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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 manuscript by Somaratne and Smettem is a continuation of the lead author's previous submission, which was rejected by a long list of reviewers through the HESS review process. The new manuscript is equally flawed and problematic, and has similar weaknesses to this previous effort. In particular, the theoretical development contains erroneous equations; the conceptual model of Uley South is flawed because it is not a hard rock limestone aquifer but largely unconsolidated; and there is no field data to match the notion of freshwater bubbles in Uley South. Specific comments are given below, albeit more problems exist with the manuscript than can be captured in a reasonable timeframe, and hence the list below is only a sub-set of the issues. It is problematic that the key references for the current work are their previously rejected manuscript and internal SA Water documents that are neither peer reviewed or publically available, as the major defense for the current paper. It is also worrying that all of their research, as ill-based as it is, leads to higher recharge estimates, which has significant commercial benefits for the lead author's organisation. That is, higher recharge rates may allow for a greater volume of extraction. It should be noted that previous studies of Uley South have calculated recharge rates largely commensurate with the level of extraction from the basin, which has seen many years of water-table decline under over-extraction by the authors' organisation. Whether or not there is a link between the flawed science in this manuscript and the desire to increase allowable extraction from these aquifers is beyond the scope of the current review, but given the affiliation of the lead author, such a notion ought to be mentioned in light of the significant bias that is presented in this manuscript, and was presented (and rejected) in the previous HESS submission.

Specific Comments: Abstract: L2 - A distinction is needed between unsaturated zone CMB and groundwater CMB approaches from the outset of the manuscript, because these two methods have entirely separate assumptions and applications. Lumping the two into one for the purposes of the Abstract is confusing and misleading. The groundwater CMB may well apply to aquifers with localized surface water inputs if the degree of mixing is such that diffuse and point sources mix, so the statement here isn't globally correct.

L2 – The precise meaning of "conventional" should be given here, particular as it might apply to either an unsaturated zone approach or saturated zone approach. The two applications of the CMB method have different "conventional" applications. Unsaturated CMB ignores preferential flow whereas saturated CMB may or may not. The second sentence L4-6 is true in hardrock karstic aquifers, but many of these, including Uley South, contain a considerable amount of unconsolidated sand materials and the karstic sinkholes only persist through the capping layer to transfer water into the sandy sediments below. In this case, the unsaturated CMB approach is certainly not going to apply, but a saturated zone CMB may well have application if sinkholes do not persist to the watertable, and the aquifer is predominantly comprising semi- and unconsolidated materials, and the karstic sinkholes are really only surface features, as is the case over the significant majority of Uley South.

L6-9 – This statement is incorrect. Many aquifers have some amount of point recharge, but the saturated zone CMB approach is not abandoned. It is not black and white in the manner being expressed here – there needs to be certain factors in place before CMB is not applicable, and the single reason here that point recharge precludes CMB (for saturated zone) is simply untrue.

L10-12 – The study does not achieve what is purported here. There is not a comparison between these methods presented in the paper. No groundwater flow modelling estimates of recharge have been offered. And there is inadequate clarity around the other methods to be able to make a proper assessment of their validity relative to the approach offered in this manuscript.

Introduction, etc

L18 – "...to water balance is..." is awkward English.

L19 – Should be "... of the land surface..."

P309, L3-4 –Saturated zone CMB and unsaturated CMB need to be differentiated, because it is not the case that the saturated zone CMB estimates diffuse recharge through the soil profile only. It is an integrating measure (if the aquifer doesn't have strong preferential flow features such as karst flow). This is a fundamental weakness of the manuscript. Even in karstic aquifers, CMB measured in karst features will provide an estimate of point recharge. The authors are, for their convenience and erroneously, mixing up the unsaturated zone and saturated zone CMB approaches to try to make a point.

P309, L7-9 – The necessary conditions mentioned here ought to be defined, because these are central to the manuscript.

P309, L14 – The reference Somaratne (2013) is cited frequently throughout the paper at points of criticality in terms of the arguments made, but the reference is not available to the general public and shouldn't be relied upon as the seminal work to be referenced. It is an unpublished report by the author that most likely has not been peer-reviewed, so it comes across as though the author is using their own unpublished work to defend the current research, rather than relying on peer-reviewed literature. Further, Somaratne et al. (2013) was submitted for publication and uniformly discredited, and yet it too is used widely as a point of reference, when rather the authors should refer to papers that have been accepted into the public domain.

P309, L15-17 – The karst features, in Uley South at least, are known to occur predominantly only in the calcrete capping layer, and boreholes contain mostly unconsolidated sediments with some hard layers in them. The authors are offering an incomplete depiction of Uley South – it is most certainly not a classical karst limestone aquifer, and this is clear from various previous geological descriptions. The seminal work by Evans (1997) describes Uley South's Quaternary deposits as (referring to the Quaternary formation): "...these aeolian sediments consist of fine sand size shell fragments. The sands are primarily cross-bedded foresets, unconsolidated or loosely aggregated. Secondary porosity (solution features) has developed as well as secondary cementation expressed as calcretised horizons at evaporation fronts (particularly at surface exposures)." In short, the aquifer has secondary porosity in the capping layer, and likely more extensively in a few locations, but is predominantly unconsolidated and hence the preferential flow features in the aquifer (e.g. karstic tunnels) that might otherwise preclude saturated zone CMB, are not evident. As such, the basis for the current manuscript's attempts to distinguish between point source and diffuse recharge are ill-founded for this system. This was the conclusion from the previous manuscript on this topic presented by the authors as a HESS Discussion (and uniformly discredited by numerous reviewers; http://www.hydrol-earth-syst-sci-discuss.net/10/11423/2013/hessd-10-11423-2013-discussion.html). The current manuscript is largely the same arguments, and these remain erroneous and ill-founded.

P309, L24 – "fresh water bubble" is the wrong term for an expanse of water covering 20 sq.km.

P309, L26 – Grammar issue with "...Somaratne et al. (2013) shown...". Also, the use of this reference is not adequate because it was discredited. The author's own work is used here as the key theoretical basis for the current research, but such a reference is not an adequate basis for this.

P309, L27 – Here and elsewhere "conventional CMB" is used but without distinguishing between saturated and unsaturated forms, which such differentiation is essential in light of the arguments in the current manuscript.

P310, L1-3 – The CMB method applied to the unsaturated zone can occur in different ways, and is oversimplified in the current description. A discussion on this topic is warranted here rather than the over-simplified statements that are offered in the current paper, i.e.: If soil chloride is measured, it is only an estimate of the infiltration to that point in the profile, and requires a steady-state assumption. Often, where land-use change has occurred, this approach will not work because transient effects may be important. Further, Cl concentration should increase with depth through the "ET active" unsaturated zone. Often, Cl may be used to evaluate the "salt bulge" in the unsaturated zone profile. Movements in the salt bulge are often used to ascertain recharge arising from land-use changes. The description offered by the authors neglects these elements and assumes that a soil Cl measurement is being widely used by hydrogeologists to calculate recharge. This is simply not the case, and more informed investigations are used that attempt to evaluate the meaning of unsaturated zone Cl profiles. A key point here is that an unsaturated zone Cl measurement only offers infiltration knowledge, whereas recharge estimates need to account for the Cl concentration at the bottom of the unsaturated zone (i.e. prior to reaching the watertable). This is not mentioned in the manuscript, and it reads that the methodologies being suggested by the authors are not the ones that are being used conventionally.

P310, L1-3 – Here, the "conventional CMB" is referred to as the saturated zone approach. Earlier, it was either saturated or unsaturated approaches. A clear distinction between the two is needed, because the manuscript is otherwise using the limitations of one approach to try to discredit the other.

P310, L5-6 – "is estimated using" is not correct. Dry deposition is estimated using a host of different approaches, most often using field data. The authors have incorrectly interpreted the Ordens et al. study as suggesting that Hutton's formula is used for dry deposition estimates, but it is merely a way of providing a distribution (inland from the coastline) of dry deposition rates, which should preferably be grounded on field measurements.

P310, L12-14 – It should read here that "diffuse recharge, which is in equilibrium with that passing through the unsaturated zone", because the unsaturated zone contains Cl distributions, not a single value as inferred here, and it is the Cl within the lower unsaturated zone that is relevant.

P311, L1-4 – There's a grammatical problem with this sentence: "...it is generally unsuccessful in using unsaturated core method...".

P311, L3-4 – The part of sentence starting with "implying that..." does not follow logically, because it is not separating the saturated and unsaturated forms of the equation. The inference that arises from these references is that the unsaturated form of the equation is not valid where macropores are a dominant flow mechanism. The authors are trying to infer that both forms of the CMB approach are invalid, but such a statement does not logically follow, and the authors need to separate unsaturated and saturated approaches, rather than lumping them together to (erroneously) form the ideas of the manuscript.

P311, L5-9 – The description of Subayani and Sen's research is not adequately clear so that the reader understands how their research is relevant to the current manuscript. What is the message in this paper that holds relevance for the current research?

P311, L10 – The reference to the author's own unpublished work at seminal points in the paper is not appropriate.

P311, L11 – The use of "point" and "diffuse" here is misleading. Both Ward et al. and Ordens et al. use "point" estimates of "diffuse" recharge. That is, they use 1D soil modelling (that also has a bypass flow mechanism to try to approximate sinkhole recharge processes - qualitatively at least) to estimate both soil and sinkhole infiltration/recharge. "Point" and "diffuse" in this context are therefore not mutually exclusive. Referring to Ordens et al.'s and Ward et al.'s point estimates and diffuse estimates does not hold meaning, because both had estimates that were both point and diffuse.

P311, L12-13 – Which "conventional CMB" is being referred to here? This is a critical point. The manner of obtaining a "total recharge" needs to be disclosed, because otherwise it doesn't make sense to obtain a total recharge that is less than point recharge, when total recharge clearly includes both bypass flow and diffuse forms. Hence, it would appear that the authors are using circular argument here – i.e. Somaratne et al. (2013) essentially do the same thing that the current study aims to achieve, and is referenced as though it is a separate point of evidence when in fact, any short-comings in that study will be transferred to the current one. Independent literature is needed to avoid this problem. Otherwise, the authors have used the purported method presented in this paper to draw a finding that is then referenced to their own previous (but similar) work to defend the same method.

P311, L13-14 – This statement is not true. These authors did not obtain the same outcomes, because they didn't apply the same method as Somaratne et al. (2013).

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P311, L14-16, and L18-19 – It should be noted that Ward et al. (2009) state that "The values of recharge obtained from the LEACHM modelling exercise are treated as relative rates only, and are intended to be used to build intuition rather than make absolute predictions…". The current manuscript is quoting recharge estimates from this report when it is clear that this was not the intention of their work. Rather, their analysis was "largely qualitative" (Section 5.3 of Ward et al., 2009). Ward et al. (2009) also state: "This modelling result is critically dependent on the assumption that all runoff becomes recharge via sinkholes; testing this assumption remains one of the key recommendations for future investigations into EP recharge processes." They make it clear that the understanding of sinkhole behaviour is weak, and that their results are not conclusive in this regard. Despite this, the current manuscript refers to Ward et al. (2009) without providing any of these caveats and in a manner that appears to extract statements out of context and in a corrupted manner with the intent to support their claims rather than properly represent the true nature of Ward et al.'s (2009) outcomes.

P311, L20 – the statement that conventional CMB under-estimates recharge is not defended or proven to this point with anything other than the authors own non-peer-reviewed report. Further, to this point, the authors are using the limitations of unsat zone CMB to discredit saturated zone CMB.

P312, L2 – Methods of recharge estimation are not "valid" or "invalid", the methods are rather distinguished by assumptions that change the degree to which they apply to different settings, and likely have biases that are characteristic. Simply "other" rather than "valid" is a more accurate description here of alternative methods. It wouldn't make sense to use invalid methods, so "valid" is superfluous. Certainly, water-table fluctuation analysis is no more valid than CMB, given the challenges in applying this method, as pointed out in a host of previous publications.

P312, L5-8 – The continued reference to a rejected paper as support for the current research is not valid and needs to be eliminated from the manuscript. What's more, previous reviewers highlighted that the description of the sites was inadequate, so to

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now refer to those descriptions for the current paper is not appropriate. Complete, rather than brief, descriptions are needed.

P312, L9-10 – Here, and in many places in the manuscript, there are English issues. The grammar of this sentence is not correct.

P312-L11 – “comprised” should be “comprises”, because the basin continues to be of these sediments.

P312, L13-14 – It is widely accepted that the aquitard in Uley South is discontinuous. This needs to be corrected here, because it’s an important aspect – i.e. that the QL and TS aquifers are strongly connected in places. The proven connection of the QL and TS is an important oversight in the conceptual model presented in Figure 5.

P312, L17-19 – A density of 1 sinkhole per 0.07 sq.km or 57 sinkholes in a 4 sq.km area does not account for the thousands of smaller holes in the calcrete that occur at diameters less that 40 cm. The calcrete is riddled with these, and the result is most probably diffuse recharge to the aquifer. It is therefore very hard to differentiate sinkhole and diffuse recharge through the soil matrix, in Uley South. Furthermore, the sinkholes are not continuous to the water table, and rather they redistribute water deeper into the unsaturated zone in many places. Certainly, there isn’t a single sinkhole in Uley South with standing water in it, as depicted in Figure 5. These aspects are critical to the current study, which neglects things like the actual characteristics of the system, seemingly for convenience despite that they are significant. A proper disclosure of the relevant elements are needed of the study area.

P313, L3 – “Bubble” is the wrong word for a freshwater body of water, either floating in saltier water or simply in an aquifer. Freshwater doesn’t create bubbles if there are density differences, and in any case, the lateral-vertical scale distortion of aquifers (small thicknesses and vast areas) does not give rise to “bubbles”. Freshwater bodies are lenses where there is a moderate-strong density variations. Otherwise, local recharge causes groundwater “mounds”. Certainly, 20 sq.km is not going to take the shape of a “bubble”.

P313, L20-22 – The surface runoff estimates of Ward et al. (2009) are especially approximate runoff calculations, using the crudest of methodologies, which they themselves describe as largely qualitative. Furthermore, the description of LEACHM’s calculation of surface runoff is not right. LEACHM calculates surface runoff through more than just the CN approach – it also rejects infiltration when the soil profile becomes saturated and is unable to receive further infiltration (saturation excess runoff). Ward et al. (2009) note significant clay layers in the soil profile that probably act to reject recharge. Ordens et al. (2012) studied the effect of these, but the authors are chosing the former study of Ward et al. (2012) and its higher recharge values, seemingly for convenience. The models of Ward et al. (2009) are strongly non-unique – a different CN would have produced considerably less surface runoff and considerably more diffuse recharge. The CN used by the authors was arbitrarily selected. Also, Ward et al. (2009) highlight that it is necessary to test the assumption that all runoff becomes sinkhole recharge. The report is clear in reporting considerable uncertainty in their estimates of runoff and sinkhole recharge, and application of these figures in the manner suggested in the current manuscript is not an appropriate approach. Ordens et al. (2012) produced a significantly more thorough assessment of Uley South recharge, and should be the primary point of reference, especially considering that it is peer-reviewed and published, whereas the Ward et al. study was considerably less scrutinised, being a grey literature document.

P314, L12-13 – Correct to: “. . .average percentages. . . were determined. . .”

P314, L17-18 – Correct to: “. . .runoff volumes from. . .”

P314, L20 – Correct to “mid-winter”

P314, L22-23 – It is not the case that a freshwater swamp indicates groundwater discharge. The swamp may “fill and spill”, allowing the flushing of salts via water losses due to exceedance of the swamp maximum water volume to a degree that allows it to remain “freshwater”. The statement needs to be modified to be globally correct or made clear that it is only intended to be locally relevant.

P314 – Somaratne 2011a and 2011b cannot be used here – they are internal project reports that lack peer review, are unavailable to the reader, and are the product of the author. I suggest that these are eliminated from the reference list and the manuscript more generally.

P315, L3-6 – The Mt Gambier Limestone and the Uley South QL sediments are not remotely similar in terms of their hydraulic characteristics, degree of consolidation and karstification, etc. The statement here needs to be changed, and Uley south should not be treated in the same manner as Mt Gambier sediments.

P315, L12 – The text referring to “these small pockets of fresher water” is out of place. Firstly, it is preceded by statements about aquifer properties, so it is not clear what “these” refers to. Secondly, there is no evidence for pockets of fresher water in Uley South, as is somewhat inferred here by the parallels between the Mt G limestone and Uley’s QL sediments. Ordens et al. (2012) present Cl variations - the authors are directed to that study.

P315, L23-25 – The geographical location where this statement applies needs to be clear – “near drainage wells” in which system? Otherwise, it reads as a global statement, which is not the case, because it will only apply under particular conditions (depending on distance from monitoring well, aquifer properties, groundwater flow direction, etc).

P315, L27-28 – This statement is not true. The mode of transport is necessary to distinguish for a host of reasons. From the perspective of the saturated zone CMB, the statement hold some relevance albeit it still requires modification, but the sentence doesn’t offer any context to this, so as it stands it is not correct. Certainly, it is not adequate in terms of unsat zone CMB.

P315 last line to P316, L1 – Somaratne et al. (2013) is essentially a discredited Discussion paper. It is not the right reference to use for this rather global and knowledge-defining statement.

P316, L1-3 – There is no basis for this statement – i.e. that Cl at recharge points is surface water concentration and elsewhere is diffuse water concentrations. This is especially the case for Uley South, where no information is presented or available to defend such a notion. The authors are fabricating notions without proper previous citations, data or modelling. The same criticism was levelled at the previous rejected discussion paper in HESS.

P316, L5-6 – The duality of recharge in the Uley South basin was first developed by Ward et al. and then extended and re-conceptualised by Ordens et al. (2012). Somaratne et al. (2013) is not the right reference here.

P332 – Figure 5 – This diagram is unclear, for the following reasons: 1. D is not attached to an arrow, 2. the arrow above the unconnected sinkhole has no label, 3. it is not reasonable to use average annual rainfall for the processes included in the diagram (the definition of P), 4. evaporation is missing from sinkholes that have the watertable in them, 5. Su and cu do not belong to each other in a coupled way as shown because one is the unsat zone storage and another is what leaves the bottom of the storage (and the unsat zone will have a concentration gradient in it), 6. it is not clear what the difference is between groundwater Cl (Cg) and diffuse recharge zone Cl (Cgd) especially for Uley South which is entirely riddled with sinkholes of varying sizes and penetration depths, 7. there is no upward and lateral flows of groundwater in this control volume, 8. there is no runoff inflow to the control volume from uphill runoff (why would there only be runoff out of the control volume when there could be runoff into it?), 9. Fluxes associated with the unconnected sinkholes are not labelled or listed, and are seemingly dropped into the watertable (inferring that they largely bypass the unsaturated zone rather than recharge it), 10. There is no ET from the saturated zone, and yet this is widely known to be significant in shallow watertable systems.

P316, L9-10 – This statement is not true because Fig. 5 has partially penetrating sinkholes in it.

P316, L10-12 – There is no basis that unsaturated zone inflows via partially penetrating sinkholes rapidly drain to the watertable. Why might they not drain slowly? Uley South has large unsaturated zones in places – up to 100 m, and here especially, a partially penetrating sinkhole might certainly take a considerable time for infiltration to reach the watertable.

P316, L18. This equation is wrong. The "D" should be a separate entity on the RHS and not a subscript of C(P+D), because dryfall is not particularly precipitation related.

P316, L19. The different components are positive and negative in an inconsistent manner. A positive value of each water flux needs to be defined.

P316, L20 – The assumptions that are listed do not simplify equation (4), some of them were used in creating equation (4) and some can be applied to equation (4) to simplify it further – it should be made clear which is used for which.

P316, L28 – Equation (5) assumes that there is no diffuse infiltration, which is an entirely unreasonable assumption, especially for a system like Uley South that contains mostly unconsolidated subsurface sediments below a calcrete surface capping that is very leaky and riddled with cavities.

P317, L2 – This equation is also wrong. It assumes that the entire unsaturated zone contains water of concentration Cu, which it doesn't – especially with infiltration at Cs. Also, there is no mention of the partially penetrating sinkholes (noting that the proportion of all sinkholes and their respective depths that are partially penetrating are unknown, and hence this is probably why this is conveniently ignored).

P317, L13 – Eqn 8 doesn't need brackets around the entire RHS. Also, the rearrangement of Eq 8 to obtain Eq 9 is entirely trivial and not needed. Eq 9 can be presented directly. Note that the term PC(P+D) is consistently wrong – D is not precipitation dependent and should not be presented as such in the equation.

P317, L19-20 – "…to the saturated zone across the watertable…" doesn't make sense to me. The saturated zone is obviously the area below the watertable. There is no saturated zone that is especially "across the watertable".

P317, L22-23 – There is no ET from the watertable in Figure 5 or the equations presented in 1-9, and so it is obvious that there will be no Cl lost to ET from the saturated zone. Either make the statement that both Cl and water are not lost to ET from the saturated zone, or leave out the statement about Cl being lost from the saturated zone.

P318, L1-3 – The authors have corrupted the meaning of "steady-state groundwater flow", which doesn't imply that there is no lateral flow or vertical inputs, but rather, there is simply no transient component to the problem. There can be changes in Cl with steady-state flow moving laterally or horizontally, and in fact, it is an essentially component to the groundwater mass balance. Without lateral flows, it is not appropriate to analyse the groundwater Cl.

P318 – L6 – Equation 10 is wrong. QpCs – QpCs = 0, and hence QpCs is eliminated from the equation. It is clear that careful proofreading has not been undertaken, and/or there are short-comings in the understanding of fundamental recharge concepts.

The remainder of the manuscript is equally problematic, riddled with erroneous equations, unsubstantiated statements, and misinterpretation. The conclusions arise from ideas that are self-perpetuating – the authors use their own previous work, mostly unavailable documents that have not been peer reviewed, to initiate notions and then substantiate their findings. These are all largely based on the same erroneous notions, that in all aquifers of any limestone content the aquifer contains well-defined freshwater bubbles that are somehow not influenced by lateral groundwater flow, mixing and transient effects. Such an idea is not supported by observations in the field. The interpretations of previous studies is badly corrupted and biased in a manner that attempts to support the notions of the current research. For example, previous MODFLOW modelling of Uley South did not obtain calibrated recharge values, as purported in this paper. The final analysis of the paper takes “diffuse recharge” (which is in the Uley Sth case the total recharge) and simply adds a manufactured inflow to it in an attempt to generate higher recharge values. It has to be said that this is the second attempt by the lead author to publish this work. The previous attempt was unanimously rejected by several expert referees, and hence this current work presents a persistence to produce corrupted research, in the face of clear guidance that there are concepts in error. The motivations for doing this are questionable, but it needs to be stated that higher recharge rates mean that the lead author’s organisation may eventually mount an argument to extract higher volumes of saleable water from the aquifers in question, and hence there are commercial interests here that shouldn’t be discounted in evaluating their research.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 307, 2014.