

Interactive comment on “Spatio-seasonal variability of chromophoric dissolved organic matter absorption and responses to photobleaching in a large shallow temperate lake” by María Encina Aullo-Maestro et al.

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Response to Reviewers – Aulló-Maestro et al. Biogeosciences Discuss.,
doi:10.5194/bg-2016-329, 2016 Anonymous Referee #2

C1 General comments: The study has investigated the spatial and seasonal variation of chromophoric DOM in a lake at several stations leading away from an inflow from a River supplying allochthonous DOM. Various methods were used to capture change in DOM over time and spatially including analysis of the absorption coefficient spectral slope, E2:E3 ratio, SUVA and Spectral Fluorescence signals. The study shows that the

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absorbance coefficient and DOC values were highest and most variable near the inflow to the lake, whereas other basins further located from the inflow had more stable values with more seasonal variability in the spectral slope of CDOM. While the study has performed what seems like a well-planned study there are some very strong statements that over reach the scope of the study. I suggest major revisions especially of the discussion.

We very much appreciate anonymous referee #2 for the time and effort put into reviewing this manuscript, his / her comments have greatly contributed to improve the manuscript.

C2

Specific comments:

1. Line 84: What platforms are these? Please explain. R1. Here we refer to the Sentinel-2 and Sentinel-3 satellites. This can easily be clarified.

2. Line 86: Please remove “the” in the part of the sentence which reads “However, CDOM is the arguably most challenging...” R2. Modified as suggested

3. Line 118: There are quite a few studies on temperate lakes, including seasonal work, see Müller et al 2014 “Hourly, daily and seasonal variability in the absorption spectra of chromophoric dissolved....”

R3. We agree with reviewer 2 and sentence has been modified as suggested and some references added. Sentence now can be read as: “There is a relatively rich literature on DOM in temperate lakes (e.g., Zhang et al. 2011; Read & Rose 2013; Müller et al. 2014) but few studies have focused on large shallow lakes like Lake Balaton with a strongly continental climate and hence our understanding of the variability in CDOM optical properties in these systems is comparatively poorer. “

4. Line 174: Mention the time of the year samples occurred to represent seasonal variability. R4. This change has been addressed for reviewer’s 1 comment and can

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now be read as: “In order to capture seasonal variability in CDOM quantity and quality, water samples were collected fortnightly at 6 long-term monitoring stations on Lake Balaton over the course of seven months (March to September 2014)”

5. Line 187: Instead of referring to the summer campaign as “intensive summer campaign” change to “spatial variability” in the whole manuscript. R5. We agree with reviewer’s 2 suggestion and the change has been made effective on the whole manuscript

6. Lines 190 and 193: Explain why two different instruments were used for the different campaigns. R6. The samples were analysed at different institutions with different instruments for practical reasons. This has been clarified in the text.

7. Line 194-195: It is not clear when the reference sodium azide was used. Please make this clear. R7. It was added immediately to preserve the samples; this has been clarified in the text

8. Line220: What was the temperature in the lake during this 7-day incubation? R8. The mean temperature of the lake is now stated in the methods

9. Line 211: Instead of writing “this wavelength” specify which wave length “this” refers to. R9. We agree with reviewer 2, his sentence was confusing and has therefore been deleted

10. Line 233: Was CDOM measured to know the start value? Give further explanation. R10. This data has now been added as suggested

11. Line 263: A seasonal variability in aCDOM, which was used to determine change in CDOM quantity, is not clear from figure 2. Either present statistical data backing this up or rephrase the statement. R11. We have re-written the statement as suggested

12. Line 263-269: The seasons are not shown in table 1, so the information that is referred to cannot be seen in the table. Add this information to the table. R12. We agree with reviewer’s 2 observation and consider useful to include this information,

therefore, the table has been modified to indicate the month of sampling.

13. Line 270: What is the relevance of comparing August values of sCDOM with June? Do the authors mean that these are the lowest and highest values? Please make this statement clearer. R13. Yes, this is correct and is now acknowledged in the text.

14. Line292: When stating something is significantly lower the statistical data must be presented. Please add this data. R14. We no longer use the term “significant” to avoid any claim of statistical significance.

15. Line295: This sentence regarding figure 4 does not present data and should be part of the methods section. R15. This has been moved to the methods.

16. Line301: Are the values of min and max mentioned in the text also in the table? Please review. R16. They are mentioned twice and indeed replicated, the text was therefore redundant and has been deleted.

17. Line304: can this statement that there was a marked variability be made with a change of what seems to be of 0,002 on a nm scale? R17. We agree with reviewer's 2 comment and have therefore modified the sentence in the text so now it can be read: “In Kestzthely (I) basin and the western parts of Szigliget (II) basin nearest the inflow of the Zala River, higher variability was observed with lower SCDOM coefficients more than elsewhere in the lake”

18. Line 309: Where is the statistical data backing up the statement that it “varied significantly”? Please add this data. R18. The text has been modified to avoid any claim of statistical significance but ranges are reported for easy comparison to previous studies.

19. Line 310-311: Refer to table 2 for the SUVA data. R19. Modified as suggested.

20. Line 314: this statement about DOC data availability should be in methods since this cannot be seen in table 2 it is misleading to refer to it. R20. We agree with reviewer 2 and the statement has been moved to the methods section

21. Line 319: Is the correlation significant for all basins? It seems like Basin I has a strong correlation, how would it look like if they were analysed separately? R21. The relationship was only significant for the Keszthely basin. In the other basins, the variability in DOC was much lower and the sample size was small. This is now clarified in the text.

22. Line 324: where is the data for these “marked alterations”? Does this refer to aCDOM? Rewrite and connect the sentences better. R22. We agree this paragraph was confusing, it has been modified and a reference to figure 9 has been added.

23. Line 325: What was the temperature during this incubation? Can you really be sure that there was no bacterial degradation, 7 days is a long time for bacteria to degrade R23. The mean daytime lake temperature is now specified in the manuscript. We do not state there was no bacterial degradation in the treatments only that the control samples suggest bacterial degradation was minimal over the experiment and certainly a minor influence compared to UV bleaching. It possible that bacterial degradation could have been enhanced in the light but it is improbable that this would explain the differences observed between the controls and treatments. The manuscript has been revised accordingly.

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DOM although you filtered through $0.2\mu\text{m}$ there are always some bacteria that are small enough to get through and grow to higher abundance over time, perhaps a portion of the DOM that cannot be measured with aCDOM was taken up like what is shown in figure 10? 0.2-micron filtration typically removes >99% of bacteria from samples (Logan et al. 1993). However, it is unlikely that the filtered CDOM samples were axenic and as such it is possible that bacteria growth and metabolism of DOM in the samples contributed to the degradation of CDOM. However, comparison between the control and experimental samples clearly shows that the degradation of CDOM was greatly enhanced under solar radiation due to photobleaching

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24. Line327: where the reductions statistically significant? R24. Statistical data have now been included ($R^2=0.952$, $p<0.0001$)

25. Line330: Why was there an increase in the dark controls? Please discuss this. R25. As now stated in lines 493-495: “The initial decrease in slope during the early part of the experiment echoes observations by Yamashita et al. (2013) and Fichot & Benner (2012) who attributed this phenomena to microbial degradation of bioavailable CDOM (Nelson et al. 2004)”

26. Line337: When stating no “significant variation” this implies statistical significance and thus data has to be presented. Present statistical data. R26. This text has been re-written to avoid confusion.

27. Line341: same requirement as the previous comment. Show statistical data. R27. We no longer use the term “significant” to avoid any claim of statistical significance.

Discussion section:

28. Line 362: Why is it surprising that the range has not been captured in the northern latitudes? Please explain. R28. Northern boreal lakes generally have high CDOM concentrations (Curtis, 1998) and one would expect the range in these lakes to exceed that observed in Lake Balaton where catchment soils are less organic than in the peat dominated catchments of the boreal zone.

29. Lines 364-365: This statement is contradictory to your results. From figure 2 it rather seems like the aCDOM and DOC values were quite stable in most basins with variation only in Basin I at station 1 probably due to the inflow of the Zala River. Please re-write this part. R29. We agree with reviewer 2, this paragraph was confusing, it has been re-written being now: “The seasonal pattern in CDOM absorption and DOC concentration varied considerably in the western basin, but was relatively constant in other basins. The annual peak(s) in aCDOM (440) and DOC occurred in spring and/or autumn some stations (e.g., ST03, ST12, ST30) were broadly coincident with or lagged

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slightly behind the annual runoff maxima suggesting a seasonal trend that was partly driven by the flushing of organic matter from catchment soils during high flow events. This pattern is common in many temperate and boreal lakes where DOC export from catchments is driven by the availability of flushable terrestrial carbon sources and the seasonality of precipitation and/or snowmelt”

30. Lines 365-367: This correlation was not shown in the results and also does not seem consistent in all basins. Where is the data for this statement? R30. This text has been re-written to avoid confusion.

31. Lines 369-373: Re-write this statement since it bases its argument on the previous statement that there could be coupling between aCDOM and DOC due to rainfall events, which was not observed in this study. R31. The peaks in aCDOM and DOC at some stations occurred in spring or autumn when runoff was high. The clear exception to this trend was the stations located near the inflow where peak aCDOM occurred in the summer due to inputs from the Kis Balaton wetland. The text has been revised to emphasise that not all stations exhibited a seasonal trend that was driven by rainfall and runoff.

32. Line 372: Isn't the Keszthely basin the same basin that is closest to the inflow of the Zala River and thus repeating what was stated in the previous sentence? R32. This has now been deleted

33. Line 385-386: Please add the reference for the water residence time. R33. Reference has been added as suggested

C5

34. Line 395: Please add the statistical data to back-up the statement made that it was “significantly higher”. R34. We no longer use the term “significant” to avoid any claim of statistical significance.

35. Lines 395-397: This information belongs in results since it is not a discussion. R35.

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We agree with reviewer 2 and the sentence has been moved to results as suggested

36. Line 398: Which studies are referred to in the statement “these studies”? R36. We agree with reviewer 2 the sentence was lacking information, therefore. It was and the references of the studies added

37. Line 418: Some references needed here about photobleaching and sCDOM, this sentence seems lost here. R37. References have been added to support the statement.

38. Line 428-429: Please complete the sentence “influenced by both the provenance and subsequent transformations....” of what? R38. Further detail is now provided in the text

39. Lines 454-457: I’m not convinced that this was due to photobleaching, this section refers to figure 2, however this figure does not back-up this claim how do you rule out a dilution effect? Re-phrase. R39. We agree with reviewer 2 this sentence was misleading; the statement has been re-written

40. Line 461: Please add a reference to this paragraph. R40. References have been added to support the statement.

41. Line 465: This data needs to be compared with the control and statistical confirmation presented in the results section. R41. Comparison has been presented in the text as suggested

42. Line 481: Here if referring to allochthonous it should be less susceptible instead of more. Please change. R42. Paragraph modified

43. Line 481: There is no visible change in SCDOM in the ALLO-CDOM. Please rephrase this statement R43. We agree with the fact that there was not visible change in SCDOM for the allochthonous samples, the statement re-phrased stressing the fact that both the spectral slope and absorption coefficient for autochthonous CDOM were lower than for allochthonous samples

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44. Line 481-482: Where is the statistical data to back-up the claim of statistical significance? Is this a comparison between allochthonous with autochthonous or with start values? Please add the data to the results section and re-phrase this discussion based on this. R44. Statement has been changed and as stated before, we no longer use the term “significant” to avoid any claim of statistical significance.

45. Line 492: Where is the data for fluorescence spectra of autochthonous material? Figure 10a and 10b only present allochthonous R45. There were more than ten orders of magnitude difference in fluorescence intensity between CDOM allo and CDOM auto samples, presumably driven by the difference in concentration. Given the low concentrations of CDOM, after Milli-Q correction, there was no measurable fluorescence signal for the autochthonous samples. Therefore, fluorescence spectra of autochthonous material have not been presented in figure 10

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46. Line 495: where is this data? R46. Please refer to response above.

47. Line 500: could this loss not be due to bacterial degradation? R47. Indeed, we suggest that this loss is due to photobleaching and not to bacterial degradation. Statement stressed in the text to avoid confusion.

48. Line 505-506: Please add a reference to this sentence. R48. References have been added to support the statement.

49. Line 506: what is meant by “elsewhere”? R49. Paragraph has been re-written

50. Line 505-509: This is a very strong statement that cannot be proven with the data from this study. Please re-write. R50. We agree and have toned down the statement in line with the reviewer’s comments

51. Line 512: Also this statement is too bold since this was not within the scope of this study. R51. Statement deleted

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52. Line 522-524: Please add a reference to this statement. R52. Statement deleted

53. Line 547: Isn't the contribution of wetlands well known? Remove "novel". R53. Removed as suggested

Technical comments:

54. Line 70-71: Please review this sentence, it seems like information is being repeated and there is a misuse of the word "whilst". R54. Sentence has been re-written as suggested by reviewer's 2

55. Line 71: In the same sentence as the above comment "...this fulfilling important role..." should probably be "thus fulfilling an important role". R55. Sentence re-written as suggested by reviewer's 2

56. Line 75: can CDOM have a behaviour? Perhaps property could be used instead. R56. Sentence re-written as suggested by reviewer's 2

57. Line 87: should be changed to "for reliable estimation of remotely..." Please change. R57. Sentence re-written as suggested by reviewer's 2

58. Line 89: should be changed to "studies have explored the application..." R58. Sentence re-written as suggested by reviewer's 2

59. Line 97: change to "size of DOM molecules..." R59. Sentence re-written as suggested by reviewer's 2

60. Line 98: I think the authors mean larger/greater molecules, not higher. R60. This text has been re-written to avoid confusion.

61. Lines 131-133: Please add references to this information about the study area. R61. References have been added as suggested

62. Line 136: should be changed to "...at that time of the year..." R62. Sentence has been changed as suggested

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63. Lines 162-164: Please add a reference to this statement. R63. References have been added as suggested

64. Line 165: what is meant by "...less noticeable..."? Less than what? R64. This text has been re-written to avoid confusion.

65. Line 219: I suggest moving "fifty-six" to Line 222 so it reads "Fifty-six samples were taken in total of which 21 were composed of..." R65. Sentence changed as suggested

66. Line 228: Please add a reference to the dominance of the phytoplankton in this particular lake. R66. References have been added as suggested

67. Line 311: if reference to figure 6d and 6e is made then SUVA should be mentioned first and then E2/E3 ratio to be consistent. Then you can say that it refers to those figures respectively. R67. Sentence modified as suggested

68. Line 315: mean value in table 2 for Keszthely basin is 9.66 not 9.67 as it says in the text. Which is correct? Please review. R68. Sentence modified to 9.66, corrected values

69. Line 317: Do you mean with increasing distance from Zala River? Sentence modified to avoid confusion R69. Yes, DOC concentrations slowly decreased with increasing distance from Zala River

70. Line 317: remove "in" before the word similarly. R70. Sentence changed as suggested

71. Line 346: Change to "there were more than ten orders..." R71. Sentence changed as suggested

Reference list: I have not checked the reference list.

72. Line 434: How does this statement connect with the data in this study: "previous studies have also found marked differences in the E2:E3 between natural waters..." Present the data from the study and then connect with what other studies have found.

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R72. Data from the study is shown in results, lines 320-321 and Table 3. Statement modified to avoid confusion. References added to support the statement.

73. Line446: Remove “in” after Lake Balaton. R73. Sentence changed as suggested

74. Line450: change “sensitive” to sensitivity. R74. Changed as suggested

75. Line456: Add: and, between the two ranges. R75. Changed as suggested

76. Line530: change to “new approaches are needed...” R76. Sentence modified as suggested

77. Tables 1 and 2: Is there a reason why values are stated as Max-Min instead of Min-Max? Consider changing to better fit with standard way of reporting such values. R77. Modified as suggested

78. Figure2: the lines connecting data points for aCDOM seem to connect in a strange way or to be disconnected. Please review and fix. Figure 9: add to legend what “DC” refers to, dark control? R78. Figure modified as suggested

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-329/bg-2016-329-AC2-supplement.pdf>

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