Review of *The Cryosphere* Manuscript #tc-2013-5

"Spectral reflectance of solar light from dirty snow covers: a simple theoretical model and its validation"

Authors: Kokhanovsky

Received: February 18, 2013
Reviewed: April 21, 2013

Reviewer: Charlie Zender, zender@uci.edu

Recommendation: Accept with minor revisions

I have voluntarily disclosed my identity in all manuscript reviews since 2004 for many of the reasons discussed here (http://www.agu.org/fora/eos/2006/10/22/anonymous-review-pros-and-cons.html). The authors are free to contact me at zender@uci.edu.

General Comments

This short manuscript presents a useful simplification of the theory of reflectance in weakly absorbing media with absorbing impurities. The theory is demonstrated for dirty snow, and evaluated against three lab or field measurements. Snow researchers in the field, snowpack modelers, climate modelers, and pollution specialists will benefit from the simple analytic expressions which yield good agreement with more complex models. The manuscript is terse yet repeats some previously derived theory. The main shortcoming is the lack of direct comparison against more complete methods like DISORT. The revised manuscript should be published in *The Cryosphere*.

Specific Comments

1. Results presented in Figure 1 are impressive. Congratulations on deriving an analytic expression that represents this complex behavior so well.
2. The lower curve in Figure 1a appears nearly always to overestimate (i.e., to appear brighter than) the observations. This is odd because the free parameters B_{i} and B_{S} were obtained by using a minimization procedure. Why are the biases not distributed evenly between bright and dark?
   The minimization procedure does not appear to have converged. Please specify "the minimization procedure" for the sake of reproducibility.
3. Do I understand correctly that tuning the free parameters to any single pollutant content and snow grain size would produce a curve that fits the data as tightly as the lower curve in Figure 1a? This is worth mentioning, since some readers will otherwise be dismayed at the relatively large bias seen in some curves for which the free parameters are not optimized.

4. Because I was late in reviewing the manuscript, I had the opportunity to read the detailed comments of Reviewer A. While I agree with nearly all of her comments, I have a different perspective on one issue. The restating of many equations from prior works of the author was helpful to me, possibly because I am unfamiliar with the details of those earlier works. For me it improves the readability and reproducibility of the theory to have all the ingredients in one place. I agree it would be helpful to more clearly identify those parts/equations of the theory that are distinctly new in this article.
5. Given the number of equations, a table of symbols would be helpful. It could show the symbol, a brief description, and the units, if any.
6. Typically when a theory or parameterization is proposed to simplify a more complete yet complex theory, one provides a direct comparison of the two approaches, e.g., Kokhanovsky vs. DISORT. This allows the reader to directly evaluate the tradeoffs in accuracy and physical self-consistency with the gain in speed and physical insight. Without diminishing the importance of comparison against observations, it would be helpful to, in addition, see a direct comparison of two competing models of the behavior of dirty snow reflectance.
7. p. 541, L4: Please describe the physical significance of the optically characteristic length a_{opt}.
8. In determining the verisimilitude of a model or theory I prefer the term "evaluation" over "validation". The former is open-ended while the latter implies a bias towards finding the model is correct.
9. As mentioned by Reviewer A, the assumption of external (instead of internal) mixtures is given scant attention in the manuscript. It may partially explain model/measurement discrepancies, yet there is no discussion of this particular issue. Coatings amplify the effective mass absorption coefficient (MAC) of pollutants by focusing additional light on the impurity (e.g., Bond et al., 2006; Jacobson, 2004). Some questions the author should address:
   (a) Ought one expect a consistent bias from the externally mixed assumption? If so, is that bias noticeable in the evaluation?
   (b) How might the theory presented be modified to account for this?
   (c) Would simply scaling the MAC be preferable to doing nothing? Bond and Jacobson suggest focusing raises MAC(soot) to $\sim 11\,\mathrm{m^2\,g^{-1}}$.

Technical Corrections

1. Title: "Spectral reflectance of sunlight by dirty snow..."
2. p. 541, L4: "can also be"
3. p. 541, L19: "we arrive at"

4. p. 542, L21: Remove “excellent”. It is a subjective qualifier whose use here is debatable.
5. p. 542, L21: “in good agreement”
6. p. 543, L15: “Brandt”
7. Figures: “microns” instead of micrometers. Axes labels should be capitalized.
8. p. 545, L12: “in snow field work and when comprehensive”
9. p. 545, L16: “integro-differential”
10. p. 545, L17: “general circulation models”