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Interactive comment on "Synthesizing long-term sea level rise projections – the MAGICC sea level model" *by* Alexander Nauels et al.

Anonymous Referee #2

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General Comments:

The ms tackles an important and nontrivial point and makes several new contributions. The authors are to be applauded for the efforts and providing the new insights. Alas, the current ms suffers from several (in my view severe, but pretty easy to fix) shortcomings that need to be mitigated to enable a careful review and to hopefully (and eventually) rise to the level of quality one expects from papers in GMD. These specific points are discussed below. The ms requires in my view a careful and responsive revision and re-review. I would be happy to look at this ms again.

Specific Points:

The ms does, in my view, not follow the data and code policy of GMD. As a reminder, here is the link describing the policy.

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http://www.geoscientific-model-development.net/about/code_and_data_policy.html These rules state: “Preferably, this section should contain the instructions for obtaining the model code and/or data, either from the supplement or from an archive with a digital object identifier (DOI). Suitable repositories can be found at the Registry of Research Data Repositories, e.g. ZENODO for model code. After the paper is accepted, a link to the GMD paper should be added to the metadata of the archive.” What is available, as far as I can tell, is code for the sea-level rise model / module. However, it will be extremely tough to reproduce the results in the paper, due to several issues. For example, the interface to call this module is not well documented, nor are the data used to drive and calibrate the model provided. The web-page the ms links to states: “The Fortran code of the model as well as required additional MAGICC input files can be downloaded as a zip-file at the top right of this page. Please note that the MAGICC sea level model is not a stand-alone tool. It is fully integrated into the simple carbon-cycle climate model MAGICC and cannot be run separately. The MAGICC model is described in Meinshausen et al. (2011). For a compiled MAGICC version including the sea level model, please contact alexander.nauels@climate-enegy-college.org. MAGICC is licensed under a Creative Commons Attribution-NonCommercial-ShareAlike 3.0 Unported License.“ Does this mean that the source code for the model needed to run the sea-level model is not accessible? Why is only a compiled version distributed? Given this situation, I would assign a very low score for reproducibility (see the discussion by Haas in the recent Risk Analysis volume (Risk Analysis, Vol. 36, No. 10, 2016, DOI: 10.1111/risa.12730). Many other studies have abided by the nice standards outlined in GMD and a publication in GMD should clearly abide by these standards. This is a decision for the editor. I cannot carefully review the ms without the ability to see and run all the code and cannot support publication as a peer reviewed study in GMD with the current state of data availability.

The ms emulates model runs that are mostly samples of best guesses on choices such as model parameters and structures. This sample is likely missing important uncertainties. Communicating the range of the projections and the discussing “probabilistic uncertainty analyses" (abstract) can lead to unfortunate misunderstandings by the users. This needs to be discussed and the communication made more clear to help the users of this tool and the results to better understand the caveats. There is a nice start towards this goal on the bottom of page 9, but this needs to be made much more clear to the reader (e.g., in the abstract). This potential misunderstanding is the even strengthened in the design of the projections, that uses a "probabilistic MAGICC design" (p. 14). For example, Figure A1 discusses a 66% range. One question the ms should address is how the ranges they present compare to expert assessments (that are cited in the ms).

The ms makes several broad and somewhat ambiguous claims that need to be updated. For example, the ms states: "This MAGICC sea level model has been designed to emulate the behavior of the latest available process-based sea level projections" (p2). This does seem to be in contrast with the findings of DeConto and Pollard (2016), which the ms cites but in my reading not considers. This also needs to flow into the discussion of ice sheet dynamics on page 13 as well as the summary in the discussion (p. 15).

How are "semi-empirical models" (p2) specifically defined? Has there been no use before 2007 (as the ms seems to imply)?

Emulators and semi-empirical models are not necessarily alternatives, as the ms seems to imply (p.2). Overall, the ms does not do justice to the very large body of literature on the design, use, and potential usefulness of emulators in the field of climate change research. This comes back to haunt the ms in the discussion (e.g., page 16). Please add to the introduction a more careful review on this issue and revise the statements on this issue in the discussion. How is "robust" (p.2) defined?

"In this study, we provide a first series of updates for MAGICC version 7 which will be consistent with the ensemble output of CMIP5 " (p. 3). Is this a statement of fact or about a possible future? "The tuned model captures key features of the individual

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ocean heat uptake and vertical redistribution behavior of every CMIP5 model ” (p. 4). Which ones?

Please add a table of all model parameters, definitions, units, and values.

Please review the formatting of all equations (e.g., equation 3) and symbol uses in the text (e.g., p6 l13).

Why select only 9 parameters for the calibration? How are the chosen? What would happen if you use all parameters? (p. 11)

Why do you choose the 5000 random parameters (I assume as a starting point)? (p. 11). Is this because there are concerns about / evidence for multiple maxima? You should (i) cite and discuss the ground-breaking paper: Hargreaves, J. C., and J. D. Annan (2002), Assimilation of paleo-data in a simple Earth system model, Climate Dynamics, 19(5-6), 371-381 and (ii) provide evidence for the assessment that the chosen method has a decent chance to be close to the global maximum. How are the weights for the RSS chosen (p.11, I also assume Table 1)? Is this choice consistent with the properties of the residuals?

The claim that the presented results are “superior” (p. 16) is unclear and not backed up by evidence. Please define specific metrics (maybe hindcast cross validation error, etc.) and provide the evidence for this.

Please review the format and missing information in the citations.

Please show the residuals for the calibration (Figure 2). Is there a discrepancy in the lower layers? If so, how is this handled in the calibration?

The fonts and line sizes in Figure 3 are too small to read. Please provide readable font sizes and line / symbol separations (as SOM if need be). Why do you choose only the 90% range for Antarctic solid ice discharge? What explains the change in range in several of the panels (e.g., a and c)? Please show the hindcasts and the residuals in for the results shown in Figure 4.

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