Response to Ian Cartwright's review

Thanks Ian for the review. Your comments will help to improve this paper for the next version. All of the reviewers comments are reproduced below and our reply is printed in italics.

General Comments

This is a generally well written paper that provides a platform with which to extend the spatially-restricted studies of recharge to broader areas. As such it will be of use to both researchers and land/water managers, and is certainly worth publishing. My main comments largely relate to the start of the paper that I found very general – there are several places where mention of studies and approaches are touched on without being specific; most of those issues are dealt with later, but it takes a bit of time to get into the details of what is being done. In the same vein, some more specifics in the abstract would have helped. I would also like to see the study put into a more global context – some comparison in the conclusions with the results of other national studies or general methodological comments that would help a similar study be carried out elsewhere would give the paper a bigger impact.

I hope that the comments are useful for what is a good attempt to distil such a broad array of data into a nice succinct study.

Specific Comments

Abstract: The abstract is a clear indication of the subject matter of the paper and the main findings. However, it would be improved by some specific details. For example the term "field-based studies" cover a wide range of possibilities – some details of the type of studies used to make the conclusions would be helpful here. Similarly, on lines 10-11 (pg 5648), the strength of the correlations or the weight that each factor has could be mentioned.

> *The abstract is already 275 words, I am not sure that it should be too much longer. A sentence will be added about the strength of the correlations from the regression equations.*

Introduction: The start of the Introduction is a clear statement of the aims of the paper and the rationale for carrying the work out. It would be good if reference were made to one or two specific instances where decisions on resources were made using such unconstrained data. If there are any publically available reports / technical documents that can be cited then that would also be useful. Without trying to apportion blame to under-resourced water managers, an example where the approach was seriously flawed would be interesting.

> *Although we do have examples of where recharge has been assigned based upon a percentage of rainfall, the documents describing this are not publicly available and so cannot be cited.*

Pg 5649, lines 15-20. This just says that there have been several studies done – try to outline why these are of significance (i.e., why you are referencing them in this paper).

> *This sentence will be expanded in the next version of the paper.*

Pg 5649, lines 21 onward. Are all of these studies reviews or are some generating primary data? It might be more appropriate to give an overview of the types of recharge studies that have been carried out in Australia rather than the reviews of the studies.

> *The listed references here are all reviews. We decided very early on that this paper would not review all the recharge studies that have been undertaken in Australia as this has been published elsewhere – hence the summary of the reviews.*

Pg. 5650, lines 9-17. It would help if the methodology were briefly mentioned here. This would give the paper more of a global focus – as it is I might read the paper if I were interested in southeast Australia, but if the methodology looked interesting, it might read it if I were addressing similar problems elsewhere.

> *This paragraph will be expanded to put the work in a global context.*

Pg. 5651, lines 16-18. Some of this repeats material in the previous section. In what is a recurring theme, this section also needs a few more details. I had to open the supplemental spreadsheet to find out what types of studies were included – a statement here in the text that the data included water table fluctuations, Cl mass balances, and radiogenic isotopes would be useful to better understand the context of the paper.

> *This is a good idea; a sentence will be added here to describe the methods used in the studies that provided data.*

Pg. 5651, lines 16 onwards. One issue that it would be useful to mention here is that notwithstanding that all the studies are from dryland areas, the different techniques applied (14C, 3H, Cl mass balance water-table fluctuations etc) will characterise recharge over different timescales. In the study that I am most familiar with (Cartwright et al., 2007) that was partially the aim. It is discussed later, but would be useful here when setting the scene. The review paper by Scanlon et al. (2002) also makes this point and also discussed difference in area scales, which also would be worth a mention.

This is a good idea; a sentence will be added here that notes the spatial and temporal scales are different with different techniques.

Pg. 5652, Section 2.1.1. This is a better level of detail and answers many issues that I had above. This section would be better as part of the introduction (section 1) as it is not a "factor effecting [sic] recharge as such". There are two forms of 14C dating: firstly the one that is applied to relatively old waters (a14C < 100) that dates longer-term recharge; and secondly the one that uses the "bomb-pulse" C to date recent recharge (a14C > 100). A similar distinction can be made for 36Cl. Are both types grouped or separated in the table?

We have debated whether to move this section and decided against it. We specifically avoided reviewing methods for estimating recharge as this has already been done (refs in introduction), the contribution of this paper is in investigating the factors that effect recharge using a sizable database.

For 36Cl, there are only two recharge estimates that are in the data-base and they both use bomb-peak 36Cl (Cook uses measurements from groundwater samples and Walker et al from unsaturated zone profiles). The reason for this is that the studies we found that use 36Cl decay (ie older timeframe) do not give actual values for recharge (ie they use terms like very old water indicating low recharge, etc). Apologies if we've missed some studies where values are given.

For 14C, all of the recharge estimates in the data-base use the "traditional" type application. Again, apologies if we have missed studies that have used the bomb-peak 14C.

Pg. 5653, line 8. Are both Annuals and Perennials non-native vegetation?

For most of Australia the annuals would be introduced and the perennials native.

Pg. 5653, lines 2-7. It would be worth stressing the Australian context here – Australian native vegetation seems to be amongst the most water efficient globally so that the replacement produced extreme changes to recharge.

This discussion is in section 3.2 where it helps to explain the results rather than explain the methods.

Pg. 5653, line 20. Cartwright & Simmons said "could" not "would" and it was only for the case where vegetation cover was lost due to increasing aridity. I agree it was very speculative.

Will change "would" to "could" in the next version of the paper.

Pg. 5654, line 22. "Australia" rather than "the nation" would be better.

Agreed, will change for next version of paper.

Pg. 5655, line 1. Not sure that karst is a problem – there are some areas of karst in Australia but in many areas they are not extensive. Could you comment on this?

There are many areas of Australia where karst features have an influence on recharge. These include the SE of SA, the Eyre Peninsula, Daly catchment in NT and areas of Cape York in Qld. How extensive they are is a bit of a moot point as the mapping does not identify them.

Pg. 5656, section 3.1. In addition to the change in land use is there an issue of changing climate. Leaney et al. (2003. Salinization of a fresh palaeo-ground water resource by enhanced recharge. Ground Water 41, 84-92) make this point nicely for the SW Murray Basin – recharge rates there were higher during past periods of wetter climate. Correlation of the longer-term recharge tracers will be with longer-term average climate, whereas some of the very short-term measurements (e.g. WTF or lysimeters) might reflect anomalously low recharge in the recent drought.

Agreed. The next version of the paper will also note climate change as an influence on recharge estimates made using different methods.

Pg. 5656, lines 17-21. Without looking at the two references, I can't judge what the difference between these two methods is. Can you explain it in a sentence or two – and more particularly does it produce different results?

The Thorburn et al (1987) method requires chloride profiles to be taken at two points in time from the same location and a recharge rate inferred from the change in the profile through time. The Walker et al (1991) method compares a chloride profile under cleared land to a chloride profile under native vegetation and uses the difference between them and the time since clearing to infer a recharge estimate. These two variants of a recharge estimate using a transient chloride profile have not been compared in a field study as far as I know.

Pg. 5657. As noted above, there are two different types of 14C recharge estimates. The vast majority represented in this study are probably the more "traditional" type where 14C has been used to date relatively old groundwater; however, there are a few instances of very recent recharge rates being addressed using 14C (often in conjunction with 3H); in these cases one might expect estimates from 14C to agree more with those from WTF than say Cl.

For 36Cl, there are only two recharge estimates that are in the database and they both use bomb-peak 36Cl (Cook uses measurements from groundwater samples and Walker et al from unsaturated zone profiles). The reason for this is that the studies we found that use 36Cl decay (ie older timeframe) do not give actual values for recharge (ie they use terms like very old water indicating low recharge, etc). Apologies if we've missed some studies where values are given.

For 14C, all of the recharge estimates in the database use the "traditional" type application. Again, apologies if we have missed studies that have used the bomb-peak 14C.

Pg. 5659, lines 6 to 21. It is also worth noting that the impact of the water-efficient native vegetation also shows up in the very high groundwater salinities. In the Murray Basin, evapotranspiration is the dominant hydrochemical process and has produced of the most saline groundwater basins globally (much of the groundwater basin has salinities of >14,000 mg/L). The stable isotope data imply a major role for transpiration rather than just surface evaporation (e.g., Herczeg et al., 2001. Origin of dissolved salts in a large, semi-arid groundwater system: Murray Basin, Australia. Marine and Freshwater Resources 52, 41-52).

This is a good idea; a comment on groundwater salinity due to transpiration will be added to the next version of the paper.

Pg. 5660, section 3.3. Not sure whether there is any data to test this, but is there an impact in storm frequency and durations. I would imagine that another "confounding factor" is whether the winter rainfall is delivered in a set of heavy storms over a short period rather than as steady rain events with some separation.

We did consider this but did not pursue it. It is currently being investigated in a companion project (via modelling) and will hopefully be published in the not to distant future.

Pg. 5662, lines 20-24. Is there any chance that many of the recharge estimates are impacted in this way (and the authors have failed to take the evapotranspiration from the saturated zone into account)? From the point of view of trying to understand deep groundwater, I never sought to distinguish between gross and net recharge. Is ET from the water table likely to be significant?

The only studies that have considered net recharge and gross recharge separately were located at Tomago and Gnangara – both very high recharge over shallow water tables. A review of groundwater discharge studies did not identify many studies that had quantified groundwater discharge by vegetation [O'Grady, A., Carter, J., Holland, K. (2010) Review of Australian Groundwater Discharge Studies in Terrestrial Systems, CSIRO: Water for a Healthy Country National Research Flagship]. I suspect that ET from the water table is more widespread than has been reported in the literature.

Pg. 5666, Conclusions. A paragraph here comparing the results of this study to others carried out elsewhere in the world (or a statement to say that this is a first) would give the paper better global impact. This is a valuable study and it should have impact outside Australia. Perhaps some comments on the potential pitfalls / lessons in doing this type of work would also be useful

A paragraph will be added to the conclusion comparing the results here to those from Scanlon et al (2006) in a global review of recharge studies.

Technical Comments (mostly minor)

Pg 5648, line 6: "database" should be single word (and elsewhere in the paper)

This will be fixed in the next version of the paper

Pg 5648, lines 18-20: The sentence “The 95 percent confidence limits about the recharge predicted using these relationships is generally greater than an order of magnitude either side of the relationship developed” is not clear.

This sentence will be re-written for clarity in the next version of the paper.

Pg 5650, lines 9-17: These two paragraphs essentially say the same thing and could be combined.

These two paragraphs will be combined in the next version of the paper.

Pg5652, line 14: order of references (should be chronological?)

We have used Copernicus’ Endnote style in keeping track of references; it has placed the references in alphabetical order.

Pg 5652, line 1: “affecting” not “effecting” I think (effecting is a verb)

This will be fixed in the next version of the paper.

Pg 5653, line 5: should be “by up to...”

This will be fixed in the next version of the paper.

Pg 5653, line 8: errant “,” in “and, Trees

This will be fixed in the next version of the paper.

Pg 5658, lines 20-21: “we can see that recharge under annuals is greater” would be better as just “recharge under annuals is greater”

This will be fixed in the next version of the paper.

Fig. 1. Strictly the state abbreviations should be defined in caption.

The next version of the paper will use the full state name in the figure.

Fig. 2. The surface geology discussed in the text was simplified. Is this what is shown on Fig. 2 – it would be most useful if that were the case.

Fig 2 shows the actual surface geology from the GA mapping not the simplified classes that we used. There are over 10,000 lithologies listed in these maps which would make reclassification a very big task. Reclassifying the geology at the points where we had a recharge estimate was reduced to 48 lithologies and then simplified. We will investigate whether the same method can be used on the entire map.

Figs 1 & 3. This might be too difficult with such a large database but is there any way that the specific studies can be tied to the maps (if you already have a key map, perhaps it can be provided in the supplementary data).

This would be a useful thing to do so people can see where each study was located but I don’t think it would work in the format of a journal paper. The database in the supplementary information contains the latitude and longitude of each recharge estimate so the studies can be identified if the reader really wants this information. We were toying with the idea of presenting the database as a shapefile but decided that everyone can access an excel spreadsheet but not everyone can access a shapefile. The information in the spreadsheet can easily be converted to a shapefile if the reader would like the information in this format.