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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.

Mathias Rotach (Referee)

mathias.rotach@uibk.ac.at

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General considerations

In this paper, the authors set out to investigate the impact of the synoptic flow conditions (and their changes over the years) to available energy for a snow pack in South East Australia. This is, first of all, a very valuable undertaking and adds to providing additional scientific understanding on potential causes and mechanisms of impact (going beyond the simplistic global change → warmer → more snow melt). For this purpose, the authors use surface data from one energy balance station (situated on the snow pack) in connection with ERA-Interim reanalysis data for the synoptic situation. It

seems to me that in both these data sources, there are conceptual problems that need to be addressed before the paper can be recommended for publication. I usually, when preparing my review (i.e., reading the paper) list 'major' and 'minor' issues separately (this is what can be found below). Still, I add those two critical issues separately – even if some of the 'major' (and even some of the minor) comments address the same topics.

I have tried to give some references for specific points raised – and they often happen to be from my own work. This is not 'to make the authors cite my papers' – it is just that it is the quickest way to get this put together. Often, there are other, equally suited papers around for the same issue.

Critical issues

1) The authors use one surface EB station in an area of large spatial inhomogeneity, and derive all their results from this one site (2 years ['seasons'] of measurements). The individual contributions to the EB are then attributed to each synoptic type and further used in a climatological study. All this, basically prompts the questions: i) how representative is this one site for any larger area? Is the very local measurement representing the [energy distribution] in the 'Snowy Mountains' to any degree? ii) Are the energy estimates accurate enough to draw any firm conclusions? As for ii) the authors take most of the necessary corrections etc. to measurements appropriately into account and also apply a quality assurance procedure. However, this leads to the necessity to basically 'produce' 50% of the data (and I am not convinced that a lookup table is the best solution to do this, if necessary, see below) with a claimed uncertainty of some 21 W/m2 (latent heat flux) and 34 W/m2 (sensible heat flux). Summed over the day this amounts to some 1.8 (2.9) MJ/m2 (and day). Looking at Fig. 10 reveals that this is more for both Q_e and Q_H than the difference between the synoptic pattern with the largest and the smallest contribution to the EB. So, this triggers two consequences: First, the accuracy of the measurements has to be increased (which means not to use the Campbell standard post processing, taking into account (estimating)

the uncertainty due to assumptions which are not particularly good (such as 'constant fluxes', such as planar fit coordinate rotation with only one plane', etc. – see some of the 'major comments'). Second, the estimation of missing data has to become better (I am pretty sure that much better accuracy can be obtained by using a sophisticated statistical model), and finally, the uncertainty of the estimates has to be taken into account when discussing the results (e.g. Fig. 10). For example, this 'bar plot' could include error bars.

Concerning the representativeness question (i), I can only say, that our own measurements of contributions to the EB in complex terrain (e.g. Rotach et al. 2017), yields huge differences in all the contributions (with the exception of the longwave balance) from site to site, at kilometre scale. We haven't investigated the impact on daily means (or sums), but looking at daily cycles shows that they can be substantial (several 100 W/m2 in the maximum and several hours of difference when the maximum occurs, i.e. when the fluxes start to 'go back'). I have no useful suggestion, how to actually estimate the potential attribution error that might be introduced when using only one site (based on published results, that is) – but at least, I think it must be discussed (maybe Bellaire et al. 2017 provide some hints on orders of magnitude).

2) My second concern is the treatment of 'days' as an entity. Each day is identified with a 'synoptic pattern' (which is determined based on mean daily values of SLP, temperature, humidity, wind, etc.). While I can see how statistically this procedure yields a synoptic pattern for each day, I do not understand, why those patterns should always start at midnight and last for a day. Looking at the patterns shows that they constitute 'snapshots' of a dynamic development (e.g. the T7-T5-T3 sequence (Fig. 9) actually corresponding to an eastward moving trough to the south of the area of interest). This means that those patterns are only 'statistical entities' with the actual position of the trough on a given day (00.00 to 24.00 o'clock) being 'closest' to one of the patterns. If the trough had built (or moved in) at 12.00 o'clock (rather than at midnight and 'stayed there' for 24 hrs) as manifested in T7, the actual pressure

distribution (on day two, say) would likely be somewhere between T7 and T5 – with possibly quite drastic consequences for the local pressure (geopotential) distribution (e.g. Fig. 4) and hence advection pattern. This, in turn, will (potentially) affect large parts of the explanations (e.g., the WAA and CAA as discussed in Section 3.1.3). Can the authors 'defend' their choice of 24 hr time segments in some more detail and comment on the potential consequences (day-to-variability within a given pattern)?

Major comments

1) L. 131: It is good to provide the information on the instrumentation. It is also necessary, however, to provide information on data post-processing. Later (l. 217 ff), I see most of it is mentioned. However, for the planar fit coordinate rotation, one would want to know whether a sectorial fit is being employed (in complex terrain, most generally the 'plane' is not the same for different wind directions. . ., see, e.g., Stiperski and Rotach 2016), and over which period the 'planes' were fitted.

2) Homogeneous snow cover: is indeed crucial. Specially mentioned are (l. 156ff) periods with T_s > 1.5 °C and periods with albedo <0.4. But surely, this is mentioned only to detail some special cases. What are the criteria for homogeneity in the first place?

3) Energy available for melt (eq. 1): First of all, this assessment neglects storage in the snowpack (if we have the sum of all the mentioned energy fluxes being non-zero, there is excess energy available (positive or negative) to heat/cool the snowpack, and if zero degrees should be reached (at the surface), this will result in snow melt. Second, eq. (1) assumes that the energy balance is closed. Of course, the EB should be closed at the surface, but it rarely is (and the authors show themselves – even If I do not quite understand what they do in Section 3.2.5 (see there) – that the EB is not closed (not at all). Even over benign surfaces, differences (to closure) are typically several tens of percent (60-90% or so). In complex terrain (as in the present case) the issue is more pronounced (Rotach et al. 2008) - because of the (local) inhomogeneity

(not only of the snow cover – also the terrain itself and hence turbulence) and the advection (also vertical – hence the importance of the coordinate rotation!). Typically, in complex terrain, we do have flux divergence (when we measure the turbulent fluxes at 3 m height, they do not correspond to the surface fluxes [which are those relevant to the EB]. See for example Nadeau et al. (2013), e.g. their Fig. 4, or Sfyri et al. 2018). Usually, it is thought that EB under-closure is due to either instrument uncertainty (must be under-capture, of course), missing processes (e.g., meso-scale quasi-steady circulations) or incomplete corrections /post-processing. Note that in complex terrain, we have, by definition, meso-scale circulations such as thermally driven slope flows (also katabatic winds are in their nature thermally driven flows) or dynamically driven flow modification. And these are associated with non-zero vertical wind (and hence vertical advection). All this leads to an often quite pronounced under-closure of the EB. Basically, then, when assessing the 'melt energy' in the way the authors do, it will be 'too large' (or at least an 'upper limit estimate'. If the EB would indeed be closed at this site (and 3m measurement height) – which you show is not the case – Q_m would be the storage/melt energy. In any other case Q_m would actually be smaller. Unfortunately, all the procedures to minimize the under closure are flow dependent – so one cannot simply ignore the 'corrections' (i.e. additional terms like advection and flux divergence terms).

4) L. 184: clustering. This clustering approach sounds interesting – but I think I am not the only one who first hears about it. As it is described, it is purely statistical (which is fair enough), but somehow one would want to know whether or not the different clusters produce different synoptic situations. Only when I checked the given reference (Theobald et al 2015) I saw to what degree different clusters correspond to different synoptic conditions. I suggest to make this very clear (not only 'was verified through manual analysis'... - whatever this means), but by explicitly referring to the 'figures below' (4, 5, 6) where these synoptic patterns can be discerned.

5) L. 256. Look-up tables. Apparently, more than 50% of the data is not measured but

statistically 'created'. I assume (but the authors will have to detail this) that the look-up tables correspond to some sort of multi-linear regression model. I do not understand what the '100 iterations' actually mean and how the authors used the available valuable measurements to create (train) the 'look-up tables' and an independent part of the data to verify (and calculate the RMSE's).

6) L. 278: ...mean daily net energy flux...: does this make any sense? Are the individual daily cycles of the available energy (or the averages, if you will) different for the different clusters (synoptic conditions that is)? Do the distributions of net daily energy overlap? And if so (what I assume), how strongly? To what degree are those means dependent on the abundance of each cluster? Before the 'climatological trends etc. are assessed, this should be analysed I detail for 2016/17.

7) L.364 (Tab 5) I note that for T2, T4 and T6 the rows sum up to 1.01 (which is probably a rounding error). But for T7 to 1.23. Something must be wrong here. This will also (potentially) change Fig 9.

8) L. 453. Energy balance closure: I don't understand what is being done here. In fact, eq. (1) is a manifestation of energy balance closure (all what doesn't balance goes into Q_m, i.e. 'melt' (and storage) - but see comment 3). Does this mean that the authors assessed the closure without Q_m (i.e., by choosing the corresponding conditions, it is assumed that Q_m is zero)? First, it should be noted that still storage can be an issue (e.g, if T<0°C the snow pack can cool, negative storage). Anyway, if so, the authors are basically saying that even under conditions of non-likely melt+storage, there are huge additional processes challenging the energy balance closure. Some were mentioned in comment 3) above. Advection, in particular (horizontal and vertical!).

Minor comments

l. 158 ...were considered not (?) to have heterogeneous.... Either 'not to have homogeneous' or 'considered to have heterogeneous..'

l. 220 to remove.... This is, in complex terrain, not to remove any errors (that is what Wilczac has originally derived it for); rather, it is to align the coordinate system with the surface.

l. 241 surely the signal has units..: <0.7mV?

l. 314 are considered to be transition types: based on what? These are static (statistical) fields, so how can the authors claim this?

l. 350 recent cold frontal passage: where do we know from? Above (l. 314), T3 was not among the 'transitional regimes'...).

l. 476 linear trends: I basically see 'no trend' for T1, T3, T4, T5..... Has any significance analysis (in a statistical sense) been performed?

l. 487 is shown to decrease: at least, I think the decrease should be put in context to the year-to-year variability.

l. 493 are these indices determined over the same time period (I hope so)? More importantly, how do they vary during this time period? The figure would gain a lot, if each dot would be complemented with an 'error bar' showing the variability of the respective index in the respective year.

l. 504 in the nineties: at least two also in the eighties.

l. 608 can indicate significant increases...: in fact, the authors seem to demonstrate a negative trend (in total energy provided to the snowpack). So, the indication might rather be a significant reduction in ablation (or a change, to be more cautious).

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