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Hydrology and Earth System Sciences Discussions

Interactive comment on "What can we learn from long-term groundwater data to improve climate change impact studies?" *by* S. Stoll et al.

Anonymous Referee #1

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General comments

This paper describes a study within which long term datasets of groundwater level and spring discharge and potential recharge modeling results are interpreted to infer the sources of variability within the groundwater datasets. The source of the variability is assigned depending on whether patterns are observed in all datasets (ascribed to a climatic source), in just groundwater levels and spring data (ascribed to a landuse change source) or solely in groundwater levels (ascribed to an abstraction source). Whilst the basis of the interpretation is superficially attractive, it is underpinned by many questionable (largely implicit) assumptions which undermine the value the study to an unknown extent.

Specific comments

1) There appears to be a surprising presupposition in the Introduction that hydrogeologists doing climate impact studies do not first try and understand system behavior under current conditions (e.g. P7624, L1-3) before starting to assess future conditions. The development of a robust conceptual model of a system is a fundamental prerequisite for any impacts modeling. That the authors final conclusion (P7638, L26) that the focus should be "on extreme groundwater drought periods rather than analysing annual averages" is disappointing in its unintentionally-implied criticism of hydrogeologists working in this subject area.

2) Potential recharge modeling using a 1D MIKE SHE model is use to determine the influence of landuse change, since the recharge outputs from the model do not incorporate landuse change whereas the impacts of any landuse change on recharge are assumed to be present within the time series groundwater data. However, there are a number of assumptions within the modelling that weaken the presupposed relationship – in essence it is assumed that the model is a perfect representation of recharge assuming no landuse change, so that any deviations between model and observations can be related to land use. In particular, the model is parameterized with the landuse around the monitoring site (rather than over the source's catchment area); runoff is neglected, so any changes in the rainfall-runoff relationship associated with changing soil conditions (e.g. compaction) or rainfall intensity are ignored; uncertainty in the estimated reference ET is ignored; all sites are assumed to have identical soils (a Cambisol, which is not even a soil type but a broad soil grouping); and there is no calibration nor assessment of the plausibility of the model outputs. Rather than the authors statement that "if certain patterns of change are observable in the groundwater level and spring data, but not in the recharge calculatiuns, the observed patterns are likely to be related to land use changes" (P7632, L19-21), I beleive that they are as likely (or more likely) to be due to conceptualisation, model and data limitations.

3) In P7631, is it surprising that there should be correlation over space, given that

it has been assumed that there is no runoff, that the soils are spatially identical, and differences in simulated percolation/potential recharge are driven solely by precipitation and actual ET? PET generally has large scale spatial structure, as do dominant modes of precipitation, whether convective, orographic or frontal? No information is given as to the main precipitation drivers in the region, which might be useful to interpret the findings.

4) The assumption of the perfect model is also apparent when the paper identifies abstraction impacts – "some groundwater levels show much more pronounced drought periods than model calculations or spring discharges. . .... this feature would be associated with pumping activity" (P7633). If the actual soil at some sites/catchments deviates from that assumed in the modeling, this could have an important impact upon the actual ET, maximum soil moisture deficit and the timing of the onset of the autumn/winter recharge. Whilst irrigation might be important as suggested by the text, no information is presented to substantiate this.

Technical corrections

The text on P7627, L29 to P7628, L5 is unclear

Conclusion 1 – the "strong relationship between winter precipitation and groundwater droughts" hardly represents the importance of the "detailed representation of the temporal precipitation distribution"! Besides the importance of temporal distribution of precipitation has been shown in the results of a number of earlier climate impact studies.

Conclusion 3 mixes the impacts of future climate change, non-climate/socio-economic hange and adaptation responses.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7621, 2011.

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8, C4179–C4181, 2011

Interactive Comment