

## ***Interactive comment on “A weekly, near real-time dataset of the probability of large wildfire across western US forests and woodlands” by Miranda E. Gray et al.***

### **Anonymous Referee #2**

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While this manuscript presents a concept that would be valuable (near real-time large fire probability), I believe it oversells its novelty over existing products and I am suspicious that there are methodological flaws (but there is not enough information in the methods section to tell for certain). As the topical editor has already noted, the products are not available at either Figshare or Google Cloud Storage (I did not try the Google Earth Engine). While the authors did locate other near real-time products that deliver similar information (e.g. Preisler et al 2016), they missed other datasets currently commonly used for planning fuel treatments and other management activity (notably Short et al's "Spatial dataset of probabilistic wildfire risk components for the conterminous United States" at <https://www.fs.usda.gov/rds/archive/Product/RDS-2016-0034/>, which

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was calibrated to previous fires using Short's Fire Occurrence Database which also provides part of the underpinnings of this current effort). This dataset and similar efforts using the FSim model (Finney et al 2011) already deliver many of the functions that the current effort proposes it is uniquely positioned to fill, including acting as "a foundational dataset for longer-term planning and research, such as strategic targeting of fuels management" (e.g. Scott et al 2016, Thompson et al 2017), "fire-smart development at the wildland urban interface" (e.g. Haas et al 2013), and "analysis of trends in wildfire potential over time" (Finney et al 2011, then Short et al's updated dataset, and also the fire potential datasets the authors reference by Dillon). Given the mature state of this previous work and its prevalence, its inclusion in the current manuscript would seem important. In addition, the authors state that other existing models do not account for long-term fuel and climate variability, but I'm not convinced their model better accounts for these due to a number of reasons: 1) their model uses a number of variables as proxies for fuel and weather that as far as I know have not been demonstrated in the literature to relate to fuel availability and flammability or to meaningfully measure weather's effect on fire, including PDSI (which in fact has been demonstrated to not be related to large fire activity (Riley et al 2013)), EVI, and NDWI, 2) as far as I can tell, their model has a static development layer (the CSP from 2001 and 2011), thus not capturing development trends except coarsely, and 3) it's unclear how their model captures prior burns, which they state are important factors in fire spread (perhaps they mean the EVI to do this, and indeed it might, but citations or analysis are needed to show that the EVI captures differences in the pre- and post-fire landscapes). Other functions that they state their model can accomplish are already delivered during wildfire incidents by a suite of models in the Wildland Fire Decision Support System, including FlamMap, FARSITE, and FSPro – which do in fact function in real time, with runs taking on the order of 15 minutes to 1.5 hours and being delivered the same day to suppression forces. However, the approach taken here is novel and could be a valuable addition to the suite of existing models – if methodological questions can be addressed.

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Moving on to model structure, I can't discern from the manuscript what the response variable in random forests is. Is it probability of large fire? Probability of small fire? Wouldn't these two be related and increase together? Is probability that the pixel doesn't burn calculated? Or is it simply predicting binary large versus small fire? Or to predict binary burning by large fire versus not burned? Also, it's unclear how predictor variables were chosen. Did the authors choose a set of variables based on their hunches about what's important? Or was variable importance in random forests used to guide selection? I am concerned about a potential flaw in model design: it appears that the authors model probability of each pixel independently – however, probability of burning is not independent and is affected by contagion from neighboring pixels. Each pixel's burning can't be considered an independent event, since burning is spatially related to its neighbors. To that end, each large fire might properly be regarded as the unit of prediction, not individual pixels. If the authors have accounted for this, methods should be shared in the paper. The manuscript is largely lacking in assessment of goodness-of-fit of the model. Also, is it feasible to predict burn probability without first predicting ignition? Burning is predicated on ignition taking place first, and then on spatial contagion: thus, the burn probability of one pixel wouldn't be independent of the nearby ones. Some discussion of this, demonstrated by out-of-bag error rates and additional goodness-of-fit metrics is needed. Also, better demarcation of prescribed fires versus wildfires is needed. Lastly, how were the outputs validated? By comparison with other existing burn probability or fire regime datasets?

Specific comments follow:

Page 2 Line 4: the sentence that ends “characteristics of fire regimes” would seem to require citations. Please add.

Page 2 Line 7: also see Finney et al 2011, Preisler et al 2016, and Preisler et al 2004, for example, who have already produced this type of work.

Page 2 Line 14: it seems that Finney et al 2011 (FSim paper below) should also be

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referenced here. The comment that follows is not relevant to FSim (“it requires detailed specification of many model inputs and is highly sensitive to misspecification of these parameters”), which is calibrated to fire occurrence in Short's FOD.

Page 2 Line 16-18: I am confused by this comment, since as noted above, FlamMap runs in seconds, FARSITE in minutes, and FSPRO in 15-90 minutes, meaning that they can and are updated subdaily during active fires.

Page 2 Line 19-20: these lines state that models like Preisler et al 2016 are constrained by availability of accurate high-resolution fire, weather and fuels data – however, later in the manuscript the authors correctly acknowledge that the Preisler et al 2016 model was run daily last year with updated weather data, with outputs available on the WFAS website.

Page 3 Line 24-26: I'm curious how fuel type (grass, brush, timber litter, etc) is accounted for in the model, as fuel type is directly related to fire spread and probability.

Page 4 Line 6: What are “large fire event days”? I'm thinking you mean an individual burned pixel. Please elaborate in this paragraph on how you decided whether burned pixels were part of the same fire – from Figure 1, it looks like you assigned pixels to MTBS fires.

Page 4 Line 23: what is meant by “there are methods that may be adapted to associate active fire information with small fire events”?

Page 4 Line 24: Why was it important to have the same number of small and large fires? There are many many more small fires than large fires in Short's FOD.

Page 4 Line 25-26: There are other (perhaps more effective) ways to remove prescribed burns from your dataset. For example, fire type is an attribute in MTBS. By using April-October fires only, you'd include most Rx burns in northern states like Montana and Idaho, and exclude large southern California fires like the recent Thomas Fire that often take place in December.

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Page 4 Line 30: I must confess I'm not familiar with the NDWI, but when I Google it, USGS calls it the "Normalized Difference Water Index" rather than wetness index, and describes it as being used to discern water from non-water. Are you talking about the same index? It seems improbable that there are two MODIS NDWI's with different calculations. . .but perhaps that is the case. Please clarify. In either case, I've not seen the NDWI used in any studies relating it to fire occurrence. So it is a good choice here? The relationship of canopy moisture and flammability is quite complex and not well understood (see for example McAllister et al 2012). Despite the lack of study of the NDWI, it could be a good predictor in your model, but not enough information on variable importance is presented in the current version for me to assess.

Page 5 Line 4-5: If I am understanding correctly, you only used MODIS values from inception up to the date of the fire to assign percentile values. Why not use the whole record? It seems in your current method, the percentile assignments would be sensitive to the date of the fire (so if a fire occurs at an index value of 100 in 2005 and another fire at a value of 100 in 2010 in the same pixel, these could be calculated to be different percentile values since the underlying distribution of values would be different).

Page 5 Line 6-7: What is the index of human modification supposed to signify with regards to burn probability? Why is it included? What was the variable importance score?

Page 5 Line 20: Can you explain more about why the CV of temperature and precipitation is "seasonality"? I don't follow. Similarly, why are temperature of the wettest and driest months and precipitation of the coldest and wettest months included as predictors? Have these been demonstrated to correlate with fire probability? Do they have high variable importance scores?

Page 5 Line 23: I think you are saying that EVI is related to fuel availability. I think you are working only in forested ecosystems, so most times EVI will be correlated with the canopy rather than the understory. However, surface fire propagates in the understory

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and crown fires are relatively rare. So is EVI really related to fuel availability? Also, see McAllister et al regarding live fuel moisture and flammability. Also in this paragraph, if you have only five years of data in some cases and you are calculating anomalies, you would have only 5 observations, right? Again, why not use the full MODIS record (or did you)? Also, are the average LSTs for both night and daytime temperatures?

Page 6 Line 5: Why was PDSI included as a variable when it has been demonstrated not to be strongly correlated with large fire activity (e.g. Riley et al 2013)?

Page 6 Line 8: Please state which NFDRS fuel model the ERC was calculated for. I believe Abatzoglou's product is for fuel model G.

Page 6 Line 11: fm1000 represents the previous 42 days ( $1000 \text{ hr}/24 = 41.666$  days).

Page 6 Lines 18-29: Were small fires assigned to a single pixel? Please explain why only one year of fires was used in evaluation (do you expect these relationships to be stationary from year to year when there is so much annual variability in area burned?). For the rest of this paragraph and the following paragraph I'm quite confused. I don't understand what the response variable in the model is (as stated above). Also, can you briefly define sensitivity and specificity? If the response variable is probability, how do you define a false negative and false positive?

Page 7 Line 10: I would like to see each of the bands illustrated by a figure, otherwise it's quite difficult for a reviewer to visualize and assess the product. How prevalent were pixels with a rating of 1 (at least one MODIS pixel was not processed or had bad quality)?

Page 8 Line 7: again, see other literature including but not limited to Thompson et al 2017 and Scott et al 2016.

Page 8 Line 11: There are other products updated daily that account for changes in weather and fuel moisture, including Preisler et al 2016. Some of the inputs to your model appear to be static, including the CSP (human development layer) and it's not

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clear how past disturbances (burns) are included in your model. Perhaps the EVI captures previously burned areas, but I know of no study that documents that. Have you assessed how your model works in recently burned vs. burned areas?

Page 8 Line 20: It's not clear how this model would provide better information to managers during active fires than the suite of models in WFDSS (FlamMap, FARSITE, and FSPro), which output information on predicted fire intensity, fire spread, and burn probability in near real-time. Please clarify.

Figure 1: This figure nicely illustrates how incomplete MODIS data is!! I've noticed this while daily following nearby fires in my area. MODIS often misses surface fires where the canopy is dense or even crown fires where the smoke plume is dense. Is MODIS then a good basis for predicting burned pixels (especially when it can be difficult to eliminate Rx fire)?

Figure 3: Please present actual values rather than "high" or "low". I don't feel I can validate the product without them.

Figure 4: I'm confused here. Why not present at-pixel values? Are these the sum or average of false positives for an ecoregion? I'm also confused as to what these mean: there is always a probability of fire, so what does it mean to have a false negative or false positive if you are predicting probability? Of course, as I said earlier, I'm confused about what the response variable is, so when I understand that perhaps I won't be confused here.

Figure 5: I'm confused here too. Is the white squiggly line the probability of small fire and the black squiggly line the probability of large fire? If so, the y-axis is incorrect. Is the vertical white line the date of a small fire, and the black vertical line the date of a large fire? What does it mean to randomly pair a large and small fire? Should they be related? Why in some cases are the black and white trends similar and in some cases different?

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## References

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