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Interactive Comment

Interactive comment on "Earth System Chemistry Integrated Modelling (ESCiMo) with the Modular Earth Submodel System (MESSy, version 2.51)" *by* P. Jöckel et al.

P. Jöckel et al.

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We thank referee #1 for the very helpful and encouraging comments. Here are our replies:

- *The paper P. Joeckel et al. gives an overview of the CCMI experiments using the ECHAM/MESSy model. Altogether, the authors have done a great job in summarizing the configurations and setup of the experiments. There is a lot of detail that will be very useful to various readers. Besides summarizing technical aspects of the model, physical parameters, and capabilities of the model are* *summarized. Comparisons to observations are performed to demonstrate the general performance of the different configurations of the model. This paper is an important paper for the community and should be published in this journal. A few aspects of the paper could be improved to make it easier for the reader get the required information.*

Reply: We thank the referee #1 for these positive comments.

- *This paper currently addresses at least two different types of readers, those who want to run the ECHAM/MESSy model themselves and need to understand how to do this, and others, that are interested in performing multi-model comparison studies based on the results of this model. Section 2 is mostly of interest to the first group of readers. It is very technical and is mostly concerned with the model structure and less with the science. An overview of the physics and other details are described in Section 3. My feeling is that the second group of readers is not interested in the details in Section 2, and readers that want to just know how the model works would be less interested the remaining part of the paper. I would suggest moving Section 2 to the supplement, or to a separate technical report.*

  Reply: The referee is right that different types of readers are addressed. Nevertheless, we are hesitating to move Section 2 away, for several reasons: (1) The described updates are important for the correct interpretation of the results in view of earlier results with previous versions of the model (the short section only lists the modifications), (2) this short technical section on model documentation is well suited for GMD(D) and would be too short for an own technical report, (3) it is important to document (repeatedly) the specific, modular structure of our system, which we believe is unique, (4) moving this part into the supplement (implying the shift of the corresponding citations) will deny those authors the proper credits.

  Nevertheless, in the revised version, we rephrase in the introduction to "In this manuscript Section 2 documents **briefly (mainly for the users of it)** the updates

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of the Modular Earth Submodel System (MESSy) and EMAC since . . . ”, so that readers not interested in the details could skip Section 2.

- *The discussions on different experiments and comparisons to observations are very comprehensive, however, sometimes difficult to follow. Less detail and figures and focusing on important results could improve the paper. The main problem to me was the naming of the different experiments that are not intuitive, and even reading the whole paper, I always had to go back and recall the specifics of the experiments. I would recommend changing the names of the experiments to make this more obvious, or improve Table 1 that summarizes the specifics of the experiments. Instead of little footnotes, it may be easier to have a row for each experiment and have the columns covering different categories, like vertical resolution, nudging, etc. To further guide the reader, it would be helpful in the text to point more often to the colors that are used to represent the different experiments so one easily identify differences in the plots. Sometimes it seems like difference between observations and models are discussed that may not be significant. It would be also helpful to give more explanations for the deviations between models and observations.*

  Reply: Indeed, given the large amount of results we obtained, the comparisons and discussions are comprehensive. Unfortunately, we need to refrain from renaming the different simulations, for several reasons:

  – The used labels are part of the output data file names and stored in the netCDF meta information (global attributes). Using different names here would require another table referencing the new names to the simulation data. This would be even more confusing for data users.

  – We have already additional manuscripts submitted (one has already been accepted), which refer to the present manuscript using the current simulation names.

- A renaming would require a recreation of all figures!

As it is now, the complete information is contained in Tables 1 and 2, although condensed to the minimum required information. We accept, however, that Table 1 could be improved as suggested by the referee and we will do so in the revised manuscript. The revised Table 1 will also include the line colours used in the figures.

The suggestion to point to the colours from within the text is well taken. We will do so for the revision.

Further, we will recheck for insignificant results.

And last, but not least, we will check again, if we can easily give more explanations for deviations between model results and observations, although we want to point out that our model would be perfect, if we had those explanations.

- *Finally, many different experiments have been performed. If all of those get submitted to the archive, the readers are left with making their own choices on what simulations to use for their analysis. Therefore for the conclusions, it would be very helpful if recommendations would be made on what experiment should be used in a multi-model comparison study for each reference experiment. Those conclusions can be made based on the comparisons to observations. For example, would be helpful to point out, if the 90L vs. 47L version should be used, or for what purposes the one or the other is preferable.*

  Reply: This is indeed a very good point, which we completely overlooked. We will add a small paragraph to the conclusions:
  "**For inter-comparison with observations we recommend to use the results of the nudged simulation with all corrections, i.e., *RC1SD-base-10a*. The simulations results of *RC1SD-base-07* and *RC1SD-base-08* should be used with caution, due to the large impact of the global mean temperature nudging, for which no specific parameter re-optimisation for the radiation bal-**

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ance has been undertaken yet. Such an optimisation will certainly alter the hydrological cycle, i.e., clouds and convection, and with it also the lightning NO_x production. Studies for which the specified dynamics (nudging) is not desired, e.g., on trends and frequency distributions, are best based on the results of the free running simulations with 90 level discretisation. Nevertheless, any inter-comparison to those with 47 levels is also desirable, in particular since the simulation with coupled ocean model was performed with 47 levels in the atmosphere. ”

- *Introduction: Line 15: What about the chemical mechanism, are there more details later, please point to later sections.*

  Reply: Unfortunately, it is unclear, how this information would fit into “*Introduction: Line 15:* ”. It is unclear what you are referring to here? Note that the complete chemical mechanism is part of the supplement.

- *Page 8644, Line 6: How long was the spin-up of the ocean, maybe refer to section 3.5.5?*

  Reply: Again, this statement is unclear. We refer already to Section 3.5.5 here, which is, however, about the “Initial conditions of trace gases”. For the spin-up procedure of the simulation with coupled ocean, we refer to 3.11 in line 25 of page 8644.

  In any case, we rephrase to “All simulations (except for those with specified dynamics, SD) start in January 1950 to have a 10 year long spin-up period (1950–1959, initialised from already spun-up states of previous simulations, see Section 3.5.5). The simulation with coupled interactive ocean (*RC2-oce-01*) was spun-up in a two-stage procedure over 500 years in total (see Sect. 3.11 for details).”

- *Page 8645: Line 22. What TOA balance are you aiming for? Are these tests done for present day? How much do you think, will the non-interactive chemistry change those tuned parameters?*

  Reply: The test simulations are performed to achieve a global, annual average equality of the net incoming SW radiation with the outgoing LW radiation at the uppermost model level (i.e., top of the atmosphere, TOA). The test simulation was performed under conditions for the year 2000 for the GHGs, ODSs, and SSTs, SICs (10 year average of the HADISST monthly SSTs and SICs between 1995 and 2004). Comparing the TOA balance of the L47 simulations with interactive chemistry, reveals an annual, global average from 1995-2004 of -0.26 $\mathrm{W\,m^{-2}}$ and 0.41 $\mathrm{W\,m^{-2}}$ for *RC1-base-08* and *RC2-base-05*, respectively.

  Comparison to the test simulation without interactive chemistry (0.1 $\mathrm{W\,m^{-2}}$) shows that these values are still in the range of $\pm$0.5 $\mathrm{W\,m^{-2}}$, only slightly larger than the uncertainty range from observations. Stephens et al. (2012)[1] give an estimate for the TOA radiation balance of 0.6 ($\pm$ 0.4) $\mathrm{W\,m^{-2}}$ for the decade 2000-2010 derived from satellite observations.

  We will include this information in the revised manuscript.

- *Section 3.5.1. How many reactive species are in the mechanism? How many reaction rates?*

  Reply: This is documented in detail in ESCiMo_MECCA_mechanism.pdf, which is part of the Supplement. Nevertheless, we will add the numbers to the revised text: on page 8650, line 21 ("In total, the mechanism is described by 310 reactions of 155 species.") and same page, line 24 ("...contains additional sulphur reactions (5 additional species and 11 additional reactions).").

[1] Graeme L. Stephens, Juilin Li, Martin Wild, Carol Anne Clayson, Norman Loeb, Seiji Kato, Tristan L'Ecuyer, Paul W. Stackhouse Jr, Matthew Lebsock, and Timothy Andrews, An update on Earth's energy balance in light of the latest global observations, Nature Geoscience, 5, 691-696 (2012), doi:10.1038/ngeo1580.

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- *Page 8654: Line 14: Where are the observed mixing ratios taken from? Line 22: Are the calculated mixing ratios based on observed values, or on the recommended values from CCMI, or are you using the seasonal cycle and latitudinal gradient from observed values, but the mean values follow the CCMI recommendations?*

  Reply: The observations are taken from AGAGE and NOAA/ESRL as stated in lines 3-7 of the same page. To clarify, we will add "…are calculated from the observed mixing ratios **(see above)** and applied ...".

  The calculated mixing ratios are those recommended by CCMI, however, we superpose a seasonal cycle and latitudinal gradient from observed values. As we state, the CCMI values differ (in the past!) from observations. To clarify, we replace "(from literature)" by "(from CCMI)" in line 21 and "calculated" by "recommended" in line 22/23.

- *Page 8655: Line 5: what aerosol scheme is used, bulk, modal, sectional?*

  Reply: Modal. We add "…is calculated **with a modal scheme with four lognormal modes (separated into hydrophilic internally mixed and hydrophobic externally mixed particles. Furthermore,** in *RC1-aecl* …"

- *Line 20, Does that mean, aerosols in the TTL (reaching up to 150hPa) are described with the stratospheric data set? Will this have an impact on the results of the simulation?*

  Reply: Yes.

  This setup is chosen, since in the current model configuration the size distribution of stratospheric aerosol particles is not represented properly. However, a configuration suitable for stratospheric aerosol particles, as e.g. used by Brühl et al. (2015)[2], leads to substantial deviations compared to the AEROCOM median

[2] Brühl, C., Lelieveld, J., Tost, H., Höpfner, M., & Glatthor, N.: Stratospheric sulfur and its implications for

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distribution in the lower troposphere. As the stratospheric heating is simulated according to the CCMI recommendations, upper tropospheric aerosol, which usually has only minor impact on the radiation budget, is in agreement with the prescribed boundary conditions. Note, that for the standard simulations anyhow only climatological aerosol particles are used, e.g. from the Tanre climatology, such that in the upper troposphere a merge between the CCMI and Tanre climatologies is used and in the upper most troposphere and the stratosphere the CCMI values are applied.

- *It would be helpful to move Section 3.10. after section 3.7, to continue describing aerosols.*

  Reply: OK, we will move the section.

- *Page 8654, Line 26: Please define RC1SD-base-10a, or point to Table 1.*

  Reply: We will point to Table 1 and Section 3.12.2.

- *Figure 1 caption, change "in comparison with" to "and"*

  Reply: Will be done.

- *Section 3.9.1 and 3.9.2: It is hard to understand the differences between experiments that have not been defined up to this point in the text. Maybe add an overview of the setup of different experiments? I guess, looking at Table 1, one can infer what experiments were performed, but the naming of the experiments is not intuitive, so it is difficult to follow in what way experiments differ.*

  Reply: This will be clarified with a revised Table 1 and an additional hint to the sensitivity studies in Section 3.1.

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radiative forcing simulated by the chemistry climate model EMAC, Journal of Geophysical Research: Atmospheres, pp. 2103-2118, doi: 10.1002/2014JD022430, URL http://dx.doi.org/10.1002/2014JD022430 (2015)

- *In general in the results section, pointing to colors of the lines of different experiments would help to identify them on the plot, since the labels are often very small in the Figures. Often, there is a discussion that differences occur due to vertical resolution, but there is no explanation why vertical resolution would cause the differences.*

  Reply: We will add the line colour information to the revised Table 1 and to the text, where appropriate. Labels in the plot are indeed to small, however, mainly due to the GMDD layout. In the revised version (full page view) they will be better readable.

  A detailed analysis of the processes causing the differences in the results obtained with different vertical resolutions is unfortunately beyond the scope of the present study, which should be seen more as an inventory. Indeed, we hope that the upcoming analyses within the CCMI activity will shed light on – at least – some of these issues.

- *Page 8663: Dust emissions depend on the wind velocity. Why do the aero and the aecl experiments result in so different dust emissions? Are interactions with clouds changing the meteorology? What are you using for the prescribed simulations for dust?*

  Reply: Dust emissions **are sensible to wind speed, but also to surface dryness as a consequence of precipitation. The aerosol-cloud interactions modify the wind speed via boundary layer processes, which are induced by the differential heating caused by aerosol impacts on clouds. Additionally, the circulation is slightly altered, such that higher mean wind speed close to the surface is obtained. Additionally, precipitation (see Fig. 14) is slightly different in the *RC1-aecl-01/02* simulation compared to the *-base-* case. For instance in Central Africa *RC1-aecl* is slightly too wet compared to GPCP, whereas the *-base-* case is underestimating precipitation slightly.**

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The simulations with prescribed aerosol use the Tanre climatology (Section 3.7.1), which explicitly accounts for mineral dust as one of the main components. Therefore, only the spectral climatological distribution of dust particles is used instead of emission fluxes.

It will be added to the revised text.

- *Page 8669 Line: 17: change to present tense: We compare . . .*

  Reply: Will be done.

- *Page 8670: Section 4.1 is somewhat difficult to follow. The authors jump in the discussion between SD RC1 and RC2 experiments. It would be helpful to summarize what the differences between the experiments are and why there are these differences, instead of pointing out all the details.*

  Reply: We agree. For the revised manuscript we will rewrite this paragraph and discuss three simulation categories: (1) nudged simulations including nudged global mean temperature, (2) the nudged simulations (without global mean temperature nudging), and (3) the free-running simulations.

- *Line 5: Are there implications for the large temperature bias around the tropopause or high latitudes? How does this this impact water vapor in the stratosphere?*

  Reply: It does impact stratospheric water vapour. A detailed analysis is, however, beyond scope and under investigation elsewhere[3].

- *Figure 12: is too small to read what experiments are displayed.*

[3] Brinkop, S., Dameris, M., Jöckel, P., Garny, H., Lossow, S., & Stiller, G.: The millennium water vapour drop in chemistry-climate model simulations, Atmospheric Chemistry and Physics Discussions, 15, 24 909-24 953, doi: 10.5194/acpd-15-24909-2015, URL http://www.atmos-chem-phys-discuss.net/15/24909/2015/ (2015)

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Reply: We completely agree. But this is only due to the GMDD layout. The figure is made for an entire page.

- *Precipitation. To me, all the simulations are representing mean precipitation rather similar. The authors described differences of different experiments in great detail. However, the figure does not allow seeing those very well, other then the RC2 simulation outlier. Maybe pointing out line colors, like RC2 are purple, RC1 are redish etc. would help. I don't think, there is a need to go into all the details, unless there is a good reason why different experiments perform differently, as the RC2 simulation. There, are the differences in precipitation compared to the data may be caused by a shift in ITCZ? Even though the paper was not intended to discuss uncertainties, if variability in the experiments is smaller than the uncertainty of the data set, what is the point in discussion those differences in much detail?*

  Reply: The referee is in principle right, stating that the shown differences (between different model setups) are small compared to uncertainties of observations and (as stated on page 8672, lines 16ff) parameterisation formulations – in line with results of Dai et al. (2006). Nevertheless, this was a-priori not clear. Therefore, we think it is important to show how robust the model results (w.r.t. the nudging setup, the vertical resolution and the role of aerosols) are, but still quantify the differences. In addition, the section was expanded in the very first (quick access) editorial phase, because the editor requested more quantitative results here. Nevertheless, we reformulate parts (referring also to Dai et al., (2006)), simplify the reading by pointing to the line colours and mention the role of the double ITCZ in the revised manuscript.

- *Page 8673: First paragraph: Why do these two SD simulations produce much less ozone deposition? How is tropospheric ozone behaving in those simulations? Is it largely underestimated, or what changes the dry deposition in these runs?*

Reply: "They produce much lower ozone dry deposition fluxes **, which is a direct effect of the, compared to other simulations, largely reduced ozone mixing ratio (about 28 to 32 nmol mol^{-1} on average) in the *RC1SD-base-07/08* simulations. The lower ozone mixing ratio, in turn, is caused mainly by the reduced lightning NOx and corresponding ozone production (see Figure 4 and Section 4.7).**"

- *Page 8675, Equation 1: What is "t"? Is methane lifetime calculated for each year? How much does the difference in CH4 lifetime depend on the amount of ozone in the tropical troposphere besides temperature. O3 is the largest source of OH in that region.*

  Reply: "t" is time. We will add "Here, we present the simulated OH-lifetime of atmospheric CH_4 **at time** t as a measure ...". The lifetime is first calculated for every output time step (i.e., 10-hourly), then averaged monthly, and then annually.

  A detailed discussion of the variations in OH and possible dependencies on ozone would be interesting, but quite comprehensive. Therefore, we think that - for this overview paper - it is beyond the scope, since it deserves a more in-depth analysis. Nevertheless, we will add some general remarks at the end of the section.

- *Figure 19, 20, 21, 22: It would be helpful to show a plot with the standard deviation of the aircraft data, if available, to get some idea how significant the differences are.*

  Reply: The figures 19 – 22, showing relative differences of simulation *RC1SD-base-10* minus *RC1SD-base-10a*, will be replaced by those showing the absolute values of model minus measurements divided by the sum of the standard deviation of measurements and model. This provides an indication about the significance of the relative differences. The relative differences of *RC1SD-base-10* minus *RC1SD-base-10a* will still be described in the text and shown as part of the revised supplement.

- *Page 8678: Line 5ff, What figure or plot are you referring to, please point this out. The description was confusing to me, until I realized that you are plotting observations minus model results. Plotting model results minus observations would make it easier to follow the text. Deviations from the observations seem to be larger than 20%. Also, the model overestimates ozone (negative values in the plot) in 0-3km below the tropopause, I would not call this "low tropospheric" values.*

  Reply: Figure 19 is referred to in the sentence before. We will modify the figures to show "model results minus observations". We will also precise the numbers in the text. With "low tropospheric values" we meant "lower values in the troposphere", this will be corrected.

- *Page 8679: Can the 5% difference between models and observations explained by the difference between prescribed fields and observations? How large do the surface values differ between model and observations? And further, are other differences at all significant? Again, at least stating the standard deviation of the measurements would be helpful.*

  Reply: As explained in Sect. 3.6, methane is prescribed by Newtonian relaxation at the lower boundary based on observations (Fig. E1 in the Supplement). The deviation of 5% is therefore rather indicating deficiencies in simulating the correct methane lifetime and / or vertical tracer transport in the troposphere. This requires further investigations. In the revised Figure 21, the ratio between the (absolute) differences (model minus observations) and the standard deviations (model plus observations) will be shown to provide information about the significance of the deviations.

- *Page 8686: Line 13,14. The lines in the discussed figure are difficult to distin-*

guish, however, it looks like, if normalizing all the experiments to the same 1980 value, the recovery date between the 90 and 47 layer simulation is very similar, but maybe I am looking at the wrong lines?

Reply: Indeed, this is not apparent from the figure. However, we calculated the anomalies to the 1979-1982 mean and found a clear difference in the return date between the 90 and 47 layer simulation. We will add this Figure as S35 to the supplement and refer to it from the text.

- *Page 8687: Line 4: another important effect could be transport and mixing changes if the modeled meteorology has been nudged towards analysis. Convection changes alter ozone by itself, not only through the lack of lightning NOx production. Mixing processes and stratosphere and troposphere exchange may also play an important role.*

  Reply: Yes indeed. We tried to use the diagnostic tracers ST80_25 and O3s (see Appendix and Table A1) to disentangle the potential STE effect from chemical effects. We found that O3s cannot be used, because the modified chemistry (basically its loss in the troposphere) also alters the cross-tropopause gradient and therefore its own STE flux. Likewise, the STE flux changes of ST80_25 cannot be simply used to "scale" the STE flux of O3 for the same reason, i.e. because the vertical gradients across the tropopause are different between O3 and ST80_25. Nevertheless, to point out this issue, at the end of the paragraph we add "**Additional effects, which are however more difficult to quantify, are direct effects on ozone by altered convection, by altered mixing, or by modified stratosphere - troposphere exchange.**"

- *Page 8690: Line 1: ". . .where the coupled ocean model has the largest impact" I am not sure what is meant here? Impact on what?*

  Reply: We reformulate to "...and the coupled atmosphere - ocean model shows the largest deviations from observations."

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