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Geoscientific Model Development Discussions

Interactive comment on "A new version of the CNRM Chemistry-Climate Model, CNRM-CCM: description and improvements from the CCMVal-2 simulations" *by* M. Michou et al.

M. Michou et al.

martine.michou@meteo.fr

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1 Response to Anonymous Referee #1

Thank you for your detailed review of our article. We have reworded the text of our article following your recommendations. Apart from these changes in the text, please find below our responses to your remarks and suggestions that appear below in italics. The amendments to the text of the paper that we propose appear in bold.

1. *In the new model version some fundamental modifications have been imple-*

mented, e.g. a new radiation scheme or the coupling between GCM and chemistry code. Such changes can have manifold impacts on the model performance, and disentangling the exact cause-and-effect relationships is not always easy due to complex feedback mechanisms. In section 4 the authors state that they "did not conduct a step-by-step analysis of what caused the differences between both models". Nevertheless I would like to encourage the authors to add a more detailed discussion about possible reasons for the differences. Currently the paper is very much "show&tell", i.e. a subset of diagnostics form CCMVal-2 is reproduced including the new model simulation and the changes between both model versions are described in the text. I am sure that in several cases good and conclusive explanations can be found, even without a detailed step-by-step analysis.

In response to your remark, we propose to amend our "Synthesis of the CNRM-CCM model performance and outlook p1154" paragraph as follows:

In the previous section we showed that the new version of the CNRM Chemistry-Climate Model, so called CNRM-CCM, had a better performance than the previous version. We did not conduct a step-by-step analysis of what caused the differences between these two models, **as that would be to a certain extent specific to our models**, but changes in the radiation scheme led to a better mean meteorological stratosphere.

......

Stratospheric temperature biases in spring and winter at high latitudes are smaller or comparable to those of the CCMVal-2 models, with the exception of the upper stratosphere between 5 and 1 hPa where the model is too warm (5 to 9 K). This warm bias extends to all latitudes, is permanent throughout the year, **and simulations performed with no retroaction with the chemistry onto the radiative scheme reveal that it is intrinsic to the GCM itself. It is related undoubtedly to the radiative scheme (Morcrette et al., 2001) that is in itself also perturbed by the 3-D distribution of the greenhouse gases. Bechtold**

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et al. (2009) report on the reduction of this warm bias in the region of the stratopause due to a new climatology of greenhouse gases. In the end, a number of biases appear in the chemistry of the upper stratosphere. The model produces too little O_3, but too much NO_2 and N_2O_5 at 1 hPa and is then at the high end of the CCMVal-2 models.

Linked with this stratospheric distribution of temperatures, the other dynamical features analysed, transition to easterlies at 60 S, strength and position of the stratospheric jets, and pressure of the tropopause **,** compare favorably to the ERA-40 and ERA-Interim reanalyses. **......**

Further developments of the model will also include the non-orographic aspects of the gravity waves, as well as the short-lived source gases containing bromine (WMO/UNEP, 2010). The latter will require a description of the tropospheric processes (e.g., emissions, convection, scavenging) that drive the evolution of these short-lived species. **CNRM-CCM is planned for use in a variety of projects linked with the interactions between chemistry and climate, in particular in seasonal and decadal predictions, where it could possibly be coupled to an interactive ocean.**

2. *My second major comment refers to statistics. The paper doesn't say anything about the statistical significance of the presented differences between both model versions, it is simply checked whether the model results are within the standard deviation of the observational/reanalysis data or not. In some cases the differences between the models are rather small. Furthermore, the study is based on one single model simulation. So in my opinion it would be worth to lay a bit more stress on statistics.*

   Thank you for raising this issue. We adressed it as described below. As our model is under on-going development, either in its GCM part or in its chemistry part, we were able to analyse four other REF-B1 type simulations (1960-2006) performed in the last months. Two differed from the one presented in the paper by slight modifications of the chemistry, and two by the versions of the GCM. So, in total we have been able to plot the exact same diagnostics for five simulations. The general conclusion is that for all but one of the diagnostics we show in the paper the same conclusions can be drawn analysing the five simulations (see as examples Figures 1 and 2 of the supplementary material). So our analysis is to a large extent statistically robust. The exception to this concerns the annual cycle of the equatorial temperature at 100 hPa (that you pointed out below in your comments) that shows some dispersion of about 1 K (see Figure 3 of the supplementary material). We add that we could analyse only one simulation performed by CNRM-ACM, in the framework of the CCMVal-2 project, and there was no possibility to perform an additional simulation because of the computing resources required. On the other hand, the notion of ensemble simulations, now used widely, for instance in the CMIP-5 project, has been very rarely used by the Chemistry-Climate models of CCMVal-2, again certainly because of the related computing cost.

So we propose to amend the text of our paper in the “Simulations analysed” paragraph p 1137 l15 as follows:

As the objective of this paper was to build upon CCMVal-2, we performed CNRM-CCM simulations as defined in Eyring et al. (2008) and SPARC (2010). We analyse here results from a transient simulation identical to CCMVal-2 REF-B1 in terms of the period simulated (1960-2006), and the external forcings used (SSTs, ODSs, GHGs, aerosol forcing, solar irradiance). As in the CNRM-ACM REF-B1 simulation, the Quasi Biennal Oscillation (QBO) was not imposed on CNRM-CCM. We also include in our analysis the outputs of the CCMVal-2 REF-B1 models as an indication of the state-of-the-art 15 CCM modelling. **Although we chose to present one CNRM-CCM simulation only, we performed during the course of the CNRM-CCM development four additional REF-B1 type simulations that differed slightly from the one presented in this paper, ei-**

ther in the formulation of the chemistry or in the version of the GCM. All the diagnostics shown below lead to the same conclusions for all five simulations, except otherwise depicted (see paragraph 3.2.5).

And in paragraph 3.2.5, we propose to add the following wording p 1146 l 20:

Interestingly, this seasonal cycle of the temperature is quite sensitive to the description of the chemistry and/or to small adjustments of the GCM. The five CNRM-CCM REF-B1 simulations performed (see paragraph 2.3) have seasonal cycles that differ, depending on the month by up to 1 K (see Figure 3 in supplementary document).

3. *p1131, l15: The future evolution of stratospheric ozone cannot be reproduced.*

   We propose to rephrase the sentence as follows: **CCMVal-2 evaluated a comprehensive number of processes that assessed the capability of CCMs to reproduce past observations in the stratosphere. Furthermore, these CCMs were utilised to predict the future evolution of stratospheric ozone and climate under one particular scenario (SPARC, 2010).**

4. *Section 2.2: In the CNRM-ACM model only the 3-d ozone field was used for radiative calculations. The new radiation scheme considers 6 additional gases. Are those 3-d fields now also provided by the chemistry model or does the radiation scheme use climatologies?*

   Indeed, in CNRM-ACM the only 3-D fields used in the radiation scheme of the GCM (Morcrette, 1991) were ozone provided by the chemistry model and water vapor provided by the GCM. The other gases considered by this radiation scheme (i.e., carbon dioxide, methane, nitrous oxide, CFC-11, and CFC-12) had climatological values that were constant throughout the atmosphere.

   In CNRM-CCM, with the radiation scheme of Morcrette et al. (2001) that considers the seven absorption gases listed above, 3-D fields are provided by the chemistry scheme for all seven gases. Water vapor of the GCM includes chemical tendencies, of importance in the stratosphere.

To clarify this issue, we propose to amend the text of our paper P1136, l11 as follows:

Seven gases are considered as absorbers, H_2O**,** CO_2**,** O_3**,** CH_4**,** N_2O**,** $CFC11$**, and** $CFC12$ **whose 3-D distributions are provided by the chemistry module of CNRM-CCM (see below).**

5. *p1137, l12: The QBO was not assimilated in the CNRM-CCM simulation, e.g. by a relaxation of the zonal winds in the equatorial stratosphere ("nudging"). Why?"*

In the CCMVal-2 CNRM-ACM simulations we had decided not to impose a QBO to our model, for several reasons. First of all, the GCM did not spontaneously generate any QBO at all, so a nudged QBO could not be merely used to adjust any characteristics of a model QBO. Second, due to the short timelines of the CCMVal-2 exercise, we had no time to conduct the necessary test experiments to evaluate, and possibly adapt, the impact of this particular nudging. Finally, we were aware that other model groups would not include this QBO signal in their simulations so our case would not be a unique one but would be part of the no-QBO model group; in the end, about a third of the CCMVal-2 models did not reproduce the QBO mode of variability.

As far as the CNRM-CCM simulation is concerned, as our objective was to run a simulation as close as possible to the CCMVal-2 REF-B1 CNRM-ACM simulation, and as CNRM-CCM does not internally generate neither any QBO signal, we chose not the assimilate the winds in the simulation we analyse in this article. It would be undoubtedly interesting to perform QBO nudged simulations with CNRM-CCM and analyse the impact of such a nudging. Other original recent nudging experiments with our GCM evaluated the impact of nudging the high latitude stratosphere onto the Northern Hemisphere winter climate variabil-

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ity (Douville, 2009), and of nudging the lower stratosphere onto reproducing the seasonal mean negative North Atlantic Oscillation (Ouzeau et al., 2011).

6. *p1141, l5 ff: The term "cold bias" is often exclusively used for the temperature error in the UTLS region.*

   Thank you for pointing this out, however this term has also been widely used in other CCMVal-2 related articles (see for instance Eyring et al. (2006) and Butchart et al. (2010)). We have left the original wording relating to explicit regions of the stratosphere so as not to make sentences more cumbersome.

7. *p1141, l27 ff: Is the discussion of temperature biases related to the period 1960-1980 or to the period 1980-2001? Please clarify.*

   The first part of the discussion on temperature biases in paragraph 3.2.1 (page 1140 l18 to page 1141 l24) refers to the period 1980-2001, as indicated in the beginning of the paragraph in the sentence "biases presented are those from the longest period with large ozone depletion available in the simulations (1980-2001)."

   Then the second part of the discussion on temperature biases (page 1141 l25 to page 1142 l5) outlines differences between biases calculated over the 1960-1980 period, and those calculated over the 1980-2001 period. For clarification purposes, we show these 1960-1980 biases in a supplementary document and we propose to amend the sentence starting page 1141 l25 as follows:

   Climatological biases in temperature calculated relative to the first 20 yr of REF-B1, i.e. 1960-1980 (shown in a supplementary document as Figure 4) are quite different from those calculated over 1980-2001 (shown in Figure 1), except for DJF between 90 N-60 N.

8. *p1146, l12 ff: Here the authors discuss rather small improvements between both model version concerning the tropical tropopause temperature. It would be very*

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interesting to see if those improvements are statistically significant or not. This comment refers to Sect. 3 in general.

Please refer to our response to your second major comment.

9. *p1148, l28: The authors state that HNO3 for CNRM-ACM is not further analyzed, nevertheless it is shown in Fig. 9. Why?*

   We propose to rephrase the sentence as follows: **As for HNO_3, we note that an error within the implementation of the heterogeneous chemistry in CNRM-ACM has been corrected in CNRM-CCM. Therefore, we will not comment on the CNRM-ACM HNO_3 any further.**

10. *Figures: I was a bit confused that the results for the new model CNRM-CCM are shown in black. I would highlight the new model version in red.*

    We chose the orange color for CCMVal-2 models other than CNRM-ACM, the red color, close to orange, for CNRM-ACM, and the black color for CNRM-CCM to accentuate the difference with CNRM-ACM. We could certainly have chosen different colors, and red for example for CNRM-CCM. But as none of the other two referees made a comment on our choice of colors we will leave them unchanged in a new version of our article.

11. *Fig. 9: There are some strange gray bars in the O3 and H2O plots - what do they mean?*

    The grey bars correspond to months where the standard deviation of the observations has a 'missing value' in the data file in the range of levels and latitudes requested. We have made this more explicit in the legend of the figure.

12. *Supplement: The authors might put all figures that are discussed in the text, but not shown, in a supplementary document.*

    We have put together a supplementary document that includes twelve figures. This indeed supports the discussion of the paper.

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