Ian Cartwright

We would firstly like to thank Prof. Ian Cartwright for his constructive comments on this manuscript, and for taking the time to provide such a detailed review. We have responded to each main and specific point individually, as well as technical corrections where appropriate. All alterations arising from this review have been made in the final manuscript that we will submit to Hydrology and Earth System Sciences for consideration for publication. Here we respond to each paragraph in the order that they are written in the original reviewer comment, and address each point as numbered in the review.

Abstract
The abstract is a clear indication as to what the paper will discuss but it could use a few more details (e.g. some key values such as the recharge rates, the timescale of land clearing etc). Also be clear as what you mean by terms such as "low rainfall" as they are rather subjective – to be clear put the definition in here. Try to make sure that the abstract gives the reader a clear understanding of the key results as it will improve the impact of the paper.

Low rainfall now defined in abstract, recharge values and details of land use change have been added.

Page 10002 Lines 14-20. The conclusion that recharge occurs mainly in the lower regions of the catchment may not be common (it certainly is not the "textbook" situation). I presume that it is due to some local effects (geology etc). However, here it is conveyed as a general conclusion, which I'm not sure will be the case.

This conclusion is supported by previous work (e.g. Scanlon's papers; Winter, 2001) which found that topography was important in arid/semi-arid regions, focusing overland flow into lowland areas where it could slow and more readily infiltrate to potentially become groundwater recharge more efficiently than on slopes. The conclusion presented here has been re-worded to more appropriately reflect its applicability to dry regions.

Introduction
The first paragraph presents a good general introduction to the rationale behind the study and places it in the appropriate broader context. I am not certain that I fully agree with the first statement in all cases. For example, in Australia where this study is set, land clearing which removed the very water-efficient native vegetation caused a major increase in recharge (and the attendant dryland salinity problem). Plantations may get the landscape back to their pre-clearing high E-T state. Are there any estimates as to whether plantations decrease recharge over and above what they were pre-land clearing?

This statement was meant to highlight that trees are greater water users than most crops or pasture, rather than over native vegetation; this has been clarified in the text.

Page 10003, Lines 8-11. This statement wouldn't mean much to anyone not familiar with SE Australia. This is essentially the dryland salinity problem that is being addressed. You need to be more specific as what the impacts and problems are.

The relevance of dryland salinity is discussed later in the text, and so we have incorporated aspects of that into the introduction to make the point clearer.

Page 10003, Lines 13-25. While the historical context is interesting, I don't think that you need it. It is well understood that groundwater recharge in semi-arid areas can occur from ephemeral stream channels etc. Just say that with reference to the reviews by Scanlon et al.

Removed.

Page 10003-10004. While the choice of references is somewhat subjective, I was surprised that Cartwright et al. 2006 (Constraining modern and historical recharge from bore hydrographs, 3H, 14C, and chloride concentrations: Applications to dual-porosity aquifers in dryland salinity areas, Murray Basin, Australia: J Hydrol, 332, 69-92) is not cited. This paper provided recharge estimates in cleared landscapes in SE Australia, discussed that recharge occurred away from the hills, and looked at the implications for tree planting as a mitigation mechanism; it also used some of the similar techniques.

This reference has been incorporated to provide context in both the introduction and discussion as the regional implications are highly relevant as suggested.

Background

This is well described and gives a clear indication of the main elements of the study site.

Page 10006, Lines 4-9. I presume that the rainfall is a long-term average, but the stream hydrographs may not be. Can you specify the timeframe that these data are collected over and give a more context? The reason for this is that given your reported variability in rainfall, if the hydrographs on the stream only represent a dry or wet period then they are likely not to record the overall situation. Also are the lengths of the hydrographs similar (for the same reason).

The time periods for the data used in this section have been added.

Page 10006, paragraph starting Line 24. The material in this paragraph is oddly ordered – discuss when and how the bores were constructed first and then the monitoring.

This paragraph has been re-ordered as suggested.

Methods

Section 3.1. I am surprised that rainfall (or more particularly throughfall) was not measured onsite. Given that one of the impacts of trees is that they intercept rainfall, this would have been valuable. Are there throughfall data from this or adjacent sites.

Rainfall was measured on site by 2 gauges, but was patchy in places due to equipment issues. Correlation of these gauges with the nearby BoM gauge was very strong, so we chose to use this record for consistency throughout the measurement period; this has been clarified in the text. Throughfall was not measured; any impacts by tree canopy interception are incorporated into the overall hydrologic impact of the trees.

Section 3.1. You mention that groundwater inflows are low, which they may be; however, are the streams ephemeral (if not they have a long-term water store somewhere in their catchment, e.g. in the regolith). This would be worth mentioning in Section 2.

The streams are ephemeral, and this has now been included in the site description.

Sections 3.3-3.4. While the methodology for these techniques is discussed elsewhere, you need to add a few more details here (analytical techniques for 14C & 3H and the detection limits and precision of the various techniques as a minimum). The reader needs to understand this without having to trawl through other references.

Section 3.5. Similar with the Rn, what is the precision of the analyses?

More detail from the cited references have been included in the text, as well as precision of analyses.

Section 3.6.1, Page 10011, Lines, 7-19. This paragraph is confusing. It commences with a discussion of the steep recession limbs which aren't well explained (all you say is that these steep limbs have been noted elsewhere, what is more important is why and whether there are implications for the WTF technique). The latter part of the paragraph discusses discharge and it is not clear how it relates to recharge estimates.

The steepness of the declining hydrograph limbs means they are not clearly exponential as is required for some methods of WTF estimation, and is likely related to the fact that the streams don't flow year-round. This is why we opted for the RISE approach, where exponential hydrograph limbs and consistent groundwater discharge are not requirements. This has been clarified in the paragraph.

Section 3.6.1, Page 10011, Lines, 25-30. The barometric pressure correction is a worry! I cannot think of why there would not be the usual inverse correlation. I presume that your loggers are unvented and that you used the barometric pressure from onsite or from the nearby weather station which should be close enough. However, the fact that you see a positive correlation needs more thought / explanation (as with the point above it is why it exists that is important).

You are right that this is strange, but the effect is consistent across the study site and has been extensively checked by multiple parties to ensure that it is not the product of error somewhere in the measurement and/or calculations. The discussion of why this could be is beyond the scope of this manuscript, and requires attention on its own.

Section 3.6.2. The assumed Cl concentration in rainfall of 4.3 mg/L seems very high by global standards and is also much higher than in Blackburn & McLeod, 1983 (Salinity of atmospheric precipitation in the Murray Darling Drainage Division, Australia. Austr J Soil Res 21, 400–434) which is a more comprehensive survey than Hutton & Leslie.

This value is derived from 3 separate datasets incorporating both wet and dry climates in the region of the study site (closer than the Victorian Murray Darling Basin), and is therefore considered representative of the study site.

Results and Discussion

There is a lot in this section, but the order that it is discussed in is difficult to follow. For example in the first section (4.1) there are interpretations that are based on the grain size data and groundwater ages; however those are not yet presented. While I don't necessarily consider that papers cannot mix results and discussion in the same section, the results need to be presented first. Sections 4.1 and 4.2 should go much later as they are mainly interpretations based on your data. While this is the exciting part of the study, the reader needs to be led through the logic a bit more.

Section 4.3. It would be much better to start off with some of this material as here are the data that you base the interpretations on.

***Having read through Section 4 two or three times, I think that you need to reorder it and attend to several rather superficial conclusions and interpretations. This is not being over pedantic, but it is difficult to follow and it detracts from what is really a solid study. As it stands you have the potential to confuse or not to convince the reader which will mean that the study does not have the impact that it deserves.

Firstly, present the data early on. There are observations mixed up with the interpretation and although it does necessarily not make for exciting reading, presenting your data first will at least mean that you ensure that you have presented it all.

Secondly, justify your interpretations better. For example at the end of section 4.3, you make interpretations based on the Rn and EC data from the stream. However, you leave the reader to surmise as to how you made these interpretations. I think I agree with the interpretations but they need to be justified. In some ways it does not help that data appear in Section 3 (for example the specific yield data are there); data would be much better presented where you use them.

We present the groundwater conceptual model first, using the basic hydrogeological data as they stand at this point in time, so as to more easily explain the more complex recharge results in context. We have simplified the presentation of the data within this, so that the data is presented clearly within the conceptual model. We then present the complex recharge data within the context of the modern model, as this data spans several periods of time within multiple land uses and the corresponding effects on groundwater levels, flow and recharge. We have altered the text throughout to more clearly present results and then interpretations. However, we think that it is better to retain the current ordering of sections.

There are data that are not well used.

For example the 14C and 3H data can be used to test whether some of the assumptions in the understanding of recharge are correct (e.g. whether there is piston flow from the surface or mixing between older and younger water in the aquifers- see Cartwright et al., 2006).

Additionally, rather than just using the 3H in a qualitative manner, it can be used to place quantify groundwater recharge. If you do do this I would probably use a Lumped Parameter Model (e.g. Cartwright & Morgenstern, 2012. J Hydrol, 137-149 and other references cited there) rather than the renewal rate method. This will give an extra dimension to your study and make use of data that are currently mentioned but not really interpreted.

We have chosen not to take this approach as it is beyond our area of expertise. In Dean et al. (2014) we considered the tritium levels measured in the context of atmospheric tritium since 1960, and found that due to the variability of atmospheric tritium and the decay of the bomb-pulse, it was difficult to ascertain exactly when the water first entered the study system. To avoid these complexities we have opted to use the tritium qualitatively, which we feel still gives a solid foundation for the discussion.

The Radiocarbon dates (Table 1) are presented without discussion. Firstly, you need to explain how you corrected the ages (corrections for calcite dissolution etc); while I wouldn't image that this was significant, you still need to go through the process. Secondly, and more importantly, what do the ages mean for the hydrogeology – are they telling you about historic recharge, mixing of old and young waters or what?

These aspects are presented in Dean et al. (2014) and were not included to avoid repetition between the two manuscripts. However, we have incorporated the relevant discussion in the text to clarify your points. The discussion of mixing of old and young water is now incorporated into the conceptual model.

Another way to view the CMB data is to estimate how long the Cl in a given volume of aquifer takes to be delivered (since you know the rainfall amounts and Cl concentrations, this should be straightforward). If it is several hundreds to thousands of years, then you can comment on what timescales the different recharge techniques are estimating recharge.

We did not incorporate this discussion as a salt balance was beyond the scope of this manuscript. The delivery and export of salt to the system is complex, considering that there was the initial land clearance by European settlers and the resulting salinization of the land by rising water tables dissolving salt previously stored in the soil profile, as well as the modern land use change. Incorporating a discussion of this as well as the natural concentration of salts in the groundwater by evapotranspiration is necessary for the suggested calculation, and is considered beyond the scope of this manuscript.

It would also be interesting to mention whether and how the different techniques compare between the forested and cleared catchments. You do this for the WTF; however, if say the CMB or 14C is reflecting older recharge rates there should be little or no difference between the two catchment types. This is important as it gives you confidence that the two areas behaved similarly prior to the landscape modification.

This is a good point, and the suggested discussion on the CMB has been incorporated into the text.

Conclusions

I guess that where to plant the trees depends on your overall objective. Other studies have advocated planting trees on the lower parts of the catchment precisely because it was desirable to reduce the water table more and that was the issue being addressed.

This is correct, and we have attempted to make this very point clear within the text, but will make sure it is reflected in the conclusions also.

Figures.

Fig. 1 is well enough known that it need not be included.

Removed.

Fig. 3 seems to show little more detail than is in Fig. 2, could these be combined.

It is hard to combine these two figures for the sake of clarity, but figures 3 and 11 have been combined to reduce the number of stand alone figures.

Fig. 9. This is in different units (Bq/L) than you quote in the text (Bq/cm3); be consistent.

Corrected.

Fig. 10. The numbers are hard to read (there is a lot of space to make them bigger).

Corrected.

References:

Dean, J. F.,Webb, J. A., Jacobsen, G. E., Chisari, R., and Dresel, P. E.: Biomass uptake and fire as controls on groundwater solute evolution on a southeast Australian granite: aboriginal land management hypothesis, Biogeosciences, 11, 4099–4114, doi:10.5194/bg-11-4099-2014, 2014.

Winter, T. C.: The concept of hydrologic landscapes, J. Am. Water Resour. As. 37, 335–349, 2001.