

Reply to Interactive comment on "Field-scale water balance closure in seasonally frozen conditions" by X. Pan et al., Anonymous Referee #1

We would like to thank the anonymous Referee for the comments and suggestions. We have prepared a response to each of the reviewer's comments, and have suggested how we will incorporate these suggestions into a revised manuscript.

With regards to the major comments:

1. "The First, there needs to be better analysis of the available data, rather than making assumptions of hydrological behaviour. For example, the effect of antecedent soil moisture on infiltration and runoff."

In this paper we do infer hydrological behavior from our interpretations of our field data set. When the data seem to strongly support a well-established hypothesis about behavior, and no alternative hypothesis is obvious, we do not propose new alternatives. This comment is interpreted to be mainly in the context of the impact of antecedent soil moisture on infiltration and runoff in frozen soils, and blends into the reviewer's next point, which we respond to further below.

2. "Second, the authors' conclusions on the influence of antecedent conditions need better justification and discussion. Explore alternative theories (e.g., the rate of snowmelt), and the subtleties of differences in soil moisture. The differences in antecedent moisture were not substantially different between the two study years."

The differences in antecedent soil moisture between the two years were profound and shown clearly (we believe) in Figure 8 (there is a problem with the legend in Figure 8, pointed out by the reviewer, that we will resolve, but this shouldn't affect the point of the figure). In a somewhat simplified interpretation, the 2013 moisture content profiles form the shape of a "Y", whilst the 2014 profiles form a "/". The left branch of the "Y" is the antecedent moisture, which in 2013 is markedly lower. That this should promote infiltration in 2013 and promote runoff in 2014 is not controversial in the literature on frozen soils, going back to Don Gray's work in the Canadian prairies, and many subsequent studies. The differences between streamflow response and surface ponding at the site in the different years are entirely consistent with these observations. The stronger criticism of this (made, in fact, by the second reviewer) is simply that we are not saying anything new (we will address this in our response to reviewer 2). In the revised manuscript, we will more clearly highlight the differences in antecedent moisture, and discuss how this strongly influences the meltwater partitioning and the water balance measurement uncertainty.

3. Third, the summary is poor, and needs to be written to provide more impact for the reader. Perhaps this can be addressed as the authors expand the discussion of uncertainty in water budget residuals.

We do agree that we could improve our summary in a revised paper and we will include a more comprehensive error analysis on our observations, by estimating error bounds for observations of precipitation (due to under-catch), evapotranspiration (due to energy balance closure), SWE (due to spatial variability) and soil moisture (due to spatial variability – this is already included).

Minor comments

1. Line 8: It is not impossible to measure water budget terms independently in the field. It is difficult, and maybe uncommon. Perhaps rephrase the sentence to say "... yet in practice it is uncommon to measure every ..."

Although it may, theoretically, be possible to measure every term in the water balance, there are virtually no examples of this in the literature; particularly at field-scales. Since this is a very important point of context for this study, and for hydrological science in general, we stand by our statement that it is “usually impossible” to measure every term of the water budget. In a revised version we are happy to include more discussion around this point. For instance, we feel it may be worth adding the qualifier “directly measure” in our sentence. For example, there is no way to directly measure the drainage flux – there are only indirect ways of inferring this (e.g. based on Darcy’s Law, based on water balances or based on tracers).

2. Line 17: The snow pack does not infiltrate. Rephrase "... melt from the snow pack mostly"

Agreed. This will be changed in revised manuscript.

3. Page 4 Line 22: I’m not sure you need this sentence on salts as it is tangential to the water budget problem that is the focus of the paper.

Agreed.

4. Page 5 Line 4: Exactly because of the issues discussed in this paper, I have always shied away from the term "water balance", and preferred to use the term "water budget". The authors might consider using the latter term when appropriate in this manuscript.

The rationale for replacing “balance” with “budget” is unclear to us. “Budget” surely implies the amount of water available for some purpose. This is not what we are discussing in this paper – we are seeking to close all terms of the water balance, or critique our inability to do so. We think then the term “water balance” is appropriate.

5. Page 5 Line 11: Drainage fluxes while measured at a point in space, [are] not point measurements, but integrated over an area. They are not measured at a point scale, and this sentence needs to be corrected.

The reviewer’s point is unclear here. We stated that “drainage fluxes can generally only be measured at point scales.” We are referring here to using either Darcy’s Law (with head gradients from tensiometers (or similar), and a parametric model for hydraulic conductivity), or a 1D soil water balance to estimate drainage – both of which would be considered standard methods and both of which are indeed point scale estimates.

6. Page 6 Line 10: *The figures in the paper are not presented in order. The first figure presented should be numbered one, not five. Furthermore, maybe the content in these couple of sentences should be presented in the results section.*

The reviewer is correct and we will remove references to Figures 5 and 8 from this part of the revised manuscript.

7. Equation 1: *The field scale vertical water budget should also include melt (M).*

Snow melt is not a flux term, but is rather an internal transformation (phase change) of water which is already accounted for within the domain over which we are defining our water balance, and hence should not appear in Equation 1. Meltwater may become runoff, which we account for in Eqn 1, or it may infiltrate and appear as a change in soil moisture (which also appears in Eqn. 1). We will clarify the domain and our approach in a revised version.

8. Page 6 Line 16: *Perhaps G should be "... net drifting snow over the field domain ..."* That might be more accurate.

We agree that the inclusion of “net” is useful, and we will revise the text to read “G is net drifting snow entering / leaving the field domain laterally”.

9. Page 7 Line 12: *I disagree that a +2C temperature is a killing frost. Maybe -2 C; is this what the authors meant?*

Agreed. This was a typographical error.

10. Page 8 Line 9: *Streamflow is typically a rate (m³/s), but here the total volume is presented. That would be yield. This sentence should read " Mean annual yield in the Brightwater"*

The reviewer is correct, and we will change the units of streamflow to m³/year. We prefer to talk about flow rather than yield.

11. Page 13 Line 14: *Again, check the order in which figures are presented.*

This will be corrected in a revised version.

12. Page 13 Line 20: *Please provide the data that supports this statement that there were similar surface temperatures. Furthermore, it is wrong that wetter soils freeze faster or deeper. Wetter soils are warmer soils because of the energy required to freeze the water content. An alternative hypothesis that the authors do not consider to explain the different soil temperatures was the growth of snowpack development relative to the air temperatures.*

The reviewer is correct about the effects of latent heat, which were absent from our discussion. We checked the temperatures more carefully, and we think that in fact differences in air temperature (lower in 2014) explain the differences in the freezing depths. We will either include an additional figure that shows the differences in the surface temperatures, or include this information as a subplot in an existing figure.

13. Page 14 Line 10: *Another flux is vapour migration from the snowpack to the soil. This is what creates depth hoar in the snowpack. Quinton was one of the first to document this.* We were unaware of the evidence for vapour migration from the snowpack to the soil, as opposed to vapour transport within the snowpack, and vapour migration from the soil to the snowpack, both of which are discussed in the literature as causes of depth hoar. If a reference to Bill Quinton's work on this can be provided, we would be happy to include a reference to this process.

14. Page 15 Line 3: *It is effective drainage area not contributing area; stay consistent to the language you used earlier in the paper.*
Agreed.

15. Page 15 Line 8: *Showing increases in soil moisture as negative is not intuitive. Perhaps this should be changed.*

We understand the reviewers point - this was done for visual effect in Figure 7. In a revised manuscript we will present this as a grouped bar plot, with all the terms being positive.

16. Page 15 Line 23: *I disagree that the soil moisture conditions were strikingly different. They were both near 0.2. This is not enough to explain the different responses.*

The pre-melt soil water content is indeed strikingly different in 2013 and 2014 – there is a 5-10 % difference in most of the profiles shown. Moreover, the shape of the profiles is completely different. We will add gridlines to Figure 8 which may make this clearer.

17. Page 16 Line 3: *These are saturated at this level? What is the porosity?*

We did not mean to imply here that the soils were saturated, just that they were relatively wet. We will modify this sentence to read “The permeability and hence infiltration capacity of a frozen soil tends to decrease under wetter conditions, as happened here in 2014.”

18. Page 17 Line 3: *if the error is due to unaccounted for soil drainage, why did it not happen in both years? Please discuss.*

This point is discussed in considerable detail in the paragraph following the one in question.

19. Page 18 Line 13: *As noted in the major comments, the authors need to expand the discussion to include the implications for hydrologists. Also, a good reference to include in this would be Barr et al. (2012). Barr et al., 2012. Energy balance closure at the BERMS flux towers in relation to the water balance of the White Gull Creek watershed 1999–2009. Agricultural and Forest Meteorology 153: 3-13.*

This is an excellent suggestion. We are aware of that paper, which is somewhat uncritical in how it applies a water balance. We will expand our discussion making reference to this paper.

20. Page 19 Line 7: *This should read "... because we did not measure the fluxes... "*

Agreed.

21. *Figure 2: Could the authors please provide an explanation for the strange soil temperatures in 2014. Also, related to the discussion the authors have a 'post snowmelt' period, it was hard to judge exactly where on these figures that was, so perhaps another vertical line would help.*

Figure 2(d) provides freezing fronts (0°C), which are interpolated from soil temperature measurements from a series of sensors (Stevens Hydro-probe) in depth. In 2014, there were three profiles with different snowpack thicknesses, which led to different soil freeze-thaw rates. P2 and P3 had thinner snowpack and thicker freezing depth than P1.

Regarding the “post snowmelt” period, we will try to more clearly distinguish this in a revised version of the paper.

22. *Figure 6: If the authors had a Geonor precipitation gauge, why not present daily precipitation rather than 10 day intervals?*

In this figure we present the 10-day accumulation so that is it easier to contrast the difference in precipitation type between the 2 years. The daily precipitation is already shown in Figure 2(a).

23. *Figure 7: There was no change in soil moisture post thaw in 2013? And shouldn't net precipitation be negative – see Table 1.*

During 2013, there was a negligible change in measured soil moisture between our last measurement date during the snowmelt event and a subsequent measurement date when soils had thawed. The purpose of presenting these data in this manner was to contrast these 2 years. In 2014, due to restricted infiltration, any measurable changes in soil moisture were delayed until the soils had sufficiently thawed. In reality, our neutron probe measurements only provide a rather coarse temporal resolution, and melt and thaw cannot be considered as discrete events. However, we feel that our generalizations are useful to contrast the timing of the infiltration processed during these 2 years.

With respect to the net precipitation, indeed, the 2013 number should be negative. This will be corrected in a revised version.

24. *Figure 8: Why are there 4 lines, but only 3 in the legend?*

Good point. This figure will be revised to reflect the actual dates of measurement, while keeping the color scheme to distinguish which hydrologic regime (e.g. pre-melt, post-melt, etc.) each measurement date corresponds to.