

Interactive comment on “Converting Snow Depth to Snow Water Equivalent Using Climatological Variables” by David F. Hill et al.

Anonymous Referee #3

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The authors address the issue of converting spatiotemporal snow depth measurements to estimates of snow water equivalent (SWE). This topic is relevant to many areas of research because of the relative ease of taking snow depth measurements over SWE. Framed in the context of citizen science or field work, snow depths collected by non-experts and experts alike can be leveraged as a low-cost input to hydrological or climate analysis. In an era of high-availability altimetry (lidar or radar) and photogrammetry (structure from motion), an ensemble of methods to convert surface heights into SWE will be critical for both targeted basin studies (ASO) as well as future satellite missions.

The authors develop three regression models to evaluate a snow depth to SWE conversion. Regression skill is evaluated using depth alone, depth separated by accumulation and ablation phases, and depth in combination with climate normal for precipitation,

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temperature as well as elevation. Their work differs from previous studies such as Sturm et al. 2010 in that the climate inputs are regressed as continuous variables. As such, any measurement of snow depth with coordinates could potentially be converted, independent of measurement scale. In general the paper is well written and clear in its advancements. The focus on estimates during the ablation phase is a clear contribution, where methods fail. Addressing that 'not all snow is equal' is a strength of the approach.

Prior to publication, I would like to see the outlier detection and validation portions of the paper revisited to reinforce the statistical analysis. While I agree that outlier detection is necessary, an enhanced description of where and when the outliers originate would help to identify potential seasonality or spatial clustering. For example, if many of the outliers are from the early snow season, does this preclude ability of the models to convert measurements that include fresh snow? There are artifacts in Figure 4 where SWE varies drastically but depth does not, are these melt events? A histogram of the outlier DOY or a table of the outlier properties may be all that is needed to address this. These additions could be used to reinforce the statement that the reduced dataset is physically plausible (Lines 229-230). For the validation, it may be of benefit to use a cross-validation (CV) to determine if the model skill is overly optimistic. Using an N-folds CV with a 80/20 train/test split would be a simple approach to achieve this. In this regard, I'd also be interested to know if the non-SNOTEL datasets actually influence the regression coefficients (What happens when the training datasets are SNOTEL only). The remainder of my comments addressed to specific lines or figure.

Lines 64-65: Are there additional references available to support this statement regarding L-band? The only cited application in the field is a conference proceeding.

Line 172: Each style of corer has its own associated bias. Could this be considered to bound or constrain errors for each region/dataset?

Line 185: I would expect readers to be unfamiliar with some of coring devices. For

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example, the Mt. Rose snow tube could be supported with Church, J. Improvement in Snow Survey Apparatus, TAGU, 1936.

Line 228: See concerns about outlier detection in the main comments. It would be important to describe the temporal aspect of the outlier detection.

Line 228: uncleaned data -> source data

Line 229: State how many outliers were removed from the other datasets via this process. Figure 4: An axis label is needed for the DOY color bar.

Line 231: How does this work for 'stations' where there are a very low number of observations, ie AK?

Table 1: Can this table be augmented with a % of retained points or an omission %? Is the BC survey missing the # of ultrasonic sites?

Line 250: Is this 50% of all measurements or 50% of each subset. If it is all of them, it could be such that the only ones removed are CONUS because of the low numbers elsewhere.

Line 256: Figure 3 is used as support for the outlier detection due to poor correlation (ie increasing h with no SWE) and but is referenced here as strongly correlated. It might be confusing to do both.

Line 283: If this is an important consideration, why is the SCAN dataset not used to train the models?

Line 290: Interesting that a static 180 works best as the DOY separator. Could a sentence on why this might occur be added to the discussion?

Line 332: I see how it would not be possible to use an absolute value here but are snow-covered regions where the February normal is below -30C.

Figure 6: Titles for each plot might make this easier to read if someone skips the

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caption.

Table 5: Include the normalized errors for completeness of the table.

Line 423-430: Might be helpful to discuss measurement errors as a contributor.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-286>, 2019.

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