Review of the manuscript MWR-D-11-00367:
Sensitivity of Simulated Climate to Two Atmospheric Dynamical Cores: Interpretation of Differences between Dry Models and Moist Models

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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 with its Eulerian spectral transform dynamical core. IAP and CAM3.1 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 and interesting.
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 in the temperature distributions 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, and the interaction between moist physics (clouds) and the dynamical cores. The differences in the climatic temperature distributions seen in full-physics simulations are linked to differences in the Eddy heat and momentum fluxes. The latter are consistently weaker in the model IAP in full-physics, aqua-planet and dry Held-Suarez runs when compared to CAM3.1. This impacts the moisture distribution, and thereby leads to feedbacks in the cloud parameterization schemes and their heating/cooling characteristics.

The manuscript is a revised version of an earlier manuscript and already greatly improved. However, the clarity of the manuscript needs to be improved further before a publication can be considered. The main points are:

1) The weaker Eddy activity in IAP could be related to both its explicit and implicit diffusion mechanisms in the dynamical core. Some more information about the diffusion processes is needed to complete the picture. In particular, what are the chosen coefficients of the second-order diffusion in IAP for both the 128x256 and 180x360 grid point configurations? Which variables (or tendencies) are filtered via the polar filter? Do you use an analytic function to determine the strength of the polar filter? If yes, which one? Is Eq. (9) an implicit diffusion mechanism that reduces the order of accuracy (to first order?) of the longitudinal derivatives as the midlatitudes and the poles are approached? Since there is no explicit diffusion of the momentum equations in Eq. (4), which mechanism diffuses the kinetic energy (KE) to avoid the accumulation of KE at the smallest grid scales? Please also quote the explicit diffusion coefficients used in CAM3.1. Note that the Eulerian spectral transform dynamical core does not apply a polar filter (contrary to the statement on page12, line 10). In addition, CAM3.1 does not directly conserve the total energy (contrary to the statement on page 10 line 21). The total energy is artificially fixed via an energy fixer.
2) The representation of the Eddy statistics in the NCEP reanalysis data set is questionable. The plots (Figs. 3c and 4c) shown for the NCEP data have very different amplitudes than other reanalysis data sets. E.g. the ERA40 annual-mean transient northward Eddy flux of temperature v'T': http://www.ecmwf.int/research/era/ERA-40/ERA-40_Atlas/docs/section_D25/charts/D37_XS_YEA.html and the annual-mean transient northward Eddy flux of westerly wind u'v': http://www.ecmwf.int/research/era/ERA-40/ERA-40_Atlas/docs/section_D25/charts/D33_XS_YEA.html show amplitudes that are about a factor of 2 bigger than the NCEP data. The match between ERA40

and the models is in fact better. I recommend checking your computation of the NCEP Eddy statistics (e.g. how did you remove the stationary portion of the Eddy statistics?). The ERA40 data look more plausible than NCEP. This needs to be clarified.

3) The description of the offline radiation calculation is incomprehensible in the current manuscript (page 19 and Table 1).
4) In order to exclude artificial differences in the model results due to the different vertical coordinates, more technical information needs to be given how the IAP, CAM3.1 and reanalysis data are compared. Are the time-mean zonal-mean data computed on model levels? If yes, are they interpolated to pressure levels before differences are computed? How are the data interpolated (linear? Using pressure or ln(pressure) in the vertical)? Such details can lead to temperature differences in the vertical that might interfere with the analyzed climate variations. Strictly speaking, if the time-means are computed on model levels, information is lost for an accurate interpolation in the vertical since the instantaneous surface pressure is needed at each snapshot. Please comment on such details, and assess whether they matter.
5) The manuscript needs some English language editing (e.g. there are many omissions of 'the' or 'a'). There are too many locations to point them out here. In addition, there are undefined symbols, inconsistencies in the figure captions, etc. I recommend recruiting a native speaker for proofreading purposes.

Detailed comments:

1) Throughout the manuscript: Whenever you refer to the 'Eulerian dynamical core', rephrase it to 'Eulerian spectral transform dynamical core', or provide the exact definition of the 'Eulerian' model early in the manuscript. In general, 'Eulerian' alone is not specific enough since it could also refer to CAM's Eulerian Finite Volume dynamical core.
2) Abstract and introduction: point out that CAM has been developed at the National Center for Atmospheric Research (NCAR). Define the NCAR acronym here instead of page 11.
3) Introduction, first sentence should read: 'The dynamical core of an atmospheric general circulation model (AGCM) refers to … to solve them.' Avoid the first 'different' in the second sentence.
4) Page 3, lines 9 and 10: the acronyms IAP and CAM need to be defined again. Mention the modeling centers here (the definitions in the abstract are independent of the introduction). Already add references for the models here.
5) Page 3, line 14: I disagree with the strong statement that 'convergence has not been achieved'. Dry dynamical cores with low diffusive properties converge at high resolutions (within some uncertainty range), as e.g. shown in idealized deterministic test cases. What is your definition of 'convergence'? It is generally the addition of physical parameterizations and their nonlinear interactions with the dynamical core that lets climate models diverge.
6) Page 4, line 20: '… will also be useful …', omit 'also' at the end of sentence.
7) Page 5, line 20: rephrase ' … (Arakawa and Lamb, 1977). However, the formulation of the governing equations and the finite-difference schemes are different in the IAP model, which contains several …'
8) Page 6, line 12: '… Tibetan Plateau'.
9) Page 6, line 16: what do you mean by 'available energy'? Available 'potential energy' or 'kinetic energy' or both?
10) Page 7, Eq. (4): add the missing equations (hydrostatic relationship and the equation of state). Why don't you need explicit diffusion in the momentum equations or the thermodynamic equation? Specify the value of the diffusion coefficient k_{sa}. This could also be done on page 10 when discussing the two resolutions. Omit the multiplication sign between $\delta \bullet \kappa$ since it looks like a scalar product at first. Use a different symbol for the switch δ since it is easily confused with δ_p. Provide the meaning of the switch δ here instead of page 8. Don't call $\delta = 0$ 'stratification approximation' since there is no approximation involved (correct?). 'Transformed equation set' or something similar seems to be more adequate. Is $\delta = 0$ used in the presented calculations? The quantity $\dot{\sigma}$ is not defined. The definition of

the 'Coriolis parameter' contains parts of a metric term $u\cot(\theta)/a$. This is confusing and if kept needs to be pointed out explicitly (don't call it 'Coriolis parameter' in this case). The 'ctg(θ)' notation is non-standard, use 'cot(θ)'.

11) Page 7, line 13: '… formulae:'
12) Page 8, line 15: misuse of the phrase 'reduced grid' which is defined as the reduction of the number of grid cells near the poles. You mean the convergence of the meridians and the associated reduction of the physical grid spacing in the longitudinal direction. Use 'poleward of ±70°', '±30°-±70°'. Which quantities are filtered?
13) Page 9, Eq. (9): Add the missing 'j' index on the left hand side. Does this provide implicit diffusion or why is it used?
14) Page 9, line 4: '… increases linearly …'
15) Page 9, line 6 & 7: the time steps are meaningless without the specification of the corresponding resolutions. Add this information.
16) Page 9, lines 13 & 14: 'iterative time' should be 'number of steps in the multi-step algorithm'. It should read '… retains short waves while preserving long waves'. What do you mean by 'better computational stability', better than what?
17) Page 10, line 7: Are you using equidistant grid spacings (in degrees) in your latitude-longitude grid? If yes, your grid is not 'the same' as the T85 grid which is a Gaussian latitude-longitude grid with slightly non-equidistant latitudinal positions. Quote the number of vertical levels. What is the time step used for the 1x1 degree simulations?
18) Page 10, line 13, caption should read 'CAM3.1 Eulerian spectral transform dynamical core'
19) Page 10, line 18 onwards: confusing descriptions. The 'dynamical control variables' are 'prognostic variables', 'the 'spectral transform calculation of the quadratic terms' is confusing, quadratic (nonlinear) terms are computed in grid-point space, not spectral space, omit 'in the' (line 20), the Eulerian core doe not conserve total energy. Instead an energy fixer is employed.
20) Page 11, line 3: omit 'respectively'
21) Page 11, line 16: move the explanation of the acronym NCAR to the introduction
22) Page 11, line 18: '… of the model IAP is similar …'
23) Page 12, line 3 and many other locations: 'Student-t test' needs to be capitalized. '… 15 annual-mean simulations. As in most AGCMS …'
24) Page 12, line 10: the Eulerian core does not use a polar filter. The choice of the triangular truncation avoids the computation of short waves near the poles.
25) Page 12, line 13 & 14: 'The distribution of the zonal wind …', '… by the thermal wind …'
26) Page 12, line 18 & 20: '… The two models overestimate the …', 'relative to CAM3.1. The jets in CAM3.1 are …'
27) Page 13, line 5-11: Do you explicitly remove the stationary component of the Eddy statistics?
28) Page 13, line 15: The comparison to the NCEP data might be questionable. The ERA40 data show Eddy activities that are stringer by more than a factor of 2.
29) Page 14, line 5: rephrase 'To contrast with'
30) Page 14, lines 12-17: While it is true that the initial states are forgotten in the HS runs, it might still be important that the two models use initial conditions with the same amount of total mass (globally averaged surface pressure). Is this the case here?
31) Page 15, line 1: specify that the Student-t test is used.
32) Page 15, line 6: I do not see the conclusion supported in Fig. 7 that the westerly jets in IAP are stronger. They appear to be about the same.
33) Page 15, line 14: '… of the maxima are smaller …'
34) Page 15, line 20: What do you mean by 'differ in their surface pressure calculation'? Do you refer to a different handling of the topography?
35) Page 16, line 4: Which SST distribution is used for the aqua-planet study?
36) Page 16, line 15: '… core simulates less …'

37) Page 16, line 21: '… horizontal diffusion processes…'. What do you mean by 'selection of different parameters'?
38) Page17, line 12: '… vertical pressure velocity …'
39) Page 17, Eq. (13): It is not obvious why the $\int_{p_t}^{p_s} \bar{v}_g \bar{\theta} dp$ drops out. Provide the physical reasoning.
40) Page 17, Eq. (14): formatting problem, e.g. subscript 'e' is hidden, overbars are misplaced.
41) Page 18, Eq. (15): formatting problems with the integral symbols, specify the limits correctly. Does the notation $|_{\varphi 1}$ indicate that the integral needs to be evaluated at position φ_1?
42) Page 18, line 8 onwards: clarify that you refer to the full-physics simulations with topography, not the aqua-planet runs
43) Page 18, line 22: symbols 'qrl + qrs' are undefined. What do they mean? Figure 14 needs a more in-depth discussion.
44) Page 19, line 4: the description of the 'offline radiation calculations' is inadequate. I have no idea what was done.
45) Page 19: line 13: '… (12) also contributes to the latent…'
46) Page 20, lines 5 & 6: what do you mean by 'dynamic tendency'. Do you refer to the total tendency dTtot? What do you mean by 'dynamic term'? Is this the total tendency, since it cannot be the 'adiabatic term' dTdyn which you compare to. Very confusing discussion.
47) Page 21, line 3:' … polar regions …'
48) Page 27, table 1: I do not understand Table 1. The meaning of the symbols in the experiment names is undefined.
49) Page 33, Fig. 3: '… by the Student-t test …'. The NCEP data are questionable as pointed out earlier, check your computations.
50) Page 34, Fig.4: check NCEP Eddy statistics
51) Page 38, Fig. 8: you need to point to Fig. 3, not Fig. 5. Add the information to the caption that these are the Held-Suarez experiments.
52) Page 39, Fig. 9: you need to point to Fig. 4, not Fig. 5. Add the information to the caption that these are the Held-Suarez experiments.
53) Page 41, Fig. 11: Add the information to the caption that these are the full-physics experiments with realistic boundary conditions (not aqua-planet).
54) Page 43, Fig. 13: It is unclear what the CLDonly experiment is.
55) Page 45, Fig. 15: provide more information on the experiments and their acronyms
56) Page 46, Fig. 16: add labels a) and b)