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Geoscientific Model Development Discussions

Interactive comment on "The Met Office HadGEM3-ES Chemistry-Climate Model: Evaluation of stratospheric dynamics and its impact on ozone" *by* Steven C. Hardiman et al.

Anonymous Referee #2

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This paper presents an evaluation of stratospheric dynamics and its impact on ozone in the UKMO HadGEM3-ES model. The authors make comparisons between the free-running and the nudged versions, mainly focusing on stratospheric dynamical properties and total ozone columns, and conclude that the dynamical processes are better presented in the nudged version, although there are still significant biases in simulating stratospheric transport, water vapour, and ozone columns. By comparing the metrics of some dynamical processes that are relevant to simulating stratospheric ozone, the authors also conclude that the present model version is significantly improved compared the previous model version that was used in the CCMVal2 inter-model comparison, for the majority of the tested metrics.

Overall, the paper is well written with sufficient detail; it will make a valuable contribution to understanding how chemistry-climate model (CCM) biases (which are mainly dynamical) impact simulated ozone columns, and can be used as a benchmark for future UKMO CCM development. The paper is appropriate for publication in GMD, after some revisions (see specific comments below). I also encourage the authors to consider the following suggestions.

Suggestions:

Although the paper's structure is clear, I think "Section 3.1 Metrics" would be better placed after the detailed comparisons of dynamical properties and ozone. Moreover, most metrics calculated are not referred to in the following comparisons of dynamical properties and their impact on ozone. My suggestion would be to split the "Results" section into two sections, i.e. "evaluation of stratospheric dynamics and ozone", and "Quantitative assessment, i.e., metrics".

More could be made of the differences in model behaviour between REF-C1 and REF-C2. REF-C1 is usually closer to observations than REF-C2, as expected.

Specific comments:

1) "Ozone concentrations" appear throughout the paper, but the authors only show total column ozone (TCO). So the authors should replace all "ozone concentrations" with TCO. They are not the same, therefore should not be mixed.

2) In the abstract, the last sentence says that ". . . that the nudged models still remain far from perfect": Could you elaborate in which sense these models are "far from perfect"? I suggest to re-phrase this statement, and point out any potential problems in applying nudging techniques. It feels like an empty statement to me.

3) P3L22: the previous version used in CCMVal2 did have interactive lightning NOx emissions and interactive wet deposition although for a much more limited range of species. Dry deposition used offline tabulated deposition velocities (Morgenstern et

al., 2009). Please correct.

4) P6, paragraph 3, you state that nudged simulations do not perform well in metrics of “tropical upwelling and QBO”; could you elaborate on any inconsistencies in treating model’s dynamics in nudging and their impact on some simulated model properties? You may want to mention the idea that wind fields used for nudging may not satisfy the continuity equation, which will negatively impact vertical velocity fields.

5) Section 3.3.1 “Extratropics” only covers high-latitude aspects. I suggest to either re-title the section to “High latitudes” or give some coverage to mid-latitude aspects.

6) P11L24: Replace “ozone depletion” with total column ozone (TCO, the standard notation). You’re not actually quantifying ozone depletion, just total columns. Also L25: Replace “column ozone concentrations” with TCO.

7) P12L12: That is technically correct, but imposing zonally invariant ozone would not improve the situation. Rather than imposing zonally invariant ozone (which would be inconsistent with best understanding of the ozone distribution), would it be more effective to work on the model to improve the factors that influence the phase of these planetary waves, such as orographic forcing? The discussion of how to impose ozone in models that cannot get the phase of the waves correct strikes me as somewhat missing the point.

8) L12L25: I noticed that there is a negative trend in tropical ozone in all simulations, but there does not appear to be much trend in the observations. Please comment on this.

9) P13L2: It is true that convection, lightning emissions, and BB could impact tropospheric ozone, but they are unlikely the main cause for the 10 DU bias in TCOs here. Actually figure 17b suggest that it’s mainly the tropopause height whose variations give you differences in TCO between the simulations. In the troposphere, to partial columns go in parallel (implying there is no significant difference in tropical tropospheric ozone

between the simulations). I think your suggestion that tropospheric processes cause this high bias is insufficiently supported by your findings. If this were purely a tropospheric problem, 10 DU would likely amount to an unrealistic 50% error in tropical tropospheric ozone. More likely, it is due mainly to an error in the placement of the tropical tropopause, which you could establish.

10) P13L21: Morgenstern et al. (2009) is a more appropriate reference here. This problem was not specifically addressed in Morgenstern et al. (2010).

11) P14L7 (cf. Figure 14): That’s surprising, considering there should be a close correspondence between the size of the polar vortex, as defined by a transport barrier, and the ozone hole (which is bounded by that transport barrier). If despite nudging these two still differ, could it be that the reanalyses are insufficiently constrained by observations during winter/spring over Antarctica? Please elaborate on the role of the transport barrier in this.

12) P14L17: Your analysis does not imply errors in any of these processes. To make such a statement, you would have had to compare tropospheric ozone against observations. See above on the role of the tropopause height.

13) P14L27: You did not directly compare this model version against other models, so I suggest to remove this half-sentence.

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