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Interactive Comment

Interactive comment on "Modelling spatial and temporal vegetation variability with the Climate Constrained Vegetation Index: evidence of CO_2 fertilisation and of water stress in continental interiors" *by* S. O. Los

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Review of Los, S.O.: Modelling spatial and temporal vegetation variability with the Climate Constrained Vegetation Index: evidence of CO2 fertilisation and of water stress in continental interiors, Geoscientific Model Development Discussions

General comments

This work continues and extends research previously published by Los in 2013. Its underpinning is the fact that current terrestrial models perform notably poorly in sim-

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Interactive Discussion

Discussion Paper

ulating either the spatial or temporal patterns of green vegetation cover; and the fact that there is now a long remotely sensed spectral reflectance record that can be used to derive NDVI, which is related to green vegetation cover – long enough for significant empirical relationships to be found between NDVI and climate. These relationships can be used to make both hindcasts for the pre-satellite era (as far back in time as we have harmonized climate data sets) and forecasts based on climate model scenarios of the future.

Los takes the view that these hindcasts and forecasts are more likely to be reliable than comparable predictions made with current process-based models, because of the latters' generally poor predictive skill. Moreover, the record is long enough to allow the detection of a systematic trend in the empirical relationships between NDVI and climate that can plausibly be attributed to the effect of rising atmospheric CO2 concentration. This was also done in Los' previous paper, but the present work is an advance in so far as the relationships found are general, applying throughout each of a small number of climatic zones.

It is good to see remotely sensed data being interrogated to answer important questions about the control of terrestrial biospheric activity. Such research may serve as antidote to excessive reliance in the current literature on models that still, to a large extent, rely on untested assumptions about the controls of biomass allocation and, therefore, the controls of foliage display and green vegetation cover, which in turn is a principal determinant of pirmary production. It is particularly helpful to have an empirically based estimate of the global effect of CO2 fertilization on vegetation cover, which is one aspect of the positive effect of rising CO2 concentration on plant productivity. This is more useful than relying on models which, in turn, rely on varying interpretations of limited experimental evidence.

Thus, my response to the basic idea behind this Discussion paper is positive. However, the methods adopted are suboptimal. I have the impression of a project executed somewhat in isolation from recent developments in carbon cycle science, rendering the results less useful and less definitive than they could have been. Deficiencies in the design also mean that there is little that process-based models can learn from the results. In rejecting process-based modelling, Los has "thrown out the baby with the bathwater": there are many problems with current models, but this paper as currently written is also free of explicit hypotheses (for example, about the magnitude and distribution of CO2 effects on green vegetation cover) which could have been derived based on sound ecophysiological theory. Deficiencies in the presentation also signal a failure to engage with current discussions in carbon cycle research. These problems could all be rectified, although a more felicitous design for the study would require some thought, and a good deal of new analysis to be done.

The framework adopted for the development of the empirical model is based on the Miami model for NPP, which represents NPP as the lesser of two functions, one based on annual temperature and one on annual precipitation. This is a problem, because it has been well understood since the 1990s (see Bonan 1993; also Zaks et al. 2007) that the apparent response of NPP to temperature across the globe is primarily an artefact of the spatial correlation between temperature and photosynthetically active radiation (PAR). Moreover, it has been known since Monteith's pioneering work with crops in the 1970s that PAR actually is a major control on NPP. Therefore, although temperature can undoubtedly affect NPP (positively or negatively), it seems perverse to build a model that confounds these effects with those of PAR.

It might be argued that temperature data are more abundant than PAR data, but I would not consider this a valid argument for substituting one for the other. Gridded sunshine or cloud fraction data are available, and allow PAR to be calculated reasonably accurately. The methods for doing so have also been available since the 1970s.

The analogy between NDVI and NPP is apt, but the reason is not explained here: namely, that NDVI depends on NPP, through the process of biomass production and subsequent carbon allocation to leaves, stems and roots. Thus, prediction of NDVI should be based on PAR as well as on climate.

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One conclusion of the paper is that the effects of precipitation on vegetation are underestimated by current process-based models. There is no discussion of the fact that this conclusion is opposite to that arrived at by Beer et al. (2010) and Piao et al. (2013), based on flux measurements and atmospheric CO_2 measurements.

The treatment of the CO_2 effect is limited. The apparent relationship between SR and the fitted CO_2 effect is presented, but not explained. It is not clear why the SR has been introduced into this analysis at all. Nothing is said about the expected interaction between the CO_2 effect and drought (because of the effect of CO_2 on stomatal conductance). The published finding by Donohue et al. (2013), of a CO_2-induced increase in green vegetation cover in warm and dry regions, is not cited. There is no attempt to show whether vegetation dominated by C4 plants has a different sensitivity from vegetation dominated by C3 plants, which might be predicted, as the CO_2-concentrating mechanism in C4 plants means that the CO_2 response of NPP (although not of stomatal conductance) is close to saturation.

The fitted model's performance with regard to interannual variability is poor by any standard. This requires a fuller discussion. The impression is given that this poor performance depends on the small signal, but I find this unconvincing because there are large signals in NDVI associated with smaller events than the epic droughts highlighted in the paper (for example, the recent Big Dry and Big Wet in Australia – phenomena large enough to susbtantially impact the global carbon balance: see Poulter et al. 2014). If both process-based and empirical, NDVI-based models are not able to capture more than a small fraction of the interannual variance in NDVI this might, surely, point to a common cause such as the failure of either approach to account for carry-over of non-structural carbohydrate reserves from one year to the next. At least, possible explanations should be mentioned.

Specific comments

p 4783 A stronger statement could be made, to the effect that process-based models

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across the board are not performing well when it comes to simulating the spatial and temporal patterns of green vegetation cover. More references should be cited here, including Kelley et al. (2013) which showed poor performance by LPJ and LPX in this respect, and Richardon et al. (2012) which showed remarkably poor performance across the North American Carbon Project models.

p 4788 It is not clear why segmented regression was chosen, as opposed to better known techniques such as LOcally Weighted Error Sum of Squares (lowess) or Generalized Additive Modelling (GAM). I am also dubious about the merit of using a method for statistical fitting that depends on random assignment of breakpoints. If a segmented approach is really in order (which I doubt, given the continuous nature of the relationships between NDVI and climate) then breakpoint regression, which estimates breakpoints based on the data, would be appropriate.

p 4798 mentions just one paper predicting future drying, while ignoring the considerable literature (and controversy) on this topic.

Fig. 1 appears to show that NPP is always temperature-limited!

Fig. 11 is hard to understand: what is the meaning of white bars on top of black bars? And what is the interpretation of significant positive correlations?

References

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