

Interactive comment on “Development of a system emulating the global carbon cycle in Earth system models” by K. Tachiiri et al.

Anonymous Referee #3

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This is a good paper, and should be accepted for publication. GMDD is a good choice of journal.

This subject area is important. There remain on-going concerns that complex models such as GCMs are unable to (a) be run in ensembles and thus provide uncertainty bounds and (b) be readily operated for different emissions profiles.

The authors recognise the importance of retaining geographical information. Hence the technique to simply read off different levels of global warming, and then from the complex models obtain anomalies is interesting and an advance on pattern-scaling. However, the original pattern-scaling work does help to provide confidence that global temperature change does provide all the information needed to make regional projections. (Incidentally Mitchell et al, 1999 was only for temperature. Huntingford, C. and

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Cox, P.M. 2000 - "An analogue model to derive additional climate change scenarios from existing GCM simulations", Climate Dynamics, 16, 575-586 (tried this for all variables that might influence of land surface).

In the Abstract, does the "low resolution 3-D GCM ocean including an ocean carbon cycle" have a name, or at least link to an existing GCM?

Given that quite a lot of complexity is retained in having a full land surface model, I wonder if the authors might also like to present this work as an important impacts tool?

Where equations are presented, and equation terms are described, then units should be presented (e.g. directly after equation (1) and elsewhere in the paper)? This is especially true for a journal that allows model description to be presented.

I'm curious about the line "In the current setting, rain and snowmelt on land is returned to the nearest ocean grid". I take it this is referring to runoff, but presumably only after evaporation and interception losses are taken account of? Maybe a wording issue?

SAT is an acronym used for the first time just after equation (2) but without it first being defined. Here, and elsewhere, an additionally quick read through of the paper might pick up a few other things like this (there are quite a few typos in the paper).

Equation (4) seems to have degeneracy? I get this to be the same as $Q_{10}^{((T-15)/10.0)}$??

Section 2.4 - make clearer at the start that MIROC3.2 is used to provide geographical "patterns" of climate change. The sentence "We are using" made me think for a moment that in fact MIROC3.2 was actually a component of the fast climate model presented.

I wonder if the authors might like to think of an overall name for this new model, rather than a collection of different model components?

I don't completely agree that it is OK to say the model performs well for business-

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as-usual / transient simulations, and this is what matters as is most likely to occur (Discussion). There does remain some optimism amongst climate researchers and policymakers that we can have a time soon of peak emissions followed by major societal decarbonisation. Well, we can hope at least....

Interactive comment on Geosci. Model Dev. Discuss., 3, 61, 2010.