

Responses to: Interactive comment on “Soil-water dynamics and unsaturated storage during snowmelt following

wildfire” by B. A. Ebel et al.

- Reviewer comments in black text, [author responses in blue text](#).

Anonymous Referee #1

Received and published: 20 January 2012

Thank you for the opportunity to review the manuscript titled “Soil-water dynamics and unsaturated storage during snowmelt followed by wildfire.” The paper is concise and very well written and should be of great interest to the scientific community. The literature review and knowledge of the subjects presented are outstanding.

[The authors thank the reviewer for the constructive comments, which were helpful in improving the manuscript.](#)

P442, L20 Reword to “...indicate that the amount of snowmelt-driven...”

[We agree with the reviewer’s suggested revision of the manuscript.](#)

P443, L28 reword to “Reported effect on snow accumulation...”

[We agree with the reviewer’s suggested revision of the manuscript.](#)

P444, L5 Maybe mention what they attribute the lower SWE to in this case?

[Drake et al. \(2008\) attributed the SWE reduction to the change in forest cover/canopy structure. The authors will include this information in the revised manuscript.](#)

P446, L20 This is a good place to call figure 1.

[We agree with the reviewer’s suggested revision of the manuscript.](#)

P446, L27 Double check this but I don’t think there is an a at the end. The main soiltype is an alfisol, but the taxonomic name uses only alf at the end to indicate that.

[Yes, the reviewer is correct, the proper name is Typic Haplustalf.](#)

P447, L17 It wasn't clear to me here what the difference is between seasonal and intermittent.

On the north-facing slopes, a seasonal snowpack denotes one that develops and persists for months and melts in a defined "melt season". In contrast, on south-facing slopes, an intermittent snowpack denotes one that melts completely over several days following a storm. The authors agree with the reviewer that this point should be clarified in the revised manuscript.

P447, L26-27 I was confused here because the figure shows UBNF as well as UBSF which are not mentioned here.

In a revised manuscript, the authors will clarify what is measured (and not measured) at the UBNF and UBSF sites in the text and Table 1.

P453, L9 May want to call figure 1 here, first mention of UBNF? Or include some description of UBNF and UBSF where you first call figure 1.

We agree with the reviewer's suggested revision of the manuscript.

P454, L26 Change to "...behaved similarly during..."

We agree with the reviewer's suggested revision of the manuscript.

P455, L 2 use "period" instead of "spell"

We agree with the reviewer's suggested revision of the manuscript.

P455, L8-9 Be consistent with previous sentence (12 and 13 Apr.) or change that one to 12-13 Apr.

We agree with the reviewer's suggested revision of the manuscript.

P458, L6 You use both montane and mountainous, might want to chose one or the other to be consistent.

This is a difficult distinction because "montane" also refers to a vegetation community while "mountainous" refers to topography/climate. The authors will be careful to use the proper term in a revised manuscript.

P459, L1-13 This could be combined in one sentence as both studies found the same low albedo effect.

We agree with the reviewer's suggested revision of the manuscript, the sentences citing Rouse (1976) and Chambers et al. (2005) would be combined in a revised manuscript.

P461, L1 Add "...data from the south-facing burned slopes..."

We agree with the reviewer's suggested revision of the manuscript.

P461, L2 Did you determine porosity assuming full saturation during retention experiments or from particle and bulk density data?

We used both methods (i.e. soil-water content assuming saturation and determination from bulk density and particle density) to estimate porosity and the results were very close between the two methods. We will clarify this in the revised manuscript.

P461, L3-4 reword to "...capacity, compared to unburned slopes."

We agree with the reviewer's suggested revision of the manuscript.

P461, L6-9 I was expecting some mention of the role of hydrophobicity here, is that not a significant effect?

The reviewer's point is well taken. Hydrophobicity can certainly be important in burned areas, both for restricting infiltration and for causing preferential unsaturated zone flow. The rates of water application to the land surface tend to be much smaller during snowmelt than during summer rainstorms, which may explain why hydrophobicity-driven overland flow was not observed during snowmelt. Also, during snowmelt, we did not observe the type of hydrophobicity-driven fingering of flow that has been observed at other places during summer rainstorms. If it is important that we clarify these points in the revised manuscript, we can certainly do so.

P462, L9 could delete "described herein"

We agree with the reviewer's suggested revision of the manuscript.

Tables. Is there a reason UBSF and UBNF are not included in the tables?

We will add UBSF and UBNF to Table 1 in the revised manuscript, per the reviewer's suggestion. We did not add UBSF and UBNF to the other Tables because the periods of record are very dissimilar since UBNF and UBSF were added in late April 2011.

Figure 6C- This graph is not technically a time series like A and B because time is not an axis.

Yes, the reviewer is correct. We have changed the Figure caption to indicate that it is a "Graph of the coefficient of variability of soil-water content as a function of the mean soil-water content for that sampling date."