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Discussions

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Interactive comment on "Application of machine learning techniques for regional bias correction of SWE estimates in Ontario, Canada" *by* Fraser King et al.

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Reviewer 1

General Comment:

This study quantifies bias between SNOWDAS assimilation dataset and in situ SWE observations in Ontario region (Canada) and compares efficiency of three different bias correction methods in terms of improvement of SWE prediction and estimated snowmelt volumes. The results indicate that there is a bias between SNODAS and in situ SWE, particularly in the period 2011-2013 and that the machine learning technique

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(random forest) approach outperforms simple mean subtraction and linear regression bias correction methods. Overall, the manuscript is clearly written and has a good structure. The topic is relevant and within the scope of the journal. I would like to make only a few general comments.

General Comment Response:

We thank the reviewer for their comments, and we will work to incorporate their suggestions to improve our currently submitted manuscript. Our responses to each of the reviewer's questions/comments is included below.

Specific Comment 1:

The results indicate that there is a clear difference in SNOWDAS agreement (against in situ SWE) in the period 2011-2013 and 2014-onwards. It will be interesting to see/understand why? Is it the change in assimilation frequency, sources used in as similation, their accuracy? I think such understanding can then support the selection of approach used for bias correction. It has some implications also for the design of this study. If there is a step change in SNOWDAS, then it is not surprising that simple mean subtraction method is not working well for the entire period. It will be interesting to see why does the random forest outperform the other methods in such case and what factors are controlling its efficiency? (Is it because using year of observation?) Will it be not more fair in this case to compare the methods in two separate periods?

Specific Response 1:

We agree with the reviewer that the change in bias post-2014 is of interest, and we mention on lines 27-30 of section 4.2 that newly assimilated datasets are likely the dominant contributing factors to the reduction in the intensity of the SNODAS bias during this period. We argue that while the bias is reduced post-2014, it is still non-zero and the approaches explored in our work continue to provide improvements to SNODAS estimates during this time. The decision tree and random forest approaches

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outperform traditional methods like SLR and mean bias subtraction due to this nonlinearity in the bias and the ability for the machine learning techniques to recognize these patterns and better correct for them. As shown in the predictor importance scores of table 2, year does play a somewhat important factor along with other climatic variables like temperature and total precipitation. We agree with the reviewer that further descriptions of bias correction model performance (with respect to bias and RMSE) when trained/tested over these two separate periods (before and after 2014) would be beneficial, and therefore additional text describing the results of these comparisons has been added to the manuscript in section 3.3.

Specific Comment 2:

I think that the referencing (used in the Introduction and Discussion) can be improved. There are some relevant papers which are not addressed: e.g. Zahmatkesh et al. (2019) evaluating bias correction of SNODAS in Canadian basins or some studies cited in Lv et al. (2019) focusing on the accuracy assessment of SNODAS. Please consider to formulate how does this study compare to these studies (in Intro and Discussion sections).

Specific Response 2:

We thank the reviewer for recommending these relevant papers from Zahmatkesh et al. (2019) and Lv et al. (2019). These references have been added in the manuscript as additional motivation to our work in section 1 and section 4.2.

Specific Comment 3:

I have to say that the part related to evaluation of the impacts of different bias corrected SWE estimates on snowmelt is not clear to me. Using monthly estimates without accounting for evapotranspiration and other processes is somewhat less robust. Comparison of observed daily discharge with daily simulations driven by a hydrologic model will be more representative example.

Specific Response 3:

We thank the reviewer for this comment, as this point may not be immediately obvious: a direct comparison between SWE estimates and streamflow is not straight forward and presents a major methodological challenge, as outlined below. We will add additional discussion regarding the relationship between SWE, snowmelt, runoff and water balance estimates in section 4.1. The primary purpose of this section (and Figure 7) is to demonstrate that SNODAS SWE values are clearly too high and unphysical, especially during the time period before 2015, where estimated snowmelt exceeds total spring runoff in several cases. After bias-correction this is not the case anymore, suggesting that the bias-corrected values are at least plausible. The methodological challenge preventing direct validation of SWE estimates against streamflow gauges is the fact that runoff is generated by snowmelt and snowmelt has to be estimated from SWE changes. However, SWE also changes due to snow fall (and sublimation); snow fall, sublimation and melt occurring during the same time period cannot be separated easily (and can cancel each other). A better estimate of melt and runoff therefore would require additional data on precipitation, precipitation phase and/or temperature at high temporal frequency and a series of non-obvious judgements (such as estimating sublimation) would be required. This could be a topic of a potential follow-up study but is beyond the scope of this manuscript. A hydrologic or land surface model, which would be necessary to properly account for sublimation and evapotranspiration would not be helpful for this purpose, as these models compute snowpack internally and one would be left with a comparison against modeled snowpack (SWE). Furthermore, if SWE values from SNODAS were to be assimilated into the model, melt and runoff values would potentially be worse, since data assimilation violates mass conservation. As a case in point, we note that SNODAS also computes snowmelt internally, however, these values suffer from biases even larger than the biases in SWE. The reason for this is likely that snowmelt is not assimilated and at the same time artifacts are introduced by the assimilation of other variables (mass conservation is violated). Unfortunately, direct observation of snowmelt is not possible.

Specific Comment 4:

How to account for scale gap between SNODAS and in situ observations?

Specific Response 4:

In our analysis, we compare gridded estimates of SWE from SNODAS (1 km resolution) to snow survey estimates (which is essentially point data taken over 10 m). Due to the relatively high spatial resolution of SNODAS, along with the fact that the in situ measurement sites are taken at distances > 1 km from each other, we do not compare multiple in situ points to a single grid cell. This allows us to complete a simple point to grid cell comparison where we assume the snow survey SWE estimate is representative of the wider, containing grid cell. This assumption of representativeness across the grid cell introduces additional uncertainty, as SWE is highly variable at even small spatial scales, and we have therefore included additional details in the paper to make these uncertainties clearer to the reader in section 4.2.

Specific Comment 5:

Fig.1b. What do the lines represent? Mean over 383 stations?

Specific Response 5:

The reviewer is correct, the lines in Figure 1.b represent the daily mean SWE on ground for all survey locations (383 sites) across the full study period.

Specific Comment 6:

Fig.2,3,4,5. Please explain the meaning of abbreviations MBS, SLR, etc. in figure caption.

Specific Response 6:

We thank the reviewer for this comment, and we have included an additional description of the abbreviations for MBS, SLR, DT and RF in the caption of Figure 2.

References:

Lv, Z., Pomeroy, J. W., & Fang, X. (2019). Evaluation of SNODAS Snow Water Equivalent in Western Canada and Assimilation Into a Cold Region Hydrological Model. Water Resources Research, 55(12), 11166–11187. https://doi.org/10.1029/2019WR025333

Zahmatkesh, Z., Tapsoba, D., Leach, J., & Coulibaly, P. (2019). Evaluation and bias correction of SNODAS snow water equivalent (SWE) for streamflow simulation in eastern Canadian basins. Hydrological Sciences Journal, 64(13), 1541–1555. https://doi.org/10.1080/02626667.2019.1660780

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