Response to Reviewer #2

We thank this reviewer for their comments on our paper and offer the following responses.

While the paper is interesting and reasonably well presented, I am not totally convinced that it adds a great deal to hydrological science understanding. It has been knw for a long time that mathematical baseflow separation approaches are not easily able to distinguish between processes contributing to low flows in rivers.

This is probably true; however, there are few papers that have discussed this. Moreover, our paper does not really discuss the low flow issues as such but the potential mismatches between the physical and chemical methods of determining baseflow at varying stages of discharge. To our knowledge this has rarely been done, and most papers that have attempted this have concentrated on making the two approaches agree rather than trying to understand whether the mismatches offer some additional information.

I accept that this paper has the potential to contribute to the quantitative understanding of these differences and perhaps that is enough to make it a sufficiently valid contribution, but I am not totally convinced. Regardless of this point, there are a number of questions that I think the authors need to address before the paper can be accepted. I was not sure whether to identify these as major or minor revisions as they are somewhere between.

I am not sure that the authors identify all of the possible processes contributing to low flows, but there is not really enough information provided about the catchment (slopes, soils, etc.) to get an idea whether other processes are likely to be present or not.

We can certainly provide more information, the original paper tried to be concise in the description of the regional setting. In brief, the catchment consists of mainly land cleared for broad acre agriculture (cropping, sheep and cattle). The basalts and alluvials have mainly sandy loam soils that are typically 2-5 m deep and which are moderately well drained. The upland areas that form the headwaters to the Barwon River are covered in remnant native eucalypt woodland. Except for the headwaters, the slope angles are low (<10%). There are abundant floodplain pools and marshes that occupy depressions in the relatively young volcanic landscape. These together with bank storage (the river is locally incised to 2-3 m below its flood plain) probably form the dominant transient water stores, although interflow and soil storage will also occur. In terms of the relative importance of the various transient stores, while we were able to constrain the timescales of bank storage, we did not advance this as the only store (this could have been cleared in the paper).

It would also be useful to have some idea of why the groundwater is so highly saline, when the surface water salinity is expected to be very low.

This was covered in Cartwright et al. (2013), but we can add more details in this paper. As in most of SE Australia, groundwater salinity is governed by the degree of evapotranspiration. A combination of low rainfall, subdued topography, and water-efficient native vegetation leads to high evapotranspiration rates and saline groundwater. Specifically for the basalt catchments, such

as the Barwon, the recent (<1 Ma) lava flows have a young topography with dammed drainage courses and lakes and wetlands formed on the basalt surface. This has resulted in the formation of numerous small endorheic drainage basins in which saline lakes formed; recharge from these lakes (those higher in the landscape are above the water table while lower lying ones recharge when lake levels are high) produces saline shallow groundwater. Currently, the river systems are re-establishing and much of this saline groundwater forms baseflow to the rivers.

On the issue of salinity observations, why are some given as both TDS and EC - this is confusing and it would better to use the same units throughout, even if some approximate conversions are necessary. I would also like to see more consistency in the wau in which numbers are presented - 3500 and 13,000 and 8.1x10ˆ3, etc.

As discussed in the response to other reviewers' comments, it would be better to use Cl for the calculations. The only reason that we used EC is that is what is measured. In terms of the consistency of numbers, we have used general notation where we have described data and scientific notation for the fluxes; however, we will be more consistent in the revised paper.

The paragraph at the end of section 3 is more detailed than it needs to be. It would be better to provide a brief summary of the flow and TDS variations instread of repeating the numbers that are clear from figures 2 to 6.

There is a general issue in how much data to present in the text. We tried to present sufficient that we were not relying on the reader to discern all values from the figures or tables. However, we agree that this disrupts the flow of the text.

As far as I can see, the surface runoff TDS is based on the rainfall and therefore it is assumed that no salinity is added during the surface runoff process - is this a valid assumption and can it be justified?

We do discuss this in the paper. In Section 4.3 we discuss using a higher value of surface water EC in the calculations (100 μS/cm). In terms of what the paper discusses, raising the assumed EC of the surface water lowers the estimates of baseflow from the chemical mass balance technique and thus our assumptions again produce conservative estimates of the mismatch between the chemical and physical techniques. This is noted at the end of section 4.3, but we can reiterate this important point in the Conclusions.

The authors look at the hysteresis effects and relate it to bank storage, but perhaps it is also necessary to consider the effects of surface runoff attenuation effects in the channels, particularly if the surface runoff is generated in the headwater areas. Will this effect influence the calculation of likely baseflow volumes. In addition, estimates are made of bank storage, but are these reasonable and can they be related to river lengths and likely bank storage availability?

It would be difficult to do much of this without specific data. The volumes that we estimated for 2002 are 12,200 ML ($1.22x10^8$ m^3). The bank length of the Upper Barwon and its major tributaries upstream of the Winchelsea Gauge is ~500 km ($5x10^5$ m). This results in an estimate of ~240 m^3 per m^3 of river. Notwithstanding that the value represents the aggregate of a number of events, it is high. However, this figure represents the total of all delayed water stores in the catchment not only bank storage and the area available for floodplain storage is considerable. This is an interesting point that we had not considered, and one that we can expand upon in the revised

paper. Surface runoff attenuation does not appear to be a problem, the delay in the floodwave arrival between the top gauging station and Winchelsea is 1-2 days and the increase in discharge between the two stations implies that much of the surface runoff is generated in the catchment as a whole.

The conclusions suggest that banl flow and floodplain storage will have a geochemistry that is similar to surface water, but is this likely if the soils are also saline? However, this information is not supplied and this issue also relates to the source of high salinity in the groundwater.

The bank storage will displace the saline water near the river bank (as was discussed in the McCallum et al., 2010 paper). Mixing does occur within the bank, which is why the chemical mass balance records some groundwater inputs during recession from the high flow events. Both the models of McCallum et al. (2010) and ours show a gradual return to pre-event conditions in the bank that reflects changes to the mix of groundwater and surface water.

Figs 2 to 4 show very large fluctuations in the GW contributions based on the CMB approach, but I did not find a great deal of discussion of this result and whether it can be physically justified in terms of time series variations in the hydraulic head of the groundwater feeding the river.

There are limited detailed time series of hydraulic heads in this catchment, but the water table does rise by up to 2m during high rainfall periods, which implies that there are changes to hydraulic gradients that are sufficient to drive changes to groundwater inputs. Again this is a valuable point that warrants discussion.

Overall, the conclusions and analysis of the results focus on the differences between the CMB approach and the different digital filter approaches. It would strengthen the paper if the focus shifted towards more verification of the CMB results. After all, most people would expect there to be differences between the digital filter approaches and more physical methods - we have know this for a long time - the important thing is whether relatively simple physical approaches can identify the sources of water accurately.

Again, while it is probably generally known, there has been little discussion on whether the mismatch is useful. The model presented here is generalised but the conclusions are consistent both with the overall results from the two approaches and with the hysteresis loops. We have focussed on simple models as these are widely used and the data required is available in many catchments.