

Comment on **essd-2022-475**

Miguel Cruz (Referee)

Referee comment on "The Portuguese Large Wildfire Spread database (PT-FireSprd)" by Akli Benali et al., Earth Syst. Sci. Data Discuss.,
<https://doi.org/10.5194/essd-2022-475-RC1>, 2023

Review of **essd-2022-475** by Miguel Cruz

I commend the authors for their work on developing a significant wildfire behaviour dataset. I found the work of great interest to the fire research community, and can see their methods and data being used by many in the I have few major comments (below) and a number of small comments that are in the attached pdf.

- The manuscript reads well, but there are a number of sentences that seem incomplete or are somewhat incoherent. I note those in the attached pdf. Authors should also be more direct and concise in their writing. The manuscript is quite long, and I made note of content that could be left out as it is not necessary.
- This is a wildfire spread database which is quite relevant. The authors should nonetheless mention clearly that no collation of data on weather, fuels, topography, etc was conducted within the context of this study. This is never mentioned. Although the authors mentioned later in the manuscript that the data can be used for better understanding of wildfire drivers, model evaluation, etc. this cannot be conducted unless other data is present. As it is, the spread data in isolation does not allow for much of an analysis.
- There are important limitations of the satellite data. Some of them are mentioned, others are not. An important point that should be made is that just because the satellite or someone says the fire is at a certain location at a given time, it does not mean that the fire just arrived there at such time. The fire might have arrived hours before, and hence the average rate of spread for a burning period is a value that is diluted, combining periods of rapid spread and no spread. This is quite relevant as fire behaviour is highly nonlinear, and averaged values over larger time periods can be misleading. These aspects should be noted in the manuscript.
- It is not clear how combining different methods to map a fire location are integrated. Do some methods have prevalence over others? Photographic evidence vs satellite information? Is there a process that is followed? If yes, this should be described.
- I have two main technical comments.
 - The proposal of a fire behaviour classes based on your data is fraught with error. It is ok to explore the distribution of your data, but to propose such distribution (which

you said, was biased to large fires) to derive a fire behaviour classification is wrong. The classification class threshold have no physical meaning, and you can realise that if you check into a number of fire behaviour and danger classifications developed from fire behaviour – operational implications. A proof that your proposed classification is meaningless, is the fact that if in the next two fire seasons you add 40 new wildfires all burning under moderate to high fire danger (lets say it is a mild fire season), your new fire behaviour classification classes will change drastically. What is the point then? I strongly suggest this is removed from the manuscript.

- The authors use their dataset and make a 'finding' that area burned is mostly a function of fire growth rate rather than rate of fire spread. This result is obvious by several reasons, the simplest one being that the rate of spread is only related to the area burned for the initial stages of a fire growing from a point source. From the moment a fire is affected by topography, fuels, and burn over several burn periods and days, it is the area growth rate that is linked with the fire area, not the rate of fire spread. I do not see this a finding, whatsoever. Of course, a 2 dimensional area growth metric is going to be more related to the final burned area than a one dimensional metric of fire propagation (ROS). As with the previous point, I strongly suggest that this is removed from the manuscript.

It is not clear why the authors depart from their main focus of the study, describing how the database was assemble, to do a spurious analysis of the data and come up with these findings, that, in my view, are not really findings. If the authors want to explore those aspects of fire behaviour, then they should do so in a different piece of work, with proper basis and analysis.

Other minor comments:

What is the certainty in the intermediate perimeters (isochrones) in figure 3? I cannot imagine you were able to collect data across all the perimeter to make such a nice polygon. Were they interpolated? If so, they might provide a false sense of certainty.

Please also note the supplement to this comment:

<https://essd.copernicus.org/preprints/essd-2022-475/essd-2022-475-RC1-supplement.pdf>