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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Fire emission heights in the climate system – Part 1: Global plume height patterns simulated by ECHAM6-HAM2" *by* A. Veira et al.

Anonymous Referee #2

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1. This paper presents the results of a study aimed at tuning an empirical plume-rise model from Sofiev using MISR stereo height data, to provide injection-height input to the ECHAM6 model. The effort results in a significantly more realistic representation of plume heights than the original assumption of the ECHAM6 model. My comments manly focus on how the limitations of the empirical plume height parameterization are presented in the paper – although it is better than the original ECHAM6 assumption, a more realistic plume-rise model, perhaps validated in more detail regionally, might further alter the ECHAM6 simulations significantly.

2. Introduction, P2, paragraph 1. I guess you can define these terms any way you

want, as long as they are applied self-consistently throughout the paper. But generally, "Plume height" means the height of an aerosol layer, whether it is at or near the injection point, or anywhere downwind; "plume-top height" usually indicates the top unambiguously. I agree that as commonly used, "injection height" is ambiguous; it sometimes means the top of an aerosol plume at injection, sometimes the median, and sometimes the profile. Perhaps "emission profile" would be better than "emission heights" when you mean the profile.

3. Introduction, P2, line 27. I think the plume-height uncertainty in the stereo-derived MPHP data is about 250 m [Nelson et al., Remt. Sens., 2013], which is not large for this application. The FRP-based methods are much more uncertain.

4. Introduction, P3, line 2. Another key reference for high-elevation plume injection is Fromm et al. [JGR 2008, doi:10.1029/2007JD009153].

5. Introduction, P3, lines 15-16. I think the FRP values included in the MPHP dataset come from MODIS, which might be worth mentioning.

6. Section 2.1, P5, Equs. 1. I understand what you are doing here, and it is certainly an improvement over injecting everything into the BL, or mixing it uniformly to some arbitrary height. But on physical grounds, we know (and also observe) that smoke injected above the BL tends to accumulate in layers of relative stability above the BL [e.g., Kahn et al., JGR 2007, doi: 10.1029/2006JD007647; Val Martin et al., ACP 2010]. So I'm wondering if using the model stability profile rather than picking arbitrary model layers above the BL might be a better strategy, requiring less subsequent adjustment.

7. Section 2.2. This is just a comment; I include it just to offer one general perspective on the approach. As you know, the main factors affecting plume rise in most cases are dynamical heat flux, atmospheric stability structure, and entrainment. Similar factors dominate cumulus convection (the main heritage for plume-rise modeling), except latent heat release replaces dynamical heat flux as the primary factor generating buoyancy in cumulus convection. So you account for the heat flux parametrically with the

P_f term in equation 2, and the atmospheric stability structure with the H_pbl and N_ft terms. Three free parameters allow each term to be tuned (a) to empirically relate FRP to dynamical heat flux, and (b) to account for the stability structure profile, with entrainment and other possible contributing factors (wind shear, latent heat release, etc.) subsumed in the parametric fit. This is probably not the most direct way to represent the main factors involved, but as you mention, uncertainties in both the parameters and the parameterizations leave more advanced models poorly constrained, and the model applied here does provide enough flexibility so the available validation data can be reproduced, as Sofiev has shown in previous papers.

8. Section 2.3, P6, lines 23-24. I think that unlike the MINX plume heights, the MODIS thermal anomaly data in the MPHP dataset is just copied from the MODIS product. If so, you might want to make that clear in the description here; also relevant to P8, lines 2-3.

9. Section 2.3, P7, lines 14-15. Note that satellite observations have detection limits, so small plumes often go undetected in the MODIS thermal anomaly data, which skews the data set. Also, FRP can underestimate the fire radiant energy flux due to partly filled pixels [e.g., Peterson et al. Remt. Sens. Env. 2013, p. 262-279], due to the opacity of overlying smoke, and other factors. I see now that you get to some of this later in the paper. . .

10. Section 2.3, P7, Figure 1. This is a neat figure in that it gives a qualitative sense of plume distribution and FRP diversity. But it is really difficult to see what the height distributions within the cluster actually look like. You might think about how that information might be conveyed, either within the figure, or as a separate plot or table.

11. Section 2.4, P8, lines 18-19. I would think that a more important reason not to have too many FRP bins is that plume injection height is only loosely correlated with FRP (e.g., Val Martin et al. 2012, Figure 7), so the added FRP-bin precision would represent an over-interpretation of the associated injection height accuracy.

12. Section 2.5, P11, top paragraph. Although MODIS FRP can be underestimated due to smoke opacity at 4 microns, other factors also contribute to the underestimation of injection height that are not due to underestimation of the Fire Radiative Power (i.e., the TOA 4 micron radiance) itself, such as low fire emissivity.

13. Section 3, P13, lines 6-7. This conclusion is very similar to that for the Freitas model, as studied by Val Martin et al. (2012) with a range of fire area and heat flux input combinations.

14. Section 3, P13, lines 14-15. This might be related to the degree to which the FRP must be scaled up to adequately represent the buoyancy produced by the fire. The 30% uncertainty is a small part of the actual uncertainty in the buoyancy that FRP is used to represent (e.g., Equation 6).

15. Section 4, P18, line 6. From P12, lines 7-8: The assumed diurnal cycle “distributes 80% of the FRP constantly during daytime (8–18 Z local time) and the remaining 20% during nighttime (18–8 Z local time).” How applicable is it to the entire global distribution of fires, especially those that inject above the boundary layer, and how strong is your conclusion that the diurnal cycle of FRP is of minor importance (e.g., Abstract, line 19)? I realize it primarily affects the vertical smoke distribution of the largest fires.

16. Section 4.2, P20, Figure 7. I’m wondering what is assumed about the amount of smoke injected by the smaller and larger fires. Wouldn’t this affect the appearance of this figure, as it seems that both amount and vertical distribution should influence the weighting of contributions to the curves?

17. Section 4.4, P22, lines10-11. From my reading of their paper, Mims et al. (2010) had only a few plumes, not a statistical sampling. I think Diner et al. (2008) reported on an early version of the North America dataset analyzed more completely in Val Martin et al. (2010).

18. Section 6, P24, line 16. This conclusion probably says more the way the meteo-

rology and FRP are represented in the empirical parameterization than on the physical factors that affect plume-rise; if you agree, it might be worth stating. Same for the conclusion stated on P25, lines 9-11.

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