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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Water balance modelling in a semi-arid environment with limited in-situ data: remote sensing coupled with satellite gravimetry, Lake Manyara, East African Rift, Tanzania" *by* D. Deus et al.

**Anonymous Referee #2**

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General comments:

This paper pursues quantifying the water balance of Lake Manyara, Tanzania, using various ground-based and satellite-based data and the conceptual distributed hydrological model J2000. Standard freely available remote sensing products (e.g. TRMM, MODIS, SRTM) and few station data in this data-scarce region are used to characterize the study area, as model input and for model parameterization. Model applications for a study period of 9 years (2001-2009) are compared to what the authors call observation

data, such as GRACE-based water storage variations and station-based evapotranspiration (Fig. 9).

The study does not make any substantial contribution to advancing hydrological sciences. It is at best a report that outlines the hydro-climatology of a specific study area which has rarely been described before and for which few data are available. A major focus and claim of the study on using remote sensing data cuts down to the application of standard products without any particular new concepts. The potentially more innovative consideration of water storage variations from the GRACE gravity satellite mission for model validation is, in turn, completely flawed in this study due to the spatial scale mismatch of GRACE data (resolution greater than about 150 000 km$^2$) and the size of the study area (about 450 and 18 700 km$^2$ for the lake surface and the catchment area, respectively). The application of the J2000 model is unmotivated and does not convey any new idea or method for basin-scale hydrological modeling in data-scarce, semi-arid environments as dealt with here. Furthermore, besides of the major flaw of using GRACE data, the study is full of questionable, at least poorly described approaches as partly already mentioned by Referee #1 and of which some major points are listed below. The manuscript is mainly poorly written, with many imprecise wordings and explanations of approaches taken in this study, many repetitions, lengthy general and unnecessary pieces (e.g. on the water balance, chapter 4.1).

I'd like to stress that even if these issues will have been corrected or more clearly explained in a revised version, I cannot recommend publication of this study in HESS due its limited scientific contribution.

Major flaws / doubts:

1) GRACE data. The resolution of GRACE time-variable gravity data (greater than about 150 000 km$^2$) is at least one order of magnitude coarser than the study area (about 450 and 18 700 km$^2$ for the Lake Manyara surface area and the catchment area, respectively). The authors use GRACE data of the GRACE Tellus website. Besides



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of filtering with a Gaussian smoother of 300km radius, the basic GRACE spherical harmonic coefficients are considered only up to degree and order 60 in these data. This roughly corresponds to a wavelength of about 660 km or a pixel size of about 330x330km (110000 $km^2$). Any smaller-scale feature cannot be resolved in these data. Thus, this product does not have a specific information content for the study area of interest here which is at least one order of magnitude smaller (or even two orders of magnitude as in the present manuscript if only the lake surface area is considered). The results may at best give evidence for the fact that the study area shows similar dynamics to a larger scale region in its surroundings. The conclusion that GRACE data may be useful for smaller lakes and basins (page 8763) is not valid.

2) What is the 'true rainfall' reference that leads the authors to the statement that TRMM and GPCP underestimate precipitation in the catchment (page 8743, line 10)? How can they derive from the few station data they have throughout the study area a basin-average value that is comparable to TRMM resolution?

3) The authors use MODIS-based land surface temperature (page 8744). This is very probably not the quantity required by the model as input for the evapotranspiration equation (which is air temperature for the Penman-Mntheith approach), nor is it directly comparable to in-situ temperature data (fig. 5) which probably are air temperatures as well (although not exactly explained in the manuscript).

4) The time resolution of the modeling approach is not clear. On page 8752, line 3, the authors state a monthly time step, other instances give evidence of a daily time step (e.g. page 8750, requirements for ET calculations with daily resolution). In particular in view of significantly varying energy fluxes between daytime and night and the corresponding non-linear ET response, using mean daily values may not be adequate for the Penman-Monteith approach.

5) Figure 9 and page 8753. Model validation for evapotranspiration (ET). Of which type are the observed ET data? ET is generally difficult to measure, how can it be



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compared to model results? What exactly are the model and station data used for this comparison? How can it be argued that they are comparable in terms of, e.g., spatial scale, land cover type?

6) What is the difference between Fig. 11 and Fig. 16?

7) Given the lake change bars in Fig.15 , Lake Manyara should experience a steadily declining water volume as negative values predominate (even for year 2007). How do the data in this figure relate to results shown in Fig. 18? Lake volume changes do not seem to be consistent in terms of magnitude between the two figures.

8) Extending my comment 7), how can a predominantly positive lake water balance as given in Fig. 17 explain the steady decrease of water volume in the lake?

9) From a statistical point of view, it does not seem to be valid to draw a linear regression for the heterogeneous distribution of Figure 19.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8737, 2011.