

Editor comments:

I finally had a chance to read carefully your manuscript, and it appears clearly that you have taken great care of the writing of the manuscript and creation of figures. The manuscript reads very well, sentences are carefully constructed and easy to read even for a non-specialist of fires, as me. I have to say no comment, except perhaps one or two technicalities and a few clarifications required on scientific aspects:

- ESD will appreciate dois of references, both in the main manuscript and in the supplementary material.**
- I am unsure of the "n.d." convention for papers which do not have a date, yet, but the publishing office will take care of that anyway.**
- page 8 line 21 : extra parentheses around Bush 2001**

The references are generated automatically by Zotero, and don't always come with DOIs indeed. To change this I'll need to remove zotero automatic fields and edit by hand. If it is ok I will keep them this way for now in case zotero is needed for some changes (including on your comments below), and will add the required information once everything else is settled.

- Equation (1) is the one that triggered some scientific comments of mine. As you say that you use a Monte-Carlo technique, you must take care of the stochastic variance of the parameters. In which case you must be using a kind of Metropolis-Hastings algorithm, which actually requires a likelihood function (not just a cost function). Can you clarify ?

Statistics are not really my area of expertise, but one of the co-authors who understands them better suggested this simple edit to the manuscript:
P. 4, l. 29: "The MCMC likelihood function employs an optimization metric, which combines average burned area and inter-annual variability at the grid-cell level".

Does that address your comment ?

- Figure 3 : "indicate variability from 8 climate models" : I have some difficulties here. Isn't it rather "uncertainty" (using the inter-model spread as a measure of uncertainty, which is standard practice though not free of questions), and in that case what is the relevance of speaking in terms of percentiles when there are only 8 samples? Please clarify.
We used the term "variability" instead of "uncertainty" because climate models have little (or no) predictive value. I don't feel comfortable talking about uncertainty, while the reality in 50 years could very well be beyond the range of projections from these models. So the percentiles show the spread within this group of model, but do not quantify uncertainties about future climate change. Does that make sense ? We mentioned low consensus/predictive value of climate projections in the first discussion paragraph, but shall I further edit the manuscript to clarify ?

Non-public comments to the Author:

I recommended your paper for a 'highlight' in EGU.

Thank you very much ! And many thanks for your support and editorial work !