Geosci. Model Dev. Discuss., 8, C1007–C1023, 2015
www.geosci-model-dev-discuss.net/8/C1007/2015/

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Improving the representation of fire disturbance in dynamic vegetation models by assimilating satellite data" *by* E. P. Kantzas et al.

E. P. Kantzas et al.

e.kantzas@sheffield.ac.uk

Received and published: 8 June 2015

Reviewer #1:

General comments:

This study proposes an algorithm to assimilate the statistical information of fire provided by satellite-based burned area products to improve the representation of fire in the dynamic vegetation model. This is a very interesting topic considering that great importance of fire in affecting both carbon and water cycles in the boreal and arctic region (i.e. focus area of this study). I think the method is reasonable, though with potential limitations in representing extreme, large fires (which is likely becoming more common in the study area with climate warming). Also, in my view, the manuscript may be better organized and the presentation (and writing) could be improved. I recommend major revision.

Specific comments:

1. One potential limitation of the proposed algorithm is that extreme, large, and rare fires may not be well represented by the model, especially in areas with low FRI, e.g. tundra areas. This is because the algorithm tends to accommodate the large fires into the areas with shorter FRI values and thus with high accumulated burned areas. Continued strong warming in the boreal and arctic regions might promote the occurrence of those large fires, and this limitation should be addressed. Also, I think this might partly explain why the FRI map generated by the new algorithm shows such great variability.

Response: The algorithm does not distinguish between low and high FRI areas when assigning fires. If a low FRI area has accumulated the potential to accommodate a large fire it will occur there. The algorithm does indeed produce large fires in high FRI areas (Figure 4, middle panel) albeit with a low frequency. In the Discussion section we added "The long FRI in the Arctic means that the 12 years of data in the GFED4 daily product is insufficient for adequate sampling of the rarer large fires. This can distort the local occurrence statistics and give rise to spatial variability in the simulated FRI, even though at larger scales the CCL runs agree well with the FRI produced by the original model. The restricted number of larger fires means that the algorithm tries to accommodate the same large fires over regular time intervals, which slightly alters the FRI produced by the original model run. It seems likely that this spatial variability would be reduced in regions and sub-regions with shorter FRI where acquiring statistically representative data is less problematic. Available fire data would then offer a more representative picture of the local fire regime. Alternatively, in such regions, at each position an empirical distribution can be fitted to the histogram of fire sizes identified by CCL, and this probability density function could be used for sampling; this resolves problems associated with unoccupied bins in the histogram."

2. The organization and presentation of the paper might be improved. For example, why not put the results from applying CCL algorithm (i.e. section 2.1.2) in the Results section? Also, adding a section in the methods (e.g. before section 2.2) introducing how the LPJ-WM model represents the fire parametrization and the associated postfire evolution of vegetation and soil carbon (?). This background information will help clarify some of the details presented in the results and discussion, particularly regarding the post-fire recovery simulations.

Response: We modified the structure of the paper so results from applying CCL algorithm are now in the Results section. We also added a description of how LPJ-WM calculates burned area. We added "The LPJ-WM DVM used in this study calculates a daily fire probability for each grid cell as a function of temperature and litter moisture (Thonicke et al., 2001). The fire probability is then summed over the course of a year, from which the length of the fire season and fraction of area burned per grid cell is derived; the values of the latter populate the BA array."

3. Fig. 4: I do not understand why the fraction burned area maps of different years were compared. Why not pick one year of model simulations within the temporal period of the GFED product? Even though the algorithm may not simulate fires with locations exactly matching the GFED data (due to statistical nature of the algorithm), it provides a visual comparison of the distribution of fire sizes between the model and the satellite product.

Response: We had different years in the burned area maps because we wanted to show the location of the fire that is examined in the discussion section, which in the model runs occurred in 1910. We've altered the text so that now all sub-figures (model runs and GFED) depict fire in the same fire season year.

4. Fig. 5: It would be helpful to provide time series of the recovery trajectory of simulated vegetation biomass and carbon fluxes. This could help diagnose how well the model can represent the post-fire vegetation recovery since the literature provides

abundant information on the post-fire recovery process in the boreal forest. Besides, the authors also acknowledged that fire had a potential large impact on the soil heat transfer (e.g. affect the stability of permafrost) and soil carbon dynamics (e.g. promote the soil carbon decomposition in the permafrost areas), and these processes are critical in characterizing the boreal/arctic carbon cycle. Does the LPJ-WM model account for those processes? This has not been addressed in the paper at all.

Response: We modified the bigger part of the Discussion section where we talk about the issues raised here concerning post-fire dynamics. "Taking advantage of this new capability requires DVMs with sufficiently rich process representations; indeed, the lack of such a capability has meant there has been no motivation for the DVMs to embody the process coupling that is set in train when severe fires occur. This is particularly true as regards the connections between fire, land cover and permafrost. It is also the reason why our post-fire analysis was qualitative and restricted to examining whether a DVM has the capability to simulate the expected ecosystem response following a fire; without all the necessary linked processes in place, it is premature to attempt a quantitative comparison of carbon and water fluxes between field data and simulations. For example, even though LPJ-WM considers permafrost, the upper boundary value for soil heat transfer is the daily air temperature provided by the climatology driver. Hence, even after a large fire that removes most of the canopy, thermal conduction is unaffected, and no account is taken of heating of the soil by incoming radiation. These shortcomings can be alleviated in an ad hoc fashion by using an extinction equation parameterized by Leaf Area Index to characterize temperature during canopy recovery but, as shown by Kantzas et al. (2013), what is really required is a more sophisticated radiative/heat transfer process. The JULES model (Best et al., 2011), for example, does consider radiative transfer through the canopy and has a recently-added permafrost representation which considers the thermal properties of organic soils (Chadburn et al., 2015), but JULES does not contain a fire component. "

Minor comments:

GMDD

8, C1007–C1023, 2015

Interactive Comment

1. Page 2880, Line 3: please remove “they”.

Response: Removed

2. Page 2882, Line 25-: the GFED fire emissions data were never used nor addressed in the study. So I do not think it is necessary to include this information.

Response: We made the GFED fire emissions part shorter.

3. Section 2.1.1: Why aggregated the GFED data to 0.25° resolution since the DVM model (i.e. LPJ-WM) was run at 0.5° resolution?

Response: We did not aggregate the GFED data; this is the resolution of the GFED product, which is created from MODIS 500m images but offered in coarser resolution so modelers can assimilate it without resampling. We added a clarification in the text. “For the period used in this study, from the mid-2000s to the present day, the GFED-BA is derived daily from the MODIS MCD64A1 500 m burned area product (Roy et al., 2008), which is based on changes in reflectance in the visible channels of MODIS, but the GFED-BA also takes into account information on active fire counts (Giglio et al., 2009). It is not offered at the MODIS 500 m resolution but instead is aggregated to a resolution of 0.25o to facilitate interfacing the fire data to biochemical and atmospheric models which run at such resolutions (Castellanos et al., 2014; Kaiser et al., 2012; Valentini et al., 2014).”

4. Page 2884 Line 8: the category (6) should only include the fire size smaller than 500 km2, right? That means that category (5) has been excluded.

Response: Category (6) includes all fires, no exclusions. We altered the x-axis of Category (6) in Figure 2 to make it clearer.

5. Page 2886, #2: The study area (i.e. Canada and Russia) covers a large latitudinal zone. So would it be very reasonable to assume the area of each grid-cell (0.5°) as a constant?

GMDD

8, C1007–C1023, 2015

Response: We don't treat the area of each grid-cell (0.5°) as a constant; we calculate the grid-cell area according to latitude and then we derive the fraction of area burned.

Page 2887, #5: should μfire not be replaced by μffire? Otherwise, please define μfire.

Response: We added a couple of numerical examples to make clear what μfire is.

Page 2887, #6: Please clarify what BAC(lat, long,y) represents. My understanding is the accumulated array BAC(lat, long) is calculated through all the years during the transit run (i.e. the total 112 years) and fixed for each grid cell.

Response: We rephrased parts of this section and altered the names of variables in the definition of array BAC as it was misleading.

Page 2888, #7: I think the authors were trying to say "the deficits in BAC are : : :", not "the shortcomings in BAC..".

Response: Changed to deficits.

Reviewer #2

Improving the representation of fire disturbance in dynamic vegetation models by assimilating satellite data. The manuscript introduces a study in which low-resolution satellite burned area products are used to identify individual fire events using the connected component labeling method for the Artic region. The statistical distribution of these fire events is used to prescribe the fire area in a DVM, while the fire return interval parameterized in the DVM is maintained. The impact of the improved fire area representation is assessed in terms of post-fire evolution of land cover, biomass and nee. The study addresses an important topic of subgrid scale variability of fire dynamics that can not be resolved with coarse-scale resolution models. I'm not an expert in statistical methods applied in this study, but the methodology applied in this study sounds valid. However, more information is needed in parts of the methodology section to fully understand the procedure applied. My major concern with the current status of the manuscript is related to the effect of the model modifications on post-fire dynamics.

This is an important implication of the introduced study and forms the major motivation to introduce the CCL method into a DVM. Monitoring, the post-fire dynamics for one specific fire event, however, does not convincingly demonstrate the impact of the CCL method for ecosystems dynamics simulated in the DVM for the artic region. In general, the manuscript could be improved in parts from a restructuring and improved writing. I recommend major revision.

Specific comments:

- title should include information about the focus on the artic region and fire size

Response: Title now is: "Improving the Representation of Fire Disturbance in Dynamic Vegetation Models by Assimilating Satellite Data: a Case Study over the Arctic."

- a short description on the fire model used in LPJ-WM is needed

Response: We added "The LPJ-WM DVM used in this study calculates a daily fire probability for each grid cell as a function of temperature and litter moisture (Thonicke et al., 2001). The fire probability is then summed over the course of a year, from which the length of the fire season and fraction of area burned per grid cell is derived; the values of the latter populate the BA array."

- is there a reference for the CCL method you are using? This would be helpful for readers not familiar with this method such as myself.

Response: Added the reference (Gonzalez et al., 2003).

- The method to derive a forest mask for Canada is not clear to me. How is the forest fire mask morphologically closed? Would it make a big difference using the GlobCover2000 mask for Canada as well? This would make the description much simpler.

Response: Using the GlobCover2000 product to identify forest pixels in Canada as we did for Russia would indeed make the description simpler but land cover products often come with large uncertainties. The CLFD involves reports from crews on the ground that marked the locations of forest fire; these data are probably more accurate that an Earth Observational product. We added a clarification in that section. The way the forest mask is morphologically closed is also described there. We added “In order to evaluate the CCL algorithm against the CLFD, the 0.25° GFED4 grid-cells that contain forest in Canada must first be identified. Instead of utilizing a land cover product, which would add unnecessary uncertainty, we built the forest map by combining the CLFD and GFED4 data. To do this, we first applied the CCL algorithm to the GFED4 data and assigned the value 1 to a grid-cell if it also contained a fire record in the CLFD. Clearly this would omit forest grid-cells where the CLFD did record any fire over its 40-year period, so to generate a forest mask the set of identified pixels was morphologically closed. This assigns 1s to grid-cells in close proximity to or surrounded by grid-cells already assigned the value 1. All other pixels were considered as non-forest and assigned the value 0.”

- The fire size classes are assigned to the categories 2-10 km2, 10-30 km2, 100-500km2, >500 km2. With a GFEDv4 resolution of 0.25 degrees I do not understand how the CCL method can detect fires between 2-10kmˆ2 when applying it to GFEDv2 data. With a coarse resolution of 0.25 this should not be resolved? I probably miss something here.

Response: The GFED resolution is indeed 0.25âĄř but for each grid-cell the area burned is given in hectares. The product is originally obtained from MODIS 500m images so the lower limit of area burned is approximately 0.25 km2. The lower limit of CLFD is 2 km2, which defines the lower limit of burned area per grid cell in our approach.

- Section 2.1.2: It would be also interesting to analyse more in detail how different the CCL produced histograms are for Russia and Canada.

Response: We tried to expand on this and added “Even though the fire size sample distributions for Canada and Russia were similar, the statistical tests show that they

GMDD
8, C1007–C1023, 2015

did not originate from the same distribution. This may be associated with the known differences in intensity between Canadian and Russian forest fires (Harden et al., 2000; Wooster and Zhang, 2004). However, it could also be a sampling artefact arising from the small number of years for which there are data in GFED. For example, if the data available for Russia had covered more years with large fires, the distribution of fire sizes would be shifted to the right and would more closely match the distribution of the Canadian fires. Hence the lack of a database analogous to the CLFD prevents safe conclusions to be drawn regarding the validity of CCL results over this region."

- Section 2.2: o I do not understand how μfire is derived. I understand that this one averaged value created out of the CCL6 database. How can this value than range between 0.1% and a value three orders of magnitude larger?

Response: We rephrased this part to make it clearer.

o BAC is defined as the accumulated burned area over n years. On Page 2887/Line21 is used as a function of the year "y" and not as the number of years "n". Please clarify. I would assume that the BA of the year y would be the appropriate quantity.

Response: We homogenized the names of the variables so BAC is now in both cases a function of y.

o Page2888/Line7: Can you quantify how often (or how much burned area) is distributed randomly as no fit was found?

Response: It happens on average once every 2 years and the area is close to 50% of an average gridcell which is distributed to about 4000 gridcells so the impact is minimal. We added this information to the text. "The chance of finding a suitable location for a particular fire event decreases with increasing fire intensity and extent, and such a location may not exist. In the rare cases when this occurs the fire is forced to fit the location that came closest to accommodating the fire and the deficit in BAC is taken from other pixels to maintain the regional average of FRI. These cases occur on average once every 2 years and the BAC deficit is approximately 50% of a grid-cell."

- Section 3/ Results: o Page 2889/Figure4: The comparison of the CCL method/LPJ-WM and GFEDv4 for only one year seems quite arbitrary given the strong interannual variability. Isn't there another way to compare the data including more information (years).

Response: In Figure 4 we want to show how fire representation significantly improves with the implementation of the CCL approach and so we arbitrary picked a year; any other year would have conveyed the same point. The strong inter-annual variability observed in the Arctic would change for each year the total area burned, which is obtained from the original run of the model, and fire locations. As the 1st reviewer notes in question 3, we are not comparing fire locations but fire size distributions; the fact that originally the model only produces small fires but now follows the fire size distribution as obtained from GFED4. We made significant changes in the Discussion section to explain what the assimilation does and doesn't. "Burned area data from GFED4 over the Arctic reveals that in a given year fires tend to cluster spatially (Fig.4, bottom), presumably because of fuel availability and conditions that locally favour fire ignition and propagation, such as high temperatures and winds, low precipitation and abundance of natural or anthropogenic ignition sources. Such local weather conditions in turn give rise to inter-annual variability (IAV) in burned area over larger regions, e.g. the increase observed in North America during El Nino years (van der Werf et al., 2004). In contrast, the fires simulated by assimilating the CCL algorithm in LPJ-WM (Fig. 4, centre), even though they have the correct size distribution, do not appear in clusters and give a smaller IAV in burned area than GFED4 data. This is because the assimilation preserves the original annual burned area produced by the model; since the area burned in LPJ-WM is nearly the same area every year, the IAV for a region is therefore forced to be small. The reason why simulated fires do not appear in clusters is because the location of each one is decided during assimilation based on random allocation of its point of ignition and the FRI of the region; even though

GMDD

8, C1007–C1023, 2015

the FRI produced by a DVM for each grid-cell depends on local climate conditions, it does so on long time scales and is relatively insensitive to inter-annual variations which, for example, could cause multiple fires to ignite in close proximity. Refining the algorithm so that it simulates fire activity in accordance with the IAV and clustering exhibited by GFED4 is a daunting task, especially as lightning, which is not considered in most DVMs, is the main ignition source at these latitudes (Stocks et al., 2002) and is projected to increase in frequency (Romps et al., 2014). Furthermore, even though it is desirable, it is not necessary for a DVM to capture the IAV and spatial variability in annual fire locations in order to make medium to long term predictions on the effects of fire activity on net carbon and water fluxes. As long as the FRI produced by the DVM has the correct magnitude and captures the trend in fire activity in accordance with climate change, and fire size is linked to a complete suite of post-fire processes, then the model is capable of accounting for the effects of fire activity on an ecosystem."

o Page 2890/Line8: The implications discussed for post-fire dynamics are interesting, but not specifically related to this particular study. Monitoring the DVM after a big fire disturbance can be done independently of the CCL method applied in this study and are not a unique feature of this study. More interesting would be to study the impact of the CCL-method integrated over the Artic region. How does a more realistic fire-size distribution impact ecosystem exchange integrated over the region compared to the standard LPJ-WM treatment, while the FRI interval is kept similar?

Response: Regarding our approach of post-fire dynamics we made significant changes both in the Results and the Discussion section. We added "Taking advantage of this new capability requires DVMs with sufficiently rich process representations; indeed, the lack of such a capability has meant there has been no motivation for the DVMs to embody the process coupling that is set in train when severe fires occur. This is particularly true as regards the connections between fire, land cover and permafrost. It is also the reason why our post-fire analysis was qualitative and restricted to examining whether a DVM has the capability to simulate the expected ecosystem response

GMDD
8, C1007–C1023, 2015

following a fire; without all the necessary linked processes in place, it is premature to attempt a quantitative comparison of carbon and water fluxes between field data and simulations. For example, even though LPJ-WM considers permafrost, the upper boundary value for soil heat transfer is the daily air temperature provided by the climatology driver. Hence, even after a large fire that removes most of the canopy, thermal conduction is unaffected, and no account is taken of heating of the soil by incoming radiation. These shortcomings can be alleviated in an ad hoc fashion by using an extinction equation parameterized by Leaf Area Index to characterize temperature during canopy recovery but, as shown by Kantzas et al. (2013), what is really required is a more sophisticated radiative/heat transfer process. The JULES model (Best et al., 2011), for example, does consider radiative transfer through the canopy and has a recently-added permafrost representation which considers the thermal properties of organic soils (Chadburn et al., 2015), but JULES does not contain a fire component."

o As the CCL method is based on accumulated data and does not account for the actually annual fire activity, i.e. fire history, I was wondering how realistic the post-fire behavior is actually captured. With the CCL method a large fire can be followed by a large fire, whereas in reality the fuel availability will be limiting fire occurrence.

Response: After a big fire, the fire potential for the grid cell will be "consumed" by the algorithm. In order for the grid cell to accommodate another large fire, several years must pass until its fire potential has increased; therefore it is not possible to accommodate a large fire in the same region within a short time span. We added in the Post-Fire Dynamics section "Additionally, since the algorithm accumulates grid-cell fire potential and then consumes it when one or more grid-cells are chosen to accommodate a fire, regions are prevented from unrealistic behavior in which big fires are separated by only a short time span. The long FRI in the Arctic means that several decades need to pass after a big disturbance before a grid cell has accumulated enough fire potential in order to experience another fire."

Section4/Discussion o Page 2891/Line7: The motivation to keep the simulated FRI

GMDD
8, C1007–C1023, 2015

Interactive Comment

unchanged is not clear to me. If the FRI could be improved based on observational data, why isn't this a desirable thing to do?

Response: We explain this in the introduction, as follows: "...Hence there are pressing reasons to improve the fire representation in the DVMs, but these models are complex, involve highly coupled internal processes, operate on a grid-cell basis, and are often embedded in climate models. In addition, significant resources have been spent to calibrate fire processes so that the FRI compares well (in some cases) with data (Prentice et al., 2011; Thonicke et al., 2010). Hence it is desirable to keep model restructuring to a minimum and preserve its estimate of FRI, while ensuring that fire characteristics, such as structure and size distribution, are consistent with observational data."

o Page 2891/Line28: Do you mean higher temporal resolution?

Response: Yes, we added a clarification.

o The discussion of the limitations of LPJ-WM with a respect to soil-heat transfer seems a bit out of context as they are not particularly related to the implementation of the CCL method. This could be better structured.

Response: Added text to this effect.

o I'm missing a short discussion on how the CCL method can be applied to model future fire projections and how applicable the method would be for other regions.

Response: In the Discussion we mention how the CCL approach would benefit when applied in other regions with lower FRI; there are no geographical limitations to the algorithm. We also rephrased the last paragraph in the Discussion section to consider the effects of a changing climate. We added "Despite the limitations described above, the assimilation methodology described here gives DVMs hitherto unavailable capabilities to study post-fire behaviour under the large climatic changes projected to occur in the Arctic. As long as a DVM has the necessary processes to simulate post-fire dynamics (e.g. canopy radiative transfer, vegetation succession, permafrost-related processes and parameterization) and is correctly calibrated against field data, model runs driven by climate scenarios can now offer insights into the role of fire by answering questions such as: (1) Will permafrost recover after a big fire when the atmospheric temperature is rising, especially in regions where it is discontinuous, and what will be the effect of the projected increase in precipitation? (2) How will post-fire vegetation succession be affected at ecosystem boundaries under the greening effect in the Arctic? (3) How will evapotranspiration be affected under increases in fire activity and precipitation? (4) How will the magnitude of fire emissions vary over sub-regions, and can changes in fire activity change the sign of the land-atmosphere net carbon exchange?"

Minor comments:

Page2879/Line7: "The unprecedented : : :" this sentence is out of context in this paragraph.

Response: Moved to the paragraph below.

Page2880/Line3: they

Response: Corrected

Page 2881/Line19: are given the value ? (1?).

Response: There was a discrepancy at this point between the document we provided and the one published. That sentence should read "For each image, pixels identified as burned are assigned the value 1 and the rest are given the value 0.". It has been corrected

Page 2881/Line 20: "We apply the CCL to this dataset." Which dataset?

Response: Clarified.

Page 2882/Line2: "e.g. total area burned" are there also other properties you analyse? Please, specify.

GMDD
8, C1007–C1023, 2015

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Response: Specified that it is only burned area

Figure2: The caption should mention that the CFDL data is identical for Canada and Russia. Also MMW and KS need to be explained and your nomenclature for passed and failed.

Response: The caption did mention it but we now also added it to the legend of Figure 2 to make it more visible. We also added an explanation in the caption for MMW and KS.

Page2886/Line20: the explanation for int_f could be shortened.

Response: Done

Page2890/Line9: The post-fire topic should have its own section.

Response: Post-fire has now its own section now in Results.

Page2891/Line1: quantitative

Response: Removed

References

Best, M. J., Pryor, M., Clark, D. B., Rooney, G. G., Essery, R. L. H., Menard, C. B., Edwards, J. M., Hendry, M. A., Porson, A., Gedney, N., Mercado, L. M., Sitch, S., Blyth, E., Boucher, O., Cox, P. M., Grimmond, C. S. B., and Harding, R. J.: The Joint UK Land Environment Simulator (JULES), model description - Part 1: Energy and water fluxes, Geosci Model Dev, 4, 677-699, 2011.

Castellanos, P., Boersma, K. F., and van der Werf, G. R.: Satellite observations indicate substantial spatiotemporal variability in biomass burning NOx emission factors for South America, Atmos Chem Phys, 14, 3929-3943, 2014.

Chadburn, S., Burke, E., Essery, R., Boike, J., Langer, M., Heikenfeld, M., Cox, P., and Friedlingstein, P.: An improved representation of physical permafrost dynamics in the

GMDD

8, C1007–C1023, 2015

JULES land-surface model, Geosci. Model Dev., 8, 1493-1508, 2015.

Giglio, L., Loboda, T., Roy, D. P., Quayle, B., and Justice, C. O.: An active-fire based burned area mapping algorithm for the MODIS sensor, Remote Sens Environ, 113, 408-420, 2009.

Gonzalez, R. C., Woods, R. E., and Eddins, S. L.: Digital Image Processing Using MATLAB, Prentice-Hall, Inc., 2003.

Kaiser, J. W., Heil, A., Andreae, M. O., Benedetti, A., Chubarova, N., Jones, L., Morcrette, J. J., Razinger, M., Schultz, M. G., Suttie, M., and van der Werf, G. R.: Biomass burning emissions estimated with a global fire assimilation system based on observed fire radiative power, Biogeosciences, 9, 527-554, 2012.

Kantzas, E., Lomas, M., and Quegan, S.: Fire at high latitudes: Data-model comparisons and their consequences, Global Biogeochem Cy, 27, 677-691, 2013.

Prentice, I. C., Kelley, D. I., Foster, P. N., Friedlingstein, P., Harrison, S. P., and Bartlein, P. J.: Modeling fire and the terrestrial carbon balance, Global Biogeochem Cy, 25, 2011.

Romps, D. M., Seeley, J. T., Vollaro, D., and Molinari, J.: Projected increase in lightning strikes in the United States due to global warming, Science, 346, 851-854, 2014.

Roy, D. P., Boschetti, L., Justice, C. O., and Ju, J.: The collection 5 MODIS burned area product - Global evaluation by comparison with the MODIS active fire product, Remote Sens Environ, 112, 3690-3707, 2008.

Stocks, B. J., Mason, J. A., Todd, J. B., Bosch, E. M., Wotton, B. M., Amiro, B. D., Flannigan, M. D., Hirsch, K. G., Logan, K. A., Martell, D. L., and Skinner, W. R.: Large forest fires in Canada, 1959-1997, J Geophys Res-Atmos, 108, 2002.

Thonicke, K., Spessa, A., Prentice, I. C., Harrison, S. P., Dong, L., and Carmona-Moreno, C.: The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model, Biogeosciences, 7, 1991-2011, 2010.

Thonicke, K., Venevsky, S., Sitch, S., and Cramer, W.: The role of fire disturbance for global vegetation dynamics: coupling fire into a Dynamic Global Vegetation Model, Global Ecol Biogeogr, 10, 661-677, 2001.

Valentini, R., Arneth, A., Bombelli, A., Castaldi, S., Gatti, R. C., Chevallier, F., Ciais, P., Grieco, E., Hartmann, J., Henry, M., Houghton, R. A., Jung, M., Kutsch, W. L., Malhi, Y., Mayorga, E., Merbold, L., Murray-Tortarolo, G., Papale, D., Peylin, P., Poulter, B., Raymond, P. A., Santini, M., Sitch, S., Laurin, G. V., van der Werf, G. R., Williams, C. A., and Scholes, R. J.: A full greenhouse gases budget of Africa: synthesis, uncertainties, and vulnerabilities, Biogeosciences, 11, 381-407, 2014.

van der Werf, G. R., Randerson, J. T., Collatz, G. J., Giglio, L., Kasibhatla, P. S., Arellano, A. F., Olsen, S. C., and Kasischke, E. S.: Continental-Scale Partitioning of Fire Emissions During the 1997 to 2001 El Niño/La Niña Period, Science, 303, 73-76, 2004.

Interactive comment on Geosci. Model Dev. Discuss., 8, 2875, 2015.