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Interactive
Comment

Interactive comment on "Performance evaluation of ocean color satellite models for deriving accurate chlorophyll estimates in the Gulf of Saint Lawrence" *by* M. Montes-Hugo et al.

Anonymous Referee #1

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The Authors evaluated several combinations of atmospheric correction (AC) and in-water algorithms for retrieving Chl and phytoplankton absorption coefficients in the Gulf of Saint Lawrence (GSL). Generally speaking, this is an algorithm intercomparison study (in this case the combination of AC and in-water algorithms). Such studies can be regionally useful and, in general, are timely given the oceanographic community's growing desire for synoptic ecosystem management using satellites. However, the manuscript severely suffers from technical issues, as elaborated upon below, and does not broadly shed light on the reasons the AC and in-water algorithms performed well or poorly. The technical issues, at least, need to be addressed before this manuscript is publishable. While the manuscript should not be published in its current form, I would

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Discussion Paper

highly encourage the Authors to prepare a major revision.

Technical Issues

The Authors propose that their evaluations can be used to make statements about atmospheric and in-water algorithms independently. Given that no radiometric satellite-to-in situ comparisons are shown, I'm not sure this is truly the case. All the Authors really reveal is the combination of AC and in-water algorithm that best match their field data. This is fine, and a modest contribution in and of itself, but broader comments about the quality of the AC and the in-water algorithms themselves are unjustified. The manuscript would be greatly served by teasing apart the components of the AC and in-water algorithms that succeed and fail in the GSL. Without doing so, the results are neither portable to other regions or to updates to the AC and in-water algorithms (both of which are often improved routinely).

The Authors show a general misunderstanding of atmospheric correction. The most critical component of AC is the selection of aerosol models and magnitudes. The standard NASA approach still uses Gordon and Wang 1994 (and Kuchinke et al. 2009 uses a modified version of Gordon and Wang 1994). Bailey et al. 2010 is not an atmospheric correction method, nor is Stumpf et al. 2003 (see e.g., the text on page 4, lines 1-11 and the first paragraph of Section 2.4). These are simply mechanisms to account for non-zero nLw(NIR). Bailey et al. 2010 does not "constrain atmospheric parameters such as aerosol concentration and type" (page 4, line 11).

The Authors repeatedly suggest that NASA has a "default a*ph curve" (see, e.g., page 10, line 3 and Section 3.4.1). This is not the case – there is no default NASA a*ph spectrum. From Figure 5, it appears the Authors have misrepresented the a*ph spectrum of GSM to be a ubiquitously adopted spectrum for all NASA algorithms. GSM, QAA, GIOP, and other algorithms all use their own a*ph relationships (e.g., the default GIOP implementation uses Bricaud et al. 1998).

Page 5, Line 4: "t_a-w/n_wˆ2 approximates to 1" is not true. If, for example, t_a-w = 1 and n_w = 1.334, then t_a-w/nwˆ2 = 0.56 .

Section 2.5: The description of GSM is inaccurate. GSM operates by assigning eigenvectors (spectral shapes) to bbp, aCDM (CDOM + non-algal particles, not CDOM alone), and aph (using a*ph, see Fig. 3), then solving for eigenvalues (magnitudes of bbp(443), aCDM(443), and Chl) that best reconstruct (spectrally match) the input Rrs via Eq. 3. The simulated annealing process was simply used to best define spectral shapes for bbp (slope of 1.0337) and aCDM (slope of 0.0206 1/nm) – it is not used in the inversion process itself as implied on page 12, line 14.

Correct the reference "Levender et al. 2005" to "Lavender et al. 2005".

Other comments

Page 3, Line 3: Reword first sentence. The "concentration" is not a pigment.

Page 3, Line 25: The mention of both 443 and 440 nm in the parentheses is confusing.

Page 4, Lines 12-21: Most of this is true, but the paragraph is not easily read. Suggest adding a supporting figure to demonstrate the "spectral overlap between constituents".

Page 5, Lines 22-24: I would argue that Bricaud et al. 1995, 1998, 2004 can be modestly used to estimate a*ph from remotely-derived Chl.

Page 6, line 27: Change "GLS" to "GSL".

Page 9, line 19: Were sums of pigments used to calculate a total Chl, such as MV + DV + allomers + epimers of Chl?

Section 2.3: What version of SeaDAS was used? Does "6 h of the satellite overpass" indicate +/- 6 hours or +/- 3 hours? Were any flags and masks applied?

Section 3.1: What statistic is being shown in Fig. 2 and how does that statistic relate to those proposed in Eqs. 6-8?

Page 15, line 8: It's been shown many times that spectral matching algorithms such as GSM suffer from fewer retrievals than empirical approaches and QAA. Recommend the Authors comment on this or at least mention how their results fit within a broader community context.

Page 16, line 14: I disagree with the statement "not clearly related to variations on time difference". Fig. 3B appears to show a clear trend for the April 2001 data.

Page 22, line 18: Recommend adding a discussion of what in situ data was used to parameterize the EC model. Doing so might reveal why it performed well in the GSL.

Interactive comment on Biogeosciences Discuss., 11, 9299, 2014.

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