Interactive comment on "Two global climatologies of daily fire emission injection heights since 2003" by S. Rémy et al.

Anonymous Referee #3

General Comments.

The paper addresses the relevant problem of prescribing 4-dimensional (time, lat, long, height) emission fields associated with the biomass burning process, a kind of emission inventory that the atmospheric chemistry modeling community is expecting for a long time. The authors developed a 12 years' climatology of daily biomass burning smoke detrainment layers applying two major approaches referred as the 'plume rise model' (PRM, Paugam et al., 2015b) and the Sofiev's semi-empirical formulation (IS4FIRES, Sofiev et al., 2012). The reviewer has a list of general comments which should be addressed before the final publication in ACP. However, I also have a more philosophical questioning which is directed not only to the authors but also to the handling editor of this manuscript. The manuscript relies on the application of methodologies developed and described by the two works cited above. But, the paper from Paugam et al.

(2015b) was not accepted for the final publication in ACP. That is for me an odd situation. I would recommend a revisit of that manuscript to warrant its publication before the present one be accepted for ACP.

Questions/Comments The text suffers from a significant number of grammatical errors and misspellings words, which prevent them to be listed here. So, the manuscript needs a deep proofreading work.

Pag 3, lines 4-7: The authors should discuss the definition of 'injection height' in the context of the flaming combustion phase. Since during the smoldering phase, a large amount of the smoke can also be produced but it is released just above the surface. Pag 3, line 8. Biomass burning also releases latent heat which also play an important role on the plume buoyancy. Pag 3, line 11: explain what do you meant with 'ambient cooling.'

Pag 4, line 9: Rosário et al. (2013) did not assess various injection height algorithms Page 4, line 9: 'MPHP' must be defined. Page 4, line 30: MPHP2 dataset needs a further reference. Page 5, line 9: The main modifications in versions v1 and v2 of PRM model should be described. Page 5, line 10: State clearly which version exactly the term 'PRM' denotes. Page 5, line 23: State clearly the numerical value of the scaling factor of Eq. 1.

Page 7, lines 15 to 21. The comment does not make sense from the physical point-of-view. The atmospheric stability plays a substantial role on convection either the strongly forced (as above a combustion zone) as well as the weakly forced (e.g., as just above the oceans) situations. If PRM produces a convection plume without the fire forcing, it should produce a deeper plume with the additional buoyancy provided by the fire.

Page 8, lines 5-10. Explain how the smoke emission from the smoldering phase is incorporated in the both methods.

Page 10, lines 26-27: Here a misapplication of the PRM is evident. PRM is 1-d column model and does not account for any lateral mixing associated with the turbulence in the PBL. Any injection layer below the firsts few hundred meters, as shown in figure 7, should be disregard since, in the real world, smoke will be mixed quickly in the PBL. This might be one of the reasons why MISR did not 'see' those shallow plumes. Should be instructive to see RMSE and BIAS without those plumes. In the inventory, the emission associated with the shallow plumes should be just included in the surface level, which will be mixed up nevertheless by the turbulence transport scheme of a 3-d atmospheric model.