Response to reviewers

MS essd-2021-466
"Optical and biogeochemical properties of Belgian inland and coastal waters"

Reviewer 1 (Bing Han)

This paper provides a database of coincident optical and biogeochemical parameters, which should be useful in a variety of applications, e.g., detection of algal blooms, ocean color algorithm development and validation. It should be especially helpful in coastal and inland waters.

This paper gives detail description of methodology and technique of acquiring and processing in-situ measurements which are state-of-art. Moreover, a certain uncertainty information are also appended. Thus it should be of good quality and well referenced.

We appreciate the reviewer for the comments, suggestions, and perspective of the impact of our work.

Yet some improvement could be made regarding to:

1) Figure index and reference should be in the order following its appearance.

We appreciate the observation. The figures now are called in correct order.

2) The sentence in line 27 begining with 'The PONDER project' is not correct in writing.

The phrase was changed to: "The PONDER project (BELSPO SR/00/325) focused on developing tools for spaceborne remote sensing of inland water systems using high spatial resolution (≤ 30 m) sensors. During the course of the project, global coverage and open access data for high spatial resolution sensors was only available for multispectral missions."

3) In line 286, 'due' should be 'due to'.

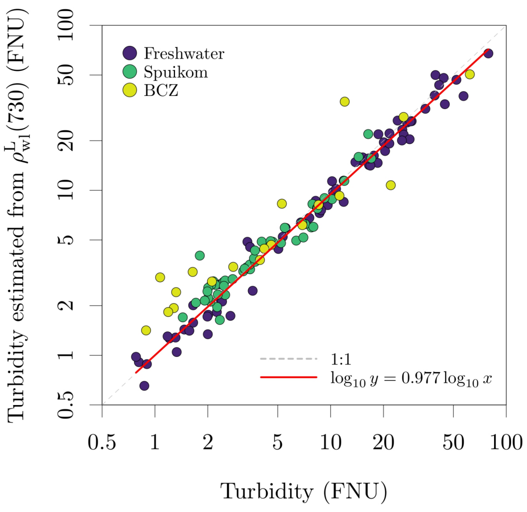
Changed accordingly.

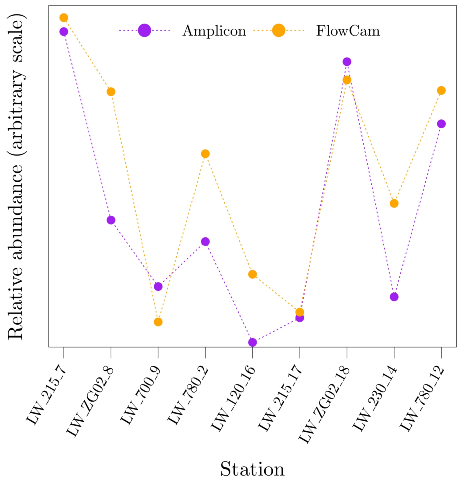
4) Method of interpolation mentioned in line 374 should be indicated explicitly.

A qualification was added to inform that it was a linear interpolation.

5) Labels of axes in Figure 9 and Figure 14 are not correctly spelled.

The labels are correct in the original figures, though the preprint pdf has blanks covering part of the text. This likely happened during pdf generation by the *production team. We will be vigilant that the proofs are corrected for such errors. The original plots are reproduced below.*





6) 'Constants' does not appear to be necessary because they are widely accepted notations. Also, generally accepted notations are highly recommended, e.g., Edn(0+) could be replaced by Ed(0+) or Es.

Though those constants are widely known and accepted, we consider good practice to indicate all symbols used in the manuscript. $E_{dn}(0^+)$ was changed to $E_d(0^+)$.

7) .kml file is not contained in the data directory as described in 'README.txt'.

The kml file is now added.

8) Station names in various data files (.csv or .xlsx) are not exactly same. For example, this in flow_cam data file is sightly different.

We appreciate noting the detail. Two differences were found:

(1) For the LifeWatch campaigns stations between the .csv and .xlsx files. The naming of the stations in the .xlsx files was updated to follow the same names as in the .csv files.

(2) The station on the Leuven-Dijle canal were reported with the informal name "Zenne" in the csv file bgc_bottom.csv. The names were updated to "LeuvenDijle" in order to conform to the other files.

9) Number of measurements should be mentioned where SD (standard deviation, I think) appears.

The description of the "SD" acronym was added to the README.txt file. An additional column with the number of data points was added to the files.

10) Number of stations in iop_ad_meas is much more than in other data files. Please double check.

Indeed, additional data was appended to the file. The extra rows were removed.

11) substrate data should include sampling location, data and time if possible.

The sediment data names include the stations where they were sampled (e.g., "sediment_SP_39") and location and time for the stations are available from the ancillary files. The floating biofilm were samples of opportunity and do not correspond to any station. The manuscript describes that those were acquired in the Spuikom in July 2018. The data for macroalgae represent averages of several samples. The manuscript describes that the samples were acquired in the Spuikom lagoon between 2017 and 2018. No change was made.

12) If possible, relationships/inter-comparison among various parameters could be provided, though figures 9, 11 and 14 already illustrated some.

In addition to those commented by the reviewer, figure 5 presents the comparison between in vivo pigment absorption and chlorophyll a concentration, and figure 6 presents the comparison between LISST VSF and spectrophotometer measurements of beam attenuation. The objective of Figs. 5, 6, 9, 11 and 14 is to show consistency of the different data types. We have limited that to the most clear/objective and relevant comparisons. We note that the manuscript already contains 15 figures and the supplementary material contains another 10 figures (one new figure was added to the supplementary materials showing the relation between turbidity and Secchi disck depth, in response to Reviewer 3 comments). Since all data is made available, the user of the dataset can plot other relations of interest.

Reviewer 2 (Arnold Dekker)

This is a worthwhile paper with interesting data well worth publishing. Its focus on inland and near coastal waters is needed. Excellent that this data set is published on Pangea. Perhaps also submit to LIMNADES?

We appreciate the time and suggestions provided with this review. This data is also planned to be submitted to LIMNADES once the data paper is published. The data submission to Pangaea is under review and not yet final.

Overall comments:

Title: as this paper does not over all Belgian waters but a subset from the middle (Mechelen) to the west of the country I would change the title to e.g. Optical and biogeochemical properties of "representative" or "a selection" or "Flanders?" representative western Belgian inland and coastal waters.

Thank you for your suggestion. Indeed the title does not appropriately represents the content and we appreciate the suggestion. The title was changed to "Optical and biogeochemical properties of diverse Belgian inland and coastal waters". The same *modification was added to the title of the PANGAEA dataset: "Dataset of optical and biogeochemical properties of diverse Belgian inland and coastal waters".*

Are you sure attenuation and turbidity fall under inherent optical properties? I don't agree as these measurements do not meet the definitions of IOPs. If you disagree please provide a reference such as from Kirk 2011 etc.

We have added the qualifier "beam" to all occurrences of "attenuation" to improve clarity and reduce possible confusion with the diffuse attenuation coefficient. As defined by Preseindorfer (1976) and retained to current date, IOPs are quantities that are independent of the "polydirectional light field" or "radiance distribution" (quoted, as it is an outdated concept not inline with modern physics; cf. Mishchenko, 2010). Per its definition (e.g., Mobley, 1994), the beam attenuation coefficient quantifies the attenuation of electromagnetic energy along a single linear propagation path (elemental interval of directions) over an elemental distance. Since it concerns an elemental interval of directions, it qualifies as an IOP. The challenge of the beam attenuation coefficient is not in this definition, but in its measurement, since: (1) any sensor has a finite acceptance angle and (2) the analyst has to consider the risk of multiple scattering depending on pathlength and particle load (Boss et al., 2009a). Similarly, turbidity is defined as the signal generated in a specific direction (elemental range of directions centered at 90º of the incident beam) by scattering of a near-infrared incident beam (elemental range of wavelengths centered at 860 nm; ISO 7027:1999; Dogliotti et al, 2015). Therefore, it corresponds to a specific scattering direction of the Mueller matrix. It is not, however, specifically defined over an elemental distance, but instead normalized by reference to a chemical standard. As with the beam attenuation coefficient, the challenge is not in the definition, but in practical measurements and for similar reasons. One aspect that helps the confusion about turbidity is a consequence of the operational definition in units of equivalence to solutions of Formazin. However, if the the volume scattering function of Formazin solutions at 90º and 860 nm would be published, it would be possible to convert FNU values to physical units of m^{-1} sr^{-1}. An interesting discussion on the tiopic of turbidity as an IOP was presented by Boss et al. (2009b). We have added the definition of turbidity as: "Turbidity, defined as the side-scattering at 860 nm relative to Formazin standards (ISO 7027:1999; Dogliotti et al., 2015), was measured in discrete samples with a portable turbidimeter (2100P ISO, HACH)."

Boss, E. S.; Slade, W. H.; Behrenfeld, M. J.; Dall'Olmo, G. 2009a. Acceptance angle effects on the beam attenuation in the ocean. Optics Express 17, 3, 1535-1550. DOI: 10.1364/OE.17.001535

Boss, E.; Taylor, L.; Gilbert, S;, Gundersen, K.; Hawley, N.; Janzen, C.; Johengen, T.; Purcell, H.; Robertson, C.; Schar, D. W. H.; Smith, G. J.; Tamburri, M. N. 2009b. Comparison of inherent optical properties as a surrogate for particulate matter concentration in coastal waters. Limnology and Oceanography: Methods 7, 803-810. DOI: 10.4319/lom.2009.7.803

Dogliotti, A. I.; Ruddick, K. G.; Nechad, B.; Doxaran, D.; Knaeps, E. 2015. A single algorithm to retrieve turbidity from remotely-sensed data in all coastal and estuarine waters. Remote Sensing of Environment 156, 157-168. DOI: 10.1016/j.rse.2014.09.020

ISO 7027:1999: Water quality – Determination of turbidity, Standard, International Organization for Standardization, Geneva, CH, 1999.

Preseindorfer, R. W. 1976. Hydrology Optics. Volume I. U.S. Department of Commerce, National Oceanic and Atmospheric Administration (NOAA).

Sometimes the word from is spelled as form-please check throughout the text

Corrected accordingly.

Specific comments:

Introduction: …With little representation of inland waters: what about the Limnades publicly available dataset with over 1500 submissions?

We consider the LIMNADES to be a remarkable collection of inland water data. However, currently not all dataset is open. A survey of the statistics on the LIMNADES (as of April 8, 2022) shows that a smaller fraction of the dataset, 750 measurements (from a total of 39,794), have license categories "A" or "B", which cover open data. Though the LIMNADES presents categories "C" and "D" as open, the data is only available after the owner of the datatset agrees to share with a particular user. We specifically refer to freely available datasets in line 18 as we believe that it is essential for community advance in algorithm development and evaluation. We have however specifically mentioned SeaBASS and LIMNADES as:"Though the data gathered in the last 50 years provide a large collection of conditions across a diverse set of environments, three major caveats are observed in the freely accessible datasets (e.g., SeaBASS at https://seabass.gsfc.nasa.gov/, and license categories A and B of LIMNADES at https://limnades.stir.ac.uk/):"

L29-30 high spatial resolution (30 m) sensors, of which current global coverage and open access data is only available for multispectral missions: PRISMA and DESIS have some open access data…. as will the recently launched EnMap.

We note that the phrase also includes "global coverage" as a criteria, which excludes PRISMA, DESIS and EnMap. Nonetheless, the phrase was changed to: "The PONDER project (BELSPO SR/00/325) focused on developing tools for spaceborne remote sensing of inland water systems using high spatial resolution (≤ 30 m) sensors. During the course of the project, global coverage and open access data for high spatial resolution sensors was only available for multispectral missions."

L 34: cover eight lakes, the Spuikom lagoon, the Scheldt estuary and the BCZ in the western part of Belgium (e.g. there are no waters sampled in Wallonia

We have updated the title to better represent the content of the study. The title was changed to "Optical and biogeochemical properties of diverse Belgian inland and coastal waters".

L 43: should CDOM be added here?

We have not performed analysis to quantify CDOM concentration, only its contribution to the absorption coefficient.

L 102: …..2003). And….   Should there be a comma here or should this sentence start with a different word?

Changed to: "The BCZ develops..."

L 114 : how long were these samples stored before analysis?

Typically the samples were processed within 4 to 6 hours from sampling. This information was added as: "...dark and cold during the transport to the laboratory, and processed within 4 to 6 hours from sampling."

L 132: I would refrain from using the term Neperian as it is 1) often spelled in different ways; 2) not well known what it means, 3) much easier to say the e power logarithm   etc…

We appreciate the suggestion. Following the reviewer comment we found the paper of Ayoub (1993) with a clear explanation of the Naperian logarithm and that though similar, it is not exactly the same as the hyperbolic or natural logarithm. However, the chemical literature uses the terms decadic and Napierian to distinguish between base 10 and base e coefficients (IUPAC, 1997). We have corrected all occureces of neperian to Napierian.

Ayoub, R. 1993. What is a Napierian logarithm? The American Mathematical Monthly 100, 4, 351-364. DOI: 10.2307/2324957

IUPAC. 1997. Compendium of Chemical Terminology, 2nd ed. (the "Gold Book"). Compiled by A. D. McNaught and A. Wilkinson. Blackwell Scientific Publications, Oxford.

Paragraphs 2.3.1. and 2.3.2. describe the use of 0.45 um filter for CDOM and a 0.7 um filter for particulates. Where do you describe the properties of the 0.45 to 0.7 um residual? This needs to be discussed. There is a paper by Laanen et al that discusses this fraction and it is not negligible.

1. See: Laanen, M.L., Peters, S.W.M., Dekker, A.G. and van der Woerd, H.J. (2011) Assessment of the scattering by sub-micron particles in inland waters; Journal of the European Optical Society - Rapid Publications 6, 110. [DOI: http://dx.doi.org/10.2971/jeos.2011.110].

We appreciate the reviewer indication of a study quantifying that fraction. The study of Laanen et al. (2011) shows that on average, 6 % of the absorption for the fraction < 0.7 µm originates from the fraction between 0.2 µm and 0.7 µm, while 94 % originates from the fraction < 0.2 µm. In this study, we have used a 0.45 µm mesh filter, considerably reducing the gap between fraction sizes. This suggests, all else being equal, that at least 94 % of the absorption signal in the fraction < 0.7 µm is accounted for, possibly more since the mesh size used in our study is two times larger than the one used by Laanen et al. (2011). However, from an operational and *practical perspective, the value of interest should be referenced to the particle absorption and not CDOM absorption. Here is the reasoning: An optical operational definition of CDOM is matter that does not cause scattering. We have not observed scattering contamination in the absorption measurements of the fraction < 0.45 µm (NIR signal centered on zero). Therefore the possible impact in the estimation method would be an underestimation of the particle absorption, due to the fraction between 0.45 µm and 0.7 µm. So the fraction of relevance in this case is (A(<0.7)-A(<0.45)) / A(>0.7), which was not in the scope of the refereed study. In our study, absorption of CDOM is only higher than absorption by particles in the UV range. Accordingly, the fractional absorption lost at 440 nm would be even smaller than 6 %. Finally, it must be considered that the for the glass fiber filter, the mesh of fibers is a irregular matrix, with the 0.7 µm mesh size representing a nominal value and the accumulation of matter in the filter matrix effectively reducing the 50 % particle cutoff size below 0.7 µm. For those reasons, we consider that a potential bias in the particle absorption is negligible.*

L 200: this effect was dependent of lake .. do you mean this effect varied across the lakes???

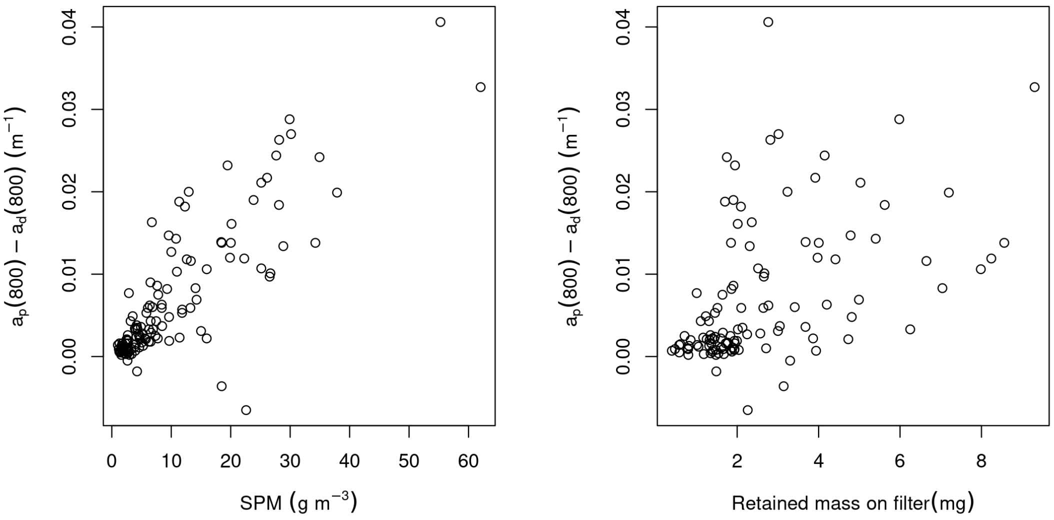
Indeed, the loss effect was very apparent for samples of a given lake and less for others. We do not have an explanation for the effect. We have changed the phrase to "The magnitude of this effect varied across the lakes, ..."

L208: … 850 nm, and including…   this is not a normal sentence structure-please revise.

The phrase was changed to: "Based on these observations, we fit an exponential function to measured a_d with sodium hypochlorite in the range of 550 nm to 850 nm, and included a point-estimate of rinsed a_d at 305 nm as $0.8a_p(305)$."

L216: is it: proportional to the concentration of particles and organic matter or is it: proportional to the organic fraction concentration of the particles? Please clarify as this is confusing.

*The former interpretation is correct. According to our data, the effect does not seem related to the organic fraction (i.e., relative) of particulate mass. Instead, it shows a linear relation to the particle load in the environment (SPM) and the organic particle load (SPM * (1 - MF)), i.e. absolute. This effect could have a small artifact contribution of filtration, but we would not classify it as an artifact since the relation to particle mass retained on filter is much less clear than to the particle concentration in the environment. See attached plots below.*



L217: this is also confusing: This loss of absorbing material was not observed in a study by Röttgers et al. (2014a) including samples from a diverse set of environments, though the authors did not apply NaClO to the North Sea or Baltic Sea samples….. so which samples from where did they apply NaClO? And is that relevant to your paper?

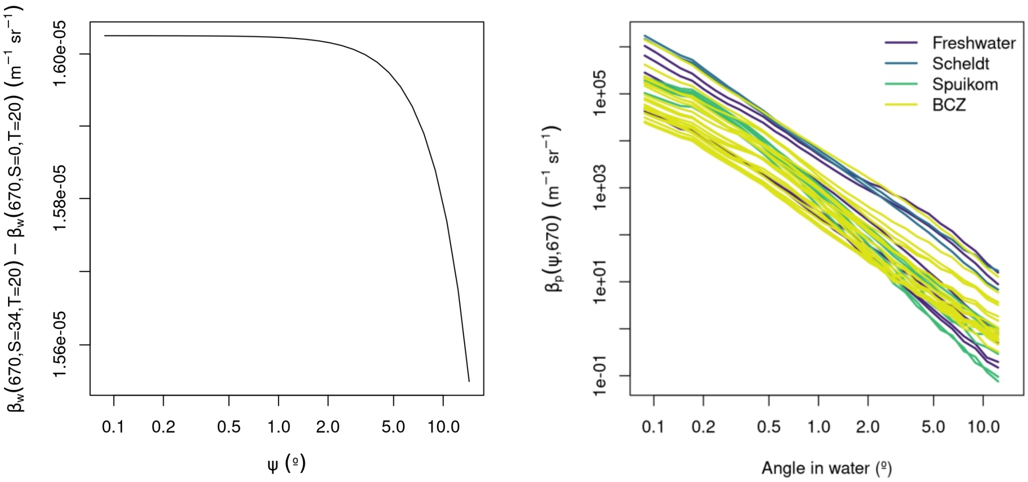
Their study applied NaClO to all cruises, except to those of the North sea and Baltic sea. This study is relevant for our research as it is regarded as a reference study for the subject. The effect was not observed in their bleaching analysis, but their bleaching analysis did not include samples from the North Sea.

L250: what is the effect of salt water on the VSF at these angles? I am asking as you calibrate with deionised water-should you calibrate with pure filtered seawater?

The additional scattered signal in the 2.5 cm or 5 cm path (depending on instrument model) arising from salt-solvated water is negligible in this waters considering the load of particles. See attached plot below on the difference of salt-solvated water VSF to pure water VSF at 670 nm and 20°C for the range of angles of the LISST instruments (Zhang and Hu, 2009; Zhang et al., 2009). Those values are negligible when compared to the measured VSFs.

Zhang, X.; Hu, L. 2009. Estimating scattering of pure water from density fluctuation of the refractive index. Optics Express 17, 3, 1671-1678. DOI: 10.1364/OE.17.001671

Zhang, X.; Hu, L.; He, M.-X. 2009. Scattering by pure seawater: effect of salinity. Optics Express 17, 7, 5698-5710. DOI: 10.1364/OE.17.005698



L331: This sentence is worrying: The water-leaving signal is not strictly Lambertian, however this approximation is commonly used for remote sensing purposes (cf. Frouin et al., 2019). …… There are many papers that describe this factor (often referred to as Q) as ranging between 2 and 5 i.s.o. PI. It is too easy to choose one recent paper that ignores all of this and says L to E conversion = PI. Given the care you take to describe your methodology this needs an equivalent amount of attention citing relevant literature that also says this cannot be ignored. This also has effect on your equation in L350……! And Line 367 where the measurement angle of 40 degrees should also be discussed with reference to the Q factor of the assumed factor of PI.

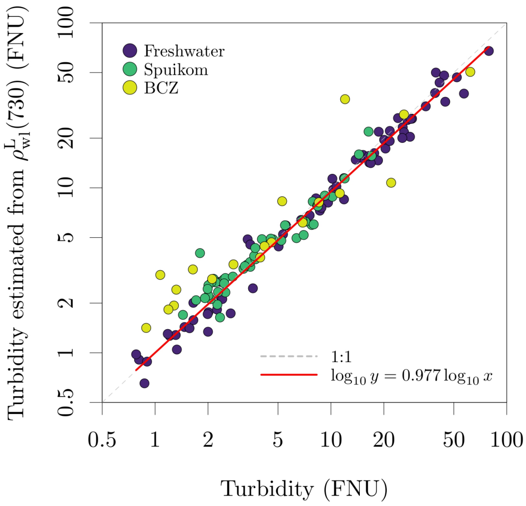
The manuscript does not suggest a value for the factor Q. Instead it multiplies by PI sr under a Lambertian assumption, hence the name "Lambert-equivalent bi-hemispherical reflectance", that is, the bi-hemispherical reflectance that would be observed (based on a given directional-hemispherical reflectance) if the BRDF was Lambertian (cf. Herman and Celarier, 1997). The real bi-hemsipherical reflectance is unknown. In that sense, the scaling by PI sr is "cosmetic", but such "Lambert-equivalent bi-hemispherical reflectance" is commonly used in the literature, though generally not including the qualifier "Lambert-equivalent" (Vanhellemont and Ruddick, 2018). The qualifier is important to justify the units of PI (i.e., sr) and to make clear to the reader/user that the bi-hemispherical value is for an hypothetical scenario.

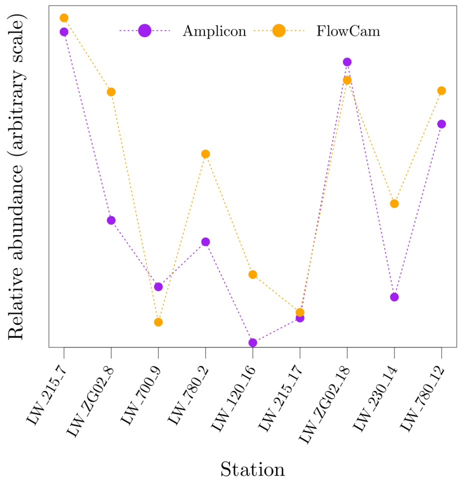
Herman, J. R.; Celarier, E. A. 1997. Earth surface reflectivity climatology at 340–380 nm from TOMS data. Journal of Geophysical Research 102, D23, 28003. DOI: 10.1029/97JD02074

Vanhellemont, Q.; Ruddick, K. G. 2018. Atmospheric correction of metre-scale optical satellite data for inland and coastal water applications. Remote Sensing of Environment 18, 586-597. DOI: 10.1016/j.rse.2018.07.015

+- L 478: Figure 14 Y-axis : elati e abundance????   What does that mean?

This label (and also for Fig. 9) is correct in the original figures, though the preprint pdf has blanks covering part of the text. This likely happened during pdf generation by the production team. We will be vigilant that the proofs are corrected for such errors. The original plots are reproduced below.





L495: the relative scarcity of similar open datasets in inland: what about Limnades?

Please see the arguments and changes presented above.

Reviewer 3 (Unknown)

This article is of high quality. The measurement protocols are very precise and seem to respect international standards as defined by the IOCCG for example. Unfortunately, I don't have many comments to make.

We appreciate the reviewer's evaluation and comments.

Line 4: the first comment concerns the term “growth season”. It's a term I don't often see in the literature. Should we use “growth season” or “growing season”? Anyway, if we understand what it refers to, I find this term imprecise. Can the authors justify the choice of this term?

To our knowledge, the term “growth season” is not uncommon in the ecological literature. A search on Google Scholar for ‘growth season + phytoplankton’ results in more than 350,000 publications. “Growing season” is also used, but 3 times less common. In our understanding the term is relevant for medium and high latitudes, representing the period from spring to autumn, since the low temperatures and low daily integrated irradiance of winter results in the baseline phytoplankton biomass. We have added the specification “(spring to autumn)” after the first occurence of “growth season” in the text.

Line 46: The authors indicate the use of "robust linear regressions". Can they clarify why they use this term "robust" ? On the other hand, can they indicate what type of regression they use? Which R function did they use ?

Robust statistics help to overcome the sensibilities of classical function estimation methods (e.g., ordinary least squares) in the presence of "outliers" or extreme values. This is done by using a different cost function than squared errors. In the particular case used in our analysis, the Huber's cost function is used, which is a piecewise function that is quadratic for "small" (< δ) residuals and linear for "large" residuals (> δ), where δ is a given residual threshold. This reduces the influence of large residuals in the value of the cost function. The function used is 'rlm' of the R package 'MASS' (version 7.3-51.5, Venables and Ripley, 2002). The term "robust statistics" and the estimation methods in its scope are widely known and well developed since the 60's. We have updated line 46 to: "Linear models used for consistency check between parameters were fitted using robust linear regression (iterated reweighted least squares with Huber's loss function; Venables and Ripley, 2002)". We have refrained from adding the specific function name to the manuscript to not encourage a cookbook approach to statistical tools.

Venables, W. N. and Ripley, B. D. 2002. Modern Applied Statistics with S, Springer, New York, fourth edn., http://www.stats.ox.ac.uk/pub/MASS4, ISBN 0-387-95457-0.

Line 57: Table 2, maybe it would be useful to add a column linked to the frequency of acquisition (fortnightly, monthly...)

We appreciate the suggestion. However, the frequency varied with year and site and can be more clearly described in the text. No changes were performed.

Line 143: the authors have chosen a 0.45 mm pore size filter. "Acceptable filter types have effective pore sizes of 0.2 µm for coastal and open ocean waters, whereas 0.45 µm pore size filters are commonly used in freshwater systems and acceptable due to the practicality of working with high particle load samples" (Mannino et al., IOCCG Ocean Optics and Biogeochemistry Protocols for Satellite Ocean Color Sensor Validation, Measurement protocol of absorption by chromophoric dissolved organic matter and other dissolved materials). Can they explain their choice? Have they carried out comparative tests to assess the impact of this choice, particularly for coastal waters?

Filtrations for CDOM were performed with 0.45 µm (pore size) filters, which as mentioned by the reviewer, is listed as acceptable in the draft version of the IOCCG protocol for CDOM measurements (Mannino, et al. in prep). We have worked with 0.45 µm pore size because the primary focus of the dataset were inland waters with high particle loads (only 9 of the 19 coastal samples have measurements of CDOM), and we wanted to be consistent with methodology throughout the study. No tests were performed at this time. However, our validation for the CDOM measurements stems from the near-zero NIR absorption coefficient (Figure 1A). CDOM absorption tends to zero at NIR wavelengths (cf. Kirk, 1976) and the presence of particles or bubbles would cause scattering, creating an apparent increase in absorption. Since our values in the NIR fluctuate around zero, we understand that the pore size was sufficient to to *avoid any significant artifact to the absorption measurements of dissolved components.*

Kirk, J. T. O. 1976. Yellow substance (gelbstoff) and its contribution to the attenuation of photosynthetically active radiation in some inland and coastal south-eastern Australian waters. Australian Journal of Marine and Freshwater Research 27, 1, 61-71.

Section 2.3.5: I agree with another reviewer's comment. How can the authors justify classifying this parameter as an IOP? Line 321: the authors indicate that there is a single linear relationship between turbidity and SPM. It would be good to discuss this result. For the freswater it is obvious. For BCZ, we see strong deviations especially for low turbidity values and for two points around 10-20 FNU. Why ?

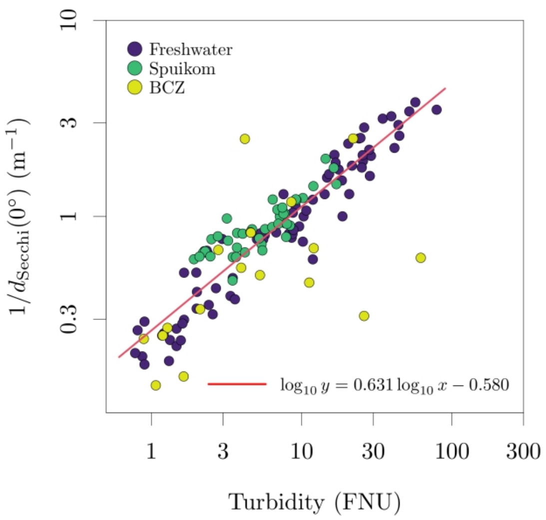
For the discussion on turbidity classified as an IOP, we refer to the answer to Reviewer 2.

In the low particle load range, outliers can occour in the SPM measurements due to the random sample of rare large particles (such as zooplankton). The difference between marine/brackish samples from freshwater samples in that low range could also be the different composition of those particles (per mass IOP). However, as a data report, we have avoided discussing the data, instead we focus on describing it, together with the methodology and its validation. No changes were performed.

Section 2.4.1: This section needs improvement!! The authors say "measured and corrected values are provided"... and then nothing ! No description of values, why? On the other hand, it is written "a tentative correction for the effect of sun zenit angle ... was applied". I looked at the data. There are 5 values (DK_01, SP_08, SP_21, SP_42, SP_46) which show corrected secchi values stronger than depth ?? This raises questions about the relevance and quality of this correction. Without discussion of its values or caveat on their quality, I recommend removing corrected secchi values from the database. Finally, why did the authors not apply a quality control for this parameter ? There are many algorithms in the literature comparing either turbidity to SPM or turbidity to IOPs.

As we understand it, the value of the Secchi disk bears no relation on how far from the bottom it is, but how far from the surface. The bottom is an impediment to measure the correct Secchi disk depth in "optically shallow" and that is why the Secchi at the bottom is recorded with a special value to indicate that (in our case, a negative value). Therefore it is logically valid, and ecologically informative, to have corrected Secchi disk depths that are deeper than the bottom. The inverse of the Secchi disk should be proportional to the turbidity, as the fraction of diffuse attenuation caused by scattering is more relevant than the fraction caused by absorption for secchi disk depth determinations (visual contrast between surrounding water and disk; Lee et al., 2015). Here we present the plot of corrected Secchi disk depth against turbidity. The larger variability is seen in the coastal waters, in particular for stations LW_710_11 (yellow point way above the line) and stations LW_710_6 and LW_700_9 (two right most yellow points below the line). We note that the Secchi disk depth measurements in the marine campaigns were performed by a different operator that at the other stations and that conditions at sea are more challenging for measurements of Secchi *disk, particularly at medium to high turbidity. The relation between SPM and turbidity and between measured turbidity and turbidity inverted from reflectance suggests that the Secchi disk depth measurements have higher uncertainty.*

We have added this plot to the supplementary material and added the following phrase to section 2.4.1: "A comparison between turbidity the inverse Secchi disk depth, corrected for the Sun zenith angle, shows a log-linear relation and is presented in Fig. S5 (supplementary material)."



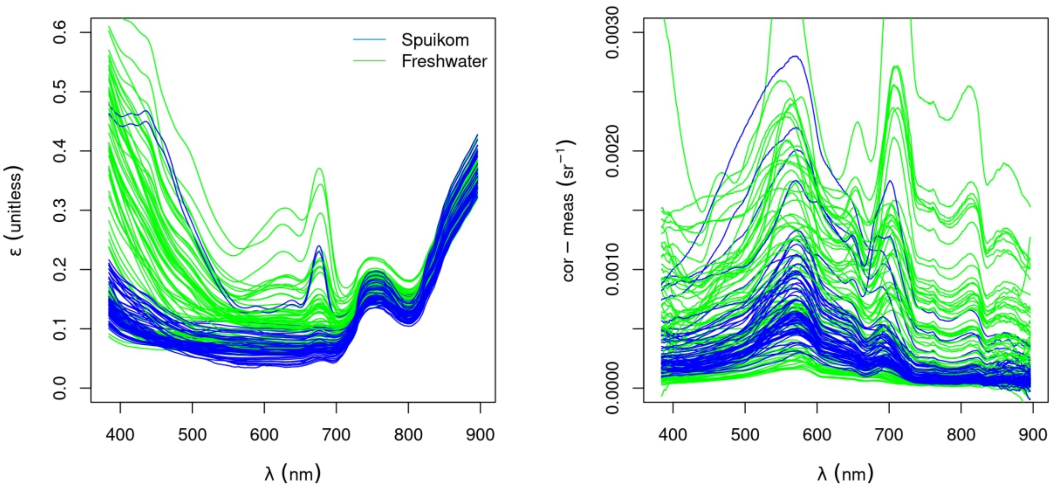
Section 2.4.2: how long is the acquisition session for a L_wl measurement per station? How many spectra are acquired per session ? You have to use the average spectrum I guess. Have you analyzed the standard deviation to possibly flag bad quality points? I looked at Figure S5. The spectrometer is hand-held. Can you guarantee that the lens's cylindrical shield at 2.5 cm below the water surface? Line 355, what are the average, min and max shadowing error values used to correct E_dn? Line 368, what are the average, min and max values of the effective Fresnel reflectance used ? Finally, you don't present any quality control for rhow_wl. Why not perform an optical closure exercise between measured rhow_wl and IOPs?

The acquisition of the series for each station is concluded in less than 2 minutes (typically less than 1 min) with the Skylight-blocking method. The procedure is: 5 spectra are acquired over the plaque, then the shield's extension to the lens is submerged and 10 water-leaving radiance spectra acquired, then the tip is removed from the water, dried with paper and another 5 spectra are acquired over the plaque. We have added the folloing sentence: "A total of 10 L_{wl} spectra and 5 plaque exitant radiance spectra were averaged per station, with the measurement sequence completed within 2 min."

We understand the reviewer's considerations related to the limited control of handheld spectroscopic measurements. However, even if the system was not handheld (cf. Lee et al., 2013), it would not be possible to guarantee, under any realistic condition, the exact depth. The same is true for all spectroscopic measurement methods and in practice ranges around the nominal values are accepted (depth, *angle). Considering that the shield extension is only 5 cm long, the maximum range of depth is +- 2.5 cm from the nominal, though a statistical argument can be used to support that most measurements are made closer to the nominal depth of 2.5 cm.*

We have performed reasonable controls, corrections and validation to the data to ensure high quality. Closure is a challenging analysis. In part this is because scattering in the water system includes the contribution of turbulence and bubbles, and because inelastic scattering such as fluorescence of CDOM and phytoplankton pigments might not be correctly parametrized in the radiative transfer model. Also of importance is that we did not measured backscattering. However, following the argumentation presented above of turbidity as IOP, we can still present an equivalent analysis: The best support for the quality of the measurements is the linear relation between measured turbidity and turbidity estimated from the NIR (Fig. 9). The high absorption coefficient at 730 nm results in increased shadowing errors, which are also dependent on the IOPs and on the depth of the opening of the shield's extension. Figure 9 presents an acceptable variance around the 1:1 line.

The spectral shadowing errors estimated for the measurements and the difference (in R_{rs} units of sr^{-1}) between the measured and corrected spectra are presented below:



Lee, Z.-P. et al. 2013. Robust approach to directly measuring water-leaving radiance in the field. Applied Optics 52, 8, 1693-1701.

Section 2.4.3: after analyzing figure S7, I wonder about the nature of the container where there is the sediment. Is it a section of PMMA or is it a kind of glass? The walls seem thick? Have you analyzed the potential impact in terms of shadowing error?

The tubes used to retrieved the cores are made of non-pigmented ("transparent") PMMA. The transmission of the side walls is certainly less than 1, however, the procedure used to estimate reflectance is expected to compensate for all effects since, all setup is equal when measuring reflectance from the sediment and from a reference diffuse reflector (circular NIST-traceable Munsel card, ~0.1 mm thick), placed on top of the sediment, under the water. With the card having the same average orientation and position of the sediment surface, the illumination is equivalent and the effects of *the sensor and walls are canceled. The phrase in line 389 was updated to: "A circular NIST-traceable Munsel card was used as a submerged reference, gently placed over the sediment, receiving the same illumination as the sediment's surface."*