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# Comment on gmd-2021-252

Ben Sanderson (Referee)

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Referee comment on "From emission scenarios to spatially resolved projections with a chain of computationally efficient emulators: coupling of MAGICC (v7.5.1) and MESMER (v0.8.3)" by Lea Beusch et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-252-RC2, 2021

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Beusch et al discuss an emulation framework for the prediction of climate change impacts as a function of emissions scenarios. The framework is structured around the coupling of the MAGICC global simple climate model, and the MESMER regional pattern scaling model.

This paper presents the overall framework, including the methodology for the calibrations of the model components. It discusses the evaluation of regional results (as assessed through the metric of annual mean temperature), and the comparison of scenario projections with MAGICC-MESMER as compared with simulations in the CMIP6 archive.

The paper is clear, and well written and provides a novel and useful tool for the impacts community. I have no major issues barring publication, just some minor revisions are required to discuss some of the structural limitations which are implicit in the approach, and some comments which might help motivate further study.

Minor Issues

1 -In the discussion of limitations and future developments, it should be noted that the model structure allows for no dependency of climate variability on warming level - though there are probably elements of internal variability which are themselves dependent on warming or forcing level (Zheng 2018, Pendergrass 2017, Dorr 2021). These limitations, and potential for future developments, should be discussed a little more.

2- Furthermore, the internal variability is represented as a pattern related to global mean temperature deviations from a forced trajectory, plus a random term with imposed regional correlations through kriging. It is unclear from the present study whether this approach adequately reproduces (in a stationary climate) the noise-covariance structure of the original ESM which is being emulated, and the tests employed here - which focus on point-level errors - do not assess the skill of the emulator in producing realistic modes of natural variability. Though it's way beyond the scope here, and not necessary for this model overview, a future review could consider the relative performance of noise representation in the current scheme and other approaches (e.g. Perkins 2020, Alexeeff

2018, Holden 2010)

3 - the use of a lowess time filter to distinguish forced and variable components may exclude low frequency elements of natural variability which would ultimately be excluded from the model. The authors could test this in cases where large initial condition ensembles are available by combining ensemble members to produce an improved estimate of the underlying forced signal, smoothing (or not, if the ensemble is very large) and using the residual to estimate the noise component of the timeseries.

4 - The introduction of the additional predictors (a quadratic dependency of regional temperatures and a term dependent on global ocean heat uptake) are interesting extensions to the model. My concern is whether there is sufficient data to unambiguously fit these additional degrees of freedom, and whether there is spatial coherence in the relative role of the non-linear term and the ocean heat uptake term over the gridded field. As the authors move towards a larger number of predictors, they might want to consider an EOF prefilter for spatial fields - allowing the model parameters to be fitted in a lower dimensional space which enforces covariance structure. This framework might also aid ultimately in the probabilistic calibration of the MESMER component.

5 - The ocean heat uptake term is a very useful extension to MESMER for representing temperatures in deep mitigation scenarios. In future versions, the authors might find it useful to partition the heat uptake by depth, to represent the pattern effects of heat stored in the oceanic mixed layer as distinct from the deep ocean.

6 - There a brief discussion of the role of non-GHG forcers in the current study, and how this might be incorporated in the future. A brief note on how the simplified current framework might therefore introduce bias would be useful. i.e. to what degree are aerosol/ghg pathway co-dependencies 'baked into' the MESMER configration, and is this evident by looking at scenario outliers like SSP3-RCP7?

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