**Geoscientific
Model Development
Discussions**

Interactive comment on "A quasi chemistry-transport model mode for EMAC" *by* R. Deckert et al.

R. Deckert et al.

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Dear Dr van Noije,

thanks a lot for your review and we are glad you find the manuscript suitable for publication after minor revisions. Would you please see below how we have incorporated your very helpful comments into the manuscript.

With kind regards, Rudolf Deckert (on behalf of all Co-Authors)

i. In the introduction the authors explain the problem of diverging meteorological states in coupled simulations and how this interferes with the analysis of responses to chemical perturbations. The QCTM is proposed as a means to simulate identical meteorological sequences despite the perturbed chemistry. Another well-known way to produce

similar sequences of meteorological variability is 'nudging'. In fact the results presented in this paper are themselves based on simulations in which the tropospheric dynamics is nudged toward ECMWF reanalysis fields. I therefore recommend including a short discussion of the nudging method and of its limitations compared to QCTM.

Good point and something that requires some discussion among the authors. Some of the co-authors have not yet come back from vacation and I'm going to be unavailable until January 18th. It would be great if you were so kind to accept a delayed final answer. I assume that the nudging reduces the noise associated with unequal dynamical states and tracer transport (would you please see also our answer to question x), but is insufficient to suppress it fully as this would require nudging coefficients of 1 throughout the model domain. The usefulness of the nudging, in contrast to that of the QCTM, would hence depend on the signal strength.

ii. In section 4.2 the authors state that the differences in the simulated mesospheric temperature climatologies obtained in fully coupled and uncoupled modes (Fig. 5a) may be due to differences in the simulated resp. prescribed ozone climatologies. The latter is based on the ozone climatology by Fortuin and Kelder (1998), which according to the original paper extends only up to 0.3 hPa. The authors should clarify in the text how they have extrapolated this ozone climatology to the top of their model (0.01 hPa) and to what extent the chosen method has affected the simulated temperature differences in the mesosphere.

We have modified the O3-related row in Table 1 into: "O3 below 0.3hPa level: climatology by Fortuin and Kelder (1998) (newline) O3 above 0.3hPa level: about $1.6 \times 10-6$ mol/mol". We have also amended the relevant text passage: "The mixing ratios of CO_2 are uniform, while those of O3 are taken from Fortuin and Kelder (1998). The O3 climatology by Fortuin and Kelder (1998) extends up to the 0.3hPa pressure level, well below the highest model layer which is centered at the 0.01hPa pressure level. For the model layers above the 0.3hPa pressure level, we use height-constant O3 mixing ratios from the uppermost layer of the climatology, about $1.6 \times 10-6$ mol/mol, the exact

value being latitude dependent. The result is significantly lower mesospheric O3 concentrations compared to the online-calculated O3 concentrations (see Figure 6b), and it must impact on the shortwave radiative heating (e.g. WMO, 2007)".

iii. The captions to figures 6 and 7 have been interchanged. Also the references to these figures in section 4.2 have been mixed up.

Thanks, done.

iv. In section 2 it is mentioned that "two blocks in North-South direction are combined for an improved load balancing with respect to the distribution of day and night grid boxes". Can the authors please clarify in the text how this is done?

We have changed the relevant sentence into: "For each variable in Gaussian representation two blocks at the same longitude and the same latitude are combined, with one block being in the Northern and one block in the Southern hemisphere".

v. In section 3.1 the five options for I_H2O_TENDENCY are not well explained. For instance, why is it mentioned for all options that the H2O tracer is defined? It is also not clear if the tracer is always defined in the whole atmosphere or for some options only in the stratosphere and mesosphere. If the first is the case, it should be mentioned how the initialization is done in the troposphere (from specific humidity). Further, for the option "I_H2O_TENDENCY=1" an offline oxidation source is not "added" but is replacing an existing representation. The authors should also better explain how the synchronization takes place and clarify when the H2O mixing ratios (tendencies) are updated from the specific humidity (tendencies) or vice versa. As for QCTM only the option "I_H2O_TENDENCY=2" is relevant, the description of the other options is better placed in an appendix. In any case, I propose to rewrite this part and clarify the above.

Good point which I can't resolve alone. Some of the co-authors have not yet come back from vacation and I'm going to be unavailable until January 18th. It would be great if you were so kind to accept a delayed final answer.

vi. The ordering of the subsections of section 3 is not logical. It makes more sense to start with the submodels RAD4ALL and H2O, and then describe HETCHEM and PSC.

Thanks a lot for the suggestion which we have implemented.

vii. In my opinion subsection 3.4 is excessively long given the little attention on PSC coupling in the evaluation section. Please consider shortening the description of the PSC submodel and leave out any details that are less relevant for understanding the chemistry-climate couplings.

Would you please see the shortened subsection 3.4 in the revised manuscript.

viii. In section 4.1 the authors state that the anomalies "tend to spread farther for the monthly averaging". I have the impression that this is a wrong interpretation caused by the difference is contours between figures 3b and 3d. Please clarify.

Thanks, we inspected our data more closely and have deleted the statement.

ix. In section 4.2 it is mentioned that CFC-11 and CFC-12 are treated differently. Please explain the reason for this.

It happened by accident that the mixing rations of CFC-11 for the coup_full simulation became offline. However, this does not affect our conclusions as the main cause for the temperature differences between coup_full and decoup_full is due to the prescription of unadapted offline fields of radiatively active substances. There is hence a strong dependence on the respectively used offline fields. We have added the following text passage into the manuscript: "The mixing rations of CFC-11 for the coupled setup became offline by accident, but it does not affect our findings and conclusions presented later."

x. In section 4.2 it is stated that "the removal of non-H2O radiative feedback artificially compensates for the tropospheric deviation in temperature". Please clarify to what extent the differences in tropospheric temperatures between (fully or partially) coupled and uncoupled simulations are also reduced by the applied nudging.

In our opinion, the nudging reduces the noise, but does not impact on the temperature signal. The caption to Table 1 states that "It (the nudging) forces the simulations to produce a similar sequence of variability, which is necessary due to the presence of feedbacks between chemistry and dynamics in the simulations coup_full, coup_PSC, and coup_rad. The nudging reduces the noise in the simulation inter-comparisons and makes these better fulfill the normality assumption inherent to the paired t-test (Fomichev et al., 2007)".

Some additional minor technical points:

1. In the abstract (line 10) change "clouds (b). Offline" to "clouds (b), and offline".

Done.

2. In the abstract (line 18) there seems to be a double space before "Toggling".

Thanks a lot for the intensive reading and the large space seems to be due to the latex text compilation.

3. Page 2191, line 11: change "After a few time steps" to "After a number of time steps".

We have modified the relevant text passage into "After a small number of time steps ..." in order to express that the two simulations diverge very quickly.

4. Page 2191, lines 15 and 18/19: change "statistical expectation values" to "time averages".

Done.

5. Page 2191, line 15: change "are hardly" to "may be hardly".

Done.

6. Page 2191, line 28: change "latter" to "the latter".

Done.

7. Page 2191, line 28: change ", or," to "or".

Done.

8. Page 2192, line 20: remove quotation marks around "setup".

Done.

9. Page 2193, line 9/10: Without further information it quite meaningless to mention the number of submodels a model consists of. Please remove this sentence.

Done.

10. Write either "sub-models" or "submodels" throughout the paper.

We decided to use "submodels" consistently throughout the paper.

11. Page 2194, line 21: The "CPL namelist" is introduced without explanation what "CPL" stands for.

Good point which I'm unable to resolve. Some of the co-authors have not yet come back from vacation and I'm going to be unavailable until January 18th. We would appreciate a lot if you were so kind to accept a delayed answer.

12. Correct "heterogenous" to "heterogeneous" (several times).

Done.

13. Page 2196, line 8: according to Table A1 "L_COUPLE_H2O=2" should be "L_COUPLE_H2O=F".

You are right, thanks, and we have corrected this mistake.

14. Page 2196, line 17: correct "CFC-l2" to "CFC-12".

Done.

15. Page 2199, line 25/26, "Yet, the water vapor tendencies remain unaffected": I

presume the author mean here the "specific humidity tendencies" in the ECHAM5 core model. Please clarify.

Yes, we meant the "specific humidity tendencies" and have modified the text accordingly.

16. Section 4 (page 2200, lines 13, 15 and 22; page 2201, lines 2, 4 and 9); caption to figure 3 (lines 2 and 7): Please add "simulation" after "sensitivity" and "reference".

Done.

17. Page 2200, line 22: change "the perturbation" to "the perturbation in emissions".

Done.

18. Page 2201, line 5: change "constant factor" to "constant percentage".

Done.

19. Page 2203, line 17: change "would increase" to "would have to increase".

Done.

20. Page 2204, line 26: change "CH4 degradation plays a minor role there" to "CH4 degradation plays a minor role in the water vapor budget there"

Done.

21. In the conclusion (line 14), change "quasi chemistry-transport mode, QCTM" to "quasi chemistry-transport model mode, QCTM".

Done, thanks.

22. Page 2207, line 6: change "both, QCTM and " to "both QCTM and".

Done.

23. Page 2208, line 13: remove "poor-in-noise" (unknown expression).

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Done.

24. Table 1: Please specify the ECMWF re-analysis that was used.

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Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/3/C665/2011/gmdd-3-C665-2011-supplement.pdf