**Responses to Referee 2 Comments**

***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,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 forests 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 forests 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.***

As we describe in the paper, while ENSO is the major driver of rainfall variability in the equatorial Pacific region, large-scale rainfall patterns in the region are also affected by other major weather systems such as the Indian Ocean Dipole (IOD) and the Madden Julian Oscillation (MJO), and their interactions with ENSO are highly complex (Field et al., 2009; Reid et al., 2012; Dobles-Reyes et al., 2013). Furthermore, land rainfall in the maritime continent is also affected by a complex of biophysical effects including land–sea distribution, orography, land cover, and local SSTs (Aldrian & Susanto, 2003).

One could in theory develop statistical models taking all this into account, but a dynamic physical model, such as ECMWF Seasonal forecast System (System 4), is the better path forward given the full complexity of meteorology in the region and the limits this places on statistical approaches.

***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.***

We have corrected all references to the RSS data in the paper as 'fire-affected' burnt area. In addition, we provide further justification in the paper for using the RSS data in its present form, as well as a fuller description of the potential issues surrounding its accuracy.

***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.***

There is indeed a drift of roughly 2 hours (from 2pm to 4pm over the study period of 1997-2001) for NOAA-14. However, this drift is expected to have only minor effects on the detected active fires as the peak time for detecting fires in that area of the world is between 1pm and 5pm (Langner & Siegert 2009). As such, we see no justification to make any further changes to the paper in respect of satellite drift.

***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.***

We disagree that a revised set of analyses based on ATSR is needed. It is important to note that the limitation of ATSR to night-time observations in combination with the fact that most active fires peak in the afternoon would most probably cause a severe underestimation of fire activity based on ATSR data alone. According to Langner & Siegert (2009), ATSR misses about 70% of all fire events, which would render a proper analysis not representative at all. So even if we were to focus only on air quality or emissions, we would obtain biased results because of mostly detecting only the long-lasting fires (in the peat areas) with distinct emission properties but missing out other fires with different emission characteristics.

***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.***

This is simply not possible as the Continuous Fields Tree Cover AVHRR dataset is only available for the 1992/93 period (http://glcf.umd.edu/data/treecover/) while the first MODIS sensor (on the Terra satellite) was launched in late 1999 (http://modis.gsfc.nasa.gov/about/design.php).

***I noticed a few typos ("has lead to") and would encourage the authors (including coauthors!) to have a thorough final check of the revised version of the draft***

This has been done.

***Figure 1: please use color instead of grey scale***

This has been done.

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

As described in the methods of the paper, rainfall is mm per gridcell. We favour using $km^2$ for burnt area or fire-affected area because grid cell sizes vary. Hence, we see no justification for changing these units in the paper.

***Please add a word of caution about these forecasts in the conclusions. In May of this year a very strong El Niño 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.***

We have added some words of caution about our forecasts in the conclusions.

Performing additional forecasts beyond 2010 is outside the scope of this paper.

Regarding 2014, it has not been a ‘very normal year’ thus far, but rather moderately dry, which was forecast by the ECMWF. There was media speculation that a major El Niño would happen, but the actual seasonal forecasts from the operational centres were more nuanced as they evolved. Figure 1 shows the ECMWF precipitation forecasts initialized in May (JJA) through August (SON). The JJA and JAS forecasts predicted moderate/strong dry anomlalies over Sumatra and Kalimantan. The forecasted dry anomalies weakened for subsequent forecasts. Rainfall activity in September/October indeed turned out to be moderately dry. It was not not severe or for long enough to induce a major episode, but enough to promote increases in fire activity and a moderate haze episode ending in mid-October. This is shown by the airport visibility at Palembang and Palangkaraya in Figure 2. Ultimately, the moderate haze episode was commensurate with the moderate drying predicted over western Indonesia as the forecasts evolved over the summer.

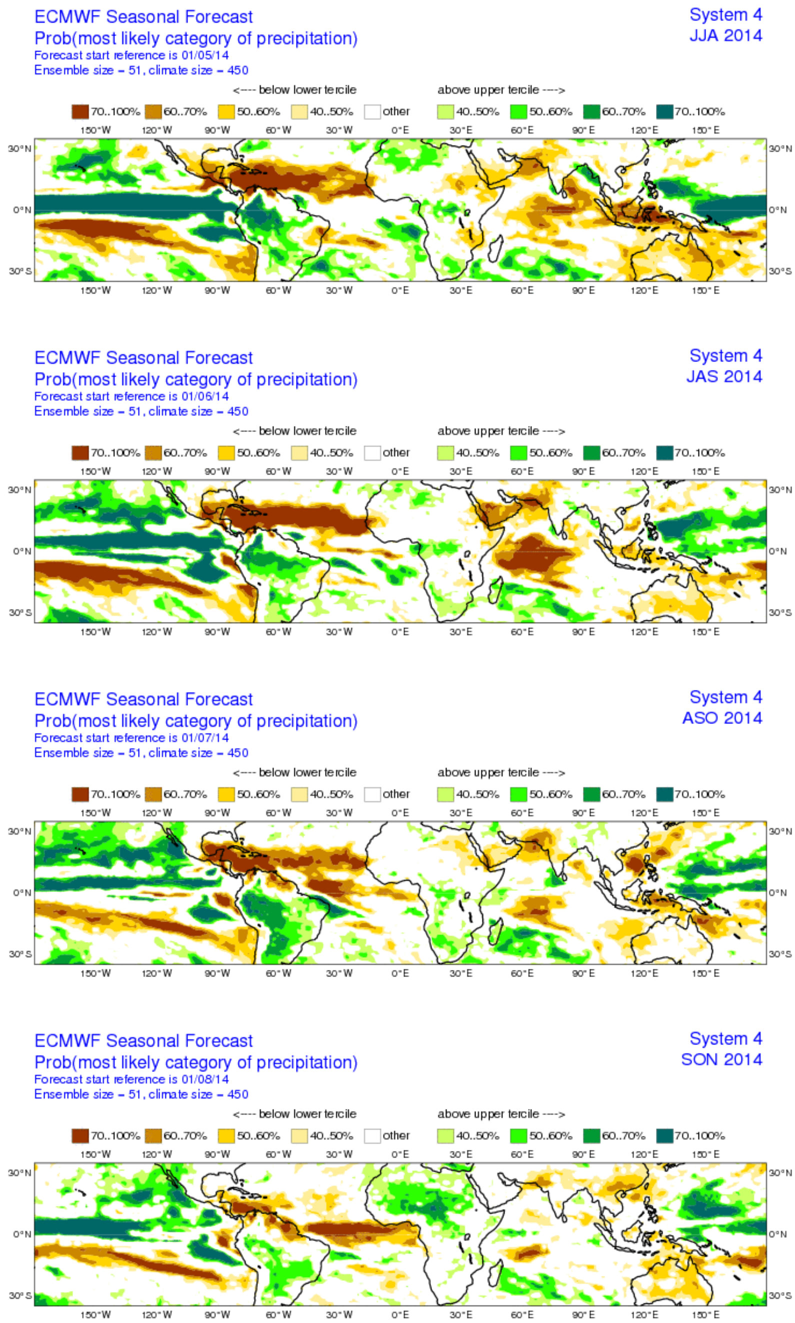


**Figure 1. Three month ECMWF precipitation forecasts initialized in June through September 2014.**

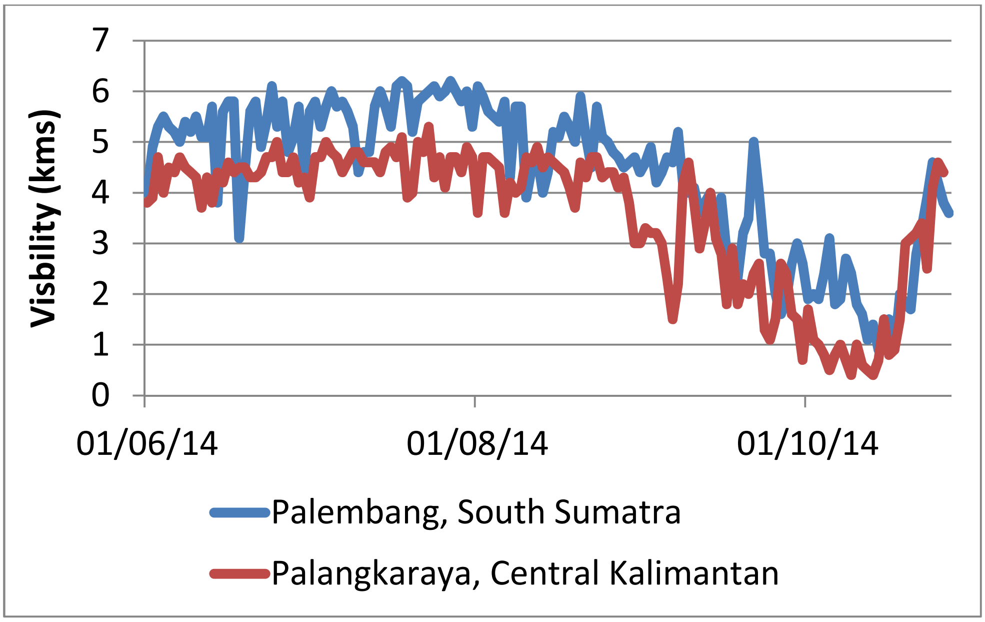


**Figure 2. Airport visibility at Palembang and Palangkaraya.**

***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.***

Fire activity in Indonesia has and will continue to depend on land use management policies and enforcement, including fire management. If future progress is to be made towards improved forest and peatland management in Indonesia, our paper shows that seasonal fire forecasts can play an important role in achieving that aim.

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