Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-199-AC1, 2016

Geoscientific Model Development Discussions
Open Access
EGU

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

Olaf Morgenstern et al.

olaf.morgenstern@niwa.co.nz

Received and published: 16 December 2016

We thank the referee for his/her detailed and thoughtful comments.

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, 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

Printer-friendly version

Discussion paper

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.

We have reorganized the text slightly; in particular, we have reordered the paragraphs (also in response to reviewer 2's comment). We have proof-read the manuscript again.

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.)*

   Indeed it has been our philosophy to report on models by exception, i.e. to discuss the general case and in addition only single out those models that deviate from that. We have reassessed the whole text with a view towards adding more detail. As for the section on emissions, this has already been detailed by Eyring et al. (2012); hence the discussion is kept short here. It has been our ambition to

give equal treatment to all models; hopefully the various minor changes applied to the text have levelled out any perceived inequalities.

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 intermodal 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.*

   The organization generally follows Morgenstern et al. (2011) but of course this is a matter of taste. The majority of model aspects discussed here is not associated with any external forcings. We accept that for some topics the reader has to look in two different places to find relevant information (one for how a particular aspect is implemented in the models and one for information on scenarios and forcings). The forcings generally define a storyline (such as best-of-knowledge reproduction of the past, middle-of-the-road anthropogenic-only climate forcings, hypothetical scenarios such as fixed-GHGs, etc). We feel it is best to leave these things separate from model definitions.

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.*

Printer-friendly version

Discussion paper

UM-CAM); and a "CTM" is a different model framework that uses (elements of) offline meteorology to drive transport, temperatures for chemistry etc.

Thanks for alerting us to this more precise nomenclature. Indeed for some models, composition is prescribed throughout parts of the domain (e.g. LMDZ-REPROBUS). We still refer to this model as a "CCM". There is no "UM-CAM"-type model here, i.e. all "CCMs" interactively feed ozone and other GHGs into radiation. We believe our usage of the terms "CTM" and "CCM" is consistent with the TOAR definition.

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".*

   We now use the term "CCMI-1" in the sense suggested by the reviewer where we feel that a distinction needs to be made between CCMI-1 and CCMI-2. We have also added a sentence on CCMI-2 in the introduction.

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.)*

   Upon re-reading the paper, we have tried to address this concern.

Specific comments and technical corrections

P1, L5: not just air quality, but also tropospheric composition (also in the Introduction)

We have added “tropospheric composition” which is more general than air quality.

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

We have added references to Morgenstern et al. (2010) (the CCMVal-2 model description paper) and Lamarque et al. (2013) – the ACCMIP reference.

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

We have cut out this phrase – it seems unnecessary.

P2, L22: Delete “activities”

Done.

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.

We accept that CCMVal-2 is significantly different from CCMI, and we have trimmed the references to CCMVal-2 to where it makes sense (keeping in mind not all models participated in that). However, there is only one CCM (CHASER) and two CTMs which had not participated in CCMVal-2. The remaining 17 models had all participated, and had been well characterized in their CCMVal2 versions. For this reason the whole paper was designed as a “sequel” to Morgenstern et al. (2010). We feel it does make sense to retain most references to CCMVal-2, particularly as in some respects the models have not significantly changed since CCMVal-2. A comparison to CCMVal-2 is also useful to characterize community-wide progress in the intervening 6 years.

P2, L32: The paper → This paper (or "The present paper"?)

This refers to Morgenstern et al. (2010) so not the "present" paper.

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

This is now spelled out explicitly.

P3, L20: "impact of ozone" – Clearer/more general example perhaps?

This line got removed, in response to reviewer 2's comment.

P4, L5: improved → increased

Done.

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

In most cases the improvements refer to the CCMVal-2 versions. In the case of MOCAGE, relative to the ACCMIP version there is no improvement. This is now made explicit.

P4, L10: where do these exception models stop? (Also "cf." → "see"? Since cf. means compare with)

We now explicitly state the model top pressures for these two models.

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

The details of nudging vary, as is spelled out in the text. Indeed two of the models are CTMs (which one could think of as an extreme case of nudging). These models only completed the specified-dynamics simulations, which this section on QBO forcing does not apply to. None of the remaining models can be considered CTMs as the QBO nudging is only used in a portion of the model domain.

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!)

We have removed this occurrence of "may". There are two other occurrences of "may" in the text which we consider are both justified.

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?

The Unified Model family are all built around the Unified Model, but this model can be configured in various ways and is subject to ongoing development. So none of the members of this family are quite the same as others, but they do have considerable similarities. In the case of ACCESS CCM and NIWA-UKCA, the only difference is that ACCESS CCM is never coupled to an ocean/sea ice model, so these models for REF-C1 and SEN-C1 type simulations can be considered identical. They are therefore usually listed together. Then there is the CESM1 family (identified by name and usually listed together). Finally the ECHAM family – EMAC and SOCOL share a similar background climate model but use different chemistry packages. We now have a subsection specifically to document these similarities. Where the model formulations are identical, they are usually listed together in the tables, to aid the reader in understanding these similarities.

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).

We have strived to give all models equal treatment. The sentences are generally based on information provided by the model PIs. This paragraph got reformulated – only two models have increased the number of represented ODSs before participating in SPARC (2013). The other 4 already had a comprehensive list of species for CCMVal-

2. (We now use “MetUM” more consistently than before).

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

We have expanded the two occurrences.

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

The sentence has been rephrased. LMDz-REPROBUS does not handle tropospheric chemistry (below 400 hPa) at all, so all chemical fields are imposed there.

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.

There is no consistency regarding how this is handled, and there also was no recommendation regarding the details of this. Partly this is because the large diversity of tropospheric chemistry mechanisms in use for CCMI-1 makes such a standardization difficult to achieve. Eyring et al. (2013a) give a detailed account of how the emissions were defined.

*P6, L22: Are CH4 *concentrations* prescribed?*

No, mixing ratios are, in all but one case (CHASER). This has been corrected.

P7, L2: “models vary” – how?

This sentence has been rephrased. More detail has been added regarding the SO2 → SO3 conversion process.

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

We have added two sentences on “numerical diffusion”.

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

We have added a reference here.

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

The CTMs do not explicitly consider this process. CHASER did not participate in either of the two identified precursor activities, so therefore progress cannot be quantified in the same manner as for the other models.

P10, L3: Reference for NEMO?

Added, also earlier for the first reference to NEMO.

P10, L8: Do the models apply the solar forcing to photolysis? (I know this is in Table S29, but good to flag here) No, not generally, except where this is explicitly stated. We now explicitly name the models that treat photolysis and short-wave radiation consistently. This does not imply the use the same scheme for both; it may only imply that they use the same solar irradiance in both.

P11, L15: similar → the same as

Changed.

P11, L17: More specifics for the solarTrend simulation?

We have added two more sentences and a reference to the SOLARIS website.

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

We have rephrased the sentence.

P13, L8 and L11: "essentially" is imprecise

We have removed both occurrences.

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).

We have now included a new multi-panel plot showing annual-mean emissions of NOx and SO2 in different years spanning the REF-C2 period. As for the influence of chemistry on the actual emission flux, we agree that this is a major determinant of model behaviour. This is discussed to some extent in the subsection on tropospheric chemistry.

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?

Yes, that is correct. We have rephrased this sentence to make this explicit.

P13, L26: Reference for MEGAN?

Added (also in response to reviewer 2).

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

Only LMDz-REPROBUS used AMIP. This is now spelt out in the subsection on surface forcings. Here we additionally now include a reference to this subsection.

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.

It was actually intentional not to separate insignificant from small trends (so that all trends that appear in red or blue hues are significant). We now stipple the parts of the globe where SST trends are insignificant in the HadISST dataset, and have rephrased the caption.

P16, L8 and L9: "can be" and "should be", but what was actually done? We have rephrased these sentences.

P17, L9 and L10: has → have (verb agreement with "models" not "a majority"/"a subset") Done.

P17, L14: "Some models" – which?

We now spell out which models are meant here.

P18, L7: "experience" by who? Ref? Folklore?

We now provide a recent exemplary reference for this.

P18, L16: are advised → should

Done.

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

We have rephrased some minor aspects of these model descriptions, but in most cases PIs felt that their models were adequately described.

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

Good idea, we now give a link to a yet-to-be-populated website that lists such errors.

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!

We have revised the tables and captions in light of this comment. We have provided explanations for numerous acronyms and names of some species. Some remain or are difficult to expand to a sensible meaning. For example, some species names of lumped

chemical compounds only make sense in the context of the corresponding chemical mechanism. In these cases, the readers are referred to the specialist literature. We have standardized the citation format.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-199, 2016.

Printer-friendly version

Discussion paper