Reply to the Review by Jaime Pitarch

Below are the replies from the Authors to the Reviewer's comments.

General comment
I am very pleased to have been given the opportunity to review this manuscript as I am aware of the lifetime work of the authors in defining the highest standards and producing high quality reference data in the field of satellite ocean color. The monitoring programs CoASTS and BiOMaP have generated lots of publications, and a remaining question was where all the data was going to be after the finalization of such programs. So now it appears that a circle is closed.
I have read the paper and downloaded the dataset. Before publication, I have a number of comments of varying importance that, to my understanding, need attention.
Reply
The Reviewer comments are duly considered and itemized. A reply and clear actions are provided for each one (for the benefit of conciseness, the figures provided by the Reviewer are omitted from the reply).

Major comments

Comment #1
Absorption from water samples is only provided at the Satlantic bands, which is regretful, as it was measured hyperspectrally. I do not know the reason to downgrade the data, and it definitely reduces its value for optical studies, also considering the growing interest in hyperspectral data (e.q., PACE). The authors are encouraged to submit the hyperspectral data.
Reply
The CoASTS-BiOMaP data set provided through PANGAEA was conceived to support bio-optical investigations with comprehensive multi-parametric near-surface quantities. Because of this, the laboratory absorption measurements were only provided at the center-wavelengths of the related multi-spectral field radiometric data.
It is definitively appreciated that *i.* the CoASTS and BiOMaP measurements can support a number bio-optical, methodological and instrumental applications beyond the strict and obvious bio-optical ones and that *ii.* hyperspectral measurements (when available) or full profiles instead of the sole near-surface data are relevant and desirable data. But this is something that goes beyond the objectives of the current work. An expansion of the shared data set could be considered as a future task, but not for the current data submission.
The objective of the work and of the related data set will be strengthened in the introduction.

Comment #2
Paragraph from line 175 to 182: on the above-water reference sensor, I see the correction for the imperfect non-cosine response. What about other uncertainty sources such as temperature and non-linearity, as it is recommended in above-water radiometry (e.g., Trios)? And are any of these corrections made to the in-water sensors?
Reply
In agreement with common know-how, multi-spectral radiometers rely on a much simpler design and technology with respect to the hyper-spectral ones. Because of this, they exhibit lower sources of significant uncertainties: the temperature dependence is negligible within the 410-700 spectral range (Zibordi et al. JTECH 2017); stray-lights are negligible assuming interference filters are of high quality and their out-of-band response is within specifications for ocean color applications (Johnson et al. AO 2021). Because of this, some uncertainties due to the potential non-ideal performance of multi-spectral radiometers are commonly not included in uncertainty budgets. Exception is the non-cosine response of irradiance sensors, which depends on the manufacturing and material of individual irradiance collectors.
These elements will be mentioned in the relevant section.

Comment #3
Line 193-196: the interval 0.3 m – 5 m looks arbitrary. Any comments on why this choice is appropriate? Does it relate to the unphysical Kd values that I report below?
Reply
The determination of subsurface radiometric values from profile data always requires the identification of a suitable near surface "extrapolation interval" exhibiting linear dependence of log-transformed radiometric data with depth. In the case of the CoASTS-BiOMaP data, the most appropriate extrapolation intervals were determined within the 0.3 and 5 m depth limits (these limits do not identify the extrapolation intervals themselves, but the values within which the extrapolation interval is generally located).
How the extrapolation intervals are determined is well stated in the manuscript. The process is definitively subjective (i.e., the extrapolation interval is chosen by an analyst with the aid of a number of ancillary information), but for sure, it is not arbitrary.
Some further clarifications will be added together with references to avoid any misinterpretation on the actual extrapolation intervals and the depth limits within which they are commonly located.

Comment #4
The transmission of upwelling radiance through the surface to form the water-leaving radiance is made with that 0.544 factor, which is not up to date with today's knowledge. I suspect that the reason to choose this value is because the difference in the final product would be minimal when using another one. However, I report evidence that this is not the case.
For a flat surface, the relationship between in-water and in-air upwelling radiance is: $Lw=\tau w,Lu(0-)$
Where $\tau w,=1-\rho nw2$
ρ is the Fresnel reflectance of the air-sea interface. Assuming unpolarized light, it has an analytical expression $\rho=12|\sin2(\theta a-\theta w)\sin2(\theta a+\theta w)+\tan2(\theta a-\theta w)\tan2(\theta a+\theta w)|$
θa and θw are the wave propagation angles in air and in water, respectively, and are related by $\sin(\theta a)=nw\sin(\theta w)$
For $\theta a=\theta w=0$, there is a singularity. One can apply the small angle approximations for the trigonometric functions, so in the limit, it is: $(\theta a=\theta w=0)=(nw-nanw+na)2$
It is commonly accepted now that it is inaccurate to assume a constant $\tau w,a$ for a given geometry due to the spectral dependence of nw (and secondary influence by temperature and salinity too). Such dependences are taken from the state-of-the-art values by Roettgers et al. (2016). Therefore, the theoretical curve for τw, can be seen in Fig. 1. In addition, I have made some Hydrolight simulations, in which the same nw values are used, but also, the transmission is affected by the surface roughness depending on the wind speed. What emerges from Fig. 1 is that increasing wind speeds reduces light transmission. In terms of the total error made by assuming $\tau w,a=0.544$, it may not seem much, but in reality, they are in the order of 1-2%, which accounts for about 20% of the total uncertainty reported for the final Rrs product. Therefore, to reduce total uncertainty I encourage the authors to consider updated look up tables for τw.
Reply
The spectral dependence of the water-air transmission factor for a flat sea surface in the interval of interest for the CoASTS-BiOMaP data set is well within +/-1%: this is confirmed by the data provided by the Reviewer and also by the work of Voss and Flora (JTECH, 2017). Because of this, neglecting the spectral dependence of the water-air transmission factor does not appreciably affect the uncertainty budget of the derived radiometric quantities.
The inclusion of the wind speed dependence in such a transmission factor adds the detrimental dependence on wind speed to L_w. In fact a properly determined subsurface L_u is marginally affected by sea state (and consequently by the wind speed). Thus introducing a wind speed dependence on the water-air transmission factor would add such a dependence to L_w. This is not a desirable dependence for data envisaged to support bio-optical modelling.
A mention to spectral dependence of the water-air transmittance will be added.

Comment #5
On the fit of simple analytical functions to variables like the Q factor and possibly others like *bb*, I did not understand if the actual data were replaced by the fits. If that is the case, I prefer then to have both data and uncertainty rather than their surrogate analytical forms.
Reply
The fit of Qn data is introduced to minimize the impact of any uncertainty affecting intra-band radiometric calibrations of multi-spectral radiometers or the extrapolation process. In practice, any deviation indicated by the fit with respect to the actual Qn values is used to monitor the performance of the L_u and E_u sensors in the field (deviations of +/-1% are typical, variations exceeding +/-2% are a warning). The fitted Qn data are those saved and included in the shared dataset. Still, both L_u and E_u data are provided, any data user may re-compute the data at his preference .
More details will be provided on Qn fitting, but no additional action is taken. Quality controlled and "smoothed" Qn values are those expected to best serve the community.

Comment #6
On the bidirectional correction in lines 233-241, it is a bit disturbing to read in line 241 that in case 2 waters "this correction may be affected by large uncertainties". There are significant parts of the dataset in case 2 waters. How large are those uncertainties? Ongoing research has proven that is it better to apply Morel than not to apply any correction at all, and Morel has shown to provide surprisingly good results in case 2 waters, not because of the qualities of the model itself, but because all bidirectional correction models underestimate the correction to be made, but chlorophyll is overestimated in case 2 waters with the band ratio of Morel, which produces a higher correction, that ends up being beneficial. In any case, I believe that this part of the processing will need update to be in line with latest developments in bidirectional studies, knowing the interest of the authors in keeping the uncertainty budget as low as possible.
Reply
Sorry if the Reviewer feels a bit disturbed by a reasonable sentence such as "this correction may be affected by large uncertainties" addressed to the Morel et al. (AO, 2002) correction for bidirectional effects applied to non-Case 1 waters. Clarifying that the implementation of this correction for CoASTS–BiOMaP data relies on actual *Chla* values from HPLC analysis and not from any algorithm as suggested by the Reviewer, the sentence is certainly well supported by the work of Talone et al. (2018).
It is emphasized that all the fundamental data for producing alternative corrections for bidirectional effects are available: any user can thus implement its own ignoring the one applied for the shared data.
The potential for producing high level radiometric data products with alternative corrections for bidirectional effects benefitting of the basic radiometric quantities included in the data set, will be mentioned.

Comment #7
On the ac-9 measurements, I have several comments that follow.
How regular were the factory calibrations? It is said that instruments have to be calibrated before and after any campaign. Is this the case with the ac-9?
Reply
The two AC9s used during the CoASTS and BiOMaP campaigns were factory calibrated on a yearly basis (obviously with a number of exceptions over almost three decades). Definitively, the instruments were sent to the manufacturer for maintenance and calibration each time there was evidence of sensitivity decay in a single band (implying the replacement of the related filter and detector). The pre- and post-campaign "calibrations" correspond to the milli-Q water offset measurements performed by the JRC team on board and with the instrument in his deployment configuration. These measurements were intended to detect and correct any minor bias affecting the factory calibration coefficients of individual bands over time (i.e., between successive factory calibrations).
Factory and field calibrations will be better detailed.

Comment #8

The Zaneveld method does not correct the non-finite acceptance angle of the c detectors as it is stated (note that the "c" is missing in line 277), and in fact it is rarely corrected by anybody. To do that, one should have a guess of the VSF between 0 and 0.93 degrees, but in any case, the "real" $ct{-}w$ is higher than the measured than the factor that varies a lot, mostly between 1 and 2.

Reply

Thanks you for catching the inappropriateness of the statement on the correction for the non-finite acceptance angle. Definitively, Boss et al., (2009) determined Ct-w(AC-9)/Ct-w(LISST-F) = 0.56 (0.40-0.73). But again, it would have been speculative any correction not supported by specific VSF measurements.

The text will be revised declaring that corrections are not applied for the non-finite acceptance angle of the c detector.

Comment #9

On the scattering correction method of the absorption data from the "a" tube, I also believe that the Zaneveld method questionable. Zaneveld overcorrects the absorption data, which leads to an underestimation. I see indirect evidence of it in Figure 5 from the manuscript, where the absorption comparison at 443 nm almost always shows negative biases with respect to the laboratory measurements (although the ac-9 provides better closure of Rrs than the water samples as I show below, so this is puzzling and needs to be addressed by the authors). I suggest using the method by Roettgers et al. (2013), that, if applied, is supposed to perform much better. This choice should be in line with authors approach of using only consolidated methods, approved by the two very good assessments by Stockley et al. (2017) and Kostakis et al. (2021).

Reply

Roettgers et al. (2013) showed that the Zaneveld et al. (1994) underestimates the absorption for the wavelength greater than 550 nm. In the blue and blue-green, and in particular at 443 nm, the agreement was shown quite good between the AC9 and the "true" absorption from a PSICAM. Stockley et al. (2017) overserved relative errors lower than 20% for the "Zaneveld et al. (1994)" correction in the spectral range 412-550 nm (lower than 10% for wavelengths 412-488nm). Thus, the negative biases observed at 443 nm documented in the manuscript and mentioned by the reviewer, cannot be explained only by the scattering correction method "Zaneveld et al. 1994".

Also, the hypothesis of negligible non-water absorption in the NIR was shown to be questionable for highly turbid waters (*e.g.*, Elbe River, Baltic Sea and North Sea) but acceptable for the oligotrophic Mediterranean Sea (Stockley et al., 2017).

It is agreed that the correction method proposed by Roettgers et al. (2013) and verified in Stockley et al. (2017) is definitively a progress with respect to Zaneveld et al (1994), in particular in the green and red spectral regions, but his universal applicability is not assured. An excerpt from Stockley et al. (2017) states: "*The performance of the empirical approach is encouraging as it relies only on the ac meter measurement and may be readily applied to historical data, although there are inevitably some inherent assumptions about particle composition that hinder universal applicability.*"

Also from Stockley et al. (2017): "*Methods experience the greatest difficulty providing accurate estimates in highly absorbing waters and at wavelengths greater than about 600 nm. In fact, residual errors of 20% or more were still observed with the best performing scattering correction methods*."

Considering the above findings, the AC9 data are provided with the correction originally proposed by Zaneveld et al. 1994, still appreciating it is far from being the most accurate. In the manuscript this explicitly acknowledged through the comparison of absorption measurements from the AC9 with those from laboratory measurements performed on discrete water samples.

Some of the above elements will be included in the manuscript to support the preference to process the AC9 data applying the correction scheme proposed by Zaneveld et al. (1994).

Comment #10
In fact, for research purposes, it is recommended that the authors share the absorption coefficient uncorrected for residual scattering, so it can be useful material to further investigate this matter.
Reply
As clearly stated in the section on 'Data availability', CoASTS-BiOMaP data not included in the current dataset are accessible upon reasonable request. The objective of the work is to provide open access to processed near-surface data accompanied by a comprehensive description of the field and data handling methods.

Comment #11
On the quantification of the uncertainties coming from the ac-9, certainly the value 0.005 $m-1$ is not a proper estimate. That is a rule of thumb estimate of the instrument precision in the user manual, which is accompanied by the 0.01 $m-1$ accuracy, also in the manual. There is no mention of uncertainty sources related to instrument absolute calibration, non-linearity, determination of the pure water measurement, correction of the temperature and salinity differences and correction of the residual scatter, some others related to the measurement protocol and the individual operator, and even some others that I may have missed. All these sources are likely to result in something bolder than the manufacturer user manual. The authors are expected and encouraged to investigate and comment on these aspects. Otherwise, how does one explain the differences that the authors find in their Figure 5?
Reply
Based on theoretical Monte Carlo computation, Leymarie et al. (2010) provided estimations of the relative errors of 10 to 40% for c_{t-w} and generally lower than 25% for a_{t-w} (5-10% when absorption by in water optically active components is high) but up to 100% for waters showing high scattering.
Stockley et al. (2017) observed relative errors lower than 20% for the "Zaneveld et al. 1994" correction for wavelengths 412-550nm (lower than 10% for wavelengths 412-488nm) and more than 50% for wavelengths greater 600nm. Twardowski et al. (2018) provided an estimate of the "operational" uncertainty (for example, considering 2 calibrated AC9 close one to the other) as low as 0.004 m^{-1} (not taking into account errors associated to the scattering corrections).
Considering these results the manuscript will be revised indicating that the uncertainties in AC9 absorption are larger than 0.005 m-1, and can reach several ten percent in highly scattering waters with more pronounced values in in the blue-green spectral regions.

Comment #12
I also have a few concerns about the Hydroscat backscattering data. First, in lines 331 and 332, what is exactly meant with the annual factory calibration "complemented" by pre-field calibration, in terms of determining the scale factor and the dark offset of the measurement?
Reply
Equivalent to the procedure put in place for the two AC9s used within the framework of the CoASTS-BiOMaP campaigns, also for the two HydroScat-6 there were regular factory calibrations tentatively performed on a yearly basis. The pre-field and post-field calibrations (determination of the spectral "Mu" response curve coefficients and gain ratios) performed in laboratory by the JRC team with a "calibration cube" and a spectralon reference plaque allowed to detect and correct sensitivity changes between successive factory calibrations.
The difference between factory and pre-field calibrations will be clarified.

Comment #13
Equation (4) is the correction for absorption along the pathlength recommended by the manufacturer. However, after investigating on it, Doxaran et al. (2016) investigated on it and found that the "0.4" is a totally arbitrary number. They proposed a more accurate expression instead.
Reply
Doxaran et al. (2016) provided findings on the basis of measurements performed in: *i*. Río de la Plata turbid waters (Argentina, with total scattering coefficient at 550 nm greater than 20 m-1 (average around 50 m-1 ?) and ii. Bay of Bourgneuf Waters (France, with total scattering coefficient at 550 nm greater than 10 m-1 (average around 40 m-1 ?)

Because of this, the empirical relationship by Doxaran et al. (2016) indicating "K_{bb}-a_{nw}=4.34*b_b" (see their figure 5b for the HS-6) refers to values of b_b spanning between "0.0" and 2.5 m^{-1}.
The BiOMaP values of b_b roughly range between 0.0005 and 0.1 m^{-1} (with values of a_{nw} < 1.0 m^{-1}). In this interval of b_b values, figure 5 by Doxaran et al. (2016) shows that simulated values follow a relationship with a much high slope than the empirical fit resulting from the whole range of simulated b_b. Thus, is that empirical fit really more appropriate for the low b_b values found in the data set than the standard relationship used here? For sure, the problem is an open one.
In the manuscript it will be stated that in the absence of any consolidated processing for HdroScat-6 data, the CoASTS-BiOMaP processing was made relying on the equations provided by the manufacturer.

Comment #14
Removal of pure water data is made after tabulated data by either salt water or fresh water by Morel, but the state of the art values are those given by Zhang et al. (2009). Their model is analytical and has an explicit dependency on salinity, so that one may use concurrent CTD data for obtain bbw accurately. Again here, the differences on the final products are likely to be small, but it is preferrable to replace old and biased values with updated ones at zero cost.
Reply
Zhang analytical values are for sure a general improvement. However, in the majority of cases it would have an almost negligible effect on the retrieval of CoASTS-BiOMaP b_{bp}. In the oligotrophic clear waters of the eastern Mediterranean Sea showing salinity values around 38.0-39.0, the difference in Beta(90degrees) between Morel (1974) and Zhang et al. (2009) is very low, *i.e.,* approximately 0.00004 m^{-1}.
The manuscript will make mention to the alternative of applying Zhang et al. (2009) instead of Morel (1974).

Comment #15
As for the ac-9 data, estimating an uncertainty of 0.0007 $m{-}1$ for bbp is wishful thinking. True uncertainties are much larger than that and are the result of a number of factors like those listed above. Can the authors look for a more realistic value based on their own research or in literature?
Reply
The value of 0.0007 m-1 was estimated by Whitmire et al (2007). The actual uncertainty is expected to be higher and dependent on many factors related to processing hypotheses (like the correction for attenuation along the pathlength evoked above). An further uncertainty source is that related to the choice of the "chi" value for converting Beta140 into bb: the standard value used here was 1.08 but Berthon et al. (2007) found that, for the Adriatic Sea, a more appropriate value (based on VSF measurements) was 1.15(+/- 0.04). Also in this case it can be said that more work would be needed.
In the manuscript the uncertainty of 0.0007 $m{-}1$ will be stated to be a minimum value, but likely to be much larger due to variability of some of the processing hypothesis.

Comment #16
On the absorption from water samples, the paragraph of lines 378-380 is confusing to me. Probably it needs rephrasing. Maybe the authors mean that the absorption of particulate material between 0.2 and 0.7 micron is negligible with respect to the fraction larger than 0.7 micron? If so, is there some evidence of that in data or literature?
Reply
The text simply states that the absorption budget misses some components that cannot be captured due to difference in pore-size of the filters used produce samples for dissolved and particulate matter absorption analysis. It is also added that likely the missing contribution is not big.
The text will be slightly revised and a citation to Morel and Ahn (*J. Mar. Res. 1990*) *will be added.*

Comment #17

CDOM measurements - usage of a 10 cm cuvette inside of a spectrometer is known to be suboptimal in oligotrophic areas like the Mediterranean Sea, even the western basin and in winter. Water is simply too clear to provide a clean spectrum at visible wavelengths. I understand that there is nothing that the authors can do to overcome this issue in case they did not use better suited instruments (like Ultrapath), so at least, an acknowledgement is needed that measurements were performed in suboptimal conditions.

Reply

It will be acknowledged that the accuracy of CDOM in oligotrophic clear water is definitively challenged by the short path-lengths of the laboratory spectrophotometers used for absorbance measurements.

Comment #18

Next type of comments is on the data present in the dataset. It is written (lines 507-511) that basic quality control criteria, like Kd to be higher than the clear water theoretical value (aw+bbw?), were required for a measurement to be included in the dataset, but I have plotted all Kd values and I see that many spectra are less than such value, and some even negative, see Fig. 2. I have repeated the analysis for KL and Ku and I have found the same issue (not shown). Same for some absorption data. Regarding bb, all values are positive, but when removing the water contribution following Zhang et al. (2009), many derived bbp values are negative. Although the number of bad spectra may be marginal, this reduces the confidence that this dataset aspires to; so this needs attention before making the public release.

Reply

The two quality indices provided for K_d and b_b spectra are obtained from the subtraction of a constant K_w value at 490 nm (0.0212 by Smith and Baker, AO 1981) and a constant b_{bw} value at 488 nm (0.000161 m-1 for salty water by Morel, Optical Aspects of Oceanography, 1974) from the corresponding K_d(490) and b_b(488) values. These indices do not have any impact on the data themselves, their negative value simply suggests some caution.

These indices were mostly introduced to support the use of data from highly oligotrophic clear waters by identifying questionable spectra challenged by the water type and the applied measurement method. Any user can ignore, use or re-compute those indices and consequently drop whatever spectrum is later judged ‘bad’. Still, the relatively small number of these spectra challenged by measurements methods applied in a critical measurement condition, cannot become the reason to question the data set.

The values of K_w and b_{bw} applied to determine the quality indices will be provided and some additional detail will be added.

Comment #19

On the phytoplankton absorption data and the chlorophyll concentration, I have plotted one against the other in Fig. 3 at 665 nm, with a highlight on the Eastern Mediterranean data. What I see is that there is the expected tight relationship, but I am concerned about a drop in sensitivity that I see in the lower end. The chlorophyll data has an evident trend towards saturation at about 0.03 – 0.04 $mg\ m{-}3$, which is too high to resolve the variability in the oligotrophic oceans. I have overplotted the public data by Valente et al. (2022) and, for the few dots in the lower part, I see that the general linear trend is continued. So, authors may try to explain, and if possible, solve this issue.

Reply

As already stated, the highly oligotrophic clear waters of the Eastern Med sea challenge the absorption and scattering methods applied. Clearly the same water type may also affects the accuracy of the derived Chla concentrations. This may certainly explain why a few (4-6 points) in the a_{ph}(665) versus Chla plot suggest saturation for the lowest Chla values. This is what the data set can provide for highly oligotrophic clear waters. Still, away from arguing with the Reviewer, his plot including an additional open access data set, only shows 3 points out of thousands below the questioned Chla values.

A statement on the challenging measurement conditions offered by highly oligotrophic clear water conditions of the Med Sea will be restated for Chla data too.

Comment #20
The dataset is optically complete, and therefore something that I am missing in the paper is an Rrs closure exercise. A high degree of closure helps to increase the confidence on the dataset. In the case that large differences appear, the individual sources have to be inspected. The authors have provided a closure exercise for absorption, which is appreciated, and where significant differences appeared. For Rrs, I have done the closure exercises myself for absorption both from the ac-9 and from the water samples. This is done in Figure 4, for the ac-9 and in Fig. 5, for the water samples. To calculate Rrs in both cases, Lee et al. (2011) model was used. Considering the radiometric data as reference, results seem to indicate that absorption from ac-9 delivers quite clean data and closure seems very good in general. On the other hand, there are clear differences when absorption from the water samples are used. The plot suggests that absorption from the water samples is much noisier at blue wavelengths and tends to underestimate the real value.
Reply
The Reviewer is acknowledged for his effort to produce closure exercises using the CoASTS-BiOMaP data.
The authors consider this further analysis beyond the objectives of the manuscript.

Comment #21
Final comment is related to the data presentation in the article. It is nice to see the spectra and the ternary plots, and readers can have an idea of the water types that are represented. There are many ways to present the dataset, here just a few that might be of interest to the reader:

Crossed relationships among IOPs

Rrs vs. of chlorophyll, compared to the global relationship

Kd vs. of chlorophyll, compared to the relationship by Morel

Chlorophyll vs. the other two water constituents

One Rrs band ratio vs. another one

TSS vs. $Rrs(665)$

Reply
Thanks for all the suggestions, clearly feasible, desirable and hopefully interesting. However, the manuscript aims at presenting the data set with some analysis, and not exploiting its content in any possible direction: major extended analyses are not requested for a manuscript submitted to ESSD with the objective to introduce a data set.

Minor comments
I think it is a requirement that the link to the dataset is shown in the abstract too.
Line 21: “applied equal” → used equally.
Line 39: “benefited of” → benefited from.
Line 54: “moderately” →moderate
Line 91: “attempting”: very vague term. What does it mean in this context, precisely?
Line 96: probably a link to the IOCCG protocol will help here, for those interested.
Table 2: the two-letter country code chosen by the authors looks arbitrary. There is a standardized one named ISO 3166-1 alpha-2, which I advise to follow.
Line 124: talking about in situ vs. laboratory measurements is confusing. Laboratory measurements are made on part of the in situ data. I prefer to talk about field instrumentation vs. laboratory measurement of field samples.
Line 209: no need to say “so called”, as this name is well consolidated and known by everybody.
Line 343. “Wattman” → Whatman
Reply
All relevant corrections will be made. Thanks.