

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.

Anonymous Referee #1

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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.

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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 optimisation as the Monte Carlo Analysis or genetic optimisation algorithms. Then it would be possible to include all important parameter for the optimisation procedure. 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. 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.

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. 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. 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. 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! 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 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. Why do you use a different parameter range for optimisation and sensitivity test, or did I get it wrong? 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. You have not shown any measures in the figures. And I do not agree that it is a good match for a site-specific optimisation as stated in the manuscript. 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.

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

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