

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

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Thank you for reading the paper and comments.

I have separately written a general comment that addresses objectives, as I think perhaps they weren't clear. I should say that there was never any intention to quantify R or k. The idea was to examine what can be measured rather than what can be modelled.

Specific points:

"...it is not clear to me how this approach provides an estimate of diel change in R, as stated in the abstract and discussion?" I don't state this in the abstract or the discus-

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sion. I do not make any attempt to quantify R or claim that I am doing so. The whole approach is about evaluating model structures, not quantifying parameters.

"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". This is the central issue. Which model do you fit and if you choose the simple model (single stage ER) how do you know it's good enough?

"...so the degree of daily temperature fluctuation could impact the resulting ratio." Yes, this is true that I do not explicitly consider temperature (although I do state that I'm not considering temperature). In other words, you are saying that I am evaluating model structure 1 (in the separate author comment) rather than model structure 2 (which is what most people use). In response to your comment, I have in the past week done additional analysis which compares the temperature regimes for the two nights (May 9th and May 16th) for all four rivers which demonstrates (to me, at least) that temperature is not the dominant control. The figure is attached. There is no evidence that the Ebble and Nadder have one temperature regime and the Wylfe and Avon have another (which would be the case if temperature was the explanation for the differences in behaviour shown in Figure 7 and figures 4 and 5). In fact, the temperature regimes for the Nadder and Avon are more similar to one another than those for the Nadder and Ebble, even though the Nadder and Ebble are the rivers with early DO nighttime minima, so it seems to me at least that temperature is not the explanation.

"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?)". I disagree with this statement. If you have sufficiently high resolution of measurement (1 minute in this case), the uncertainty associated with when $dO/dt = 0$ depends only on the accuracy and precision of the instrument, and the occurrence of spurious data points as with the Nadder in Figure 5. Otherwise, it's robust and reproducible. With regard to uncertainty of nighttime regression, yes, it does depend on what portion of the night you use, but I discussed this in Figure 11. It seems to me that the important point is that the rela-

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tionship between the two differs systematically between rivers. So, for example, even though the Wylye and Ebble are both Chalk and groundwater-dominated to the same extent in that they both have BFI of 0.9 (Table 1), nevertheless the relationship between DO saturation at zero DO change and the corresponding ratio calculated using Hornberger-Kelly is completely different (Figure 7). A corresponding argument applies to the Nadder and Avon.

"...and the ability to estimate and constrain k (Appling et al. 2018)." Yes, but their justification for the use of simple models "that are more resistant to overfitting and are likely sufficient for many streams" is axiomatic and based on little evidence, as far as I can see. If the model structure is inappropriate, there is not much point in constraining k.

I am not saying that simple models are never appropriate or that respiration can never be approximated by a single stage process, but the question is when are they appropriate?

What I am trying to do is provide an easily measured metric that can be used as an additional aid to give some indication as to the appropriateness of model structure. It's not a silver bullet, it's just an additional way of characterising the system.

Response to specific comments: Yes, ecosystem metabolism. Pertinent processes are those in the equation. In other words, assuming that the equation captures the oxygen behaviour and no significant processes have been omitted. Groundwater. This is addressed above. Despite the fact that the rivers have very similar groundwater regimes, nevertheless they have differing oxygen regimes as described, so that the difference in oxygen behaviour cannot be attributed to differences in groundwater regime. Yes, normalised.

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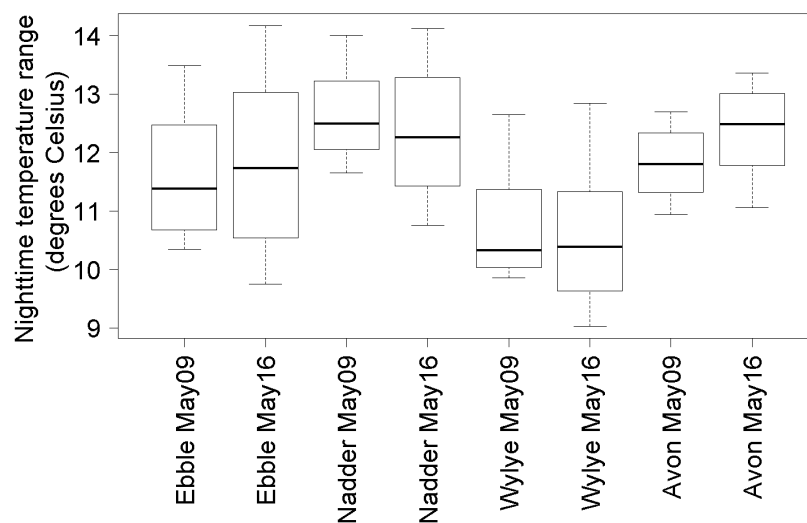


Fig. 1.

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