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Interactive comment on "Modifying a dynamic global vegetation model for simulating large spatial scale land surface water balance" *by* G. Tang and P. J. Bartlein

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Dear Referee,

We appreciate the valuable comments from you. We addressed each of your comments in the revised manuscript. Our responses to your comments are listed below in italics following each specific comment.

We also appreciate your helpful suggestions. If you have any further suggestions for changes, please let us know.

Sincerely,

Guoping Tang, Ph.D. Assistant Research Professor Division of Earth and Ecosystem Sciences Desert Research Institute Reno, NV 89512

Anonymous Referee #1 Received and published: 11 February 2012 Dear Authors, Thank you for your submission. Although it is not ready for publication yet, I believe that with a significant overhaul of the text (including a more robust justification of the methodology) and additional comparisons to previous work at the global scale, it may then be reconsidered for re-review. I wish you the best of luck in re-working this interesting article and look forward to what I am sure will be a significantly improved contribution. Please also note the supplement to this comment:

Response: In the revised manuscript, we made the following changes: First, we almost re-wrote the whole introduction section to support the rationality for developing the LH model, including the advantages of using satellite-based data for simulating land surface water balances. Second, additional simulation using LH for the entire world was run. The model results were evaluated using observed discharges for ten large rivers worldwide. We also compared LH-modeled surface runoff at the global scale with previous published data like Gerten et al. (2004) or Cogley (1998). These comparisons and evaluations still suggested that LH performs well at the global scale. Third, we highlighted in the revised manuscript that the addition of solar radiation for snowmelt computation greatly improved LH's estimate of stream flow for rivers located at mid-to-high latitudes. Gerten et al. (2004) suggested the LPJ-DGVM tended to underestimate the surface runoff at the high latitudes of the Northern Hemisphere. With the addition of radiation effect on snowmelt, LH modeled very well the annual discharges for the Mackenzie River in the North America and the Yenisei River in Russia. Fourth, the Discussion section in the revised manuscript was revised to match closely the contents in Results section. The conclusion section was rewritten to conclude the main findings from this study. Fifth, all figures were re-plotted for readability and the whole manuscript was edited by an English editor.

Rephrase "Towards these ends, we first introduced. . .".

Response: Revised throughout the manuscript.

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.

Response: We are lack of validation data for the whole United States and the whole research period. This is especially true for observed soil moisture and ET data. It is why we used local or regional (smaller than the US) observed soil moisture and ET data in the model's evaluation. In fact, the 12 major river watersheds cover most of the conterminous United States. The Vorosmarty et al. (1998) ET and the composite runoff data from Fekete et al. (1999) used to compare the spatial patterns of LH-modeled ET and surface runoff cover the whole conterminous United States. To assure the reliability of LH, we ran LH for the entire world in the revised manuscript as you suggested. Further evaluations on LH-simulated runoff at the global scale and for ten large rivers worldwide are made. These additional evaluations still demonstrated that LH is able to correctly simulate land surface water balances.

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.

Response: We mentioned in the abstract and introduction section that satellite-based data often provide distributed information about land characteristic in a study region, which can be a major reason why LH performs well in the conterminous United States. In addition, the reduction of model parameters makes LH easier to parameterize in

practice, which also contributed to the model's accuracy. Third, we discussed in the Discussion section that the accuracy of input data such as land cover, soil, and climate data all may contribute to the model's accuracy in the conterminous United States. Nevertheless, additional efforts are still needed to examine how land cover changes across space and time affect LH's performance, which will be part of our future work.

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

Response: Added.

Line 18: Remove "most".

Response: deleted most.

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

Response: Changed throughout the revised manuscript.

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.

Response: We mentioned here that "global climate change" is expected to intensify the global hydrologic cycle.

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.

Response: We appreciate your suggestions and cited most papers you suggested here. We revised this paragraph in the revised manuscript.

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

Response: We provided more evidences to support the value of incorporating satellite-based data into a DGVM for simulating land surface water balances. These advantages include the simplification of model structure, the reduction of model parameterization, and the contribution of model prediction because satellite data can provides high resolution distributed information about the land characteristics. We cited several papers to support above statements.

(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).

Response: We deleted this statement in the revised manuscript.

(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.

Response: We revised the text and listed several aspects in the conclusion section that may need to be improved in the LH's future application, including the consideration of human water withdrawal from river, the simulation of water routing among grid cells, and the inclusion of other meteorological factors. In addition, we revised this paragraph and discussed the potential limitations of using DGVM to simulate land surface water balances.

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

Response: Added "The".

Page 1210 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.

Response: As we stated before, we revised this paragraph to highlight the advantages for incorporating satellite-based data into LPJ-DGVM for simulating regional scale land surface water balances. Such advantage includes (i) the simplification of model structure, (ii) the reduction of model parameterization task, and (iii) the contribution of the reliability of model results because satellite-based data provide distribution information about land characteristics. We cited several papers to support the rationality of incorporating satellite data into a DGVM for simulating land surface water balances. In addition, we discussed limitations of using LPJ-DGVM to simulate land surface water balances.

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.

Response: We cited several papers to support this statement in the revised manuscript.

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?

Response: We cited several papers to support this statement and discussed the advantages of using satellite-based data in hydrologic models.

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

Response: We changed "coterminous" into "conterminous" throughout the revised manuscript. We also changed "toward these ends" into "towards these ends".

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

Response: We deleted these sentences and rewrote this paragraph according to another reviewer's comments.

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 $\sim$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'.

Response: It is true that LH still models plant photosynthesis and thus requires parameterizing some vegetation-related parameters. However, LH does not simulate the biogeochemical cycle of carbon flux in the vegetation and soil systems. As a result, vegetation-related parameters were greatly reduced although not totally excluded. Due to our limited knowledge, we are not sure if an earlier version of LPJ-DGVM has been designed to run using prescribed land cover types. To our understanding, as a DGVM, LPJ simulates vegetation or land covers in a cell. In addition, the development of LH in this study aims to simulate land surface water balances instead of vegetation. If similar work has been done before by using or modifying LPJ-DGVM, we are sorry to hear that.

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.

Response: We selected the United States as a study region largely because the US has relatively rich data for testing LH at the regional scale. Theoretically, we hypothesized that the well-evaluated hydrologic component of LPJ-DGVM at the global scale will still perform well in a region like the conterminous US. We evaluated LH's performance at the global scale in the revised manuscript.

Section 2.2 – 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.

Response: Thanks we revised this section as best as we can.

Section 2.3 – 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.

Response: We added one figure to show that LH-simulated stream flow in winter and early spring under the addition of radiation effects on snowmelt was much closer than

a degree-day method to the observed values. We also discussed why the addition of radiation caused LH to better estimate river stream flow at mid-to-high latitudes.

Section 2.4 Line 13: Do you mean "Table 2" here?

Response: We revised "Table 1" into "Table 2"

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

Response: We revised.

Section 2.5 Table 4: Remove the decimal values in the "drainage area" column.

Response: We removed.

Section 2.6 Lots of grammatical errors and rephrasing needed in this section.

Response: We asked an English editor to have edited the whole manuscript.

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).

Response: We tabulated the results introduced in section 3.1 as possible as we can for enhancing the readability. We discussed the difference between LH-simulated and Vörösmarty ET data in "Discussion" section in the revised manuscript. Overall, we discussed more thoroughly about the model results in the "Discussion" section in the revised manuscript.

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.

Response: We made deeper analysis of model results in the "Discussion" section in the revised manuscript. We removed Section 3.4 from the revised manuscript, largely because LPJ-DGVM was not parameterized for simulating terrestrial natural vegetation in the conterminous United States. Because of the model's parameterization, we suspect it is inappropriate to compare LPJ-DGVM and LH simulated water balances in the conterminous United States. To compensate for this, as you suggested, we added model simulation at the global scale and evaluated LH-simulated surface runoff and river discharges for 10 large rivers worldwide. In addition, we tabulated when appropriate the model results for improving the readability. For example, we deleted the text included in the parenthesis as possible as we can.

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

Response: Revised.

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

Response: We added the calculated R-squared and Nash-Sutcliff coefficient in Table 6 for enhancing the readability of this paragraph. We also revised corresponding paragraphs.

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.

Response: We added one figure to show that the addition of radiation for snowmelt computation greatly improved estimates of stream flow in winter and early spring. We

put this figure and related results in the "Results" section in the revised manuscript.

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

Response: Figures are re-plotted for readability.

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.

Response: We agree that such statement can underline the importance of the whole study. So, we deleted this sentence in the revised manuscript. We stated in that way in the former manuscript because the core hydrologic component of LH is the same as that in LPJ-DGVM. We do not want to mislead the reader that LH is better than LPJ-DGVM in simulating land surface water balances. Differing from LPJ-DGVM, LH prescribed land covers. In addition, the addition of solar radiation for snowmelt computation greatly improved estimates of monthly stream flow in winter and early spring. The hydrologic evaluation of LH at the global scale also justified the advantage of incorporation of radiation effect for snowmelt computation. In the revised manuscript, we discussed why LH did not perform well in some regions such as in arid environment.

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.

Response: We added one figure to demonstrate that the addition of solar radiation effect on snowmelt enabled LH to better simulate monthly stream flow in winter and early spring than simulation without considering solar radiation. The application of LH globally also suggested that LH simulated well annual discharges for the Mackenzie and Yenisei rivers, two rivers located at high latitudes of the Northern Hemisphere.

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.

Response: Our initial purpose for developing the stand-alone LH model is to take full advantage of the hydrologic component of LPJ-DGVM for regional scale water balance study. We also assumed that the well-evaluated hydrology component of LPJ-DGVM will still perform well at regional scale study. Therefore, making the hydrologic component of LPJ-DGVM accessible to general users is our initial and practical purpose. We agree that LPJ-DGVM is an intermediate level complex model. However, it still could be challenging for users who are not ecologists or vegetation modeler to use it for hydrology study. Nevertheless, we sincerely appreciate that you made valuable comments from the scientific standing point. We followed your suggestion to run LH at the global scale, and to further evaluate LH's reliability. Our additional evaluation at the global scale still indicated that LH developed in this study is reliable, largely because the core hydrologic model is almost the same as that in LPJ-DGVM.

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.

Response: We revised the whole conclusion section and concluded the main findings from this study in the revised manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C916/2012/hessd-9-C916-2012-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1207, 2012.