Reply to reviewer # 3

My thanks for the comments – which appear below in italics and responses follow in plain text.

1. relevance. As noted by other reviewers, the practical and/or theoretical relevance of this paper is unclear to me. Getting lower bonds for Mean Transit Times does not seem to be an obviously relevant scientific problem; therefore, the author should better discuss the important implications of this work for hydrology and interpretation of environmental time-series. Interestingly, the paper by Jim Kirchner who apparently inspired this work, suggested that the mean transit time is a poor representation of this type of systems (as noted by the author in the introduction). In fact, tracer dynamics are mostly driven by the fraction of young water, and its time-variability.

Getting lower bounds to mean transit times can certainly be of practical relevance for catchment situations, given a high lower bound can be established. This indicates the existence of a large mean transit time (whatever its true value may be), which can have implications for how the catchment interacts with its environment:

> Additionally, catchments with long transit times are more likely to be resilient to short-term (years to decades) variations in rainfall but will respond to climate or land use changes that cause longer-term (decades to centuries) changes in groundwater recharge and flow.
> Cartwright and Morgenstern (2016).

On smaller time scales, establishing that a mean transit time exceeds even a few months may give indication that any contaminant released into the catchment may take some time to flush itself out.

However, it is not for me nor the reviewers to make a judgement call on the value of the concept of lower bounds to mean transit times. It should be left for users to try it out for themselves and see if they find if this approach has use for them.

It is of course desirable to estimate part or all of a transit time distribution but this technical note is just concerned with the possibility of quickly gaining at least some information of catchment storage behaviour via a nonparametric approach with minimal assumptions.

The fraction of young water does have some connection to the lower bound in that a small proportion of young water would be expected to result in a higher lower bound for the mean transit time.

2. tools. there are several papers that have shown unambiguously why the lumped convolution approach is misleading. It is quite surprising to see that the lumped convolution approach is still used (in this case in a discrete fashion). Therefore, in my view section 3 is completely useless - as that lumped formulation is not able to describe real-wold catchment dynamics. Note that Kirchner [2016a] used that approach only to explain the idea of aggregation bias in simple terms. Here the steady nature of TTDs (or MTTS) is a fundamental assumption (in fact, all the numerical examples shown in the paper refer to steady state TTDs).

It should not be so surprising for the reviewer to see that the lumped convolution approach is still used. See in this journal, for example, Mosquera et al. (2016), who no doubt will consider that their work is not useless. That paper also outlines some practical difficulties involved with using time-varying models.

However, the lower bound paper is not in any case concerned specifically with the steady state condition. It is therefore confusing to see the reviewer's reference to time-invariant transit time distributions supposedly being a fundamental assumption. Section 3 was titled as being for the case of the steady state situation, which by definition has the steady state assumption. The rest of the lower bound paper is concerned with conditions other than steady state, going on to the next level of complexity (time-varying transit time distributions with constant means), with some brief comment at the end of Section 4 about the more general situation of time-varying means.

In hindsight, it certainly would have been better to have considered only the most general case with time-varying means. A revised version of the paper would take that approach. Also, the example does need to be an illustration of the general time-varying case.

3. tools. I have several problems also with the time varying version of the approach (section 4). first of all, the author apparently uses "forward distributions" and not "backword distributions" in equation (5). If this is correct, then the convolution in eq. (5) should refer to mass fluxes and not to concentrations, as in section 3 (see also the periodic nature of the input in Figure 2). Overall, the insisted use of the terminology "tracer time-series" is confusing. The meaning (and the nature) of the kernel linking input-output signals in environmental systems changes, depending on the quantity involved (concentrations, mass fluxes, water fluxes), as extensively discussed in the literature of the 70s by Niemi, Zuber and many others.

The method is only applicable with backward distributions (predicting the output time series at the observation point at a given time). It may be the arrangement of the time ordering indexing gave the impression of forward distributions. All the time series are defined to be flux weighted (product of flux and concentration), and not concentrations (line 26, page 2). The driving force of the flux variation is irrelevant to the LP methodology, which is only concerned with tracer particle fluxes and not their causes.

Moreover, the constraint that the mean of the time-varying distributions is the same (Section 4) is untenable. There are tens of experimental and theoretical studies that show unambiguously how the mean travel time changes depending on hydrologic conditions (e.g. catchment wetness).

It was only an example of the next level up of LP complexity. It was incorrectly specified in the paper in any case and there are no problems in removing it, along with the steady state model.

4. results. Based on my previous comment I find quite surprising that all the numerical examples refer to the steady state system.

Yes .. all examples need to be for time-varying general nonparametric models.

5. as noted by other reviewers, the referencing is definitely inappropriate. I think the author is missing a huge number of papers about travel time formulation and application. As an example, the references to Kirchaner, Selle and Peralta-Tapia whan talking about TTD time-variance should be properly complemented by those works where the idea of time-variant TTDS has been proposed, proved theoretically and then applied. The relevant missing papers are too much to list them here.

As noted in response to other reviewers on this topic, this short technical note is just concerned with proposing a methodology for combining linear programming with nonparametric transit time distributions to obtain a lower bound to mean transit time. To balloon out the text to incorporate a massive introduction covering all that has gone before in parametric transit time modelling would be a needless distraction and go far beyond the size and scope of a short technical note with narrow focus. However, because a revised paper will now be themed only on the time-varying general model, it would certainly be appropriate to trace the initiation and subsequent development of time-variability of transit time distributions. Part of this would need to overview the extent to which means of sequential transit time distributions may differ, as this can be set up as an LP constraint when obtaining the lower bounds.

Overall my impression is that this work is a nice mathematical exercise that unfortunately disregards the physical processes involved in catchment transport processes, and (as such) it as a reduced potential for real world hydrological applications.

I guess “nice” is being used here in the context of “irrelevant”. However, the reviewer’s main issue appears to have been that the nonparametric lower bound approach supposedly does not allow for full degrees of freedom for time-variability of transit time distributions. Hopefully this concern could be offset by considering in a revised paper only the general time-varying case, together with examples.

Taking into account all reviewer comments, the obvious synthetic example to use in a revised paper would be for a sequence of different L-shaped distributions with variations of both mean and shape, together with input and output time series including a component of measurement error. By “L-shaped” is meant (for a probability density function) that the first derivative is everywhere ≤ 0 and the second derivative is everywhere ≥ 0. This includes, for example, all gamma distributions with shape parameter ≤ 1. The required linear constraints would be applied to all the different time-varying nonparametric transit time discrete distributions. This would create a much larger LP setup than was used in the steady state example, but is certainly achievable for current LP packages.

Cartwright, I, Morgenstern, U, 2016. Contrasting transit times of water from peatlands and eucalypt forests in the Australian Alps determined by tritium: implications for vulnerability and the source of water in upland catchments. *Hydrol. Earth Syst. Sci*., 20, 4757–4773.

Mosquera, G. M. et al., 2016. Insights into the water mean transit time in a high-elevation tropical ecosystem. *Hydrol. Earth Syst. Sci*., 20, 2987–3004.