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Atmospheric Chemistry and Physics Discussions

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

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We thank the reviewer for their very helpful suggestions which in almost all cases have been incorporated into the manuscript

After encouragement from all three reviewers we have prepared a detailed Supplementary Section which provides a quantitative assessment of model performance for meteorology and simulated primary BB emissions (BC/CO ratio) and secondary pollutant (O3) concentrations, both in background conditions and during the fire. More detail is provided in response to specific reviewer comments below.

Our response to reviewer comments are prefixed with > Changes to the manuscript are in inverted commas " "

Reviewer 2 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.

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.

> a comprehensive evaluation of TAPM and CCAM meteorology against observations has been provided in the Supplementary section (pages 1-8 and Fig S2-S8), including evaluation of wind speed, wind direction, temperature, humidity and PBL height. The following paragraph referring to the meteorological comparison has been included in manuscript

"Qualitative and quantitative assessment of model performance for meteorological parameters were undertaken for both TAPM and CCAM. Hourly observed and modelled winds, temperature, humidity and PBL are compared and discussed in the Supplementary section (Figures S2-S8). Briefly, both TAPM and CCAM demonstrated reasonable skill in modelling the meteorological conditions, with the TAPM simulations slightly better than the CCAM with respect to the low level wind, temperatures and relative humidity and CCAM simulations slightly better in terms of PBL height."

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.)

> A quantitative assessment of model performance in reproducing concentrations of BC/CO and O3 at the receptor has been undertaken and is presented in the Supplementary section. These measures follow the framework discussed in Dennis et al. (2010), and use the performance goals described in Boylan and Russell (2006) and provide quantitative evidence that the best overall agreement with the observations for both primary (EC/CO) and secondary (O3) species is for the TAPM-CTM run with MCE = 0.89. Further details about the analysis undertaken and resulting changes to the manuscript have been provided in response to Reviewer 3, and in the Supplementary section.

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.

>as suggested, the BC:CO ratio has been used to compare observed and modelled concentrations in the quantitative/statistical analysis in the Supplementary Material

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.

>As requested the winds and BC from TAPM during BB1 have been presented in an additional figure in the manuscript (now Fig 4). As the reviewer is interested in the impact of meterorology on O3, the O3 generated from the fire for both CCAM-CTM and TAPM-CTM during BB1 is now also presented in Fig 7.While the differences in O3 from the fire are partly due to differences in wind fields, they are also due to the absolute concentration of O3 simulated from TAPM-CTM and CCAM-CTM, as demonstrated by Fig 7.

The following text has been added to the manuscript:

"Figure 7 shows the TAPM-CTM and CCAM-CTM concentration isopleths of O3 enhancement downwind of the fire during BB1 at 11:00 and 13:00 on the 16 February. Figure 7 shows that there are differences in wind fields between TAPM-CTM and CCAM-CTM as well as different simulated concentrations of O3 generated from the fire. This is discussed further in Section 3.1.2.".

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

>changed as suggested

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.

>changed as suggested

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.

>changed as suggested

Pg 3, ln 22: changes "kms" to "km".

>changed to 'a few kilometers'

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.

> as suggested we have moved the paragraph discussing sensitivity studies on page 5 line 11-20 earlier, as we agree this makes the introduction read more smoothly. We have however retained the paragraph on pg 3 line 25-29 which discusses the different components of a BB model, because this is important context for the following discussion of challenges in representing each of these components.

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

>changed as suggested, text has been changed to:

"In this work, measurements of black carbon (BC), carbon monoxide (CO) and ozone (O3) are compared with model output. BC measurements were made using an aethelometer (Gras, 2007), CO measurements were made using an AGAGE gas chromatography system with a multi-detector (Krummel et al., 2007) and ozone measurements were made using a TECO analyser (Galbally et al., 2007)."

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?

>changed to CSIRO CTM

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.

>to clarify this, lines 20-24 have been replaced by the following text.

"The models represent two unique (and independent) approaches for generating the

meteorological fields required by the chemical transport model. For CCAM, 20 km spaced simulations over Australia were used by the CTM (with the same grid spacing) to model large scale processes on the continent including the emission and transport of windblown dust, sea salt aerosol and smoke from wildfires. Note that the governing equations for TAPM do not enable this model to simulate spatial scales greater than 1000 km in the horizontal and thus only the CCAM meteorology was available for the continental-scale simulations. TAPM and CCAM 12 km spaced simulations were then used to model the transport of the Melbourne plume to Cape Grim by the CTM (at 12 km grid spacing) with boundary conditions provided by the continental simulation. Nested grid simulations by the CTM at 3 km and 1 km grid spacing utilised TAPM and CCAM meteorology simulated at matching grid spacing. The 1 km spaced meteorological fields were also used to drive a 400 m spaced CTM domain which encompassed Robbin's Island and Cape Grim. This domain was included in the nested grid system because we wanted to better numerically resolve the spatial extent of the fire and the process of plume advection between Robbin's Island and Cape Grim."

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".

>as suggested this has been changed throughout text

Pg 9, In 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.

>Paragraph has been condensed as suggested. As suggested the NOx/NMOC ratio has been included in Table 1, and is discussed in text. Savannah EF for all other species were adjusted to reflect MCEs typical of temperate areas (in line with the MCEs

corresponding to the CO emissions). We have clarified this in the modified text below.

"In previous smoke modelling work, CCAM-CTM and TAPM-CTM used savannah EF from Andreae and Merlet (2001). However, as Robbins Island is in a temperate region, the A&M savannah EF used in the models were adjusted to reflect temperate EF based on the following methodology. Minimum, mean and maximum CO EF for temperate forests from Agaki et al., (2011) were used for lower (0.89), best estimate (0.92) and upper MCE (0.95). For all other species, savannah EF (corresponding to MCE 0.94) were adjusted to EF for MCE 0.89, 0.92 and 0.95 using published relationships between MCE and EF (Meyer et al., 2012; Yokelson et al., 2007; Yokelson et al., 2003; Yokelson et al., 2011). For example to adjust the Andreae and Merlet (2001) savannah EF (corresponding to an MCE of 0.94) to our temperate 'best estimate' EF (corresponding to MCE of 0.92) the Andreae and Merlet (2001) NO EF was reduced by 30%, the NMOC EFs were increased by 30%, the BC EF was reduced by 30% and the OC EF was increased by 20%. Table 1 gives emission factors for the original savannah EF (Andreae and Merlet 2001) and the adjusted EF used in this work. The NOx/NMOC ratios used are also shown, and vary by a factor of 3 between the low and high MCE scenarios, mainly driven by the variability in NO emissions with MCE. The EF calculated from observations are shown for comparison (Lawson et al., 2015).

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

>As suggested we have modified Table 1 to include EF calculated from Lawson et al., (2015). We have also included in Table 1 the MCE corresponding to the EF from Lawson et al., (2015) and Andreae and Merlet (2001).

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.

>We agree. As suggested, the plume rise section has been merged into the emissions section. The text now reads:

"With respect to plume rise, the Robbin's Island fire was a relatively low energy burn (Lawson et al., 2015), and as noted by Paugam et al., (2016) the smoke from such fires is largely contained within the planetary boundary layer (PBL). Given that ground-based images of the Robbin's Island smoke plume support this hypothesis, in this work we adopted a simple approach of mixing the emitted smoke uniformly into the model's layers contained within the PBL. The plume was well mixed between the maximum of the PBL height and 200 m above the ground, with the latter included to account for some vertical mixing of the buoyant smoke plume even under conditions of very low PBL height. The high wind speeds particularly during the second BB event, also suggest that the plume was not likely to be sufficiently buoyant to penetrate the PBL."

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

>this section assesses the impact of meteorology on simulated pollutant concentrations. To make this clearer, the subheading 3.1.1 has been renamed "Sensitivity of modelled BB species to meteorology"

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.

>while we agree this would be an interesting investigation, we feel this is outside the scope of the current paper.

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.

>this is actually the hour of just BB2. The axis has been re- labelled to reflect this (now Figure 8). The caption has been rewritten to make the locations clearer.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2016-932/acp-2016-932-AC2-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2016-932, 2016.

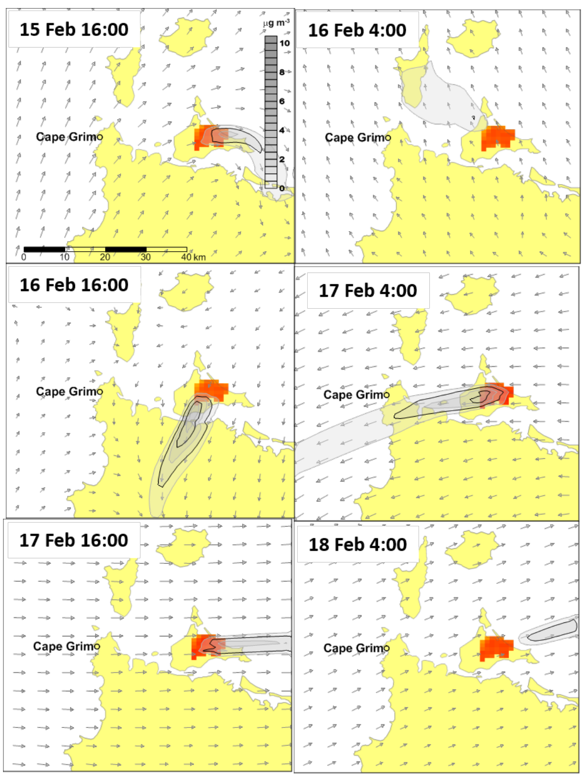

Fig. 1. Figure 4. Model output of BC for TAPM-CTM at 12 hour time intervals during BB1, showing the Robbins Island BB plume intermittently striking Cape Grim, and then the change in plume direction wit

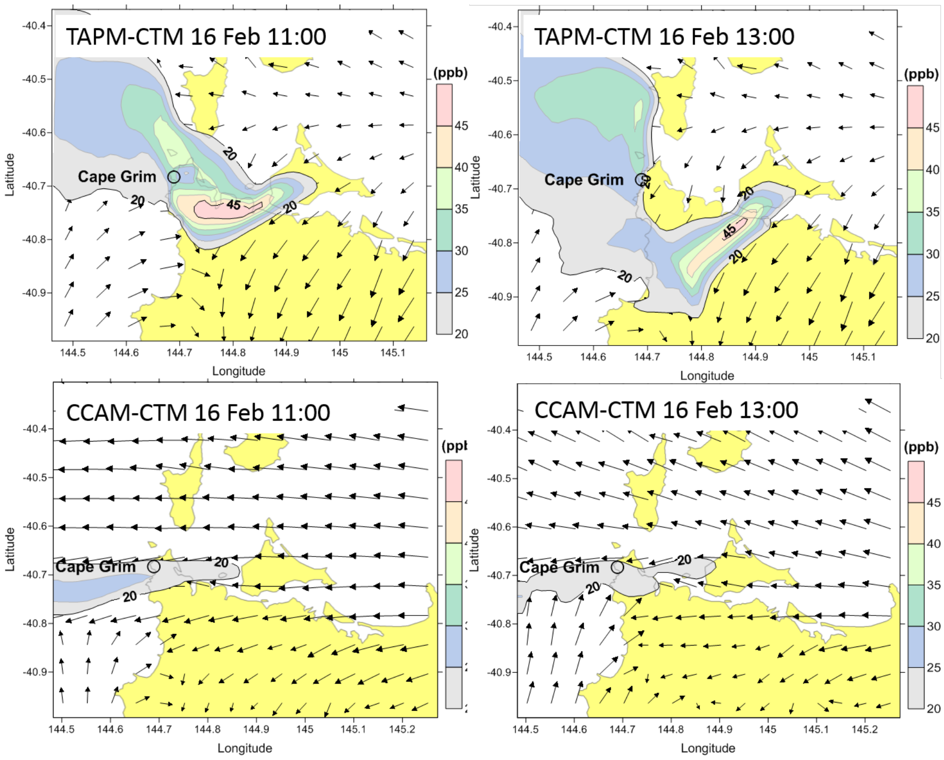

Fig. 2. Figure 7 Model output showing O3 enhancement downwind of the fire during BB1 at 11:00 and 13:00 on the 16 February for TAPM (top) and CCAM (bottom). The spatially variable plume and complex wind field

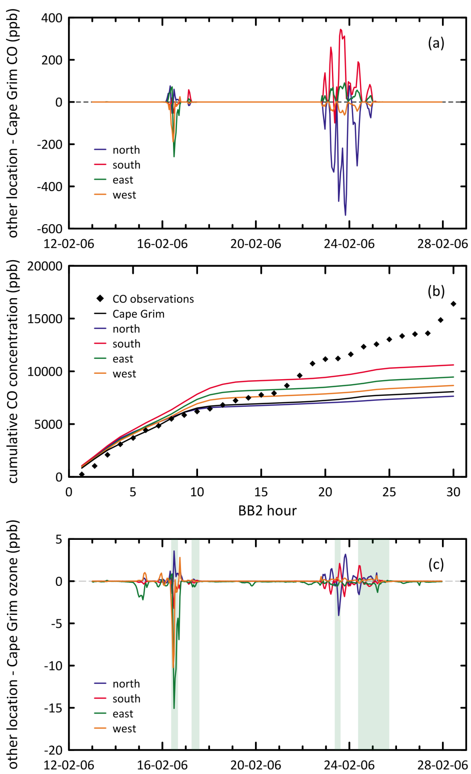

Fig. 3. Figure 8 Simulated spatial variability using TAPM-CTM with MCE=0.89 showing a) time series of CO over two weeks of fire (BB1 and BB2 shown), b) the observed and modelled cumulative concentration of C