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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Theory of the generalized chloride mass balance method for recharge estimation in groundwater basins characterised by point and diffuse recharge" *by* N. Somaratne and K. R. J. Smettem

**Anonymous Referee #1**

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The conceptualisation presented by Somaratne and Smettem is a demonstration of the flawed notions they harbour about basic hydrogeological processes. The following describes the key problems.

1. No sensors used routinely are installed at the water table. The majority of the Uley South piezometers sample all of the unconfined aquifer - from watertable to aquifer basement and represent integral samples thereof. The sampling strategy suggested here is entirely fictional and absurd, invented in an attempt to defend a flawed notion.



2. 50 mg/L is not reflective of rainfall, it exceeds it significantly.

3. 5GL is an enormous volume of water. This volume is 2000 olympic swimming pools, or 10m depth of water across 100 football fields. Adding that (instantaneously it would seem) to a single karst conduit would probably drive water into the matrix through micro-fissues in all but the most voluminous of caverns. Over long timeframes, diffusion will take it into the matrix (e.g. up to 10s of metres in 5000 years, and 100s of metres in 50,000 years, according to a basic Ogatta-Banks estimate), not to mention the role of smaller conduits. The authors use this conceptual model to defend arguments pertaining to Uley South. Uley South has sandy sediments, despite that the authors wish to oppose several well-documented reports by others saying as much. This model doesn't apply, despite the authors suggesting it does. Even if the aquifer was solid concrete, diffusion will still act to mix into/flush the matrix over geological timeframes and over significant distances, especially given the microfractures in concrete and even higher heterogeneity of natural sediments.

5. The authors suggest that their method assumes constant Cl in the groundwater system, but their method doesn't do any calculations whatsoever for the aquifer system. They do a mass balance across the watertable, and ignore completely the mass balance below the watertable. Doing a mass balance across the water table (which is never actually used in their final equations) does not amount to an aquifer water balance.

6. The obscure notion of lenses due to karst processes completely violates the authors own conceptual diagram here and the arguments of the sensors missing the freshwater. Lenses would be found floating on the saltier water, not caught in karst features, and if lenses indeed occurred, the sensors are almost perfectly placed to observe them, in complete contradiction to the arguments here.

7. Where are the bimodal water types in Uley South, that are suggested here? There is no evidence of this. Perhaps this is because the aquifer is mixing - but if so, why is

there no water at or fresher than the mass-balance estimate of 71mg/L from their ill-conceived estimates. The conceptual model fails when compared to field observations, especially of Uley South.

8. The only way a conventional CMB would under-estimate recharge in a karst aquifer is if someone was ill-informed enough to measure in a few places and make sure they missed any of the karst features. Indeed, this would only happen if they were ill-informed enough to use the monitoring strategy proposed by the authors in their diagram, which is an entirely impractical arrangement in any case. It would seem that the authors are unaware of the approaches to monitoring the aquifers that they discuss. Also, there is no evidence that reasonable hydrogeologists are measuring the hard rock Cl values in an area known to be karst and making recharge estimates from these. This is a strawman notion. The authors are creating absurd ideas and then refuting them to try to defend the obscure notions of this research.

The editors have now received uniformly negative feedback from numerous reviewers, including a world-leader on the topic in Prof. Warren Wood. If the journal is indeed adopting a peer review process, then the peer reviewers are recommending rejection. Added to this, my analysis has shown, with painstaking detail and despite almost uniform disagreement from the beligerant authors, that the authors' research is completely flawed, in both this paper and their previous one. I implore the editors to follow the advice of peer-reviewers, but also to consider that the lead author's organisation was at the centre of a parliamentary inquiry on the water resources management of one of the study areas (according to the authors comments), for good reason given historical water level declines. Their ridiculous notions may eventually lead to short-term commercial gain and perhaps the collapse of aquifers due to over-estimation of recharge and allowable pumping, if their method is considered as "peer reviewed" by HESS (despite being unanimously discredited by the peer reviewers). The editors must surely see that any method that produces exceedingly high recharge should be considered with significant scepticism, if a precautionary principle is considered, not to mention



that the current method produces recharge that completely violates mass balance, as I have shown. The current paper's short-comings evolve from the previous one, which was equally flawed, and it is distressing to find out from the authors that the journal continues to consider it for peer-reviewed publication, and it has now given rise to this more recent manuscript, which provides more clarity on the fatal and irreconcilable errors in their scientific notions.

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