

Additional comments on Wu et al. Biogeosciences Discussions

Ray Leuning, 1 November 2001

The discussion of the paper by Wu et al has been of great interest and has prompted this response from 'Reviewer 2'.

Considerable emphasis has been given by others to my comments concerning the definition of a model parameter as being a constant. I have done enough ecophysiological modelling of vegetation to know there is no such thing as a constant parameter in biology, there are few enough in physics. My criticism concerns the use of parameters that are adjusted every two days and then claiming that the model explains 83% of the variance in the annual half-hourly measures NEE (L313). Of much greater interest is to understand or describe the variation in the parameters shown in Fig 4 in terms of more fundamental processes. These 'sub-modules' may then have more stable parameter values but I concede that this leads to increasing model complexity and hence the possibility of over parameterization.

A major concern that still remains is what seems to me an artificial separation of 'direct response from biotic changes' (L207). How can one separate cause from effect that is independent of the model chosen? This is acknowledged by the authors on P21 when they contrast their results with those of Richardson et al. (2007) who used a different model and thus attributed different degrees of control of NEE etc by climatic and biological factors. Figures 5 and 6 are thus not very useful because they are specific to this paper.

Note that the oscillations in the parameter k seen in Fig. 4a are likely an artefact of over-fitting. There also seems to be a strong negative correlation between r_b and E_0 during the growing season (Fig 4b).

My earlier criticism of the paper and one reason for recommending that it not be published concerns the question 'what advances our understanding and knowledge?' In my opinion this is best achieved by a combination of modelling and data and the paper by Wu et al follows this path. Unfortunately, I think the model they use is not adequate for the task they propose, and because model performance is now totally dependent on the seasonal and interannual variation in parameters that has been determined by fitting the model to the data every two days. It is highly unlikely that this model and the parameter values shown in Figure 4 will be applicable to any other site and this site-dependence greatly diminishes the value of the model and the findings of this paper. Again, I acknowledge similar problems with other models. Many of these points are recognised in the Discussion, but in my opinion the paper does not add significantly to what is already known and does not describe new methods or insights and hence I recommend that it not be published.

My final criticism concerns the overuse of correlation analysis and a lack of mechanistic description of basic process governing the carbon balance of their forest. Clearly, GPP is driven by photosynthetically active radiation absorbed by leaves and hence there must be a high correlation between the two on a diurnal basis. The correlation will necessarily decrease with increasing averaging time because GPP depends on many other factors, such as how

much leaf area is present (phenology) and the rate of diffusion of CO₂ through the stomata which in turn depends on the water availability to the roots and so on.

L475-477 Respiration fluxes at the soil surface depends on the relative rates of carbon input by the roots to heterotrophs and the rate of autotrophic respiration. Carbon in the soil accumulates from photosynthesis from past and present seasons. The degree of correlation between GPP and soil respiration will never be simple, it depends on the size of the soil carbon stock relative to the input rate via the roots as well as on microbial activity which varies with soil water and temperature distributed over the depth of the root zone. Processes such as these processes cannot be captured by the model used in this paper, nor will they be elucidated by simple correlation analysis.

Detailed comments

L183 Eq 3. Is this valid given the serial correlation in the data?

L269 and elsewhere. Do not confuse correlation with causation. Soil moisture was measured at very shallow depth.

L330 – 333 Of course one would expect a better fit by continually adjusting parameter values.

L353 the effect of climate variability on TER than on GPP for example. TER is generally dominated by soil respiration and this depends on the amount of carbon and nutrients available for root and microbial respiration modulated by soil temperature and water content through the whole root zone. This buffering will decrease the correlation of TER with climate variables compared to above-ground processes such as photosynthesis and leaf growth and senescence.

L403-407. The ranking of ecosystems in terms of ‘functional change’ given here is not valid because different models were used by different authors. The same model should be used for such a ranking to be valid. In any case, how does this help us understand ecosystem function?

L488 A steady average LAI is not sufficient, you also need to consider timing of leaf out and leaf fall.

Table 2 Only show data for half the matrix.

Fig 2 is very hard to read, especially in monochrome. I suggest plotting the mean or median with shading of ± 2 s.d to indicate the spread across years.