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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of the Australian Community Climate and Earth-System Simulator Chemistry-Climate Model" *by* K. A. Stone et al.

Anonymous Referee #1

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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. 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

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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.

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 titel and abstract of the paper.

Section 2: Why is the changeover for Ref-C2 in 2005? I don't think it matters, but is 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 mayor driver for SAM changes, I believe.

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

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

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

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

Section 4.2: How does this compare to the more comparable UMUKCA based models? 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? What is the (possible) impact of the "coarse" horizontal resolution?

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. Is there an issue in how

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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)?

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?

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. 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).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 19161, 2015.