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Interactive comment on "Understanding the effect of fire on vegetation composition and gross primary production in a semi-arid shrubland ecosystem using the Ecosystem Demography (EDv2.2) model" *by* Karun Pandit et al.

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Response to interactive comment on "Understanding the effect of fire on vegetation composition and gross primary production in a semi-arid shrubland ecosystem using the Ecosystem Demography (EDv2.2) model" by Karun Pandit et al.

Reviewer 2

General Comments: In this study Pandit et al. aim to understand the effect of fire on vegetation composition and primary production in sagebrush semi-arid

ecosystem using a newly developed shrub implementation (Pandit et al., 2019) embedded within EDv2.2. I commend the authors for their addition of a shrub PFT into a DGVM and their work towards better representation of vegetation dynamics in semi-arid systems. The aims of the study were:

Aim 1: understand the effect of fire on vegetation composition.

Aim 2: understand the effect of fire on primary production.

I have a number of major concerns with respect to this submission. (1) as reviewer 1 pointed out, simulations run to examine how fire affects modelled GPP and compare this with satellite derived NDVI lack a "fire-off" control which uses the same initialisation random seeds, therefore the presented results cannot at this point be attributed to fire effects. These effects could also be due to climate forcing. This lack of control greatly reduces the ability to associate modelled changes in GPP with fire and thus many of the stated results. (2) There is a lack of formal statistical testing on the effect of fire on modelled GPP and fire on NDVI values resulting in a heavy reliance on apparent visual changes being taken as results. I find it necessary that the authors carry out proper significance testing, such testing will greatly improve the manuscript quality.

While the study does attempt to address relevant aims I do not believe they have reached them. There are no concrete conclusions reached in the abstract or discussion which would contribute to understanding the effects of fire on vegetation composition or productivity in semi-arid shrubland systems. Overall this manuscript seems to be more like a model development study than a biogeosciences study.

Thank you for the comments. As per your suggestion and reviewer 1's suggestion, we will run a control simulation (no fire scenario). This will provide a two-way comparison; (i) between control and fire scenario from the model output, and (ii) between fire simulation from the model and satellite observation (MODIS GPP). As per your suggestion, we will apply a simple t-test to compare burnt and unburnt areas between modelled GPP and MODIS GPP.

Please see our note above to Reviewer 1 about more clearly stating our research questions and subsequent sections for clarity.

Specific Comments:

The shrub implementation used by Pandit et al. has already been published in geoscientific model development in 2019, as such I have not gone into detail on the validity of this implementation. Given that the stated aims of the study are to investigate fire effects I found that the lack of proper description of fire in the model greatly impeded my ability to assess the results. Fire apparently affects mortality which is influenced by height (line 69) and on line 124 the two fire severity parameter values used are presented. I am clueless as to how this all works, how fire is distributed across patches, how the shrub implementation influences the probability of mortality, how grasses are treated with respect to fire mortality, and what is fueling fire. I have no idea what the red line in Fig. 3 (disturbance rate from fire) is showing me.

As per your comment and reviewer 1's comment we will add a section under methods to elaborate on the model itself and the fire module. We will describe how the fire generates, burns and expands in spatial and temporal terms. In addition, we will include a description of the important parameters that would be influential in causing severe damage and potential recovery for shrub and grasses. The red line in the Fig 3 represents the amount of damage (proportion of grids burnt every year) resulting from fire. It is defined by the available fuel and user selected fire intensity parameter. Available fuel includes all aboveground biomass including grass biomass.

The bulk of new methods presented appear to have already passed peer review and are presumably valid. Fig. 1 is almost identical to Fig. 1 in Pandit et al. (2019), Table 2 appears to be identical, and large sections of text are very sim-

ilar to the 2019 paper which is fine for a methods section. Thank you for your comments. However, in our previous paper (Pandit et al., 2019), from the same study area, we used only two EC tower sites to validate our model. Moreover, in this study, we are only focused on exploring the effect of fire on vegetation dynamics, and we modeled a longer time span. With regard to modelled GPP, GPP appears to be about 50% too low (Fig. 4) apart from at one site, this large discrepancy makes me question whether the approach used is appropriate to understand the effect of fire on GPP. Perhaps I have missed it but the authors only appear to mention this apparent large underestimation on lines 165 and 251 with no further discussion. Please put numbers to this, e.g. GPP at RMS with low fire severity is 50% lower than the observed mean for the 2015-2017 time period. Also the authors should explain why they think the model can appropriately investigate the effect of fire on modelled GPP in spite of these generally rather large underestimations at the plot level.

We will discuss this issue further. Our primary objective in this study was to understand the effect of fire on vegetation recovery/composition and on primary production . Nevertheless, we will provide justification for such a low model outputs compared with the observation. We will provide numerical comparisons as suggested. We performed our model validation for shrub parameters in our previous study (Pandit et al., 2019), where we benchmarked our model using only two EC tower points (LS and WBS sites), which are at the lower elevation. Results for WBS site is good, and LS is also not very far off. However, the sites RMS and US which were not benchmarked are far off the observation. In our another study which is in review (Dashti et al, in review), we found elevation to be a major factor behind poor model performance for the other sites. Our primary focus in this work was towards understanding the effect of fire by exploring the fire module in the EDv2.2 model by running simulation for different alternate fire scenarios. Our assumption here was we could infer such comparisons using a fairly adapted EDv2.2 model for shrubland based on our previous study.

A major concern with regard to the simulations run to produce Fig. 5, as reviewer 1 pointed out, there is no control simulation run for this area with fire turned off which uses the same initialisation random seeds, therefore the presented results cannot be attributed to fire effects. This lack of control precludes associating modelled changes in GPP with fire and thus many of the stated results, e.g. lines 170-174.

As stated above we will perform a no-fire simulation. We hope this will provide better comparison as suggested.

It is puzzling why the authors chose to compare modelled GPP with NDVI. A much better comparison would have been to compare modelled GPP with satellite derived GPP. Indeed, some of the r-squared values from the supplement are very low (RËĘ2=0.044944, 2015 unburnt). I am not an expert in satellite derived products but MODIS products appear to be available at the same resolution as simulation runs for the time period. If these data are available simulated GPP should be compared to satellite derived GPP and a control "no-fire" run included.

Thank you for your suggestion. In our revised manuscript, we will compare our model outputs with MODIS GPP information as they are readily available and will make our comparison more justified.

Overall, a great deal of work needs to be done by the authors in order to allow proper assessment of whether the results are sufficient to support the interpretations. Given the shown response, or lack thereof, of GPP to fire at the plot level (Fig. 3) and the above mentioned lack of control I remain to be convinced that the changes in GPP presented in Fig. 5 are the result of fire. The lower panel plots in Fig. 5 do not show any clear difference between GPP change in fire vs non-fire areas. In general I would suggest the use of statistical methods to test whether there is a statistically significant difference in GPP between fire

and non-fire sites, this would remove the need for eyeballing the results and the need for words such as "suggests" (L172), "hint" (L172), "resembled" (L175), "subtle" (L180). Statistical methods should also be applied to the NDVI changes (NDVI change fire vs no-fire areas) as well as the comparison of GPP change and NDVI change (%change GPP no fire vs %change NDIV no fire) (%change GPP fire areas vs %change NDIV fire areas). I see no signal in the NDVI values which would delineate fire vs no fire areas but proper method can resolve that. Adding a similar satellite derived GPP comparison to modelled GPP, using appropriate statistical methods, would greatly help the authors better make their case.

We observed considerable effects of fire at the plot level as seen in Figure 3. As stated above, we will provide more information on the fire module. We applied average annual meteorological data to remove interannual climate variability, which would otherwise be a major driving factor in GPP simulation. As we kept every other thing constant, and only changed fire parameters, we state that the results in our point simulation are from the fire.

We used similar parameterization, as with point simulation, to run the spatial simulation (Figure 5). However, we used actual annual meteorological data that would allow us to compare with respective years of satellite derived data. We agree that there was little, if any, evidence of fire damage in the NDVI maps. We will explain further in our discussion section the possible conditions that may lead to such situation, including rapid recovery of vegetation (by annual or perennial herbs) as suggested by previous few studies.

As stated above, we will also run a control simulation with no-fire scenario for us to observe and compare between fire and no-fire conditions. In addition, we will apply statistical methods (t-test) to compare model outputs with the satellite observation. As stated above, we will use MODIS GPP instead of Landsat NDVI.

Minor comments:

L13 + L148 – how do you explain shrub dominance and lack of conifer growth in the absence of fire, shouldn't there be conifer growth in the area which would potentially replace shrubs?

We did not include conifer growth in this study since many of these locations do not have conifers. Moreover, future studies could improve conifer PFTs for local conditions to include in the simulations. We will expand on this in the discussion section.

L15 GPP already written out on L10

Thank you. We will correct it.

L21: how are you investigating spatial dynamics? Can fire spread between grid-cells? Perhaps make it more clear what you mean by "spatial behaviour of post-fire ecosystem restoration".

We will rephrase the sentence to make it more clear. In this model, although the fire ignition is local it can spread into adjacent grids given favorable conditions such as fuel availability and moisture content. This behavior in interaction with other factors like climate and topography would influence post-fire ecosystem restoration.

L34: citep(Bradley 2018)

We will correct it.

L69: a much better description of fire is needed as commented above.

As stated above, we will elaborate further on fire module in the EDv2.2 model.

L99: backslash âĂŤ (/textitPoa secunda).

Thank you. We will correct it.

L112: table 2. It looks identical to Pandit et al., (2019), not adapted. Perhaps I'm mistaken.

We made minor changes in this table and will make more clear.

L147: Off by a decimal place? âĂŤ 5.0-5.5 kgCm−2yr−1

We will round it to about 5 kgCm-2yr-1.

L153: it's not clear to me how this fire disturbance works or what the red line is showing. I dont see disturbance following GPP that closely. Why is disturbance highest when shrub GPP is highest rather than when grass GPP is highest? What is fueling the fire? Grass should add a great deal of fuel to the fire yet disturbance is highest when shrub GPP is highest. How often are fires happening?

We agree that grass may lead to more fuel continuity and hence more frequent and larger fires. However, the fire return intervals may not perfectly align with the most fire-prone fuel conditions. In our model simulations, the disturbance is mainly related to aboveground biomass, so it tends to follow GPP. It appears that under the current model parameterization and structural composition, the shrub GPP has more impact on fuel availability compared to grass, because of the woody nature of the PFT compared to grass and higher biomass storage rates. It looks like the mean fire return interval is somewhat close in length and aligns with peak biomass. Future work on PFT parameterization and fire module would improve the results.

Fire here is an ongoing process after the 25th year. So, the fire related damage will increase when there is available fuel and it will reduce when there is no fuel (aboveground biomass). If we compare this trend as a fire return interval, we can compare this with studies showing fire return periods ranging from 35 years to 435 years for different type of sagebrush ecosystems.

L158: At LS, why does high fire severity lead to a more stable shrub proportion of GPP? L162: How do you define stability?

One reason may be that due to high fire damage, shrub has yet to recover before additional fire damage. We defined stability as a long-term maintenance of GPP level for shrub and grass rather than short fluctuation. We will further explore the literature and revise the sentences to explain this phenomenon.

L170: the GPP change 1 year after fire looks to be about the same for the entire study area. why would the biggest change in GPP come two to three years after fire? It's hard to tell whether the changes in GPP are the result of fire or climate.

As described above, disturbance due to fire in the model behaves as a continued process instead of one-time effect. So, there could be spatial growth in fire from one grid to another depending upon fuel and moisture condition. A grid cell not meeting a threshold to get burnt could be burnt next year with slight increase in biomass. In addition, we should definitely take into consideration the effect of climate into these damages. A comparison between fire and no fire scenario, as stated above, may elucidate more details related to this.

Table 3: what are the * behind every Pearson number supposed to indicate? They mean significant.

L205: Cite the literature you are referring to.

Thank you.

L212: Cite the literature you are referring to.

Thank you.

L246: "larger contributor to GPP in this ecosystem" citation needed.

Thank you.

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