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Discussions

Interactive comment on "An automated gas exchange tank for determining gas transfer velocities in natural seawater samples" *by* K. Schneider-Zapp et al.

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Review of Schneider-Zapp "An automated gas exchange tank . . ." GENERAL COMMENTS The paper describes a self-contained and automated apparatus to study air-sea gas exchange. A priority is to enable reproducible and accurate results and that feature is adequately demonstrated by repeated experiments with MilliQ waters. It is also demonstrated that signals related to surfactant activity can be readily resolved. Overall, the paper achieves what it sets out to do, describing the apparatus in a cogent manner and convincing (this reader at least) that the results are sufficiently accurate and reproducible to make this apparatus genuinely useful. SPECIFIC COMMENTS

One possible criticism is that the turbulence (and gas transfer) is driven by a convenient method (a baffle) rather than necessarily a realistic mechanism. I do not think this is a major issue and it does seem sensible (as argued in the introduction) to focus on maintaining a sealed tank and achieving reproducible results rather than dwelling on the precise characteristics of the turbulence, wave field and other physical characteristics. It is worth noting also that wave spectra are measured, therefore there is a record of the wave field generated by each setting of the baffle. Results are described for natural water samples (from various distances offshore of the north east of England). It would be useful to see more results, but those reported here are adequate to demonstrate both the utility of the apparatus and the significant role of surfactants in controlling gas transfer rates. The abstract is slightly misleading in that 3 gases are mentioned here, but in fact the results for one gas, nitrous oxide are very limited (appearing only in Figure 5). Perhaps there should be some explanation that the apparatus has been designed and tested (in respect to the equilibration component, Figure 5) for 3 gases, but transfer velocities are reported here for only 2 gases? The only major doubt I have about this paper is the contention at line 15 of page 710 that D is approximately Cw for SF6 throughout. Since the system is closed, I'd expect the head space to equilibrate with the water volume during the course of the experiment and sensibly (to get accurate transfer velocities) the experiment should be run long enough for this to occur substantially. Solubility is largely irrelevant to that argument (contradicting lines 15 to 19 of page 710) but will equilibrate less simply because it has a lower transfer velocity. This criticism may be relatively unimportant if the mathematics in Section 4.4.1 has been rigorously applied to all the gases. The remainder of the review deals consecutively with sections of the manuscript, raising minor points. The introduction places the study in context. The material is inevitably selective and not all of the material is necessary, but generally it is balanced and well written. Thus, I have no objections to this section. The description of the system design (Section 2) and system components (Section 3) is quite detailed and carefully explained. This treatment is appropriate in my opinion, since it gives confidence to the reader that the system is very capable and in principle enables replication by another research group. I have little comment beyond stating that I found the explanations clear and convincing. Section 4 on experimental procedure is generally good but there a few places where the explanation was inadequate for me, while other material was probably superfluous. The explanation of sample volume calculations at Lines 4-9 of Page 707 mystified me. At line 26 and following of page 707, it would be worth citing one or more sources on the calculation of propagated errors. There is a disconnection between lines 8-12 of page 708 in Section 4.2 and the description of subsection 4.4.3, if not a contradiction. Were these two sections written by a different person? In my opinion Section 4.4.3 is superfluous and discarded, but there should be some better explanation in 4.2; firstly, why n = $\frac{1}{2}$ is unsurprising for this apparatus and MilliQ; secondly, why (if this is the case ?!) n= $\frac{1}{2}$ was used for normalization throughout. The theory in sections 4.4.1 and 4.4.2 is important and needs to be there. I am quite familiar with this theory as I have had to do something similar in the past. Inevitably it is not an easy read, but as far as I can see it is done correctly and explained reasonably clearly. As noted already, in my opinion section 4.4.3 could be discarded. Section 4.4.4 on wave spectra seems to me slightly misjudged and could be omitted. For those (including me) familiar with wave spectra generally and the particular explanation given by Phillips, the material is recognisable and superfluous. For those less interested, the material is unnecessarily mathematical and pedantic. I suggest that it is sufficient to state that the 400 Hz time series of elevation is used to calculate a power spectrum in frequency. The only other things worth noting are that the baffle frequency and its harmonics are readily evident in the power spectra (this has been done in the caption of Figure 9) and the tail above an angular frequency of 100Hz is almost certainly affected by noise. Section 4.4.4 could be omitted and the final paragraph of 4.2 used to provide sufficient explanation. I do not have much comment on Section 5. In my opinion the results are fairly and concisely reported. The conclusions are succinct and appropriate. The figures are generally well drawn and clear, though some of the text (especially in Figure 4) strained by tired eyes. In summary, I think this paper is worthy. It succeeds in describing and proving a very

capable laboratory apparatus and method.

Interactive comment on Ocean Sci. Discuss., 11, 693, 2014.