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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of prescribed SSTs on climatologies and long-term trends in CCM simulations" *by* H. Garny et al.

Anonymous Referee #1

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Review of ACPD manuscript 9-4489-4524-2009 Impact of prescribed SSTs ... by Garny et al.

This manuscript describes two simulations of present day climate that use different boundary values of Sea Surface Temperature (SST). The model used is well documented in other papers, and the nature of the simulations is identical to that prescribed in SPARC/CCMval.

The main result of this study is that two simulations with the same identical model but different SST produce similar overall climatology but different trends when examined on shorter time spans (Fig. 9).

The main concern I have about this study is that a result like Figure 9 could be mislead-

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ing if not supported by additional information. I wonder if using another SST, one that is not consistent with the physics of that model, is in fact the cause of this pathology. I am not aware of how this model was tuned in the first place, but I know that when a model is put together the clouds, convective parameterizations, hydrological cycle etc are all tuned with some input dataset. Maybe the model used in this simulation in concert with one of the two SSTs is bringing the model climatology off track, and without proper re-tuning it gives rise to those different trends. To me it looks like that in one case the model is trying to compensate for an internal balance in the first couple of decades and then it finally catches up with the expected trend. Have the authors verified that global energy budgets (in/out, net, top/bottom) are correct and comparable in the two simulation? I think this is extremely critical and once this is properly shown I think the results would be much more robust. also, the authors need to indicate with supporting evidence from observations which trends are realistic.

Until then I cannot recommend the manuscript to be published.

Detailed comments:

Page 4495. The term on the rhs of the Xˆ2 expression should be squared.

Figure 2. How many ENSO are forced from each dataset? The global averages are useless in order to assess actual and relevant variability. Suggestion: plot NINO3.4-like index.

Figure 3. Ozone has such a small concentration in the troposphere that even statistically significant anomalies below ˜100 hPa are questionable since they apply to near zero values. I think water vapor would be a much more useful constituent, both as a tracer in the UTLS region (tape recorder) and in the troposphere to highlight regions of enhanced/inhibited tropical convection. In fact, I would be curious to see the tape recorder in the two simulations.

Figure 7 and 8. Page 4504, line 4. How do you reconcile this statement with the error

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bars that are overlapping in most cases?

Figure 10. This analysis is predicated on the assumption that stationary PW are the only players. The authors are neglecting the transient components.

Page 4506, line 23. Why? Transient PW are not prevented from propagating into the summer easterlies.

Page 4507, lines 1-8. The chicken and the egg problem. Are the changes in EP resulting from different zonal mean zonal winds, or are the the waves in fact causing the changes? the question cannot be answered easily without further analysis.

Page 4509, line 5. I don’t understand, aren’t the two simulations set up identically for GHGs, the only difference is the use of the SST dataset.

Conclusions. What is the practical implication of this work? Should we stay with observed SST and avoid fully coupled models? Or these result suggest that the use of SST/sea-ice data sets coming from other models is to be avoided?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4489, 2009.