**Hydrology and
Earth System
Sciences
Discussions**

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 reviewer,

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,

C937

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Anonymous Referee #3 Received and published: 20 March 2012 General comments This article describes the application of a modified dynamic global vegetation model to simulate the hydrology on a large scale. The hydrology component of such a model is used and, to avoid long model run times, the vegetation cover is derived from satellite data instead of being calculated by the model itself. To assess the validity of this approach, important hydrological quantities, such as soil moisture, evapotranspiration and surface runoff, are compared to existing measured and simulated data. The results obtained are promising and show the ability of the presented model to accurately simulate ET, soil moisture and discharge.

Interactive Comment The article in its present form does not address important issues, which should be examined in greater detail. Proof reading by a native english speaker could improve the quality of the language. Long sentences with additional information given in parantheses often hamper the readability of the article. Some parts of the article should be rewritten into shorter sentences. This would allow the reader to understand the article more easily.

Response: We carefully revised the manuscript to address each of your comments. We asked an English proposal editor to have edited the whole manuscript.

In the introduction the advantages of using such a model should be stated more precisely. Despite the reduced complexity of the model it is not clear what the advantages of such a model are, compared to the original model, but also compared to other global hydrological models, such as WaterGAP by Döll et al. (2003) or similar models. At the end of the introduction you should provide a short overview over the structure of the article, including section numbers.

Response: In the revised manuscript, we discussed the advantages of incorporating satellite-based data into a DGVM for simulating land surface water balances. These

advantages include (i) the simplification of model structure, (ii) the reduction of uncertainty resulting from model parameterization, and (iii) the contribution to the reliability of model prediction. In addition, we discussed the limitations of DGVM in simulating land surface water balances, including the ignorance of water movement and their spatial connectivity among simulated grid cells, the generalization of plants as a few plant functional types, and the challenge of model parameterization. At the end of introduction, we provided a short review over the structure of the revised manuscript. We agree that it is valuable to compare our suggested model with LPJ-DGVM or other global hydrological model. However, since our current study mainly focused on modifying LPJ-DGVM for simulating land surface water balances and evaluating the modified model, we did not compare in detail the differences between LH and other hydrologic models. Such comparisons among different models require additional efforts, which will be part of our future work. Nevertheless, we discussed some limitations of our modified model such as ignorance of water routing and human consumption that were considered in WaterGAP (Döll et al. 2003). We cited Döll et al. (2003) in the revised manuscript.

The description of the methodology should be more precise. Most of the readers of this article will be hydrologists, thus the most important parts of the model need a better explanation. Especially the processes modelled in the vegetation water balance / photosynthesis module and soil water balance model should be described in more detail. This would help the reader to better understand which parameters are used for which process.

Response: We did our best to revise this section and made it more readable to general users.

I also suggest to rearrange the contents of Section 2 to improve clarity. The layout of the sections could be organized as follows or similar: 2. Methodology 2.1 The LH model 2.2 Vegetation water balance 2.3 Soil water balance 2.4 Reference methods and data 3. Data 3.1 Land cover and soil properties 3.2 Meteorological data

According to this structure Page 1211, lines 3 – 12 would belong to section 2.1. Lines 13 – 23 belong to section 3.1 and Page 1211, lines 24 – 28 belong to section 3.2. The other sections can be divided accordingly.

Response: We followed your suggestion and reorganized the Section 2 in the previous manuscript into Section 2 (Methodology) and Section 3 (Data) in the revised manuscript.

Additionally, specific contributions of the author and the improvements made to the model are not distinguished clearly from the work done by others. A concise description of the original model and a more detailed description of the improvements made to the model would foster the readers insight.

Response: The core hydrologic component of LH is almost the same as that in LPJ-DGVM (version 1.2). Compared to LPJ-DGVM, which simulates terrestrial vegetation, LH predefines vegetation/land covers in a study region. As a result, LH excluded sub-models in LPJ-DGVM that are used to simulate sapling establishment, the allocation of carbon among different compartments, and the competition of vegetation for light, water and nutrients, as well as soil organic carbon decompositions. In addition, LPJ-DGVM (version 1.2) uses a degree-day method for snowmelt computation. LH considered the effects of both temperature and solar radiation on snowmelt. These are major differences. In Section 2, we clearly stated that LH considered the effects of both temperature and solar radiation on snowmelt. Due to above changes, the model structure of LH is greatly simplified and different from LPJ-DGVM for simulating land surface water balances.

It is also not clearly visible from the model description that the model runs on a monthly time step.

Response: As LPJ-DGVM, all model input data are in monthly- step. The monthly-step climate data are further interpolated into quasi daily values in the model's run time. The simulated daily values of hydrological variables were aggregated in the model into

monthly values. We stated this in the revised manuscript for clarification.

Results: It is hard to keep track of the different parameters, with which the simulated data ET and soil moisture are compared to the observed data. I would recommend you to collect the parameters calculated in a table. This would help the reader to understand your comparison.

Response: In the revised manuscript, we tabulated some paragraphs as possible as we can to enhance the readability of this section. In addition, we deleted those texts included in the parentheses to enhance the readability. An English editor edited the whole manuscript.

The structure of the discussion (Section 4) should strictly follow the one in Section 3, followed by more general observations and insights.

Response: We revised and reorganized this section such that its contents correspond well to those in Section 3 (i.e., the Results section).

You state on Page 1226, line 23 that the LH model incorporates static land cover but no dynamic simulation. A static landcover could still be used in a dynamic model, which simulates the seasonal changes of the vegetation cycle. It is not clear to me if such dynamics are part of the original DGVM or not. If they are simulated in the DGVM, why are they not used in LH?

Response: Yes, prescribed land covers can still be used in a DGVM to model the seasonal changes of vegetation cycle, such as the growth of plants. LH only simulates the phenology dynamic of vegetation, specifically for summer green vegetation. The simulation of vegetation phenology in LH (e.g. when leaf tends to on and off) is the same as that in LPJ-DGVM on the basis of air temperature. Since LH mainly aims to simulate land surface water balances, other seasonal changes in vegetation cycle that are unimportant or irrelevant to surface hydrology simulation are excluded in LH.

According to Page 1227, line 19 you observe that the simulation in the Sacramento river

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basin is not particularly successful due to the intensive use of water. It is well known that global hydrological models have difficulties to cope with river basins featuring large reservoirs. Why was the Sacramento river still included in the comparison? A few lines later, on line 23, you state based on the poor fit between simulation and observation in same river basin, that the discretisation of the soil layer is not sufficient. I think this statement is not valid, considering the main reasons for the poor fit.

Response: We agree that large reservoirs have an important role in shifting river's hydrography. Before we test the LH-simulated stream flow for the Sacramento River, we have no idea about the specific condition of the Sacramento River watershed. We do not know in advance how LH will perform in this watershed. It is why we still selected this river/watershed as a testing watershed. The discrepancy here provided us more insights into the LH's behavior in watersheds that are highly interrupted by human activities. Later, we stated that the withdrawal of water from the Sacramento River is responsible for such a poor fit between LH-simulated and observed river stream flow in the Sacramento River watershed. Followed your suggestion, we removed soil layer as a major causes for such a poor fit.

Also you state that the vegetation distribution of the LPJ-DGVM is not necessarily the same as the one derived from the GLC data. How does this influence the model results?

Response: Another reviewer suggested us to run LH at the global scale for further evaluation on LH's reliability. To address this comment, we added model evaluation at the global scale using observed river discharges for ten large rivers worldwide. As a result, we removed the comparisons between LH and LPJ-DGVM simulated data from the revised manuscript. Besides, we think it may be inappropriate to compare LH and LPJ-DGVM simulated three hydrologic variables in the conterminous U.S. largely because we keep all parameters that are same between two model simulations identical in the previous manuscript. In other words, LPJ-DGVM was not parameterized correctly to simulate vegetation and hydrology for the U.S. The parameterizations of

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model parameters are always challenging. As far as how vegetation/land covers affect simulated runoff, we believe that additional simulations based on the same model but different land cover inputs should be performed and analyzed, which will be our future efforts.

On Page 1228, line 8, you write that some improvements need to be done to the model. Which of the simulated processes or model parameters need special attention and why?

Response: We listed some limitations that need improvement in the LH's future application in the revised manuscript. Such improvements include, for example, consideration of human withdrawal of water, the routing of water among simulated units, and effects of other meteorological factor (e.g., vapor pressure deficit) on evapotranspiration.

You were running simulations using a different atmospheric CO_2 concentration. These simulations should also be described in the methodology section.

Response: We added one paragraph in the Data and Method section to describe our experimental simulations.

On Page 1229, line 24 you draw the conclusion that the model presented in this article is useful to assess the effects of land cover changes. The same statement can be found in the first few sentences of the abstract and at the end of the introduction. However, this model is only capable of incorporating static land cover. How exactly could it be applied to assess natural or antropogenic land cover changes?

Response: We revised the Abstract and Conclusion sections in the revised manuscript. It is inappropriate to make such statements that were not tested in our previous manuscript. Technically, as for other hydrologic modes, LH can be run under different land cover and climate scenarios. Results from different scenarios can be compared to test how changes in land covers and climate affect land surface water balances. We appreciate your comments.

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Page 1210, line 14 The sentence "(. . .) because satellite-based land covers are often thought of high accuracy in representing the land characteristics." is too general. The accuracy of such data depends on the raw data, which are uses as well as on the sophistication of the processing. I agree that the GLC dataset used in this article is considered to be of high quality.

Response: We revised this paragraph and cited several papers to support why satellite-based data are valuable in modeling land surface water balances.

Page 1212, line 12 The "minimum water scalar value" should be defined properly. Is it equivalent to Wmin?

Response: W_min stands for minimum water scale value here. We revised it.

Page 1212, line 16 How is the leaf area calculated from the leaf longevity. Is the leaf area equivalent to the leaf area index (LAI) which is used later?

Response: We changed "leaf area" into "specific leaf area", which does not equal leaf area index. The calculation of specific leaf area in LH is the same as that in LPJ-DGVM. It is based on Raich et al. (1998) and calculated as follows: SLA=0.0002×eˆ((6.15-0.46 logâĄąãĂŰ(LeL×12.0))ãĂŮ) where LeL refers to Leaf longevity and SLA (m2 g-1) refers to specific leaf area. We cited Raich et al. (1998) in the revised manuscript.

Page 1212, line 19 Equation (1): The parameters used in the formula calculating the relative soil moisture are not well described. w1 and w2 are the fraction of available water in each layer (i.e. the volume of available water divided by the volume of drainable pores of the soil). In my understanding, roots usually diminish the drainable porosity. Thus, the relative soil moisture in soil, excluding the roots, would be w nôĂĂĂf , where n is the drainable porosity. If the meaning of wr is different, please add a proper definition.

Additionally, I suspect that you add two intensive properties of the soil. Please clarify the meaning of the single variables and explain why you simply add the relative water content of two layers. As the equation presents itself now, wr could take a value greater

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than 1 under certain circumstances. This contradicts the definition in the text (line 18).

Response: As in LPJ-DGVM, w_1 and w_2, the fractions of available water in the upper and lower soil layers are dimensionless and must be less than 1.0, respectively. They are defined as the ratios of actual water in two soil layers to soil field capacity of these two layers. f1 and f2 are fractions of roots distribution in the two layers. The summation of f1 and f2 always equals one. Since both w_1 and w2 are less than one, the relative soil moisture (wr) in the whole soil layers is less than one. wr is used to calculate daily evaporation of soil (see equation 8). We added "f1+f2=1" in the proper place for clarification.

It is also not clear to me which values are used for f2. In Table 1 only the values for f1 are indicated.

Response: The fraction of roots in bottom soil layer (f2) always equals one minus the fraction of roots in the upper soil layer (f1). In both LH and LPJ-DGVM, f1+f2=1. We mentioned it in the footnotes of Table 1.

Page 1218, line 8 and 9 According to this, the discharges simulated by the LH model are converted into m3 sôĂĂĂ1. In the results section most of the data is still indicated in mm.

Response: Yes, we only converted LH-simulated monthly surface runoff (mm) into m3 s-1 when compared to the USGS observed flow stream data. The rest of data are still in millimeters.

Page 1218, line 13 Equation (11): Add the corresponding indices to the variable srf (srfi;j).

Response: We appreciate your suggestion and revised it.

Page 1218, line 21 to 25 This sentence is too long and should be split in one describing the combination of the two river basins to the "Alabama River" and one on the Nash-Sutcliffe coefficient. Generally, the latter is well known among hydrologists and does

not necessarily need to be indicated here.

Response: We revised it and deleted the definition of "the Nash-Sutcliffe coefficient".

Page 1222, line 9 – 11 "Nevertheless. . . ": This sentence is not very clear and needs reformulation.

"The simulated soil moisture in this region shows a higher variability compared to the observed data."

Response: We revised.

Page 1222, line 12 Units are missing: 29 mm.

Response: Thanks and we added the unit.

Page 1226, line 9 Replace "aerodynamic" by "meteorological".

Response: Thanks and we replaced "aerodynamic" by "meteorological" in the revised manuscript.

Page 1226, line 12 – 16 "Actual ET is considered. . . ": This sentence is too long and not clear. Split it into smaller parts. "Actual ET increases with an increasing water vapour pressure deficit. This explains the differences between ET simulated with the LH model and the values found by Vörösmarty et al. (1998) in the (. . .) river basins. While the latter considered the influence of the water vapor pressure on the land surface hydrology, this is not done with the LH model."

Response: We revised these sentences in the revised manuscript.

Figure 4 The sudden phase shift of the simulated to the observed ET in April 98 should also be mentioned in the text (Section 3.1).

Response: We mentioned it in the Results section of the revised manuscript.

Technical corrections âĂć The original model used (LPJ-DGVM) should always be identified by the same name. It often is referred to as "the predecessor". Thus, it

is not always very clear which model is meant. I would also suggest to omit the short form of it, as it is defined in section 2.6.

Response: We used LPJ-DGVM throughout the revised manuscript.

âẮ The typesetting of the formulas could be enhanced to improve the readability:

– Equation (2) could be written as a fraction to omit the parentheses.

Response: We revised.

– Equation (5): Omit the brackets.

Response: We deleted the brackets as appropriate.

âẮ Use the variable names with the proper indices in the same manner throughout the document.

– Use Emax instead of Emax, as it refers to the same quantity as Ep and Eeq

Response: We changed Emax into E_max throughout the whole manuscript.

– Also use indices instead of long variable names, if possible. Use long variable names only for well known expressions, such as LAI (leaf area index).

– The foliar vegetative cover is identified as FVC in Figure 1 and as fvc in Equation (3).

Response: We changed FVC in Figure 1 into fvc as in equation (3).

– The variables w1 and w2 are used in Equation (1) and in Equation (9), but do not have the same meaning nor units.

Response: We changed the variables w1 and w2 in equation (9) into ãĂŰSWãĂŮ_(1,i) and ãĂŰSWãĂŮ_(2,i) , respectively.

âẮ Table 1: Add f2 for the second soil layer.

Response: We added in the table footnotes that the fraction of roots in the bottom soil

layer equals one minus the fraction in the upper soil layer.

âẮ Table 5: Add the Nash-Sutcliffe efficiency, the RMSE and R2 to the table. In contrary the standard deviations can be removed.

Response: We added the calculated R-squared and the Nash-Sutcliffe statistics in Table 5.

âẮ Figure 2, 5 and 7: The indicated coordinates of the x and y-axis should be rounded to whole numbers and positioned accordingly.

Response: We re-plotted these figures to enhance readability in the revised manuscript.

âẮ Figure 4 and 6: Set the ticks of the x axis to the first of January of each year. Use the same font throughout the image.

Response: We re-plotted two figures to enhance the readability in the revised manuscript. Note: for panel B in Figure 6, observed data always starts from April of a year due to data missing.

âẮ Figure 3, 4, 6, 8 and 9: Set the ticks of the y axis to more "regular" values (e.g. 100, 120, 140, . . . instead of 102, 119, 135, . . .).

Response: We re-plotted these Figures in the revised manuscript.

âẮ Figure 4, 6 and 9: The gray used for the observation is too light. Use a black dashed line instead.

Response: We recreated those Figures to enhance the readability.

âẮ Figure 8 and 9: The single plots are too small. Please select the most important ones for plotting and use a table to indicate the performance parameters calculated in all river basins.

Response: We enlarged the whole Figure but still put all panels together for compari-

son.

References Döll, P.; Kaspar, F.; Lehner, B.: A Global Hydrological Model for Deriving Water Availability Indicators: Model Tuning and Validation Journal of Hydrology, 270 (1-2) 105-134, 2003

Response: We cited this paper in the revised manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C937/2012/hessd-9-C937-2012-supplement.pdf

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