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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A study of the influence of forest gaps on fire-atmosphere interactions" *by* Michael T. Kiefer et al.

Anonymous Referee #3

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General Comments:

This paper follows on from several recent publications that have focused on the development and use of the ARPS-Canopy model developed by Kiefer and colleagues. This new canopy model is a useful recent development, and is now being used to study the role of forest canopies in fire-atmosphere interactions. Fire-atmosphere interactions can significantly impact on wildland fire behaviour and therefore directly affect firefighter and civilian safety, so are of considerable importance.

This paper focuses on a specific application of ARPS-Canopy to look at the role of forest canopy gaps on fire to atmosphere interactions (note: no atmosphere to fire feedback is modelled). While this is an important topic that has received little prior consideration, I found the sensitivity analysis to be a little underwhelming. Of the sim-

ulations presented, consideration is primarily given to varying the forest canopy gap location relative to a time-invariant idealised representation of a low intensity fire. The sensitivity analysis reveals what in my opinion is fairly limited difference in the fire to atmosphere interaction between the four cases that include an idealised fire. As a result of this limited sensitivity, I also believe the authors need to be cautious in their conclusions. For example, the authors state, based on their results, that there is potential for forest canopy gaps to substantially affect the vertical and horizontal transport of heat away from the fire. I'm not certain that such a definitive conclusion can be drawn from this set of simulations. I am interested to know if the authors have results for a higher intensity fire, as these may show more pronounced sensitivity.

The methodology seems to be generally robust for a sensitivity analysis of this kind. The various aspects of numerical model configuration are broadly in line with what I would expect. There is good spatial resolution in the forest canopy and the model top, while fairly low, seems reasonable. The one question I would ask the authors is to clarify how the inner and outer model domains interact at the boundary, and if there are any issues regarding the transfer of momentum and turbulent kinetic energy at these boundaries. It may be useful to include additional references, if any exist, to other applications of ARPS that have used a similar high resolution configuration. While the experiment design and analysis methodology seem appropriate, they are somewhat limited in scope. The authors only consider the location of the forest canopy relative to the fire, and otherwise have an identical fire intensity and size.

The paper is well written throughout and well structured. As this is a sensitivity analysis, many of the figures directly compare the same variables between simulations. However, due to the colours chosen for the contours and limited difference in variables between simulations, in a number of figures (e.g. Figs 2-7) it is difficult to clearly discern the key differences between simulations. Additionally, the colour bars shown in Figs 2-7 appear to me to be continuous, rather than discrete, which makes it difficult to properly determine the values in those plots. Figure captions are at times overly

verbose and it could be useful to shorten where possible. In Figs 8-11, some of the labels are difficult to read due to their close proximity to the box and whisker plots. The labels are also given to two decimal points, which seems excessive.

Specific Comments:

Abstract:

1. “This study examines the impact of forest gaps on fire-atmosphere interactions” - it may be more appropriate to state that you consider only fire to atmosphere interactions, as ARPS-CANOPY does not consider the atmosphere to fire feedback.

2. The final sentence of the abstract is a little off-topic relative to what is actually discussed in the paper.

Introduction:

3. Line 81-85: Again it may be clearer to use the term “fire to atmosphere interactions” rather than “fire-atmosphere interactions”. The hyphen suggests to me that there is feedback in both directions.

ARPS-CANOPY Overview:

4. Line 108-112: In these idealised simulations, does the day/night cycle affect the ground radiation budget as discussed here? I notice that the simulation is started at noon local time, so I wonder what effect this has on the simulations as they progress throughout the afternoon.

Model Configuration and Parameterization:

5. Line 134-135: If the Coriolis force is computed as a function of central latitude only, then what value for the central latitude is used? I’m assuming 40N based on details provided later. Is this term important for such a small model domain?

6. Line 143: Is there another term to describe a “rigid lid” upper boundary i.e. what kind

of boundary condition is this? I’m not very familiar with the terminology of boundary conditions, so this comment may be ignored if appropriate.

7. Line 149-150: Can you briefly quantify or more clearly describe the stable stratification above 1 km?

8. Line 153-155: How do the authors determine that a quasi-homogeneous and quasi-stationary PBL has developed? Is there some numerical test based on TKE or some other variable?

Experiment Design:

9. Line 158: “Following a 30-minute spin-up period” - what is being spun-up for 30 min? Based on the previous paragraph, is this the spin-up period in the inner domain simulation once it is initialised (i.e. after 3 hours of outer domain simulation)?

Analysis Methodology:

10. Line 181-182: Do you mention at any point the frequency of the instantaneous wind components and temperature values e.g. is it every second or minute?

Mean Variable Analysis:

11. Figs 2-3: It would be useful to see wind vectors in the xz plane showing the u and w wind compoent wind vectors for the “with fire” simulations, to better show that clockwise circulation within the forest canopy. It is possible to discern it from Figures 2 and 3, but not as easily.

12. Line 220: “unstable boundary layer” I thought that a neutral static stability was used for the background? Or is there some local instability induced by the idealized fire?

13. Line 227 and previously: Acronym “SGS” is only used twice after it is first defined in paper. It may be simpler just to write subgrid scale each time, as its not a particularly intuitive acronym for some readers?

14. Line 239-240: I find it interesting that a superadiabatic lapse rate is evident given the background atmospheric conditions described in the methodology. It is also not specified in which direction the weak horizontal gradient in temperature goes. I think that some additional details could be useful here.

15. Line 244: “a 4% difference” - It might be clearer to state temperature difference as an absolute value rather than as a percentage change. I personally don’t have a good sense of what this 4% difference means.

16. Line 261: “potential to play a substantial role” - this statement seems too confident given the fairly limited difference between the gap and no gap cases.

Instantaneous Variable Analysis

17. Line 270: “the median is about 40% larger than in zone U” Again I would prefer to see absolute values rather than a percentage change. Or why not simply state the new median?

18. Figures 8-11: It is not clear to me precisely why the outliers are calculated for these box and whisker plots with a maximum whisker length, w, of 1.5. I also think that the description of how the outliers are determined would be better placed in the main text, such as the methodology or results section, rather than at the bottom of an already verbose figure caption.

19. Line 283: There is no quantification of distribution width or skewness, so it depends only upon a visual inspection of the box and whisker plots. I think these statements would be better with some quantified data (e.g. standard deviation or skewness, both are easily calculated) to support them.

20. Line 285-286: “the effect of the gap on the median vertical velocity value is ambiguous” I wouldn’t say it is ambiguous, as you then describe the gap effects. Perhaps “inconsistent” would be a better word here?

21. Line 292-293: Conceptually, it’s not particularly clear to me why a reduced-drag re-

gion (i.e. forest canopy gap) would be considerably more turbulent than the surrounding forest. I would have expected the opposite e.g. winds high above the boundary layer tend to be more laminar due to the reduced effects of surface friction. Is it related to the concepts described in the mean analysis e.g. clockwise circulation developing within the canopy gap?

Summary and Conclusions

22. Line 316: It might be useful earlier in the paper to compare 25 kW mˆ-2 to a typical grass or forest fire, to give some physical sense of how intense the heating is.

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