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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 #1

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This is a scientifically sound paper in which the authors use a GCM to simulate patterns in wildfire emissions heights. They evaluate the performance of their model using the MISR Plume Height Project data and show that the distribution of plume heights in their simulations is more than reasonable. Finally, they show that most plumes do not inject above the boundary layer and that only small increases in plume heights result from major increases in FRP. Overall, the paper is well written, and the science is sound. After some minor corrections, I recommend this paper for publication.

General comments:

I don't have many bad things to say about this manuscript. The authors have paid considerable attention to detail. If I had any general comments at all, it's that the paper does read a bit long in places. Section 4, in particular, feels a bit too long, especially considering that many of the data presented are also shown in tables and figures. The authors may consider trimming parts of this section to help the reader. But this is not necessarily mandatory for publication.

One other general comment: The authors rely heavily on the modified Sofiev plume height parametrization for their analysis. This modified SP model uses FRP as a major driver. As the authors are probably aware, there is not conclusive evidence that FRP is inherently tied to plume height (numerous studies contradict each other). This is probably due to saturation or obscuration remote sensing effects. Nevertheless, I have hesitations with this heavy reliance on FRP, and a word or two on this in the Summary or Conclusions section would probably help clarify.

Specific comments:

1. page 6650 – lines 20-25. For the initial simulations you do no nudge, but for all subsequent simulations you do nudge, so perhaps line 22 should read: "For all other simulations . . .."

2. Page 6651 – lines 12-15 – why did the authors choose 4km? Was this an arbitrary height?

3. Page 6653 – As mentioned in "General Comments" – It is not explicitly clear that FRP is a reliable predictor of plume height. The literature supports this. This caveat should probably be suggested.

4. Page 6653-6654 - It should probably be mentioned that the MISR plume height data you used was all digitized using the red band only. While no conclusive validation study exists (yet), there is evidence that the latest version of the MISR plume height digitization tool (MINX), which includes retrievals from the blue band, is significantly

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more accurate for thin plumes. This could be mentioned. The global data from 2008 (which were not published prior to April 2012) include the blue band. It would be interesting to see a comparison of that new data with your ECHAM6-HAM2 simulations.

5. Section 6 is more of a "summary" and less of a "conclusion." For example – on page 6668, lines 1-10, the authors mention that the use of FRP and meteorological conditions improve the distribution of plume heights, but there is no mention of "why" this happens. Additionally, in lines 20-25, the authors mention that introduction of a diurnal cycle and a doubling of FRP did not substantially increase or modify plume height distributions. Do the authors have any insight on why this is the case? This conjecture would be particularly useful for the community. Certainly the model simulations offer some insight as to why a doubling of FRP does not significantly increase plume height? Also, going back to my initial comments in "General Comments", if a doubling of FRP does not significantly increase plume heights in the simulations, then why is FRP used to "modify" the SP in the first place? It would seem like FRP has less of a connection to plume heights than the authors seem to suggest? Perhaps a sentence or three of discussion on this in the Conclusions would be helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 6645, 2015.