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Interactive comment on "Description and evaluation of the UKCA stratosphere-troposphere chemistry scheme (StratTrop vn 1.0) implemented in UKESM1" *by* Alexander T. Archibald et al.

Anonymous Referee #2

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In this manuscript, Archibald et al aim at giving a comprehensive description and evaluation of the gas phase chemistry scheme used in the Earth System Model UKESM1. A first part is dedicated to the description of the implementation of the physical and chemical processes in the model, a second part to a very detailed evaluation of the model, from deposition on the ground to the upper atmosphere.

In general, this manuscript is very well written and pleasant to read, due to its very clear organization. Many details are given, and they all are usefull to get a precise description of the model, but it gives a very long manuscript. Maybe some tables (in particular tables 2 to 6) could be moved to the supplement. It would not change the

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essence of the manuscript and make it lighter.

Beyond the form of the manuscript, the content is very interesting and well addressed. In my opinion, it corresponds to what is expected from a paper on description/evaluation of a model, that can help the other developers. I would then recommend its publication, after the authors have answered or addressed the following questions or remarks :

1. General remark The authors mention that 2 simulations have been run with the model, one in free run (FR),and the other in nudged mode (ND). That is of course common and very interesting. But all along the manuscript, the evaluation is made sometimes on the FR simulation, sometimes on the ND one without any justification (except in the confrontation to satellite measurements section). Strikingly, for instance, the 4.1 section begins with FR and continues with ND.. Could the authors explain what has driven their choices? Moreover, I could not find a comment on the eventual difference of behavior of the two modes (FR and ND). I think that the manuscript would benefit from being completed by this analysis.

2 Specific comments/remarks

Section 2.2, page 6 line 8-10 : could the author give more details about the solver used in the model? The solver is known to be a crucial point and would deserve a few lines to describe its characteristics (accuracy, conservatism etc..)

Section 2.6.1 page 21 line 41 : 'For VOCs, emissions of all C2 and C3 VOCs are included as ethane and propane, respectively' . Could the author precise if there is any aggregation coefficient linked to the reactivity to the species?

Section 2.6.2 page 22 line 36-38 : 'However, it has been argued that wide model agreement is achieved rather due to model tuning than due to a high level of process understanding'. First I don't understand what this sentence has to do with here. Second, assuming it has its place, what should be inferred from it? It sounds a little too

elliptic for such a precise paper.

Section 3; table 11 : the amount of NOx emissions is set as 130.1 Tg(NO)/year, that corresponds to roughly 60.7 Tg of N. This is considerably higher than the average of previous studies. In Young et al 2013 for instance, ACCMIP models had in average rather 47Tg of N / year, and the variability was linked to the LiNOx emissions, that have been calibrated here. I think it is indispensable that the author give a comment on that point, before comparing to other studies. Maybe this higher emissions could explain some following discrepancies.

Section 4.1 : could the author specify how the average is made (what output frequency is used, and what is the temporal resolution of the TOAR database?)

Section 4.2 : I did not see any global budget of the total dry deposition of ozone in the model (despite a reference to the 1000Tg generally thought to be a good estimation)

Section 4.4 : Comparison to observed H2O2. I find this subsection questionable :1) monthly means are compared to instantaneous measurements, 2) some observations are from the beginning of the 1990's - can we really be sure that nothing has changed ? As the authors mention, caution has to be applied and I think nothing can be really deduced from this comparison. I would simply skip it.

Section 4.5 page 50, line 22-25 : could another explanation simply be that emissions are underestimated in the Northern Hemisphere? Section 4.5 page 51 line 12-14 : the authors mention that the model captures the inter annual variabllity. Could they precise here (or in the emissions section) if an inter annual variability takes place in the emissions? Depending on this, the comment could be different.

Section 4.5 page 52, line 15-16 : the authors mention that higher tropospheric columns of NO2 in summer could be attributed to higher emissions. Figure 5 seems to show that it is not the case. Section 4.5 page 52, line 20 -22 : this sentenceÂă'These biases . . . troposphere)' is unclear to me. Section 4.5 page 53, line 1-7 : this paragraph is not

in agreement with the injections of biomass burning emissions in altitude as described in section 2.6.1

Section 4.6 page 57 line 9 :typo (two times ‘region’)

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