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Geoscientific
Model Development
Discussions

Interactive comment on "Incorporation of the C-GOLDSTEIN efficient climate model into the GENIE framework: the "genie_eb_go_gs" configuration of GENIE" *by* R. Marsh et al.

Anonymous Referee #1

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Firstly, few thoughts related to published comments by the Reviewer 2 and the Editor. I personally strongly support the appearance of the journal specially designated for presentations of the models development. Description of model formulation and their performance is a very important part of modelling business which is, unfortunately, underappreciated and with the exception of the so-called "state-of-the art models", it is hard to get modelling description published in peer-reviewed journals. However I strongly believe that whatever aspect of modelling development is presented, the papers in GMD should be of use for a broad scientific community, not just a narrow circle of model users. And, of course, the papers should satisfy the standard requirements for the peer-reviewed journal: they should be scientifically sounded, internally consis-

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tent and clearly written. Based on these criteria I cannot recommend the manuscript by Marsh et al. for publication in GMD in its present form but I would strongly encourage the authors to resubmit a fundamentally reworked version of this manuscript again.

General comments

1. Content and language

Model development consists of many levels including model formulation (governing equations), numerics, technical implementation, optimisation for a given hardware, visualisation, data management, user manual, reporting of modifications and bugs fixing and even (as in the case of the reviewed manuscript) elements of marketing. All these aspects are important but not all of them are appropriate for the peer-reviewed journal because most of this information is only of use for a narrow group of model users. The manuscript by Marsh et al. contains a mix of a bit of everything and is hard for reading. The names of files and directories, compiling options, format of the input/output are not of general interest. I cannot rule out that in the future a mix of English, UNIX and FORTARN woill become usual for the scientific publications but I personally would strongly oppose this tendency.

2. Model names

The model is introduced in the paper as "genie_eb_go_gs" also known as "C_GOLDSTEIN", also known as "GENIE-1" but which should be "strictly" (page. 3) named "genie_ea_go_gs". This is more than enough to confuse everybody. I understand that the model described in the manuscript is slightly reworked C_GOLDSTEIN which was used already in a number of previous studies and now is implemented into GENIE framework. But are the "genie_eb_go_gs" and GENIE-1 the same model or other versions of GENIE-1 are also possible? And if so, what is the meaning of "GENIE-1"? Moreover, if this model version should be "strictly" named (according to Lenton et al.) "genie_ea_go_gs" then why you named it differently? And, after all, what "eb", "go" and "gs" stand for? I guess that "eb" means an energy balance atmosphere, "go"

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- GOLDSTEIN but what is the meaning of "gs"?

3. The model versions or just a framework?

Important question to me is whether the manuscript presents a set of model versions which are properly tuned, tested and recommended by the model designers for use or the manuscript just presents "unique selling point" of a toolbox which is still under construction and everybody can do with this tool whatever he/she wants. I, personally, would be very concerned if different workers will use completely different model versions with different grids and parameters sets but refer to them as "genie_ea_go_gs" or GENIE-1. How then one can know which model version was used?

4. Model description

The authors explained that the "genie_ea_go_gs" only marginally differs from C_GOLDSTEIN which was described in previous papers. This allows the authors to choose how detailed should be the description of model formulation but, in any case, the model description should be self-sufficient. If the authors do not want to present governing model equations then most of the tables given in the manuscript are completely useless for the readers. How one can guess what is, for example, "depth scale-moisture" or "advective coefficient of heat" (and why it is equal to zero) or "wind stress scaling"? Actually, tables 2-5 contains mostly trivial constants (like year length) which one can easily find in any reference book (but note that Stefan-Bolzman constant is not equal to 1000), while in the Table 6 non-trivial atmospheric, oceanic and sea ice parameters are put together without any classification. My suggestion is either to present all model equations (I guess there are not so many of them) or to remove all information which cannot be understood without them. In addition, I strongly recommend not to mix up in the same sentences the model physics and technical details of the program codes. My opinion is that presented model description is insufficient to get even a rough idea about model without using a number of previous publications. For example, very few people know what is the "frictional-geostrophic ocean model" and

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one has to go back to Edwards et al. (1998) to find model equations. In the description of the ocean model, temperature and salinity are named "dynamical" variables whilst velocity is not even mentioned although, obviously, it is computed in the model. To the contrary, in the description of the atmospheric model, surface wind and wind stress are mentioned as output (and shown in Fig. 2) but, in fact, they are not computed but prescribed from the present day climatology. Of course, this is trivial for the authors but it may not be so for the readers. Table 6 contains a value for "net freshwater flux from Atlantic to Pacific". Is it a flux correction? Then it should be explicitly stated in the text. The value of 0.32 Sv is a huge one and requires an explanation. The manuscript does not describe how shortwave radiation is computed. Figure 3 makes an impression that the planetary albedo is prescribed and is constant in time whilst in the text the albedo feedback is mentioned which implies that the planetary albedo is not constant in time. It remains unclear whether the model includes a land surface scheme or nor. On page 10 a simple land surface scheme of Williamson et al. is mentioned but the next sentence stated that "genie_eb_go_gs" configuration does not include land surface scheme and then "other land options" are mentioned. Do these options represent different modifications of "genie_eb_go_gs" or should they be named differently? In short, for those who are not familiar with this model, the model description is not very helpful.

5. Different meshes

The authors present four different model versions with different spatial grids. How these different grids are produced: objectively or manually? Why 36x36 and 72x72 grids have the same land/sea masks and bottom topography? Why Greenland is detached from America on three grids but not on the fourth? Whether Indonesian Throughflow, mentioned on page 6 as important for the ocean circulation, is opened or not?

6. Model performance

The manuscript contains more than forty (!) figures but they give a very little information

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about model performance. Figures from 8 to 36 resemble pictures from an animation movie - they all look very much the same even if they are not. Many figures are redundant and provide no essential information. Four spin-up figures (4-7) provide no other information than at least 1000 year spin-up is required to reach a quasi-equilibrium state which is well-known fact. Sensible flux over the continents and "atmospheric absorption of the shortwave radiation" shown in Figs. 20-23 are meaningless because sensible heat flux is not computed over the continents and atmospheric absorption over the continents and the oceans has completely different meaning in the model. The average convection depth shown in Fig. 17 (I would rather show maximum convection depth) should be shown in meters and not in the level units because this is inconvenient and because different model versions have different number of layers. Only Fig. 37 contains some comparison (and only for annual and zonally mean characteristics) of the model versions with empirical data. I would strongly suggest to give a more detailed presentation and analysis of model performance against observational data. In particular, I would suggest to show differences from the observational fields rather than absolute values for the atmosphere surface temperature, SST and surface salinity. Especially I would be interesting to see comparison of simulated precipitation with the real one. Since the model has constant in time diffusivity and planetary albedo, it would be interesting to see how good are the models in reproducing of seasonal variability of atmospheric temperature and precipitation. I believe, figures 13-16 should incorporate empirical data. There are already reasonably good estimates of the meridional heat (energy) transport in the ocean and in the atmosphere. They should be shown in the Fig. 32 for both seasons. In a view of so different sensitivities of AMOC to the freshwater flux in different model versions, it would be interesting to see how net freshwater flux into the Atlantic Ocean looks like for different model versions. In short, I would suggest to reduce number of figures but make them more informative.

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