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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Global-scale modeling of groundwater recharge" *by* P. Döll and K. Fiedler

**Anonymous Referee #3**

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Review of "Global-scale modeling of groundwater recharge" by Döll and Fiedler

The paper describes the usage of the global water balance model WGHM to simulate global groundwater recharge at 0.5 degree resolution. The model is tuned against discharge measurement at more than 1000 gauges globally. The groundwater recharge is a model internal variable and is not used in the model tuning, except for a rough comparison with a modified groundwater recharge algorithm for the arid areas. Global-scale groundwater recharge had not been reported earlier with the other global water balance models. The authors have studied earlier estimates of groundwater recharge and noted that some values are not updated in these publications but can be traced back though different publications to L'vovich (1979).

The manuscript is well written and I enjoyed reading it. After addition of some (not too



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long!) information on the uncertainties and representatively of the model results as well as some minor corrections I support its publication in HESS.

My general concern is about the different uncertainties in the simulation results. The authors have used two precipitation datasets as precipitation uncertainty is a major source uncertainty in these types of models. They then take the average of the simulation results with these two datasets as the best estimate. This is reasonable, but instead of stating that, e.g., the global groundwater recharge is 12666km3/yr I would like to see a śxxx after this number, reflecting the two results with the different precipitation datasets. This is not the full uncertainty in the results, but at reasonable estimate. I do not think the readability of the paper will decrease by adding these numbers to the text.

WGHM is a conceptual hydrological model, where the simulated runoff is divided into "fast surface and subsurface runoff Rs groundwater recharge Rg" (p. 4076). It is not explained what differs the subsurface runoff from the groundwater recharge. I also miss a discussion on the representatively of the simulated groundwater recharge to actual measured recharge. The division into fast runoff and groundwater recharge in conceptual models is rather made to get a good fit to the measured hydrograph but not to mimic the actual flow paths. A comparison with measurements is made in Fig. 3 for semi-arid areas, but what about the humid? It surprises me that Fig 8 show rather few areas with a groundwater recharge share of the total runoff above 50%, while I had expected humid areas to have a groundwater share of 60-90%. Do you have any explanation to this? How come that e.g. Finland and Sweden has a lower groundwater recharge in Fig. 8 than Germany/Poland/Denmark and that such small areas of North America have a groundwater recharge higher than 50%? On p. 4072 is the scale-dependence mentioned, but it would also be interesting to know how you consider that in connection to the measurements in Fig. 3.

In table B1 I suggest an additional column, stating the percentage of cells of the country belonging to calibrated basins where the estimates can be expected to be somewhat



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more certain, at least when no correction factor is applied to the calibration. I also miss a discussion of the latter, i.e., the influence on the groundwater recharge ratio to the total runoff in areas where the correction if the total runoff is applied.

Further specific comments:

Earlier groundwater recharge studies with WGHM are mentioned on p. 4073-74, but no comparison with these values is given in the discussion. It would be interesting to know if the new algorithm for arid areas and the updated dataset of gauged stations cause large or small deviations to the earlier results. Additionally, is it the results of this manuscript that will be included in WHYMAP (p. 4074) or some further developed results?

The explanation of the catch ratios (p. 4082, row 10-12) is not clear. Did you use the data by Adam and Lettenmaier, or did you do this analysis yourself? In the latter case: - What was the source of the climatic stations? - Was both measured and actual precipitation provided at these climatic stations, or was it the wind that you used to calculate a correction factor? Did you use the temperature too, or was it only used in eq. (3)?

The Method part should state that the model is calibrated against the runoff with both precipitation dataset. Right now I found that information in the Results part at first.

What was the source of the country border data? This is especially interesting as you exemplify with several countries which sovereignty too my understanding is not internationally totally clear (Falkland Islands, Svalbard, Western Sahara) while another discussed country, Taiwan, is not included table B1. Hydrological papers are not about diplomacy, so I have no problems with your list, but I would like to know the source of your country borders. More interesting, scientifically, is the exemplification with these rather remote countries, where you have used a regionalised model parameter instead of calibration. As I assumed earlier, that makes the results less reliable for those countries.



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p. 4084, row 5-6 says: "except for the dry Australia and Oceania". What is the size of Australia and Oceania in your continental division compared to Europe? Could it be a size effect too?

Are the B/A (when calculated for the two precipitation datasets, separately, and not on their average) very similar between the two precipitation datasets in Table 1 and B1, or can you see any effect of the absolute limit for groundwater recharge in semi-arid areas between the two precipitation datasets?

Can you please discuss why the deviation is 1.1% for the groundwater recharge for the total land area, but only 0.02% for the total renewable water resources, while, on the other hand, the two deviations are in the same range for the continents (Table 1). The deviations for the groundwater recharge seem to sum up to the deviation for the total land area, while the deviations for the total renewable water resources seem to even out for the total land area. Is the latter because of the calibration, or because the two precipitation datasets even out in the ungauged areas? The small (0.02%) difference is surprising when the difference is 7% between the new and old total global runoff estimate (p. 4086). Is the difference in the ungauged areas between the two precipitation datasets, used in the current paper, small, while they differ a lot from the old precipitation dataset in the ungauged areas?

Can you please motivate further why you use the net cell runoff as a measure of the total water resources, instead of the total runoff (p. 4086 / Appendix B)? Why should the evaporation of water that is added from upstream cells be included, when not the runoff of these is included? To me, it seems more natural to use the total cell runoff, as you do in Figure 8. Additionally, why does the number of total water resources for Germany differ so much between p. 490 (397 mm/yr) and table B1 (316 mm/yr). The evaporation from open waters of "imported" rivers cannot be 81 mm/yr, can it?

"Comparing simulated groundwater recharge with the independent estimates in Fig. 9" (p 4088, row 15): I did not understand that it was the FAO estimate that was meant



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here as it is so much criticised in the sentence before and as you now consider it as a truth for the arid areas.

How is the modelling efficiency (e.g. p. 4088) calculated?

Can you exemplify with some other countries on p. 4089 than Brunei and Réunion, as these are not included in Table B1?

Are the f values arbitrary set or have you tested several different values? Are they the same throughout the earlier published WGHM results, or have you changed them over the years? Have you tested if the results are very sensitive to the selection of the f values?

Table A1: I suppose that ravg here is if the whole cell has the same slope class, while eq. A1 describes the calculation of a more common ravg that do not have any of these distinct values, but it would be good if it could be clarified in the header of this table.

What is meant with "The coverage classes were related to the average areal coverage value Cpg." (p. 4097)?

Why did you need to rasterise the permafrost map when you write that you Brown (1998) provide digital data?

Technical corrections

Table 1: "renewable groundwater resourcese" should be placed in the last column, not in the second last.

long-term instead of lomg-term (p. 4098)

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