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Discussions

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Interactive comment

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

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Thank you for your review! Broadly, we will make alterations to the manuscript to clarify and broaden its applications.

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that are around in the field of global hydrological modelling since more than a decade, and are in the meantime somehow outdated.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

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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) 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.

Response: This reviewer is clearly approaching this paper from a climate modeling perspective. While there are commonalities between forecasting models and climate models, there are some distinctions that might not be appreciated. The climate model operates at half degree resolution on monthly timesteps and runs decadally. A flood forecasting model driven by a numerical weather model is more highly resolved spatially and temporally to capture individual flood events. The reviewer suggested initiatives like global dam watch which provides precisely the type of information needed to implement the non-data-driven D03 & H06 approaches, e.g. active storage volumes and total storage capacity. The majority of the more sophisticated approaches the reviewer alludes to require site specific operational rule curves or training data which are not contained in the Global Dam Watch's GRaDv1.3 database that has these attributes for 7,300 dams, which is still incomplete considering there are 38,660 dams geolocated in the GlObal geOreferenced Database of Dams (GOOD2). Likewise, the global (monthly) irrigation estimates by Huang et al. (2018) mentioned below are difficult to disaggregate at the spatial (100m -12km) and temporal scales (hourly to daily) of the forecasting models. Currently, a common practice in large-scale spatially continuous forecasting systems like the NOAA National Water Model is to neglect reservoirs altogether. Thus any approach that outperforms run-of-the-river conditions represents an improvement. The approaches the reviewer considers 'outdated' happen to be the most readily implementable, for instance the CaMa Model the review mentions was paired with H06 (The authors will consider developments from Masaki et al. (2017) in their revised manuscript.

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).

Response: A true directly observed inflow is not available for nearly all reservoirs, including those maintained by the U.S. Army Corps of Engineers. There are two ways that one can acquire an inflow; estimated using a streamflow model (as in Masaki et al., 2017) or use a back calculated inflow. The authors have chosen to utilize a back calculated inflow because this accounts for all other withdraws from the reservoir, such as irrigation, seepage, etc. This allows the authors to focus exclusively on the reservoir routing methodology utilized. This is also the reason that we do not account for such withdraws as this would be double counting withdraws. We will add details about back calculated inflow to the manuscript.

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.

Response: The authors will adapt the title and manuscript to better reflect the application of the paper to hydrologic 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.

Response: It is correct that the DO3 calculation is a proportion of current storage over the time step. However, It should not be inferred that a k_r of 0.9 leads to a 90% release of total storage volume. A second factor using the proportion of active storage taken to the 3/2 power decreases the proportion of storage released for a given time step (Equation 4 on line 196). The intent of D03 is to simulate outflow approaching inflow when available storage is low and inflows are high. This is the type of outflow decision that will be made during such cases as, in reality, spillways will begin to open and outflow will approach, or exceed, inflow. We will undertake and add an analysis of k_rd in relation to IR to the revised manuscript. Our results do not suggest the KGE of 0.095 is a good modeling result. However, we do substantially improve model performance, over run-of-the-river, for the majority of the cases we analyzed. The 0.095 KGE referenced by the reviewer is an example from the Yazoo Basin Headwaters Project used to transparently illustrate a case in which the non-data driven models do not perform well. This site specific analysis represents an attempt to analyze why DO3 performs poorly under certain instances. In fact, we consider the reason why the performance is low for this particular reservoir in Section 3.3. A stacked bar graph is a great idea for inclusion into the manuscript. The authors will add this to the manuscript.

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

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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.

Response: The dams included in Table 2 were selected to illustrate that conclusions for reported by Hanasaki et al. (2006) at the monthly timestep do not necessarily hold when the impoundment ratio is such that outflow bears minimal relation with inflow. This will be clarified in the revised manuscript. The authors will analyze Masaki et al. (2017) and attempt to relate the results of this study with that of Masaki et al. (2017). The authors are willing to segregate Section 3 (Results and Discussion) into a Results Section (currently Sections 3.1 & 3.2) and a Discussion Section (currently 3.3-3.7) if the editors feel this provide additional clarity.

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.

Response: The reviewer is correct that the D03 study does implicitly account for water withdrawals in the hydrologic simulations which they perform. This accounting of water withdrawals is similarly accounted for in the inflow that our study uses. The inflow that our study uses is back calculated from storage fluctuations. However, the D03 equation has no explicit term for withdrawals, a portion of the H06 formulation does. We will clarify these differences within our manuscript.

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?

Response: Our intention in Section 3.8 was to illustrate how remotely sensed data could be assimilated into non-data-driven methods to improve them. Unlike data-driven methods, the non-data-driven methods we consider are conceptualizations of reservoir operations that can be adapted to be a data driven approach, but do not themselves require training data in order to be implemented. We see this as an advantage. The authors will clarify this point in the manuscript by explaining that non-data-driven methods can be linked to statistical fitting techniques, but that they are capable of being employed independent of such pairings. CaMa-Flood has been coupled off-line with H08 (Mateo, 2014), an integrated water resources model that includes the H06 reservoir operation module evaluated herein (Hanasaki et al., 2008).

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.

Response: The authors will update the manuscript to refer to the Döll Method and Hanasaki Method as D03 and D06, respectively.

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.

Response: The authors will update the manuscript to reflect these requested changes.

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.

Response: The authors will update the manuscript literature review to include works such as Masaki et al. (2017).

Units or dimensions are missing in the equations

Response: The authors will update the manuscript equations to include units and dimensions.

Unit "cms" in discharge time series figures should be written as m3 s-1

Response: The authors will update the manuscript to reference discharge as m3 s-1

Fig 6. is a "best" performance of KGE.

Response: The authors will add update to Fig. 6. to describe the figure as the "best" performance of KGE.

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