G. Bertrand

We would firstly like to thank Dr Guillaume Bertrand 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.

General comments:
In my opinion, this paper can be a valuable contribution to better understand hydro(geo)logical processes and foresty/pasture interactions. The paper is generally well written and easy to follow. The pictures are of good quality.

I have two main concerns that should be, in my opinion, addressed in a revised version. At first, I have some reserves about the CMB method and especially on the chloride concentration calculation in precipitation. Using arithmetic median is not adapted to my knowledge. A better estimation could be done through to Volume weighted mean as I mention several times in my comments. Same remark for the estimation from weirs. Weighting the data is important given the natural variability of chloride caused by the amount effect (=dilution effect), when precipitation events are great.

We apologise for the misunderstanding here, the rainfall Cl content is all volume-weighted in the source references, as is the chloride content of the streams at each weir. We have used the volume-weighted values in our calculations. The median calculated is of the yearly volume-weighted averages from each of the studies. We have modified the text to clarify this.

Secondly, I suggest the authors to strengthen their discussion about the management option concerning tree plantation. They address 'trees" in a very general way on the basis of their results from eucalyptus plantations. But there is an abundant bibliography about forestry management and gw (including eucalyptus, see suggested references), that I advise to include/discuss, to give more perspective on the interesting results which were provided.

We agree that it would be useful to give more perspective on the implications of our results, and we have expanded the discussion to incorporate some of these points.

Specific remarks:
P10002 – I suggest to moderate a little bit, and may be remove "negatively"...Actually a forest may reduce the availability of groundwater for public water supply or industrial purpose, that´s true, and it is perceived negatively by these users. But for people involved in agroforestry and its depending economy, it is not necessarily seen as "negative" but just as a consequence. As you say in P 10004: ***"what areas should be planted to maintain or intercept groundwater recharge, depending on the management application"*** what is consistent with the fact that the impact can be viewed as positive or negative, depending on the areas and depending on the users.

This point is well taken, and we have reworded the opening paragraph to more accurately reflect the multitude of applications for plantation forestry.

P100004 – How increase of recharge can lead to a rise of salinization of water and of land surface?? If you want to evoke the consequences geochemical variability, you should at least evoke the associated process to help the reader.

More details have been added here to better contextualise the example used.

P10009 – What is the age of the groundwater? Actually negligible chemical variability seems somehow contradictory with the fact that you studied a local groundwater system, generally much less buffered from a geochemical point of view than a regional system.

We have added details of the groundwater age (see also response to comment below). The seasonal variability in the groundwater chemistry is negligible; details are provided in reference cited.

P10009/ Pàragraph 3.4 - At first this sentence should not be in a methodological paragraph. Secondly when speaking about already known relevant information for your study, Please inform a bit more the reader. It is OK to me if you refer to a previous work, but you should be a bit more informative this paper. I would like to have an idea about the age estimation without needing to see in other papers. May be you could summarize all what you already know in the context part! If I summarize your paragraph, you say that the methodology could be read in another paper and you do not inform about ages, so in my opinion this paragraph is useless for the reader, in its current form, at least.

A fair point and the relevant details and information have been incorporated into the text.

P10011 - Is there any known explanation for this inconsistency? I have to admit that I am not a specialist about barometric effect in gw. But perhaps you could consider this hypothesis: May be it is because you are focusing in a recharge zone and not a discharge zone. in a discharge zone there is schematically 2 strengths which are opposed, a gw arriving from upland with its own pressure and the atmosphere pushing over (the few example I know about barometric effect analysis were done in discharge zone. In the recharge zone, the gw is not so much arriving but rather going away, deeper in the system and the two strength are not opposed....If so, would it not be a new (or no?) method to identify recharge zone from discharge zones?

This issue is very complex: it is not that the barometric pressure and groundwater fluctuations are not correlated (they are, very well), but that they are inversely correlated – the opposite of what is expected. The effect is consistent across the site and is not the result of error with the measurements or calculations; further discussion of why this occurs should be pursued, but is not relevant to the aims of this manuscript.

P10012 - In Healy and Cook´s paper, I did not read that exactly: yes indeed specific yield is spatially variable but "Specific yield is treated as a storage term, independent of time [...]". Actually in this paper, the time issue is much more addressed because of the estimation of specific yield may require a lot of time (sometimes several years) and therefore the obtained results are changing if you do not wait the same duration. So I suggest you to reformulate to avoid any misunderstanding.

This section has been rewritten to clarify this.

P10013 - Why did you use the median? How can you be sure it is representative? To my knowledge, the unique way to have a representative concentration in the precipitation reaching an hydrosystem is to calculate the volume-weighted mean concentration (e.g. Appelo and Postma, 1994; Celle-Jeanton et al., 2009; Bertrand et al., 2008).

We apologise for the misunderstanding here, the rainfall Cl content is all volume-weighted in the source references and we have used these values in our calculations. The median used is of the yearly volume-weighted averages from each of the studies used for the CMB rainfall input value. A sentence has been added into the CMB method section to clarify this.

P 10016 - Why the E.C is informative about gw discharge?? If you want to use this tracer you have to provide in the text the gw conductivity and the rainfall (or better, the recharge as you can calculate this by taking into account evapotranspiration) conductivity as well. In many system, such a conductivity would be interpreted as a first insight as a recharge by groundwater! So if your case is different, you have to prove it with the data. In addition, you say that the gw recharge is less important

in the eucalypt catchment but you mention higher conductivity (7700±2300 Scm 1)...what could appear contradictory with the common statement that higher the gw participates, higher is the E.C. Please clarify.

The groundwater at the study site is saline, the values of which are provided in Table 1, and the rainfall is very fresh (values provided in the CMB methods section). The salinity of the streamflow comes from salt in the soil zone and groundwater discharge, therefore the higher salinity in the eucalypt stream could be due to higher groundwater input, but we have chosen not to calculate this in this paper. Therefore this qualitative inference has been removed, and the 222Rn data is used on its own.

P 10018 - What is better than an arithmetic median in rain, for sure. I strongly suggest you to propose to use the Volume-weighed mean since the beginning of the paper, or at least the surface water estimation. It would shorten your paper and would avoid to read results people would have difficulties to agree with since the beginning. (Once again, consider to have a look to at least in Appelo and Postma, 1994 but also to many other papers aiming at characterize the input signal in hydrosystems from a geochemical perspective.) Nevertheless, still, you seem using only arithmetic calculations: "***Cro is calculated from the average EC measured at each weir (averaged over the available data at the weirs from May 2010 to February 2013), converted to Cl using the EC: Cl ratio for the study site dataset (0.39 and 0.37 for the pasture and eucalypt catchments respectively)"*** OK but what kind of average? arithmetic? Volume weighed? (with the discharge at the weir?)? The second option should be used as for precipitation (replace Hprecip by discharge in the equation mentioned above). I would also suggest to select only entire hydrologic year data, e.g. between March 2011 and February 2013 in your case (=2*12 months).

Please see previous comment regarding volume-weighted averages. The chloride content of the streams at each weir is also volume weighted, and this is now stated in the text to avoid further misunderstanding here.

P10018 - Please could you explain how the native vegetation would impact the CMB calculation?? More evapotranspiration with the native vegetation? if yes, say so, but please clarify to help the reader.

Recharge prior to European land clearance would have been in a steady-state equilibrium, and recharge was lower than under cleared land covered in pasture. A sentence clarifying this has been added to the text.

P10021 - This paragraph is interesting. However, it gives the impression that all this statement is based only on your results. Eucalyptus are known to be strong consumers of (ground)waters, but are they representative of the whole range of tree specie options the managers, farmers have? Is there any other type of plantations that would be possible? As you mentioned, tree plantation somehow facilitates infiltration through to roots pathways, but they can have negative feedback if consumption of (ground)water increases. However, if we imagine that one can set low water consumer species, would it not be a valuable way of management? (And knowing the local hydrologic specificities, is it reasonable to set eucalyptus in the upland part? Will they be able to adapt in a drier environment??). For example, some studies have shown that water uses may change during the hydrologic year but that some biological (e.g. growing, flowering) or ecological (competition) processes may impact water uses, processes which are supposed to depend on species, or at least to genus (cf. e.g. Bertrand et al., 2014, and references review therein). Other studies have shown that through to an establishment with wider spacing, one can regulate water uses by trees (see references below).

Therefore, I suggest you to document a bit more about eucalyptus ecology and adaptative and/or management possibilities, and also to discuss the opportunity to set other type of vegetation, presenting variable added values/impact from an economic perspective and from a water consumption perspective. In my opinion, addressing these two points in your discussion would strengthen much

more the outputs of your paper and would provide a valuable information for the community. In addition it would discuss more what you promised in the paper title: ***" Where to locate a tree plantation within a low rainfall catchment to minimise impacts on groundwater resources"*** (You do not mention only eucalyptus) A rapid search in internet led me to discover the following references which can be useful to you to discuss these points.

These points are excellent. We agree that it would be useful to give more perspective on the implications of our results, and have expanded the discussion to incorporate some of these points. However, the aim of this study was to provide a hydrogeological perspective on the impacts of plantation forestry versus grazing pasture, and to show how this may apply to current management practices. The aim is not to suggest new or alternative methods to control water consumption by trees, but rather to show how recharge varies spatially and the implications of this to locating tree plantations within a catchment. This is all presented from a groundwater perspective, and so the title has been altered to reflect this:

"A groundwater recharge perspective on locating tree plantations within low rainfall catchments to limit water resource losses."

Figures

Figure 4: 4a: I am not used to read barometric pressure in cm. Is it not supposed to be expressed as hPa?

The barometric pressure here is presented as cm of equivalent water for comparison with the water level data; this has been clarified in the figure.