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Discussion

A Photographic Study of Cavitation in Jet Flow[1]

J. Raabe.[2] With and without polymer additives at different air contents in underwater jet flow by "transillumination" in high contrast enlargers combines the detection and observation of many high interesting phenomena with an intricate and refined, up-to-date measuring technique.

The authors may be congratulated on their effective experimental work.

Some questions may arise from the description of the experimental apparatuses, some others about the phenomena observed and some about supposed errors concerning time intervals.

On the one hand on page 132 it is mentioned that the inlet pressure to the nozzle was measured by a calibrated pressure gage. On the other hand the only varying pressure in the test according to the denotation and σ-formula used on page 133 seems to be the pressure p, which is defined as that around the jet and taken as atmospheric. Shall this mean that the atmospheric pressure was measured by pressure gage?

On page 132 it is mentioned that the camera used a rotating mirror to provide image-motion compensation of a predominantly axially-moving jet stream.

In this context the duration of an illumination period of one flash would be of interest. Obviously one aim of the snap shots was also the observation of the cavity growth, which simultaneously happens with the axial motion of the whole cavity. Since only the latter was compensated, the bubble growth indicated by the sequence of figures could be obscured by a too long duration of an individual flash. One example for this: The first cavity downstream from the nozzle in Fig. 7 shows (taken as a radial bubble) within the time interval mentioned of $\Delta t = 20\,s$ and by accounting for the length scale of approximately 10:1 a radial increment of

$$\Delta R = 0.803.10^{-3}\text{m}$$

This yields a "radial" growth rate of the bubble

$$R = \frac{\Delta R}{\Delta t} = \frac{0.803\ 10^{-3}}{20\ 10^{-6}} = 40\ \text{m/s}$$

Under the assumption that the tests were carried out in an altitude of sea level and with a water temperature of $T = 293$ K (20°C) the differential pressure $p-p_v$ can be approximated by

$$p-p_v = 98963\ \text{Pa}$$

and the density of water by

$$\rho = 10^3\text{kg/m}^3$$

From this yields the average jet velocity as

$$v = 14.08\ \sqrt{1/\sigma} \tag{1}$$

Introducing into (1) $\sigma = 0.13$ from Fig. 7 $\sigma = 0.13$ one obtains

$$v = 39.1\ \text{m/s}$$

That means: Axial and radial velocity components of the phenomena to be observed are of the same order of magnitude.

This is obviously contrary to the authors expectation when they compensated by a rotating mirror *only* the axial component.

Moreover, the last results reveal some doubts about a too long duration of one flash period in relation to the period between subsequent figures. Assuming a bubble growth rate according to

$$v = \sqrt{\frac{2}{3\rho}(p_v - p_\infty)} \tag{2}$$

in which p_∞ means the pressure outside the bubble (usually a tensile stress) and approximated by $p_\infty = 0$ it yields from (2) with $p_v\ (T = 20°) = 2383$ Pa and $\rho = 10^3\text{kg/m}^3$

$$v = \frac{2.2830}{3.10^3}\quad 1{,}4\ \text{m/s}$$

The discrepancy between the measured growth rate of bubble $v = 40$ m/s and this last reasonably expected value is in my opinion partly due to too a long duration of the flash. This deforms the cavities during light exposure time in connection with a supposed turbulence-induced cross-motion of these cavities.

Otherwise the very fundamental question would arise for the physical reason of a bubble growth rate of $v = 40$ m/s as shown e.g. in Fig. 7. I cannot imagine the pressure p_∞ around the bubble to be strongly negative in a free jet.

In connection with turbulence as one of the origins of this phenomenon it would be of interest to measure in such tests, simultaneously, the turbulence level.

The pressure of turbulence is indicated by the rippling of the jet boundary.

From drag reduction by polymere addition one knows also the influence of polymere additives on velocity profile and a turbulence level distribution.

Therefore it seems advizable and would be of high interest to measure during such observations the structure and intensity of the turbulence, which obviously here originates from instabilities of the vortex layer on the jet boundary. This measurement could be eventually realized by Laser-Doppler techniques.

An indication that the cavity formation has something to

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[1] By J. W. Hoyt and J. Taylor, published in the March, 1981, issue of the ASME Journal of Fluids Engineering, Vol. 103, No. 1, pp. 14-18.

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do with the ripple on the jet boundary may be recognized from Fig. 2, in which cavities are only situated on stations, where the jet boundary is convexly-outwardly curved.

It would be of interest to know the authors opinion about turbulence influence on cavity formation and its measurement.

Turbulence may also cause a slippage of the cavities in direction and across the main flow.

If one neglects the effects of bubble cross-motion, slippage, bubble growth rate, to a long light exposure time and assuming the bubble motion to correspond that of the jet velocity according to equation (1) and the latter to be constant over one cross section and the length scale of the figure mentioned to be correct, some obvious errors are contained in the figures with respect to the time interval indicated.

Fig. 2 Δt should be 30 μs instead of 10 μs
Fig. 3 Δt should be 19 μs instead of 10 μs
Fig. 5 Δt should be 60 μs instead of 80 μs
Fig. 8 Δt should be 9.4 μs instead of 28 μs
Fig. 9 Δt should be 17 μs instead of 28 μs
Fig. 10 Δt should be 31 μs instead of 42 μs

The "should be-values" were calculated by myself in tracking e.g. the front part of a definite cavity in two subsequent figures. By this a cavity path Δx is fixed. By the jet velocity v according to eq. 1) the time interval is obtained as

$$\Delta t = \frac{\Delta x}{v}$$

On the one hand the discrepancy between these calculated and mentioned time intervals may be due to the neglected effects of cross and lengthwise bubble slippage and bubble growth rate. On the other hand it may be caused by the impossibility to coordinate corresponding cavities in subsequent figures.

Also the coordination of wrong figures may be the cause for it and at last the mention of wrong time-intervals in the text may be an error.

A. Shima[3] **and T. Tsujino**[3]. This work found experimentally in jet cavitation that an addition of drag reducing polymers (polyethylene oxide and polyacrylamide) occurred the cavitation inhibition. As pointed out, a lowering of surface tension implies decreased cavitation resistance, but the authors should pay attention that the surface tensions of drag reducing polymer solutions are not always less than that of pure water. That is, as shown in Fig. 1,[4] an addition of polyethylene oxide into water makes small the surface tension, as well as the result of author,[5] while the value of surface tension for a polyacrylamide solution is almost similar to that of water. Hence the surface tension is not an essential factor as the suppression of cavitation inception due to polymer additives is discussed. We think that it is important to examine non-Newtonian characteristic of the flow field and the bubble behavior in polymer solutions. According to our theoretical investigation[4] on the bubble dynamics, the damping of the bubble oscillation in a PAM solution appears more strongly than the cases of polyox solution and water (see Fig. 2). We will expect further consideration on their points from the photographs of great interest conducted in this paper.

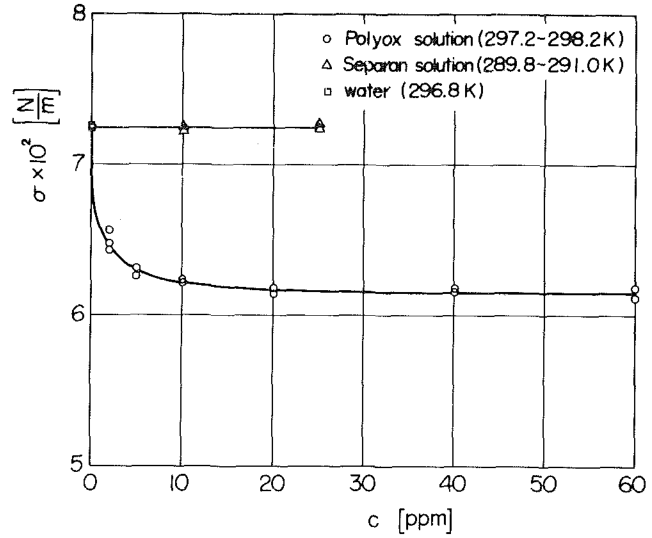

Fig. 1 **Relation between surface tension σ and polymer concentration C**

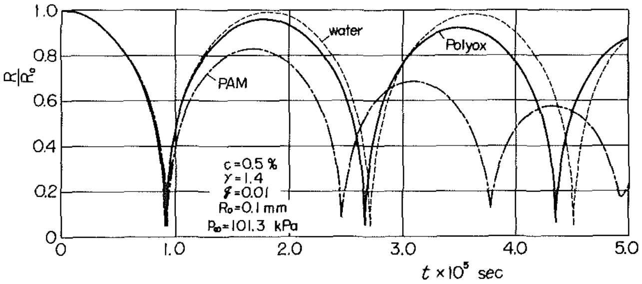

Fig. 2 **Comparison of bubble radius-time history in a polyox solution with a PAM solution**

(γ: ratio of specific heats of gas in a bubble, R_0: initial bubble radius, q: pressure ratio inside and outside of a bubble R: bubble radius, t: time, p_∞: liquid pressure far from a bubble)

Authors' Closure

The comments by Professors Shima and Tsujino are most welcome, since they introduce new experimental data regarding the properties of polymer solutions. In view of their data showing large differences in the surface tension of the two polymers used in our study, it appears that surface-tension modification may not be an important parameter in the cavitation characteristics of polymer solutions. Both poly (ethylene oxide) and polyacrylamide solutions show major cavitation appearance changes compared with pure water, even in concentrations of 25 ppm. Other non-Newtonian characteristics of these polymer solutions may have important influences on bubble oscillations, as shown in the discusser's second figure. We await with great interest the publication of the details of the calculations shown in the figure.

Professor Raabe asks as to the value of atmospheric pressure used in our calculations; since the computed cavitation number is more sensitive to velocity than local atmospheric pressure for these jet experiments, we used a standard value (101.3 kPa) for the atmospheric pressure.

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[3] Institute of High Speed Mechanics, Tohoku University, Sendai, Japan.
[4] Shima, A. and Tsujino, T., Unpublished.
[5] Hoyt, J. W., "Effect of Polymer Additives on Jet Cavitation," ASME JOURNAL OF FLUIDS ENGINEERING, Vol. 98, 1976, pp. 106-112.

Professor Raabe also questions the usefulness of the image-motion compensation technique as applied to cavitation photography, since he computes the bubble growth rate to be of the same order of velocity as the jet itself. Accepting his calculations as to bubble-radius velocity for the moment, it is still apparent that stopping the axial motion of the jet by the camera mechanism reduces the amount of axial bubble motion to be stopped by the flash. Experiments have shown that we consistently achieve sharper photographs when using the motion-compensation technique.

We have gone over our equipment, electronics, and test records carefully, and are convinced that the flash duration was very close to 1.5 μs, well within the 1 to 4 μs specified in the text. Since the bubbles in the pairs of photographs can be expanding, contracting, rotating, and nonspherical as well as translating, we have no faith in calculations which purport to show bubble growth rate or bubble axial velocity, based on scaling these photographs over the rather long time range involved. Our timing-interval electronics were not very sophisticated (being constructed by the authors themselves) but nevertheless, based on our calibrations, we feel that they yield much more accurate time intervals than one could estimate by scaling bubble movement from the photographs. In going over our results again, we did find a caption error regarding time intervals which we have corrected in the final text, so we are grateful to Professor Raabe for drawing our attention to this area. We agree with Professor Raabe that details of the turbulent flow in the cavitating region of the jet would add grately to our understanding of the physics involved.

We especially thank the discussors for their comments and additional information, all of which combine to make the study of the effect of polymer solutions on cavitation even more intriguing.

Turbulent Boundary Layer Flow Through a Gap in a Wall Mounted Roughness Element[1]

I. P. Castro.[2] My only major criticism of this work is the author's use of ideas developed to describe the growth of an internal layer after a step change in roughness (for which the final equilibrium flow differs from the initial one) for their case, which is essentially one of a wake flow relaxing back to the same initial equilibrium flow. I believe this has led to some confusion in their thinking. They state, quite correctly, that "the mechanism of flow readjustment downstream of the distortion proceeeds by a flow modification that works outwards from the wall" but confuse this internal layer, whose characteristics are determined mainly by the wall conditions (as in the roughness change case) with the "wake" behind the gap; the "edge" of the former is mistakenly taken as δ_i. The latter is simply the outer edge of the wake which does, of course, also move outwards but at a rate which is certainly not determined by wall parameters. The authors do not state to what distance from the wall their Clauser-plots for determining C_f had the usual log-law behaviour, but I surmise that it was always substantially less than δ_i. Indeed, despite the author's claim that equation (4) gives 'a useful description of the internal layer growth', Fig. 4 does, in fact, show that δ_i is always larger than that given by equation 4 by a factor of 3 or 4 in the early stages of relaxation of the wake. With their definition of δ_i I do not believe that z is a relevant length scale at all.

The final comment concerns the author's remarks regarding Fig 4. It would have been interesting to plot profiles measured at the same $x_{/D}$, rather than $x_{/H}$. Far downstream the flow will (to first order) only know about the change in the drag of the block (or, perhaps the change in *moment* of momentum - Counihan, Hunt and Jackson, 1974) which is presumably determined solely by changes in $D_{/H}$. It is therefore more likely that the proper scaling necessary for any possible downstream similarity should be based on $x_{/D}$. In fact, the mean flow profiles demonstrate that the thickness of the inner region is considerably greater for $D_{/H} = 0.5$ ($x_{/D} = 448$) than it is for $D_{/H} = 10$ ($x_{/D} = 22$), in line with the preceding argument.

Authors' Closure

The authors do not believe that the flows studied can be described as "essentially a relaxing wake flow;" in fact two of the flows ($D/H = 5,10$) have their wake component *reduced* by the notch. The readjustment of the flows is initiated and driven by the wall flow because it is the wall flow which quickly adjusts to the new conditons. The outer (wake) flow is driven back to equilibrium conditions on its inner boundary by the wall flow. This is illustrated in figure 2 where the outer wake flow is invarient (rather than "relaxing") from $X_{/H} \simeq 8$ onwards and is not changed until the inner readjusting flow works its way upwards through the wake. The growth in the height of this inner flow (for which δ_i is a standard notation) is a central feature of the flow as it is a measure of the flow's readjustment after the perturbation. It is difficult to see how the growth in the outer edge of the wake, which would be governed primarily by the entrainment of inviscid fluid by the outer flow, could be of the same importance.

The use of Z as a nondimensionalizing parameter does give a collapse of the results which is substantially better than that given by any other parameter. The results shown in Fig. 3 show a fairly high degree of correlation with each other even though they come from widely differing flows, $D/H = 0$ to $D/H = 10$. The use of Z is offered simply as the best procedure known to the authors.

The use of X/D as the scaling parameter does not result in downstream similarity either in the mean velocity or the longitudinal turbulence field. This can be appreciated by converting the X/H values on the profiles in Fig. 4 into X/D values and then comparing profiles with approximately corresponding values of X/D for the two gap sizes ($D/H = 1$ and 5). The authors hold to their view that far downstream of the gap the flows have forgotten the details of their distortions and are reapproaching equilibrium in a similar manner and that Fig. 4 supports this view.

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[1] By W. H. Schofield, D. S. Barber, and E. Hogan, published in the March, 1981, issue of the JOURNAL OF FLUIDS ENGINEERING, Vol. 103, No. 1, pp. 97-103.

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