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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.

**Anonymous Referee #2**

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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.

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 fuelling fire. I have no idea what the red line in Fig. 3 (disturbance rate from fire) is showing me.

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 similar to the 2019 paper which is fine for a methods section.

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.

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.

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 (Rˆ2=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.

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.

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?

L15 GPP already written out on L10

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".

L34: citep(Bradley 2018)

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

L99: backslash — (/textitPoa secunda).

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

L147: Off by a decimal place? — 5.0-5.5 $kgCm^{-2}yr^{-1}$

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?

L158: At LS, why does high fire severity lead to a more stable shrub proportion of GPP?

L162: How do you define stability?

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.

Table 3: what are the * behind every Pearson number supposed to indicate?

L205: Cite the literature you are referring to.

L212: Cite the literature you are referring to.

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

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