

## **Modelling fire frequency and area burned across phytoclimatic regions in Spain using reanalysis data and the Canadian Fire Weather Index System**

The authors model fire frequency and burnt area using phytoclimatic regions as integrative spatial units for modeling. The modeling of daily fire frequency is done with good accuracy while for burnt area some limitations are obvious and the authors suggest that in the future, fire frequency predictions are more reliable and preferable. Additionally, they suggest that temperature and some FWI components are the most important variables driving the temporal patterns of fire frequency across the phytoclimatic regions of Spain.

### **General Comments**

In general terms I think the work is interesting and well supported by the methodology applied. However, it has important limitations that could be overcome and significantly improve the work.

#### **Writing Issues:**

- There are some grammar mistakes along the manuscript that should be improved with spelling check and with a more thorough revision. Nothing too serious but worth to take a look and make some improvements.
- Be careful with mixing past, present and future in your sentences. My suggestion is to report what you did, what the results are and will be the implications of your research findings.
- The verb “result” is frequently confounded with the verb “to be”. Example (pag 4): “In practice both indices resulted highly correlated and only SPEI..:” should be replaced by “In practice both indices were highly correlated”.
- I have some doubts in the use of the word “skill” to describe model “accuracy” or “performance”.

In my opinion, the most interesting and important result of this paper is the use of phytoclimatic regions as modelling units and the advantages it has when compared with grid-cell modelling. Additionally, I think the manuscript would benefit from exploring this key issue a little deeper: in conceptual terms (fuel, climate, fire relationships in the different phytoclimatic regions) and in terms of implications for future research/management.

Following my previous comment, I think the paper would benefit from a more thorough exploration of the implications of the major findings of this work (research, management...). Additionally, stating the advantages for future applications of using phytoclimatic zones instead of grid-box approaches is important and useful.

The understanding of why models do not perform well, is limited and poorly described. The work would greatly benefit from exploring this key point more thoroughly, i.e. understanding why, where and when models do not perform well is, in my opinion, more important than just evaluating how they perform. This could shed some light on future improvements and important components that are missing from your modelling approach (ignitions, human activities, etc). Bear in mind that I am not suggesting you include more variables and redo most of the work, a simple residual analysis crossed with some variables that were not included in the work, would be useful and improve the work's value.

I disagree that you demonstrate the usefulness of ERA-Interim reanalysis data. You demonstrate that using that data, along with the modeling approach, can provide interesting results. But to demonstrate the usefulness you would need to do some extra analysis, such as use multiple datasets and assess their impact on the capability of predicting fire frequency\burnt area. So, at most, you state that this study

along with Bedia et al 2012 suggests that the ERA dataset is suitable for studying fire history in Spain.

It seems to me that the “human” component is missing from the work. I understand and accept that this was out of scope and that the aim was to model fire patterns using only climatic data. However, I believe it is important to shed some light on how this “human component” may influence your results and what is its relative influence (the two major patterns in terms of phytoclimatic areas might be also linked to this and not only climate?)

### **Specific Comments**

The first sentence of the abstract is confusing, please rewrite.

In the abstract, in my opinion the sentence “allowing to test model performance under different fire regimes” is incorrect. The use of phytoclimatic units allows you to test model performance under different climatic and fuel conditions. You have no *a priori* information about the fire regimes and it makes sense you keep that way.

The colors in figure 1 are confusing, particularly, the “redish” colors. I suggest you try to improve this.

I suggest making a reference to section 2.6 and table 2 in the last paragraph of section 2.5 so it is clear why SPI was discarded.

I suggest shortening the caption of figure 3.

It seems that figure 6 should be figure 5 and vice versa.

Please provide a more detail justification for the statement “bearing some sort of memory on the antecedent conditions” present in last paragraph of the Conclusions.

### **Questions**

**Q1:** In the abstract and conclusions you mention that fire frequency predictions are more suitable for past fire history reconstruction and pose several advantages over burned area. What are those advantages? This should be in the manuscript.

**Q2:** The definition of phytoclimatic regions was based on Spanish Meteo Agency data and not in ERA-INTERIM. Did you make any comparison between both meteorological datasets? If they have significant discrepancies what do you think will be the impact on your results and major findings?

**Q3:** Why do you think that in most phytoclimatic regions, the time series of fire frequency and burnt area follow so closely? Some works have shown that, for instance in agricultural regions, the fire frequency can be high but with very low burnt area.

**Q4:** Did you try to make any temporal aggregation of fire frequency data? Weekly, biweekly or monthly, for instance. Predicting daily fire frequency is challenging, do you think that, both in terms of research and fire management applications, a larger time step would be as useful (or even more) and that results would be better? If not, please justify.

**Q5:** In section 2.6.1, for grid-box model training you sampled fire absences based on information from

the entire phytoclimatic zone. Why? Moreover, don't you think that this procedure is bringing the grid-box and areal approaches closer together? As I mentioned previously, I think that difference between both is one of the most important results of this work.

**Q6:** The way you performed the fire occurrence model training involved bootstrap techniques (page 6, sec 2.6.1)?

**Q7:** Can you provide a more detailed information of the main causes behind the bimodality in region 10-15? How well does your model fit to the annual cycle in these regions? (see the residuals analysis suggestion above and I suggest reading Le Page et al. 2010).

**Q8:** What about region 9, don't you think this exhibits some sort of bimodality also?

**Q9:** Monthly-varying threshold – So is this monthly or seasonal? In page 9 sec 3.2 you mention seasonal. From what I understand this is done at a monthly basis without “looking” to the other months, i.e. you don't do a “moving temporal window” approach, so if I understood correctly, the term “seasonal” cannot be used or should be used in another way.

**Q10:** I think that it was expected that areal approach would provide better results than the grid-box approach. For management purposes working with broad and large phytoclimatic regions can be a limiting factor. Do you have any suggestion on how to “decompose” or “break” this broad regions into smaller contiguous areas that would enhance the usefulness of your work for fire management purposes?

**Q11:** In section 3.2, how did the sample size (N) vary with the burnt area threshold? Could the results shown in the first paragraph of this section be significantly affected by the sample size and condition your findings? I suggest you put the N in tables 3 and 4.

**Q12:** What do you think are the main reasons for the correlation decrease from smaller to larger area thresholds? It seems that this result is contradictory with figure 3, please confirm.

**Q13:** In Table 3, the bimodal phytoclimatic areas do not have significant models' performance. What do you think are the main reasons for this, how does this limit your work's findings and how can this be overcome in the future? Is the earlier fire peak captured by your model predictions? (again the residuals suggestion, above).

**Q14:** In conclusions, if the good model performance in terms of RSA does not directly translate into a good reproducibility of fire frequencies then what does this say about the capability of RSA to provide reliable model performance indications? What do you suggest as alternatives? (I suggest you take a look at the Model Efficiency Index developed by Nash and Sutcliffe, 1970)