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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Near real-time adjusted reanalysis forcing data for hydrology" *by* Peter Berg et al.

Anonymous Referee #2

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In this manuscript Berg et al. provide a method to produce "near real time" global forcing data for hydrological models. The methodology is closely based on the methodology used for the WFDEI dataset and extends it with minor modifications to produce "near real time" global forcing data. Overall I think there is an insufficient review of other relevant datasets and methodologies that are clearly linked with this manuscript. In addition, the discussion on the methodology and its advantages and limitations is insufficient. Below I have outlined my major and minor comments. A major review is necessary for this manuscript before it can be published.

Major comments:

1.) In the introduction the authors claim that "forcing data for large scale hydrological models is essentially not available. . ." – This claim is not correct. In fact there are

numerous, (semi) operational global hydrological models that have solved the problem of forcing data in different ways, for example the Global Flood Awareness System, the Global Flood Monitoring System, and the Global Flood Forecasting System. Please refer to their relevant scientific articles on what type of global forcing data they use to derive hydrologic model initial conditions. In addition, see also the recent article from Hirpa et al. (AMetSoc, 2016) on this topic which should be considered by the authors. Finally, also ERA-5 is now available and produced in "near real time". This should also be included and discussed.

2.) Numerous datasets that are claimed to lack temporal coverage have in fact coverage also of recent years including the MSWEP dataset. The manuscript should reflect the latest status of these datasets.

3.) Furthermore, the authors have omitted completely the TRMM (and now Global Precipitation Measurement) datasets that represent an important source of near-real time precipitation forcing which is clearly the most important variable for the forcing data of hydrological models. Those need to be at least mentioned/referenced with an explanation of why those are not used in this work.

4.) The authors claim that their method is similar to the method used in the WFDEI dataset. Yet, in Fig. 2d the relative difference between WFDEI and GPCC is considerable whereas the relative difference between GFDCL and GPCC is very small. This suggest that the changes introduced by the authors in comparison to the methodology to WFDEI have a significant impact. Instead of claiming that this is simply due to the use of different precipitation sources this should be further investigated and explained.

5.) This manuscript has almost 20 (!) abbreviations. Some of them are spelled out before their first use, others not. Some important ones such as GFDOD1 and GFDOD2 are not properly explained. Even though I am familiar with most of the abbreviations it makes this manuscript very hard to read. The authors should at least include a table with an overview of the most important ones (maybe expand tables 1 and 2) or maybe

add them as an annex.

6.) What is the effect on hydrological simulations when you update with the new observational data on the 10th of each month? This might lead to a significant discrepancy between simulations done on the 9th and then, with the updated dataset on the 10th. Clearly that represents an issue for hydrological forecasting but is not properly discussed by the authors.

7.) Figure 6 d seems to suggest that there is actually less or at least similar bias in the average upstream runoff difference when compared to 6b and 6c. This seems to contradict Figure 5 where the OD period shows the highest absolute difference. Please explain more in detail why this is the case.

8.) The GFD claims to be a global dataset for hydrological models. Yet, the hydrological validation was only performed for catchments in northern latitudes. There is currently no hydrological validation for basins located in tropical climates. The validation should be improved including also basins from these regions.

9.) The manuscript lacks a paragraph on future developments.

10.) The title of the manuscript should be modified and the authors should define in the text what they mean with "near real time". "Near real time" suggests that data is updated within hours or at least days and not monthly.

Minor comments:

- Please add the datasets used for WFDEI to Table 2

- Please add a reference for GHCN-CAMS into the references

- P.10, line 14: last sentence is unclear. Please describe further and rephrase

- What is the difference between GFDOD1 and GFDOD2?

- The nonlinear scale in Fig. 2 and 4 makes it very difficult to look at the results.

Basically everything greater than +-25% is hardly distinguishable. Please choose a different scale or use the one applied in Fig. 6 (and possible also a different color scale)

- In the section on "Meteorological evaluation" the authors write ". . .and focus instead on comparisons to the WFDEI dataset." However, in the following evaluation you compare the GFDCL mostly with GPCC, EI or OD. Please clarify.

- Does Fig. 2 show the relative difference of EI, GFDCL and WFDEI to GPCC7/CRUts? Please make this more clear.

- Is the precip bias between EI and GPCC7 in line with other studies looking at the precip bias of EI? If yes please add the relevant reference.

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