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Interactive comment on "The Global Fire Atlas of individual fire size, duration, speed, and direction" *by* Niels Andela et al.

Anonymous Referee #2

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The paper is relevant as it tries to provide a new approach to the analysis of fire regimes, by analyzing different parameters of individual fires extracted from global burned area products. This effort is relevant to better parameterize fire models, as well as to understand fire trends affected by changing climate and socio-economic conditions. The main problem I found in this paper is their ambition to qualify single fire activity from a product that was not derived from this purpose. Recent papers (Padilla et al., 2015; Padilla et al., 2014) have found that global burned area products have important omission and commission errors, particularly for small fires Chuvieco et al., 2018; Roteta et al., 2018. They provide a good image of fire activity at global scale, meanwhile the analysis is done at global or at much continental scale. However, establishing characteristics of single fires from these products may be quite misleading. If the

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authors do not provide better validation datasets, the parameters they analyze at global scale may be in fact confusing. In my view, this is the main weakness of the paper. The authors are assuming estimations from a dataset that is not really validated. Until the MCD64A1 is fully validated, and we better understand their strengths and weaknesses, deriving such detailed analysis as presented in this paper may create more confusion than knowledge. In fact the comparison (validation is not an adequate term for what the authors include in the manuscript) analysis show a high degree of uncertainty even for the simplest variable (fire perimeter). When perimeters are compared with those derived from higher resolution data (MTBS), the correlations are low (for the authors, line 578: they are “reasonable correlations (r2 ranging from 0.3 to 0.5)”, but we should remember that they imply than 70-50% of the variance is unexplained). Therefore, in my opinion the subsequent analyses derived from this dataset are quite likely to be erroneous. The comparison they made with active fires and MTBS shows also poor agreements in all biomes. What about fire speed or direction? I suggest that they at least compare their results with specific very large fires where fire growth is available for different forest services, to check if at least for those large fires their estimations are correct. Very large fires could also be assessed using Landsat data, at least for fire perimeter-size and shape. Are you sure that Australia had a single fire of 42.000 km2? They could also compare their outputs with models of global fire weather conditions (Jolly et al., 2015; Pettinari and Chuvieco, 2017), as well as include some comparisons with fire spread and duration published by fire behavior experts. On the other hand, I doubt about the utility of providing global averages of different fire parameters, such as fire duration or progression by continent. In this regard, some of the comments included in the results section may seem quite trivial or difficult to justify empirically. What is the point of concluding that “fire duration exerted a strong control on fire size and total burned area”? Is this not the case in the vast majority of fires? In summary, the authors should make an additional effort to really validate their product and better identify the weaknesses of current analysis.

Specific comments Line 45: Worldwide, fires burn an area larger than the size of the

European Union every year (Randerson et al.,2012; Giglio et al., 2013). Please include total area in km2, the reader does not need to know the size of the European union to understand your sentence. Line 55: you claim that burned area reduction is occurring in the last two decades, but Andela et al., 2017 paper refers only to the 2001-2017 period (1995-2001 with more uncertainty), so you could only claim that the reduction is observed in the last few years, as you do not have date from several decades ago. Line 65: Our understanding of global fire activity is also severely constrained by the coarse resolution data we are based on our analysis. Recent analysis of burned area estimation comparing coarse and medium resolution data shows that in fact we may be losing a significant part of fire activity (Roteta et al., 2018, https://geogra.uah.es/fire_cci/sfd.php), particularly in tropical regions. Line 88: update (Giglio et al., submitted) Lines 155-164: How did you proceed in the case of small fires (a few pixels)? You claim that local minima are deleted when they do not spread forward in time. Lines 180-187: Fire spread is obviously associated to wind speed and slope, not just to fuel availability. Therefore the assumptions made by the authors seem quite arbitrary for a global product. Have they made any validation of their persistence algorithm? It is not clear what happened with areas that burned 2 times, were they assigned 6 or 8 day persistency? The thresholds are in fact overlapped. Line 195. It is not clear if two active fires that merged were assigned a single perimeter or two. It seems they were divided, but most forest services would probably consider them as single one. Lines 240-. . . It is not clear what the authors did when areas were not observed by clouds or cloud shadows. What is the impact of unobserved periods in fire progression? Were the geometrical deformation effects caused by off-nadir observations taken into account? Figure 3 shows direction of spread that are not very realistic, as all sort of directions are included, even for neighbor pixels (North and South directions in contiguous areas??) It is not clear why did you include MCD64 in Figure 4, as the date information should be the almost the same as the Global Fire Atlas. I would recommend changing it to a single graph showing dating accuracy for the four major biomes The fire dominant direction will probably be more useful for fire modelers ex-

pressed in degrees. Other authors have done similar analysis, a recent one by Laurent et al., 2018 Line 440. I doubt that any fire behavior modeler would agree with: "... the dominant direction typically represented less than half of the pixels". I think the approach by Laurent et al (2018) using the dominant direction of the evolving ellipsis is more adequate in this regard, as most fires have a dominant wind direction. I do not understand the meaning of using average NDVI values to show extreme fires. I do not see the relation.

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