Geosci. Model Dev. Discuss., 1, S20–S27, 2008
www.geosci-model-dev-discuss.net/1/S20/2008/

Geoscientific
Model Development
Discussions

Interactive comment on "GENIE-M: a new and improved GENIE-1 developed in Minnesota" *by* K. Matsumoto et al.

K. Matsumoto et al.

Received and published: 18 June 2008

We appreciate the comments from three anonymous reviewers, Daniel Lunt, Bob Marsh, and Rene Redler. In addition we received useful suggestions by personal email from others. We first address comments that were commonly raised by multiple reviews. Then we will make a point-by-point response to comments raised individually.

Commonly raised comments:

1) A major concern was that our overall tuning procedure, while transparent, appears subjective in that we separated it into (a) objective physical tuning, (b) targeted physical tuning, and (c) targeted biogeochemical tuning. Targeted tuning following objective tuning may in fact undo some of the benefits gained by objective tuning. In addition, our choice of targets for the objective tuning (surface air temperature and humidity and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

ocean temperature and salinity) should have ideally included anthropogenic transient tracers.

First, as suggested, we have a new Figure 1 that illustrates the three steps in a flow chart. As all three reviewers accurately allude to, we relied heavily on targeted tuning to achieve an improved GENIE-1 for marine biogeochemistry. We viewed objective tuning as an unbiased way to give us a good starting point for targeted tuning (Line 491, revision). The reviewers agree on the value of targeted tuning or “expert judgment”; one reviewer commenting “I would advocate a more expert opinion approach to tuning…human can assimilate more information than automated process.” Lenton et al. [2006] actually showed in their study that that objective tuning produced inferior GENIE-1 results to subjective tuning. Furthermore, Edwards and Marsh [2005] in their first objective optimization of GENIE-1 found that the acceptable range of parameter values after optimization remained as large as the range across the initial ensemble, further demonstrating that objective tuning is valuable yet its value is not absolute. These are discussed in revision (Lines 305-312)

We appreciate the first reviewer’s assessment that we had placed too much emphasis on objective tuning in our manuscript, possibly distorting our “portrayal of how the end result was achieved.” In our revised manuscript, as suggested, we bring targeted tuning more to the fore and deemphasize objective tuning (Lines 23, 306-308).

As for our choice of targets in objective tuning, indeed it would have been better had we included anthropogenic transient tracers such as CFCs. As noted by a reviewer, we were “steered on practical grounds by existing procedures” to use physical climatology fields (Line 303). We believe that this is acceptable given our premise that “expert judgment” is ultimately more useful than objective tuning. Also, GENIE-1 followed the same development path in which the physical model was objectively optimized [Edwards and Marsh, 2005] separately and prior to the biogeochemistry model [Ridgwell et al., 2007].

GMDD

1, S20–S27, 2008

Nevertheless it is interesting to speculate what we could have achieved had we been able to include transient tracers as targets. Siegenthaler and Joos [1992] showed that their 1D diffusion-advection ocean model tuned using temperature was incompatible with the same model tuned using bomb radiocarbon. While GENIE and their 1D model are obviously different, their finding would suggest that it is unlikely that we would have achieved a perfect model outright with transient tracers included as targets in objective tuning. In this case, we would need to subjectively determine the relative weighting of the targets to achieve a model that is more compatible with the physical fields or with the transient tracers. That is an expert judgment, and we cannot get away from it.

2) In tuning the biogeochemical model, why investigate the two model parameters that we did and not others such as the fraction of production that goes into the dissolved phase versus the particulate phase? Also, why tune the model to oxygen and export POC instead of ALK or DIC?

First, the fraction that goes into the two phases is a free model parameter. However, as adopted by OCMIP2 after careful consideration, global analysis indicated that it needs to be about 1/3 particulate phase and 2/3 dissolved phase in order to match observations (Line 98). Unless shown otherwise, we adopted the value here.

We chose to determine in our model the two parameters (POC sinking rate and NO3 half-saturation constant) because they are the critical parameters that control the interior distribution of nutrients and surface production. After all, in ocean biogeochemistry, production and remineralization are arguably the two most important processes (Lines 433-437).

We chose oxygen and POC export production because they are very diagnostic of marine biogeochemistry. They are both affected by physics (circulation, gas exchange, etc) and biology (nutrient uptake and remineralization). It is not as common to tune to DIC, whose measurements include the anthropogenic component, or ALK, which is not as integral a biogeochemical tracer.

GMDD

1, S20–S27, 2008

3) The additional FW flux needed to restore the Atlantic MOC after implementing the Kv(z) profile should be acknowledged as a step backward, because it would suppress the sensitivity of MOC to perturbations. We agree and have explicitly noted this in our revised manuscript in two places including the conclusions (Lines 413-416, 501-502).

4) Why is Gasex not adopted just because it does not seem to make as much a difference? The Wanninkhof coefficient used in GENIE-1 is the de facto standard in models today, and we decided in the end to continue adopting it here. In our revision, we note the new study by Sweeney et al that suggests a lower value. The implication (more effect on “slow” equilibration gases like radiocarbon) is also noted in the revision (Lines 247-255). We are open to modifying this in the future as empirical studies becomes clearer.

5) The naming of our model was subject to considerable scrutiny. At various stages of manuscript preparation and review, we received contradictory suggestions that ranged from “Don’t use “GENIE” and instead use something like Minnesota ESM… to… “Do not use GENIE-1 in the title.”

These suggestions partly reflect the lack of a coherent naming scheme. In the literature, the climate model consisting of ocean, sea ice, and EMBM atmosphere components is referred to as both C-GOLDSTEIN and GENIE-1. The primary distinction is that the latter is part of the GENIEfy effort (SVN version controlled codes). GENIE-1 also refers to the climate model plus marine biogeochemistry model, which in a non-modularized version would CB-GOLDSTEIN. When the EMBM atmospheric component in GENIE-1 is replaced with GCM, the climate model is called GENIE-2.

The suggestion to not used “GENIE” stems from our working with CB-GOLDSTEIN as our starting point (i.e., not working with the SVN code). The Bern University group is in the same category and they call theirs the Bern 3D model.

The main argument for retaining “GENIE” in the name is that important,

GMDD

1, S20–S27, 2008

genetic information about the model can be conveyed immediately. For example, it is not clear that the Bern 3D model is essentially GOLDSTEIN (with important improvements of course). Also, retention of “GENIE” would acknowledge the hard work of the “founders”.

Until now we had called our model GENIE-M for these latter reasons. However, given the reviews, we will rename our model to Minnesota Earth System Model for Ocean biogeochemistry (MESMO). This is to avoid confusion with the GENIEfy project efforts and our use of the non-modular code (see Lines 142-148).

Individually raised comments:

Response to Lunt: (1) Supplemental materials – in the revised manuscript, we have a top level document and make references to supplemental materials in the main text. (2) Naming and definitions of GENIE – refer to Common Comment #5 above. (3) Figure 3 (now 5) caption is enhanced.

Response to Marsh: (1) We have uploaded the entire source code as a supplemental material so that the details of code modification can be examined. Kv(z) was indeed modified in tsetpo.F. (2) To take advantage of GMD (and depart from “traditional style” manuscript), we have also uploaded as supplemental materials a movie of sea ice, export production, pCO2, and mixed layer depth. (3) Regarding Marsh’s more sophisticated Kv(z) scheme following Philander and Pacanowski (unpublished), I am not able to cite it in the main text of my manuscript. The handling editor and I have agreed to not use “personal communication.” However, this discussion itself is published and citable, so I hope that this acknowledgment will suffice for the time being. We will await the description in Marsh’s own paper. (4) Contrary to the comment, the Lunt et al. 2006 paper does not contain description of the SW radiation seasonality. I have modified my paper nevertheless to say that we’ve activated the seasonal switch that already existed in the code, so as to not improperly imply that we’ve coded this (Line 177). (5) Regarding FW

GMDD
1, S20–S27, 2008

flux, see Common Comment #3 above.

Response to Redler: (1) Different model components are coupled as given by Edwards and Marsh [2005].

Response to Reviewer 1: (1) Regarding the overall model calibration, see Common Comment #1. (2) Regarding biogeochemical tuning, see Common Comment #2. (3) Latitude grid spacing is about 3 deg at EQ and about 20 deg at pole – now noted in revision (Lines 74-75). (4) Frictional geostrophic lacks the momentum acceleration and advection from the N-S equation. Now noted in revision (Lines 75-79). (5) Production partitions – see Common Comment #2. (6) The appropriate Sabine number to use is 118 not 106, as correctly noted by Reviewer 2. (7) Description of objective tuning is now reduced by about half in the main text and the rest is in the Supplemental materials. See also Common Comment #1. (8) Sentence regarding ssmax and FW flux is clarified. Regarding the description of ssmax and GM, Kevin Oliver kindly suggested an alternative phrasing, which we adopted (Lines 180-186). (9) B is correct as noted originally – it is a proxy of biomass in terms of the limiting nutrient. (10) We believe the N cycle information is useful to others developing GENIE. (11) Captions for Figures 6 and 7 (now 8 and 9) are enhanced.

Response to Reviewer 2 (late submission; comments not online) (1) Description of NGSA II is reduced by half (see Response to Reviewer 1 #7 and Common Comment #1). (2) We have done additional simulation, where as suggested, we used the original GENIE-1 parameters with vertical resolution increased only. We have a new column (G1-16lev) in Table 2 and full paragraph in our revised manuscript (Lines 282-288). (3) As stated originally, we did not pursue seasonal wind stress forcings because the objective tuning with these were unreasonable in terms of MOC and production (both too high). We’ve repositioned this in our revision, so that this is something we’d like to further pursue in the future and not as a dead-end effort. See conclusions (Lines 509-513) (4) The preindustrial spinup starts from 1765 – as in OCMIP2. Now noted in revision (Line 275). (5) Weak MOC response due to

GMDD

1, S20–S27, 2008

FW flux – the point is well taken and consistent with Marsh’s comment (see Common Comment #3). (6) Our standard Kv(z) profile is described by Eq 1. Our decision was guided by observations by Ledwell, historical treatment by Bryan and Lewis, and NGSA II. Equation 1 for Kv(z) was not given correctly – it is now corrected. In our revision, we have a new figure (4) and a new paragraph that describes the sensitivity to the choice of the shape of the Kv profile (Lines 396-405). (7) Regarding the choice of DIC and ALK – see Common Comment #2.

Response to Reviewer 3 (1) Regarding summary’s calibration in general, see Common Comment #1. (2) Regarding title, see Common Comment #5. (3) Regarding the balance between objective and targeted tuning in the abstract, see Common Comment #1. (4) We appreciate the careful reading and editorial suggestions. We revised the manuscript incorporating the many of the suggestions. (5) OCMIP connection: DOP is included; DIC14C is a separate tracer from DIC and it is cycled just like DIC except with isotope fraction associated with production. (6) Equation 3 describes the temperature dependence – it is in fact a variation of Q10=2, except that the formulation considers the freezing point of about -2 deg C (numerator does not go negative). This is Maeir-Reimer’s original formulation that we adopted. (7) Seasonal winds were indeed implemented before the NGSAII in the hopes that we will achieve a more truly seasonal model. Unfortunately as described in response to Reviewer 2 #3, both MOC and export production were unreasonably high. We made the change before NGSAII for wind stresses, because they seemed like such significant change in BC compared to others. But again, we’ve repositioned this in our revision as a future direction and not as a dead-end effort. (Lines 509-513) (8) Regarding Gasex,– see Common Comment #4. (9) We appreciate the suggestion to have a new figure (a flow chart of some sort) to illustrate the tuning process. We have a new figure (Figure 1). (10) Description of NGSA II was indeed heavy – we have reduced it by half (see Response to Reviewer 1 #7 and Common Comment #1). (11) We appreciate suggestions to enhance the visibility of figures, although we feel that visibility is quite variable from one person to another.

GMDD
1, S20–S27, 2008

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

GMDD

1, S20–S27, 2008

Interactive Comment