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Geoscientific Model Development Discussions

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

Anonymous Referee #2

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The manuscript presents recent updates to the ECHAM/MESSy Atmospheric Chemistry (EMAC) model, describing the version that was used for a large set of simulations to be submitted to the Chemistry-Climate Model Initiative (CCMI) model intercomparison project. Selected results from a number of the simulations specified for CCMI are presented, including a comparison of different model configurations for many of these experiments. For example, results are compared between versions of the model with 47 and 90 vertical levels, including prognostic tropospheric aerosols versus specified aerosol fields and two different approaches to nudging the model dynamical fields to reanalysis for the Specified Dynamics simulations defined by CCMI. The effects of a number of problems with the CCMI simulations that were discovered after the simula-

tions were quite advanced are also investigated by comparing these simulations with follow-on simulations with these errors corrected.

The manuscript presents a great deal of information that will serve as an important resource for people analyzing the CCMI simulations and provides several interesting insights into how different choices in setting up the CCMI simulations affect the final results. The impact of the number of vertical model levels on stratospheric age of air and the effects of nudging wavenumber zero for temperature on lightning are good examples of findings that will be interesting to the modelling community. My only significant concern with the manuscript in the current form is that I find the sheer volume of different simulations, including variations that test the effects of bugs, is overwhelming for the reader. None of the analyses presented here show any significant differences between the RC1-aero-06 and -07 or the RC1-aecl-01 and -02 simulations that were run to test the effects of problems with the black carbon and organic carbon emissions. Yet the presentation of these four separate simulations complicates the interpretation of the results by the reader. It would seem possible to present the important caveat about the aero and aecl simulations while simplifying the presentation of the results by reducing the number of individual simulations discussed. A similar argument could be made about the RC1-base-07a, 08a and 10a simulations.

A related concern, if the article is to serve as a reference for the set of EMAC CCMI simulations, is that it is not clear how the different simulations described in the manuscript correspond to simulations that will be available for analysis within CCMI. For example, the RC1-aecl-01 simulation stops at 1972 and the RC1-aecl-02 simulation covers 1965-2011. Will the EMAC REF-C1 simulation with the model setup of aecl be constructed by combining these two simulations?

Aside from difficulties dealing with the number of simulations, I have no significant concerns with the manuscript. A few minor suggestions are given below.

Page 8640, Lines 15-21. It is mentioned here that the chemistry and reaction rates

have been updated. From the wording, it is not clear if the reaction mechanism has been modified or if the updates were just to update the reaction rates. If there have been some modifications to the chemistry, it would be helpful to have them briefly described. The issue of the chemistry is discussed in more detail in section 3.5.1, but what updates have been made, if any, are not mentioned.

Page 8643, Lines 3-5. It is not clear to an outside reader what SCALC is designed to do. The use of the term 'channel objects' is also a mystery – to me, at least.

Page 8646, Lines 21-27. It is mentioned that the changes to the clouds produce a 3.4 W/m2 increase in the shortwave balance, that is designed to offset the -3.3 W/m2 net balance using the original set of parameters. Is there any impact on the longwave balance from the revised parameter values and what is the overall radiative balance with the revised parameters?

Page 8648, Line 7. I believe 'divers' should be 'diverse', but both are valid English words with subtly different meanings.

Page 8659, Lines 14-17. Here it is stated that the isoprene emissions are reduced by a factor of 0.6 to give realistic isoprene mixing ratios in the boundar layer. Is there a physical reasoning behind the reduction, perhaps to account for reactions within the canopy, or is it a purely pragmatic choice?

Page 8667, Line 18 – Page 8668, Line 3. The error with extinction is discussed here, section 3.12.1. Since most of these runs use prescribed SSTs, the impact of the volcanic eruptions will already be present in the tropospheric temperatures. Does the error also impact the infra-red interaction of the aerosols, in which case the stratospheric temperature response will be significantly effected?

Page 8673, Lines 17-26. Figures 16 and 17 show values of the deposition flux from sedimentation, yet many of these simulations use specified aerosol fields. There is, apparently, a calculation of sedimentation for model runs using specified aerosols. In

Section 3.5.4 there should be a mention that sedimentation is calculated for simulations with specified aerosols and, perhaps, a brief description of any important features of how this calculation is performed.

Page 8676, Lines 5-11. This section of text places the EMAC methane lifetime alongside the methane lifetime from other models. I have no objection to this discussion, but the text should also discuss the observationally based estimates of methane lifetime of closer to 11.2 +/-1.3 years from methyl chloroform (Prather et al., 2012).

Page 8678, Lines 6 – 9. The text discusses how ozone in the summer decreases more rapidly in the model than in the observations, leading to an underestimate in the model above the tropopause that peaks during June-July-August. Figure 21 shows an overestimate of CH4, notable because the model underestimates CH4 in much of the upper troposphere. Could the underestimate of ozone and overestimate of CH4 be related to problems with cross-tropopause transport?

Page 8679, Lines 24-26. Could the authors clarify what they mean by 'The seasonal cycle is, however, reproduced when taking more model data into account (not shown).' Is this referring to more data in the same region of the atmosphere, or sampled over different geographic locations?

Page 8685, Lines 6 – 9. Total column ozone from the model is compared with the Bodeker Scientific dataset (BSTCO) for the years 1980-2011. Averaging over the 1980-2011 period mixes years from the early 1980s, when ozone depletion was more modest, with years in which it was more fully developed from the mid-1990s onward. Wouldn't a comparison of, say, 1995-2011 be a more straight-forward averaging period? Do the trends over $\sim$ 1980-1995 contribute to the variance used in the test for statistical significance

Page 8738, Caption for Figure 19. There should be mention in the figure caption, as there is in the text, that the data used is restricted to the latitudes 35 – 60N.

Page 8691, Line 6. The term 'sulphite' is used here, as it is in a few other places through the manuscript. Should that be 'sulphate'? I'm not sure myself, but I am more used to seeing sulphate.

References: Prather, M. J., C. D. Holmes and J. Hsu, Reactive greenhouse gas scenarios: systematic exploration of uncertainties and the role of atmospheric chemistry, Geophys. Res. Lett., 39, L09803, doi:10.1029/2012GL051440, 2012.

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