

Interactive comment on “First forcing estimates from the future CMIP6 scenarios of anthropogenic aerosol optical properties and an associated Twomey effect” by Stephanie Fiedler et al.

Anonymous Referee #1

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Fiedler et al. present a modelling study in which they interpret the future emission scenarios of Riahi et al. (2017) using a simple model implemented in a GCM. The two aspects that provide added value compared to the Riahi et al. paper in my opinion are that geographical distributions are shown here, and that the scaling of Stevens et al. (2017) allows to convert the emissions into forcing values given the assumptions in the simple MACv2-SP approach (with some extra model information added from the simulated cloud fraction- and cloud droplet concentration distributions). As far as I understand, one of the co-authors, Gidden, prepares another manuscript for Geophys. Model Devel. that possibly covers the former aspect in a similar way.

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The manuscript suffers from poor explanations. It is very difficult to follow the set-up of the simulations (e.g. number of years integrated, boundary conditions for these simulations). The main problem, however, is that it is completely unclear which aerosol species are assessed. In subsection 2.1 of the “Methods” section (p3, l12) the authors explain they distinguish between “biomass burning” and “industrial” aerosol emissions, with substantially different single scattering albedo. This interpretation is taken up later in the discussion of results. In contrast, subsection 2.2 (p4, l7) explains that only sulfate and nitrate are investigated.

In summary, after these explanations are added, and after the specific comments are addressed, I believe the paper has some merit and should be published in Geosci. Model Devel.

p1 l12 – it might be instructive to also report the 2090s forcing relative to 2015.

p2 l10 – Mauritsen et al.: drop “and”

p3 l5 – “need” → “desire”

p3 l27 – w is not explained. Is it some sort of weight? I don’t think it is meaningful not to specify the number of species in the sum (and in the text). Isn’t it just two?

p3 l29 – p4 l1: I do not understand what the authors do here or mean. A figure might be helpful, or more text to explain.

p4 l4 – is that result shown somewhere, or is there a reference? - the statement needs to be corroborated or withdrawn.

p4 l8 – the relative relevance of sulfate and nitrate depends on the abundance of SO₂ and oxidants. It thus seems an oversimplification to assume it is constant with time and geographical location. Some assessment of the error introduced is at least necessary.

p4 l9 – some discussion why in particular absorbing aerosol can be neglected is necessary.

p4 l12 – is this the global mean value (if so, only 0.5% difference)? Is this similar in individual regions?

p4 l26 – how can this affect clear-sky radiation balance?

p5 l8 – these are equilibrium simulations, if I understand correctly. Then “six” does not mean anything, but the length of the integration is relevant.

p5 l11 – similarly, what does “three experiments” mean in this context?

p5 l13 – more precisely, this is decreasing the tunable parameter τ_{gl} , I believe?

p5 l14 – what motivates the name “LBG”?

p5 l17 – what defines a specific year (i.e. why call it “2000 – 2010” rather than “2005 aerosol”)? Is the sea surface temperature from observations?

p5 l21 – what is “without τ ”, especially for the Twomey effect? Don’t the authors rather mean, “with scaling factor (Eq. 2) for 2090 and with scaling factor of 0”? The “180 annual estimates” make me conclude there are 6 realisations (with whatever difference between them) of 30 years integration time each. Is this correct? The authors need to explain clearly what they did.

p5 l23 – I am lost and cannot understand why thirty. What is different between the thirty realisations?

p5 l25 – I strongly suggest not to overload the symbol “E” (that stands for emission scaling) but to use a different one.

p6 l25 – it would be good to motivate this analysis. I would guess that there is no added information here compared to the emission scenarios. Maybe this section can be dropped.

p8 l12 – is this correlation not just by construction of the simple model?

p8 l14 – I did miss the introduction of absorption. Didn’t p4 l7 explain that only sulfate

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and nitrate are used?

p8 l23 – a discussion on these rapid adjustments is necessary. What exactly happens here in the model?

p12 l1 – journal is missing

Fig. 1 – a figure showing the geographical extent of the plumes is necessary. I suggest to rather use the same scaling for all panels. A global-mean curve would be useful.

Supplementary material: I wasn't able to open the netcdf file. Is there a formatting mistake?

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