The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-43-RC1, 2020

The Cryosphere
Discussions

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

Anonymous Referee #1

Received and published: 6 March 2020

General considerations

This is a resubmission of a paper with a similar title and focus, for which I have provided a quite detailed review on the previous version. The authors have made quite some changes (improvements!) with respect to the first version (data post processing and gap filling), but in my view have not been able to appropriately address the truly major concerns: the representativeness of one surface energy balance (EB) site and the treatment of EB (non-)closure. The authors have decided to 'avoid' the problem (e.g., by no more showing/discussing the evidence with respect to EB closure) or to 'downplay' it (the representativeness issue). I have two major comments making my points below. My third major concern on the first version had been the identification of synoptic patterns on a daily basis: here, the authors have added two sentences defending their choice – which is fine in principle. However, I still think that the authors miss out some potential or, in other words, introduce some unnecessary variability by choosing a not optimal reference time scale.

Major comments

1) Energy balance closure

The authors have added an additional version of the surface energy balance equation, which, at least formally, addresses the non-closure and introduces the residual (Qres).

→ On l. 221, the authors claim that 'Qres calculation and comparisons of snow pack energy flux terms were performed using the terms in eq. (2)'. This equation contains a 'energy balance closure term' (Qec). This term, however, is not available from the measurements. How did the authors make those 'calculation an comparisons'? (note that the non-closure is not just the sum of the 5 measured terms – because it also includes the Qres (i.e., the energy available for melt and internal [in the snow pack] energy storage).

→ Furthermore, when presenting the results, the 'ec-term' is not shown (and therefore not discussed) – of course, this is no wonder when it was not measured and cannot be derived from the measured terms. What is presented in the results section is the 'total net energy flux' (Section 3.2.4) – but it is not mentioned how this was determined: sum of the 5 measured (Q*, Qe, Qh, Qg, Qr) as in eq (1) [and called Qm]? At least, when comparing Fig. 6a and Table 2 (the entry for Qm), one gets the impression that it is indeed Qm what is now called 'the total energy flux'.

→ finally, in Table 2, Qm is listed, even if on l. 210 it is stated that Qm can be more accurately expressed as Qres...

So, overall it appears that the authors have, basically, added a new equation (which is never used thereafter), do not discuss the issue, and still present the same data - and now seem to call it ‘the total energy flux’ instead of ‘melt energy’. It is, unfortunately, so that the residual is also ‘energy flux’ – simply not accounted for in the form of the terms in eq (1) [it is local advection, flux divergence, storage, . . .]. This is not a subtlety. In the first version the authors had a short discussion on the energy balance closure (some 30% on average!) – so, more than half of the ‘total energy flux’ seems to be unaccounted for. Rather than thoroughly discussing this, the authors have decided to simply not show it in the revised version.

2) Representativeness

The energy balance related to the ‘synoptic types’ is assessed based on one surface energy balance station. The authors address the issue by including a short paragraph on the relative abundance of different species – and conclude that there will be ‘some uncertainty’ (l. 126) when applying the results of one site to the wider area of the Australian Alps. It is, however, not [only] the representativeness of the surface cover that determines the energy balance. In fact, on a 3m EB tower, the footprint (different for different wind directions – and hence synoptic conditions; but this just as an aside) of the flux measurements does hardly incorporate, the claimed percentages for different surface vegetation types.

What is relevant in complex terrain is the very local variability of the surface energy fluxes. One can measure the surface EB at a handful of sites within a few kilometers horizontal distance and one gets substantially different daily cycles for the EB components. That is, on the same day (same synoptic conditions) one site exhibits a strong daily cycle in Qh, say (resulting in a strongly positive daily sum) while a site 2 km apart with a different local slope, local exposition, ‘exposure’ to local flow regimes, local surface characteristics (on Fig. 2, I see many of those potentially relevant. . .), Qh starts to decrease long before local noon leading to an overall small (sometimes even negative) daily sum. Which one of the sites now produces the characteristic ‘response’ to the synoptic pattern? (And, more important: do those two sites show the same characteristic daily cycles on days with a given synoptic pattern? Of course, this latter question cannot be addressed with only one site – but at least it can be answered for the one site that is available – is there a characteristic daily cycle for a given synoptic pattern? In other words, is a ‘median heat flux’ a useful variable?).

I am not saying here that it is impossible to establish the surface EB terms for a region [in complex terrain] in relation to synoptic flow patterns. But I am saying that it is extremely difficult with only one station. And if only one station is available (and this can happen), the upscaling approach must be very careful and at least try to address the uncertainty involved (rather than sweeping it under the rug).

(some) Minor comments

l. 401 sentence

l. 433 first of all, Tab 2 yields 22 occurrences for T5 (not 24 as claimed), and second, this number does not seem to be very high (rank 3 out of 7, but much closer to the small end than the two really abundant). Fig. 6a seems to suggest that the large number is at least partially due to a few cases with up to 10 MJ day-1 (upper whisker).

l. 512 which has only one. . ..: T7 seems to be negative, too (Fig. 6a) in the median. . .

l. 518 . . . and synoptic patterns T3 and T4. . .: first of all, above (l. 516) the synoptic patterns associated with anti-cyclonic influence are identified as T1, T2, T4 and T7. Second, T3 and T4 do not have the largest negative energy fluxes, neither in median (Fig. 6a) nor in total amounts (Fig, 6b). Finally then, why would only T3 and T4 increase in frequency?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-43, 2020.