

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

Anonymous Referee #2

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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

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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*

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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.

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.

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*

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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 ± 0.01 K. As presented here, I see no evidence to separate the noisy fields presented in Fig. 2 from natural variability or model noise.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7323, 2010.

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