Comment on essd-2021-113

Anonymous Referee #1

Referee comment on "Refined burned-area mapping protocol using Sentinel-2 data increases estimate of 2019 Indonesian burning" by David L. A. Gaveau et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-113-RC1, 2021

Overall this is an interesting topic and highlights the various limitations in burned area products. I applaud the effort that went into creating the validation dataset, however, as highlighted below, there are some crucial details lacking in the training and validation descriptions that are necessary for a full review. Please see below for specific comments.

Specific/Major Comments

- Section 2.1: I assume the November and December 2018 images were only used for the pre-fire window analysis? How did the authors account for any burns that occurred during those 2 months?
- Section 2.2: What happens if a pixel burns more than once in a year? This might occur in the agricultural regions?
- Line 162: Are the authors not calculating dNBR here? Also, does the moving window continue beyond the first instance of detecting a potential burn? i.e. if the drop occurred on February 1st, does the moving window continue to Dec 31 to see if it burned again?
- Line 177: Can the authors please provide a map of the locations of the 988 training pixels and their associated land cover types in the supplementary? Is 988 training pixels enough? How was that number decided upon?
- Line 183: dNBR already shows burn severity (here is an article with more information: https://un-spider.org/advisory-support/recommended-practices/recommended-practice-burn-severity/in-detail/normalized-burn-ratio). Did the authors quantify these values over their training pixels or simply rely on the color? I suggest the authors quantify these values to ensure the training pixels are in fact medium-to-high severity especially since the authors are prioritizing mapping high burn severity fires to reduce false positives.
- Section 2.4.1: As with the training samples, what land cover types were associated with the validation samples? Secondly, what size burn scars were these validation pixels associated with? For example, if all validation pixels were associated with very large burn scars then the validation results will be biased because large burns are easy to detect. Also, I assume the training and validation samples did not overlap?
- Line 310-311: Please explain why the authors only chose the cardinal directions?
- Line 389: While doing some reading into power laws and fire size, I came across this

paper from the US Forest Service with the following quote: *"Newman (2005) specifically excludes fire size distributions, while admitting that they might follow power laws over portions of their ranges. Current opinion is divided among those who would globally assign power laws to fire-size distributions (Minnich 1983; Bak et al. 1990; Malamud et al. 1998, 2005; Turcotte et al. 2002; Ricotta 2003) and those who would attribute them only to portions of distributions or rule them out altogether in favor of alternatives (Cumming 2001; Reed and McKelvey 2002; Clauset et al. 2007; Moritz et al., Chap. 3)"* - https://www.fs.fed.us/rm/pubs_other/rmrs_2011_mckenzie_d001.pdf

Please can the authors go through the literature and ensure their power-law assumption is correct and justify it in the paper.

- Line 399: The current analysis does not support this finding regarding agricultural burning. Based on Figure S3, the small patches are likely associated with the small burn patches surrounding the larger burn scars. Agricultural burning is a very difficult fire type to map and although the current methodology is likely to map more agricultural burning than MODIS (due to the finer resolution) the mapping methods and validation assessment was not adequately designed for agricultural burning. The authors can mention that the S2 mapping is better suited for identifying "small fires". Furthermore, it was noted on lines 427 – 433 that the approach omitted hard–to–detect fires (e.g. savanna grasslands) which are much easier to detect than agricultural fires therefore that statement is not supported.

Minor Comments

- Line 23: change to "..which occur on.."
- Line 27: Should the size of the intermediate fires read (100ha – 1000ha) similar to what you have in the main body of the paper?
- Line 88: change "excepting" to "except"
- Introduction: When are the peak burning months? It seems based on the GWIS country profiles (https://gwis.jrc.ec.europa.eu/apps/country.profile/charts) that the peak occurs August – October and since the authors are also referring to agricultural fires then please also include the cropland burning months.
- Line 137: Remove "and finally" after "Fourth. The authors go on to a final step on line 139.
- Figure 3: There is no h panel (line 679)
- Line 237: Should that be referencing Figure 3? Also, there is no panel h
- Line 696: change to "minimum mapping unit"
- Figure 5: Please add to the caption that the light grey represents countries outside of Indonesia. This was confusing at first before I looked at a map.
- Table S3: please add the meanings of Am and Wh to the caption
- Line 370: add a comma between "Figure 6 Figure S2"
- Line 702: Change to "MCD64A1". There were a few other instances where the A was lowercase (i.e. MCD64a1)
- Line 377: It would be interesting for the authors to create a 3-panel figure showing this scar from each of the 3 products to show the omissions made in MCD64 and the Official dataset.
- Line 443: change to "addressing"