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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Rapid and reliable assessment of methane impacts on climate" *by* Ilissa B. Ocko et al.

Anonymous Referee #1

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This is a well-motivated paper examining the response of simple and advanced climate models to methane forcing and reflects work carried out in a major and innovative study. In its present form, however, it has a number of problems that might be overcome by a substantial revision. At heart, I ask whether an alternative title (and hence perspective) for this paper could be something along the lines of "The difficulty of using small ensembles of simulations of an ESM with large interannual variability to validate simple climate models for cases of small forcings". Such a perspective would still lead to a valuable publication.

General comments

1. Too much of the paper is written in language that implies that the CM (or AM3) is validating MAGICC. While this is a natural assumption for large forcing (or large

ensemble) cases, I am not sure this is necessarily the case here. The issue is that the overall transient response to historical methane forcing is around 0.5 deg C and the unforced variability in CM3 (Figure 7 and line 10-15) shows persistent and rapid unforced changes of this magnitude are possible. From a structural point of view, I feel that Figure 7 should be one of the first figures in the paper, rather than the last, where it appears as almost an afterthought. It should be accompanied by clear statement that there is a major difficulty in using ESMs to validate simple models in cases where the forcing is much less than, say, 1 W/sq.m, especially in ESMs where interannual variability is high. While the text in the intro and conclusions says that one issue is that CCMs are generally inaccessible, it could also be argued on the basis of this paper that they are potentially inappropriate tools when discussing small forcing scenarios. The issue is raised briefly at line 2-20 but the text never really returns to it when CM3 results are discussed.

2. I have a related concern about the (unforced) radiation budget variability in AM3, and how this looks compared to available observations. I note "unforced variability" in AM3 is briefly mentioned at 7-28 and 8-11, but it is unclear whether this is a major or minor issue. This is important to know for understanding the reliability of forcings derived from AM3 simulations (in Figure 2 and 3) - there are several surprising features in those forcings which may have their origin in this variability. These include the fact (line 8-9) of the AM3 methane forcing being close to that of CO2 before 2000, and exceeded it in the 1970s (which, if substantiated, would constitute a major result) and the fact that (line 8-12) AM3 methane forcing decreases when methane is flat (how can that be?) and others referred to below. I am a bit suspicious as to how and why the AM3 methane and CO2 forcings diverge after 1995 after being so close to each other before this and would like to see a clearer explanation on page 8. See also my main comment on 7-25.

3. The lack of clarity about the large unforced variability in CM3 until near the end means that some of the earlier discussion attempts a too deterministic analysis of

CM3-MAGICC differences.

Specific comments

4-26: The paper uses two quite different meanings of ensemble but does so in an interchangeable way. At 4-19, the MAGICC ensemble could be considered a perturbed-physics one. At 6-24 the CM3 ensembles are initial condition ensembles. This major distinction gets lost later, for example, in Fig 5 caption, but I have other concerns. Is a 3-member ensemble CM3 considered enough given the size of the applied forcing (and the size of this model's unforced variability)? While I appreciate the point made at 6-19 to 6-22, would it be useful to highlight the MAGICC configuration that matches CM3 as closely as possible, particularly for climate sensitivity? As far as I can tell (and sorry if I miss it) the reader is never told what the climate sensitivity of CM3 is, and therefore it is hard to judge whether it might be an outlier in the MAGICC ensemble. Finally, I presume the AM3 results were not from an ensemble?

7-25 and Figure 2: I understand that stratospheric aerosol RF is not a topic for this paper, but the difference between MAGICC and AM3 is very striking and it seems that the AM3 forcing from individual volcanoes is persisting for unrealistic lengths of times (most notably Agung) and leaves a signature which looks unrealistic in the surface temperatures (in Figure 4). Has this been discussed in earlier papers? Is it some lingering indirect effect of the eruptions? Is it due to the smoothing process? I feel it should be mentioned briefly here, not least because it may impact on the ability to extract the methane signal from CM3.

8-14 and Figure 3: There are quite a few concerning features in this figure, which leads on from my other comments. Given the smoothly varying methane concentration (Figure 1) how can the direct AM3 forcing fluctuate so rapidly? Is this just the unforced variability of the radiation budget in AM3? Also it seems that there is a strong degree of anticorrelation between the AM3 direct and indirect forcings most apparent in some periods (1920-1940 and 1980 onwards) which lead to the total forcing (in Figure 2)

being much smoother – so again, is this anticorrelation just an artefact of the analysis method, or robust? Finally, the fact that the methane indirect forcing falls to near zero in the 2000's would, if correct, constitute a major result. The text (8-20) says that it is "mostly tropospheric ozone" but this needs substantiating by showing some measure of the tropospheric ozone variations in AM3, and some assurance that the feature is a clear signal above the noise.

8-6 to 8-13: I have quite a few comments on this paragraph. (i) I think comparison of MAGICC with IPCC needs to be more carefully done. I am almost certain they both use the same radiative forcing expressions and hence the only reasons for differences would come from different scenarios of trace gas change, and possibly different handling of N2O overlap. (ii) wording like "MAGICC reasonably reproduces . . . AM3" (8-6) and "not captured by MAGICC" (8-11) implies to me that there is a belief that the AM3 radiation code is superior to the radiative transfer codes used to generate the RF expressions in MAGICC and IPCC. Of course, this may be the case, but if this cannot be established with certainty, the wording should be more cautious. And as noted in my general comment (2) without knowing the size of the unforced variability in AM3 it is hard to place any differences in perspective.

9-13: It is hard to know what conclusions to draw from this paragraph – is it that the CM3 signals are unreliable because of the difficulty of extracting a small signal from a small ensemble given the size of the unforced variability (especially in the 1900-1915 period when methane forcing was less than 0.2 W m-2)?

10-10 and especially 10-13: Again it is hard to know what conclusion to draw here, and there is nothing to lead the reader. I believe that this paragraph should come much earlier in the paper and be flagged as a major caveat when trying to extract small signals from ESMs, and in particular CM3 given its such large variability. As it is, this aspect is presented almost as an afterthought.

11-12: "useful threshold" – I agree with this, but I don't believe the paper applies this

threshold. If it did, I think one conclusion could be that some of the early 20th century signals attributed to methane in AM3/CM3 are not robust and should not be interpreted as such. This may be what 11-13 to 11-15 is trying to say. If so, it needs to be stated more clearly, earlier in the paper and reflected in the abstract.

Additional comments

Throughout: I suggest that the 4 in CH4 is subscripted throughout, for consistency with CO2.

Throughout: There is a lack of consistency in the labelling of simulations – compare the abbreviation column of Table 1 with Equations (1) to (4).

1-7: The abstract does not highlight any of the scientific results emerging from CM3 – the fact that methane-induced warming is competitive with CO2 for long periods and the high variability of its indirect effect. My other comments indicate some scepticism about these results, but it seems odd not to mention them at all in the abstract if the authors stand by them.

2-3: I suggest this sentence is re-worded to avoid the appearance of policy advocacy. The standard and effective IPCC wording is "if we want to avoid warming then we need to reduce emissions".

2-10: I looked at Etminan's paper and could see nothing on temperature change.

2-16: For sure, the GWP concept has a lot of problems, but the strong variation of its value with time horizon (e.g. compare GWP(20) with GWP(100)) does capture "important temporal distinctions".

4-1 and 4-7: Is it correct that MAGICC is driven by concentrations for 1765-2005 and emissions for 2006-2014? The fact that the methane lifetime is mentioned as being updated at 6-15 adds to confusion as to whether the model is concentration or emission driven. The (possible) emission/concentration confusion is also present in the paragraph after 6-15. But then 7-17 and Figure 1 seems to clearly imply that both models

are concentration driven. So I ended up a bit confused.

4-15: There are significant differences in the CO2 expression used in the two references given here – I doubt that MAGICC uses the IPCC FAR expression.

4-25: Units of climate sensitivity missing.

5-19: A minor query, but I wasn't sure what the "indirect feedbacks of CH4 on CO2" were – it could include the fact that CH4 is oxidised to CO2, or that CH4-induced climate change impacts the carbon cycle or CH4-induced changes in O3 impact the carbon cycle by their effect on vegetation (or all three).

6-15: Coupled with my comment at 4-15, I am confused. Does MAGICC use the AR5 forcing expressions for methane or does it use the radiative efficiencies (which are W m-2 ppbv-1 normally defined for small perturbations from present day)? I don't think MAGICC can use both and this needs to be clarified.

7-6: I found this sentence a bit cryptic. Presumably N2O and its overlap are considered in the AM3 radiation code? I was not sure why the dependence should lead to a "likely overestimate" of methane forcing.

8-28: I don't agree with NASA and NOAA temperature series being labelled as "independent observational datasets" when there is, of course, a huge commonality in the underlying data, and I believe in some of the applied corrections to that data. A better wording could be used.

11-1: "accurately" – the word "adequately" is used at 9-1 and I believe it a much better description of Figure 4.

11-21: One issue with MAGICC is that (as I understand it) only the executable, rather than the source code, is available, which is at odds with the title at 11-19. It may be worth making this clear to readers.

18-4: Delete "technically"?

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