

Interactive comment on “Global re-analysis datasets to improve hydrological assessment and snow water equivalent estimation in a Sub-Arctic watershed” by David R. Casson et al.

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This study presents and evaluation of several global datasets (models and satellite) applied to a Canadian watershed. The main goal is to investigate the quality of these datasets and compared with a locally calibrated model in terms of maximum SWE, snowmelt volume and peak discharge. Despite the local focus, this study is of general interest to the land-surface and hydrology communities as it addressed the added value and caveats of using large-scale datasets in local watersheds. The manuscripts is well organized but it lacks several methodological details to allow the reader to fully understand the results, and replicate. In my opinion, this manuscript is of interest for

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publication in HESS once these methodology details are clearly detailed.

Calibration, validation, testing: (page 5). Please clarify the observations used over the different periods “calibration, validation and testing”. The calibration period is fully automatic? the validation include some human intervention? How was the length of each period decided ?

Results in table 3: Which period was considered for the scores calculation in table 3? If the full period was considered, WFLOW-HBV has a clear advantage since most of the period was used in the calibration. If this is the case, please clarify indicating clearly the period used in the validation.

How was the snowmelt volume and peak discharge calculated from the global models ? Was it also estimated using the survey SWE observations ? If yes, which methodology was used ?

It is not clear from the results the affirmation in the discussion (Pag 10 L11:) “Local model maximum annual SWE . . . than snowpack survey data” Please clarify this point. Also on this point, later another sentence suggests a similar results (pag 10, L28): “Study results demonstrate that SWE . . . in the Snare Watershed”. In both cases, it is suggested that SWE observations are not as good predictors of the local model. Then latter in the discussion: (pag 12 L 16) “as the study shows that ground data is a . . . and peak discharge”. Please clarify this point, as this is crucial in this study: Is there an added value of the local model when compared with the survey SWE data in predicting snowmelt volume and peak discharge? (see also the previous comments on clarifying the methodology used)

page 5, L6: “is a based” : “is based”

page 5, L16-17: page 5, L21: “are conceptual rainfall-runoff models”: The models listed in table 1 are not conceptual rainfall-runoff models. I suggest to change the sentence to: “A set of global hydrological and land-surface models were considered in this study

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and presented in table 1”

pag 7, L20:25: Despite a different region in Canada, Snauffer et al (2016) also evaluated several reanalysis and GLOBSNOW. It is worth to cite this paper that also highlighted some limitations of GLOBSNOW.

Table 2: Please define KGE, PBIAS, RSR

Figure 3: Please add panel names (e.g. a, b c). In the lower panels the blue line refers to GlobSnow or WaterGap ?

Figure 4: Also add panel names (e.g. a, b). In addition to the scores in table 3, the scatter plots comparing observed SWE vs. model SWE would be also informative. I suggest to also include these plots (if too much in the main article, at least as supplementary information) .

Figure 5 and pag 9 L16: How were the observations interpolated to the 25km grid ?
Page 9, L27: As mentioned above, please avoid the term “conceptual model” here and in other locations. In several places please check the usage of “Study results”
E.g. pag 10 L28 “Study results” should be “This study demonstrates. . .” or “Our results suggest that..” Also in pag 11 L20 “Study results”

References: Snauffer AM, Hsieh WW, Cannon AJ. Comparison of gridded snow water equivalent products with in situ measurements in British Columbia, Canada. J Hydrol. 2016; doi:10.1016/j.jhydrol.2016.07.027

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