Hydrology and Earth System Sciences Discussions

Interactive comment on "An integrated model for the assessment of global water resources – Part 1: Input meteorological forcing and natural hydrological cycle modules" *by* N. Hanasaki et al.

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Received and published: 30 November 2007

As a fellow global modeller I read the Hanasaki *et al.* paper "An Integrated model for the assessment of global water resources – Part 1: Input meteorological forcing and natural hydrological cycle modules" with great interest. I notice that the reviewers seem to question the novelty of a new global hydrological model. As you are interested in ensemble simulations, and the number of global hydrological models is low, I think the development of a new model can be motivated. My comments are of a rather practical nature:

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p 3539: I get the impression that the model was not tuned at all, but on page 3549 you describe two types of tuning. I think it should stated more clear here that you actually do some tuning (just like Arnell, 1999, who also wanted to avoid calibration).

p 3541: "For runoff, the major shortcomings of the GSWP1 were its short simulation period and its tendency for underestimation". I'm not objective here, but I think it would be appropriate with a reference to our work in Widén-Nilsson *et al.* (2007), where we compared 1987-1988 simulations with 1961-1990 simulations and noticed that we also got much lower global runoff simulations for 1987-1988, although not as low as 29000 km^3/yr.

p 3543: F-GSWP2-B0 precipitation, which is corrected, is compared with CRU data which is not corrected, except for the former Soviet Union. I think this major difference between the two datasets should be noted. An agreement cannot be expected.

p 3545: To me, bucket type runoff formulations sounds rather old-fashioned and wrong. Can it really be classified as a bucket, when you use a leaky bucket? I think the notation "bucket" can be removed.

p 3546, row 8: "largest number of downstream river gauging stations", you must mean the largest number of upstream gauging stations, that the selected station is the down-most.

p 3546, row 14-16: How did you collect the simulation data? When they are freely downloadable over the internet (like R-F02) I would like to see the link, and when you got the data personally I expect to see that in the acknowledgements.

p 3547: "Of these four data sets, R-BR75, R-D03 and R-F02 are regarded as observation-based runoff products". In Widén-Nilsson *et al.* (2007) we also considered R-BR75 as data-based, while R-D03 was considered as a simulation product and R-F02 as a combination of the two. Can you motivate why you make this different classification? Is it because of the correction factors? Despite calibration and cor-

rection factors, I think estimates made by running climate data time series through a hydrological model, are model-based and not observation-based estimates, while the other estimates, like R-BR75, are not made with a hydrological model and should be classified as data-based.

p 3548 row 17: Is the little symbol before P in the R_{net} expression explained somewhere? (I have not read Budyko (1974)).

p 3551, row 4: "was within the plausible range". Why is the plausible range the one defined by the minimum and maximum of BR75, F02 and D03? BR75 is old, and runoff varies within time, and F02 and D03 have used models as well and are sensitive to e.g. the precipitation input. If you still want to use the word "plausible", add some error bar to the other estimates, otherwise just discuss if it is above or below the range of some of the other estimates.

p 3555: "There were some basins with errors >20% because the period selected for scaling in these studies may have differed from ours." These results of the previously published simulations are very interesting, especially if it is a time period effect only, but there are some other possibilities as well. From the paper, I understand that you have retrieved the gridded simulation results, i.e. the runoff fields. In D03 is only the first correction factor allowed to influence the runoff fields. Can the large bias come from basins where this second correction factor was used? Are the comparisons made for basins where D03 and F02 have gauge data such that a correction was made, or might their simulation data come from basins that were ungauged for them and thus not corrected?

p 3563, row 16: 100 days = 3600 s/h * 24 h/day * 100 days = 86400 * 100, but 365 is added here as well. Why?

I'm looking forward to see the next version!

Best regards

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References

Arnell, N. W. (1999), A simple water balance model for the simulation of streamflow over a large geographic domain, Journal of Hydrology, 217(3-4), 314-335.

Widén-Nilsson, E., S. Halldin, and C.-y. Xu (2007), Global water-balance modelling with WASMOD-M: Parameter estimation and regionalisation, Journal of Hydrology, 340(1-2), 105-118.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3535, 2007.

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