January 4, 2021

Dr. Kirsten Thonicke
Associate Editor
Biogeosciences

Dear Dr. Thonicke,

Please accept our revised manuscript *"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"* for consideration for publication in Biogeosciences.

In this revised manuscript, we have addressed your comments. We have uploaded a response to comments, track-changes version of the manuscript and a clean version of the manuscript. Please let us know if you have any further comments or instructions regarding the manuscript.

Thank you.

Sincerely,
Karun Pandit
Corresponding author
Postdoctoral researcher
University of Florida
karunpandit@gmail.com
315 708 3901

Response to Associate Editor's Comments

Dear Dr. Thonicke,
Thank you so much for your time and efforts in providing these valuable comments which have helped us to improve this manuscript. We have tried to address your comments point by point carefully. Please let us know if we misunderstood any of your comments or instructions.

Our responses are provided in blue fonts. Line numbers we have referred in the response correspond to tracked changed version of the revised manuscript.

Associate Editor Decision: Reconsider after major revisions (09 Nov 2020) by Kirsten Thonicke

Comments to the Author:

Dear Dr. Pandit,
thank you very much for your revisions and detailed responses. Your manuscript has advanced but still requires further improvements or explanations.

One major issue that I still have is that you write in line 285 that the simulated pattern might be influenced by the model not being in equilibrium. I think this is a crucial point. If the simulated fire and GPP pattern that you analyse in your manuscript are additionally influenced by the model getting into equilibrium then it is unclear if the pattern you describe are solely driven by climate and you can draw the conclusion as you did. I apologize for not spotting this point earlier, but I regard this as a crucial point which I ask you to cross-check in your modelling protocol, if your model results are in equilibrium with climate and you then only simulate the influence of transient climate on fire and GPP. Because this point might require more time to revise and adjust the results shown in the manuscript, I will reconsider your manuscript after major revisions (it is more work than minor revisions).

Thank you for your important comment.

The mean annual GPP for the entire study area is in equilibrium after ~ 20 years of model runs (see figure below). After 25th year (in 2015) of near-bare-earth simulation, we ran the model further with fire experiments. The figure below shows mean annual GPP beginning first year after simulation to few years beyond 2015 with no fire scenario for the entire study area.

We understand the number of years applied for spin-up simulation is limited compared to the number of years commonly suggested for DGVMs to reach equilibrium. We should note that drylands are generally low productive ecosystems, and it is expected that productivity measures such as GPP reach equilibrium in a relatively short time. In previous studies using the Ecosystem Demography (ED) model, even in more productive ecosystems such as forests, productivity variables reached equilibrium rather quickly with similar vegetation *initialization (0.1 plants/m^2) as ours. For example, Hurtt et al (2004) has shown that LAI reached its peak at around 20 years of model simulation whereas Moorcroft et al., 2001 suggested that the biomass growth rate drops sharply after about 30-50 years of simulation.*

We acknowledge that 20 years of spin up may not be enough for some slow processes such as soil carbon pools which might require hundreds to thousands of years to reach an equilibrium. Indeed, over hundreds of years, fire ultimately changes the soil carbon pool and hence GPP through different processes such as a change in biome distribution, microbial composition, lateral carbon transport, etc. (e.g. Calvo et al, 2015; Chapin III et al, 2009). Such multi-century analyses are out of the scope of our current study as we focussed on multi-year and multi-decadal relationships between fire and GPP. We made the assumption that the effect of slow processes that affect soil carbon pools on the GPP-fire relationship can be ignored. Following this consideration on equilibrium, we removed the sentence in line 266 "… the simulated pattern might be influenced by the model not being in equilibrium."

We also added the following text in the quotation mark at L 173-176 and Figure 1 below in the supplement.

"To perform these simulations, we initialized EDv2.2 with a near-bare-earth scenario of 0.1 plants m^2 for all allowed PFTs (i.e. C_3 grass, shrub, northern pines and late conifers) from 1990 and ran it for the following 25 years. Our analysis indicated that 25 years of spin-up was sufficient for GPP to reach equilibrium."

Figure 1. Mean annual gross primary productivity (GPP) averaged over the region from 1991 (first year after simulation) to 2022 based on spin-up from near-bare-earth (0.1 plants/m^2) vegetation initialization

In addition to this point I have the following points which I ask you to implement in the revised version:

Abstract/ Introduction
Line 4: correct to Dynamic Global Vegetation Model to come up with the abbreviation DGVM.

Thank you for pointing this out, this has been corrected for DGVM.

Line 11: Why EDv2.2 is described as "dynamic vegetation model" not DGVM? If only "dynamic vegetation model" is meant, then correct the justification for a DGVM in the sentences before and justify dynamic vegetation models instead. This also applies to your respective paragraphs in the introduction (lines 48-69).

Thank you, corrected here and checked throughout for consistency.

Methods
Line 89: delete "about the model"

Removed

Line 111-112: add meaning of μ and $\mu 0$ for λ, correct to "relative area"

We added the meaning of above terms in the equation.

Line 115: decription and explanation of terms in equation incomplete and unclear, please correct. Explain in a separate sentence what the basic idea of equ. 2 is. Why does soil depth play a role in fire ignition? This is totally unclear. Even if it has been described in a previous paper, this is an important feature of the model that the reader of this manuscript needs to fully understand.

Thank you. We have elaborated on equation 2 and the role of soil depth in calculating soil dryness.

Study area
Use "RCEW watershed" throughout the manuscript when you refer to the regional simulations, and "soda fire" when you refer to the Soda fire regional simulations. This way you make the use of these terms consistent with what is shown in Fig. 1.

We clarified this in Section 2.5 (as Soda Fire scenario and RCEW scenario), and then referred to these scenarios in the subheadings of 3.2.1 and 3.2.2, as well as in Figures 4 and 5 captions.

Caption Fig. 1: Add the information that the grey area in the large map marks the extent of the Great Basin.

Added.

Line 125: define here, clearly in the text, that you have another definition of your study area to run your regional simulations.

We clarified this in the Study Area section, referring to Figure 1b to demonstrate the study areas of the regional simulations.

Line 205: replace "making comparisons with mean monthly GPP of July from the model." with "comparing them against simulated mean monthly GPP values of July."

Corrected.

Methods and results:
1) replace "long-term" with decadal or "mult-year". "Long-term" is unspecific and people have different understanding what "long-term" stands for. Use "seasonal" instead of "short-term", again this is a unspecific description of time scale.

Thank you for the suggestion. We replaced long-term with multi-decadal and short-term with multi-year.

2) Use subscripts of C3 and CO2 throughout the manuscript consistently.

Corrected to C_3 throughout the manuscript.

Results
Line 241: it is "underestimated" not "underpredicted"

Corrected.

Fig. 2: the fire model simulates fire fractions every year. Is this realistic (how does it compare to observed data) and how can it be explained by the design of the fire model (main assumptions, refer to equ. 1 and 2).

When the fire sub-module in EDv2.2 is turned on it leads to disturbances in a continued manner (at annual steps) depending upon the availability of fuel (aboveground biomass) and soil dryness as given in Eq. 1 and Eq.2. This model assumption is useful in performing multi-decadal or multi-century analyses for ecosystems which are frequently affected with fire, such as dryland ecosystems in the Western U.S. In such ecosystems, with the fire module on, the EDv2.2 model should be able to closely reproduce vegetation conditions similar to observations. In our multi-decadal point-based analysis we observed regrowth and co-existence of shrub and C_3 grass (closely resembling the ecosystems) that what was simulated with fire on. In addition, this assumption also helps us analyse approximate fire return intervals (as revealed by peak disturbance rates in Fig. 2). However, such assumptions may not work well if we are doing simulations for a limited number of years.

We have added the following text to further elaborate the assumptions of the model at L91-96.

"Along with other disturbance factors in EDv2.2, the fire sub-module creates and maintains age- and size-based heterogeneity at sub-grid levels to closely resemble a broad range of structure and composition in a disturbed ecosystem. For example, a study from South America by Longo et al. (2019a) showed that this model represented a fire disturbed ecosystem like woody savanna very well."

Line 245 ff and throughout the section: Please rewrite "loss of GPP" think of recovering GPP instead, Specify which row in Fig. 4 you refer to, because Fig. 4 actually shows a comparison between the fire and no-fire experiments. GPP is low, and still less than the initial conditions in 2016. In 2019 this spatial pattern is still different from 2016. This description is missing in your results paragraph. It is not "loss", it is rather "reduced GPP".

Corrected.

Line 255: replace "later" with "latter"

Corrected.

Line 270: Check if corrected sentence is complete.

Line 270 appears OK, please let us know if we misunderstood.

Line 285: if your transient simulations have not yet reached an equilibrium, then please revise your modelling protocol accordingly, and re-run the simulations so that the simulation results are a result of climate and fire and not modelling artefacts. This is crucial for the manuscript to get published!

Please refer to our previous response and figure.

Line 299: delete remaining part of an old sentence: "For EDv2.2 GPP"

Deleted

Caption of Figure 6: explain colors

Corrected

Discussion
262: Forkel et al. 2019 is a global study. It is not comparable to the spatial and temporal scale of your study, please delete.

This has been deleted, thank you for the correction.

Line 374: if you see potential errors occurring from WRF, specify which those could be and how do they influence your results. Same applies for MODIS GPP data (line 379).

We elaborated at lines L 338-341. We stated that there is an additional source of error in using WRF given it is a modelled product compared to potentially using field meteorological data. Likewise, we cited some of the sources of uncertainties associated with MODIS derived GPP.

"Our use of modeled meteorological data from the WRF model rather than any field measurements may be an additional source of error. While making these comparisons, we understand that there are also sources of uncertainty associated with MODIS derived GPP such as mismatching resolutions and limited optimizations (Robinson et al., 2018)."

Entire text: please revisit the revised text again where formulations can be sharpened more to the point. See example for line 205.

Thank you for your suggestion. We have made several edits to sharpen the points.

Conclusion:
The fact that GPP is still underestimated by 20% should be reflected in the confidence description of your results. The model is missing the elevation gradient in GPP which is also reflected in the flux-tower comparison. This should be clearly stated in the conclusion.

Thank you for your comment. We have added the following text at L 345-348 in the conclusion to summarize these points.

"While on average the model underestimated GPP compared to flux tower data (≈ 45%), we observed that the model performed well for the lower elevation sites compared to the higher elevation sites. In these simulations, variations due to the elevation gradient was not well captured as the model parameters we used were primarily developed for lower elevation sites."

Thanks for your understanding and looking forward to see your revised manuscript.
Best wishes,
Kirsten.

Other changes
We made changes to captions of Figures A2 and A3 as they appeared to be incorrect.

Reference

Hurtt, G. C., Dubayah, R, Drake, J., Moorcroft, P. R., Pacala, S. W., Blair, B. J., Fearon, M. G. 2004. *Beyond potential vegetation: combining liar data and a height-structured model for carbon studies*, Ecological Applications, 14(3), 873–883

Calvo, M.M. and Prentice, I.C. 2015. *Effects of fire and CO2 on biogeography and primary production in glacial and modern climates*. New Phytol, 208: 987-994.

Moorcroft, P. R., Hurtt, G. C., and Pacala, S. W, 2001. *A method for scaling vegetation dynamics: The ecosystem demography model (ED)*, Ecological Monographs, 71(4), 557-586.

Chapin, F.S., McFarland, J., David McGuire, A., Euskirchen, E.S., Ruess, R.W., Kielland, K., 2009. *The changing global carbon cycle: Linking plant-soil carbon dynamics to global consequences*. J. Ecol. 97, 840–850.