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The Cryosphere Discussions

Interactive comment on "The effect of black carbon on reflectance of snow in the accumulation area of glaciers in the Baspa basin, Himachal Pradesh, India" *by* A. V. Kulkarni et al.

Anonymous Referee #1

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I recommend rejection of this paper based on the flaws discussed below. There are numerous smaller flaws that also would need to be addressed before the paper could be published but I will not enumerate the smaller flaws, given the magnitude of the problems discussed below.

I have no doubt that black carbon (BC) is causing some reduction of the snow albedo on Himalayan glaciers, but I have no confidence that this paper is correctly quantifying the albedo reduction, nor that it is correctly quantifying the BC mixing ratio that would be responsible.

1. The measurements presented are of surface reflectance from two satellite sensors,

MODIS and AWiFS. The MODIS land surface reflectance product is used directly. For AWiFS, the authors use the signal measured and themselves calculate spectral reflectance and albedo. The paper asserts that large changes in the retrieved surface reflectance/albedo in the Baspa basin of the Indian Himalaya coinciding with anomalously large biomass burning events are due to darkening of snow by deposition of BC from the fires. They then conclude this demonstrates that contamination by BC form forest fires “can influence the mass balance of the glaciers” in this region. However, there is no mention of the spatial resolution or accuracy of the retrievals of these land surface reflectance/albedo products. Of particular concern is that accurate land surface reflectance measurements require removal of the effects of atmospheric aerosol. The satellite measurements were made during the springtime fire season when smoke plumes were often covering this region of the Himalaya. The visible-wavelength radiance reaching a satellite can be reduced either by BC in snow, or by BC in the atmosphere above the snow, or both. Distinguishing their relative contributions to the measured radiance would require advanced active remote sensing (e.g. HSRL lidar) rather than the passive remote sensing used in this paper.

MODIS: In theory one could use the MODIS AOD product to remove the effects of atmospheric aerosol, but this does not appear to have been done. However, even if it was, I still would not find the result credible: MODIS is known to have large biases in its retrievals of AOD over land, and in a regional of such variable-reflectance terrain atmospheric aerosol effects are unlikely to have been accurately retrieved – especially in a case such as this where the atmospheric aerosol itself was likely very spatially inhomogeneous in optical depth.

AWiFS: A description is given (Section 3) of how the AWiFS “Digital Numbers” are used to calculate reflectance and albedo. The atmospheric correction procedure used in this paper for AWiFS ("dark object subtraction", DOS (Negi et al. 2009a)) is appropriate for a scattering aerosol but not for an absorbing aerosol. It appears that the change in reflectance and albedo due to the presence of atmospheric plumes of absorbing aerosol are not accounted for. Based on this, one must conclude that the reflectance (Fig. 7) and albedo (Fig. 8) data from AwiFS are the planetary reflectance and albedo, yet they are attributed completely to changes in snow reflectance/albedo. Further, while efforts are made to account for variable surface terrain (surface slope, etc.) there is no discussion of the uncertainties in AWiFS-retreived albedo associated with these approximations.

2. The retrieved decreases in albedo are too large to realistically be fully due to BC in snow. On page 1367, darkening via contamination by soil is discounted because it would require >1mg/cm2 of soil to produce the observed reflectance change. However, the authors state, "a very small amount of black carbon, i.e. around 0.37 mg/cm2, can reduce reflectance by the required number." Very small by what standard? 0.37 mg/cm2 = 370,000 ng/cm2; if this is distributed over the snow's top 1cm (a generous depth, given that deposition in this case would all be via dry deposition) this would equate to 1.1 MILLION ppb of BC in the snow for a snow of density 0.3g/cm3. It is impossible to imagine that such concentrations could be reached in snow via the deposition of atmospheric aerosol from sources on the order of 10-100km from the glaciers. In fact it is difficult to imagine getting these concentrations of BC in snow from anything other than directly dumping coal dust onto the snow. For reference, earlier studies in the Himalaya/Tibetan Plateau region show mixing ratios of BC in snow and glaciers of <100 ppb, and generally ~10-40 ppb (Xu et al. 2006; Xu et al., 2009a, 2009b; Ming et al., 2008; 2009). The fact that a concentration of 1.1 million ppb BC in the snow in the high Himalaya would be required to produce the observed spectral reflectance seriously changes calls into question the study's results, especially given the very broad geographic coverage of these large albedo changes. In contrast, this reviewer suspects that absorbing atmospheric aerosol plumes of realistic optical depths could produce the observed decrease in planetary reflectance, and this change will manifest in the retrieved surface reflectances if atmospheric aerosol effects are not properly accounted for. As noted above, it is not at all clear that the effects of atmospheric aerosols are removed from the MODIS data products, and it is pretty

clear that they are NOT removed from the AwiFS retrievals.

3. There are internal inconsistencies in this paper, and also inconsistencies with published work. The following discussion refers several times to Figure 9, which shows the reduction of snow albedo by specified amounts of mineral dust or black carbon (in the form of coal dust), in units mg/cm2. I have converted these values to a mass-mixing ratio (ppb or ppm) by using the assumption that the added dust or BC is uniformly mixed in the top centimeter of snow, and that the snow has a typical density of 0.3 g/cm3.

(a) On p 1362, line 23-24, "120 to 280 ppbw can reduce the snow albedo by 4 to 8% in the visible region". These numbers disagree with Figure 9 by three orders of magnitude.

(b) Figure 9 shows that BC is twice as absorptive as mineral dust. This result is in conflict with Warren and Wiscombe 1980, who found BC to be 100 times as absorptive as mineral dust.

(c) As noted above, the BC mixing ratios inferred in this study are >4 orders of magnitude higher than found in in-situ measurements.

(d) The caption of Figure 9 does not state how the data were obtained. I assume they came from the experiments of Singh et al 2011, in which coal was ground into a powder of particles with diameter <0.5 mm and then spread onto the snow surface. Let us therefore assume that the mean radius of this coal dust was 0.2 mm. The mean radius of atmospheric soot is about 100 nm, so the BC particles used in the glacier experiment differ in volume from atmospheric BC particles by ten orders of magnitude. Large BC particles have smaller mass-absorption cross-section (m2/g) than do smaller particles; this difference probably explains the discrepancies in (a,b) and may contribute to the discrepancy noted in (c). It is therefore inappropriate to make use of the coal-dust experiments in this paper, or to refer to them.

In conclusion, given the difficulty of retrieving AOD over land in general, the fact that this region during the period of interest is influenced by biomass burning plumes (i.e. they are spatially and temporally inhomogeneous) and the fact that the area of interest has highly variable slope/surface roughness and a wide range of surface albedos, coupled with the low spatial resolution of the satellite data, the authors are unlikely to be able to disentangle changes in atmospheric aerosol optical depth and snow albedo. Thus, I recommend outright rejection of the paper, rather than acceptance with major modifications.

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