

Earth Syst. Sci. Data Discuss., author comment AC2
<https://doi.org/10.5194/essd-2021-466-AC2>, 2022
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Reply on RC2

Alexandre Castagna et al.

Author comment on "Optical and biogeochemical properties of diverse Belgian inland and coastal waters" by Alexandre Castagna et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-466-AC2>, 2022

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 Pangea is under review and not yet final.

Overall comments:

Title: as this paper does not cover 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.

We appreciate the reviewer suggestion. Indeed the title does not appropriately represent 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quantities that are independent of the "polydirectional light field" or "radiance distribution" (quoted, as it is an outdated concept not inline with modern physics; cf.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 90° 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 90° 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 dataset 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 Napierian 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 occurrences 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. 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 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???

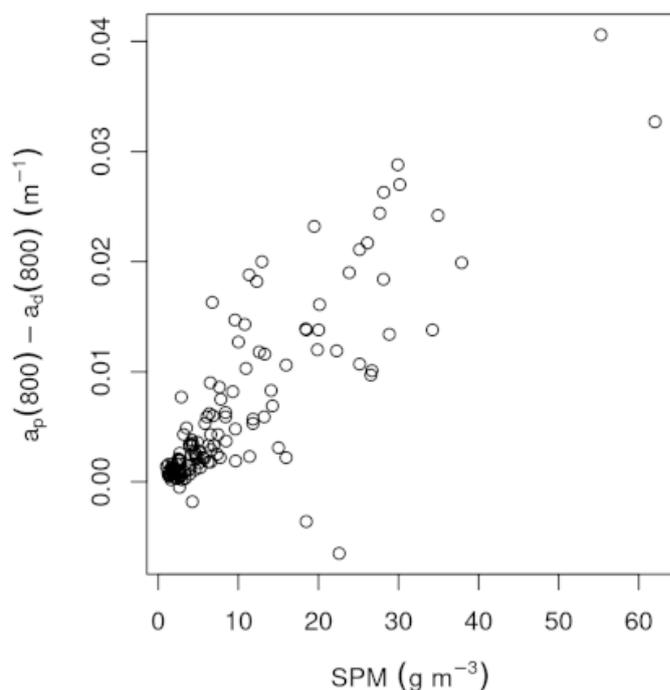
Indeed, the loss effect was very apparent for samples of a given lake and less for others. We do not have an explanation for the effect. We have changed the phrase to "The magnitude of this effect varied across the lakes, ..."

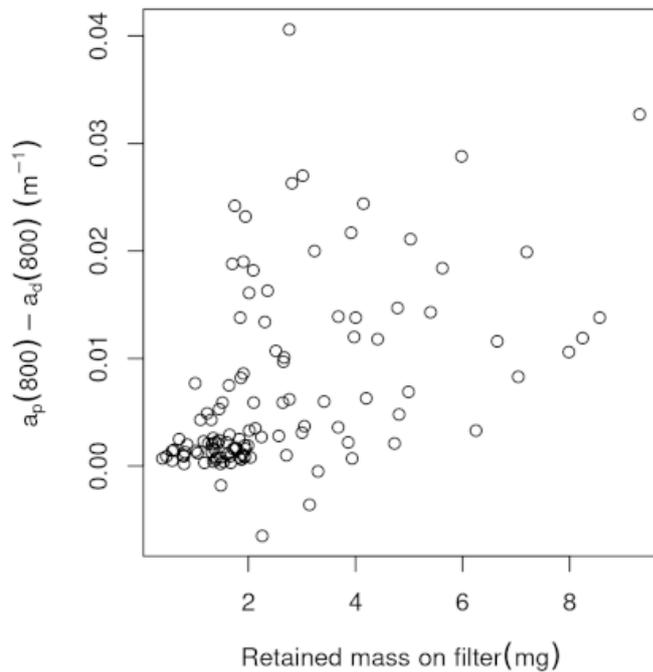
L208: ... 850 nm, and including... this is not a normal sentence structure-please revise.

The phrase was changed to: "Based on these observations, we fit an exponential function to measured ad with sodium hypochlorite in the range of 550 nm to 850 nm, and included a point-estimate of rinsed ad 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.



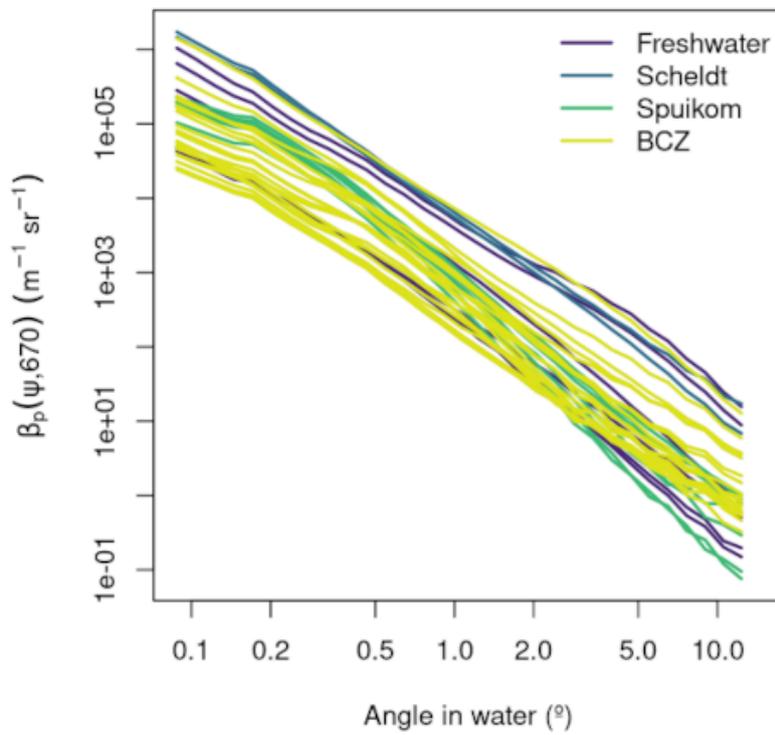
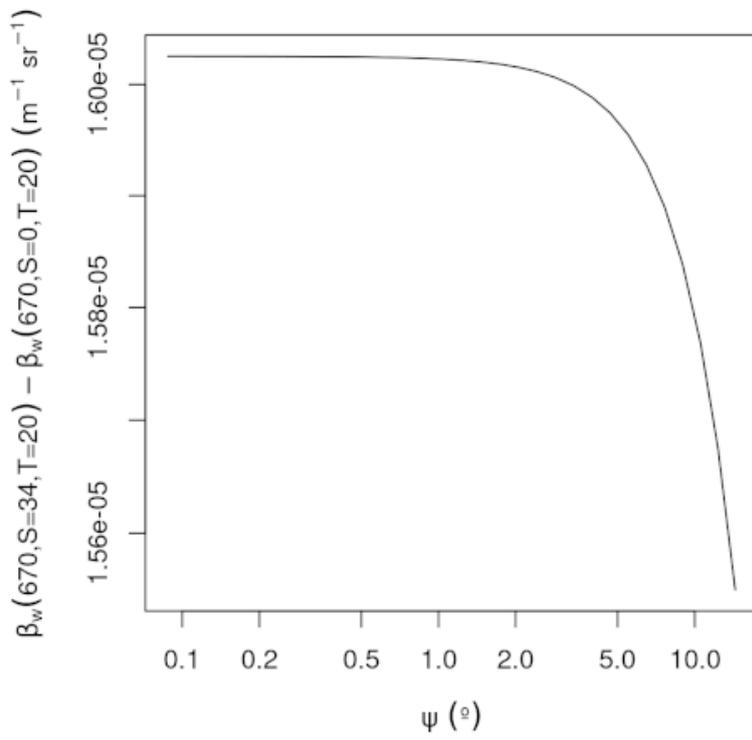


L217: this is also confusing: This loss of absorbing material was not observed in a study by Röttgers et al. (2014a) including samples from a diverse set of environments, though the authors did not apply NaClO to the North Sea or Baltic Sea samples..... so which samples from where did they apply NaClO?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

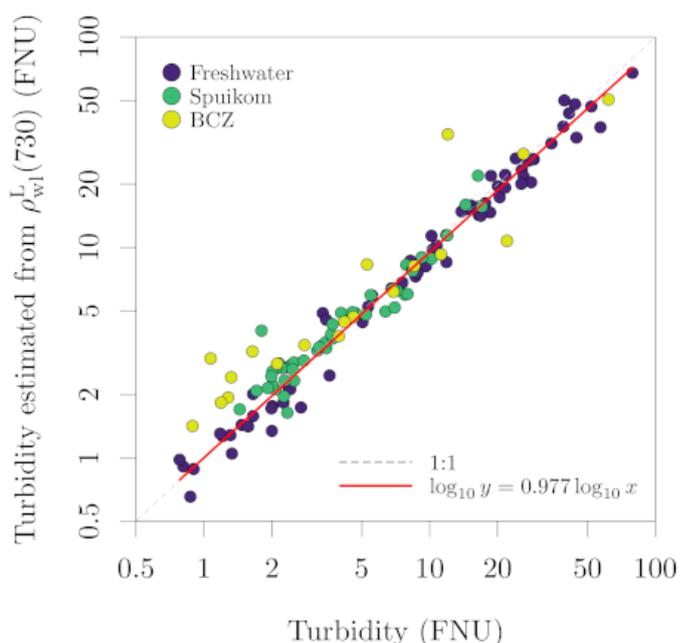
The manuscript does not suggest a value for the factor Q. Instead it multiplies by PI sr under a Lambertian assumption, hence the name "Lambert-equivalent bi-hemispherical reflectance", that is, the bi-hemispherical reflectance that would be observed (based on a given directional-hemispherical reflectance) if the BRDF was Lambertian (cf. Herman and Celarier, 1997). The real bi-hemispherical 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.

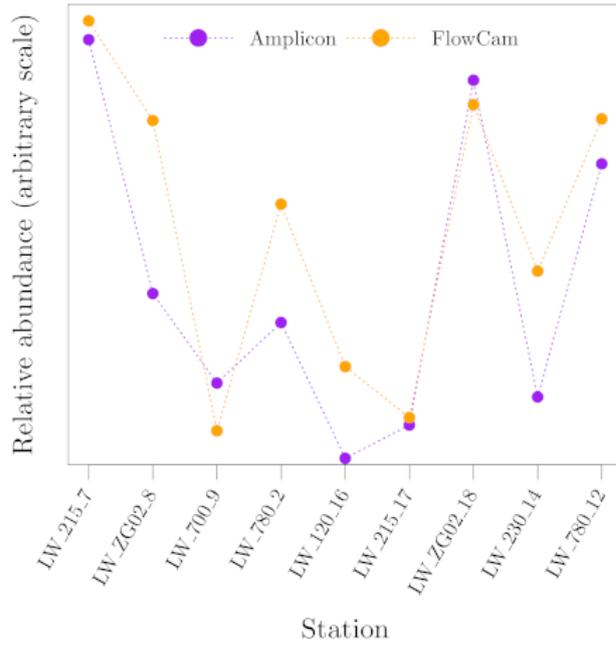
Herman, J. R.; Celarier, E. A. 1997. Earth surface reflectivity climatology at 340–380 nm from TOMS data. Journal of Geophysical Research 102, D23, 28003. DOI: 10.1029/97JD02074

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.