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,

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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

Anonymous Referee #2
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Major comments:
(1) It is not very clear in the manuscript what does "using the static land covers into DGVMs" (e.g. pg. 1210, line 8) actually mean. Does it mean that one land cover map is used for the entire simulation period? I would think that the idea is to use a time series of land covers at certainly temporal interval. If this is the case, then the term "static" land cover does not seem to me appropriate.

Response: We appreciate your good comments and sorry for the vagueness of static land covers. We revised it as prescribed land covers in the revised manuscript. In our study, satellite-based land covers are used to predefine the land characteristics for the entire study period. Because LH prescribed land covers, time-series land cover data at different intervals still can be used directly as model input to test how changes in land covers affect land surface hydrology. As the development of remote sensing techniques and subsequent data, we believe such type of models will become more contributive to understanding the dynamics of land surface hydrology under changing land cover and climate condition.

(2) If the land cover map used is really "static" (i.e. one map for the entire period), one may expect that a major difference between the LH and LJP-DGVM could be on the ability of the models in representing interannual and monthly/seasonal variability of the water balance components. So, the authors should discuss this aspect in comparing the results with particular reference to modeling the impact of climate and land use

change.

Response: One reviewer suggested that further evaluation of LH at the global scale must be added for justifying the reliability of LH. As a result, we added LH's evaluation globally. Such evaluation included comparison between LH-simulated and observed 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 other words, LPJ-DGVM was not be parameterized correctly for the U.S., which greatly affected LPJ-DGVM's ability to simulate terrestrial vegetation and further land surface hydrology in the U.S. The parameterizations of model's parameters are always challenging. As far as how vegetation/land covers affects simulated runoff, we believe that additional simulations using the same model but different land cover inputs should be performed and analyzed, which will be part of our future efforts. We are sorry we made some statements that were not tested in the previous manuscript.

(3) Equation (10) seems to me like a degree-day method except for the addition of a radiation term. But I find it unusual that the term Pr is included in the equation. Please elaborate. The manuscript should also discuss how the values of c1 and c2 are obtained and specify their typical ranges.

Response: Yes, we added the effects of solar radiation to the degree-day method in LPJ-DGVM for snowmelt computation. We carefully checked both the equation and an earlier published paper (Gerten et al. 2004, Journal of Hydrology). Daily precipitation is used in the degree-day method of LPJ-DGVM for calculating snowmelt. We added the possible ranges for the two empirical coefficients (i.e., c1 and c2) in the revised manuscript.

(4) Most, if not all, equations in the manuscript are defined for daily input. But the climatic input data described in Sec. 2.4 (pg. 1215) are in monthly time step. It is not very clear if the interpolation discussed in the same section is also for temporal disaggregation.

Response: Both LH and LPJ-DGVM simulate land surface hydrology at daily-step. It is why most equations in our manuscript are defined at daily-step. Presently, monthly-step climate data are mainly used in LPJ-DGVM as inputs to drive the model. These monthly-step climate data are further interpolated in the model's run time into quasi daily values for hydrology and other simulations. At its current stage, the design of LH followed LPJ-DGVM. So, LH also interpolates monthly-step inputs into daily values during its run time. The interpolation approach introduced in the previous manuscript was only used to downscale CRU climate data to match with2.5 arc-minute PRISM data. They are not used in the model's run time to interpolate month-step inputs to daily-step values. We mentioned that the monthly-step input climate data were further interpolated linearly into daily values in the model's run time in the revised manuscript.

(5) As LH is largely a land surface hydrological model, the authors should discuss how this model compares/differs in structure and process representation with other

commonly know land surface models, e.g. VIC (Liang et al. 1994, J Geophys Res 99), MATSIRO (Takata et al. 2003, Global and Planetary Change 38), LaD (Milly and Shmakin 2002, J of Hydrometeorology 3), ORCHIDEE (Ngo-Duc et al. 2005, J Geophys Res 110), etc.

Response: We agree that the comparisons between LH and other hydrological models will be very helpful for readers to understand the difference between LH and other hydrologic models. However, such comparisons need additional efforts and will be part of our future research. In addition, the main purposes of this study focused on modifying LPJ-DGVM for land surface water balance simulation and evaluating the modified model. To compensate for this, we briefly discussed the advantage and disadvantages of LH model in the discussion section and revised the conclusion section, through which readers will have some general ideas about the pros and cons of LH model or the potential difference between LH and other water balance models. We sincerely wish that our response to your suggestion here was to some degree acceptable.

Minor comments:
(6) Pg. 1208, line 11: not clear what is "them" referring to?

Response: We revised the abstract.

(7) Pg. 1208, line 11: ": : :then ran: : :" – seems to me not a proper use here.

Response: We revised this sentence.

(8) Pg. 1208, line 13: "proves" – is this a right work here?

Response: We changed "proves" into "justified"

(9) Pg. 1209, lines 13-16: "The importance of : : : land surface" – I would remove or reformulate this sentence.

Response: We slightly revised this sentence.

(10) Pg. 1209, line 29: The authors talk about limitations of DGVM, which is fine but does the method proposed in this paper overcome such limitations?

Response: LH was developed on the basis of LPJ-DGVM for the purpose of simulating large spatial scale land surface water balances. Compared to LPJ-DGVM, LH greatly reduced the task of model's parameterization. In addition, the model's structure was greatly simplified in LH. LPJ-DGVM is mainly a global scale dynamic vegetation model. Some limitations such as the generalization of plants as a few plant function types, the ignorance of water routing among simulated units, and the exclusion of human withdrawal of water from river still exist in LH. We discussed these limitations and pointed out that these limitations should be improved in the model's future development. Nevertheless, there are great advantages in developing LH for

simulating land surface water balances. We discussed these advantages in the revised manuscript.

(11) Pg. 1210, lines 13-15: "(iv) contribute to the accuracy : : :" – Does the result show that it actually contributed to improve the accuracy? Should be discussed.

Response: We cited several published papers to support the advantages of using satellite-based land covers in hydrological models. Satellite-based data often have higher spatial resolution and thus provide distributed information on land characteristics. They are generally thought to be contributive to simulating land surface water balances as demonstrated in previous studies. We cited several studies in the revised manuscript.

(12) Equations (1-10): There are dimensionless parameters in most of the equations used. It would be very helpful for the readers to understand what these equations actually do if the range of values that these parameters may take are specified.

Response: We added as possible as we can the potential ranges of some parameters following the corresponding equations.

(13) Equations (3) and (8): I suppose the parameter fvc is not in % but a fraction.

Response: We appreciate your comments. The vegetation continuous field data from the GLCF community at the University of Maryland are in percent. We transferred GLCF data into fraction of foliar vegetation cover in LH. We redefined "fvc" as fraction of foliar vegetative cover in a grid cell in the revised manuscript.

(14) Equation (11): For clarity, the 1/n should be taken out of the summation sign.

Response: We put "1/n" in front of the summation sign in equation (11) and thanks.

(15) Pg. 1218, lines 18: the "adjusted R squared" should be defined.

Response: We changed "adjusted R square" into "R-squared", which equals the square of the correlation coefficient.

(16) Pg. 1218, lines 16-17: "We also converted : : :" – is it necessary to mention this?

Response: The unit of USGS stream flow data is in cubic feet per second. For clarity, we mentioned it here.

(17) Pg. 1219, line 16: "surface hydrology" – should be more specific.

Response: We deleted the comparison between LH-simulated and LPJ-DGVM simulated three hydrological variables for the conterminous United States in the revised manuscript. As a result, it is no necessary any more to specifically describe "surface hydrology".

(18) Pg. 1219, line 25: ": : :ET is modeled to decrease.." – please check the meaning.

Response: We revised this sentence to "Annual ET is modeled to decrease spatially form South to North".

(19) Table 4: The drainage area should be expressed in km squared.

Response: We converted the drainage area from square miles to square kilometers in Table 4.