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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 #1

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In this manuscript the authors i) discuss the concepts of aerosol direct radiative effect (DRE) and direct radiative forcing (DRF) and ii) present new calculations of DRE and DRF based on their model. The manuscript does not bring new insight on either item i) or ii). Although factually correct, the discussion on DRE/DRF is poor, incomplete and mostly repeats bits and pieces stated elsewhere. The new calculations are just adding one more model to the existing pool of model results but do not inform the reader on intermodel differences and uncertainties. Moreover the radiative transfer calculations have a number of deficiencies as explained below. For these reasons I cannot recommend publication at this stage. Publication of a revised manuscript would require a substantial amount of additional work to make this contribution a more original one.

Major comments

The manuscript shows a surprising lack of knowledge of the literature on the subject.

- The authors are right that the distinction between DRE and DRF is somewhat confused in the literature. However there is whole lot of articles where the distinction is made very clearly, as it is the case in most modelling studies but also in some early observational studies (e.g. Bellouin et al., Estimate of aerosol direct effects over land and oceans from MODIS, Nature, 438, 1138-1141, 2005; Bellouin et al., Estimates of aerosol radiative forcing from the MACC re-analysis, Atmospheric Chemistry and Physics, 13, 2045-2062, 2013; etc.). The IPCC (2013, chapter 7) is also very clear on this. The title and the introduction suggest the authors are the first ones to clarify the concept when in fact they should be pointing to the right literature on the subject.
- Calculations of DRE are not new, see e.g. Boucher and Tanré, Estimation of the aerosol perturbation to the Earth's radiative budget over oceans using POLDER satellite aerosol retrievals, Geophysical Research Letters, 27, 1103-1106, 2000, and many others since then. The LW contribution to the DRE was calculated by e.g. Reddy et al. (JGR, 2005) and some early papers by Jacobson among others.
- There is some literature on how to differentiate feedbacks from forcing in relation to biogeochemical cycles (e.g. Gregory et al., Journal of Climate, 2009; Raes et al., JGR, 2010). This literature is ignored here although the authors frame their paper around the importance of aerosol feedbacks. A similar framework to that of Gregory et al. was used by Carslaw et al (ACP, 2010, cited in the present manuscript) and feedback parameters in unit Wm^{-2} K^{-1} were provided. This metric, also used in Chapter 7 of the IPCC (2013), seems more appropriate than DRE alone when it comes to quantify aerosol feedbacks.

The authors perform their radiative calculations with a rather accurate radiative transfer

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code. This is an interesting feature of their study. However it is well known that DRE and DRF are very sensitive to the model's surface albedo, relative humidity and cloud properties. In this respect the authors are not doing any better than other publications, and they miss here an opportunity to set a higher standard in aerosol DRE and DRF calculations.

- Little information is provided on the cloud distribution and SW/LW properties beyond the assumed size of the cloud droplets and ice crystals. It is clear that a large fraction of the spread in aerosol DRF is due to the input cloud climatology (e.g., Stier et al., Host model uncertainties in aerosol forcing estimates: Results from the AeroCom Prescribed Intercomparison Study, Atmospheric Chemistry and Physics, 13, 3245-3270, 2013).

- The authors do not consider diurnal variations in the surface albedo. It is well known that the ocean surface albedo has large diurnal variations (because of diurnal variations in the solar zenith angle) that are positively correlated with the aerosol upscatter fraction (which also varies with solar zenith angle). Neglecting this covariance results in systematic biases in aerosol DRE estimates, while assuming a Lambertian surface introduces further uncertainties (see Bellouin, et al., Estimating the aerosol direct radiative perturbation: impact of the ocean surface representation and aerosol non-sphericity, Quaterly Journal of the Royal Meteorological Society, 130, 2217-2232, 2004).

- The authors consider aerosol to form an external mixture, when it is well known and for a long time that internally-mixed BC increases absorption substantially (IPCC, chapter 7, 2013, and many references therein). The reference to Cappa et al. (2013) to justify the uncertainty on the absorption enhancement is misleading as there is plenty of evidence in favour of an enhancement effect.

It is not clear how the conclusion that "SW-only aerosol DRE or DRF estimate would

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overestimate the cooling effect by 5–10%" comes from (page 32937, line 15). The 10% upper bound is about correct for the DRE but is an overestimate for the DRF because most of the LW effect comes from non-anthropogenic aerosols such as dust and volcanic stratospheric aerosols.

Minor comments

page 32927, line 20: The sentence has no meaning. Should DRF be ERF here? This would make more sense, although still confusing I think.

page 32927, line 23: I do not see how something published in 2007 can confuse something written in 2013. IPCC (2013) is very clear about the distinction between DRE and DRF (they are called REari and RFari by the way).

page 32934, lines 25-27: the sentence "aerosols are typically more scattering than surface albedo" does not mean anything. The authors are comparing apples and oranges here.

page 32946: the reference to the IPCC SPM 2013 is inadequate as RF/ERF and RFari/REari are not discussed in much details there. The authors should refer specifically to Myhre et al, chapter 8, 2013 and Boucher et al., Chapter 7, 2013.

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