Tang and Bartlein Review

Major Comments

1. You need to state your rationale for doing this work. Why are you testing a modification of the LPJ DGVM with satellite inputs? Have you identified a particular weakness with the current use of plant functional types (PFTs) in LPJ, or a particular strength of using satellite inputs? Please mention this in the abstract and develop this reasoning fully in the methodology.

2. There are too few comparisons with previous, but similar work (e.g. Gerten *et al*., 2004 and Murray *et al*., 2011 for hydrology and major rivers). This would provide more context for how the model performs against previous versions and at the global scale.

Gerten, D., Schaphoff, S., Haberlandt, U., Lucht, W., and Sitch, S. 2004, Terrestrial vegetation and water balance – hydrological evaluation of a dynamic global vegetation model, *Journal of Hydrology*, 286, 249–270

Murray, S.J., Foster, P.N. and Prentice, I.C. 2011, Evaluation of global continental hydrology as simulated by the Land-surface Processes and eXchanges Dynamic Global Vegetation Model, *Hydrology and Earth System Sciences*, vol. 15, p.91-105

3. Study across a larger spatial extent (i.e. global) is required for this fairly radical approach to be accepted (in terms of accuracy and reliability) and detailed comparisons need to be made with other global scale studies for evaluative purposes (see above references for suggested hydrology papers to compare against).

4. Interesting and encouraging adjustments are made to the snowmelt regime, but what effect does this have on simulated hydrology? This could, for example, be over-riding or offsetting effects gained from the change in input structure, in cold environments.

5. Manuscript needs reading through and correcting, perhaps by a native English speaker, as there are many grammatical and spelling errors. I have corrected the most obvious ones, but my main focus has been on the science.

<u>Abstract</u>:

Line 1: Remove, or at least rephrase "are easier to grasp". This sounds too colloquial and, in the least, would need clarification.

Line 4: Change to "water balances".

Line 9: Rephrase "Towards these ends, we first introduced…".

Line 15+: Only relatively local/regional results are quoted in the abstract, for narrow durations. For example, values are quoted for the Everglades for 1996-2001, whereas the study captures the whole of the USA for 1982-2006. Such selectivity masks the general performance of the model. Please provide results relevant for the entire spatial domain and time period covered so that the model performance can be more reasonably judged in the abstract.

Line 15+: Please *explain* why the model succeeds in the regions you have tested it in. Assuming this is to do with the change in inputs, what in particular makes the satellite input more reliable than the PFT approach? In contrast, assuming the model performs less reliably outside of the highly selective space and time periods chosen (see previous comment), please provide (brief) details as to why this is.

Line 17: Add "data" after "observed".

Line 18: Remove "most".

Line 25: Change to "water balances" and change to "studying the effects".

Introduction – Page 1209

Line 13-16: There is an opportunity here to mention the importance of studying hydrology, in the context of global climate change and population growth.

Line 18: "that has its own hydrologic model" should be changed to "which also simulates hydrology". The end of this sentence is also missing some key citations. In addition to some of those already included in the manuscript, you could also add:

Müller, C., Bondeau, A., Lotze-Campen, H., Cramer, W., and Lucht, W.: Comparative impact of climatic and nonclimatic factors on global terrestrial carbon and water cycles, *Global Biogeochemical Cycles*, 20, GB4015, doi:10.1029/2006GB002742

Murray, S.J., Foster, P.N. and Prentice, I.C. 2011, Evaluation of global continental hydrology as simulated by the Land-surface Processes and eXchanges Dynamic Global Vegetation Model, *Hydrology and Earth System Sciences*, vol. 15, p.91-105

Rost, S., Gerten, D., Bondeau, A., Lucht, W., Rohwer, J., and Schaphoff, S.: 2008, Agricultural green and blue water consumption and its influence on the global water system, *Water Resources Research*, 44, W09405, doi:10.1029/2007WR006331.

Line 20+: This is a set of weak arguments and does not represent a convincing rationale for performing the study.

(a) For example, the fact that DGVMs simulate biogeochemical and ecological fluxes is of *benefit* to simulating hydrology, for exactly the reasons you have mentioned in the previous paragraph. Traditional hydrological models which do not include dynamic vegetation lack accuracy in their simulation of water at the land surface. The 'target-audience' argument is very weak (please also amend the relevant parts of the discussion).

(b) The parameterization of PFTs is indeed a challenge and the most convincing element of your argument. However, it is at present unsubstantiated by citations. What evidence do you have for this difficulty, and how (un)successful are our attempts at parameterizing vegetation to date? This needs probing much more deeply. There are also studies which advocate the benefits and further refinement of PFTs (e.g. Brovkin *et al*., 2012); this would provide more balance to the literature review.

Brovkin, V., P. M. van Bodegom, T. Kleinen, C. Wirth, W. K. Cornwell, J. H. C. Cornelissen, and J. Kattge, Plant-driven variation in decomposition rates improves projections of global litter stock distribution, *Biogeosciences*, 9, 565-576, 2012

(c) Technology has advanced to the extent whereby running such models is not a limiting factor to their use. Of course we would like to further speed up these simulation runs, but I believe this is a trivial point which should be removed (including in the discussion).

(d) Is there proof that the exclusion of local-scale processes, particularly seed dispersal, are of critical importance to improving the simulation of the *large scale* water balance. LPJ includes numerous algorithms for simulating plant competition, cessation, growth and spread. Are these forefront causes of inaccurate hydrology which constitute a new approach to the data input of DGVMs? If so, then by all means provide evidence for this. My feeling however is that there are many more important areas which require improvement to better the simulation of hydrology in LPJ. For example, better treatment of permafrost/glaciers, interception fluxes and groundwater storage and transfer, incorporation of river routing, to give a few examples. The model also does not currently estimate water withdrawals. These are surely of much greater importance for improving large-scale water dynamics on a 2.5° grid (i.e. relatively coarse spatial resolution) than local-scale dispersal of seeds.

Line 21: Missing "the" before "land".

<u>Page 1210</u>

Lines 3-17: Again, I am afraid that this line of reasoning does not stand up to scientific rigor. While I am all for the use of satellite data in land surface modeling, the argument that reducing the complexity of DGVMs by substituting modeled vegetation for prescribed vegetation in an attempt to target a non-ecologists who are interested in hydrology, does not stand up. In addition, the LPJ DGVM is in fact *not* complex relative to many other land surface models (see papers by Sitch *et al*., 2003 and Gerten *et al*., 2004, who often state that the model is of "intermediate complexity"). There are many advocates of PFT-refinement (e.g. Brovkin *et al*., 2012 to give one of many examples) whose work require *critical evaluation* in order for a more convincing and balanced argument to be presented.

Line 14: "satellite-based land covers are often thought of high accuracy in representing the land characteristics" – this is quite a bold, but unsubstantiated claim. You need to provide evidence for this, as it presumably forms the lynchpin of your study.

Line 15-17: “In fact, satellite-based data have been widely used in modeling the land surface water balance (e.g., Glenn et al., 2007; Song et al., 2000).” This is a better statement of evidence, but you need to take it to the next level in order for it to be convincing for the reader. To what extent did the satellite data improve the simulated hydrology? What approach did these studies take? How did they incorporate the data into their models? Did they identify any particular strengths/weaknesses/areas for improvement or further research?

Line 21: Better to use “conterminous” throughout. Please also rephrase “towards these ends” throughout your manuscript.

Line 23+: Some of this text is more suited to the methodology section.

Section 2.1 (Page 1211):

In general, the methodology section is written much more coherently and precisely.

However, a major concern arises in this section. One of the arguments previously presented for the use of satellite data is that former LPJ versions require parameterization of vegetation features and processes. Yet the suggested approach does not circumvent this problem. Little end-benefit is gained in terms of the land cover classification scheme used, as former LPJ versions also tend to use that of Hansen *et al*. (2000) and subsequently generate ~10 land cover groups. If the end-product at this stage was a radically different (but accurate) global land cover map to what has been previously used in similar studies, then this might constitute the use of this alternative approach. At the moment however, this approach comes across as ‘reinventing the wheel’.

Why did you study the United States in particular? This needs to be stated. However, current science in this discipline is increasingly moving towards global scale studies of environmental fluxes (see all manner of recent publications on LPJ and the LPX DGVM). A manuscript which advocates a major change to the way inputs are treated in DGVMs would surely need to provide evidence of accuracy and reliability in approach across the globe.

<u>Section 2.2</u> – this section seems reasonable and is well written. However, it needs to be made clear exactly how the approach (and more specifically, algorithms used), differ from those in Sitch *et al*. (2003) and what the perceived benefits of these changes are.

<u>Section 2.3</u> – again, fine in theory and a very interesting development of the snowmelt algorithm. Results need to be subsequently shown to highlight the difference this particular development has on hydrology simulations, particularly in cold environments.

<u>Section 2.4</u>

Line 13: Do you mean “Table 2” here?

Line 23+: This is a very long sentence – please split this into two sentences.

Section 2.5

Table 4: Remove the decimal values in the “drainage area” column.

Section 2.6

Lots of grammatical errors and rephrasing needed in this section.

Section 3.1

Page 1220, Line 7-16: This paragraph is very wordy and descriptive and should instead be tabulated. In fact, the following paragraph could also be condensed into a table.

This section is highly descriptive and requires detailed explanation. Why, for example, were the ET values so different in the three rivers identified? Please provide a more thorough analysis throughout these results (including section 3.2, where output data analysis and explanations are also lacking).

Section 3.3

As runoff is a key indicator of hydrological regime, I would expect to see a much deeper analysis of the results generated. At present there is a lot of description and not much in the way of insight. Why, for example, does the model work well for some regions/rivers, but not others? In particular, I would expect to see a *much* deeper analysis of correlations between river flows and land cover, especially given the nature of the paper’s aims and objectives.

Significant improvements to the readability of the results (including section 3.4) would be achieved by replacing the text with tabulated versions of the findings.

Page 1222 - Line 20: Spelling error: replace with “annual”.

Page 1323: Again, most of these results should be tabulated for brevity.

Line 8-9: This needs to be rephrased to avoid stating the obvious.

There is no mention of how changes to the ice-melt regime has impacted on simulated runoff. Please demonstrate this.

Figures 2 – 9 (and accompanying supplementary figures): Please amend these so that they have more sensible y-axis values and intervals.

Discussion

Page 1226 – Line 22-24: “Although LH incorporates static land covers, rather than dynamically simulating them, it is able to simulate the land surface water balance as well as its predecessor (Gerten et al., 2004)". There is a serious problem with this statement which undermines the entire study. Firstly, there is very little mention or comparison with the Gerten study; this needs to be addressed. But more importantly, if the LH model only simulates the water balance "as well as" the former model version, then this does not demonstrate progress and more importantly, does not convince the reader that the proposed technique should be adopted. This statement is also far too broad in the context of the comparisons presented – in some regions the model performs well, but in other regions the simulations require addressing and justification.

Page 1227 – line 9-11: "The root causes is LH considers effects of both temperature and solar radiation on snowmelt while the DGVM considers only effects of temperature on snowmelt." This may well be the case, but has not been demonstrated in the work. Please also note the grammatical error.

Page 1228 – lines 8-29+: This material is more suited to the introduction.

Based on the evidence presented in the study, I am still to be convinced by the discussion that this approach is ready to be used as an alternative to dynamic simulation of vegetation.

The discussion will also need re-writing to incorporate the exploration of comparisons to LPJ / LPX at the global scale. A robust comparison will favor the consideration of this alternative approach.

Conclusions

The conclusions section needs re-writing in line with the new work to be done and to remove the unfounded statements in the manuscript.