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

Anonymous Referee #2

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Review:

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.

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Interactive Discussion
Discussion Paper

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

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

- 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.

- 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.

- 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 2-10km*2 when applying it to GFEDv2 data. With a coarse resolution of 0.25 this should not be resolved? I probably miss something here.

- Section 2.1.2: It would be also interesting to analyse more in detail how different the

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CCL produced histograms are for Russia and Canada.

- Section 2.2:

o I do not understand how muffire 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?

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.

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

- 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).

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?

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.

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- Section4/Discussion

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

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

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.

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.

Minor comments:

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

Page2880/Line3: they

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

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

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

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.

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

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

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Page2891/Line1: quantitative

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