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Biogeosciences Discussions

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

F. Sedano and J. T. Randerson

fsedano@umd.edu

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1) First, the use of all MODIS active fire detections without consideration of confidence is very uncommon and requires proper justification. It is my understanding that those detections with a confidence rating of less than 30% are basically junk, and should not be included in the analysis. This is the most common method I know of for subsetting the active fire detections based on the reliability of the data.

We agree with the reviewer that active fires with limited reliability should be excluded from the analyses. In fact, the description in the text was not accurate. Our analyses removed active fires with low confidence: Active fires with confidence below 50

(accounting for 77.3% of the total number of active fires in the study area during the period 2002-2011) were not considered in the analyses. The text has been modified accordingly, removing the sentence "For this study, we used active fires with low, medium, and high detection confidence levels from both the Aqua and Terra satellites".

2) Second, I'm concerned with the identification of the lightning strike that "began" a particular fire. After a burn is extinguished, we look at the landscape and think of the fire as a singular event that consumed the contiguous area comprising a scar. However, in examining the active fire data record, it is clear that burn scars often represent several points of ignition that converge to form the eventual 'burn'. The methodology as described only counts the first ignition within a burn scar and ignores subsequent ignitions, which are likely as important if not so in terms of the burned area. Take the progression of active fire detections in the 2004 Boundary Fire perimeter, for example. The ignition in the south occurred first, and another occurred several days later in the middle of the burn scar. Most of the burned area spread from the second ignition, not the first.

The role of secondary ignition points in the development of wildfires in Alaska is difficult to fully understand with the data we have. As noted by the reviewer, our analyses consider only single ignition points. Additional analyses not included in the manuscript of the evolution of the largest fires in Alaska often showed more than one coincidence in time and space of active fires and the lightning strikes. In our analysis, the first of these concurrent active fire-lightning strikes event was considered as the ignition of the fire. The other concurrent events mostly occurred along the direction of spread of the fire. Therefore it is not possible to establish whether they correspond to secondary ignitions or they are simply the result of lightning strikes in areas in which the fire is advancing. Given this, and in the absence of additional information we have decided not to consider them as secondary ignition points. Furthermore, whenever ignition points occurred in the vicinity of an ongoing wildfire days after its ignitions, they were considered as independent fires in the Alaska Large Fire Database (LFDB). Because of their evolution, neighboring fires could eventually merge, but LFDB would still count them as different fires (often part of a “fire complex”). In the particular case of the Boundary fire, the active fire records show an initial ignition in the center of the perimeter (mid June) from which the fire spread in all directions (predominantly North and South); it them moved to the West later in July, and continue further South in August before dying. The records also show that the “Wolf creek” fire occurred just south from the Boundary fire in June. This fire moved north and eventually merged with the Boundary fire later in July. If we considered these two wildfires as one, we would be definitively talking about 2 ignition points within the same wildfire. If we consider them as individual events (as it is the case in the LFDB) we would be describing one ignition point for each wildfire.

3) With respect to the lightning data, I’m also concerned about the problematic upgrading of the system in 2010 to a TOA system. As I understand it, the sensor improvement means that many more strikes will have been detected for the last two years of the study.

The period of study in the manuscript covers 2002-2011. Therefore only 2 years of records overlap with the updated system. Although upgrades to the lightning detection system are likely to result in more strikes being detected, our analyses do not show a significant increase in the number of lightning strike detections or number of lightning-induced fires after 2010. More years of data will be required before we can understand the impact of the 2010 upgrade in the historical records. According to the specifications of the system, the 2000 upgrade ensured fire a detection efficiency of 99% and a median location accuracy of 500 m or better. This would indicate that the pre-2010 system should have already provided a reasonable sampling the lightning strikes in the Alaskan Intermontane Boreal Interior.

Specific comments as follows:

4) Page 1311 Line 11: Reference to permafrost ‘levels’ is unclear. Do you mean ‘active layer depth’? Corrected: for clarity “Levels” has been substituted by “active layer

depth”.

5) Page 1311 Line 21: Barret should be Barrett. Corrected

6) Page 1313 Line 25: Was there any processing of the MODIS data to remove the multiple fire detections caused by the ‘bow-tie effect’?

Although products from new instruments such as VIIRS implement “bow-tie” correction processing (“bow-tie” deletion), MODIS MCD14ML does not account for this artifact. We did not implement any operational procedure to remove “bow-tie” effects”. However, in a preliminary analysis, we identified groups of active fires with exactly the same attributes and after a visual inspection, removed from the active fire dataset those considered as multiple detections. While this procedure does not guarantee the complete removal of all multiple detections, it contributes to minimize the problem.

7) Page 1314 Line 9: The use of low-confidence detections is addressed in my general comments, but there is an additional problem due to the lack of Aqua data for the period prior to 2003. Active Fire Detections from Aqua are only available after mid-season 2002, so the first year of the analysis is undersampled.

The problem of low-confidence detection has been addressed in a previous comment (only confidence levels higher than 50% used for the analysis). Terra MODIS initial acquisition corresponds to February 2000. Aqua MODIS initial acquisition corresponds to June 2002. The period of study covers 2002-2011.Although there is a mismatch of six months between the initial dates of the study period and the Aqua MODIS initial acquisition, the impact of this time lag has to be quite limited: Fire activity before DOY 150 is normally scarce and the majority of the active fires of 2002 should still be registered by Aqua even with this six-month time lag.

8) Page 1314 Line 13: It’s interesting that you used the MODIS Veg Indices product for this region. Given the strong BRDF issues in high northern latitudes, especially in areas of more pronounced topography, it’s common to use the MODIS NBAR product

to calculate vegetation indices to avoid this problem. As pointed out by the reviewer the effect of anisotropic reflectance of land surfaces can have a relevant impact in the analysis of remote sensing data. This impact will be more important in studies that require monitoring of land surfaces or the comparison of sites in different locations. While Nadir BRDF adjusted reflectance MODIS products (MCD43A4) correct BRDF effects, we believe that the MOD13A product, used in this study, does not introduce important BRDF distortions. The compositing method of MOD13A1 has been designed to minimize view-angle effects and BRDF related issues. Also, since the spectral bands extracted from MOD13A1 are used to produce a ratio spectral index, the impact of BRDF effects is also reduced. Finally, this study uses the MOD13Q1 product to calculate NBR and dNBR spectral indices, from which, we define a spectral threshold. The result of this operation is a binary mask that identifies unburnt islands. Given the level of precision required to create this mask, the magnitude of the BRDF effects is unlikely to have a significant.

9) Page 1318 Line 7: It is true that dNBR has been extensively tested as a proxy for fire severity in boreal forests, but you've neglected to mention that many of the analyses that evaluated the performance of NBR-family indices for detecting variations in severity have determined that it is not useful. I do think your use of the metric is justified here (NBR would be preferable to dNBR, given the issues with phenology and a short growing season in the region) because you are using dNBR to detect burned areas, not burn severity. But the citations you mention curiously avoid papers that have pointed out the shortcomings of dNBR for severity mapping, including several that were published in the same special issue of IJWF as the cited Murphy et al. 2008 paper.

We acknowledge the discrepancies about the use of spectral indices to measure fire severity. The original manuscript cited several publications using dNBR to demonstrate the use of these indices in this context. We did not include additional citations pointing out the shortcomings of dNBR to keep the description within reasonable limits, since this is not the main goal of this work. The revised version of the manuscript includes

some citations questioning the validity of spectral indices for fire severity mapping. As the reviewer mentions the main reason to use dNBR in this study is not measuring severity but burned areas and, the spectral signal of the bands involved in the calculation of the spectral index are sensitive to pre and post fire surface conditions. As for the spectral index used in the study, we still think dNBR offers some advantage over NBR: by normalizing post-fire NBR values with pre-fire conditions we minimize the effect of local environmental conditions, which allows us to define a single threshold for the whole area of study.

10) Page 1319 Line 29: I'm curious why you chose the interval from 1500 to 1800 (p.m. is redundant here) for calculating the VPD. Usually indices associated with fire weather are calculated from the noon or 1300 interval, the time of maximum solar elevation.

As the reviewer mentions the 1200-1300 interval (maximum solar elevation) is commonly used in fire weather indices. We have used the 1500-1800 cycle (NARR dataset is divided in 3-hour cycles) instead. The rationale to use this cycle is that the cycle following the period of maximum solar elevation should provide information on the persistence of favorable conditions for fire spread. However, ince fire ignition and spread of large fires (responsible for most of the burned area in Alaska) is commonly associated to the persistence of relatively large periods of warm temperatures, we believe the results would have not been significantly different if the 1200-1500 had been used instead.

11) Page 1320 Line 4: What is the spatial resolution of the VPD dataset?

The VPD dataset is created from the NARR relative humidity and air temperature. Therefore the original spatial resolution of the VPD images is 32 km. Daily VPD images have been subsequently resampled to 500 m to be integrated with the annual fire-spread maps

12) Page 1320 Line 11: Which fire perimeter do you mean? The daily burned area or the entire burn perimeter. This is not clear.

This sentence refers to the entire fire perimeter (it has been re-written in the revised version of the manuscript). However, since the original spatial resolution of the VPD maps is 32 km, and provide information of the general meteorological conditions in the region of the fire rather than variations within fire perimeters. Therefore, there should not be large discrepancies using daily burned perimeters or the entire burn perimeters.

13) Page 1320 Line 21: Replace ‘was’ with ‘were’. Corrected

14) Page 1320 Line 22 to 23: The problem with this assumption is discussed in my general comments

15) Page 1322 Line 17: Subterranean hotspots reference needs a citation. The word “subterranean” has been removed

16) Page 1322 Line 3: I would find another phrase to replace “spread rate” which foresters will insist refers to something other than what you are describing here (i.e., the rate at which the front moves in a line perpendicular to the front).

The term “fire spread rate” has been replaced to avoid confusion. When possible the shorter “fire spread” has been used.

17) Page 1325 Line 10: Your reference to a fire management scheme that is designed to “maintain the current fire regime in future decades” presumes a lot about fire management efforts and requires citations to support this description.

We agree with the reviewer that maintaining the present fire regime implies a number of assumptions about fire management strategies. We have modified the sentence to avoid assumptions about long-term goals of fire management efforts

18) Page 1325 Line 21 to 22: I’m unclear as to how this information would benefit fire management agencies, since it’s nearly impossible to assess which of these quiescent, smoldering fires represent a threat versus those that are likely to die out.

Our analyses indicate that a limited set of fires was responsible for a considerable share

of the burned area and that some of these fires evolved very slowly during the first days. This fact opens an opportunity for fire management. As challenging as this may be at the moment, as we improve our understanding of the factors behind fire spread and their parameterization we will reach a stage in which it will be possible to identify fires of high potential damage before they grow too large. As mentioned in the manuscript more information is needed to identify landscape characteristics that enable fires to grow to very large sizes but the combination of improved weather forecasts, land cover information and satellite data has the potential to, in a near future, provide information to better inform fire suppression activities.

19) Page 1327 Line 28: Strike ‘in’. Corrected

20) Page 1328 Line 24: Comma needs to follow ‘i.e.’ and replace ‘melt’ with ‘thaw’. Corrected

21) Page 1329 Line 7: Twenty out of how many? Twenty out of the 134 fires registered during 2004.

The number of the total number of fires has been included in the text together with the reference to the table with the annual number of fires.

22) Page 1329 Line 11: This isn’t actually possible given the remoteness of most fires characterized by slow initial growth; given how many of them do not become large fires. In fact, even those that do burn large areas are not a priority for suppression unless they threaten human settlements or infrastructure.

The original sentence in the manuscript could imply that some of the large fires could have been suppressed and the total burned area considerably reduced. This sentence has been re-written to make clear that rather to past wildfires we only refer to suppression opportunities in the future. We also understand that while today’s priorities limit fire suppression to situations in which human settlement and infrastructure are threated, these priorities could change in the future. As mentioned in the manuscript, the potential future implementation of international carbon tax mechanisms or other climate policies could justify the allocation of more resources for fire suppression in more remote areas.

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