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Comment on nhess-2022-47

Anonymous Referee #2

Referee comment on "Characterizing the Rate of Spread of Wildfires in Emerging Fire Environments of Northwestern Europe" by Adrián Cardil et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-47-RC2>, 2022

The preprint "Characterizing the Rate of Spread of Wildfires in Emerging Fire Environments of Northwestern Europe" by Mario Tapia et al. presents a systematic investigation of wildfire rate of spread (ROS) derived from VIIRS 375 m fire products. The article is well-presented and overall clearly written.

The authors propose a methodology to formalize the quantification of a fire behavior variable which, undoubtedly, is frequently estimated in an ad-hoc manner by users of fire detection data, specifically from the fire and natural hazard community, when managing a specific fire event. The authors' approach is potentially suitable to use as the basis for developing a remotely sensed product of interest to the user community. As such, the work is innovative and of undeniable interest. As the article's area of interest is north-western Europe - not a region known for very large or disruptive wildfires and therefore, in the light of climate change induced greater expected future prevalence of the wildfire hazard - it also contributes to enhanced understanding of the fire regimes in this part of the world.

Notwithstanding these strengths in scientific significance and quality of the presentation, I perceive a certain number of weaknesses that should be addressed before the manuscript is accepted for publication.

- **Definition and structure of the study area** (2.1). To the reader who is not immersed into the study of this area, the choice of study area appears at least somewhat arbitrary. Was the intent here to study an area of Europe somewhat under-represented in the study of wildfire, and therefore to apply boundaries so as to stay clear of the Mediterranean region in the south and the Scandinavian/boreal region in the west and north? The eastern boundary and the choice of the 49th parallel should be better justified. If this is a commonly studied area thus delineated, a citation should be added. This point may appear as a formality, but I believe it is more significant than that, especially when it comes to the statistics presented for the countries outside the

British Isles. For example, a quick look into German fire statistics shows that, contrary to the findings presented here, wildfire activity tends to peak in the month of August. It also shows, however, that German wildfires are dominated by fire events in the Land of Brandenburg, which is cut in half by the eastern border of the study area here. Given this kind of limitation, and the extremely small sample size of fires outside the British Isles, I do not think that per-country statistics (3.3 and Fig 4) should be presented for the countries other than the British Isles.

▪ **VIIRS data description, limitations, pre-processing and exploratory statistics.**

Section 2.2 needs to clearly describe which VIIRS product was used (I presume VNP14IMGTDL_NRT), and also confirm that the study is based only on S-NPP VIIRS data (no NOAA-20 data, which would duplicate the data record in the last year or so). Given that the filtering for retained fire detections ("real" fires) ended up rejecting ~90% of fire clusters, it is odd that clustering happened before filtering. The filtering criteria are also not very clear. A cleaner approach would have been to filter by land cover type (or, potentially, by using available GIS data of nature preserves, forested areas etc.) *first* and then cluster the remaining events. Regardless, it would be instructive to see some minimal exploratory statistical description of the retained fire events - how many by year? By land cover type? Their final number - 256 - is very small compared to the known fires in this area over the 9 years of the study time. This is to be expected as it is known that VIIRS misses many detections. But this fact is a rather relevant limitation of the study, which needs to be discussed. As-is, it seems likely that the results are dominated by particularly large fire years in specific sub-areas, which may very well skew the ROS statistics presented in the results. For example, the 2019 peatland fires in Scotland and Northern England may account for a rather outsized part of the results.

- **Algorithm description.** In my view, the chief interest of this work is the ROS vector generation algorithm. More effort should be deployed to describe its strengths and limitations. For example, in section 2.4 and Fig. 2, the fire detections are not points, but VIIRS pixels of at least a size 375 x 375 m (or substantially larger if the acquisition is off-nadir). The VIIRS data includes complementary information (which may include x and y pixel extent, depending on the product used, and does include a confidence rating) - was this information used in any way and how stable are ROS derivations to this. Also, fire spread has an extremely strong diurnal pattern, so the reporting of spread km/h is a value that has undergone averaging. In Fig 2 you present an example with ~14 h between successive acquisitions, but VIIRS overpasses can re-image the same spot with an interval of 90 min or up to several days, and the ROS values you would obtain would be radically different given the diurnal variation. At the very least you should report the distribution of delta-t values used for ROS calculations, and possibly apply a correction factor based on expected temporal fire activity patterns.

Some more localized comments:

16/17: Given the substantial statistical limitations of the study, I think that this sentence overstates the amount of insight gained for understanding of fire regimes.

31: An anomaly is probably an understatement. There is a long record of fire use for landscape management by successive human populations.

34-36: The increasing peatland megafires should probably be mentioned here, especially since my suspicion is that they dominate the dataset this study is based on.

39: Moritz et al. (2015) reference needs to be re-formatted.

45/46: What about ignitions?

90: The capitalization of n/Northwest/ern Europe should be unified.

120: Extraneous semicolon.

Figure 1: Please revise the legend. It should indicate the origin of the land cover classification. Also, the hotspots - or hotspot clusters? - are in subfigure a), not b).

101/102: The capitalization choice "northern Atlantic Biogeographical region" is odd here and in the following.

137-143 [re: spatial clustering] There are other algorithms that also do not require cluster centers and number of clusters to be indicated a priori. With about 40,000 detections per year this is not a data volume that would be a problem for example for a variant of DBSCAN. Not that the outcome is going to be very different, but the clustering methodology comes across as somewhat clunky. The 5 km and 20 detections threshold aren't very well justified. (Also, where these distances measured in a projected coordinate system, that is, was the whole dataset reprojected, and if yes to which coordinate system?) Later, in the Results section, there is insufficient reporting on the impact of the parameter choice in clustering on the final dataset of fire events.

153-159: Missing references for these methodologies. (Also, a diagram would have been helpful.)

163: Whenever the word heuristically is used, there should be a justification of the

heuristics being applied and ideally an estimate of the uncertainty involved.

Figure 2: The labels a) and b) are not clearly applied. The yellow points appear in b) only. There are no yellow polygons. The VIIRS fire detections at such high resolution should not be visualized as points as they are at least 375 x 375 m in extent.

175/176: There is no description of the final step of the algorithm, that is, the selection of *one* final vector. Is it the one of maximum length, some sort of average, a Gaussian model? What drove the choice of method, and what is the variability of the outcome? It seems to me that each ROS value should come with an uncertainty. As fire can grow in complex ways between successive detections, there is a need to report on what was found - and given the dataset was only 254 fires, case studies should be presented that show typical cases beyond Fig. 2 only.

179: These are not false detections. They are true detections of thermal anomalies that are not of interest to the study.

193-195: This sentence is unclear to me.

203-208: Section 3.1 should be expanded as a lot of questions remain open. These 254 fire events led to a substantially higher number of "fire spread timesteps". From Fig. 5 my guess is that their number was 758 or thereabouts. Did each of the 254 events contain at least one spread timestep? (If yes, that would be almost surprising - was there anything in the clustering methodology to ensure that each fire had at least two successive acquisitions?) How were the fire spread events distributed - my guess is that a small number of long-running fires dominate the fire spread events. A histogram would be helpful. Also, I miss a discussion of latitude effects on the likelihood of repeat fire detections (because of satellite orbital properties). The entire discussion in 3.2 and 3.3 is tainted if these biases aren't transparently described first.

Figure 3: The caption, and the preceding text, should make clear that this burnt area is not the same as that detected in remotely sensed burnt area products, or delineated in the GIS systems maintained by fire managers.

Figure 4: Fires were not detected contrary to the datasets made available by fire management agencies (eg. https://www.ble.de/DE/BZL/Daten-Berichte/Wald/wald_node.html) . This is understandable but needs to be discussed.

251 ff (3.4 and Fig. 5): The values of n vary wildly between the classes. So maybe classes could be grouped to generate similarly sized datasets. What do the error bars represent?

269: The authors should agree on one choice of spelling of burnt/burned area.

270 ff: The authors discuss some sources of biases (fire size), but should expand on the shortness of their dataset (only 9 years of fires) and how it can skew the results regarding fire activity timing.

286 ff: Not all fire management areas apply the same prescribed burn processes, and permitting is also not homogeneous. This paragraph should be shortened and moved to an earlier location in the manuscript, as it addresses a very minor point. There are some formatting issues with parentheses.

300/301: What sensor limitations?

341/345: The VIIRS 375 m is unlikely to detect any smoldering peat fires, so this is not surprising at all. You correctly state that what you're seeing is surface fires. The spread of the peat fire is entirely invisible to your methodology.

347: Formatting issue.

366ff: How are the low-to-moderate values you're getting impacted by averaging over a diurnal activity variation? Actual instantaneous spread may have been much faster.

401 ff: Please remove redundancy in the Conclusions section with what already has been said.

To conclude, in my view a quick, accurate and well-understood method to calculate VIIRS-based ROS for fire events would constitute a valuable and welcome contribution to the scientific record and toolset at the disposal of the fire management community. But the authors need to be careful to clearly describe the statistical limitations of their approach when it comes to statements about the NW-European fire regimes, and expand the presentation of the methodology itself.