Review of "A weekly, near real-time dataset of the probability of large wildfire across western US forests and woodlands" by Gray et al.

https://doi.org/10.5194/essd-2017-136

General Comments: This is a very ambitious effort to collate numerous disparate databases, develop a model of large fire probability, and create a real-time system for distributing and updating data. I applaud the authors for taking on this challenge. That said, there is substantial room for improvement with respect to the documentation of the modeling effort that should be undertaken before the paper is accepted. I also believe that a more compelling set of figures could accompany the figure to elucidate its utility.

Specific comments/considerations

1. The modeling framework is poorly described. The authors talk a lot about the data inputs, but it's not transparent how the model was created (other than RF), whether a single model was used for the entire region, how exactly the 10-different models compare, which variables of those selected were significant predictors.
2. The validation of the data/model is poorly described making it challenging to assess whether the model is "good". This needs to be improved upon for the credibility of the dataset.
3. It is a bit challenging to understand the mismatch in spatial scales that define a large fire (400ha) and the spatial scale of output (250m). Technically, a large fire would consists on a continuous group of at least 64 250m x x250m pixels. The configuration of fuels and topography would appear to be important in determining if a given pixel could be part of a large fire. This would require not only considering the fuel/topography/weather at each individual pixel, but neighboring pixels. I don't know how this is dealt with in the model.

Technical Corrections/Considerations

1. Line 25, I am unsure whether it is appropriate to provide links to cloud storage or GEE image collections that may be temporary and not available in a few years time. I think the link to the doi is appropriate, and would suggest keeping the links to the other sources in the main text. This is for the journal to decide though.
2. The first paragraph is a bit rough and could use revision. Specifically you are contrasting the research and operational needs that operate at different spatiotemporal resolutions and highlighting the different data needs. I think the point should be made that while many datasets exist to support such efforts, and effort to synthesize and model large fire risk using an empirical framework at such fine-scales is a gap.
3. Line 9, Page 2, insert "moderate temporal resolution (weekly)" to emphasize that you are not only modeling at high-spatial resolution.
4. Line 16, Page 2, replace "misspecification of these parameters" to "assumed model parameterizations"

5. Line 19, Page 2, I believe that models of fire spread potential are run operationally on fires with FlamMap. It might be best to specifically address the upshot of the proposed modeling platform here over FlamMap (e.g., different goals, spatial extent).
6. Line 29, Page 2, "show that both long-term normals and variability in climate and vegetation…"
7. Line 31, Page 2, It might be useful to specify how flammability is being used here and throughout. It could refer to fuel dryness, but also fuel abundance, or their combination. Long term climate exerts an influence on biomass production and the biogeography of vegetation and hence sets the stage for fuel in addition to average fuel-moisture which limits fire. Also, it might be useful somewhere in the text to specify the timescales of "short-term weather". To some, this is on the matter of hours-to-days in terms of wind-driven fire. In this paper it likely refers to sub monthly timescales related to vegetation dryness.
8. Line 32, Page 2, Rather than say flammability here, I think you really mean biomass or fuel abundance.
9. Line 8, Page 3, Does ignition refer to anything that would potential spread fire into a pixel (ignition, or fire spreading into region)?
10. Line 12, Page 3, It is probably worth caveating somewhere that ignitions are not random, but adhere to specific spatial patterns tied to anthropogenic activity or lightning. It is fine to state that this is not part of the modeling framework.
11. Line 8, Page 4, How are prescribed fires excluded here? MODIS will pick up large prescribed fires. You could use the Short FPA data to exclude any large fires that might have been prescribed. Note that your seasonal window April-October will not eliminate prescribed fires as they occur in many regions in late Sep/Oct. That said, I doubt they are a sizable number given the size thresholds used for large fires.
12. Line 16, Page 4, Is this NLCD data specific to a certain year. Ideally, this could be land cover pre-fire.
13. Line 18, Page 4, Please define size threshold for small fires.
14. Line 25, Page 4, I understand wanting a balanced database. But what consequences do you think there are by selecting small fires in the same month/year from ecoregions where a large fire occurred. There is a lot of spatiotemporal autocorrelation in your primary drivers of temporal variability in your model that could weaken your relationships. Presumably, you'd want random samples of small fires from ecoregions.
15. Line 30, Page 4, I don't complete understand how EVI and NDWI were used here. Long term averages implies averaging over years and months. But in many regions that are snow covered, you would have poor EVI data that is unrepresentative. It would also be useful to provide a basis for using these variables for live fuel moisture .
16. Line 20, Page 5, I think you used 1981-2010 climate normals, as this is the standard 30-year period. I am confused by the variables listed. I believe none of these are inter-annual. Furthermore, if you used monthly temperature and precipitation, what additional value is there in having annual sum/averages, and CV.
17. Line 27, Page 5, See Boer et al. (2017) for LST as a proxy for fire danger.
18. Line 7, Page 6, The NFDRS typically involves a fuel model. What fuel model was used and was is consistent spatially?

19. Line 19, Page 6, How many large fires occurred in total in 2015/16? I think that by "randomly" using 400 large fires, and pulling from a number not much larger than that is a limiting factor (e.g., not very independent).
20. Line 25, Page 8, How do you suspect the model handles non-stationarity? It was built with historical conditions.
21. Table 1: Missing ERC from section 4. Table 1 figure is a bit awkward and might not be needed.
22. Figure 1 caption: Use SI units. Figure 1 itself shows MTBS. Why introduce MTBS here? Also, do you require a different training seed within each large fire? Note that these really won't be independent points due to autocorrelation.
23. Figure 3, I think it is best to just show a single map, but specify the exact date, and perhaps show any large fires that actually occurred.
24. Figure 5, This is a poor figure. I don't understand what the 10 different points refer to, why the 0.45 cutoff is shown, and didn't realize that there was a model for small fires in addition to large fires. It also looks like this includes data through 2017. If so, please update the caption.

References:

Boer, M. M., Nolan, R. H., Resco De Dios, V., Clarke, H., Price, O. F. and Bradstock, R. A. (2017), Changing Weather Extremes Call for Early Warning of Potential for Catastrophic Fire. Earth's Future. doi:10.1002/2017EF000657