Response to anonymous Reviewer #3

Reply explanation: The reviewers' comments are shown in black, while the author's replies and revises are shown in blue.

General comments:

Comments: The paper by Yang et al. presents a methodology to compute the uncertainty in the estimation of the long-term baseflow index (BFI) from streamflow and conductance timeseries in rivers. The paper develops equations on the sensitivity of the BFI that, to my knowledge, are new. However, I find that the overall significance of the paper is rather limited. In particular:

1) The authors mention in the title the "two-component hydrograph separation". This is a rather broad and active field of research but the authors narrow their focus on one single index (the BFI, which expresses the long-term ratio between baseflow and streamflow) and they compute it with a very specific methodology.

2) The methodology for the hydrograph separation (equation 1) is based on several assumptions (not mentioned in the manuscript) that are typically not met in the field. One of these is the fact that the parameters of equation (1) are supposed to be fixed during an event (or for an entire time series, as done by the authors). Finding a methodology to relax those assumptions is, in my view, more useful than evaluating the sensitivity of the present methodology to small measurement errors.

In other words, I feel that the authors improve the uncertainty evaluation of an index that, as currently defined, has major constraints and limited reliability.

Reply: We are very grateful to the anonymous reviewer for reviewing the paper and affirming the sensitivity equation. There are two main purposes in this paper. One is to analyze the sensitivity of the long-term series of baseflow separation results (BFI) to the parameters and variables in the conductivity two-component hydrograph separation equation (Sect. 2), and the other is to derive the uncertainty of BFI (Sect.3).(Page 3, lines 1--3).

Sensitivity analysis can quantitatively describe the impact of parameters and variables on the results of base flow separation (so that future users can clearly know which parameter has a greater impact, and should be more cautious when choosing the value). Uncertainty analysis can quantitatively describe the trusted range of BFI calculated by the conductivity two-component hydrograph separation method. The sensitivity analysis equation and uncertainty analysis equation of this paper can be easily applied to the two-component baseflow separation methods of other tracers because they usually have a unified equation form.

Reply to 1): BFI is a form of presentation of long-term series of baseflow separation results. And BFI is a hydrogeological parameter useful in modeling un-gaged basins (Lott and Stewart, 2016) and is believed to represent the effect of geology on basin low flows (Gustard et al., 1992). The total amount of baseflow in a long-term series can be easily obtained by multiplying the BFI by the total streamflow, where the long-term series can be months or years. Researchers in water resources management and assessment usually want to analyze the transformation between groundwater and streamflow by determining the baseflow under a long-term series, so this paper is necessary for them.

Stewart et al. (2007) applied the conductivity two-component hydrograph separation method (also known as CMB) to 10 real-time USGS gauging stations in Florida, Georgia, Texas, and Kentucky. Cartwright et al. (2014) applied this method to the Barwon River catchment, southeast Australia. Miller et al. (2014) applied this method to estimate the baseflow in 14 snowmelt-dominated streams and rivers in the Upper Colorado River Basin. Mei and Anagnostou (2015) indicated that the tracer based hydrograph separation method yields the most realistic results among various methods, because the tracer based method with the highest physical basis. Lott and Stewart (2016) applied this method to 35 basins in USA to calibrate five other baseflow separation methods. The conductivity two-component hydrograph separation method has its own limitations, but there seems to be no better method at present (Miller et al., 2014; Lott and Stewart, 2016).

Reply and Revise to 2): The conductivity two-component hydrograph separation method is indeed based on some assumptions. We have added these assumptions to the manuscript (Page 2, lines 3--7).

The field test (which was located within a $12km^2$ drainage basin in southeast Hillsborough County, Florida.) of Stewart et al. (2007) showed that the maximum conductivity of streamflow can be used to replace BF_C, and the minimum conductivity can be used to replace RO_C. The field test may be limited, but the conclusions have been applied to many basins in USA (as mentioned above). Miller et al. (2014) pointed out that the maximum conductivity of streamflow may exceed the real BF_C, so they suggested that the 99th percentile of the conductivity of each year should be used as the BF_C and assumed that the baseflow conductivity varies linearly between years.

Considering that the parameters of the conductivity two-component hydrograph separation method may be varied during the time series, we have changed the application of sensitivity analysis and uncertainty estimation based on the strategy of Miller et al. (2014) (Sect. 4.1 & 4.2).

Specific comments:

Comments: Variable names are rather confusing to a hydrologic community, as Q is conventionally used for streamflow. I invite the authors to adopt a notation based on the papers they refer to (e.g. Miller et al. 2014, Genereux 1998).

Reply and Revise: Well taken, we have changed the nomenclature and used SC as the variable name of specific conductance throughout the paper.

Comments: Besides English grammar errors, the language needs to be improved as the text is often difficult to understand. I invite the authors to revise the use of the term "specific": it seems that they use "specific" to say computed/available. (e.g. specific discharge appears to be just an available timeseries of discharge). Similarly, the use of "specific values" at page 3 Line 17 and "specific" conductivity values (the correct form is specific conductance or electrical conductivity).

Reply and Revise: Well taken, we have asked an English native language agency to check and correct the grammar and structure of the manuscript. We have revised the mistake use of the term "specific" throughout the paper.

Comments: Section 2.2. What is, ultimately, the purpose of this section? Is it to show that the sensitivity of BFI on streamflow and conductance measurements is low (and so it can be removed from subsequent equations like eq 20)?. If so, please make it clearer. What sounds interesting to me is that BFI sensitivity only depends on the integral of the (little) errors on Q and y. But once this is clear from the formula (eq. 15 and 16), then there is no need to show Figures 1 and 2 as the result is implicit from the definition of random errors on Q and y. Instead of the current Figures 1 and 2, why not showing an example of the methodology applied to a case study time series? It would make it easier to understand the usefulness of the approach.

Reply and Revise: Well taken, the ultimately purpose of Section 2.2 is indeed to show that the sensitivity of BFI on streamflow and specific conductance measurements is low (which can be ignored in estimating the uncertainty of BFI). We have made it clearer (Page 5, lines 3--6).

This section really does not need so much description. We have reduced the description and have added Fig. 1 and Fig. 2 and related descriptions to the Supplement S1 (Page 5, lines 3--31).

Comments: Section 3.1: Please make explicit assumptions on the requirement to apply the error propagation formula (eq 17). For example, "tiny" errors means that errors on Q and y should be small random errors related to the analytic uncertainty of the instrument, i.e. no systematic error.

Reply and Revise: Well taken, we have added the assumptions of the error propagation formula (Gaussian error propagation) (Page 5, lines 34--37).

Comments: Page 1 Line 22: rather than “can effectively identify” use “aims to identify”

Reply and Revise: Well taken, we have replaced the words as suggested (Page 1, line 26).

Comments: Page 1 Line 25: “is considered the most effective separation method”. By which standards?

Reply and Revise: *Kendall* et al. (1998) and Miller et al. (2014) indicated that stable isotopes are generally considered to be the most accurate chemical tracers for hydrograph separation. Klaus et al. (2013) and Lott and Stewart *(2016)* indicated that stable isotope tracers are considered to be the best geochemical method for hydrograph separation. Mei and Anagnostou (2015) indicated that the tracer based hydrograph separation method yields the most realistic results among various methods, because the tracer based method with the highest physical basis.

This paper follows the statement in the above articles. Our statement may not be objective enough, so we have made some changes (Page 1, lines 29--32).

Comments: Page 2 Line 1: I guess this is limited to the particular conditions at which Stewart et al (2007) applied the method. But this is not enough to generalize.

Reply and Revise: The field test site of Stewart et al. (2007) was located within a $12km^2$ drainage basin in southeast Hillsborough County, Florida. The conclusions (the maximum conductivity of streamflow can be used to replace BF_C, and the minimum conductivity can be used to replace RO_C) of the test were applied to 10 real-time USGS gauging stations in Florida, Georgia, Texas, and Kentucky. Then, the conclusions were applied to 35 basins in USA to calibrate five other baseflow separation methods (Lott and Stewart, *2016*).

The field test may be limited, but the conclusions have been applied to many basins in USA. Considering these, we have added the particular conditions for the field test to the manuscript (Page 2, line 13).

Comments: Page 2 Line 30: here and after I guess it should be equation (A1) rather than Appendix A1

Reply and Revise: Well taken, we have replaced the presentation of the citations as suggested (Page 3, line 10; Page 3, lines 20 and 30; Page 4, line 10).

Comments: Page 4 Line 2: unclear what is meant by “random analysis errors”. Please define what you mean by “tiny errors in Qck and yk”.

Reply and Revise: Well taken, we have revised the description of this paragraph (Page 4, lines 27--35).

Comments: Page 4 Line 2-5: This statement is unjustified. Please either formulate it as a hypothesis (e.g., if the errors follow a normal distribution: : :) or remove it.

Reply and Revise: Well taken, we have removed it.

Comments: Page 4 Lines 6-7: "The uncertainty of [: : :] is: : :": please avoid these unjustified general statements. Instrument precision depends on the particular instrument at hand and streamflow precision depends on a very large number of factors. You can simply reformulate the sentence stating that you assumed errors of Lines 11-18 is particularly unclear

Reply and Revise: Well taken, we have changed the statements.

Comments: Page 4 Line 17: which "average error"

Reply and Revise: We have moved this paragraph to the Supplement S1. It should be "relative error (%) of $\sum_{k=1}^{n} SC_k$ and $\sum_{k=1}^{n} y_k$" not "average error", and we have revised it.

Comments: Page 5 Line 13-17: what is the rationale behind the choice of these particular types of uncertainty (W terms)?

Reply and Revise: The uncertainty terms in Gaussian error propagation should be of the same type. One has some choice in the type of uncertainty to propagate, but all the uncertainty values must be the same kind of quantity: either all average errors, all standard deviations, etc. (Genereux, 1998; Ernest, 2005).

"While any set of consistent uncertainty (W) values may be propagated using Gaussian error propagation, using standard deviations multiplied by *t* values from the Student's *t* distribution (each *t* for the same confidence level, such as 95%) has the advantage of providing a clear meaning (tied to a confidence interval) for the computed uncertainty would correspond to, for example, 95% confidence limits on BFI" (Genereux, 1998).

We have added this rationale to the manuscript (Page 6, lines 5--8).

Comments: Page 7 Line 4-5: "During the rainstorm [: : :] the streamflow is almost entirely from the rainfall runoff": this is a serious misinterpretation of hydrological processes. It is well known since at least 15 years that in most catchments the event-water is not a major component of streamflow (and very often it only accounts for a few percent of total flow). See e.g. the commentary by Kirchner (2003) on Hydrological Processes (https://doi.org/10.1002/hyp.5108).

Reply and Revise: We are very grateful to the reviewer for pointing out this problem and giving the reference. Previously, we were mainly concerned about the effect of water-rock interaction on the chemical composition of groundwater and baseflow, and missed the brilliant debate on the "old water paradox".

After reading Kirchner's (2003) article, we thought the "old water paradox" is that in the same basin, most isotope tracers show that the flood streamflow contains a large amount of pre-event water, while dissolved salt tracers show that the flood streamflow contains less pre-event water (Isotopic fluctuations are very small in the process of flood streamflow, while the fluctuation of dissolved salts is more obvious).

Van Verseveld (2009) tried to explain the "old water paradox" through a hillside sprinkling experiment, the results show that "mass transfer to the immobile domain, dispersive mixing and rapid transport via lateral subsurface flow explained rapid mobilization of old water and thus the first part of the double paradox in a plausible mechanistic way", "the supply limitation of DOC, in combination with the vertical and lateral flow paths, controlled the variable DOC chemistry in lateral subsurface flow". Kienzler (2010) also tried to explain the first part of the "old water paradox" through some hillside sprinkling experiments, the results show that "shallow soil may already contain significant amounts of pre-event water, which can be rapidly released from small, saturated patches of the soil matrix", "an intensive exchange between overland flow and shallow subsurface flow might be quite common, … overland flow and fast subsurface flow, may, at the same time, produce rapid discharge responses and deliver substantial amounts of pre-event water to the stream". We have not found a strong theory to solve this paradox, and the existing theories are mostly plausible.

Just our opinion, the isotope composition of rainfall runoff has changed significantly in the surface or shallow soil, while the change in dissolved salt is not obvious. Therefore, the isotope tracer may classify the soil flow and the return flow into the subsurface runoff, while the dissolved salt tracer may classify the soil flow and the return flow into the surface runoff. And we do not think that soil flow and return flow are strictly subsurface runoff (which should be the water flow through the aquifer with uniform hydraulic connection). The above is just our experience. We believe that it is difficult to say that event-water is not a major component of streamflow until this paradox is thoroughly explained or proven to be correct in one part and biased in the other.

Considering the existence of the "old water paradox", we have removed the description that may be wrong in the paper (Page 8, lines 14--16).

References:

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Cartwright, I., Gilfedder, B., and Hofmann, H.: Contrasts between estimates of baseflow help discern multiple sources of water contributing to rivers, Hydrol. Earth Syst. Sci., 18, 15-30, doi:10.5194/hess-18-15-2014, 2014.

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