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Hydrology and Earth System Sciences Discussions

Interactive comment on "A simple groundwater scheme in the TRIP river routing model: global off-line evaluation against GRACE terrestrial water storage estimates and observed river discharges" *by* J.-P. Vergnes and B. Decharme

Anonymous Referee #2

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General comments

This paper describes the global-scale application and evaluation of a physically-based hydrogeological module coupled to the TRIP routing scheme, with a spatial resolution compatible with global climate models. Two independent validation data sets are used, viz. a long-term data base of about 3500 river discharge measurements (1900 potentially influenced by groundwater), and the terrestrial water storage (TWS) deduced from GRACE over 6 years. In this assessment, the drainage and surface runoff simulated off-line by the ISBA land surface model are used as input to the system, the drainage term constituting the groundwater recharge. The paper includes an analysis of the sensitivity of the simulated discharges and TWS to the precipitation forcing of ISBA, whether they are corrected toward the GPCC or CRU data.

I agree with all the comments of Reviewer 1. This work is original and quite well described, and it is an important contribution to the land surface modeling community. Of particular relevance is the global-scale characterization of the groundwater system from available hydrogeological information. Thus, I recommend this paper to be accepted for publication in HESS, subjected upon minor revisions to account for the comments below.

Specific comments

Model description (Section 2)

- Are Tθ and Tϕ really different? W should be river width.
- There is a unit inconsistency around q_{riv} and Q_{riv}, which are written to be in m/s and m^3/s respectively, whereas L13 p 8219 says that Q_{riv} has to be converted to kg/s
- I would appreciate a better explanation of the different elevations and heights than a mere reference to Decharme et al. (2012). A sketch could be useful here.

Model parametrization (Section 3.1)

- What is the advantage of GMTED2010 against the widely-used Hydro1k hydrologically conditioned DEM?
- P8221, L9: what is “this category”? According to the text, it should be the latter one, thus the “complex hydrogeological structures”, but since they are overlooked

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in the model, “this category” must rather be the first one, thus the “major groundwater basins”. Please clarify this.

- Like Reviewer 1, I would have liked some complements on the final ground water layer in the TRIP model: fraction of the continents with modeled aquifers, % of the modeled aquifers covered by the different lithologies/parameter sets of Table 1.
- I did not understand whether carbonate-rock aquifers were removed only the upper Mississippi basin or world-wide (p8221, L20-21). If not world-wide, how do you justify this? Do you have information about the actual karstification?
- I would also have appreciated a brief discussion about the relevance of the selected aquifer systems for global land surface and climate modeling, in particular regarding the water table depth (WTD), which is known to be crucial for groundwater/surface interactions. Section 3.2 mentions the computation of an equilibrium WTD for initialization, and the mean WTD over the 1960-2008 period could also be calculated. I would be very interested by the spatial and even more by the statistical distribution of this WTD. If I understand that a thorough validation of this field is not devisable because of the scale mismatch between wells and TRIP grid-cells, couldn't some regional assessments be made in densely surveyed zones, such as in the US (see Fan et al., 2007)? If such assessment was made in France in Vergnes et al. (2012), couldn't you remind the main conclusions?

Main evaluation results (Section 4.1 and 4.2)

My first concern is about the definitions of the annual ratio and efficiency, which are not reserved terms and need to be specified. This should include a reference to Nash and Sutcliffe (1970) for the efficiency, and the meaning of RMSE should be stated. Reviewer 1 asked if the efficiencies were computed on daily or monthly values, and the same question applies to the correlations. I also wonder if it is really useful to

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give the used statistics on the full time series and on the monthly anomalies, since the responses are logically very similar.

Secondly, the analysis is very descriptive, and all arising questions are not answered in the discussion part. The ones I would have liked to get answers to are:

- Why is there a deterioration of the river discharge score with ground water in North America?
- Is there a simulated aquifer in the Mekong basin? More generally, it would be nice if the areas where aquifers are simulated could appear on the maps, maybe by hatching.
- Most of the remaining flaws in simulation GW are attributed to the absence of flooding processes in TRIP: can't other processes be poorly be represented, such as river velocity in TRIP, or the surface water budget in ISBA (especially where the annual ration is significantly different from 1)?

Lastly, it is written that "In general, groundwater increases the memory of the system by shifting the TWs signal" (p8228, L6-7). The simulation design would allow the authors to go beyond this very general statement, and to provide interesting pieces of evidence, using for instance lagged correlation or spectral analysis.

Sensitivity to the precipitation forcing (section 4.3)

I found this part rather weak compared to the rest. I would suggest either to remove it, or to strengthen it, by explaining the rationale of this sensitivity analysis, in particular with respect to groundwater modeling. It would also be interesting to give some quantification of the differences induced by the precipitation forcing, for instance using histograms for TWS and precipitation itself. Spatial means would also give interesting quantitative insights.

Discussion and Conclusions

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These two sections exhibit many repetitions and could probably be condensed into one section. Moreover, some conclusions are overly strong, because not well supported by the results. It is the case regarding the memory of the system (see above), the water table head distribution (P8230, L20-22; p8234, L10-12), the advantage of GPCC over CRU (p8232, L27-28), or the more realistic baseflow (p8234, L4). A more specific comment regards the deterioration of efficiency scores in some areas, including the eastern part of the Mississippi river, which is related to deficiencies in the WHYMAP data base (P8230, L20 to P8231, L11). Yet, Section 3.1 mentions that a USGS hydrogeological map was also used in the US. Could you please discuss this more thoroughly?

Technical corrections

P8216, L10: calibrated AGAINST in situ measurements

P8218, last line: corresponding HERE to the effective porosity

P8221, L10: think of a better expression than “squeeze out”

P8226, L3: remove the first occurrence of “the global”

P8226, L16: “what” instead of “which”

P8226, L18: “station” instead of “stations”

P8226, L19: “of” can be removed

P8231, L16-17: I would not use “temporal gap... filled in”, but maybe “delay... reduced”

P8234, L5: contributes TO storage

Reference not cited in the paper

Nash, J. E. and J. V. Sutcliffe (1970), River flow forecasting through conceptual models part I - A discussion of principles, Journal of Hydrology, 10 (3), 282–290.

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