Hydrology and
Earth System
Sciences
Discussions

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

Peter Berg et al.

peter.berg@smhi.se

We appreciate very much the reviewer's comments which we will answer below.

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 con-

sidered by the authors. Finally, also ERA-5 is now available and produced in "near real time". This should also be inclued and discussed.

Such claims are of course only subjectively correct, and we will rewrite that statement. From our point of view, we need consistent data for temperature and precipitation that are close to observations. This issue has been solved in different ways for different projects with a global approach. Each of the listed systems above have made their own versions of forcing data, but they do not share the data themselves openly as far as we are aware, thus not "available". We will discuss these different global systems and how they have solved the issue in the revised introduction. We will also discuss gridded and satellite products that can be used as forcing, including their pros and cons. Thanks for the Hirpa et al (2016) paper, which we have traced to the paper Hirpa, F. A., Salamon, P., Alfieri, L., Pozo, J. T. D., Zsoter, E., Pappenberger, F. (2016). The effect of reference climatology on global flood forecasting. Journal of Hydrometeorology, 17(4), 1131-1145. We will include the results in the revised introduction. Regarding ERA-5, we intend to make use of this once available for a long time period during 2018, and make a new version of GFD based on that. We add this to discussions and outlook.

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.

Our intention with temporal coverage was for the "near real-time", which to us is until at least last month. We will mention the full extent of WFDEI-CRU and MSWEP.

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.

We will mention these data sets and discuss their coverage and quality and how it relates to hydrological modeling.

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.

The method is very similar, but the version of GPCC is different between WFDEI and GFDCL, which makes all the difference. This is already mentioned on several occasions in the paper, but we will make sure that this is even more clear.

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.

We have tried various versions of this, and found the current to be most clear in combination with brevity in the text. We will consider other ways to describe the data, and to perhaps remove some abbreviations. The GFDOD1 and GF-DOD2 refers to the two separate months of GFDOD, however, as there are only minor differences between these months, we will remove them from the paper completely and only discuss GFDOD.

6.) What is the effect on hydrological simulations when you update with the new obser-

vational 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.

This is indeed an issue. We will discuss this in more detail in the revision and also propose different solutions to the problem.

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.

The steps of the color scheme around zero (-5 to 5%) in this figure is hiding the systematic differences which on the whole has a large impact. We will adjust the steps in the plot.

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.

We have only made detailed simulations for the two norther regions, however, there are operational forecasts made with HYPE initialised with GFD for more tropical regions, such as Niger. We will link to such operational systems and their performance.

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

This will be added.

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.

Near real-time is of course in general not well defined and depends on the application. No system can every be "real-time" altough some describe their systems that way. The word "near" therefore works as an alert to the reader that it might not be up to their standards of what "real-time" is. We will therefore keep the title, as it points out the added value of GFD in comparison to WFDEI which is more epidodically updated. To mention but one similar definition; in the precentation of GLOFAS in Alfieri et al. (2013), ERA-Interim is introduced as near real-time data, with its three month delay.

Below, we silently accept all changes unless commented.

Minor comments:

- Please add the datasets used for WFDEI to Table 2

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

Its already there: Fan and van den Dool (2008)

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

- What is the difference between GFDOD1 and GFDOD2?

We remove these as described above.

- The nonlinear scale in Fig. 2 and 4 makes it very difficult to look at the results. Basically everything greater than +-25

We will revise these figures.

- 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.

We do not see what such a reference would add. Our method is clearly defined and there is no room for other studies showing different results.