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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 #3

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This paper presents model results of a water balance simulation over the Lake Manyara catchment area and compares these results to GRACE gravimetry measurements of storage change over that region, as well as altimetry of nearby lakes. In simulations of the catchment, evapotranspiration/evaporation is the primary water export from the system. The scientific question addressed by this paper is: Can we simulate the water balance in the semi-arid Lake Manyara catchment using the J2000g model and does this match what remote sensing data would suggest? The authors find that J2000g

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model simulations of evapotranspiration and lake water balance are consistent with observations of evapotranspiration and GRACE equivalent water thickness, respectively.

Hydrologic modeling and remote sensing fall within the scope of HESS; however, the paper does not present novel concepts, ideas, tools, or data. The paper is not about the development of the model or of the data set against which modeling results are compared. Since the authors do not address what their results suggest about the processes involved or the broader implications of these results, I do not recommend this paper for publication.

The authors suggest that they combine remote sensing data and in situ observations with model simulations; however, although several such data sets are described (in great detail at points), the paper does not clearly state which and how remote sensing data is incorporated into the model. Furthermore, the paper shows a somewhat circular logic in that the authors state both that they are using the model results to "test the utility of GRACE satellite gravimetry total water storage (TWS) data on a smaller un-gauged lake catchment basins and minor lakes that are not included in the global satellite altimetry mission network" and that "the results were validated by comparing. . .the lake water balance with. . .satellite gravimetry GRACE equivalent water thickness data. . ."

Which remote sensing data sets were used as input to the model and which were compared to the model are of key importance in this study; however, the authors refer to 2 different remotely sensed precipitation data sets and 1 in situ data set without stating which was used to force the model. Similarly in situ and remotely sensed temperature data sets were described without saying which went into the model. Also, virtually no details are given on the source and quality of the ground-based meteorological observations. As commented by referee #2, the type of ET measurement can be quite important.

A lot of space is spent describing the parts of the J2000g model that are pretty basic,

HESSD

8, C5029–C5032, 2011

such as the water balance equation, and ET calculations that could have simply been cited. For this paper, the connections between the lake and terrestrial area should have been more clearly described. For example, is the land surface allowed to runoff into the lake? Does baseflow transfer between the terrestrial area and the lake in one or both directions? How is flow routed over the land surface? How are baseflow and runoff calculated? Without this information, it would not be possible for fellow scientists to reproduce the results. The authors also suggest that future efforts should address groundwater interactions; however, without a description how the subsurface processes are currently represented (and given that groundwater accounted for only 0.27% of the total catchment inflow in Table 2), this is a difficult assertion to assess. In addition, "groundwater" is listed as an inflow in Table 2, but from the description of the modeling approach, I wonder where this groundwater inflow is coming from, the lake?

The paper may have been more novel had the authors compared the model performance given in situ meteorological forcing to that given remotely sensed meteorological forcing. The breakdown of the water budget and its temporal variability might have been more interesting had the authors considered what the data say about the variability in the region. The authors mention in the introduction: "In the East African Rift the topography of the rift escarpments and volcanic highlands plays an important role in controlling local climate and thus the lake basins." The authors never address what role the topography played in the simulated water budget. There are plenty of questions raised peripherally in this paper that are not addressed in the discussion but that, if addressed, would enrich the scientific merit of the work. As it stands, I agree with reviewer #2 that the paper currently reports the results of a model simulation over one basin without significantly advancing hydrologic science.

Additional comments: 1. Does the title clearly reflect the contents of the paper? The title does not make sense. Satellite gravimetry is a form of remote sensing. The title says "remote sensing coupled with satellite gravimetry", which might imply that the satellite gravimetry was somehow integrated into a modeling framework with other re-

HESSD

8, C5029–C5032, 2011

mote sensing data; however, GRACE data was simply evaluated against a model that was forced with other remote sensing data. 2. Is the overall presentation well structured and clear? The paper did not flow very well. The methods, results, and discussion were mixed in many places. 3. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? *The colors and labels on the axes do not match the colors and labels in the legend for many cases in Figs. 10, 11, 16, 17. *Overall, I found the level of detail given to the input data sets to be excessive. Since the authors did not derive or modify the data sets, it would be more appropriate to simply cite the source, time step, spatial resolution. *In the section on topography, the authors say: “Surface runoff causes soil erosion”, and though true, this is irrelevant to their study. *In section 3.1.1, the authors should clarify which precipitation data set they used or how they merged data sets. *The discussion of soil type (3.1.2) is inappropriately detailed given that a soil map is not provided and the effects of soil type are never discussed.

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