Review of the revised version of 'The Beijing Climate Center Climate System Model (BCC-CSM): Main Progress from CMIP5 to CMIP6' by Wu et al.

General comments

The authors have addressed the issues from the review of the 1st version of their manuscript. In the following there are a number of comments and requests for clarification related to modified or new parts of the manuscript. Once these points are clarified and the manuscript is revised accordingly, I recommend its publication, so that this model documentation paper becomes available to the community.

Detailed comments

L250… : "… In BCC-AGCM3-MR, the gravity wave drag generated from convective sources is introduced as in Beres et al. (2004), but drag by blocking effects is still not involved. … "
The Beres scheme parameterizes convective sources for the parameterized gravity wave drag. What about other sources, which may be important for the higher latitudes?
Has the McFarlane scheme been tuned for the model simulations?

L420: "… Figure 2 … The whole system in BCC-CSM2-MR nearly reaches its equilibrium after 600 years. … "
Thanks for adding Figure 2. You claim that the system has nearly reached an equilibrium after 600 years. But from Figure 2 one cannot distinguish, whether the system is still equilibrating, or the system has already equilibrated in combination with an energy leak/source of ca. +0.4W/m^2, or whether the equilibration is still ongoing in presence of a leak/source. What can be concluded from this figure is that the net TOA energy flux fluctuates without obvious trend around ca. 0.4 W/m^2, while the SST of the 2nd 300 year period is a bit higher than in the 1st one.
Additionally, I would like to ask the authors to display the global mean surface (or near surface) temperature, either in place of the SST or in addition. The main reason is that the Gregory plots (as in your Figure 6), which are relevant for judging "equilibration", relate the radiative forcing at TOA to the global mean surface temperature, not to SST.

L458-459: "… When a 9-year smoothing is applied … "
The caption of Figure 4 writes: "… of 11-year smoothed … "
Please clarify.

L466-467: "… There are two members (r1i1p1f1 and r2i1p1f1 in Fig. 4) of historical simulations of the CMIP6 model show a hiatus towards the end of the simulation that resembles the observed one … "
r3i1p1f1 is not comparable to the observed hiatus. This realization has a short spell of colder years centered at 2010, which is different to the lack of warming in the observational record and in r1i1p1f1 from ca. 2000 to the end of the simulations.
Please modify your text.

L472… : As you describe the earlier hiatus appears in r2, but not in r1, and the later hiatus occurs in r1, but not in r2. This clearly excludes any simple response to forcing, and makes internal variability a much more likely reason.

L485… : Figure 5 shows in most places between 60S and 60N a warm bias, so that the area average likely is also positive, i.e. the model is rather warmer than ERA-Interim. In Figure 4, however, the

60S to 60N average of the r1 simulation almost always is below the observed record, especially due to the strong response to the Mt. Pinatubo eruption. How does this fit together?

L528-529: Is the similarity of the ECS by chance, or was this a goal of the tuning procedure? Please clarify this in the manuscript.

L576… : "… The zonal mean of zonal wind biases in the high latitudes of the stratosphere in BCC-CSM2-MR have increased near 10 hPa, where model biases may be partly caused by not yet involved gravity wave drag that generated by blocking effects. … "
Blocking mainly affects the tropospheric circulation in regions of steep topography. The polar night jets are rather influenced by gravity wave drag. In any case, the too strong polar night jets indicate a too weak drag. Maybe some non-orographic gravity wave sources are not represented, see earlier comment.

L596: "… which is possibly due to inadequate gravity wave forcing to drive the QBO. … "
This is one possibility. But there is also the other one: The wave-meanflow interaction based on resolved waves (Kelvin waves, mixed Rossby-gravity waves, …) is probably not realistic. One reason that would contribute to such a deficiency is the relatively coarse vertical resolution that would affect the vertical wave lengths and the wave damping.
(For the planned QBO-article: The separate analysis of resolved wave meanflow interaction and parameterized wind tendencies in the QBO domain, together with advective tendencies, will be important to understand the nature of the simulated QBO.)

L633: "… the period ENSO periodicity … " → "… the mean period of ENSO … "