Atmos. Chem. Phys. Discuss., 8, S3884–S3885, 2008
www.atmos-chem-phys-discuss.net/8/S3884/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantitative performance metrics for stratospheric-resolving chemistry-climate models" *by* D. W. Waugh and V. Eyring

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Received and published: 18 June 2008

I am not fond of the idea of applying a rigid metric to chemistry-climate modelling results for the purpose of ranking the models. The reasons for this are as follows:

1. The results depend strongly on the choices of parameters that are made, and the weights that these are given; the authors acknowledge this. So hence when defining a metric there is a potential for endless, futile debates over the details of the metric which are fruitless.

2. Assuming that in advance of a model intercomparison campaign a metric of grading has been agreed, owners of the participating models will take a close look and

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will feel a certain pressure to include non-physical fixes in their model to improve the grade their model will attain. Examples for this, known from the literature, are artificial changes to photolysis rates to compensate for transport deficits, unrealistic boundary conditions for certain species, or other such inventions. While technically improving the correspondence of the model compared to observations, such fixes might devaluate predictions based on the model. The metric will fail to take such subtleties into account.

My view is that the present situation, where all participating models are included in overview papers and published model descriptions and hindcast results guide authors in subjectively assigning confidence levels to the model predictions, is not as untenable as Eyring and Waugh suggest. I agree that there are models that are clearly inferior to others but using a mathematical metric may just produce a false sense of objectivity when human judgement is still needed.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10873, 2008.

ACPD

8, S3884–S3885, 2008

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