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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A review of approaches to estimate wildfire plume injection height within large scale atmospheric chemical transport models – Part 1" *by* R. Paugam et al.

Anonymous Referee #1

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1. This is a good review of wildfire plume injection height state-of-the-art. I found some minor technical issues, some grammatical issues, and a few significant omissions, all enumerated below. In my opinion, this is worthy of publication in ACP after revisions are made.

2. Introduction, P3, lines 9-10. You have four inventories mentioned, and only three references, so the use of "respectively" is ambiguous here. Also, the following reference might be worth including in the discussion: Kukkonen et al., Applicability of an integrated plume rise model for the dispersion from wild-land fires. Geosci. Model Dev., 7, 2663–2681, 2014, doi:10.5194/gmd-7-2663-2014.

3. Section 2, P5, line 21. I think you mean: "... and therefore even though such large fires..."

4. Section 2, P5, lines 23-26. Since this is a review paper, another example is Mims et al. (IEEE Trans. Geosci. Remt. Sens. 48, 25-35, 2009).

5. Section 2, P6, last paragraph. You could mention here something about the temporal and spatial resolutions that might be appropriate for coupling to CTMs. For example, a CTM might not resolve hour-to-hour differences in an individual smoke plume, even a large one.

6. Section 3, P8, lines 12-15. You might note here that Pyrocumulus convection is much more common than pyro-cumulonimbus events. Another interesting and relatively recent reference is Peterson et al., BAMS 2014, "The 2013 Rim Fire: Implications for Predicting Extreme Fire Spread, Pyroconvection, and Smoke Emissions," doi: http://dx.doi.org/10.1175/BAMS-D-14-00060.1. And I recall Kahn et al. (2007) emphasize the role of entrainment as well as atmospheric stability structure.

7. Section 4.1.1, P10, lines 5-6. Another advantage of CALIOP is sensitivity to low-AOD aerosol layers aloft, even downwind from the source; it tends to retrieve higher plume elevations than the stereo-height or the thermal-band methods due to this sensitivity.

8. Section 4.1.1, P10, line 20. Were there many cases where CALIPO had collocated data over actual active fire areas?

9. Section 4.1.2, P11, line 15. I think the Terra and A-Train are separated by about 3 hours local time (10:30 AM vs. 1:30 PM), not 4.

10. Section 4.1.2, P11, line 18. MISR acquires 9 images at multiple angles (not 8). Also on P12, line 1.

11. Section 4.1.2, Figure 4. I've seen many MINX stereo height images on their web site that are prettier than this one. Since this seems to be an arbitrary example, you

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might pick a more attractive one.

12. Section 4.1.2, last paragraph. I don't think the ATSR technique can retrieve or compensate for cloud motion (due to wind) between the multiple views (I recall they only have two views). More generally, are there any papers that use the split-window, CO2-slicing, or other thermal IR techniques for smoke plume heights? I know these are used for volcanic plumes. Also, proxy techniques for estimating smoke plume height, e.g., using CO sounding, might be discussed here.

13. Section 4.2, P13, last 3 lines. I think the LCL generally refers to a property of the ambient atmosphere. The plume must reach its own lifting condensation level to release latent heat, but this level might not be the same as that of the surrounding atmosphere. You might want to make that clear to readers.

14. Section 4.2, P14, lines 5-7. The factor of 5 range for beta represents the range of flaming to smoldering in practice, but I'm not sure what you mean by a "constant partition," as the phases within a fire can change rapidly.

15. Section 4.2, P14, lines 7-11. In addition to the value of beta, wouldn't this depend on the stability structure of the atmosphere, and maybe also the horizontal wind shear and conditions for entrainment?

16. Section 4.2, P14, last two lines. Maybe say "the best available option. . ." to allow that someone might come up with another idea.

17. Starting on P14, I noticed many minor grammatical errors, missing articles, incorrect word usage, etc. To take just a few examples: "In the later work [should be "latter"], the author use [should be "authors"]. . ." And: P16, lines 7-8. Should be: ". . . the different types of existing plume rise models. In particular. . . the use of a plume rise model. . . which aims to derive a smoke emission system for air quality models such as. . . Here, we limit our review. . . originally built. . ." And P18, lines 4-5: ". . . is that ambient shear a the sub-grid level is not represented. This certainly over-predicts. . .

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more sensitive to . . .” P 19, line 24: “. . . does not differ from . . .” P22, line 1. “. . . the use of PRMv0 improves the results, when compared with . . .” p22, line17. “. . . of these studies emphasize. . .” There are many more, so you might want to have the text copy edited.

18. Section 5.2.1, P17, line 19. Do you mean: “. . . turbulent heat flux. . .”

19. Section 5.2.1, P17, line 22. The convective heat flux is modeled as a fraction of the model flux averaged over the host model grid cell, or do you mean something else?

20. Section 5.2.2, P19, paragraph 2. Both Val Martin et al. (2010) and Kahn et al. (2007) indicate that: “smoke injected above the BL tends to accumulate in layers of relative stability.” Would this be worth mentioning somewhere here?

21. Section 5.2.2, P20, lines 1-3. Didn’t Val Martin et al. (2013) test a range of fire-area and energy flux estimates using the PRMv1 model, and conclude the same thing (underestimation of high plumes)? It might be worth mentioning which methods did better.

22. Section 5.2.2, Figure 7b. This figure is over-plotted too much to be especially useful. You might consider a density plot instead.

23. Section 5.3, P23, lines 1-5. I’m not sure the non-uniqueness of the classes is that large an issue, provided the underlying plume height data set is extensive enough to be partitioned based on regional properties (e.g., surface vegetation type) and environmental conditions. Also, some of the uncertainty in FRP for this application might be reduced or eliminated by the Dozier approach discussed in the next paragraph. Plus, see Note #20 above. In summary, this section could conclude with a position on using the statistical-model approach, with better-constrained FRP and maybe some adjustment based on the atmospheric structure.

24. Section 6, P24, line 21. Do you mean “solve” here – which would exclude statistical approaches – or “account for” – which could include such methods?

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25. Section 6, P24, line 27. I think you mean “Despite the demonstrated diurnal bias of the MISR-derived plume heights. . .” That is, the issue is with MISR’s lack of diurnal sampling, not with the stereo technique itself.

26. Section 5, Captions for Figures 9, 10, and 11. I think Nelson et al. (2013) might be a better reference here, as it is more current.

27. Section 5, P26, top. Another example, for an even larger fire, is Figure 14 in Kahn et al. (2007).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9767, 2015.