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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Estimated total emissions of trace gases from the Canberra wildfires of 2003: a new method using satellite measurements of aerosol optical depth and the MOZART chemical transport model" *by* C. Paton-Walsh et al.

**Anonymous Referee #1**

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The paper presents a method for determining emissions of trace gases from wildfires that is complimentary to inventory and burn-scar based methods. The approach relies on an observed correlation between column CO and AOD, which it should be noted must only hold under certain conditions such as those described here (so the method may not be very general). The authors claim uncertainty in their estimates is similar to inventory based estimates such as those from GFED.



While I appreciate the effort and the goal of this work, in a way it's hard to really appreciate how useful this method is. It almost certainly only works in cases where there aren't other sources of aerosol in the affected region (i.e., how do you screen out anthropogenic aerosols or dust, etc., if they are present?). And the uncertainty analysis seems kind of sketchy to me (i.e., "we can make a guess [that the uncertainty is] half this value [the bias due to missing grid boxes]"); a lot of the uncertainty analysis seems like guesswork. I also am skeptical, as detailed more below, about the double-counting estimate in the emissions. I think that element can particularly be addressed more reasonably the paper could be suitable for publication because it is a fairly novel idea.

1. Page 981: "MOD04" is the generic product name of the MODIS aerosol retrieval data set. But what wavelength of AOD are you using in this analysis? Presumably 550 nm, but you should say. 2. Page 983: Regarding the model runs, the argument is that CO destruction by OH is relatively insignificant compared to dispersion of CO. This seems reasonable over the scale being considered. But I don't understand the statement that if this were not the case you should omit CO chemistry in the second model run. Why? What measurements are you referring to in this statement? 3. Page 984: The estimate of the "double-counting" of emissions used in the model run is central to the uncertainty in this analysis. If I read correctly, the emissions used on "day 2" of the simulation are based on the observed enhancement for that day. Some of what is in that enhanced amount is due to CO left over from "day 1" emissions. So the simulated "day 1" distribution of emitted CO are used to correct day 2 a posteriori. The method is to take the remaining "day 1" CO and multiply by the fraction of MODIS grid boxes with AOD values. It's not clear to me what this means or whether it makes sense. I think what is meant is to take the integrated CO of the "day 1" tag in the region and multiply by the fraction of MODIS grid boxes with AOD above the threshold value of 0.2. But this doesn't make sense because I think it implies that that remainder CO is uniform in its distribution (that is, the amount is the same in all the relevant grid cells). What makes sense to me is to screen the "day 1" model remainder CO in the region to retain only model grid boxes (or fractions thereof) that are represented in whatever

MODIS AOD grid boxes meet your threshold criteria. The total of all of that CO is then the amount that you would double count as you applied your “day 2” emissions. I think the assumption here is that the model has perfect transport, which we know is wrong but maybe not too wrong. 4. Page 985, Line 19: I think you mean Eqs. 3 and 4. 5. Page 986, line 14: it should be clarified that when you say larger particles have longer lifetimes you’re talking about sub-micron particles as large compared to nano-particles. I know, this is evident in the subject of the paper, but I’m a dust guy and the sentence just reads weird as written. 6. Page 989: I’m curious about the background and threshold AOD values chosen. Why didn’t you consider the case where background and threshold were both 0.11?

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 977, 2010.