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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Stratospheric temperatures and tracer transport in a nudged 4-year middle atmosphere GCM simulation" *by* M. K. van Aalst et al.

**Anonymous Referee #2**

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The strength of this paper is that the results are presented quite clearly. However, there are a few weaknesses that need to be addressed before the paper can be published. I will first give a brief description of what my concerns are and will then list my point-to-point comments.

Major concerns: 1. The purpose of nudging only the free troposphere and not the entire atmosphere is not given. As a stand alone paper, it is important to talk about why this approach is necessary. What are the advantages of doing this in comparison to nudging the whole domain? What are the specific concerns nudging the whole

atmosphere (from the perspective of stratospheric transport, etc.)? Have any previous studies showed that it is beneficial to do this?

2. The paper compares the GCM results with observations but does not adequately addresses the cause of the discrepancies. For example, when presenting the results of the discrepancy in the timing of the March 2000 warming, deeper exploration and discussion is needed. One simple thing to do is to look at diagnostics of planetary wave activities and use that to explain the results. Manney et al. (2003), which is quoted in the paper, contains some diagnostics (momentum flux from the troposphere, etc) that should be looked at. Only with these diagnostics can the reasoning in the paper be supported.

3. When comparing model results with satellite observations, colocated data should be used. It is not very informative to compare avarage data at fixed GCM latitudes to observations along satellite. It should not be hard to simulate the satellite track over your data set and thus pick up colocated data. The paper does indicate this as a problem but nothing was done to address the issue. Comparing colocated data should be a standard practice, especially when model data is available globally and at all time. It's not a big issue to lose some information on seasonal cycle in model results in order to compare "apple to apple."

Here are the specific comments: 1. Line 22, page 963: "The QBO nudging allows to..." should be "the QBO nudging allow us to..."

2. Line 23, page 963: what is the vertical resolution?

3. Line 22 to line 26, page 964: Need to rewrite this part. Try something like "it is desirable to limit the nudging to the smallest extent possible to prevent artificial tendencies from masking the model errors. This is also because in some cases the model modes may..."

4. Line 10, page 965: delete "for the previous model version (MAECHAM4)."



**ACPD**

5, S263–S267, 2005

5. Line 10, page 965: change "the latter" to "that version."

6. Line 16 to line 18, page 965: How many vertical levels? What is the vertical span?

7. Line 12 to 13, page 966: change "and filtering out all components faster than 24h" to "and filtering out all components wiht period shorter than 24-hour."

8. Line 21, page 966: "the low vertical resolution", again, what is it?

9. Line 23 to 25, page 966: The word "assimilation" is used for the nudging of the QBO. I suggest that this be changed to "forcing" or just "nudging". This is not assimilation in the standard sense. This is especially so since you artificially extends the data up to 3 hPa.

10. Line 21, page 967: change "tropopause diagnostics from" to "tropopasuse height calculated using..."

11. Line 10, page 968: delete "also" in "particularly also."

12. Line 1, page 969: change "have been released" to "were released".

13. Line 2, page 969: delete "have been".

14. Line 25, page 970: is MA-ECHAM5 MAECHAM5/MESSY?

15. Line 11 to line 23, page 972: this is an example where a concrete diagnostic, say EP-flux analysis, should be used to back-up the claim/explantion. This is not done in the discussion session.

16. Line 9 to line 15, page 973: Another example where more concrete explanation and analysis are needed.

17. Line 18, page 973: change "is smaller than of" to "has a smaller amplitude than that in ECMWF."

18. Line 20, page 973: should be 5K.



**ACPD**

5, S263–S267, 2005

19. Line 10 to line 11, page 974: "it is higher in the northern summer and very small in winter; at higher altitudes, it is ..." How about "the difference is largest in northern summer and very small in northern winter. At higher altitudes, ..."

20. Line 23, page 975 to line 10, page 976: need to use colocated data when compared with HALOE.

21. Line 15 to line 16, page 976: it is claimed that ther reason of the quantitative comparison is "too high tropopasue and a too weak vertical gradient". It seems to me that the weak gradient itself is a symptom that need explanation.

22. Line 17 to line 27, page 976: again, no in-depth explanation is provided here or in another section about why the descent rate is good at some altitudes and nad at others.

23. Line 14, page 977: it is claimed that "the match is very good." The question is, what is your standard? How good is very good? You have not shown any statistical measures or even simple percentage error in any figure. I believe the comparison need to be more concrete. Otherwise, the presumably "quantitative" comparisons are really "qualitiative."

24. Line 13 to line 19, page 978: again, the failure of the model is not adequately explained. Also it seems that the H2O results are worse than the CH4 results. Why is this so?

25. Line 7 to line 8, page 979: "and may partly be related to the model's relatively coarse vertical resolution, ..." You can easily verify this by running the model with a different vertical resolution. The problem with quoting only results from other studies are that many of these results are model-dependence and can not be generalized too much.

26. Line 15, page 979: delete "for".

27. Line 15, page 979: change "ones" to "biases."



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5, S263–S267, 2005

Interactive Comment

28. Line 19, page 979: change "for too weak gradients" to "for gradients that are too weak."

29. Line 20, page 979: change "a too high tropopause and too weak gradient" to "a tropopause that is too high and vertical gradient that is too weak."

30. Line 12, page 980: change "would be" to "is".

31. Line 13, page 980: delete "however".

32. Line 12 to line 15, page 980: This statement about diffusive tracer advection is not necessarily true. It depends on the nature of the excess of horizontal mixing in yout model. Is it due to instability or is it diffusive in nature? This may explain your test with various schemes.

33. Line 28, page 980: use "nudging" instead of "assimlation."

34. Line 23, page 982: use "nudging" instead of "assimilation."

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 961, 2005.