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Geoscientific Model Development Discussions

# *Interactive comment on* "Development and implementation of a new biomass burning emissions injection height scheme for the GEOSChem model" *by* Liye Zhu et al.

**Anonymous Referee #1**

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This manuscript presents impacts of improved vertical distribution of biomass burning emissions in the GEOS Chem CTM model. The manuscript title is in somewhat misleading because it text is not self-contained regarding the description of the development of injection height parameterization. And actually, critical details about the developed parameterization is referred to an in preparation paper by Val Martin and Kahn. So, this paper should at least be published as a companion paper of the Val Martin and Kahn one. In particular, I believe a fair comparison between the standard and new injection schemes shown in section 3, would require to include the simulated profile of CO using the standard scheme with the increased CO emission.



Questions: Page 3 lines 124-128. The authors wrote: "MISR equator-crossing time during the day is about 10:30 AM, so the diurnal distribution of emissions is not sampled, and in particular, the mid-late afternoon, when wildfires tend to be most intense". The afternoon fires not only tend to produce more substantial emission rates but also they inject smoke in higher altitudes. How is this accounted for in the parameterization of the injection height?

Page 10 lines 318-325. The new injection scheme does not substantially improve the simulation of CO. It not only underestimate the CO amount in the entire atmospheric column but also produces a monotonic decrease from the surface to upper levels, not being capable of to simulate the enrichment layer present just above 700 hPa. The authors should comment on these features.

Page 11, section 3.2 Should be noted that for the Amazon basin 700 hPa is well above the boundary layer. So, the standard model also includes the lower part of the free troposphere.

Page 11, lines 372 . . . The new scheme injects a larger amount of CO above 700 hPa. However, no improvements are shown in the simulated CO profile above this height (figure 8C). The authors should try to explain this result.

Page 12, lines 378. . . I agree that increasing CO by 1.5 produces a better comparison with observations (figure 8C). But, are you not only rescaling? How about increasing by 2.0? Probably, the simulation will be even better. However, all the simulations present a monotonic decrease of CO, at least from the surface up to $\sim$850 hPa, while the observations show a more 'zig-zag' behavior with CO-enriched layers just above the surface and 900 hPa not present in the simulations. How to explain the model behavior? Too much vertical mixing in the model? Too coarse vertical resolution?

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