Response to Comments of McMahon

General comments This is a timely paper that contributes to our understanding of the groundwater-surface runoff processes and to the interpretation of the separation of river flow into surface runoff and baseflow. Until recently, this separation has been carried out by graphical separation methods or digital filter techniques denoted as physical techniques in the Cartwright et al (2013) paper. As shown in the example provided in the paper, these techniques appear to significantly overestimate the contribution to river flow from high salinity groundwater relative to the salinity measured in the river. Given this increased understanding of physical and chemical modelling of low flows in streams, are we now at a time that, when time-series of chemical data (e.g. EC) are available at a stream gauging station, we should attempt to partition the physical baseflow estimates as calculated by one of the physical techniques into two components: 1) high EC water, often from regional groundwater and from other sources of high EC sources, and 2) low EC water from more local sources. The high EC component would exhibit long storage delay times mainly from regionally connected groundwater-stream systems, whereas the low EC component would be for storages with short storage delay times including riparian storage and local groundwater water which is made up of delayed overland flow, interflow, and bank flow. Thus, the difference between physical baseflow time-series estimates and the time-series based on chemical data could be considered to be baseflow derived from local groundwater. For water managers, whether involved in water resources or environmental flow management, such information would be of value.

We thank the reviewer for these positive comments. The explanation given above is probably more clear than ours in the original paper.

Specific comments P5955, Ls 12-13: Given there are relatively large areas of surface saline discharge (Figure 1), should not this estimate be increased to account for these?

This is a point that was bought up by other reviewers, and we reiterate the response to reviewer 5 here. In practice in this and other catchments, it is difficult to be certain of the net EC of groundwater that actually interacts with the river (as opposed to groundwater that is a few tens of metres away) without a density of monitoring bores that is unrealised in many, if any catchments. As with other studies (e.g. Yu et al., 1999; Gonzales et al., 2009) we have used the EC of the river at baseflow to estimate the EC of the groundwater component (Section 4.3). Since the data is for a single gauge, it does represent an estimate of the average EC of groundwater up catchment of that gauge. This is justified on the following grounds.

1. **It is not possible that the net EC is lower than this otherwise the calculated fluxes are negative at baseflow times.**
2. **It is possible to assign a higher EC to the groundwater component but this would have the effect that rover at low flows would always have a considerable component of surface water (which is unlikely during the prolonged very low flows).**
3. **In most years the highest EC in the river water during the low flows in the summer is similar. This is consistent with the inference that the river is groundwater fed at this time and that the river EC represents the groundwater EC than it is of the groundwater having a higher EC and the river being fed by a similar mixture of groundwater and surface water.**

4. **In terms of the calculations, assigning the groundwater EC to the maximum EC recorded in the river gives the maximum baseflow flux as calculated by Eq. (3). Nonetheless, this is still considerably lower than the baseflow fluxes calculated by the physical methods. Thus, we have been conservative in our approach and in our comparisons.**

Some of this is discussed in Section 4.3, but we can provide additional justification of this point. In particular is important to note that while there is some saline groundwater in the catchment, the average TDS is much less than 13,000 mg/L (EC <20,000 μS/cm); again this will be clarified.

P5955, Ls 13-17: I can accept that an increase in EC due to river evaporation may not be measurable, but not that "... evaporation in the river does not increase E values". Maybe the authors would like to comment here.

Yes, this was poorly worded. What we meant to say was that the stable isotope data (which we can reproduce in the final paper) indicates that there has been limited in-river evaporation.

Technical comments P5948, L6: replace "is" by 'are'

Correction noted

P5951, Ls8-19: I assume these data are for Winchelsea. It would be helpful for this to be indicated in this sentence.

Yes, that is correct and it can be clarified in the text.

P5955, L1: Some readers may not know what is meant by "conservatively" in this context.

We will probably convert the EC's to Cl in the final paper (as suggested by other reviewers). The comment regarding conservative behaviour (i.e. no input from halite dissolution, no biogeochemical reactions) still applies and we can define it in the final version.