

***Interactive comment on* “Emulating IPCC AR4  
atmosphere-ocean and carbon cycle models for  
projecting global-mean, hemispheric and  
land/ocean temperatures: MAGICC 6.0” by  
M. Meinshausen et al.**

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**Response to Anonymous Referee 3**

General remark (same for both reviewers): First of all, an important general note: We are fully aware that this response comes very late, beyond the standard turnaround times for ACP. Nevertheless, we believe that this comprehensive model documentation and application is of considerable interest for future applications of MAGICC, which is

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why we would like to ask the editors and reviewers to accept our sincere apologies for this lateness. In the course of the two years, the described model parameterisations have been shown to be robust in the multiple model applications that followed our first draft manuscript. Thus, no changes had to be made to the substantive part of the model description.

There are, however, three major revisions that we have implemented: a) We followed the recommendation of reviewer 3, and split up the paper into two, a “model description and calibration” paper (Part I) and an “application” paper (Part II). b) We have updated the application examples. We noted previously the potential future use of MAGICC for forthcoming intercomparison exercises. MAGICC has indeed been used to assist in the design of the RCP pathways that will be the basis for model simulations under CMIP5. We have therefore included the use of MAGICC in the RCP process as one additional application example in Part II. c) We have now included a section (Section 6.2 in Part I) on possible alternative explanations for increases in the effective climate sensitivity over time – following the suggestion of reviewer 2.

We believe that we have been able to address all reviewers’ comments, as detailed below. We are truly thankful for the time spent by both reviewers, and for their insightful comments that have helped to improve the paper(s).

### Response to Anonymous Referee 3

We thank Referee 3 for the detailed and constructive comments, which helped us to substantially improve the paper. In particular the suggestion to split the paper into two was a very helpful one, which we have followed. We detail our responses to the reviewer’s comments (provided in *italics*) below.

*REVIEW COMMENT:1. A lot of work has been done and I can understand the temptation to cover all of it in great length and detail. However, in its current version the paper is far too long. I would suggest either cutting the paper down substantially or, alternatively, split it in two, with one part dealing with the mere technicalities of the improved model (e.g., in the form of a technical note), and the other part dealing with applications of the model. In its present form the main point of the paper, which is IMHO the emulation of different scenarios, gets somehow lost.*

REPLY: We followed the reviewer's suggestion and now split this lengthy paper into a first part covering the model documentation and calibration and a second part, which highlights its applications in the context of IPCC AR4 SRES scenarios as well as the forthcoming RCP pathways. We very much appreciate the reviewer's comment. Thus, Part I describes the model's structure (with most of the detail given in an Appendix) and describes the procedures for calibration of MAGICC to the AR4 AOGCMs and C4MIP carbon cycle models. For example, model description and the discussion of the calibration results for climate sensitivities and other parameters is given in Part I. Part II covers the application aspects, i.e., how forcings are harmonized and what the effects of those harmonizations are. Part II also presents ensemble results of temperature projections for the IPCC SRES scenarios. Part II now includes one additional application related to the new RCP pathways. We provide temperature projections and inverse emission estimates based on the calibrations described in Part I.

*REVIEW COMMENT: 2. Quite a substantial part of the paper is written in a rather imprecise and vague, at times even rather unscientific language, which unfortunately tends to disguise the quality of the (huge amount of) work done.*

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*To quote just one example (p. 6157): "In summary, imperfect knowledge with regard to the forcings in CMIP AGCMs leads to ambiguities as to whether differences in their climate responses are due to different climate responses or ..."*

*I suggest a careful review of the whole text and general tightening up of the paper. Especially the use of "climate response" and "climate feedback" should be carefully checked. Such a revision will most probably also cut down the length of the paper substantially.*

REPLY: Our intention was to provide the first fully comprehensive documentation for a widely used simple climate model accessible to its diverse range of users. Given that some of the model's users (e.g. in the integrated assessment community), while generally familiar with the concepts, are not experts in the field, we gave quite lengthy discussions in relatively general terms in the original paper. In our revision, as requested, we have tightened and streamlined the text in various places.

### Specific comments:

*REVIEW COMMENT: p. 6156, l. 9: "structurally different"; different AOGCMs use different parametrisations; "structure" is IMHO not the right word to use in this context*

REPLY: We consider 'structurally different' to be the correct term, in the sense that structural uncertainties differ from parametric uncertainties. However, given that the sentence is not essential here and in our effort to slim the whole paper, we deleted the sentence.

*REVIEW COMMENT: p. 6156, l. 16 and following: "parameterisation" is a word that should feature in this paragraph*

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REPLY: We changed the headline from ‘Spanning structural uncertainties’ to ‘Parameterisation of structural uncertainties’ and revised the text, specifically mentioning parameterisation in this context.

*REVIEW COMMENT: p. 6156, l. 17 "what key processes should be included and how they could be modelled." rather prosaic*

REPLY: Wording of this section is clarified now as: “One advantage of simple models is that they can be used to examine the effects of structural uncertainties in more complex models. Structural uncertainties in AOGCMs arise from the way certain processes or components (such as clouds) are ‘parameterized’ or expressed in relatively simple terms – these parameterizations are structural components of the model. Within these parameterizations there may be a number of parameters, and parametric uncertainties arise from the uncertain values of these parameters.”

*REVIEW COMMENT: p. 6156, l. 21 "In practice, a strict separation between these two types of uncertainties is not possible." Why not?*

REPLY: We refer to the following ambiguity: if there are two models that have structural differences. Suppose that model A gives a warmer response to given forcing than model B. In general, without carrying out a large set of simulations changing both the two models’ structures and the parameter values within their common parameterizations, it is not possible to tell whether the difference is due to the structural differences, or to parameter differences. This is a rather esoteric (and relatively unimportant) point. In short, it is only to say that a) the same aggregated response can sometimes be obtained in structurally different models, with appropriate parameter settings – an effect that we make use of when emulating AOGCMs with a simple model, b)

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uncertainty analysis studies that test parametric uncertainty in one rigid AOGCM model structure might not be representative of the full uncertainty that one would obtain, if both different model structures and parametric uncertainty would be taken into account. As well, a flexible simple model, which can span the full range of AOGCM response characteristics might hence be helpful for such synthesizing uncertainty assessments. We rephrased the particular section (following from the excerpt given in the reply above.).

“... from the uncertain values of these parameters. Thus, two models can differ in their aggregated response characteristics, either because they use different parameterizations, or the same structure, i.e., parameterization, but different parameter values. In fact, we take advantage of this in the present study by “parameterizing” the structural uncertainty range of more complex models (O’Neill and Melnikov, 2008) by estimating the parametric values within the more flexible MAGICC structure that fit the AOGCM results. This approach is distinct from perturbed physics studies with intermediate complexity models or AOGCMs (Murphy et al. (2004)), which often concentrate on assessing parametric uncertainties within a fixed and comparatively more rigid model structure alone.”

#### References:

O’Neill, B. C. and N. B. Melnikov (2008). "Learning about parameter and structural uncertainty in carbon cycle models." *Climatic Change* 89(1-2): 23-44.  
Murphy, J. M., D. M. H. Sexton, D. N. Barnett, G. S. Jones, M. J. Webb, M. Collins and D. A. Stainforth (2004). "Quantification of modelling uncertainties in a large ensemble of climate change simulations." *Nature* 430(7001): 768-772.

*REVIEW COMMENT: p. 6158, l. 1 "to separate radiative forcing and climate response uncertainties in AOGCMs"; please clarify*

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REPLY: We revised the wording and hope that the following newly introduced sentence clarifies this issue: “In summary, imperfect knowledge about the forcings in CMIP3 AOGCM experiments leads to ambiguities as to how much of the differences in their temperature projections are due to different climate responses (feedbacks, inertia, etc.) or simply an expression of different (sometimes limited or erroneous) radiative forcing implementations.”

*REVIEW COMMENT: p. 6158, l. 9 "coupled responses" do you mean the response of coupled models?*

REPLY: Our answer is “Yes” in terms of coupled carbon-cycle climate models, and “No” in terms of coupled atmosphere-ocean models. We changed “coupled” to “joint” in order to hopefully avoid this misunderstanding.

*REVIEW COMMENT: p. 6164, l. 6 "for contributions from black carbon" contributions to what?*

REPLY: Thanks for spotting this. A “due” was too much in the end of the sentence, so that the full sentence reads now: “It is now possible to account directly for contributions from black carbon, organic carbon and nitrate aerosols to indirect (i.e., cloud albedo) effects”

*REVIEW COMMENT: p. 6174, l. 25 "the procedure minimises the weighted least squares" weighted with what?*

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REPLY: This rather complex detail is described in Appendix B (Lines 2210 to 2245 of Part I).

*REVIEW COMMENT: p. 6176, l. 17 This is an interesting comparison!*

*REVIEW COMMENT: p. 6178, l. 23 "RMSE of 0.46" Is this value unchanged from the value you got for calibration method I?*

REPLY: We are not sure we understand this comment. Under calibration method I, comparing emulations with the original AOGCM results, the RMSE is 0.21 °C. On the other hand, if one only compares one original AOGCM result with that for a random other AOGCM, the expected value for the RMSE is 0.46°C. Thus, the RMSE of 0.46 °C is completely independent of any simple model results or calibration method and shown here simply for comparison. We however might have missed the point of the reviewer?

*REVIEW COMMENT: p. 6179, l. 28 "had the same physics (i.e., the same climate sensitivity, ocean mixing, etc.)" One of those imprecisions. Unlike ocean mixing, climate sensitivity is not a process itself, but rather a reaction to processes. It is a feature of the model resulting from the model's reaction to forcings, interaction of different physical processes, feedbacks etc.*

REPLY: We agree with the reviewer that the previous wording could have lead to the misperception that climate sensitivity were an inherent model process. While revising the text, we deleted the wording in question.

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*REVIEW COMMENT: p. 6182, l. 10f "After 1970, the forcing adjustments approximately offset each other and start turning negative ..." Imprecise. After approx. 1970 they are negative.*

REPLY: Thanks. This sentence is now moved to PART II and has been changed to "After approximately 1970, the sum of forcing adjustments is negative, and is dominated by volcanic forcings spikes."

*REVIEW COMMENT: p. 6191, l. 1 "key quantities of AOGCMs" Delta T is the quantity being emulated, but it certainly is not the only key quantity simulated by an AOGCM*

REPLY: Agreed. This particular sentence was cut during the revision. Furthermore, we now included that notion in the abstract of Part II, by referring to "globally aggregate characteristics of these more complex models" rather than key quantities of AOGCMs. As MAGICC does not only emulate Delta T, but as well oceanic heat uptake and land-ocean warming, we prefer mentioning 'globally aggregate characteristics' rather than Delta T alone.

*REVIEW COMMENT: p. 6191, l. 13 "structural dependence" please clarify*

REPLY: We inserted the following example to explain the structural dependency in this context: "For example, many AOGCMs share (to a varying degree) model components, such as the MOM ocean code (Bryan et al., 1969). To illustrate the problem, consider the hypothetical case where two AOGCMs are absolutely identical, but are submitted to an intercomparison exercise under different names by different modeling groups. Should that particular model then carry twice the weight in the ensemble average? Obviously not (Tebaldi et al. 2009, Knutti et al., 2010, Santer et al., 2009)".

## References:

- Bryan et al., 1969, Vol 4, No 3, October 1969, pages 347–376, J. of Comp. Physics
- Knutti, et al. (2010). "Challenges in Combining Projections from Multiple Climate Models." Journal of Climate 23(10): 2739-2758
- Santer, B. D., et al. (2009). "Incorporating model quality information in climate change detection and attribution studies." PNAS 106(35): 14778-14783.
- Tebaldi et al. 2009 "Joint projections of temperature ...." Journal of the Royal Statistical Society Series a-Statistics in Society 172: 83-106

*REVIEW COMMENT: p. 6191, l. 16 "interdependence" what does that mean?*

REPLY: Hopefully, the above example clarifies the issue of dependence / inter-dependence.

*REVIEW COMMENT: p. 6194, l. 6 "A consequence of this is that calibrations will over- or under-estimate the climate feedbacks of an AOGCM." Imprecise.*

REPLY: We clarified the text to read now: "A consequence of this is that calibrations, even if perfect, may over- or under-estimate the climate response of an AOGCM under a given forcing depending on whether the estimated forcing is more or less than the actual AOGCM forcing."

*REVIEW COMMENT: p. 6197, l. 19ff "... whether differences in climate response are truly due to different climate responses or ..." Imprecise; please clarify.*

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REPLY: Thanks, the second 'climate responses' was meant to read 'climate feedbacks'. In this way we broadly characterize the aggregate climate response as a function primarily of climate feedbacks and forcing – consistent throughout the paper.

*REVIEW COMMENT: p. 6216, l. 6ff Confusing; please tighten up and clarify.*

REPLY: Paragraphs are slimmed down and hopefully clarified.

*REVIEW COMMENT: p. 6216, l. 13 "AOGCMs often show a land-ocean-warming ratio (RLO) that decreases over time but stays above unity ..." Sutton et al. ("Land/sea warming ratio in response to climate change ...", GRL, 34, 2007) note that the RLO is "fairly constant in time"; see also Huntingford and Cox ("An analogue model to derive additional climate change scenarios ...", Clim Dyn, 16, 2000) and Joshi et al. ("Mechanisms for the land/sea warming contrast ...", Clim Dyn, 30, 2008) You might want to amend/reconsider ... Please comment.*

REPLY: Thanks for pointing this out. However, as shown in Figure 11, 12 and 13 of Part I, our own analysis of the CMIP3 model archive in terms of the AOGCMs' projected land-ocean warming ratio does indicate for some models slight decreases in the land-ocean warming ratio at times in the idealized runs 1pct2x and 2pct4x when forcing is stabilized– and in some cases even during the periods of increasing forcing (e.g. IPSL, GISS-ER). In no case, the land-ocean warming ratio turns back to unity, though. Furthermore, there are some models without any observed decrease (e.g. HadGEM1). We believe this analysis is consistent with the analysis of Sutton et al., who show slightly decreasing land-ocean warming ratios in the right section of their Figure 2 and state in their discussion:

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“After the forcing is stabilized in the 1pc-stab integrations (year 70) most models show a small (10%) decrease in warming ratio. This decrease is likely to be a consequence of the ocean approaching equilibrium. Importantly, however, the warming ratio remains significantly above unity, suggesting that the large heat capacity of the ocean is not the primary reason for the enhanced warming over land.”

Whether these decreases in land-ocean warming ratios from the idealized runs should be observed in the multi-forcing runs is an open question. For example, the analyses of the SRES A1B scenario by Joshi et al. (shown in their Figure 2), do not suggest a general tendency for decreasing land-ocean warming ratios over time. This result, however, might be complicated by the fact that the SRES A1B scenario is subject to strongly decreasing sulphate emissions causing a relative increase of forcing over land areas during the 21st century.

We clarify the text to read now: “In the idealized forcing runs, AOGCMs often show a transient land-ocean warming ratio that slightly decreases over time, but stays above unity, combined with an increasing effective climate sensitivity in some models. . .”

*REVIEW COMMENT: p. 6126f The purpose of the amended heat exchange formulation is to make the exchange more sensitive to changes in ocean temperature as opposed to land temperature changes. Now for certain combinations of parameters negative exchanges, i.e., from ocean to land, are possible, even if the change in land temperature is already larger than the change in ocean temperature. Is that correct? Please comment.*

REPLY: Yes, this is correct. In other words, this amended heat exchange formulation allows having a zero change in the ocean-to-land heat exchange, even under an

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above-unity land-ocean warming ratio. Without such an ‘asymmetric’ weight of ocean and land temperatures in the simplified ocean-land heat exchange formulation, the observational evidence could not be modeled: In particular, Fasullo Trenberth (2008) ("The annual cycle of the energy budget. Part I: Global mean and land-ocean exchanges." *Journal of Climate* 21(10): 2297-2312.) show in their Figure 8 estimates of a roughly constant ocean-to-land energy flux over the period 1978 to 2005. Over approximately the same period, the HadCRUT3v dataset (Brohan et al. (2006) ("Uncertainty estimates in regional and global observed temperature changes: A new data set from 1850." *Journal of Geophysical Research-Atmospheres* 111: D12106.) shows a positive land-ocean warming ratio (see their Figure 12, which shows land-ocean warming differences). With the formulation in previous MAGICC versions of a ‘symmetric’ influence of the warming differentials over land and ocean, the emulation would have estimated a net increase in the land-to-ocean heat exchange. Thus, we believe that this new heat exchange formulation better reflects the observed behavior of the climate system. We acknowledge, however, that there are still large uncertainties in regard to future land-ocean heat exchanges.

Regarding the physical processes involved, we hesitate to provide an hypothesis. MAGICC is merely providing a very simplified picture of the combined effect of both sensible and latent heat fluxes in the atmosphere, and possible heat fluxes through runoff, that are spatially variable and non-linear.

Simply speaking, there could be roughly constant latent heat fluxes despite the positive land-ocean warming ratio, although the indicators seems inconclusive how heat fluxes might change under expected warming conditions. One indication for the net latent heat flux can be land-to-ocean runoff, i.e. continental freshwater discharge, as the conservation of mass would require a long-term balance between these two water fluxes. Although some studies indicate observational evidence for increased

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land-to-ocean runoff over past decades (Labat, D., et al. (2004). "Evidence for global runoff increase related to climate warming." *Advances in Water Resources* 27(6): 631-642.), more comprehensive analysis suggests no or even a slightly negative trend (Dai, A., T. T. Qian, K. E. Trenberth and J. D. Milliman (2009). "Changes in Continental Freshwater Discharge from 1948 to 2004." *Journal of Climate* 22(10): 2773-2792). At the same time, Fasullo and Trenberth (2008) ("The annual cycle of the energy budget. Part I: Global mean and land-ocean exchanges." *Journal of Climate* 21(10): 2297-2312.) and Trenberth and Fasullo (2009). ("Changes in the flow of energy through the Earth's climate system." *Meteorologische Zeitschrift* 18(4): 369-377) indicate that the land-to-ocean energy flux is roughly constant since 1980 (although the decadal variations are judged to be largely spurious).

Joshi et al. provide a conceptual model for land-ocean warming ratios, highlighting the possibility that surface temperatures could change due to moisture limitation in the boundary layers over land, which directly leads to lower lapse rates over the land than over the ocean and consequently higher land surface warming – while the middle to upper tropospheric temperature gradients over land and ocean areas would in fact not change. Thus, assuming the main heat exchange occurring above the boundary layer, one could again argue that there is no reason to assume a decreasing ocean-to-land heat exchange even if surface temperature gradients work in the opposite direction. These are complex issues that a simple model like MAGICC cannot hope to resolve. We simply note some evidence that the earlier parameterization of assuming that changes in land-ocean heat exchange are proportional to changes in the differential surface temperature gradient might be too simple.

*REVIEW COMMENT: Technical corrections:*

*p. 6159, l. 17 "on average"*

*p. 6159, l. 25 "with regard to"*

- p. 6162, l. 26 "with respect to"
- p. 6163, l. 17 "(Wigley, 1991b)"
- p. 6164, l. 19 "allows for"
- p. 6166, l. 10ff "optimizing a smaller (I) or larger (II,III) set of ... using idealized scenarios only (I,II) or ... as well (III)."
- p. 6168, l. 23 "parameters"
- p. 6170, l. 6 "The forcings"
- p. 6177, l. 14 "For others"
- p. 6178, l. 1 "0.206" Why are you giving additional significant numbers? It was 0.21 before.
- p. 6178, l. 25 "of the original AOGCM's" original or actual?
- p. 6181, l. 6 "on average"
- p. 6182, l. 26 "differences"
- p. 6183, l. 2 "manifest themselves"
- p. 6187, l. 19 "to match as well" I think you mean "to find a constant climate sensitivity that allows to match the rather high forcing part ... equally well as ... "
- p. 6188, l. 1 "represents"
- p. 6188, l. 13 "Table 4 taken from Meehl et al. (2007)." The Table caption says Randall et al., 2007.
- p. 6188, l. 28 "similar to those in"
- p. 6191, l. 28 "under-"
- p. 6192, l. 5 "temperature evolution above 1980-1999" I suppose you mean the evolution between 1980 and 1999?
- p. 6194, l. 3f why the brackets?
- p. 6195, l. 13 "calibrations are done"
- p. 6195, l. 19 "data was not available"
- p. 6197, l. 11 "sensitivity estimates"

- p. 6198, l. 6 "Scientists"*
- p. 6215, l. 2 "energy balance equation"*
- p. 6215, l. 7 alpha is not explained*
- p. 6217, l. 7 "Some AOGCM runs"*
- p. 6217, l. 19 "can lead to changes over diagnosed" Over?*
- p. 6217, l. 20 "Hansen et al. (2005) show that"*
- p. 6218, l. 6f "response after a change in forcing ... parameters are adjusted."*
- p. 6218, l. 12 "scales the"*
- p. 6218, l. 16 wrong Greek letter?*
- p. 6219, l. 1 "are both, the geographical and the vertical distribution"*
- p. 6219, l. 13 "normalising"*
- p. 6219, l. 18 "normalised"*
- p. 6220, l. 2 "which in turn depends on"*
- p. 6240, Table 1 I might have missed it, but I think you do not explain what the scenarios are. Esp. important for 20c3m and commit, which are not so obvious.*
- p. 6244, a "These climate sensitivities were"*
- p. 6253 Table caption "M6.0 tuning"*
- p. 6261 The Figure is rather small, but I could not find "c)"*

REPLY: We are most grateful for this set of technical comments. It is rare that reviewers take the time to report these glitches and technical oversights and we would like to express our thanks – especially given the longer-than-usual length of this manuscript. We have addressed each one of the above technical comments.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6153, 2008.