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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying immediate radiative forcing by black carbon and organic matter with the Specific Forcing Pulse" *by* T. C. Bond et al.

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We thank Dr. Ramanathan and Dr. Ahmed for their interest and for their very detailed listing of additional effects to consider. We agree with many of the reviewers' statements, but for some of their comments, we do not think that a full discussion should be included in this paper.

Uncertainty due to emissions. The reviewers state that we do not give any quantitative uncertainty estimates for the final forcing values. They also describe large uncertainties in emissions which can lead to uncertainties in final forcing values.

We would like to remind these reviewers that our paper describes the development of values for *forcing per emission*. In the past, e.g. IPCC AR4, values of forcing for black carbon were averaged no matter what emission rate was used, and the average was presented as a "best-guess" forcing. In fact, precisely *because* emissions are uncertain, it is imperative to separate model uncertainty from emission uncertainty. Should emissions be found to be biased low, as Dr. Ramanathan has suggested here and elsewhere, the forcing can be scaled up by the new estimated emission rate. Our Table 2 clearly states that the forcing is determined for a given emission rate. Table 1 also provides uncertainty by region. Our error, however, was not carrying this information forward to the abstract. In the revised version we have done that and have stated that it is for a given emission rate. We have also emphasized the importance of developing forcing per emission values.

The reviewers state that we *"[treat] the empirical approach as unsuitable for evaluating BC forcing."* We apologize if anything in the paper appears critical of the empirical approach. This was not our intent. In fact, we stated: "The possibility that all models could be incorrect is a serious one." We did state that a value of forcing-per-emission (i.e., SFP) could not be determined from the empirical approach because high observed forcing could also be caused by increased emissions.

Uncertainty due to model diversity. The reviewers choose four models which they propose should provide forcing values. These from Caltech, GFDL, the Hadley center, and Stanford. Of these, two (Caltech and Stanford) are included in our range of model outputs (original Figure 6). We had missed the Hadley center model (originally described by Jones et al., 2007). This model did exclude biomass burning, but it also used a higher emission rate for fossil-fuel burning than did the other models. Forcing per emission for this model is within the range of the other models, although it is higher than average. We thank the reviewer for the reference and have added it to the SFP compilation. The ensemble adjustment using the median then becomes 1.06. Using the mean, it is 1.09. (The difference between mean and median is greater than in our previous compilation.)

The GFDL model is not included because it simply chooses a burden with the justification that the results match other forcing estimates (see Ming et al., 2010). This means that the GFDL model in that paper does not give an independent estimate of forcing. In fact we are not quite certain of the model parameters as the BC burden added in that study was much higher than that of other models (compare with Schulz et al. 2006). After tracing the emission references in this paper back through 3 papers, we suspect that the emission rate is 14 Tg yr^{-1} , so this model would predict SFP of +0.61 GJ g^{-1}.

We do not deny that modeled values of +0.55 W m^{-2} forcing for black carbon have been published and are based on physically sound models. However, other forcing values have also been published. We have attempted to include all the valid model results, including three cited by the reviewers and ten others. We are interested in the reviewers' reason for including one estimate that did not provide a value of emissions, and excluding ten other estimates. We also point out that the ten other models used to estimate uncertainty contain some with SFP values as high as those of the models that the reviewers recommend. We offer Fig. 1 to support this statement. It is a revision of our cumulative BC-SFP figure from the original paper, now showing BC-SFP from 13 models. Baseline SFP values (for uncoated aerosol) from the four models selected by the reviewers are marked on this figure, spanning the entire range of our SFP values. We suggest that the models identified as "converging" are not actually converging. They are obtaining the same forcing values due to a fortuitous combination of inputs.

Uncertainty due to additional effects. Reviewers cite three additional effects that we have not analyzed or added (greenhouse effect, inclusion in cloud droplets, and brown carbon). We agree that we did not include these contributions to forcing. We did not intend to include every contribution, only those for which multiple model results exist. We have now added a statement in the "Caveats" section indicating that these effects and others also deserve analysis.

Cloud forcing. The reviewers very generously discussed studies about cloud forcing and its constraint with observations. This discussion was in response to one small

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statement in the abstract: “However, important processes, particularly cloud changes that tend toward cooling, have not been included here,” and a short section (6.1) which looks forward to the fact that cloud changes must be included for a full estimate of forcing. This section contains the sentence: “Such changes often result in negative forcing.” The reviewers present evidence that cloud changes may not result in cooling. They state that *“it may be premature to suggest that the BC-cloud changes will result in a negative forcing as implied in B2010.”* We do not think that “tend toward” and “often result in” are definitive statements. Our statements capture the fact that most model studies do find that cloud changes due to aerosols add negative forcing. Further, the studies summarized by the reviewers do not capture the entire picture. It is well known that the first indirect effect (microphysical changes in clouds) has a lower sensitivity to high aerosol concentrations than to low aerosol concentrations. Studies conducted over polluted continents and in smoke plumes will reflect that smaller dependence. The reviewers also point to the “semi-direct effect” which causes positive forcing by evaporating clouds. However, they neglect other facets of cloud thermal forcing, such as increased atmospheric stability which can result in greater cloud cover and hence negative forcing.

To summarize, there is little value in criticizing our statements about cloud forcing. These statements were included in the paper only to state that our picture was incomplete, and they fairly reflect the state of the literature. Furthermore, the critique provided by the reviewers also addresses only a subset of the effects of global-average cloud forcing, and does not provide a convincing justification to revise our statements.

Regional dependence. The reviewers close by mentioning the regional dependence of BC forcing, including its dependence on precipitation and snow forcing. We agree with the reviewers on this point. We hope that the measures we present here can be useful, as they were designed to assist in examining both regional forcing (our original Figure 3, which is now renumbered in the new version) and changes relating to the surface budget (our original Figure 5).

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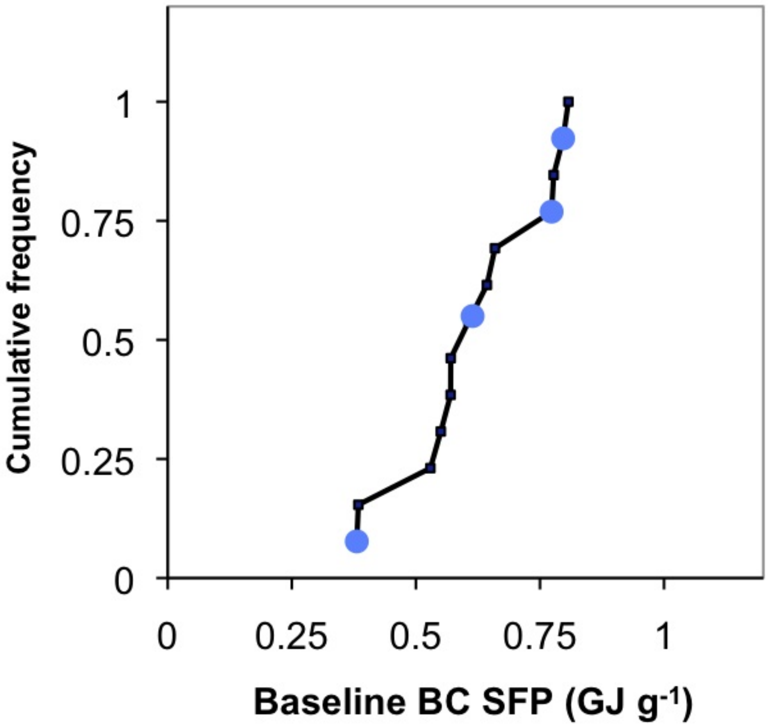

Fig. 1. Cumulative frequency distribution of global SFP from 13 models used in our paper (black line), compared with values from models proposed for use by reviewers (blue dots).

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