Atmospheric
Chemistry
and Physics
Discussions

Open Access

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

Anonymous Referee #1

Received and published: 17 December 2013

The study builds on the work by Ichoku and Kaufman (2005), introducing a new revised methodology to estimate global biomass burning emissions using a top-down approach based primarily on MODIS environmental satellite data. The author's reformulation of the previous methodology is clear, and the potential improvement was demonstrated throughout the manuscript. However, there are several limitations that must be addressed in order to make it appropriate for publication. There is a tendency to highlight the advantages of the current methodology (specially compared to bottom-up approaches) while minimizing, or in some ways neglecting, the limitations still involved in the approach. This is reason for concern as it could mislead less in-

formed readers. Provided the study is properly phrased, the authors can maintain its advantages over the previous implementation while making the readers fully aware of the outstanding issues still plaguing the calculation of emission coefficients. Below the authors will find the list of specific comments:

Page 27238 line 27: Should add range of Ce for Russian boreal forest as reported for other regions.

Page 27239 lines 2-9: Consider breaking this one statement into separate sentences. A few other parts of the manuscript also require attention with the use of excessively long statements.

Pages 27239-27332 (Introduction): Please incorporate additional citations - several sentences go without proper referencing (e.g. Pg 27239 line 19->20, Pg 27330 lines 2, 14, 24, among others)

Page 27330 line 3: replace "several decades" with "several years"

Page 27330 line 9: must cite Kaufman et al. 1998 (already in reference list) and Wooster, Zhukov, and Oertel (2003; doi: 10.1016/S0034-4257(03)00070-1) as two of the first studies to introduce the FRP concept and it's relationship with biomass combustion.

Page 27331 line 8: Replace "some serious adjustment" with "a bias correction"

Page 27332 lines 8-10: This statement should be moved to the end of the previous paragraph (remove line separation). Then, start new paragraph with sentence "Section 2 provides. . ." (lines 10-20).

Page 27333 line 14: Replace "satellite measurements of fire and smoke" with "satellite measurements of fire energetics and smoke"

Page 27333 lines 22-23: Replace "That Wooster et al. (2005) study indicated that. . ." with "Wooster et al. (2005) indicated that. . ."

Page 27335 line 1: Delete "when referring to MODIS data"

Page 27335 line 4: Use "Active fire observation products from MODIS. . ."

Page 27335 line 10: The unit (MW) in this case should read W/mˆ2, unless you are assuming a 1kmˆ2 pixel area (the next paragraph eq. 3 is linked to Collection 4, which used W/mˆ2). Please rectify or else clarify the statement.

Page 27335 line 13: When referring to the MODIS Collection, I suggest you simplify it to "4" or "5" throughout the manuscript. The use of the two extra digits "00" is less commonly found in the literature (chances are the products won't evolve beyond the single digit versions anyway).

Page 27335 line 17: "data" is plural here and elsewhere whenever applicable. Found several sentences with incorrect use throughout the manuscript.

Page 27336 lines 15-17: I suggest adding a brief description of the main difference in overall global performance affecting the current study.

Page 27336 line 27: I suggest adding a brief explanation why you chose to work with the 850mb data and then point to Ichoku and Kaufman (2005).

Page 27337 line 21: Replace "have had to be made" with "were required"

Page 27340 line 5: Add ". . . mass extinction efficiency derived from Reid et al. (2005)".

Page 27340 lines 8-11: It would be informative to have a histogram plot of T or at the very least some statistics describing the range of T values encountered. This will help readers interpret your results. For example, I assume the greater T gets the less representative your instantaneous FRP retrievals will become (basically the bulk of the plume will have originated several minutes earlier and therefore under different energy release and biomass consumption rates compared to what the instantaneous FRP retrieval provides).

Page 27340 lines 16-18: How about those cases when active fires are also found within

the downwind AOT pixels (or within upwind AOT pixels with smoke advecting over the fire pixel itself), how is the fire-emitted AOT calculated in those cases? With 10x10 km cells and a 3x3 sampling window I would expect to see many such cases in areas of high fire activity where multiple land-use fires co-exist in a relatively small area (e.g., Africa, Amazonia, South East Asia).

Page 27341 lines 5-25 (ending on next page line 5): There is no real need for this brief intro to the following sections. It provides superficial information only and could serve as a distraction to the reader (to me it looks like the authors are saying "if you don't want to read all the details here is a brief summary for you"). This is a recipe for disaster as it could lead to misinformed users of your product.

Page 27342 line 17: High latitude plumes tend to be large and well-defined. Once again I wonder what might happen in Africa, Amazonia, and South East Asia where the bulk of biomass burning occurs, and where multiple fires can be found within close proximity. Please elaborate adding more information to the readers.

Page 27342 line 20: Should it read ". . . measure plume heights (MISR) . . ." ?

Page 27342 line 25: The assumption is correct, i.e., that the proportion of cells contaminated by background smoke or haze is regionally dependent. I would add that such proportion should be higher in the same global hot spots mentioned above (Africa, Amazonia, South East Asia). The authors should make that clear and also discuss the implications involving the application of the current methodology to those regions.

Page 27343 lines 1-2: Should this statement read "we augmented (or improved) the sampling of AOT representing the plume to include the four downwind pixels, . . .". The original sentence ("we restricted the sampling. . .") doesn't seem to properly describe the direction of change from the previous version of the method, which used the maximum AOT retrieval only. Basically you relaxed (as opposed to restricting) the method in order to include a larger sampling of the plume's AOT.

Page 27343 line 9: In other words, the majority of the retrievals were in areas of widespread burning, where background smoke was detected and therefore subtracted from the fire's AOT. This result highlights the need for improved description of the multiple-fire scenario in both upwind/downwind cells (see comment above).

Page 27343 line 16-17: What about wind speed used to estimate the rate of smoke release (eq.s 11/12)? Isn't that also a requirement?

Page 27344 line 1: Delete "certain"

Page 27344 line 2: Can you list a few examples where much lower/higher injection heights are found?

Page 27344 lines 13-14: Wouldn't it be more appropriate to consider the distance to the center of the downwind pixels instead? Statistically speaking that should better represent (on average) the location of the plumes detected by the AOT product. I am just wondering whether using the distance to the outer edge of the 3x3 window could add any bias to your estimates. Did you estimate the average plume location within the 10km AOT cell by looking at the available 250m RGB data or MISR plume data? What does that show?

Page 27345 line 13: I believe Fig 3 should be numbered "2" and vice-versa. The two are out of order the way they appear in the manuscript.

Page 27347 line 25: So is the global emissions analysis built on 10% of the available data? I am concerned this could describe a highly skewed sample. This is worrisome. I think the authors need to elaborate further on this and address potential limitations more clearly. This is the only sentence I could find in the manuscript stating such limitation with the analyses.

Page 27348 line 1: "Using all available data" in reality means "the 10% of valid data", correct? Please clarify.

Page 27351 lines 16-17: This is a highly subjective decision and a questionable one

as you are admittedly replacing a first (and potentially higher confidence) retrieval with a lower confidence one in order to increase your overall r^2. It's like being right for the wrong reason. This kind of decision must be properly discussed making the potential consequences clear to the reader. Please elaborate.

Page 27352 line 5: The dominant land cover may not describe the actual fire-prone classes although in the next sentence the dominant land cover type is used anyway. This needs some further consideration and or justification.

Page 27352 line 10: A 15x15 degree (?) window is a huge area to sample/extrapolate. Chances are you are mixing different fire regimes as opposed to "identical fire-prone land cover type". More clarification and or warning flags are required so that the reader is fully aware of the limitations involved with the current methodology.

Page 27352 line 17-19: I don't understand why the sample window is configured in such a way, decreasing from the initial 15x15 to 3x3 size. It would make more sense to start with a small sample window (3x3), and gradually grow it (to a max of 15x15) until you reach the minimum number of valid data. Please clarify. Also, the eight cell QA>QAmin requirement seems to conflict with the minimum 3x3 sampling window size. You have a total of eight cells around your target cell (the one to be filled), right? Assuming one cell provides your minimum QA value, there can only be seven more cells to meet the QA>QAmin requirement (also assuming the entire window sample is composed of the same dominant land cover type). Am I missing something?

Page 27352 line 28: Fig 7 itself does not provide enough information to the reader. For example, what is the percentage of cells with QA 0,1,2,3,4? A graph (or table) showing QA levels on one axis (rows) and the r^2 on the other axis (columns) and the percentage/frequency describing each data point (similar to Table 3) would help. For example, for QA 0 x% shows an r^2 between 0-0.2, y% shows an r^2 between 0.2-0.4, and so on for the entire r^2 range and QA classes.

Page 27353 line 4: Replace "rough" with "low confidence"

Page 27353 line 9: I assume "them" refers to FEER.v1 gridded product? Please rephrase.

Page 27353 line 11: I think the authors should organize this section better. You start the first sentence suggesting that a quantitative "evaluation" is required to determine the product's suitability for application. However, you begin with a comparison with the previous implementation of your methodology. That serves no "evaluation" purpose as IK05 builds on a similar methodology and does not constitute a validation data set. Please consider rephrasing.

Page 27353 line 13: The authors must properly introduce the reader to IK05. The text implies that the reader has seen it and is familiar with the previous study, when that may not be true.

Page 27353 line 21: Add "with the exception of East Kazakhstan where they are practically equal".

Page 27354 line 18: Add "In other words,…"

Page 27355 line 17-19: Please be more specific: MODIS data should be available for longer periods (as demonstrated in the current study) and so should the NCEP re-analysis data.

Page 27356 line 6: replace "transitions" with "transition"

Page 27356 line 25: I am having trouble understanding why the MODIS FRP in collections 4 and 5 would cause such an impact when they are essentially the same (the major exception being the pixel area multiplication). Could the authors have incorrectly used that data set with IK05?

Page 27357 line 3: I am assuming "fire-generated" and "fire-emitted" are the same. Please use one definition only.

Page 27357 line 5: Should it be "IK" to conform to Table 5?

Page 27357 lines 8-9: Attributable to what algorithm change? Please cite the appropriate study which documents the mean global increase observed by the AOT product's science team going from Collection 4 to 5.

Page 27357 lines 9: Are you suggesting there was a two fold increase in AOT values between MODIS collections 4 and 5? Please clarify.

Page 27357 line 11: "algorithmic changes relating to AOT" may at first be interpreted as MODIS AOT algorithm changes. Please clarify.

Page 27358 line 17: I am wondering what effect might result from using monthly average FRP values without proper consideration of diurnal variations in fire activity. Errors could be large. A few MODIS FRP data points (assuming multiple detections by Terra and Aqua for the same fire) may not provide a good sample to derive the time-integrated FRE for a fire. Chances are it won't work any better for large regional samples. Please elaborate.

Page 27359 line 26-27: This should be of no surprise as all products are - either directly or indirectly - governed by input MODIS fire counts. Please make that clear to the reader.

Page 27360 line 26: Add "FEER.v1"

Page 27361 line 16: Use "burned biomass estimates to calculate emissions"

Pages 27361-27365: A number of previously defined acronyms are described in full again. Please avoid that.

Page 27361 line 17: Add "used to multiply time-integrated satellite measurements of fire radiative power …". Must highlight the challenges associated with retrieving representative daily FRE totals. Makes it sound artificially simple.

Page 27361 line 25: Not necessarily true - 10% of usable input data reported by the authors and the need to fill in grid cells using reference land cover products indicate

otherwise.

Page 27362 lines 15-16: "there are still possible sources of uncertainty" is clearly an understatement. Several limitations still exist, many of those were highlighted above and should be properly acknowledged here and elsewhere in the manuscript to avoid misleading readers/users.

Page 27362 line 18: Add "uncertainties in the satellite retrievals of AOT and FRP (including the time-integration methods to derive the required FRE data), ..."

Table 4: Add a label (e.g., "Region Average") to identify the corresponding bold values in the table

Table 5: Should it read "... deriving the predicted changes in Ce from IK to FEER.v1 according to ..."?