Reviewer 1

The manuscript "Characterizing the Rate of Spread of Wildfires in Emerging Fire Environments of Northwestern Europe" by Mario Tapia et al. was resubmitted in revised form. The manuscript is substantially improved from its original form, specifically:

- Fig. 2 & 4 are much improved, and Fig. 5 is now believable. Clearly some additional data analysis was performed, which removed the bulk of the inconsistencies
- The exclusion of some fires in France and Germany also solidifies the analysis as the fire regime under study here is now more credible as a coherent biogeographic area. As the authors decided to retain the by-country analysis, this was really necessary. (I still don't see this analysis as overly useful, but at least in the current form it is reasonably coherent.)
- The description of clustering and filtering makes sense to me now, as does the description of the algorithm.
- Overall, the manuscript reads much better, and the small points I raised have been addressed.

Thank you very much for all the comments. They were very useful to improve our manuscript.

There remain some weaknesses, but I think I can understand where they come from. With only 327 vectors from a bit more than 100 fire events, providing an error estimate for the ROS may not be very meaningful. I think the authors should address this clearly in the discussion section as a limitation, especially given that the selected example in Fig. 2 does have enough vectors to allow for calculation of a standard deviation for example.

We clearly addressed this limitation (lack of data) in the discussion as suggested. Note that we are not estimating variation of ROS within each fire so we did not analyze the standard deviation by fire (Fig 2). The descriptive analysis of the vectors was performed on the factor "Month" and "Country", in this case there was no associated error estimation due to lack of data. However, we did show the variability of ROS by factor through box plots, showing range and quartile as a deviation measurement as well. (Fig 4).. We did analyze the standard error on the "Land Cover" factor, indicating the number of observations per group.

The other point I would ask the authors to elaborate on in 1-3 sentences is the sentence (l. 141 f.) "Cell size and 20 hotspot threshold values were heuristically set." What happens when the cell size is much smaller/larger? Also, how many vectors can be obtained typically from 20 hotspots? (Maybe this should go into the results section.)

When cell size is larger, there is a higher probability that an "active fire cell" (i.e., cells where a fire hotspot is located) has a neighboring active cell. Thus, hotspots located in these cells will be merged into the same fire cluster. When cell size is smaller, it is more likely that these two active fire cells will be clustered into different

fires. We heuristically defined the cell size as a minimum distance in which two co-occurring hotspot detections can be precisely distinguished as two different fires.

The number of vectors basically depends on fire duration, number of time steps and, thus, the number of vertices generated for each fire.

There are also some minor infelicities of language & punctuation that have crept back in during revision, which should be addressed through careful proofreading at the copy editing stage.

We have corrected these faults in the new revision of the article.

My overall judgment is that automatically deriving ROS from VIIRS will surely be well received in both the user community and the study of fire regimes, and while the work as presented leaves some avenues for future refinement and research, it is a respectable and worthwhile contribution to the research area, ultimately worthy of publication.

Thank you very much for your comments.

Reviewer 2

Summary: The authors present a new dataset of the rate of spread for 102 fires in northwestern Europe extracted from VIIRS thermal anomalies. They use this dataset to explore the seasonal pattern and differences between vegetation types of ROS. The manuscript is overall well written and has a logical flow to it. While it is great that the authors have produced this new dataset, my major comment on the manuscript is that the analysis performed seems a bit superficial at moments.

Major comments:

I find it a bit of a weird paper in the sense that the manuscript reads as if the authors extracted these new ROS data for a set of fires, and then didn't really know what to do with it. Specifically, I miss a clear research question, with the current manuscript only giving a relatively simple description of the RoS values and some comparison between seasons and vegetation types.

> **We stated that the goal of this study was to characterize the ROS in an area of increasing risk in Europe in the last sentence of the introduction. We have quantified and described the ROS and its variability in a territory that has not been as well studied as, for instance, the Mediterranean regions. So, we created this novel dataset for the scientific community and a replicable methodology that could potentially be used in the future with improved input data. With this, we analyzed the temporal variation of ROS, variation among land covers and also for the final burned area, separately, as an initial but necessary characterization. We think our findings clearly fit in the scope of the journal since they addressed the aforementioned research questions.**

On the other hand, for a paper describing a new dataset, the dataset seems too limited in scope, only covering 102 fires, almost all of which occurred in the UK.

> **We analyzed the entire VIIRS dataset since the beginning of satellite data acquisition in 2012. The study area was defined *a priori*, in order to develop our understanding in this understudied and increasing fire risk area: NW Europe. Thus, the boundary for this was the Northern Atlantic biogeographical region above 49th parallel, as is already justified in Materials and Methods. We agree that the dataset is limited, but the number of clustered fires and their spatial distribution is a result of the proposed methodology and is part of the characterization itself. Indeed, this speaks to the necessity of characterizing wildfire behavior in these regions of increasing risk, in lieu of pre-existing comprehensive datasets.**

Other papers have been published this year describing similar methods of extracting fire behavior information from VIIRS active fires (of which I am a co-author for all clarity), see references below. I don't know whether the current paper was inspired by previous presentations on the subject, or whether the methods were developed in parallel. Be it one way or another, in my opinion, the novelty of this paper should lay in the science questions you want to answer with the new RoS data produced, which is currently an underdeveloped part of the paper. For example, if the focus of the paper is on increasing climate-driven fire risk in NW-Europe (which I deduce from the introduction), I would expect to see some analysis in this regard.

Thank you for your comment, it has been very interesting to revisit your articles, which are already cited in the new revised version of the manuscript. For this article, we are interested in characterizing ROS in an area of increasing risk with a novel methodology. Studying the climatic drivers of ROS is outside the focus of this article.

The authors make relatively bold statements on differences between countries e.g. Line 229 " Fires observed for Germany, The Netherlands and Northern Ireland appear to deviate from this pattern.". Such statements are based on data from 1 or 2 fires in the case of Germany and the Netherlands, and therefore most probably not robust. I would like to see a more robust statistical analysis to avoid these kinds of statements.

We agree with the reviewer in the sense that the lack of data does not allow us to find differences in terms of ROS among regions. We recognize our data can not be used to perform hypothesis testing or further analysis over ROS on this. Thus, we want to emphasize that we are not doing this with "Region" as a factor, as we did with "Land cover". Taking this in consideration, we consider that it is still important to consider the few observations referring to these countries, since a low number of fires is also a descriptor of the fire dynamics in these countries.

Minor comments:

Line 21: There is a point too much after Median.

Addressed

Line 114: you indicate that you used the NRT product, but I guess this is a confusion and only the case for the last couple of months of the data you used, as this product is removed after 2-3 months, once the standard science quality data is available.

Addressed

Figure 1: So, in practice, the dataset on RoS you generated covers the UK and Ireland, with barely any fires in the Netherlands and Germany, and none in France or Denmark. I have to agree with the previous review that this selection of the study region is somewhat odd, and I don't think that you can say that these results are valid for the mainland Europe part of the study region, as these don't seem to be sufficiently represented in the dataset.

As indicated above, the study area was justified in the Materials and Methods section. We wanted to represent NW Europe as the Atlantic bioregion above the 49th parallel, since this is an underrepresented area in wildfire science. This criterion was established a priori, so we sought all available information in the VIIRS historical record. Although there are fewer fires in some regions than in others, this reflects the fire regime of each region by itself. On the other hand, we have not stated in the article that we are fully representing mainland Europe. Mainland Europe encompasses different bioregions outside the scope of this paper, with probably different fire regimes.

Line 181: I am a bit surprised by the fact that you calculate ROS by connecting the new vertex to the closest previous vertex, and not by calculating the minimum distance from the new vertex to the previous perimeter (as this is your best estimate of where the fire line was). The authors have realized this, as they decided to add extra vertices (line 184), so why not go for the more straightforward option of directly calculating the minim distance to the perimeter?

Yes, very interesting option. We agree that it would have been a better approach to compute the distance as it is suggested. The main reason not to do it in the present code is that it would make the overall workflow a bit more complex as it would require to keep track of new points that are not simply the middle point of each section, but most importantly, because we did not think about that possibility during the development. For future analysis we will definitely try to include this improvement in the algorithm.

Line 224-225: RoS is calculated at 12h timesteps. However, the detection probability in VIIRS is much higher at night than during the day. Do you know how this might have influenced your results?

Considering that perimeter generation relies on edge hotpots, there should be a negligible magnitude of error associated with this phenomenon, as it can only influence ROS and Burned Area estimations if there is a hotspot that is being missed by this difference in detection probability and, at the same time, this point is also part of the perimeter construction (located at the edge of the fire). Thus, this effect could be more likely to happen in smaller fires. If this happens, the effect on our results may be different depending on the variable. There could be an underestimation for burned area by day due to the omission of a hotspot during the perimeter generation. On the other hand, there could be an overestimation of the rate of spread from day to night, as we are skipping a hotspot that should have been made part of the previous perimeter and consequently considering a larger distance over a shorter time.

Figure 4: I find it a bit odd to see the y-axis label on the left side of the figure, while the values are on the right-hand side of the figure, and would suggest putting both on the lefthand side. Also, but this is a bit more personal preference, I generally prefer not to include the grid as a background within each plot.

Thank you for this comment. This specific edit has been made for the new revised version of the manuscript.

Figure 5: How was the land cover type calculated here? Is it the dominant type within a fire, or has this been calculated at a 12h basis with each new RoS estimate?

Information associated with land cover was extracted for each vector in its respective overpass.

Figure 6: Did you take the mean, median, or max RoS per fire here? This is a nice finding, but also here, a deeper exploration of what is driving fire size could make the paper more interesting. Does RoS explain much more of the final fire size compared to fire duration, landscape type, burnable surface (as these are often patchy burnable lands), etc.?

The vector data used for this model was the maximum ROS per fire and time step. We have specified this in the methods and results.

Best Regards,
Stijn

References:

Hantson, S., N. Andela, M. L. Goulden and J. T. Randerson (2022). "Human-ignited fires result in more extreme fire behavior and ecosystem impacts." Nature Communications 13(1): 2717.

Chen, Y., S. Hantson, N. Andela, S. R. Coffield, C. A. Graff, D. C. Morton, L. E. Ott, E. Foufoula-Georgiou, P. Smyth, M. L. Goulden and J. T. Randerson (2022). "California wildfire spread derived using VIIRS satellite observations and an object-based tracking system." Scientific Data 9(1): 249.

Editor comments

From my own reading, and from reading the reviewer reports, it appears that your manuscript has significantly improved. However, several issues still remain. These will have to be addressed before publication of your manuscript can further be considered. I list here some of the remaining issues:

- I concur with the reviewers that the study area delineation remains somewhat cumbersome, especially now that from looking at Figure 1 only a handful of fires on the European mainland seems to be included. The sample size for these regions/countries seems critically low. Can you consider limiting your study to the British Isles?

The inclusion of Germany and the Netherlands in Figure 4 seems problematic. Can you please justify this, or alternatively remove this regions from the study?

> **As mentioned above in the responses to reviewer 2, we chose to study Northwest Europe as the Atlantic bioregion above the 49th parallel because it is a region underrepresented in wildfire science. To develop a novel method and to study this region was our main objective. For this we sought all available VIIRS historical records. In addition, we want to emphasize that we are not doing hypothesis testing or deeper types of statistical analyses over "Country" as a factor, as we acknowledge that there is no sufficient sampling size for each one. At the country level, we want to do general analyses of ROS for all available data. Although some regions have fewer fires than others, this also reflects the fire regime of each region itself.**

- I checked your supplementary data files and wonder why you have not chosen for a data format like shapefiles. Now, there are separate files for the geospatial data (KML/Z files) and its attributes (CSV files). I believe this dataset could be more elegantly shared using for example a shapefile format. In addition, while it is clear to me what the added value of the 'point' and 'polygon' layers is, it is not entirely clear to me what the added values is of the 'vector' and 'raster' layers. Can you please clarify? Including a readme file with your dataset could also be helpful.

The algorithm works with kml files as output. Files have internal temporal metadata meant for visualization in, for instance, Google Earth. Conversion to shapefile loses information for this. Then, we propose to gather all the separate KML files into a smaller number of grouped files, for a more adequate presentation of the results.

CSV files contain explicit Rate of Spread information. We added this type of data for a faster analysis of the data, with their respective coordinates, initial and final time stamps.

- Some new papers have been published covering related methodologies. I recommend a discussion of the similarities and differences of your methods with these published methods. Here are some references that you could consider including:

Hantson, S., N. Andela, M. L. Goulden and J. T. Randerson (2022). "Human-ignited fires result in more extreme fire behavior and ecosystem impacts." Nature Communications 13(1): 2717.
Chen, Y., S. Hantson, N. Andela, S. R. Coffield, C. A. Graff, D. C. Morton, L. E. Ott, E. Foufoula-Georgiou, P. Smyth, M. L. Goulden and J. T. Randerson (2022). "California wildfire spread derived using VIIRS satellite observations and an object-based tracking system." Scientific Data 9(1): 249.

As mentioned for Reviewer 2, we are considering this for the revised version of the manuscript

- Please consider a sensitivity analysis of the threshold of 20 fire pixels for considering a fire cluster.

Indeed we consider that it is important to review the result of the algorithm and the perimeters as a function of the parameters introduced. One of the first steps within this work was to review all the perimeters generated as a function of different values of α. As we explained in Materials and Methods, we assessed 4 values of α: 1, 3, 5, 10 until we found shaped polygons spatially coherent with their shape and with the position of the hotspots and so we finally fixed 10 as the most coherent value for fire perimeters.