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# *Interactive comment on* "Snow cover duration trends observed at sites and predicted by multiple models" *by* Richard Essery et al.

**Anonymous Referee #2**

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Summary:

The authors present an evaluation of 30-year trends in simulated snow-cover duration metrics. In-situ data from four well-instrumented sites are used as validation of multiple snow models forced with bias-corrected, gridded meteorological data. A simplified snow model consisting of two main parameters is used to provide process-level insight into the simulated snow sensitivity to climate. The authors report limited (statistically significant) trends in snow-cover onset and more notable declines in the date of snow-cover depletion. The simplified model is used to demonstrate that regions most sensitive to the influence of warming are those where snow historically persists late into the spring and summer.

The paper is well written and easy to follow. The topic is clearly germane to the scope



of The Cryosphere. The focus on snow-cover duration metrics (start-date, end-date, and days with snow-cover) makes the results relevant to a wide audience (ecologists, hydrologists, climate scientists). I have some questions about the exact definition of these metrics and sensitivity of results (see general comments). I especially like the use of the simplified model and the fact that it generally spans the response of the multi-model ensemble; nimbly side-stepping the vague inference commonly required to describe the varied responses of very complex models. I support publication, but am requesting a re-analysis of the model output regarding how snow-cover metrics are calculated.

General Comments:

1) The authors use the term "continuous seasonal snow-cover" to describe start and end dates. I agree that snow-cover start- and end-dates should be based on "continuous" seasonal snow data; however, the authors' definitions are at odds with this. Line 109 states "Start and end dates for continuous seasonal snow cover were found by searching for the first and last dates with snow depths exceeding 2 cm before and after the dates of maximum snow depth in each year."

Trujillo and Molotch (2014) define continuous seasonal snow-cover start and end dates as the last start date before maximum accumulation and the first snow-disappearance after maximum accumulation. This definition limits assessment to seasonal snow-cover and excludes early and late snow accumulation events that would arguably be 1) short-lived, 2) possible synoptic outliers, and 3) potentially of limited hydrological or climatological significance. My concern is that >2 cm of snow may accumulate in September or June, and last only a few days, while the true measure of continuous seasonal snow-cover may persist from November to May. Please ensure that your analysis focuses on continuous seasonal snow-cover as stated in the paper rather than the methods described on Lines 109-110.

2) How are the conclusions stated on line 135 (radiation-driven melt occurring when

air temps are below 0C) impacted by the underlying assumption of 2PM that ignores snowpack cold content? Is this consistent with observations? Is it consistent with models that include representation of cold content dynamics? For example, according to López-Moreno et al (2013) referenced in this paper: "With cold temperatures, solar radiation at the end of winter is not sufficient to trigger melting, ..."

3) Many methodological descriptions are included in the Results section. Please consider moving to the Methods section.

Specific Comments:

Line 12: What does it mean to "demonstrate climate change"? Be more explicit.

Line 13: "Reports from the second onwards have. . ." should be rewritten for grammar.

Line 195: The final sentence of the paper seems to dispute the origin of a "slower snowmelt in a warmer world" hypothesis stated as the title of a 2017 paper by Musselman et al. A quick scan of the López-Moreno et al (2013) paper doesn't include this messaging or the words "slow", "world" or "hypothesis". Pomeroy et al (2015) is a conference proceeding not accessible at the link provided. The work of Musselman et al (2017) was built upon graduate student work published in

Musselman, K. N., Molotch, N. P., Margulis, S. A., Kirchner, P. B., & Bales, R. C. (2012). Influence of canopy structure and direct beam solar irradiance on snowmelt rates in a mixed conifer forest. Agricultural and Forest Meteorology, 161, 46-56.

Page 53 of that paper: "Reduced ablation rates were observed at lower elevations where snowmelt commences earlier in the year when solar elevations are lower. At upper elevations snowmelt continues later into the year when solar elevations are higher, resulting in greater seasonal ablation rates."

Please either be complete and accurate in describing the origin, or succinctly cite the paper stating the referenced hypothesis.



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