Geosci. Model Dev. Discuss., 4, C583–C593, 2011
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Geoscientific Model Development Discussions

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Interactive Comment

# *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.

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Received and published: 29 August 2011

## 1 Response to Anonymous Referee #3

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. *The discussion of model improvement and comparison to previous version is*

*quite lengthy. It might therefore be useful to try to summarize it, possibly in a Taylor diagram that would show where the improvements were concentrated.*

We plotted a Taylor diagram that presents the results of the diagnostics analysed in our article. For graphical purposes, negative correlation coefficients have been set to zero (see dots on the vertical axis), and normalized standard deviation higher than 1.7 have been set to 1.6. Furthermore, we withdrew the identification of the individual diagnostics as the objective here was to get an overall picture of the performances of CNRM-CCM, and a possible comparison to CNRM-ACM. We propose to add the following sentences in our paper p 1155 l13:

**As an overall picture of the agreement between the observations and the CNRM model outputs, in either phase and amplitude of the annual cycles, or of the vertical or latitudinal distributions, we plotted a Taylor diagram (Taylor, 2001) of the diagnostics analysed in the previous paragraphs of the paper (see Figure 13 of the supplementary document). For graphical purposes, negative correlation coefficients have been set to zero (see dots on the vertical axis), and normalized standard deviation higher than 1.7 have been set to 1.6. Furthermore, the identification of the individual diagnostics has been withdrawn from the figure as the objective was to get an overall picture of the performances of CNRM-CCM, and a possible comparison to CNRM-ACM. Though individual evolutions discussed in the paper between CNRM-ACM and CNRM-CCM are not identified in this Taylor diagram, interesting outcomes can be made: a number of diagnostics have poor skills, either because of a very low correlation with observations (see for instance $CH_4$ or $CO$ in Figure 13), and/or because of an amplitude of the signal far from that of the observations (standardized deviation lower than 0.5 or higher than 1.5, see for instance $H_2O$ in Figure 9). In contrast, a substantial number of dots lie in the portion of the diagram close to the REF line (similar variances of models and observations), and delimited by**

**a correlation coefficient higher than 0.9. Finally, it appears that CNRM-CCM has a larger number of satisfactory dots than CNRM-ACM.**

2. *It is mentioned in several sections that the change in radiative transfer scheme is, at the very least, a strong contributor to the improvements. It would therefore be very useful to have a better understanding of what the switch to RRTM provided. In particular, it might be useful to provide a zonal-mean distribution of heating rates from both schemes for the same conditions. This would enable the reader to understand those changes in a no-feedback framework.*

   Morcrette et al. (2001) present a detailed study on the switch in radiative transfer schemes. After a description of both schemes, they show one-dimensional comparisons on standard atmosphere, including boundary fluxes, clear-sky heating rates, boundary fluxes for standard atmospheres with a low-level cloud or a high-level cloud. Then impacts on analyses and forecasts are described, detailing the LW radiation fluxes (ORL or net at the surface), and the objective scores obtained for the forecasts. Overall, Morcrette et al. (2001) indicate that:

   The impact of RRTM_EC on analyses is mainly felt in the stratosphere for temperature, and right below the tropopause for temperature and humidity. The effect in the stratosphere is a “pure” radiative effect, with temperature adjusting to slightly different LW radiative heating, because RRTM_EC includes the full treatment of the Voigt line broadening present in the line-by-line model, whereas the M91/G98 scheme only has a more approximate treatment of the Voigt line profile. In the tropics, the increased destabilisation provided by RRTM_EC in clear sky slightly enhances the convection, with the uppermost clouds moving up, thus creating the temperature dipole around 100 hPa, the increase in specific humidity close to the level of detrainment, and the change in cloud cover.The increased convection gives a slight increase in high cloudiness over the ITCZ and a slight decrease away from it. However, the impact on the wind remains small.

   We propose to add the following sentence to p 1141 l 24:



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In summary, RRTM has shown an essentially positive impact over a wide range of parameters, in particular for the surface radiation and the temperature in the stratosphere. Impacts right below the tropopause for temperature and humidity, as well as on convection in the tropics have also been observed **(see Morcrette et al. (2001) for details on changes in a number of radiative fields).**

3. *Section 2.1.1. should include a description of the gravity-wave drag. It is very possible that some of the biases in the dynamical field could also be related to misrepresentation of GWD in the model. Some discussion of this would be useful.*

   Actually, gravity wave drag plays an important role in driving the meridional circulation in the middle atmosphere, so it is likely that some of the biases in dynamical fields could be related to a misrepresentation of the GWD. However, in this paper, we focus on the improvements of a new version of our chemistry-climate model. For the two versions presented here, the parametrization schemes of orographic (Lott and Miller, 1997) and non-orographic gravity waves (Bossuet et al., 1998) are identical. So, the adjustable parameters in these schemes have not been tuned for this comparison.

   A new parametrization scheme for non-orographic gravity waves, based upon the generalized spectral parametrization proposed by Scinocca (2003), is currently developed in the GCM, but is not yet tuned for the chemistry-climate version of the model. Results are very promising (Saint-Martin, 2010) : the new parametrization allows an improved representation of the zonal-mean circulation and temperature structure and a better simulation of the equatorial stratospheric variability but does not yet lead to a realistic simulation of the QBO. This will be a priority for us in the further developments.

   We propose to amend the description of the gravity-wave drag (p1133, l1) by including the omitted description of the non-orographic part of the scheme.

   The model includes a subgrid scale orographic scheme based on Lott and Miller



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(1997) and Lott (1999). **It also contains a non-orographic gravity-wave drag parametrization (Bossuet et al., 1998), which links the GWD to the tropospheric convection.**

4. *It is unclear of the 560 hPa values of chemical species (section 2.1.2) are specified. Are those constant, climatological? Where does the Ox/O3/CO come from (since they will be strongly affected by tropospheric chemistry)?*

   The relaxation values consist of yearly global values of surface mixing ratios as recommended by the CCMVal-2 activity, either for ozone depleting substances, or for greenhouse gases (see SPARC (2010)). As for $CO$ we adopted a constant value of 94 ppb. This is a global mean valid for the year 2000 that we keep constant throughout the time of our simulations (1960-2100). As for $O_x/O_3$, they are a personal communication from F. Lefèvre and consist of an empirical formula based on observations. The relaxation time is of seven days.

   We propose to amend our paper p 1133 l 26 as follows:

   **while for higher pressures the mixing ratios of a number of species (namely $N_2O$, $CH_4$, $CO$, $CO_2$, C$FC11$, $CFC12, CFC113, CCl_4, CH_3CCl_3, CH_3Cl, HCFC22, CH_3Br, H1211, H1301, O_x, O_3, Cl_y, Br_y, NO_y$) are relaxed towards evolving global mean surface abundances (see SPARC (2010) for the ozone depleting substances and greenhouse gases, and the CNRM-CCM technical documentation for the other compounds).**

5. *Page 1134, line 15: What time constraints are being discussed?*

   These time constraints refer to the scheduling of the CCMVal-2 activity. Outputs of the model simulations were to be archived at the BADC (British Atmospheric Data Center) before a given date in order to allow time for analysis and publication of the results. These results were aimed to serve as input to the WMO/UNEP (2010) report.



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We propose to modify the text of the paper as follows:

**In order to meet the time schedule of the CCMVal-2 activity,CNRM-ACM simulations were performed using two different horizontal resolutions:**

6. *Page 1135, line 25: spell out CMIP5 and include reference (Taylor et al., 2009)*

   We have modified the text of the paper accordingly.

7. *Page 1141, line 17: if the radiative scheme is not the only thing acting, what else contributes?*

   Other possible candidates for this change in temperatures in the stratosphere are (1) the fact that 3-D distributions of radiatively active gases are considered in CNRM-CCM while yearly constant values were used in CNRM-ACM. Bechtold, et al. (2009) report on such a change in the IFS model which is the weather forecast version of our GCM; (2) the resolution at which the chemistry is resolved (T21 for CNRM-ACM and T42 for CNRM-CCM, while the underlying GCMs were run at a T42 resolution). This certainly resulted in a better representation of the various barriers to the transport of the chemistry species that are inherent to the stratospheric circulation; (3) the frequency of the $O_3$ coupling, every 6 hours for CNRM-ACM, and at the time-step of the physics for CNRM-CCM (900s in our case); (4) small adjustments in the GCM part of the model including that of the horizontal diffusion, of the gravity wave drag parametrisation and of the time step of the model. All these evolutions between the two versions of our chemistry-climate model are mentioned in our paper, and on page 1141 l 17 we wanted to emphasize on the change of radiative scheme only.

8. *Page 1143: it would be useful to include a zonal-mean figure of the zonal-mean wind. This would clearly indicate where some of the U biases are located.*

   We have included in the supplementary document latitude-pressure cross-sections of differences in zonal wind between ERA-Interim and CNRM-ACM (first



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and third columns), and between ERA-Interim and CNRM-CCM (second and fourth columns), for DJF and MAM (first row), and for JJA and SON (second row) (1989-2000 period) (see Figure 10). These plots indicate clearly that U biases of the model are different in CNRM-ACM and CNRM-CCM, both in their strength and in their position. In general, biases have smaller values for CNRM-CCM throughout the stratosphere, particularly at high latitudes of the winter hemisphere. We can also add that CNRM-CCM behaves well when compared to the CCMVal-2 models (not shown).

We propose to add the following sentence p 1143 l5:

**Further evidence in the reduction of biases in the zonal wind, going from CNRM-ACM to CNRM-CCM, during the four seasons, appears in Figure 10 of the supplementary material.**

9. *Page 1146, line 5: the water vapor has a small seasonal cycle. Any explanations for this behavior?*

Both the annual cycles of water vapor and temperature of the CNRM simulations have a smaller amplitude than observed. This is coherent with the interaction between fields, where temperature regulates the water vapor entry from the troposphere and reciprocally water vapor impacts on the temperature through radiative feedback. Gettelman et al. (2010) analysed other diagnostics at the tropical UTLS, and from that of the ratio of water vapor to the saturation vapor mixing ratio at the cold point concluded that CNRM-ACM had potential problems in fundamental transport, variability and/or condensation processes in the TTL. The error in the calculation of heating rates in CNRM-ACM because of volcanic aerosols perturbed in an important way the variability of the lower stratosphere temperature (see Figure 10 of the paper), and consequently the related variability of water vapor. This is certainly the major part of the explanation for the behavior of CNRM-ACM. This volcanic heating issue was resolved in CNRM-CCM and led to a better estimation of water vapor throughout the stratosphere. However,



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the overestimation of 100hPa water vapour in March is still apparent in CNRM-CCM (see also comment 10). This suggests that there remain some issues in correctly simulating transport, variability and/or condensation processes in the TTL as noted by Gettelman et al. (2010) for CNRM-ACM. We note however that CNRM-CCM lies within the range given by the other CCMVal-2 models.

10. *Page 1147: it is quite significant that the model does not seem to have a tape recorder (based on the bias at 50 hPa, Fig. 7e). It would be nice to comment on this significant bias. Is that related to my previous comment?*

Indeed, the zonal mean at 50 hPa in October is too flat suggesting that the minimum in tropical water vapor has not propagated upwards. In contrast, in March (see Figure 7d) when there was less latitudinal variability, the zonal mean is well simulated by the model. The relative minimum in October at 50 hPa in the tropical latitudes is related to the minimum at the tropopause in January-March, associated with coldest tropical temperatures. As outlined in the previous comment, the minimum in the annual cycle at the entry point of the tape recorder is not low enough in CNRM-CCM, and correspondingly low mixing ratios do not appear at 50 hPa. Furthermore, the latitudinal variability in October is also disrupted by anomalies in the phase of the tape recorder of our model which is too small. Other possible causes for the bias seen in Figure 7e could encompass too much chemical moistening and/or too much mixing with higher latitudes that would wipe out the tape recorder signal.

So we propose to add the following sentences in our paper p1147 l2:

**We can however note that the zonal mean in Figure 7e is too flat, which is related to too much water vapor entering the stratosphere in March (see Figure 7b). Anomalies in the CNRM-CCM phase of the tape recorder (see paragraph 3.2.3) may also prevent the model from reproducing the observations.**



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11. *Section 3.2.8: it is quite amazing how strong the SH ozone hole is at 50 hPa in October. Since the temperature bias is most likely gone or reversed, why is the ozone hole so strong?*.

    In response to your question, we plotted the mean annual cycle (same years as the ozone plot i.e. 1992-2001) of the 50 hPa temperature difference between the ERA-Interim reanalysis and respectively CNRM-ACM and CRNM-CCM. And indeed, model temperatures at high Southern latitudes are colder than the re-analysis from August through November for CNRM-CCM (and for the entire year except April to September for CNRM-ACM). These biases in temperature are also revealed by Figure 1 of our paper. We propose to add the following sentence to our paper in the paragraph related to the ozone hole (p 1153 l 25):

    The two CNRM models overestimate Antarctic ozone depletion, in all periods shown, in a lesser extent however for CNRM-CCM than for CNRM-ACM. **This is linked to biases in temperature (see Figure 1) that remain too low throughout the SH spring (see also Figure 11e).**

12. *Page 1153, line 5: is it possible that the photolysis in the upper part of the model is too strong? Do you include some O3/O2 above the model top?*

    Given the results obtained with the CNRM-ACM and CNRM-CCM simulations, we cannot incriminate the photolysis rate for the destruction of $O_3$. Indeed, the same chemical scheme implemented for CRNM-ACM and CNRM-CCM, with identical photolysis rates, yield different zonal distribution of $O_3$ in the upper part of the stratosphere, for pressures lower than 5 hPa outside the polar regions (see Figure 11 of the supplementary document). We do not include any $O_3/O_2$ ratio above the model top. The model top (first model level being at 0.07 hPa) evolves freely.

    We propose to amend our paper p1153 l 5 as follows:

    However, the most disturbing diagnostic concerns the O3 levels: while they are quite satisfactory in the mid-latitudes for CNRM-ACM, they are far too low



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for CNRM-CCM. **As CNRM-ACM and CNRM-CCM use the same chemical scheme, with identical photolysis rates (Madronich and Flocke (1998) and Brasseur et al. (1998)), these rates cannot be directly incriminated.** A misrepresentation of the temperatures at this altitude ....

13. *Page 1153, line 25: I'm not sure "satisfactory" is the correct term here. These seem like large negative ozone biases. What is the vertical structure of the ozone hole (again, possibly a zonal mean figure would help)*

    It is true that the wording we used was a little awkward. So we propose (1) to rephrase it as follows, and (2) to add a figure that shows the yearly cycles of ozone at various latitudes in our supplementary document:

    **Results are confirmed by looking at climatological yearly cycles of the total column ozone over high, mid and tropical latitude bands as in Tian et al. (2010) (see Figure 12 in the supplementary document).**

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