

Interactive comment on “Seasonal forecasting of fire over Kalimantan, Indonesia” by A. C. Spessa et al.

Anonymous Referee #2

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Review of ‘Seasonal forecasting of fire over Kalimantan, Indonesia’ by A. Spessa et al.

The authors use precipitation datasets (observed and forecasted) and satellite-derived fire activity data to test how well fires can be predicted in Kalimantan with a lead time of several months. This topic fits very well in the scope of the journal. Most of the research presented more or less repeats earlier work as mentioned in the text. The inclusion of seasonal rainfall forecasts is the main novelty. The work could be an interesting addition to the literature but I do have one major question about the results and several smaller suggestions regarding the methodology.

My main question: after reading the paper I was left wondering whether the inclusion of the rainfall forecast adds skill compared to simply using ENSO forecasts and statistical

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relationships between actual ENSO conditions and fire activity. For example, are the spatial patterns better predicted? I am asking because a large part of the skill of the seasonal weather forecasts comes from the same information as what is used in an ENSO forecast on which earlier studies were based. Because the use of the ECMWF seasonal forecasts is trumpeted as the main innovation the authors have to prove that this adds something over basing the fire forecast on ENSO forecasts (which then can be converted to rainfall rates) alone.

Methodological issues: RSS is an active fire product but you consistently label it as burned area, please change that throughout the paper. They are different quantities, and simply converting an active fire to the area of the grid cell to get area burned is wrong. This ratio varies spatially for various reasons (see for example Giglio et al., 2006, <http://dx.doi.org/10.5194/acp-6-957-2006>). Comparing RSS and a “real” burned area product in terms of m² burned as done in 4.4 is useless and confusing to readers.

In addition, the NOAA AVHRR active fires used in the RSS active fire product suffer from satellite drift (changing overpass time) which in combination with a strong diurnal cycle yields erroneous time series. I don’t think this has been corrected for in the original publication so please be careful with using this dataset to look at time series.

My final comment about the fire datasets used: most seasonal forecasts are used to say something about air quality or emissions in general, not so much burned area. Why haven’t you simply used the ATSR active fires to base your analysis on? Active fires to some degree integrate the effect of burned area and fuel loads, and it is the only consistent time series covering 1997-2010. You would have avoided a lot of potential issues if you had done so, and I would advocate redoing the analysis this way. Granted, ATSR has some issues as well, mostly related to being limited to night time observations. But it is easy to argue this is less of an issue than the other datasets used: the burned area dataset simply has the disadvantage that it does not account for fuel loads, and the active fire dataset you used is a composite of various datasets which are probably not able to generate a consistent time series.

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You use an AVHRR 1993 tree cover dataset and two MODIS tree cover datasets (2000 and 2010). Then to get the 1997 tree cover you interpolate between the AVHRR and MODIS tree cover sets. To be scientifically sound, this requires a comparison between AVHRR and MODIS for a time period that they overlap.

I noticed a few typos (“has lead to”) and would encourage the authors (including co-authors!) to have a thorough final check of the revised version of the draft

Figure 1: please use color instead of grey scale

Figure 2 onwards: please clarify units (mm per what? Km² -> fraction of grid cell makes it easier to interpret)

Two final comments: Please add a word of caution about these forecasts in the conclusions. In May of this year a very strong El Nino was forecasted, but we are now left with a very normal year. Your forecast would have probably not been very accurate. Since you have “only” used data up to 2010, how did 2011-2013 do according to your forecast? Using these years (and ideally also 2014) to test your system would have been a nice addition.

Second, please realize this work is to a large degree academic. It would be great to have an accurate fire danger forecast but in reality I doubt it will be of much use in Indonesia over the next years or even decades. Fire is used as a tool to convert or prepare land and the dryer it is, the easier that gets. As long as there is no alternative for the use of fire the forests and peatlands of Indonesia will be burned whether there is a good fire forecast or not. That is no critique of the work, but just a side note.

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