Response to 2nd series of comments by Colin Prentice

1. 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.

Response: the purpose of the paper is to develop and test a leaf seasonality (sub)model that reproduces spatial, seasonal and interannual variability observed in the NDVI. This model is to be implemented in a land-surface model or ecological model that uses satellite observations (e.g. SiB, CASA) or has the option to use these (e.g. JULES). For models using satellite data it is straightforward to implement the CCVI with only minor modifications. The model with the CCVI implemented can then simulate photosynthesis, GPP, NPP, transpiration and albedo. The advantage is that a model that was previously restricted to simulations of the satellite era can now be used for historic simulations (pre satellite ers) and simulations of climate change. The CCVI uses the idea that NDVI is limited by temperature and precipitation, the idea of the environment limiting plant processes can be found in a range of ecological and plant physiological models. Given that models like JULES, SiB and CASA are widely used and have been accepted by the community the approach to find a substitute for satellite data appears valid. It may not satisfy those interested in ecosystem modelling, but it does address a need to simulate realistic fluxes of mass and energy between the land and atmosphere in climate models.

The CCVI approach is different from a model that "grows leaves" in response to absorption of carbon from the atmosphere and subsequent allocation to part of the plant, although it is perhaps useful to point out that several "mechanistic" leaf seasonality models do not just build leaves by allocating carbon, but also use temperature to calculate leaf out and leaf growth, similar to the CCVI. It was intended to develop the CCVI as a simple model first that does one thing well and add complexity or adapt it to a different model as needed. Once the CCVI is built into an ecological model such as CASA or a land-surface model such as JULES or SiB, PAR is incorporated and GPP or NPP can be calculated.

2. 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 (Vcmax) 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 CO2 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.

3. 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.

4. 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.

Response to 2-4: comments 2-4 relate to the fact that PAR is not incorporated in the CCVIm, I will therefore respond to these together. The role of PAR is very important for the calculation of GPP / NPP and by no means is it my intention to convey the message this is not so. Part of the carbon absorbed by photosynthesis will find its way in leaves, but the timing of allocation of carbon to leaves, as pointed out by the reviewer, does as a general rule not follow the seasonal cycle of incoming solar radiation. This may in part explain why models using a direct relationship between monthly NPP and allocation of carbon to leaves fail to simulate the leaf seasonal cycle.

I would like to expand on my earlier point regarding photosynthesis. When looking at the role of radiation at leaf level we see a saturation effect, photosynthesis increases with radiation at low light levels and then saturates at intermediate to high levels. This saturation plateau increases with temperature (and with the amount of Rubisco in the leaves). The reviewer points out that over longer time periods, i.e. months, vegetation will adapt and increase its capacity to photosynthesise thus raising its saturation plateau. There is, however, a lot of literature that reports a saturation effect both at leaf and at stand level especially when vegetation is stressed. Furthermore there is evidence that excess radiation and excess photosynthesis can be damaging to plants, that plants use only a small proportion of radiation for photosynthesis and also that plants have adopted ways to dissipate the excess energy absorbed by leaves in the form of latent heat, sensible heat and to a minor extent fluorescence. It is therefore important to test if the plateauing effect found in the daily photosynthesis cycle disappears over longer (monthly) time steps as suggested by the reviewer. I added analysis to this effect; I calculated partial correlations between monthly NDVI on the one hand and radiation, temperature and precipitation on the other. The partial correlations are a measure of association between one variable (e.g temperature) and NDVI, taking into account the effects of the other variables (e.g. radiation and precipitation). In monthly data, NDVI seasonality is most closely correlated to temperature in mid-to-high latitudes, the correlation is very high and exists throughout. In low latitudes precipitation is dominant. Monthly radiation, when the effects of temperature and precipitation are considered, is, for most of the land surface, either weakly correlated with monthly NDVI or negatively correlated. The exception is the Sahara (and other deserts) where the NDVI signal (in the Sahara) is linked to variations in atmospheric water vapour (the near infrared AVHRR channel contains a water vapour absorption band). The negative or zero partial correlation of NDVI with radiation indicates that the saturation effects observed at sub-daily time steps cannot be ruled out and likely affects NDVI at monthly time steps.

To summarise: the data show that at monthly time steps temperature and precipitation are the key

variables for predicting NDVI; PAR appears either not to contribute to NDVI or to inhibit it. My interpretation is that PAR is available in ample supply and is not limiting NDVI. Thus although the rubisco / vmax may increase during the season as suggested by the reviewer, the evidence that temperature is limiting growth in temperate and high latitudes is much stronger than that radiation is limiting growth. PAR is still important to calculate NPP or GPP, but that is not the purpose of the current study.

The point that PAR may become limiting in the future as temperatures continue to rise was also investigated. The analysis indicates that temperature has to increase considerably, at 60 degrees N the annual mean temperature needs to increase by more than 6 degrees C for radiation to become limiting. The radiation limitation can be used to limit NDVI in case these large changes in temperature occur. It is not implemented in the current CCVIm (1.0) version.

The above analysis was included in the text as were references to literature that reports strategies adopted by vegetation to deal with excess radiation.