

Interactive comment on “Properties of black carbon and other insoluble light-absorbing particles in seasonal snow of northwest China” by Wei Pu et al.

Anonymous Referee #2

Received and published: 30 December 2016

I have read the manuscript titled: Properties of black carbon and other insoluble light-absorbing particles in seasonal snow of northwest China. Overall, I feel that the manuscript is well written and is worthy of publication in The Cryosphere. The first referee made multiple comments regarding the chemical analysis. As this is not my area of expertise, I will comment on some additional issues that I noticed.

This research (as well as many publications reporting on this topic) suffers from one common uncertainty. Since results are presented from data collected in one moment in time, how useful are the results in reality? How representative is that one point in time for representing conditions at any other time in the snow year? Different weather conditions can significantly affect the snow ILAP concentrations. While this is a common

[Printer-friendly version](#)

[Discussion paper](#)



problem with this type of measurements that can only be overcome by more intensive sampling, I feel that the authors should include wording that state that the results are from one time measurements at each location and may not be representative of the long term characteristics of the snow in that location.

If I recall correctly, the Hegg technique requires that the snow is relatively fresh. There do not appear to be any comments regarding the time since the most recent snow storm for each of the sites (other than the mention of the 13 sites where it was snowing during collection). Could this affect some of the chemical analysis as some chemical constituents may have washed out of the snowpack?

Page 3 line 11, Using the SNICAR online model (<http://snow.engin.umich.edu>), using the default snow constants then either 0 or 10 ng/g, the broadband albedo reduction is closer to 0.3% rather than 1%. The Warren and Wiscombe (1980) paper shows possible values with some relatively extreme cases depicted. Could the authors clarify what snow conditions they are using and then clarify the appropriateness of these conditions?

Page 14 line 19: Earlier the authors stated that they sampled in 5 cm steps down through the snow pack. Is this comparable to other studies where you compare the results? If the surface sample is the top 5 cm of snow, how does the density of the snow affect the measurement (if dry deposition on the surface is the main source, then the 5 cm of snow would dilute the measurement significantly with density as an additional factor). If all of the BC is on the surface, then sampling the top 1 cm of snow versus the top 5 cm of snow (assuming uniform density) would give you a factor of five difference in mass mixing ratio since BC is generally reported in a mass per volume unit.

Nomenclature: The different variable names are not well defined at their first use in the manuscript. I.e. What specifically do “MAX”, “EQUIV”, and “EST” in superscript mean next to C subscript BC? It might be nice to have these and the many others in a table

[Printer-friendly version](#)[Discussion paper](#)

for easy reference. If they are equivalent to something used in other literature (eBC in Grenfell), please list the equivalents as well.

Page 18 line 2: Something is missing: “vertical differences were missing at could sites, . . .”

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-233, 2016.

TCD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

