

## ***Interactive comment on “Generation and analysis of a new global burned area product based on MODIS 250 m reflectance bands and thermal anomalies” by Emilio Chuvieco et al.***

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Verbatim comments from 2nd reviewer

\*\*\* Start of comments \*\*\*

General Comments

This revision is a step in the right direction, but there has unfortunately been no improvement with respect to the main problem I see with this new dataset. While the authors' stated goal is to “present how the product was generated, its estimated accuracy and relations to existing products”, I would argue that the authors have a responsibility

C1

to demonstrate that their new dataset actually does what they claim. In this revision the reader remains in the dark as to whether the 250m Fire cci BA product is an improvement over the existing 500m MODIS BA product. Are the newly-mapped small burns real or merely commission errors? The authors do not provide an answer, remarking that “Further analysis should check whether in fact the new product is more sensitive to small fires.” Why not answer this question now, before modelers begin using the dataset?

The critical missing piece in this effort is an assessment of product accuracy as a function of fire patch size. I find the authors' justification for this omission unconvincing: “A full assessment of this product is beyond the scope of this paper and would require additional processing efforts.” Why is such an analysis beyond the scope of the paper? Why not conduct those additional processing efforts now? How else are users to know if the claimed benefit of this new product is actually real?

The additional complicating feature of this new dataset is that the existing Collection 6 MODIS BA product seems to map 11% more burned area while also having lower omission and commission error ratios. What burned area that is mapped in the MCD64A1 product is not being mapped in the new dataset? The authors provide no information. Equally puzzling is how the new 250m mapping algorithm can be better at detecting small burns when it seems to perform more poorly even for larger burns. Presumably the detection of large burns would benefit from the higher spatial resolution as well. Again, the authors provide no explanation.

Ultimately the sole evidence offered in support of the 250m Fire cci product is its higher correlation with the (unpublished) Sentinel-2 fire-patch data, but here the authors' analysis remain ambiguous and unclear: “We did not use Sentinel-2 data as reference for validation. Only to compare with the fire patch database.” The authors nevertheless interpret the results of this comparison as “...indicating the benefit of the higher spatial resolution of MODIS Fire cci v5.0, as it captures the shape of smaller fires and provides better agreement with the fine scale resolution of Sentinel-2 data.” The authors seem

C2

to be ignoring the possibility that a significant fraction of the numerous small burns in 250m dataset are actually commission errors. The very high commission error ratio they report for the product (0.512) should be a strong warning in this regard.

I note lastly that a user of remote sensing datasets I find it mildly disconcerting that the authors “disagree with the statement that creating new products is not relevant if they do not show clear improvements over existing ones”. Clearly we should expect some improvement or advantage over the status quo, lest we give developers license to generate a limitless series of “quick and dirty” or otherwise questionable datasets. In my view product developers should demonstrate some material advantage over existing datasets, and by this standard I think the present work falls short.

#### Specific Comments

P6 L15: The Landsat burned area reference maps seem to have been made with a new, unpublished algorithm. Are these maps in some way superior to the Padilla et al. (2014, 2015) approach previously used by this group?

P8 L1: I am not sure the patch analysis can reasonably be described as a “climate assessment”. Later in section 3.4 this analysis is referred to as “Fire patch analysis”.

P8 L24: Here the authors use a second unpublished mapping algorithm and dataset. Does the Fire cci Sentinel-2 mapping method differ from the new approach adopted for Landsat?

P9 L10: “...which implies the errors are more equilibrated in the MODIS Fire cci v5.0 product.” This statement remains somewhat misleading in light of the results shown in Figures 3 and 4. The authors will please note that in my original comment I never claimed that “bias is not relevant for accuracy assessment”. Bias is of course relevant. I was merely objecting to the authors’ description of the v5.0 Fire cci product as “better equilibrated” (now “more equilibrated”) when in fact the Fire cci v5.0 and MCD64A1 products trade places as the “more equilibrated” product from year to year (Figure 4).

C3

P13 L11: Change MCD65A1 to MCD64A1.

P14 L10: “Comparison of our results with the national fire perimeters in three different fire regimes... showed very satisfactory results, with lower errors than those estimated by the statistical validation.” But note that this part of the analysis was very heavily weighted toward Australia, where the national fire perimeters were derived from 250m MODIS imagery. The majority of this analysis, therefore, consisted of comparing one 250m MODIS product against another, which one would fully expect to yield lower errors than would be obtained through validation with independent reference data.

As I suggested in my earlier review, it would be helpful to show an example of the true small burns that are now detected in the Fire cci v5.0 product. I find the authors’ justification for not including such a figure to be unconvincing: “The greater sensitivity to small patches was not derived from the validation dataset, but from the fire patch analysis. A single figure would not be significant to show the global effect described in the text.” Surely some of the Landsat-based reference images will contain numerous small fires that can be used to illustrate the improved performance. Preparing such a figure should be straightforward. See, e.g., figures 4–6 in Ramo and Chuvieco (2017). 2

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C4