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Comment

***Interactive comment on* “The atmospheric chemistry general circulation model ECHAM5/MESSy1: consistent simulation of ozone from the surface to the mesosphere” by P. Jöckel et al.**

Anonymous Referee #3

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General comments:

This paper aims to be a documentation paper, establishing the accuracy of ECHAM5/MESSy1 in simulating chemical climate from the surface to the mesopause. The strategy taken is to nudge the model to tropospheric meteorological fields during the time period 1998–2005, which allows a more precise comparison with observations than would be possible with a free-running model.

The strength of this approach is that the validation of chemistry and radiation where the

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model is not constrained to observations (the whole domain for chemistry, the middle atmosphere for radiation) is more precise than would be the case with a free-running model, because transport and temperature errors are reduced. The weakness of the approach is that transport and temperature are constrained by observations, so this is not a validation of ECHAM/MESSy1 as a predictive CCM. Moreover, the vertical resolution is likely to be rather higher than would be affordable for an ensemble of CCM predictions, and we know that the representation of transport and trace species depends on vertical resolution, so once again this is likely not a validation of the version of ECHAM/MESSy1 that would be used for climate simulation. None of this is really discussed in the paper; rather, what we have is pretty much a description of the model fields and the extent to which they agree with observations, with essentially no analysis. (The whole point of CCMVal was to move beyond this sort of thing.) The paper needs to be much clearer about what its purpose is, and what it does and does not do in this respect given the chosen methodology.

The descriptive nature of the paper is somewhat problematical. Normally this sort of thing is not considered publishable: where is the new scientific result? Now I am certainly sympathetic to the need to document models, and provided the extent of the validation is made clear (see above paragraph), then this does serve a useful purpose as a reference for subsequent papers. Unfortunately this is not the criterion for an original publication. Yet there is in fact a very interesting scientific result here, namely that the representation of stratospheric temperature and transport seems to be quite strongly constrained by the troposphere. This suggests that many of the systematic errors in free-running CCMs are associated with a poor representation of tropospheric climate, rather than with e.g. gravity-wave drag. I find this quite surprising and interesting. So, I would urge some development of this line of thought. With so many figures already it may seem perverse to ask for more, but it would strengthen the scientific content of this paper if some kind of analysis was presented along with the basic fields. Even a few additional figures could make a big difference. (There is no page limit in ACP, right?)

One example would be global-mean temperature vs altitude in the stratosphere. Austin et al. (2003) and Eyring et al. (2006 JGR in press) find that CCMs exhibit substantial differences in this diagnostic, which should be radiatively determined. It would seem surprising if ECHAM/MESSy1 constrained in the troposphere can do better than free-running ECHAM/MESSy1, except perhaps the lowest part of the stratosphere, but it would be interesting to see how far up the influence of the troposphere extends for global-mean temperature.

Another example would be a plot of wave flux into the stratosphere vs polar temperature. I appreciate that 8 years is not many data points, but the comparison is with specific years so it should be OK. The nature of the biases in CCMs are quite evident from this diagnostic (e.g. temperature/gravity wave drag or planetary wave forcing), so it would be interesting to see how the tropospheric constraints improve this diagnostic for ECHAM/MESSy1.

Specific comments:

p. 6958, line 6: I don't know why people persist in misrepresenting horizontal resolution in spectral models. A T42 spectral representation contains exactly the same amount of information as 42 latitudes by 84 longitudes (the so-called linear transform grid). That corresponds approximately to 4 degrees, not 2.8 degrees. Of course a quadratic transform grid is used for the nonlinear terms, for accuracy, but it is subsequently truncated back to T42 which is equivalent to the linear grid. Sometimes the physics is performed on the quadratic grid, but that's a waste of time as the information is lost when transforming back to the spectral domain. So the real information content of a T42 model is equivalent to 4 degree resolution. The NWP community represents resolution this way; we need to be honest and follow suit.

p. 6959, line 4: The stratospheric circulation is driven by wave drag, not by radiative heating. Read Holton et al. (1995).

p. 6959, line 6: With chemistry, the models should be called CCMs, not GCMs. GCMs

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are generally not regarded as including chemistry.

p. 6968, lines 10-19: It would be helpful to explain at this point why simulation S2 was performed. Otherwise this is quite obscure.

p. 6973, line 22: First, the comparison should not be to Austin et al. (2003) but to Eyring et al. (2006 JGR in press), because the temperature biases are somewhat improved in the current generation of models and you need to be up to date. Several of the authors of the present paper are authors on Eyring et al., so there is no excuse for not knowing about this. Second, I am not convinced that the biases are really that small; there seem to be quite a few significant biases in the polar regions.

Figure 5: Explain why S1 is shown for DJF but S2 for the other seasons.

p. 6974, lines 13-15: Unless I am missing something, this sentence is content-free: what else could it be?

p. 6991, lines 6-7: You say that the downward transport is too weak in the Arctic. I would like to see some discussion of the consistency of this with the temperature bias (which was stated to be small, though I'm not so sure).

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