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Interactive comment on "Age-ranked hydrological budgets and a travel time description of catchment hydrology" *by* R. Rigon et al.

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Comments of Reviewer #1

We thank the reviewer for her/his observations, which helped us improve our manuscript. This response tries to use the practice of the interactive discussion, and does not yet produce a new manuscript, which will be submitted after the closure of the review phase, upon request of the Editor. We introduce here, however, the main adjustments that we will make in the final version of the revised paper based upon the reviewer's suggestions.

R1 - The value of a travel time based description of catchment hydrology

has been increasingly acknowledged over the recent years. Therefore, this manuscript comes timely and could eventually be an interesting contribution to literature. Presenting a considerable string of formalisms, describing various aspects of travel times, and which are, as far as I can see, mathematically sound, the authors delve deep into the topic.

A 1 - We thank the reviewer for this recognition.

R2 - However,even after reading the manuscript three times I struggle to see what the actual intended contribution is. What do the authors want to convey to the reader? This needs to be made much clearer. Is it a review of existing concepts? Is it an extension of existing concepts? If it is a review, the description of the concept needs to go further back toinclude earlier work and detailed descriptions thereof. If it is rather an extension of existing concepts, it needs to be clarified what the novelty is and how it fits into our current understanding. In other words, what are the main findings? What do we learn?

A2 - If the reviewer did not understand the main contribution of our work, it is certainly our fault, and we will be more clear in the revised version of the paper.

Our work is absolutely a short review of existing concepts that were collected from many (theoretical) papers where they were scattered and used not systematically. Besides presenting the concept in a new and organised way, our paper contains some, we believe non-trivial, clarifications and extensions.

The paper, as a proof of concept, includes one example derived from a real case (Posina river basin). Besides, our paper comes with open source code that implements the theory and is available to any researcher.

Clarifications include:

- The concepts of forward and backward probabilities (as conditional probabilities) and a small change in notation that should not be overlooked;
- their relation with the water budget (and the age-ranked functions) from which they were derived;
- the fact that time-invariant forward probabilities usually imply time-varying backward probabilities, i.e. travel time distributions.
- The rewriting of the BBR's (Botter, Bertuzzo and Rinaldo) master equation as an ordinary differential equation (instead of a partial differential equation).
- The role and nature of the partitioning coefficient between discharge and evapotranspiration (which is unknown at any time except asymptotically).
- The significance of the SSF (formerly called SAS) functions with examples.
- The relationship of the present theory with the well known theory of the instantaneous unit hydrograph.

Extensions include:

- New relations among the probabilities (including the relation between expectancy of life) and travel time probabilities.
- An analysis of the partitioning coefficients (which are shown to vary seasonally)
- An explicit formulation of the equations for solutes which would permit a direct determination of the SSF on the basis of experimental data.

- Test of the effect of various hypotheses (e.g. linear model of backward probability, and in the revised version, gamma model for the backward probabilities).
- In the revised version, we also add information and clarify some links of the present theory with Dehlez (1999) and Duffy (2010).
- Besides, answering to a question posed by the third reviewer has brought to an extension of Niemi's relation (and a new normalisation), which will be included in the revised version of the paper
- The presentation of Niemi's relation as a case of the Bayes Theorem.
- A system of equations from which to obtain the SSFs.

R3 - In either case, the presented work needs to be put into a broader context. The authors refer to a few key publications, but they ignore many other recent contributions that address the issue from similar and/or different perspectives. Such a broader context will help the reader to better appreciate the relevance of the presented work. I would thus invite the authors to discuss their methods and findings with respect to methods, results and findings from a wider range of other (also more experimental) studies including, for example Birkel et al. (2010, 2011, 2014), Fenicia et al. (2010), Van der Velde et al. (2010, 2015), McMillan et al. (2012), Hrachowitz et al. (2015), Rinaldo et al. (2015) but also with the work of Cvetkovic, Fiori, Dagan, etc over the past years.

A3 - We gladly accept the suggestions of the reviewer, and will provide a more extensive treatment of the subject in the revised paper, which will include the papers she/he cites and a few others. As a general comment, we remind the reviewer that the large part of the literature on the subject we read is based on the (very limiting) hypothesis of stationary velocity fields, and therefore travel time distributions (TTD) are time-invariant, while our theory is centered on a time-variant approach, which we claim is unavoidable. This can be derived from the water budget coupled with BBR's master equation.

To our knowledge, there are no clear contributions before 2012 in our discipline's literature, and the findings we collect are scattered among various papers. Delhez's group at Lovain (e.g., Delhez, 1999, and Delhez et al., 2001) actually proposed a formalism that is similar to ours but it used concentration instead of probabilities, added creation destruction terms, and uses a completely different parameterisation of the fluxes, in such a way that is hard to recognise that its equation (4) is, in concept, our equation (9). Delhez's theory was more recently used in our contexts by Duffy (2010) who made clear that actually the equation can form dynamical systems whose solution estimates conjointly concentrations and ages. His formalism is foreseen to be compatible with ours, and we will add some phrase about it in the revised manuscript. Our equation is also equivalent to the one presented in Ginn (1999), e.g. equations (10) and (12), but again in a form which is far from our notation and concepts, and not easily understood. Carrera (1998) remarkable work, can be commented the same way. Going back into the literature, Campana (1987) also wrote an equation for water age distribution. He used a discrete time formalism, that is also not easily translatable into our derivation. We will add part of this information in the revised version of the paper.

Other comments:

(1) P.1, l.17-18: there is, in my understanding, little that remains unclear. Perhaps provide some examples. (2) P.2, l.26: again, I sort of disagree, there is little that remains unexplained. Please also give an example here to clarify.

Recent contributions on time-variant distributions in groundwater hydrology include Alì et al., 2014, Cvetkovic et al., 2014, and Soltani and Cvetkovic, 2013. Notably Soltani and Cvetkovic (2013) made a small mistake, in which their Figure 3 has the actual time in abscissa, while it cannot be. In fact, as it results clearly from our formalism, backward probabilities are functions of the actual time, and forward probabilities are functions of the injection time. Both can be seen, instead, as functions of the "travel time". It could be just a negligible oversight, but it is possibly a sign that the formal part of the theory and the related concepts were not well assessed before our paper. In fact, in other papers that inspired us, there are other many such small imprecisions that obfuscate the understanding of the reader, such as: confusing joint probability with conditional probability; taking for granted the knowledge of the partition coefficient when multiple outlet are present; and integrals that should be limited to the actual time t, go to infinity instead. Figure 4 of Hrachowitz, 2013, where one of the formulas is incorrect, is another example.

We will try to convey all this information better in the revised version of the manuscript. In general, we remark that all was already done is actually not true. Moreover, we think that discovering the concepts we clarify in the original paper could have been daunting even for a trained reader as the reviewer certainly is.

(3) P.2, l.33-41: please be more specific here: what is the research hypothesis to be tested?

In lieu of a general hypothesis statement, we pose the following questions: does the theory of Travel Times, as developed in recent years constitute a consistent unique framework ? Does it have hidden parts that there are not consistent or unexplained ? How it relates to the instantaneous unit hydrograph theories ? How can it be used ? What generates time varying backward probabilities ?

The answers we present are:

- The theory is consistent and gives a complete and unique view of the travel time theory.
- There are parts that are not fully explained (the role of the partitioning coefficient, for instance).
- The theory can be used once the water budget is solved. However, in this case, probabilities remain unknown until time goes toward infinity.
- On the contrary, as traditionally pursued in the instantaneous unit hydrograph theory, when forward probability is given, all is known; but with probability choice we make a precise statement about the future.
- Time varying backward probability are easily generated by time varying rainfall (and are therefore unavoidable), even in the classical case of systems described by time-invariant forward probabilities.

(4) P.2,l.47 and 48: this should read as ". . .the time at which. . ." to avoid confusion, as we are (as a simplifying assumption) not talking about a time interval over which the input occurs but an instantaneous input.

We corrected the paper accordingly to the suggestion of the reviewer.

(5) P.3,l.57-58: please add the respective dimensions

Done

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Discussion paper

(6).1 P.3,l.67: this can only be solved analytically if piecewise linear functions of inputs are available and, more importantly, with the assumption of only one storage component in the system, which may be quite an oversimplification for most catchments.

We agree with the reviewer.

The water balance as given here cannot resolve the non-linearites in the system, including interception, vadose zone dynamics, storm flow connectivity, etc.. Thus the practical utility of such an analytical solution, if not used in an operator splitting strategy that accounts for different system components, remains limited. Please qualify the statement accordingly.

The generalisation to multiple cascading storages is quite obvious, but more complicated to explain. This is the reason we limited ourselves to a simple system. For more complicated and interconnected systems the water budget must be solved numerically. It is actually written at line 68, but we will made it more clear and general. When more than one reservoir is present, we have a set of budget equations to be solved simultaneously. We will write more clearly these concepts in the revised manuscript.

(7) P.4,l.86: Really? I would be surprised by that as this is implicit (and has to be) in essentially all approaches that somehow track fluxes through the system. I could imagine that this has already been explicitly formulated earlier. Please check, in particular papers by Cvetkovic, Dagan, Fiori, Russo, etc.!

Yes. We already partially answered to this question in the above points. We went back to the literature in various fields. Even if we share the reviewer incredulity about the result, the answer is, to our knowledge, that there is no trace of equation (9) in papers of the authors he/she mentions (or others, to our knowledge). Certainly many authors treated advection-dispersion equations: but only very recently they moved to treat time-dependent travel time probabilities. The original formalism (Dagan, 1989) is really general, so it contains what equation (9) conveys. But it does it as Navier-Stokes equation is contained in Newton's second law of dynamics: a lot of assumptions and mathematical treatment have to be done to pass from the latter to the former. Obviously, there is always the possibility that we overlooked some contribution.

(8) P.5, section 4: that is all fine and true, but nothing new. It remains unclear what the purpose of this section is. Please clarify!

Without the definitions contained in this section the paper loses its validity. To remark what the backward probability is and how it relates to the quantity of the water budget is deemed essential to understand the whole structure of the theory, even if the result that differentiates our contribution from the others is that the BBR's master equation is an ordinary differential equation in our derivation, instead of a partial differential equation. This was obtained by considering the explicit form of the travel time variable and exposing the dependence of the equation on the injection time. One original result is also represented in equation (21), which is the fact that the backward probability, thought as a function of t (domain in which it is not a probability) is null when it is not raining. In itself this is a little theorem, that we could have added as an extension in the list of answer A2.

(9) P.6,l.136: I may have missed something here, but how can P(Tr/t) not integrate to one (and it seems it actually does in figure 2)?? As far as I understand, it is the sum of storages of all given ages present in the system over the total storage at any time t. Also: how does ageing contribute here? Please clarify.

The integral of $p(t-\tau|t)$ is equal to 1 only if integrated in τ. When it is integrated in t, its integral is less than 1. The notation used in many papers (without the symbol | to indicate "conditional to") does not help to understand this. However, Figure 2 may not be clear, since in the abscissa is named "Time" and it should be "Injection time" instead. We will change it in the revised version of the manuscript.

(10) Figures 2 and 4: I am a bit confused by this figure. How can three injections at three different injection times (tau1, tau2, tau3) plot on top of each other when the x-axis is the actual time t measured by a clock? Should here the x-axis not rather be the time since injection?

This is true, their origin was shifted to coincide for comparison. Otherwise they would have a different origin. We will modify the caption of the Figure to make it clear.

(11) P.8,l.161ff: that is correct, but has been shown and discussed earlier (e.g. Benettin et al., 2015, Fig.6; Hrachowitz et al., 2013, Fig.9). Please put into context.

The concepts expressed at line 161 ff and in the two examples cited by the reviewer are completely unrelated. Fig 6 in Benettin 2015 shows that their estimates of the hydrological and transport parameters show a consistency across different simulations. In particular they could compare the parameters' posterior distributions resulting from the calibration of individual years, as an independent verification of the reliability of the calibration algorithm. Fig 9 in Hrachowitz, 2013 instead shows the results of the transit time distributions for the outflows considered in 4 different hydrologic regimes and for the 3 case studies. What we are saying instead is simply that at an finite time we do not know the shape of the forward distribution. What we know is only the actual state of the system, obtained solving the budget up to the actual time.

(12) P.10,l.173-176: sure, nothing wrong with that. It remains, however, unclear, what the relevance of this is. We may be able to extrapolate the splitting coefficient for the future, but what exactly does the knowledge of this help us when future climatic forcing is unknown? Please clarify.

The importance of Θ is well described in the Appendix A that we decided to move back into the main text in the revised version of the paper. The knowledge of the partitioning coefficient is important to characterise the basin response.

Besides it is important in the Niemi's relation between the backward and forward pdfs. As shown in Appendix A, Θ tends to a final value after a time, characteristic of the basin. Besides Θ is shown to vary seasonally, which is kind of obvious. This fact adds time variability to catchments responses which is usually neglected or accounted for with less clean methods.

(13) P.11,l.198: should read as ". . .what was written. . ."

Done.

(14) P.11,eq.45: please clarify what the difference is to the relations discussed by Botter (2011) and Benettin et al. (2015)

No difference. We added a citation to them.

(15) P.14,275ff, eqs.57-61: again, this is what is essentially done in most recent tracer based approaches. Please put into context.

This is true only for equations (57) and (58) (in fact we cited Rinaldo 2011). The next equations are conceptually simple but they were derived from our new formalism. The derivation is so straightforward that we would be surprised if it is new. However, as matter of fact, they were not used in recent papers in the literature. They can be used to determine or at least to constraint the form the SSF functions. Parallel to Duffy (2010), we can show that we could avoid to use the SSF by simply inferring $p_Q(T_r|t)$ directly, by evaluating simultaneously the water and the tracer budgets. In any case, we believe that using the SSF adds knowledge of the hydrologic mechanisms. Some phrase about the direct determination of the $p_Q(T_r|t)$ will be added in the final version of the manuscript.

(16) P.14,l.294-296: there are definitely obstacles to adequately determine SSF in reality. Not only due to uncertainties in precipitation and tracer input, but also due to oversimplified models and the related uncertainties (e.g. in Q) that typically lead to considerable equifinality (i.e. the well-known closure problem; Beven, 2006).

Any tracer measurement (we would say any hydrologic measurement) is difficult to obtain, but this does not prevents experimental hydrologists from taking measurements and all of us try to infer knowledge from them.

Equation (61) is the tracer budget, and evaluating tracer inputs and outputs is what people in the field do daily. We cannot so easily say that their work is vain or useless. The SSF function (or the backward probability of discharges, as we remark in answer to comment 15) is just obtained by algebraic manipulation. The case we exploit (with just one storage) is purely illustrative, but generalisation to more complex systems, as those presented in Hrachowitz et al., 2013, is straightforward. In the revised manuscript we will add an explaination of how to do it.

(17) P.14,l.304ff: sure, but not new. See for example the work of Bertuzzo et al.

(2013) or Benettin et al. (2015).

We disagree. What we are saying here is that coupling the two equations (water and solute transport) we are able to determine the SSF exactly. In the cited studies, SSF values were imposed a-priori and the results checked a-posteriori. In particular Benettin et al. (2015) states: "*Despite the intuitive essence of the formulation and the advances achieved by using a transformed travel time domain [van der Velde et al., 2012; Harman, 2015], the use of SAS functions is still a challenge, because few real-world applications have been proposed in the literature and numerical solutions can be computationally demanding. A compelling alternative is to model the catchment through a series of physically meaningful storage partitions (typically, one for the shallow soil and one for deeper groundwaters) and assume a random sampling (RS) mixing scheme within each storage. This enables the use of analytical solutions that are particularly easy to implement. Moreover, the RS assumption was shown to give reasonable results in systems with high degrees of heterogeneity [Benettin et al., 2013a; Ali et al., 2014] and has been successfully applied to different settings [Bertuzzo et al., 2013; Benettin et al., 2013b] including comparisons to spatially distributed 3-D numerical models [Rinaldo et al., 2011]. Under the RS approximation, the SAS function is equal to unity, hence the travel and residence time distributions coincide [Botter, 2012; Hrachowitz et al., 2013]*" etc. Clearly Benettin's very recent paper is looking for a feasible method for obtaining SSF. We gave one more.

(18) P.15,l.317: maybe mention that a linear reservoir here entails complete mixing/uniform SSF

The fact that a linear reservoir implies complete mixing is written after line 338. In the same section it is also shown that if the set of linear reservoirs have a mean travel time λ_τ which is dependent on the injection time, there is not complete mixing.

(19) P.15,l.319: please clarify in detail what R_τ is.

It is already clarified below equation (63). Moreover it is also explained in the List of symbols.

(20) P.15,eq.63: as it is presented right now, this ignores the critical difference between celerity and velocity, or in other words that the response time distribution of a pressure wave routed through the system is significantly different to the travel time distribution of an actual input signal (McDonnell and Beven, 2014). This needs to be made clear!

The example used in this section is just a classical, commonly known, example. The first author recently argued about the concepts of wave celerity and water speed (e.g. Rigon R., Celerity versus velocity and the travel time problem,http://abouthydrology.blogspot.it/2016/06/celerity-vs-velocity.html, last retrieved 2016-07-04) and concluded that all the effects of pressure waves (which travel with a celerity different from the velocity of water) are the (only) cause of time-varying travel time backward distributions. However, in the example presented here, what is time-invariant is not the backward probability, but the forward one. The rationale of using it in the paper is to show how its choice determines the SSF (to be 1) and the mean travel time. Subsequently the hypothesis of time invariant linear storages is relaxed, by using time variant linear storages to better reveal the nature of the SSF functions. It is also shown that, a time invariant forward probability does not imply a time invariant backward probability, which is obtained below at equation (69). This fact is quite unexpected and, accordingly to the heuristic in Rigon (2016) cited above, implies the existence of a travelling signal that alters the stationarity of the velocity field. This will be made clearer in the final version of the manuscript.

(21) P.15, eq.68: this does essentially boil down to the convolution integral used in many earlier studies starting from the 1960s or so. Please put into context and highlight the relevance here.

That’s true, equation (68) is the convolution of effective rainfall impulses with the travel time distribution. What is not trivial here is to note that the backward probability (the one that affects tracers) is actually dependent on the amount and timing of rainfall input. Therefore, even in this case, where we have a time invariant forward probability, we obtain a time-variant backward probability. This time variation is indeed trivial because it has an obvious dependence on the rainfall inputs. However, both the numerator and denominator of the equation depend on the rainfall inputs, and so the time-variation scheme of the backward probability can be understood but not simplified (for instance by factorizing the rainfall input). We will put all of this in context and we will highlight it better in the revised version of the manuscript.

(22) P.16,l.335: of course, as already argued by others previously (e.g. Rinaldo et al., 2011)

After more thought, yes, it is a result that is implied in Rinaldo et al., 2011. We will add a citation here.

(23) P.18,l.423: should read as “. . .damped. . .”

Corrected accordingly

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