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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Black carbon absorption effects on cloud cover, review and synthesis" *by* D. Koch and A. Del Genio

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We thank the two anonymous reviewers for their constructive comments. We provide a response to each comment below. Please note that we have also changed the manuscript title. Dorothy Koch

Anonymous Referee #1 There has been a lot of (apparently) inconsistent results on the semi-direct effect due to black carbon aerosols. This makes this review paper quite useful and timely. I recommend publication in Atmospheric Chemistry and Physics. My comments follow below:

The authors describe the increase/decrease in cloudiness due to the semi-direct effect as a "radiative forcing". I think it would be useful to better differentiate what is a

forcing and what is a feedback. The change in cloudiness can be seen as a (fast) feedback in response to the black carbon forcing (ie heating of the atmospheric column by absorption of solar radiation). There is a growing body of literature on this (e.g. T. Andrews and P. M. Forster (2008), CO2 forcing induces semi-direct effects with consequences for climate feedback interpretations, Geophys. Res. Lett., 35, L04802, doi:10.1029/2007GL032273). The feedback can still be quantified in Wm−2 as it is done nicely at the end of the paper.

Response: This is a good point. We have added to the introduction "Note that these cloud responses are actually fast climate feedbacks (e.g. Andrews and Forster, 2008) in response to the aerosol direct forcing, therefore we now use the terminology "cloud feedback" or simply "semi-direct effect" for the radiative effect that results from the cloud responses."

The review paper discusses low- and high-level clouds. What about mid-level clouds? Can low- and high-level clouds be defined upfront, especially in the context of Figure 1?

Response: We have revised the Introduction: "Reduction/enhancement of low to mid-level cloud cover has a positive/negative climate radiative effect, therefore causing a warming/cooling; the reverse applies to thin high-level or cirrus clouds (e.g. Chen et al., 2000)."

The flow in Sections 4-5-6 could be improved. Section 4 starts with a description at the process level but quickly moves into describing results from global models. This is because understandingly the process can only be looked at in large-scale models; however it does not flow very well with Section 6 which is about the semi-direct effect in global models.

Response: We have added some clarifying adjustments that should improve the flow: 1) We have changed the title of section 4 "Enhanced low-level convergence over land regions" 2) In section 4, the first paragraph, we have clarified the use of global models to look at regional effects: “Most of the studies are performed with global models because only in such models can the large scale circulation respond to changes in AA’s however the studies focus on particular regional changes.” 3) In the first sentence of section 5 we clarify that the previous discussion has “focused on particular regions”. 4) We changed the title of section 6 to “Global impacts of AA on clouds”. 5) In the first paragraph of section 6 we added the sentence: “Here we discuss studies that have presented net global changes in cloud response.”

It would be useful to summarise the local and global model experiments / processes / results in a Table.

Response: We have added a table summarizing the studies that provide semi-direct effect estimates.

Can the authors explain what a Qflux climate experiment is?

Response: We added the sentence to section 6 “In a Qflux climate experiment, ocean temperature response is approximated by including an energetic adjustment in a slab ocean layer without allowing ocean heat transport to change.”

I suspect that some of the experiments described in Section 6 are not for black carbon aerosols but for carbonaceous aerosols. Carbonaceous aerosols also scatter radiation, and the cloud and climate response to those can be different. It would be good to stress which experiments are for carbonaceous aerosols and not black carbon aerosols. Likewise some experiments may include an aerosol indirect effect, which again can lead to a different cloud response than the one expected from just a semi-direct effect.

Response: Table 1 provides information on species and effects included. We have also included this information for studies not included in the Table and have added discussion on species and effects. Under Roeckner et al.: “We note that the experiments included changes to organic as well as black carbon, and to indirect as well as semi-direct and direct effects; these could also contribute to the cloud cover enhancement.”

Under Penner et al we also clarify that “carbonaceous aerosols” includes both BC and OC, in both the Table and the text.

On lines 4-5, page 7337, I’m not sure where the –0.18 Wm^{-2} comes from. Shouldn’t it be (0.7-1)*0.33=–0.10 Wm^{-2}?

Response: We have corrected this.

I’m not sure what is meant by a “positive cloud climate sensitivity” on page 7340, line 4 and “cloud-climate forcing response” on page 7340, line 17? Do the authors mean “positive cloud feedback”?

Response: For the first case we now clarify: “However, global climate models are already known to be diverse in the magnitude of their cloud feedbacks (e.g. Soden and Held 2006).” In the second case “a net negative semi-direct effect for absorbing aerosols”

Page 7342, lines 30-31: reference title is missing.

Response: That paper is removed.

Page 7343, line 21: “Perlwtiz” should be “Perlwitz”.

Response: Fixed.

Should the authors discuss the results presented in Jacobson’s papers?

Response: Under section 2, Cloud burn-off, we have added: “The effect may be further enhanced due to a low-cloud positive feedback loop as described by Jacobson (2002) in which cloud loss leads to increased opportunity for BC absorption.” Unfortunately although Jacobson includes many BC effects, it is difficult to distinguish them in his papers. So for example, Jacobson 2002 has a substantial negative forcing due to BC+OC induced cloud changes, he primarily ascribes these changes to the indirect effect; but it is difficult to know the role of the semi-direct effect without a separate simulation turning off indirect effects.

Figures 1f and 1g: if the model concentrations have been divided by 100 to fit the colorscale, then the unit should be “100 ng / kg” rather than “ng / kg /100”. Same with RH on Figure 1h.

Response: That figure is removed because the Koch et al. study will not be published in time.

Anonymous Referee #2

1 General Remarks This manuscript aims to review and synthesize the effect of BC absorption on cloud cover through a literature review and examples from a sensitivity study with the GISS GCM. This is a timely review of a difficult area of research with very incoherent literature. However, in my opinion this manuscript falls short in providing a sufficiently quantitative review of the literature and, more concerning, in providing an unambiguous synthesis of the actual physical effects. As a consequence, the conclusions put together in Fig. 1, which aims to provide a “summary of aerosol absorption effects on cloud cover”, seem somewhat arbitrary and there are physical explanations that would allow to come to entirely different conclusions (or arrows in Fig. 1). As a result, I do not think that this review paper is sufficiently robust to provide the implicitly suggested certainty (as discussed below). Thus, I cannot recommend it for publication in ACP. Although there were a number of specific minor issues, I will focus in the following on the major issues underlying my recommendation.

2 Major Issues 2.1 Qualitative Statements While it is clear that the literature is diverse and covering a wide range of conditions, it does not seem sufficient to reduce the phenomenon to “Absorbing Aerosol”, as it is done throughout the manuscript. The actual absorption optical depth across all cited studies varies vastly and it is most likely AAOD (or single scattering albedo) that determines the relevance of BC absorption effects over its role as CCN/IN. In particular, cloud resolving LES studies have been selectively conducted for highly polluted areas (e.g. INDOEX 1999 in Ackerman et al., 2000) but the conclusions drawn in this manuscript do not differentiate or even state the

range of conditions covered. For example, “Cloud burn-off” is treated in the brief Section 2 based on two LES studies (with specific but certainly not globally representative conditions). From there onwards, this qualitative summary is used to draw general conclusions, such as “Although BC within the cloud layer enhances cloud evaporation” in Section 3.1 and eventually in the conclusions and the summarizing Fig. 1. I acknowledge that there are insufficient studies available to draw globally valid conclusions. However, this manuscript still goes ahead to do so - leaving an impression of causality or certainty where the evidence is sparse. This is most evident in the conclusions “The sign of the cloud change depends on several factors. First is the altitude of the AA’s relative to the cloud or potential cloud level. For AA’s within the cloud layer, absorptive heating burns off the clouds and moisture. AA’s below cloud level can enhance convective activity and increase cloud cover. AA’s above cloud-level stabilize the underlying layer and can result in either decreased or increased cloud depending on cloud type and underlying conditions. AA’s above stratocumulus clouds tend to enhance cloud cover.” It would be nice if the conclusions in this research area were that simple. However, the presented evidence is not sufficient to convince me of that and it is easy to construct examples where these conclusions fail, e.g. where absorption below cloud level enhances convective inhibition with quite contrary to the stated effects.

Responses: First, we have added a Table summarizing available aerosol optical properties and cloud and radiative responses in the studies that estimate the semi-direct effect to facilitate comparison of the studies. Second, the reviewer makes an excellent point, that the literature on the semi-direct effect is sparse, diverse and confusing. This is the reason for writing this manuscript. The problem is important and many claim to understand the semi-direct effect based on the part of the elephant that they have been examining. Our goal is to motivate further research on the topic. We accept the precaution that the elephant we have constructed may not stand the test of time. However by making an initial sketch we wish to construct a framework that can be improved and challenged by future work. Therefore we have changed the language considerably in the manuscript to reflect this “straw-man” approach: 1) In the abstract we have added the sentence “We attempt to categorize the effects into several likely regimes.” In the abstract we use words like ‘may’ and ‘can’ to indicate the tentativeness of our hypotheses (these were in the original manuscript). 2) In the final sentence of the Introduction we add: “We construct a tentative framework to organize the various semi-direct effect studies that can be extended or altered by future research.” 3) Throughout the text we have changed the language to point to the evidence in the limited literature rather than claiming certainty about the particular mechanisms. In general these changes occur in the first paragraph of each section/subsection. 4) Similar tentativeness has been added to the ‘summary of regional variations’, section 5. The first sentence now reads: “From the studies on how clouds respond to AA’s that have focused on particular regions, we propose a straw-man framework, provided in Figure 1.” 5) In the second paragraph of the discussion we have added “We have constructed a tentative framework to organize previous research on the semi-direct effects as summarized in Figure 1. The framework rests on a relatively small number of studies, to be extended and improved as more research warrants. According to the studies we have examined, the sign of the cloud change seems to depend on several factors.” 6) Figure 1 caption now reads “Suggested framework to organize aerosol absorption effects on cloud cover.”

Additionally, the assessment of BC efficacy in Section 6 is ignoring (presumably significant) differences in the simulated AAOD (partly because this is not always reported) and goes as far as deriving “maximum BC radiative-cloud forcing estimates for these models”. Clearly, the uncertainties are very large and I was missing any discussion of errors as well as their propagation through these estimates.

Response: We have added available information on aerosol properties used in these studies, in Table 1. Most of the global studies are based on present-day vs pre-industrial BC and probably do not differ so much. However we have added the following sentence at the end of the discussion: “However it should be noted that BC efficacy reduction may be caused by various factors in addition to cloud changes, so that this method of extracting cloud response from efficacy and BC forcing is meant

to be qualitative.” We have added to the discussion the following on model variability: “Most studies do not report statistical significance of their results, however two studies indicate very large standard deviation. Penner et al. (2003) estimated a standard deviation about equal to the semi-direct effect, -0.39$\pm$0.38 and Wang (2004) estimated one that was double the effect, -0.16$\pm$0.31. Cloud cover interannual variability in global models is large, and it is not surprising that these aerosol impacts would also be quite variable; this contributes to the uncertainty in the semi-direct estimations.”

2.1.1 Sensitivity study The presented literature review is complemented by a sensitivity study based on a coupled climate experiment using the GISS model, referred to throughout as Koch et al. (2010). It is worth pointing out that this work is actually unpublished and still in review (which should have been clearly referenced throughout the manuscript, not only in the details of the references), yet it provides prominently one of only two figures in this manuscript. The study is simply described as “transient twentieth-century coupled climate experiments of Koch et al. (2010) for a sensitivity experiment in which pollution BC was set to zero from 1970 to 2000 and the climate compared to the case with all sources. Pollution BC includes all fossil and biofuel sources and changes in biomass burning since the year 1890, mostly tropical and African-grassland." It is impossible to judge on the relevance or quality of this study but I do not think that references to unpublished work should be considered in a review type manuscript, as assessed here. Furthermore, the conclusions drawn from this study, such as “ In the BC-reduction experiments, the climate cooled only an average of about -0.03 C during the three decades, in part apparently due to concurrently increased cloud cover.” are not convincing, at least not from the description presented here. Even if this study were based on an ensemble with a very large number of members, it is unlikely that the implied accuracy could be achieved to present statistically significant results up to +-0.01K. As presented here, I see no evidence to separate the noisy fields presented in Fig. 2 from natural variability or model noise.

Response: Because the submitted Koch et al. study is slower in the review process than this study, we are removing it from the manuscript. We feel it is not essential for this manuscript.

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