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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of CESM1 (WACCM) free-running and specified-dynamics atmospheric composition simulations using global multi-species satellite data records" *by* Lucien Froidevaux et al.

Anonymous Referee #2

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The manuscript aims to evaluate the stratospheric composition of the free-running and specified-dynamics version of CESM1 (WACCM). The evaluations are based on comparisons to satellite measurements including single-instrument and merged data records. The model diagnostics include zonal monthly mean comparisons, seasonal and semi-annual cycles as well as long-term trends. All evaluations are described in detail and valuable information on various aspects of the model performance is provided. Overall, the manuscript is of great interest for scientist directly working with WACCM or potentially with other earth-system models. Therefore, such a detailed

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Discussion paper

manuscript would seem much more appropriate in a journal focused on geoscientific model development/validation and I would urge the authors to submit it to a journal focused on this topic.

Major comments

1) The paper delivers a lot of valuable and detailed information, however, is overall very long. In particular, the number of figures could be reduced from 32 to around 20. To give one example, Figure 2 is only discussed very briefly in the text in order to illustrate mean biases and annual cycle differences shown elsewhere and could be removed.

2) Differences are often only listed and not explored more in detail. To give one example, model HCl shows systematic differences in the lower stratosphere (evaluation based on Fig. 4) and a discussion relating those differences to shortcomings in the model transport or model chemistry would be interesting. Given the length of the manuscript, one could focus on the gases for which the detected differences are discussed in terms of model behavior (e.g., HNO3). Differences for other gases can be mentioned in the manuscript with the according figures being moved to the supplement.

3) In section 3, existing evaluations of WACCM and the WACCM composition in particular should be discussed. Such references come up in the latter part of the manuscript. If they are given combined in this section, it will easier for the reader to identify what the current challenges are and what is new in this manuscript.

Minor comments:

1) Consider changing the title to 'Evaluation of CESM1 (WACCM) free-running and specified-dynamics stratospheric composition simulations using global multi-species satellite data records'.

2) Page 5, line 31 – Page 6, line 2: This text could be moved to the discussion of the MLS data record in section 2.1.

3) Page 7, line 24: Do you mean all earth system model or just WACCM with the term

‘general model underestimation’?

4) Page 9, line 7 -10: Here, and also in other places, the sentence is too long for easy understandability. Consider splitting into two sentences at the semicolon.

5) Page 12, line 5-8: The statement is made for the upper mesosphere. But isn’t it also true for the stratosphere?

6) Page 13, line 7: MIPAS has been used earlier in the manuscript.

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