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Geoscientific Model Development Discussions

Interactive comment on "Review of the global models used within the Chemistry-Climate Model Initiative (CCMI)" *by* Olaf Morgenstern et al.

Anonymous Referee #1

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Review of "Review of global mean models used within the Chemistry-Climate Model Iniative (CCMI)" by Morgenstern et al.

This manuscript provides information on the global chemistry models that have supplied simulation data for the CCMI phase 1 simulations (CCMI-1), as well as some detail on the set up of the various simulations, including emissions, sea surface temperature and sea ice boundary conditions, and other driving data. Papers of this sort can be an invaluable resource for scientists analyzing multi-model output, especially when they record important distinctions between the model as used for the study, compared to the version that might be described in the standard reference.

This manuscript does contain a lot of useful information on the different models used for CCMI-1, and the authors must be commended for what must have often been a

tedious project! However, I believe it could be improved if it was organized slightly differently, and with a more proportionate/balanced amount of space spent describing each model and process. I would also recommend that the authors read through the final text carefully, as it occasionally reads like a final draft rather than a final version. The manuscript is otherwise extremely useful and uncontroversial, and I would not foresee there being any issue with its eventual publication.

I have detailed my general criticisms below, together with a non-exhaustive list of line-by-line comments (many of which are catching specific instances of a general malady).

General criticisms 1. The time spent discussing the different aspects and different models seems a little disproportionate. For instance, there is often reference to the specific aspects of the MetUM family of models, but not the same information for the others. With regard to the detail on the forcings, the section on solar forcing provides a good amount of detail, whereas the information is more scant in some of the other sections (e.g., ozone precursor emissions). One suggestion here would be to describe the general case and list the models that differ from that, including in the way that they do. This is the kind of information that the reader will need to be able to get clearly and easily when they are considering their own analysis of the ensemble. (I appreciate that this is about balancing what to have in the supplement vs the main text.)

2. A suggestion related to the above: The authors could consider putting the description of the CCMI-1 simulations/scenarios first before launching in to describing the models in general (resolution etc.) and then how each one might be set up differently for a given boundary condition or forcing file. For this second part, the authors can then (for example) have a sub-section that describes the aerosol modules and aerosol emissions, saying how the latter are differently implemented depending on the inter-model differences in the former. This way the reader only has to look in one place to understand model differences and implementation differences for the given area of interest.

3. Description of the models. Not all the models are CCMs I believe (e.g. P1, L16), in that not all couple chemistry and climate throughout the whole atmosphere. In chapter 7 of the (unfinished) IGAC TOAR project, there is a suggestion for a naming convention for global chemistry models: a “CCM” is where chemistry is coupled to climate in a two-way matter (although is this the case throughout the atmosphere for all the models here?); a “chemistry GCM” or “GCM with chemistry” is where the chemical changes do not feed back onto the climate (e.g. UM-CAM); and a “CTM” is a different model framework that uses (elements of) offline meteorology to drive transport, temperatures for chemistry etc.

4. A general point about CCMI that the authors should get across: unlike CMIP5, CCMVal, ACCMIP, HTAP etc, CCMI is not a “MIP”, or even a modeling experiment. Rather it is a community that addresses questions related to chemistry-climate issues, by running, analyzing, evaluating and improving global chemistry models (cf. GEIA and iLEAPS for example). As such, the present manuscript should be clear that it is describing the models and experiment set up for the CCMI Phase 1 simulations (CCMI-1). It could also make reference to the phase 2 experiment, being conducted jointly with Aercom and representing the chemistry and aerosol communities’ contribution to CMIP6 (AerChemMIP). Phase 2/AerChemMIP might involve different models, so we should perhaps be careful not to talk about “CCMI models”.

5. Not a serious criticism for such a paper, but there are often statements made without citation or justification. Some examples are given below, but could the authors please check this when editing (e.g., the importance of clouds for climate sensitivity etc.)

Specific comments and technical corrections P1, L5: not just air quality, but also tropospheric composition (also in the Introduction)

P2, L11: “Previous generation models” – when? Reference?

P2, L12: Suggest rephrasing the “multi-faceted” bit

P2, L22: Delete “activities”

P2, L25 (and throughout): I understand that it is convenient to refer to CCMVal-2 for previous model descriptions, but not all the CCMI-1 models took part in that experiment. This is acknowledged in some places, but perhaps just better to remove some of the references to CCMVal-2 as this is different.

P2, L32: The paper -> This paper (or “The present paper”?)

P3, L6: MOCAGE may not have been in CCMVal-2, but it was in the other CCMI-1 forerunner, ACCMIP.

P3, L20: “impact of ozone. . .” – Clearer/more general example perhaps?

P4, L5: improved -> increased

P4, L6: what was the baseline for these resolution “improvements”?

P4, L10: where do these exception models stop? (Also “cf.” -> “see”? Since cf. means compare with)

P4, L15: SD runs – are all the models nudged, or are some actually closer to CTMs? (CESM family perhaps?)

P4, L15: QBO “may not” require specific forcing – don’t we know this for certain? (There are other examples of the use of “may”, when these should be nailed down!)

P4, L29: How similar are the “MetUM” family of models? Could the authors comment (and include in the text) on how many other families there are? What are the implications for model independence and assessing structural uncertainty in the CCMI-1 ensemble? . . .In any case, could these “families” be identified up front to help the non-expert reader?

P5, L27: Unified Model based -> MetUM (?) . . .Another example of MetUM being singled out, with only a brief mention of 2 other model families (SOCOL and CESM).

P6, L5: Spell out “w.r.t.” (other occasions too)

P6, L12: “constituent fields” – which constituents are meant?

P6, L15: How are the emissions done for the lumped mechanisms? Do models just emit the reactive carbon for the species that they have, or is the total amount proportionally shared out over the available VOCs? With reference to my earlier general comment, this could be a good example of where a description of a process could be combined with a description of the specifications for the simulations.

P6, L22: Are CH4 *concentrations* prescribed?

P7, L2: “models vary” – how?

P7, L13: Consider (briefly) describing what is meant by “numerical diffusion”

P8, L10: Reference for statement about clouds and climate sensitivity?

P9, L1 and L6: What about SW and LW treatments for non-SPARC models?

P10, L3: Reference for NEMO?

P10, L8: Do the models apply the solar forcing to photolysis? (I know this is in Table S29, but good to flag here)

P11, L15: similar -> the same as

P11, L17: More specifics for the solarTrend simulation?

P11, L21: Helpful if spelt out what this simulation is

P13, L8 and L11: “essentially” is imprecise

P13, L10: Consider a map of NOx emissions changes over different time periods, since it shows the complexity of the changes for anthropogenic emission changes. Similarly for this section and the following one, the authors could discuss how the differences in the model chemistry schemes can result in a large inter-model differences in the actual

VOC emission flux (NOx and CO to a lesser extent).

P13, L17: To be clear: is it that REF-C2 uses MACCity to 2000 and then RCP6.0, and REF-C1 uses MACCity to 2010?

P13, L26: Reference for MEGAN?

P14, L3: “is recommended” – what else was used, and by which model/what models?

P14, Fig 2: Suggest that contours are overlaid as lines, else the non-significant trends and those between +/-0.1 are not separated. Masking of non-significant trends might be better done with (say) a gray palette with this color bar.

P16, L8 and L9: “can be” and “should be”, but what was actually done?

P17, L9 and L10: has -> have (verb agreement with “models” not “a majority”/“a subset”)

P17, L14: “Some models” – which?

P18, L7: “experience” by who? Ref? Folklore?

P18, L16: are advised -> should

P18, Appendix A: The model information varies wildly in detail. Can this be harmonized? Erring on more detail (e.g. CESM) would be useful.

P25, Appendix B: Will CCMI be keeping a website with a list of known issues that arise after the publication of this description?

Tables (including Supplement): Could the authors please review the captions to ensure that they can be understood without too much cross-referencing. Table S5 seems particularly esoteric, including the abbreviations in the table itself (FFSL?). In addition the citations are often missing brackets etc. Perhaps not a big deal you might think, but this is a subject that is about attention to detail and it gives the reader confidence that the information has been compiled with care!

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