REPLY TO COMMENT ON PAPER "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.

We would like to thank anonymous Referees for the critical evaluation of paper. The referees have given numerous comments; many of these comments can be used to improve the quality and content of the paper. However, we would like to reply the major concerns raised by referees, which lead to rejection of the paper.

Reply to comments of Referee 1

Major comment:

"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".

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.

Reply:

Major concern of referee appears to be presence of absorbing aerosols in the atmosphere and its effect on snow reflectance estimates from AWiFS data. In order to understand the influence of atmospheric BC on snow albedo, we have carried out Radiation Transfer calculations for aerosol profile above snow. The aerosol profile was obtained from Calipso data. This suggests that the reduction in albedo due to atmospheric BC above snow is only 6%. The change in albedo due to the entire composite atmospheric aerosol (including BC) above snow is 8% (Fig. 1). Even if, we assume that aerosol layer optical depth above snow is 1.0, which is extreme case and is nearly impossible, reduction in albedo is less than 15%. This suggests that a mean drop in reflectance of 21 ± 5 %, which was observed during the active fire period in the accumulation area and at some places, the drop was as high as 50 ±5 %; this can only be explained by the deposition of black carbon.

Comment:

"Further, while efforts are made to account for variable surface terrain (surface slope, etc.) there is no discussion of the uncertainties in AWiFS-retrieved albedo associated with these approximations".

Reply:

The digital elevation model (DEM) was used for the terrain corrected reflectance. Where the local incidence and local viewing angles were calculated and this incorporates all slope and aspect information. Kokhanovsky et al. (2007) presented a possibility for direct determination of the spherical cloud albedo from single reflection function measurements for the special case of optically thick snow. This technique requires no prior information on the particles size and optical thickness. It has also shown that the absolute value of error is below 3% at optical thickness ≥ 10, for all the considered solar zenith angles and nadir observations which is suitable in case of snow study.

For topographic normalization cosine correction method was used, where for a low illumination, i.e., small values of $\cos(\theta_0)$, the corrected reflectance was too large and the corresponding parts of an image were overcorrected. In this case, very low illumination slopes i.e. $\theta_0 > 75^\circ$ were not selected for the analysis. As ART theory has large errors for $\theta_0 > 75^\circ$ (Kokhanovsky, 2004a).

Comment:

"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/cm^2 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/cm^2, can reduce reflectance by the required number." Very small by what standard? 0.37mg/cm^2 = 370,000 ng/cm^2; 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/cm^3. 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 reflectance if atmospheric aerosol effects are not properly accounted for".

Reply:

The field reflectance data was collected previously and we wanted to use field information in present investigation to make limited argument that the loss in snow reflectance cannot be explained by the contamination of soil. Since, first reading was obtained by darkening of snow by the contamination of 1mg/cm^2 of soil or BC and then 0.37 mg/cm^2 was obtained by interpolation. We agree that the experiments has scope for improvement, however original argument that the loss in reflectance cannot be explained by contamination of soil is still valid.

Reply to comments of Referee 2

Comment:

"I am surprised that there is no discussion regarding precipitation. Indeed, between 10-15 April and 1-5 May, surface reflectance in accumulation areas of glaciers may depend on how frequent and significant snowfalls are in these areas during this period. And precipitations are usually significant in April in western Himalaya, and very variable from year to year, which may undoubtedly significantly explain the inter annual variability observed for spring reflectance changes. I think that a discussion regarding this is needed, but I believe that it will be difficult to extract a signal of reflectance decrease only. In Fig 5 caption, the authors explain that no reflectance decrease has been reported for 2004, because of "unusual snowfalls". What does "unusual snowfalls" mean? And I am sure that usual but year-to-year variable snowfalls have also a significant influence that is not easy to assess without in-situ measurements. For instance, in 2005, there is an increase in surface reflectance between 10-15 April and 1-5 May, probably explained by fresh snow deposition".

Reply:

The precipitation is considered in the investigation. The period between 10-15 April and 1-5 May is optimized by considering forest fires in the year 2009 and snowfall events in the years between 2000 and 2012. During this period, no snow fall was measured between 10-15 April and 1-5 May, except for the year 2004. Therefore, the change in reflectance for the year 2004 was not plotted in Figure 5. This aspect is partially mentioned in the methodology section and it can also be further elaborated.

Comment:

"In abstract and introduction, the authors provide a quick (and erroneous) state of the art on glacier mass trends or ELA variations in Himalaya, over the last two decades. Even though it is sometimes unclear to which part of the Himalayas they are referring to, and to which time period (mainly in introduction), they are reporting that "the glaciers in the Pir Panjal and Greater Himalayan mountain ranges are losing mass at a rate of almost a meter per year. The ELA has also shifted upward by 400 m in the last two decades" (p1360, lines 5-7 in introduction as well as p 1369, lines 2-3 in conclusion). These figures are supported by Sangewar and Kulkarni (2011: technical report from DST), Dobhal et al. (2008: 6 years of mass balance of Dokriani 1992-95 and 1997-2000) and Heaberli et al (2001: report from WGMS) (p 1361, line 23) which is weak, and incomplete. The authors write that "Measurements of the mass budget for glaciers in the Himalayas are relatively few and only of short duration" p1361, line 20. I agree with this statement but the authors are probably aware that the longest continuous mass balance record ever published in India (and more generally in the Himalayas) comes from Chhota Shigri Glacier in Pir Panjal Range, not far from their study area (some tenths of kms towards north west) (Dobhal et al., 1995; Wagnon et al., 2007; Berthier et al., 2007; Azam et al., 2012; Vincent et al. 2013). None of these papers are quoted in this present study and we kindly invite the authors to refer to these papers to revise their figures. Indeed, Vincent et al. (2013) recently showed that Chhota Shigri Glacier is representative of western Himalaya, and experienced

balanced (or even slightly positive) conditions in the nineties before starting to lose mass at a moderate rate (–0.17 ± 0.09 m w.e. yr^{-1} for the period 1988-2010, and –0.44 ± 0.16 m w.e. yr^{-1} between 1999 and 2010, far from -1 m w.e. yr^{-1}). For a wider overview across the Himalayas, Karakoram and Pamir regions, the authors are invited to read additional papers like Kääb et al. (2012), Gardelle et al. (2013) among others. These papers show that the response of glaciers along these large ranges is much contrasted, because they are influenced by different climatic regimes. In western Himalaya, glaciers are not retreating as fast as stated by Kulkarni et al in this present manuscript. See Vincent et al (2013) for a review of all available mass balance measurements in Northern India (Western Himalaya)".

Reply:

We agree that there is a scope to improve the discussions. Thanks for listing papers, which were not referred in the paper.

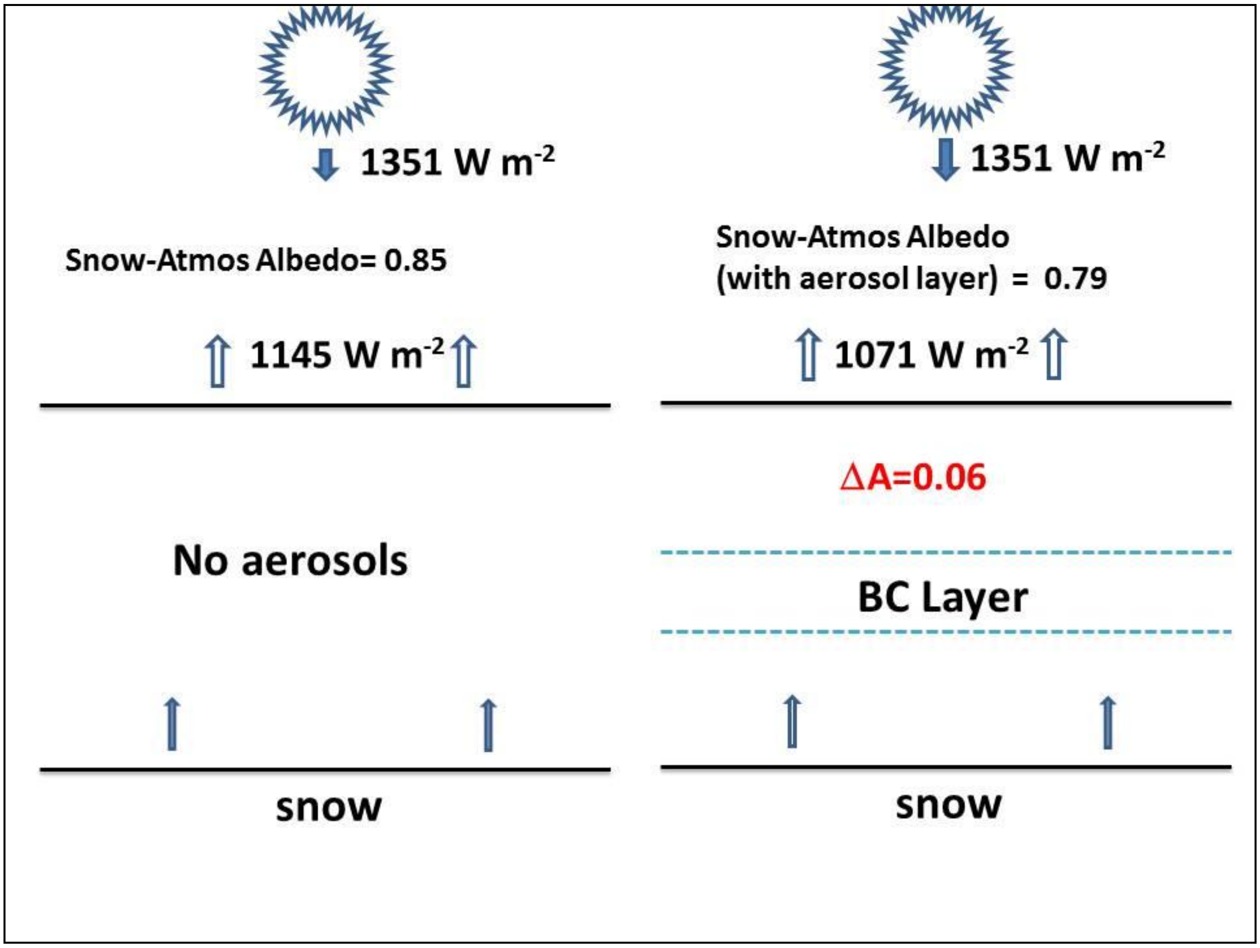

Fig 1: Influence of atmospheric aerosol layer on estimates of snow albedo obtained from satellite data.

References:

Kokhanovsky, A. A.: Cloud optics, Springer-Praxis, Chichester, Netherlands, 2004a.

Kokhanovsky, A., Mayer, B., von Hoyningen-Huene, W., Schmidt, S., and Pilewskie, P.: Retrieval of cloud spherical albedo from top-of-atmosphere reflectance measurements performed at a single observation angle, Atmos. Chem. Phys., 7, 3633–3637, doi:10.5194/acp-7-3633-2007, 2007.