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

Anonymous Referee #3

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General comments and recommendation

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 investigations 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:

* 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.

* The other weakness of this study is related to the links between snowfall and sea ice that are mentioned by the author trough 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 (De Santis et al., 2017). This point should be clarified when preparing a new article.

List of comments

A point-by-point list of comments is provided below, with suggestions that could be taken into account to conduct a more complete study.

Introduction:

* 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.

* P.2 L.9: annual mean of air temperature observed on average in TV by Obryk et al. is -20°C, so 18.5°C seems to warm (maybe a – sign is missing?).

* P.2 L.27: 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.

Methods:

* P.3 L.25 to L.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.

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

* P.4, L2: is the snow density systematically equal to 83 kg.m-3? That sounds like a strong assumption.

* P.4, L.9: "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?

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

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.

* P.5: 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. . .)

* P.5, L20: what does mean the “c.” before 0.5 mm in this sentence?

* P.6, L.2: a reference to Figure 4f is given whereas there is no visible f) in Figure 4.

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

* 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.

* P.7, 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?

Discussion

* P.8, 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-2 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)?

* P.9, L.11: it is claimed that there is no correlation between snow cover in TV and sea-ice extent, but there is neither any figure, neither any number to evidence this finding. This should be illustrated with numbers or should appear in a previous publication. Same remark can be done with the temperature observations.

* P.9, L.28: “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 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.

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.

* P.10, L8: "Sublimation is the greatest contributor to ablation of snow" -> Could you mention where such finding is applicable, please?

Conclusions

* P.10, L25: "Our record shows a clear increase in snowfall…" -> could you remind where and when did you observe this trend, please?

* 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.

* P.10, L.28: 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?

* P.11, 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.

* Data availability: If the article is published, the data used in this study should be made available on a web interface, with a doi reference.

References

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