Response to Referee #2
"Ocean bubbles under high wind conditions. Part 1: Bubble distribution and development" by Helen Czerski et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-103-RC2, 2021
27th January 2022

We are grateful to the reviewer for their comments and have prepared a revised manuscript which addresses their concerns. Our response to each point and a description of the changes made are given below (reviewer comment in blue, author response in black).

Panel (a) of Figure 3 indicates that bubble ~ 12 microns could go down to 6 m at a wind speed of 18 m/s. I recall backscatter data at ocean station Papa (by Svein Vagle) showing that bubbles of around 20 to 30 microns go down to more than 10 meters at a wind speed of ~10 m/s. I believe a figure showing the data is in the textbook "Chemical Oceanography and the Marine Carbon Cycle" by Steve Emerson (I apologize that I do not have the book with me right now and do not have the figure number). Do bubbles of 12 microns dissolve quickly in the water column and do not exist below that depth? Or is there any limitation in the observation that bubbles deeper than 6 m cannot be detected?

We think that the figure you refer to is Figure 10.9 in Chapter 10 of the Emerson book (although there is a spelling mistake in SV's name in the text). This figure shows data from a 120 kHz sonar, which would be resonant with bubbles of 27 microns in radius (detectable by both camera and resonator in our experiment). The wind speed given is 12 m/s. It shows backscatter intensity contours between -35 and -70 dB, and although the -70dB contours do reach to 10 metres, the ones at -45 dB (comparable with our data, see the scale on Figure 3) are much closer to ours, showing a contour that rarely reaches beyond 5m. We also note that the Vagle data is averaged over a period of 2.25 hours, whereas ours are 1 and 10 second averages. The way the Vagle data is processed would allow extremely small numbers of resonant bubbles at depths of 10 m to appear to create deep plumes. We think that the data in the Vagle plot is reasonably consistent with ours if the backscatter intensity (making their detection more sensitive) and time-averaging are taken into account, although they do appear to see slightly deeper bubbles at 12 m/s. There is a more in-depth discussion of similar data (but with a 200 kHz sonar) in the Vagle 2010 paper that we cite, which also shows contours that are in general agreement with ours at the same backscatter intensity level. We have added a note on this comparison to the discussion section (line 330). We note that we were sampling at a single location and it's not clear how similar bubble behaviour will be in very different areas of the ocean (with different temperatures, mixed layer depths and gas saturation levels), and that therefore a reasonable similarity between these two sites is notable.

Also for Figure3: I would suggest revising the ylabel for panels b and c as "void fraction". The current ylabels seem to be more appropriate as titles.

This has been changed in the revised manuscript.

Regarding the results in Figure 4: Figure 10 shows that the void fraction at 4 m could lag behind that at 2 m. Is that already considered in the correlation presented in Figure 4?

The data in figure 4 are one minute averages (stated in the caption) and the lags shown in Figure 10 are between 0 and 66 seconds (stated in the main text). The majority of the lags were 30 seconds or less and would have fallen within the same 1 minute averaging period. We have looked again at the data and conclude that the timing mismatches on the scatter plot are not the main influence on the lack of correlation on the scatter plot, partly because of the time averaging and partly because the data in Figure 4 includes all periods when the void fraction at 4 m was above the noise whereas the data in Figure 10 only shows the peaks (20 points, each at the peak of that plume). The point that we wish to make here is that the plume structure is locally heterogeneous, rather than to discuss specific causes. We consider that at least some of the mismatch between void fractions at 2m and 4m is likely to be caused by shear currents over longer periods of time, although other advection processes are also likely to contribute. We have added text to clarify this in the manuscript.

Currents in Figure 6 are useful to explain void fractions in Figure 5. I would suggest the author also try to connect Figure 6 to explain Figure 3. For example, the downward current at around 18:30 is strong than at around 18:10, but the increase in the void fraction is smaller. Is it because there is no breaking wave observed at 18:30?

Our full description of the mechanisms we propose is given in the companion paper, and there we suggest that the heterogeneous surface layer (the top 1m) is created by waves breaking randomly across the surface, but that it is only where this layer is pulled downward by Langmuir circulations that a deep plume can form. However, there can only be a plume if there were bubbles in the upper layer before it was advected downwards. The composite figure showing sidescan sonar data from Zedel & Farmer (figure 9 in their 1991 paper) shows long heterogeneous bubble plumes with large scale patterns evident but considerable local variation, which is consistent with what we see. We have added a comment about the comparison between Figure 3 and Figure 6, and the implications for heterogeneity on lines 375-380.

Although this is mentioned in the text, I would suggest adding that the measurement is at 3.8 m in the caption of Figure 6.

This has been added in the revised manuscript.

Figure 9: I would suggest the authors clarify if the rising/falling wind means a sustained period of rising/falling wind, like in Liang et al. 2017 JGR-Oceans referenced in the manuscript. Or do the authors include substantial periods when the wind is fluctuating?

The manuscript (lines 448 – 450) describes the distinction: "*If the hourly averaged wind speed was lower or higher than the mean of the previous two hours by 0.5 m/s, the data from that hour was labelled "falling" or "rising" respectively. Approximately one quarter of the data fell into each of those two categories. Otherwise, data points were considered ambiguous and are excluded*.". This is the only method used to separate the two categories. Periods of fluctuating wind are likely to be considered ambiguous and will therefore be excluded. We do not use the broad definition adopted by Liang et al, which compared a single 24h period of steeply rising winds to a separate 36h period of steeply falling winds. We have added brief text to the caption of figure 9 to clarify that the method used to separate rising and falling winds is in the main text.

The data in Figure 9(a) show that void fraction is higher at falling wind than at rising wind until about 20 m/s. This could be interesting results and I have different thoughts from the paragraph starting from line 456. Breaking waves are bigger at the rising wind, but Langmuir turbulence is stronger during the falling wind. I wonder if there is a possibility that void fraction at this depth is primarily due to Langmuir circulations at wind speed < 20 m/s and gets more contribution from breaking waves when wind speed > 20 m/s. The same argument could be used to explain that void fraction is mostly larger during

falling wind than during rising wind at 4 m. However, I could not make sense of why the void fraction is the largest when wind speed is at 10 m/s and 12.5 m/s. Is there any sampling error there?

We have added this potential explanation to the manuscript (lines 464 – 467). We are confident that there is no sampling error, but we feel that our original suggestion of hysteresis at the extremes of the wind speed range can explain the situation at the low wind speeds. We also note that a relatively small fraction of the data set was collected at the very lowest wind speeds, and so it may have less statistical significance.

Regarding the presentation of Figure 9. I would suggest stating in the caption that panel a is from data at 2 m and panel c is from data at 4 m.

This has been added in the revised manuscript.

Figure 9(b) is referenced after Figure 9(c). Perhaps the order of the panels could be changed.

We have chosen not to do this because we think it's more intuitive to compare figures (a) and (c) if they are directly above one another so that they effectively have a common x-axis. We are happy to make this change if the reviewer insists on it.

Both "parametrization" and "parameterization" are used. I would suggest the authors pick one of them.

The Oxford English Dictionary and papers from our colleagues in this area suggest "parameterization", so we have checked that this usage is now consistent throughout the revised manuscript.