

Authors Response

We thank the two reviewers for their largely positive comments on the paper that we have addressed in the revised version of the manuscript.

Anonymous Reviewer #1

We thank the reviewer for their comments on the manuscript, most of which were requests for more detail and clarification of specific aspects of the work. The following (in **bold**) are a list of amendments and responses to reviewer comments (page and line references refer to the revised manuscript).

General comments

The paper contains a considerable amount field data ranging from field parameters such as EC to major ions and age tracers. Presentation and interpretation of the data is driven from a geochemical, somewhat stationary point of view. I think the variability between the sampling periods have not been explored sufficiently. For example, water table variations and variations of EC are hardly used for interpretation.

Try to go beyond the standard geochemistry textbook data analysis. I suggest to look at your data from a more explorative view rather than squeezing the data into a fixed framework of interpretations e.g. Mixing analysis in Figure 11

Bank storage is a dynamic issue. So how variable is geochemistry between the sites over time? This could be quantified based on coefficients of variation to get rid of the different mean values). Which parameters (or ratios of parameters) are most variable. Is there any chemical variability correlated to hydrologic dynamics?

The main conclusion is that gaining conditions may hamper bank filtration, which is indeed an interesting finding. Support it with more data (EC, Ratios...). Too much text is dedicated to explain the geochemistry, which does not really lead to the dynamics of bank storage and gw- sw interaction.

The temporal dynamics associated with bank storage still present a number of questions and challenges. In this paper we used repeated determinations of major ion geochemistry and radiogenic tracers to understand those processes. As the reviewer notes there are also EC and head data from the sites that were collected continuously over two to three years. Presentation of EC data has been made in section 3.2 and further discussion of the variation in EC values between sampling periods has been presented (Pg 16, line 29 to Pg 17, line 2). However, fully integrating the EC and head data into this paper would be difficult as it represents a considerable volume of data that would require extensive explanation. The aims of this paper were focussed on characterising the impacts of bank infiltration on the chemistry of bank water, and we

have retained these aims in the revised version. The hydrochemistry data that we present here provides the conceptual understanding of processes in the Tambo river banks that is necessary to understand bank exchange in this setting. Interpreting the EC and head data fully would require a separate publication, but one which needs the understanding provided by this study to provide the necessary context.

We have tried to clarify the variation in geochemistry at each site over time, with specific reference to the impact of changing hydrological conditions on chemical concentrations and ratios. The variation in groundwater chemistry during different periods is illustrated via colour coding in figures 5, 6, 7 and 8 and are noted in text – eg:

Higher Na:Cl ratios tend to be associated with low rainfall periods, suggesting greater water rock interaction at these times (Pg 11, lines 13 to 17).

Higher Cl:Br ratios tend to be associated with higher rainfall periods, suggesting mobilisation of Cl from the soil profile (Pg 11, lines 18 to 24; Pg 11, lines 28 to 30).

We have also reported the relative variations in parameters in section 3.5 as suggested by this reviewer.

Specific comments

p. 1653 l. 5: To some extent understanding a concept also means to have some quantitative understanding. Maybe reformulate. p. 1653 l. 15: The concentration of what? What does this imply?

We have clarified the sentence at Pg 3 lines 5-6. Where the concentration of solutes in groundwater and surface water differ, bank infiltration can result in the development of a zone of mixing in the river banks between the regional groundwater and the infiltrating river water. This zone will contain water with solute concentrations that are an intermediate between the two. Observations and modelling imply that solute concentrations in this zone can change slowly, as the regional groundwater gradually replaces the mixed waters. This implies that the residence time of bank storage may be on the order of months (Pg 2, lines 25-32).

p. 1654 l. 1-6: The step from the conceptual understanding of bank storage to the limited number of field studies in Australia is quite big and not needed and somewhat implies a limited scope of your work. Later you bring it back to the general picture (p. 1655 l.4). I suggest to remove this local aspect at this prominent place in the introduction.

We have moved this section to later in the introduction where the specific aims of the project are outlined (Pg 3 line 31). This has been included to emphasise that while bank storage is generally recognised as a potentially important process, there have been few local studies. Noting this in the Australian context is important as the Australian climate and river regimes are different from those in many other parts of the world and there is no certainty that bank storage in Australia will be exactly the same as elsewhere

Section 1.1. This section contains a lot of information on regional geology (p. 1655 l. 14-26). Is this really relevant for understanding the hydrologic conditions at the sites? In contrast I miss some hydrogeologic information of the well transects. Please add information how the screened sections of the wells are related to the local stratigraphy in relation to this: Figure 1 please add some stratigraphic information to Fig. 1 a) b) c).

Section 1.1 has been revised to omit superfluous geological information and include the more relevant hydrogeological information. There is not a lot of detail available on the large-scale regional hydrogeology of this area due to a lack of groundwater monitoring bores. However, it is most likely that the Tambo River represents a discharge point for groundwater from the shallower aquifers rather than the deep aquifers and we have noted this in this section.

The local stratigraphy at the bore transects previously described (Pg 1656, lines 20 to 23) indicates the presence of clays. These have been detailed more thoroughly (Pg 5, line 29 to page 6 line 2). Unfortunately as clay rich lenses are on the order of 10's of cm's these are difficult to illustrate in Fig. 1. While clays were difficult to identify via mud rotary drilling, their presence at shallower depths is indicative that they are responsible for the formation of the semi-confined aquifer. Given that this is consistent with the regional hydrological setting, we are confident in this interpretation.

p. 1656 l. 5: I suggest to use m^3/s for stream discharge.

Units have been changed from m^3/day to m^3/sec in text (Pg 4, lines 20 to 22) and in Fig. 2.

p. 1657 l. 7: To my knowledge the Hvorslev method requires some prior information on anisotropy. Please provide the value applied.

It is correct that an anisotropy ratio can be included in slug test analysis, however most studies use the simplified Hvorslev equation that was used here, especially when the screen length is significantly larger than the screen radius (e.g., Fabbri et al., 2012: Estimation of Hydraulic Conductivity Using the Slug Test Method in a Shallow Aquifer in the Venetian Plain (NE, Italy). Italian Journal of Groundwater. DOI 10.4409/Am-045-12-0048). This makes an assumption that the sediments are isotropic which may not be the case. However, hydraulic conductivities were only determined to provide a general characterisation of the hydraulic properties of the sediments and not for detailed modelling purposes. We measured the water level response with a logger (1 second interval) and used a down-hole pump to lower the water levels. The recovery times were commonly <10 seconds (and sometimes shorter), which means that the estimated K values are likely to be minima. Given that we do not know the anisotropy ratio and that the K values are only used to generally characterise the sediments, we have not recalculated these values. We have explained our method in a little more in text (Pg 6, lines 15 to 20).

Section 3.1. It would be helpful to provide also hydraulic head differences between the river and the gw, rather than absolute levels alone. Also any indication of variability would be good (tie together the Information provided in Figure 3 and add the information from the other sampling campaigns)

As suggested, the difference between groundwater and river levels have been added to the discussion in section 3.1. These have not been discussed for the Kelly Creek transect as tidal changes limit the use of river gauging data in this setting. As mentioned previously, analysis of groundwater and river elevation at greater temporal frequency are beyond the scope of this paper as the amount of potential data is too large to be discussed in a concise manner.

Section 4.1. I find the interpretation of the data speculative and not very elucidating for the "source of water" question. I think it is not the key point of the paper to explain all the processes that control bank water chemistry. Despite this, the section is difficult to read. I suggest to present the interpretation site by site and join it at the end, or alternatively go separately through the parameters and compare them for each site. Now it is a mix of both.

The reviewer is correct that the key point of the paper is not to explain all of the hydrochemical processes occurring within the river banks. However, we feel that characterisation of groundwater chemistry is necessary to allow the impacts of bank storage to be discussed effectively in section 4.4. The discussion in section 4.1 (Pg 12, line 13 to Pg 13 line 17) summarises the major hydrogeochemical processes and is now shorter.

p. 1664 l.16-19. How realistic is this model assumption? I thought the site is gaining thus GW is discharging to the stream and not "lost" to deeper groundwater. Upwelling of deeper groundwater is one of your key interpretation of the old ages close to the river.

The renewal rate model approximates the activity of ^3H in a store assuming that a certain volume is lost from that store each year and replaced by a similar volume from the recharge. The renewal rate model has been applied to the recharge at river banks, but it is probably more applicable to recharge via rainfall. The main point that we wanted to make here is that the covariance of ^{14}C and ^3H activities is model independent. We have removed the original Eq. (1) and used the EPFM as the primary flow model (Eq. 2 in the original manuscript) as that is more appropriate for the type of system that we are studying. That being so, we have redrawn the covariance curves in Fig. 9 based on the EPFM model; however, we have noted that the same conclusions would have arisen if we had chosen a dispersion or renewal rate model. The groundwater residence times in Tables 1-3 are from the EPFM.

p. 1665 l.5-6. What are the reason for selecting this model and not another one.

This EPFM model was chosen as it approximates well groundwater flow in near-surface aquifer systems (e.g. Zuber et al., 2005; Morgenstern 2010). As discussed above, the conclusions that mixing has occurred is model independent and would be predicted

from alternative conceptualisations of the flow systems such as the dispersion model or the renewal rate model.

p. 1668 l. Why have the activities only been analyzed ones?

The ^{14}C and ^3H activities were only analysed once partially due to the scope of the project. While it would always be nice to have more data, these data show the mixing clearly. Further, other hydrochemical data can be used to understand the behaviour of the system during the other sampling rounds.

p. 1668 l. Does the river flood over the banks during this event?

The river gauging data indicates that over-bank flooding did occur briefly (for a few hours) and that some of the bores closer the river may have been overtopped during these floods. However, the bores furthest from the river were not overtopped and the higher levels of the banks where these bores are situated was not flooded. These bores were used for mixing trend analysis.

Figure 3: It would be more telling if the topography of the section would be included. You may use the drawings of Figure 1 a) b) c) as a basis. Also why don't you show the results of all sampling campaigns?

Site topography has not been plotted on Fig. 3 as the vertical scale is exaggerated to show the differences in groundwater elevations (which are < 0.5m); the topography is shown on Fig. 1. We have included representative trends mainly for clarity – including all the trends makes the figure very busy without conveying any more understanding.

Figure 4: It seems that there is a yellow river signature in the GW bubble. Obviously there is considerable variability e.g. between Feb (yellow) and March (red). Particularly the Feb data suggest a slope different than LMWL and GMWL.

There is a river sample from February that has similar $\delta^{18}\text{O}$ and $\delta^2\text{H}$ values to most groundwater samples; this is now discussed on Pg 9 lines 16-19. The dashed lines in Figure illustrate the typical $\delta^{18}\text{O}$ and $\delta^2\text{H}$ values in groundwater and surface water; this has now been clarified in the figure caption. We are not sure whether the changes in slope of the arrays are that significant; however, we have noted this in the text (Pg 9, lines 6 to 8). The differences may relate to higher evaporation during summer (February), however as such changes are unlikely to be related to bank exchange they have not been discussed further in section 4.

Figure 6,7,8 Why do Figures 6,7,8 not show the same parameters (c is always different). It would help to follow the interpretation if all figure would show the same set of parameters. Why not adding d and e and show everything.

Figures 6,7 and 8 highlight the major processes occurring at each site as discussed in text. Producing extra figures to maintain consistency would add to the text without necessarily providing much more understanding. As the reviewer noted earlier, the

original manuscript had some superfluous discussion of geochemical processes that we have cut down.

Figure 11. I honestly have problems with the interpretation here. The mixing lines seem arbitrary and hardly represented in the data

The mixing curves presented in Figure 11 were constructed using the molar concentrations of the end members (river, deep groundwater and shallow groundwater end members) as shown in Tables 1 to 3. The curves show the concentrations and resulting ratios predicted for the mixing between the end members using a two-member mixing model (ie. $[x \times \text{End member A}] + [(100 - x) \times \text{End member B}]$ where x is percentage and A and B are the concentrations of two end members). The chemical concentration of a mixture of the two end members will plot along the curves shown in Figure 11.

Anonymous Reviewer #2

The main comment of this reviewer was that the paper did not discuss local and regional flow systems. While it would be ideal to integrate the groundwater-surface water system in the Tambo River catchment to a full description of the regional hydrogeology, in practice this is not possible. Our aims in this study were to examine specifically the groundwater-river interaction and the bore network that was constructed was done so for this purpose. We know considerably less about the deeper regional flow systems than we about the local ones that interact with the river. There are few bores in this part of the Gippsland Basin and while the stratigraphy of the basin has been studied (e.g., Birch, 2003), there are sparse details on the specifics of the hydrogeology (aside from the measurements that we report in this paper, there are no measurements of hydraulic conductivity nor regional scale-age distributions in the Tambo Catchment).

We have added some more details of the regional hydrogeology (Section 1.1). While the deep regional groundwater is artesian, there is little evidence that this regional groundwater generally discharges into the middle reaches of the Tambo River. The deep groundwater has a higher salinity, is generally anoxic, and locally has higher temperatures than the shallow groundwater. There are a few springs fed by this deeper regional groundwater around Lake King; however, these are rare elsewhere. The majority of groundwater that discharges into the middle reaches of the Tambo River catchment is most likely derived from the shallower aquifer systems (Southern Rural Water, 2013). This is consistent with the presence of clays in the Gippsland Basin sediments that produce a compartmentalised aquifer system. At Tambo Upper the groundwater in the deeper (24 m) bore is artesian and anoxic and may be derived from the regional groundwater system (although this is not easy to prove). In any case, there is not much of a density contrast between the different groundwaters that would influence mixing. The “deep” groundwater has a TDS of up to 2000 mg/L, which at 20 C would have a density of 999.8 kg/m³ whereas the river water which infiltrates the banks with a TDS of 60 mg/l would have a density of 998.3 kg/m³.

While the papers of Toth are clearly valuable in conceptualising groundwater flow systems, there seems little value in a summary of that work (which is well known generally) in this paper; we have added the reference to Toth 1963 to explain the type of flow system that we are dealing with as this will be familiar to most readers. Also to reduce confusion we have been more specific as to our use of the term “deep” noting that at most sites we could be dealing with a localised system that has shallower and slightly deeper parts to it rather than mixing between local and regional systems.