Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving confidence in deep drainage estimates, for arid and semi-arid areas using multiple linear regression with percent clay content and rainfall" *by* D. L. Wohling et al.

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This paper makes a useful contribution to the literature on the development of generic recharge relationships and is of particular relevance to the Australian situation. I believe with more consideration the structure of the paper could be improved which would considerably strengthen the appeal of the paper. I recommend the manuscript for publication with minor corrections. Comments specific to each section and detailed comments are provided below.

Comments specific to each section.

Introduction First paragraph should be revamped. As it currently stands it is not particularly inviting and the sentences don't link well together. Sentences starting on lines 19, 20 and 22 (pg 4536) are an example. The leading sentence needs to be attention grabbing, at the moment it isn't.

I'm not keen on the use of the word 'drainage' in the context of potential recharge. There are already enough terms in use for potential recharge without adding another. Besides which 'drainage' has a lot of other connotations in hydrology. If the authors want to use drainage, then please stick with the more conventional 'deep drainage', the use of just drainage is too ambiguous.

Methods Tense changes between present and past, please be consistent. E.g. Ln16 "...we filtered the database..." and then ln20 "... that the MLR is preformed.". Ln 21. "The data was re-queried...":

I question whether Ln 15-30 (page 4540) and 1-25 (page 4541) are necessary. It is a very dry read (even as a reviewer I just skimmed through this, without really taking any of it in) and the information could be easily summarised in table form. I also question whether this text belongs in the Methods section – a summary table yes, but study by study descriptions I think not.

I would suggest setting the statistical methods section up in terms of Hypothesis to test (e.g. as per Petheram et al. 2002 – as that is on hand, but there are many other examples in the literature). Clearly stating each hypothesis concisely sets out the logic of the paper and facilitates a succinct results section.

Results Currently the results seem to be a bit of a ramble. I couldn't see a structure and my attention drifted. If the statistical methods section set out the testable hypothesis clearly, then the results can be concisely reported in tabular form. This helps to make a clear distinction between results and discussion (which may be a little more speculative in nature).

Discussion Makes some important points, particularly the applicability of developing and applying empirical relationships such as these with a GIS framework. Some discussion on the applicability of the relationships in countries other than Australia may be useful to an international audience (given the differences b/w Australian soils and vegetation and those of the northern hemisphere for example).

Detailed comments

Abstract

Page 4536 Ln 6 – poorly justified?? Ln 10 – insert 'average' . . .clay content of the top 2m. My first reaction on reading this was do you mean average or maximum clay content. From my reading I later found out it was average, but this should be said in the abstract as this is an important point/finding.

Ln 12- two comments. i). Are you sure about this? What about Watson et al. 1976 for example, they provided 95% confidence intervals on recharge estimates made in different rainfall zones of Nevada. ii). This doesn't fully account for the uncertainty in using these data to estimate deep drainage. It doesn't account for measurement error, errors due to assumptions (a potentially systematic source of error) or systematic bias in where measurements are taken for example. There is also the question of scale. Recharge is scale dependent. Uncertainty estimates are provided but what scale are these estimates applicable (i.e. point, paddock, hillslope, subcatchment, region)? This is not addressed in the paper? If the data were predominantly point scale measurements (i.e. based on chloride displacement) then it would be wrong to assert that these uncertainty bounds were applicable when estimating recharge at the catchment scale (well I certainly wouldn't be using them).

Introduction

Page 4536 Ln 15 – The leading sentence needs to be attention grabbing, at the moment it isn't. Ln 26&27 – 'transient environment' – jargon and ambiguous to the layman

(non-recharge specialist).

Pg 4537 Line 1 – it's not just limited to the CFD method. What's written is not particularly accurate. Basically it is a balance between the resource use/value and the cost. If the land was high value (e.g. as a nuclear waste repository or irrigated land) then expensive measurements become viable. Line 5 – are you sure Kennett-Smith et al. (1994) was the first to introduce a surrogate measurement for drainage? What about Turc (1954), Mandel & Shiftan (1981), Sinha & Sharma (1988), etc or Sophocleous (1992) as you mention later. Ln 19 – 'aforementioned work'?? which? Ln 18 – logic is difficult to follow here. Many, many studies have shown rainfall explains a large degree of variation in observed recharge variation (e.g. Petheram et al. 2002 found it explained 60% of the variation under annuals). However, the use of Fig 2 (a relationship between clay content and recharge) seems to be a rather convoluted way of making this point? Perhaps the authors are trying to demonstrate that if you incorporate clay content then the relationship with recharge still holds?? Seems rather convoluted to me. I would suggest revisiting the logic and structure of the first few paragraphs in the introduction.

Pg 4538 Ln 2. insert . . . . . . over 172 studies across. . . ..'dryland areas of'. . ..Australia. Ln 10 It may be worth saying that this is because most studies in the literature do not report clay content and when it is reported it is not reported in a consistent manner (sometimes it was reported as the average of top 1m, top 2m or maximum value instead of average value). This is an important point as it would be good if future studies provided this information (in a consistent manner) to help the future development of generic relationships. I think one of the contributions this paper could make is putting forward a strong case for future studies to report potential surrogate parameters in a consistent manner e.g. average clay content in top 2m Ln25 – Three sentences in a row start with Keese et al. 2005. I suggest you mix it up a bit. Ln27-30 using present tense?

Pg 4539 Ln 11 – '"best metric of clay content". . . - for an aim of the paper it's a bit ambiguous as to what this means.

Methods Pg 4541 Ln 25 – change 'methodology' to methods. You are not studying different statistical methods. i.e. methodology is the study of methods.

Pg 4542 Ln 7 – remove 'a'

Results P 4544 Ln 18-19 reword sentence.

Pg 4545 Ln 4 reword…"Using this larger data…" ??? Ln 6- point (b) it's the best of what? How can you say this when there are many other proxy measures you haven't tested. Besides which I don't see that average clay content explains more of the variation in the data than rainfall?

Ln 5-10 Sentence too long making it hard to follow. Ln 10 – across Australia? The paper is titled improving estimates across arid and semi-arid regions of Australia. Given their geographic bias' do you expect these relationships to hold across Australia (e.g. what about tropical northern Australia)?

Pg 4546 Ln 1 – I don't think the authors have demonstrated that their 95% confidence intervals are an improvement on Crosbie's. To me this would imply the authors have tested their and Crosbie's 95% CI against an independent dataset and found their 95% CI to be more robust. Ln 3 – the use by Crosbie et al. of Australia wide coverage of soil (and veg) data is an important point. Ln7 – "… without any such consideration to the level of uncertainly involved". I think that statement is too harsh. Many studies in the literature devote a considerable proportion of their discussion to issues of uncertainty in the development and application of generic recharge relationships. Yes most don't quantify confidence intervals but in many cases this was a deliberate decision. Ln 14-17 – what is the basis of this sentence? I don't necessarily disagree but it has come from nowhere. Can you provide a reference? Ln 17-18 "…. Greater dependence on local recharge". – greater than what? I don't actually understand this sentence. Please reword? Ln20 – I don't agree. Where water fluxes are low and residence times are long (i.e. deep unsaturated zones) the temporal variation in recharge reduces with depth and approaches a long term average constant value.

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Page 4547 Ln 1 I'm not entirely sure what the authors mean by this sentence. Yes I agree different management practises can potentially affect results but with upscaling relationships like this you can also get compensating errors. Ln 5-10 Glad to see this discussion in here. I think it is a very important point. The development of relationships from regional scale GIS data and then their application again through regional scale GIS data is attractive but fraught with danger. If the authors have data demonstrating this point/issue I would have thought that its inclusion would be a valuable contribution to the literature.

References cited above not already cited by authors.

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