

## ***Interactive comment on “Tune in on 11.57 $\mu$ Hz and listen to primary production” by Tom J. S. Cox et al.***

**Anonymous Referee #1**

Received and published: 8 May 2017

This study presents a significant development to the frequency-based GPP estimation method by Cox et al. (2015, doi:10.1002/lom3.10046). The backbone of the method is the same: GPP is extracted from dissolved  $O_2$  time-series by assuming that it is the only significant component having an exactly 1 day period. Contrary to the preceding study, the novel method theoretically allows getting daily GPP without having to split the  $O_2$  time-series in windows and considering GPP being constant inside the windows, thanks to the complex demodulation technique.

This elegant technique will really contribute to metabolism estimations, but future applicers must know under which circumstances the assumptions remain acceptable. Potential violations of the basic assumption (e.g. that GPP is the only 1-day periodic process within the  $O_2$  time-series) are extensively discussed in Cox et al. (2015), but only briefly mentioned here. GPP must change relatively slowly – so water mustn't be eutrophic,

C1

the water column must be deep enough to provide stability but it should not stratify: Shallow systems are not recommended due to intense atmospheric exchange, deep systems due to hindered vertical mixing. Besides the mentioned limitations, others may be identified too: atmospheric exchange can show strong diurnal period when diurnal  $DO$  fluctuations are high (e.g. in hypertrophic conditions daily maxima can reach >150% saturation and minima can drop below  $2 \text{ mg L}^{-1}$  on the same day, generating a huge saturation deficit/surplus) or when piston velocity is affected by regular coastal winds (large lakes often generate systematic local winds at dawn and sunset) and their corresponding horizontal currents.

The paper would benefit from some revision to put more focus on the real benefits of the proposed method. It would be nice if the benchmark could be Cox et al. (2015), and not the bookkeeping method again.

In summary – and in accordance with the paper's statement –, the proposed technique seems to be most suitable to estuary applications, where frequency-domain calculations allow separating the tidal components in the  $O_2$  time-series. However, due to the presence of some almost daily tidal components it turns out that a 15 d filter needs to be applied on GPP in order to get a reliable estimate. At this point, it would be nice if the authors could explicitly point out the benefits of the new method compared to the one in Cox et al. (2015), where mean GPP was calculated in 14 d sliding windows, which apparently provided the same results.

Overall, the presentation of results should be revised to really focus on the benefits of the new method, especially when compared to Cox et al. (2015). While this method has theoretically finer temporal resolution, the same kinds of figures are provided (fig 1,2,4top: time vs. estimated and modelled GPP), leaving some doubt if the new method delivers anything more than the previous one. Relations between estimation error and time or  $O_2$  are not shown.

Convincing power could be increased if:

C2

- Fig 1 (bottom), 2 (bottom), and 4 (top) would show something more than the corresponding versions in Cox et al. (2015) [fig. 3 top, fig. 6 top].
- the only non-synthetic application (Hörnum Tief) included some reference data, like in Cox et al. (2015). In its present version there is no way to judge if the calculated values had any reference to reality.

#### SPECIFIC COMMENTS

Pages 5-6: Findings about tidal components should be better organized in results: please use some subsections.

Page 7 Lines 4-5: These Fourier techniques make the implicit assumption that air-water exchange has a period that is far from 1 day, which in practice would mean a constant exchange rate during a day (shorter periods than a day seem unrealistic). This is not conceptually superior to the assumptions made in traditional methods. For the case of transport it may be true (but see remark about coastal winds and corresponding currents above)

Page 8 Lines 3-5: Half of the rather brief conclusions relate to the preceding study. It would be nice to achieve a healthier balance between the new and the old findings. The last sentence isn't really a conclusion: What does 'they' stand for? Moreover, nothing is said about 'HOW they influence' GPP estimates.

#### TECHNICAL CORRECTIONS

Page 5 Line 31: change "interfe" to "interfere"

Equation 6: in second equation, shouldn't it be  $y'(t)$  instead of  $y(t)$ ?

Page 8 Line 4: change "esatimate" to "estimate"

Figure 1: Please add proper units to the O2 concentration ( $\mu\text{M L}^{-1}$ ?).

Figure 2: Please add proper units to the O2 concentration ( $\mu\text{M L}^{-1}$ ?) and GPP ( $\mu\text{M C3}$

$\text{m}^{-2} \text{d}^{-1}$ ?).

Figure 4: Please add proper units to GPP ( $\mu\text{M m}^{-2} \text{d}^{-1}$ ?) and label plus units to the horizontal axis of the middle panel ( $f [\text{d}^{-1}]$ ?). Please add labels to the vertical lines of P1-K1, O1, Q1.

Figure 5: Please add labels to the vertical lines of P1-K1, O1, Q1.

Figure 6: Please add proper units to GPP. Please add labels to the vertical lines of P1-K1, O1, Q1.

---

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-81, 2017.