I don't think this should be published as a research paper. However, it may have some value as a teaching tool. The paper makes a good start by noting the large uncertainties in the photosynthesis parameterizations used in the CMIP-5 inter comparison, and they note the variety of approaches used to parameterize productivity in existing models. However, the analysis that follows does little to address any of these uncertainties; their estimate of global GPP is an extreme outlier (nearly 2x the mean); the structure of model itself is not innovative (similar to CASA and SDBM), and no new result is presented to demonstrate how existing models could be improved. The sequential approach of narrowing-in, starting from the top of atmosphere solar radiation to consider, atmospheric attenuation, elevation, satellite determined FPAR and temperature limits is implicit in other light use efficiency models. It would be helpful if there were some discussion of the relative merits of this approach relative to other alternatives. While I would agree with the observation (last line of the abstract) that water is the most important factor limiting global productivity, there is no analysis to support this. I also agree with the conclusion that Earth system models should make greater use of satellite data, but again, this is basically a statement of opinion.

This reviewer has missed the key point that our model is derived from first principles, represented in an extremely simple way – and yet, it is able to reproduce key features of the geographic and seasonal patterns of GPP, suggesting that a great deal of the complexity in current DGVMs could (usefully) be dispensed with. A related point was made in the paper previously published in *Biogeosciences* by Smith et al. (2013). Our MS is complementary to that paper as its focus was on demonstrating traceability of land carbon-cycle parameterizations from observations; our focus is on the theoretical underpinnings of models specifically for the primary production component of the carbon cycle.

In our revision of the MS we have taken some trouble to spell out the ways in which our model is an advance, and we have suggested directions for further development. The main improvements are described in our responses to the first anonymous reviewer.

We respond now to the second reviewer's specific comments, one by one.

1. *the analysis that follows does little to address any of these uncertainties*

Our revision of the MS states more explicitly the following points which were implicit, and apparently not articulated clearly enough, in the first version. (1) As there are large disagreements among DGVMs, their fundamental assumptions cannot all be correct; hence the need to re-examine those assumptions, which can best be done by starting with a simple model developed from a sound and explicit theoretical basis. (2) Current DGVMs rely on many poorly known and implicitly tunable parameters, but the tuning is rarely done transparently or optimally; hence the value of a simpler model (as we present) in which the number of parameters is minimal, and no parameter is tuned to the observations that will be used for evaluation. (3) The complexity of current DGVMs make it generally impossible to track down the sources of differences among models, suggesting the need for a fresh start, building on the huge advances in data availability since most of the current "state-of-the-art" models were originally designed.

2. *their estimate of global GPP is an extreme outlier (nearly 2x the mean)*

The generally accepted range for global GPP is about 110-130 Pg C a^{-1}. Unfortunately, the CMIP5 models' estimates range from about 100 to 210 Pg C a^{-1} with a mean close to 140 Pg C a^{-1}. So although our model estimate is presumably too high, it is only about 1.5 x the mean (and not outside the range) of current "state-of-the-art" models.

3. *the structure of model itself is not innovative (similar to CASA and SDBM)*

This is not quite true. In CASA and SDBM, a maximum light use efficiency (of NPP) is calibrated to observations, and somewhat arbitrary multipliers are used to reduce LUE as a function of temperature and drought. By contrast, our model is derived directly from Farquhar's photosynthesis model for C_3 plants as GPP = $\varphi_0\ I_{abs}\,(c_i - \Gamma^*)/(c_i + 2\Gamma^*)$, where the maximum LUE (φ_0) is based on independent measurements, and growing season temperature, evapotranspiration deficit and elevation modify LUE through their effects on c_i – which are innovatively predicted, based on a theory (and field evidence) first published in 2013.

4. *No new result is presented to demonstrate how existing models could be improved*

An expanded discussion on "implications for modeling strategy" in our revised manuscript now gives pointers as to how existing models could be improved. We argue that ecosystem models should be traceable to observations and explicit hypotheses. We suggest that better models could be achieved by incorporating improved understanding of fundamental plant processes, together with the greatly enhanced availability of large-scale observational data sets. Our model represents a first step towards this goal.

5. *The sequential approach of narrowing-in, starting from the top of atmosphere solar radiation to consider, atmospheric attenuation, elevation, satellite determined FPAR and temperature limits is implicit in other light use efficiency models. It would be helpful if there were some discussion of the relative merits of this approach relative to other alternatives.*

This sequential approach might be "implicit" in other LUE models in the sense that a similar demonstration could be done, in principle, with a different LUE model. However, (a) it has not been done, to our knowledge, (b) the results would be much less interpretable in the case of any model that adopts different parameter values for different biomes or PFTs, such as the MODIS GPP product, (c) the first reviewer evidently holds a different opinion, noting that "the application of successive constraints on primary production ... helped elucidate how and why we observe its spatial distribution" and described Figures 1 and 3 as "fascinating". As for alternatives, it is not obvious to us how else such an exercise could be done.

6. *While I would agree with the observation (last line of the abstract) that water is the most important factor limiting global productivity, there is no analysis to support this.*

The largest constraint on GPP occurs when fAPAR is introduced into the model. But it's true that we did not previously show any analysis to prove that the largest constraint on fAPAR is water availability.

Annual fAPAR is the remotely sensed equivalent of foliage projective cover (FPC). It has been known since Ray Specht's pioneering work in the 1970s (e.g. Specht, 1972) that water availability is the dominant control of evergreen FPC at a continental scale, so we did not think the point was controversial. However, we have now included an analysis of our fAPAR data set by sequential regessions. We show that nearly half of the spatial variability in fAPAR can be explained by the Cramer-Prentice α coefficient, a widely used measure of plant water availability (which estimates the ratio of actual to equilibrium evapotranspiration, similar to the ratio of actual to potential evapotranspiration, proposed by Specht to be proportional to FPC.) Mean growing-season temperature contributes about a further 10% and soil cation exchange capacity (a measure of fertility) an additional 1%.

7. *I also agree with the conclusion that Earth system models should make greater use of satellite data, but again, this is basically a statement of opinion.*

This statement of opinion is also probably not controversial but in any case it is not the main conclusion of this paper. Our main purpose was to analyse the controls on GPP, a topic of central importance for carbon cycle modelling yet one for which there is no consensus in the literature (as we discuss in our Introduction). A subsidiary purpose (which we have made more of in our revision) was to provide some pointers towards the design of a next-generation vegetation model. There is an element of subjective opinion in this too, inevitably, but we do not pretend otherwise.

References

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