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Biogeosciences Discussions



# *Interactive comment on* "The 1 % and 1 cm perspective in deriving and validating AOP data products" *by* S. B. Hooker et al.

**E. Boss (Referee)**

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Review of: The 1% and 1cm perspective in deriving and validating AOP data products by S. B. Hooker, J. H. Morrow, and A. Matsuoka.

Reviewer: Emmanuel Boss, University of Maine

This paper has for objective to introduce new technology to measure radiance in the ocean, demonstrate the advantage of this technology (vertical resolution, spectral range), and demonstrate that this technology could be used to obtain the absorption of dissolved materials from ratios of diffuse attenuation coefficient.

This paper is of interest to the readers of BG and present new and exciting results. However have some major comments on the manuscript that I feel should be dealt with



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Interactive Discussion

Discussion Paper

in order to make it more readable and useful to the community. I outline them below. In addition I am returning an annotated PDF where more minor comments are provided.

Major comments: 1. The link of this paper to the Malina special issue is tenuous at best. One of the data sets used, was collected during the Malina campaign. Very little (if any) in this manuscript has anything to do with biogeochemistry of the Arctic. This is, in essence, a method paper. There are specialized journals for such papers (as the author are well aware).

2. The paper is too long spending a lot of space with lengthy description of the need for and advantages of the technology used here, as well as its history. I feel that these section are not scientific and read like marketing materials. Please, if you want this paper to be read and appreciated, minimize these sections.

The introduction section is not related to the science content of the paper and contributes little to it. Same is true with the 'Next generation perspective' section. It is sufficient to state the need to expand radiometry to coastal environment, to UV and NIR (for better separation of CDOM from phyto, to deal better with particle backscattering) to make the case that higher vertical resolution is needed (due to higher attenuation levels in coastal waters and in the UV and NIR in open ocean waters). The section entitled: 'a kite shaped profiler' reads like a sales pitch. Please focus on the salient features of the technology that are relevant to this paper citing the appropriate papers that demonstrate those features (e.g. closure with technologies of demonstrated accuracies etc'). The development history is not relevant to the science at hand.

3. Several claims of the authors lack validation (unless provided elsewhere in the cited papers). In particular the 1cm vertical resolution requires that non-hydrostatic effects be taken into account (hydrostatic sensor cannot provided accurate sea surface due to water acceleration in the presence of waves). Unless you are using a laser or other device to obtain the distance from the water surface, I think you have to do significant more work to convince your audience of your ability to do distance within

cm in an ocean where waves are present. Do you convert pressure to depth using the local density measured by a collocated CTD? Additionally, Kd is known to change the most near the surface, where effects of scattering (diffusing) and absorption (focusing) compete in reorienting incident light (for a layer of constant IOPs). This is well known and with the addition of the effects of wave focusing (mentioned in the paper) will make near surface estimate of Kd within 1% extremely challenging (and depth dependent).

4. The analysis of the sensitivity of the Kd spectra could be made much more concise if presented in optical rather than physical depths (=Kd x z). In that space wavelengths will be more similar (see pages 9506-7).

5. Rˆ2 is not a measure of accuracy. It is greatly influenced by dynamic range in the data and provide no information on how well the model fit the data. Statistics such as average or root mean square deviation or root mean square relative deviation (ratio of deviation from model to magnitude of variable) provide the information regarding how well we can expect a model to perform (e.g. provide a_g(440) within 20% or +/-0.1mˆ-1). Rˆ2 does not.

6. The term optically deep is most often used to denote regions where the bottom makes not contribution to ocean color. In this paper it is used to denote waters with low attenuation. This is not consistent with the literature (p. 9510).

7. Water classification using Kd have a long history (e.g. Jerlov water type). Since you are continuing in this research line it is appropriate to cite these studies.

8. K_d is presented as the best way to study in-water optics (e.g. for classification) w/o providing the well-known limitations of K_d: 1. Variable, even in homogenous waters. 2. Susceptible to wave focusing. 3. Can only be measured during the day and in the part of the water where there is sufficient light. If you go to such length to promote radiometer as a tool to study CDOM, it will be nice to provide a balanced perspective including the disadvantages of the technology compared to, say, a $3,000 CDOM fluorometer or a transmissometer with a filter on the intake.

9. There seem to be little in term of explanation as to why the ratios of K_d in the UV and NIR should correlate (albeit not linearly) with ag(440). It will be useful of the authors started with a simple model (e.g. Kirk's) where K_d=(a+bb)/mu and explored the dominating terms that may cause the observed relationship. There do exist turbid estuaries where absorption by non-algal particles dominates that by CDOM (e.g. Estapa et al., 2012, L&O). Should we expect the observed relationship to work there as well? Having more theoretical background will help establish the likelihood that the results provided can be generalized beyond the two environments where they were used.

Dear authors: I am often wrong. If you feel that any of my comments is wrong please feel free to contact me. If proven wrong, I will be happy to change it.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C3580/2012/bgd-9-C3580-2012-supplement.pdf

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