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Interactive comment on "A bio-optical model for remote sensing of Lena water" *by* H. Örek et al.

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Answers to questions and remarks by the reviewers

First and foremost, we would like to thank the two reviewers for their interest in and support for our study. We have read their comments and felt that they will really help to improve the manuscript considerably. Obviously, we are aware that in parts the MS is somewhat descriptive as remarked by the anonymous reviewer, but this reviewer also makes the point that data from this part of the world are few and far between. Having said this, we agree with his point and will strive to make the content more scientifically interesting.

In the following, we have tried to deal with all of the points made by the reviewers (in italics)

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1 Anonymous review:

Title will be changed to express that the manuscript does not describe a final bio-optical model of the Lena water because of the limitation in data, but provides a first contribution to such a model.

2 Review by Emmanuel Boss:

2.1 General remarks:

Figures partly no good quality (excel copy and paste):

Yes, of course Dr. Boss is correct. We apologize for this, this was a result of the time-pressure to submit the MS for the special issue, and of course the figures will be corrected and replaced.

Quality of English text

The text will be edited by a native English speaker (one of the co-authors)

Remark 1: 1. To develop a bio-optical model what one needs are mass specific optical parameters. These parameters should be highlighted, their variability computed, and compared to literature values (so that one can evaluate how 'anomalous' Lena river water may be). The actual range of values observed is much less interesting (as it could probably not be similarly generalized to other seasons and times). Comparing CDOM/DOM, cp/SPM, bp/SPM, cp/POC, ap(676) absorption height/Chl, ap/SPM etc' to literature values will be much more informative.

We agree, because of the short observation period, this paper can only contribute to a bio-optical model (s. also title change). Of interest are ranges, spectral properties and

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relationship between IOPs and mass concentrations, these will be provided. Further comparisons to literature will be added where possible.

Remark 2: Uncertainties in all derived quantities should be provided.

This is a bit tricky. We will add the uncertainties, where possible, but obviously the problem with derived qualities is always the error increase. Adding two quantities will yield a derived quantity with the standard deviation which is also the sum of the standard deviations, etc. In some cases the uncertainties of such derived quantities are thus meaningless.

Remark 3: The method of fitting the data to obtain spectral slopes should be provided (e.g. log-linear vs. non-linear). The appropriate one to use depends on knowledge of errors of spectra (e.g. relative or absolute). It is usually assumed that the errors are constants at each wavelength (and hence a non-linear fit is most appropriate).

We agree. In fact, a non-linear fit was used, but we admit that this was not very well explained. We will improve on this in the methods section.

Remark 4: Particulate carbon: is it POC or POC+PIC?

We apologize for this omission. Particulate carbon in this case was everything that ended up on our filters. Hence, it is the total of organic and inorganic carbon.

Remark 5: I don't see any data of in-situ fluorescence. As it been used? If not, remove mentioning them.

We measured Chlorophyll in situ by a special fluorometer, which will be added in the method chapter. (HPLC method is already given in the text. All chlorophyll-a values used are HPLC measurements).

Remark 6: Provide indication of scattering method used with ac-meter

Method to determine scattering coefficient from ac-meter measurements will be added.

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Remark 7: Units are often missing when spectral slopes values are provided.

This is an unfortunate omission. Obviously we will add the units where missing.

Remark 8: Humic substances are a sub fraction of DOM that is extracted following very specific protocols. Have you analyzed the DOM for this fraction? If not, avoid using this term.

The term 'humic substances' has been used here as a standard term to summarize all organic degradation products, will be clarified or changed in the text.

2.2 Comments by E. Boss in the manuscript (supplement.pdf)

2.2.1 Abstract

question/remark: could be inorganic <- Estapa

answer: this is true and likely, will be mentioned also later in main text incl. citation

2.2.2 Main text

2.2.3 page 4889, line 15 is this tested here?

answer: not for water but for land in particular methane emission, statement will be relaxed

2.2.4 page 489, line 23: how? :

answer: must be clarified, but the nature of DOC, such as humic or fulvic acid of which the absorption spectra are known

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2.2.5 page 489, line 4: do you mean chlorophyll fluorescence? Can you provide instrument used?

answer: yes, information about the instrument and measurement principle will be added.

2.2.6 page 4893, line 5: this depends on scattering method used. You should do a sensitivity analysis. Also, you could look at your CDOM spectra and see how well they look compared to Eq. (1) to give you a sense of your likely error.

answer: We remove this sentence, because we only put it to support our previous sentences about the blank corrections.

2.2.7 page 4894

2.2.8 line 8: did you do a non-linear fit or a log/linear fit. The result will be different. Best method is a nonlinear fit where the wavelengths are weighted by the magnitude of the uncertainty in each wavelength.

non linear fit, but not weighted, RMSE is always very low and r^2 is always around 0.98-0.99)

2.2.9 line 24: are you computing it for all contributors (water included) particles only or all but water? What do you expect readers to use it for?

answer: only particles, provides info about the dominant optical process for this spectral band or wavelength range

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2.2.10 page 4895

2.2.11 line 13: all wavelengths? Wouldn't water dominate in the NIR?

answer: should read the main source of absorption of water constituents

2.2.12 line 25: another way is to corroborate this is by looking at the 676nm peak and HPLC.

answer: good hint, this will be checked

2.2.13 page 4896, line 8: do you have uncertainty estimates for those slopes? Is +/-0.0001 implied by your significant digit the true uncertainty? Where are the units? (nm^-1)

answer: uncertainty estimates , only few data, so only ranges are provided but will be checked if more than this is possible, units will be added

2.2.14 line 18: you could use these to reconstitute absorption to get a sense of how negligible it likely is.

answer: will be quantified

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2.2.15 page 4897

2.2.16 line 15: I thought attenuation is dominated by particulate scattering.

answer: here only the variability of c is meant, but comparison with b will be checked

2.2.17 line 21: There is a lot of work on cp/mass (e.g. Hill et al., 2011) and bp/mass (Babin et al., 2003). Can you put your results in that context?

answer: a comparison will be added, chapter 4.5 has to be checked and revised

2.2.18 page 4898

2.2.19 line 3: DOC and humic substances are not the same. have you measured the humic fraction?

humic substances (not humic acids) is used here as a general term of organic degradation products, details are given in other publications by Kattner et al., but sentence will be reformulated to make this clear.

2.2.20 line 15: you mean not included in the statistics. By now you mentioned it 3 times in the publication. . . .

answer: has to be checked

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2.2.21 line 19: can you compare CDOM/DOC with the literature?

answer: will be checked

2.2.22 page 4899

2.2.23 line 6: Reference wavelength should not affect your spectral slope, only the value of the amplitude you obtain for the exponent.

answer: no, will change, because the exponential decrease is only an approximation of the absorption spectrum

2.2.24 line 17: units?

answer: units will be added

2.2.25 line 17 and 22: do you have any proof of that?

answer: this paragraph has to be checked and reformulated. We need to clarify the difference between main stream and arms particularly Kurunyag, because the slopes from the tributary Kurunyag are clearly different then the main stream which can only be explain by either composition or particle size. All corrections are done with same methodology.

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2.2.26 page 4900

2.2.27 line 1: I think you mean CDOM. Most DOC has no color.

answer: this is true, will be changed

2.2.28 line 7: Is this a speculation or do you have data to support this claim?

answer: only by visual observation during sampling

2.2.29 line 20: How does scattering/TSM compare with other studies (e.g. Babin et al., 2003)?

answer: will be added

2.2.30 page 4901

2.2.31 line 5: How do you know that the inorganic particles do not absorb (e.g. recent work by Estapa showing strong absorption due to iron oxides bound to particles)?

answer: this is true, but then you find absorption bands like at xxx nm, which we could not observe here, nevertheless in previous studies of tidal flat sediment we found also absorption bands around 670, which were clearly not due to chlorophyll.

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2.2.32 line 14: to establish such a model the mass specific coefficient need to be determined and compared.

answer: mass specific coefficient will be summarized and included here

2.2.33 page 4902

2.2.34 line 12: humic are a subset of DOM. Did you specifically measure humics?

answer no, but s. remarks to this question already above degradation products , will be clarified here again

2.2.35 line 13: This sentence suggest only DOM influenced the Secchi depth. Secchi depth is determined by diffused attenuation + beam attenuation. Since CDOM does not dominate the beam attenuation I do not think this statement is correct.

answer: during our observation period light attenuation was dominated by absorption by CDOM, this was also evident in the visual perception when the Secchi disc disappeared, which was different from attenuation by scattering material

2.2.36 page 4903

2.2.37 line 4: unit and value

answer; unit will be added and also comparison to values of other areas, value corresponds also to mean value of NOMAD data (which is around 0.016)

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2.2.38 line 7: it could be an artefact of the fitting method if based on a log-linear fit (e.g. Twardowski et al., 2004).

answer: fit was done by curve fitting, but check if exponent of adg was meant

2.2.39 line 12: How about fluorescence at 685nm (MODIS has a fluorescence channel)?

answer: we measured also reflectance spectra (not included in this paper), but no sign of fluorescence in the range 680 - 690, may be due to the high absorption in the blue, so that all light is used for photosynthesis, nevertheless also the absorption around 670 could be used (check reflectance spectra)

2.2.40 line 24: could look at 676nm line height/[chl] to get a sense of packaging by cells.

answer: will be checked

2.2.41 page 4904: units and comparisons

answer:units will be added and also comparison where possible

2.2.42 table 1, 3: any uncertainties in these values?

answer: not available in these papers (tbc)

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2.2.43 figure 4: if you do a semi-logarithmic fit you get a different answer than if you non-linearly fit an exponent to the data. It can be shown that such a fit assumes constant proportional errors (as opposed to constant absolute error assumed when fitting an exponent). This CAN introduce a spurious correlation of concentration and spectral slope.

answer: has to be checked, but curve fit was done non linear.

2.2.44 figure 8, fig 11: this number should be compared (at appropriate wavelength to literature values (e.g. review of Hill et al., 2011).

answer: will be added

2.2.45 figure 12: Given the short dynamic range, a linear fit should be attempted as well and slope compared to literature values

has been done, and could be added

2.2.46 figure 13, 14: Is this POC?

answer: no, but to be checked

2.2.47 figure 15: no grid lines in graphs, can you spell out 'y' and 'x'?

answer: will be changed, POC will be checked

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