

# Review of “Modelling hydrological impacts of light absorbing aerosol deposition on snow at the catchment scale”

## General comments

I have found this manuscript much improved from the previous version. The structure is far clearer and more precise. Additionally, I believe that the reviewer addressed many of the comments that reviewers addressed in the first version.

However, two subjects that were addressed in the previous review should be further explored. Although this aspect has been improved since the original manuscript, I believe that the authors should engage more with observed processes within the snowpack. First, in the first review the authors were asked to put the research in the context of field studies that have been done on this topic. This was recommended as a means to show the robustness or ability of their model. Although this work and the associated processes have been cited and mentioned, the further comparison and description with model results should be done. Specific instances are described below.

Second, I understand the constraints of this project, and realize that the focus is on black carbon, however, other LAISI are very important and likely have a larger role than black carbon in reducing albedo. For the sensitivity studies this should not be a large problem. However, for the catchment modeling I think this is a bigger issue. I believe that the framework presented here could be quite a useful tool to implement LAISI in to hydrological modeling, so it seems important to explain its ability in the best possible way. To me it seems that since these sources are not accounted, the efficiency of the model when compared to real discharge data might not be accurate. Aside from dramatically changing the paper, I am not sure the best way to improve this. However, further commentary on this or work by the authors to address this subject would improve the paper and make the work more relevant and usable in other applications.

## Scientific Comments

- Pg 3, Line 19. What is deposition field?
- Pg 4, Lines 1-15. This comment was made in the previous review. I think some citations need to be added here. Have other researchers published with “Statkraft”? What are “methods-stacks”? I think this paragraph can be improved.
- Pg 4, Line 19. add 2009 (if this is the correct year of the paper) following Kirchner’s.
- Pg 7, Line 3. Describe “surface layer thickness”.

- Pg 7, Line 7. Describe “melt amplification”
- Pg 12, Lines 10-35. I think the are interesting results and the paper would be strengthened if these were compared to real observed black carbon trends. For instance comments in line 23-25 align with observations made by Sterle et al., 2013 and Delaney et al., 2015. Especially given the lack of mineral dust and other LAISI in the model, I believe the confidence in the model would increase if such processes and results would related to observed data and work.
- Pg 13. Once again, the authors should engage with the body of research regarding field measurements. How do the scavenging ratios used in the sensitivity study compare to those observed?
- Pg 14. Section 5.1.4. This section I found confusing and am not sure of its intent and actual meaning It seems strange to state that thicker snowpacks have more LAISI. I assume this is due to your prescribed initial concentration and scavenging assumptions. However, black carbon in the snowpack is a manifestation of atmospheric conditions and scavenging ratio, regardless of snowpack quantity. Of course thicker snowpack will have a lower concentration. Additionally, BC in the snowpack is not steady through out the profile(Sterle et al. 2013, Xu et al. 2012, Delaney et al. 2015) and can often occur in events as dust on snow work in Colorado by Painter, as well as by Delaney et al. 2015, Kaspari et al. 2015 and Hadley et al. 2007 among others show. As a result I believe that this assessment should be refined.
- Pg 15, Section 5.2.2. There are many places where these model results could be compared more with field measurements for the literature. Additionally, maybe make some comments about how well these concentrations agree with literature values. To me they seem on the upper end of snowpacks outside of Asia, but some commentary would be good.
- Pg 19, Section 5.3. As mentioned above, the non-BC LAISI species in the model is a notable short-coming. Is it possible to determine what the main source of LAISI is in the catchment?
- Pg 19, Line 28-19. I cannot easily see where this point is explored in the text. See my comment regarding Section 5.1.4.
- Pg. 20 Line 11. “model tool” is a bit funny. Maybe “tool” or “model” or “modeling tool.”