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Interactive comment on "Oxygen dynamics and evaluation of the single station diel oxygen model across contrasting geologies" *by* Simon J. Parker et al.

Anonymous Referee #2

Received and published: 4 July 2019

I'm sorry this review is delayed but the delay has resulted in clarification of a number of my questions which were picked up by the other referee. In particular, I now have a much better understanding of the purpose of the paper. This was not clear on the initial reading.

I would therefore strongly recommend that the abstract be revised to better reflect the purpose of the paper. i.e. the point stressing that this paper is " about evaluating model structures, not quantifying parameters", needs to be right up front.

It is also worth reflecting on George Box's pithy aphorism that all models are wrong but some are useful. This is highly relevant to this manuscript as it begs the question

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'useful for what?' There are a number of generalizations and simplifications implicit in solving the 'simple' metabolism model of Odum into three components viz. (ecosystem) respiration, primary production and reaeration. The author points out the likely effects of autochthonous vs allochthonous carbon on respiration rates and how this is likely to be time dependent on a daily time frame. I totally agree with this. The key point for me though is 'does this matter?' What question is driving the use of stream metabolism in the first place? If it's mechanistic understanding, then nuances matter very much. If it's about aggregating organic carbon loads across time and space, contrasting watersheds with different land use for example, then it doesn't matter anywhere near as much, if at all.

As a general point, I find discussion centered on changes in DO based on temperature often inadequate as the obvious effect of temperature effect on DO solubility is neglected. An increase in night time DO is expected if water temperature falls. Framing the discussion in terms of %DO saturation is much more useful to examine the interplay of R and k but in this instance makes it more convoluted to then talk about change in DO = 0. Of course if temperature doesn't change (nor atmospheric pressure or salinity to be pedantic) then change in DO = 0 would correspond to a change in %DO saturation of 0.

Minor point: Page 2, Line 4. Suggest changing 'components' as this word was used in the previous sentence to refer to P, R & reaeration.

Because this is novel, I would like to see a little more explanation about HOW R/k can be used to interrogate the validity of the model used for fitting diel O2 curves. This may be immediately obvious to those who routinely inverse model metabolism, but to many readers this won't be clear at all.

Page 2, Line 20. Equifinality hinders resolving ER and k when %DO saturation is very close to 100% or when there is very little change in %DO over the day, it is not a universal problem.

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Page 3, Line 26. It has already been stated that precision and accuracy of the DO sonde data is of fundamental importance in reliably identifying points where the change in DO is zero. Yet there is no mention at all of how accuracy of the deployed sondes was verified. Probe drift would be a major confounding factor in this analysis.

Page 5, Line 13. The cause of this sudden change in the rate of decline?

Is there any significant time-of-day dependent topographic shading of any of these streams over the study reach integrated by the sonde? If so, this may then affect time of peak DO.

Reaeration will not only depend on temperature (in a well-known relationship) but also on wind (there is a lot of lakes' literature on this topic) and on discharge. A change in discharge will almost certainly change k and this relationship will be idiosyncratic for each site depending on stream channel shape, wetted area, roughness etc. Are these additional factors responsible for some of the variation observed in this data set?

Bearing in mind the already posted review and the author's responses, I still believe this is an interesting paper that definitely warrants publication. However, to be more useful to the general reader and in particular those undertaking studies where stream metabolism is being measured, I strongly recommend the author provides an additional paragraph or two which guides the user through checking the inherent model assumptions when modelling their data. This can be in a series of steps checking whether the assumption of an invariant R (temperature effects notwithstanding) has a significant effect on overall metabolic parameter estimates.

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