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

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We thank S. Valcke for her valuable comments, which will clearly improve the quality of the manuscript. Here we reply to the specific comments:

1. The comment is precise and detailed. We will include this information in the revised manuscript.
2. We indeed agree that one of the issues in the integrated approach is the coding effort, which depends on the coding quality of the legacy model (here MPIOM). The MPIOM code used here is well structured with distinct initialisation, time integration and finalising phases, and also a separation between output routines and main model code. The creation of the MESSy submodel interface layer was

hence rather straightforward, with a manageable coding effort due to the possibility to compile and pack the MPIOM subroutines as a library. In addition, because neither the time manager of MPIOM, nor the MPIOM output routines are required anymore, because they are replaced by the corresponding MESSy generic submodels, the creation of the interface layer reduced the code complexity of MPIOM.

One key aspect of the MESSy approach is the strict separation of the process formulations from the model infrastructure (e.g., time management, I/O, parallel decomposition etc.). Conflicts with I/O units, for instance, do not occur, since they are generated dynamically at run-time. The legacy models are reduced to their process formulations.

It must also be stressed that all modification of the original MPIOM code have been enclosed in pre-processor directives (`#ifdef MESSY`), which allow to reproduce the legacy code if compiled without the directive. In total, apart from the modifications in the main routine contained in `mpiom.f90`, about 20 modifications in 11 different files were required. The majority of these modifications are to restrict write statements to one PE, in order to reduce the output to the log-file.

The main changes in the original source code modify the input of the initialisation fields (salinity and temperature from the Levitus climatology) which were originally hard-wired at the beginning of the year. In EMAC/MPIOM, the model can be started at any time of the year and the code will automatically use the corresponding month from the climatology for initialisation.

Another main modification is related to the selection of various parameters for coupled and non-coupled simulations. In the MPIOM code, this selection was implemented with preprocessor directives, hence reducing the model flexibility at run-time. In the EMAC/MPIOM coupled system, the preprocessor directives have been substituted by a logical namelist parameter, and in one case (`growth.f90`) the routines in the coupled case were moved to a new file

(`growth_coupled.f90`).

With "using the same high level API to the MPI" we mean that both, MPIOM and EMAC share the same subroutines contained in `mo_mpi.f90`. We will add these information in the revised version of the manuscript.

3. Indeed, the ocean and atmosphere subdomains do not match geographically. We will clarify this in the revised version.

4. We agree that a more sophisticated approach could render the "gathering" of information superfluous. The gathering is indeed required because the initial information on the interpolation weights are calculated sequentially on the global fields, and it is hence straightforward to store these values for the global fields. Nevertheless, the coding effort to remove also this gathering routine is small and planned for future versions of the code. We will discuss this in the revised manuscript.

5. We thank the referee for the suggestion. We will include the proposed text in the revised manuscript.

6. The label is indeed not correct, due to the page layout changes for GMDD. The right figure shows the field after the interpolation, as an example for the interpolation procedure. The main goal of the figure is to show that the interpolation routines visually preserve the patterns of the original field. Additionally, the fluxes are not only globally conserved, but also locally, thanks to the sophisticated interpolation procedures (see SCRIP library). The converging lines in the right figure are an artifact due to the figure compression. We will substitute the figure with a non deforming compression format.

7. This is probably a misunderstanding. Although it is completely true that the load balancing influences the model performance, it must be stressed that differences

in the load (im)balancing *arise* from the different coupling methods. In particular, in our case, the two model components and model setups are the same for COSMOS and EMAC/MPIOM, thus the load imbalance is purely dependent on the coupling method (external versus internal). Moreover, it must be stressed that in the case of the simulation with the OASIS3 coupler, numerous simulations have been performed with different task distributions between the components to achieve the "maximum" possible model speed, and hence reduce the load imbalance to the minimum. As conclusion, although the load balancing is playing an important role in the model efficiency, it can also be considered as an "indirect effect" of the coupling method adopted. We will clarify this in the revised manuscript.

8. First of all, we admit that the goal of this comparison was not clearly enough stated. The aim of our comparison is not to assess which coupler was the fastest in general, but rather to show that our internal coupling approach does not deteriorate the model performance. We will clarify this in the revised manuscript.

   Both, sequential (internal coupling) and concurrent execution (external coupling) of the components, have their advantages and disadvantages (see also reply no. 17), and different applications/resolutions of the models might require different approaches. In reply to the specific comments:

   - The 58 seconds have been estimated from the linear regression between the different run times, as shown in Fig. 4. The result implies that the EMAC/MPIOM model in the applied setup on the used machine needs always 58 seconds longer than COSMOS-1.0, independent of the total time and the number of tasks. This 58 seconds is almost exactly the time, which is required for the initialisation phase of the model until the end of the first time step. A large fraction of this time is consumed by the initial calculation of the interpolation weights, which in EMAC/MPIOM is performed on-line,

whereas in COSMOS the weights are read from pre-processed files. Thus, the on-line interpolation during the initialisation phase causes a bias, which is independent of the number of tasks. Since we are interested in the performance during the time integration phase, we subtracted this bias for a fair comparison. The performance gain/loss w.r.t. the number of tasks is then analysed by the regression analysis.

- As explicitly stated on P.465 in L.23, we determined the best load balance by repeating the simulations with different task distributions among the components.
- We agree that the OASIS3 coupler can be used with more than one dedicated task for the transformation. However, in our analysis, the additional tasks for the data exchange would further reduce the COSMOS-1.0 performance, because less tasks are available for the domain models, given a fixed number of tasks to be distributed. The impact of additional tasks for interpolation/data exchange is certainly different at a low number of total tasks compared to a high number of total tasks.

We agree with the referee, that our analysis does not allow a general conclusion, which is valid for all model setups, resolutions task numbers etc. This was not the aim, and we will clarify this in the revised manuscript. Yet, our results do provide an indication that the integrated coupling approach is working with a satisfactory performance, which is - at least for the tested setup - comparable to the performance of the external approach (the fasted we could achieve). Further studies with higher resolutions are clearly required, but beyond the scope of the present work. Finally, we also agree with the referee that it is indeed difficult (if not impossible) to "a priori" estimate which coupling method yields the better performance: it depends on the resolution, the model setups, the number of available cores, the architecture etc. As mentioned before, we will rephrase the goal of the comparison in the revised manuscript, and we will extend further the discussion on

the performance, based on this reply.

9. The dataset is based on the work of Hurrell et al. (2008), and " it is a merged product based on the monthly mean Hadley Centre sea ice and SST dataset version 1 (HadISST) and version 2 of the National Oceanic and Atmospheric Administration (NOAA) weekly optimum interpolation (OI) SST analysis". It will be added to the revised text.

10. We will increase the area coverage of the figures.

11. We indeed compared the HadISST data with the TRANS simulation, and the picture present an incorrect label (is not PI, but TRANS). We apologise for the mistake.

12. We agree that "reasonably well" is a strong statement here. However, the model is visually performing better in this region than in the open Pacific Ocean. In fact during July, August and September, the model seems to reproduce the correct variability (although slightly higher) in the central Indian Ocean. Also during April, May and June the patterns produced by the model are qualitatively similar to the observed. However, as suggested by the referee, the model is strongly overestimating the observed variability during October, November and December in all the Indian Ocean (especially in the Southern part), while in January, February and March the open ocean shows a too high inter-annual variability over the central-south Indian Ocean and a too low variability near the northern coasts.

13. Indeed the model simulates a too high ice coverage (see also comment no. 15). Although we stated (section 5.2) that the ice coverage is overestimated (leading to a somewhat erroneous position of the MOC, see page 471,L28), not enough emphasis has been put on this issue in the manuscript. We will substitute "seems to predict" with "is clearly predicting", and we will underline the ice coverage overestimation by the model in the revised version of the manuscript.

14. Figure 11 shows the maximum depth of vertical convection. We believe that 50 years is a sufficiently long period for a sound statistic. We hence believe that additional years would add only some minor details, if at all. A period of 50 years has been used in other studies as well (see as example Jungclaus et al. (2007)). Nevertheless, we will replace the figure based on the entire time series of the simulation (i.e. 1850-2000).

15. We thank the referee for pointing this out. Indeed the ice coverage overestimation (see reply no. 13) has a strong influence on the convection. Although this is clearly visible in the North Atlantic, also the southern ocean is strongly affected by this issue. It is hence correct to rephrase the sentence with "convection occurs mainly well outside the Weddel Sea and Ross Sea, with some small convective events occurring all around the southern ocean, with the major events occurring between 0 and 45 degree East.".

16. We will substitute the sentence with: "It is shown that the internal coupling approach of EMAC/MPIOM followed in this study shows a comparable run-time performance as the COSMOS coupling approach using OASIS3, and does not deteriorate the effective performance of the model components."

17. We partially agree with the referee regarding the scalability issue of the code, yet some clarifications are required.

    It is clear that the scalability of the integrated approach is limited by the worst scaling model in the coupled system, but also the external approach is similarly affected. In the hypotheses of a fixed number of available tasks for both coupling methods, the serial approach will be limited by the"less scalable component". Nonetheless, the external coupling approach will also encounter the same problem: while we could theoretically increase the number of tasks used in the lower scalable component, this will lead to a reduction of tasks in the more effective scalable components. We hence firmly believe that the scalability issue is

present in both method, and it is an intrinsic problem of the AO-GCM components rather than a coupling method issue, and this must be solved inside the single component.

However, we agree that scalability is an issue in the integrated approach for other reasons: it is known that any given code has a better scalability at low number of tasks than at high number of tasks. This is generally due to the increase in the communication complexity, which hampers the perfect scalability at any increase in tasks number. It is to be expected that the concurrent execution of the components has an intrinsic better scalability, simply because its model components use lower number of tasks.

Additionally, if there are restrictions on the number of tasks in one component model (and/or an unlimited amount of cores is present) the external approach is superior, since more tasks can be used. This, to our opinion, is the main limitation of the integrated/sequential approach. Assume two model components, A and B, which scale up to n and m tasks, respectively. In the internal approach $min(n, m)$ tasks can be used efficiently with a perfect load balance, and the optimum is achieved, if $n \approx m$. In the external approach $n+m+c$ tasks (c for the coupler) can be used, thus for $n \approx m$ twice as much tasks as in the internal approach (plus c). With the actual trend in the High Performance Computing, where larger multicore machines are produced every year, this capability is extremely important.

The overall scalability of the coupled model does not depend only on the scaling capabilities of the single component. The load (im)balance of the external approach strongly depends on a perfect synchronisation of the component models and the coupler, which can probably hardly be reached, because the number of cores is discrete and the component models might have restrictions on the number of usable tasks. For example, in the external approach, if component A reaches the maximum speed with smaller amount of tasks than component B (i.e. $n_{max} \ll m$ tasks), it does not help to add more tasks as the coupled model

will have always the same speed of component A.

We therefore conclude:

(a) The scaling and load imbalance issues cannot be regarded separately. The external approach has advantages for the scalability, the internal approach for the load balance.

(b) The scalability issue is an intrinsic problem of the component models rather than a coupling method issue, and this must be solved inside the single components.

(c) A general statement about the performance and scaling features of internal versus external coupling method is simply impossible, because it depends on the coupled components and the available computational power.

Similarly, no general statement on the (re-)coding efforts in both approaches can be made, since it depends on the coding quality of the legacy domain models. In both cases, internal and external approach, the legacy models must be equipped with additional infrastructure and interfaces.

We will extend the discussion on these issues in the revised manuscript and provide a list of all advantages and disadvantages of both approaches.

We will include all the technical corrections suggested in the revised manuscript.

References

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