Questions Regarding Nuclear Emissions in Cavitation Experiments

Taleyarkhan *et al*. (*1*) claimed evidence for D-D (deuterium-deuterium) fusion in cavitation experiments with deuterated acetone. A number of inconsistencies in that study, however—involving data on neutron yield, the reported response of the detector used, and coincidences between sonoluminescence (SL) and scintillator pulses—cast doubt on that claim.

As pointed out by Taleyarkhan *et al*. (*1*), if the tritium observed were due to D-D fusion, it would need to be accompanied by a similar neutron yield. However, the reported neutron yield (4×10^4 to 7×10^4 neutron/s) was a factor of 10 to 20 lower than the reported tritium yield (7×10^5 atoms/s) (*1*), a discrepancy that contradicts the claim that the tritium was due to D-D fusion. As discussed below, the effects cited in (*1*) as possible explanations for that discrepancy are not sufficient. Moreover, increasing the mismatch in the data, the claimed neutron yield was calculated based on an estimated detection efficiency for 2.5-MeV neutrons, which is a factor ~100 below levels that would be consistent with the reported detector set-up.

The detector used for the singles measurements [note 22 in (*1*)] was a liquid scintillator 5 cm thick and 5 cm in diameter. In describing the threshold setting of this detector, Taleyarkhan *et al*. stated that the cutoff was seen at channels 15 to 20 [figure 2.4(b) in supplement 1, online supplemental data for (*1*)] and that the 2.5-MeV edge lay around channel 40 [note 26 in (*1*)]. Thus, the effective threshold was set at or below a pulse height corresponding to 50% of the 2.5-MeV neutron edge. Using the standard benchmarked code SCINFUL (*2*), we calculated the detection efficiency for 2.5-MeV neutrons under those conditions to be 18.9% or greater. Taleyarkhan *et al*. also reported that the distance of the detector from the center of the test chamber was 5 to 7 cm. Choosing 6 cm as the mean, the solid-angle factor would be $\pi \times 2.5^2/(4 \times \pi \times 6^2) = 4.3 \times 10^{-2}$, which implies a net detection efficiency of 8×10^{-3} for the experimental geometry—inconsistent with the reported efficiency of 1×10^{-4} to 2×10^{-4} [note 26 in (*1*)]. These simple calculations suggest that the reported neutron yield should be reduced by almost two orders of magnitude.

Taleyarkhan *et al*. argued that at least part of the neutron-tritium difference may have been due to three factors: (i) "neutron energy losses by scattering in the test chamber"; (ii) "reduced detection efficiency for large-angle knock-ons from 2.5-MeV neutrons"; and (iii) possible nonuniformities in T concentration in the acetone [(*1*), p. 1872]. An upper limit for effect (i) can be derived using the total reaction cross-sections for 2.5-MeV neutrons to calculate the probability of scattering before leaving the acetone, assuming that all such scatterings result in loss of the neutron. The detector apparently viewed the interaction region at an angle of ~45° [figure 1 in (*1*)], so that the distance traveled in the acetone was 4.5 cm; those values suggest an upper limit to the losses of about 48%, and decrease the effective efficiency by a factor of two.

Effect (ii) is already accounted for by the choice of detection threshold and is correctly treated in the SCINFUL code. Effect (iii) is difficult to evaluate quantitatively, but we note that the acetone was agitated by cavitating bubbles for 12 hours, during which any dissolved tritium would have been dispersed through the volume. Taleyarkhan *et al*. proposed no mechanism that would concentrate any tritium produced by the reactions in a small fraction of the acetone volume, while leaving the larger fraction (75%) of tritium already present as a contaminant fully dispersed.

The detector response reported by Taleyarkhan *et al*. is inconsistent with the accepted response for liquid scintillators. The reported pulse heights corresponding to 14-MeV and 2.5-MeV neutrons were at channels 110 and 40, respectively [note 26 in (*1*)]. From published light curves (3), the pulse heights corresponding to these energies must be in the ratio of 10:1. Assuming that the electronics used by Taleyarkhan *et al*. exhibited an approximately linear response, the reported pulse heights could thus only be explained by a 32-channel offset in the data; however, the spectra presented [figure 2.4(a) and (b) in supplement 1, online supplemental data for (*1*)] clearly show that any offset is less than 5 channels. Absent a reasonably consistent accounting of the detector response, the neutron data reported cannot be reliably interpreted.

Finally, Taleyarkhan *et al*. reported coincidences between SL and scintillator pulses. They acknowledged that an independent experiment showed that "the coincidences observed may be random in nature"; indeed, as we argue below, the data reported in (*1*) would also have been dominated by random coincidences.

In discussing this issue, Taleyarkhan *et al*. stated that "the influence of random coincidences between SL and scintillator flash signals in the region of bubble collapse was estimated to be insignificant." Their calculation that only 0.03 to 1.6 random events would be seen in a typical 1600 second run with a 20-μs coincidence window [note 30 in (*1*)] appears to support this statement. However, their coincidence experiment did not distinguish between events occurring during the period of bubble collapse and events occurring at other times, in particular during the PNG (pulsed neutron generator) pulse. For the period of the PNG pulse, Taleyarkhan *et al*. (*1*) reported an average count rate of 500/s (p. 1872), a pulse width 12 μs (p. 1869), and a repetition frequency of 200 Hz (caption to figure 5); thus, the instantaneous rate during this period was $500/(12 \times 10^{-6} \times 200)$ counts/s $= 2 \times 10^5$ counts/s. The number of SL pulses seen during the time of bubble collapse was 1/s [note 30 in (*1*)]; the number seen during the PNG pulse was 30% of this, or 0.3/s [figure 3(a) in supplement 1, online supplemental data for (*1*)]. Substituting these two rates into the expression used in note 30 of (*1*), we find that the number of random coincidences to be expected in a 1600-s run is $(20 \times 10^{-6}$ s) $\times$ $(2 \times 10^5$/s) $\times$ (0.3/s) $\times$ (1600 s) ~ 2000.

This calculation overestimates the expected number of random coincidences by about a factor of two, because the coincidence time used (20 μs) is longer than the PNG pulse width (12 μs). Even allowing for that overstatement, however, ~1000 random coincidences would still be expected per 1600 s run—about ten times the rate reported by Taleyarkhan *et al*. Furthermore, the time structure for those randoms would be very similar to the shape of the PNG pulse, i.e., a peak with a width [full width at half maximum (FWHM)] ~4 to 6 μs. [(*1*), p. 1869]. Because the observed coincidence rates were dominated by randoms, any measurement of true coincidences must include a sufficiently accurate estimate of random rates to be meaningful.

The experimental evidence cited by Taleyarkhan *et al*. for D-D fusion rests on three basic observations: excess tritium, excess neutrons, and coincidences between neutrons and sonoluminescence light. As we have detailed here, however, the study presented significant internal inconsistencies in the measurements of neutron singles and neutron/SL coincidences, as well as a very large quantitative mismatch between the tritium and neutron data. These inconsistencies cast serious doubt on the claimed evidence for D-D fusion in these experiments.

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Response: We appreciate the opportunity to clarify issues and respond to the comments of Saltmarsh and Shapira on our study (*1*).

First, Saltmarsh and Shapira suggest that the number of neutrons detected appears smaller than that deduced from the tritium data cited (*1*) and that the detector efficiency does not match predictions of the SCINFUL numerical code. The neutron emissions reported (*1*), however, when appropriately corrected for losses (*2*), are in the range of $\sim 3 \times 10^5$ to 4×10^5 neutron/s, which is compatible (within one standard deviation of uncertainty) with the derived production rate of about 7×10^5 atom/s from the tritium data. Saltmarsh and Shapira used the neutron detection rates (*1*) that were not corrected for losses in the acetone, glass, etc. (*2*).

The detection efficiency for 2.5-MeV and 14-MeV neutrons was actually measured in (*1*). Despite the availability of direct experimental evidence (*3*) for both independent detectors, Saltmarsh and Shapira relied on theoretical calculations for detector efficiency in which the effect of discriminator thresholds and counting efficiency for different-energy neutrons were not included. The SCINFUL code predictions of detector efficiency presented by Saltmarsh and Shapira do not match the count rates measured (*2*) from a Pu-Be source of known strength with either their detector or ours; we thus conclude that, for our detector at least, the computer model is not accurate. Statistically significant increases in neutron emission—on the order of 10 or more standard deviations of effective change—were consistently measured (*1*) only in the 2.5-MeV range during tests with chilled cavitated deuterated acetone, and not for tests with natural acetone. Strict control of system geometry was maintained for tests with the baseline and control liquids (deuterated and natural acetone, respectively). The corrected value of $\sim 3 \times 10^5$ neutron/s agrees well with the derived tritium emission data; the difference, a factor of ~ 2, is within detection uncertainties.

Saltmarsh and Shapira also argue that the light output values shown in the online supplemental data for (*1*) are inconsistent with expected values for NE-213 liquid scintillation detectors. Using published light output curves, a 14-MeV proton recoil edge channel number of 110 implies that the "theoretical" Compton edge for cesium-137 emissions, which lies just below the 2.5-MeV neutron energy cutoff, should be around channel 10; Saltmarsh and Shapira suggest that, because the number of neutron counts below channel number ~ 20 in (*1*) is close to zero and because channel numbers between 30 to 40 were used in (*1*) as corresponding to the cutoff range for 2.5-MeV neutrons, the neutron data in (*1*) are questionable. However, we carefully calibrated our detector for determining the channel range for 2.5-MeV neutrons using sharp Compton edges from cesium-137 and cobalt-60 sources and for higher energies using a Pu-Be isotope source and 14-MeV pulsed-neutron generator source. It is significant to note that with our multichannel analyzer (MCA) and system settings, there was an offset in pulse height, such that zero pulse height corresponded to approximately channel 21. When this shift is taken into account, the ratio of our observed pulse height for 14 MeV to that for 2.5-MeV proton recoils is ~ 6 to 8. The various channels corresponding to edges for cesium-137, cobalt-60, Pu-Be, and 14-MeV neutrons exhibited a linear variation of light output with energy. This is in line with, and well within, the spread of 20 to 50% uncertainties of experimental data conducted with several detectors of different size, shape, and age (*3*–*7*). Therefore, we see no incompatibility—although, in retrospect, we should have noted in (*1*) that correcting the neutron spectra for the ~ 21-channel offset is necessary for comparisons with light output from similar detectors.

Finally, Saltmarsh and Shapira, examining our coincidence data, argue that our report has not provided any evidence of real (i.e., nonrandom) coincidences. The coincidence measurements in (*1*) were conducted in two modes of operation. In Mode 1, no false SL signals occur; therefore, there were no false coincidences. However, real coincidences were monitored only for chilled, cavitated deuterated acetone [figure 5A in (*1*)]. In Mode 2, with a higher bias voltage to the photomultiplier tube (PMT), false SLs occur during PNG operation, and this will indeed lead to false coincidences. Some readers—Saltmarsh and Shapira in particular—have misinterpreted the statement about the fraction of false SLs occurring during PNG firing: The quoted value (*1*) of 30% for false SLs corresponds only to the SL events recorded by our MCA for the first 100 ms after PNG firing. The rate of false SLs is actually more than 10 times smaller than the value of ~ 0.3/s used by Saltmarsh and Shapira; if this correction is made, the number of random coincidences occurring in Mode 2 operation (during PNG firing) amounts to less than 100, which is in line with our reported measurements of 60 to 70 (attributed to false coincidences occurring during PNG operation).

With cavitation turned on, we recorded around 30 to 45 true coincidences out of 100 total coincidences in a typical run. This was only observed during cavitation of deuterated acetone with testing at ~ 0°C, an effect that disappeared for tests at ~ 20°C and was not observed for natural acetone at any temperature. The random coincidences during the period of bubble implosion are estimated at $\sim < 3$ (*1*). Therefore, true coincidences were indeed observed during cavitation of chilled deuterated acetone during both Mode 1 and Mode 2 operation.

In addition to observing coincidences in a multitrace digital oscilloscope, we also obtained time-correlation data with conventional particle counting systems by taking time spectra using an MCA. The MCA data clearly showed (*1*) very statistically significant nuclear emissions (increases of 100% or more above background) precisely during the time interval corresponding to bubble implosion only for tests with deuterated acetone at ~ 0°C, an effect that was not observed for tests with deuterated acetone at higher temperatures nor for tests with the control fluid, natural acetone. Thus, two sets of measurements, oscilloscope and MCA, provided confirmation of time-correlated coincidence events.

Saltmarsh and Shapira conclude their comment by expressing doubt on our assertion that we have provided evidence of D-D fusion during acoustic experiments with deuterated acetone. We maintain, however, that our study (*1*) does indeed provide compelling evidence for fusion. That evidence includes the observation that statistically significant tritium activity increased only in chilled (~ 0°C) cavitated deuterated acetone; comparable-scale evidence for statistically significant neutron emissions (time correlated with SL emissions) in chilled cavitated deuterated acetone; the absence of neutron emissions and tritium production in irradiated control tests with natural acetone; and confirmatory HYDRO code (*1*) simulations that predict hot ($\sim 10^6$ to 10^7 K) and highly compressed conditions within the bubbles imploding in these experiments.

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