

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-258

Anonymous Referee #2

Referee comment on "Improved CASA model based on satellite remote sensing data: simulating net primary productivity of Qinghai Lake basin alpine grassland" by Chengyong Wu et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-258-RC2>, 2022

Review of "Improved CASA model based on satellite remote sensing data: Simulating net primary productivity of Qinghai Lake Basin alpine grassland" by Wu et al.

The study "Improved CASA model based on satellite remote sensing data: Simulating net primary productivity of Qinghai Lake Basin alpine grassland" by Wu et al. suggests and tests to drive the CASA model, simulating NPP, with remote sensing data only. To validate the new model formulation, they compare simulated NPP results to alpine grasslands in the Qinghai Lake Basin. Relying only on remote sensing data instead of multi-source data e.g., ground observations has several advantages, therefore the aim of this paper is valid and useful. The paper also demonstrates an improvement in model accuracy for the given study region. I see, however, large problems in the presentation of the methods and results, as well as in the approach to validate the model. Also, the quality of the paper, in general, should be improved to be published in GMD.

My major concerns are as follows:

- There are many problems with the grammar and structure, which sometimes obstruct the understanding of the paper. This issue is prevailing over the whole paper, especially in the introduction.

- The authors only use one study region and claim a large improvement in the CASA model. In the Abstract they write to provide a reference for rapidly simulating grassland, farmland, forest, and other vegetation NPP. They also write to satisfy requirements of e.g., precision agriculture. While this is not further discussed in the paper, I would be also careful to make such statements by only comparing to one study region and a limited time window. I would like to have a slightly larger discussion about the study site. In L94 the authors write that it is a typical empirical test site. But since most of the vegetation is grasslands and alpine meadows, other locations should differ a lot. Is there another reason, why this study site has been chosen? Generally, it is of course, ok, to choose one study area and improve the model for this region. But the authors should be careful in claiming that they achieved general, large-scale model improvement and only provide results for one specific location. Is there a reason, why this site is special and/or important? Would it be difficult to check the new model also for other sites and just compare it to other published NPP values (taking on-site measurements is obviously more complicated)?

-The structure of the paper could be largely improved. For example, the intro should be rewritten to better introduce the state of the art and compare it to what has been done in the study. Sometimes, it is not entirely clear, what has been done in the study and how was CASA used before. Some parts of the Introduction should go to methods (see minor comments), while parts of the Discussion could be in the Introduction. Parts of the results would fit much better to methods (see minor comments). The Discussion generally talks only about very few results of the model and is in large parts more like a summary.

-I would like a better Discussion about strengths and weaknesses compared to the traditional approach and a better Discussion about the (large) errors between the traditional and new approaches. It would also be good to have an overview of how useful the model could be, especially as in the Abstract the authors write about precision farming, but this is not taken on in the paper. Especially, since the error is up to 50% or 85% (in the traditional approach), I would like to read about how such a large error is possible and how much use the model could have. With such large errors, any improvements should be put into context. For example, how do other models perform? Do they also have such large errors? I also wonder, why some of the RS data has not been used before in the CASA model.

- Adapting Table 1 with the different inputs for the "old" and "new" CASA model would

greatly help for a better overview. Sometimes the model description and description of input sources are a bit mixed between the two model types.

-There are sometimes references missing for several statements or model calculations. If they were developed by the authors, some reasoning or development of the method is missing.

Minor comments:

L31: In many global models, NPP is also calculated and not just an input.

L37/38: Instead of "process models", I would write "process-based models".

L43-L69: This is too much detail for the introduction. This part could be shortened for the relevant details to present the approach, while the details should be in the method section as a model description

L44-47: T_{e1} , T_{e2} , and e_{max} sound like plant-specific parameters but the authors write that they are usually calculated by air temperature or RS data. Please clarify.

L52-53: How did you determine the coefficients a and b for your time and location? In 4.1.1 you write that they were adopted from Liu et al. But are these values specific for the

study region? (But all this should be part of the methods)

L81/82: "we hope to use..". The authors should better write what they did and achieved or not achieved.

L87: (5) does not really fit the other (1)-(4), which state the different input variables used. Instead of (5) just write where and to what you apply the model with the new input sources. Also, the sentence "the RS data-driven CASA model was tested with multi-source data-driven CASA model" should be rewritten, because it makes not so much sense as it currently stands.

L127-132: Is there an example, where this procedure has been done before? Is it a standard procedure to measure AGB? Maybe the authors could provide some literature here.

L146-152: I don't understand why the factor of 0.5 for the proportion of the radiation which can be absorbed by plants is necessary when FPAR is another input for exactly this. What is the difference between the two factors? And why is 0.5 a constant over all regions and plant types?

L149-152: Here again, the description of T_{e1} , T_{e2} , and e_{max} do not fit the Introduction. Why should e.g., e_{max} be calculated by RS data when it is the maximum possible efficiency? Again, the model description part of the Introduction should be part of 3.1.

L169-170: Why create 10 levels and not use a continuous result for diffuse_proportion and transmissivity? How is the linear relationship developed?

L173-174: Do you have any citation for the statements in this sentence?

L185-189: Please rewrite this paragraph due to bad English sentence structure. And this paragraph would probably fit better to the methods.

L203-2004: I would not call the approach superior based on one location. Just write that it yielded better results for the study region.

L207-210: This would also fit better to methods.

L233-234: Again, you compare the results just to one study area and only to July 2020 but claim a major improvement of the model. For more evidence, it would be beneficial to compare your results to more data. Are there any NPP datasets available for a larger region or a longer period, to which you could easily use and apply the model to?

L240: Again, I would not write superior, due to scarce evidence, just write that it performed better for the given data points.

L251-262: Much of this is not really discussed but would probably fit well into the introduction.

L271: What do you mean by that the WSC results of your improved approach are unique?

296-288: Would it not be possible to model NPP for the full year as well? Results could be much easier compared to the reported NPP.

L299: The title of the subsection does not fit the text.