

Biogeosciences Discuss., referee comment RC2
<https://doi.org/10.5194/bg-2021-152-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on bg-2021-152

Anonymous Referee #2

Referee comment on "Resolving temperature limitation on spring productivity in an evergreen conifer forest using a model-data fusion framework" by Stephanie G. Stettz et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-152-RC2>, 2021

General comment:

The introduction of a cold temperature scaling function into CARDAMOM significantly improves CARDAMOM's ability to represent the interannual variability observed in the tower-derived mean spring GPP though it results in a slight degradation of CARDAMOM in representing the seasonal cycle of GPP at an evergreen needleleaf site. The cold temperature scaling function doesn't improve CARDAMOM's ability in estimating summer or annual GPP at the site. The result suggests other environment controls might impact summer and annual GPP variability while CARDAMOM doesn't include that scheme. It's a good paper in proving that the added cold temperature scaling function does make CARDAMOM in capturing the interannual variability of observed mean spring GPP which could not be seen in CARDAMOM before the cold temperature scaling function is added.

Specific comments:

C1#: lines 160-162. Fig. 2 only shows the scatter points of spring GPP and spring air temperature. It doesn't show the scatter points of spring GPP and summer air temperature or scatter points of spring GPP and winter precipitation. I suppose you actually intend to might use a figure showing the relationship between the environmental forcings and mean spring temperature instead of mean annual GPP as shown in Figure S2.

C2#: lines 164-167. I don't understand the reason why you investigate the correlation between the environmental forcings and mean annual GPP here. I think if you move the result described here to somewhere near line 377, it will make the story well connected. Otherwise, the reader has to go back and forth to understand your point. Some might even forget what you already described here when they read the lines in the far behind and get confused there.

C3#: lines 205-207. What is the $T_{\min}(t)$?

C4#: The equation (2) will scale GPP between photosynthesis shutdown (0) and photosynthesis initiation (nominal GPP) when $T_{\min}(t)$ is between T_0 and T_g . Does the physical scheme as described by the equation exist in reality? Or Whether in reality does

such GPP between photosynthesis shutdown and initiation exist? Or the equation is just an empirical equation and it doesn't represent the actual process at all.

C5#: line 225. 'parameter optimization, and data assimilation'. To my understanding, the Model-Data Fusion proposed by Bloom and Williams (2014) which is used in your study is a framework to optimize the parameters in DALEC model and it is a parameter optimization method instead of data assimilation. Data assimilation and parameter optimization are two different methods to reduce model uncertainty. You're not using the 'data assimilation' method in your study. The so-called data assimilation mentioned here is actually parameter optimization. You could read some papers about data assimilation to know the difference between the two methods.

C6#: lines 249-253. A table that shows the difference between the within-model experiments described here is better than the text for readers to remember and check the difference when they read the results and discussion later.

C7#: Do you have any insight that whether the cold temperature scaling function will also improve the fall GPP simulation? I bet so.

C8#: lines 388-390. The model intercomparison provides the direction or strategy which you can take to further improve the annual GPP simulation at the site. The statement is easy for readers to understand why you do the model intercomparison here. Instead, the original sentence is hard to understand especially 'discern common environmental controls in ...' because the difference of environmental controls on different models is so subtle at the point until you reveal them later in lines 450-457.

C9#: lines 455-456. 'high correlation and reduced error'. I can see the higher correlation between CLM5.0 and the observations from both Fig7(a) and Table2, but the RMSE and MBE are higher for CLM5.0 compared with CLM4.5. The reduced error means RMSE or MBE? or something else? If the reduced error here means RMSE or MBE, it's not consistent with what is shown in both Fig7(a) and Table2.

C10#: line 474. 'improved model estimates of productivity at Niwot Ridge' in what sense? if compared with CARD, CARDcold is actually slightly worse in representing the accuracy of seasonal cycle of GPP. You'd better to address in what sense the improvement is. Otherwise, it's not accurate.

C11#: line 482. Instead of 'Western US', subalpine temperate forests might be more reasonable.

Technical corrections:

T1#: line 194. Figure 3. The x-labels of the three sub-figures should be maximum, minimum and mean monthly air temperature respectively as described in lines 181-183 instead of spring air temperature.

T2#: line 414. 'b.) monthly GPP'. 'monthly GPP from 2015 to 2018' is clear. As I read it, I have to go to lines 256-257 to make sure all the data are from the same period.

T3#: lines 440-442. The description about SIB3-exp1 and SIB3-exp2 here is contrary to that in line 250 and lines 407-408.

T4#: lines 450-453. Could you please mention the figure or any table at the end of the sentence in lines 450-453 to support your point? It will be easier for us to follow your

point if you add the figure or table from which you conclude your point.