Geosci. Model Dev. Discuss., 4, C285–C289, 2011
www.geosci-model-dev-discuss.net/4/C285/2011/

Geoscientific
Model Development
Discussions

Interactive comment on "The atmosphere-ocean general circulation model EMAC-MPIOM" *by* A. Pozzer et al.

Anonymous Referee #2

Received and published: 24 May 2011

General Comments

Pozzer et al. present a new approach to couple the ECHAM/MESSY Atmospheric Chemistry model (EMAC) and the ocean general circulation model MPIOM using the Modular Earth Submodel System (MESSy) infrastructure. After outlining the new approach based on internal coupling, model performance in terms of CPU-time is compared to an external coupling approach implemented in the COSMOS climate model. COSMOS relies on the widely used OASIS coupler. In a second part the new model EMAC-MPIOM is evaluated by comparing various quantities like sea surface temperatures, ice coverage, etc. to measurements, re-analyses, and other model data.

Generally, I found the paper interesting to read because it presents a novel approach. The presentation is mostly clear and substantial conclusions are reached. However,

the comparison of the performance of the internal and external coupling methods needs to be extended and presented in more detail, I think. The discussion of the evaluation of the new EMAC-MPIOM seems sufficient at this point. The authors give proper credit to related work. The paper addresses relevant scientific questions within the scope of GMD. It should be published subject to minor comments and technical corrections listed below.

Specific Comments

p. 458, l. 9: Maybe be a bit more specific by what you mean with "same configurations" in both model systems, e.g. in terms of spatial sampling/resolution, timesteps, etc.?

p. 458, l. 11: Suggest to replace "more efficient" by "more efficient in terms of CPU-time"?

p. 461, l. 10-12: It is not clear to me what you mean by "The representation is a basic entity of the submodel CHANNEL [...]", please try to explain this for a reader who is not so familiar with the MESSy concept, in particular since this feature was newly introduced in V2. The motivation became a bit clearer after reading p. 462, l. 1-6. Maybe reorganize this section a bit.

p. 461, l. 27-28: I was wondering about the motivation for choosing the parallel decompositions of both submodels in the way they are depicted in Fig. 2? Please add one sentence to explain this.

p. 462, l. 13-14: At this point I was already wondering which variables need to be exchanged between the A- and O-GCM. Maybe you could refer to Tab. 1 already here? I was wondering also about the amount of data to be exchanged and how frequently this needs to be done?

p. 464, l. 6-13: Which of the different SCRIP interpolation scheme has been selected and why was it selected (performance/accuracy)? I have noticed, this information seems to be available in the electronic supplement, but I suggest to put it in the paper.

p. 465, l. 5-7: In principle, it seems okay to deduce information on the performance of different coupling approaches by comparing the run-time requirements of rather different model systems (ECHAM/MESSy versus COSMOS). However, can it really be guarantied that both model systems apply exactly the same sub-model code, are compiled with the same compiler arguments, use the same software libraries, etc? Wouldn't it be technically easily feasible and more reliable to directly measure the CPU-time required just for the coupling step? In this context, it would also be interesting to see the run-times for your new system if you run it a) decoupled, b) one-way coupled (just a2o or o2a), and c) fully coupled.

p. 465, l. 8-13: Since you haven't tested or present results only for just one specific software and hardware environment (Intel cores, Fortran compiler, MPI library), it should be clearly pointed out that the absolute timing results are not likely to be transferable to other conditions.

p. 465, l. 27 - p. 466, l. 2: How about the MPIOM? Do the modifications made to implement it in the MESSy infrastructure cause performance degradations as well?

p. 466, l. 2: Please point out where on the MESSy web site this information can be found.

p. 466, l. 3-12: I found this discussion a bit confusing because two things get mixed up. In the begin the bias introduced by computing the interpolation weights is discussed. Here I was wondering why you didn't implement the COSMOS approach and read the weights from pre-computed tables? Are you subtracting the 58sec bias from your results based on the assumption that this could be done, in principle? Later in this paragraph it is mentioned that the "interpolation calculation" is the most expensive procedure. Does this still refer to the calculation of the weights or does it refer to the calculation of the interpolation results based on given weights?

p. 466, l. 13-15: You might point out here that the OASIS approach may also not scale well for very large numbers of tasks, if the communication via a master node becomes a bottleneck? The better scalability of the internal coupling approach implemented in EMAC-MPIOM seems like a major advantage to me considering that hardware development tends towards systems with more and more cores. You might stress this advantage here or at p. 467, l. 5-9?

p. 468, l. 23: Add a number to "largest biases" to be more specific.

p. 469, l. 23 - p. 470, l. 10: Please clarify if/when you refer to Fig. 8a or Fig. 8b.

p. 470, l. 17: Why did you select September and March?

p. 471, l. 4-15: It is speculated that the differences in ice coverage are due to different model resolutions. Would it be possible to confirm this hypothesis by simulation? It does not seem intuitive to me that a coarser model grid causes larger variability.

p. 472, l. 17: Why do you compare to the "PI" simulation? Shouldn't be "TRANS" the more realistic one?

p. 473, l. 25 - p. 474, l. 4: I understand that CO_2 doubling causes a 2.8K increase of mean surface temperature in the EMAC-MPIOM and a 3.35K increase based on another model with OASIS coupling, but for the same experiment? Does this really imply that 0.55K difference in the results are just due to the different coupling approaches?

p. 475, l. 11-16: I think the outlook given in this paragraph is not really relevant or necessary for the paper and could be removed.

p. 481, Fig. 1: Maybe you could modify the figure a bit to make more clear that internal and external coupling also relate to running a single or separate binaries?

p. 483, Fig. 3: In this case it might be interesting to carry out a little experiment and apply a backward transformation to the transformed data. The results could be compared to the original data in order to assess the interpolation errors.

p. 484, Fig. 4: The color-bar is labeled "CPUs number", do you mean "cores" here?

p. 493, Fig. 12: y-axis label is missing.

Technical Corrections

p. 463, l. 17: change "chase" to "case"

p. 463, l. 23: change "Thanks" to "Due"

p. 466, l. 11: change "calculations" to "calculation"

p. 469, l. 18, change "CMPI3 Meehl" to "CMIP3, Meehl"

p. 470, l. 4: change "showed" to "shown"

p. 471, l. 20: change "long time period" to "a long time period"

p. 480, Tab. 1: change "thikness" to "thickness"

p. 483, Fig. 3: change "top" and "bottom" to "left" and "right" in caption

p. 486, Fig. 6: change "AMIPII" to "AMIP II" in caption

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