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# *Interactive comment on* "Daily burned area and carbon emissions from boreal fires in Alaska" *by* S. Veraverbeke et al.

**Anonymous Referee #3**

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General comments The development of the Alaska Fire Emissions Database has value for advancing our understanding of the methods and datasets of value for these types of emissions products, despite being based on approaches and data sets previously used in other similar studies. The value of the work presented is in exploring in detail the various data layers available for emissions mapping in Alaska and testing their relevance for quantifying fire emissions in boreal Alaska. The overwhelming problem of the manuscript and outputs presented is that the work does not provide significant advances from current state of the art. The claim that the AKFED provides the first publically available data available on emissions for Alaska is not valid, because this has been accomplished in other studies (see next paragraph) and the work presented provides only an assessment in black spruce types. Furthermore, all of the input data used in this study has been developed and used in previous products with varying



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success. I would say that the value of the work presented in this manuscript is not in providing improved estimates of fire emissions for Alaska, but rather in detailing the data of value for emissions mapping and needs for improvement in several of these inputs. In my opinion, the manuscript needs to be re-focused to state its true value – in providing a comprehensive review of data and methods for Alaska fire emissions mapping and detailing the data inputs of value and their drawbacks. There is neither new data nor approaches presented in this work.

The use of field data to complement and assess modeled fuel consumption was completed by Kasischke and his colleagues in several studies (Kasischke and Hoy 2014, Genet et al. Barrett et al.). Exploration of dNBR for consumption in the context of fire emissions has been deeply explored. These past activities were duly noted by the authors, and relevant discussions included, and the critique by Kasischke in the discussion reviews the gaps related to these aspects of your study. However, the employment of FCCS and Consume to estimate state-wide fire emissions was not covered in Kasischke's review and not acknowledged in the manuscript, although these tools have been employed by French et al. (2014) and used in BlueSky emissions approach and products. The manuscript fails to recognize this emissions work, which is reviewed in several papers including French et al. 2011 (supplemental materials) and French et al. 2014:

French, N. H. F., D. McKenzie, T. Erickson, B. Koziol, M. Billmire, K. A. Endsley, N. K. Y. Scheinerman, L. Jenkins, M. E. Miller, R. Ottmar and S. Prichard (2014). Modeling regional-scale fire emissions with the Wildland Fire Emissions Information System. Earth Interactions 18: 1-26 DOI: 10.1175/EI-D-14-0002.1.

The French et al 2014 paper describes WFEIS, an online system that provides open access to tools for fire emissions modeling for Alaska and the Lower 48 states. WFEIS was developed by French's team and colleagues at the US Forest Service FERA lab where FCCS and Consume were developed. WFEIS operates as a kind of "spatial Consume", allowing users to choose data inputs and providing defaults for assessing



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daily emissions based on the weather conditions present on the day of burning. In addition, pre-calculated estimates of emissions by year are available on the WFEIS web site (an output produced for the NASA CMS program; see http://wfeis.mtri.org/examples – the AK results are on the ftp server – see link below CONUS table of results). The authors should consider citing French et al 2014 and the on-line products, since it provides the only nation-wide (with the exception of Hawaii) method of estimating emissions at moderate spatial resolutions (1 km grid scale), and can serve as an excellent comparison to AKFED results. Because WFEIS uses basic tools for estimations and does not directly employ field-derived information on fuel consumption, it is likely that the approach presented in this manuscript will provide improvements for Consume and the WFEIS approach.

One general aspect of the study that needs clarification is the fact that two models are developed – one for depth of burning in the organics and one for carbon consumption; this was not clear nor consistently presented throughout the manuscript. Terminology is not consistent (belowground consumption is used in the Results section while belowground carbon consumption is used elsewhere). The considerations for going from depth of burn to carbon consumed are not well described (the first paragraph of the methods section has the review of this). Please spend more effort explaining the way field data (what data?) is used to get to carbon stock from fuel loading or other metrics. On page 17587 line 5 the field data is reviewed. The first line should explicitly say what field data is used (e.g. “Field data on depth of burn, carbon content of the duff fuel layers, etc). Was the data fuel loadings or fuel consumption; how was it measured? It is my understanding that many methods were employed, and this should be noted. The issue comes into play when trying to understand the results section (Page 17595). Line 16 reports the relationship between depth of burn and variables R2 – 0.25, p,0.001), while line 23 reports belowground consumption (R2 = 0.05, p<0.05). What is the difference? Are these the same? Is this carbon? Eq. 2 tells me the relationships of both depth of burn and carbon consumed are one and the same. I think there needs to be more clarification of the difference between these two values. What conversions are



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used from depth of burn to carbon?

In a similar vein, there needs to be a bit more description of the field data collections. More on this, rather than having the reader rely on going to the sources of the data, would be helpful. I assume that the data are collected in one location (the lat/long provided), or are the data shown in Supp Table 2 averages for a distributed "site"? How large are sites? Do they represent the surrounding landscape so you can say that the point of collection or mean value of the site data represents the full area of a pixel? A one to two sentence comment on the field collections will go a long way to allowing the reader to understand the value of the field data. Field data collections for connecting to remote sensing metrics need to be collected in a way to represent the site that is sampled by the pixel. If this is the case, this should be stated, since these data were not necessarily collected for remote sensing validation purposes, it may be they do not represent a pixel. This needs to be understood.

The relationships found between depth of burn/carbon consumption and individual variables, as well as multiple variables, is very weak in most cases. Also, in previous studies many of the variables were seen to be uncorrelated to the phenomenon of interest. There is a concern that the weak results (R2 = 0.40 and 0.29) indicate that the variables used are not very helpful for quantifying emissions with any reliability. The ability to load many weakly related variables into a regression analysis – some of which have been shown to have no biophysical reason for providing predicative ability – does not mean the model has value. This needs to be strongly defended if the paper is to claim that emissions predictions are valuable for use by practitioners. The other reviewers have provided guidance in on this, so more is not required by me.

Finally, while "uncertainty" is presented, there is little acknowledgement that the field data used as the metric for developing the emissions model may not fully represent the situation. Field data also have error and uncertainty, and in relying on a regression-based assessment for uncertainty, the reliability of the dependent variable is not included. In this case, with the data collected by several teams and with varying methods



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(and some concern in the community that the data are biased due to the methodology), this factor should be discussed when reviewing the uncertainty of the results.

Overall, the manuscript is well written and reviews the data used for emissions modeling at regional scales in a comprehensive fashion. A refocus of the purposed of the exercise is required, since the estimation of emissions is derived exclusively from data and methods developed under previous research efforts rather than new work by the authors. The comparison work is the real value of this effort and should be highlighted.

Specific comments P17582, line 20: Work by Ottmar and others on Consume should also be cited. Se recent citation: Ottmar, R. D. (2014). Wildland fire emissions, carbon, and climate: Modeling fuel consumption. Forest Ecology and Management 317: 41-50 DOI: 10.1016/j.foreco.2013.06.010.

P17582 Line 24: ". . . is larger than . . ." – larger in what way? By mass, volume, other?

P17583, line 14: Include French et al. 2008 (included in your reference list) instead of Veraverbeke et al, 2010 because French et al 2008 was cited in this earlier work by the author and the French paper is in the context of boreal fire.

P17588, line 2. The term "time of burning" is used throughout the text, but is also (actually) "day of burning". This term needs to be consistent and to more accurately describe it, "day of burning" is preferred.

Page 17591, lines 6-10. The resolution of the MODIS data is confusing. As a remote sensing person I think I understand, but to non-RS readers, it is difficult to understand why the resolution is 500m on line 7 and 463 m on line 9. I think this needs to be revised. I also suggest you use the term "grid scale" rather than resolution. The resolution of a RS system is determined by the system, not the post-processing. In fact, resampling will not improve resolution if resampled to a finer scale, and resolution is determined by more than the pixel spacing.

Page 17592, line 10. The FCCS code 931 number is not relevant unless you want to



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include a review of the codes. I suggest not including it, but saying the 500 m of barren pixels is determined from the FCCS map – that is what you are saying, I think.

Page 17599, line 21. The statement that elevation was the most important variable to depth of burn is confusing to me. In the Results section it is said to be dNBR and Tree cover. Please explain. Also, Figure 4 gives no evidence that elevation is important on its own. Presentation of these results is confusing and should be clarified.

Page 17600, line 26. Change heading to "Date of burning"

Page 17606, lines 9-17. The inclusion of albedo in the discussion on future directions is extraneous to the topic of the manuscript. Albedo changes are relevant for climate forcing, but not to estimating emissions. A mention of albedo as relevant along with other impacts of fire is OK, but this text covers the topic too much and should be removed.

Figure 4: The colors of text is not needed – it confuses and is difficult to read. Also, where is the aboveground model results? If not included, there should be an explanation of why in the caption.

Supplementary Table 2 should provide the values of the data used from the "T" reference rather than relying on the reader to have to translate to consumption with the curve. In other words – provide the number used, not the topographic class.

Typing errors Page 17584, line 25: "and their influence" is repeated.

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Interactive comment on Biogeosciences Discuss., 11, 17579, 2014.