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Interactive comment on "Comparison of Generalized Non-Data-Driven Reservoir Routing Models for Global-Scale Hydrologic Modeling" *by* Joseph L. Gutenson et al.

Anonymous Referee #2

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Gutenson et al. submitted a manuscript to HESS with the title "Comparison of Generalized Non-Data-Driven Reservoir Routing Models for Global-Scale Hydrologic Modeling" where they assess the performance of two reservoir operational schemes at daily and monthly resolution and for 60 reservoirs in the US with the aim to find a proper method for implementing in a large scale hydrological forecast model. While the manuscript gives interesting insights esp. into the sensitivity to the outflow coefficient of the method used by Döll et al. (2003) (hereafter referred to D03) and to the daily / monthly time step behaviour, I do have doubts with the relevance and novelty of this manuscript and have several comments as listed below.

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Relevance and novelty:

The authors use two approaches that are around in the field of global hydrological modelling since more than a decade, and are in the meantime somehow outdated. A clear motivation (along with explanations and citations) is missing in the introduction that frames why it is required in the purpose of this manuscript (for hydrological forecast models) to use data-free schemes. Is it the specifics of hydrological forecast models (which is a way contradicting to the future outlook section where the authors indicate that assimilation schemes would be possible – if this is the case, why could not improved reservoir operation schemes be included)? In the publication of D03, "reservoirs are treated like global lakes, due to lack of information on their management" (Döll et al., 2003, p 112), hence it is not a reservoir algorithm per se. Having said that, it is true, that this approach is indeed data-free (except maximum storage). However, the Hanasaki et al. 2006 approach (hereafter referred to as H06) is not data-free (information about storage capacity, purpose, water demand downstream are required). Since Döll et al., 2009, the H06 approach was adapted and implemented into their GHM. Nowadays the most GHMs have further advanced reservoir schemes and consider e.g. also reservoir operation years (see www.isimip.org) and the reservoir schemes of some models have been evaluated by Masaki et al. (2017). Again, it is hard to understand, why the future of dealing with reservoirs in hydrological forecast models (as the authors indicated in Section 3.8) should lay in an algorithm that was not developed specifically for reservoirs. Current state of the art in reservoir operation schemes is much advanced since this two approaches (e.g. see the citing articles of H06 and Döll et al., 2009) and nowadays initiatives like http://globaldamwatch.org/ try to make the best out of available global scale information about reservoirs. Nevertheless, the research questions are well formulated and there are some very interesting technical aspects of the manuscript such as the sensitivity of to the outflow coefficient of D03 method and the time step assessment but it is questionable if this is worth it to publish in such a widely framed journal like HESS, mainly because those approaches (esp. D03) are outdated. I would encourage the authors to include (among the suggestions

below) more up-to-date approaches in a potential revision? That could widen up the usability of the findings of the work to e.g. the global hydrological modelling community.

Other major issues:

Essential to the performance measures of the approaches are inflow and outflow streamflow data from the 60 reservoirs. The inflow data were back-calculated from outflow data and storage changes (lines 509 ff). This is too vague and needs much more details, otherwise it is a black box and nothing that is reproducible. Furthermore, the authors have not quantified the uncertainty or plausibility of this back-calculation (only that inflows can be sometimes negative which is a sign that the back-calculation misses essential details) which must be definitely included. As a first guess, the authors should back-calculate the inflow of reservoirs from Nashville district with the same method as for the others and compare this to observed inflows (that are available when I am interpreting line 510 correctly).

From the title of the manuscript, it is not clear that the manuscript is motivated from the perspective of hydrological forecast models. Especially the first paragraphs of the introduction irritated me with respect to the title (global hydrological modelling). I therefore suggest to rephrase the title to better reflect the focus of the manuscript to specific needs of forecast models.

The sensitivity study of k_rd of the D03 method is very interesting and coming to the conclusion that the suggested parameter (0.01) is not optimal for most of the reservoirs, but 0.9 is. A factor of 0.9 means that 90% of the actual storage volume is being released by each time step. This seems to be – on a daily time step – very high, mimicking nearly a complete flow trough of inflow to outflow of the reservoirs. An analysis of k_rd in relation to IR would be very meaningful. I assume that those high k_rd values should occur only with low IR (or low S_t values) so that the reservoir only little modifies the river flow. If this is not the case, I would see that as indicator, that the D03 method simulates the river flow modification well for a false reason. In addition, Fig 5 mentions

"maximise KGE and minimize nRMSE" – which values are typical for e.g. the k_rd of 0.9? Close to 1 or rather close to or below 0? A maximum KGE of 0.095 (as displayed in Fig 6) is indeed maybe the maximum for this reservoir but I would not see this low efficiency metric as sign for good modelling result. I suggest therefore to use the classifications of the 4 KGE levels (lines 212 f) as stacked bar for better interpretation of Fig. 5. The major purpose of the 60 reservoirs is flood control (for 43 reservoirs). This goes well along the assumption that for H06 method only the non-irrigation purpose is being used (by the way, there are global (monthly) irrigation estimates available from global hydrological models, e.g. Huang et al., 2018)). Could the authors please assess more in detail how the methods analysed relates to flood events (in particular here, the uncertainty information of the back-calculation approach would be required)? This could test the two approaches if it holds true for such events.

The manuscript reads in principle well but the mix of considering all reservoirs and the focus of some for specific analysis is not very clear, probably because a clear difference between a results and discussion section is missing. What are the criteria to select specific reservoirs for focus analyses (e.g. selection of the 7 dams in Table 2)? For the discussion, I would suggest to read Masaki et al., (2017) with the aim of trying to relate your results to those of their study (they also dealt with e.g. Fort Peck), that could place your study better to recent literature.

Section 3.7: The authors state, that only H06 includes withdrawals in their method. While not completely wrong, this is in a way misleading. The analysed approach of D03 relates only to the outflow of the global lake / reservoir. However, in the D03 paper section 3.5 details of how water abstraction is considered from reservoirs / global lakes. So, water use is considered in the storage equation of their model and hence indirectly in the outflow calculation (as this is impacted by actual storage). The same holds true for evaporation (for both approaches). Lines 504-508 needs to be therefore rewritten to avoid misleading conclusions.

Section 3.8. seems to be contradicting. On the one hand, the authors argue that e.g.

the D03 method will be implemented in a river routing model, on the other hand, they argue that data-driven approaches (assimilation of remote sensing products) could be the future. What is the general message then? What about recent implementations of reservoir algorithms in the global hydrological models? Could they be implemented in river routing models? How does other routing models, e.g. CaMa-Flood deal with reservoirs?

Minor and formal issues (not complete):

At various places in the manuscript, the authors use “Döll Method, Hanasaki Method” in various different writing styles. I suggest to use abbreviations throughout instead (e.g. D03 / H06) for better readability and consistency.

Table 1 gives insights into the statistics of the reservoirs used for testing. However, it would be very informative to have those kind of statistics for every reservoir, including the coordinates and purpose, e.g. at appendix or as supplement. That could help interpreting the other figures e.g. Fig 7. I would also suggest to include the performance metrics for each reservoir and method (daily and monthly time step) to this table which increases interpretation possibilities (e.g. as excel file for downloading). Please also provide numbers of the reservoirs to Fig. 1 to relate the reservoir characteristics and interpretation to specific locations of the US.

The introduction contains many relatively old references (e.g. the effect of reservoir regulation to streamflow) that could be enriched with more recently published work.

Units or dimensions are missing in the equations

Unit “cms” in discharge time series figures should be written as m^3 s-1

Fig 6. is a “best” performance of KGE <0.1 meaningful at all?

Fig 7. Is very interesting – for which purpose is this dam created (see also my comment to Table 1)?

Fig 8: X-Axis labels not very common (sugget to use months only); why is the KGE (0.3 for Union City similar for wet and dry years and the simulation (same as 1.1 for Arcadia lake) whereas it is not the case for inflow/outflow assessment? The y-axis title is strange (“Outflow”) as content shows also “Observed Inflow”. I suggest for this (and similar diagrams) another title (e.g. streamflow or river discharge)

Fig 9b wrong header (should be daily?). Furthermore, could the authors explain, why in Fig 9a there is a low peak in monthly D03 time series within 2011 (is it the instability mentioned in Sect. 3.6?)? Is k_rd the same for Fig. 9a and 9b?

Fig 10 and the interpretation would highly benefit if the initial simulated outflow (without the adaption of Eq 5) is displayed as well.

541: Maybe I have missed it – but have the authors somewhere assessed the suitability at sub-daily time steps? If not, please modify this bullet point. Furthermore, the authors assessed only non-irrigation reservoir algorithm from H06. The whole study design is intended to handle non-irrigation purposes (except one reservoir) so it is unknown how the approach works in reservoirs that are constructed for irrigation purposes. This should be made clear as well.

References: some Discussion papers cited (Coerver et al 2017, Döll et al., 2009) which needs to be replaced by final published versions, formally the reference list is far away from being consistent, needs carefully revisions. I have not checked if all citations from the manuscript are listed in the references and vice versa.

References

Döll, P., Kaspar, F., and Lehner, B.: A global hydrological model for deriving water availability indicators: model tuning and validation. Journal of Hydrology, 270 (1-2), 105-134, https://doi.org/10.1016/S0022-1694(02)00283-4, 2003.

Döll, P., Fiedler, K., and Zhang, J.: Global-scale analysis of river flow alterations due to water withdrawals and reservoirs, Hydrol. Earth Syst. Sci., 13, 2413-2432,

https://doi.org/10.5194/hess-13-2413-2009, 2009.

Hanasaki, N., Kanae, S., and Oki, T.: A reservoir operation scheme for global river routing models, J. Hydr., 327 (1-2), 22-41, https://doi.org/10.1016/j.jhydrol.2005.11.011, 2006.

Huang, Z., Hejazi, M., Li, X., Tang, Q., Vernon, C., Leng, G., Liu, Y., Döll, P., Eisner, S., Gerten, D., Hanasaki, N., and Wada, Y.: Reconstruction of global gridded monthly sectoral water withdrawals for 1971–2010 and analysis of their spatiotemporal patterns, Hydrol. Earth Syst. Sci., 22, 2117-2133, https://doi.org/10.5194/hess-22-2117-2018, 2018.

Masaki, Y., Hanasaki, N., Biemans, H., Müller Schmied, H., Tang, Q., Wada, Y., Gosling, S. N., Takahashi, K., and Hijioka, Y.: Intercomparison of global river discharge simulations focusing on dam operationâĂŤmultiple models analysis in two case-study river basins, Missouri–Mississippi and Green–Colorado. Environ. Res. Lett., 12, 5, 055002, https://doi.org/10.1088/1748-9326/aa57a8, 2017.

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