

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

Anonymous Referee #2

Received and published: 5 September 2015

SUMMARY

This manuscript presents an evaluation of the Australian Community Climate and Earth System Simulator-Chemistry Climate Model in the Southern Hemisphere, focusing on the model's representation of Antarctic ozone depletion, stratospheric temperatures, the polar vortex, as well as past changes in surface winds. To this end, the model data are compared to both observations (ERA-I, Bodeker Scientific, ozonesondes) and multi-model datasets (CCMVal2, CMIP5). While such model-measurement comparisons are needed to improve the current generation of chemistry-climate models and to gain confidence in their performance, this study lacks new or significant results that

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would be of interest to the wider community. Also, the comparisons are not performed according to current best practices (see detailed comments below). I hence cannot recommend the manuscript for publication in the current form. I also suggest the authors to consider submitting this manuscript to the GMD journal instead of ACP, since it would fit that journal's scope and readership better.

GENERAL COMMENTS

1) The current discussion of the results of the model is very descriptive and does not yield much insight on model performance or improvements over a previous (or parent) model. While it is good to see that the ACCESS-CCM does not completely lie outside of the CCMVal2 range, this was not to be expected anyhow since models with overall rather weak performance metrics are included in the CCMVal2 database. A more insightful comparison could be achieved by highlighting/adding also the differences between the NIWA-UKCA model that was used in CCMVal2 and forms the basis of the new ACCESS-CCM. Commenting on improvements in the performance in comparison to the NIWA-UKCSA model and reasons for possible improvements would be more valuable to the wider community than the current results.

2) The bigger problem I see with this manuscript however is that the authors try to interpret results from model-measurement comparisons that do not live up to the standards to current best practices, e.g. comparing the same time periods or accounting for potential sampling biases (as explained below in the case of CIO satellite measurements). Due to these deficiencies the conclusions of the paper (or explanations for processes/mechanisms behind the differences between the model and the observations) cannot be trusted either.

SPECIFIC COMMENTS

(Note: page/line numbers indicated are from printer-friendly version of manuscript)

P19163 L12-14 Please specify why these model intercomparisons are expected to be

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of any help, e.g. 'which focus on process-oriented evaluation of model performance'.

P19163 L20 change to 'halting and reversing the' since otherwise the full success of the Montreal Protocol is not acknowledged.

P19163 L21-23 My understanding is that the community moved away from defining ozone recovery according to the three phases defined in the 2006 WMO ozone assessment, since the second phase does not explicitly account for the impacts climate change can have on ozone. E.g. increased stratospheric temperatures or increases in the Brewer Dobson circulation can alter ozone distributions and lead to an apparent ozone recovery, which may not be attributable to the decline in stratospheric chlorine and bromine. Also, knowledge has advanced since Dameris et al. (2014). A study by Shepherd et al. (Nature Geoscience, 2014, doi:10.1038/ngeo2155) now disentangles the effects of climate change and decreasing EESC and shows that ozone recovery is in fact already taking place.

P19164 L2-3 This impact does relate to the Earth system, in particular humans and ecosystems, but the way it is placed in the text implies that UV changes may affect climate. I suggest adding some clarification.

P19166 It is not clear whether the model underwent specific improvements since Morgenstern et al. (2009).

P19167 L8 Why do you choose to follow RCP 6.0 after 2005? An explanation should be given, since this is a puzzling difference to the use of the first simulation.

P19167 L17 If it is not prudent you should not do it. Please rephrase to something like 'a limitation to the comparison is given by the absence of winter-time observations'.

P19168/9 All descriptions of observations (3.1, 3.5, and 3.6) need statements about measurement quality and stability. It is not clear what you mean by having taken into account 'all data quality control considerations' in 3.6.

P19169 L8-12 It is not clear to me why and how you account for the a priori of the
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measurement. Please improve this description.

P19170 L7-15 The discussion of the figure seems limited. In order to be more valuable to the scientific community, it should also include a discussion of potential limitations of the Bodeker Scientific TCO database. For example, the comparisons by Hassler et al. (ACP, 2013, doi:10.5194/acp-13-5533-2013) of different TCO databases indicate that the Bodeker Scientific TCO database may be low-biased in the tropics and at high latitudes.

P19173 L1 It is not clear how you determine the largest differences. Do you take the maximum difference anywhere along the profile even if it were to be in the troposphere? If so, I wouldn't see the value of having the table without indication of the altitude these numbers pertain to.

P19175 L 25 to P19176 L4 I don't agree that this is a fair comparison. CIO has a very strong diurnal cycle, especially at altitudes below 10 hPa and with night-time values that often come close to satellite instruments' detection limits. The differences can be expected to be larger in winter than in summer and vary with height, depending on the availability of sunlight. The following discussion (L5-23) of potential model shortcomings seems therefore too hypothetical. The comparison should be repeated for day-time values only (for both model and Aura-MLS) to allow for a fair model-measurement comparison. If you have already found that the results do not depend on taking into account the diurnal cycle, then you will need to show this in the paper or provide references that argue for the validity of the approach. PS: L14-16, Or maybe rather the inability of the authors to make a valid comparison?

P19176 L5-25 See previous comment. This may or may not be true.

P19178 L7 I am not convinced that there is a direct link between the differences in the SAM trend and the ozone depletion in the two simulations. Strong ozone depletion after 1980 is seen in REF-C1, but there is no equivalent change in the SAM, instead the SAM stays more or less flat after 1980 in this simulation.

P19180 L20 You mentioned earlier that the heat-flux comparison showed discrepancies between model and ERA-interim. Maybe a too weak heatflux led to the too cold temperatures in the Antarctic middle stratosphere, which in turn may be the reason for a too strong ozone depletion, and not the other way around? I don't think your evaluations allow for a conclusion of this chicken-and-egg problem.

P19180 L23-25 See comments above on the validity of your model-measurement comparison, I hence don't agree that you have attributed the differences in ozone to deficiencies in the representation of ClO in the model.

P19181 L6-9 This seems to contradict your earlier statement (P19180 L12-14) that the ozone vertical profile at Melbourne shows very good agreement between ozonesondes and model during all seasons. The problem may be that the Bodeker Scientific TCO database indeed has a low bias as also indicated in Hassler et al. (2013)? In other words, there may be an inconsistency between the TCO and ozonesonde observations you use for your comparisons? Or did you mean inside the polar processing regions?

P19190 Figure 4 Why did you not make an apple-to-apple comparison using the common time period 2003-2010? I understand you do not want to include 2002 due to the ozone hole splitting event that year. It seems however dangerous to include years with different EESC loadings, the way it is done currently.

P19194 Figure 6 Same as for previous figure, comparison should be made over the same time period so to avoid potential sampling biases resulting from trends in the ClO species.

TECHNICAL COMMENTS

Title:

I expected a much broader evaluation of the performance of this model than what is offered. Please specify. E.g. suggest to fit in something like 'Evaluation of Southern Hemisphere chemistry-climate processes in the ...'. Main problem really is the

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humongous name of the model under evaluation, but I assume its name cannot be changed anymore.

P19163 L10&12 Needs some references (see WMO, 2014 and references therein)

P19167 Section 3 Observational datasets You list here both observational and model datasets you are comparing to, so this title is not adequate. Suggest changing title or moving the model data used for comparisons into a new section.

P19168 L3 I don't think that you can evaluate the performance of your model with earlier model data, since these may be wrong too. You can at best compare them to each other to test improvements or consistency.

P19190 Figure 2 Why are the CCMVal2 data limited to 1965-2000, while the data should be available from 1960-2005?

P19172 L25 Please provide references that provide the theoretical backing for this approach.

P19175 L7 'radiatively active gas' → 'radiatively active gases'

P19181 L4 'possible' → 'possibly'

P19182 L3 where/what is the CCMI web portal?

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

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