## Response to comments by M. Hrachowitz

The manuscript "Contrasts between chemical and physical estimates of baseflow help discern multiple sources of water contributing to rivers" by Cartwright et al. highlights some very interesting aspects for identifying the sources of baseflow. The paper is well written and structured and it will make a very interesting contribution to literature. I have only a couple of general comments that I would like to encourage the authors to address in order to strengthen the relevance of their results.

**We thank Dr Hrachowitz for these helpful comments that will help improve the clarity of this paper. We also thank Dr Hrachowitz for highlighting some additional literature. Our specific responses are detailed below.**

(1) Although I guess it is justifiable, I think it is nevertheless a rather strong assumption that EC acts conservatively. I would thus firstly invite the authors to add one or more additional references, supporting this assumption.

**Clearly this is an important point and one that we can expand upon in the revised paper. In southeast Australia, Cl is the dominant anion and is the major contributor to EC in all but the lowest salinity waters. Cl itself is conservative (i.e. it is only derived from rainfall and concentrated by evapotranspiration) as evidenced by the Cl/Br ratios being close to those in local rainfall. The dominant process that controls the concentration of all major ions is evapotranspiration with limited mineral dissolution. There are a number of papers that discuss this aside from Cartwright et al. (2013) or other papers from our group (e.g., Bennetts et al., 2006. J. Hydrol. 323, 178-192; Raiber & Webb, 2006. Geochim. Cosmochim. Acta, 70, A515) that we can cite. We were attempting to describe the background hydrogeology in a concise manner but agree that it would be good to expand the discussion of this point. In response to the comments of other reviewers, it may have been best in hindsight if we had used Cl instead of EC and will consider doing that for the final paper.**

Secondly, and this is the only real concern I have about this paper, it would be good if the authors explored and discussed the effects of relaxing this assumption with respect to at least two processes: the temporal scale and magnitude of dissolution of ions stored in the soil and the temporal scale and magnitude of evapoconcentration. If I understand correctly, neither of them are considered at the moment. This closely links to the fact that the authors did not make any attempt in consequently quantifying the uncertainty in their results, which in simply end-member mixing analysis can be considerable (please include e.g. Rice and Hornberger, 1998; Hrachowitz et al., 2011). It would thus be fantastic if the authors could include a simple sensitivity analysis, by just varying the degrees to which these effects occur, i.e. for how long it takes the water in the system to take on the chemical composition of groundwater (this can be a simple linear relationship), so that they can report some sort of uncertainty ranges in their results, which will potentially effect the interpretation of the findings (e.g. page 5956, lines 13-20). The same is true for the digital filters: instead of using just one parameter value for each filter, use a set of different values and report the range of baseflow estimates in the results. This will significantly increase the relevance of the results.

(2) To bring the work a bit more in the context of previous work I would encourage the authors to discuss their results with a bit more depth, also including more references. For example, a) the notion that soil moisture/shallow groundwater is often geochemically distinct to water stored in

deeper groundwater (i.e. addressing the assumption of a conservative behaviour, but also the mixing mechanisms in the soil), e.g. Stewart et al. (2010); Rouxel et al. (2011); Hrachowitz et al. (2013), or b) how does the observed hysteresis and its interpretation relate to earlier work, e.g. Aubert et al. (2012); Murphy et al. (2012); Hrachowitz et al. (2013).

**We thank the reviewer for these suggestions. Currently, we are assessing this body of literature with a view to incorporating this in the final paper.**

3) A clearer definition between the terms “groundwater” and “base flow” should be given early in the paper and for clarity and consistency it would be good if the authors then sticked with this distinction for the rest of the paper. To me the two terms seem to be currently used in a confusing way – sometimes interchangeably, sometimes describing different mechanisms.

**It was our intention to use “baseflow” to represent all delayed sources of water. Groundwater is one component of baseflow, but not the only or even at times the major component. This is how we defined baseflow in the introduction. We agree that the terminology is not standard in the literature (with baseflow and groundwater inflows being occasionally used interchangeably). That was not our intention here and we will check through the paper to ensure that we are consistent in our definitions.**

(4) I would like to encourage the authors to provide units of flow in mm/yr throughout the manuscript for convenience for the reader (it is just easier to read).

**We presume that the comment was suggesting “$m^3$/year” which is a discharge. The units of ML/year are generally used in Victoria for river discharge; however, we can readily change this to $m^3$ if that is preferred by HESS.**

(5) The focus on bank storage might be a bit narrow, as water stored in other components of the system (e.g. unsaturated zone; cf. Hrachowitz et al., 2013) might be equally important. Therefore, perhaps use a more general expression.

**We agree that there are probably several components of delayed storage (as mentioned in the introduction) and will clarify this elsewhere in the paper.**

(6) In the data sources section (1.2) it is stated that river and groundwater EC were monitored, but no details are given for the groundwater monitoring set-up. Only later in the manuscript it becomes clear that the groundwater EC used here was not actually measured in boreholes. Please clarify this upfront.

**The EC of the groundwater as is described in Section 2 is measured in conventional boreholes, we use the river EC to assign the aggregate EC of the groundwater that flows into the river. As is discussed in the response to Reviewer #5, we will better justify the value of the EC of the groundwater component. In terms of the discussion of the groundwater EC values in section 2, we will emphasise that there are measured EC values of groundwater in the catchment to avoid confusion.**

(7) Please make it clearer earlier in section 2 that salinity in the soils/groundwater mainly originates from evapoconcentration.

**This will be clarified in Section 2.**

(8) on page 5951, from line 7, for clarity please make it explicit that you are referring to stream water EC.

**We will clarify this.**

(9) page 5953, line 2: should read ": : :Barwon River using the following..."

**Extra "the" in this sentence will be removed.**

(10) p.5957, line 22ff.: this description seems to fit better into the methods section.

**We agree that this would be a better place for the modelling introduction.**