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Climate of the Past Discussions

Interactive comment on "A comprehensive history of climate and habitat stability of the last 800 000 years" *by* Mario Krapp et al.

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This paper presents a method for estimating climate and ecological changes over the past 800,000 years. This is incredibly ambitious and I welcome the endeavour and aims of the research. However, I am somewhat perturbed by its execution and cannot presently recommend it for publication. I have some queries that arose from my reading of the work. Some may be due to my own misunderstandings, but as a collection I feel they bring the validity of the proposed method into question.

The remit of this research is to build an emulator of climate and ecology as simulated by the HadCM3 model. Any errors in HadCM3 will therefore be unavoidably replicated by the emulator: yet this is not an obstacle preventing useful information being gleaned

from such a tool.

HadCM3 has previously been used to build emulators - and therefore several training sets exist (e.g. Arayo-Melo et al, 2015, and Lord et al., 2017). Given these have been designed to sample parameter space in a near-optimal form, I was surprised that the current work uses only snapshots of conditions that have existed over the past 120,000 years. The authors provide no explanation for this choice, nor discuss its limitations.

A second issue with this training set, as well as others, is the ice-sheet extent. In previous work, ice volume has either been considered an input parameter (Arayo-melo et al., 2015) or emulated through a fixed adjustment (Lord et al., 2017). Ice-dynamics, and the substantial lags that they introduce into the Earth system, are completely neglected in this work. This effectively assumes that ice sheet impacts are wholly and instantaneously determined by CO_2 and orbital configuration. I anticipate this would explain elements of the model-data mismatch shown in Figs 6 7. There is also no recognition that last glacail cycle may not represent all glacial cycles (despite the mid-Brunhes transition).

A further unanswered question arising from the choice of training data revolves around the ecological reconstruction. Only surface temperature, humidity and precipitation are emulated, and then the biomes estimated off the back of this data. HadCM3 has an dynamic vegetation model (Triffid), although I'm unsure whether it was incorporated in this simulations. Certainly offline simulations of the Sheffield Dynamic Global Vegetation Model using the full HadCM3 climate model output have been performed for a subset of the training simulations (Singarayer et al., 2011, doi:10.1038/nature09739). Using these data could provide a useful comparison to the ecological modelling component in section 5 - i.e. can the emulator replicate the simulator response.

Moving beyond the training data choices, I have four questions about the choice of the method applied. Firstly, the linear (or log-linear) regression used at each grid point is different to previous efforts. Arayo-Melo et al. (2015) spent substantial effort developing an approach that inherenetly builds in the spatail covariances inherent in climate using EOFs dimension reduction. The justification you give to avoiding this approach is that linear regression results in "well-behaved". Isn't this just another way of saying that you avoid non-linear transitions, but are these not a widely-accepted feature of the climate system. Also you should bear in mind that the whilst the functions are well-behaved in time, you have removed any such condition in space. Personally, I prefer the dimension reduction approach, as it pulls out climate features from any grid-point noise.

My second question about the methodology is why the function in equation 5 was selected for precipitation. Specific humidity is strongly related to temperature, unlike relative humidity) so it unclear to me that you can treat them as indepedent variables. The relationship between them can clearly be seen in Fig 2D - where the patterns are approximately opposite to each other.

My third methodological question revolves around downscaling. I appreciate your effort to downscale the climate results using High-resolution models. However, I wonder if you have applied them is the most optimal method. The (low-res) emulator captures climate changes from a (known) mean state. Your downscaling approach acts to modify those cliamte changes by modelling the resolution dependent aspects of those changes. If you want to convert the emulator output from climate anomalies to absolute climate, you must build back in the known mean state. Your choice of mean state is not explicit, and one wonders whether this might most appropriately be a very-high resolution satellite dataset (see question later about Fig. 6)

My final question about the methodology is where are the error bars on your estimate. Whilst I recognise that you cannot capture the error associated with HadCM3's biases, it must be possible to provide error bars of how well your model emulates the simulator. This is surely vital for the verification shown in Fig. 4 - does the true simulator response lie within the error bar estimates? What is the additional errors introudced by the simplicity of the ecosystem model applied to the emulator outputs.

I have an additional question about the validation in Fig. 6 C and D. What is being assessing here? The emulator only models *changes* in climate, not absolute variables as shown in Fig. 6D. I suspect that the assessment in Fig. 6C is more about the resolution of underlying simulator and little to do with the emulator. In fact, previous efforts (e.g. Lord et al., 2017) have used the present-day climate as the "mean" from which anomalies are calculated - which instead potentially allows the use of ERA-20C as the baseline. Under the test shown in Fig. 6, such a slightly revised emulator would be perfect.

Given the quantity and importance of these questions about the creation and validation of the emulator, I have chosen not to review the results in any detail.

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