

Tracking recent extremes and interannual variability of global fire emissions using a near-real-time extension to the Global Fire Emissions Database

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General comments

The new version of the paper has solved most of my concerns, although some issues persist. Even though the authors claim to have answered the concerns of two reviewers over the uncertainty estimation, the new analysis they provide does not seem to be very convincing, as they focused on comparing with the reference product GFED5, instead of providing a full uncertainty characterization by taking into account the different factors of uncertainty relevant for their model, related to the input data, including the static hotspots mask, the estimation of BA from AF, and the emission model: fuel loads and CC. To me this is more an inter-comparison analysis than an uncertainty characterization.

In fact, in the text the section where this comparison is made is entitled “Evaluation of the GFED5NRT”, including also the comparison with HR data. The aim of this section is to show that the product is consistent with GFED5, rather than the analysis of the uncertainty factors. In fact, the references to the uncertainty of the product are mostly introduced in the supplementary material and a brief comment in the conclusion section (lines 447-49). I wonder why the uncertainty (expressed as the dispersion around the estimated mean) is not considered in the time series (figure 4 or 7, for instance).

Minor issues:

Line 102-3: “generating two independent time series of burned area and emissions estimates with minimal latency”. Please indicate the value.

Line 116-18: “To reduce commission errors associated with the coarse spatial resolution of MODIS (500 m), burned area estimates were calibrated using higher-resolution (20-30 m) observations from Landsat and Sentinel-2.” I indicated in the previous version of my review that MODIS BA errors are mostly omission, and this should be the main motivation to use higher resolution BA products, but commission is still emphasized here, why?

Lines 231-33: “In the first step, biome-specific scaling factors, termed effective fire area (EFA) scalars, were derived for multiple geographical regions by relating science-quality VIIRS active fire detections to GFED5 burned area data during the MODIS-VIIRS overlap period (2012–2022). Be more specific with the regions, are

those used for the analysis of BA trends? If so, please refer here to the table-figure describing them. It is odd that you used GFED BA as a reference instead of the original HR BA datasets, as this implies increasing the uncertainty.

Line 242-43: In principle, the Regftp scheme could yield up to 224 EFA scalars globally, from the product of 14 continental regions and 16 burning types” in line 179 you mentioned 3 burning types, so I assume you mean here the combination of burning types and land covers. Please clarify.

Line 268 “each grid cell are calculated as the arithmetic mean of the SNPP- and NOAA-20-based estimates when both”. Since NPP is close to end its lifetime, I assume you have adapted the system to include NOAA-21 AF. It would be good to mention somewhere, and comment on the potential implications.

Line 308: “Australia, we relied on the regional data compiled within the ONFIRE database”, please quote the original paper describing the dataset.

Line 352. Here we compare the GFED5NRT burned area estimates with VNP64A1. Overall, the spatial distribution of GFED5NRT burned area in 2023 shows moderate to good agreement with the VIIRS-based burned area product”. This statement is vague, and the figure S6 shows strong differences between the two datasets with much higher estimations in GFED5NRT. A more adequate figure should show the differences between them in terms of total BA in each grid cell. Ideally, the authors should show differences with other global BA products, such as those derived from FireCCI (the MRBA60 product covers 2003-2025). Another figure with annual trends in total BA estimates from different products would be useful.