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Interactive Comment

Interactive comment on "Does discharge time source correspond to its geographic source in hydrograph separations? Toward identification of dominant runoff processes in a 300 square kilometer watershed" *by* Y. Yokoo

Anonymous Referee #1

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The study compares a numerical separation of streamflow (called time source) with a separation employing EMMA (geographic source) in a 3000 km2 watershed, a part of the Abukuma river, Japan.

The paper is well written and very well structured, and I think that comparison between tracer based mathematically based hydrograph separation are urgently needed and thus of general interest to the reader of HESS. That said, I think that the manuscript suffers from several methodological short comings, and does not hold what was promised in the title and introduction. I will aim to outline my concerns in the following also accounting for some recent work that seems to go beyond the presented work here. Also I am aware of the challenges that come with the scale of the investigated watershed.

The first major concern is the application of the EMMA method.

i) At first we need to be very carefully, since Uhlenbrook and Hoeg (2003) outlined the strong limitations of hydrograph separations on mesoscale (their catchment was one order of magnitude smaller than the one in this manuscript). Uhlenbrook and Hoeg doubt and clearly reason their doubt that the separation can give qualitative results. So I am already concerned about the scale

ii) The use of turbidity as a hydrological tracer: The author reports that 5 parameters were measured hourly, and never outlines why he decided for EC and turbidity. I do not see how a separation based on turbidity would be possible, since its behavior is not close to be conservative. There can be sediment deposition, and erosion even within the streambed, decreasing or increasing the observed turbidity not allowing any inference about the mixing of different end-members. Which would then in turn invalide both, the EMMA and the comparison of its results to the filtering.

iii) Identification/Defining of End-members: I do admit the challenge of measuring and defining end-member event in small headwater catchments, and it is a crucial (and cruel) task on the scale of this work. On first sight the use of an area minimized triangle seems to be a very appealing quick-fix. Nevertheless as stated by the author on page 10939 L13, this method has never been tested, so we simply cannot assume it is appropriate. The author argues that this testing is not in the scope of the current work, but I strongly disagree in this point, if we do not know if this is an acceptable assumption, the application to the dataset cannot be analyzed as done here. Further, when applying EMMA on longer time series, accounting for dynamic end-member would be necessary.

Second point of major concern is the numcerical-filter separation:

i) It needs to be better outlined why exactly the filter-separation autoregressive method was chosen of all available methods. E.g. Gonzales et al. (2009) used 9 different methods and compared that to the results of EMMA based separation, although they only did a separation in two components. Also Rimmer and Hartman (2014) used different separation methods and even used the hydrochemistry to constrain the method and the chosen parameters.

ii) The choice of the parameters and the effect of the choice need to be outlined better, it seems somewhat arbitrarily. The same holds true for the separation in 5 components. Why 5 when comparing to three components from EMMA? Can the method identify the maximum number of active components?

Third point of major concerns is the comparison of EMMA and Numerical-filtering:

i) How was the correlating on P.10941 L11ff done? What means “best correlated”? Was there a subjective criterion?

ii) It seems that a liner fit in figure 6a is not the best fit. It does even seem that the values of Qa decrease until Q5 reaches values of 1E-2. It also seems that the majority of points is located below the indicated regression line in figure 6a. How is the regression calculated? Please give the regression equation for figures 6-8 including the p-value.

iii) I am concerned about the smoothing of component Q1. The smooting window is over 2 month, and I highly doubt that the instream transport of water can be that long on the actual scale, if we regard the in stream transport usually be less than 2 weeks even in the world’s largest river basins. So I would argue that this explanation falls short, and there is either a serious problem with the determination of Q1 or the determination of Qc.

iiii) The comparison of EMMA versus numerical separation should also be done for both, exemplified events and the long term behavior. I.e. what are the differences in discharge volume for the individual events, and on the long term?

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These methodological issues make the final results relatively vague and speculative. I also do not see the general novelty of this work as it might be too much focussed on a single filtering method, although the general idea behind the work, combining numerical-filtering and tracer based hydrograph separation is indeed an important topic in watershed hydrolog. This is especially true if we consider the challenges of long term measurement programs of hydro-chemistry. The manuscript is, in my opinion, not acceptable in its current version, because of the fundamental limitations regarding the methods. This limitations leave doubt whether the results would have been similar when the methods would be more sound. I am not sure, if a revision is feasible for the current manuscript, although the topic is of interest.

Minor comments: Title: there seems to be a typo in the title, the catchment area should be 3000 sqaure kilometer not 300. I am also not sure if it would be better to say that it compares numerical separation with tracer based separation? Since the definition of time source for the numerical separation might be misleading? I.e. is an arbitrarily mathematical separation really time sources? There are also some typos of authors names: "McNamura" should be "McNamara", "Klause and McDonnell" should be "Klaus and McDonnell". Maybe use the reference manager to directly place the citation in the text to avoid typos, since the references are cited in the right way in the reference list. If not, please rigorously check the citations.

P10933L4: Please clarify hpw Barnes (1940) did this exactly.

P10933L16ff: See work by Rimmer and Hartmann (2014)

P10934L1-5: Define "similar". Also if they did not verify their estimates how were they able to determine similarity?

P10934L25: Does this mean the work of Haga and Yokoo (2011) did this separation only for events or they did not verify their results at all? Please clarifiy.

P10935L2-4: Please rewrite the sentence "Their discussion. . ." it reads confusing. At the end of the introduction I was still missing what was actually achieve by comparing mathematical and chemical hydrograph separation (I also missed some references to papers I am aware of e.g. Gonzales et al. (2009)) and was not completely sure what the research need is. This needs to be made stronger

P10935L15ff. I miss physiographic information of the catchment. Please add.

P10936L3ff. No need to report unused data, e.g. dissolved oxygen.

P10936L11: "Time source". I am not sure of that really is a time source, or simply a distinc flow path. Time source can easily be misinterpret as event and pre-event water (from isotope based separations)

P10937L23ff. This seems arbitrarily.

P10939L8ff. The author needs to outline the choice of the chemical parameters used. Following my major comments I do have serious doubts regarding the use of turbidity. Further comments see above.

P10940L23. The behavior of the groundwater contributions is indeed very uncommon. I think it is too simple to say that this is how EMMA behaves in watershed of this scale. I would argue that this is a relic of the choice of the endmembers and the use of turbidity.

P10941L10ff. See major comments. How was the correlation done etc., linear regression maybe not best fit.

P10942L9ff. Known latest since Uhlenbrook and Hoeg (2003).

P10943L2: I still think this agreement is vague, and uncertain to the method limitations. I would want to see, volumes and how are the different from each other (also in percentage), for baseflow and events.

P10943L14-19. Figure 9 seems to be unnecessary, in my opinion.

P10944L4-7: I still miss reasoning why the chosen way of describing endmember is a

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valid assumption.

P10945L1: This is speculative (again considering the amount of assumptions made in EMMA).

P10945L17: I am not sure if a 2 month time delay (1585h) is realistic?

P10946L4ff. A quick search shows that this was already done in earlier work (Gonzales et al., 2009, Rimmer and Hartmann, 2014). There might be more. So the author needs to be more convincing.

P10946L19: Installing and automatic sampler can be possible, no need to start high frequency observations.

P10946L20: Typo “luquid”

P10947L13-17: Same issue, if a delay model would be needed this makes no sense from two points: At first: 66 days is too long. Second that would change the mixing at the catchment outlet, thus the EMMA would need to create different results. Figures: 4-8 are relatively small in the printout, figure 9 is not necessary as it does not present novelty

References Optimal hydrograph separation filter to evaluate transport routines of hydrological models A Rimmer, A Hartmann Journal of Hydrology 514, 249-257

Comparison of different base flow separation methods in a lowland catchment AL Gonzales, J Nonner, J Heijkers, S Uhlenbrook - Hydrol. Earth Syst. Sci., 13, 2055-2068, 2009

Quantifying uncertainties in tracerâĂŘbased hydrograph separations: a case study for twoâĂŘ, threeâĂŘand fiveâĂŘcomponent hydrograph separations in a mountainous catchment S Uhlenbrook, S Hoeg Hydrological Processes 17 (2), 431-453

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10931, 2014.