

Geosci. Model Dev. Discuss., author comment AC3  
<https://doi.org/10.5194/gmd-2021-258-AC3>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Reply on RC2

Chengyong Wu et al.

---

Author 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-AC3>, 2022

---

Dear expert,

We are grateful for your comments. Those comments are valuable and very helpful for improving our paper, which is useful to the development of CASA model.

### **The major concerns are replied 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.*

Thank you for pointing this out. We will carefully check the grammar and structure and correct the relevant errors accordingly.

*- 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)?*

Thank you for your rigorous comments. According to your nice suggestions, we will be careful to make statements such as "satisfy requirements of e.g., precision agriculture". Since the field observation NPP data in other sites were not obtained at the time of this study, this study site (Qinghai Lake basin, its vegetation mainly is grasslands and alpine meadows) was chosen to validate RS driven CASA model. This site is not special. We also

hope that the new model (RS driven CASA model) will be validated in other sites.

*-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.*

Thank you for your suggestion. We agree that the structure of the paper should be partly improved. We will try to adjust its structure under the condition of no affecting the reading and scientificity.

*-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.*

We understand your concern about it. The strengths and weaknesses of the traditional approach have preliminarily discussed in the paper.

The SOL simulated by traditional approach and improved approach of sample 7 (its error is up to 85%) is  $271.39 \text{ MJ}\cdot\text{m}^{-2}\cdot\text{month}^{-1}$  and  $695.40 \text{ MJ}\cdot\text{m}^{-2}\cdot\text{month}^{-1}$  respectively. The average measured SOL of Gangcha solar radiation observation station is  $725.61 \text{ MJ}\cdot\text{m}^{-2}\cdot\text{month}^{-1}$  (Table 3). The distances of this station from the sample 7 is about 43 km (the following Fig).

So for sample 7, the errors of traditional approach (multi-source data driven CASA) is mainly caused by the parameter SOL and the spatial interpolation method.

Some RS data has been used for calculating several parameters of the CASA model before, for instance, some studies used RS data to calculate the parameters of WSC which could be found in L65-67.

- *"Adapting Table 1 with the different inputs for the "old" and "new" CASA model would greatly help for a better overview".*

Thanks for your valuable suggestion. We will try to adopt it.

-*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.*

Thanks for your suggestion. The references about model calculations might have cited but did not write details formula in the paper, which might be added in the revision.

**The minor comments are replied in the following:**

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

Thanks for your suggestion. We will delete this sentences which is not precise.

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

We are pleased to adopt your nice advice. Thank you.

*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*

Thank you. We will try to shorten this part. If all the details present in the approach, there might be a question: it is difficult to describe the rationality of proposed RS data driven CASA model.

*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.*

Yes. The  $e_{max}$  is a plant-specific parameters.

Excessively low temperature can limit plant photosynthesis and excessively high temperature can increase the respiration consumption of plants. Temperature stress factors  $T_{e1}$  and  $T_{e2}$  represent the effects of low temperature and high temperature on Light Use Efficiency of plants.

*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)*

Thank you. The coefficients  $a$  (0.24) and  $b$  (0.46) were adopted the July values from Liu et al.

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

Thank you. Acting on your recommendation, we will rewrite this statement.

*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.*

Thank you for your nice suggestion. We will rewrite this paragraph.

*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.*

We sincerely appreciate the valuable comments. We will add the following literature into paper.

Ministry of Ecology and Environment, PRC.: Technical specification for investigation and assessment of national ecological Status: Field observation of grassland ecosystem, available at :<https://www.mee.gov.cn/ywgz/fgbz/bz/bzwb/stzl/202106/W020210615510937790570.pdf>, Last modified: 12 May 2021(in Chinese).

*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?*

According to Potter et al. (1993), FPAR is the fraction of the incoming photosynthetically active radiation (PAR) intercepted by green vegetation, and the factor of 0.5 accounts for the fact that approximately half of the incoming solar radiation is in the PAR waveband (0.4-0.7  $\mu\text{m}$ ).

*L149-152: Here again, the description of  $T_{e1}$ ,  $T_{e2}$ , and  $e_{\text{max}}$  do not fit the Introduction. Why should e.g.,  $e_{\text{max}}$  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.*

Thank you for your suggestion. At region scales,  $e_{\text{max}}$  is usually determined by vegetation type or Land-use and Land-cover change that derived from RS data, which we will write in revision.

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

Thank you for advising this scientific issues. In very clear sky conditions, the typically observed values of transmittivity are 0.6 or 0.7, and the typical values of diffuse\_proportion are 0.2 (<https://pro.arcgis.com/en/pro-app/latest/tool-reference/spatial-analyst/area-solar-radiation.htm>). So the linear relationship is developed:

$$\text{diffuse proportion} = 0.2 + 0.055 \text{ level}_{\text{cloud cover}}$$

$$\text{transmittivity} = 0.6 - 0.055 \text{ level}_{\text{cloud cover}}$$

Under 10 levels of total cloud cover, the step length of 0.055 is determined after repeatedly testing. Under the condition of the continuous total cloud cover ranging from 0 to 10000, it is an interesting and scientific issues for determination the step length.

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

We understand your concern about it. The shortwave infrared reflectance is negatively correlated with water content, which is a common point of RS scientific fields. The following literature may partly support this point.

Fensholt, R., & Sandholt, I. (2003). Derivation of a shortwave infrared water stress index from MODIS near- and shortwave infrared data in a semiarid environment. *Remote Sensing of Environment*, 87(1), 111–121. doi:10.1016/j.rse.2003.07.002.

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

*Thank you for pointing this out. We will rewrite this paragraph.*

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

Thank you for your nice suggestion. We will rewrite this statement as follows: “for simulating SOL, the improved approach significantly increased the accuracy in the study area.”

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

Thank you again for your suggestion.

*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?*

We totally understand your concern. So far, we do not obtain field observation NPP datasets in other sites. If there are any grassland NPP datasets available for a larger region or a longer period, we are eagerly to use them to check model!

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

We gratefully appreciate for your valuable suggestion. We will rewrite this statement as follows: “RS data driven CASA significantly increased the accuracy of grassland NPP in the study area.”

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

Thank you for your suggestion. Due to this paper focused specifically on improving the parameters SOL and WSC, if the L251-262(Discussion SOL) were completely put into introduction, it might be difficult to describe the simulation SOL by introducing RS cloud cover. We will put this statement “Astronomical solar radiation passes through the atmosphere, it is weakened by atmospheric scattering and absorption, and finally transmits to earth surface (so called surface solar radiation), which means that atmospheric conditions significantly affect surface solar radiation” into the introduction section.

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

Thank you for pointing this out. This sentence might be written like this " The WSC result of our improved approach is certain as long as the same RS data is input in formula (3)-(5)".

*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.*

Thank you for your insightful suggestions. Of course, it can be modelled NPP for the full year though monthly NPP of growing season.

Qinghai Lake Basin is located on the Qinghai-Tibetan Plateau, which has a severely cold climate and short growing season. Vegetation is in its growth stage in July, its biomass reaches the highest values for the whole year before declining at the end of August or beginning of September. As there is no field observation NPP data of other growing season, we just model NPP for the July of 2020 in the paper. In further, once field NPP of other growing season were obtained□we will compare it to the reported annual NPP.

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

Thank you for your insightful suggestion. Because the section 5.4 is about the discussion of simulation results with RS data driven CASA, We will delete the subsection 5.4.1 and 5.4.2 and rewrite the title of section 5.4.

Thank you for your valuable comments.

Kind regards,

Chengyong Wu