The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-48-RC2, 2019

Interactive comment on "Quantifying the impact of synoptic weather types, patterns, and trends on energy fluxes of a marginal snowpack" *by* Andrew Schwartz et al.

Anonymous Referee #2

Received and published: 13 May 2019

General Comment:

This manuscript tackles the interesting problem of linking the energetics of a marginal snowpack to large-scale atmospheric circulation using a synoptic weather type approach. Meteorological observations obtained from an automatic weather station over two winters (2016 and 2017), including direct measurements of the turbulent heat fluxes using an eddy covariance approach, form the basis to establish the long term synoptic controls on snowpack variability determined using ECMWF ERA-Interim data over a 39 year period. The key conclusion is that energy available for melt has decreased over the long term due to a reduction in the number of precipitation generating

cold fronts and associated preceding warm air advection.

The key conclusion is significant so two major comments are provided that reflect on the evidence provided to demonstrate this. In doing so, it needs to be acknowledged that the comments already provided by RC1 (Mathias Rotach) have been reflected on. Some of the key points are raised again, while others are not as they have already been carefully discussed.

Major comments:

1. Bormann et al. (2012) demonstrate that snow cover is decreasing in Australian alpine regions using MODIS data, which is consistent to some of the work cited in L84-L96, but the key finding in this manuscript is that energy available for melt has decreased. This finding is contrary to what one might expect in a warming world, where higher air temperatures typically lead to more energy being available for melt. However, it is argued in L582-L588 that reductions in precipitation (snowfall) might be causing the decrease in snow rather than changes in energy available for melt. If it is the latter and the authors wish to maintain that energy available for melt has decreased, it is the view of this reviewer that more evidence is still required. There is no evidence in the present manuscript to demonstrate that precipitation has actually decreased. One of the key issues is whether the proportion of precipitation that falls as rain rather than snow has changed over time. It could be argued that the modelling approach in this study does not adequately account for the feedback associated with warming and the associated changes in phase of precipitation. But perhaps more importantly, it is the method used to link the optimum number of synoptic weather types (L273-L282) to the mean daily net snowpack energy flux representative of each of the types that is the most tenuous. Looking at changes in frequency of the synoptic events over time seems like an interesting pursuit but I am not sure whether the energy balance values presented in this manuscript over two winters are adequate to build a representative snowpack energy flux for each weather type over 39 seasons. Reasons for this are given in the following three points.

2. RC1 raised the issue about how representative one site is for determining the energy available for melt for the entire alpine region. The answer to this question has actually been answered by some of the coauthors of the present manuscript in Bilash et al. (2019). It is clearly stated that "individual point measurements are unable to fully represent the variability in the snowpack across a catchment" and that "we show that recognizing and addressing this variability are particularly important for studies in marginal snow environments". This would seem to suggest that using a point based measurement to represent the energy status of the snowpack for the Australian Alps for the purpose of linking it to large-scale atmospheric circulation is unsatisfactory.

3. It could still be argued that a point based approach is a valuable first cut at the problem, but for this to be the case it is essential that the observational data and model are of the highest quality. The marginal snowpack being described appears to be particularly vulnerable to rain on snow events, and periods where snow disappears completely from the ground at some locations in the alpine catchment. It might be advisable to use a snow model that can account explicitly for the changes in temperature structure of the underlying soil and snowpack to enable a much more careful time series of snow depth and structure to be described, as well as to identify unequivocally when melt is occurring. This would allow the energy for melt to be explicitly described, as opposed to the energy balance of a cold winter snowpack, which is likely to have quite a different energy balance compared to conditions defined by melt (e.g. Cullen and Conway, 2015).

4. The post-processing and quality of the eddy correlation data has been raised by RC1, in particular the limitation of using look up tables to fill a large proportion of missing data. Importantly, it is unknown how much data during the prefrontal and frontal (periods of precipitation) are actually real, which is somewhat crucial given the importance placed on these weather types on both the energy status and precipitation. The question that begs to be asked in relation to this methodological issue is why depend on eddy correlation data to build a time series of energy exchanges over the

two winters? Would an approach used by Bilash et al. (2018) not be more suitable, which would be to use high quality eddy correlation data to support a bulk aerodynamic method? Even the use of a degree-day approach might allow the key findings of this manuscript to be more robustly tackled, which is whether the energy status (arguably governed to some extent by warming) or changes in precipitation are controlling the change in snow cover through variations in weather types over a long period (39 years). The suggestions by RC1 about the post-processing of the eddy covariance data are very important, and once these have been done it is recommended that the authors carefully consider whether another approach should be adopted to construct an energy balance over multiple years (e.g. to use a more sophisticated snow model, or apply a bulk aerodynamic and/or a degree-day modelling approach).

Other comments:

1. L43-44: It is stated that research related to synoptic influences on energy balance over marginal snowpacks are rare. To provide greater context for this, it might be useful to more carefully define marginal as there are a number of research articles linking energy exchanges over snow and synoptic conditions that the authors have not considered (e.g. Yarnal, 1984; Romolo et al., 2006; Käsmacher and Schneider, 2001; Matthews et al., 2015; Isaksen et al., 2016, Cullen et al., 2019).

2. L198-L288: No information about the calculation of QG is provided in the model description, despite being explicitly referred to in Section 3.2.3 and again in Section 4.1. As noted above, it might be worthwhile considering using a more comprehensive snow model that allows the evolution of the snowpack to be reconstructed and compared to observations. The approach to define whether a period is snow covered or not is described in Section 2.3 but from this it is hard to know just how much snow there is on the ground at any one time. Modelling the evolution of the snowpack and comparing that to observations would seem crucial given the emphasis on establishing the changes in the energetics of the snowpack over a longer temporal period.

3. Section 3.2; It could be useful to more carefully compare the energy balance results derived using the eddy covariance data in this study to those generated by Bilash et al. (2018) using a validated bulk aerodynamic approach. Monthly and entire cold season (winter) data could be compared prior to presenting it within a daily synoptic type framework. This suggestion stems from the fact that the presentation of the energy balance results in the current manuscript are framed differently to those described by Bilash et al. (2018), where it is stated that 80% of the energy is sourced from incoming longwave energy. It may be that the data are essentially the same but the way they are presented places emphasis on different components of the energy balance. For example, on L519 it is stated that net shortwave radiation contributes the largest amount of energy to the snowpack, which is a different message to Bilash et al. (2018).

4. L534-L537: It is suggested that “many prior works have not made the distinction” between shortwave and longwave radiation. Historically, this may have been the case but there are numerous examples of more recent research over snow and ice that have used similar radiometers to those used by the authors to carefully identify the importance of individual radiation terms. The importance of moisture and clouds has been focused on by a number of researchers working over mid-latitude glaciers and ice sheets.

Technical and specific corrections

L132: There is a need to be more explicit that two winters are used as the observational dataset, not to just state when the observations began. L324: K and L are not explicitly defined in the manuscript in the form presented. L334: Manual classification of cloud cover requires clarification.

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