Reviewer comments on Biogeosciences Discuss., doi:10.5194/bg-2016-329, 2016

General comments. This paper documents variability in DOM along a longitudinal transect in Lake Balaton during 2014. The authors report that DOM concentrations generally ranged from 8 to 16 mgC/L in surface waters (which is relatively high), with values above 10 mgC/L mostly in the eastern end of the lake which receives inflow from a large wetland. They also report four optical properties of the DOM (absorbance, spectral slope coefficient, SUVA and E2/E3) as potential indices of DOM source or internal processing (especially photodegredation). Unfortunately, the choice of wavelengths for some of the optical measurements was not optimal, thus limiting comparison with other systems and studies. Although the paper reports apparently new information about Lake Balaton, there are several matters in the text, tables and figures that will require re-thinking and major revision.

Specific comments.

1. Abstract. Line 35. The data on Fig 9c and 9d are not convincing evidence that UV irradiation caused any change in the absorbance of "autochthonous" DOM. Omit sentence
2. Abstract. Omit last two sentences. Nothing new here.
3. Lines 70-73. Unclear sentence. What nutrients are we talking about? Re-think and re-write.
4. Line 93. The wavelength 440nm is not commonly used for CDOM. 440nm is routinely used for Chl-a because it is the absorbance maximum for that pigment. The preferred wavelength for CDOM is in the UV, usually less than 370nm. Choice of 440 (blue) rather than UV needs to be justified.
5. Line 95. The spectral slope coefficient recommended by Helms et al (2008) and by Fichot and Benner (2012) is calculated over the UV range 275-295 nm. But in Balaton, the authors used a very different range (350-500 nm). Choice of this range needs to be justified given that the authors cite Helms et al and Fichot and Benner.
6. Line 113-120. Not true. There is a relatively rich literature on DOM in temperate lakes. Literature search needed.
7. Lines 124-127. For objective #1, add "over the course of one year." For objective #3, since there were no direct measurements of the underwater light field (e.g.Kd PAR, or spectral Kd), this is not a bona fide objective.
8. Line 174-175. One year of data is not sufficient to document a representative seasonal cycle. Caveat needed.
9. Line 180. A 0.7um filter will not remove small phytoplankton, bacteria or large organic colloids. Explain why 0.2um filters were not used, and what the consequences of including small particulates might be.
10. Lines 211-212. Helms et al (2008) recommended S275-295 as an indicator of in situ DOM photoprocessing for future DOM studies (not E2:E3). That needs to be acknowledged.
11. Section 2.5. CDOM photodegredation. Except for Comment #13 (below), this is a very interesting and well planned experiment. Kudos.
12. Lines 226-234. In nature, autochthonous DOM is more likely the exudates from live phytoplankton rather than dead cell remains (which would be colonized by microbes and sink out). Since exudates would be in the supernatant not the pellet, the method of grinding and digesting the pellet needs to be justified.
13. Section 3.1. Seasonal variability. Caveat needed to acknowledge that seasonal changes were characterized for only 1 year, and may not represent the "average" seasonal cycle across multiple years with different weather patterns

14. Line 263. Rephrase sentence. With the exception of STO.Basin 1, the data on Fig 2 do not indicate high CDOM variability across most of the lake. In 5 of the 6 basins, DOC and a(440) were relatively stable.
15. Line 286. The evidence for two peaks in DOC on Fig 2 is weak at best. Rephrase.
16. Line 296. Add "during July when interbasin differences were likely to be highest."
17. Lines 303 to 308. This paragraph needs to be re-thought. The range of Scdom across stations is actually quite wide in the scheme of things for natural waters. Compare to Helms et al (2008) and other studies where Scdom gradients have been reported.
18. Line 318. Re-think. Concluding that DOC varied more than these two optical properties seems a consequence of scaling rather than a property of the variable. Comparison of CVs or Z-scores is needed here
19. Section 3.3. Photodegredation experiment. This section needs to be re-written. The results shown on Figure 9 indicate that UV irradiation had a significant effect only on the absorbance of allochthonous cdom. This result is consistent with the data on Fig 10a. That's all that one can say with confidence about the photodegredation experiment. There is no discernible effect on Scdom or on "autochthonous" cdom in these data.
20. Section 4. Discussion. This section has a some overstatement, speculation and misinterpretation that needs to be removed .
    a. L 356-363. Actually, DOM across most of the lake is relatively constant (Fig 2&5c), as observed in other large lakes. High DOM in Balaton was restricted to stations near the inflow from a high DOM river. In studies of other large lakes, such stations were likely not sampled because they skew the data and contribute little to the total lake mass of DOM.
    b. L364-374. It is an exaggeration to say that aCDOM and DOC varied seasonally throughout the system. This was only true for Basin I. In the other 4 basins, aCDOM and DOC were relatively constant throughout the year (FIG 2)
    c. L413-424. Photomineralization would not affect Scdom, it would only affect DOM concentration. But photobleaching can have a large effect on Scdom (e.g.Helms et al, 2008), and the data for Lake Balaton shown on figs 9 and 10 suggest that the humics are the fraction of DOM that is being bleached.
    d. L457. Again, microbes mineralize (respire) DOM or turn it into biomass rather than bleaching it
    e. L471-480. This is an over-interpretation of the results shown on Fig 9. It is unsupported speculation. Omit paragraph
    f. L481-491. This is also over-interpretation and speculation. In fig 9c, there was no difference between the irradiated "autochthonous" samples and the dark controls. In 9b, there were no controls. Omit paragraph
    g. L492-500. The low fluorescence signal from "autochthonous" DOM is likely due to its low concentration. Re-write
    h. L 505-519. Absent measurements of the underwater light field in the lake, it is speculation to propose light limitation by DOM in such a shallow, well-mixed and eutrophic lake. Ten percent of incident solar irradiance is generally sufficient to support strong phytoplankton growth, and the authors would need to show that attenuation by DOM was sufficient to reduce downwelling light penetration below that level in the epilimnion. Re-write paragraph
21. Table 1. Change notation to (mean±SD). Specify date range in heading
22. Table 2. Are these data for July 2013 or July 2014 (or both)? If both, how was interannual variation accounted for. Specify dates in heading

23. Fig 2. Check the trace for aCDOM(440). It behaves weirdly during autumn in some of the plots. Needs to be fixed
24. Fig 5. Add “measurements made in July…” to legend. In Fig 5c the 5 DOC categories exaggerate actual spatial variability. The three highest categories are separated by less than 1 mgC/L, while the low category spans more than 8 mg.C/L. The category indicated by a yellow X is highly questionable (zero DOC? I don’t think so). Re-think this.
25. Fig 9. Panel 9d is superfluous. Omit