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Interactive comment

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

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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.

RESPONSE:

We acknowledge that the sensitivity study partly needs to be revised and further discussed. Please see the response to general comment 4, and the responses to the specific comments on P12, ln 24-25; P16, ln 21-24 and P18, section 5.1.4 further down for more details.

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Discussion paper

The implementation of LAI in snow processes in a hydro-power 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.

RESPONSE:

We acknowledge a lot of work has been done on the topic of LAI, largely with respect to climate. We are working at a different scale, however, and also – to our knowledge – applying for the first time an 'online' calculation of the albedo response to aerosol deposition in a hydrologic forecasting framework. Prior analyses have applied prescribed albedo forcings, or have used land surface models. Regardless, we will make a better effort to place the work in the context of the extensive existing literature.

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.

RESPONSE:

* To "Paper is very hard to read, and logic often hard to follow": This comment appears in all three reviews given – we take it very serious and will accordingly work together to improve the structure and the conciseness of the writing.

* To "forcing": We use the expression "forcing" to describe the meteorological forcing

of the model, in particular the input variables temperature, precipitation, wind speed, relative humidity and aerosols deposition. Furthermore, the expression radiative forcing is used to describe the additional energy uptake from solar radiation by snow due to light absorbing impurities in snow and ice (LAISI), compared to snow with the same properties, but without LAISI. We will clarify those two definitions in the text and replace misleading statements with the correct expressions.

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.

RESPONSE:

We will work on the readability of Section 2 and clarify how the model is put together. This goes in hand with removing unnecessary repetitions of methods-details, outlined in other studies (e.g. Kirchner, 2009 method in Sect. 2.1). By shortening the methods parts to the core methods we used, we hope to increase the understandability and the readability of this section.

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.

RESPONSE:

To give the reader a better overview, we will give a summary table of parameters (+ values) used in the model study. Furthermore we will more specifically line out the methods used.

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).

RESPONSE:

The bodies funding our activities, are interested in the potential impact of BC deposition to hydropower operations in Norway and India. We are working in both regions, however, data paucity presents a challenge in both cases. For India, hydrologic data for the regions of interest are challenging to obtain. For Norway, as you mention, there are sparse observations of BC in snow. We selected Norway initially due to the high quality hydrologic data and availability of deposition model output for the region. The modeling and observations available to validate the BC transport were published and found scientifically robust (e.g. Hienola et al., 2013), so we selected to use this region initially.

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 real-

ity, 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.

RESPONSE:

* To “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.”: We acknowledge this concern, and in fact it is common to other reviewers. Please see our response to Reviewer #1, comment 1. We will address it within a revised manuscript.

* To “I am also a bit concerned with the fact that BC is only deposited in snow during the accumulation phase” In our experiments, we aim to show the contribution of different model parameters and settings to the accumulation of LAISI in the top layer and the resulting differences in the response. For this reason, we try to exclude factors that have the potential to mask the isolated effects or lead to speculative results. One of those factors is the input of aerosol to the snowpack via deposition during the melt period. Furthermore, we in our experiments we investigate the snowpack evolution under idealized conditions, e.g. no precipitation during the melt period. For this reason, we don’t expect a large input aerosol from deposition to the snow pack during the melt period, since by far the largest fraction of aerosol deposition is from wet deposition. This idealization is limited to the sensitivity study. In the case study, we use aerosol deposition as prognosed by REMO-HAM on a daily timestep.

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.

RESPONSE:

We need to clarify our text to explain that we are in fact using deposition rates as input time series to our model. This is in fact original, and as we indicate: "allows for an additional class of input variables" (p1 ln5, p23 ln 8). By stating " new class of input variable", we intended to make clear that we provide the possibility for a new 'forcing variable' but never claim we are "improving the atmospheric to surface deposition process". Clearly, some improvements in our wording our required and will be included in a revised manuscript taking into consideration this comment.

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.

RESPONSE:

In our case study, the discharge of the scenarios where BC is applied (ARF scenario) is lower compared to the scenario where no BC deposition is applied (no-ARF scenario), even though the snow albedo is lower in the ARF scenarios. The reason for this is that even though the potential melt in the catchment is higher in the ARF scenarios, the actual melt is lower simply due to a combination of lower snow storage in the catchment and lower snow covered area in the ARF scenarios – the melt is limited by this. This is what we refer to as “melt limitation”. We will reword and clarify this in the text.

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.

RESPONSE:

We will reword the sentence and clarify the statement.

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

RESPONSE:

We will remove “high mountain”.

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”

RESPONSE:

We will remove the word “affected”.

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

RESPONSE:

We will add the required references to the revised paper.

P2 Ln 18-19: statement needs reference.

RESPONSE:

We will add the according 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?

RESPONSE:

Flanner (2007) estimated scavenging ratio's for hydrophilic and hydrophobic BC based on work done by Conway et al. (1996). We use the values estimated by Flanner (2007) in our study to simulate melt scavenging of BC (see Eq. 15 and 16 in Sect. 2.2.2, Aerosols in the snowpack).

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.

RESPONSE:

As mentioned above, we acknowledge that the introduction needs to be revised and a broader overview about significant contribution from recent literature needs to be given. We will provide this in the revised paper. However, including the literature in the here stated comment, there is to date no hydrologic catchment model allowing deposition of LAISI as additional meteorologic forcing. The motivation of our work is thus justified and we are convinced to fill an important gap with the contribution of our model.

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

RESPONSE:

Maybe a better choice would be “increasing complex representation of the physical processes”

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)

RESPONSE:

Meteorological forcing: Temperature, precipitation, wind speed, relative humidity, radiation, aerosol deposition. We will add this to Sect. 2.1.

P4 ln 11: what is meant by “efficient simulation”?

RESPONSE:

With “it is optimized for highly efficient simulation” we mean “computational efficient”, in the sense that it uses computational resources very efficient (or in other words: simulations run fast). We will clarify this in the text.

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

RESPONSE:

Yes, we acknowledge an improved discussion of the model framework is required.

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?

RESPONSE:

Our SNICAR implementation calculates the broadband hemispheric reflactance of snow (“snow albedo”) as function of

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* snow optical grain size

* solar zenith angle

* thickness of the snow layers (in mm SWE)

* mixing ratios of hydrophilic and hydrophobic BC in each of the layers

which are calculated each time step by the snow routine. SNICAR is called in an intermediate step and used to update the snow albedo, before the time step's energy and mass balance is calculated. We will clarify this in the text.

P8 10-17: how is r connected to radiative transfer model? Where is r used in your implementation?

RESPONSE:

r is the optical grain size of snow, one of the input variables to SNICAR. The snow albedo strongly depends on r. We will add to the text what role r plays in determining the snow albedo.

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 solid precipitation follows a certain probability distribution function." This newer approach is based on which previous method? What did you further develop?

RESPONSE:

The tiles are a representation of subgrid snowpacks, used to represent the subgrid snow distribution. Each solid precipitation event is assigned to those tiles, according to a multiplication factor. The multiplication factor for each tile is based on a gamma distribution, assuming that the the subgrid spatial distribution of precipitation is well represented by this distribution. The coefficient of variation of each grid cell, which defines the gamma distribution, originates from work done by Gisnås et al. (2016)

[Small-scale variation of snow in a regional permafrost model]. The method is similar to the one used in Aas et al. (2017) [A Tiling Approach to Represent Subgrid Snow Variability in Coupled Land Surface–Atmosphere Models]. We will try to describe this more clear in the text and add the missing Aas et al. (2017) reference.

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

RESPONSE:

As described in the comment above, we will re-write this paragraph and describe the concept in more detail. The Aas et al. (2017) reference also should help to clarify the concept.

P11, In 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).

RESPONSE:

The REMO simulation ran offline, separately from the hydrologic model. We acknowledge that we should include a discussion about precipitation timing in REMO and the used observations in the hydrological model, and the resulting implications for our study.

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.

RESPONSE:

Hydrological model: 01.09.2006 to 31.08.2012

REMO-Ham: 01.07.2004 – 31.12.2014

The discrepancy between the two periods might lead to some confusion. The important information is that the REMO-Ham simulation period covers the hydrologic simulation period – we might only state the hydrologic simulation period in the paper, and that we have full coverage of this period from REMO-HAM simulation.

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

RESPONSE:

We will reword the sentence.

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.

RESPONSE:

Since we melt the snowpacks under idealized conditions, e.g. undisturbed from precipitation (sold and liquid), this is realistic in the scenario in the sense that BC input mostly happens as wet-deposition, and as such during precipitation events. The idealized conditions are required to identify the contribution of certain model concepts to the evolution of BC concentration and impact on melt. However, we should discuss this during this in the text and also discuss the implications of idealized versus real conditions. In the case study, we use of course continuous input data from Remo-HAM.

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

RESPONSE:

We will reword the sentence.

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

RESPONSE:

The BC is uniformly distributed in the snow at melt onset, such that the mixing ratio of BC is the same in both layers. We will clarify this in the text.

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

RESPONSE:

Model parameters/tuning parameters that are estimated during the calibration process of the simulation. We will clarify this in the text.

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.”

RESPONSE:

We will reword the sentence accordingly.

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.

RESPONSE:

Thank you for catching this error. The correct statement is: “... increase in BC by a factor of circa 15, 30 and 60 for maximum layer thicknesses of 16.0, 8.0 and 4.0 mm, respectively,...” instead of of “... 4.0, 8.0 and 16.0 mm, respectively, ...” (as shown in Fig. 3a). We will change this in the paper.

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

RESPONSE:

e.g. Warren and Wiscombe (1980) [A Model for the Spectral Albedo of Snow. II: Snow Containing Atmospheric Aerosols] and Flanner et al. (2007) [Present-day climate forcing and response from black carbon in snow]. We will add a reference to support this statement.

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.

RESPONSE:

Again, this is a good comment and it should be discussed in the paper. However, since we use idealized conditions with a melt period which is not interrupted with neither snow nor rain events, the exclusion of BC input to the snow pack is arguable, since the main mechanism contributing to BC input in the snow pack is due to wet-deposition.

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

RESPONSE:

This is correct. We plan to include this in the paper.

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?

RESPONSE:

“total sum of daily discharge” refers to the sum of daily discharge over the simulation period (so the sum over several years, not only the annual sum). This is the same for all scenarios. Our argue is then that it follows that the impact on the ET between the different scenarios is negligible – but we can look deeper into this and support our argument with a plot. Furthermore, we see that differences in discharge of our ARF scenarios to the no ARF are counter balancing, meaning that a decrease of discharge in the beginning of the melt season is followed by a decrease later in the melt season (comparing ARF with the no-ARF scenario). By splitting up the melt season into those two periods, we quantify these increases/decreases. This is visualized in Fig. 7b.

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

RESPONSE:

That is correct. We will correct this.

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.

RESPONSE:

We acknowledge that the paragraph requires rewording and a more clear explanation of our intentions.

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

RESPONSE:

This is correct, and we will mention this in the text. However, the during the melt period (and that's what we refer to here), fresh snowfall doesn't play a large role, as one can see in the continuous drop of SWE during the melt season in Fig 7a).

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

RESPONSE:

We will revise this sentence (also compare with comment on "pg. 21, lines 26-29" of reviewer #1).

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.

RESPONSE:

Even though we use decoupled precipitation and wet deposition, we expect observed daily precipitation (used in the hydrologic model as meteorological forcing) and wet-deposition from REMO-HAM to be consistent. Since we calculate BC mixing ratios in falling snow before redistributing it to the tile level, we think that the discussion referred to herein is legitimate (also compare with the response to comment 1 of reviewer #1). We do appreciate this comment, however, and intend to include a more inclusive investigation and discussion of these aspects in a revised manuscript.

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

RESPONSE:

Since our model is not coupled to an atmospheric model, no feedback between the land surface and the atmosphere is represented. Thus, the albedo of snow-free surfaces does not impact runoff through the snow-albedo feedback. However, the evapotranspiration is impacted – which then has implications for the discharge generation. We will ad this to our discussion.

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?

RESPONSE:

To " is this clean snow, or LAISI case?": LAISI case. We will clarify this in the revised manuscript. To "wouldn't snow thickness, somewhat indirectly, affect albedo, since surface snow layer LAI impact snow albedo?" This is correct – the choice of the maximum layer thickness has an impact on the snow albedo – primarily due to LAISI accumulation in the surface layer during melt. We discuss this in the sensitivity study (see Sect. 5.1.1).

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.

RESPONSE:

We acknowledge the weakness in our conclusion and will consider the suggestion for improving our reasoning in the revised manuscript.

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.

RESPONSE:

We will remove “in snow”.

P4 ln 2: too many “hydrological”/”hydrologic”/’hydropower” terms in one sentence. Please revise.

RESPONSE:

We will revise the sentence.

P5 ln 27: “central addition” is awkward. “Main addition”?

RESPONSE:

We will replace “central”.

P15: word “Stronger” is repeated 2x. Remove one.

RESPONSE:

We will remove one.

P22 ln 20: “are” is repeated 2x back to back

RESPONSE:

We will remove one.

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