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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous coastal measurements of ozone deposition fluxes and iodine-mediated particle emission fluxes with subsequent CCN formation" *by* J. D. Whitehead et al.

Anonymous Referee #1

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Review of Whitehead et al., Atmos. Chem. Phys. Discuss., 9, 20567–20597, 2009

This study takes the approach of using direct ozone and particle flux measurements to study the impact of intertidal algae on the overlying atmospheric aerosols and chemistry. Overall, I think the authors have done a nice job of presenting their results in a clearly understandable way. Their results are quite dramatic, and although these flux measurements are challenging, it seems to me that the main conclusions are well supported by the data. I think this was a creative and interesting study that will give many of us pause regarding our assumptions about aerosol processes in many coastal urban areas.

Some specific (and often petty) comments that the authors should consider (but need not necessarily modify the manuscript in response to):

1. vd for ozone – In the discussion about turbulent transport of ozone to surfaces, I was a bit surprised to see the statement: "There is little information on the processes governing rb above water and it is difficult to select an appropriate parameterization for deposition to either the sea surface or the exposed sea floor." This part of the flux problem is the same for all gases. Hence all the literature regarding water vapor transport to water and land surfaces is relevant. So, I was expecting to see reference to classic papers like Kaimal et al., 1972, Kondo et al., 1975, etc.. Of course there are many complications for the open water case (fetch, wave state, etc.). The water vapor fluxes (in conjunction with a surface temperature measurement) would allow rb to be computed.. Anyway, this is a minor point, since as the authors state, rb is small compared to rs.

Kaimal, J. C., et al. (1972), Spectral characteristics of surface-layer turbulence, Q. J. R. Meteorol. Soc., 98, 563– 589.

Kondo, J. (1975), Air-sea bulk transfer coefficients in diabatic conditions, J. Boundary Layer Meteorol., 9, 91–112.

2. Figure 1 – the overlay obscured useful information in the photo. The overlay itself needs a plain background to be easily understandable.

3. "In order to ensure that the presence of the jetty wall was not influencing the air flow at the sensor location, the vertical wind angle was examined" – does this mean the mean vertical wind was zero? Does "was not found to deviate" mean "did not deviate from vertical"? This could be written more clearly.

4. With the caveat that I have zero expertise in this area. . .I am inherently dubious about particle flux measurements. If the particles are hydroscopic, they should be

changing sizes in response to the local water vapor level on very fast (millisecond) time scales. That makes for a very complex signal to deconvolute, as there is a probably a large water vapor flux from the surface and the detectors respond to different sizes with different efficiencies. So, can one really obtain a unique solution to the flux? I realize this is a big subject and would rapidly consume a paper like this one. However, I think the reader needs some assessment of the uncertainty derived from this (and perhaps other) issues. Somewhere, the authors need to state their best estimate of uncertainty on the measured fluxes (for particles and ozone). This said, I think the paper's conclusions will hold up because the differences in particle fluxes between low flux and high flux conditions are orders of magnitudes.

5. p 20572, l21. "least empirical" is gratuitous

6. The wind angle criteria seems a bit generous. Allowing the sector to be along the coast means that given typical variance in wind directions, a large fraction of the winds might be out of sector. Safer to put the cut-off at 60 degrees from the coast or something like that.

7. 20575 "Various issues relating to sources of uncertainty, data quality control and in particular analysis techniques using the GFAS and similar instruments for ozone flux measurements, are discussed extensively by Muller et al. (2009)." Very awkward sentence, ambiguous.

8. How do we know that the difference between high and low tide fluxes was not somehow due to the different height of the sensors? Are there control experiments where the sensor/intake height is varied within one tidal cycle or varied inversely with tidal height?

9. 20579. I would think that mixing induced by bottom shear (given the shallow depth of the water) would also enhance surface turbulence in this environment relative to that in the open ocean. Also line 14 remove parentheses

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10. Section 4.2 line17. The statement that direct deposition is "dominant" doesn't seem to make sense. Low tide resistance is about double that of high tide. So, direct deposition roughly equal to the other mechanisms, right?

11. "This may therefore be considered a lower limit to surface resistance to an exposed seafloor with non-uniformly distributed Laminaria beds amongst other species of seaweed." It is unclear exactly what "this" refers to. Anyway, it is not clear why it should constitute a lower limit, rather than an upper limit.

12. 20580 line 1 Should emphasize "gas phase photochemical destruction"

13. 20580 line 8 "fully consistent" is redundant. Either it is consistent or not.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20567, 2009.