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Interactive comment on "Vapor pressure deficit controls on fire ignition and fire spread in boreal forest ecosystems" *by* F. Sedano and J. T. Randerson

Anonymous Referee #2

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The paper entitled "Vapor pressure deficit controls on fire ignition and fire spread in boreal forest ecosystems" by Sedano and Randerson describes an effort to establish the influence of a surface moisture variable on fire ignition and spread in Alaska between 2002 and 2011. The authors employ MODIS imagery to establish fire ignition and spread characteristics and North American Regional Reanalysis (NARR) meteorological data to indicate atmospheric moisture conditions associated with the identified fires. Results indicate that vapor pressure deficit (VPD) is related to the probability that a lightning strike will develop into a fire ignition and that above-average VPD increased the probability that fires would grow to large or very large sizes.

I find the paper to be generally well-written and the authors present their arguments

clearly, concisely, and with considerable clarity. I find the techniques presented herein to be scientifically interesting and worthy of publication. However, there are two major points as well as a number of comparatively minor issues I would like the authors to address before I feel the manuscript will be ready for publication.

Major issues:

1) The manner in which the authors employ the NARR data in this study requires additional description and consideration. NARR surface temperature and moisture variables are known to exhibit errors when compared against surface meteorological observations and historical simulations of diurnal weather conditions. Since the NARR does not assimilate surface observations of temperature and moisture as part of the reanalysis process, many investigators have found that substantial errors in these fields can occur for any given date/time. Climatological studies that employ long-term (monthly or greater) means of NARR variables are not uncommon. But this study employs values of surface temperature and RH from a single time period for each fire day, a technique which I cannot find a precedent for in the literature. In the meteorological community, it is generally not considered reasonable to characterize meteorological conditions for a given day using surface variables from the NARR. Using these surface variables to calculate a derived quantity such as the VPD arguably compounds the uncertainty, since VPD has an exponential dependence on temperature. I feel the authors must present either supporting evidence that their VPDs are reasonable or citations of analogous studies that have found reasonable results using the NARR in this fashion before the reader can be certain that the relationships presented here are defensible.

2) I am having difficulty with several terms that are used in this manuscript to describe the relationship between meteorological variables and fire characteristics:

In the title and throughout the paper, the authors suggest that VPD "controls" fire ignition and spread. While I believe some element of my difficulty could be due to differences in the way other disciplines use this word, I question whether VPD truly represents a "control" on fire ignition and fire spread in individual fires. I am quite prepared to accept that it has an influence, and that the influence can be assessed statistically employing the methods used in this study. But to me, "control" is a very strong term that implies a direct involvement in the physical processes that affect ignition and spread. And in that context, I feel this sort of statement is not defensible. Abatzoglou and Kolden (2011), the study on which this work builds, never suggests that any weather or climate variables control fire growth. For that reason alone, I am uncomfortable with the idea that VPD can be said to be a "control" variable for wildfire growth and ignition.

I am aware that other studies employ the word to describe the relationship between environmental variables and fire variables. But my understanding is that the term is most commonly applied when considering relationships between fire characteristics that do not have clear physical mechanisms (e.g. seasonal acres burned) and climatological mean environmental variables. I feel the authors should revisit each example of the word "control" in this manuscript and determine whether it is consistent with the manner in which the term is used in the existing literature.

Additional examples of terminology that I feel should be assessed in a similar context include:

P. 1322, line 7: I find "driver" here to be too strong. I accept that a statistical relationship exists between the variables. But "driver" to me implies a physical mechanism and I know of no established physical link between VPD and fire size. The discussion later in the manuscript of "climate drivers of interannual variability in wildfire activity" is more consistent with how I understand this term to be used in the literature.

P 1322, line 24: "regulated", again, suggests to me that a specific process has been identified, which has not been done in this manuscript or the literature.

Minor points:

P 1310, lines 15-17: Nowhere in the Results is the probability of ignition or spread

analyzed. Only in the Discussion is this result mentioned, and no probability analysis appears in the manuscript.

P 1313, line 12: "improved understanding" is vague and difficult to support rigorously. It would be better to state that the analysis provides information about the temporal and spatial dynamics of wildfires, rather than suggesting (without additional supporting evidence) that understanding is improved.

P 1316, line 9 and line 15: It is not clear to me what constitutes a "robust" spatial interpolation approach and how inverse distance weight interpolation (IDW) satisfies the criteria. Just saying it is "robust" without a definition is not sufficient. I am not questioning the use of IDW. I just don't see what justification there is for calling it any more or less robust than other techniques without additional information on how "robustness" is defined.

P 1321, lines 8-10: According to the caption for Fig. 3b, I see higher VPDs being associated with non-fire lightning strikes. I presume either the caption in Fig. 3b is incorrect or I have misread something. In any case, I disagree that Fig. 3b demonstrates that high VPD values are "required" for a fire to start. That higher average VPDs were observed does not necessarily mean that anomalously high VPDs are required for any fire to start, which is what this statement suggests to me.

P 1322, line 3: Again, I disagree with this statement. Higher average values do not necessarily mean that high VPDs are necessary for fire ignition in all cases.

P 1322, line 10: I do not think it is valid to generalize to "fire weather" when only one variable has been analyzed.

P 1323, line 8: I do not agree that positive average VPD anomalies indicates that drier than normal atmospheric conditions are needed to sustain fire spread rates. They help, but are they needed? I think this is too strong.

P 1323, line 10: I do not agree that they provide evidence of VPD allowing fires to grow.

One needs a mechanism to make this sort of statement, and none is presented here.

P 1323, line 15: I think "require" is too strong here as well.

P 1324, line 14: Similar to some of my comments above, I feel "enable" in this statement suggests cause and effect where none is established. I think they are associated with each other, but I do not feel there is evidence here of cause and effect since no physical mechanism is presented.

P 1326, line 12: I question whether this information is likely to considerably improve fire weather systems or fire behavior models. And I do not believe this study does anything to "develop more mechanistic models of fire spread" (line 19). The CFWI is now regularly analyzed and forecast using gridded meteorological data during the fire season in several different agencies and regions. Fire behavior models rely heavily upon physical mechanisms to describe the processes involved. Your study does not present any new physical mechanisms, only statistical associations. While the information presented here is interesting and potentially important, I am not convinced that considerable improvements in existing systems/models are likely to result from this study.

P 1327, line 20: "must align" is too strong. Your analysis shows that when they are aligned, there is a greater potential for high fire years. But it does not follow that high fire years can only occur when these conditions are aligned. Similarly, I do not find the use of "need" in the subsequent sentences to be supported by these results.

P 1328, line 6: "will lead to"? Based on what evidence or information can you make this claim?

P 1328, line 10: I question whether this understanding really "opens the possibility." I agree that the information is potentially useful in this context, but I also think it is possible to take actions of this sort with existing tools, if the desire and resources are available.

P 1329, line 15-end: Statements in the Conclusions may need to be revisited if revi-

sions are undertaken based on some of the suggestions above.

Interactive comment on Biogeosciences Discuss., 11, 1309, 2014.