Answers to Anonymous Referee #2 "Combining fire radiative power observations with the fire weather index improves the estimation of fire emissions"
By Francesca Di Giuseppe et al.

This is an important contribution to the literature on estimating emissions from fire radiative energy (FRE). The implications for existing methods of emissions estimates is timely, relevant, important, and perhaps most importantly, relatively straightforward to implement. The writing is very concise, perhaps in some places too much so.

We would like to thank the reviewer for appreciating this paper. We also believe is a first step toward taking a stronger modelling approach in the use of FRP observations to improve fire emissions. As he/she also points out this paper tries to prove a concept; that is possible to model FRP starting from previous day observed FRP and a simple function of weather conditions. The ease of implementation was a strong requirement as we wanted to test the approach in an operational setting. We obtained a limited (encouragingly) positive improvement. Probably by having a better model for the FRP, for example by considering other variables and not only the FWI as a predictor, there is scope for improve the results in the future.

The authors only refer to fire radiative power (FRP), and not FRE, the integration of FRP over time, which is used to estimate biomass combustion, from which emissions estimates for various species are calculated. The use of a lagged FWI is an interesting choice, as several fire weather indices from the FWI family already have 'memory' from the previous day (fine fuel moisture code, duff moisture code and drought code).

We model the FRP instead than the FRE as this is the variable that is mostly directly related to biomass burning consumption. Also the FWI is a measure of fire danger integrated over a day therefore changes in the FWI are more likely to be related to the integral over time of FRE.

Also, FWI was developed for Canadian temperate and boreal forests, is it really appropriate to apply this method of inferring FRP to tropical and subtropical systems as the authors do (without good result)? It requires justification.

The FWI was originally developed for the boreal forest however has been applied successfully in many other part of the planet. Showing where there could be large errors in modelling FRP changes as a function of FWI changes is a good suggestion. We have attempted to identify regional variations by using the land cover mask that is implemented in GFAS to attribute the dry matter combustion rate to each grid point. In GFAS the land cover classes are derived from the dominant burning land cover type in GFED3.1 and additional organic soil and peat maps (full details are given in Heil et al. 2010).

By using this land cover mask we have estimated the error between FRP and persistence and between FRP and the FWI model for all the points for which a valid FRP was observed. The 2d error density plots

shown highlights that the FWI is very effective in reducing the errors when there is a overestimation of fire activity and works equally well in different land covers.

We have added the plot to the paper and and explanatory paragraph which reads

In the new approach changes in FWI are used to predict changes in FRP. The accuracy of this assumption depends on how a good predictor is FWI of fire evolution. The FWI was developed to describe fire danger and behavior for the boreal forests of Canada and its accuracy might be smaller for different vegetations. To understand the expected reduction of error in different ecosystems figure \ref{fig:land_cover_class} partitions the results shown in figure \ref{fig:obs_comparison1} by land cover using GFAS classification. In GFAS the land cover classes are derived from the dominant burning land cover type in GFED3.1 and additional organic soil and peat maps, (full details are given in \citet{heil:10}). The density plots show a substantial reduction of the overestimation errors for all land cover types. One interesting aspect is that positive biases are reduced more than negative ones. This behavior can be explained noticing that the distribution of FWI, by using FWI values only when fire events are observed, is conditionally sampled towards high values \citep{digiuseppe:17}. At extreme value, the FWI flattens out and increases in its value are limited. Instead an increase in precipitation and humidity can produce a sudden FWI decrease, which translate to large negative value for the modulation factor. Negative FWI increments are therefore larger than positive ones. This asymmetric behavior means that errors are mostly corrected for overestimation of FRP on missing observation locations.
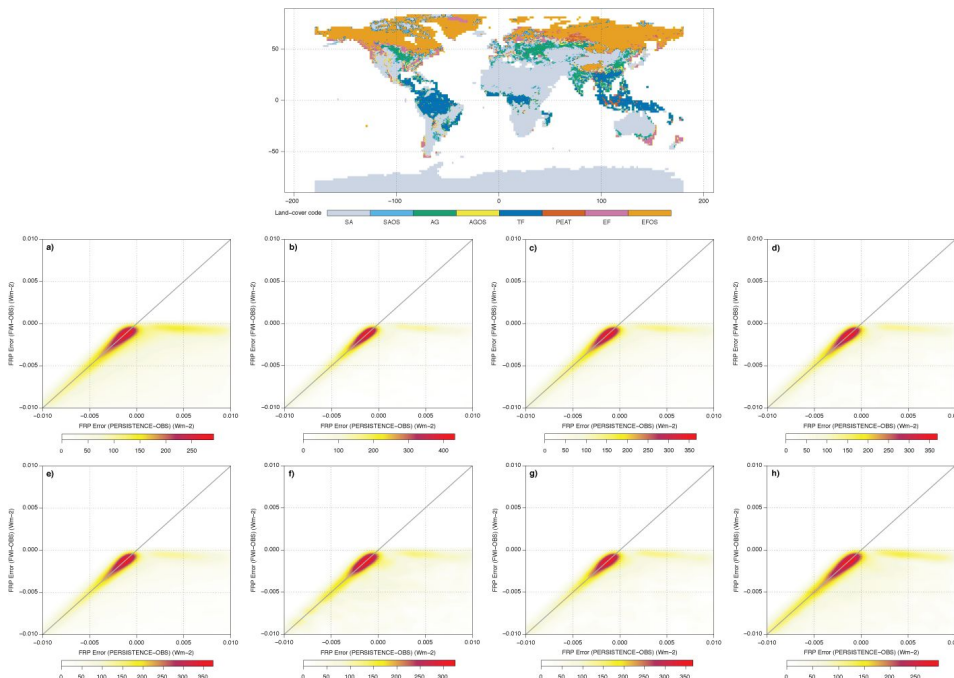

Figure caption

Upper panel: Land cover class map based on dominant fire type in GFEDv3.1 and organic soil and peat maps. Gaps in land areas have been filled (see Kaiser et all (2012) for details). Lowe panels: 2-dimensional probability density functions (PDFs) of the observed FRP departure of MODIS compared to the two observation operator models relaying on the assumption of persistence and on the FWI. The PDF refers to different land covers a) Savanna (SA), b) Savanna with Organic Soil (SAOS),c) Agriculture (AG),d) Agriculture with Organic Soil (AGOS), e) Tropical Forest (TF), f) Peat (PEAT), g) Extra-Tropical Forest (EF), h) Extra-Tropical Forest with organic Soil (EFOS)

Some terminology seems unclear, such as reference to "overestimation" (which implies some reliable reference) when comparing GFAS and MODIS data.

Our reference is what is observed in terms of FRP from MODIS. We then compare this value to the persistency assumption and the FWI based model.

Also, I'm not sure how "missing data" were identified, as no cloud masking algorithm is described.

GFAS uses the MOD14 product from MODIS collection 6. The MOD14 files contains a fire classification pixel which is used to process valid observations.
MOD14/MYD14 fire mask pixel classes

1. not processed (missing input data)
2. not processed (obsolete; not used since Collection 1)
3. not processed (other reason)
4. non-fire water pixel
5. cloud (land or water)
6. non-fire land pixel
7. unknown (land or water)
8. fire (low confidence, land or water)
9. fire (nominal confidence, land or water)
10. fire (high confidence, land or water)

We do not apply any masking algorithm ourself and just rely on MODIS classification. This clarification has been added in the text:

The FRP (ρ) observations from MODIS used in this study are available at 1 km resolution from the MOD14 product of MODIS collection 6. This dataset also provides a pixel classification attribute to flag missing observations (mostly because of cloud cover) and low confidence measurements, which is used to disregard invalid observations. Valid observations are first area-weighted by the portion of satellite footprint included in the grid box area in order to provide mean FRP density on a grid box of 0.1 degree.

Is it possible that real zeros are being filled in with data using this method? This should be addressed in the text.

No this is not possible. FRP =0 is a valid observation and is therefore not processed neither with the FWI method nor with the persistence one. Thus, pixels for which, at DAY -1: FRP >0 (Observed "active "fire) and at DAY 0: FRP= NA (Missing observation) are going to be filled by the modelling approach(es)

Instead, pixels for which, at DAY -1: FRP =0 (Observed "non-active "fire) and at DAY 0: FRP=NA (Missing observation) are **not** going to be filled by the modelling approach(es).

Finally, is this a global analysis? The study area should be stated explicitly or if it's a global analysis it should say so.

GFAS is a global system and so is our analysis. We have stated this clearly now in the abstract and in the introduction.

The atmospheric composition analysis and forecast for the European Copernicus Atmosphere Monitoring Services (CAMS) relies on biomass burning fire emission estimates from the Global Fire Assimilation System (GFAS). GFAS is a global system and converts fire radiative power (FRP) observations from MODIS satellites into smoke constituents.

Specific comments: P9L4: Why is this remarkable? Can you be more explicit?

The reviewer is right that this sentence needs more explanation. In IFS small changes in the initial conditions might not be visible in the forecast. If these changes are "perceived" by the model as random noise they are just dumped as spurious oscillations in the model (there is a digital filter implemented in the IFS for this purpose). The fact that changes produced by the introduction of the FWI persist through the forecast it means that they are "compatible" with the model dynamical state.
This has been now elaborated in the text.

Finally it is remarkable that changes in the biomass burning emissions from GFAS which, are used to initialize IFS, have an impact over the whole 5 days of model integration, signifying that these changes are compatible with the model dynamical state. On the contrary random noise would be just dumped as spurious oscillations in the model by the digital filter implemented in the IFS.

P9L7:Why is this the expectation? Couldn't it have just as easily underestimated emissions if peaks were missed? (Indeed, could this not still be happening if overpasses occur outside of peak burning hours?)

This question is in line with the previous question of the reviewer and highlights that we need to be more careful in specifying our reference. In this paper our truth is provided by the FRP observed by MODIS. If this is our reference and we compare *only* with valid observation we expect the FWI model to provide a decrease in the predicted FRP and thus in the emissions. This is also visible in the plot included in the previous question. The decrease in predicted FRP is is due to the fact that FWI negative increments are larger than positive ones. As a consequence we also expecte a decrease in emissions.

However this doesn't mean that GFAS in its standard configuration is necessary underestimating emissions as to assess this we would need to compare to measurements of fire emission which are not available at the global scale. An hint that GFAS dataset might underestimate emission is provided by our figure 8 which shows an underestimation of aerosol optical depth when compared to 90 Aeronet stations in North-America. However this result cannot be generalised. We have tried to be clearer rewording the sentence as follows:

The reduction in the GFAS emissions when the new FWI model is used is consistent with the fact that large FWI decrements are more likely than increments, during fire burning events. Negative increments decrease modeled FRPs, when compared to persistence, and lower the predicted emissions. It is more challenging to verify if the reductions are also improving the GFAS errors given the shortage of available measurements of fire emissions.

P9L15: I'm not sure that "assumptions" is the right word here? Operator versus non-operator? These aren't assumptions, exactly, maybe "methods"?

Agree - changed text

P9L16: Not sure about "experiment", as that's not really what this is, again "method" may be more appropriate.

Agree - changed tex

P9L18: This is great (40% result), it should be highlighted in the abstract

We have reworded the abstract as follows:

One of the consequences of this assumption is an increase of fire duration, which in turn translates into an increase of emissions estimated from fires if compared to what is available from observations.