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DISCUSSION

A. Thiruvengadam[1] (*written discussion*)—I agree with Mr. Heymann's overall objective in general. I have been engaged in similar attempts for the past several years. Mr. Heymann very kindly has referred to these efforts in his paper. I have some comments on his "normalized erosion resistance" in which he compares the relative resistance of materials with a "standard material" which is a stainless steel. In the field of cavitation erosion, parallel approaches have been made over the years. For example, Beeching[2] during the period 1937 to 1942 used a material called admiralty propeller bronze as a "standard material" and tabulated his "figure of merit" for 25 alloys used in his cavitation erosion tests. If some one wants to relate Beeching's "figure of merit" with Heymann's "normalized erosion resistance" he will have to procure an admiralty bronze, sand cast as per the technology of 1937! While this is possible, it is tedious. Because of this reason, I suggested the inclusion of pure metals such as commercially pure annealed nickel for the ASTM round robin test.

While relative erosion resistance is good enough for a screening process, it is a bit more useful to incorporate some basic physics in terms of a commonly agreed upon energy parameter for the "standard material" so that we can calculate some energy levels in a laboratory test as well as in the field applications. For example, if we supply 100 W into the vibrational power of a test specimen, we can measure the power that goes into heat, noise, etc. in terms of some basic units. Similarly if we can measure the erosion in the same units, then we are likely to get an overall picture. An "erosion resistance" normalized in such a way to represent the energy absorbing capacity of the material seems more attractive to me from this point of view.

J. H. Brunton[3] (*written discussion*)—The author has made yet another very valuable contribution towards the ideal of being able to predict erosion behavior in a given environment. The concept of a "normalized erosion resistance" based on 18–8 stainless steel seems a particularly helpful procedure in simplifying the data. While it was not the intention in the present paper to consider the physical processes leading to the results

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[2] Beeching, R. "Resistance to Cavitation Erosion," *Transactions*, Institution of Engineers and Ship Builders of Scotland, Vol. 85, Paper No. 1026, March 1942, pp. 210–254.

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obtained, there is always a danger that the treatment presented here may be used elsewhere outside its proper context. It would be disturbing to see a theoretical edifice constructed from data fitting processes which ignored physical mechanisms. Many examples could be given where uncertainty in the mechanism undermines the predictive procedure. To mention but two; one must ask which impact pressure matters, the central water-hammer pressure, or the undoubtedly higher but more fleeting jetting pressure? These have different damaging potentials and a different dependence on C and V. Again, there is the uncomfortable result that outward liquid flow does not begin always along the line predicted by the simple theories. The area subjected to compressible deformation of the drop appears to be considerably bigger than that predicted by the water-hammer theory. An answer to these questions, at least, is needed before a reliable interpretive theory can be proposed.

D. E. Elliott[4] (*written discussion*)—The use by the author of the tangent from the origin to the erosion damage curve, as a method of relating the erosion resistance of a material to that of a standard material, is probably as good as any other single parameter. However, this method does not take any account of the different shapes of the erosion curve that various materials exhibit in any particular testing system. This could make a difference, by a factor of two, between the performance in service and the comparative erosion rate tangent assessment. As Mr. Heymann pointed out, a factor of two to one is not large in relation to the overall range of material resistance to erosion, but could mean a difference between reblading a machine once in its life or three times.

For a realistic quantitative assessment of erosion life there appears no alternative, at the moment, to carrying out a trial using a droplet size range similar to that expected in practice and determining the erosion/water impacting curve. As pointed out at the *Second Meersburg Conference on Rain Erosion* by Baket et al, even these erosion curves must be backed up by a metallurgical examination of the eroded surface. This is because the weight or volume loss is not always the best criteria, since geometry of the erosion pits varies. This was shown to be particularly important for rolled stellite where impact in the direction of rolling gave a high penetration type of erosion accompanied by comparatively low weight loss. On the other hand, impact perpendicular to the direction of working gave a higher weight loss but a lower penetration. Furthermore, in some instances, (high hardness titanium alloys) metallurgical examination showed that although they exhibited low volume losses, they became extensively cracked and were, therefore, unlikely to be of practical use in turbine design.

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TABLE 6—*Comparison between normalized erosion resistance and Beeching's "figure of merit."*

Material	Hardness Range, BHN	*Beeching's Results* F_m Range[a]	Equivalent N_e[b]	*This paper* N_e Range from Fig. 2
Brass	55 to 175	0.89 to 2.1	0.33 to 0.78	0.14 to 0.50
Aluminum-bronze	128 to 169	3.0 to 4.15	1.10 to 1.53	0.95 to 1.70
Monel and Ni-Cu alloys	139 to 253	1.4 to 3.25	0.52 to 1.20	0.15 to 1.40
Gun metal	62 to 84	0.76 to 1.0	0.28 to 0.37	0.28 to 0.32
Cast iron	179 to 244	0.40 to 1.2	0.15 to 0.44	0.14 to 0.26

[a] F_m = "figure of merit," in seawater.
[b] Conversion is based on $N_e = 0.37\ F_m$.

F. J. Heymann, (author's closure)—I appreciate Dr. Thiruvengadam's comments and concur with some of them. For instance, I should like to see the normalized erosion resistance scale pegged by more than one standard material, and some pure metal such as annealed nickel would be a suitable bench mark in the lower half of the scale. In addition, some suitable material then should be picked in the upper half.

As I stated in the paper, I chose the austenitic stainless steel as a primary standard because it has been tested widely and falls in the middle of the range. Moreover, it is a common material which can be expected to remain well known and widely used in the foreseeable future. Beeching's[2] "admiralty propeller bronze," on the other hand, was a highly specialized and relatively obscure material. Contrary to Dr. Thiruvengadam's statement, however, Beeching's "figure of merit" (F_m) can be related quite well to the "normalized erosion resistance" (N_e), as shown in Table 6. The conversion I chose is based on the values for aluminum bronze in the hardness range DPH 150 to 180: the average N_e from Fig. 2 is about 1.5, and the average F_m from Beeching's[2] data is 4.07. Hence, $N_e = 0.37\ F_m$.

I must, indeed, acknowledge that the idea of a normalized erosion resistance scale, based on "standard materials," was stimulated by Thiruvengadam's "concept of erosion strength" (Ref *6* of the paper). That really remains as its essential idea, when the arguments therein are divested of the—to my view—circular and possibly misleading tie-in with strain energy concepts.

The hypothesis that erosion is governed by energy transfer, or energy absorption, is an attractive one and eventually may be shown to be correct; but so far it has not been proven as yet, and the energy balance involved undoubtedly is very complicated. It surely now has been convincingly established, in this paper and elsewhere, that the erosion resistance of materials is *not* linearly dependent on their strain energy (see, for instance, Refs *2*, *3*, and *5* of the paper). I have shown elsewhere[5] that even Thiruvengadam's own data (Fig. 2 of Ref *6* of the paper) suggests a dependence on about the 1.8th power of strain energy. Nor is there, to my knowledge, any experimental evidence which shows that the erosion rate of a given material is proportional to the power absorbed by it, when the intensity of attack is varied. Experiments capable of testing this hypothesis certainly would be desirable.

The extreme difficulty in measuring the power absorbed by the eroding material can be demonstrated by a numerical example taken from Dr. Thiruvengadam's own work.[6] Using additional information kindly supplied to me by Thiruvengadam et al, I have calculated the kinetic energy of the impacting water required to produce unit volume of material loss, in the nickel specimen at 350 ft/s. This quantity, which may be called "specific energy of erosion,"[7] is about 10^9 psi, compared to the strain energy of nickel which Thiruvengadam et al give as 20,000 psi. Therefore, if the latter really represented the "erosion strength" in absolute units, then the "efficiency of energy absorption" in this example would be of the order of 2×10^{-5}. Can the energy which goes elsewhere really be measured accurately enough to determine the absorbed energy by a process of elimination, as Dr. Thiruvengadam suggests?

The whole point of the "normalized erosion resistance" approach, as presented in my paper, is to be able to quantify, generalize, and compare results *without* having to adopt any hypotheses concerning erosion mechanisms. This, I hope, should help in eventually determining what the physical damage criteria really are.

A similar comment, I think, applies to Dr. Brunton's misgivings. The uncertainties concerning some of the physical processes, which he lists, undoubtedly are present, and the answers to these questions are needed before a physical or *causal* theory for quantitative prediction of erosion damage can be proposed. But these answers are not required for a *descriptive* or phenomenological model; on the contrary, it seems to me that a well-established descriptive model should help in arriving at these answers. Essentially, all that I have tried to do here is to organize empirical data in such a way as to reveal more clearly what can be generalized from them and, therefore, must be true regardless of the underlying physical reasons. I hope to have shown here that the fifth power dependence of the rationalized erosion rate on impact velocity, which various investigators in

[5] Heymann, F. J., "Erosion by Cavitation, Liquid Impingement, and Solid Impingement: A Review," Engineering Report E-1460, Westinghouse Electric Corp., Lester, Pa., 15 March 1968.

[6] See p. 249.

[7] General expressions for "specific energy of erosion" may be deduced from Eqs 9 and 10 of the paper. This yields approximately $8 \times 10^{19} N_e V_o^{-3}$ N/m² for drop impacts, and $7 \times 10^{17} N_e V_o^{-2.4}$ N/m² for jet impacts, with V_o the normal impact velocity in m/s.

the past have observed for their individual results, is indeed a general empirical law which is valid over a large range of parameters. I have taken pains *not* to construct any "theoretical edifices," and have claimed only that any theoretical edifice seriously offered must explain, and must not conflict with, the generalized findings shown here.

I must concede that not all of the generalizations incorporated into my descriptive model are equally well founded; and in particular those concerning the erosion rate-time relationship are very rough and were included only because the time- (or damage-) effect must be accounted for in some manner. In that sense, Professor Elliott's remarks are well taken. I wonder, however, whether the alternative he suggests always will result in a much more accurate prediction. Whenever an accelerated test is substituted for the prototype field environment, some test parameters necessarily must be changed. Even if the spectrum of drop sizes, velocities, and angles were reproduced accurately, the rate of liquid impingement must be increased by orders of magnitude in order to achieve a comparable shortening of test times. This means that the depth of the liquid "surface layer" will be greater, and this undoubtedly has an effect on both the maximum erosion rate and the erosion rate–time history.

With Professor Elliott's second point I have no quarrel at all.

In conclusion, I thank all of the discussers for their interest, and hope that this paper and discussion will at least stimulate other authors to present their results in rational forms which can help to substantiate, disprove, or modify the generalized prediction method suggested here.