Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-314-RC3, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Data-based mechanistic model of catchment phosphorus load improves predictions of storm transfers and annual loads in surface waters" *by* Mary C. Ockenden et al.

Anonymous Referee #3

Received and published: 7 July 2017

General comments

This paper explores the use of Data-Based Mechanistic modelling to gain understanding of the dynamics of phosphorus transport using a high resolution TP concentration and load time series data set. To my knowledge, such a study has never been conducted using an hourly time series data set. As such, the paper will be useful to anyone working in phosphorus transport – a key component to understanding eutrophication issues. It is particularly worth noting that the authors are making the data openly available for which they are to be applauded. Presumably this will include the necessary meta-data to enable future users to understand the uncertainty in the measurements.

Overall, the paper is very well written, and suitable for publication after the issues noted below have been dealt with.

Specific comments

Given that approximately 3 years of data have been used in this study, could the authors comment on the magnitude of the error/uncertainty in the result if only daily data were used, or even worse, data collected at varying intervals (as is frequently the case)?

The authors cite Young (2010) who argued that the sampling period should be at most 1/6th of the quickest time constant. Given this and finding that the time constant for the fast pathway varied between 2 and 15 hours, using hourly data at the low end of this scale would mean the sampling frequency would be too slow – questioning the statement in the abstract that hourly data are "necessary". For time constants near 2 hours, hourly data would be inadequate by the condition given by Young (2010) – does this mean that the authors are revising the recommendation given by Young (2010)? My thought is this is not the case, so the phrasing in the abstract seems a little strange.

Page 2, line 25: The authors state that USLE is a process-based model. In a broad sense it could be called this, but I feel it would be more accurate to describe it as an empirically-based model.

Page 4, line 23: Can visual inspection really determine whether there has been a significant loss of information? Was the inspection merely looking at the hydrograph (i.e. plotting the data)? Could some objective measure be developed for this?

Page 6, line 26: A minor point, but there are some issues with the description of NSE. The numerator is the variance of the model residuals if and only if the mean residual (i.e. bias) is identically zero. It is a reasonable approximation to the variance if the bias is small, however it is a biased estimate as it will always be a little larger than the actual variance. On page 8 (line 22), the authors state that the model bias was less than $\pm$ 10% for all three catchments. What would the impact of this amount of model

bias be on the comparison between R_t^2 and NSE? An even more minor point is that the normalisation factor for the variance is usually 1/(N-1) rather than (1/N).

Page 7, line 25: Text seems to jump a little at this point. Maybe add a sentence bringing "storage" into the picture before the closing sentence for this paragraph?

Page 8, line 11: Presumably, the rating curve would need to be extrapolated to get to this flow level, which would also contribute to the uncertainty in the estimated discharge?

Technical corrections

Page 7, line 14: space missing after Avon

Page 9, line 23+24: The flow fractions for the slow flow component are not really needed as this are just 100

Page 9, line 32: Space missing after "drain flow"

Page 12, line 7: space missing after "have"

Page 12, line 31: space missing before "Yang"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-314, 2017.