Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-551-RC2, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modelling hydrologic impacts of light absorbing aerosol deposition on snow at the catchment scale" *by* Felix N. Matt et al.

Anonymous Referee #2

Received and published: 22 January 2017

General Comments:

Major revisions are necessary before this paper is to be published. While I appreciate the various sensitivity tests to try and isolate the impact of various parameters as they relate to impact of light absorbing impurities (LAI) in snow in models, some are not executed or interpreted correctly in this study. The implementation of LAI in snow processes in a hydropower forecasting model, as attempted in this work, is important, as is indeed mostly lacking. However, I don't quite agree with some of the phrasing in the Introduction that claims LAI in snow in hydrologic models (or land surface models that have physically-based hydrologic processes) has been up to now understudied or lacking. Many examples can be found in Qian et al., 2015 (AAS), Light-absorbing particles in snow and Ice: measurement and modeling of climatic and hydrologic impact.

Generally, here are some of my concerns or things that are unclear based on the current manuscript: 1. Paper is very hard to read, and logic often hard to follow. There are many run-on sentences that are very wordy. Some terms seem to be used to refer to various different processes, and need clarification; e.g. "forcing" is at times used as LAI in snow forcing, whereas during other times it is used to refer to meteorological forcing. Being clearer in explanations is needed. 2. I also found the organization of the paper to be cumbersome, which plays into readability of the manuscript. a. For one, Section 2 on Modeling Framework is difficult to piece together, to understand how various components of the framework work together, and how the actually set up this modular "model platform for hydrologic purposes". Clear definitions of model setup and model meteorological inputs (sec 2.1) need to be specified. b. In addition, authors often describe several methods or quote values for parameters, but don't clearly state which they end up using in present study, or what modifications have been made. Reader is left lost in previous studies or potential methods. e.g. sec 2.2.3, or p8 – SNICAR implementation in hydro model not clearly laid out. 3. While it is true LAI in snow play a significant role on energy and water balance across many mountainous regions throughout the world, the authors don't state if this is in fact a problem in Norway, or for the catchment they chose for the case study. What is the motivation for choosing this catchment, if LAI in snow observations are lacking here, making drawing realistic conclusions difficult (which even they admit e.g. p22 lines 22-23)? Why not do a case study with this new model over a region that does have in situ observation of LAI in snow, e.g. Painter et al, 2012 (Dust radiative forcing in snow of the Upper Colorado River Basin: 1. A 6 year record of energy balance, radiation, and dust concentrations, WRR); or Kaspari et al., 2015 (Accelerated glacier melt on Snow Dome, Mount Olympus, Washington, USA, due to deposition of black and mineral dust from wildfire); or Zhao et al., 2014 (Simulating black carbon and dust and their radiative forcing in seasonal snow: a case study over Northern China with field campaign measurements). 4. If I understand their modeling framework correctly, BC deposition is decoupled from meteorological forcing applied, making the entire discussion of distribution of wet deposition of BC difficult to rationalize realistically. Authors should at the very least discuss how this decoupling impacts their results and conclusions. I am also a bit concerned with the fact that BC is only deposited in snow during the accumulation phase. In reality, BC deposition, and LAI in general, in snow does not suddenly stop with melt onset, and in fact, certain LAI species such as dust are mostly deposited during the springtime (e.g. Painter et al., 2012, WRR) and therefore during the snowmelt period. This means deposition of LAI in snow during melt season play a significant role in melt magnitude and timing (and not only during accumulation). If deposition of BC in snow tends to indeed occur mostly during accumulation in Norway, or in the catchment in their case study, then authors should state that as an explanation (with appropriate references) for why they set up their experiments the way they did. 5. Authors use the phrasing of "addition of deposition rates of LAI" throughout manuscript (e.g p1 ln5, p23 ln 8) as a way of communicating the improvements they contribute – this is misleading, as what they really use is the LAI mass and concentration. By using "deposition rate" they suggest they are improving the atmospheric to surface deposition process, the rate and temporal distribution of LAI in snow, when really they are implementing a way for hydrologic model to account for LAI in snow (and in fact actual deposition and precip inputs are decoupled here). This needs to more accurately be represented throughout the paper.

Specific Comments:

P1 ln 12-13: confusing sentence; what is meant by "melt limitation" . . . this confusing term remains confusing throughout the paper (e.g. p20, ln20). A clearer description of the concept is needed.

P1, abstract: "Central effect" or "min, max, mid effect estimate" terminology hasn't yet been described, so use in Abstract leads to reader being confused as to what it's referring. Ln 14: "The central effect estimate produces reasonable surface BC concentrations in snow" The effect produces BC concentrations? Wouldn't BC in snow be the element producing an effect? Re-word sentence with clearer statement.

P1, ln 20: what's the difference between "mountainous" and "high mountain" – is there a need for mentioning both environments? Not the same?

P1 ln 24: "affected areas" is not adequate use of the phrase – these areas didn't experience an extreme event, and were not "affected". Suggest removing word "affected"

P2 ln 5-7 and ln 7-9: need references.

P2 Ln 18-19: statement needs reference.

P2 Ln 24-28: what's the point of that lit review? How does it impact the present work? What did you take from it, or how did you improve it?

P3 ln 1-2 and that paragraph in general: "investigating the impact of LAISI on the snow melt and runoff predominantly use empirical formulations to investigate the impact of LAISI on the radiative forcing in snow, by observing the net surface shortwave fluxes over snow and identifying the contribution from the LAISI through determination of the (hypothetical) clean snow albedo" – inaccurate, misrepresents previous work and the context of this work. There have been several improvements to LAI in snow representation in hydrologic or land surface models in recent years (albeit further developments continue to be needed), e.g. Zhao et al, 2014 (ACP), Oaida et al., 2015 (JGR), and see Qian et al., 2015 (AAS) for a more complete list and overview of observations and modeling of LAI in snow. Authors of present study show reframe their motivation or gap their new work is filling given these previous developments.

P3 ln 13-15: Entire sentence is awkward; what is meant by "complex abstractions"?

Sec 2.1: clearly state what variables the model needs as inputs (this is later alluded to on page 11, but needs to be more clearly stated under Modeling Framework section)

P4 ln 11: what is meant by "efficient simulation"?

P4 ln 12: why is ET module important? The whole modeling setup needs to be more clearly defined and laid out

P8 ln 1-9: how exactly did you integrate SNICAR within hydrologic framework? Which variables in 2.2.1 were updated by SNICAR output . . . and what does SNICAR output? P8 10-17: how is r connected to radiative transfer model? Where is r used in your implementation?

P10 ln 9-14: A bit unclear how these tiles are defined? Is it based on elevation? Also, "In our model, we further developed an approach assuming that the spatial distribution of each single event of 10 solid precipitation follows a certain probability distribution function." This newer approach is based on which previous method? What did you further develop?

P10: the concept of "multiplication factor" is not quite clear.

P11, ln 17-30+: is the REMO simulation ran offline, separately from the hydrologic model? Is there a discrepancy between deposition timing in REMO and hydrologic model meteorological precipitation input/events? How does that affect your study? (Also see General Comment 4 above).

P11, sec 3.1: what is the simulation period for hydrologic model, vs for REMO? Might want to even state the hydrologic model simulation period more clearly a bit earlier in the paper, in the intro to Section 3, before 3.1.

P12 ln 10-12: run on sentence. Please revise.

P12, ln 24-25: why did you chose to only deposit BC during accumulation period, and not throughout entire simulation period, or at least during both accumulation and ablation periods? Also see General Comment 4.

P13, ln 9-11: Sentence should be better integrated, and phrased more grammatically correct.

P14 ln 2-3: is BC distributed throughout the top layer, or entire snowpack?

P14, ln 19: what do you mean by "all free model parameters"?

P15, ln 4-6: confusing sentence. "The central graph in Fig. 3a shows that the choice of the maximum surface layer [insert "thickness"] strongly determines the increase in the [insert "magnitude of"] surface concentration over the melt season - leading to a strong increase in surface BC until [insert "through"] the end of the melt season with an increase in BC by a factor of circa 15, 30 and 60 for maximum layer thicknesses of 4.0, 8.0 and 16.0 mm, respectively, compared to the pre-melt season BC concentration."

P15, ln 4-9: These 2 sentences, if I understand correctly, seem to be at odds with each other: the latter, "The thinner the surface [. . .]" implies that the 4mm layer selection would have the strongest effect, yet it's only increasing BC by a factor of 15, smallest of them all.

P15, ln 19: "the mean radiative intensity diminishes with depth due to absorption in snow and LAISI and scattering, leading to a less effective absorption of LAISI in deeper snow. " needs a reference

P16, ln 21-24: what about new BC deposition? You mention on the ways the output of LAI from snowpack affects end of season LAISI amount, but what about the input, which may vary through time, and which again brings me back to general comment 4.

P18, section 5.1.4: The number of earlier meltout should probably be scaled by total length of meltout season of each snowpack to more realistically and accurately represent the impact of snowpack thickness

P19, ln 1-24: this entire section is rather convoluted and the conclusions not easy to follow. Because of that, some of the results seem at odds with each other. Please reorganize and be more concise in your analysis. . . . ln 7: "total sum of daily discharge" refers to net annual sum of runoff? And it is about zero? Yet later in the paragraph the % change increases? Perhaps I am misunderstanding the stats – a more clear explanation would be helpful. One idea is to also put all these values in a table, for easier comparison. You mention ET, is there a plot to support the conclusions you are mentioning?

P19, ln 30: wouldn't the 1.5, 5.1, and 10.3 mm values be negative?

P20 ln 3-8: I am not quite sure what you are trying to say about having an analysis at the catchment scale. The links you are trying to draw don't seem that obvious or easy to follow.

P21 ln 2-4: scavenging ratio is not the only factor determining if BC accumulated in top snow layer. What about new snow?

P21, ln 26: "Qualitatively, [. . .]" – this sentence is not a very strong, supported, conclusion.

P21, ln 13-19: reason (iii) is rather confusing. The whole concept of "wet deposition" of BC in this explanation doesn't quite add up for me when this study has BC and precipitation "falling" separately (processes decoupled). It's possible I am misunderstanding the explanation, which might suggest a more clear explanation would help.

P23, ln 1-5: I would argue that normalizing SCF isn't necessarily more relevant to inpact on runoff, as total surface albedo (both snow and snow-free surfaces) influences snowmelt thought the snow-albedo feedback.

P23, ln 12-15: "The maximum thickness (in SWE) of the surface layer herein has rather little effect on the snow albedo and melt rate as long as the maximum layer thickness is sufficiently small." – is this clean snow, or LAISI case? "However, the evolution of the LAISI surface concentration is highly sensitive to the choice of the surface layer extent." If LAISI concentration is affected by snowpack thickness, then wouldn't snow thickness, somewhat indirectly, affect albedo, since surface snow layer LAI impact snow albedo?

P23 ln 27-29: I am not sure the evidence presented is enough to conclude improvement in hydrologic modeling. The shift found by comparing LAISI and no-LAISI scenarios certainly suggests an impact of LAISI on discharge timing, but one would have to compare LAISI, no-LAISI, and observed runoff over same period of time to conclude that a hydrologic model with LAISI processes present brings simulated runoff closer to

observations, over the no-LAISI simulated runoff. You could add no-LAISI discharge to figure 5 to have a more robust conclusion on model improvement.

Technical Corrections:

"LAISI in snow" is used in several parts of paper (e.g. p8 ln 18), which is redundant since LAISI already contains "in snow" by their own definition. Please revise.

P4 ln 2: too many "hydrological"/"hydrologic"/'hydropower" terms in one sentence. Please revise.

P5 ln 27: "central addition" is awkward. "Main addition"?

P15: word "Stronger" is repeated 2x. Remove one.

P22 ln 20: "are" is repeated 2x back to back.

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