Hydrol. Earth Syst. Sci. Discuss., 9, C6250–C6254, 2013
www.hydrol-earth-syst-sci-discuss.net/9/C6250/2013/

Hydrology and Earth System Sciences Discussions

Interactive comment on "A critical assessment of simple recharge models: application to the UK Chalk" *by* A. M. Ireson and A. P. Butler

Anonymous Referee #2

Received and published: 11 January 2013

General comments

The paper's topic is interesting and important but the paper is still very far away from publication. Several key areas remain unclear, and the motivation/ rational for a range of important simplifications and approaches are not articulated convincingly, see below for details. Some key references to recent calibration and inversion approaches on Richard equation type models are missing. The paper is also very long and somewhat unfocused. I have a range of suggestions below that might help to address these issues. Apart from these structural aspects I am challenging the verification of the proposed framework. The abstract and most sections of the paper read as if it is all based on field data. However, closer examination of the calibration strategy suggests that de-

spite the field-data touch of the paper, it is a modeling exercise based on synthetic data (Refer to comment 1 for details). The authors have therefore demonstrated that their suggested framework is useful in comparing different modeling approaches to the synthetic data generated by this particular Richards model (itself an extremely simple 2D model). Whether or not the framework is of any use to real field data or more complex geometries and topographies is implicitly assumed, but not shown. I therefore find the conclusions overstated and not convincingly established. A framework that is sold as a "rigorous quantification of the timing and magnitude of groundwater recharge" (see first paragraph in the conclusions) cannot be based on synthetic data of an oversimplified 2d model. I nevertheless think the authors should be given a chance to revise the paper and therefore suggest a major revision. I am happy to re-review a potential submission of the paper.

1) If I understood the calibration strategy correctly, a Richards equation model parameterized with calibrated parameters from another site (page page 12070, line 13) is used. The boundary conditions are then adjusted until the observation data at a new location are reproduced. This is already a key issue— the gradient through the system co-varies with the hydraulic conductivity. In a 2d model there will be multiple combinations of K and a gradient that reproduces the available heads. This "calibrated" model is then used to generate synthetic data. This synthetic data is subsequently used in the model comparison, not real field data. The paper should clearly state that this is a synthetic modeling exercise and tune down the general applicability of the method in the conclusions.

2) Only 9 parameter combinations were found for the Ruston and Ward model. This is a disappointingly low number, but rather typical for pure MC analysis. There is no reason to believe that the 9 combinations are a sufficient sample of the parameter space for subsequent analysis. I was also wondering about the used objective function, no details are given on this point.

3) A DEM is available for the area. Even in the 2d approach the changing slope could

have been considered, but nevertheless the slope is assumed constant in Fig. 1. Given the choice of the ET model, the depth to groundwater will be important and therefore even small elevation differences could affect recharge dynamics as well as surface ponding or runoff, which are not taken into account (12072, line 17).No reason is given for this simplification.

4) The role of the Duffy model, and the fits to ET in particular seem rather irrelevant for this paper.

5) I do not understand the section on hydraulic properties. The authors argue that preferential flow is ignored, because it is unimportant. 2 lines later a fracture with a hydraulic conductivity of 4000 m d-1 is introduced. Wouldn't this fracture have exactly the same effect as preferential flow introduced through e.g. dual porosity?

6) The reasons to use a 2d model only are not convincing. Pumping seems to play a role, therefore 3D effects would be important. I agree with the authors that with their calibration approaches parameter space cannot be searched efficiently, but this is no excuse to limit the study to 2d. There are more efficient methods that could tackle such problems, see below for suggestions.

Other comments

- Perhaps a flow-chart with the main steps would help to clarify the calibration strategy.

- The short literature review on estimating groundwater recharge: The mentioned methods yield only point measurements, there is no mention on methods that yield spatial distributions. I suggest adding the paper of Szilagyi, 2011 and Brunner 2004. Using such data in the proposed framework would be an interesting future undertaking

- The authors should at least mention some of the more efficient calibration methods. The MC method suggested here are extremely inefficient. This type of problems would be a perfect application of subspace methods such as Null Space Monte Carlo (e.g. Keating et al 2010) or Marcov Chain approaches. A recent wwr paper (Brunner, Do-

herty, Simmons, WRR, 2012) on data worth in calibrating recharge models showed that the dimensionality of the parameter space and the parameter identifiability can be calculated efficiently using such subspace methods. As the implications for model simplification are discussed, this paper should be cited here.

- The figures are hard to read and should be improved. For example, figure 1 shows a large map of the UK, but the conceptual model setup is very small.

- Figure 5 and 6 could be deleted

- The conclusions are very long. The key messages are hard to identify.

- Units in equation 17 are missing, I assume this I 6m2/d? is this number based on the try and error result of the boundary search on page 12071?

- I guess equation 11 (page 12073) should be written for the denominator field capacity minus wilting point and not the other way round.

Fig 8: Too much information in one figure, very hard to read. BP is BF in fig c? Fig c is confusing, the caption mentions identifiability but then RMS is shown. Are the enlarged points used to calculate identifiability? Same issues (including typo) in fig. 10

Szilagyi, J., V. Zlotnik, J. Gates, and J. Jozsa (2011), Mapping mean annual groundwater recharge in the Nebraska Sand Hills, USA, Hydrogeology Journal, 19(8), 1503-1513.

Brunner, P., P. Bauer, M. Eugster, and W. Kinzelbach (2004), Using remote sensing to regionalize local precipitation recharge rates obtained from the Chloride Method, JOURNAL OF HYDROLOGY, 294(4), 241-250.

Keating, E. H., J. Doherty, J. A. Vrugt, and Q. J. Kang (2010), Optimization and uncertainty assessment of strongly nonlinear groundwater models with high parameter dimensionality, Water Resour Res, 46.

Brunner, P., J. Doherty, and C. T. Simmons (2012), Uncertainty assessment and implications for data acquisition in support of integrated hydrologic models, Water Resour Res, 48,W07513.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12061, 2012.