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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.

Anonymous Referee #2

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The new global BA product presented in the manuscript is of great interest since it potentially provides time series of global BA maps at 250 m spatial resolution on monthly basis. This sort of information is certainly missing for many applications related to fire monitoring and climate modelling. As a general comment, I think the authors should better and quantitatively prove the contribution of the new product in detecting small burned areas, with respect to existing ones. The authors well review existing products but the inter-comparison and the assessment of the ability in detecting small burned patched is not well developed. This is a central issue since the comparison with other global products shows that, for example, MCD64A1 overall performs better than MDOIS Fire cci. To this aim, the authors use BA from Sentinel 2 but little explanation

is given and no figure and/or tables are provided for such important results. I think the revision of the manuscript should first focus on this topic. I list below my general and specific comments.

GENERAL COMMENTS

1. The authors claim that one of their goals is to "improve the information available to climate modellers on spatio-temporal patterns of fire occurrence" (Page 10, Lines 26-27) and that "The goal of generating this product was to complement existing BA products (. . .) as well as to improve detection rate of small burn patches" (Page 3, Lines 14-19). Even if these objectives are realistic thanks to the use of full resolution red/NIR 250 m MODIS bands, in my opinion, little quantitative assessment of the above statements is shown in the validation.

2. Further, what do the authors mean with "small burned patches"?

3. The monthly pixel BA product is least presented and validated. All figures and tables show BA annual synthesis which could "hide" monthly trends within the year by balancing out omission and commission from different months. In general, the temporal resolution (monthly and biweekly) of both products is neglected in presenting results and validation.

4. The analysis of the quantitative difference between the global BA products (Figure 4) could be integrated with metrics other than the correlation R2; for example, MAE and/or RMSE that better represent difference in quantitative assessment. For example, MODIS Fire cci is systematically lower(higher) than MCD64A1 c6 (GFED 4) and since they have the same trend, R2 is high but the amount of BA is different among the products. Moreover, MODIS thermal anomalies is a common source of information and this could explain the good agreement of temporal trends of the annual estimates (i.e. it is widely accepted that active fires/thermal anomalies are good indicators of fire seasonality). For what concerns section 3, I'd suggest the authors to present first the validation of the MODIS Fire cci BA product and then inter-comparison analysis.

5. Figure 6, to my understanding, shows that MCD64 is systematically better than MODIS fire cci, according to all metrics except relB. This is also confirmed at page 8, Lines 23-25, where the authors state that MODIS Fire cci performance, as measured by the Dice coefficient, has lower accuracy than MODIS MCD64A1. If so, how do you explain it? If MODIS Fire cci is supposed to better represent small burned areas and to improve spatio-temporal representation of burned patches, where do the authors see the improvement with respect to MCD64? If the improvement brought by the new product is proved by the comparison with Sentinel 2 (Page 10, Lines 30-33) this topic deserves a central part in the manuscript. May be this point should be better addressed.

6. Concerning the description of the methodology, there are some parts of the implemented approach for both BA product development and validation, that deserve better explanation.

7. Compositing daily MODIS reflectance over the month is certainly a suitable approach; however, it should be better proved (or discussed) how the compositing criterion chosen by the authors does really reduce the BRDF effect.

8. It is not fully clear to me how the authors derive the CDF for burned and unburned areas and how they define adaptive thresholds over the globe. Is this step carried out for each tile?

9. In the validation section (2.3), the authors should specify how they derived yearly values of the metrics from the sampled validation units. Alternatively they should provide a valid link to the project's report so that the reader can access all the information.

10. As I commented above, the role of Sentinel 2 data and BA Sentinel 2 product derived within the Fire cci project seems to be crucial. All information should be provided. What is the algorithm the authors mention on Page 7, Lines 5-6? Is there a reference for this product? This part is very unclear and results are poorly described and discussed. I'd suggest the authors either to clarify this point or to drop this com-

parison. But in the case of the latter, they should prove the contribution for detecting small burned areas.

11. No results are shown on the fire date products which is one of the user's requirements and certainly a key information for fire regimes assessment.

12. The analysis of fire patches and fire shape is interesting. I wonder whether it is correct to calculate and compare the shape index between two products with different spatial resolution?

SPECIFIC AND MINOR COMMENTS

1. Page 4, Line 1. Cloud shadow masking is often critical when the objective is to detect low albedo surfaces. Does shadow masking had an effect on the detection rate of burned areas and burn date? Do the authors evaluate this issue?

2. Page 6, Lines 15-24. Please be clearer for the less known metrics, such as the Dice coefficient, even if they are reported in Padilla et al. 2015; the authors could provide formula;

3. Page 6, Line 24. Please do not refer to unpublished material (e.g. in preparation or submitted);

4. For section 2.5 I'd suggest the authors to use a title that better describes the content of this section (i.e. fire size and/or fire shape/patches);

5. Page 7, Lines 14-15. It is not clear how the authors set a 107 ha threshold value;

6. Figure 4. I'd advice the authors to use the same range for the x- and y-axis, draw the 1:1 line and use the same colors as in Figure 3.

7. Table 2. Could it be possible to provide all results and not only average and extreme values?

8. Figure 6, for the Oe and Ce graphs I'd suggest to use the same y-axis range;

9. Page 8, Line 26. “Global commission and omission error ratios” be consistent with description of accuracy metrics given in 2.3: are the metrics ratios or the errors as computed from the confusion matrix?

10. Figures 1 and 2 should have highlighted the areas masked as unburnable. Moreover, why white regions (no burning) apparently cover different areas if they are from the same BA product (cell product)?

11. Page 10, Line 10. Shape index SI acronym should be introduced before.

12. Page 3, Line 10. Replace "ENVISAR" with "ENVISAT"

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