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DISCUSSION

M. S. Plesset[2]

The authors are to be complimented for the excellent work reported in this paper. Their studies are distinguished by the wide range of their observations, and their findings will be of great value in our attempts to get a better understanding of cavitation erosion.

One of the main objectives of this program, as I understand it, was a comparison of cavitation damage in the field with the damage observed in the laboratory with a magnetostrictive device. They suggest that their water tunnel system simulates field conditions. I would like to indicate some reservations regarding their water tunnel observations as they might compare to other water tunnel experiments. The working section of their water tunnel has a diameter of 5.3 cm which is unusually small; in addition, their models ranged from about 1 cm to 3.3 cm in diameter. Ordinarily, one would consider this water tunnel unacceptably small. In addition to the obvious difficulty of a large boundary layer in their working section, there is the serious problem of scale effects affecting all their observations. Perhaps these unusual conditions explain some of the behavior which they have found. They report, for example, the greatest damage rate in the flow conditions characterized as "Zone B" in which large cavitation clouds are formed and carried off periodically. This flow condition is not the one in which the greatest erosion effects are ordinarily encountered. In larger facilities the destructive range is expected to occur in conditions more closely like their Zone A, since cavitation inception, or the early stages of cavitating flow, is the flow regime in which the strongest cavitation effects are to be expected. In cavitation inception only very small cavitation bubbles are formed and these extend over a small region of the flow. In spite of the small size of these cavitation bubbles and the small region of flow over which they are observed, the most intense noise radiation is encountered together with the greatest potential for damage.

In their experiments with the magnetostrictively induced cavitation the authors have verified the observations already made on the variation of cavitation damage rates with exposure time [15].[3] It is gratifying to have an independent corroboration of the behavior which we reported in detail some years ago.

The authors have emphasized that there are chemical, or corrosive, effects in cavitation erosion. Again these observations are similar to findings previously reported [16]. One of the simplest ways to reduce corrosive effects and to emphasize purely physical effects in cavitation damage in vibratory tests in the laboratory is to go from water with dissolved air to a less active liquid with inert gas. We reported some time ago [16] cavitation damage observations in liquid toluene from which any dissolved air was removed and which was saturated with gaseous helium. The cavitation damage measurements were then made with a helium atmosphere over the liquid toluene. As compared with the water-air system no great change in physical effects is to be expected but the damage rates in the toluene-helium system were considerably reduced corresponding to a reduction in chemical effects.

A general feature of cavitation damage with vibratory devices in the laboratory is the high rate of material removal. These high rates of material removal do tend to reduce the expected chemical effects in cavitation damage. Good correlation with field observations is therefore not easily obtained. It was to overcome this deficiency in this kind of laboratory experimentation that the "pulsed technique" for generating cavitation damage was developed [16]. This method goes far to improve the significance of laboratory results so that a more significant relationship with field experience is to be expected. The pulsation method for generating cavitation damage may be more useful than water tunnel experiments, particularly those of questionable scale.

I believe that the authors measurements of the hardness of the specimens after exposure to cavitation are particularly elegant. They show that the surfaces of the specimens have received work-hardening as a result of exposure to cavitation. They also find the thickness of the work-hardened layer and they have obtained photomicrographs of damaged specimens. It has been known for a long time that metals and alloys are damaged in cavitation by plastic deformation and work-hardening [17, 18]. Not only was photomicrography utilized in those experiments, but more importantly the effect of plastic deformation and work-hardening by cavitation was demonstrated by the use of X-rays. The X-ray technique is a useful and powerful one in bringing out the plastic deformation process. In these previous studies the depth of the plastic deformation was found to be significantly less than that reported by the present authors who find an effect for depths from the surface in excess of 100μ whereas our previous X-ray studies showed that the cavitation effect disappeared at depths of 35 to 50μ.

The authors give as their main conclusion that the mechanism

[2] Professor of Engineering Science, California Institute of Technology, Pasadena, Calif. Mem. ASME.

[3] Numbers in brackets designate Additional References at end of discussion.

of cavitation damage in the vibratory unit is different from that in the water tunnel. I believe that this conclusion is not justified. It is clear that the basic mechanism of plastic deformation and work-hardening is operating for both these methods of generating the damage. The authors' own data show the same depth of work-hardening of the specimens. There are some differences in their findings of work-hardening as a function of depth. The temperature behavior in the water tunnel data shows a very clear increase in going from 20 C to 50 deg C. The data for the vibratory unit, on the other hand, do not show the same clear increase with temperature although the trend seems to be the same. In this connection, I would call the authors' attention to recently reported findings [19] on the temperature effect in cavitation with a vibratory unit. In those experiments a very clear rise with temperature from 0 deg C to approximately 50 deg C is found for materials similar to those used by the authors.

I believe that in arriving at their conclusion the authors here put too much weight on some differences between photomicrographs for specimens damaged by the two means which they used. The fundamental process of plastic deformation is known to be the same and the differences pointed out by the authors could readily be ascribed to differences in scale of the two kinds of collapsing cavities generated in the two facilities.

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Authors' Closure

The authors wish to thank Prof. Plesset, a pioneer in the field of cavitation erosion research, for his interesting discussion and comments on the following points, their reply to which is as follows:

1 The authors recognize themselves the same fact pointed out by the discusser that there are some wall effects in the water tunnel system used in their study as shown in Fig. 5. Nevertheless, it does not deny their principal conclusion. Because, for instance, the rate of cavitation damage always denotes its maximum value in the zone Ⓑ, not only for the d/D value of 0.420 but also over the whole range of that experimented ($0.180 \sim 0.624$) as shown in Fig. 32 [20].[4]

2 It is to be praised that the discusser adopted a liquid toluen-helium system to get a pure cavitation erosion not accompanying corrosion. However, on the other hand, it is to be noted that the results obtained from this should be separated in discussion from the usual water-oxygen system with cathodic protection techniques because of their differences in physical and chemical conditions, as already suggested by the discusser himself [21]. Here, another example is given by the present authors in Fig. 33 [22] which shows fairly the effect of adhesion work of an environmental liquid on the critical Reynolds number of the sphere in a pipe.

3 Considerable differences in the thickness of the work-hardened layer by cavitation erosion between the discusser's data and the authors' have not been come from the difference in measuring method between a X-ray technique and that of micro-Vickers hardness, but have rather been caused by the difference in test conditions, that is, by the relative connection between a metal used and its environmental conditions, especially such as temperature, cyclic collapse pressure intensity, its frequency, duration time of test and therefore total numbers of cycle.

[4] Numbers in brackets designate Additional References at end of Closure.

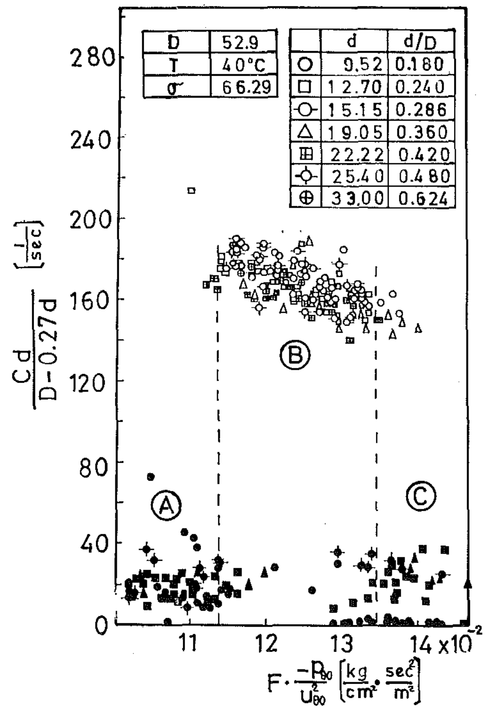

Fig. 32

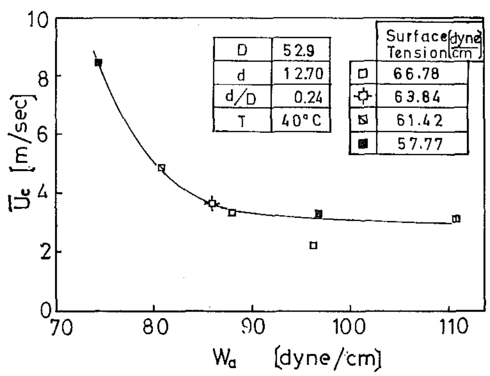

Fig. 33

4 The authors should like to repeat their main conclusion without hesitation that the mechanism of cavitation damage in the magnetostriction vibratory unit is different from that in the water tunnel system.

It is well known that the plastic behavior and the strength of metals depend not only on their compositions and structure but also on loading speed, test temperature and time, even without citing two typical instances such as high temperature creep and low temperature brittleness of metals. Moreover, the concept of mechanical equation of state for a solid material is generally of approved [23].

Having these understandings we can easily interpret the fact shown in Figs. 30 and 31, and also assert the validity of our discussion and conclusion mentioned in this paper.

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