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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Description and Evaluation of the Specified-Dynamics Experiment in the Chemistry-Climate Model Initiative (CCMI)" *by* Clara Orbe et al.

Anonymous Referee #3

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In this paper the authors provide a broad-brush comparison of a set of "specified-dynamics" (SD) simulations of chemistry climate models (CCMs) with the underlying reanalyses used to drive the SD simulations as well as the models' free-running versions. They concentrate on differences either due to: 1) different underlying "dynamics" (reanalysis product), 2) different nudging methods, 3) model biases of the underlying GCM. They find that in particular meridional and vertical winds show substantial differences, which also affects tracer distributions. These differences come about primarily due to differences in implementation of the SD method and are found to be of comparable magnitude as differences among to underlying GCM (free-running model versions). The most important take-home message is therefore that analyses of output by these

SD simulations should carefully take into consideration the discussed issues due to implementation of the SD method.

This is a very valuable contribution and I presume will be a core reference for those working with output from SD simulations. Given the wide scope, the analyses necessarily lack detail, e.g., by taking latitudinal averages, but to me this is fine. The point of this paper seems to be less a thorough analysis of all the potential issues with the SD simulations but rather to provide a helpful starting point for those wishing to analyse output from the SD runs. The paper is well written and I only have minor comments.

Detailed comments:

page 1, line 2: "Here" could be removed

page 10, line 20: "Next" seems a strange way to start sentence here; how about "We therefore also compare ..."

page 10, line 21: I don't quite follow this argument for restricting the average to 60S-60N. If you do the usual cos(lat) area weighting then grid points near the pole are naturally de-emphasised. If there really are issues with a few grid points then restricting it to ∼85S-85N would have been more reasonable. The promised discussion about the sensitivity of choice of latitude bounds at the end of this paragraph (section 5) is a bit cursory (in section 5 you basically state that you've looked at the latitudinal distribution and it looks okay – that's probably not the kind of sensitivity analysis that most readers would expect).

page 12: I would find it helpful if you included a bit of discussion on the sources of interannual variability (e.g., due to ENSO, QBO)

page 13, line 22: "not intuitive and" could be removed

page 13, line 25: "worse" -> "greater"

page 13, line 28: "including" -> "included" page 15, line 6: "better interannual variability" – do you mean "more realistic interannual variability"?

page 17, line 6: "... when inferring dynamics-tracer relationships" – it may be important to clarify that this refers to impact of dynamics on tracers, but not the other way around ("relationship" suggests it could go both ways) ...

page 17, line 23: "including" -> "included"

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