Reply on RC3
Alexandre Castagna et al.

Author comment on "Optical and biogeochemical properties of Belgian inland and coastal waters" by Alexandre Castagna et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-466-AC3, 2022

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 occurrence 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 ‘rim’ 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.

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.


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 freshwater 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 occur 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 following sentence: “A total of 10 Lwl 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 Rrs units of sr-1) between the measured and corrected spectra are presented below:

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.”