Second Submission

Ms. Ref. No.: bg-2018-508

Title: Composition and cycling of dissolved organic matter from tropical peatlands of coastal Sarawak, Borneo, revealed by fluorescence spectroscopy and PARAFAC analysis

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1. Overview and general recommendation

1.1. Recommendation

Accept after minor revision

No additional review is needed as long as all the comments below will be addressed.

1. 2. Comments to Author

Thank you for your revision. I am satisfied with the revision and have only minor comments.

Regarding your responses to the editor, I agree with you. NaN_3 does not absorb >300 nm and I did not actually refer to any interactions between Fe(III) and NaN_3.

2. Minor comments

2.1. Supplementary information about NaN_3 (relatively major comment)

I appreciate the authors' efforts. However, is the label for the Y axis in Figure S1 maybe wrong? The percentage contribution of NaN_3 blanks to the total Napierian absorption coefficient (CDOM + NaN_3) reaches almost 100%, which means that the absorbance of solution containing CDOM + NaN_3 was almost completely occupied by NaN_3. The highest value seems around 95%. This means that NaN_3 adsorbed 95% and CDOM adsorbed only 5% (NaN_3 19 times higher than CDOM)? Perhaps the Y axis should be 'The percentage contribution of NaN_3 blanks to the CDOM Napierian absorption coefficient?' If so, 100% means the equal contribution from NaN_3 and CDOM. But you're indeed saying 'The NaN_3 accounts for 0 – 95% of the <u>total</u> absorption coefficient at 250 nm'….

2.2. PARAFAC should be written out at the first use. P3L8

2.3. Were water samples for the photodegradation experiment sterilely filtered directly into quartz bottles? It's unclear. P3L22 & P4L5

2.4. No year for the citation McDonald et al. P4L15

2.5. References to $S_{275–295}$ and $SUVA_{254}$ are needed. P4L18

2.6. Were the sample fluorescence intensities normalized before PARAFAC? P5L15

2.7. Were the removed four outliers during PARAFAC modeling projected onto the validated model later and their results are reported? Did the model fit well? P5L20

2.8. Interpretation of FI values according to Cory et al. (2010) P4L12–L18 & P8L21–L30 (relatively major comment)

After correction for all spectroscopic biases, and with Fluoromax, FI only varies from 1.2 to ~1.6 (Cory et al., 2010). traditional FI range of 1.2–1.9 (McKnight et al., 2001) is no longer valid. Your data showed that FI ranged between 1.1 and 1.6, which is a full range that FI can take. Thus, saying `The fluorescence index (FI) was very low across the whole study region` while citing Cory et al. (2010) does not make sense to me. I suggest you remove McKnight paper and change the interpretation of FI that reflects Cory et al. (2010). In fact, both papers were written by the same authors and thus they changed the interpretation, but still other researchers are citing the old paper. One possible cause of this is that the original authors changed their FI calculation in Cory and McKnight et al. (2005) paper WITHOUT saying any reason. Nine years later they revealed the reason for the first time. However, you can refer to the paper by Maie et al (2006) 'Chemical characteristics of dissolved organic nitrogen in an oligotrophic subtropical coastal ecosystem' (*2.3 Optical measurements*), where you can find that they knew the reason beforehand (they knew the Cory's Ph.D. dissertation).

2.9. FI as a tDOM tracer P9L9 & P14L5

Related to the previous comment. FI value of 1.5 or 1.6 does not ensure that your coastal sample is dominated by tDOM. Nevertheless, I totally agree with your assertion that FI is not a good indicator of DOM origin. It's likely that the fluorescence indices generally work better in the order of HIX>BIX>FI.

2.10. Assumption for tDOM estimate by PARAFAC

The sentences 'Our approach assumes firstly that C1 is exclusively terrestrially derived, and has no non-terrestrial sources in estuaries and marine waters (P12L28)….The first assumption is probably broadly valid: as discussed above, Fmax values of C1-like components in open-ocean waters are very low relative to the values across our study area (P12L30)'

and

'Alternatively, there could be additional sources of C1-rich DOM within the Samunsam estuary (P12L23)' and 'In the Rajang River, C1–C4 all showed positive deviations from conservative mixing,

suggesting that there were additional inputs of all of these components in the Rajang estuary (P7L24)' are counter-intuitive.

To me, it seems you are still struggling with the use of C1 as a tDOM tracer because of additional C1 inputs from estuarine environments. However, the estuarine vegetation (peat and/or mangrove) is also terrestrial. Maybe you can solve the problem by including this vegetation to tDOM sources?

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