Hydrol. Earth Syst. Sci. Discuss., 9, C6316–C6320, 2013
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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

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Received and published: 15 January 2013

We thank anonymous reviewer #2 for this review. Whilst there are some useful points within this review, particularly with respect to clarity about our objectives, we feel there are also some fairly major misunderstandings and misreadings of our manuscript. We will try to clarify things for the reviewer in this response, and we will address the legitimate and useful points raised by the reviewer in a revised manuscript. In the original manuscript we presented a "benchmark" detailed model to simulate a continuous groundwater recharge signal, which we then we tried to reproduce using two simple modeling approaches. In a revised manuscript, we will remove the first of these simple modeling approaches, namely "emulation by linear models", from the paper altogether.



Whilst this analysis does present some novel and important findings, these are somewhat peripheral to the overall focus and conclusions of the manuscript. Removing this section will also address some of the reviewers concerns (point 4 below). The revised paper will be considerably shorter and more focused. These changes are also in response to more substantive comments by anonymous reviewer #1.

Specific comments:

1) The term "synthetic data" is inappropriate, and it is not correct to describe this as a "synthetic modeling exercise". We are using a physically based model to reproduce the observed water table response along a hillslope transect, and extracting from that the simulated recharge flux. We are doing this because recharge is not observable, and the premise of our paper is that this hybrid model-data approach offers us a rigorous way to test recharge models that data alone, or indeed models alone, do not allow. We will try to clarify the approach used in the text, and will describe this as a "hybrid approach". We expand on this in the response to reviewer #1.

2) A Monte Carlo analysis was performed using the simple model, and is described in Section 3.2. We took 10,000 parameter combinations, not 9. The reviewer is presumably referring to the sentence that reads "This allows us to explore all of the model configurations considered by Rushton and Ward (1979), who were only able to consider 9 parameter combinations due to the limitations on computing power at that time" (P12079, L18). We also do describe the objective function that was used (P12080, L 3, which reads "The objective function minimized the root mean squared error of the water table simulated by the simple and benchmark models, at a point in the centre of the hillslope transect, in order to minimize the impact of the lateral boundary conditions on the response"). We would respectful suggest that it appears from this comment that the reviewer hasn't read the paper very carefully.

3) The reviewer here states that no reason was given for not taking ponding or runoff into account, and references P12072, L17. This is incorrect – a reason was given

on the exact line they are referencing, which reads “The high saturated hydraulic conductivity at the surface means that there is no need to account for surface ponding or runoff.” We do not think this would be unclear to readers of HESS, but we will anyway include a reference and modify the sentence to read “The high saturated hydraulic conductivity, and hence high infiltration capacity, at the ground surface means that there is no need to account for surface ponding or runoff (Mathias et al., 2006).”

4) We concede this point to the reviewer. This section is not central to our overall argument, and we have therefore removed it.

5) This is a fair comment and we will modify the text to clarify what we are arguing. As we responded to Reviewer #1: “We do not consider preferential flow and fracture flow to be the same thing, as discussed in the citations given here. We've clarified this in the text. In our view it is necessary to consider two forms of fracture flow: preferential and non-preferential. This distinction has been discussed and justified elsewhere (Ireson & Butler, 2011; Ireson et al., 2012). Essentially, non-preferential fracture flow occurs when the matrix and fractures respond in equilibrium and thus the equivalent continuum approach is justified. Terminology is of course subjective and debatable – we hope that we have provided an adequate definition/explanation of our use of terminology here, so that even if there is not a universal consensus on this, our approach is at least transparent.”

6) Pumping does not play an important role in our transect, except during two droughts when the groundwater was pumped to supply a stream, and we have noted this limitation during these periods. We aim to apply the simplest model possible to the field locations that we are interested in, which in this case is a transect that (we believe, based on regional head data and regional models, cited in the paper) follows a groundwater flow line. Hence a 2D model is justified. Moreover, the purpose of the hillslope model is to provide a robust estimate of recharge at a point in the centre of the hillslope, and nothing else. It is necessary to use a hillslope model to do this, because the recharge signal is unobservable, whilst groundwater levels – an integrated response to



recharge – can be (and are) observed, and hence the recharge model can be validated against them. We would again refer the reviewer to our response to reviewer #1.

Other comments:

- We will consider including a flow chart, or schematic diagram describing the overall approach we used, including the two different models. The calibration strategy, per se, is not complicated, and does not require any further elaboration.

- We will expand our review, taking into consideration the suggested references.

- Regarding more efficient calibration strategies, we see no reason to address these in this paper. The only model calibration is using a simple recharge model that is not at all computationally demanding (described in section 3.2) and whilst Monte Carlo methods may not be the most efficient way to find an optimum parameter set, they are simple and effective means for exploring the parameter space.

- Figure 1 will be improved, as per the reviewer's suggestions.

- Fig 5&6 – will be deleted, as per the reviewer's suggestions.

- We have shorted the conclusions considerably, removing the first paragraph altogether and removing reference to section 3.1 (now removed). We will try to tighten up the key messages.

- The reviewer is correct – units will be including in Equation 17. The number used was based on a trial and error search.

- Equation 11 is correct as given.

- Errors with labelling will be corrected in Figure 8 and 10. The dotty plots are used to indicate parameter identifiability. The highlighted 10 points are the 10 best solutions, and the output from each of these is shown in the time series plot in these figures, indicating that the model performance is essential identical over this range of parameter values (equifinality, as discussed in the text in Section 3.3).

References:

Ireson, A. M., Butler, A. P., 2011. Controls on preferential recharge to Chalk aquifers. J Hydrol. 398, 109–123.

Ireson, A. M., Butler, A. P., Wheater, H. S., 2012. Evidence for the onset and persistence with depth of preferential flow in unsaturated fractured porous media. Hydrology Research, 43 (5) p 707–719.

Mathias, S.A., Butler, A.P., Jackson, B.M. and Wheater, H.S., 2006. Transient simulations of flow and transport in the Chalk unsaturated zone. J Hydrol, 330, p 10-28.

Rushton, K.R. & Ward, C., 1979. The estimation of groundwater recharge. J Hydrol, 41, p 345-361.

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