

# ***Interactive comment on “Evaluation of the interactive stratospheric ozone (O3v2 module) for the E3SM version 2 Earth System Model” by Qi Tang et al.***

## **Anonymous Referee #2**

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### GENERAL COMMENTS

This paper reports on the new ozone chemistry module developed and implemented in the U.S. Department of Energy’s Energy Exascale Earth System Model version 1 (E3SMv1). The paper is well suited for publication in GMD and I believe will be of interest to a wide range of readers of GMD. While it may look like I have suggested many changes below, none of them are substantive and I expect that the authors can work through these and implement them (or not) within a couple of weeks. As such, I suggest that this paper can be published with minor corrections.

### SPECIFIC COMMENTS

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Page 1, Line 9: Why 'feedbacks'? Often these are just one-way processes and not feedbacks in the strict sense of the word i.e. A affects B and then B either affects A or affects things that affect A.

Page 1, Line 16: Presumably stratosphere-troposphere exchange of ozone was implemented in E3SMv1 but was simply not tracked or diagnosed?

Page 1, Line 18: Satellite observations of what exactly? And what variables are you comparing here between E3SMv1 and UCI CTM? Just ozone or also other variables?

Page 1, Line 21-23: I found this sentence very confusing. I associate the QBO with the stratosphere and here you are talking about 'SST forcing does not match the observed quasi-biennial oscillation' and then 'mostly matched with the UCI CTM'. What does it mean for the QBO to be 'matched with' the UCI CTM? Do you mean that the UCI CTM simulates the QBO in stratospheric column ozone well?

Page 2, Lines 2-4: I see a rather large disconnect between the first and second sentences of the Introduction. I agree with the first sentence but when I think about climate models needing to represent GHG concentration distributions correctly, I think primarily about CO<sub>2</sub>, N<sub>2</sub>O, and CH<sub>4</sub>. My first thought is not atmospheric ozone. A better formulation of the first sentence would be 'Accurate simulation of past climate evolution and projections of future climate depend, rather weakly, on correct representation of atmospheric ozone'. But that is not a very motivating start to the paper. I would suggest rewriting the first sentence so that it better motivates why getting ozone right in climate models matters.

Page 2, line 14: Is it worth defining what 'full' means in this context? I don't know but maybe you should think about it.

Page 2, line 15: Is this true? My recollection, though I may be wrong, is that about 50% of the CMIP5 models had interactive ozone.

Page 2, line 28: It wasn't quite clear to me what you meant by 'greenhouse ozone-

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depleting gases'. I guess you mean the CFCs and HCFCs? Gases with a non-zero ODP \*and\* non-zero GWP? It is an unfamiliar term (to me) and so maybe you want to consider using something better known.

Page 3, line 5: I think you should be more specific and say which 'other model'.

Page 3, line 8: It wasn't clear to me what you meant by 'these errors were not symmetrical'. Presumably the climatology overwrite would also place low tropospheric concentrations of ozone into the stratosphere? But what do you mean by 'these errors were not symmetrical'?

Page 4, line 10: Can you please cite a few papers that support the assertion that 'This model has proven robust and reasonably accurate'.

Page 4, line 13: I guess then that what you really want is a tropopause-indexed ozone climatology e.g. Sofieva, V.F.; Tamminen, J.; Kyrölä, E.; Mielonen, T.; Veefkind, P.; Hasler, B. and Bodeker, G.E., A novel tropopause-related climatology of ozone profiles, Atmospheric Chemistry and Physics, doi:10.5194/acp-14-283-2014, 2014?

Page 5, line 6: 'lower boundary sink' of ozone presumably?

Page 5, line 11: Again it is not clear to me what you mean by 'set to match the observed Antarctic ozone'? Do you mean that tunable parameters in the O3v1 module were set so that simulations of stratospheric ozone using this module would replicate the characteristics of the observed Antarctic ozone hole? If that is what you mean, perhaps that's what you should write.

Page 6, line 12: Is it driven by forecast winds or reanalysis winds?

Page 6, line 13: I know what you mean by 'time-specific observations' but other readers may not. Perhaps better to say 'the true state of the atmosphere rather than a state with the same climate but different weather as would be the case with E3SM'.

Page 7, line 11: Have the MERRA TCO data been validated? e.g. do you see a clear

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discontinuity when you go from measured values to MERRA-filled values in any daily TCO field?

Page 7, line 13: The minimum TCO over what geographic domain?

Page 7, line 21: What is 'milli-cm-Amagats'? I have never seen that before. Wouldn't "1DU =  $2.69 \times 10^{16}$  molecules/cm<sup>2</sup>)" make more sense to more people?

Page 8, line 6: Couldn't you also patch the SCO fields with MERRA ozone data or are the MERRA ozone data not vertically resolved?

Page 8, line 12: I would refer to these as 'biases' rather than 'errors'.

Page 8, line 12: What, exactly, is 'excellent in the tropics but too great at high latitudes'?

Page 8, line 14: But is still biased high right?

Page 8, line 20: By 'STD' I assume you mean standard deviation? I think you need to state that more clearly.

Page 8, line 26: In what way is this 'peak QBO-like variability'. You are not showing anything with a quasi-biennial oscillation time scale.

Page 8, line 28: It is not clear to me at all that 'STD/SCO provides a second test of the overall stratospheric circulation'. I think that you need to demonstrate that far more robustly. It is certainly not self-evident.

Page 9, line 7: Seeing the phrase 'is probably adequate' in a paper does not fill me with confidence. Can't you do the statistical test and make a definitive statement?

Page 9, lines 7-8: It wasn't clear to me what you meant by 'The jump in the longterm UCI CTM STD'? Do you mean the change in standard deviation between the two periods for which the UCI CTM standard deviations were calculated?

Page 9, line 8-9: I have never known a switch in satellite data to cause changes in the wind-driven circulation! That would be very impressive. I think that you need to be

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much clearer in saying what you mean.

Page 9, lines 18-19: I don't understand the sentence 'The UCI CTM scores slightly better because of the high-latitude SCO' at all. First better than what? And second 'because of the high-latitude SCO' is not an explanation for anything.

Page 9, line 25: Does not always perform better than what?

Figure 4: I think it would be worth stating in the figure caption over what period these climatologies were calculated.

Page 10, lines 9-10: I don't think the sentence 'This metric has been a standard test for 2D and 3D stratospheric chemistry models for decades' is necessary. Let's say it had only ever been used once before. Would that make your analysis any less appropriate?

Page 10, line 10: While I could take a guess, it wasn't entirely clear to me what you meant by 'The model goal'. I think you should describe what you mean more clearly so that the reader doesn't have to guess.

Page 10, line 13: In what way are the seasonal upward shifts in the contours in the winter 'odd'? To me, they look entirely as you would expect.

Page 11, line 11: I have no idea what you mean by 'This metric is a tough one'? Tough like Sylvester Stallone or tough like Arnold Schwarzenegger? I was also confused by 'but we will need to add some other models to see how well it works outside of Linoz chemistry'. OK then go and add more models if that's what you need.

Page 11, line 17: This is somewhat true. Equivalent Effective Antarctic Stratospheric Chlorine increased quite a bit from 1990 to 2000 and then decreased more slowly thereafter.

Figure 5: I am surprised that you are using minimum TCO as a metric when Müller, R.; Grooß, J.-U.; Lemmen, C.; Heinze, D.; Dameris, M. and Bodeker, G.E., Simple measures of ozone depletion in the polar stratosphere, Atmospheric Chemistry and

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Physics, 251-264, 8, 2008, warned against using it.

Page 11, line 22: Sorry by 'ozone column' do you now mean SCO or TCO?

Page 12, lines 6-7: So why wasn't O3v1 tuned with a better PSC temperature threshold?

Page 12, line 10: I suspect you mean Figure 3d here?

Page 12, lines 10-13: It is not clear to me what you mean by the 'dynamical conditions'...'remain relatively isolated from the ozone hole chemistry'? Dynamical conditions play a huge role in the efficacy of ozone depletion chemistry in the Antarctic stratosphere. That's what accounts for all of the interannual variability in Antarctic ozone depletion.

Page 12, lines 13-15: I don't understand the purpose, meaning, or relevance of these last two sentences. Unless you have compelling reasons not to, I would suggest just deleting them.

Page 12, line 22: By 'STE flux' do you mean the flux in general (i.e.  $\text{kg/m}^2/\text{sec}$ ) or do you mean the ozone flux specifically?

Page 13, lines 3-4: There is no place for a sentence like this in a paper. Either you did collect 'enough different models with enough similar results' to build a Taylor diagram or you didn't. So which is it? Otherwise what are you hoping for the reader to conclude from this sentence? It seems like speculation with no purpose.

Page 13, line 6: I find this sentence very confusing. What, exactly, is set to the lowest four layers? The tropospheric ozone loss? But why would that be quantitatively equivalent to the STE ozone flux? Maybe I am misunderstanding something here? But if I am, it is possible that other readers would too. I think that you need to explain yourself much more clearly here.

Page 13, line 7: Do you really mean 'averaged over latitude and month'? So you have

a monthly mean for every longitude? I wouldn't understand why you would do that.

Page 13, line 10: But how are you getting zonal means when you averaged over latitude (I am assuming you meant averaged over all latitudes)?

Page 13, line 17-18: Replace 'peaks in May and bottoms in Dec' with 'maximizes in May and minimizes in December'.

Page 13, line 18: Regarding 'the peak extends to Jun'. Here and throughout, there is no need to use abbreviations for months in the manuscript text. That extra 'e' isn't going to blow out your publications budget.

Page 14, line 4: Are you going to be examining the changes between O3v1 and O3v2 in greater detail (i.e. digging into how the coding of O3v1 and O3v2 differs) or are you going to be examining how changes from O3v1 to O3v2 affect the distribution of ozone in the UT/LS etc.? My primary complaint about this paper is that you are being too vague in your writing and it is often not clear exactly what you mean.

Page 17, line 21: I don't understand what you mean by 'defined proportional to the reverse of lambda'. Do you mean the inverse of lambda?

Page 18, line 10: Delete the sentence 'Running these metrics with O3v1, O3v2, and the UCI CTM was informative.' If this wasn't the case you shouldn't have written the paper so to some extent it is self-evident.

Page 18, line 14: In what way is the temperature threshold for PSC formation 'delicate'?

Page 19, line 10: I would suggest informal editorial comments such as 'So we must accept good fortune:' should have no place in a paper. Unless of course you can cite a paper, perhaps from the humanities, that supports the assertion that 'we must accept good fortune'.

Page 19, line 15: I don't like to see phrases such as 'they seem to have much less impact on the fidelity' in a paper. Either it does impact the fidelity or it doesn't. Do a

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test so that you can state categorically which it is.

## GRAMMAR AND TYPOGRAPHICAL ERRORS

I understand that the author's first language may not be English. It is not for me to say, but perhaps the second author could wordsmith the paper? To meet the standard of writing required for this journal, the quality of the writing needs to be improved, unless the journal employs a copy editor to do so. The list of corrections I have listed below is not complete and in some cases may only reflect my own writing style. They should be taken as suggestions.

Page 1, line 20: I think it would be clearer to say 'reduced bias' rather than 'shows improved mean bias'.

Page 2, line 15: Replace 'adopt mean climatological distribution' with 'adopt a mean climatological distribution of ozone'.

Page 3, line 1: Replace 'CTM' with 'CTMs'.

Page 3, line 16: Should this be 'E3SMv1' rather than just 'E3SM'? Likewise on line 23. Please ensure consistency in nomenclature throughout.

Page 4, line 23 and elsewhere: I would suggest you use the word 'shortcomings' rather than 'problems' in this context.

Page 4, line 24: Replace 'it is removed' with 'where it is removed' otherwise the grammar is wrong.

Page 5, line 19: Do you mean 'forced only with observed SSTs'?

Page 5, line 22: Delete 'check'.

Page 5, line 26: I would suggest shifting this sentence starting 'It would be interesting...' to the conclusions section where you might provide an 'outlook' of how this research could be extended.



Page 6, line 6: You have already defined the SST acronym earlier. You don't need to define it again.

Page 6, line 10: Replace 'through the end' with 'through to the end'.

Page 6, line 15: Replace 'are carried' with 'is carried'.

Page 7, line 1: I would suggest 'tropospheric ozone column' rather than 'tropospheric column ozone'.

Page 7, line 8: Replace 'on the NASA's' with 'on NASA's'.

Page 8, lines 21-22: This sentence is grammatically incorrect.

Page 9, line 15: Replace 'suggesting well-captured annual' with 'suggesting a well-simulated annual'.

Page 10, line 3: Replace 'as diagnostic of chemistry' with 'as a diagnostic of chemistry'.

Page 11, line 26: 'frequent minimal values like 1995 and 2001' is worded poorly and should be rephrased.

Page 14, line 12: Replace 'at the lower stratosphere' with 'in the lower stratosphere'.

Page 14, line 22: Replace 'tropopause to the stratosphere' with 'troposphere to the stratosphere'.

Page 16, line 21: Replace 'thermo-dynamical' with 'thermodynamic'.

Page 17, line 3: Replace 'associated to' with 'associated with'.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-293>, 2020.

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