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Interactive comment on "Biomass burning fuel consumption dynamics in the (sub)tropics assessed from satellite" *by* N. Andela et al.

Anonymous Referee #2

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General Comments:

This paper describes an approach for combining satellite observations of active fires and burned areas to generate maps of average fuel load consumption (kg m-2) across South America, Africa, and Australia at 0.25 degree grid cell resolution. The ability to develop such a map is at the forefront of wildland fire science, and the production of an accurate map would certainly improve our understanding of global biomass burning and the pyrogenic carbon budget. However, as the authors demonstrate and fully admit, there is uncertainty in their map.

Uncertainties in estimates of fuel load consumption undoubtedly stem from the limitations of confidently measuring fire activity from satellite sensors. To address these satellite sensor limitations, the authors compare fuel load consumption estimates ob-

tained from geostationary and polar orbiting platforms. However, as broached in the specific comments, it is not entirely apparent that their technique for aligning the active fire and burned area products completely restricts comparisons between the geostationary and polar orbiting observations to the same fire activity. Satellite sensor limitations are also addressed by calibrating satellite measurements with field measurements and deriving an alternative, sensor specific conversion factor relating radiant heat release and fuel consumption. However their intent for using an alternative conversion factor – rather than applying a bias correction to satellite-based estimates of FRP and FRE – deserves more consideration.

Aside from the techniques used to produce their map of fuel load consumption, the authors do not fairly acknowledge one aspect – and perhaps the one overarching aspect: the role that environmental conditions and fuel moisture contents play in fuel load consumption. Although NPP and time since fire are used to explore geographical differences in fuel load accumulation, little to no credit is given to the impact that environmental conditions and fuel moisture contents have on consumption completeness. It is very difficult to confidently interpret fuel load consumption estimates between geographical regions without knowing the environmental and fuel moisture conditions at the time of burning.

In my opinion, uncertainties in their fuel load consumption map due to satellite sensor limitations, and the somewhat incomplete interpretation of their map in the absence of fuel moisture contents, does not detract from the overall worthiness of this work. It is a very good start in the right direction, and at the very least, exposes areas for further refinement and opens arenas for further exploration. With some further clarification and explanation, I feel that this article can contribute to our understanding of global biomass burning and pave the way for more accurate fuel load consumption maps in the future.

Specific Comments:

1. Page 2, Line 19: For brevity the authors refer to fuel consumption per unit area burned (kgm-2) simply as fuel consumption. Granted the authors state this up front, but this is the only place that it is mentioned. Anyone skipping the introduction and skimming the methods and results might confuse the traditional sense of fuel consumption (kg) with the authors definition of fuel consumption (kgm-2). Moreover, in the conclusions on Page 22 Lines 22-25, are the authors talking about fuel consumption (kg) or fuel consumption (kgm-2)? The pre-burn mass of fuel per unit area (kgm-2) is typically referred to as the "fuel load". The authors should consider whether or not the term "fuel load consumption" better describes what they are attempting to estimate. In my opinion, the terms "fuel load consumption" and "fuel load consumed" are more accurate descriptors that pose less of a chance for confusion.

2. Page 4, Line 26 – Page 5, Line 2: Does a description of the "mean fire return period" belong in the Data section, or should it be moved to the Methods section? Also, I understand that the authors are trying to quantify the amount of time between fires as a way of explaining fuel accumulation and eventually fuel load reduction. However I think they may have their terms confused, and I'm not entirely clear on how the "mean fire return period" is calculated. The authors state that: "We estimated the mean fire return period based on the 14 years of MCD64A1 burned area data, by recording how many times each 500 m resolution MODIS grid cell had burned during the 2001 – 2014 period and then dividing this by the 14 years." According to this definition, the "mean fire return period" in a 500 m grid cell can range 0.07 "fires" per year if the grid cell burned once to 1.00 "fire" per year if the grid cell burned every year (with 0's excluded). First things first: metrics with units of inverse time are frequencies. Rather than calculating the "fire return period," it seems to me that the authors are calculating the fire frequency of a 500 m grid cell. The inverse of the fire frequency is the fire return period, which in this case would range from 1 yr if the grid cell burned every year to 14 yrs if the grid cell burned only once during the study period. On Page 12, Lines 23-24, the authors report a "mean fire return period 1.75 years", leading me to believe that they are computing a frequency, but reporting a period. Please confirm?

Also, the authors state that: "We then calculated the mean fire return period for each 0.25° grid cell as the mean return period of all 500 m grid cells within each 0.25° grid cell, weighted by burned area." Again, from their description, I would expect the "mean fire return period" to be considerably less than 1.00 unless every 500 m pixel within a 0.25 grid cell burned every year, which doesn't agree with values such as 3 – 8 years reported on Page 15, Lines 13-19. Beyond the period/frequency issue, I do not understand how the "mean fire return period" in a 0.25 degree grid cell is weighted by burned area. Please elaborate. To me, it almost sounds like the authors are trying to calculate a "fire rotation", or the amount of time it takes to burn an area equal to the size of the study area. All in all, the authors should confirm and clarify their computation of a "mean fire return period" for a 0.25 degree grid cell.

3. Page 7, Lines 11-20: It seems to me that aligning the active fire and burned area products is absolutely crucial to the estimation of fuel load reduction. Although aligning the active fire and burned area pixels is technically feasible, the underlying "squishy" part of this process (which the authors briefly touch upon) is ensuring that the FRP detected by SEVIRI is only emitted from burned areas detected by MODIS, and conversely, that all the burned areas detected by MODIS contribute to some of the FRP measured by SEVIRI. Can the authors perform a sensitivity analysis to quantify the impact of the 15- day window on their estimates of fuel load consumption? It seems to me that expanding the 15-day window around the burned area detection date would result in more active fire pixels associated with the same burned area and thus result in higher estimates of fuel load consumption. Similarly, contracting the 15-day window would result in lower fuel load consumption estimates. Please confirm, and consider warning the reader about the sensitivity of fuel load consumption estimates to the 15-day window. Also, SEVIRI grid cells with burned area detections but no active fire detections were excluded from the analysis. However the authors never describe how they treat SEVIRI active fire pixels with no corresponding MODIS burned area detections. Were there any?

4. Page 7, Line 23: Please confirm the value of the conversion factor. There are several instances that reference a value of 0.356 kg MJ-1, and there are several other instances that reference a value of 0.368 kg MJ-1. Which value are you using?

5. Page 7, Lines 22-29: Please see my comments concerning the interpretation of the "mean fuel load consumption" calculated using observations accumulated over long time periods (Page 8, Line 27 – Page 9, Line 2).

6. Page 8, Line 27 – Page 9, Line 2: Yes, I agree with the authors here. However I think they are overlooking a critically important aspect. Accumulating observations over long time periods (e.g., over many years) precludes a seasonal analysis. For the moment, consider a hypothetically static pre-burn fuel load that does not vary from the end of one rainy season to the beginning of the next rainy season. For a constant pre-burn fuel load that does not change over time, fuel load consumption will still vary depending on when during the dry season the landscape burns due to seasonal oscillations in fuel moisture contents, which drive seasonal oscillations in consumption completeness (Hoffa et al., 1999). Accumulating observations over long time frames fails to resolve the seasonal oscillations in consumption completeness and thus fuel load consumption. I strongly suggest that the authors warn the reader that estimates of fuel load consumption calculated from observations accumulated over long time periods are more representative of values observed at the peak in fire activity when the satellites detect the most active fire pixels and burned area pixels within a 0.25 degree grid cell.

Here's the really important bit though: the seasonality of fire activity is not always synchronized with the seasonality of fuel moisture contents and consumption completeness (Le Page et al., 2010). Hence peaks in fire activity may not always coincide with identical fuel moisture conditions. Across the majority of Brazil, for example, the peak in fire activity generally occurs when fuels are driest (i.e., the middle of the fire season coincides with the middle of the CBI season, according to Figure 4 of Le Page et al., 2010). In contrast, across much of Africa, the middle of the fire season occurs before

the middle of the CBI season. Therefore, even if the pre-burn fuel loads are identical between South America and Africa, the consumption completeness (%) at the time of peak fire activity would differ between the two locations due to differences in the seasonal synchronization of fire activity and fire weather, which would then lead to different estimates of fuel load consumption.

The authors do a nice job of using NPP and time since fire to explain geographical differences in the pre-burn fuel load, however they do not account for differences in consumption completeness (%), a value just as important in traditional estimates of fuel load consumption. In my opinion, the inability to identify fuel moisture conditions at the time of burning hinders a complete and confident interpretation of the geographical differences in fuel load consumption. Aligning the active fire and burned area pixels with a map of fuel moistures at the time of burning would be the ideal solution. However if the authors forgo such an analysis, they should at least make it extremely clear to the reader that fuel load consumption depends on the pre-burn fuel load AND consumption completeness, and that the latter is influenced by the environmental conditions and fuel moisture contents at the time of burning, which are not accounted for here.

Hoffa, E. A., Ward, D. E., Hao, W. M., Susott, R. A. and Wakimoto, R. H.: Seasonality of carbon emissions from biomass burning in a Zambian savanna, J. Geophys. Res., 104, 13841 – 13853, doi:10.1029/1999JD900091, 1999.

Le Page, Y., Oom, D., Silva, J. M. N., Jonsson, P., and Pereira, J. M. C.: Seasonality of vegetation fires as modified by human action: observing the deviation from eco-climatic fire regimes Global Ecol. Biogeogr., 19, 575–588, 2010.

7. Page 10, Line 9 – Page 11, Line 14: Since the MCD64A1 product is used for both estimates, can it be assumed that the differences between FC MODIS and FC SEVIRI are entirely attributed to the different active fire products and the different methods for converting FRP to FRE?

8. Page 10, Line 9 – Page 11, Line 14: I'm curious about how much overlap there is in

the fire activity that's driving the two estimates of fuel load consumption. I mean, are FC MODIS and FC SEVIRI driven by the same fire activity, or are they two different sets of fire activity, or is the fire activity that that drives FC SEVIRI a subset of the fire activity that drives FC MODIS? Based on the author's statement on Page 8, Line 21: "In contrast to the approach based on SEVIRI data, here all burned area observations were included", it would seem to me that FC SEVIRI is driven by fire activity that is a subset of the fire activity that is driving FC MODIS, if FC MODIS is limited to 2010-2014. Can any insights be gained by limiting the calculation of FC MODIS to the time period used to calculate FC SEVIRI (2010 – 2014)? Perhaps not the bias, but it seems to me that the scatter in the relationship between FC MODIS and FC SEVIRI could be due to the possibility that MODIS and SEVIRI are observing different fire activity within the same 0.25 grid cell. I may be mistaken, but I don't think this was ever offered as an explanation for the scatter. Unless the authors can demonstrate that estimates of FC MODIS and FC SEVIRI are driven by the same fire activity, then I think they have to concede that the scatter in the relationship could be attributed to the possibility that MODIS and SEVIRI are observing different fires within the same grid cell. Note that if fuel load consumption is homogeneous within a 0.25 degree grid cell, then it doesn't matter what fire activity MODIS and SEVIRI observed. However by their own admission, fuel load consumption is heterogeneous, and therefore has the potential to induce scatter in the relationship between FC MODIS and FC SEVIRI if MODIS and SEVIRI observe different fires within the same grid cell.

9. Page 12, Lines 7 – 14: I think this section raises a very interesting question: is it reasonable to derive alternative conversion factors (kgMJ-1) for different satellite sensors? To be honest, I don't know the answer to this question, so I leave it to the authors to pontificate. Note that the authors specifically state that: "Although the FRE per unit area burned can be converted to fuel consumption using the conversion factor found by Wooster et al. (2005) during laboratory experiments which we have used so far, some instrument specific issues may further affect the FRE estimates from space (see methods). In order to correct for uncertainties in the MODIS derived FRE estimates,

we derived an alternative conversion factor by comparing the MODIS FRE per unit area burned directly to field measurements instead (Fig. 3a)." Here the authors admit that the reason for deriving an alternative conversion factor is because of the biases and uncertainties of estimating FRE from MODIS. If this is the case, then shouldn't the adjustment be more appropriately applied to the detection and conversion of FRP to FRE? Why should it be necessary to adjust an empirically derived "physical constant" due to satellite and sensor limitations? Are the authors suggesting that satellite sensor artefacts be incorporated into combustion chemistry and the relationship between radiant heat release and fuel consumption? Note that 0.356 kg MJ-1 (or 0.368 kgMJ-1) is a laboratory derived value, and as such its inverse represents (as close as possible) the amount of radiant heat released per kg of fuel consumed. Adjusting this value to 0.572 kg MJ-1 means that its inverse has a different interpretation: the amount of radiant heat measured by MODIS per kg of fuel consumed. The difference in the meaning is subtle, but not trivial. For instance, there's a difference between (a) the radiant fraction, and (b) the fraction of total heat released that is measured as radiation by MODIS. The other option is to use the laboratory relationship between radiant heat and fuel consumption (which has a universal, physical meaning), and separately derive a MODIS specific FRP-FRE-adjustment factor to account for sensor limitations. I'll leave it to the authors to explain why deriving alternative, sensor specific conversion factors– as performed here – is a better option than using field measurements to derive an adjustment factor that's applied during the conversion of FRP to FRE. Although the two calibration options will obviously give you the same results, they nevertheless have different meanings.

10. Page 15, Lines 8 – 20. To follow on from my previous comment, the authors recognize the role of productivity and fire return periods on the accumulation of fuels, and thus account for the impact of the pre-burn fuel load on fuel load consumption. However, the authors do not equally acknowledge the role of environmental and fuel moisture conditions at the time of burning and their influence on consumption completeness. Ideally the analysis should account for fuel moisture contents at the time of

burning. If such an analysis is not feasible, then the authors should make every effort to remind the reader of the impact of different fuel moisture conditions on consumption completeness and thus fuel load reduction.

11. Page 18, Lines 15-17: Couldn't the "large natural temporal variation in fuel consumption combined with the different periods of data availability" be discounted if FC MODIS and FC SEVIRI are compared between 2010-2014? Also, in addition to the different sensor characteristics, aren't there differences in the methods for converting MODIS and SEVIRI measurements of FRP to FRE (i.e., dividing by detection opportunities vs. temporal integration)?

12. Page 19, Line 30 – 34: This is one of only a few places where the authors disclose to the reader that fuel load consumption depends on the pre-burn fuel load and consumption completeness.

13. Page 20, Line 9 – 15: Indeed. Due to different management goals, fires are lit earlier in the dry season in Africa compared to South America, and particularly in Brazil where fires are generally lit at the peak of the dry season (as shown in Figure 4 of Le page et al., 2010). Hence fire management practices not only determine the fire return period, which affects the fuel load accumulation, but fire management practices also determine what time of year the fires are lit, and thus under what fuel moisture conditions the fires burn. Whilst the authors conclude that fuel load consumption across Africa is relatively low compared to Australia or South America due to differences in the fire return periods, one could also argue that based on the maps presented by Le Page et al. (2010), fuel load consumption is also lower in Africa since fires are more often lit earlier in the dry season when fuel moistures are higher and consumption completeness is lower.

Technical Corrections:

1. Page 1, Line 23-24: Incomplete sentence or incomplete thought.

2. Page 2, Line 19-21: Consider changing the sentence to read: "…is a key indicator of the consequences of changing management practices, vegetation characteristics and climate on fire regimes, as well as a key parameter required in fire emissions estimates."

3. Page 2, Line 21-23: Consider changing the sentence to read: "Yet, spatiotemporal dynamics of fuel consumption on a continental scale remain largely unmeasured and poorly understood (van Leeuwen et al., 2014)."

4. Page 3, Line 23: Consider changing the sentence to read: "…creating the first fully satellite-derived fuel consumption map for Africa."

5. Page 4, Line 1-2: Consider changing the sentence to read: "…in an attempt to provide more statistically representative fuel consumption estimates, particularly in less frequently burned grid cells.

6. Page 4, Line 7-8: Consider changing the sentence to read: "Finally, we used our fuel load reduction map to explore the drivers of fuel consumption in the study regions."

7. Page 4, Line 19-24: It may be helpful to remind the reader here that the MCD64A1 burned area product is also used by GFED.

8. Page 6, Line 24: Consider changing the sentence to read: "…first derived a fuel consumption map for Sub-Saharan Africa."

9. Page 8, Line 33: Grammar, pluralize: "Minimising the impact of these types of perturbations…"

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