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Review of "Distinct surface response to black carbon aerosols"

Anonymous Referee #1

Referee comment on "Distinct surface response to black carbon aerosols" by Tao Tang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-186-RC1, 2021

General comment:

This study analyzed the PDRMIP multi-model ensemble to show that changes to the surface energy budget are distinctively different in the case of the BC-induced climate change from those enforced by other forcing agents such as CO2, scattering aerosol, and solar insolation. Specifically, the authors show that the climate response of the surface air temperature is governed by changes to multiple components of the surface energy budget in the BC case, contrary to other scenarios where surface radiative flux changes is the major factor that mostly determines the temperature response. I think this study is a nice follow up for recent studies that similarly assessed the surface energy budget change for some of the forcing agents to identify their different characteristics of climate responses. I would recommend the paper be considered for publication in ACP contingent upon if the authors appropriately address my specific concerns described below.

Specific comment:

Line 34-39: The numbers listed here should be shown with appropriate units (dimensions) since these numbers are for a unit BC forcing. For example, the energy flux changes (radiative, sensible and latent) should be dimensionless (rather than having the unit of Wm^{-2}) given that they are normalized by the TOA radiative flux change.

Line 38-39: I'm a bit surprised that the sensible heat flux change (2.53Wm^{-2}) is larger than the latent heat flux change (1.30Wm^{-2}) simply because of my naïve understanding that the latent heat typically dominates the turbulent heat transfer from the surface to atmosphere. Can the authors explain why the opposite (i.e. sensible heat is larger than latent heat) occurs in the BC-forced scenario? The enhanced stability just explains the sum of latent and sensible heat changes but no explanation for their partitioning.

Section 2.2: I'm wondering if the energy balance described by equations (2) and (3) indeed applies when analysis is restricted to land grids only. Don't these equations need additional terms for energy exchange between land and ocean? Did the authors confirm that the balance relationship of (3) is indeed true in the model data analyzed? This can possibly be addressed by adding a bar for the residual in Fig. 4.

Minor point:

Line 101: relative -> relatively

Line 205: "wind speed (U) is likely the only main factor driving the change of latent heat flux": How about the specific humidity (qa)? The enhanced stability in the BC case can also change qa.

Line 256: I don't understand what 'bottom-up' means here. To my understanding, the energy balance constraint discussed throughout this paper is all 'top-down' regardless whether it is for top-of-atmosphere or surface. Why is it called 'bottom-up' when the surface energy response to solar insolation change is discussed? Please clarify.