

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2021-186-RC2>, 2021
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Comment on acp-2021-186

Anonymous Referee #2

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-RC2>, 2021

This paper explores the climate response at the surface to BC aerosol forcing in a set of sensitivity tests with a variety of global climate models. The paper draws a contrast between the surface response to BC forcing and the surface response to a variety of other climate forcing agents, including greenhouse gases, solar variations, and mostly scattering sulfate aerosols. The paper argues that the response at the surface to the attenuation of downward solar radiation attributable to BC aerosols elicits a significantly stronger compensating response in the surface turbulent fluxes compared to the other forcing agents. This is attributed to the increased static stability and closely related reduction in surface winds that the authors argue is more significant in response to BC aerosol forcing compared to the other forcing agents. The paper is useful contribution and the approach is fairly robust, apart from the usual limitations of global climate models. The paper is suitable for publication following some minor revisions.

Major comments:

The individual figure elements in figures 2, 3, 5, 6 and 7 are very small. Presumably the purpose of showing a global map would be to reveal the regional differences evident in the map, only some of which are discussed in the text. Nevertheless, such variations are difficult to discern in maps as small as included here. If the intent is for the reader to simply zoom in on a computer screen, then I suppose it is sufficient. This is perhaps an editorial decision regarding whether the figures must be accessible to the reader in print form, or whether it is sufficient to presume the reader will zoom in electronically. Nevertheless, much of the argument rests on what are essentially global energy budget

arguments with only limited discussion of the regional differences apparent across what amounts to more than 60 global maps. The authors could consider conveying more of the quantitative results in simpler bar figures, such as figure 4 and including global maps only for key results where the regional variations are central to the argument.

The results shown in figures 2 and 3 describe the basic physics underlying the main point of the paper, but only for energy budget considerations over the land areas. Of course, from a global perspective, the total energy balance is strongly determined by the ocean response. The authors acknowledge that they are only focusing on land, but this choice is not justified in the discussion. Perhaps the notion is that the temperature response addressed in the following figures is more important to people over land because that is where the people are. If so, then the authors should state it, since the choice to ignore the ocean when considering surface energy balance responses to forcing agents is not intuitive.

The differences between the various forcing experiments shown in figure 5 look very dramatic. In particular, the BC experiment stands out strongly in comparison with the other forcing agents, which of course is part of the point of the paper. However, how much of that is simply because the surface forcing per unit of TOA forcing is greater for BC? Put another way, if the solar forcing were reduced by enough that the reduction in solar insolation at the surface were comparable to the attenuation of 10x BC, would there be important responses in the turbulent fluxes that are not evident in figure 5? Typically, forcing from reductions in solar forcing, or forcing from purely scattering aerosols are of a similar magnitude at the surface as they are at the top-of-atmosphere. However, the surface forcing of a perturbation in BC at the surface can be several times that of the TOA forcing (see e.g. Magi et al. 2008 where the surface forcing efficiency of smoke can be approximately 10x the forcing efficiency at top-of-atmosphere).

In the abstract and discussion, the authors refer to a "top-down" influence of BC aerosols. I think I get what the authors are trying to imply by "top-down", but I find the explanation to be rather vague. For example, static stability is typically quantified by a potential temperature gradient in the lower troposphere. That is above the surface, I suppose, but not the top of the atmosphere, or even top of the troposphere. I think that if the authors are going to include this notion, especially in the abstract, they need to be quite a bit more specific about what exactly is defining the volume or geometry of the space they consider having a "top" and "bottom" and why a term that implies a downward action is appropriate. In my opinion it does not make the physical argument any clearer.

Finally, while I find that the robust results from an ensemble of simulations comparing responses to a range of individual forcing factors to be a valuable contribution to the literature, I find the notion that "a clear and detailed mechanism of surface radiative response to BC is still lacking" (line 76) to perhaps be overstating the level of ignorance. While not purely a study of BC aerosol forcing, I would refer the authors to Liepert et al. (2004) for an early introduction to the notion that aerosol forcing at the surface can yield turbulent flux responses that are important on global scales.

Minor comments:

All of the quantitative forcing values noted in the abstract have positive signs, even while most of them are meant to quantify reductions in the forcing. Typically, reductions in a forcing value should be given a negative sign. Alternatively, and perhaps more rigorously, since many of the forcing quantities are directional (upward or downward) the authors could define changes that contribute an increase in the net downward forcing as positive in sign and decreases in net downward forcing as negative.

In line 181 I think "and all grid were given" should read "and all grid cells were given".

Reference:

Magi, B.I., Fu, Q., Redemann, J. and Schmid, B., 2008. Using aircraft measurements to

estimate the magnitude and uncertainty of the shortwave direct radiative forcing of southern African biomass burning aerosol. *Journal of Geophysical Research: Atmospheres*, 113(D5).

Liepert, B.G., Feichter, J., Lohmann, U. and Roeckner, E., 2004. Can aerosols spin down the water cycle in a warmer and moister world?. *Geophysical Research Letters*, 31(6).