In this manuscript, Veira et al. use a GCM to test the sensitivity of the climate to perturbations in emission heights of wildfire smoke plumes. The authors conclude that the model adequately simulates the magnitude and temporal pattern of biomass burning emissions when compared to satellite data, and that large perturbations in emission heights produce only minor climate variations. Overall, the paper is well written, the methodology well justified and the conclusions appropriate. After some minor corrections, I recommend this paper for publication.

General comments:

I have very few comments about the general structure of the manuscript. Much of the justification for particular methods is discussed in Part I. Two minor points:

1. During discussion of radiative forcing (RF), it is important to note that this forcing is from direct aerosol impacts only (I believe). You touch on this on page 6717, lines 5-10 with: "the RF represents exclusively the radiative perturbation that is introduced by wildfire emissions […] regardless of anthropogenic sources." However, I believe it would be beneficial to the reader if it was made clear that the RF being reported was from direct interference with radiation, and not indirect impacts on clouds or on semi-direct impacts on atmospheric warming.
2. It is not well described in the manuscript why the "SURFACE" simulations actual emit aerosols into the first **two** layers of the model, and not just the surface layer. It seems that if you wanted the more "extreme" lower-boundary you might just emit aerosols into the lowest layer. Probably a sentence or two justifying this decision would clear up this confusion.

Specific comments:

1. Page 6700, line 5, "Does the […] matter on the global scale" – what do the authors mean by **matter**? Perhaps this should say something like: "Does the […] enhance, dampen or change the sign of the globally averaged climate response" (?)
2. Page 6700, line 8. As above, what is meant by "is **appropriate** for […]"?
3. It may be too late to change this, but is there a reason the authors chose "Aerosol Optical Thickness" instead of "Aerosol Optical Depth"? The MODIS product (and in general the aerosol community) seems to prefer "AOD".
4. Page 6702-6703: This is an interesting problem; namely, why the emissions dataset needs to be multiplied by 3.4 to produce reasonable AOD in the GCM, as discussed extensively in Kaiser et al. (2012). Tosca et al. (2013), referenced later in the manuscript, encountered a similar problem, though the multiplier for CESM simulations was closer to 2.0. Randerson et al. (2012) postulated that part of this problem may be due to an under-representation of small fires in global emissions datasets, but this does not seem to address the underlying issue of why (most all) GCMs produce low

biases in AOD with reasonable emissions data input. I wonder if the authors have any insight on this problem, and whether it may be worth mentioning?

5. Page 6703, lines 10-15: Why do the authors choose to inject aerosols into the bottom **two** layers of the model for the SURFACE simulations, rather than just the lowest layer? As described above, this should probably be clarified.
6. Page 6709, line 13: Are these S. Hemi. changes positive or negative?
7. Page 6719, lines 10-15: The wording here is a bit hard to follow. Tosca et al. (2013) do not calculate a 'true' surface RF. What they do calculate is the net change in surface shortwave due to fire aerosol emissions. Their calculations are therefore a response, not a true RF. They do, however, calculate TOA RF (as the authors mention). The way this sentence (line 14) is worded is confusing; it may be helpful to add "However" to the beginning of "In contrast to our study […]"
8. Page 6721, lines 1-3: Why do the authors conclude that a 5-25% change in deposition rates represents only "limited sensitivity." To me, 25% seems to be a reasonably large change.
9. Page 6721, lines 15-23: As I understand it, the calculated TOA RF is for all fire, most of which is probably natural; a back of the envelope guess might assume that 40-50% of global fires are anthropogenic. Since you compare your modeled RF to the IPCC anthropogenic forcing of 0.9Wm-2, it would be worth mentioning that your calculated RF values would be cut approximately in half (?) if we consider only anthropogenic fire contributions.