

Interactive comment on “Increased bacterial growth efficiency with environmental variability: results from DOC degradation by bacteria in pure culture experiments” by M. Eichinger et al.

Anonymous Referee #2

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The paper presents bacterial growth efficiencies from monospecific batch culture experiments using pyruvate as carbon substrate surrogate. As core data the authors follow DOC, POC and O₂ concentration changes over several days, in experiments that are either supplied with a full load of 8 mM DOC or in pulsed additions. From the POC and DOC data BGEs are calculated with different methods including three modeling approaches. The main finding is that pulsed DOC additions and the inclusion of maintenance energy requirements (addressed by the Marr-Pirt and DEB models, respectively) exhibit higher BGEs as compared to unpulsed experiments and/or direct calculations from POC/DOC changes and the Monod model.

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In general I found the paper interesting and in part well described (particularly the introduction, methods and results section). Estimating bacterial growth efficiencies still is a delicate task as the mechanisms influencing bacterial growth efficiencies are generally difficult to measure. As a result bacterial growth efficiencies usually are not considered in carbon models. The presented paper shows a novel though experimental way to assess the potential influences on bacterial growth efficiencies. Thus overall the paper is appropriate for Biogeosciences.

General comments:

In the methods section I would have liked to see a bit more on how the DOC and POC were measured including analytical details.

To my mind there are a lot of abbreviations in the manuscript, which is usually fine but I think abbreviations are easier to remember if they somehow reflect the original term. E.g. why was the abbreviation B chosen for the unpulsed experiment? The abbreviations used for the different models are unreadable (page 797). Is this usual practice or is it possible to form the abbreviations in the equations a little more incisive?

Why was oxygen consumption measured? I did not quite understand why this measurement was not used to calculate BGE?

I think that BGE estimates from an artificial experiment as presented here are as good or bad as the in situ measurements. Most of the more recent field data suggest low BGEs (including all the probable biases of conversion factors etc.) and these data span quite some temporal and spatial scales. Most of these data have been acquired from oxygen consumption measurements and bacterial production estimates for good reasons: The needed analytical precision and accuracy of DOC measurements is usually not sufficient to measure decrease in a sensible time. Furthermore I think there are more papers on temporal and spatial dynamics in BGE estimates from the field than discussed (cited). Overall I would suggest to edit the discussion such that it is clear(er) to reader what potential insight can be gained from the conducted experiment

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and compare this better with reports from the field. I.e. the shortcomings of both approaches should be discussed in more detail and thus overall the discussion is not really satisfying.

Specific comments:

Introduction: no comments

Methods:

Maybe I understood something wrong but was there only one experiment conducted?

Page 795, line 7: DAPI is 4',6-diamidino-2-phenylindole

Results:

Page 796, line 16: ...recalcitrant-to-degradation DOC... It's not clear to me what that is. Please describe differently.

Page 801, line 15-16: Flowcytometry or epifluorescence microscopy is quite sensitive, thus I don't think that the accuracy is a problem here? For the POC measurements I cannot tell as the analytical basic information is missing in the methods section. In any case, I suggest to rephrase this sentence to: Probably due to the low POC concentrations at the onset of the experiment accurate measurements were difficult (or similar).

Page 802, paragraph 3 and 4: Is there any value e.g. a coefficient of variation that could indicate to the reader the goodness of fit for the different models? A good or not so good fit are quite subjective terms.

Discussion:

Page 803, line 17: Were the respiration measurements used for anything else than indicating the maintenance respiration? I would suggest to discuss a little more on maintenance respiration and the problems surrounding it. There have been attempts

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before that try to measure this.

Page 804, line 15-18: This sentence is not clear to me. Please rephrase.

Page 804, line 24: ...maintenance from the reserve, component that would not... Either something is missing here or it's a copy and past error. Please rephrase.

Page 804, line 25-28: I guess the abbreviations of jEM and jVM are typos as they are nowhere introduced

Page 806, line 5-7: The Carlson and Ducklow citation plus the argument the authors draw from it is repeated on page 807 line 8.

Page 807, line 28: It is not clear to me where the authors demonstrate the conversion factors of BP may vary. Please explain this part better.

Page 808, line 3: What is the average CCF generally reported and what is the source of these CCFs?

Page 808, line 12: There are more recent papers on seasonal variations in BGE from the field than the review cited.

Figures:

The figures are too small, particularly fig 4 and 5

Fig 4. Why did the authors change the sequence of presenting P and B. I would suggest to leave the original sequence of presentation with B first and than P.

Fig. 5. Why did the authors change the lines for P and B? I suggest to change B to solid and P

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