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Hydrology and Earth System Sciences Discussions

Interactive comment on "Reconstructing 20th century global hydrography: a contribution to the Global Terrestrial Network- Hydrology (GTN-H)" *by* D. Wisser et al.

Anonymous Referee #1

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General comments

This paper presents an analysis of global runoff data, and model results for natural and disturbed discharges to ocean basins. The paper is based on the WBM model, which uses a grid-based approach. While the model has been published previously, there is very little assessment of the sensitivity of the conclusions to parameter values (either assumed or calibrated). This is where the paper is weakest. There needs to be an assessment of what parameters contribute most strongly to the results presented, and which parameters are not significant. There will be parameters for which any reasonable value will not significantly influence the conclusions (for example the beta

and gamma parameters in equations 5 and 6), but there may be parameters that could influence the results significantly. Understanding the significance of the conclusions requires adequate testing and reporting of the model sensitivities.

Specific comments

Page 2680

Line 13-14 - Here, the conclusion is that there is no significant trend in the terrestrial discharge over the last century - is this allowing for changes in climate over that period (i.e. no trend due to human intervention) or is this combined effects of climate and water use? I suspect the later, which suggests that looking at the streamflow data only is not sufficient to permit determination of any trend. First the climate influence needs to be removed so that the human influence can be seen. It should be noted in that case that the determination of the impact of human intervention will be model dependent. Conclusion does not seem to agree with the statement in the introduction (lines 4-7 page 2681) - the implication is that the trends seen on a regional scale reported by Milly et al (2005) is not seen when the data is evaluated on a global scale. See also comment on line 20-25, page 2681).

Page 2683

line 15-18 - is the snowmelt model of Willmott et al. applicable on a global scale? without checking the reference supplied (I don't have access at the moment), it is difficult to determine this, but presumably this is an empirical model fitted to data at a small number of locations.

line 7 - The alpha is set to 5.0 in this application. What is the justification for this? Or is this an assumption? How sensitive is the model to the choice of alpha?

Page 2686

line 2 - A fixed value of beta means that differences in groundwater response are not being considered in the model. That is, all aquifers have a value of 0.0167 (approximating this as a time constant (really continuous time rather than discrete time) gives Tau of just under 60 days. How sensitive are the conclusions to this assumption? The constant partitioning of 50% of the excess water to runoff and 50% to groundwater is a reasonable - and necessary - one (I've used it myself at times) but it is a coarse assumption. What is the sensitivity of the conclusions to this? In the case of mean annual discharge to the oceans, this would have minimal effect, particularly since any shortfall in water for irrigation is taken from fossil groundwater resources.

equation 10 - C_1 should read c_2. The Muskingum form usually results in one of c_0, c_1 and c_2 being negative, resulting in a negative flow on occasion. I assume this is the case in your application? Having no information on the values of C and D makes it difficult to determine this. How have you handled the negative values? Or have you constrained the values of C and D?

$C+D>1$

$1+C>D$

$1+D>C$

$D-1<C<1+D$ if $D>1$

$1-D<C<1+D$ if $D<1$

The sum of the c_0, c_1 and c_2 values is by definition 1, ensuring conservation of mass. The result is that transmission losses are not being considered (other than irrigation, reservoirs etc.). If there are significant losses from the streamflow (e.g. recharge), there will be a tendency to overestimate the discharge in some locations. Presumably, any such loss is assumed to return to the stream before the stream discharges to the ocean. Given the coarse assumptions made regarding the aquifer properties, this is a reasonable approximation to the system.

Page 2687

line 21 - How sensitive are the results to the assumed values of these 4 parameters? What is the distribution of values in Kingston, 1998?

Page 2688

Line 22-25 - in addition to evaporation loss, any groundwater loss is also neglected in this equation. The usual assumption is that siltation seals the base of the reservoir relatively quickly. Concerning the evaporation loss, buffering of the temperature leads to increased evaporation in the winter, and decreased evaporation in the summer. Ignoring seasonal effects the evaporative loss from a reservoir can be over 1 m in a year. This may be negligible for very deep reservoirs

Page 2689

line 24 - dam evaporation rate less than reference crop rate? Shallow depth of the small dams (2m) means the water will be approximately isothermal. Generally, water in small dams will be more turbid that water in big reservoirs, so the albedo will be higher, and hence solar heating of the water less.

Page 2690

Section 2.6 - what about socio-economic constraints to groundwater use? In some areas, farmers will not have access to deeper groundwater systems, leading to crop failure in extreme conditions.

Page 2691

section 3.1.1 - good to see that sensitivity analysis of the results to the 2 approaches has been done. The small difference suggests that, with respect to the conclusions at least, the spatial coherence of the rainfall is not important.

Page 2692

assuming that all areas equipped for irrigation will be irrigated in any particular year should result in an overestimation of the amount of irrigation (assuming that the estimate of the area equipped is correct). This will effectively mean that there is an unknown irrigation efficiency factor included in the model - analysis of the sensitivity of the model to the efficiency factor should enable the authors to assess the impact of the uncertainty in the area irrigated each year would have on their conclusions.

Page 2694

line 10 - I recommend change "thus increasing runoff" to "thus increasing runoff assuming total leaf area is unchanged". The impact of increased CO2 on plants is not just a change in the transpiration per unit leaf area. There may also be an impact on the leaf area. In both cases, the impact will be species dependent, so it is difficult to conclude that increased CO2 will result in increased runoff unless the change in total leaf area is also addressed.

Hamon (1963) is an empirical model - scale of applicability?

Page 2695

line 1 - while the parameters have not been calibrated to match the observed discharge, presumably the selection of parameter values has been made based on an understanding of the model behaviour built through previous use of the model.

Page 2696

line 4 - is equation 21 constrained to be between 0 and 1? Certainly, a perfect model gives a value of 1. If the predicted value is the mean observed flow, then d_2 will be 0. If the predicted values are extremely high, then d_j will approach 0. However, if the predicted value is zero everywhere, then?

Figures and tables

Page 2725

caption for Figure 3: shift the (c) to before differences

Page 2726

Both Fig 3 and 4 show the significant trends in modelled annual values of runoff. What is different between this figures (other than a reversal in the look-up table)?

Page 2727

Figure 5 is difficult to read. Given that Figures 3 and 4 are in colour, why not give Fig 5 in colour?

Page 2728

conditions misspelt in caption

Table 3: There is an increasing trend for the Atlantic and Artic Oceans, while the Indian and Pacific Oceans show the greatest impact from irrigation. Figure 3 suggests that there is a significant increase in the discharge from Canada into the Atlantic, and also from southern Brazil to northern Argentina, as well as an increase in the Amazon. Presumably the increase for the Amazon is driven by deforestation. What is driving the increase in the natural outflow from eastern Canada and southern South America? Are these also driven by deforestation?

While the increase in the discharge from south western Australia may be significant, I would suspect this would still be very small contributor to the overall discharge given the area is arid to semi-arid. Could you give a figure showing the distribution of predicted discharge?

Figure 3 also shows that the biggest impact due to irrigation is in the region of the Himalayas. Is your network model taking into account the interbasin transfers which dominate through India? I suspect this is not considered in the model. Given the difficulty in getting streamflow data in the region, what confidence do you have in the model's performance in this region? Given the strong irrigation signal in this area, presumably this is the main contributor to the difference between the predicted natural and disturbed flows into the Indian Ocean. Is this correct?

The dominant contributor to global flows (Fig 6) is the inflows to the Pacific Ocean. Presumably this is due largely to the inflows from Asia (China through to Thailand).

Fig 7: I suggest using the same format as for Fig 6 (i.e. a black and grey line rather than the dotted line). No obvious trend in plot for Amazon - assuming the clearing of the forest has been included in the model, the suggestion is that the amount of clearing has not yet had a significant hydrological impact (at least in terms of annual runoff). Is this correct, or is there a trend in the rainfall?

Fig 8: the model does not do as well for the Amu Darya and Sr Darya rivers. For the Amu Darya, it appears there is a lag not accounted for in the model, with observed flows delayed with respect to the modelled flows. Reason?? For the Syr Darya, the model is over predicting peak flows during the wet years. Could this be a problem with the rainfall data, streamflow data or the model?

Fig 9: Contemporary misspelt in caption

Minor errors

page 2680,

line 12 and 13: given the remainder of the sentence, suggest you change "governed by variations in the climate signal alone" to "governed primarily by variations in the climate signal" . Another alternative would be to replace "primarily" above with predominantly, and change "secondary" to "a minor".

Page 2683

- sign appears where there should be a < in the condition for equation 2. Suggest increasing space between equations and conditions as in the first line these run together.

page 2684

I suspect there is a - missing from the denominator in equation 3?

Page 2687

line 21 - nu doesn't appear in previous equations. Presumably it should appear as a power in equation 14? Also, there is an unbalanced bracket on lines 21-22.

Page 2693

line 21 - "dams over 15 m"? - presumably 15m deep?

Page 2700

line 24 replace Sean with Sea

Page 2701

Line 7 - 1975-200, complete year for end of period

line 9 remove exemplarily - I don't think the emphasis is warranted here.

Page 2702

line 23 "with the a reduction"

Page 2703

line 14: again, I would question the use of exemplarily

missing reference to:

FAO/UNESCO, 2003

AQUASTAT, 2008

Hunger and Doll, 2007 (should be 2008?)

Hanasaki et al (2007) (should be 2008?)

missing citation

Vorosmarty, C. J., Willmott, C. H., Choudhury, B. J., Schloss, A. L., Streans, T. K.,

Robeson, S. M., and Dorman, T. J.: Analyzing the discharge regime of a large tropical river through remote sensing, ground-based climatic data and modeling, Water Resour. Res., 32, 3137–3150, 1996.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2679, 2009.