Author Reply to Referee 1:

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

The authors would like to thank the Referee 1 for the lengthy comments. We note that Referee 1 has used this discussion forum to make accusations against the lead author's organization. Such accusations are baseless and of no interest to the international hydrological community.

The purpose of the HESS discussion forum is to initiate scientific debate/ intellectual discussion among authors, editors, referees and others from the scientific community interested to contribute to the advancement of hydrological science.

In this spirit, we invite Referee 1 to participate in this forum and any future forums while maintaining professional ethics and with mutual respect.

Anonymous Referee #1
Received and published: 13 January 2014

General Comments:

Referee 1: 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.

Author Reply: The authors are unaware that the previous submission (Hydrological functions of sinkholes.. doi:10.5194/hessd-10-11423-2013) has been rejected. As of the date of Referee 1' comments, 13 Jan 2014, the paper is undergoing major revisions. How does the Referee 1 knew it was rejected? We invite Referee 1 to go through all the Author Replies prior to making such a comment.

Referee 1: The new manuscript is equally flawed and problematic, and has similar weaknesses to this previous effort. In particular, the theoretical development contains erroneous equations;

Author Reply: The equations are considered correct and we have provided additional comments under the specific comments related to equations.

Referee 1: 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.

Author Reply: Hard rocks are the igneous and metamorphic rock, and soft rocks are the sedimentary rocks such as Limestone. Does Referee 1 mean consolidated Limestone such as calcrete ???. Therefore, there is no such rock as 'Hard rock limestone'. We have not mentioned in this paper nor in the the previous submission that 'fresh water bubbles' exist in Uley South. The term, 'fresh water bubble' is used only for the Poocher Swamp fresh water plume for a specific reason, which is clearly mentioned in doi:10.5194/hessd-10-11423-2013.

Referee 1: 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 defence for the current paper.

Author Reply: This is not correct. With regard to doi:10.5194/hessd-10-11423-2013, we have provided the answer above. In addition to our internal reports, we have provided a long list of references to establish the scientific background, this include Ordens et al (2012).

Referee 1: 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.

Lead Author Reply: This is an allegation against the lead author's organisation and in response the lead author provides the following answer to Referee 1. Apart from the Referee 1, only one other person has publically made such allegations, Dr. Adrian Werner at Flinders University of South Australia providing a submission to the Natural Resources Committee, EP Water Supply Inquiry (Please refer to the Official Hansard Report, Parliament of South Australia, Friday 7 September 2012). The lead author invites Referee 1 to contact Dr. Werner at the Flinders University and to jointly provide answers to the following questions.

In page 6, of the above mentioned Hansard report Dr. Werner stated that their research shows that the average annual recharge of the Uley South basin is 60 mm.

1. If 60 mm is the average annual recharge, how is it possible that the average annual groundwater extraction of 6.8 GL/year, which is equivalent to this volume of recharge, can occur without saline intrusion?
2. How is it possible that water levels in the basin have been either stable or rising since 1999 (see graphs provided below)?
3. Why did Dr. Werner not mention the recharge outcome of his own peer reviewed model (Werner, 2010), which produced average annual recharge of 146 mm?
4. The Werner (2010) model produced through flow to the sea of about 20 GL per year. If the total recharge (60 mm per year) is extracted, where does this additional water in the model come from?
5. How do the outcomes of the recharge calculation using generalized chloride mass balance method influence the water allocation plan, when the generalized CMB calculated recharge is the lowest of all other valid alternative methods, that include the adopted recharge value for water allocation plan?.

A serious allegation is made by Referee 1, stating that the basin has seen many years of watertable decline under over-extraction by the author's organisation. A similar allegation is found in Ordens et al (2012), where Dr. Werner is a co-author. To establish this concept, Ordens et al (2012) used data from monitoring well ULE 101(among many other monitoring wells available in the basin, see Fig. 1 in Ordens et al 2010), which is located near the Swamp (see Fig. 2, DOI 10.1007/s10040-011-0794-2). There are several flaws with the science in the above published paper:

1. ULE 101 is not a representative monitoring well for the Limestone aquifer due to its location near the Swamp, and completion 5 m into the Tertiary Clay layer (the lithological data is available from Water Connect, url: http://www.waterconnect.sa.gov.au/Systems/GD/Pages/default.aspx#Unit Number).
2. When interpreting water level data, one should not compare with annual rainfall as given in Fig. 2 in Ordens et al (2012). The water levels should be plotted with cumulative deviation from the mean rainfall and water level behaviour interpreted according to wet and dry cycles.

Somaratne (2013) provides an analysis and interpretation of water level behaviour using three long-term monitoring wells which are representative of the general limestone aquifer away from the pumping influence (Figure 1); and annual extraction and water levels of monitoring wells located in the central basin (Figure 2).

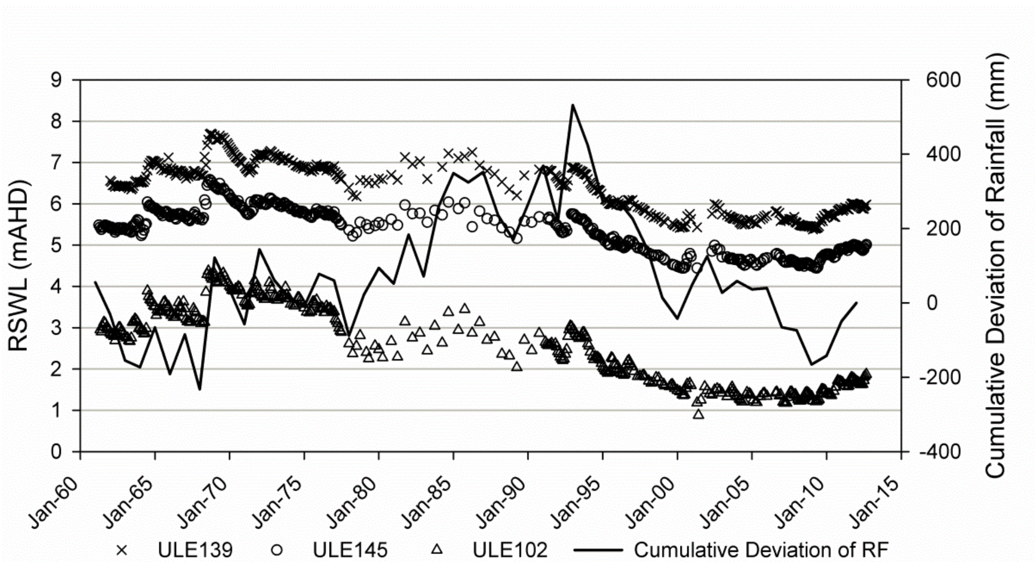


Figure 1. Long-term watertable fluctuation and cumulative deviation of rainfall

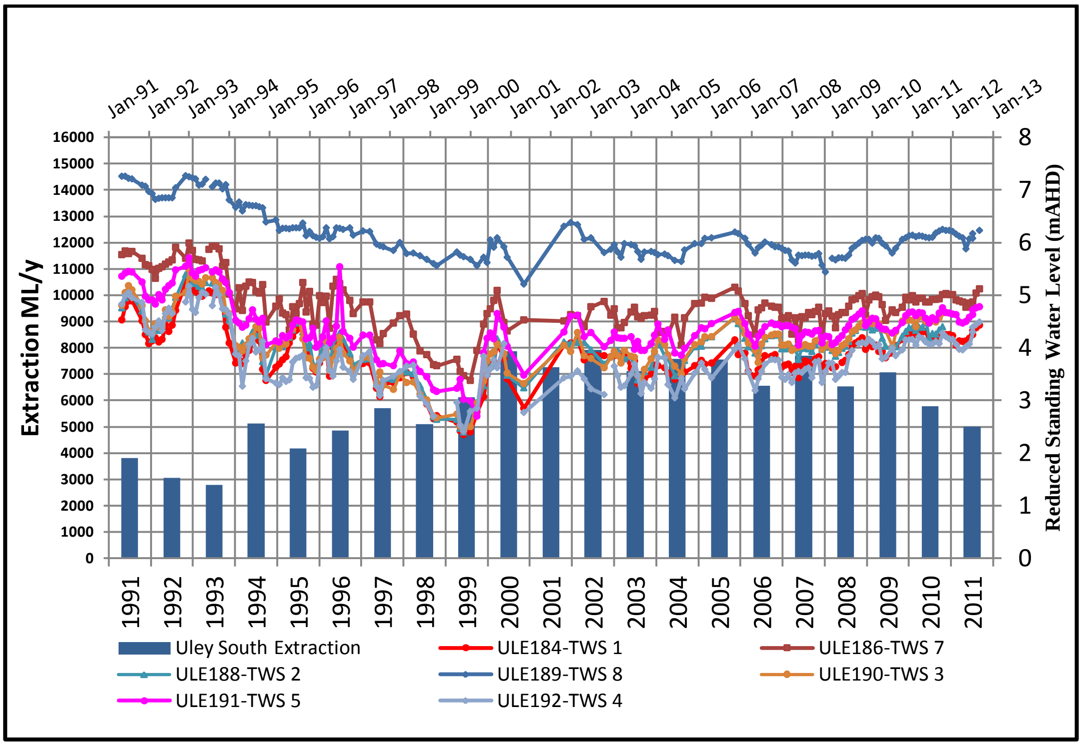


Figure 2. Watertable fluctuation in the Central Basin

Could the Referee 1, conclude that continuously declining water levels exist in the basin; or any relationship to wet-dry period as given in cumulative deviation from the mean rainfall ?.

Could the Referee 1, see that despite low pumping, water levels declined in 1990-1999 periods due to a dry cycle?.

Could the Referee 1, see that despite increased pumping from 2000-2004, water levels continued to rise due to a wet cycle ?.

Could the Referee 1 see that, water levels are either stable or rising in the basin since 1999 ?.

The lead author invites Referee 1 to down load water level data from Water Connect URL given above and analyse himself.

This statement adds nothing to the debate on the validity of the science presented in the paper. The assertion of declining water tables due to over extraction conflicts with the findings in (Zulfic, DEWNR groundwater status reports) that show stable groundwater levels, driven primarily by recharge. The referee's statements also ignore the water resource management regime of the lead author's province in which a provincial government regulator sets the maximum extraction volumes that can be extracted by the separate government water utility.

Referee 1: 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.

Lead Author Reply: These comments of the referee focus on the credibility of the lead author and his employer rather than scientific concerns and as such we refrain from responding to them. Referees need to focus on advancing hydrological science and this is what the HESS expected through a Scientific Discussion Forum.

Specific Comments:

Referee 1: 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.

Author Reply: The conventional CMB is defined in the manuscript, which is sometimes called theclassical CMB (See Subayani and Sen, 2006 _). The aim of this paper is to introduce a generalised CMB that extends the conventional CMB in aquifers dominated by point recharge. We have shown that from theoretical derivation tha the conventional CMB can only be obtained when $Q_p = 0$, that is no contribution from point recharge (See Eq. 12 in Page 318 and Eq. 13 a in Page 319). Another problem with the application of the conventional CMB to groundwater basins with point recharge is how to get representative chloride values. We have shown (and cited some literature) in the previous manuscript, doi:10.5194/hessd-10-11423-2013, that when groundwater mixing occurs, obtaining representative samples is a near impossible task.

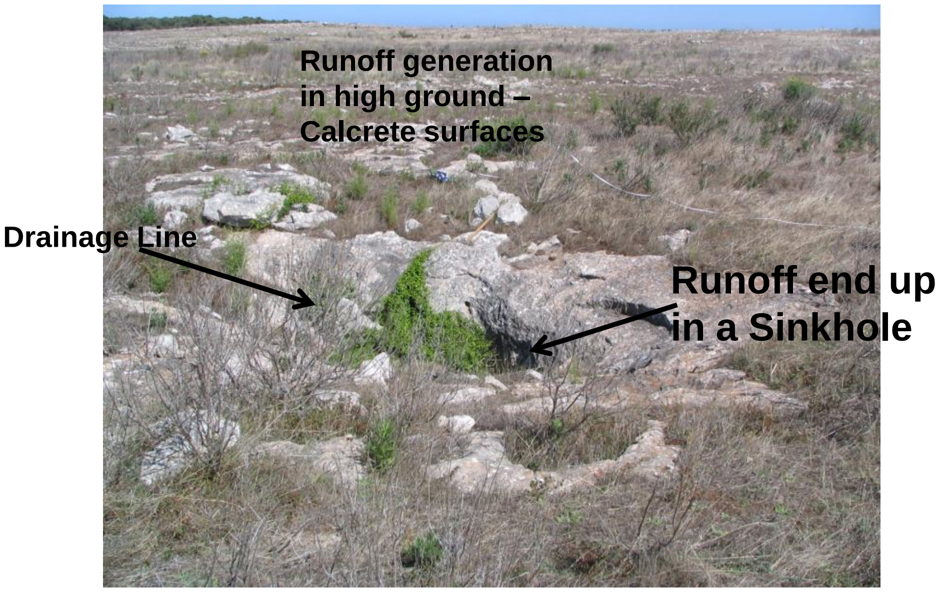
This is eloquently stated by Referee 5 of the doi:10.5194/hessd-10-11423-2013 manuscript, Hydrological functions of sinkholes.... Relevant Referee 5 comments and Author Replies are provided below in *Italics*:

"***Referee 5-3:*** *Point recharge (at sinkholes) feeds karst conduits with very rapid groundwater flow; it is important to realize that sinkholes almost always feed conduits (otherwise the sinkholes would not exist). Some of the recharge around sinkholes will also seep slowly into the granular porosity around the recharge point. However, most of the recharge to the granular porosity of the aquifer is through surface recharge across the entire surface of the aquifer; recharge to the granular porosity from the point recharge areas would be a relatively minor contribution restricted to those areas. The authors need to reinterpret their data with this in mind; studying the recharge to the granular* *porosity around point recharge areas is useful, but remember that most groundwater flow is through the conduit that is fed by the sinkhole.*

Author Reply 5-3: *Authors agree with the view of Referee 5. In the revised manuscript, we have highlighted the fact that in the point recharge dominant zone in Uley South, 10 mg/L reduction of chloride concentration is a result of mixing of point recharge water with diffuse recharge and ambient groundwater. Referring to Fig. 6 & 7, we have described that low salinity water in the upper part of BLA 107 and BLA 164 are the result of both diffuse recharge and point recharge mixing; and low salinity in the profile at depth in BLA 164, is the interception of conduits carrying low salinity water from point recharge source (drainage wells) These observations are supported with Herzeg et al (1997) observations at Poocher Swamp monitoring wells.*

The findings regarding calcrete surface generated runoff and that flow to sinkholes, as well as calcrete surface contributes to diffuse recharge, is supported by a presentation made by the first author two years ago to a group of local hydrogeologists. We present the relevant slide below, which is self explanatory. This photo shows the typical landscape of the Uley South basin.

Evidence for non-applicability of CMB Method
Main Catchment Features are numerous sinkholes



Referee 5-4: *Therefore, applying the CMB method to the groundwater in the granular porosity around a point recharge area will always underestimate the amount of recharge through the sinkhole, because it is not sampling the conduit groundwater flow, which is completely separate. To estimate the conduit recharge, using the minimum groundwater chloride in the granular porosity around a point recharge area will give the best value, but this will still be an underestimation.*

Author Reply 5-4: *Agree with the Referee 5's Comment. Minimum groundwater chloride is still subjective.*

Referee 5-5: *Furthermore, on a Cl vs δ^{18}O plot, conduit flow directly recharged through a sinkhole will plot near rainfall, because the very rapid recharge allows only minimal evaporation. Groundwater in the granular porosity will have been recharged mainly through surface infiltration, and will have been subjected to evapotranspiration during infiltration, so it will have elevated chloride and heavier δ^{18}O, and will plot well away from rainfall.*

Author Reply 5-5: *Agree with the comment. This is clearly evident in Fig. 4b and 4c, and the description given in page 11429 and 11430 under Section 3.1.*

Referee 5-5: *Groundwater in the granular porosity around recharge points will have been recharged partly by relatively rapid recharge through the sinkhole, so will plot in an intermediate position between rainfall and most groundwater. However, a lack of intermediate points between rainfall and most groundwater does not discount point recharge (contrary to Ordens et al), it just indicates either that there is little seepage into the granular porosity around the recharge point, or that this was not sampled, or both.*

Author Reply 5-5: *We agree with the comment. We have observed sample biasness because of the lack of monitoring wells near point recharge sources. In addition, small catchments feeding Uley South sinkholes or Mt Gambier drainage wells do not generate sufficient runoff volumes to generate a fresher water plume that extends to monitoring wells. It is very difficult to track conduits flow using a single monitoring well since the network of conduits and their depths are unknown. Herzeg et al (1997) used 3 wells at 10 m, 50 m and 150 m away from the two sinkholes in Poocher Swamp. The first two wells had been completed shallow, 6 m below water level and the third one (at 150 m) completed at 50 m depth (about 35 m below water level). Maximum water level rise had been observed at the well 150 m from the sinkhole indicating a direct sub-surface connectivity to sinkholes. This indicates the complex nature of tracking flow paths.*

If the arguments of Ordens et al (2012) applies to Mount Gambier drainage wells (where a gap of 43 mg/L exists), then one has to conclude that 'drainage wells are not directly recharging the aquifer, but rather distribute in the unsaturated zone and undergo evapotranspiration process'. This is not in fact the case as the drainage wells are openhole construction below watertable (direct recharge being visible in Fig. 2 – where recharge is occurring through the drainage wells). "

Above Referee 5 comments highlight the problem associated with dual recharge i.e obtaining representative chloride samples and under estimation (always) of recharge by the conventional CMB.

Referee 1: 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.

Author Reply: Please see page 321, Lines 1-10 for cited references.

Introduction, etc

Referee 1: L18 – "…to water balance is…" is awkward

Author Reply: Agree and corrected. Thank you.

Referee 1: English. L19 – Should be "… of the land surface. . ."

Author Reply: Agree and corrected. Thank you.

Referee 1: 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.

Author Reply: This is the mis-conception that prevails among some hydrogeologists. It is very difficult to obtain representative groundwater chloride when point recharge is present, due to the problems with ensuring adequate sampling . When a groundwater compartment occurs, there can be wide gradation in the range of chloride values between ambient groundwater and point recharge source water. The degree of mixing varies as shown in the manuscript doi:10.5194/hessd-10-11423-2013, according to the amount of point recharge and saturated thickness of the aquifer and conduit and flow paths distribution etc.

Referee 1: P309, L7-9 – The necessary conditions mentioned here ought to be defined, because these are central to the manuscript.

Author Reply: We have cited references for interested reader to go through. We have also provided in the revised manuscript of the doi:10.5194/hessd-10-11423-2013 in response to Dr. Adrian Werner's short comments.

Referee 1: 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.

Author Reply: It is not uncommon to cite unpublished or submitted literature as references. If the SA Water internal report, Somaratne (2013) is a concern to Referee 1, please compare the water level decline issue that you raised under General Comments. We have shown how peer reviewed published Ordens et al (2012) is flawed compared to analysis and descriptions provided in Somaratne (2013). Somaratne et al (2013) is still under review and, then how could the Referee 1 discredited it. –

Referee 1: 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.

Author Reply: The above conceptual model is now considered incorrect. The calcrete is described as carbonate-cemented gravels/sands . The carbonate is subjected to dissolution forming sinkholes which do not necessarily end at some depth. Evans (1997) developed this conceptual model based on ^{18}O isotope distribution with chloride. The findings of Evans (1997) were continued by Ordens et al (2012) using the same data. The best answer as to why this conceptual model is now considered incorrect is given by the Referee 5 (see *Referee 5-5*) as quoted above and no further description is provided here.

Referee 1: P309, L24 – "fresh water bubble" is the wrong term for an expanse of water covering 20 sq.km.

Author Reply: We consider this is an appropriate term for the Poocher Swamp plume as it floats on brackish water and is subject to changes in size with climatic trends.

Referee 1: 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. It is under revision so how can this recurring statement to 'discredited' be made. It seems to reflect prejudice by the reviewer.

Author Reply: corrected to "…Somaratne et al. (2013) have shown…". Thank you.
With regard to references, unfortunately no other literature exists questioning the validity of conventional CMB, apart from the Somaratne et al (2013).

Referee 1: 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.

Author Reply: This was addressed at the beginning of Specific comments quoting Referee 5's comments on an earlier manuscript (Hydrological functions of sinkholes...).

Referee 1: 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.

Author Reply : The above lengthy explanation is correct and we are aware of them. The intent of this paper is different and is reflected in both the Title and content. We do not want to deviate from our main focus, which is to develop a more generalised CMB method that includes both point and diffuse recharge components. However, we have given key references such as Walker et al. (1991) and Cook et al (1992) etc for interested readers to get additional information on how the CMB method applies using unsaturated zone chloride.

Referee 1: 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.

Author Reply : Conventional or Classical CMB applies to both using unsaturated zone (lower part) chloride (c_u) and groundwater chloride (c_{gd}). In the absence of point recharge, c_g=c_{gd}.

Referee 1: 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.

Author Reply: The referee's comment is considered a misunderstanding of the text. We have never mentioned that Hutton (1976) equation is used for dry deposition estimates, rather including dry deposition. Line 5 reads : "rainwater including contributions from dry deposition (Ordens et al. 2012), which is estimated using the empirical formula developed by Hutton (1976)".

Referee 1: 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.

Author Reply: agree and corrected. Thank you.

Referee 1: P311, L1-4 – There's a grammatical problem with this sentence: ". . .it is generally unsuccessful in using unsaturated core method…".

Author Reply: This is the term used by Wood (1999). Agree and Corrected in the revision. Thank you

Referee 1: 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.

Author Reply: The referee's comment is considered a misunderstanding of the text. The Referee 1 should not think that measuring groundwater chloride in a point recharge basin and applying to conventional CMB (using c_g) will produce total recharge. This is what we have shown in an earlier manuscript, and Eq. 12 of this manuscript. When there is point recharge in the basin, conventional CMB does not apply or cannot be derived. You can derive (as we have shown) only a Generalized CMB (Eq. 12).

Referee 1: 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?

Author Reply: We agree that Subayani and Sen's work has no direct relation to this research. It was included as another modification to CMB method.

Deleted from the revised manuscript. Thank you.

Referee 1: P311, L10 – The reference to the author's own unpublished work at seminal points in the paper is not appropriate.

Author Reply: As mentioned before, there is no other work that extends the conventional CMB to a more generalized form in point recharge dominant basins. Future researchers wouldn't have this problem as they have our works as references.

Referee 1: 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.

Author Reply: Agree with the comment. We used the term ‘point recharge’ from Ward et al (2010) as it is a conceptual model based on total recharge derived via sinkholes (see highlight). The Ordens et al (2012) work is obviously a point estimate of diffuse recharge as are many other recharge estimate methods.

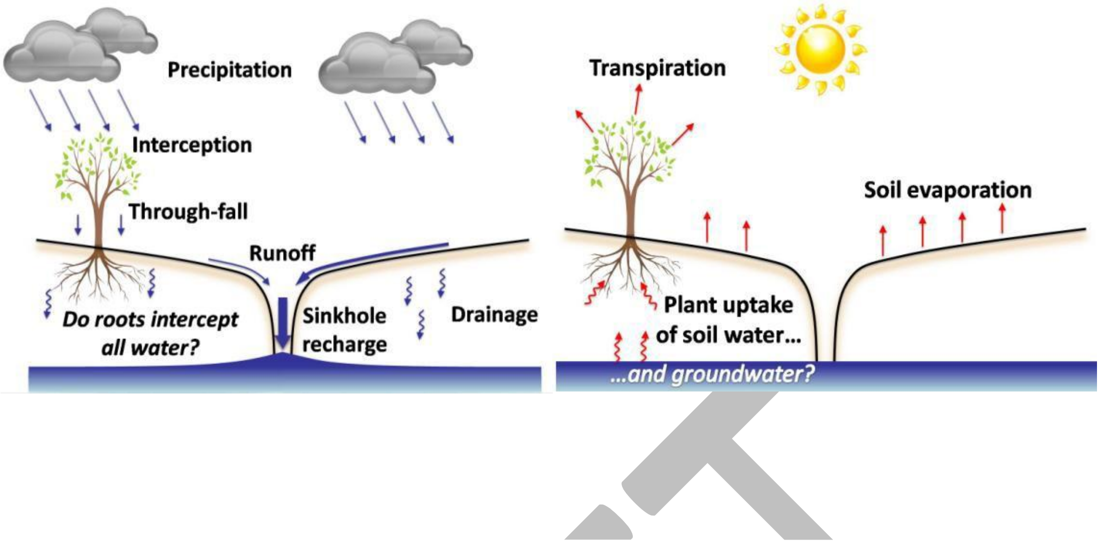


Figure 3 – Conceptual model of recharge and ET, for vegetated and non-vegetated sites. Trees intercept rainfall and transpire soil moisture and potentially groundwater. Runoff is assumed to become recharge via sinkholes. The model considers drainage and evaporation from an unsaturated “soil” zone.

Referee 1: 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.

Author Reply: We applied the conventional CMB method using groundwater chloride obtained from the point recharge zone primarily to obtain conservative (higher recharge). We have replicated the use of the conventional CMB to estimate recharge according to Ordens et al (2012), where groundwater chloride contains both point recharge and diffuse recharge chloride. We knew that such an application of the conventional CMB is not valid (due to theoretical limitations and the difficult task of getting representative samples) , yet we wanted to demonstrate to the reader how the total recharge estimate using the conventional CMB compared with the point recharge estimate alone.

For the three case studies described, total recharge estimated by the conventional CMB resulted in a lesser amount than the point recharge component alone. Since total recharge is a combination of both, point and diffuse recharge components, these results clearly indicate that the conventional CMB is underestimating actual total recharge. It is therefore concluded that the actual total recharge is larger than the one estimated via the conventional CMB.

We hope this is clarifies the matter for Referee 1.

Referee 1: 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).

Author Reply: What we are aiming to demonstrate is that the presence or absence of point recharge makes the difference.

Referee 1: 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.

Author Reply: We agree that the Ward et al (2010) study is not a detailed modelling work and as such have attempted to give sufficient information on how the critical parameters were obtained and what are the sensitive parameters etc. We have clearly mentioned in page 313, Line 27 that " The LEAHM modelling result is critically dependent on the assumption that all runoff becomes recharge via sinkholes". As such we cannot agree with the concluding comments of Referee 1 above.

Referee 1: 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.

Author Reply: As mentioned twice before, to our knowledge, these are the only works that have challenged the application of conventional CMB to point recharge dominant groundwater basins. Therefore, no other references exist to date. This is why we consider these two manuscripts are important in order to allow scientific debate, leading to further development of hydrogeological science.

Referee 1: 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.

Author Reply. The use of CFC, when mixing occurs between younger and older waters is an example where a recharge estimate method is not valid. However, we have changed the wording to "other alternative methods". Thank you.

Referee 1: 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 now refer to those descriptions for the current paper is not appropriate. Complete, rather than brief, descriptions are needed.

Author Reply: As of 13 Jan 14 (date of Referee 1's comments), the earlier manuscript has not been rejected. We agree that Referees had concerns, largely on the structure, rather than the content. The revised manuscript (following Referees and Editors guidance has been/or will be resubmitted). We consider it particularly disingenuous of the referee to continually make reference to Somaratne et al (2013) as 'discredited' and 'rejected' when it is still under review. This is introducing undue bias into the open review process.

Referee 1: P312, L9-10 – Here, and in many places in the manuscript, there are English issues. The grammar of this sentence is not correct.

Author Reply: Will re-edit the complete manuscript. Thank you.

Referee 1: P312-L11 – "comprised" should be "comprises", because the basin continues to be of these sediments.

Author Reply: Agree and changed.

Referee 1: 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.

Author Reply: Agree. However, presence or absence of Tertiary clay layer does not affect the recharge. The Tertiary Clay layer is below the watertable. Therefore, there is no need to make any changes.

Referee 1: 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.

Author Reply: There is no need for sinkhole shafts to continue to the watertable and beyond to become a point recharge source. There can be sinkholes with main shafts ending in the deeper part of unsaturated zone, but with minor conduits/solution features/fractures/cavities extending vertically downward as well as laterally carrying point recharge into the watertable. Please refer to Gun (1983) for all the point recharge mechanisms, including internal runoff etc.

With regards to the comment on the presence of water in the sinkholes, the visible presence of water is not necessary to be a point recharge source. This is clearly illustrated at Poocher Swamp where, if the Referee 1 visits at this time of the year, they are most likely to see only a dry sinkhole, because the vertical shaft ends up at some depth in the unsaturated zone (and then may continue as horizontal conduits to the watertable). Similarly, you may see water in the large sinkhole in Uley South, which is called Fern Sinkhole (a protected heritage site) after a wet period. See photos below.

Dry Sinkhole in Poocher Swamp Fern waterhole-Uley South

Referee 1: 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 den- sity 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".

Author Reply: We have addressed this issue above and consider the term both appropriate and in common use.

Referee 1: P313, L20-22 – The surface runoff estimates of Ward et al. (2009) are especially approximate runoff calculations, using the crudest of methodologies, which they them- selves 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 differ- ent 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 be- comes 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.

Author Reply: Thanks for the comment. This may be the reason that Generalized CMB estimate lower recharge values compared to other valid alternative methods. We suggest improving runoff estimation by using appropriate rainfall-runoff models.

Referee 1: P314, L12-13 – Correct to: "…average percentages… were determined…"

Author Reply: Corrected as suggestion. Thank you.

Referee 1: P314, L17-18 – Correct to: "…runoff volumes from…"

Author Reply: Corrected as suggestion. Thank you.

Referee 1: P314, L20 – Correct to "mid-winter"
Author Reply: Corrected as suggestion. Thank you.

Referee 1: 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.

Author Reply: This has been studied and reported. Poocher Swamp rarely spills annually. The last major spill was recorded in 1984. There needs to be an extreme event before the Swamp spills. Even if it spills, a large volume remains in the Swamp.

Referee 1: 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.

Author Reply: We have addressed the reference to the internal report above.

Referee 1: 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.

Author Reply: We only compared variability of hydraulic parameters and karstic nature of the aquifers.

Referee 1: 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.

Author Reply: We have explained this issue in Manuscript doi:10.5194/hessd-10-11423-2013 (Hydrological functions of sinkholes...). Small catchments contributing to sinkholes or drainage wells never generate large volume of surface runoff to create measurable fresh water plumes (these are only created when a large quantity of surface water enters the aquifer as a point recharge source).

Referee 1: 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 (de- pending on distance from monitoring well, aquifer properties, groundwater flow direction, etc).

Author Reply: Drainage wells are only present in Mount Gambier city in the Blue Lake capture zone which we have described in the site description. See page 313, Line 1.

Referee 1: 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.

Author Reply: The statement is conditional, and valid only if the chloride mass can be accounted for. The unlikely possibility of accounting for chloride mass is a difference issue, and that is why we have developed the generalized CMB to handle it mathematically.

Referee 1: 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.

Author Reply: We addressed this issue three times above and object to continued reference to a paper under review as 'discredited'.

Referee 1: 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.

Author Reply: We have presented data using Mount Gambier and Poocher swamp fresh water bubble to support the statement. In natural sinkholes such as in Uley South, it is difficult to measure as the extent of conduits are unknown, unless some salinity probes are installed all around the sinkholes.

Referee 1: 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.

Author Reply: We agree that Ward et al (2010) produced a conceptual model showing duality of recharge. Changed the text to include the reference. Thank you.

Referee 1: 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.

Author Reply: All above points are discussed and included in the text. Including all parameter descriptions into the Figure would certainly be a messy diagram. Note that this is a conceptual diagram, and only controlling essential parameters that are needed for equations are shown in the diagram. The rest is described in the Text.

Referee 1: P316, L9-10 – This statement is not true because Fig. 5 has partially penetrating sinkholes in it.

Author Reply: Please read the next line, which states unconnected sinkholes add runoff deeper into the unsaturated zone, ...

Referee 1: 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.

Author Reply: We refer to the text in doi:10.5194/hessd-10-11423-2013 (Hydrological functions of sinkholes...). In the conceptual model we have described the autogenic and allogenic zones with the coastal zones (where deep unsaturated zone exits under sand dune) excluded from the point recharge dominant area.

Referee 1: 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.

Author Reply: The equation is written for **Δ**t time. When P reaches the ground surface with c_p, it mixes with D and total chloride in P and is then c_{p+D}. At the end of **Δ**t time, some of P may evaporate and c_{p+D} is further enriched to becomes c_s.

We have written the equation 4 to relate to the ground surface.

Referee 1: P316, L19. The different components are positive and negative in an inconsistent manner. A positive value of each water flux needs to be defined.

Author Reply: Parameters are defined in the Fig. 5. P is the only input and Q_p, Q_0 and F are the outputs.

Referee 1: 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.

Author Reply: The assumptions are clear and correct. This is explained above in relations to c_p and c_s issues.

Referee 1: 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.

Author Reply: Infiltration into the unsaturated zone is always diffuse infiltration. Please refer to the page 311, Lines 24-26.

Referee 1: 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).

Author Reply: We have defined the C_u as 'chloride concentration of recharging water'. That is what it matters, as c_u is the chloride leaving the unsaturated zone.

Referee 1: 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.

Author Reply: Outer brackets of Eq. 8 removed. Thank you. In Equation (9) we have rearranged the terms. The issue of Pc_{p+D} addressed above.

Referee 1: 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".

Author Reply: To reach the watertable, chloride mass has to cross the watertable plain. This is what it meant. Just above the watertable, the capillary zone is also saturated.

Referee 1: 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.

Author Reply: As mentioned before, not all the terms are shown in the Figure but only the essential terms involved in equation development. This is described in the text as: "...there is no chloride loss from the saturated zone through evapotranspiration.", see page 317, Line 24.

Referee 1: 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.

Author Reply: We agree that we are working with chloride mass balance and changed the text to "......It is also assumed that lateral fluxes, and upward and downward leakages do not result in changes in chloride concentration....". Thank you.

Referee 1: 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.

Author Reply: The equation 10 is considered correct. Mass balance is not needed to arrive at:

$Q_p C_s - Q_p C_s = 0$ as Referee 1 suggests. Instead it was used to relate two unknowns in derivation of the generalized CMB, that is the relation between c_u and c_{gd}. Please see the page 318, Line 7.

Referee 1: 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 un- available 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.

Lead Author Reply: The paper is submitted as a discussion paper to allow discussion and debate on the relative merits of the views and information submitted with the aim of furthering the science of hydrogeology. It is hoped that the scientific community will robustly test the validity of the points raised in the paper and this, over time, will either validate or discount the paper. On that basis the allegations raised by the referee are irrelevant as the points in the paper will either stand or fall based on their own merits, irrespective of the credibility of the author or his organisation.

Despite that, the allegations raised above cast serious doubt on both the credibility of the author and his current employer as well as the integrity of the referee. It is inappropriate to raise such serious allegations in this type of forum and is suggested that if the referee has evidence to back these allegations that they are provided to the appropriate authorities for investigation.