Atmos. Chem. Phys. Discuss., 13, C10253–C10256, 2013
www.atmos-chem-phys-discuss.net/13/C10253/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global top-down smoke aerosol emissions estimation using satellite fire radiative power measurements" *by* C. Ichoku and L. Ellison

Anonymous Referee #2

Received and published: 19 December 2013

This paper builds on the previous work of Ichoku & Kaufman (2005), seeking to improve the approach and reduce uncertainty by applying new data (e.g. MODIS C5) and methodologies. For example, attention to the revising the algorithmic approach to estimating fire AOT is noteworthy and welcome. The authors are commended for addressing uncertainty in their data and algorithm as well as attempting to fill gaps in the data. Comprison with other inventory sources of emissions (e.g. GFEDv3) is helpful, but as the authors suggest, is not yet a matter of validation. Nevertheless, the paper offers the approach and resources (e.g. their website) to continue to improve biomass burning emission estimate using satellite data.

Page 27330, Line 21: Andres et al., 2012 citation not in the reference section.

Page 27336, Line 20-21: Spatial and, especially, temporal resolution seems like it would introduce error. Winds, especially local fire-affected and generated weather can change rapidly. I realize data limitations may not allow a finer detail of understanding, but comparison with local weather station data (when available), perhaps conducting some analysis within the U.S., could reveal the potential and real bias introduced by using this course resolution dataset.

Page 27339, Lines 16-17: "except that for the. . ." – Awkward statement, rephrase.

Page 27338, Line 18 and 27376, Figure 1: this appears to be an ideal case. What happens when wind speed and direction make quadrant selection ambiguous? In addition, and perhaps more importantly, how can you/do you differentiate smoke sources when fires occur across the landscape? I am thinking of savanna regions such as Africa or Australia where widespread fire activity will result in you 3x3 window being "contaminated" with additional fires and thus sources of smoke.

Page 27340, Lines 13-18: Derivation of L – clarity/better description is needed. Assuming the length is based on the dimensions of the downward pixels would seem to be, at times, a gross overestimate of the plume length.

Page 27341, Line 24: Why rationale for 6 points as the minimum. Why not more/less (though the latter seems trivial)?

Page 27342, Lines 4-5: Applying Ce from regional averages to individual pixels, let alone near-real time, sounds like ripe with potential bias. Hopefully this is discussed later, but a caveat should be mentioned. . .e.g. "even in near real time, while addressing/accounting for/keeping in mind the inherent biases. . ."

Page 27343, Lines 10-11: How is the improvement in AOT methodology better than Ichoku & Kaufman (2005)? Refer to later analysis in the paper to support this statement.

ACPD
13, C10253–C10256, 2013

Page 27345, Line 12: Define “semi-randomly”. How do you choose these sites? This last question is addressed, broadly in the figure caption (i.e. fire type, biome, etc...) but should be in the text as well with greater specifics for, presumably, how this is “semi-random”.

Page 27347 Line 12 and Figure 3: Describe the meaning of the color-coding. Although it seems rather intuitive, it is good practice to define color-coded schemes such as this.

Page 27348, Line 12: I would suggest using a different word than “clean” as this implies the data is without impurity...as you go on to discuss it is not and even underlying errors in FRP and AOT estimation might are included in “clean” data.

Page 27349, Line 23 & Figure 5: The difference seems rather negligible, with the greatest differences occurring near the low-end of Ce. It might add value to suggest what the resulting difference is in terms of emission load.

Page 27350, Lines 9-10: Justify why filtering and outlier processing were primarily driven by Aqua-MODIS data. Why was Terra-MODIS not given equal footing?

Page 27352, Lines 10-12: Awkward sentence; considering rewording.

Page 27352, Lines 10-19: This process of gap filling based on a 15°x15° grid cell window would seem difficult to achieve if the requirement is to find identical fire-prone land cover type. Traveling that far from the center, candidate cell would almost certainly incur differences in land cover, land use, and fire regimes for many areas around the globe.

Page 27352, Line 22-29 and Figure 7: It is worth pointing out that most of the gap-fill resulted in low Ce values, even for areas which presumably have higher real Ce (e.g. portions of northern North America and Asia, and SE Asia – Indonesia/Malaysia). In addition, the figures 7b and 7c see to reflect fire counts (high confidence, r2) and areas of persistent cloud cover (low r2)

Page 27358, Lines 17-24 & Figure 9: The GFASv1.0 FRE is a very rough summation

ACPD
13, C10253–C10256, 2013

of FRP and can hardly be considered FRE. Though it may be an available data source of “FRE”, I would strongly encourage you to clearly describe the limitation of this estimate of FRE and corresponding emission especially when comparing with other (i.e. bottom-up) inventories of emissions. You should also point out in the Figure 9 caption that you not just using monthly average FRP, but a monthly “FRE” value generated by multiplying the average by number of days per month.

Page 27359, Lines 27-29: “Globally,..” Reword this sentence, specifically the “constitute only about 55% of it”. “It” is confusing and vague.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27327, 2013.