Thanks to the authors for their in-depth response to my questions and comments, and the addition of the expanded Section 2, which has greatly improved the readability of the manuscript. I'm rating the manuscript as minor revisions at this point, though the latter are things that should be addressed (for example, one of their formulae has an error I missed the first time around – hopefully a typo). I also have a few further comments on the revised / added sections of the manuscript, which should be addressed prior to publication.

My page and line numbers are with respect to the track-changes version of the revised manuscript:

Page 6, line 168. Please mention towards the end of this paragraph that the lateral tendencies were based on finding a best fit of a parameter to observations.

Page 7, line 181. Are there any observations to indicate an annual variation in LAI over time, or is it pretty constant year to year? i.e. could a variation in LAI explain some of the 2015/2013 differences in the model results?

Page 10, line 270, also page 11, line 309: Add a sentence or two at the end of the deposition description to explain how the numerical values of deposition and emissions were included into the model. I think from the revised paper that the they were included into the vertical diffusion solver as individual layer rate of change terms, please confirm in the text. The authors should also clarify exactly what they mean by "mass injection", since it could be interpreted in different ways in terms of the model numerics. For example, mass injection could be interpreted as simply adding or removing mass to/from the model layer, as a separate operator. That approach has the disadvantage of introducing discontinuities in the concentration profile in the vertical, which can then lead to other numerical issues. However, adding the mass as a ->rate of change<- term to the concentration rates of change will provide a much more stable solution without discontinuities. I think (?) that this is what the authors have done – could they please confirm it in the text? It would be best if this could be stated in an equation in the main manuscript or the SI where the emissions and deposition terms are included and their units stated.

Page 10, line 285: "We select a value of k that best improves representation of observed O3 concentrations in 2015": was the same k value used in both years, then? I'm wondering if that might explain some of the differences in model performance between the two years. This along with the 2015-specific sigma-w value should be mentioned in the discussion of possible reasons for differences between the two years.

Page 11, line 297 "extrapolated". It might be better to state this as "extrapolated and bounded by a maximum concentration of 50 ppbv above 2000m".

Page 13, line 360: to what extent might the use of 2013 sigma-w values potentially influence the differences in the model results between 2013 and 2015? For example, Figure S13 was calculated from equations S3 to S5 (which do not depend on sigma-w) above the canopy – and in that above canopy region, the K values look rather different between the two years, with 2013 being more stable during the daytime compared to 2015. Equation 18 in the main manuscript is using 2015's sigma-w for 2013, in that case. Is there any observational evidence (perhaps from other years) that might indicate that the sigma-w (55) values don't vary much between the years? The authors should add a sentence or two acknowledging that the use of 2015 sigma-w values might have a substantial impact on the 2013 results. For example, this could be mentioned as a possible reason for model differences noted at line 535-538, page 14, and could be mentioned as a source of uncertainty prior to the new Table in section 2.3, and on somewhere between lines 634 and 665 of the revised manuscript.

**Page 13, line 372: The author's equation (19) contains an error, hopefully just a typo in the manuscript rather than something that appears in the analysis: the denominator derivative in the storage term should be "dt" not "dx". I went back to the Rummel et al (2007) reference to check this; their equation (1) uses "dt". Could the authors please confirm that they did in fact use d/dt in their calculation of the storage term of equation 19, in their analysis? The equation makes much more sense dimensionally in that case, with the final term being the rate of change of the vertical integration of the species concentration, rather than the derivative with respect to species concentration, as it currently appears in both the original and revised manuscript. This caused me some confusion on the first round of review.

Page 14, line 537: “suppressed” misspelled. Also, I agree with your reasoning here – Raupach and others noted that canopy turbulence can be intermittent under stable conditions – which make it challenging to represent using K theory without time-dependent sigma-w values.

Page 34, line 1135: Note that in the case of regional models, simplified multi-layer models (Makar et al., 2017, Nature Comms) or parameterizations to try to capture the effects while preserving the original model layer structure (Wang et al., 2025, ACP) are being adopted. That is, the community has started to recognize that this is an issue for 3D modelling – the Wang et al (2025) approach avoids the computational issues while incorporating some of the layering impacts.