Analysis fire patterns and drivers with a global SEVER-FIRE model incorporated into Dynamic Global Vegetation Model and satellite and on-ground observations

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The paper presents a dynamical fire model coupled with a vegetation scheme that is the global extension of a previous regional version designed for the Iberian peninsula. In general the topic is quite hot in the community. There is consensus that for many aspects we should go toward integrating more and more processes into the Earth system modelling, as it is proved that one process can improve the predictability of even not affected variables.

While the topic and all associated developments are very welcome I found that the paper does not live up to my expectations. First of all I should agree with the previous comment that the introduction feels more like a rant about others work that a fair assessment of the quality of the presented model. Moreover most of the time there is no scientific justification on why other approaches appear to be inferior. I do not find that the use “of rather complicated equations” (line 16) could be considered as an objective metric to judge the (non)- quality of a model in my opinion

But what annoys most is the conveyed idea that satellite data and their use is almost inherently wrong and/or inferior to local measurements. This is just a personal thinking of the authors contradicted in large part by the tangible improvements that satellite data have brought to many communities including oceanography, numerical weather prediction and obviously fire mapping. Clearly there are limitations in satellite data but so there are in using local observations or even fire lab experiments as the representativeness is a serious issue there.

In my opinion statements like “No satellite derived data are used as an input of the model. Only physically based or just ’common sense’ based equations from on-ground observations allow direct implementation of SEVER-FIREModel...” should be removed as they have no quality justification apart from the liking of the authors.

Essentially I highly recommend to rewrite the introduction removing all the assertion that cannot be justified scientifically and highlighting the innovative aspect of the model proposed.

Methods

The description of the model is a bit chaotic possibly due to the fact that a big part of the model had already been developed, therefore equations seems to appear out of no-where.
I understand the need to describe the model but if modelling components were fully described somewhere else than a reference to a previous publication should suffice. Specific points:

1. the fire danger index is a byproduct of the model and not a model component and should be put later.
2. In the equation (7) for the number of expected fire from lightning I was expecting to have a soil moisture component as that would discriminate between wet and dry lightning. I believe the parameter moist is a constant ? or is this soil moisture? Please clarify.
3. The parametric equations (5) and (6) need some justification are these teh fit over some data ? Is this published somewhere else? If so they should be removed and the paper should only concentrate on what is new in this model.
4. In the analysis in figure 2, how you make sure the fire were ignited from a lightning ?
   Equation 9. I wonder how you set the parameter a. Why did you decided that 1 fire over millions of hectares is a reasonable number? Also what is it millions of hectares? 1,2 ,10 ?
5. I suppose equations 9 and 10 have been derived somewhere else ? as all appears pretty cripticat
6. The need for a simplification when considering human induced fires is understandable. One thing I would add is a fire management factor. So in Europe it is not just a matter of GDP- or wealth but also of controlling program in place.
7. Page 11 line 5 you mean EFFIS? Suppression makes sense in Europe and 2 days is probably reasonable. However there are many places were suppression does not take place. Is this a global parameter ? Please comment on this

Data

1. Please specify if the GFED dataset used include small fires.

Results

Results are difficult to judge as the datasets used for validations are affected themselves from large uncertainties. The model seems to produce reasonable spatial patterns for burned areas and a good improvements in the burning temporal variability especially when large anomalous conditions take place as the ones induced by ENSO. I do not see the lack of a big improvements as a problem as this is a first overall assessment of the global model and components can be tuned and improved if a specific aspects is proven very relevant for the fire process.

Final remark

The paper is very dishomogeneous in the way is written. The discussion for example is very nicely worded while the method session is badly explained and difficult to follow as many equations are just taken out without clearly stating if this is the outcome of a previous analysis (I suspect that is the case). A throughout re-writing of the introduction is a strong requirement as at the moment, a part from upsetting an entire community, is not making a good service to the model either as does not explain what are the innovative aspects
Finally as this is the presentation of a new model or at least a substantial development of an old one, I would suggest to extend the “code availability” section giving more details about the model itself (programming language, input output format/ licence etc etc)