high-humidity areas which will experience greater melt" These areas will melt? Surely you don't mean that. Do you mean snow in those areas? "Sublimation is the greatest contributor to ablation of snow" Not true, in most temperate regions of the world melt is the biggest factor with sublimation playing a very small role.

[M, D, & M] This is referring to the fact that in high-humidity areas the snow is more likely to melt rather than sublimate. We should have clarified that this is specific to Taylor Valley. On pp 669, Gooseff et al. (2011) says "Sublimation is the most signiïïňĄcant process ablating snow on the valley ïňĆoors."

P10 L8: "Under these assumptions, reduced snow volume and increased snow persistence will further reduce the soil moisture contribution of snow which could have mixed effects on subsurface ice and soil communities. While there would be less melt to recharge subsurface ice, the increased duration of snow cover could act as a buffer and slow ablation." These two sentences are wrong showing a misunderstanding of the heat and mass transfer of snow over soil, particularly a relatively warm snow over much colder permafrost at depth.

[M, D, & M] We did a very poor job with word choice here and we agree with a previous comment that other authors have done a better job relating snow to hydrology and

ecology. The section on Implications for Hydrology and Ecology will be removed.

P10 L29: The predictive capability of high spring snowfall at FRLM to indicate high summer snowfall at BOYM only a short distance away is odd. This appears to be a case of correlation without causation, and not examined carefully by the authors. Furthermore it is very odd that no other station pairs show this, which makes me think this is specious and not worthy of inclusion in the conclusions.

[M, D, & M] This relationship will be excluded from the manuscript.

P11 L8: This sentence is unsubstantiated by anything prior in the report and should be deleted.

[M, D, & M] We propose instead to revise this sentence to: "A continued increase in snow cover and persistence increase the albedo of Taylor Valley which slows glacial melt, thereby slowing the increase in hydrologic and ecologic connectivity predicted by the MCM LTER (Wlostowski et al., 2016)." Because Taylor Valley hovers around 0°C during the summer an increase in albedo following a snow event can temporarily stop glacial melt. We mentioned this earlier in the manuscript (P2 L21).

P11 L11: This paragraph should be deleted; it doesn't say anything substantial.

[M, D, & M] We agree and that will be removed.

Figure 1. I've always thought that no acronyms should be included in a figure without explanation, otherwise, the reader has to search the text for interpretation. I recommend AWS be spelled out too.

[M, D, & M] The acronyms and AWS will be spelled out.

Figure 2. This is a confusing figure. The caption says the monitored area is outlined in red, but I only see a red line, not a polygon, so no 'area' is outlined. After some inspection I realized it was at the bottom of the photo. It would help the reader if the photo was cropped to minimize much of the sky in order to emphasize the monitored

area. 4 'perennially melted moat'? Is it always melted? The moat is part of the lake? Unclear The last sentence is awkward, please revise for clarity

[M, D, & M] We will crop the image and add labels to the figure. The reference to the 'moat' will be removed because it isn't discussed elsewhere in the manuscript.

Figure 4 I think it is important here to show missing data. Otherwise the plot is misleading, no bar is interpreted as zero snowfall. For example, it appears HOEM had snow snow accumulation between 1994 and 2006. The bar graph to the right, are these averages? If so, what is the sample size of each? It bears on the statistical differences between seasons and between stations.

[M, D, & M] We made a version of the figure showing missing data, but it distracted from the actual data. Maybe a light gray shading would be a good way to indicate where data are missing.

Figure 6. In the legend the mean is indicated by X-bar. But X-bar would be a snow year. Persistence is on the y axis, Y-bar? Why is the resolution of the photo +/- 0.5 days? This was not explained in the Methods.

[M, D, & M] That is a good point. The figure will be updated to y-bar. The photos are daily, so the resolution is half of the measurement which would be 0.5 days.

Figure 7. (b) where is the scale for accumulation? The tick marks suggest a scale different from (a).

[M, D, & M] The scale for accumulation is indicated on the left side of (a).

REFERENCES:

Fountain, A. G., Lyons, W. B., Burkins, M. B., Dana, G. L., Doran, P. T., Lewis, K. J., McKnight, D.M., Moorhead, D.L., Parsons, A.N., Priscu, J.C. and Wall, D.H. (1999). Physical controls on the Taylor Valley ecosystem, Antarctica. Bioscience, 49(12), 961-971. Von Storch, H. and Zwiers, F.W. (2001). Statistical analysis in climate research.

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