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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilation of satellite information in a snowpack model to improve characterization of snow cover for runoff simulation and forecasting" *by* L. S. Kuchment et al.

Anonymous Referee #1

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General comments

The work of Kuchment et al. presents a technique integrating satellite data and a snowpack model in constructing spatial snow water equivalent (SWE) field for improved runoff simulation. The authors show excellent knowledge of remote sensing, snow and runoff modeling. The paper is fairly well written, with some minor grammatical issues throughout the text. The paper would benefit from addressing several issues presented as follows.

Specific comments

First, the title of the paper could be more focused. The term "satellite information" is too general. The term "forecasting" is not really discussed in the following write-up.

Second, the motivation for the work needs more justification. There are numerous snowpack models and runoff generation models. What is the rationale of using the ones applied in the current study? In addition, the title tells that the ultimate objective of the study is to improve runoff simulation. However, description on current runoff simulation (e.g. models applied, simulation procedure in snow dominated areas, accuracy, etc.) is missing in the text. Why is it necessary to incorporate satellite data for improved runoff simulation? What if the current runoff simulation is satisfactory enough for practical purposes?

Third, the paper seems to be a bit unbalanced in terms of organization. Description on the snowpack model and runoff generation model takes up more than one third of the full length of the paper, while the models themselves are well established and are not the focus of the study. In comparison, the presentation of results and the discussion, which would be a major contribution of the work, should be significantly expanded.

Fourth, the snowpack model is calibrated against snow depth, but it is used to predict SWE. This might raise concerns about the validity of applying a depth-oriented model in predicting SWE. Discussion on this point would be helpful. In addition, the model is calibrated at point scale (19 meteorological stations) using ground-based measurements and applied to areal scale using gridded satellite data and interpolated ground-based data. Are model parameters calibrated from the point scale representative? Discussion on scale issues and the corresponding uncertainty would be helpful.

Fifth, initialization of the snowpack model (Section 4) could be improved. Specifically, assumptions on the initial volumetric moisture and snow density seem not robust. Using AE_DySno SWE map on March 1st as initial SWE is questionable as well since it is of low accuracy. Using a spin-up period might avoid the initialization problem (starting model simulation at a day within non-snow period before March 1st, and use the simulated states at March 1st as the initial condition).

Sixth, comparing the runoff generated from constructed SWE field (using the snowpack model and satellite data) with the AE-DySno SWE derived runoff is an acceptable option, but could certainly be improved. According to the authors, "the estimated accuracy of SWE is 25%......This accuracy may substantially decrease in forested areas and during snow melt…… " (p.5509). Yet, the inaccuracy in SWE could propagate to runoff during runoff simulation. Therefore, the resulting snowmelt hydrograph is questionable. The paper would be stronger if alternative hydrographs are available for comparison. A candidate might be the one simulated from the snowpack model without using satellite data.

Last, visual comparison is helpful. However, statistic metrics (e.g. root mean square error, bias, etc.) would provide a more direct way to quantitatively evaluate the potential benefit of the SWE field derived from the proposed technique. It is recommended that these metrics are calculated and discussed throughout the calibration, validation, and simulation practices presented in the study (Figures 5, 9, and 10).

Technical corrections

(1) Both "physical based model" (e.g. p.5506, line 5) and "physically-based model" (e.g. p.5506, line 20) are used in the text. It is better to make them consistent.

(2) p.5506, line 14, are "maps of surface albedo" weekly? From Table 1, it might not the case.

(3) p.5506, line 21-22, the "spring seasons" should be specified. In addition, should the comma in "2003, 2005" be "and"?

(4) p.5508, line 29, the ")" before "(Fig.1)" should be deleted.

(5) p.5509, line 4, "The snow seasons lasts…" needs revision.

(6) p.5509, line 20, "(Chang, Rango, 2000)" should be "(Chang and Rango, 2000)".

(7) p.5511, lines 9-10, is snow depth (used for model calibration) also measured at the ground-based stations?

(8) The article could use a grammatical review for all the equations described. For example, there is a comma before "(5)" (p.5512, line 11) which is not necessary; p. 5515, "was defined as" is used in line 3 while "is defined as" is used in line 10; ":" is used before some equations (e.g. Eqs. (5), (6), (10), (12), etc.) but not for others (e.g. Eqs. (7), (14), (10), (21), (22), (26), etc.); p.5514, line 2, there should be a space between "k" and "is von Karman's constant" (same case for line 4, p.5514 and line 7, p.5516, among others); p. 5514, line 8, "Eq. (28)" is not correct; p. 4415, line 23, a period is missing in the end; p.5516, lines 11-12, grammatical check is required.

(9) p.5516, lines 15-16, model input, output, simulation time step could be specified.

(10) p.5517, line 4, "random fitting procedure" needs clarification. Necessary references can be cited.

(11) p.5517, lines 8-9, the cold season is from November to June as told from Figure 5. However, in p. 5509, lines 3-4, the cold season is defined as "(October to March)". It should be consistent.

(12) p.5517, lines 9-11, what is "standard error"? "training dataset"? "independent dataset"? Are "7 cm" and "9 cm" site-averaged values? Other statistic metrics can be calculated here at each station. More discussions on calibration and validation results would be helpful.

(13) p.5517, line 14, "spring season" needs clarification (how is it defined?).

(14) p.5517, line 15, "model was run for each 0.01 grid cell…", how is the SWE map ("March 1 data is used as initial SWE", p.5517, lines 23-24, which has the grid size of 0.2 degree) incorporated into the model?

(15) p.5518, lines 2-3, “winter season” needs clarification.

(16) p.5518, lines 9-11, the statement is too strong. It is valid for the studying period (2000-2004), but might not be valid “from year to year”.

(17) p.5518, “Figure 7” is used in line 23 while “Fig. 7” is used in line 26. It should be consistent.

(18) p.5519, line 8, “...to simulate runoff generation”. “generation” here could be deleted.

(19) p.5519, line 18, “Eq. (1)-(26)” should be “Eqs. (1)-(26)”.

(20) p.5522, “Fig. 9” is used in line 1, and “Fig. 10” is used in lines 8 and 11. Previously in the text, “Figure” is used instead. It should be consistent.

(21) p.5522, line 12, “...accuracy of hydrographs simulation” grammatical check is required.

(22) p.5522, line 13, “maximum error” needs clarification.

(23) p.5522, lines 14-16, note that the constructed SWE lead to overall underestimation as well. More discussions (e.g. uncertainty in inputs, initial conditions, parameters, etc.) here would be helpful.

(24) p.5531, Fig. 5, numbers and texts on the axis are hard to read. Changes to font size are recommended.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 5505, 2009.