Steve

Thanks for your very generous and editorial review. You raised some points and concerns. So let me get to those.

frozen version: the version that is now placed (along with programming code) on the ftp-site is frozen in as MACv2 – also in the context of the Tellus and ACP paper (potential future changes will be referred to as potential versions 3 or 4 …). Recent updates were done to made corrections to the format (correct times in netcdf files), to add programming code or to cater to some special MACv2 output requests

concerns on using output from global modeling: It is nice that you refer to the 2006 AeroCom modeling paper, which displayed a lot of diversity. In those 'bottom-up' approaches there were different paths (e.g. component strengths, water uptake) to eventually come up with approximately the same global AOD. Thus you state concerns that this will affect MACv2 optical properties. These modeling uncertainties are avoided here by relying on ambient (measured) AOD (amount) and (observation tied via inversions) AAOD (absorption) data. But it is true that AODf (SU+OC+BC) and AODc (DU+SS) monthly global distributions from 'bottom-up' simulations are applied for spatial context. But firstly, the observational data are just scaled (no absolute modeling values come into play) and secondly, a 14 model ensemble median distribution is used so that spatial distribution outliers of particular models are avoided. There is certainly a valid question, why model data of AEROCOM phase1 and not from the more recent phases 2 and 3 are used. This is simply, because many more submissions into the AeroCom data-base are not well tested. And with a focus on anthropogenic fine-mode AOD or a related process, often natural components (dust and seasalt) are not given the needed attention. Many model display for natural aerosol extreme features so that the diversity of ensemble data of more recent model submissions for total AOD (and also AODf and AODc) is much larger. That stated, for the fine-mode AOD (AODf) there is improvement. This is also the reason, why for the analysis of the 1.indirect effect model output of AeroCom phase 2 is discussed on compared to similar associations from satellite observations. The unique approach to component radiative impacts in this paper is that AOD component contributions (and even dust coarse sizes) are in a 'top-down' approach deduced from (2D column) aerosol optical properties (AODf, AODc, AAODf, AAODc) and ambient REf (for fine-mode size)… so that component assignments are possible without the need to deal with water uptake uncertainties.
For the anthropogenic definition there, there is admittedly a strong reliance on global modeling although only in a relative sense (as ratios - and NOT absolute values - from ensemble modeling are applied).

concerns about the missing uncertainty discussions: There are many uncertainties and I tried to focus on those that matter most: anthropogenic definition and representation of indirect effects. For instance: What is the pre-industrial state? Can it be reduced to fine-mode AOD additions? How much fine mode AOD was there? How much BC was there? Is it sufficient to limit indirect effect to Twomey? How important are spatial variation of the Twomey effect?
I do not think that other issues matter that much as this paper works with observations, which use data on ambient aerosol (water uptake is non-issue) and on absorption (so that SSA and even dust size). The impact of surface reflection (and temperature for large dust) are an issue, but only if they change (and

he in the forcing simulations they do not). So in summary I consider my uncertainty estimate (under all asumptions made) as fair. And the uncertainty range, as suggested, was already stated via the range in the abstract.

minor points

- thanks for noticing the missing negative sign for the likely aerosol TOA forcing range

-'at TOA' … has been added (and climate warming by BC is stronger at all-sky than at clear-sky) still +0.55W/m2 would be the maximum BC warming and only if all BC todays would be anthropogenic … what is not. So Aeronet based data are help to set an upper limit rejecting the large BC warming suggestions on global scales (e.g. Ramanathan, Carmichael). And considering all the scattering co-emitters with BC emission the climate warming mitigation even on short time-scale is rather limited

- I would stick with radiative properties (in describing the single scattering properties at 550nm)

Dear reviewer.

Thanks for having a look at the revised paper !

The author has adopted most of the suggested changes, and satisfactorily justified not adopting the others, apart from one of the original major comments regarding omitting cloud adjustments in their work

I am aware that there are likely cloud adjustments to the imposed radiative forcing. Although my method has some advantages (e.g. as not being affected you variability of freely developing and possibly also even so slightly changing clouds), my method is static and cannot account for these aerosol induced changes other than the (the via satellite associations) prescribed 1.indirect effect on low altitude clouds. I am banking here on multi-decadal climate simulations with different models, which suggest that these adjustments (when the same direct and indirect forcing is imposed) are relatively minor.

"In particular, the paper aims to provide an estimate of the aerosol forcing, but ignoring cloud adjustments seems a significant omission and requires much stronger justification, particularly when discussing potential uncertainties. For example, on P2L9-10 the author states that feedbacks (adjustments) can be considered secondary, though the provided citation makes no such assertion – they just assume it for their purposes."

That paper (Fiedler et al) makes as distinction between static and instantaneous radiative forcing (based on direct and satellite based 1 direct effects as in my approach) and one, where feedbacks on clouds are permitted (there referred to as 'effective radiative forcing) … and the differences are on the order of 10%, which I consider secondary. I will try to strengthen this point in the uncertainty discussions

Author response: "Here off-line radiative transfer simulations a used. Thus, no feedbacks or (earth system) adjustments to the forcing can be considered. Fortunately, there is a recent RFMIP exercise where in long-term simulations (with fixed SST) compare results of the (direct and first indirect) forcing from dual calls with the effective radiative forcing, which allowed atmospheric adjustments to the forcing in global models. The forcing differences are found to be relatively small on the order of 10% (Fiedler et al., 2019)."

Yes that was (and still would be) my response.

While I appreciate these cannot easily be accounted for in the off-line framework used by the author, this omission requires justification in the introduction and an acknowledgment in the discussion on uncertainties. In particular, neither Fiedler et al. 2017 or 2019 show that cloud adjustments to the aerosol indirect effect are small. They choose to ignore cloud adjustments to simplify their framework, see in particular Section 2.2.2 of Fiedler et al. 2017. Whilst a similar argument could be made here, it should be done explicitly.

It is true that the rapid adjustments directly via cloud microphysics are not explicitly simulated when MACv2-SP is implemented in the radiation code, although one can incorporate a net effect on the radiation balance as effective parameter. Some models, however, choose to implement the MACv2-SP perturbation of the cloud droplet number in their cloud microphysics, not the radiative transfer calculation only. This is the case for EC-Earth so far (see Fiedler et al., 2019). The model therefore simulates cloud adjustments like more complex aerosol-climate models do. The contributions from cloud adjustments will be further assessed in RFMIP, but based on the current evidence the contribution of rapid adjustments is small compared to the instantaneous radiative forcing.

That said, the point should be made that cloud feedbacks are ignored, at least in the uncertainty discussions. We know relatively little about cloud responses and even simulations with global models have to be questioned as smaller scale processes and responses are in global models are heavily parameterized and possibly unable to represent actual effects.

the abstract has been rewritten and the second sentence now states: “This model-setup cannot address rapid adjustments by clouds, but these effects are believed to be small.”

In the uncertainty estimates this sentence was added
“Finally all presented results refer to an instantaneous impact. Short-term cloud adjustments are not included, but they are much smaller (on the order of 10%) in comparison (Fiedler et al., 2019) with a tendency to reduce the aerosol radiative effects”.