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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Rain or Snow: Hydrologic Processes, Observations, Prediction, and Research Needs" *by* A. A. Harpold et al.

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We appreciate the reviewer's positive overall comments on the manuscript. We make detailed responses below below and have made the editorial changes wherever possible. We also agree with the more major comments regarding validation of the microphysics schemes and the potential influence of temperature-only PPM in large-scale forecasting of phase under changing climate.

Response to specific comments:

1) Line 21: Change "The review" to "This review" or "Our review". The previous sen tence structure made it unclear which review is being referred to and required the reader to go back to the previous sentence wondering what review is being mentioned.



This was changed. We also made similar changes on line 114 and 795.

2) Line 184: either here or elsewhere, it should be mentioned that it is important to validate these microphysics (or other properties if you move this to the discussion) over various land surfaces / types. A microphysics scheme that performs well in Iowa (flat prairie) may not perform well over Idaho (complete mountain terrain) or the Oregon Cascades (coastal warm snow).

This was expanded on line 183 to read "The rain-snow line predicted by atmospheric models is very sensitive to these microphysics (Minder, 2010) and validating these microphysics across locations with complex physiography is challenging."

We agree that a discussion of verifying the microphysics schemes, in particular for the complex terrain that is the focus of the paper, would strengthen the paper. We have a added several sentences beginning on line 602-609: "These schemes vary greatly in their accuracy with "mixed phase" schemes generally having the best verifying simulations of precipitation in complex terrain where much of the water is supercooled (Lin, 2007; Reisner et al., 1998; Thompson et al., 2004; Thompson et al., 2008; Morrison et al., 2005; Zängl, 2007; Kaplan et al., 2012). Comprehensive validation of the microphysics schemes over different land surfaces types with a focus on different snowfall patterns (e.g. warm maritime, flat prairie, etc.) is lacking. In particular, in transition zones between mountains and plains or along coast lines the complexity of the microphysics becomes even more extreme as differing air mass characteristics become juxtaposed.

3) Line 248: The "(" should be moved to before 1967 based on how the reference is integrated into the sentence

This was corrected.

4) Line 303: need parenthesis instead of brackets

This was corrected.

5) Lines 433, 601: a space is needed between references

This was corrected.

6) Line 583: what is meant by “performing the best”? The best precipitation over mountains? Lowest errors in climatology? Lowest errors in variability? Please clarify. This was clarified on line 616: “These schemes vary greatly in their accuracy with “mixed phase” schemes generally having the best verifying simulations of precipitation in complex terrain where much of the water is supercooled (Lin, 2007; Reisner et al., 1998; Thompson et al., 2004; Thompson et al., 2008; Morrison et al., 2005; Zängl, 2007; Kaplan et al., 2012).”

7) Line 641: “too” not “to”

This was corrected.

8) Line 783 and Figure 3: The authors should consider adding an accompanying western U.S. climatology map of humidity to show it has significant spatial variability (implied by the statement here and similar ones elsewhere, but not presently shown).

This is a good suggestion. We will add a map as a second panel to Figure 3 using the University of Idaho Gridded Meteorological Datasets, which is essentially NLDAS-2 data downscaled to 4 km.

9) Conclusion/Discussion: I would like to see a paragraph added here or in the previous section (5.6) discussing the implications of this review / points raised for findings from climate change studies focused on snowfall. For example, there are several done at the global scale / continental scale (O’Gorman 2014; Cayan and Pierce. 2013; Kapnick and Delworth 2012). These studies present large-scale changes in snowfall mainly due to temperature (all use temperature-based metrics for phase partitioning), but based on this review, miss the non-temperature induced sensitivity of phase type, likely with nonlinear consequences. Should the changes found in these studies be expected as the temperature signal at some point overwhelms all other signals? Or might the



differences due to climate change be non-linear in all cases? A nice final point of this manuscript would place this study within the framework of these larger scale studies / findings as it is implied that reviewing and exploring phase type will have consequences for understanding future water availability and change.

This is an excellent point that was hinted at in the discussion but not fully addressed. We expanded section 5.6 on line 861 to read: “Because broad-scale techniques applied to assess changes in precipitation phase and snowfall have relied on temperature, both regionally (Klos et al., 2014; Pierce and Cayan, 2013; Knowles et al., 2006) and globally (Kapnick and Delworth, 2013; O’Gorman, 2014), they have not fully considered the potential non-linearaties created by the absence of wet bulb depressions and humidity in assessment of sensitivity to changes in phase.”

10) Figure 1: The arrows and curly bracket should be changed to be a different color (not grey) to provide contrast. Perhaps red or blue? They presently do not stand out easily / show the movement of information as presently shown. A more contrasting color choice will make this figure easier to read and understand.

We agree and have made these changes to a new figure that is attached.

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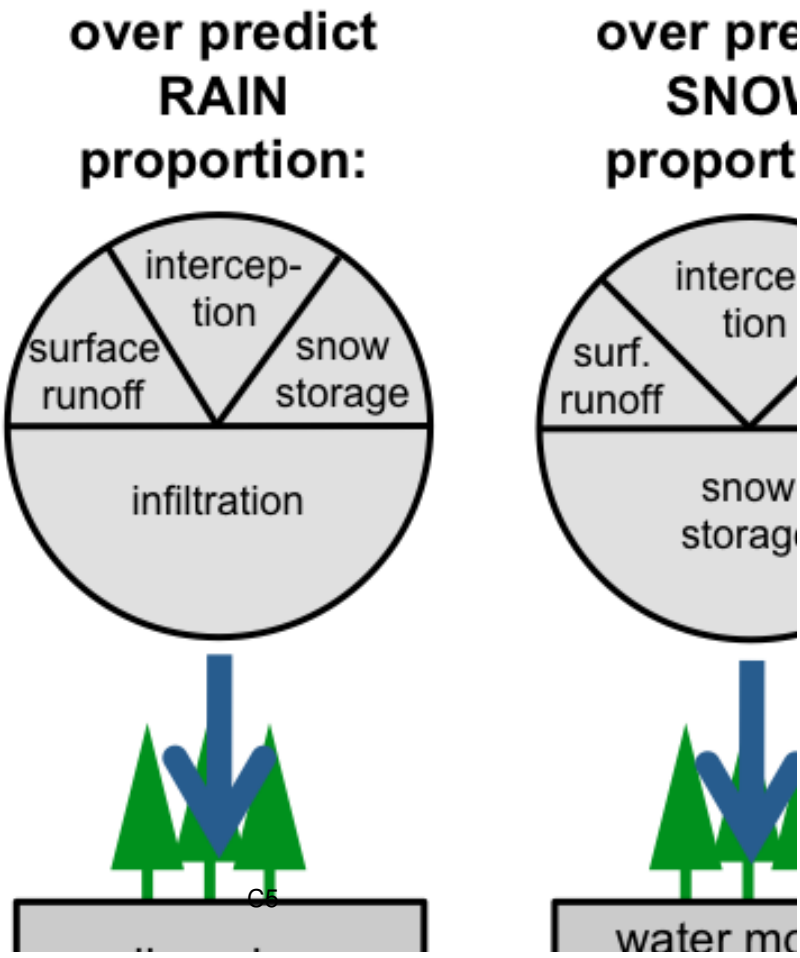
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