Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-172-RC1, 2019

Geoscientific Model Development Discussions

Interactive comment on "Beijing Climate Center Earth System Model version 1 (BCC-ESM1): Model Description and Evaluation" *by* Tongwen Wu et al.

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Received and published: 10 September 2019

This paper provides a description and evaluation of the aerosols in the BCC-ESM. The paper provides a reasonable overview of the model characteristics and sufficient comparisons to be useful. However, it suffers from a certain number of omissions and lack of details that should be fixed before publication moves forward.

My main concern in this paper is the statement at lines 316-318: "The whole system in BCC ESM1 fluctuates around +0.7 W m-2 net energy flux at TOM without obvious trend in 600 years (Fig. 1b), and the global mean surface air temperature shows only a small warming (Fig. 1a)". If this is the case, then there is a real problem with this model. There cannot be a significant TOA imbalance without a significant trend in surface

temperature, unless the ocean is taking up all that excessive forcing. Which would mean huge drifts in the mean ocean temperature. The authors need to clearly identify if this is a mistake, or the difference between TOA and TOM, or whether there is a drift in ocean temperatures. But as stated, this means there is a huge non-conservation of energy in the model.

Another concern is that the authors make considerable use of the CMIP5 concentrations (by the way, a correct reference to this data would be Lamarque et al., ACP, 2010), which is a somewhat circular evaluation. Indeed, the CMIP5 data were generated using a chemistry model very similar to the one used in BCC-ESM1. It is true that the emissions are different, but then the main evaluation this analysis provides is on the similarity of emissions. I would therefore encourage the authors to expand their model evaluations to include more observations. For example, the paper https://www.geosci-model-dev.net/9/1853/2016/gmd-9-1853-2016.pdf includes analysis against aircraft observations.

While I understand that the focus is on aerosols, it cannot be ignored that the rate of formation of sulfate is dependent on the levels of oxidants in the troposphere. It would therefore be very useful if some documentation and evaluation of oxidants (at the very least ozone) is included in the paper.

Minor comments

1. Lines 155-157: why is convective transport not considered?

2. Lines 189-191: Following the work done in CAM4, it would be quite straightforward to include some basic representation of NH3 chemistry (see Lamarque et al., GMD, 2012, section 5).

3. Line 207: reference Hoesly et al.

4. Lines 251-254: this is an important aspect of the model that needs more discussion. In particular, what is the aerosol indirect effect in this model?

5. Line 257 (and other places): it is AerChemMIP, not AeroChemMIP

6. Lines 273-276: which emissions are those? The CMIP6 (as the CMIP5) had all emissions necessary for tropospheric chemistry, as long as some splitting of lumped emissions (like total VOC emissions) were performed.

7. Line 288: volcanic, not volcano

8. Line 290: this is confusing. It is really not clear that stratospheric aerosols are represented in this model. Are those really stratospheric emissions, or tropospheric emissions of the non-eruptive volcanoes?

9. Line 293: what are the total NOx emissions from lightning (in TgN/year)?

10. Lines 301-303: this is not clear. Are you describing the relaxation time (of 10-days) of the concentrations towards the climatology? Is the climatology changing over the course of the historical period?

11. Line 337: there are some anthropogenic/biomass SO_2 emissions in 1850, just small ones.

12. Line 373: the correlation really only reflects that the lifetime of SO_2 is very short and not changing much, and therefore the burden will directly follow the emissions.

13. Line 376: what is the "NCAR data package"?

14. Line 400 (and others): a lot of analysis compares to Liu et al (2005). It would be useful to include more publications, especially more recent ones.

15. Figure 5: why is the BCC ESM1 data also shown as 10-year averages? Also, are those the results of a single ensemble member? More details on the simulation would be useful; in particular I am assuming that this is a fully coupled simulation.

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