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Interactive comment on "A description and evaluation of an air quality model nested within global and regional composition-climate models using MetUM" *by* Lucy S. Neal et al.

Anonymous Referee #1

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The manuscript by Neal et al. presents the results of new simulations using an air quality model nested within a regional composition-climate model, which, in turn, is nested within a global model. The unique aspect of this work is the high level of consistency between the different models involved. Exceptions also exist, such as the different chemical mechanisms, photolysis treatments, and calibration factors used in the models. These inconsistencies, however, offer some opportunity for further insight. The model is described in detail, and the results for NO2, ozone, SO2, and PM are evaluated against different datasets. A neighbourhood approach is also used to assess the impact of the "double penalty problem" on the comparison.

The manuscript is clear, well written, and within the scope of the GMD journal. The

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methodology used is sound, and the conclusions well discussed (although in some cases that I mention below, a bit more insight would be helpful). Therefore, I suggest its publication following the minor revisions and clarifications suggested below.

SPECIFIC COMMENTS:

Page 1, Line 2: I suggest appending “simultaneously” at the end of this sentence.

Page 1, Lines 14-15: “derived from a combination of modelling and observations” -> be more specific, does this refer to reanalysis data, and if so, which?

Page 1, Line 19: Please subscript “10”.

Page 1, Line 21: “consistency between nested models is also important” -> This looks like a key conclusion, so I suggest being more explicit about what is meant. In fact, after reading the manuscript I am not sure how the results presented in it lead to this conclusion. Can the authors please explain or remove?

Page 2, Line 26: I suggest changing “constituents” to “pollutants”, as otherwise it is a bit unconventional to mention ozone before CO2 and methane as important for climate.

Page 2, Lines 44-45: Not clear what is meant – are they more inhomogeneous than, e.g. cloud distributions?

Page 2, Lines 46-47: Not sure why the second part of the sentence in the parenthesis is relevant here.

Page 5, Lines 140-143: Why has the 2D scheme been chosen instead of the more detailed approach? Is it due to the computational cost?

Page 5, Lines 145-148: Perhaps this sentence would seem better placed earlier on, when the use of CLASSIC is first mentioned. Again, are UKCA aerosols not chosen because of computational cost?

Page 5, Line 157: Is 89 a higher or a lower number compared to what is used in the

standard (non-ExtTC) UKCA?

Page 5, Line 164: Please add space after “ozone”. Also, I suggest adding “simulated” before “oxidant species”.

Fig. 1: I suggest either removing the more coarsely distanced lines from the map, to avoid confusion, or to add tick marks next to them. I also suggest that the resolution of the different domains is mentioned again in the caption.

Page 7, Line 214: Why were 20 years of spin-up needed?

Page 8, Line 251: I suggest adding “in the focus area” at the end of this sentence.

Page 8, Lines 255-258: Does this imply that the calibration described just above will not mean much for AQUM performance? That is a bit confusing.

Page 11, Line 321: The fact that the simulation is for average conditions around year 2000 has been elusive throughout the Experimental Setup section. I suggest clarifying this at the very beginning of the section.

Fig. 2: Why would some spatial structure existing in RCCM over central Britain disappear when moving to AQUM? Also, please place “-2” and “-1” in superscripts.

Page 13, Line 364: I am confused again – why 2001-2005 while earlier it was mentioned that the runs are designed for being representative of $\sim$2000?

Fig. 4: Please subscript “2”, “3”, “10” etc. in pollutant chemical formulas/abbreviations (also in other parts of the text), and change “2p5” to “2.5”.

Page 14, Line 383: “sies” -> “sites”.

Page 15, Line 386: Maybe I am wrong, but wouldn’t the seasonality of emissions over the UK be similar across the different scales? Not sure why the global model in particular would have an issue with the seasonality of emissions.

Page 15, Lines 404-405: So, the photolysis scheme seems responsible. The discus-

sion below is useful, but maybe some further insight would be required here given this counter-intuitive behaviour, i.e. a more detailed model performing less well. At least some basic insight on whether key photolysis reactions for ozone (NO2, O3->O1D) become faster or slower?

Page 16, Lines 422-423: However, the more focused domain performs even worse, which should be mentioned.

Page 17, Lines 447-448: And AQUM performs somewhat better – worth mentioning. Also, the fact that SO2 performs ok is some (admittedly not so solid) indication that sulphate may not be the main contributor to PM biases? May be worth considering, in order to provide a bit more insight into why the PM biases occur.

Page 20, Line 519: Final dot is missing.

Page 21, Lines 570-571: The first reason had been marked with (i), so the second should be marked with (ii) for consistency. Also, this second reason is much less transparent here in the conclusions compared to (i), e.g. for a reader that just goes through the conclusions.

Page 22, Lines 582-583: These reasons are not mentioned in the main text, I think, and should be expanded a bit more either here or there.

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