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Interactive Comment

Interactive comment on "Beyond direct radiative forcing: the case for characterizing the direct radiative effect of aerosols" *by* C. L. Heald et al.

Anonymous Referee #3

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Since I was asked to be a third reviewer (given the large discrepancy between the opinions of the first two), I will refer to the comments made by the previous reviewers in the hope that this will be useful to the editor. However, I wish to emphasize that I read the paper and formed most of my opinions before reading the other reviewer comments.

While I agree with the substance of the criticisms made by the first reviewer, I think that the overall recommendation against publication was harsh. I find the paper to be useful and acceptable for publication with some substantial revisions in tone that should be feasible in a relatively short time. For this reason, I list them as "minor revisions" even though I feel pretty strongly about the need to reframe the discussion.

A basic problem of the paper is that it overpromises (e.g. in the title, “the case for characterizing the direct radiative effect of aerosols”). The essential argument presented in the paper is that climate change feedbacks, modifying DRE, are likely to be more important in the future than anthropogenic DRF. This is plausible but largely speculative in the sense that this paper does not actually quantify magnitude of the climate change feedbacks or compare them to DRF. There is a literature that does quantify climate feedbacks on aerosol DRE, some of which is cited in the discussion, but this paper does not attempt similar calculations (it uses fixed 2010 met fields). Therefore, it really falls short of the state-of-the-art. The only evidence for the argument is that, according to these numbers, DRE is five times greater than DRF. This is not new (DRE has been quantified in numerous prior studies and is widely recognized to be much larger than DRF due to sea spray and mineral dust). Also, the magnitude of DRE is insufficient to make the argument since the magnitude of the climate change impacts on natural aerosols obviously matters. So, the paper really does not present any new, quantitative results or analysis to advance this argument beyond the current state of the literature – even if I find it plausible.

In reality, it provides a (useful) single-model quantification of DRF and DRE that falls short of these stronger claims. This is sufficiently useful to merit publication, but the tone of the paper should really be scaled back to reflect this reality. In particular, I recommend a few things:

1) The current title should be abandoned in favor of something more in line with the results presented, such as “Aerosol radiative forcings and effects in GEOS-Chem”.

2) Statements, even if plausible, that are not directly supported by quantitative results in this paper such as “while the climate feedbacks on aerosols under rising global temperatures will likely amplify.” should be removed from the abstract.

3) p. 32928, lines 23-25: “In this study our objective is to quantify these two metrics, and make the case that the DRE is a necessary complement to the DRF for aerosols.”

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The paper presents results to obtain the first objective. As already indicated, there are no concrete results for the second. The second objective should be deleted.

4) I don't think it's necessary to delete all discussion of climate feedbacks on DRE. It's fine to cite them in the introduction as motivation for calculating DRE. It's fine in the final discussion to speculate a little on the implications of DRE being much larger than DRF. I draw the line at anything, especially in title/abstract, that claims this paper has demonstrated that climate feedbacks on DRE are greater than DRF.

5) The literature on climate feedbacks/natural aerosols that appears in the discussion (plus other papers not cited) really should be cited up front in the introduction to avoid giving the reader the initial impression that the argument is original to this paper.

6) While the authors should respond to the specific "deficiencies" in radiative transfer calculations mentioned by the first reviewer, in my judgment, these are not serious enough to preclude publication. If at all possible, I do think the paper would benefit greatly from another calculation where BC is treated as internally mixed as I also believe it's more realistic despite the recent Cappa paper. At the very least, the limitation should be noted earlier (in the methods section), and the results section should discuss how much its omission is likely to affect the numbers presented (e.g. if we double BC absorption, total DRF does change notably).

Minor comments:

The introduction discusses the effective radiative forcing (ERF). Since the authors are using a CTM with fixed met fields, their paper cannot really address this issue. While I don't have a problem with an introduction that strays a little from the main topic of the paper, does this discussion really need to be here?

p. 32931, lines 22-23: "This approach greatly increases computational speed while maintaining the effective accuracy of a line-by-line calculation." Since a correlated-k method must degrade accuracy (even if by a negligible amount), this statement de-

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serves some quantification. Alternatively, it may be deleted since the accuracy of RRTMG for line-by-line absorption calculations doesn't seem to be terribly important for aerosol effects.

p. 32935, lines 18-24 regarding AeroCom I BC biases: the authors correctly point out that the models sometimes overpredict BC mass concentrations but neglect that they also underpredict BC over Asia. The discussion should be more objective/balanced.

p. 32939, lines 11-18 "Uncertainties in our estimate of DRE are likely even larger than uncertainties on DRF." (and subsequent paragraph of discussion) This is pretty speculative as the paper doesn't include any sensitivity/uncertainty calculations.

p. 32940, lines 10-12 "This calls into question the usefulness of separating so-called "anthropogenic" and "natural" aerosol forcing." Please be careful. There are some gray areas where the distinction is murky, but most of the time, the distinction is pretty clear and the separation is useful. Let's not throw the baby out with the bath water.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32925, 2013.