

The authors have addressed all the issues raised by me in the previous comments. However, some the comments are not correctly addressed, possibly due to my unclear requests. Please clarify them before the work can be published in ACP.

1. Citing the previous comment, "This work introduced a new concept, OC's forcing, which has not been widely recognized by the societies. The introduction of a new concept must be previously supported by measurement, as we all know. The OC's forcing is just like an aerolite in the whole paper. The authors should firstly list some literatures that clarified OC has the characteristic of radiation absorption in snow as well as it does in atmosphere claimed by Bond and Bergstrom (2006) and Kirchstetter et al. (2004). The online SNICAR model only simulates the reductions of snow albedo caused by BC and dust, but cannot have the ability to simulate the forcing of OC. The mass absorption cross-section of OC in the atmosphere cannot be directly used for it in snow."
  - For the issue of OC's forcing, the author list some studies concerning the OC's radiation absorption in the atmosphere. My question is whether the forcing still exists in snow, and how to apply the SNICAR model to correctly simulate the forcing, for the model builder himself does not mention this. I strongly suggest the author to consult Dr. Mark Flanner, the model builder to get suggestions.
2. Citing the previous comment, "Paragraph 2 in Page 19721. Ming et al. (2013) in Adv. Water Resources suggest BC deposited in Himalayan and High Asian glaciers cannot significantly affect their energy balances, which is a very minority but different viewpoint from most literatures listed here, which should not be neglect here."
  - The author just put the reference in the context, without making any notes. Please address the original intension raised by the reviewer in the paragraph.