Dear Sirs:

Thank you for the opportunity to participate in the *Biogeosciences* review process. The paper I am the corresponding author for was originally titled, "The 1% and 1 cm Perspective in Deriving and Validating AOP Data Products" (BG-2012-132) by Stanford Hooker, John Morrow, and Atsushi Matsuoka. On 5 October 2012, I had a teleconference with the Guest Editors for the Malina Special Issue, Drs. Marcel Babin and Simon Bélenger, regarding conflicting and difficult to interpret reviewer comments—*evidencing at times a recurring loss of objectivity*—obtained during the discussion period. A principal difficulty was that the general overview paper presenting the apparent optical properties (AOPs) of the Canadian Arctic as determined by the Malina campaign, which BG-2012-132 hoped to ultimately reference, did *not* emerge as originally planned.

The desire of myself and my co-authors to withdraw our manuscript was discussed during the teleconference. The Guest Editors requested that the manuscript *not* be withdrawn, which I agreed to, and instead suggested the following three additions to make the paper compliant with the objectives of the Malina special issue: a) a paragraph in the introduction explaining Malina, b) a figure showing in-water spectra and water type classifications, and c) a map showing stations and classifications. In addition, I agreed to help with the production of a general overview paper of AOPs, which would be drafted by Dr. David Antoine with assistance from myself, Dr. Simon Bélenger, and Dr. Atsushi Matsuoka.

A follow-on teleconference on 5 November 2012 regarding the emerging content of the general overview paper resulted in the BG-2012-132 paper being split into two parts: some of the original material was to be moved into the (Part I) general overview paper, and the remainder would be a (Part II) specific example of applying the AOP observations to end-member analyses for algorithm development and validation (plus the original instrument development and sensitivity analysis work, which established the degree of care needed to measure the spectral end members). The new (Part I) paper was planned to make use of aspects of how the original paper organized the data and optically classified the sampling. Consequently, it was anticipated that the new (Part I) paper would discuss aspects of the original (Part II) paper in some detail. To prevent redundancies, while at the same time fulfilling some of the reviewer comments that wanted aspects of the original paper revised (both in terms of expanding and reducing content), the Part I and Part II papers would be constructed as a linked pair of papers fulfilling the requested changes by the Guest Editors, as well as the Reviewers comments that were deemed objective.

On 9 November 2012, I sent an e-mail message to Dr. William Miller explaining how and why the BG-2012-132 paper was being split into two parts, and he approved the plan. Also, on 4 January 2013 I received a message from Ms. Anne Brekerbohm that the BG-2012-132 manuscript title had been changed to reflect the two-part plan. Below are the responses to the specific Guest Editor and Reviewer comments regarding the original BG-2012-132 manuscript with respect to the two papers it has become:

- "Apparent Optical Properties of the Canadian Beaufort Sea, Part I: Observational Overview and Water Column Relationships" (which contains a minority of the original submission), and
- "Apparent Optical Properties of the Canadian Beaufort Sea, Part II: The 1% and 1cm Perspective in Deriving and Validating Data Products" (which contains the majority of the original submission).

For brevity, the two papers are hereafter referred to as Part I and Part II, respectively. The authors' replies to the *major* comments received during the discussion period are split into three parts: a) the comments from the Guest Editors, b) the comments from Reviewer 1, and c) the comments from Reviewer 2. For the latter two, there were many comments that were difficult to decipher or apply, because they were not complete sentences, did not suggest a revision, were incorrect, or were lacking in scientific objectivity. When multiple comments about an aspect of the paper were made, particularly if some of the comments were sentence fragments, they have been collected into a category representative of the gleaned intent of the fragments. In addition, the manuscript was revised to respond to *minor* comments embedded in the annotated PDF files posted by the reviewers.

Guest Editor Comment 1: Add a paragraph in the introduction explaining the Malina activity and the field campaign.

Authors Response 1: The authors agree and have added the requested paragraph with appropriate summary details. In keeping with follow-on comments below, and the anticipation now that the overview paper will appear as planned, this paragraph was kept short.

Guest Editor Comment 2: Add a figure showing in-water spectra and the water type classifications determined in the original manuscript.

Authors Response 2: The authors agree and have added the requested figure plus supporting text, which highlight the end-member analyses and the legacy versus next-generation remote sensing perspective with regards to the classification scheme.

Guest Editor Comment 3: Add a map showing stations and classifications.

Authors Response 3: The authors agree and have added the requested figure plus supporting text, which introduce the classification scheme.

Reviewer 1 Comment 1: The reviewer believes the link of the paper to the Malina special issue is tenuous.

Authors Response 1: The authors were anticipating the ability to link their work with a promised general overview manuscript that never appeared, but to be timely in the submission requirements, submitted their paper with the expectation that that overview paper would ultimately emerge. The missing manuscript is now being drafted using parts of the authors' manuscript plus other contributions from the lead Part II author. The authors original manuscript has been split into two parts as noted above.

Reviewer 1 Comment 2: The reviewer believes the technological descriptions, the introduction, and the next-generation perspective sections can be reduced in length while improving the focus on "the science at hand."

Authors Response 2: Aspects of this comment are contradictory with other comments, which request more technical information regarding selected aspects of the instrumentation. Consequently, the authors adopted a balanced approach wherein *all* manuscript sections were shortened while trying to briefly expand additional technical compositions to deal with other comments. At the same time, the identified sections were revised with a greater emphasis focusing on the scientific pursuits of the manuscript. The authors disagree with the assertion that there is no need for a firm connection between the technology and the science derived from using the new sampling system. The first and second authors maintain a data processor accessible to the community and have seen first hand how ignorance of the important technological features of scientific instrumentation can lead to needlessly degraded data collection and an inability to conduct the kind of analyses presented in the manuscript. The authors believe an elevated awareness is needed within the community of practice to prevent an avoidable loss in quality, and wanted this manuscript to provide clear lessons and documentation as to what can be achieved if the sampling protocols and capabilities of the instrument are strictly followed. In the spirit of the reviewer's comments, aspects of this objective have been retained in a more balanced perspective.

Reviewer 1 Comment 3: The reviewer questions whether or not there is proper validation for the performance criteria established in the manuscript, e.g., the 1 cm resolution capability. The reviewer points out that K_d is known to change the most near the surface where the effects of scattering and absorption compete in reorienting the incident light within a homogenous layer, which is confounded by the effects of wave focusing and will make the near-surface estimation of K_d to within 1% extremely challenging.

Authors Response 3: The manuscript has been modified to provide significantly more information concerning the resolution that is achievable with the new profiler in terms of the technical specifications (e.g., with respect to the pressure transducer) and what was actually achieved during the Malina campaign. This discussion includes more data from other sensors on the profiler, and the use of more information dealing with the extrapolation intervals.

Reviewer 1 Comment 4: The reviewer states the analysis of the sensitivity of the K_d spectra could be made much more concise if presented in optical rather than physical depths ($= K_d \times z$). The reviewer asserts that in the optical depth space wavelengths will be more similar and cites pages 9506-7 of the original manuscript.

Authors Response 4: The referenced pages deal with Fig. 2 of the manuscript, which is based on the sensitivity analysis of the water-leaving radiance, $L_W(\lambda)$, and band ratios of the remote sensing reflectance, $R_{\mathrm{rs}}(\lambda) = L_W(\lambda)/E_d(0^+,\lambda)$, where $E_d(0^+,\lambda)$ is the global (i.e., direct plus diffuse) solar irradiance measured with a separate above-water sensor with matching spectral channels to the in-water sensors. No sensitivity analysis was performed on $E_d(0^+,\lambda)$, so the analyses were concerned with the sensitivity of $L_W(\lambda)$ or the ratios of $L_W(\lambda)$ used in a standard chlorophyll algorithm (OC4V5) to a variety of circumstances wherein the sampling protocols were not followed. Within this context, there are three undecipherable aspects regarding the reviewer's comment:

1. None of the variables involved with the sensitivity analyses in Fig. 2 are $K_d(\lambda)$ nor use $K_d(\lambda)$ in their formulations, so the reviewer apparently misinterpreted the optical properties being discussed in Fig. 2 and inherently assumed the wrong variable;
2. The original Figure 3, which is the only other figure that presents sensitivity analyses is also based on $L_W(\lambda)$ and $R_{\mathrm{rs}}(\lambda)$, and does not make any use of $K_d(\lambda)$; and
3. The proposed $K_d \times z$ normalization term is a dimensionless quantity without a physical unit and is, thus, simply a pure number that is not useful to the proposed normalization.

Nonetheless, the authors address the optical depth concept in "Authors Response 10" for Reviewer 1 below, because the reviewer made a suggestion or comment on more than one occasion.

Reviewer 1 Comment 5: The reviewer asserts R^2 (the coefficient of determination) is not a measure of accuracy, and recommends the root mean square error (RMSE) be used instead.

Authors Response 5: The authors believed the physical presentation of the data in graphical form (either in the manuscript or referenced documentation) along with the R^2 value would convince the reader as to the accuracy of the derived results or algorithms, because both R^2 and RMSE use the sum of squares due to error (SSE): $R^2 = 1 - \mathrm{SSE}/\mathrm{SST}$, where SST is the total sum of squares, and $\mathrm{RMSE} = [\mathrm{SSE}/\nu]^{1/2}$, where ν is the residual degrees of freedom. The authors have added the RMSE statistic to the citations in the manuscript where fitting variables are discussed.

Reviewer 1 Comment 6: The reviewer asserts the term optically deep is most often used to denote regions where the bottom makes no contribution to remote sensing, whereas in the manuscript it is used to denote waters with low attenuation. The reviewer points out this is not consistent with the literature and cites page 9510.

Authors Response 6: The authors agree and the cited material has been revised to use *radiometrically* deep and shallow to avoid confusion.

Reviewer 1 Comment 7: The reviewer states water classification using K_d has a long history (e.g., Jerlov water type), and because the manuscript continues this area of research, it is appropriate to cite relevant prior studies.

Authors Response 7: The water classification scheme used in the paper was not intended to compete in any way with Jerlov's work, which did not directly include waters as turbid as those sampled during Malina or as extensive a spectral domain. The adopted scheme was intended to make the sensitivity analyses more accessible by partitioning the large dynamic range of the problem into smaller parts, so some generalized conclusions could be provided with easy to understand procedures (i.e., blue, green, and red reflectance peaks). The authors agree their work could be used within the context of Jerlov's schemes and have modified the relevant text along with the appropriate Jerlov citations.

Reviewer 1 Comment 8: The reviewer asserts K_d is presented as the best way to study in-water optics (e.g. for classification) without providing the well-known limitations of K_d: a) variable, even in homogenous waters; b) susceptible to wave focusing; and c) can only be measured during the day and in the part of the water where there is sufficient light. The reviewer goes on to state (verbatim), "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."

Authors Response 8: The authors believe this is a specious set of comments. The authors do not state K_d is the best way to study in-water optics—indeed, all the sensitivity studies are conducted using L_W, which the reviewer appeared to *not* understand. The manuscript simply provides a capable technique to derive a water constituent that was derived in complex arctic water masses and then validated in a variety of other complex water masses (the Gulf of Maine and several of its rivers and tributaries, as well as parts of the watershed for the Chesapeake and Delmarva Bays). There are pros and cons to all measurements, and the manuscript correctly points out the difficulties with wave focusing and how C-OPS was purposely designed to overcome wave focusing effects by improving the number of high-quality observations that can be collected in near-surface waters.

Furthermore, the first and second authors have been using C-OPS—*which has 10 decades of dynamic range as is stated in the manuscript*—to collect AOP measurements under the full Moon (Fig. 1), so it is incorrect to say K_d can only be measured during the day. In fact, the capabilities of the instrumentation documented in the manuscript are sufficiently sensitive to allow full Moon observations to be extended to include the time period between the Disseminating and Gibbous Moon. As for the variability problem, many parameters exhibit this attribute (e.g., chlorophyll and pigment concentrations), so it is not unique to K_d. It is certainly true that sufficient light is needed, but this is the whole point of the paper: to support next-generation remote sensing, which involves near-surface waters where there is sufficient light. As for the comment about documenting the disadvantages of the technology compared to readily available alternatives this contradicts the prior comment requesting more focus on the science and less on the technology. The purpose of the paper was not to evaluate competing technologies, but rather to use the measurement of AOPs to advantage, because the satellite makes the same measurement.

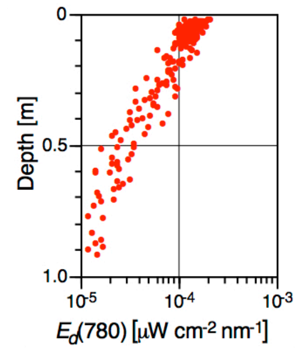

Fig. 1. The vertical profile of $E_d(780)$ under the full Moon.

Reviewer 1 Comment 9: The reviewer states the manuscript provides little explanation as to why the K_d ratios in the UV and NIR should correlate (albeit not linearly) with $a_{\mathrm{CDOM}}(440)$ and suggests the authors should embark on a modeling effort to provide an explanation. The reviewer goes on to posit that turbid estuaries exist wherein absorption by non-algal particles dominates that by CDOM and wonders if the observed relationship will work there as well. The reviewer concludes by asserting that a more theoretical background will help establish the likelihood that the results provided can be generalized beyond the two environments where they were used.

Authors Response 9: The authors disagree the manuscript does not explain why the derived algorithm works as well as it does, because the manuscript discusses how the selected two-channel K_d ratio captures the absorption and scattering processes that determine the attenuation of seawater. It is not a required burden for scientific discoveries to be explained from a modeling perspective, indeed many are not. The authors did not set out to write a modeling paper, and this comment is contradictory to the stated request of making the paper more focused and shorter. The adopted approach works well enough to deserve publication, so the authors did not include a modeling component to the manuscript.

The validation exercise was not restricted to just "two" environments and, in fact, included the Canadian Arctic, the Gulf of Maine involving multiple environments, plus multiple environments in the Southern Mid-Atlantic Bight (see Authors Response 8). In addition, the techniques documented in the manuscript have been used in the analysis of other challenging environments, since the submission of the paper. As one example, Fig. 2 presents the $K_d(320)$ versus $K_d(780)$ relationship for a transect of stations into the Black Water (BW) National Wildlife Refuge for the Chesapeake Bay, for which two data types were collected: a) the open expanses of the coves, estuaries, and embayments (green); and b) source waters from the BW transect (red), which included sampling in marsh waters as shallow as 1.5 m. BW stations have a large range in $K_d(320)$ values, with large source water values (BW 55) that fall along a mixing line (red) that is distinct from the rest of the Bay until the end of the transect in open waters (BW 63). A comparison of this plot with two similar analyses provided in the manuscript provides additional confirmation that the

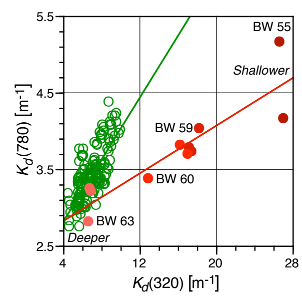

Fig. 2. The $K_d(320)$ versus $K_d(780)$ relationship for a transect into the Black Water refuge.

techniques developed in the manuscript have greater applicability and are worthy of publication. This new material was not added to the manuscript, because it is the subject of a different journal article that is being prepared for publication.

Reviewer 1 Comment 10: Suggestions were made to cast aspects of the sensitivity analyses and presentation in terms of optical depth.

Authors Response 10: The authors agree there are alternative analytical approaches to the sensitivity analyses and chose the very accessible approach of using a real depth displacement (z). The authors disagree about the need to change this approach for many reasons. An optical depth approach would add another, potentially confusing element, to an already multidimensional problem. The $K_d(\lambda)$ values (which have units of inverse meters) do not represent a position in the water column, and individually represent a rate that varies as a function of depth. Optical depth (units of meters) can vary significantly at different wavelengths (as noted by the reviewer) quite independently of position z in the water column. In other words, the optical depth indicated by $1/K_d(\lambda)$ occurs at a depth, but is not a description of the position of the instrument as a function of depth. Lastly, the instrument is not deployed in optical depths; it is deployed in real depths.

The sensitivity discussions involve L_W (and not K_d as the reviewer incorrectly assumed, as discussed above), which is a surface value, not a depth value. The authors retained the more natural approach of real depths, because it is more accessible to the reader.

Reviewer 1 Comment 11: Questions are posed regarding how the pressure transducer used with the profiling instrument (in terms of the contributory sources of uncertainty in the design and performance of the device) and the environmental conditions encountered in the field, influence the uncertainty in the depth records.

Authors Response 11: Based on repeated comments, the Reviewer appears to believe a pressure measurement uses technology identical to other sensors, but this is not the case. The positional uncertainties of the pressure transducer scale with the *location* of the sensor within its working range. The authors have modified the text to clarify this point.

Reviewer 2 Comment 1: The reviewer believes Sects. 2.2 and 3 are too long and provide a lot of historical considerations and details about the instrument's specifications, some of which are available in a NASA Tech. Memo. The reviewer suggests the removal of Sect. 3 and also suggests a better explanation in the introduction as to why the Malina field campaign was of interest to the testing of the new technology.

Authors Response 1: The authors agree with the intent of these comments and have modified the manuscript to make it shorter.

Reviewer 2 Comment 2: The reviewer suggests the manuscript should address or discuss methodological problems that are specific to high latitudes or to low solar elevation, and gives an example associated with the direct component for the Fresnel reflectance and how it varies as a function of large solar zenith angle.

Authors Response 2: The solar zenith angle for the Malina data set ranged from 53–79° and had an average value of 62°, which is within or close to the 75° threshold for some of the data processing corrections, e.g., computing the bidirectional correction for the exact form of the normalized water-leaving radiance. The vast majority of the Malina data were collected under overcast—*diffuse*—conditions, wherein the Fresnel reflectance does not vary appreciably (i.e., literature values are to within 2% of the value used in the data processing scheme). In addition, the majority of the data were collected in quiescent waters from a small vessel (also discussed above), so wave focusing effects were minimized. Under these conditions the convergence between the extrapolated in-water $E_d(0^-,\lambda)$ taken through the air–sea interface for comparison with $E_d(0^+,\lambda)$ (per the bounding condition used in the processor) is easily satisfied with minimum manipulation of the extrapolation interval (assuming the interval is defined in a homogenous layer, which is required). Now that the methodological section of the paper has been moved into Part I, the authors have suggested to the lead author of Part I that this point be addressed in Part I.

Reviewer 2 Comment 3: The reviewer requests two additional figures for the benefit of the Malina project: a) map depicting the station location using the same symbols as in the original Fig. 5, b) a figure with the R_{rs} spectra measured during the cruise. The reviewer requests a linkage between the R_{rs} spectra and the water types used in the sensitivity analysis.

Authors Response 3: The authors agree and have added both figures with accompanying explanatory text, with additional R_{rs} spectral detail provided in Part I. The linkage between the R_{rs} spectra and the water types used in the sensitivity analysis has also been added to the manuscript.

Respectfully yours,

Stanford Hooker