Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-47-RC1, 2016

Geoscientific Model Development Discussions

Interactive comment on "Coupling of the regional climate model COSMO-CLM using OASIS3-MCT with regional ocean, land surface or global atmosphere model: description and performance" *by* Stefan Weiher et al.

Anonymous Referee #1

Received and published: 31 May 2016

Introduction

Coding and technical aspects of coupling Earth System Models are often relegated to institutional reports seldom referenced or widely read, and outcomes of work in coupling and load balancing are often blindly used by physical and biogeochemical modeling groups. Therefore I commend the authors for documenting their expansive coupling work, and for submitting it to be reviewed for a journal with a readership that bridges the coupler development and physical modeling communities. I do, however, have reservations about the final results, methods, and one comment about the scope

of the cited literature.

Significance of this paper in the context of other work

The manuscript's introduction could extend the perceived reach of the work if it were to illustrate its international significance. There are several latest-generation regional coupled earth system models in development in the U.S. and Canada, some of which use MCT and takes advantage of the work of Craig et al. (2012) that could have been cited and have appeared in the reviewed literature in recent years. The reason I mention these publications is to say that the introductory argument perhaps could be further enhanced, since work on load balancing high-resolution regional coupled earth system models is taking place in many parts of the Earth System Modeling community. This helps to widen the appeal of the current manuscript, and its significance.

Efficiency versus accuracy

This paper discusses a considerable number (five) of different coupled model configurations using CCLM, however only scant information is provided on each one of these configurations. It would be particularly useful to view maps of model domains to demonstrate the individual configurations for each of the coupled model systems in Table 2. This would help make it clear exactly how much ocean, land and sea ice exist in the respective model domains. Such details can have a large impact scalability and parallel efficiency, especially in the cryosphere (sea ice and snow). Therefore I suggest providing greater detail on the physical configuration of each of the models chosen, because this, too, has an enormous impact on the model solution. To illustrate this point, I focus here on the implementation of CICE Version 5 for CCLM+TRIMNP+CICE.

The computational efficiency of the solution in CICE is heavily dependent upon the total number of sea ice thickness categories used, the number of tracers needed, for example, by melt pond and ice-age tracking and biogeochemistry, and most importantly, the sea ice mechanics solution. If CICE 5 has been configured to use anisotropic (Elastic Anisotropic Plastic; EAP) sea ice mechanics, then it will definitely be expensive, and, could take as much as 30% of the total model execution time in pan-arctic fully coupled regional models, if a highly converged plastic sea-ice solution is required (2-second sub-cycling). However, if using the Elastic Viscous Plastic (EVP) sea ice rheology with 10-second sub-cycling, the time to solution of the sea ice model greatly improves, with only slight degradation of the plastic solution. In this configuration, the sea ice model could take only 10% of the total core time of running the model. It is still unknown as to which of the two variants is physically more accurate. This is precisely the same CICE Version 5.1 code, in the same coupled framework, using MCT, but with two different namelist settings yet to be fully explored in the literature. Further issues with the CICE coupling are discussed in the appendix.

This CICE anecdote drives at my main criticism of this paper as it currently stands: It seems to be a vacant conclusion to discuss model efficiency without discussing model accuracy. The most efficient model one can design is a constant number, but seldom is this model the most accurate. The only way this limitation in the current manuscript can be remedied is to explicitly state the configurations used for each particular model in the tests presented, including graphically representing the domains used. However, due to the number of different models and model configurations used, this may balloon the paper to unmanageable proportions. However, as the paper currently stands, there is too little information available for it to be useful for other groups trying to address coupled model efficiency in their particular configurations.

Conclusion

In some respects, the scope of this paper is too large and should be refined. The concluding arguments would be far more compelling, and, I believe, interesting to the modeling community, if it explored individual coupled configurations, and efficiency related to a group of relatively standard model settings in each component model. However, this is probably beyond the scope intended by the authors, and therefore one way to make sure the good work already done is published would be to: 1) Provide greater details of each of the models used to produce the results, including model domain maps,

of the model configuration tables, the latter in an appendix; and 2) Provide at least some indication of the accuracy of the solutions. Otherwise, one is left to wonder as to how exactly the results were produced. Currently the paper fails the reproducibility test , because insufficient information is provided to repeat the experiments. This, alone, is grounds for significant revision, which I hope the authors will undertake.

—-

Appendix – CICE configuration and coupling

This appendix addresses technicalities of the CICE setup that were puzzling to the reviewer. First, the authors may be interested to know that there were important bug fixes in the code between version 5.0 and 5.1 of CICE (update is in Hunke et al., 2015), however these would be unlikely to influence computational performance. Setting this aside, there are further improvements in the computational performance of the model using EAP that are being updated by the University of Reading at the current time. It is impossible to know whether or not this affects the results in this paper, because the CICE configuration used in this paper is never made clear. Also, and perhaps I missed it in the text, whether or not the namelist option "distribution_type" is changed in CICE is not discussed. This affects computational performance.

Most importantly, however, is the information within Table 5 on how CICE is coupled to CCLM. My understanding is that the âŁŮ symbol indicates fluxes being passed from CCLM to CICE. If this is the case, there is only one feedback from CICE to CCLM in Table 5 (SST), which draws into question the physical consistency of the coupling. If this were to be a fully coupled model, then there must be more feedbacks that just surface temperature to the atmosphere. For sea ice, the most important feedback is either albedo or reflected shortwave radiation, passing back from the sea ice model to the atmosphere, but neither is listed, which leads one to assume that albedo is being calculated in the atmospheric model independently. Given the sophistication of the Delta-Eddington albedo parameterization in CICE, this seems odd. This inconsistency should be addressed before publication.

It is also odd that the atmosphere is calculating sensible and latent heat fluxes, given that the CICE configuration has five sea ice thickness categories each calculating an independent surface temperature upon which turbulent fluxes are based. Hence the turbulent heat fluxes must be inconsistent with the surface stress term, which is being calculated internally in CICE in the configuration given. When this calculation is done within CICE, assuming Monin-Obukhov stability calculations are being performed, the drag coefficient accounts for the individual surface temperature of each of the five sea ice thickness categories. If this calculation is not being performed in CICE, then the only alternative would be for the sea ice model to use only neutral drag, which would also be inconsistent with the sensible and latent heat flux components of turbulent transfer being passed from the atmosphere. The only way to remedy this is either to specify surface stress from the atmospheric model, or to fully use the turbulent transfer calculations in CICE, and pass the sensible and latent heat fluxes back to the atmosphere from the sea ice model. This is the reverse of what is currently being done, or at least described in this manuscript. This inconsistency should also be addressed before publication.

References

Craig, A. P., M. Vertenstein, and R. Jacob (2012), A new flexible coupler for earth system modeling developed for CCSM4 and CESM1, Int. J. High Perform. Comput. Appl., 26(1), 31–42, doi:10.1177/1094342011428141.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-47, 2016.