Reviewer #3: I am sorry to drag this out. The paper has a very interesting Analysis and conclusion which should be published so I hesitate to reject it flat out. But the experiments are not quite sound. I assume the results would hold up with a clean comparison but unfortunately the current comparison seems even less clean than originally implied with the determination that the time step is different in the parameterizations in the two models. If published as is the work will seem rather careless and questions will remain. I think the authors need to rerun the IAP simulation with 1200s time step for the parameterizations. In their response to reviewer 3 they state that IAP model and CAM model have been tuned. But it seems like they have not tuned the parameterizations in IAP, they just used what they thought were standard CAM values. In the paper they continue to refer to the same CAM3.1 physical parameterizations being used, e.g. line 6, pg 2, line 12, pg. 4.

I am not sure why the authors are so hesitant to include plots of the KE spectra in the paper. I would expect that spectra plots would back up their conclusion based on the eddy transports that the IAP model is more diffusive. The spectra plot they included in their response does not actually back this conclusion, but there is something fishy about this plot. Either there is something wrong with their implementation of CAM or there is something wrong in their spectra calculation, the latter I assume. I have never seen a plot of the spectra from the standard spectral transform CAM that did not follow the -3 line. Their plot seems to tail off much faster. While I have seen these plots from CAM often enough, they don't seem to be commonly published although that -3 structure is described in words in the CAM documentation of the choice of diffusion coefficient. I have found two plots of CAM spectra in the book: Lauritzen, P. H., C. Jablonowski, M. A. Taylor and R. D. Nair, Eds.: Numerical Techniques for Global Atmospheric Models, Lecture Notes in Computational Sciences and Engineering (Tutorials), Vol. 80, Springer: Fig. 14.6, pg 504 of Skamarock's paper and Fig. 13.4, pg 411 of the Jablonowski and Williamson chapter.

The authors have only made cosmetic changes in response to the last review. They changed the title of the paper to refer to "two atmospheric models" but apparently left the term "two atmospheric dynamical cores" throughout the paper. I can live with the term dynamical cores. But I would prefer "same resolution" be changed to "same number of grid points" or something like that, resolution remains too vague, especially when comparing spectral and grid point models.

I recommend a major revision to rerun the IAP model with parameterizaton time step of 1200s to match CAM, and a redo of the analysis using this run. I assume the conclusions will be the same, but only by doing the run and analysis will we know for sure. Otherwise it is speculation.

Some other minor points came out in my rereading of the paper that I missed before.

Lines 8-12, pg 3, when describing the discretization methods, the authors state that the control variables of air motion are different in the two cores, describing each. This implies that this is the major difference. I actually think the spectral versus grid point is more significant.

lines 20,21, pg 4. it would be nice in the reference to past and current generations of cores to parenthetically list which is which, i.e. I guess they intend past (IAP) and current (CAM). The reason I suggest this is that my first thought was that CAM3.1 actually uses a rather dated spectral scheme from the past and I think many numerical people would think that as well.

line 11, page 27, "shap-preserving" -> "shape-preserving"