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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Snow cover sensitivity to black carbon deposition in the Himalaya: from atmospheric and ice core measurements to regional climate simulations" *by* M. Ménégoz et al.

Anonymous Referee #1

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This study applies a global aerosol-climate with nested high-resolution grid over the Himalaya region to model black carbon transport, deposition, and impacts on snow cover. The study evaluates simulated BC in this region against near-surface atmospheric measurements made at the NCO-P site and concentrations from a shallow ice core collected in the general vicinity of NCO-P. The study is novel because it applies a high resolution model grid over the domain of interest. This is an appropriate modeling technique to study regional impacts, especially in mountainous areas like the Himalaya, and I expect we will see more studies like this as similar capabilities are introduced in other climate models.

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The most problematic issue with this study is that simulated BC concentrations in snow are about 60 times larger than the ice core measurements, bringing into serious question the accuracy and usefulness of the simulated impacts of BC on snow cover. The model bias is not even mentioned in the abstract, nor is there any attempt to correct for the bias and produce more believable results. A simple way that the bias could be addressed would be to scale the simulated BC concentrations by factors that produce more realistic BC concentrations in snow. I suggest that the authors conduct additional sensitivity studies of snow cover impacts with scaled concentrations of BC in snow that are more consistent with measurements. The range of scaling factors chosen for these studies may differ from the 60-fold bias that is reported, because of model-measurement inconsistencies described by the authors in section 3.3 (e.g., different surface elevations between model grid cell and ice core site), but the range of scaled BC concentrations should bracket the authors' best approximation of actual BC concentrations in this region. If no additional simulations are conducted, the snow cover results need to be re-framed as sensitivity studies resulting from BC concentrations that are biased high.

Minor issues:

Abstract and Conclusions: The authors argue that the model accurately captures the seasonal cycle of BC deposition and surface-air concentrations (i.e., "reproduces the seasonal variations", Conclusions line 24), but this isn't exactly true. While it is encouraging that the model provides larger BC concentrations during the inter-monsoon season, the amplitude of the monsoon/inter-monsoon variation seems to be about 2-fold larger in observations than in the model (e.g., Table 1 and p.31021, line 20).

Are the radiative impacts of dust in snow also treated? Absorption by dust in snow reduces the albedo perturbation caused by BC, implying BC impacts could be overestimated if dust is neglected.

Section 2.1: Which BC emission inventory is used? What is its horizontal resolution

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(also in the stretched grid domain)? Do the emissions vary interannually during the period of analysis?

p.31019, line 19: What value of the BC absorption coefficient is assumed in converting measured aerosol absorption coefficient to equivalent BC concentration? Does dust contaminate this estimate?

p.31020, line 11: ".. mean resolution of 6.6cm." - Roughly what is the corresponding time resolution of this thickness? (i.e., how many samples per year?) This can be easily calculated, but it is worth listing.

p.31020, line 26: How does the model deposition in this gridcell compare with that in the gridcell of the actual ice core site?

p. 31023, line 14: BC -> aerosol

p. 31023, line 15-19: How large is the observed vertical gradient in BC, and how much of the model bias can therefore be explained with the 1000m difference in model/observation altitude?

p. 31023. bullet 2: Do you simulate BC concentrations in the bottom model snow layer as well as the top layer? If so, it seems that you could conduct a more realistic comparison with measurements by incorporating simulated BC amounts throughout the snow column.

p. 31025, line 5: "Kaspari et al (2011) measured average rBC concentrations in snow of 7ug/kg..." - I believe this is much higher than what was reported in Kaspari et al (2011). Please confirm or clarify the statement.

p. 31025, line 4: "because" does not seem like the right choice of word in the context of the whole sentence.

p. 31027, line 13: "... which certainly explains most of the difference between the observations" - Do we know this "certainly"? I would think that differences and/or errors

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in the measurement techniques could also explain much of the difference.

Section 4: How, precisely, is snow cover duration calculated?

Section 4: The previous question relates to the impacts assessed over the Tibetan Plateau. Although the Plateau has little snow cover, that snow which exists should still "feel" the impacts of light-absorbing impurities, and presumably decrease in duration. Does snow cover over the Plateau decrease at all in duration, or is this result an artifact of how snow duration is calculated, combined perhaps with the model's ability to resolve patchy snow?

p. 31028, line 20: Is this increase in net solar energy an annual-mean?

p. 31028, line 24: "Similarly" implies that a direct comparison applies here, but the two numbers really should not be compared as apples and apples, because they represent very different quantities, as described in subsequent sentences.

Figure 3: The red contoured lines in panel (b) are somewhat difficult to distinguish from red pixels. I suggest using a different color for the contours. Also, I suggest mentioning in the caption that these simulations were conducted with nudged atmospheric conditions.

A similar study that should be cited is Qian et al (2011): http://www.atmos-chem-phys.net/11/1929/2011/acp-11-1929-2011.html. This study also suffered from model biases such as excessive snow cover, and hence results were framed as sensitivity studies. I suggest that a similar approach be adopted for this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31013, 2013.