Replies to Anonymous Referees

Replies were prepared by Brian Magi, Sam Rabin, Elena Shevliakova, and Steve Pacala. Our replies follow the specific referee comments and are indented in italics. Thank you to both the referees for their time and suggestions.

Reply to Anonymous Referee #1

Page 2, lines 20-22: There are some issues with the English in this section. Also a definition of projected climate change is needed.

> *We added a reference to Krawchuk et al. (2009), which is one study that attempts to understand the impact of global warming/climate change on future fire regimes. Thanks for pointing this out. The sentence is grammatically sound, as far as we can tell.*

Page 2, lines 23-27: What is an agricultural burning practice. Some examples of practices and how the signal can be detected/measured/observed from space is needed. Our hypothesis here is to consider the flammability of fuels as an indicator? What is a non-agricultural land cover. Avoid general terms or ambiguities in this section.

> *Lines 11-15 define various agricultural burning practices, with references. We added the definition we use for 'agricultural' and 'non-agricultural' land cover - thanks. Your sentence starting with 'Our hypothesis' is unclear, so we have no response.*

Page 3, lines 1-5: Why is the distribution of the globe best served by the one you choose? More justification is needed. Does it fit the agricultural distributions of the world. In fact can you not section your results against the results of the temporal distributions that you find.

> *As we state in the text, the 14 regions are based on previous studies of fire patterns observed from space and products (such as GFED emissions) that are based on those space-based sensors. The reference to Giglio et al. (2006b) clarifies this further. As we state in the Conclusions (p.15, lines 1-9), the 14 regions do have weaknesses that are revealed through our Discussion. Our discussion is structured against regions of most intense to least intense burning. We felt that there were too many types of fire seasons observed (How many peaks? Does it correspond to flammability/lightning? Are agricultural and non-agricultural fire seasons similar? etc.) to structure a discussion against the temporal distributions.*

Page 3, Section 2.2: I have concerns in this section. I understand that GFED database derives its burned areas from the MODIS fire count data (or thermal anomalies). Therefore, are you not using the same data set twice (or one that is similar)? Explain that MODIS and AQUA collect active fire data 4xdaily under cloud free conditions.

You are correct - GFED depends on MODIS fire counts. We adjusted the wording in the manuscript to focus on results using GFED and removed much of the unnecessary mention of MODIS (Section 2.2, for example, has been revised). In our Conclusions, we point out that our results are essentially the same using MODIS fire counts rather than GFED burned area (as expected).

Page 3, line 25: What does 5' mean (minutes, seconds?). This HYDE data is critical to your analysis and your treatment of it in the manuscript is weak and not sufficient. How has the product been validated? Are there areas of uncertainty? You make assumptions in Section 2.3 that need to be better supported. Please think about strengthening this section.

5' means 5 minutes or 5/60 degrees; we have edited that sentence to clarify – thanks for pointing that out. Our study is not intended to be a validation of the HYDE dataset. HYDE 3.1 and its predecessors have been extensively cited and analyzed in the literature, as a search for papers that cite Klein-Goldewijk et al., 2011 via Google Scholar would indicate. HYDE 3.1, for example, is the key input for the historical land-use scenarios (Hurtt et al 2011, in Climatic Change) in the Climate Model Intercomparison Project, phase 5 (CMIP5) experiments, which are the basis for the climate projections of the IPCC Fifth Assessment Report (AR5). We tested our methods against other land-use datasets (SAGE, HYDE-SAGE), but the results were essentially the same for the decade we discuss. HYDE had the most recent land-use map with a peer-reviewed publication to support it. Relevant to your comment is text that we added in our Section 2.3 in response to Referee #2 regarding the use of HYDE 3.1 data.

Results Section: The interpretation relies heavily on a comparison with the findings of Korontzi et al. (2006) and Le Page et al. (2010). This takes place in most subsections. The authors need to consider who the presentation of these results is made with the greatest impact. Hence, what you need to make clearer is what the differences between your results and those published before. More importantly why the results differ and the magnitude of the difference. You need to avoid repetition of results and if they are all identical, the paper need not be published. Furthermore, how do the results of the burned areas versus the hotspot data differ?

In no case are our results merely identical to results from either of those papers. Korontzi et al. (2006) dealt only with cropland burning; although in some cases we find nearly identical results for agricultural burning, we also present non-agricultural burning, which Korontzi et al. (2006) do not. Le Page et al. (2010), on the other hand, present only patterns of overall burning as compared to what might be expected in a human-free world, whereas we separate overall burning into agricultural and non-agricultural burning. Thus, our work represents an advance with respect to both of these studies. We mention their results either (a) to show that our statistically-derived results are similar to those derived directly from satellite imagery (and therefore that our method is valid), or (b) to interpret our results. For example, if our "agricultural" (cropland + pasture)

seasonality looks like the "cropland" seasonality from Korontzi et al. (2006), then it follows that cropland burning is more extensive than or is timed similarly to pasture burning. Indeed, the fact that we had to rely so heavily on these two studies indicates a paucity in the literature of comparative analysis of fire seasonality at global scales, something that our method is designed to address.

Burned area and hotspots essentially result in identical seasonalities. See our response to your comment above. Thanks.

Conclusions, lines 24-29: This whole section seems to be detached from the results you have presented. I think more work is needed before you make these claims.

This part of our Conclusion is intended to reflect where the research is going, as implied by phrasings like "can be" and "could be." We do not suggest we have proven anything, but we intend to test the methods in a way that the results might be further generalized beyond the "agricultural" and "non-agricultural" separation.