We thank the reviewer for insightful and constructive comments and provide answers to the comments below. The reviewer comments are in normal font, our replies are in bold font.

General:

The paper is potentially interesting but the scientific issue/hypothesis of the paper at this moment is unclear. Besides the unclear hypothesis the manuscript does not go into the issues in enough depth. The paper maybe worthwhile to publish in HESS after improvements and added in depth analysis.

Probably, the focus of this paper is more in hydrologic engineering than hydrologic science. We do not propose a new scientific hypothesis or method. We combine different datasets and methods into an operational forecasting system for poorly gauged basins. There is some innovative value in doing this, as recognized, for instance, by reviewer 2. We agree that the scientific discussion lacks detail in places and will address that in the revision, following your detailed comments below.

Detailed comments:

Abstract I doubt/am not sure if it is appropriate to highlight TIGER-NET and the funding behind the project/paper in the abstract. It is unclear what a competitive forecast is.

We mention TIGER-NET not so much because of the funding, but because of the whole concept: Free, open-source, public-domain tools for IWRM in Africa. We would like to keep the reference, unless there is a clash with the journal's policies. "Competitive forecasts" is probably fuzzy wording. Maybe "useful forecasts" would be better?

The scientific hypothesis of the paper is unclear. What scientific issue/problem is being researched? The operational goals of the TigerNET project are listed, but this is inappropriate for a scientific study. What is the added value of the work conducted and what is the relationship to work done elsewhere/previously? Or is this just another case study? As a result, a clear experimental setup to test a hypothesis is missing and the scientific contributions stay unclear.

See answer to the general comment above. The purpose of the paper is to document how we reached the operational goals of TigerNET using free, open-source and public domain data and tools and to evaluate the performance of the system. It is more than a case study, because we describe a system that can be used for any basin and just use one particular basin for demonstration purposes.

In the introduction some DA papers are being mentioned. I think in operational hydrologic DA there is not yet, a preferred method, variational approach have also been proposed by Seo et al 2003/2009 and Lee et al 2012. Some papers also try to update both the hydrologic and routing states (see e.g. Rakovec et al., 2012)=>See Liu et al. 2012 for the references for all these papers.

Our intention is not to review or judge the merits of various DA approaches. We just mention that the Kalman filter is one of the most widely used methods, which is true to our knowledge. We discuss the issue of combined updating of hydrologic and routing states (see page 11090, lines 8-20). We agree that this discussion may be too superficial and will include the suggested references in the revision.

I looked at the reference (NOAA, 2014) but could not find the GFS forecast from 2006 onwards. There are only forecasts from 201208 onwards (ftp://nomads.ncdc.noaa.gov/GFS/Grid4/) are

available. Therefore, it is unclear what data is being used for Figure 2 and further results. It needs to be clear which data is being used otherwise it is impossible to judge the results.

The data was downloaded from
http://nomads.ncdc.noaa.gov/data.php#hires_weather_datasets
The most recent data is online on the server, the older data is archived offline but can be ordered for FTP download.We hope this answers the question, we could provide detailed technical descriptions of what exactly was done on the web interface but this would probably be beyond the scope of this discussion forum.

I also wondered why GFS is being used and not GEFS for which a hindcast exists from 1984 onwards, see ftp://ftp.cdc.noaa.gov/Projects/Reforecast2/). Especially, because in the discussion it is mentioned that no EPS is available but NOAA also provides GEFS/GENS (http://nomads.ncep.noaa.gov:9090/dods/gens) with 21 ensemble members. So this needs to be revised in the manuscript.

This is a very interesting comment, thank you. Using GEFS would definitely be a very interesting route for further development of the system. However, to our understanding, the different GEFS members differ only in terms of the initial conditions used for the forecasting run, and would thus only capture a fraction of total precipitation uncertainty. We will amend the discussion to bring in these ideas, but will not be able to set up our system with GEFS within the time frame of this revision. We will though definitely keep it in mind for the future.

Verification metrics: I think it is necessary to use persistence as reference forecast and analyse the CRPSS and maybe some other metrics (BSS, ROCS, etc) This may make clear what the source of the the skill is because the main question that remains unanswered in my opinion is where skill is coming from: updated initial conditions or bias corrected GFS forecasts or/and how important the hydrological model is especially for these short lead times.

We report the persistence index (cf table 4) in order to compare the performance of our system to the persistence reference (i.e. last available observation). We do not see how to compute CRPS of the persistence reference, as this would require an estimate of the standard deviation of the persistence which we do not have. Additional input as to how to understand/address this comment would be very helpful.