Hydrol. Earth Syst. Sci. Discuss., 4, S1672–S1677, 2007
www.hydrol-earth-syst-sci-discuss.net/4/S1672/2007/

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.

N. Hanasaki et al.

Received and published: 7 December 2007

Dear Elin Widén Nilsson,

Thank you very much for your helpful comments. Here are our replies.

—

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

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:

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).

> What we want to argue here is that we only adopted the parameter modification methods that are applicable to whole globe and future projections. We will add this point to the text.

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 km3/yr.

> We will cite Widén-Nilsson et al. (2007, p114) that showed a cause of underestimation in GSWP1: "The low value of total simulated runoff reported by Oki et al. (2001) should partly be related to the short time period (1987-1988). WASMOD-M also simulates low runoff, 92 percent of the 1961-1990 average value, for this period."

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.

As we described in the text, we showed the CRU data as a yardstick or a benchmark, not as the correct data.

p 3545: To me, bucket type runoff formulations sounds rather old-fashioned and wrong.12288;Can it really be classified as a bucket, when you use a leaky bucket? I think the notation "bucket" can be removed.

Well, your comment is true if we just focus on the hydrological process of the original bucket model by Manabe (1969). However, we adopted the whole processes of the model, energy/water separation, evaporation-soil moisture relationships, etc. A number of improvements were applied, but still our model is heavily influenced by the original bucket model.

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 downmost.

Thanks for your correction. "The downmost river gauging stations" is correct.

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.

We will add URL links to the reference section. The runoff data of Nijssen et al. (2001) and Fekete et al. (2002) are freely downloadable from the following websites:

```
http://www.ce.washington.edu/pub/HYDRO/nijssen/vic_global/index.html
http://www.grdc.sr.unh.edu/html/Data/index.html
```

The runoff data of Döll et al. (2003) was kindly offered by Professor Petra Döll. We will add this in the acknowledgement.

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 correction 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.

We distinguished observation-based earlier studies from others, not data-based ones (Page 3547, line 22). We consider that R-BR75 (the runoff data of Baumgartner and Reichel, 1975) is a hybrid product of gauge observation and water balance, and both R-D03 and R-F02 are hybrid products of hydrological simulation and gauge observation. This classification is different from that of Widén-Nilsson et al. (2007).

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

We will add the explanation of symbols.

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.

> Since there is no correct data in global water balance, the evaluation of water balance simulation becomes subjective to some extent. In our paper, we compared seven global runoff products that have at least 5 degree zonal mean spatial resolution. Among them, BR75, F02, and D03 are based on best available observed streamflow data (Page 3547, line 22), and widely used in dozens of publications. We used the range of these three as a benchmark.

p 3555: "There were some basins with errors >20 percent 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?

> Thank you, we will add this discussion. Only the first correction factor (a maximum change of 100 percent is allowed) was applied to the runoff product of D03, and it can be a cause of errors.

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?

HESSD

4, S1672–S1677, 2007

Interactive Comment

The comparisons you suggested were not carried out because we didn't collect detailed information on runoff correction of D03 and F02. What you suggested is a kind of intercomparison of global runoff products, which sounds very interesting, although, it is beyond the scope of our paper.

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

Thanks for your correction. "100days = 86400*100s" is correct.

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