Biogeosciences Discuss., 9, C3623–C3626, 2012
www.biogeosciences-discuss.net/9/C3623/2012/

Biogeosciences
Discussions

Interactive comment on "Satellite-based assessment of climate controls on US burned area" *by* D. C. Morton et al.

Anonymous Referee #1

Received and published: 29 August 2012

This manuscript presents an analysis of climatic effects on the interannual variability in burned area across the conterminous United States and Alaska. The study takes advantage of two long-term spatial databases of burned area from the Global Fire Emissions Dataset and the Monitoring Trends in Burn Severity project. The study does a good job of documenting the spatially heterogeneous responses to burned area to climatic variability across different regions of the United States. This research also highlights the potential for using large-scale multitemporal fire and climate databases to carry out spatially explicit analyses of climate-fire interactions. The specific results presented here seem to mainly corroborate research than has been done previously. Overall, this is a well-executed study and a well-written paper that presents a synthetic perspective on climate-fire relationships at a national level. However, there are a number of points that need to be addressed to enhance the clarity and enhance the impact

of the paper.

General Comments

One general question I have is about the consistency of the MTBS dataset over time. Based on my knowledge of this project, I am fairly confident that they have consistently included all of the large fires over the 500 and 1000 acres thresholds back to 1984. However, as indicated in the manuscript, other smaller fires were also mapped based on special requests. My assumption is that most of these additional fires occurred during the active years of MTBS (the 2000's), not during the 1980's or 1990's. Could there be temporal biases? If so, could they be influencing the reported trends and climate-burned area correlations?

It is unclear what type of correlation analysis was used – I assume that these are standard Pearson correlation coefficients, which require assumptions about multivariate normality and linearity. How well do the data meet there assumptions? A Spearman's or other rank correlation would be less powerful, but also less sensitive to departures from these assumptions. At the very least, it would be valuable to repeat some of the correlation analyses (e.g., Figures 5, 7, and 8) to check and make sure that using a different type of correlation coefficient doesn't change the results.

The discussion generally does a nice job of synthesizing the results and putting them into broader context. However, it mostly emphasizes the concordance between the results of this analysis and previous work. It would help if the discussion could more explicitly highlight the novel results from this paper and how they extend our understanding of climate-fire relationships.

Specific Comments

Page 7863, Line 22-26: I understand the rationale for expecting lower burned area in the MTBS datasets in areas with smaller fires and fires on private land. However, I don't understand why the MTBS estimates would be higher than GFED in other regions (e.g., NP and SW). Do these differences simply reflect error in the GFED (and MTBS) estimates? Or are there other types of biases at work in these regions? Also, I would not say that the GFED and MTBS estimates are "similar" for the AK region.

Page 7862, Line 12. I can't really discern a strong trend of increasing correlation with radiation and temperature as a function of latitude in Figure 5. I can see more negative correlations (green pixels in the southern portions of the maps, but these seem to be mainly in pixels that are dominated by water.

Page 7865, Lines 19-20. From what I can tell Figure 8 doesn't include any results about climate controls on potential evapotranspiration. It only reports results of correlations between climatic variable and burned area.

Page 7866, Line 23: ". . . despite lower PE values during the January-March fire season in the region." I assume you mean lower PE values during the fire season than during the other months of the year, but please clarify in the manuscript.

Page 7866, Line 27. It's not clear to me how low absolute values of PE necessarily provide evidence that BA is insensitive to PE anomalies.

Page 7867, Line 4. I don't think that Figure 11 supports the statement that "Climate controls on US burned area (expressed as PE) increased over the past 30 yr", since the results have clearly shown that not all regions are equally sensitive to PE and also suggest that climate-fire relationships are temporally heterogeneous.

Page 7871, Line 10. "Non-climate drivers of regional BA were also important for the seasonal and interannual variability of fire activity across the southern US." It's not clear how this concluding statement arises from your work, since only climatic factors were considered in the analyses. Are you assuming that non-climate factors are more important in the southern US because climate-fire relationships were generally weaker in these areas (e.g., Figures 7 and 8)?

Figure 5. It would help to indicate the critical values of the correlation coefficient at an

alpha level of 0.05 (these should be the same for all pixels if they have the same n).

Figure 8. Why aren't the MTBS BA 1997-2010 results also reported in Figure 8?

Interactive comment on Biogeosciences Discuss., 9, 7853, 2012.