

Interactive comment on “Optimizing shrub parameters to estimate gross primary production of the sagebrush ecosystem using the Ecosystem Demography (EDv2.2) model” by Karun Pandit et al.

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The manuscript tries to provide a new parameter set for the representation of shrubs in the ED2 – DGVM. The implementation aims to improve GPP estimation in shrublands. Yes, shrublands are under-represent in DGVMs and need more consideration, but I think the present manuscript need an extensive revision to show that shrublands work well within the ED2 model. For two sites a simple methods is used to optimise the parameter values, but the study provide no cross-validation and no further application is given.

This initial study had 2 years of data available from the flux towers (2015-2016), and thus we maximized this available data. Since submittal of the paper, an additional year of flux tower data became available (2017) and we have now included this for subsequent validation in our revision. Additional revisions (please see below) have also been made.

As I have general caveats about the methods used in this study I will list them here and will not go into much detail.

1. Most importantly, the method used here to optimise parameters is not state of the art. There are a lot of methods usually applied to solve the problem of parameter optimization as the Monte Carlo Analysis or genetic optimisation algorithms. Then it would be possible to include all important parameter for the optimisation procedure.

We agree that additional optimizations (and sensitivity) should be performed; for this analysis we used the exhaustive (brute force) method due to computational and study limitations. We spent extensive time on developing the shrub (representing sagebrush) PFT for the EDv2.2 model (e.g. establishing allometric relationships) and several preliminary model run-ups to match with the ecosystem conditions. We've modified the paper to highlight this intent and the conclusions we may draw from the existing work. Again, this research is intended to introduce the sagebrush PFT and its implementation in EDv2.2. Additional robust optimization and sensitivity analyses, and broad spatial scale analysis are the next steps. And we have suggested these in the Discussion and Conclusion sections.

2. Secondly, the same as for the parameter optimisation, the parameter sensitivity measure should be performed with a more comprehensive method (e.g. using partial rank correlation coefficient (PRCC) or Fourier Amplitude Sensitivity Test (FAST)). A freely available paper (<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2570191/>) gives a overview of the methods, which can be used to conduct parameter optimisation and sensitivity tests.

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We used the Sensitivity Index (SI) which is a straightforward linear (and thus efficient) approach. Again, our intent here was to perform preliminary analyses to demonstrate the sagebrush PFT and more robust analyses will need to take place to demonstrate the value of using ED within sagebrush-steppe. Please also see above responses.

3. Another point is that the authors should use both sites to optimise the parameter set, if they want to apply the model on a broader scale. Furthermore, I didn't understand why the study provides the 10 best ensemble means, these can't be better than the best estimate. But anyhow the authors don't provide a cross-validation. Hence it is impossible to evaluate the performance of the optimised parameters as these are used for the optimisation already.

We agree, and we have optimized the second site (WBS, see section 2.6 and Figure 4) similar to LS and modified the relevant text accordingly. In addition, we calibrated the model using two years of flux tower observation (2015 and 2016) and used 2017 observation for validation, which we did not have at the time of submittal. We agree that we need to perform validation with additional observation sites, in order to evaluate the model performance at the regional scale. However, the two additional observation sites in the region are very different from our calibration sites in terms of vegetation composition and morphology. Our intent here is to present the 10 best simulations for each site to document our results, about the range of parameter combinations and potential reference to further studies on sagebrush PFT.

Some other important points are striking:

Metrological data are used for a different time period as the GPP data to which parameters are optimised. If you perform a parameter optimisation specifically for a site, you should use the metrological data of this site, which are normally provided by the EC tower data. But at least the same time period needs to be used.

During submittal, we used random years of the meteorological forcing data (WRF) (from 2005-2015) because the data were not available beyond 2015 for the domain we

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used (at 3 km). We agree with your comments and, in the revision, used meteorological forcing data (WRF) for the same years as the model simulation years which ranged from 2001 to 2017, using 1 km resolution data.

The authors state that the equilibrium is reached after 15 years, which seems to be very short. Figure 2 gives a hint that equilibrium is maybe not reached.

For the previous version of the manuscript, we used eight years for sensitivity analysis which was shown in Figure 2. In this version, we have revised the sensitivity analysis with a 15 year run. A strength of this study is that we are able to initialize the EDv2.2 model using the current state of the ecosystem. In our study we initialized the model with the mean cohort figures based on inventory data (section 2.3. main manuscript) from each of the study sites following approaches similar to other studies (Medvigy and Moorcroft, 2012; Antonarakis et al., 2014). To clarify, we modified the manuscript accordingly (P.8.I.13).

It is not clear to me if the ED2 model used here includes the nitrogen cycle or if the fire dynamics is turned off for the optimisation procedure.

We ran the optimization by turning off the fire dynamics in the model. But, it includes the nitrogen and other biogeochemical processes in a DGVM. Please note that in recent years the two sites have not been disturbed by fire.

It is strongly stated in the introduction that fire dynamics plays an important role in the global carbon balance, but isn't treated in the study!

Correct, but again this study is focused on developing the shrub PFT and initial ED modeling runs for sagebrush ecosystems. Our study lays the foundation for future studies that can incorporate fire dynamics and other disturbance effects. We have emphasized this in the introduction and conclusions in the revised manuscript.

Authors mentioned that they have changed the allometric equations, but it is never written how, please add that to your manuscript as it is an important information. But also

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how the used parameter are applied in the model would be a nice additional information. This would help the reader to understand why parameters are sensitive or maybe not.

Thank you for the comment - we provided the shrub allometric equations in P.7.I.7 and additional information here P.6.I.13 to P.7.I.3. We used these coefficients as some of the sagebrush PFT parameters as shown in supplement Table S1.

Why do you use a different parameter range for optimisation and sensitivity test, or did I get it wrong?

We used a broader range (based on literature and other land models) in our sensitivity analysis in order to cover the entire range of possible values of the sagebrush parameters. We used these findings to be more efficient and realistic in our optimization. We clarified this in the manuscript section 3.2.

And how did you define the parameter range? I missed some references here. The TRY database is an extensive source to determine the parameter range.

We used existing literature for defining sagebrush (or common shrub) parameters, and also a range of parameters for shrub PFTs adopted by other land models (like CLM) to define the parameter ranges (Please see reference column in Table 4). We reviewed the TRY database and they have limited information (eg. *sla*, shrub height and leaf width) for sagebrush.

You have not shown any measures in the figures. The Bias, RMSE, NSE are in the tables and we have now added the measures (NSE, RMSE) for the best case to the figures, as well.

And I do not agree that it is a good match for a site-specific optimisation as stated in the manuscript.

In this revision, we optimized both sites with meteorological data and using representative vegetation conditions from the respective locations trying to match the site con-

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ditions (P.8.I.5). Given the complexity of these sites, we feel that the representation of the optimization is sufficient for an initial demonstration.

Lastly, there are a lot of statements in the abstract and in the introduction about the global importance of shrublands for the global carbon cycle, but authors don't show an application.

We have revised the manuscript to focus more on the sagebrush PFT development and preliminary performance evaluation of the ED model runs. We have also discussed that with this first step in sagebrush parameterization we could scale up the model performance to regional scales with further refinement in parameterizations (see Conclusions).

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-264>, 2018.

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