Responses to Reviewer #2

Dear Reviewer,

Thank you very much for the extremely valuable critical comments on our work. Below we present our detailed point-by-point responses and the description of actions taken in regards to your comments. We believe that we have provided satisfactory explanations to your criticisms, and have made appropriate revisions in our paper.

Reviewer's comment:
General comments*: This paper is a continuation of a series of paper by the current authors on the parameterisation of the variability in phytoplankton absorption coefficients In the Baltic Sea. The analysis is extensive and thorough and is of some value for regional ocean colour remote sensing work.*

I have the following ***major concerns****:**1**) The manuscript falls short of drawing necessary conclusion from the results presented here. There is very limited discussion on natural sources of observed variability, how much of it is real and what ecosystem processes can be inferred from findings. For example: does the data suggest that an increased amount of photo-protective pigments influences the relationship in the summer months? And if not, why not?*

Author's response:
We would like to thank the Reviewer for noticing some of the positive aspects of our work. Regarding the major concern about the lack of necessary conclusions, we would like to point out that we tried to present only the most important observations and not to over-expand the work with other elements of a speculative nature. In our work we have documented that in the case of the Baltic Sea and the data collected during different periods of the year, one can see systematic differences when coefficients of classical one-component formulas for a_{ph} (phytoplankton absorption coefficient) are matched to the data from various months. Because these differences are systematic, we believe that is not likely that they are caused only by possible measurement errors, because the latter should have rather a random character. Additionally, the differences observed between monthly specific parameterization variants qualitatively resemble the ones that occur between various parameterizations given by different authors for different marine environments. As we write in the final paragraph of our paper: '*...This particular observation reminds us that all such parameterizations are always quite far-reaching simplifications of relationships occurring in nature. The variability of these relationships that we recorded throughout the year in the Baltic Sea seems to indicate that only the use of a much more elaborate mathematical apparatus, using a much larger number of variables describing the composition of pigments and other features of the phytoplankton population* (i.e. 'packaging effect')*, could further and more radically improve the accuracy of the spectral description of the light absorption coefficient...'*. But, as we know for practical purposes there is often a need to look for simplified solutions. As we have documented, the simple parameterization of a_{ph} as function of *Tchla* can be improved if the additional "degree of freedom" is added. We believe that it can be done either in a practical manner, by matching parameterizations to particular periods of the year, or alternatively by adding the additional variable, which would allow to get different estimated spectra of a_{ph} coefficient while the concentration of chlorophyll *a* remains the same. From our analyses it has emerged, that if an addition of only one variable is concerned, the best candidate seems to be the ratio of $\sum C_i$ to *Tchla*. But obviously we have considered different possibilities; different ratios between

pigments groups (*PSC*, *PPC*, *Tchlb* and *Tchlc*) and *Tchla* were analysed. However, we understand the curiosity expressed by the Reviewer, and to satisfy it, we have prepared additional drawings documenting the scenario when as an additional variable the ratio of photoprotective carotenoids to chlorophyll *a* is considered (see Figure R4). These additional results can be compared with Figure A1 from the new version of the manuscript (or with Figure 8 in an original manuscript). Such comparison indicates that the influence of *PPC/Tchla* on the examined relationship is noticeable, but it is statistically less significant than the effect parameterized by changes of $\sum C_i/Tchla$.

Author's changes in the manuscript:
In order to respond to the Reviewer's concern, in addition to the arguments presented above, we have also expanded the scope of information given in the Introduction section regarding the influence of different pigments composition on the phytoplankton absorption spectra. In regards of the conclusions, we believe that the most important of them (not speculative in nature) were already given in the last paragraph of the article.

Reviewer's comment:
2*) I find myself unable to judge how much of the presented finding are actually novel and how much is a re-iteration of previous papers by the authors on the same dataset. Please clarify the progression from one paper to the next and highlight the additional value of this paper. Also justify why the results presented here were not published in previous paper.*

Author's response:
As a first part of our answer let us quote a new, modified version of the third paragraph of the Introduction section: *'...In one of earlier work by our research team (Meler et al. 2017a) we have already provided initial version of power function parameterisation adjusted to the data collected in the southern Baltic Sea. However, these preliminary analyzes were mainly focused on the joint data from marine and lacustrine environments, were based on a much smaller set of data than the one we currently have, and the initial parameterization was provided only for all the data pulled together regardless of the time of acquisition. On the other hand, in other paper published lately we have been able also to identify significant differences in the absorption properties of Baltic phytoplankton at different times of the year (see Meler et al. 2016b). In another preliminary study, limited to just a single light wavelength of 440 nm, we have also demonstrated differences between the coefficients of the relevant simplified parameterizations when they have been tailored to data gathered at specific times of the year (see Meler et al. 2017b). It is in this context, therefore, that we have decided in the present paper to re-address the problem of determining practical forms of a simplified parameterization of the phytoplankton absorption coefficient appropriate to Baltic Sea conditions....'*
When the first work cited above (Meler et al 2017a) was originally prepared, only much smaller dataset was available to its authors. Only having an extended set of data allowed us this time to carry out analyzes taking in which variations occurring through the year could be taken into account. Please also note that in our current work we have followed suggestions given by the scientific community. We have decided to use currently preferred expression for the "beta factor" for the T-R method (according to Stramski et al. (2015)), and *Tchla* concentration data from HPLC analyses (and not, as before, according to the spectrophotometric measurements). In a series of our works we try to document our findings, but obviously this must be done in a gradual way, due to the limitations in the length of individual scientific articles.

Author's changes in the manuscript:

The third paragraph of Introduction has been modified.

Reviewer's comment:
***3**) The comparison of different parameterisations on this dataset does not add much value to the paper – especially if the performance is tested using the same dataset used for development.*

Author's response:
In the revised version of manuscript, we tried to clearly separate two issues. In the Section 3.3, we refer to estimation errors of our new formulas (obviously these errors are calculated at the same dataset as the one used for "training", and this is by no mean a validation of our formulas). And in the section 3.4 we presented the variability of coefficients between selected examples of parameterizations from literature and also the errors made when they are applied to our dataset.We believe that results presented in both of these sections, can be valuable to the interested reader, even if the 'classic' validation of our new formulas was not performed. All these results, in general, help to document the limitations of simple one- (or two-) component parameterizations.

Author's changes in the manuscript:
Sections 3.3 and 3.4 have been significantly modified

Reviewer's comment:
***4**) The final recommendation as to which parameterization should be used (and under which conditions) is not clear. Quantify the benefit of the new parameterization and weigh it against processing time/costs.*

Author's response:
On the basis of our analysis of estimation errors, it can be expected that in the case of data sets from different periods of the year, on average only small differences between the various versions of parameterization will be noticed. However, when the information on the $\sum C_i/Tchla$ is available, it is, in our opinion, advisable to try using the two-component version, in order to get a slightly better accuracy of estimating of a_{ph} in the most-important ranges around 440 and 675 nm absorption peaks. In the case of analyzing the data from selected months only, the differences between parameterizations may be much more pronounced. In practice the properly selected monthly parameterization variants may be the best solution. The qualitative benefit of two-component parameterization and "dynamically selected" monthly parameterization is the fact that they allowed for different absorption magnitudes and shapes for the same chlorophyll *a* concentrations. In our opinion the differences between processing time is not an important issue in resolving "forward" problems (finding a_{ph}, when the *Tchla* or *Tchla* and $\sum C_i$ are known). Obviously differences will arise when trying to solve "inverse" problems (estimating *Tchla* or *Tchla* and $\sum C_i$ when the absorption spectrum is known).

Author's changes in the manuscript:
We have modified section 3.3 and clarified a recommendation that has been given in the final section of manuscript.

Reviewer's comment:
***5**) Additionally, the language requires major revision (preferably from a native speaker) which should aim to shorten the article significantly and improve readability & understandability.*

Author's response:

We have rearranged some sections of the text to improve readability and understandability. The revised manuscript has been also corrected by a native speaker.

Author's changes in the manuscript:
See various changes marked in colour throughout the revised version of manuscript.

Specific comments:

Reviewer's comment:
1) Explain the selection criteria for literature chosen for comparison. Can you use a more systematic approach to select a small (max. 5!) number of parameterizations from the hundreds available in the literature (e.g. only choose chase 2 waters or compare 2 case1 and 2 case 2 water studies)? McKee et al. (2014), for example, did some analysis on the NOMAD data set which includes a variety of different water and geographic locations.

Author's response:
In the first version of the manuscript, literature items were selected for comparisons, which concerned both case 1 waters and case 2 waters from different marine and lacustrine environments. Indication of the results achieved in lake waters was aimed at referring to an earlier work by Meler et al. (2017a), in which one-component parameterization was presented for the combined data set acquired in the southern part of the Baltic Sea and in lakes in the Polish coastal zone, pointing to their similar optical properties.
In the current version of the manuscript, as suggested by both Reviewers, the selection of literature items for comparison has been modified and limited. In addition to the classic work by Bricaud at al. (1995), we are now referring mainly to five other examples documenting the diversity of results obtained in different marine environments (Stramska et al. (2003), Staehr and Markager (2004), Matsuoka et al. (2007), Nima et al. (2016), Churilova et al. (2017))

Author's changes in the manuscript:
Appropriate changes were made in the text and in Figures 4, 6 and 9.

Reviewer's comment:
2) Revise terminology throughout article: - Use 'case 2 waters' rather than 'case 2' – Use 'at short wavelengths/in blue spectral region' instead of 'short-wave part of spectrum' -In my opinion, using 'absorption' rather than 'light absorption' is sufficient and would improve readability.

Author's response:
Corrected in accordance with the reviewer's suggestion.

Reviewer's comment:
3) p. 4. Line 9: 'Because of 10 weather- and sea-state-related limitations, the proportion of data collected in open water regions exceeds ca 30% only for data collected in February, March, May, September and October (see Table 1).' – This is an example where the text can be shortened significantly: 'The amount of data collected in open waters was limited due to adverse weather (< 30% for the majority of months).'

Author's response:
Corrected.

Reviewer's comment:
4) Methods, Table 1: Were any spectra excluded from the data – if yes, what were the selection/data quality control criteria?

Author's response:
The data set was first subjected to a preliminary qualitative control - data for which a set of associated data was missing was removed, non-physical data was removed, e.g. negative absorbance values. Secondly, after the parameterization, estimation errors were calculated and the so-called outliers - values clearly differing from the typical (laying outside the limits of +/- 3 SD) were removed.

Reviewer's comment:
5) p.5, paragraph 1: Justify why you did not correct for scattering offsets by forcing the spectra through zero, in red/NIR? Are you aware of the systematic errors your choice potentially introduces to the data. Lefering et al. 2016 for detailed analysis.

Author's response:
In general, we did not use correction due to the scattering effect, because in the spectrophotometric measurements we use the so-called T-R (transmission-reflectance) method according to Tassan and Ferrari (1999, 2002). It is generally accepted that such a correction is not needed when using this particular method. Our final data was corrected, however, by forcing final values in the range of 740-750 nm to be close to zero. The methodical section has been improved and supplemented in this matter.

Reviewer's comment:
6) p. 6, l. 19: How did you assess reproducibility of samples? Did you measure any replicates at all, if yes how?

Author's response:
In the collected data set, due to logistic limitations, generally no measurements were made on multiple samples. However, the separate tests has revealed that the average uncertainty of a_{ph} measurements due to subsampling was 9.1% (± 1.5%, SD). The methodological section has been supplemented with this information.

Reviewer's comment:
7) p. 6, ll.22-24: Moving averages can lead to a reduction of chl a absorption in the red. Did you observe this effect? Consider lo-ess smoothing.

Author's response:
The procedure of smoothing absorption spectra carried out by us does not significantly reduce the value of light absorption coefficients in any band. This information was added to the text of the work.

Reviewer's comment:
8) p. 7, l. 11: Highlight that you calculate the RMSE/standard deviation on relative errors. I would be interested to see the range absolute errors as well.

Author's response:
The data set analyzed in the work covers a wide range of variability, i.e. almost three orders of magnitude. We believe that in this situation, the statistics of relative error and, above all, the

statistics on logarithmic values should be used for analyzes (see the last paragraph of Methods Section). However, to satisfy the Reviewer's curiosity, we present the version of Table 3 supplemented with the arithmetic statistics of absolute error (see Table R1).

Reviewer's comment:
9) How is the standard error factor interpreted? What is the ideal, what a reasonable value?

Author's response:
The standard error factor allows to calculate the range of statistical errors according to logarithmic statistics. As in standard arithmetic statistics we can calculate the error range by adding or subtracting the value of standard deviation (SD), in logarithmic statistics similar range can by calculated by multiplying/dividing by the standard error factor (x)). The ideal value is the case when x = 1, because it would mean that all approximate values accurately reflect empirical values. When the target physical quantities change for about three orders of magnitude, we believe that standard error factors of the order of 1.5 and less should be treated as reasonable (x=1.5 means the main range of variability is between 67% and 150% of original value, and the statistical error range is between -33% and +50%).

Reviewer's comment:
10) p.7, l.28: I understand that low measurement sensitivity can cause issues at blue/UV wavelengths. How do you explain the observed artefacts at 550 – 650 nm.

Author's response:
This is the range where absorption takes small values compared to other bands, they are close to noise, and the smoothing procedure is not fully effective there. In this spectral area, absorption by phycobilins is also possible.

Reviewer's comment:
11) p.7, ll. 26-29: Use neutral and objective language (here and throughout document)!For example, avoid expressions like 'undesirable artefacts'.

Author's response:
Corrected across the manuscript in accordance with the reviewer's suggestion.

Reviewer's comment:
12) Overall the absorption data appears to be of low quality. I know that it's not possible to repeat the measurements with an improved protocol but a thorough and more detailed analysis of data quality is required. Do you have any information with regards to bleach contamination resulting in low absorption values < 440 nm?

Author's response:
We are aware of the fact that the data obtained by spectrophotometric method can be generally characterized with inaccuracies, associated with many factors. We tried different techniques to avoid such inaccuracies. The use of bleaching agent to obtain spectra of light absorption coefficients by non-algal particles has been 'neutralized' by rinsing bleached filters with clean particle-free seawater. It is possible, however, that in some cases a slight amount of the suspension collected on the filter could also be rinsed during the flushing, resulting in the a_{NAP} measurement being underestimated. Practically, we tried to correct for these possible undesirable effects by correcting to zero the final a_{ph} values in the 740-750 nm range. In this matter, we have completed the methodical section of the manuscript. The effectiveness of such

procedure was checked on blank filters. The average relative difference were not higher than 2.8%.

Reviewer's comment:
13) The absorption is expected to vary several orders of magnitude across all wavelengths and samples. This is not a new result. Interesting would be what the implications for your error analysis are? Do you see larger absolute errors for low absorption or do measurement errors increase linearly with strength of absorption?

Author's response:
Please note that in our measurements we used filter pad technique and different volumes of water were filtered for different samples (see the supplemented methods section). In this sense, the signal from weakly absorbing samples was always 'amplified'. The proportions between absorption signal at the blue peak to the instrument noise, being a main source of uncertainty, was kept often at similar level, regardless of the fact the absolute values of absorption in original water samples changed over almost three orders of magnitude. For that reason the relative uncertainty due to instrument noise is at the similar level, regardless of magnitude of absorption of phytoplankton in seawater.

Reviewer's comment:
14) p.9, l. 14: Use neutral and objective language (here and throughout document)! Another example: 'fairly well'.

Author's response:
Corrected across the manuscript in accordance with the reviewer's suggestion.

Reviewer's comment:
15) p. 11, ll. 4-5: Delete sentence, unnecessary self-citation at this point: 'We ourselves had already reported similar ranges of variability earlier (see Wozniak et al. 2011,Meler e t al. 2016b, 5 2017a and b).'

Author's response:
Deleted.

Reviewer's comment:
16) Section 3.2.1: What happens if E(lambda) > 1?

Author's response:
E> 1 means the statistically observed increase in a^*_{ph} with increasing chlorophyll. It has been explained in the revised manuscript text.

Reviewer's comment:
17) Section 3.2.1: How can the annual variability in A be explained? Can it be linked to extent of packaging effect or the amount of photo-protective vs. photosynthetic pigments?

Author's response:
The general relationships between absorption and the size of phytoplankton cells, intracellular pigments concentration and pigments composition are mentioned in the Introduction section (which was also supplemented). The data collected by us does not contain direct information about particle sizes. However, the analyzes carried out show that the influence of the proportion

of accessory pigments to *Tchla* is significant, and may be used to 'construct' the two-component parameterization. An example of similar analyzes concerning photoprotective pigments as separate group is presented in the additional Figure R4. I the case of our data, however, it turned out that a better candidate for an additional variable is $\Sigma C_i/Tchla$ and not *PPC/Tchla*.

Reviewer's comment:
18) Section 3.2.1: What can be inferred from your analysis of R2? Does is correlate with any known ecosystem dynamics? Considerably shorting the paragraph unless you justify its value.

Author's response:
R^2 is only a parameter initially describing the quality of parameterization when matched to the measured data.

Reviewer's comment:
19) P.12 l. 8: Do you mean '690 – 700 nm'?

Author's response:
In the new version of the manuscript, this fragment has been modified.

Reviewer's comment:
20) Section 3.2.1: What do you derive from the observed differences in performance compared to Bricaud's parameterisation? Why do you observe less variability and what does that mean for the ecosystem?

Author's response:
The following sentence has been added to the text: '...These differences indicate that the combined influence of the packaging effect and the decrease in relative accessory pigment concentrations manifests itself differently in our Baltic Sea dataset than in the original oceanic dataset of Bricaud et al. (1995)....'

Reviewer's comment:
21) Please explain the metric of the colour index in more detail. What the reasons for the observed flattening (drop in colour index) of the absorption spectrum? Are you expecting a certain magnitude in your colour index drop and why?

Author's response:
In the Introduction section, the sentence has been modified / added: '...*Another important factor, where absorption by phytoplankton is concerned, is how densely the strongly light-absorbing pigments are 'packed' within the internal structures of individual cells (the so-called 'packaging effect', see e.g. Morel and Bricaud 1981, 1986). Increasing intracellular pigment concentrations and cell size flatten the real absorption spectra compared to what one may expect from a simple addition of the absorption coefficients of individual pigments*....'
In section 3.2.1, the sentence is added: '...*As a simplified measure of spectra 'flattening', one can analyse, for example, the changes in the ratio of* $a_{ph}(440)$ *to* $a_{ph}(675)$*: this ratio is sometimes referred to as the 'colour' or 'pigment' index (see e.g. Woźniak and Ostrowska 1990 a and b; see also Bricaud et al. 1995)*....'

Reviewer's comment:
22) p. 18: Shorten paragraph. Only highlight key finding shown in Table and consider deleting/rephrasing the following sentences: 'We have carried out such an analysis, but we do

not present its detailed results due to the fact that they are very extensive. Here we limit ourselves only to stating that the use of general or monthly parameterizations for individual months 10 has little effect on the level of statistical error according to logarithmic statistics, although it may have, as generally expected, a very significant impact on the level of systematic error.'

Author's response:
The paragraph has been reformulated. Tables 3 and 4 are now discussed in one paragraph entitled: 3.3 Estimation errors of the different variants of parameterizations.

Reviewer's comment:
23) Section 3.2.2 The use and exchange of a_ph,cal/a_ph,m and a_ph in this section is confusing. What information can be gained from a_ph,cal/a_ph,m and how can the ratio be related to a_ph in this context?

Author's response:
These are only mathematical details - at the request of Reviewer #1 they are now moved to the new Appendix 2; ratio of a_{ph} calculated to a_{ph} measured was used to determine the functional form of the "patch" for equation and 3.a and allowed to find the final form of equation 4.

Reviewer's comment:
24) Fig. 8 (e) & (f): Justify the use of an exponential over a linear fit.

Author's response:
The analyzes were carried out on logarithmic values, therefore the fits we found in the logarithmic space are linear fits.

Reviewer's comment:
25) p. 27, ll. 1-14: There is limited value in the comparison with other parameterisation using the same dataset (see comment above).

Author's response:
Comparison with selected examples of parameterizations from the literature was only done for illustrative purposes. In no way was it an attempt to validate our results (obviously, for such a purpose we would need to acquire a separate dataset, not used in parameterization 'training')). In new Figure 9 (simplified version of original Figure 12) we only wanted to document that using different literature parameterization on our dataset is often burdened with a large either positive or negative systematic errors and with statistical errors often higher, then errors which we were able to achieve with our new parameterizations matched to our data. (please, see also our answer to comment number 2 of Reviewer#1 and an additional Figure R1).

Reviewer's comment:
26) p. 27, ll. 21 – end: The conclusion of superior performance based on the standard error factor cannot be justified. 'If we take into account both systematic and statistical errors, the superiority of the new parameterizations developed specifically for Baltic Sea conditions is undisputed.' Poor scientific language!

Author's response:
Changed; the final sentence of the last paragraph of section 3.4 is now as follows: '*(...) But since for the total estimation accuracy the contributions of both systematic and statistical errors have*

to be taken into account, one can expect that overall, none of the literature examples can attain the accuracy that we achieved by matching our new parameterizations to our own dataset....'

Reviewer's comment:
27) p. 29, 32: Delete sentences: 'Practical remote sensing algorithms are a good example in this respect. Their task is often to solve complicated "reverse" problems. Starting with measurements of sea colour, such algorithms, through intermediate steps during which different inherent optical properties of seawater (including light absorption)are retrieved, should yield basic features of different seawater components in the final stage of their operation. One such feature may be the concentration of chlorophyll a, which is often treated as a practical measure of the biomass of live phytoplankton contained in water. In our opinion the parameterizations presented in this work could be used in practice for such purposes.'

Author's response:
That sentence has been deleted.

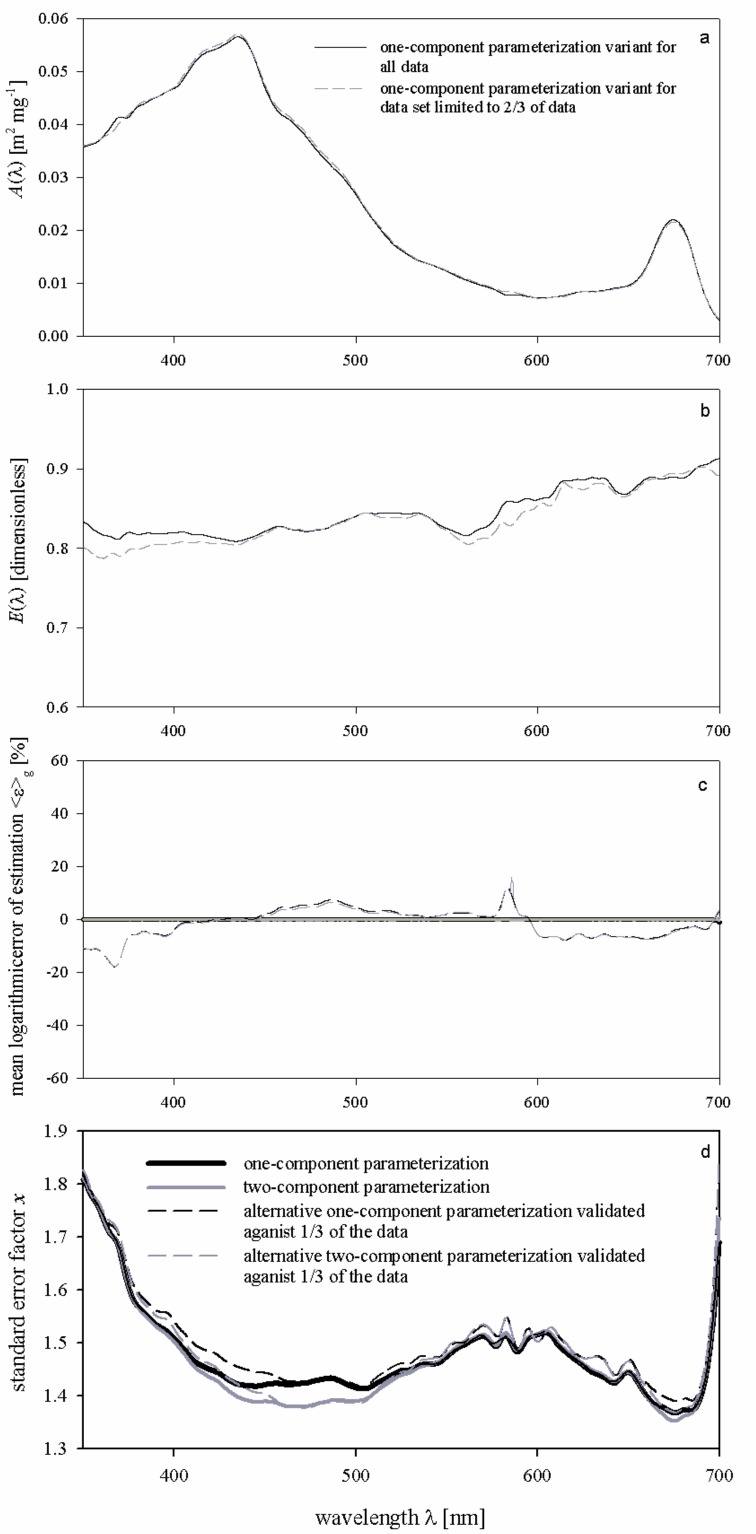

Figure R1. (a) and (b): Comparison of coefficients *A* and *E* of the one-component parameterization: for original variant matched to all data, and for the alternative variant matched to the data set limited to 2/3 of available data; (c) and (d) comparison of the main characteristics of the estimation error logarithmic statistics (mean logarithmic estimation error and standard error factor) calculated for original variants of one- and two-component parameterizations (all data used for training/all data used for error calculation) and for alternative variants validated against 1/3 of data.

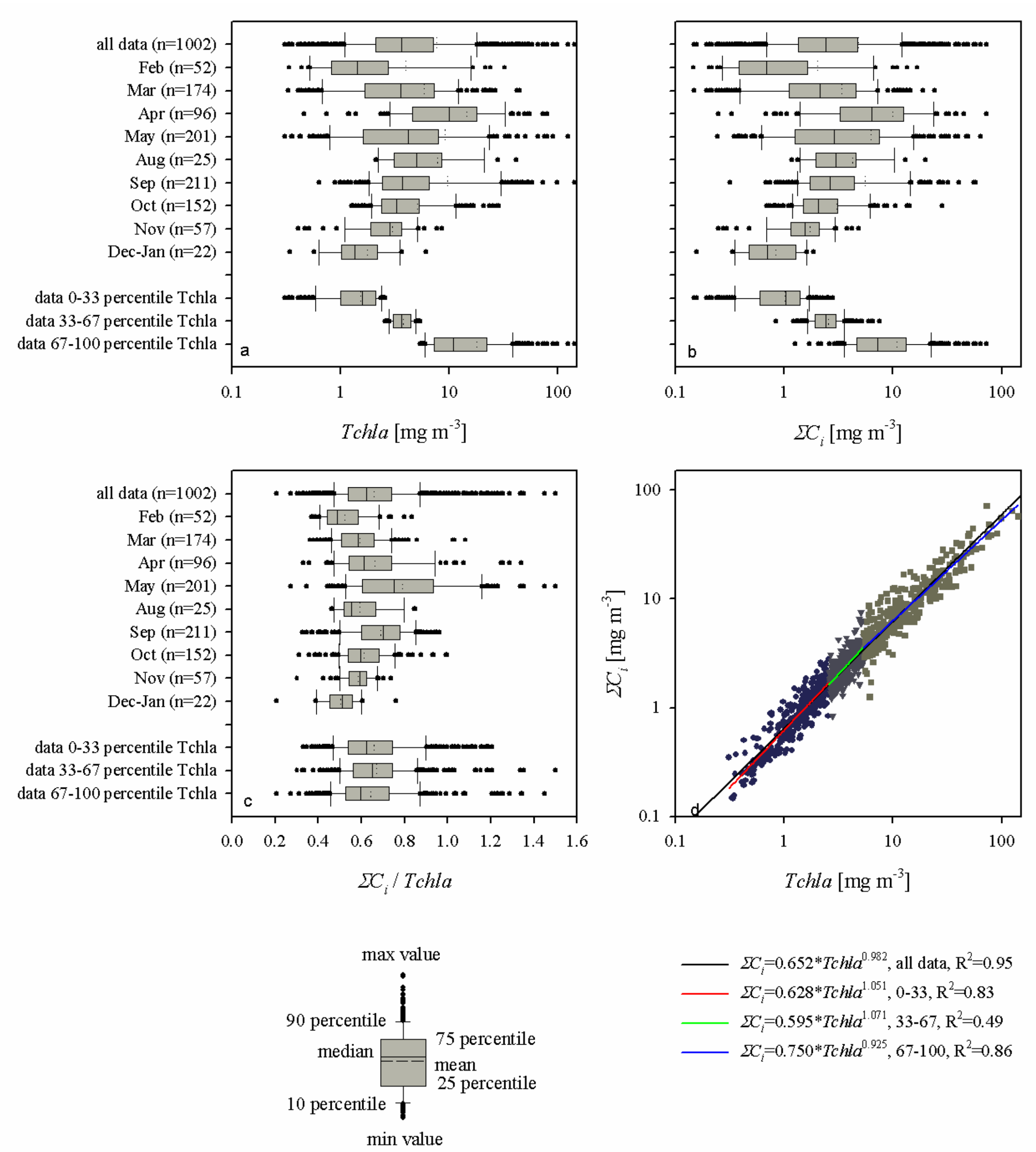

Figure R2. (*extended variant of Figure 3 from the revised manuscript*). (a): Box plot presenting the range of variation of chlorophyll *a* concentration (*Tchla*) for all the data analysed, for each sampling month, and for three selected ranges of *Tchla*; (b): as (a) but showing the sum of accessory pigments to the chlorophyll *a* concentration (ΣC_i); (c): as (a) but showing the ratio of the sum of accessory pigments to the chlorophyll *a* concentration ($\Sigma C_i/Tchla$); (d): graph illustrating the relationship between ΣC_i and *Tchla* – solid lines represent simple functional approximations of the relationship (the equations are given below the panel).

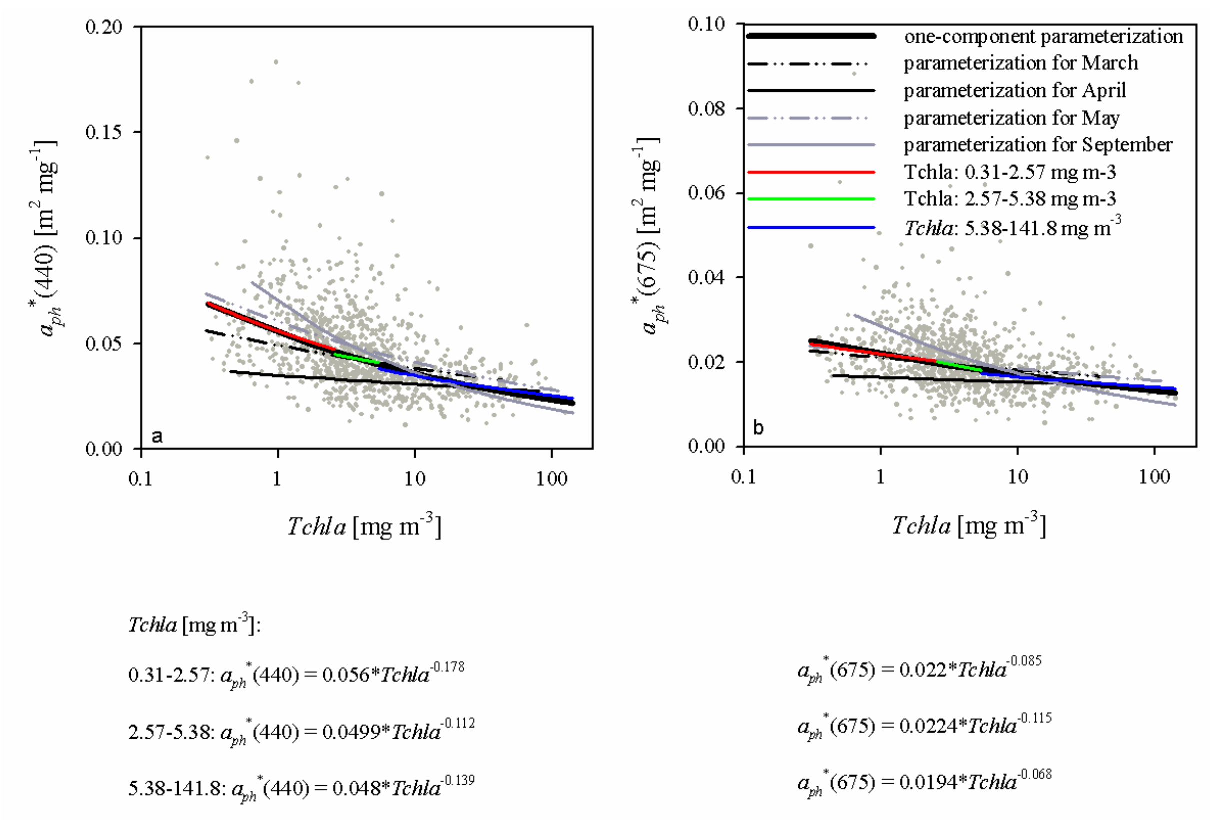

Figure R3. (*alternative to Figure 6a and b from the revised manuscript*) (a): Relationship between coefficient $a^*_{ph}(440)$ and the chlorophyll *a* concentration *Tchla* and its functional approximations determined in this study for all the data analysed, for selected sampling months, and for limited ranges of *Tchla* (see the legend to panel b; equations are given below the panel); (b): as (a) but for $a^*_{ph}(675)$. The grey dots on each panel represent individual data points from our database.

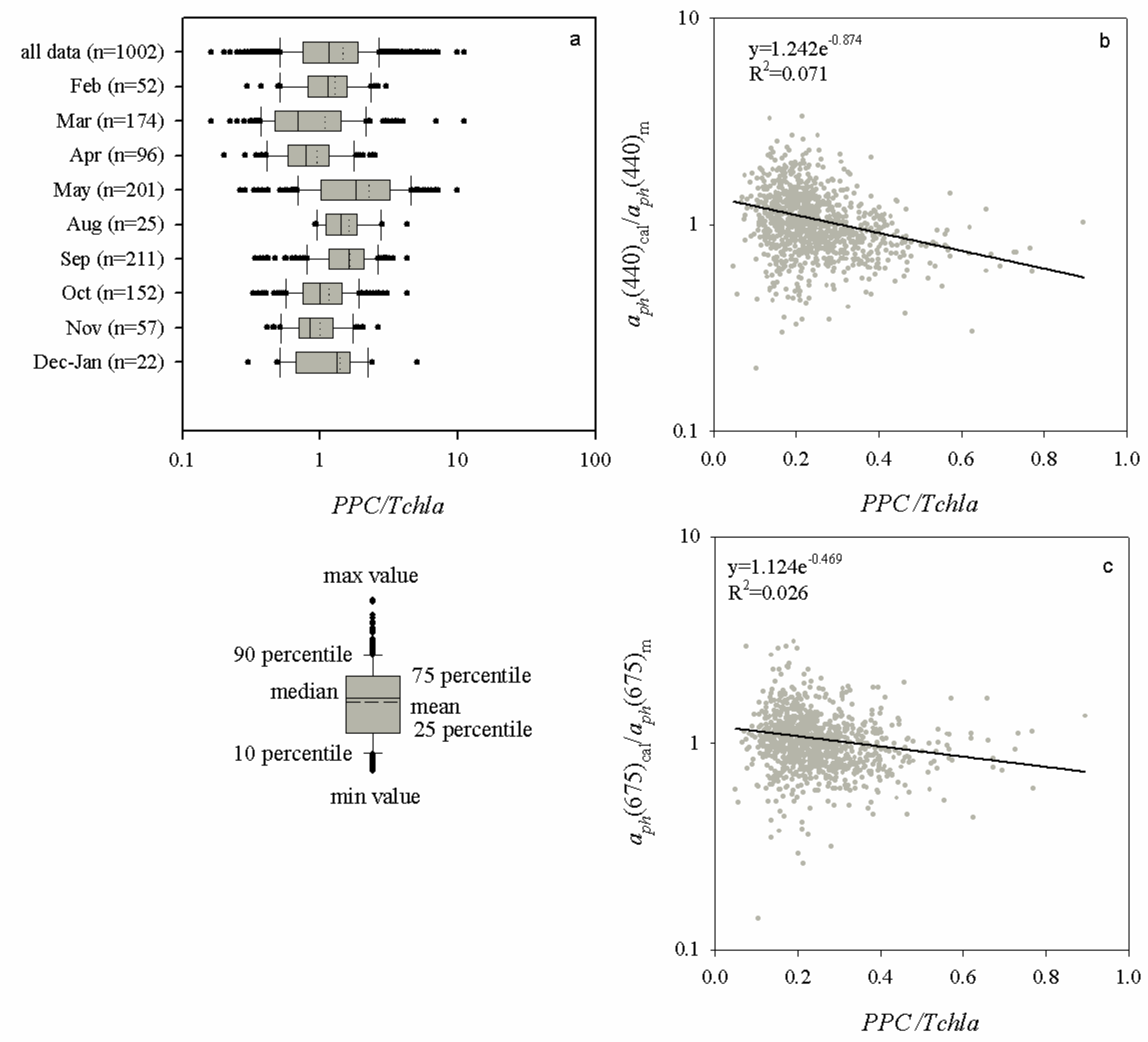

Figure R4. (a): Box plot presenting the range of variation of the ratio of the photoprotective carotenoids to chlorophyll *a* concentration (*PPC*/*Tchla*) for all the data analysed, and for each sampling month; (b) and (c) relations between the ratio $a_{ph}(\lambda)_{cal}/a_{ph}(\lambda)_{m}$ and the pigment concentration ratio *PPC*/*Tchla*, and their functional approximations (the equations are given in the panels).

Table R1. Arithmetic statistics of estimation absolute errors* of coefficient $a_{ph}(\lambda)$ in selected spectral bands when the different variants of the parameterization derived in this study were applied to the entire dataset (n= 1002). The calculated values are given for three scenarios: when the general variant of the one-component parameterization was used; when variants specific to individual months were chosen (the first alternative value is given in parentheses); and when the two-component parameterization was used (the second alternative value is given in parentheses).

λ [nm]	absolute systematic error $\langle\varepsilon_a\rangle[m^{-1}]$	absolute statistical error $\sigma_{\varepsilon_a}\,[m^{-1}]$
350	-0.044 (-0.037; -0.044)	0.22 (0.21; 0.22)
400	-0.025 (-0.023; -0.027)	0.17 (0.16; 0.18)
440	-0.019 (-0.017; -0.022)	0.15 (0.14; 0.15)
500	-0.009 (-0.008; -0.010)	0.09 (0.08; 0.07)
550	-0.005 (-0.005; -0.005)	0.05 (0 04; 0.05)
600	-0.005 (-0.005; -0.005)	0.03 (0.03; 0.03)
675	-0.007 (-0.007; -0.009)	0.06 (0.06; 0.06)
690	-0.005 (-0.004; -0.005)	0.04 (0.04; 0.04)
700	-0.003 (-0.003; -0.003)	0.02 (0.02; 0.02)

***) Arithmetic statistics of the absolute error:**

- mean of the absolute error (representing the systematic error according to arithmetic statistics):

$\langle\varepsilon'\rangle = N^{-1}\sum_{i=1}^{N}\varepsilon'_i$, where $\varepsilon' = P_i - O_i$, O_i - observed/measured values, P_i - predicted/estimated values

- the standard deviation of the absolute error (representing the statistical error according to arithmetic statistics):

$$\sigma_{\varepsilon'} = \sqrt{\frac{1}{N}\left(\sum_{i=1}^{N}(\varepsilon'_i - \langle\varepsilon'\rangle)^2\right)}$$