Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-221-AC2, 2017

Geoscientific Model Development Discussions
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EGU

Interactive comment on "Climate pattern scaling set for an ensemble of 22 GCMs – adding uncertainty to the IMOGEN impacts system" *by* Przemyslaw Zelazowski et al.

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Received and published: 1 February 2017

Comment 2.1. The paper describes a system for emulating CMIP3 climate models in terms of land surface variables at low computational cost, including its design, performance, applications and limitations. The manuscript is clearly written. I understand the importance of the attempt and usefulness of the tool, although honestly I felt that CMIP3 models are relatively dated and the authors might have been able to adopt CMIP5 data sooner. I have a couple of overall comments, which are rather my impression and not something really critical.

Response 2.1. We thank the referee for this review and a number of useful comments. We accept that a dataset based on the CMIP5 dataset is required. However, as we

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Discussion paper

outline in the paper, a number of studies are based on the combination of the IMOGEN model and the presented set of patterns. This paper is intended to describe in a greater detail the dataset and how it was derived. We intend to work with an equivalent dataset based on CMIP5 and believe that there will be a benefit that we can refer back to the dataset described here, allowing comparison.

C.2.2. In my view, a climate emulator like the one presented here based on an energy balance model and pattern scaling is particularly powerful when it is applied for exploring a wide range of different scenarios (e.g., intermediate levels between RCPs). If the authors agree with this, it should be mentioned with more emphasis.

R.2.2. Agreed; we have already mentioned this briefly in the abstract, and in the beginning of the Introduction, but now we have expanded the later according to this suggestion (from line 9). In the Introduction, we now write:

"It allows interpolation away from a limited number of available GCM simulations, enabling a time-efficient assessment of surface meteorological changes for alternative non-standard future scenarios of changed GHG concentrations. This can include, for example, new scenarios that fall between the current Representative Concentration Pathways (RCP, Taylor et al., 2012), and potentially to investigate the current focus on targeting pre-defined future temperature thresholds such as two degrees"

C.2.3. The applications described are mostly focused on ecosystem impacts. I reckon however that the tool has a potential to be applied to a wider range of impact studies, including water resource, agriculture, health and so on. It could also be emphasized.

R.2.3. Agreed, and some of these potential applications we hope to pursue, so having IMOGEN v2.0 published will be helpful. Combining this comment with the suggestion from the Referee #2, we have added a new part to Section 4 "Applications" in which we undertake an assessment of IMOGEN performance in terms of ability to project changes to impacts, rather than just the direct climatic changes. We focus on mean annual total runoff, and making a direct comparison to GCM estimates of change. We

also added a new figure (#7). Please see response 3.1 below.'

Minor comments:

C.2.4. P.2, L.2, "Global Climate Models (GCMs, also called Earth System Models, ESMs)": In my understanding, a GCM is called an ESM especially when (or only if) it incorporates some biogeochemical components. Simply paraphrasing them sounds uncomfortable to me.

R.2.4. Agreed. Following this suggestion, and the one from Referee #2 (3.1.), we have dropped the use of "Earth System Model" description in the case of the analysed CMIP3 data.

C.2.5. P.3, L.1, "Intergovernmental Panel for Climate Change": "on" instead of "for".

R.2.5. Corrected.

C.2.6. P.4, L.33, "50 km (e.g. MIROC3.2hires model,": The atmospheric resolution of MIROC3.2-hires is T106, which is approximately 100 km in mid-latitudes instead of 50 km.

R.2.6. Corrected.

C.2.7. P.5, L.14, "climate regime c": Not clear what it means.

R.2.7. Changed to "climate regime in the decade c"

C.2.8. P.5, L.22, "one regression co-efficient, rather than two." Not entirely clear to me. Do you mean only slope rather than slope and intercept (or, equivalently, intercept is always zero)?

R.2.8. Yes, that is what we meant, and this is now stated more clearly in the same place. We now write: "This implies that the regression line starts at the origin of the co-ordinate system, so the intercept equals zero, and there is a fit with just one regression co-efficient, the slope"

C.2.9. P.5, L.33, “(ii) a constant ratio of mean land and ocean surface (SST) rate of warming, ν, (iii-iv) climate sensitivity over land λl and ocean λo (W m-2 K-1)”: Instinctively, it sounds over-specification to me, as it looks like v could be calculated by λl/λo (at least approximately). It might be my silly misunderstanding, but a bit of further explanation might be helpful to other readers as well.

R.2.9. Whilst we agree that having individual climate sensitivities over land and ocean might initially seem an over-specification, the concern we had is that the ocean component of the global energy balance behaves very differently to the land surface. The ocean is a huge store of thermal energy, currently making the planet lag significantly - by decades - behind the true level of committed warming for current atmospheric GHG concentrations. Relative to the oceans, the land operates so as to have almost negligible thermal capacity, and hence we wanted to capture both effects individually. This also suggests thermal energy flows by atmospheric transport from the land regions to the oceans regions. Initially in Huntingford and Cox (2000), we tested this by modelling an advection term k*(dT_air-dT_ocean), but the fit was poor. Instead we found we could close the equations, capturing GCMs well, with simply a land/atmosphere fixed contrast. This gives an implicit advection, with "k" being a function of time. It is still an area of open research as to why both historical measurements and GCMs all project near-constant (in time) land/atmosphere contrasts.

C.2.10. P.6, L.13, Eq (2): “(is, ms, g)” in the r.h.s might have to be “(gs, ms, i)”.

R.2.10. Agreed – thank you very much for spotting this mistake.

C.2.11. P.16, L.22: “Shiogama, H., Shiogama, H.,”: duplicated. Please delete one.

R.2.11. Deleted

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