

# ***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 #3**

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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. 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. Also, better parameterization of dust sources is needed. 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

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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. 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.

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

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- 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.
- 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.
- 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.
- 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.
- 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?
- Pg 9, Lines 13-31. I would recommend looking in to 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.
- Pg 10, I gather that there are 3 parts to your models, the hydrology component,

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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?

- 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?

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- 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.
- Pg 14, Lines 11-15. Why is a spin up required? What parameters are modified to calibrate the model?
- Pg 15, Section 5. Put your modeled BC concentrations in the context of other measured concentrations.
- 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?
- Pg 16, Section 5.1.2. Line 20-24. This amplification is far larger than has be documented in some field studies, compare.
- 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 snow-pack? Also, what values do you use as background values? Pre-industrial? early

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- season? Additionally, how do you account for the effects of dust in this case study?
- Pg 18, Line 25. What are reasons for underestimates?  $15 \text{ m}^3 \text{ s}^{-1}$  is quite a bit.
  - 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.
  - Pg. 19, Line 25. Why was this time period chosen?
  - Pg. 23, Section 6. In the conclusions section, I would recommend adding some comments about the case study.

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