DISCUSSION

A. A. $Fyall^1$ (written discussion)—One of the disadvantages of smallradius, high-angular velocity rigs is the high repetition frequency of impact. While the metals are less susceptible to such effects than polymeric materials with their often complex relaxation spectra, this feature should not be ignored. The frequency of this rig is much higher than that occurring in the natural phenomena.

A simple correlation would be the evaluation of, say, 1100–0 aluminium on the in-board position, using two jets; then test at the outboard position using one jet at the same speed, thus effectively reducing the repetition frequency by a factor of three. A change of one or two orders of magnitude of frequency would be more acceptable but is outside the scope of the present apparatus.

Additionally, the equipment ensures exact coincidence of blows from the interrupted jet. Practical problems of rain or steam turbine erosion show relatively long intervals between even half-coincident impacts. Gross erosion often occurs by overlap of damage areas rather than by augmented damage of a localized site. One queries if this feature of the rig may not account for the apparently low values of threshold speeds. Typically, King² gives U_c for 99.5 percent aluminium as 417 ft/s. Examination of the surface topography of erosion by this rig and by more conventional rigs or by flight evidence may establish this point.

On the effect of velocity on erosion rate, the following comments are offered.

The validity of using peak erosion rate is somewhat questionable. The amounts of material lost at this time are so low as to indicate very little loss in thickness. (Incidentally, the mean depths of erosion are quoted as being calculated but do not appear in the text). The practical application of erosion data would require a knowledge of the steady state of erosion. Rolls Royce Ltd. have state, as an example of aero turbine blade erosion, that the maximum permissible loss in thickness would be 0.2 in. at the top, that is, in the maximum damage area, in 10,000 h of operation. With a specimen of only $\frac{3}{32}$ in. diameter, erosion to such a depth could introduce serious limitations, as the physical boundary conditions, by no means, could be accepted as semi-infinite.

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² Fyall, A. A. and King, R. B., eds., *Proceedings*, 2nd Meersburg Conference on Rain Erosion and Related Phenomena, Royal Aircraft Establishment, Farnborough, England, 1968.

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Heymann² suggests that if the velocity dependence is expressed in terms of the absolute velocity, then the exponent will lie between 4 and 6. However, he also states that the best equation for all data is given by:

$$E_{\alpha}\left(\frac{U}{U_{c}}\right)^{4}\left(1-\frac{U_{c}}{U}\right)$$

An alternative formula is the Royal Aircraft Establishment version

Rate
$$\alpha (U - U_T)^{2.5}$$

In Ref 3 of the paper, the threshold concept was used by Thiruvengadam and Rudy, but resulted in

I peak
$$\alpha (U - U_T)^5$$
.

This seemed at variance with the coexistent formula

I peak αU^5

A check calculation on the published data reveals for stainless steel

I peak $\alpha (U-150)^{3.4}$

which is much more consistent with Heymann's analysis. The equation is, however, fairly insensitive to the value of U_c .

As the work discussed involved the determination of the threshold velocities, it would seem advisable to include the concept in any equation determining rate, as, in some measure, it represents the strength of the target material.

The "water-hammer" equation of de Haller has been extensively modified by various workers, and the true expression for the impact pressure would appear to be somewhat controversial. (See Heymann,² and discussion thereto by Engel, Brunton, and Field.) The reasons for direct mathematical equivalence appear somewhat tenuous and not amenable to physical explanation. The apparently low threshold speed may again be a feature.

At the risk of being repetitive, I must say that, as was criticised previously by Professor Hammitt,² the theory of erosion is somewhat oversimplified to explain the complex mechanisms of damage by erosion or cavitation.

Also, like Kean,² I would like to see the results of other forms of cavitation erosion embraced by the solution.

Despite these criticisms (or perhaps more properly, queries), I have found this a most stimulating and interesting paper.

J. H. Brunton³ (written discussion)-I would like to congratulate the

³ University Engineering Department, Cambridge, England.

authors on their interesting paper and raise one point of discussion. The results presented in Figs. 5 and 6 relating the inception of erosion damage to fatigue failure, use, as a measure of erosion endurance, the number of impacts to produce visible plastic depressions at $\times 10$ magnification. For the purposes of this comparison, would it not be better to measure erosion endurance as the number of impacts to produce first weight loss, or at least, first fracture? This would mean that both the fatigue results and the erosion results would then refer to the number of cycles required to produce appreciable crack propagation. A correlation here, if it exists, would be much easier to explain than the one investigated. Since the erosion curves for fracture or first weight loss would be to right (larger number of impacts) of those for plastic depressions, the multiplying factors needed to match the water-hammer pressure with fatigue strength would be lower.

To the authors list of possible explanations for discrepancy between fatigue strength and impact pressure could be added the one that impact pressure is not the same as the stress induced in the solid. The maximum surface stress can be greater or smaller than the impact pressure and depends on the distribution of this pressure over the surface. The production of smooth depressions depends upon the maximum shear stress induced in the solid; the indications are that this stress is less than the impact pressure. In fatigue tests, of course, surface stress is the stress which usually is plotted against cycles to failure.

J. F. Ripken⁴ (written discussion)—The curves showing rate of volume loss for the impact erosion of stainless steel include comparative data for variations of the impact velocity. These variations were obtained by different combinations of rotor revolutions per minute and radial position of the specimen on the rotor. If the data are to be used to obtain a meaningful relation between the velocity and the loss of volume, it is essential that the test data yield curves with similar shapes and rate loss values regardless of which combination of revolutions per minute and radius were employed to give a particular impact velocity. The impact data for stainless steel do not appear to have this consistency. For example, data for a specimen velocity of 348 ft/s obtained with a disk speed of 10,000 rpm show a peak rate loss of about 6×10^{-4} cm³/h after 2.4×10^6 impact cycles, whereas a specimen velocity of 350 ft/s obtained with a disk speed of 6700 rpm shows a peak rate loss of about 20×10^{-4} after 1.6×10^6 impact cycles. These are rather serious shifts in maguitude that are in need of explanation.

Could it be that radial acceleration of the water following impact with a specimen moving on a curved path is significant to the erosion mechanism? The radial accelerations can differ with the radial position even though the tangential velocities may be adjusted to be comparable. The

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cross section of the erosion groove in stainless steel, as shown in Fig. 8, is distinctly asymmetrical. Is this asymmetry part of the same problem? Similar asymmetry of erosion patterns have been evidenced in the results from other rotary erosion facilities involving cavitation erosion as well as impingement erosion.

It is speculated in the paper that attenuation of the erosive forces may ultimately occur as a consequence of the liquid which may remain in roughness craters. While this is quite conceivable for test devices or prototype systems experiencing linear motions, it is difficult to conceive how appreciable liquid residuals can remain in craters which are rotated at high speed in a gas or vapor. This strong centrifugal removal of the liquid cover should occur in either rotary impingement test devices or in prototype steam turbines.

F. J. Heymann⁵ (written discussion)—It is very encouraging that several investigators are now examining the relationship between erosion and fatigue in a well-planned manner which may lead to some sound quantitative conclusions. This paper is a distinct contribution toward this aim. The authors find that some agreement between erosion and fatigue S-Ncurves is obtained when the water-hammer stresses (author's Eq 3) are multiplied by a factor ranging from 1.5 to 5.0. This seems to support recent analytical results⁶ which predict that the maximum pressure due to impact of a curved liquid body, in velocity range here of interest, is approximately three times that given by Eq 2.

The agreement should be regarded with caution, however, since we are comparing locally applied unidirectional repeated compressive stresses, on the one hand, with uniformly applied alternating compressive and tensile stresses, on the other hand. A true comparison must take into account the difference between unidirectional and alternating fatigue environments, the question of which stress components actually govern fracture, and the effects both on stress distributions and material properties caused by the impulsive nature of the liquid impact forces. The literature still seems to lack a serious attempt to analyze this, and the authors would perform a real service if they would tackle that problem.

Another question concerns the criterion for the number of cycles to failure for the erosion results. The authors have used the number of impacts to cause the appearance of plastic deformation on the surface, but there are arguments for suggesting that the nominal number of cycles to failure is represented by the number of impacts at which the erosion rate reaches its peak (Ref 18 of the paper).

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⁶ Heymann, F. J., "On the High-Speed Impact Between a Liquid Drop and a Solid Surface," *Journal of Applied Physics*, Vol. 40, Dec. 1969, pp. 5113-5122.

This brings me to the matter of the mathematical relationship between the erosion rate-time curve and fatigue life distribution functions. I must express my serious misgivings about the physical rationale for the authors' use of the Weibull type cumulative distribution function (Eq 5) for the "efficiency" η in their "differential equation of erosion" (Eqs 3 and 4). The derivation is described only briefly in this paper, but was given in detail in their Ref 2. There it is shown that $\eta(t)$ represents the time dependence of the *instantaneous erosion rate* (or erosion intensity, to use the authors' term), attributable to the prior history of exposure and the statistical nature of erosion or fatigue. If *n* were zero, *I* would be proportional to η . The appropriate analogy here, in my view, is with the frequency distribution rather than the cumulative distribution, as I shall explain:

Suppose one could set up a large number of fatigue machines with identical specimens, and start all tests at the same time and the same stress level. The *total* number of specimens failed after some specified number of cycles will be determined by a *cumulative distribution function*, such as Eq 5. But the *rate at which specimens fail at that time*, will be determined by the *frequency distribution function*, which is the derivative of the cumulative function.

Now a surface subjected to erosion can be regarded as composed of a multitude of surface elements analogous to fatigue specimens. Each time a piece or element is removed from the surface by the cumulative effect of the impact stresses, it is as though a fatigue specimen has failed. Clearly, therefore, the *cumulative* material loss is related to the *cumulative distribution function* in fatigue, and the *erosion rate* is related to the *frequency distribution function* in fatigue.

But there is another point to be considered: a fatigue frequency distribution function necessarily declines toward zero at very high lifetimes when there are no specimens left to fail. But erosion rates keep on going. The reason is, of course, that fatigue distributions apply to a *fixed* population of specimens, whereas in erosion we have a *regenerating* population: each time an erosion fragment is lost, a new surface element is exposed to erosion. It is as though each time a fatigue specimen fails, it is replaced instantaneously with a fresh specimen from an unending supply. The result of this is that eventually the variation in individual lifetimes washes out as far as the total failure rate or erosion rate is concerned, and the latter approaches a constant value (if attenuation effects due to increasing surface roughness are ignored). The mathematics and some results for this kind of analysis have been given in Ref 18 of the paper.

One of the several types of erosion rate-time curve obtainable from the above analogy is indeed similar to what a cumulative fatigue distribution curve necessarily looks like: namely, it begins at zero, and rises after a time to approach asymptotically a constant value.

I believe, therefore, that the apparent success of the author's theory is

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due entirely to the fortuitous cancelling out of two errors; namely, that of making an analogy between the cumulative distribution function and erosion rate, and that of neglecting the regenerating quality of the population. While it may be quite justifiable to use the Weibull distribution as a convenient and simple curve-fitting function which has the required overall properties, no real physical meaning should then be inputed to it, and no meaningful comparisons can be made between the parameters contained in it and the values for those parameters derived from fatigue tests.

A. Thiruvengadam (principal author)—The constructive and encouraging comments of the discussors are well taken. These criticisms have enhanced the value of this paper. The comment made by Mr. Fyall about the relatively higher frequency of impact of jets on the metals tested is well taken. It would be very interesting to study frequency effects, particularly on polymeric materials. The cumulative number of impacts and the statistical distribution of these impacts are very important when we try to extend these results to practical problems such as rain erosion or steam turbine erosion. I agree with Mr. Fyall that further investigations along these lines are necessary.

Dr. Brunton suggests that it would be better to define threshold in terms of first fracture instead of the appearance of plastic indentations. In fact, we did collect some additional results using fracture as a criterion of threshold. In this case, the threshold water-hammer pressure tends to agree more closely with the fatigue strength. Dr. Brunton's explanation for the discrepancy between fatigue strength and impact pressure is another mechanism worthy of consideration in future investigations.

Professor Ripken's remarks about the apparent lack of consistency between the results at 10,000 and 6700 rpm are useful. Within the experimental accuracy of our investigations, we feel that there is no lack of consistency. The figures are a bit confusing mainly because of the different scales used in both the figures for 10,000 and 6500 rpm. If one examines these results, keeping in mind that the rate of erosion is very highly sensitive to impact speed, he would agree that the peak rates and times are consistent. With regard to the second point raised by Professor Ripken, I believe that the liquid may be entrapped in the rough grooves caused by erosion in spite of the removal of some of the impacted liquid by centrifugal force.

Mr. Heymann questions the use of the cumulative fatigue failure distributions for the efficiency function $\eta(t)$. I have explained in the body of the paper the reasons for my usage of the cumulative Weibull type distributions. In view of the fact that a fractured particle was subjected to the impact-stresses over a number of cycles, from its initial exposure to its final failure, it is only proper to use the cumulative failure probability function. It would be a mistake to use the frequency distribution function as Mr. Heymann suggests. The correlations between the theory and experiments are by no means "fortuitous."

However I do appreciate Mr. Heymann's point of view that the erosion process is very complex and certainly needs continuing examination both theoretically and experimentally. In this sense, the comments and suggestions of all the discussors are well taken.