

Interactive comment on “Hydrologic Flowpath Development Varies by Aspect during Spring Snowmelt in Complex Subalpine Terrain” by Ryan W. Webb et al.

Ryan W. Webb et al.

ryan.w.webb@colorado.edu

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The authors would like to first thank you for taking the time and effort to review our manuscript. We appreciate the comments that have been made and our responses can be found below immediately following a reiteration of the comment.

In the present paper, the authors study the influence of preferential flowpaths in a deep snowpack on soil moisture at the hillslope scale and investigate notably its spatial variability. They perform a qualitative analysis of the SWE and soil moisture evolution between surveys and a statistical analysis (pearson's correlation coefficient) of near surface soil volumetric water content and topographical (aspect, slope), hydrological

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(peak SWE, date of peak SWE) variables. One of their main results is the increase in SWE observed at the beginning of the melt season at the toe of the north aspect hillslope. According to the authors, this increase is due to lateral flow in the snowpack. Even if this hypothesis is probable, the arguments provided in this study remains qualitative (observed ice veins at the snow-soil interface) and are not robust enough for such a scientific publication.

Response: We agree that only observations of ice veins would not be robust enough for publication in this journal. Points are made from multiple observations in the text and combined in the discussion based upon not only increases in SWE at both locations on the hillslope and at the toe of the hillslope (Fig. 3) in combination with the frozen “ice veins”, but also less infiltration to 12.5 cm and 20 cm depths on the slope (Fig. 5), similarities in soil moisture between snowmelt and overland flow rain events, and the observations of snow density and saturated layers in the snow increasing downslope. We apologize for not making this clear in the writing and will revise to further highlight the multiple points of our argument. In revisions, we propose to further quantify the lateral flow by means of an energy balance model to estimate meltrates and calculate the required lateral flow accumulation along the hillslope necessary to produce the observed increases in SWE.

The introduction is well written and documented even if it is sometimes difficult to see where the authors want to go. In my opinion, some of the observations presented in the methods are the first weakness of this study: the authors present (and measured?) only the total SWE for each snow pit. A detailed profile with snow density and depth would be very useful in order to support the results (lateral flow as the main cause of SWE increase). Indeed, an in-depth comparison of snow layers could be very useful.

Response: We agree that a detailed snow density profile at all locations would be useful. However, due to constraints of an individual surveyor making observations at more than ten locations in a single day this was not feasible. The objective of this study was to gather more information spatially rather than at only a few points. However, we

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are happy to add the depth observations to figures to help clarify that more data was collected than it seems the reviewer is aware of and we apologize for not making this more obvious in the writing. Furthermore, detailed profiles were collected on the first survey of each season and will be added.

The Results part is very difficult to read as it is essentially a description of figures: there is no underlying theme and the different paragraphs have few connections. Response: We apologize for this section being difficult to read. This is a writing style choice that the authors made where we are presenting only the results in the “results” section and produce the underlying theme and storytelling in the “discussion” section of the paper. We will revise the “results” section to better connect the paragraphs. The discussion is pretty good but does not bring any new argument.

Response: We believe that we do bring new arguments. No studies that we know of have combined snow observations with soil moisture observations at this scale and in this type of environment. Furthermore, our development of the conceptual model showing that water flow through snow is more important on the north aspect slope vs. south facing slopes as a result of snow and soil parameters has not been argued for in the literature to our knowledge.

Finally, the authors are citing throughout the document their own paper currently under review (Webb et al., in review) but as a reviewer, the significance of this reference remains hard to assess.

Response: We agree with this point entirely and will revise the manuscript by also citing fully published papers to support these points.

Again, thank you for your time in reviewing our manuscript.

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