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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 #1

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This manuscript uses a unique approach to evaluate whether the assumption of a constant rate of ecosystem respiration is valid over a daily cycle as assumed in most aquatic ecosystem metabolism models. He evaluates whether the point at which the rate of change in oxygen concentration for a given day is equal to zero (i.e., dO2/dt = 0) provides information about the ratio of ER/k within and across stream types. He then argues that because this technique does not agree with results from the nighttime regression approach of Hornberger and Kelly (1975) that the assumption of a constant daily rate of ER is invalid.

However, beyond that, it is not clear to me how this approach provides an estimate of

diel change in R, as stated in the abstract and discussion? We know that ER changes over the course of a day in response to temperature (Holtgrieve et al. 2010) and carbon substrates (Schindler et al. 2017, Sadro et al. 2014) but you generate a single ratio using this approach, not explicit rates of ER.

In addition, I'm not sure what the ratio (ER/k) really describes – how does this get you additional information that you don't get by fitting a metabolism model, since the ratio of R/k doesn't give you any information on their relative magnitudes. And, wouldn't you still face the issue of equifinality (many values of ER and k that could produce a given ratio)?

Further, you discuss the importance of correcting ER and k for temperature, but then don't consider that in your estimation of their ratio – wouldn't the diel variation in temperature have a lot to do with when the point of dO/dt = 0 occurs as well? In addition, the temperature correction is different for the two, so the degree of daily temperature fluctuation could impact the resulting ratio.

Zero change in DO has an equal element of uncertainty to it (when does DO/dt = 0?) as does fitting a nighttime regression (i.e., where does night begin?) so I'm not sure what you gain through using this approach? In addition, using the nighttime regression technique is no longer the most common way of estimating reaeration rates because of some of the shortcomings you mention and cite.

In summary, I have concerns about the significance of these findings given the degree to which the field has moved on in terms of approaches to metabolism models (e.g., Appling et al. 2018, Song et al. 2016) and the ability to estimate and constrain k (Appling et al. 2018, Raymond et al. 2012). In addition, the results as presented spend a significant amount of time discussing the degree of coherence in diel oxygen patterns (e.g., timing of max O2), rather than comparing potential daily fluctuations or cross-system differences in important metabolic parameters. I recognize that assessing differences in the magnitude and timing of daily fluctuations has some meaning in terms of understanding the magnitude of processes, but this discussion would be more meaningful had those processes also been quantified.

Specific comments: Page 2, Lines 3-4: “Primary production can be quantified by partitioning a single DO time series into its component fluxes, namely photosynthesis, ecosystem respiration and aeration” – perhaps you mean to say “ecosystem metabolism” Page 3, lines 3-5: What is the “pertinent process”? Page 3, lines 19-20: “primarily groundwater fed”. . . couldn’t the point of zero change reflect the O2 concentration of groundwater input as well? Page 4, lines 27-28 – wouldn’t it be “normalized” not “detrended”?

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