Hydrol. Earth Syst. Sci. Discuss., 7, C1458–C1461, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C1458/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Evaluation of global continental hydrology as simulated by the Land-surface Processes and eXchanges Dynamic Global Vegetation Model" *by* S. J. Murray et al.

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Received and published: 15 July 2010

General remarks: The paper presents a runoff validation of a global vegetation model using different observational datasets. I have four main concerns with the present manuscript, which need to be addressed in a revised version: 1) Not all of the results are convincing in terms of the quality of the model simulations; 2) It is stated several times that some processes need to be improved in follow-up versions of the model yet they mostly have been addressed in a related model version (LPJmL), which should be reflected here; 3) The analysis of the influence of water withdrawals is doubtful, be-

cause water consumption and withdrawals appear to be mixed up; 4) The presentation of the results is rather descriptive and the paper would benefit a lot if some key results were discussed in relation to published literature on recent variations and trends in global runoff (and soil moisture / evaporation).

More specifically,

1) Section 3.1.5 states that modeled runoff agrees well with observations, but this is not obvious from Fig. 4. See below for possible further explanations why the simulation quality is poor in some regions (e.g. precipitation bias). What may be the reason for the differences to the LPJ version (for which such a validation has already been performed some years ago), i.e. what processes are represented differently in LPX? That said, section 2.1 is too short and should include more detail on e.g. computation of runoff, evapotranspiration, water-vegetation coupling (and potential changes in process representations compared to the former LPJ). I assume, however, that the differences in forcing data (esp. precipitation) are the main cause for the runoff differences between the two model versions, so it would be very helpful if at least one other climate dataset was used, also for section 3.1.6 (see also point 4). In section 3.2.2 it could be emphasized that the lack of river routing produces the mismatch between modeled and observed seasonal peaks.

2) The Introduction (p. 4221) states that DGVMs require hydrological benchmarking; I miss some literature here, because this has actually been done with some DGVMs (note, among others, the studies by Coe (IBIS), Gerten (LPJ), Piao (ORCHIDEE), Gordon (several DGVMs), and others at regional scale, e.g. in China). That said, the present study is not as novel as it claims to be. Furthermore, in parallel to LPX the LPJmL model was developed from LPJ (the 'root' model of both LPX and LPJmL), and these developments include an implementation of river routing and crop irrigation, i.e. processes that are suggested here as necessary future developments (see studies by Rost et al., Water Resour Res 2008; Biemans et al., J Hydrometeor 2009). These model developments and applications require consideration in the present paper, so as

to describe the state of the art in DGVM-water modeling. Page 4221 lines 3-5: LPJmL is not a previous version of LPX but a complementary version. The last sentence of the Discussion may also be adjusted in the light of these facts.

3) I presume that water extraction (water use, water withdrawal) is mixed up here with water consumption. It is only the latter that is relevant for the net change in runoff, while the (gridded?) Oki data obviously represent water extraction data of which a large amount returns to the river system after extraction/application. In other words, the present number (3448 km3/yr) used to represent the human influence is too high; please use water consumption (maybe from the Döll et al. 2009 paper), which is in the range of 1000-1500 km3/yr. But already now the figures suggest that the influence of water consumption / extraction is not significant in many regions, and as such its influence is a bit overstated (esp. relative to biases in precipitation that may lead to misrepresentations of runoff), as has also been found in previous studies. Thus, Fig. 3 may be removed, and the abstract needs to be adjusted accordingly. Similarly, the soil moisture analysis in section 3.2.3 is doubtful, as it refers to point-scale observations only while in Fig. 9 these results are shown together with basin-scale results. Even if the soil moisture sites were located in the respective basins, they should not be compared with the discharge data in a common plot. And: A sensitivity analysis of whether a change in the snowmelt routine will improve the results would be nice (p. 4232, lines 11-18).

4) The paper can be advanced from a mere validation study to a more thorough analysis of trends in regional/global runoff, in relation to previous studies. In particular (and this should be discussed already in the Introduction) there are the papers of Gerten et al. (Geophys Res Lett 2008) and Piao et al. (PNAS 2007) that analyzed 20th century variations and trends in global runoff / runoff with similar models. In this context, the findings shown in section 3.1.1 merit a much more in-depth discussion. For instance, it is a bit surprising that the authors found a decrease in runoff; is that also seen in other studies (given that the hydrological cycle is expected to accelerate, i.e. runoff to

increase, with global warming)? What may be the reasons for this decline (is it seen in the CRU precipitation)? And what may explain the differences to other studies (Dai, Piao, Gerten, Milly, Milliman; see also the large differences in Figs. 1 and 3)? Section 3.2.1 is valuable and novel as it considers the PDSI; but why not use soil moisture instead of runoff for this comparison? Here it would be interesting again to discuss why both soil moisture (PDSI) and runoff decline whereas other studies show an increase (in runoff), and this could be addressed by considering alternative climate datasets.

Minor comments:

5) Section 2.3, p. line 22: The CRU data don't reach back to 1850, do they?

6) p. 4230 line 9: Be more precise: The error in precipitation measurements in high latitudes is attributable mainly to undercatch of snow.

7) Move the first paragraph of section 3.2.1 and the first paragraph of section 3.2.3 to the Methods.

8) In general, the findings / validation is described in a somewhat negative way (e.g. section 3.2.2), please highlight the good results.

9) p. 4233 line 5: Note the Beer et al. study (Geophys Res Lett 2008) which provides an LPJ-based analysis including permafrost, demonstrating that this improved a lot the runoff estimates in northern latitudes.

10) I' m not sure what Table 1 tells us?

11) Fig. 5: What do the colors in the map mean?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4219, 2010.