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Interactive comment on "Biomass burning at Cape Grim: exploring photochemistry using multi-scale modelling" *by* Sarah J. Lawson et al.

Anonymous Referee #2

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This paper evaluates two different models against how they capture transport of chemical and formation of secondary O3 formation for two biomass burning events in Tasmania, for which the plume intersected with measurements taken at Cape Grimm. Different MCEs were used to drive emissions to test the sensitivity to uncertainty in this parameter. Further sensitivity simulations were run without fire emissions from Tasmania, and without emissions from Melbourne.

The paper is reads well and covers an important topic, using interesting set of model experiments and source of data. However, more clarity is needed in describing the methodology and a more quantitative analysis of the data is required to draw the conclusions the authors have drawn. In addition, there are a few sections which seem long-winded and discuss non-essential information, and the paper would benefit from being made more succinct in these sections.

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Discussion paper

I think the other two reviewers have done a thorough job of picking up the main points of contention and so I have tried to avoid repeating them. I mostly add some minor points I think should also be picked up on. If the paper is revised appropriately, along with the comments from the other reviewers, I think the paper would be suitable for publication.

Major corrections:

Section 3.3.1: Please provide some figures/tables showing evaluation of the model windspeed and other meterological parameters against observations.

I would like to reemphasise Reviewer #3 in saying some kind of quantitative/statistical analysis of the data is required, particularly for the interpretation of Figure 5. I struggled to see which scenario supposedly matched the data better, please state exactly what metric you are using to make this decision (peak height etc.) Given that you later show such high spatial variability and missed plumes, I'm not convinced stating which MCE happened to give the best peak height is very illuminating. Perhaps discussing which gives the best ratios (OC:BC, CO:BC etc.) against measurements would be more useful.

Pg 12. The differences between the two meteorological models in recording the O3 peaks must be due to differences in air-mass history, from differences in wind fields. However, the authors only present wind fields from CCAM in Figure 4. Please also present winds form the other model for comparison, and discuss in section 3.1.

Minor corrections:

Pg1, ln12: insert "a" before 'High resolution".

Pg1, ln 18: As you use the acronyms for the two models later in the abstract, I think it would be best to introduce them here.

Pg 2., ln1. Add "further" as in "TAPM-CTM is further used to..." to make it clear you used one of the models for a further set of experiments.

Pg 3, ln 22: changes “kms” to “km”.

Pg 5. ln 11-20. This paragraph repeats statements that were made earlier, but with more references to back it up. I think this paragraph should be moved earlier, replacing the paragraph on pg 3, ln 25-29. Doing this should condense the introduction a bit and make it read more smoothly.

Pg 6, ln 14-6. Please give details on the instruments (with appropriate references) for the BC, CO and O3 measurements.

Pg. 6, ln 18: Does the CTM really not have a name? Just saying CTM seems too general and ambiguous to me. Maybe refer to it as the CSIRO CTM as Emmerson et al., (2016) do?

Pg 7., ln 16-20. Its not clear whether you use the same resolution and nesting for both models. On first reading, I thought you used one for modeling the globe and nested the other inside. Please be consistent with plurals: if referring to both models, say models. If only referring to one, please say which one. Never say “The Model”.

Pg 9, ln 1-18. This paragraph is very dense and not very clear. I think it would work better if you explain the methodology in the first couple of sentences, then describe how all the key species change with increasing MCE in one sentence (referring to the table). Please also discuss the net change in NOx:NMOC ratio, as this is key for O3 formation. I don’t understand why you use temperate biome emissions for CO, and savannah for all the others.

Pg 9. ln 24-8. Please also present the EFs you calculated from the previous work for comparison (perhaps in the table)?

Pg 9, ln 30-Pg 10. Ln 13. Given that you don’t actually use a plume-rise parameterisation, I think this section is redundant. You can merge this section into the previous emissions section; just saying that low energy burn of the fire justified mixing in the PBL with a minimum height of 200m.

Pg 11. The section “Primary species – CO and BC’ should be a new subsection (it is not part of meteorological evaluation).

Pg 16, ln 26-28. This is an important point. The authors also have the perfect dataset to investigate it – presumably they also have data from the courser nests (1km, 3km etc.). Comparison between the finest nest and a few of the courser ones may be interesting.

Tables and Figures:

Figure 6. I think there is a mistake on the labeling of the x-axis on panel b – should these be dates? The caption should be written clearer to say the locations are 1km North, South etc. of the Cape Grimm site.

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