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Interactive comment on "Updated MISR Dark-Water Research Aerosol Retrieval Algorithm – Part 1: Empirical Calibration Corrections and Coupled 1.1 km Ocean-Surface Chlorophyll-a Retrievals" *by* James A. Limbacher and Ralph A. Kahn

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Received and published: 14 March 2017

The authors would like to thank Alexei Lyapustin for his feedback. As a result, we have made some changes to the structure of the manuscript.

This paper describes an updated MISR research algorithm for aerosol and chlorophyll retrieval over Case 1 waters. The improvements include a standard explicit model of underlight as a function of Chl, and, importantly, improvements in calibration, including

new de-trending analysis particularly important for climate research applications. This is a solid work that needs to be published with minor revision.

General comment: While generally the paper is written reasonably well, an improvement in structure/logic would be very helpful. Currently, the text unfolds almost unstructured as a story: the details of algorithm are mixed with calibration, past and new work, and at some point it becomes rather confusing as to what new is specifically done here. It could help if you could structure these things upfront in Introduction.

We have restructured the paper such that we believe it now flows more clearly.

Page 2, lines 31-32: Ocean reflectance in the blue can be higher than 5-20%; as far as I know, Gordon referred to Red band with respect to relative contribution.

Blue albedo in case I waters appears to peak at about 20% according to Figure 2 from our paper. The 5-20% is from our work here, and might be specific to the MISR channels.

Page 3, lines 21-22: Be more specific: which model is used - is it isotropic Cox-Munk, or Nakajima-Tanaka, or something else?

We have made it clear in the text that the model is isotropic Cox-Munk.

Page 3, line 28: How is the MISR Standard product surface pressure aliased from nearby mountains to over ocean?

As a single surface pressure value is used for a 17.6 km region, and we do retrievals at 1.1 km resolution, it is possible for errors to manifest themselves as we approach the coast (if there are mountains nearby).

Page 3, line 29: To use all 4 bands, you need to accurately limit to Case-1 waters. How is it done?

The $\chi 2$ parameter (calculated over all 4 wavelengths) and our $\chi 2$Chl parameter should be effective at limiting results to only case I. Because the Chl parameterization is based

on a case I framework, our model-measurement fits should be very poor in case II conditions; this is true for the cases we've tried. This is now mentioned in the text.

Page 8, line 28: Lyapustin et al. technique does full atmospheric correction of MODIS TOA data, contrary to this approach, as described. Thus, the core assumption that the AOD is assumed temporally stable, should be explicitly mentioned.

We agree, and have added that the AOD is assumed to be temporally stable.

Page 12, line 7: To give reader a good reference frame for MISR data, can you provide the same numbers for the SeaWIFS - Terra comparison (these are widely available).

We could provide the numbers in a table (as a Terra-SeaWIFS comparison on the SeaBASS website takes only a few seconds to compute), but they would be for a completely different sample subset, and so might not be comparable to the data presented. However, we have commented on comparisons between MODIS-Terra, SeaWIFS, and SeaBASS in the text, including mentioning that SeaWIFS agrees better with MODIS-Terra than MISR does.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2016-360/amt-2016-360-AC1-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-360, 2016.