Sietse Los has provided a thoughtful riposte containing many points that should help to improve a revised MS. It would be good to see some of the reasoning behind some of his key choices stated explicitly in a revision.

Among the central points he makes, quite correctly, are (a) that NDVI is not the same thing as NPP (especially from a land-surface physics point of view), (b) that absorbed PAR (as used in the light use efficiency model for GPP) depends on all of the factors that influence NDVI including soil moisture and temperature, (c) that an appropriate model for NDVI on a monthly time step is not necessarily the same as an appropriate model on an annual time step, and (d) that monthly NDVI is indeed not generally a function of *this month's* PAR. Plant allocation strategies include storage of fixed carbon over seasonal and even interannual time scales, so I would not expect to see the same seasonal cycle of foliage cover as is observed for GPP. I would also expect to see a strong effect of the temperature dependence of biomass growth (as opposed to photosynthesis) on the seasonal cycle of foliage cover.

In this reply I would like to take advantage of the interactive discussion forum format to mention what, for me, are still outstanding issues – not necessarily for this manuscript, but for future progress in this field. I entirely agree that the prediction of green vegetation cover is a major weakness of current dynamic global vegetation models and Earth System models (ESMs), and that analysis of the NDVI record will be central to the task of strengthening it. However, I am not entirely convinced by some of the author's arguments in favour of the CCVI as presented here.

- It is argued that inclusion of (for example) hydrological calculations of plant water availability would create an undesirable inconsistency with the calculations already embedded in ESMs. This is a good point. On the other hand, ignoring known mechanisms (such as the influence of PAR on GPP, which ultimately provides the carbon required for allocation to foliage) also creates an inconsistency. All ESMs as far as I am aware incorporate an influence of PAR on GPP, and explicitly allocate carbon to foliage by one mechanism or another (however poorly or simplistically they do so). So I cannot escape the notion that what we really need is *not* a stand-alone model for green vegetation cover that can be "plugged in" to an ESM, but rather an improvement to the representation of foliage allocation in ESMs. Given the unfortunate fragmentation of contemporary ecosystem science into narrow specialisms, this could probably best be achieved through collaboration between experts in remote sensing, ecophysiology, and ecosystem modelling.

- I disagree with the author's inferences from the Farquhar model. He cites work by Alton indicating that the PAR response pf GPP at canopy level is "flatter" than the PAR response of photosynthesis at leaf level. But to me, the crucial point is that the PAR response of GPP is flatter *on longer time scales* (e.g. monthly) than the instantaneous response of photosynthesis. What Fig. response 1.1 shows is that electron-transport limited photosynthesis is not, or only weakly, temperature dependent whereas Rubisco-limited photosynthesis is strongly temperature-dependent. However, in this Figure, Rubisco capacity (V_{cmax}) is assumed constant, as would be the case in a short-term experiment. Over weeks to months, acclimation of the Rubisco capacity takes place, such that the linear relationship between GPP and absorbed PAR remains intact. With monthly (as opposed to daily or half-hourly) timesteps, based on CO_2 flux measurements, it can be shown that there is approximate proportionality between GPP and absorbed PAR, even though a strong saturation effect is usually apparent over any one diurnal cycle. And if GPP is proportional to absorbed PAR, then it is certainly "limited" by PAR, as well as by other factors.

- My comments about the Lieth relationship between temperature and NPP being an artefact of the correlation between temperature and PAR were meant to apply to annual values, and to the latitudinal gradient. I accept that temperature directly affects the seasonal cycle of foliage display and also that temporal variations in cloudiness may have a muted effect (not least because of direct-diffuse radiation partitioning). But there is still an oustanding issue for any model that makes predictions of latitudinal gradients in vegetation function based on temperature without considering the latitudinal gradient in PAR – simple because under a changing climate, temperature may change, but insolation does not.

- Finally, I take the point that cloudiness data are not as "good" as temperature data in that they are (a) somewhat subjective, (b) sparser in space and (c) less extensive in time than temperature data. Mike Hutchinson, ANU has developed a work-around for this problem, which has been applied to Australia (see http://portal.tern.org.au/monthly-daily-incident-1970-2012/19754: accessed 25 September 2015) and probably could be applied globally. But does it matter? I think it does, to the extent that using temperature as a surrogate for PAR could lead to errors in the projection of climate-change impacts.

Colin Prentice