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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of lightning and other meteorological factors on fire activity in the North American boreal forest: implications for fire weather forecasting" *by* D. Peterson et al.

Anonymous Referee #2

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Referee report on "Effects of lightning and other meteorological factors on fire activity in the North Americal boreal forest: implications for the fire weather forecasting" by Peterson et al

The paper deals with the pressing problem of creation of a list of generic parameters that may explain the features of the fire seasons in different parts of the world. The specific area of the study is the boreal zone of the North America. I found the paper interesting and, albeit somewhat lengthy, quite well-written. However, I also have a few concerns, which, to my mind, should be addressed in the revised version of the paper. They are summarized below. All-in-all, they sum up to major revision, mainly because

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the uncertainty estimates that are missing may require some extra analysis.

General comments

The paper is dedicated to formal correlation analysis of more-or-less arbitrary set of parameters of fires and weather, which all have one main advantage: they are available from existing observations or models. Not denying the power of such "random-walk search" and admitting that several other authors used some of these variables, I would still appreciate a more extensive discussion justifying the completeness of the selected list or explicitly denouncing it. It is also evident that some of the parameters are essentially the different indicators of the same physical processes – and must be strongly correlated. One could consider then the reduction of the list or, at least, explicit discussion of the matter.

So far as I got from the input data section, the lightning data are from the observations, while rain is from the model output. Needless to say, these two datasets are not exactly comparable, especially for convective conditions. So, the "dry" strikes can be dry just because the model failed to predict the storm that took place in reality. The opposite problem holds for "wet" strikes. The estimate of the uncertainty that comes out of this is missing.

A similar kind of overlooked uncertainty is the limited ability of MODIS to register the fires that take place under clouds – possibly being started by the very thunderstorm that prevented MODIS from seeing it.

The above data compatibility issues should be assessed in the section 3.4 Data integration, which is now a technical description of spatial reprojection exercise followed by a simple algebra to compute new derived quantities. I suggest to reduce the technicalities but add more scientific analysis of the datasets compatibility.

The result section seems to be too long. The tiny details of correlations between different parameters, differences between the regions, etc seem to be rather features of the

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10, C3466–C3468, 2010

study setup than the reflection of general processes driving the fire seasons. At least I found few options and attempts to generalize the findings. The paper would get only stronger if this section is made shorter and more concentrated on strong dependencies.

Specific comments The “FRP flux” does not seem to be an appropriate term: the FRP is already a flux of FREnergy. Since the “FRP flux” in the paper is just the MODIS FRP over the grid cell normalised to one squared meter of the surface area, something like “normalised FRP” or “area-scaled FRP” would be much better.

Abstract. The term of “dry lightning” is used but not explained.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8297, 2010.