Geosci. Model Dev. Discuss., 5, C905–C908, 2012
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Geoscientific
Model Development
Discussions

Interactive comment on "Optimising the FAMOUS climate model: inclusion of global carbon cycling" *by* J. H. T. Williams et al.

Anonymous Referee #2

Received and published: 19 November 2012

This manuscript describes an attempt to improve the parameterisation of an earth system model using an ensemble technique. Tuning global models, and understanding the uncertainties associated with the chosen parameter values, is an ongoing problem for modellers. Studies in which objective methods are used, such as this one, are therefore to be welcomed. I have a number of significant issues regarding clarity and presentation of the model and results and, if these are adequately addressed, this manuscript should be suitable for publication in GMD.

1. While I realise the aim here is not to fully describe the terrestrial and marine models, the descriptions provided need to be be considerably strengthened:

a) Regarding the terrestrial model. We are told (page 3091) that there are five PFTs in the terrestrial model, but little more. Some basic text describing the key driving

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variables, presumably light and precipitation, should be provided. Also, to what extent are emergent PFT distributions a function of competition for these resources versus imposed bioclimatic limits? Adding some basic text of this sort will set the context for the description of parameter values that are to be varied, and which are described in detail. I have further queries regarding the terrestrial model. Is it TRIFFID that is being used and is it run in fully dynamic mode? The text appears to be ambiguous. On page 3093, for example, we are told it is run in "equilibrium" mode, suggesting a non-dynamic setup akin to the BIOME models that preceded the DGVMs. Bearing in mind that the manuscript is for a wide audience, the authors need to be more precise, and avoid jargon, in the model description.

b) The description of the ocean model, HadOCC, on page 3092 is also inadequate, being afforded just a few lines. To say simply that "phytoplankton and zooplankton populations . . . are limited purely by nitrate availability" will not do. For starters, the phytoplankton are also limited by light. And the zooplankton are not limited by nitrate per se but rather exert top-down control on the phytoplankton. Everything is controlled by physics, and in particular the seasonality of the surfaced mixed layer and how that links with the seasonal forcing of irradiance. The text is both imprecise and does not adequately set the scene for the parameter tuning exercise that follows.

2. The model is initialised at 1860 and each ensemble member run for 200 years. The authors correctly point out that the ocean model will not come into equilibrium in this time, but that the surface nitrate fields should do so, and so this is acceptable. On the terrestrial side, however, surely trees can live for more than 200 years? Surely the terrestrial model will not come into equilibrium within this time frame and so will be sensitive to the initialisation?

3. Why were only 7 parameters selected for adjustment in the terrestrial model, and 20 in the marine model? The terrestrial model actually has many more parameters than the marine model, so it is surprising that so few are selected for the terrestrial model and yet nearly all the parameters of the marine model are selected. The authors need

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to make clear and justify the strategy they are using. It looks ad hoc.

4. I have considerable problems with how the authors have chosen to compare the output of the terrestrial and marine models to data and thereby select the best ensemble members:

a) Again, starting with terrestrial. PFT distributions predicted during the ensemble runs are compared with the dominant PFTs shown in Figure 1. But are these distributions reasonable to compare with? Equatorial Africa, for example, appears to be dominated by C3 and C4 vegetation whereas surely some sort of rainforest should dominate? The authors probably realise this and have therefore chosen to focus solely on Amazonia for the first ensemble trawl, but we are then talking only 28 land gridboxes dominated by broadleaf trees. So the parameter adjustment has only to lead to the emergence of this PFT – is this really a critical test of the model and enough to distinguish between the different ensemble members? The second phase involves comparison at the global scale but the reader is presented only with some rather brief text (top of page 3099) describing this and the associated biases. The authors should instead provide a Figure, e.g. showing the global distributions of predicted PFTs in the best 7 ensemble members. Figure 3 is interesting. If the results of the best 7 are shown pictorially, these should then be identified individually in Figure 3. This would be most informative and interesting for the reader. The authors should also consider offering some discussion about how well the key parameters can be constrained by data in context of the ranges shown in Figure 3, thereby addressing the issue of uncertainty.

b) I find the presentation of results for the marine ensemble exercise thoroughly unconvincing. For starters, the reader is not even shown pictorially how well the predicted nitrate fields of the best ensemble member compares with the climatology. Such a comparison is essential – Figure 6 on its own will not do. Many of the ensemble member scores are similarly high (Figure 6). It would be interesting to know how disparate the parameter values associated with these members are, which would give an indication of the underdetermination in the system. In general, I'm sceptical about the

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merits of comparing only with nitrate as most marine biogeochemical models tend to do a good job at getting this field correct providing physics is reasonable. Indeed most of the errors in nutrient distributions are usually due to aberrant physics. The authors should consider showing, for example, the predicted nitrate distributions for several of the other ensemble members, e.g. the best one and a random selection of the others, to give the reader a feel for the degree of variability here. The authors should also give some other statistics relating to their best run (terrestrial model too), such as global primary production, chlorophyll distribution, etc, to convince the reader that this exercise has been meaningful. Finally, the authors should comment on the "best" parameters as identified in Table 3. For starters, the zooplankton assimilation efficiency of 0.908 is unrealistically high. The sinking rate of 6.488 m d-1 is also rather low. It is no good having fitted parameters if these cannot be justified physiologically.

5. Much more Discussion should be provided. Topics include: the underdetermination of model fitting by data; linked to the underdetermination issue, the suitability of the chosen datasets for constraining the model; to what extent are the terrestrial and marine models state-of-the-art: the HadOCC model, in particular, is rather rudimentary – would it not have been better to use a more realistic ecosystem model?; ocean physics – is the ocean ensemble work not seriously compromised by the coarse resolution physics which likely dominated mismatches between model and data, rather than ecosystem model parameterisation?

Interactive comment on Geosci. Model Dev. Discuss., 5, 3089, 2012.