Atmos. Meas. Tech. Discuss., 7, C5141–C5144, 2015
www.atmos-meas-tech-discuss.net/7/C5141/2015/

Atmospheric Measurement Techniques Discussions

Interactive comment on "A theoretical study of the effect of subsurface oceanic bubbles on the enhanced aerosol optical depth band over the southern oceans as detected from MODIS" *by* M. Christensen et al.

Anonymous Referee #3

Received and published: 14 March 2015

1. A considerable effort has been made here to identify the contribution of subsurface bubbles to the lower boundary condition used for MODIS aerosol retrievals over ocean. The authors are honest in concluding that the likely magnitude of this contribution at wind speeds below 12 m/sec is negligible, and similarly for the southern ocean band of high-AOD retrievals on average. However, I'm not sure they take adequate account of uncertainties in other aspects of the ocean surface model, specifically the wind-dependent whitecap contribution. They develop what should be very useful machinery to study the impact of the ocean boundary condition on aerosol retrievals, but perhaps

they could reach stronger conclusions if they applied it to the whitecap model itself, or to all the wind-dependent contributions (whitecap + subsurface bubbles) together as a single factor.

2. Introduction, P12797, lines 19-21. According to the MISR Level 2 Aerosol Product Quality Statement, the First Look aerosol product uses climatology for the wind speed, but the Final product uses QuickScat and other sources to account for wind speed explicitly.

3. Introduction, P12798. A general question: If the wind is strong enough to create subsurface bubbles, it apparently would also produce surface bubbles. So wouldn't the conditions under which the longevity of subsurface bubbles matter most be those where the wind is initially strong, and then dies down? I'm wondering whether this would be very significant for satellite aerosol retrievals, as even those that take account of varying wind speed rely on three-hourly or coarser temporal resolution wind data. I agree that the greater uniformity of subsurface bubbles could have a larger impact on higher-spatial-resolution aircraft observation, as you mention.

4. Introduction, P12799, lines 4-9. Another general question: I'd expect that subsurface bubbles could be a bigger issue for ocean color retrievals. For example, if subsurface bubbles are present but not accounted for, chlorophyll-a concentration deduced from light absorption by that species might be underestimated. I'm not familiar with the Zhang and Lewis work you cite, but is this discussed specifically in the ocean color retrieval literature? I guess another possibility is that ocean color retrievals are simply not done when wind speeds exceed some value, maybe 7.5 m/sec, i.e., when poorly calibrated whitecap models themselves tend to predict non-linearly increasing contributions to ocean surface reflectance, and with large uncertainties.

5. Introduction, P12800. You might consider mentioning how polarized observations might mitigate multiple-scattering effects at shorter wavelengths for this problem. The work of Chowdhary et al. might be relevant here. I'm thinking that many of the current

aircraft instruments used for aerosol remote sensing have polarized channels.

6. Introduction, P12797, line 4. Come to think of it, you might also reference earlier on Kalashnikova et al., AMT 6, 2131–2154, 2013.

7. Section 2.1, P12801, lines 12-24. I agree it is fair to compare with the MODIS C5 product due to the connection to previous analyses and prior claims. However, it seems more relevant, and more useful for future work, to compare with the MODIS C6 data, especially as C6 includes the better ocean surface model that you mention. I realize this represents extra work, but I think it will make the paper much more useful, as it will calibrate how effective the simpler wind-speed-dependent lower boundary whitecap condition in C6 also mitigates the subsurface bubble issue you raise.

8. Section 2.1, P12803, line 5. It might be fair to reference Levy et al., AMT 6, 2989–3034, 2013 here as well. Also on P12808, line 17. And again for Collection 6 on P12809, lines 10-14.

9. Section 2.2, P12804, line 21. This should probably be: "red and near-infrared wavelengths. . ."

10. Section 2.2, P12804, Equs. 1. (a) Is W a fractional or the total area, and should Ref_wc be multiplied by W? (b) More might be said about how the glint term is evaluated, as it can be significantly view-angle dependent. (For example, I see you assume a Lambertian surface in the next section; if used here, this might not be a good representation of the glint term for interpreting remote sensing radiance observations.) (c) Also, you seem to be assuming that other water-leaving reflectance terms are negligible where whitecaps are present; if so, this might be worth mentioning.

11. Section 2.2, P12804, lines 24 ff. It might be worth mentioning the estimated level of uncertainty in these factors. (I guess this applies specifically to Equs. 2.) This would also address in part whether half and double bubbles (P12806, top paragraph) are the appropriate limits to use.

12. Section 3.1, P12807, Figure 3. Given the scatter of points in Fig. 3a (especially north of 30S), and the relative contribution of the surface shown in Fig. 3b, is this approach really sensitive enough to assess the contributions of subsurface bubbles to TOA radiance? Perhaps you are in a position to put some constraint on one or more of the larger terms.

13. Section 3.2, P12807, line 12. Is the Koepke [1984] whitecap model good enough to apply at wind speeds greater than maybe 10 or 12 m/sec in this context?

14. Section 3.2, P12807, paragraph 2. Certainly by changing one forcing term in one direction, you would expect to produce a systematic difference compared to the no-bubble case. However, in terms of practical application, it would help to address whether these differences rise above the uncertainties in the system as a whole. Similarly, you plot the average values in Fig. 4, but some measure of the spread would also be of interest, because satellite retrievals for a given location and season tend to be performed over a limited range of angular values. (BTW, I think I know what "AZM" is, as distinct from "SAZM," but I did not notice the former defined in the text.)

15. Section 3.2, P12808, lines 20-22. Does this consider uncertainty (as distinct from the variation) in the Koepke [1984] whitecap model itself?

16. Section 3.2, P12809, line 4. At wind speeds in excess of 10 m/sec, or even less, uncertainties in any satellite-retrieved AOD would be much greater than 0.01 anyway.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 12795, 2014.