

Interactive comment on “Precipitation-fire linkages in Indonesia (1997–2015)” by Thierry Fanin and Guido van der Werf

D. C. Zemp (Referee)

delphine.zemp@pik-potsdam.de

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General comments The authors examine that the relation between rainfall and fire in Indonesia, with a particular focus on the regions with high number of fire events. Their main findings are of interest for the community of Earth System scientists and beyond, in particular those regarding thresholds of accumulated rainfall prior to high fire years, as well as the importance of minor rain events during the dry season to limit fires, as this has potential implication for predictions and model development. The results are generally clearly presented and discussed by referring to relevant previous studies. My major concerns relate to the methodology based on linear correlations for the merging of the datasets. In my point of view, this approach is over-simple as it omits potential non-linearity in the different datasets. This is problematic as the authors use the merged time series to conclude on the differences in fire and rainfall patterns during

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1997 and 2015. Furthermore, as it is clearly mentioned in the introduction, fires in Indonesia are tightly coupled with land-use change. The Mega Rice project is mentioned in the discussion of the results, but it would be interesting to include land-use change data in the analysis of spatio-temporal variability of fires as well. If the authors think that it is outside the scope of the study, they should in my opinion tune down the conclusions on the role of rainfall patterns to explain fire events.

Specific comments

1) Description of the study region (L. 135-143) This section uses a lot of vague expressions such as “most of”, “largely covered by”, “abundant”, “a large scar of”, “widespread area”, “extensively”. . . The authors should give quantitative estimates and provide associated references.

2) Merging precipitation datasets To merge the precipitation datasets, the authors use a linear regression based on the 4 driest months (L. 168). What is the rationale behind this procedure? Unless the authors have a solid justification for this choice, they should deepen the analysis of the (linear or non-linear) similarities between the full time series (and not only dry months). For example, while a linear correlation seems appropriate in most of Indonesia (as shown by high R² values in Fig. 2b), it is not in the case in the northern part of the country. The authors mention the low inter-annual variability as explanation, but this statement should be justified by quantitative analyses. To strengthen the methodology, further assessment of a possible non-linearity between both time series would be appreciated.

3) Merging fire datasets: The authors mention the spatial resolution of the Terra product (L. 182), but not of ATSR. Please give this information. Why are the data combined at 1° lon/lat grid? Is this the resolution of ATSR? The “correction factor” that the authors calculate (L. 195) again assumes linear correlation between the time series without justification. The authors show this correction factor for grid cells with high fire detections (Fig. 3b), but in my point of view they should deepen the analysis of non-linear

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similarities between the time series.

“Our fire correction factor compensated for the lower sampling rates from ATSR over Terra between 2001 and 2011 in all Indonesia (R^2 is 0.97)” (l. 200). The authors should better explain their arguments. What correlation was quantified?

How is the “fraction of daytime fire burning over night” (l. 211) calculated? Is this ratio calculated on a yearly or daily basis? The authors should give more details on their methods.

3) Results

Fig. 1: What is the unit: number of active fires per year? Furthermore, I suggest to increase the size of the colorbar and to add ticks in order to make the figure more informative.

Fig. 5: It is not clear what is meant by “monthly fires” and “annual fires” in the figure caption.

Fig. 9: Is it an average over Indonesia? Perhaps this could be specified.

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