Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-113

Andrew Gettelman (Referee)

Referee comment on "UKESM1.1: development and evaluation of an updated configuration of the UK Earth System Model" by Jane P. Mulcahy et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-113-RC1, 2022

Review of "UKESM1.1: Development and evaluation of an updated configuration of the UK Earth System Model" by Mulcahy et al.

by Andrew Gettelman

This manuscript is a comprehensive assessment of updates to the UKESM1, documenting a new version of a major Earth System Model. The manuscript is well written and should be acceptable for publication in Geoscientific Model Development with minor revisions.

One general comment is that it's still unclear to me how you are ascribing sensitivity of the model to SO2 and SO4 partitioning, and the processes that are acting. You talk a lot about deposition, but very little about oxidation rates. See detailed comments below. This could be better clarified in the manuscript. Otherwise this is an excellent and comprehensive treatment of model evaluation.

Specific comments are below.

Page 1, L12: note this is a reduction in magnitude of aerosol ERF (which is negative)

Page 3, L59-63: This discussion is confusing. I read it 3 times and it still doesn't really make sense. Is the problem SO2 deposition or SO2 oxidation? It seems to be both, but you just say it's SO2 deposition I think. Please clarify.

Page 3, L70: where is section 3?

Page 3, L80: So is GA 7.1 also part of HadGEM3-GC3.1? The terminology is a bit confusing.

Page 5, Table 1: How is DMSO + OH —> 0.6SO2 a balanced chemical reaction for S?

Also, might note you have neglected things in these reactions (eg. DMS + OH —> SO2). Maybe better to have the whole reactions here?

Page 5, L141: does the SO4 go into a different mode?

Page 6, L152: maybe add a sentence on how these values were derived from AMIP runs? What was the methodology in brief?

Page 7, L164: 'snow metric' is strange. Just call it the TOA outgoing clearsky SW flux over land…

Page 7, L172: so the tuning darkens the present so it is warmer and does not change as much in the future?

Page 7, L174: Capitalize Dust Optical Depth

Page 8, L199: What does it mean that 'tuning was omitted'? A parameter value came from somewhere. Is it that the protocol suggested that sub-grid gravity wave flux be adjusted to get the right period of the QBO? Or was this found after UKESM1 was released? Please be a bit more descriptive of the process.

Page 8, L204: mean QBO period…..(also line 205)

Page 8, L211: What does the parameter do? I guess the tuning controls the LWP and the SW cloud radiative effect?

Page 9, L229: what are the SSTs in the piClim-control

Page 9, L234: what is the second piClim-control-2014 experiment called?

Page 20, L401: what is 'mean Nd' averaged over within the column? It's given as a concentration per unit mass, so it's not column. Averaged over cloud layers?

Page 21, L435: Does the improvement when below 700m is included indicate that OHC is not partitioned at the right layers in UKESM?

Page 23, L465: what is the mechanism by which weaker aerosol forcing lowers AMOC? That does not seem trivial or obvious. Please explain how this is 'consistent'

Page 25, L496: For the Antarctic sea ice you state there is no significant difference between UKESM1 and UKESM1.1. But is there an increase or decrease over time, or no change? And how does that compare to observations.

Page 27, L527: does 'stronger' mean less negative? If so, awkward. It's actually a reduction in magnitude. Please clarify. Also the 'increases' is a decrease in magnitude right? (Less negative). Might be more clear to use magnitude.

Page 27, L529: why is the aerosol effect positive over China and India?

Page 35, L658: do you want to comment on TCR being high as well? Also you might note that the cold temperature bias does not seem to be related to high ECS, since changing it did not alter ECS.