

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 3 COMMENTARY ON MYERS ET AL MANUSCRIPT

This paper aims at describing snowfall and snow cover data that has been sampled in the Taylor Valley, in Antarctica. The protocol and the stations used to get the measurements are correctly described, and acquiring such data in this remote area is clearly a huge effort. The paper is well presented. However, the investigations based on these data are superficial, because many conclusions are not based on solid investigations, and some of them appear speculative. I would recommend to either publish this dataset in a web interface/journal with a DOI reference, or to conduct more inves-

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tigations to prepare a manuscript. In such a form, I would recommend to reject this article.

There are two major points for which the study is not appropriate to make a scientific article to my point of view:

1. The trend analysis is based on very short timeseries, a point that strongly limits the possibility to evidence any climatic trend in the area. To my point of view, however, it would be interesting to provide a study focusing on the interannual variability. Such a study would require considering more variable/processes than the snowfall rates and snow persistence that are observed by the authors. In particular, the conclusions suggesting climatic signals in this area could be based on temperature/wind/pressure data, using observations at the local scale and potentially reanalysis at the regional scale.

This would lead credence to the section devoted to the teleconnections between the polar and the tropical to middle latitude areas.

2. The other weakness of this study is related to the links between snowfall and sea ice that are mentioned by the author through all the article, whereas there is not any sea-ice data in the study. In addition, the authors claim that a sea-ice reduction is expected in this area, that would favour a precipitation increase in relation to more moisture in the atmosphere. Even if such precipitation increase is expected in Antarctica under climate change, the sea-ice did not show any clear trend over the last decades, and even a slight increase in the Ross sea

[M, D, & M] We are very grateful to Reviewer 3 for their detailed comments which will greatly improve the readability and quality of the manuscript. In its current format the manuscript reads more like a data paper. The reference to Von Storch and Zwiers (2001) will be particularly useful in future analyses of this dataset and others. The discussion section will be refocused to discuss the results rather than giving so much weight to hypothesizing about what might be causing the trends we see.

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LIST OF COMMENTS:

P2 L9: Annual mean of air temperature observed on average in TV by Obyrk et al. is -20°C , so 18.5°C seems to warm (maybe a – sign is missing?).

[M, D, & M] Yes, the temperature should be negative and will be reflected in the revised manuscript.

P2 L27: The sea-ice extent in the Southern Hemisphere has been increasing over the last decades in particular in the area of the Ross Sea (de Santis et al., 2017), so should we expect a decrease in snowfall? This should be considered in the introduction and all over the manuscript.

[M, D, & M] More care will be taken to describing the relationship between snowfall and persistence and sea ice extent. We recently compared sea ice extent to the persistence dataset and saw a correlation during the Fall. This isn't discussed and will be added to the paper near P6 L20. More attention will be given to the decreasing direct snowfall, increasing persistence, and increasing sea ice extent in the Ross Sea.

P3 L25-30: It is claimed that the observation of precipitation is considered only when the wind is not exceeding 5 m s^{-1} . But is it realistic to consider that there is no local snowfall with stronger winds? When the snow is drifted away with the wind, this does not mean that there is no snowfall, isn't it? I would expect more explanations for the situations when snowfall occurs during windstorms.

[M, D, & M] We agree, it is unrealistic to assume no accumulation under higher winds. This comment highlights an issue mentioned by another reviewer regarding the language used to discuss snowfall, snowcover, precipitation, etc. Fountain et al. (2010) categories accumulation during wind speeds $> 5\text{ m s}^{-1}$ as 'wind drift' and accumulation under lower wind speeds are termed 'direct precipitation'. They use 5 m s^{-1} as the cutoff based on the definition of katabatic/foehn winds described by Nylén et al. (2004) and conditions during these wind events preclude snowfall. Because we exclude ac-

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cumulation above 5 m s⁻¹, the snow that is reported is direct precipitation or snowfall depending on whether or not it was measured by the weighted gauge or ultrasonic sensor respectively. The language will be updated to reflect these changes.

P4 L2: Is the snow density systematically equal to 83 kg m⁻³? That sounds like a strong assumption.

[M, D, & M] It is not. The issue of snow density was brought up by the other reviewers as well. 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/>).” We feel that the accuracy of our results could be impacted by the snow density measurements and will reach out to others who may have measured it. We will also include an additional paragraph in the discussion regarding how variability in snow density impacts our results derived from the ultrasonic sensor.

P4 L9: “Winter excluded for the same reason” -> which reason? The sentence is not clear. You mean that you do not focus on the winter season because of the lack of sunlight, isn't it? What are the limitations related to this protocol?

[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.

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Dates coincide with statistically distinct climate conditions (Obryk et al., 2020).” Little is known about the physical and ecological processes which occur in winter, but they are gaining more interest in the scientific community. Winter snowfall contributes to the mass balance of the surrounding glaciers. The impact of winter snow on ecology is an ongoing study.

Results: The discussion focusing on the volume of precipitation variability appears speculative, in particular because of the shortness of the time series as well as because of the missing data. The potential links between the spring snowfall at FRLM and the summer snowfall at BOYM is far from being clear visually. Even if the correlation is significant, would it be possible that this happened by chance? I would suggest providing also a power analysis (e.g. Von Storch and Zwiers, 2001) to estimate whether such a significant correlation has been obtained “by chance”. The trends computed over such short periods should be considered very carefully also.

[M, D, & M] 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 and a power analysis would benefit our interpretation of the correlation.

P5: Even shown in Figure 1, the names of the stations presented in the results and in particular in Figure 3 should be fully explained/detailed (BOYM, EXEM, HOEM, etc...)

[M, D, & M] The abbreviations will be removed from the figure.

P5 L20: What does mean the “c.” before 0.5 mm in this sentence?

[M, D, & M] It means roughly or about or circa. We used ‘c.’ rather than ‘~’.

P6 L2: A reference to Figure 4f is given whereas there is no visible f) in Figure 4.

[M, D, & M] It should say Figure 4b. There was originally a Figure 4f, but the figure was

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edited and this sentence was not updated by accident.

P7 L22: “precipitation in terms of a snow year” -> Does it mean that the winter period is also included in the annual value? Or is the winter period excluded for the two sets of observation?

[M, D, & M] Yes, winter is included here. Winter is only included when we compare our dataset to the one published by Fountain et al. (2010). We will make this more clear in the introduction and methods.

P8 L13: Again, it could be interesting to give an estimation of both the spatial and the temporal variability of the snow density, because the choice of a constant value of 83 kg m⁻³ seems arbitrary. Also, it would be interesting to estimate the uncertainty of snowfall rates that directly emanate from the density uncertainty.

Did you consider to measure drifting snow, like Amory et al. (2020)?

[M, D, & M] The MCM LTER has snow density measurements from the glaciers with dates and we will comb through those data to see if any measurements were taken immediately following snowfall from our record. There is a lack of publications about snow density in the MDVs. We did not consider measuring drifting snow, but based on that paper, it seems unlikely that the dataset we present captures drifting snow because accumulation during wind speeds > 5 m s⁻¹ is removed.

P9 L11: It is claimed that there is no correlation between snow cover in TV and sea-ice extent, but there is neither any figure, nor any number to evidence this finding. This finding should be illustrated with numbers or should appear in a previous publication. Same remark can be done with the temperature observations.

[M, D, & M] We will include a correlation matrix illustrating both of these.

P9 L28: “the increasing persistence may be indicative of the changing climate” -> This sounds very speculative, because the timeseries are very short, with missing data, the snow accumulation changes have a strong spatial variability as evidenced by the

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signals that differ from one site to another. Also there is no analysis in the article concerning the mid-latitude to polar teleconnections, and in a general way the article does not include any investigations related to snowfall/snow persistence and atmospheric variables, which led few credence to the hypothesis appearing in the discussion.

[M, D, & M] We agree that it is speculative. It would be better to reword this to specify that the Fall is the least influenced by atmospheric oscillations and trends during the Fall may be representative of a background signal rather than reflective of atmospheric oscillations.

Implication for Hydrology and Ecology: The discussion related to the hydrological consequences is not clear. A situation with reduced snow volume and increased snow persistence (L.8) is pointed out as a situation that would favour a decrease of soil moisture. That's true, but if I have understood this article, I do not see in the previous section such opposite trends for snow persistence and snow accumulation.

[M, D, & M] Figure 3 shows increasing snow depth and Figure 6 shows the increasing persistence, although the increase in persistence is most notable in the Fall. This section will be moved immediately following the discussion section on trends in precipitation to highlight a discussion of our observations prior to discussing them.

P10 L8: "Sublimation is the greatest contributor to ablation of snow" -> Could you mention where such finding is applicable, please?

[M, D, & M] This is referring to the fact that in high-humidity areas the snow is more likely to melt rather than sublimate. In dry regions, snow is more likely to sublimate. We should have clarified that this is specific to Taylor Valley.

P10 L25: "Our record shows a clear increase in snowfall..." -> Could you remind where and when did you observe this trend, please?

[M, D, & M] This is a typo and should say 2007 rather than 2009. This is evident in Figure 3.

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P10: “Snowfall has been decreasing trough 2017 [from 2009...], which contradicts the expected increase in snowfall in polar regions under warming conditions” -> To my mind, this sentence is a too-simplified view of the climate change in polar area, because this observed decrease of snowfall occurred only over 8 years (2009-2017), a short period for which the internal variability of the climate system can lead to any change of precipitation because of its chaotic nature, even if it is superimposed with the long-term warming trend observed over the last decades. Similarly, the authors could write an opposite sentence when they describe the increasing trend of snowfall that they observed between 1995 and 2009.

[M, D, & M] It may be better to rewrite this saying that the record isn't quite long enough yet to reveal any long-term controls of sea ice on snowfall in the MDVs. On shorter time scales (5-8 years), sea ice does not appear to influence snowfall.

P10 L28: The sentence has a grammatical issue, with a capital letter after a comma, and maybe a missing verb somewhere, and a blank space located at the middle of the sentence?

[M, D, & M] This will be corrected. A comma was accidentally used instead of a period. The space will be removed as well.

P11 L2: Again, it is claimed that there is no link between the increasing precipitation and the reduced sea ice, but there is no number evidencing any sea ice retreat, and such number cannot be found in the citation Fountain et al. (2010). Also, an impact of the synoptic-scale atmospheric conditions is suggested, but without any corresponding result shown in the article.

[M, D, & M] Sea ice has been expanding over the past decade and although snowfall has been declining, they do not correlate even when we introduced a lag in the data. It may be best not to discuss possible controls on snowfall and snow cover in the MDVs.

Data availability: If the article is published, the data used in this study should be made

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available on a web interface, with a doi reference.

[M, D, & M] The data are accessible on the MCM LTER website and references were provided on P3 L25.

Table 1: Is the uncertainty shown in Table 1 includes the uncertainties of snowfall related to wind impact of sensors?

[M, D, & M] No, the uncertainty does not include wind. It is strictly related to the accuracy of the sensors.

Figure 1: What do you think about extending the area shown in Figure 1? This would allow to evidence that the Taylor Valley is a valley surrounded by mountains/glaciers.

[M, D, & M] The figure will be expanded.

Figure 2: a) and c) are mentioned in the caption, but not b).

[M, D, & M] (b) will be mentioned here: “The snow event captured in (b) has a persistence...”

Figure 5: The temporal resolution of the heatmap should be specified in the caption (daily resolution?).

[M, D, & M] Daily is correct. We will add it to the caption.

Figure 7: It seems that the number of days are centred over an average value, because there are negative values. This should be detailed in the caption.

[M, D, & M] This just highlights that the relationship does not do a good job at predicting low snow cover years. We will include a better description of this in the manuscript and figure caption.

References: Amory, C.: Drifting-snow statistics from multiple-year autonomous measurements in Adélie Land, East Antarctica, *The Cryosphere*, 14, 1713–1725, <https://doi.org/10.5194/tc-14-1713-2020>, 2020.

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Von Storch, H. and Zwiers, F.W., 2001. *Statistical analysis in climate research*. Cambridge university press.

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