

## DISCUSSION

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*A. F. Conn*<sup>1</sup>—This question is actually to allow the authors to emphasize the differences one might expect between their corrosive magnetostrictive erosion tests and the effects which one might observe during cavitation erosion caused by flow of a corrosive fluid past a surface. This point was not brought out during the oral presentation of the paper. Since much of the discussion which occurred after the talk was related to concern about the proposed micromechanism of hydrogen stimulation of the generation of pits and how this model seems to differ from certain observations of field difficulties which involve flow-generated cavitation erosion, the differences for vibratory tests in the laboratory should be discussed.

*C. M. Preece*<sup>2</sup>—Although unstressed aluminum alloys may be passivated in distilled water, when they are subjected to a tensile stress, they are found to be susceptible to stress corrosion cracking even in moist air. You should, therefore, consider both salt water and distilled water as active, rather than inactive, environments for aluminum alloys.

You postulate that atomic hydrogen is produced during cavitation, yet you do not consider the role of hydrogen embrittlement in erosion. Should you not take this into account in view of the fact that all the alloys you studied are known to be susceptible to hydrogen embrittlement?

*J. W. Tichler*<sup>3</sup>—The results of this interesting study are interpreted exclusively in terms of Thiruvengadam's "Theory of Erosion."<sup>4</sup>

In this theory, Dr. Thiruvengadam attaches special importance to the peak rate of volume loss. The time at which this peak rate occurs ( $t_1$ ) appears as a characteristic time in a distribution, associated with the

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<sup>4</sup> Thiruvengadam, A., in *Proceedings, 2nd Meersburg Conference on Rain Erosion and Allied Phenomena*, Vol. II, A. A. Fyall, Ed., Royal Aircraft Establishment, Farnborough, England, 1967, pp. 605-651.

break out of wear particles. However, as has been stated previously,<sup>5</sup> it is not readily understandable why this characteristic time, which should anyhow be related to material properties, must be equal to the time at which the rate volume loss goes through a maximum. In other words,  $t_1$  appears in two formulas, namely, in the  $r$  of Formulas 7 and 9 of the theory,<sup>4</sup> and it is not made clear, why  $t_1$  stands for the same property in both these formulas.

Another problem is that the cause of the decrease in erosion intensity is not clearly described. It is suggested that the decrease of the erosion intensity should be due to an increase of distance between the eroding surface and the collapsing cavities. However, it is much more probable that the decrease in erosion intensity at increasing mean depth of erosion is related to the surface condition of the eroding material. Eisenberg et al.<sup>6</sup> did already observe that the peak of the rate-time curve coincides with the appearance of the first deep craters in the eroding surface. They did not, however, attribute the effect to gas bubbles which can be trapped in the craters, as was shown by Tichler et al.<sup>7</sup> More objections against the theory have been formulated by Hammitt.<sup>8</sup>

From the foregoing it follows that the physical meaning of the constants introduced in Thiruvengadam's theory (namely, the characteristic time  $t_1$ , the "shape parameter"  $\alpha$  and the "attenuation exponent"  $n$ ) is not clear. Thus, expressing the effect of seawater (compared with distilled water) on these parameters is not very useful to deepen the insight.

In my opinion it is more useful to analyze the mean depth of erosion versus time curve (as strongly encouraged by the new ASTM standard, mentioned by the authors), rather than the rate-time curves, and to investigate the effect of corrosion on the phenomena of uniform material removal and pit formation.<sup>5</sup>

From the authors' observations, it turns out that the resistance against uniform material removal is lower in seawater than in distilled water. This effect is more pronounced for the HY 130 and SAE 1020 steels than

<sup>5</sup> Tichler, J. W. and De Gee, A. W. J., in *Proceedings*, 3rd International Conference on Rain Erosion and Associated Phenomena, Vol. II, A. A. Fyall, Ed., Royal Aircraft Establishment, Farnborough, England, 1970, pp. 847-879.

<sup>6</sup> Eisenberg, P. E., Preiser, H. S., and Thiruvengadam, A., *Transactions*, Society of Naval Architects and Marine Engineers, Vol. 73, 1965, pp. 241-286.

<sup>7</sup> Tichler, J. W., Van den Elsen, J. B., and De Gee, A. W. J., *Journal of Lubrication Technology, Transactions*, American Society of Mechanical Engineers, Series F, Vol. 92, 1970, pp. 220-227.

<sup>8</sup> Hammitt, F. G., discussion on "Theory of Erosion" by A. Thiruvengadam, *Proceedings*, 2nd Meersburg Conference on Rain Erosion and Allied Phenomena, Vol. II, A. A. Fyall, Ed., Royal Aircraft Establishment, Farnborough, England, 1967, pp. 650-651.

for the HY 80 steel, which might be attributed to the 1 to 2 percent of chromium content in the HY 80 steel. For Al 5086 H117 the effect is less pronounced than for the steels. If the effect cannot be explained by simple addition of the erosion and corrosion effects, there are two possible explanations: either the corrosion is highly accelerated by chemical activation of the surface due to the impingements, or the erosion is accelerated due to decrease of the erosion resistance of the surface material by corrosive (that is, chemical) effects. I have the impression that the latter explanation is the correct one.

Further, the authors observed that in seawater the craters are shallower than in distilled water. This observation, in combination with the observation that the decrease of erosion rate is less pronounced in seawater than in distilled water, substantiates the hypothesis that the decrease in erosion rate is due to bubbles trapped in the craters. Obviously, gas bubbles will be trapped in deep craters more easily than in shallow craters.

*Terence McGuinness and A. Thiruvengadam (authors' closure)*—The authors greatly appreciate the constructive comments and questions in the discussions. The problems highlighted could be the basis for further meaningful research.

In referring to the different conditions created by various tests, Dr. Conn has raised an interesting and often discussed question. The basis of the vibratory device was to serve as a screening test for material properties, and, as such, the necessity of simulating the flow field was not that critical. As discussed in Refs 5 and 31 of the paper, if various hydrodynamic parameters are controlled and dynamic similitude attained, the flow field is essentially modeled.

There are currently several laboratories performing parallel experiments on both continuous-flow, venturi-type devices and vibratory tests. A more definitive answer to Dr. Conn's question should be forthcoming. However, it is felt that the mechanism of hydrogen stimulation of micropits is independent of the type of fluid flow involved in generating cavitation. Hydrogen remains on the surface of hydrophobic (water hating) and corrosion-prone alloys (Ref 31 of the paper). Hydrophobic materials are those materials that have low wetting abilities and thus have a tendency to form gas cavities with an adjacent liquid in cracks and crevices (Ref 31 of the paper). Also, hydrogen, acting as a stationary cavity, would follow the course of a pit into a material surface. Tulin's theory (Ref 35 of the paper) did not specify how the cavitation bubbles were generated. The important parameters were the presence of a stationary cavity and the shock wave of a nearby imploding bubble or jet.

The proposed mechanism of micropit formation at the cathode region

by hydrogen stimulation, nevertheless, does not exclude other additional causes of accelerated cavitation erosion. Such damage by hydrogen can be caused by electrochemical action at anode areas deep within erosion pits.

Hydrogen embrittlement could possibly be another cause of accelerated erosion damage. There are results indicating that many alloys are susceptible to embrittlement. Thus, such a phenomenon could be examined further in relation to erosion, as Dr. Preece indicated.

The implication of hydrogen embrittlement is not in contrast to the authors' mechanism of pit formation. Both mechanisms lead to local weakening of the material surface, the initiation of pits or cracks within the material and the focusing of energies in small volumes of material. Only the origins of the two mechanisms differ. Thus, for corrosion-prone materials, electrochemical action could initiate hydrogen formation. Even without a corrosive environment, hydrogen embrittlement could be acting at the surface, locally weakening the material's erosion strength. However, at the high strain rates of cavitation, it is very likely that hydrogen is not forced into the surface but rather remains as a gas bubble affixed to the surface ready to act as an ultrajet (Ref 31 of the paper).

Dr. Tichler's question of the physical meaning of the various constraints in the "Theory of Erosion" has been raised for several years. Actually both Tichler's research and others have demonstrated exactly the characteristics of the theory in question. The difference lies only in the intensity of erosion found in the test apparatus. Dr. Tichler's research, for the most part, has been concerned with intensities near the threshold of erosion. The resulting data often were much more erratic than results from higher intensity tests. One simple reason is related to the fatigue failure mechanism of material loss. Since fatigue failure and erosion are assumed to be statistical in nature, it is quite possible that for numerous test intervals in a threshold intensity test, a material might not exhibit substantial weight losses. The resulting curve of weight loss or mean depth of erosion versus time would be very erratic. It is suggested that Dr. Tichler conduct further tests in the range of the ASTM standard (Ref 6 of the paper) one watt per meter squared intensity to confirm these statements.

Even though the hydrogen stimulation mechanism is electrochemical in nature, the proposed erosion is assumed to be purely mechanical. This, of course, does not preclude changes in surface conditions, but the conditions only prepare the material surface for accelerated damage by reducing the erosion strength.

The resistance against uniform material removal, as mentioned by Dr. Tichler, was observed to be lower in seawater than in distilled water. But the cause of shallower craters was explained in Fig. 17. More

erosion occurred on the walls of the initial pit than in the bottom of the pit. The total amount of material removal is greater when the walls are damaged more quickly than the pit bottom due to the smaller distance between the mean eroded surface and the origin of erosion intensity. The formation of gas bubbles trapped in deep craters is not necessarily the reason for the erosion rate decrease. Even without the entrapped bubbles, a deep cratered material would have its mean surface of erosion at a much further distance away from the collapsing bubbles than the material just mentioned.