

Response to Anonymous Referee #1

We would like to thank the reviewer for taking the time to give a thorough and comprehensive review. The comments and recommendations in this review have lead to a great improvement of this paper.

Referee comments and statements are in bold. Author replies are in italics. Updated figures are at the end (supplied for reference).

Initial statements from referee

The paper evaluates a new version of a chemistry-climate model (with a heritage). The model, now called the Australian Community Climate and Earth-System Simulator Chemistry-Climate Model shares common components with the UMUKCA as previously used by the Met Office, Cambridge and NIWA. The paper is in principal suitable for ACP, but requires some improvements in the discussion of results before publication. Single model studies are still valuable, but it would be good if the new model could be put more into context with its heritage in CCMVal-2.

We think that including more comparison and discussion about the model's heritage is a good idea. Therefore, we have included comparisons with the CCMVal-2 UMUKCA-UCAM and UMUKCA-METO models, the direct predecessors to ACCESS-CCM.

Also, when comparing to observations care should be taken to compare like with like as far as possible (e.g. time intervals, representativeness, etc.). In some areas of the paper the reader gets the impression that a disagreement could be a model deficit, or just exist because apples and pears are compared. In summary, I believe that most (all) my concerns can be remedied and that the paper could be publishable afterwards.

We have also updated all climatology comparisons to only include 2005-2010 averages to alleviate any concerns about not comparing like to like. Also the MLS comparison has been updated to only include ~3pm values in both MLS and ACCESS-CCM.

In its own words the emphasis of the paper is “. . . analysis of ozone and temperature vertical profiles at Australian, New Zealand and Antarctic sites. Analysis of diagnostics related to climate impacts most relevant to the Australian region, such as shifting surface winds through analysis of the SAM metric and the stratospheric polar vortex are also included.” and this emphasis should be reflected stronger in the title and abstract of the paper.

Yes, we agree that the title is perhaps too broad. We have changed the title

to: “Evaluation of the ACCESS chemistry climate model for the Southern Hemisphere”.

Changes to the abstract have also been made to further highlight the emphasis on important metrics for Australian climate, such as the SAM.

Section 2: Why is the changeover for Ref-C2 in 2005? I don’t think it matters, but it should be explained. Details about the prescribed SSTs: Have the SSTs from the coupled model evaluated against observations? For the common analysis of the recent past, the SSTs (and sea ice) will be a major driver for SAM changes, I believe.

Thank you for bringing this to our attention. We agree this needs to be clarified: REF-C2 forcings follow CMIP5 until 2000. From here they follow RCPs, because, when the RCPs were constructed, the 2000–2005 period was used as a harmonisation period for emissions. Therefore, we have updated the following text

p. 19167, line 8. Updated the following text: “...and after 2005, all forcings follow RCP 6.0.” with: “After 2000, all forcings follow RCP 6.0, as this was the beginning of a harmonisation period for emissions (2000–2005) (Meinshausen et al. 2005).”

Subsection 3.1: A small discussion of pros and cons should be provided for the chosen ozone data base.

Thank you. We agree that a short discussion of the pros and cons is a good idea. Therefore, we have updated the sentences:

p. 19167, line 15. Updated the sentence: “This database is assimilated from satellite observations and spans the period from 1979–2012, where offsets between datasets have been accounted for using Dobson and Brewer ground-based observations.” To “This database is assimilated from satellite observations and spans the period from 1979--2012, where dataset offsets and drifts have been accounted for using Dobson and Brewer ground-based observations. This has the advantage of including long-term Dobson and Brewer measurement stability.”

p. 19167, line 16. Added in the sentence: “However, it is important to note that the version of the dataset used includes interpolation. Therefore, a limitation of this comparison is the shortage of wintertime observations. This...”

Section 3.2: This would be the opportunity to link to the heritage of the model.

This is a nice suggestion. We have updated the following text

p. 19167, line 24. Added the sentence, “...future projection, and sensitivity simulations. This project included precursors to ACCESS-CCM model, such as the UМУKCA-UCAM and UМУKCA-METO models, with the model

improvements since then described in Section 2.”

Section 3.6: What do you mean with “MLS CIO measurements has taken into account all data quality control considerations”?

MLS measurements are supplied with different data screening parameters. It is recommended that the user apply them to the data before being used scientifically. We have updated the following text

p. 19169, line 7. Changed the sentence: “...comparison of the model data with the MLS CIO measurements has taken into account all data quality control considerations.” To: “...data quality control considerations, such as, precision, quality, status flag and convergence (see Livesey et al. 2011).

Section 4.1: TCO 2001-2010, why this period? Later you seem to exclude 2002.

We decided to keep the same amount of years when comparing dataset climatologies. For this case, we chose 10 years over the 2001–2010 period. In other cases, for example the ozonesonde comparisons, we chose 2003–2012, as Davis observations did not extend before 2003.

However, we agree that including 2002 is not the best practice, as it is an anomalous year. Also, in keeping with other comments, we have since altered the temporal range used in all climatologies in the paper: over the 2005–2010 period. This shortens the climatology period by 4 years, but is the longest period available for all datasets used, and gives very similar results as before.

Section 4.2: How does this compare to the more comparable UMUKCA based models?

We have incorporated time-series comparisons with the UMUKCA models and introduced a description of ACCESS-CCMs differences compared to the UMUKCA predecessors.

p. 19166, line 23: Included the paragraph: “The ACCESS-CCM model is a direct successor to the UMUKCA-UCAM and UMUKCA- METO CCMs that contributed to CCMVal-2, the second interaction of CCMVal. A number of advancements to the model were made since. Regarding the stratospheric chemistry scheme. The UMUKCA models and ACCESS-CCM both follow Morgenstern et al. (2009), with only minor adjustments made to include the halogenated very short lived substances: CH₂Br₂, CHBr₃, update the advection of total nitrogen. Other more major changes to the chemistry in ACCESS-CCM are the introduction of FASTJX instead of FAST-J2 (Bian and Prather, 2002), the introduction of tropospheric chemistry, approximately doubling the number species and reactions included only in the stratospheric scheme (O’Connor et al., 2014), and the addition of isoprene for tropospheric chemistry. In addition, the UMUKCA models used HadGEM1 as the

background climate model, with the major updates in HadGEM3 being to the convection, cloud and boundary layer schemes, among others, described in Hewitt et al. (2011)."

Analysis description corresponding to Figures 2 and 3 has also been updated accordingly. Also, please see Figures 1 and 2 at the end of this document for the updated figures that include UМУKCA comparisons.

Do you have a feeling for the model biases without the chemistry? In other words, is there a way to distinguish/quantify the bias due to the interactive chemistry?

This is an interesting question. Unfortunately, with the current model setup, we can't separate model biases without chemistry. However, an interesting way to look at this question in more detail is to look at CMIP5 simulations, which prescribe zonally averaged ozone. This can result in incorrect simulation of stratospheric and tropospheric climate (e.g. Gillett et al. (2009), Waugh et al. (2009)). For example, Waugh et al. showed that ozone's influence on the SAM was underestimated within CCMs when zonally averaged ozone was prescribed.

What is the (possible) impact of the "coarse" horizontal resolution?

Coarse model resolution could definitely be a reason for model biases. Fine scale structure, such as gravity waves, is not captured as well in the model compared to the ERA-Interim (not shown in the paper). This could be a cause for cold biases in higher latitudes, for example, see Austin et al. (2003) (doi:10.5194/acp-3-1-2003). Therefore, we have added in the following sentence.

p. 19172, line 13. "The large cold biases seen at 50 and 30 hPa may be due to reduced heat flux in the model compared to ERA-Interim (not shown). A possible cause of the reduced heat flux could be the coarse resolution of the model inadequately representing fine-scale structure (e.g. Austin et al., 2003)."

Section 4.3: Why not a common period (as long as possible)? Excluding 2002 might be sensible (depending on the variability of the model, which could be discussed more), but maximising a mismatch seems counter-productive to me.

Thank you, we have since updated the comparison time period to be consistent amongst the datasets for all climatology comparisons to 2005—2010, which is as long as possible.

Is there an issue in how you represent the position of the station in the model? Would it help to clarify the position of the stations relative to climatological features of TCO (strong gradient, zonal asymmetries)?

This is interesting and a good point. The location of the stations was definitely a concern for the case of Davis station, as this is likely near the edge of the polar vortex. This was the reason for our inclusion of the South Pole ozonesonde site: to make sure the conclusions at Davis were not because of sharp gradients and misrepresented zonal asymmetries. Other lower latitude sites are outside the polar vortex region, and therefore less influenced by these potential affects associated with the polar vortex.

p19175, l12: Sounds very vague and needs more explanation or a slightly improved discussion . . . might be related to the systematic biases (and the resolution) as well. Again, using different periods for MLS data and the model doesn't help. I think a common period would help. In addition: Are you comparing like-with-like, how do you average?

This sentence was designed to introduce the CIO comparison, but we agree that other potential biases, such as resolution need to be added to the discussion. Also, to alleviate any concerns, the MLS comparison now uses the same periods. Including only 3pm values averaged over the grid box site of Davis. The following sentences have been changes:

p. 19175, line 12. "The large differences seen in the vertical structure of perturbed springtime ozone between the REF-C1 simulation and ozonesondes is either chemical or dynamical in nature, or some combination of both." To "Apart from any systematic biases, such as due to the coarse resolution of the model, the large differences seen in the vertical structure of perturbed springtime ozone between the REF-C1 simulation and ozonesondes are either chemical or dynamical in nature, or some combination of both."

Section 4.4: I am not quite convinced by the SAM discussion (and why does ERA- Interim finishes early?). I find any trend hard to see from the data. It is apparent that Ref-C1 and Ref-C2 differ (because of the SSTs and sea ice), but what is it telling me? Is the interactive ozone more important in forming the trend/long-term variability than the prescribed boundary conditions? You touch on this, but I feel the point needs to be made stronger.

Thank you for pointing out the ERA-Interim data finished early, this has since been rectified.

Unfortunately, with the current model setup, we cannot completely distinguish between the effects of stratospheric ozone and prescribed SSTs and SICs on the Southern Annular Mode. This is an unfortunate drawback of the simulation setups. We have also run sensitivity simulations with fixed GHGs, SSTs and SICs at 1960 levels (CCMI SEN-C2-fGHG), and you can clearly see a distinct influence from stratospheric ozone changes on the Southern Annular Mode. We have not included the SEN-C2-fGHG simulation in the paper plots, as it is outside its scope. However, as you suggested, we also think that it is

important to stress ozone's role in modulated this aspect of tropospheric climate, therefore, we have added in the following.

p. 19179, line 5. "With the current model setup, we cannot completely distinguish between the influences from stratospheric ozone changes, GHGs, and the prescribed SSTs and SICs. It is clear that the REF-C1 and REF-C2 simulations are distinct from each other, with the only major difference in the simulation setups being different SSTs and SICs. This indicates that SSTs and SICs are having a noticeable influence. However, the influence from stratospheric ozone has been captured in a sensitivity simulation with fixed GHGs, SSTs and SICs at 1960 levels. This simulation (not shown), shows a clear influence from ozone on the SAM, indicating that the increasing trend in the summer SAM shown here, in REF-C1 and REF-C2, is influenced significantly by ozone."

Given the emphasis you formulated in the beginning you could provide some more information on regional (Australia as a big region) impacts (maybe using a revised Figure 8).

Thank you for pointing out the missing link to the introduction. The following paragraph has been added

p. 19179, line 5. "South East Australia is likely to experience a higher probability of rainfall due to a positive SAM trend during summer. This is due to a southward shift of the westerly winds resulting in more prominent easterlies over this region, enhancing orographic driven rainfall (e.g. Thompson et al., 2011). However, the slight increase in the SAM seen during autumn in all datasets will have a different effect, as in this case, a southward shift of the westerly winds will decrease the penetration of cold fronts northwards."

Updated Figures

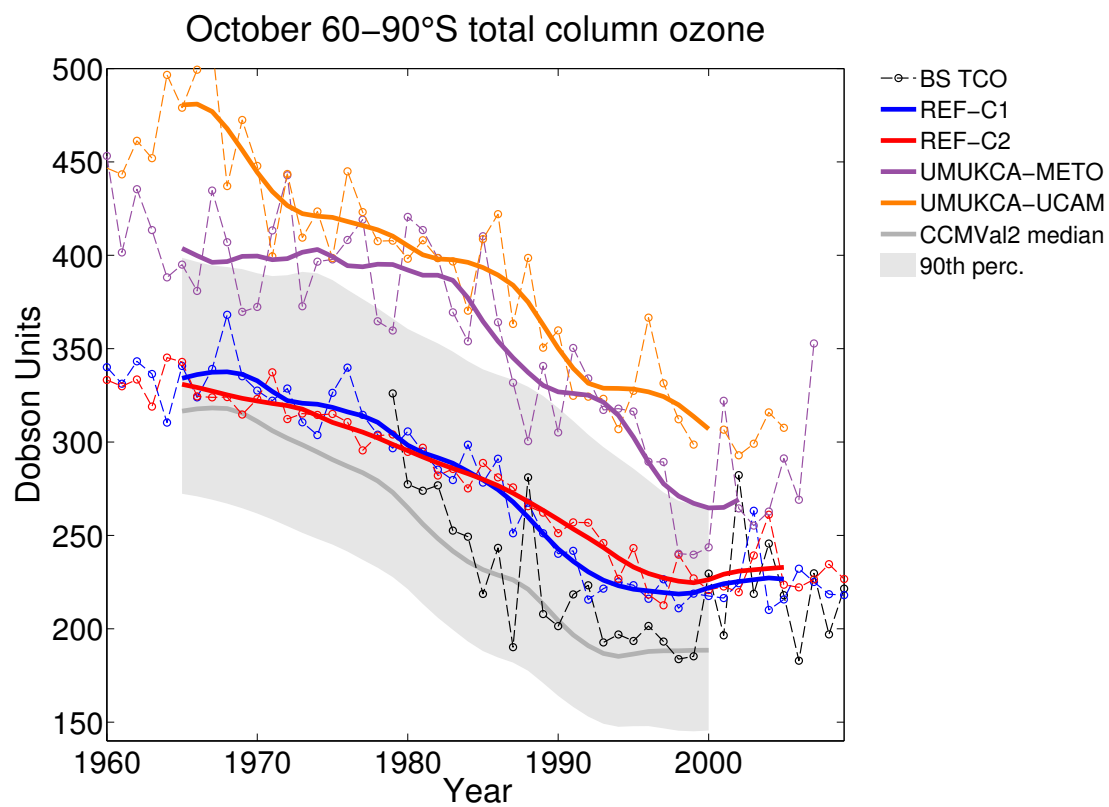


Figure 1. Update to figure 2 in manuscript.

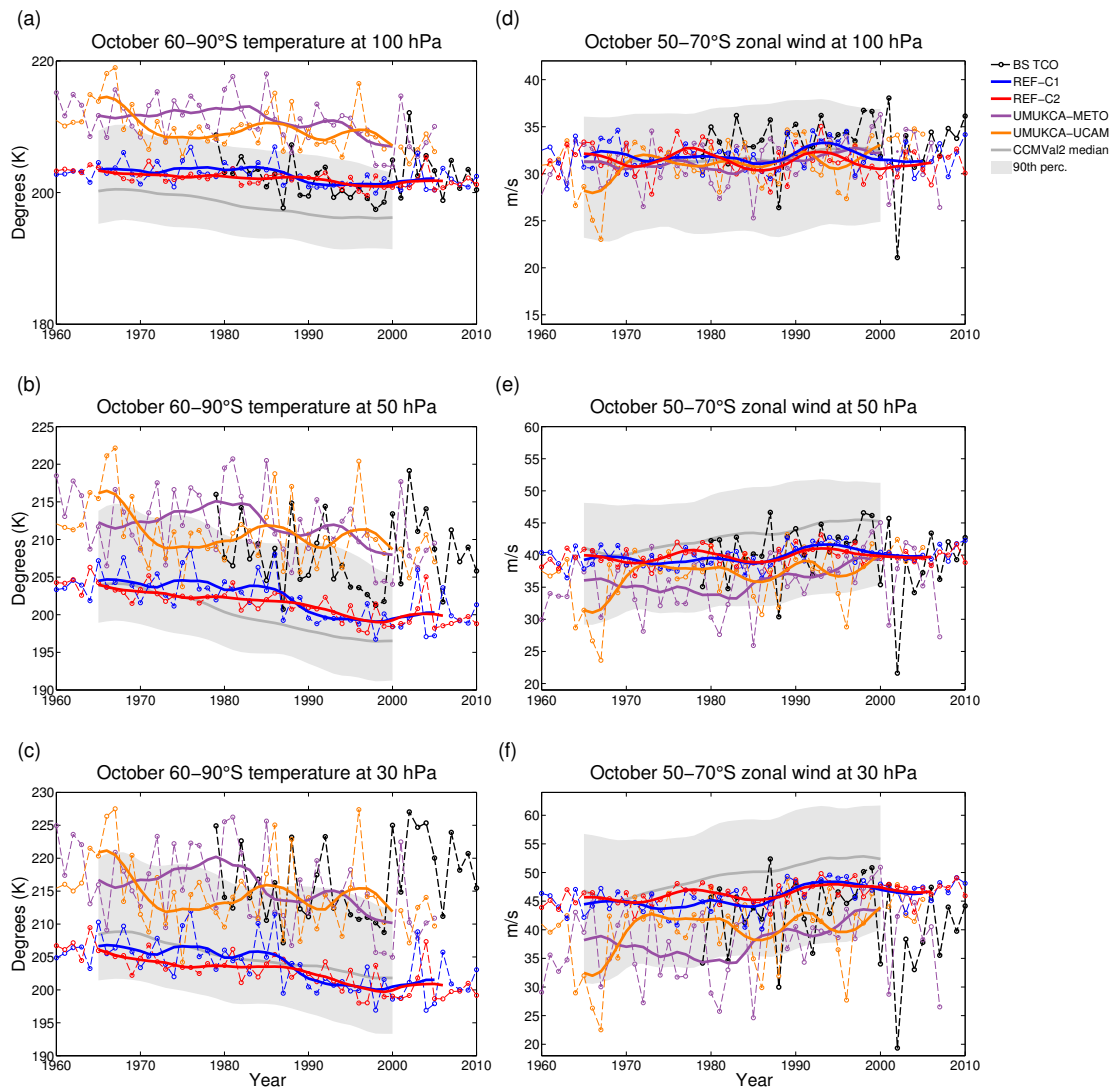


Figure 2. Update to figure 3 in manuscript.