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Interactive comment on "On the role of the seawater absorption to attenuation ratio in the radiance polarization above the Southern Baltic surface" *by* Włodzimierz Freda et al.

Anonymous Referee #1

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General Comments

The authors of the manuscript have conducted large number of polarimetric simulations of water conditions in the Baltic sea, over two seasons, and three different water types. An analysis is conducted which looks at the relationship between the absorption to attenuation of the water, and the upwelling polarization signal. It is determined that a correlation exists, and that it also depends on the Sun angle and the wind speed. A discussion about the direction of maximum DoP is also given.

The authors should be commended for the large amount of work that obviously went into this analysis, and especially for the inclusion of light polarization, which many

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Discussion paper

scientists avoid.

That being said, however, I believe there to be several fundamental scientific problems with this manuscript that must be addressed before it is suitable for publication. I wish to convey to the authors that, although I have written many comments, it is because I am both familiar and passionate about the subject, and wish to see it properly conducted and thereby make an impactful contribution to the field.

My critique may be distilled down to a few main issues:

1. I found the literature cited by the manuscript very lacking. Almost all citations are prior to ∼2012, and there has been many advances in the field in recent years.

2. The analysis focuses on “max(DoP)”. The maximum DoP is almost always either in, or near, the specular reflection point for above water simulations due to the inherent Fresnel reflectivity of the mean sea surface. (Figs 2,3,4,5,6 of this manuscript) This is an infeasible place to be measuring polarization for ocean color, since any measured signal from the ocean will be overwhelmed by the reflectance of the Sun. Ocean color satellites will not measure at this geometry. The paper would be much more applicable if the max(DoLP) were limited to feasibly measurable angles.

3. I believe that the contribution made by section 3.5 of the manuscript is marginal at best. Most of the conclusions about the direction of max(DoP) are ‘known’, or can be determined easily from Snell’s law and the knowledge that the maximum DoP occurs at scattering angles near 90 degrees. The underwater simulations are illustrative, but the explanations given for the direction of the DoP are inaccurate. See the specific comments below for further details.

Specific Comments

1. Pg 2 line 1-2: Garaba and Zielinkski, 2013 have very little to say about the polarization of above surface light. Nothing about improving the accuracy using polarization. This seems a poor choice of citation.

2. Pg 2 line 2: Ibrahim et al 2012 do not make any beneath surface measurements, it is entirely based on radiative transfer simulations.

3. Ibrahim et al have improved the work after 2012. A citation to the more recent work should be included:

a. A. Ibrahim, A. Gilerson, J. Chowdhary, and S. Ahmed, "Retrieval of macro- and micro-physical properties of oceanic hydrosols from polarimetric observations," Remote Sensing of Environment, vol. 186, pp. 548-566, 2016.

4. Pg2 line 6: Reduction of Sun glints: Requires more references. There is a wealth of literature on this subject beyond Zhou et al, 2017. Too many to list here.

5. Pg2 line 8-9 Insufficient citations about polarized surface reflection, see also:

a. T. Harmel et al., "Polarization impacts on the water-leaving radiance retrieval from above-water radiometric measurements," Applied Optics, vol. 51, no. 35, pp. 8324-8340, Dec 10 2012.

b. T. Harmel and M. Chami, "Estimation of the sunglint radiance field from optical satellite imagery over open ocean: Multidirectional approach and polarization aspects," Journal of Geophysical Research: Oceans, vol. 118, no. 1, pp. 76-90, 2013.

c. C. D. Mobley, "Polarized Reflectance and Transmittance Properties of Wind-blown Sea Surfaces," Applied Optics, vol. 54, no. 15, pp. 4828-4849, 2015.

d. M. Hieronymi, "Polarized reflectance and transmittance distribution functions of the ocean surface," Optics Express, vol. 24, no. 14, pp. A1045-A1068, 2016/07/11 2016.

e. R. Foster and A. Gilerson, "Polarized Transfer Functions of the Ocean Surface for Above-Surface Determination of the Vector Submarine Light Field," Applied Optics, vol. 55, no. 33, pp. 9476-9494, 11/16/2016 2016.

f. D. D'Alimonte and T. Kajiyama, "Effects of light polarization and waves slope statistics on the reflectance factor of the sea surface," Optics Express, vol. 24, no. 8, pp. 7922-

7942, 2016/04/18 2016.

6. Pg 3 line 1: There are many RT models that include light polarization:

a. B. Lafrance and M. Chami, "OSOAA (Ocean Successive Orders with Atmosphere - Advanced) Users Manual," Université Pierre et Marie Curie Laboratoire d'Océanographie de Villefranche, France 2016-11-04 2016.

b. A. A. Kokhanovsky et al., "Benchmark results in vector atmospheric radiative transfer," Journal of Quantitative Spectroscopy and Radiative Transfer, vol. 111, no. 12-13, pp. 1931-1946, 2010. [And references therein]

c. S. Korkin, A. Lyapustin, A. Sinyuk, B. Holben, and A. Kokhanovsky, "Vector radiative transfer code SORD: Performance analysis and quick start guide," Journal of Quantitative Spectroscopy and Radiative Transfer, vol. 200, pp. 295-310, 2017/10/01/ 2017.

d. Y. Ota, A. Higurashi, T. Nakajima, and T. Yokota, "Matrix formulations of radiative transfer including the polarization effect in a coupled atmosphere–ocean system," Journal of Quantitative Spectroscopy and Radiative Transfer, vol. 111, no. 6, pp. 878-894, 2010.

e. F. M. Schulz, K. Stamnes, and F. Weng, "VDISORT: An improved and generalized discrete ordinate method for polarized (vector) radiative transfer," Journal of Quantitative Spectroscopy and Radiative Transfer, vol. 61, no. 1, pp. 105-122, 1999/01/01 1999.

7. Pg 3, line 2-4. This is a false statement. Polarized radiative transfer has been happening for decades, and polarized radiative transfer of the ocean since the 1970s (Plass and Kattawar). One example:

a. G. W. Kattawar and C. N. Adams, "Stokes vector calculations of the submarine light field in an atmosphere-ocean with scattering according to a Rayleigh phase matrix: Effect of interface refractive index on radiance and polarization," Limnology and Oceanography, vol. 34, no. 8, pp. 1453-1472, 1989.

8. Pg 3, line 9: The 90 degree relative azimuth plane has been in use for a long time prior to Piskozub and Freda, see for example:

a. C. D. Mobley, "Estimation of the remote-sensing reflectance from above-surface measurements," Applied Optics, vol. 38, no. 36, pp. 7442-7455, 1999.

9. Pg 3, line 13-15. There have been many studies about the measurement and modeling of light polarization in coastal areas (to name only a few):

a. S. Sabbah, A. Barta, J. Gál, G. Horváth, and N. Shashar, "Experimental and theoretical study of skylight polarization transmitted through Snell's window of a flat water surface," Journal of the Optical Society of America A, vol. 23, no. 8, pp. 1978-1988, 2006/08/01 2006.

b. A. Tonizzo et al., "Polarized light in coastal waters: hyperspectral and multiangular analysis," Optics Express, vol. 17, no. 7, pp. 5666-5683, 2009/03/30 2009.

c. A. Tonizzo, A. Gilerson, T. Harmel, and A. Ibrahim, "Estimating particle composition and size distribution from polarized water-leaving radiance," Applied Optics, vol. 6, no. 10, pp. 5047-5058, January 2011.

d. A. Lerner, S. Sabbah, C. Erlick, and N. Shashar, "Navigation by light polarization in clear and turbid waters," Philosophical Transactions of the Royal Society B: Biological Sciences, vol. 366, no. 1565, pp. 671-679, 2011.

e. T. Harmel et al., "Polarization impacts on the water-leaving radiance retrieval from above-water radiometric measurements," Applied Optics, vol. 51, no. 35, pp. 8324-8340, Dec 10 2012.

f. Y. Gu et al., "Polarimetric imaging and retrieval of target polarization characteristics in underwater environment," Applied Optics, vol. 55, no. 3, pp. 626-637, 2016/01/20 2016.

g. A. El-habashi and S. Ahmed, "Chlorophyll fluorescence and the polarized underwater light field: comparison of vector radiative transfer simulations and multi-angular hyperspectral polarization field measurements," vol. 9827, p. 98270U, 2016.

h. J. Liu et al., "Polarization characterics of underwater, upwelling radiance of suspended particulate matters in turbid waters based on radiative transfer simulation," SPIE Remote Sensing, vol. 10784, p. 7, 2018.

i. A. C. R. Gleason, K. J. Voss, H. R. Gordon, M. S. Twardowski, and J.-F. Berthon, "Measuring and Modeling the Polarized Upwelling Radiance Distribution in Clear and Coastal Waters," Applied Sciences, vol. 8, no. 12, p. 2683, 2018.

10. Pg 4, line 18: Piskozub and Freda (2013) only write 2 paragraphs about their Monte Carlo algorithm; I would hardly call this a “description”. I would have liked to see (or have been pointed to) some benchmark results comparing the code to others, so that the reader can have confidence the code is physically correct.

11. Pg 5, line 2-3: While in general the V component is negligible, the biggest source of circular polarization is total internal reflection of upwelling light by the sea surface. The off-diagonal elements in Voss and Fry are indeed zero, but this has to do with scattering only, and is not by itself justification for saying the V component is negligible.

12. Pg 5, line 6: Reflection and refraction by flat surfaces are described completely by the Fresnel equations, not ‘basically’.

13. Pg 5, line 10-12: I am confused about which particle scattering Mueller matrices the authors are using. They state (line 11) that Voss and Fry 1984 is used for the water, and Volten et al (2001) is used for the aerosols. Then, the following sentence says that Mie theory is used for the phase functions. Only the (1,1) element of the Mie calculations were used? What parameters were used for the Mie calculations? How were they determined and are they representative for the Baltic Sea?

14. Pg 5, line 14: All results are specified in the principle plane. This seems to be incorrect, since polar plots of all azimuth angles are given thoughout.

15. Pg 5, line 20: There is a new 'recommended data processing' for ac-9 and ac-s instruments, the PROP-RR method, which may be applied to previously acquired data. It may be worth investigating whether this has a significant impact on the results given that the ratio of a/c is being analyzed. See:

a. N. D. Stockley, R. Röttgers, D. McKee, I. Lefering, J. M. Sullivan, and M. S. Twardowski, "Assessing uncertainties in scattering correction algorithms for reflective tube absorption measurements made with a WET Labs ac-9," Optics Express, vol. 25, no. 24, pp. A1139-A1153, 2017/11/27 2017.

16. Pg 5, line 29-31: I am very confused about these sentences. Why is aw subtracted and then added again? Perhaps subscripts should be added to clarify? (a_t) for a total, and (a_pg) particulate + CDOM absorption for a - aw. Or perhaps make it a formal equation that makes sense mathematically. Put a sigma (sum) symbol if sum is meant. Same for pg 6, line 1.

17. Pg 6, line 13: In my opinion, using the isotropic Cox-Munk slope distribution is preferable to choosing an arbitrary directional wind value. A directional wind will introduce an asymmetry into the above-surface light field, the effect of which is not being analyzed.

18. Pg 6, line 13: I would suggest choosing a more reasonable second wind speed other than 15 m/s. The limit of applicability of Cox-Munk wave slopes is 14 m/s, and when the wind is this strong, gravity waves will introduce significant uncertainty into the polarimetric measurements (in-situ) due to strong tilts in the instantaneous sea surface. TOA measurements should be unaffected, however.

19. Pg 8, Fig 2: The projection of the polar plots, or at least the zenith values of the concentric circles should be indicated.

20. Pg 8, Fig 2: The wind-speed used (5 or 15 m/s) is not indicated.

21. Pg 8, Fig 2: What aerosol optical thickness was used for the simulations at each

wavelength? What is the spectral relationship (or angstrom coefficient) used to determine it? Was it based on seasonally averaged measurements?

22. Pg 8, Fig 2c-2d: I am suspicious of the ∼1000x increase (10ˆ3/10ˆ0.04) in maximum upwelling radiance from the summer to the winter. The authors should double-check that these intensities are correct.

23. Pg 8, line 1-2 : Intensity is stated to be irradiance, but units of radiance are given.

24. Pg 9, Fig 3: No wind speed, aerosol optical thickness values, or zenith labels are given.

25. Pg 13, line 11-13: I am not convinced that any increase in wind speed (and therefore an increase in the surface roughness) would cause an increase in the DoP. I just don't see any way in which a rougher surface will result in more polarization than a smooth one. Any increase in roughness should cause at least a partial de-polarization of the reflected/transmitted light field. This is also stated by the authors on pg 16, line 19. For example, see:

a. R. Foster and A. Gilerson, "Polarized Transfer Functions of the Ocean Surface for Above-Surface Determination of the Vector Submarine Light Field," Applied Optics, vol. 55, no. 33, pp. 9476-9494, 11/16/2016 2016.

26. Pg 13, line 22: "The type of water has less influence on the DoP than the season and its representative SZA." This would seem to contradict the title of the article.

27. Pg 14, line 12: This sentence seems to contradict also with the previous comment.

28. Pg 15, lines 6-11: The authors are (partially) correct that the Fresnel reflection matrix depends only the refractive index of the medium and the incidence angle (but also on the refractive index of the air, and the imaginary part of each refractive index, which governs absorption). However, the authors are incorrect to use that as justification that the observed differences come only from the water-leaving component of the radiance. The polarization of the reflected component intrinsically depends on the polarization

[I,Q,U,V] of the downwelling skylight. The reflected Stokes vector is the downwelling Stokes vector multiplied by the reflection matrix. Therefore significant DoLP variability may be introduced in the reflected component due to polarization of the skylight component, which is then combined with DoLP variability coming from the water-leaving part.

29. Pg 15, lines 16-17: Although I disagree with the reason given for the non-linearity, more importantly, the DoP may never be greater than one. If this occurs in any case, there is a significant problem with the simulations which should be addressed, or a better explanation must be given.

30. Pg 15, line 23-24: I believe the reason for the wavelength independence is because the max(DoLP) is always looking at the direct reflection of the Sun, which has little to do with the water body. See General comment #2.

31. Pg 16, line 13: Generally speaking, the DoLP tends to decrease after multiple scattering events because of the number of photons originating from different directions (and with different polarization), however the authors statement is not universally true and strongly depends on the scattering angle. For example, unpolarized light scattered by Rayleigh particles at 90 degrees becomes fully polarized. Individual scattering events often increase the polarization of the scattered light.

32. Pg 17, line 5-7: This is the expected behavior. The underwater SZA corresponding to above-water SZA of 45 and 75 degrees is $\sim$30 and $\sim$45 degrees, respectively. Since the planes of constant DoLP are orthogonal to the SZA (in single scattering), this results in a 'tilt' of the planes of constant underwater DoLP of $\sim$60 and $\sim$45 degrees (from the horizontal).

33. Pg 18, line 16-17: More likely the reason is that the measurement of Tonizzo et al, 2009 included scattering by hydrosols with different phase matrices than the Voss-Fry matrices used here.

34. Pg 19, line 7-8: The “HPR” for a flat surface, should be exactly the Brewster angle, which is ∼53 degrees, not 58. Additionally, the refractive index of the water used should be specified somewhere.

35. Pg 19, lines 5-14: I am not certain this paragraph adds anything to the discussion. I am not aware of any significance to SZA + Zenith = 2 * Brewster angle, and the “HPR” has no information about the water body, since (as defined by the authors) it is ‘reflected’ radiance.

36. Pg 20, line 25 to Pg 21, line 2: This statement is inaccurate. I believe there is a misunderstanding by the authors about the nature of the relationship between the reflection matrix (or Fresnel amplitude coefficients for parallel and perpendicular directions) and the reflected light field (and polarization thereof). The perpendicular and parallel Fresnel coefficients alone do not dictate the degree of polarization of reflected light. Only when they are applied to an incident light field is the DoP of the reflected light known exactly. They can say something about the possible ranges of DoP, but barring a few specific cases the actual reflected DoP may only be known after considering the coefficients and the incident light field together.

37. Pg 20, line 16-17: I disagree with this statement. When the SZA is very high (winter), the “HPM”, in the authors terminology (the angles of highest underwater DoP), are allowed to propagate upward through the surface, because they fall within Snell’s window (cone of angles less than the critical angle). When the Sun is higher in the sky (lower SZA), the peak DoP falls outside Snells window and is internally reflected by the sea surface, and therefore does not propagate above the water. This would seem to contradict the statement by the authors. See also Fig 4 of:

a. A. Ibrahim, A. Gilerson, T. Harmel, A. Tonizzo, J. Chowdhary, and S. Ahmed, "The relationship between upwelling underwater polarization and attenuation/absorption ratio," Optics Express, vol. 20, no. 23, pp. 25662-25680, Nov 05 2012.

38. Pg 21, line 6-9: Isn’t Fig 8 a simulation of below water? This would seem to be

directly comparable with Ibrahim, 2012.

Technical Corrections

1. Pg 3 line 1: I do not see an entry in the references for Chami, 2001. Also, see specific comment #7, because this citation is out of date. (see LaFrance and Chami, 2016).

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