**Atmospheric
Chemistry
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Discussions**

Interactive comment on "Soot microphysical effects on liquid clouds, a multi-model investigation" *by* D. Koch et al.

Anonymous Referee #1

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This paper discusses an intercomparison among six global models of the indirect plus semidirect effects of biofuel soot and of fossil-fuel soot.

Major points

1. The benefit of the paper appears to be primarily that it gives information about the variation among a few models in the field in certain behavior, namely how the modeled cloud fields respond to different levels of black carbon. However, the paper lacks context in that the authors do not discuss whether nor demonstrate that the models represent clouds physically or not. Very little is mentioned about what physical treatments are included and what treatments are missing in each model. This is one of the most important points that the paper can make and is relevant since one conclusion could be that none of the models tested is adequate for simulating the effects of black

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carbon on clouds thus climate. This cannot be determined unless a proper identification of processes treated and missing (e.g., cloud drop nucleation, condensation, collision-coalescence, breakup, sedimentation, precipitation, evaporation, scavenging of aerosol particles, how BC interacts with cloud drops and how radiation interacts with cloud drops and BC together, and the associated numerical methods in all cases) in the models is given. It is suggested that a table be provide of both important cloud processes treated and missing in all models. Also details of resolution should be included (e.g., number of cloud drop sizes (or bulk treatment), number of clouds formed per column at a given time, and types of clouds that can form).

2. Based on the analysis of the first comment, the authors need to provide information as to whether some models are more suited for simulating the effects examined than others. Merely providing results from several different models without scrutinizing whether some models are better than others results in a false sense that all models are equally valid so the wide range in results is attributed to "uncertainty." This is not a correct attribution since some models simply have no business simulating cloud effects of BC. Do models with certain characteristics (e.g., those that assume more hygroscopic organic matter or models with coarser vertical resolution provide different results from other models? Similarly, are there other models in the field that might be more appropriate for a comparison?

3. The paper presumes that everyone is in agreement that knowing indirect and semidirect effects in isolation is relevant for determining the effects of black carbon on climate. Yet, the authors provide no evidence that indirect radiative forcing is even linearly additive to direct forcings or other forcings, so they have not demonstrated that there is any point to simulating the indirect or semidirect effects in isolation. Further, they provide no evidence that the forcing calculations of BC effects on clouds are proportional to climate response. In fact, it is well known that when two effects are isolated, they give individual climate responses that sum to a different value compared with when the two effects are combined and a single response is determined. This is simply because

feedbacks operate between the two effects that are ignored when they are calculated independently. The authors need to state clearly in the abstract and conclusion that their results may or may not have relevance to the overall effects of BC on climate for this reason and state explicitly that the overall effects depend on several other processes not examined, including the effects of BC on snow and sea ice albedo, on surface water evaporation, and on cloud absorption (e.g., as discussed next).

4. All models tested ignore the treatment of cloud drop absorption by BC inclusions (both nucleated and scavenged BC), so the authors cannot say as they do that they are determining "cloud radiative responses." The authors need to clarify in the abstract and text that they are ignoring this effect and, as such, cannot draw conclusions about the effects of BC on cloud radiative responses (thus overall effects of BC), only on the responses that they treated, and only based on the detail they treated.

5. None of the models appear to treat radiative transfer through discrete size-resolved cloud drops or ice crystals for each wavelength of solar and thermal-IR, so it does not appear possible for the models to account for multiple scattering of cloud light through BC particles that lie interstitially between cloud drops and below or above clouds. As such, the statement that the paper includes "semi-direct" effects appears overly optimistic. The authors should first clarify exactly what aspect of semi-direct effects each model treats and then state clearly that these treatments are simplistic relative to what could be treated in an ideal model. Are separate spectral radiative calculations performed for cloudy and clear portions of each grid cell? How are cloud optical properties versus aerosol optical properties calculated when aerosols are present within clouds?

6. The indirect effects examined here appear to apply only to warm clouds, but not mixed phase clouds or ice-only clouds (except one model is stated to treat mixed phase clouds in an uncertain manner). Given that most clouds worldwide are mixed-phase, it is unclear what the difference in results would be if BC effects on such clouds as well as ice clouds were considered. The authors need to acknowledge clearly in the abstract and paper that their results do not apply to mixed-phase (except the one model) or

ice-only clouds and they are not treating explicit cloud microphysics in any clouds in any of the models.

7. Although all models are run forward in time and differences are taken among multiple simulations for each model, there is no significance testing of the results. As such, it is not possible to tell whether any of the results are statistically significant relative to deterministic chaotic variations due, for example, to a random change in initial condition in each model. The authors should each provide a global plot of the regions of the world where results are statistically significant (using a t-test) relative to a set of random-perturbation simulations.

8. It appears that simulations were run for only 1 year. If so, how do we know that results over 1 year are representative of what occurs over 2, 3, 4, 10, or 20 years? Cloud effects are climate responses, so the changes in temperature due to indirect effects and radiative heating will result in changes in feedbacks that will alter clouds and natural emission over a period much longer than one year. If the simulations are one year long, they do not appear long enough. The authors should really run their tests over at least 15-20 years if they have not done so.

A couple of these points are discussed in more detail below.

If the models tested are treating indirect and radiative effects of clouds physically, they should be able to simulate the increase in cloud fraction or optical depth with increasing aerosol optical depth followed by a decrease in cloud fraction or optical depth upon further increase in cloud optical depth in the presence of absorbing aerosols, as found by satellite correlations (e.g., Koren et al., 2004, 2006; Ten Hoeve and Jacobson, 2010). The reduction in COD with increasing AOD is due to radiative absorption in cloud drops (cloud absorption effect) as well as interstitially between drops and below clouds (semi-direct effect). The authors are including only the semi-direct effect and only part of that effect apparently, since much of the effect is due to multiple scattering within clouds, and such scattering cannot be simulated correctly when clouds are treated as bulk

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properties and individual wavelengths do not interact with individual cloud drops of different size and aerosol particles of different size between the cloud drops.

With regard to the cloud absorption effect, this has been simulated in 1-D studies in which nucleation scavenging of BC was treated (e.g., BC was treated as a single core; e.g., Conant et al., 2002). However, to account for washout (impaction scavenging of BC), it is necessary to treat polydispersion of BC in cloud drops. The absorption due to multiple BC inclusions in cloud drops is much higher than that due to a single inclusion of the same summed volume at most visible wavelengths (e.g., Jacobson, 2006, Figure 1). The effect of such treatment appears to be a much stronger tropospheric warming (Figure 4a of that paper), which is stated by Jacobson (2010) to be found strong at the surface as well in that study.

Table 1. Please include the number of vertical layers in each model, the model top height, the number of layers, in the boundary layer, the number of layers in the troposphere, and the number of layers in the stratosphere (if applicable).

Table 1.Three models assume either 50%, 65%, or 70% of OC from fossil fuels is hygroscopic. This assumption appears unjustifiable, as almost all OC from liquid fuel combustion is insoluble lubricating oil or unburned fuel oil. The authors need to self criticize results from these models.

Table 2. The title of the table says "total soot emissions," where soot is particulate matter yet the emissions seem to include "14 TgOC from natural terpene sources" which imply gas emissions. This is inconsistent. Also the ratio of OC/BC of 10:1 for fossil fuels is unsubstantiated if this is particulate matter emissions. The ratio should be 1:1 or 2:1. The authors should have a separate category for biomass burning and separate gas from particle emissions. If the organics include gas emissions, how are they converted to particles in the models? Finally, please provide references for all numbers in the table.

Table 2. How is it possible for there to be more diesel emissions than total fossil-fuel

emissions?

References

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