Review of the manuscript MWR-D-11-00132:
Sensitivity and Interpretation of Zonal Mean Climate from two Atmospheric General Circulation Models with Different Dynamical Cores

Authors: H. Zhang, M. Zhang and Q. Zeng

Recommendation: Major revisions

Major Comments:

The manuscript describes a model intercomparison between the models IAP AGCM4.0 and NCAR's Community Atmosphere Model version 3.1. The models utilize different dynamical cores but employ the same CAM3.1 physics package. Such an approach has the potential to reveal the impact of the dynamical core on the climate simulation and is very valuable. The manuscript is built upon a hierarchical approach. First, full-physics simulations are compared with realistic sea surface temperatures (SST) and sea ice forcings. The differences between the models are then further evaluated in idealized model experiments, in particular in dry dynamical core simulations with the Held-Suarez forcing and in full-physics aqua-planet simulations with idealized SST data. The intercomparison focuses on the evaluation of the time-mean zonal-mean climate and compares mean fields, Eddy statistics, the strength of the residual circulation and the interaction between moist physics and the dynamical cores.

In general, the research addressed in the manuscript is interesting, but the manuscript suffers from a variety of weaknesses and even includes incorrect simulations. The main weaknesses are:

1) The IAP AGCM4.0 model is mostly unknown in the modeling community and most references were published in Chinese. Therefore, the manuscript represents an opportunity to introduce the design of the dynamical core in a rigorous fashion. There is an attempt of a model description in section 2, but this description is even for an experienced modeler incomprehensible and incomplete. Since the manuscript claims that the simulation differences are mostly influenced by the choice of the dynamical core, the incomplete description leaves me guessing which design decision in the dynamical core could have contributed to the differences. I suggest including a full and comprehensive description of the dynamical core (more details on this are listed below).

2) The models are compared with an identical grid spacing of 128x256 grid points. However, it is well known that spectral models represent a nominally higher resolution than grid point models with the same number of grid points. E.g. the study by

Williamson, D. L. (2008), Equivalent finite volume and Eulerian spectral transform horizontal resolutions established from aqua-planet simulations, Tellus, Vol. 60A, 839–847

showed that the climate of the CAM Eulerian spectral transform model with the spectral truncation of T85 (128 x 256 grid points) resembles the climate of a finite-volume grid point model with the finer latitude x longitude resolution of 1° x 1.25° (181 x 288 grid points). This is due to the very different diffusive characteristics of grid-point models that need more resolution to resolve sharp gradients.

Therefore, the conclusion of the manuscript that IAP AGCM4.0 is more diffusive is no surprise since it probably should be compared to a coarser T63 spectral resolution. Since Eddy statistics tend to become stronger at higher resolution, the current intercomparison with a T85 model could be very misleading. I would like to see some of the IAP AGCM4.0 experiments repeated with a 1° grid spacing to truly gain confidence in the conclusions of the manuscript. I expect the differences between the models might be significantly reduced.

There are also other aspects that deserve more attention. E.g. the way the physics package is coupled to the IAP AGCM4.0 dynamics must clearly be described (time-split versus process-split). The Eulerian model is process-split. Another important aspect is whether the two models used identical physics time steps. In addition, were the empirical physics tuning parameters the same?

3) A severe weakness is the manuscript is that the Held-Suarez simulations are wrong. This is easily revealed by a comparison of the mean temperature fields (Fig. 5) to the temperature fields in the cited papers

Wan, H., M. A. Giorgetta and L. Bonaventura (2008), Ensemble Held–Suarez Test with a Spectral Transform Model: Variability, Sensitivity, and Convergence, Mon. Wea. Rev., Vol. 136, 1075-1092

Held, I. M. and M. J. Suarez (1994), A proposal for the intercomparison of the dynamical cores at atmospheric general circulation models, Bull. Amer. Meteor. Soc., Vol. 75, 1825-1830

The stratospheric temperatures in Fig. 5 are about 10 K too warm in comparison to published results.

Most likely, the wrong CAM 3.1 simulations are due to an undocumented bug in the Held-Suarez setup at NCAR. The bug is the application of an energy fixer in the Eulerian dynamics routine ‘tfilt_massfix.F90’. The energy (temperature) correction ‘beta’ is applied in the line

t3 (i,k) = t3(i,k) + beta

but ‘beta’ is incorrectly implemented for Held-Suarez experiments. This line needs to be commented out. I came across this bug some time ago. The next page shows my own CAM Eulerian and semi-Lagrangian simulations with the Held-

Suarez forcing with and without the energy fixer. Is the same wrong energy fixer applied in IAP AGCM4.0? If not, there must be another bug in the Held-Suarez setup of IAP AGCM4.0 that needs to be found. In any case, all Held-Suarez experiments need to be fixed and repeated, I recommend at the CAM T85 and IAP AGCM4.0 1°x1° resolutions.

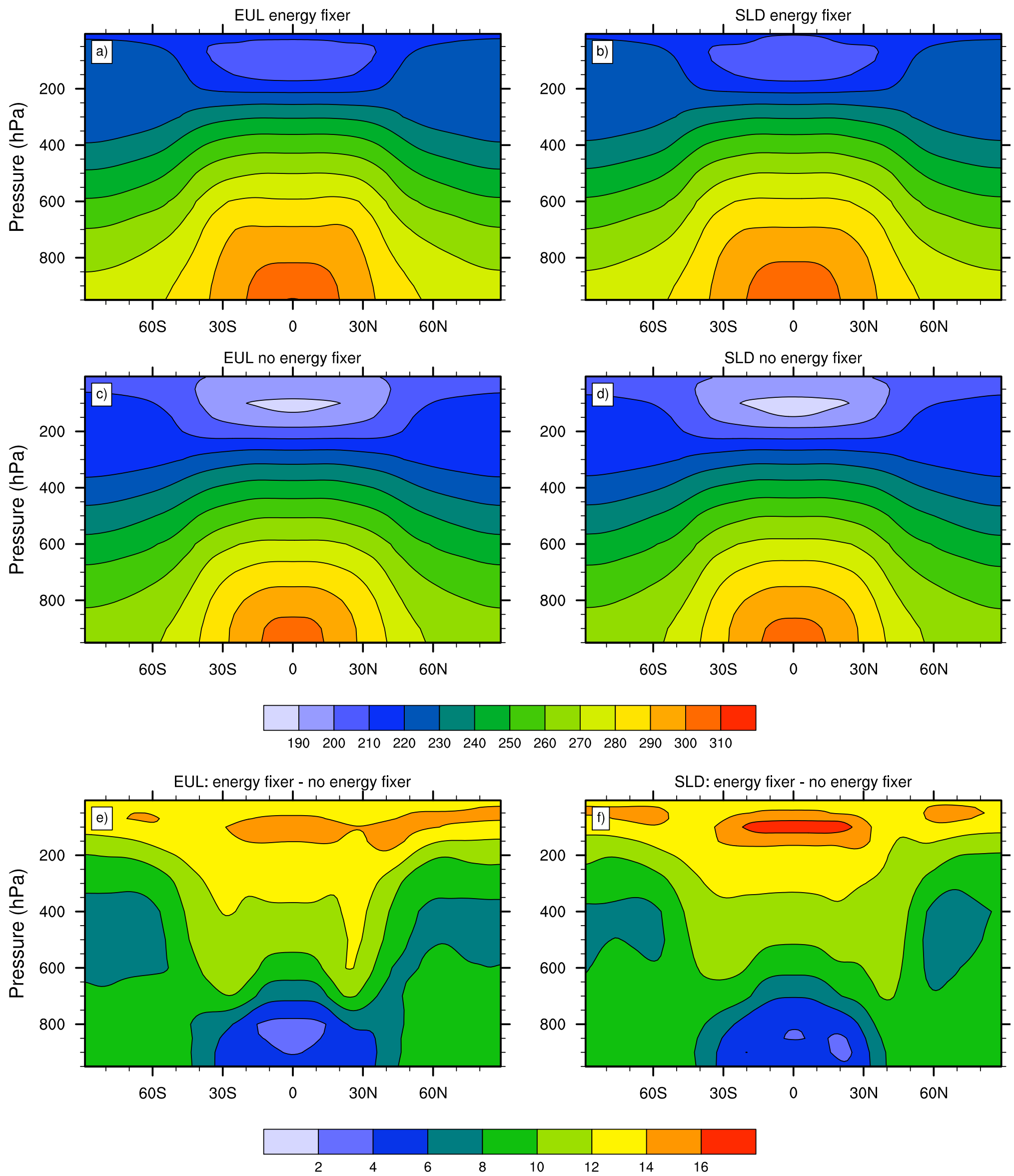

Figure: (a-d) 1000-day-mean zonal-mean temperature (in K) in the Held-Suarez tests simulated with the CAM spectral (a,c) Eulerian and (b,d) semi-Lagrangian dynamical cores. (a,b) are simulations with the wrong energy fixer, (c,d) are simulations without energy fixer. (e,f) show the temperature differences (in K) between the energy fixer and no energy fixer runs. The wrong energy fixer leads to significant heating throughout the domain.

4) The comparison of the full-physics simulations with realistic boundary forcing to the reanalysis data is important to gain an understanding of how well the two models compare to observations. This component of the manuscript needs to be strengthened. For example, a quick comparison of the mean transient Eddy statistics (Figs. 3 and 4) to the mean transient Eddy statistics of the ECMWF ERA-40 atlas:

   Annual-mean transient northward Eddy heat flux $\overline{v'T'}$:
   http://ecmwf.int/research/era/ERA-40/ERA-40_Atlas/docs/section_D25/charts/D37_XS_YEA.html

   Annual-mean transient Eddy momentum flux $\overline{u'v'}$:
   http://ecmwf.int/research/era/ERA-40/ERA-40_Atlas/docs/section_D25/charts/D33_XS_YEA.html

   reveal distinct systematic differences between the models and ERA-40 reanalysis data. Both models seem to severely overestimate the strengths of the Eddy statistics. Why did you not include the comparison to the NCEP reanalysis data for the Eddy statistics? I suggest adding these reanalysis results to support a more objective evaluation.
   In addition, it is unclear how the mean transient Eddy statistics were computed in the model simulations. Please add the specifics that explain the computation.

Minor comments and corrections:

1) Introduction, page 3: add a reference that describes the vertical σ coordinate, explain the acronym 9L (9 levels)
2) Page 4, last sentence of introduction, typo in 'summery'
3) Section 2: this section is incomprehensible. Include a complete description of the dynamical core, including the mathematical representation of the model equations. Which form of the equations is used (Eulerian versus semi-Lagrangian, vector-invariant, etc), what are the prognostic variables, what is the order of the scheme in the horizontal and vertical directions, are finite-differences used in the vertical, what is the exact dissipation mechanism (4^{th}-order hyperdiffusion?), what is the diffusion coefficient, do you apply a polar Fourier filter, is the model hydrostatic with shallow-atmosphere approximation, …?
   I do not understand
   a) what you mean by 'nonlinear iterative time integration method'
   b) how the exact conservation of available total energy is achieved
   c) what you mean by 'internal consistence according to physical laws'
   d) the meaning of a 'flexible leaping-point scheme'
   e) the phrase 'flexible but permissible substitutions to smooth without violating energy conservation'
   f) the concept of a 'time splitting method to economize CPU time'. What is split here?

Either add references (published in English) for all points a-f) or explain these aspects with sufficient details.
End of middle paragraph: Point out that you use the 'standard Eulerian spectral-transform method', since CAM has another spectral-transform semi-Lagrangian dynamical core.

4) Page 6: acronym AMWG is not explained.
Text on pages 6-8 and captions of Figs. 1-4: be specific in the discussion about the time averaging. Are these 15-year-mean data or annual-mean data (as mentioned in captions). Is it only one annual-mean or the annual-mean of all 15 years?
5) Section 3b: some of the differences in the Held-Suarez experiments (especially the shift in the position of the zonal jets) might not be statistically significant. I recommend using a statistical test to gain confidence in the differences, e.g. according to the significance tests described in
G. J. Boer, B. Denis (1994), Numerical convergence of the dynamics of a GCM, Climate Dynamics (1997) 13: 359-374

Fig. 7a: I do not see any reason why there is an asymmetry in the Eddy heat flux in the IAP model near the tropopause. What is the sampling rate? What is your explanation?
6) Page 10/11, the Neale and Hoskins paper was published in 2000, not in 2001.
Second paragraph: typo in 'Neal' should be 'Neale'
7) Page 12/13, section 3d: I assume IAP is based on the primitive equations. Why don't you use the correct formulation of the transformed Eulerian mean (TEM) equations for the primitive equations as e.g. outlined in

Andrews, D. G., J. D. Mahlmann and R. W. Sinclair (1983), Eliassen-Palm Diagnostics of Wave-Mean Flow Interaction in the GFDL "SKYHI" General Circulation Model, J. Atmos. Sci., Vol. 40, 2768-2784

The current form is the quasi-geostrophic (QG) form. Provide your reasoning why the QG form is chosen and what the consequences of the QG approximation are. Define $\left(\overline{v^*},\overline{w^*}\right)$ as the meridional and vertical residual velocities. The constant 'C_p' should not be capitalized in the definition of κ. There is a typo in the $\nabla \bullet \vec{F}$ definition of $\overline{G}$ (scalar product is missing). The symbols ρ and N are not explained.
Bottom of page 13: 'the' missing before 'upper troposphere'
8) Page 14 and Fig. 11: I find it rather difficult to evaluate the residual stream function, to take the derivatives of it, and virtually picture the residual velocities. I suggest presenting the $\left(\overline{v^*},\overline{w^*}\right)$ velocities directly which simplifies the interpretation.
9) Pages 15/16, section 3e: In the CAM Eulerian model there is also frictional heating that compensates the kinetic energy losses by the 4$^{\text{th}}$-order horizontal hyperdiffusion. Is it accounted for in the analysis and how do you classify this heating? Does IAP have frictional heating? Please present details how the

different diabatic heating terms are assessed? E.g. do you use standard output variables in CAM? If not, be more specific.

10) Typos in references:
    Collins et al. and Hurrell et. al. : capitalize the term ‘Community Atmosphere Model’
    Neale and Hoskins: the year is 2000
    Wan et al.: typo is second author’s name, it is: M. A. Giorgetta
    unify journal acronym for ‘Chinese Journal of Atmospheric Sciences’ , different variants are used
    Zuo: what does PLA stand for?
11) Almost all figure captions of Figs. 1-9: typo ‘counter interval’ needs to be ‘contour interval’
12) Figs 1-4:clarify in captions whether these are 15-year means
13) Caption of Fig. 3: typo K m s-1, use superscript
14) Caption of Fig. 4: typo in physical unit for c), needs to be $m^2 s^{-2}$
15) Figs. 5-8: new figures and analysis are required
16) Fig. 9: it would be informative to see the fields for IAP as well, not just the difference plots. Please add these.
17) Fig. 10: The EP fluxes have different physical units in the two directions. Correct the specification of the units.
18) Fig. 12: this plot needs to be re-plotted with the new HS runs. Does this plot contain the contributions from frictional (diffusive) heating in the dynamical core if applicable?
19) Figs 13, 14: what is the meaning of ‘qrs + qrl’?