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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

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.

(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.

(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.

(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.

(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.

Minor comments:

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

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

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

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

(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.

(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.

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

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

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

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

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

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

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

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