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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "What can flux tracking teach us about water age distributions and their temporal dynamics?" *by* M. Hrachowitz et al.

**M. Hrachowitz et al.**

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General Comments

Comment: This paper nicely and innovatively combines runoff and chloride transport modeling in three Scottish catchments to infer information on travel time dynamics of water inside these catchments. The paper describes the entire process of fitting hydrological and tracer models to 3 catchments. A major innovation is the evaluation of multiple mixing concepts and their effects on travel times of water. Additionally, the introduction gives a very complete overview of the state of the art in conceptual hydrologic and transit time modeling, which is a big achievement given the large number of papers that haven been written on catchment travel times during the past years.

Reply: We would like to thank to reviewer for his/her positive evaluation of our manuscript.

Comment: The largest problem of this paper is its length. The paper could probably have been subdivided into 2 or 3 papers. Possible ways to shorten the paper would be to focus on the travel time distributions and skip the 4 wetting regimes, the long power tails and the travel time distributions of chloride. This would shorten section 4 considerably . However, it is up to the authors and I will not further object to the length of the current manuscript. Below I listed several major and minor comments, but nothing really major, and I recommend this paper for publication after minor revisions.

Reply: We agree with the reviewer that this is a long manuscript and we initially shared the same concerns. However, after lengthy discussions with many colleagues from the catchment hydrology community and careful deliberation we came to the conclusion that the presented information can only be fully appreciated if it is shown in the full context. This is especially important as much of the literature on water age is quite scattered, which also contributed to the fact that some important aspects of the topic (as highlighted in the manuscript) were simply forgotten or, when seen out of context, not considered relevant by wide parts of the community over the past 3 decades or so. As the presented information further does not offer a clear splitting point for providing 2 papers (i.e. which part would go into which paper?) as the information is very interwoven, we would thus strongly prefer to present the information in one single manuscript instead of splitting it up into 2 papers.

Specific Comments

Comment: Eq. 34: Here you add discharge and evapotranspiration distributions which give the overall distribution. I don't think this is correct: adding 2 distributions never yields another distribution. As I understand it, in Eq 34 pTQ(ti,tj-ti) and pTE(ti,tjti) are partial distributions as they do not sum up to 1. In my opinion you have to weigh the distributions with the fluxes similarly to eq. 35. This weighing fac-



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tor determines how much of the rainfall goes to discharge or to evapotranspiration: Ptot(ti,tj-ti)=RQ(ti)/Rtot(ti)*pTQ(ti,tj-ti)+RE(ti)/Rtot(ti)*PTE(ti,tj-ti) In which Rtot(ti) in the total amount of precip at ti, RQ(ti)/Rtot(ti) is the fraction of Rtot that ends up as discharge Q, RE(ti)/Rtot(ti) the fraction of of Rtot that ends up as evapotranspiration, E. With this definition pTQ(ti,tj-ti) and PTE(ti,tj-ti) are real distributions and Eq. 34 and 35 follow similar reasoning/patterns. This problem is further reflected in Fig.11 where it is clear that the different traveltime "distributions" do not sum up to 1. If they do not sum up to 1, they are not distributions!!. To me this is very confusing. The same happens in Figures 6,8 and 12. Here I do not understand why many of the distributions do not sum up to 1. They should if you refer to them as age distributions, as you say in the figure subscript. Somewhere you state that you only look at 5 years, but the cumulative distributions do not indicate 100% will be reached in the future. Please correct this.

Reply: We thank the reviewer for this very important comment. Equation 34 was indeed wrong and will be adjusted accordingly. Furthermore, it is also true that not the actual, formal transit time distributions were shown in Figure 11 (they did not add up to unity). However, these actual transit time distributions conditional on runoff and evaporation cannot be consistently shown here. The reason is that due to computational limitations water could only be tracked for 5 years. Thus, most of the incoming precipitation signals are not completely recovered after 5 years. The proportions of the input signals leaving over the exit routes runoff and evaporation could for this reason not be scaled (i.e. normalized). Thus, what we are showing instead of the actual transit time distributions conditional on runoff and evaporation are proxies which formally are "conditional finite measures" (Bogachev, 2007), or more casually spoken the two parts of the joint distribution of pT,tot resulting from Qtot and Etot. If the entire precipitation signal was recovered these two parts of the joint distribution would add up to 1. However, as for most instances here the entire input signal was not recovered after 5 years, the proxies used here ("conditional finite measures") do not add up to unity. This will be better explained in the revised manuscript. The reason the distributions (sic!) in Figures 6 and 8 do not add up to one is different. In these figures the actual distributions are shown, i.e. for pF, the amount of water with a certain age between 1 and 1827 days (5 years) the comes to runoff on a certain day, scaled by the total runoff on that day. As in some cases a certain amount of water in the runoff is older than the tracking limit of 5 years, this does not show up in the distributions provided. The same is true for pR.

Comment: In section 3 I cannot find how chloride transport with transpiration is conceptualized. From section 4.6 I think to understand that chloride is not taken up by plants while water is(transpiration). Im curious how you can justify this assumption. You have relatively low chloride concentrations in the groundwater and plants need chloride. I do not think that plants will put in the large effort to completely separate chloride from the water. Instead it is much more likely that plants take up this chloride (look in literature for chloride concentration in vegetation). In your system chloride is then cycled via mineralization of organic matter. This of course yields much longer transit times for chloride than you have now calculated. The delay caused by the plant uptake and mineralization is substantial (likely to years) and could also explain part of the large delays now entirely attributed to mixing with deep storage (Ss/Sp, overestimation of your storage).

Reply: This is an interesting comment. The actual chloride uptake is implicitly accounted for in the lumped adjustment factors (although net chloride balance is negative, i.e. more going out than in). But the reviewer is correct in assuming that chloride turnover by plants was not considered in the present study. Although it is well known that plants turn over chloride, the understanding of the mechanisms is incomplete, at best (e.g. Kauffman et al., 2003; Lovett et al., 2005; Zalesny and Zalesny, 2009): what are the actual mechanisms? What are the general magnitudes of chloride turnover? What differences of chloride turnover rates can be expected for different plants? How is chloride actually routed in the xylem from roots over the trunk to the leaves (is there any “mixing” occurring? Is it rather piston-like routing?)? What are the time scales of the movement through the plant? Without any secured information on that, introducing chloride turnover by plants into the model would have merely resulted in some kind of



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arbitrary mechanism, parameterized with one or more parameters. This in turn would, in the absence of detailed process knowledge, essentially only increase the degrees of freedom of the model and eventually lead to increased equifinality and eventually to overparameterization. This, together with the low actual transpiration rates in the study catchments (∼15-34% of annual precipitation, resulting in low sap flow and reduced tension driven chloride uptake), the fact that in the cool Scottish climate organic matter turnover (i.e. litter fall) in both heather moorland vegetation and coniferous forest is relatively limited (cf. Liu et al., 2004) and the fact that in the large majority of previous tracer studies using chloride it was not accounted for (e.g. Kirchner et al., 2000; Shaw et al., 2007; Dunn et al., 2008; Page et al., 2008; etc.) resulted in our decision to omit plant chloride turnover. However, possible effects and sensitivities of model results to different model architectures (also including different possible representations of plant chloride uptake/turnover) will in detail be analysed in a manuscript that is currently under preparation. In the present manuscript we will thus add clarifications that chloride uptake by plants is implicit in the lumped adjustment factors and that chloride turnover by plants could potentially result in somewhat changed results, which are difficult to quantify.

Comment: Section 4.3 I can't work out what we are exactly looking at in figs 6,7"8,11. You model an age distribution for every time step (Fig 9). Therefore I guess that in fig 6 Im looking at some sort of average distribution. I wonder if it is a time averaged distribution or a flux averaged distribution. This makes a huge difference. More info is needed. And clarify why you look at a time averaged distribution in stead of a flux averaged one.

Reply: The reviewer is right that we did not properly explain what is shown in Figure 6. In fact it is the temporally averaged, unweighted distribution constructed from the respective median values for every transit time during the four individual wetness conditions (dry, wetting-up, wet, drying-up). The averaged distributions were used here as flux averaging is implicitly at least in part accounted for by splitting the averaged dis-



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tributions into four different wetness conditions and as the main objective really was to assess the temporal changes in the system. This will be better explained in the revised manuscript.

Comment: Page 11368,line 25 a summary of

Reply: Ok

Comment: Page 11371, line7, manure and fertilizer input are major sources of cl, just as road de-icing.

Reply: In principle the reviewer is right, however, none of these applies in significant proportions to any of the study catchments which are rather pristine, thus showing no road network, no livestock farming and not being fertilized (forest and heather moorland)

Comment: Page 11371 line27: This more of a general comment on the flexible model structure approach: You say you use the information available, but there is so much more information that you do not use in this model approach. For example topography, soil maps, geological maps, vegetation cover etc: : : I'm sure that these sources of information could constrain your models. For example I'm curious if the fitted storage volumes on discharge and chloride loads are possible if you consider the amount of pore space in your catchments. In the end the amount and dynamics of storage largely controls your travel times/chloride dynamics.

Reply: We agree with the reviewer that "soft information" can help tremendously in the choice of suitable models. In fact, this is what was done here. A priori developed model architectures were based on basic soil (HOST) and geological maps. The influence of vegetation cover was rather used as hypotheses to be tested and it turned out that the catchment with the largest extent of forest had to be represented with an additional interception reservoir in order to get an adequate stream flow model. This will be made clearer in the revised version of the manuscript. In the absence of suitable data (e.g.



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deep groundwater, tracer composition of deep groundwater) it is however problematic to estimate the "real" available storage volumes. The numbers obtained from calibration seemed rather reasonable for the region, although storage in many catchments could be much larger than previously thought (e.g. active groundwater was discovered at depths of >60m(!) in Plynlimon, which is also an upland catchment). But we agree with the reviewer that available storage controls travel times. We thus highlighted that further studies using additional data (e.g. tritium) need to be done to more rigorously test some of the hypotheses in this manuscript.

Comment: Page 11377, line 5, I -> italic(I)

Reply: Ok

Comment: Page 11378 (Eqs 25-26) -> Eqs 26,27 ?

Reply: Ok

Comment: Section 3.3: I think this section would improve if there was a conceptual figure that explains the different mixing schemes, and how they are parameterized. How mixing is parameterized is one of the innovations of this paper but it is very hard to understand from these sections. I think you built it up nicely: First complete mixing, then adding parameters Sp and CM for incomplete mixing, than sumax, _cm and _cm for partial mixing Section 3.3 How do the lag functions in the hydrological model influence the travel time distributions? If you introduce lag functions you modify the storage in the system. Is this accounted for?

Reply: We agree that a figure would help here and we are going to add this to Figure 3. The lag functions delay the travel times, however, in the present case these delays in travel times are insignificant as the maximum time lag used is 2.7 days (Ts, Table 3). Nevertheless the travel time distributions account for these lag functions as water/tracer is routed and tracked through the entire system.

Comment: Page 11381, line 20 rephrase "be on the one hand be"



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Reply: Ok

Comment: Page 11391, line 5 “a break in at”

Reply: Ok

Comment: Page 11392, line 11 Hence, although

Reply: We are not sure what the reviewer refers to here.

Comment: Page 11398: what new information can be obtained from pT that could not be derived from pF discussed earlier?

Reply: In fact much of the information could be derived from pF and pR. However, much of the literature in the field of travel times is rather scattered which strongly contributed to the fact that some important aspects of the topic were simply forgotten or, when seen out of context, not considered relevant. In the light of these facts we felt it useful that the “full story” is for once written down.

Comment: Page 11398: line 19: Here you mention 5 years. Are these “distributions” determined over only 5 years? The shape of the cumulative distributions does not indicate that 100% will ever be reached. Why not? I do not understand why only 65% of the water entering during dry periods has left this catchment after 5years.

Reply: Yes, due to computational limitations water/tracer could only be tracked for 5 years at most. Simply spoken, on average only 65% of the incoming water left the catchment after 5 years because the remainder is still locked up in the catchment. During dry periods water is predominantly stored in the unsaturated zone and excess water rather recharges groundwater than being released over fast flow path ways. As the groundwater stores are relatively large compared to the other stores in the system, water will remain there for quite a long time until it is turned over, an effect which is reinforced by the partial mixing assumption. 100% will eventually be reached but after very long time periods as discussed in the long tails section. This also supports findings by Godsey et al. (2009) and Basu et al. (2010), who observed long-term



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near-chemostatic conditions in a wide range of catchments.

Comment: Page 11400 I think plant uptake of chloride cannot be ignored. Ignoring plant uptake leads to an overestimation of your groundwater storage Ss/Sp, in order to buffer modelled seasonality in chloride concentrations.

Reply: See reply above. We will discuss possible effects of chloride uptake by plants in the revised version.

Comment: Figs 6,7,8, 11 why stop the cumulative distribution at 1000 days? Why not continue to 10000 days as several of the distributions did not yet reach their maximum: 6.11 and 6.12

Reply: We initially planned to do the analysis with tracking the water for at least 10000 days. Unfortunately this was made impossible by computational limitations.

Comment: Page 11435, Fig 12 What do you mean with a solid solute?

Reply: Should read as "dissolved solid"

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