Ocean Science
Discussions

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Interactive comment on "The Mediterranean Ocean Colour Level 3 Operational Multi-Sensor Processing" *by* G. Volpe et al.

Anonymous Referee #1

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This is the review of the manuscript "The Mediterranean Ocean Colour Level-3 operational multi-sensor processing" by Volpe et al. The paper is mostly a description of the near-real-time/delayed-time processing of ocean color data in the framework of CMEMS. Results are mostly based on a validation analysis. Overall, the text reads more like a project report than a scientific paper, even if intended for a special issue on the European Copernicus Marine Service. It is actually incomplete or confusing in its description of various aspects of the processing (as shown by the list of secondary comments). So the text should be thoroughly re-organized to aim at clear descriptions (starting with the actual objectives of the paper) and the 'scientific' part should be reinforced before being considered for publication. These points are further developed below.

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Discussion paper

As demonstrated by the list of secondary comments, the description of the processing is often unclear and the pertinence of some processing steps is insufficiently supported. I understand that the authors ran into objective difficulties in documenting the processing chain with some elements that are fairly technical and apparently not fully described in literature (bow-tie effect, removing outliers, bias correction, smoothing. . .). I am wondering if some elements are described in more details in CMEMS (or other) reports / ATBDs that could be cited while simplifying the description of technical elements. An alternative is to use this paper as an opportunity to justify the choices made in the various processing steps and focus the work essentially on these. After the 'technical' part, the 'scientific' part is restricted to 2 pages (out of 13 pages of text) and could easily be reinforced with more discussion (currently there is very little description and discussion of the results, no comparison with published validation results, . . .). Based on the objectives of the paper, the authors should choose which part ('technical' or 'validation) to strengthen.

Besides the various points listed below, a more general lack of information can be noted when it comes to the comparison of processing chains in the validation analysis, CMEMS processing versus OC-CCI. The paper says that the CMEMS product used for validation is the near-real time (NRT) output. For me, this would mean that the data used in the validation analysis are those obtained in NRT mode, preserved as the processing went (i.e., computed with preliminary ancillary data, calibration at the date of acquisition, with climatology computed with very little data, etc. . .). In that case validation results reflect the quality of the data as downloaded NRT by users. Otherwise they are DT data, or even fully reprocessed data if they result from a consistent processing all the way through the time period (in terms of calibration, climatology computed with multiple years. . .). This (and implications) must be made clear in the manuscript (actually, how the validation results evolve as data are brought from NRT to DT and to reprocessed data is an interesting point). Besides the mode (NRT, DT) actually associated with the CMEMS data used in the analysis, more discussion should be given comparing those with the CCI data. The manuscript forgets to mention other

large differences between the CMEMS and CCI processing, including the atmospheric correction for certain sensors. In the extraction step, the grids of the products are also different (1-km versus 4-km if I am not mistaken). The study should clearly identify all possible sources of differences between the CMEMS and the CCI stream.

I have an issue with the field data. They are introduced in the study to explain the MedOC4 algorithm and the validation analysis. In such a context, a word on the uncertainties associated with the data would be necessary. Not clear to me is the distinction between the data serving for the development of MedOC4 and the data used for validation with match-ups (points of the former going into the latter are not fully independent validation points). As they are used for validation, BOUSSOLE and AERONET-OC data should be described a bit better. While I'm not familiar with the BOUSSOLE data distribution, there is a clear data policy for use of the AERONET-OC data (offer of authorship if I'm not mistaken) and I'm wondering if this has been respected (there is not even an acknowledgment in the manuscript).

Below are detailed comments, with requests for clarification/corrections and suggestions for improving the text. I'd recommend numbering the sections and sub-sections.

Page 1

l.10: I'd suggest: "multi-sensor processing applied to the Mediterranean Sea by the Ocean Colour Thematic Assembly Centre of the Copernicus. . .": The abstract should be readable by readers who don't know about CMEMS, TAC, . . .

l.11: "A basin-scale. . ."

l.12: "to fine-tune"

l.14: "than those"

l.15: "The Mediteranean. . .": information associated with this sentence should be relocated in the beginning of the abstract.

C3

l.21: "CMEMS delivers..." rather than 'includes'

l.27: "users who"

l.28: please define all acronyms at first use.

Page 2

l.2: "near-real time (NRT) and delayed time (DT) modes."

l.3: in OC jargon, monthly data computed from daily data are still termed L3.

l.15: is there a reference for this approach?

l.17-18: heavy sentence about 2 important benefits; should be reworded.

l.20: "are derived"

l.23: "is foreseen": so it is still not the case, which is at variance with the abstract.

l.26: "DT data"

l.26: "precise": what does this indicate exactly?

l.27: "both to be accurate": does it mean DT and REP? then the sentence should be restructured. What does 'consistent' mean here?

l.27: "For the sake of timeliness..."

Page 3

l.4: "This work...": this paragraph describes the structure of the work, but the primary objective of the whole study is not clear. Page 2, line 24, "one of the aims" is mentioned, but how does it fit here, and what are the other aims?

l.14: "relies on an in situ..."

l.18: odd sentence; it means "absorption due to CDOM, absorption due to algal and non-algal particles, absorption due to TSM, and both AOPs and IOPs." absorption is

part of the IOPs, so which are the others measured?

l.20: are the in-situ IOPs used in this study?

l.30: are the authors sure about the choice of acronym . . . a large part of the scientific community is sufficiently well-versed in latin languages to understand the meaning of the word. . ..

Page 4

l.2: "normalised by"

l.3: Kl and Ku should be defined.

l.9: "using the primary sub-surface quantities, it is then"

l.10: "such as the Q-factor"

l.18: "Chl": are Chl data from BOUSSOLE used in this work?

Page 5

l.6: "using the OC-CCI. . ."

l.7: this sentence reads: "OC-CCI. . . at 1-km . . . rather than at 4-km for OC-CCI": unclear.

l.14: "Single-sensor pre-processing for NRT/DT modes" I presume.

l.15: "quality-checked": what does it entail?

l.15: I assume that the atmospheric correction applied in these cases is l2gen. This should be mentioned together with an appropriate reference.

l.22: "that reflects". . . "stripes originate"

l.32: "the dimension . . .": it happens also for the other sensors, doesn't it?

Page 6

l.2: "these missing values": what is the benefit of this step?

l.6: and what about SeaWiFS and MERIS?

l.6: "space agencies": in that case it is only NASA.

l.7: "atmospheric correction failure": this is a strong step; I would assume that Rrs associated with that flag is just not usable as the AC failed according to the software. What is the criterion used by the software to consider the AC a 'failure'? what is then the status of Rrs? The authors should also support this decision with robust evidence that the resulting Rrs is actually valid.

l.8: "to avoid"

l.8: "salt and pepper": please define what this refers to.

l.9: "removing all isolated pixels…": in general this part needs clearer explanation. How is an "isolated pixel" defined?

l.13: please provide characteristics of the map (extent, resolution).

l.17: "spectra" from different missions?

l.21: "differ by"

l.25: "In general": may be removed.

l.30: "in theory": and in practice? Might be removed.

l.31: "apply algorithms to derive geophysical products"

Page 7

l.3-5: not clear what the idea is here.

l.7: "Differences between MODIS and VIIRS"; and what about SeaWiFS and MERIS? In general some aspects of the manuscript associated with NRT/DT seem focused exclusively on MODIS/VIIRS (that has some logic since they are still active) but how

SeaWiFS and MERIS are handled should also be described as the related products are used in the validation analysis.

l.11: "normalized": does the BRDF correction make use of the OCI Chla value? In that, it is not consistent with the MedOC4 values. This should be acknowledged.

l.17: "we tested": by doing what?

l.19: "inter-calibration": it is certainly a factor but it is not the only one that could apply. I would still argue that geometry can play a significant role in the differences. Operating the atmospheric correction with different bands might also have an impact through the AC code (eliciting different responses by the AC code and its assumptions/simplifications).

l.22: it does not seem that the bias correction operated by CCI is described in that reference.

l.25: "climatological": what are the periods used for each sensor? is it the same?

l.28: "two steps": but then 3 are listed.

l.29: "temporary" ?

Page 8:

l.1: aren't the same equations used by CCI?

l.5: "weight w"

l.8: "and time": not necessary.

l.8: "as for OC-CCI": this should be written before. I think CCI also operates some type of spatial averaging.

l.12: "deemed insufficient": on the basis of what?

l.13: "40%": what is the reason for so many missing values at 670 nm? Negative

values? But earlier in the text, it is written that the quality check removes all incomplete spectra (if Rrs is <0 at any one band). So at that point, working with incomplete spectra for match-ups is inconsistent with the processing chain.

l.22: which "method"?

l.24: "are only the result of the merging procedure"

l.26-29: I have to say that I don't understand how the approach works. Volpe et al. (2018) not being published, the explanation should be clearer. For instance, is the climatology field computed as in the previous section? And then it is not clear how the smoothing operates.

Page 9

l.2-8: The examples are not so obvious to me. I don't even see the Rhone plume on any map (should be a pattern leaving the coast…).

l.12: "In both cases": this is true for the cases shown but is it a general result? In any case, there is no reason to expect that the bias-corrected merged data would be closer to the field data than a simple average.

l.15: "The climatology field is obtained…": climatology is also mentioned in the 2 previous sections (bias correction, merging), so it is not clear what this SeaWiFS Chla climatology is for, nor why it is computed in a different way.

l.20: "this has been estimated": how? (or where?)

l.22: "To overcome…": but this type of filtering was already introduced for the Level-2 data. Do outliers appear again later in the processing ? Speaking of 'biases' here is not appropriate.

l.32:"Even though the latter now show performance comparable to that of empirical algorithms…"

Page 10

l.1: "in discussing the characteristics that data must have to be used. . .."

l.2: "pointed out that . . ..": the sentence should be re-written.

l.2: "theoretical": to be replaced by 'semi-analytical' (what is a theoretical algorithm?)

l.10: "weather conditions"

l.15: "To identify. . . the identification. . .": sentence to be re-written.

l.17: "average spectra": the covariance matrix is also needed for such an approach.

l.21: "user survey": refers to the CCI user consultation?

l.6-24: in general I think this paragraph may be too long as the approach has already been well described in literature. Regardless of length I don't find it clear for a reader without a prior knowledge of the method.

Page 11

l.12: "unable to perform"

l.15: "Inherent Optical Properties": I was therefore expecting validation results for the IOPs while there are none.

l.18: "in most instances": some example stats would be appropriate.

Page 12:

l.2: "used the NRT": meaning data processed as they were in NRT conditions (with preliminary ancillary data, calibration known at the time of acquisition, preliminary climatology, etc. . .)?

l.12: "common spectral behavior": is this a point that could be discussed?

l.18: "significantly": was any statistical test performed?

Page 13

l.4: "significant"; what does it mean here?

l.5: "In the NIR": in the red?

Table 4: why are there less matchups at 670 nm (I understood that only complete spectra were kept)? The fact that the Multi and CCI products have a different number of match-ups should be discussed.

l.12: "NRT": or DT?

Figure 1: no so easy to distinguish validation from development points.

Figure 3: remind that the climatology fields are from a daily climatology.

Figure 5: "Pope and Fry 1972"?

Table 5: writes 'REP' versus 'Multi' while Table writes 'CCI' versus 'Multi'; coherence is needed to avoid confusion. While Table 4 shows different numbers of match-ups for the 2 products, here the numbers are the same. Is there an explanation for this?

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