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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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General comments

My knowledge of hydrological models is not broad, so I do not believe I am qualified to comment on the viability and implementation of the model. However, I have commented on the structure, content and more scientific issues that I see in this article. My first criticism is that the paper is long and should be shortened and restructured.

RESPONSE:

We received criticism about the structure and length about our manuscript from all referees – and take this criticism accordingly serious. We work together and will reword and restructure parts of the paper. This includes:

* Rewording the introduction, with focus on citing recent published literature that is of interest for the here presented paper, and that have been missing in the first submission. Also, we will move some of the aspects (espacially LAI in snow physics) mentioned in the introduction to the methods part.

* Shortening the methods part: We will lay more focus on our approach of handling LAI in the snowpack and the implementation of SNICAR in our model and remove large parts that are not necessary for the here discussed implementation.

* Furthermore, we will focusing on putting our research in the context of other work.

Secondly, I did not find a useful quantification of how LAI from the ARF model are integrated in to the snowpack, as no field measurements of LAI from the area are available.

RESPONSE:

We calculate BC mixing ratios in snow from wet- and dry-deposition fields determined with REMO-HAM. Similar REMO-HAM simulations (similar setup and same region) and observations available to validate the aerosol transport were published and found scientifically robust (see Hienola et al., 2013). However, we acknowledge the need for a better discussion of our results – especially the magnitude of order of BC mixing ratios in the surface layer throughout the melt season in the case study. This includes the comparison with observations of BC mixing ratio in snow collected in the proximity of our study region (e.g. Forsström et al., 2013).

Also, better parametrization of dust sources is needed.

RESPONSE:

We use BC as only source in our simulations – however, we will add a discussion how this simplification is impacting our results – in particular how this is influencing the impact of BC on albedo and snow melt (additional other LAISI lower the impact of BC) and the overall impact of LAISI on BC (the overall impact of LAISI on albedo and snow melt would be higher than our model suggests).

My final criticism, and one I take very seriously, is that the authors fail to cite and recognize substantial research that has been done in this field, leading to comments in the text that I believe to be speculative. Additionally, the authors only briefly put their research in the context of other work on the subject matter, both modeling studies and field observations, which further needs to be addressed. Although I acknowledge that implementing processes observed in field observations is not always possible or practical in numerical models, as this model attempts to quantify and reproduce physical processes in the snowpack, far more heedance must be paid to this body of research.

RESPONSE:

We will review and reword our paper with a focus on excluding “speculative” conclusions. Furthermore, we will work on our discussion part of the paper to better put our research in the context of published work on the subject matter.

I have made specific comments to these issues in the section below. Although, the paper is not publishable in its present state, I believe that this model when presented clearly and in a manner that is standard to scientific papers, has the potential to serve as a valuable tool and compliment other models that integrate the dynamics of light absorbing impurities into snowpack evolution and hydrology.

RESPONSE:

Thank you for recognizing our contribution. We do feel strongly this is a unique contribution, and one that is missing presently from the hydrologic modeling community (more so than from the climate modeling community). We hope to address this deficit.

Scientific Comments

Introduction. I would recommend commenting more on the state of hydrological modelling and the need for integrating LAI into these models. Much of the information regarding snow physics can be condensed and put into the methods section.

RESPONSE:

We will reword the introduction and put more focus on the state of LAI implementation in hydrological model and the gap of knowledge that we address.

Pg 2, Line 14. This is an example of a comment that needs to be cited. Warren and Wiscombe present a model about snow, they do not address BC sources in a comprehensive manner. Mahowald, Ramanathan and Bond are some of the researchers who have explored this topic.

RESPONSE:

We will give an appropriate reference for this statement.

Pg 2, Line 17. This topic has been discussed in several recent papers including Xu et. al. 2012, Hadely et. al. 2007, Delaney et. al. 2015, Sterle et. al. 2013, Skiles et al. 2016, and Adolph et. al. 2016. Additionally, this topic might be better put in the method section describing scavenging parameters.

RESPONSE:

We will move most of the paragraph (including the here cited statement) into the methods part. We will furthermore put the topic into a broader context, including the above mentioned references.

Pg 2, Line 30. I think that Kasapri et al. 2015 did relevant work on this topic.

RESPONSE:

Assuming Kaspari et al. (Accelerated Glacier Melt on Snowdome, Mt. Olympus, Washington due to Deposition of Black Carbon and Mineral Dust from Wildfire) we acknowledge similarities in topic. However, they estimated the impact on snow melt and runoff by doing a “first-order estimate of the impact on snowmelt by doing a simple energy analysis.” which is quite distinct from this work. We respect, however, it should be cited.

Pg 3, Line 3. From what I understand their albedo measurements are largely done with a spectrometer which calculates albedo over a broad range of values. I do not think that 'hypothetical' or 'empirical' are the proper descriptions of their methods.

RESPONSE:

Their results are in fact empirical using the definition: "a relationship supported by experiment and observation". We only wish to show that the prior approaches are using observations and prescribing albedo changes, rather than including on online calculation of albedo based on LAISI deposition rates. We will refine the text in consideration of this comment.

Pg 3, Lines 16-27. Here I think this needs to be clearer about the lack of knowledge in this field and the specific accomplishments of this article in reducing this knowledge gap.

RESPONSE:

We will revise and reword the paragraph with focus on the specific accomplishments of this article in reducing the raised knowledge gap.

Pg 4, Lines 2-10. Please provide a more detail description of Shyft, are there other papers that have used it? If so, please cite . Also, if appropriate please outline your addition to the model framework here.

RESPONSE:

SHyFT is a new Hydrological model framework developed by Statkraft (https://github.com/statkraft/shyft). We are currently working on a manuscript.

Pg 5, Section 2.2. I think that this section should be condensed and restructured. I found much of the energy balance work to well known and possibly a bit too much detail. Also I believe that your contribution should be clarified from those whose work you implement.

RESPONSE:

This will be part of the restructuring and rewording of the methods part. Since many of the Energy balance formulations are well know, we will shorten this part of the methods an focus on the description of our implementation to the existing model framework.

Pg 8, Lines 10-15. For the description of grain size evolution, did you develop this? or is this from someone else? If so, please cite. Has this method been applied to other studies, if it was, how well did in manifest real snow conditions?

RESPONSE:

We accidentally stated the wrong equation in the paper. In an older version of the current snow routine, we were using this equation, which we developed by ourselfs. However, we then changed to a formulation by Taillandier et al. (2007) for dry snow and Brun (1989) for wet snow, on which our here presented model results are based on. This formulation has been used in other studies, e.g. Gabbi et al. (2015). We will change the paper accordingly and add the correct equation and references.

Pg 9, Lines 13-31. I would recommend looking into other work about scavenging including Xu et. al. 2012, Delaney et. al. 2015, Sterle et al. 2012, Schwarz et. al. 2013. It is worth noting that the Conway et. al. 1996 experiments used synthetic soot, with properties and particle size distributions that may not occur naturally. Although Conway et. al. 1996 is an important paper, other such work has been done on this subject and should be considered. Additionally, I would recommend moving this section to a part that discusses the sensitivity study.

RESPONSE:

We will move most of the here described to the discussion of the scavenging ratio sensitivity study, including a discussion of the above listed literature.

Pg 10, I gather that there are 3 parts to your models, the hydrology component, SNICAR, and your addition. I think the interaction of these components should be better described. Would it be possible to make a figure of this?

RESPONSE:

This is correct. SHyFT provides the model stack, which defines the hydrological model. We exchanged the "default" snow-routine in the model stack with the snow routine we developed. A part of our new snow routine is the couplinig of to SNICAR: The snow routine handles alongside standard energy balance and mass balance calculations the mixing ratio of aerosols in the snow pack, the zenith angle of the sun and the optical grain size of snow, which are input to SNICAR. From this, SNICAR calculates and returns the broadband albedo of snow – which is then used in the energy balance calculations of the snow routine. We will make this clear by adding a more detailed description and consider to support our description with a sketch of the coupling.

Pg 11, Line2 16-33. A couple sentences from about Pietikäinen et al. 2012 would be good. Although, it is not my field of study I understand that dry deposition rates are quite poorly constrained, could you comment on this? Also, the REMO-HAM simulation period lies outside of the study period. Why?

RESPONSE:

We will add some more detailed information about REMO-HAM from Pietikäinen et al. 2012 and a short discussion about limitations (e.g. problems with dry deposition handling in REMO-HAM) and how this potentially effects our results.

To "Also, the REMO-HAM simulation period lies outside of the study period. Why?":

Simulations with REMO-HAM, which is used to calculate deposition rates offline, are conducted for the period 01.07.2004 – 31.12.2014. The hydrologic simulations for the case study, using the deposition rates from REMO-HAM as input, are conducted from September 2006 to September 2012. Thus the REMO-HAM output covers the total time period of the hydrologic simulations. However, we acknowledge that the mismatch in dates can lead to confusion. For this reason, we will reword the REMO-

HAM simulation description.

Pg 13, Lines 24-26. This is an example of statements where a citation must be added. Uncited statements, such as these, are not appropriate in scientific literature and are one of the reasons why I do not believe the paper publishable.

RESPONSE:

We will add the appropriate reference. We also will review and reword our paper with a focus on excluding uncited statements as referred to herein.

Pg 14, Lines 11-15. Why is a spin up required? What parameters are modified to calibrate the model?

RESPONSE:

We use a spinup time of one year (1 September 2005 to 31 August 2006) to in order to achieve good estimates for the model state variables. We will add this to the revised manuscript. Furthermore, there is a mixup of dates in this paragraph: First we write we use a "study period of 6 years, from September 2006 to September 2012" (Pg 14, Line 6). Later we write, we run the model until October 31 (Pg 14, Lines 11-12). We will state the correct dates in the revised manuscript.

Pg 15, Section 5. Put your modeled BC concentrations in the context of other measured concentrations.

RESPONSE:

We will do this in the revised manuscript.

Pg 15, Section 5.1.1. These findings should be put in the context of existing literature. Also, it seems that in your experiments the various cause about 10 days of difference in meltout. Put this in the context of other hydrological modeling methods. Is this an improvement? is this amount of variability standard for say a T-index model?

RESPONSE:

We will add a discussion to put this in a broader context in the revised manuscript.

Pg 16, Section 5.1.2. Line 20-24. This amplification is far larger than has be documented in some field studies, compare.

RESPONSE:

To identify the isolated effect of the maximum model surface layer thickness, we chose to set the scavenging ratio to 0 (all LAISI stays in the snowpack during melt). This does not necessarily result in realistic results but demonstrates that results can significantly depend on the choice of this parameter. However, we acknowledge that we need to discuss our model experiment results in a broader context. We will do this in the revised manuscript.

Pg 18, Section 5.2. Is there a BC dataset collected in a similar manner as Sterle et al. 2013, Delaney et al. 2015, Xu et al. 2012, Adolph et al. 2016 that could be used to see how well the model reproduces BC concentrations in the snowpack? Also, what values do you use as background values? Pre-industrial? Early season? Additionally, how do you account for the effects of dust in this case study?

RESPONSE:

To "BC dataset": There is no data specifically on the Atnsjoen catchment. However, there is data from Scandinavia available that can allow evaluation of the here presented results (e.g. Forsström 2013; Elemental carbon measurements in European Arctic snow packs) due to the proximity of our study region and the sampling site. We will add an extended discussion about this in the revised manuscript.

To "What values do you use as background values": "No ARF scenario" refers to a scenario, in which deposition of BC is set to zero, simulating a hypothetical clean snowpack. Results from these runs are used to identify the contribution of BC to snowmelt and discharge generation.

To "effects of dust in this case study": We don't include dust in our study. However, we will include a discussion on how this affects our results in the revised manuscript (see also comment response to comment to "conclusions b" of reviewer #1).

Pg 18, Line 25. What are reasons for underestimates? 15 m3 s−1 is quite a bit.

RESPONSE:

There is actually not an underestimating of 15 m3 s-1, but the model underestimates flows where the observation shows flows between 0-15 m3 s-1. The reason for this might be that the parameters chosen for Kirchner are not perfect for the low decrease of discharge during winter.

Pg 19, Lines 1-24. I found this paragraph hard to follow. I would recommend focusing on the trends as opposed to the specific numbers.

RESPONSE:

We will consider this suggestion in the revised manuscript.

Pg. 19, Line 25. Why was this time period chosen?

RESPONSE:

We refer to this time period as "melt season" because of the drop from snow maximum to no snow in the catchment during this time.

Pg. 23, Section 6. In the conclusions section, I would recommend adding some comments about the case study.

RESPONSE:

We will add this in the revised manuscript.

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