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Geoscientific Model Development Discussions

GMDD
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Interactive Comment

Interactive comment on "Presentation, calibration and validation of the low-order, DCESS Earth System Model" *by* G. Shaffer et al.

Anonymous Referee #2

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Comments on "Presentation, calibration and validation of the low-order, DCESS Earth System Model" by G. Shaffer, S. Malskær Olsen, and J. O. P, Pederson

General comments:

The manuscript of Shaffer et al. presents the configuration, calibration, and simulations of a low-order Earth system model that includes the representation of the atmosphere, ocean, sediment, land, and lithosphere. In response to the increasing need of studying interactions between different components of the Earth system and their feedbacks to the Earth climate, it is urgent to develop Earth system models (ESMs) of different complexities. The model presented in this manuscript makes a unique contribution to the effort of ESM development by including multiple gases/tracers and detailed representation of sedimentation and weathering processes, which makes it particularly suitable for the study of long-term (over ten of thousands of years) change in the Earth system. This manuscript is well written and I recommend its publication in Geoscientific Model Development after addressing the reviewer's comments.

My only major concern is the insufficient discussion of model calibration procedure, particularly for the atmosphere and ocean part (section 3.1). While an enormous amount of effort is devoted to model description (22 pages), much less information is given to model calibration, except for the brief discussion "We arrived at such a best fit, pre-industrial, steady state solution by trial and error changes of the values of the parameters in Table 1" (Page 66, lines 14 to 15). As a reviewer (and I guess as a reader too), I would like to ask these questions:

1) How is the "best fit" solution defined? Is the criteria of "best fit" determined based on a single metric or multiple tracers (One parameter value could give the best fit for ocean temperature, while another value could give the best fit for radiocarbon).

2)What is the methodology used to calibrate each parameter for different model components? For example, are atmospheric and ocean physics parameters tuned together or separately under individual boundary conditions (e.g, tune atmospheric parameters under fixed ocean temperature and/or tune ocean transport parameters under fixed upper boundary conditions)? Are parameters governing ocean physics and biogeochemistry tuned together or one after another?

Although some of the answers could be found in the original paper of the HILDA model on which the present model is based (as stated by the authors "In this process we drew in part on the experience of the HILDA model calibration", for the scope of Geoscientific Model Development I think a clear and detailed discussion of the calibration procedure is as important as model description.

Specific comments:

GMDD

1, S28–S32, 2008

Interactive Comment

1 Introduction

Since a major scope of the Geoscientific Model Development is the description, development and evaluation of numerical models of the Earth System, the authors may want to cite more studies on the Earth system modeling to reflect continuous development made in Earth system models, and add a brief discussion of the unique feature of the DCESS model, compared to other box and Earth system models. Some recent papers using Earth system models are:

Lenton, T. M., and C. Britton, Enhanced carbonate and silicate weathering accelerates recovery from fossil fuel CO2 perturbations, Global Biogeochem. Cycles, 20, GB3009, doi:10.1029/2005GB002678, 2006.

Schmittner A., A. Oschlies, H. D. Matthews and E. D. Galbraith, Future changes in climate, ocean circulation, ecosystems and biogeochemical cycling simulated for a business-as-usual CO2 emission scenario until year 4000 AD, Global Biogeochem. Cycles, 22, GB1013, doi:10.1029/2007GB00295, 2008.

Brovkin, V., A. Ganopolski, D. Archer, and S. Rahmstorf, Lowering of glacial atmospheric CO2 in response to changes in oceanic circulation and marine biogeochemistry, Paleoceanography, doi:10.1029/2006PA001380, 2007.

2 Model description

Page 58, lines 17-18: please clarify why the results in Friedlingstein et al. (2006) support the neglect of temperature dependence of NPP in Eq. (24).

Page 58, lines 19-21: What are these fixed ratios based on?

3 Model calibration and validation

Page 66 lines 1 to 19: It looks to the reviewer that this paragraph is a general description of model configuration instead of calibration procedure. I would suggest combining this paragraph with the first paragraph of section 2 "Model description";.

GMDD

1, S28–S32, 2008

Interactive Comment

3.1.1 Calibration procedure:

As stated in the general comments, it is not clear to the reviewer how the model calibration was actually conducted. The authors gave a list of targeting observations against which model parameters are calibrated. However, it is unknown how these observations are used in the tuning processes. For example, profile of each tracer (T, S, 14C) can be used to calibrate ocean transport parameters, and presumably, calibration based on different tracers would yield different parameter values. Biogeochemical parameters can also be tuned against different biogeochemical tracers (PO4, DIC, ALK). The authors should clarify the step-by-step calibration procedure.

Page 68, Line 15-16: Give the source of data used for ocean profiles of PO4, O2, DIC, and ALK.

Page 68: Line 1-2: Give corresponding observations for ocean tracers. Otherwise, it does not add much information to give values of the modeled ocean mean PO4, DIC, and ALK.

Page 70, Line 1-4: Give the source of the data-based estimates of calcite saturation depth (CSD).

3.3.2 CO2 uptake rates and atmosphere tracer evolutions

It would be useful to know how the modeled bomb 14C and/or CFC inventories compare with data-based estimates.

3.4.1 Simulations over 100, 000 years

It will be useful to put DCESS model results in the context of other Earth system model results. For example, the authors can compare DCESS-simulated airborne CO2 in year 1,000 and 10,000 in response to a 5000 GtC CO2 injection with those from other models. A helpful reference is (Table 5 of the following paper)

Archer D., V. Brovkin, 2008, The millennial atmospheric lifetime of anthropogenic CO2,

GMDD

1, S28–S32, 2008

Climatic Change DOI 10.1007/s10584-008-9413-1

4 Discussions and conclusions

Page 84, lines 1-2: "We have put considerable emphasis on and much effort into calibrating the DCESS model to pre-industrial conditions by fitting to available data". However, this is not reflected in the manuscript. The discussion of model calibration for the atmosphere and ocean should be expanded.

Page 84, lines 17-20: The message implied here to the reviewer is that potential changes in ocean circulation and mixing in the future warming world are not important to climate change, which is not true. The discussion of the lack of ocean dynamics in the DCESS model and its implications need to be revised.

Technical issues:

Page 42, line 13: give full name of the HILDA model.

Page 46, line 25: "and 0 for the low-mid latitude and high latitude sectors" "0" should be "l and h"

Page 46, line 18 change "may be" to "is"

Page 48, lines 11 to 12 "weathering of organic carbon ... due to weathering of ..." This sentence needs rephrase.

Fig. 3 provide the source of the data-based profiles

Fig. 4 provide the source of the data-based profiles

Fig. 5 provide the source of the data-based profiles

Interactive comment on Geosci. Model Dev. Discuss., 1, 39, 2008.