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AUTHORS' CLOSURE ON DISCUSSIONS ON "A STATISTICALLY VERIFIED MODEL FOR CORRELATING VOLUME LOSS DUE TO CAVITATION OR LIQUID IMPINGEMENT"

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The authors first of all would like to thank the various discussors for their very significant contributions to the subject matter of this paper. Much new data and many pertinent points have been added in these discussions for which the authors are grateful. Where additional elaboration on our part seems desirable, this is made in the following paragraphs, which consider the various discussions in alphabetical order.

Both Prof. Elliot and Mr. Heymann, with respect to the first portion of the paper which involves very high velocity rocket sled "rain erosion" tests on rain erosion type materials which are generally not highly resistant to erosion (as compared with metals), make the point that for such materials at such velocities, fatigue is not an important erosion mechanism. Hence, the lack of success of the threshold velocity concept, proposed first by Pearson of C. E. G. B. for turbine blade erosion applications where fatigue failure is predominant, is not surprising. This point is further corroborated in the discussion of Messrs. Rao and Rao. We fully agree. We also agree with Mr. Heymann's comment in this regard that the threshold velocity must be a function of many variables other than material mechanical properties such as test duration, droplet size, surface roughness, and as Prof. Elliot points out, the extent of continued surface wetting, especially for very small drops.

Dr. Engel points out the probable necessity of dividing materials to be considered into various groupings if a good correlation with material mechanical properties is to be achieved. We agree that this is probably required if close correlations are to be achieved, since "there are as many mechanisms of multiple-drop-impact erosion as there are broad groups of material properties". We have not been able as yet to pursue her suggestion that this might usefully be accomplished on the basis of brittleness and work-hardening capacity, but agree that this might be a useful approach.

It is indeed encouraging to note the similarities in correlation of damage rates between our data set and that of Mr. Heymann with respect to mechanical property groupings. As he mentions, there is a maximum spread of a factor of about 4 around our best fit line (his Fig. 1) as applied to his data set (or to our own), giving an overall range of the data at a given ultimate resilience, e.g., of a factor of about 15. However, our "factorial standard error of estimate" is about 2.5 for this case (Table 1 of Closure) indicating that approximately 2/3 of the data points will lie within this factor from the best fit line. This is of course still inconveniently large for predicting damage for engineering design purposes, though it should be useful in determining whether a given design is clearly in a feasible regime or clearly not so. Meaningful predictions for marginal cases are of course still not possible. However, this relatively large factor of uncertainty may not be surprising when it is realized that the damage rates of a resistant alloy such as Stellite 6-B and a non-resistant one such as soft aluminum differ by a factor of about 10,000, and that the data set includes points from several different types of cavitation and impingement facilities, all considered together.

Partially as a result of Mr. Heymann's suggestion, we have tried a correlation of maximum damage rates with the mechanical property in question raised to an exponent, which is then adjusted to a best fit value (Table 1 of closure). Our best fit exponent for the term $(UR x E^2)$ is then 0.659 which agrees very closely with the value of 2/3 mentioned by Mr. Heymann in his discussion. We also found that the best exponent for UR is 0.998, confirming the validity of the energy model approach when this term is used, i.e., a unity exponent is required for this model. As shown in Table 1 of closure the correlation coefficient for our data with Mr. Heymann's suggested term $(UR x E^2)$ improves from 0.684 when this term is raised to unity exponent to 0.744 when the term is taken to best fit exponent. However, each value is less than the correlation coefficient for our data with UR alone (raised to unity power), which is 0.811. On the other hand, the factorial standard error of estimate improves from 2.86 when the term is taken to unity power to 2.35 when taken to best fit exponent. This compares with 2.52 for UR alone. Hence the combined term provides a better fit in terms of standard error of estimate when raised to its best fit power than does UR alone, although its standard error is inferior when both are raised to the first power.

Table 1 (closure) also indicates that Brinell hardness (BHN) provides a relatively good correlation when raised to unity power, and a better correlation when raised to its best fit power (0.734). In this latter case the correlation coefficient is substantially inferior to that of UR and slightly inferior to that of $(UR x E^2)^n$. This new information confirms the long-standing practice of using Brinell hardness as a correlating parameter. It is to be recommended still in the light of these results because of its simplicity and ease of measurement, as well as the fact that its performance as a correlating term is only slightly inferior to results to be obtained with much more complex parameters which are also much more difficult to measure. A general conclusion from Table 1 is that in terms of a basic model the use of UR is justified by the fact that the best fit exponent is approximately unity as required by the energy model, and the best correlation coefficient, indicating that the best "explanation" of the data is obtained with this parameter. However, the data also indicates that the use of the strain energy (SE) rather than ultimate resilience in such an energy model, as suggested most recently in Dr. Eisenberg's paper in this symposium, is quite unjustified. The best fit exponent for this parameter (Table 1 of closure) is 0.738 rather than unity as should be the case if its use in the energy model were valid, and the resulting correlation coefficient is only 0.517 (vs. 0.811 for UR). In addition the standard error with this parameter is substantially larger than that with <u>any</u> of the other parameters tried. Also, for the 0.517 "sample correlation coefficient" with 33 points for SE, the "minimum population correlation coefficient"[1] is only about 0.2 (vs. 0.64 for UR). Thus the statistical evidence for a good correlation with SE, even when raised to its best exponent, is weak. The minimum (and maximum) population correlation coefficients are shown in Table 1.

The smallest factorial standard error (2.25) for our data is provided with the term UR x BHN raised to its best fit exponent (0.720). This term was suggested by Rao et al[2] as a result of their work with a venturi. Their data points are also incorporated into our own data set used for this paper. However, for this combined term the correlation coefficient is again slightly less than for UR alone.

Plots of our data against the various mechanical property groups discussed are not included here with the exception of the plot against UR which is Fig. 3 of the paper, since they have been published elsewhere[3].

Messrs. Rao and Rao, in addition to providing some of the data points for the paper itself, have suggested empirical relations for a better fit of the rocket sled droplet impact data (discussed in the early part of our paper) as a function of velocity and angle of impact. They suggest dividing the overall velocity range for a typical material (Pyroceram) into a low velocity region where the damage rate is substantially independent of velocity and a higher velocity region where it is not. If this is done, and best fit values for K, V_o, and α are chosen, the match between eq. (3) in the paper and the actual data points is much improved over that obtained if eq. (3) is used for the entire region. We believe that this is a possible useful approach which should be applied to the remaining data if a better predicting relation for these rain erosion type materials is desired. However, it cannot be applied to a new untested material unless an understanding of the relation between the measurable material mechanical properties and the limiting velocity to divide the regions, can be found. The present state of the art unfortunately does not as yet allow such a prediction.

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3. F. G. Hammitt, Y. C. Huang, T. M. Mitchell, Discussion of "Cavitation Erosion Studies with Venturi and Rotating Disc in Water", B. C. S. Rao, N. S. L. Rao, and K. Seetharamiah, ASME Paper 69-WA/FE-32, to be published <u>Trans. ASME, J. Basic Engr.</u>, 1970.

Correlating Relation	n (Where Applicable)	Sample Correlation Coefficient	95% Confidence Limits for Population Correlation Coefficients	Factorial Standard Error of Estimate
$\frac{1}{MDPR} = C(UR)^n$	0.998	0.811	0.64 - 0.91	2.52
$\frac{1}{MDPR} = C(UR)$	--	0.811	0.64 - 0.91	2.52
$\frac{1}{MDPR} = C(UR \times BHN)^n$	0.720	0.798	0.62 - 0.89	2.25
$\frac{1}{MDPR} = C(UR \times E^2)^n$	0.659	0.744	0.52 - 0.86	2.35
$\frac{1}{MDPR} = C(BHN)$	--	0.742	0.52 - 0.86	2.75
$\frac{1}{MDPR} = C(BHN)^n$	1.788	0.734	0.52 - 0.85	2.38
$\frac{1}{MDPR} = C(UR \times BHN)$	--	0.716	0.49 - 0.84	2.57
$\frac{1}{MDPR} = C(UR \times E^2)$	--	0.684	0.44 - 0.82	2.86
$\frac{1}{MDPR} = C(SE)^n$	0.738	0.517	0.21 - 0.73	3.24
$\frac{1}{MDPR} = C(SE)$	--	0.498	0.17 - 0.72	3.30

A Statistically Verified Model for Correlating Volume Loss Due to Cavitation or Liquid Impingement

by F. G. Hammitt, et al

Discussion by F. J. Heymann
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This paper gives me great satisfaction, because both its objectives and its findings are largely similar to those of my own contribution to this Symposium (1). Let me, therefore, underscore some of the agreements and point out some of the differences.

The authors' statistical analysis of the Rocket Sled data gives further quantitative support to a conclusion which I had tentatively reached in a previous paper (2), and is now more thoroughly confirmed by the assemblage of data displayed in Fig. 8 of Ref. 1: namely, the velocity dependence of erosion can often be adequately expressed by a simple power law, without introducing a "threshold velocity". But there is an important proviso: these findings apply to conditions under which erosion proceeds rather rapidly, and may not be true at very low velocities or with very small drop sizes.

Actually, two distinct approaches have been used at times to determine threshold velocities; the indirect method, by fitting an assume d velocity law to erosion rate data obtained at high velocities; and the direct method, involving low-speed tests to find the highest velocity at which no erosion sets in within a reasonable time. In my opinion there is no good reason for assuming that these two methods should yield the same results. Firstly, erosion mechanisms at high impact velocities are not identical to those at very low velocities, and may not be desscribed by quite the same simplified law. Secondly, the damage potential of impacts is affected by the surface roughness, and once erosion is started, it may be kept going by impact velocities which could not initiate it on a smooth surface. (I am indebted to W. D. Pouchot for this observation). Thirdly, the "incubation period" has been shown to increase with a high power of the reciprocal impact velocity, making it difficult to run a test long enough to establish conclusively that erosion will not eventually begin at low velocities. Finally, if there is a physical threshold velocity, it may well be drop-size dependent (2).

All of this tells us that it may be much more easy for us to predict the amount of erosion to be expected under severe conditions than under marginal conditions; unfortunately, in many practical instances -- particularly in long-life equipment -- the impingement conditions must be in the marginal zone, since even a very low erosion rate could lead to unacceptable erosion damage over a span of 10-20 years.

Let us now turn to the second part of the authors' paper, the correlation between erosion rates and target material properties. Their major finding is that a proportionality between ultimate resilience (UR) and reciprocal erosion rate ($MDPR^{-1}$) provides the best correlation which is dimensionally consistent with the assumed energy transfer hypothesis (their Eq. 1). In other words, erosion resistance ($\mathcal{E}$) is found proportional to UR. This is very similar to my qualitative findings in Ref. 1: the Normalized Erosion Resistance (N_e), when plotted against UR on log-log coordinates, showed approximately a first power relationship (Fig. 6 of Ref. 1). I pointed out that this may be significant because it results in an erosion resistance which is dimensionally the same as other strength or energy properties.

The quantitative agreement between the authors' and my findings is actually quite remarkable, as can be seen on Fig. 1. This is the same as Fig. 6 of Ref. 1, except that superposed on it are the "best fit line" and "factorial standard error of estimate" boundaries taken from the author's Fig. 3. (In order to locate these lines uniquely, a conversion between my N_e and the authors' standardized MDPR was required. The value $N_e = 1.0$ is defined as the erosion resistance of an austenitic stainless steel of hardness BHN 170. Such a material is found in the authors' Table IV (6th from bottom) and had an MDPR of 0.653. Hence $N_e = 0.653/\text{MDPR}$ is the desired conversion.)

The most important thing to note, in Figure 1, is that the authors' data points and my data points show about the same scatter band; in both cases its vertical "height" encompasses a factor of about 15. Furthermore, in both cases some highly erosion-resistant materials, like stellites, have been left out, and would have increased the scatter if they had been included. By no stretch of the imagination, therefore, can this correlation be considered to give a useful tool for quantitative engineering predictions of erosion behavior.

It is true that I found a somewhat (but not much) improved correlation with S_u^2E (or UR x E^2), whereas the authors obtained a worse correlation with that parameter. The reason may be that the author's correlation model permitted only a linear dependence on UR x E^2 (see their Table VII), whereas Fig. 7 of Ref. 1 suggests a dependence of N_e on the 2/3 power of S_u^2E. It would be interesting to see what would result if the authors tried out the equation $\mathcal{E} = a\ (\text{UR} \times E^2)^b$, or $\log \mathcal{E} = a + b \log (\text{UR} \times E^2)$, compared to $\log \mathcal{E} = a + b \log (\text{UR})$.

Admittedly, the correlation with S_u^2E is dimensionally inconsistent with the authors' Equation 1, as they point out. But this is not inevitably an impediment. While the energy transfer hypothesis of Eq. 1 is an attractive one, it is not the only one possible. In Ref. 1 I discussed this and suggested that new experiments must be carried out in order to discover the proper physical foundation for an erosion rate relationship, from which the dimensions of erosion resistance can then be deduced. Until that has been accomplished, we should not put any avoidable constraints on our correlation attempts. In fact, the authors' failure to improve their correlation by including the acoustic impedance ratio is an argument <u>against</u> the energy transfer theory, since the energy transmitted in an impact should be approximately proportional to the acoustic impedance ratio, if it is small. On the other hand, the impact <u>stress</u> is little affected by variations in the acoustic impedance ratio, again provided it is small as is true for the data considered. Thus, the authors' results provide no positive verification of their assumed "generalized erosion model."

In summation, the authors' findings would lead me to precisely the same conclusions which I reached in Ref. 1: namely, that <u>no</u> correlations with conventional material properties have led to a useful prediction ability, and that at this stage of the game we ought to regard erosion resistance as an independent property, to be measured in erosion tests, and to be expressed quantitatively relative to one or more "standard materials" which should be incorporated in all test programs. This gives us the best opportunity of gaining more knowledge and insight without being fettered by preconceived ideas and constraints.

Even this approach cannot be expected to result in a unique repeatable erosion resistance value for each material, since in all probability more than one mechanism and more than one material parameter is involved in the physical erosion process. But the approach that I suggest promises to offer at least a first approximation of some practical usefulness.

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Title: A Statistically Verified Model for Correlating Volume Loss Due to Cavitation or Liquid Impingement

Authors: F.G.Hammitt, Y.C.Huang, C.L.Kling, T.M.Mitchell, L.P.Solomon

Discusser: Olive G. Engel [1]

The correlation found with ultimate resilience recalls the classification of materials into two groups on the basis of erosion resistance given by Von Schwarz and Mantel [2]. In the first group are materials for which the work of elastic deformation is lower than the energy delivered by a single drop [3] impact. If, in addition to being in this category, the material is brittle, the spots struck by the impinging drops are shattered. Most metals are plastically deformable and the surface metal, at the spots where the drop impacts occur, is deformed and work-hardened until the limit of ability to deform is reached; when this limit is reached, the surface is broken.

Von Schwarz and Mantel found that the following properties gave the greatest drop-impact-erosion resistance to metals in the first group: hardness, ability to deform while cold, and extensive cold working properties. Von Schwarz and Mantel concluded that the high capacity for cold working of certain alloys gives them good drop-impact-erosion resistance in spite of an inferior Brinell hardness and suggested that this explains why Brinell hardness is not a consistently good criterion of drop-impact-erosion resistance.

In the second group Von Schwarz and Mantel placed all materials for which the elastic work of deformation is so large that the energy delivered by a single drop impact is not sufficient to deform them. For these materials, damage sets in first at imperfections. For materials in the second group, Von Schwarz and Mantel concluded that drop-impact-erosion resistance is determined by hardness and fatigue strength.

The role that is played by the properties of a solid under erosive attack leads to the generalization that there are as many mechanisms of multiple-drop-impact erosion as there are broad groups of material properties [4]. The fact that the mechanism by which erosion occurs affects the rate of erosion suggests that better correlations with erosion rate may result if tested materials are grouped on the basis of their properties. If highly resistant alloys, tool steel, and Stellite 6B are excluded from the analysis, then, on the basis of the classification of Von Schwarz and Mantel, it might be informative to make an analysis of the remaining metals after they have been divided into groups on the basis of (1) brittleness, and (2) work-hardening capacity.

The fact that the resistance of Stellite 6B is much greater than is expected on the basis of its mechanical properties strongly suggests that an understanding of the microscopic processes involved in drop-impact and cavitation erosion is required in order to be able to predict the resistance of materials to this form of attack and to be able to formulate new alloys that will have a built-in resistance.

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1 Nuclear Systems Programs, General Electric Company, Evendale, Ohio

2 M. Von Schwarz and W. Mantel, Z. des Ver. Deut. Ing. 80, 863 (1936).

3 Von Schwarz and Mantel used a rotor and jet apparatus so that the drop is really a short section of a jet that is struck from the side.

4 I am indebted to Dr. Albrecht Herzog for this insight given during a conversation at the Wright Air Development Center, Wright-Patterson Air Force Base, Ohio, in 1953.

2. A Statistically verified Model for Correlating Volume Loss Due to Cavitation or Liquid Impingement. - by F. G. Hammitt et al.

The use of the concept of a threshold velocity V_o was introduced by Pearson (C.E.G.B.) because of the similarity between erosion damage and fatigue where a relationship of the type shown in Equation 1 of the paper had proved very successful. Pearson correlated his data for low speed erosion experiments and found good agreement, thus giving support to the idea that, in this region, the process is similar to fatigue. However, much of the data that Professor Hammitt has used is derived from high speed impact where the stress levels can be far above the fatigue strength of the materials. Thus we could not expect that the data would conform to the correlation proposed for low velocity impact.

We may, therefore, have to think of erosion as a process which changes its character as the impact velocity increases. The initial process, when the velocity is near the threshold, being one of fatigue, changing to one where energy considerations are dominant when the velocity becomes high compared with the threshold value.

Furthermore when the size of the impacting droplet becomes small (of the order of 100 microns) the impact stress levels can be significantly changed by the existence of water films on the surface as described in the paper by Mr. Pouchot. It is therefore likely that the threshold velocity term will have to include a factor to account for the attenuation of the stress level due to water films.

A STATISTICALLY VERIFIED MODEL FOR CORRELATING VOLUME LOSS DUE TO CAVITATION OR LIQUID IMPINGEMENT, BY

F. G. Hammitt, Y. C. Huang, C. L. Kling, T. M. Mitchell and L. P. Solomon

Discussion by:

B. C. Syamala Rao[1] and N. S. Lakshmana Rao[1] - The authors made a simple and elegant approach to understand the effect of velocity on rain erosion and to determine the material parameter ξ and the energy transfer coefficient, η, by considering the erosion data from several devices. The predicted MDPR values from the best fit for Equation (3) in Table I, show a very wide deviation from the actual MDPR. In order to understand this further, we studied a plot of log (MDPR) + log (Sin θ) as a function of log (V Sin 0) shown in Figure 4. The trend of experimental points clearly indicate two different regions: (i) where the velocity has a negligible effect on the erosion, and (ii) where the velocity has a very significant influence on the erosion. The first region can be related empirically as

$$\text{MDPR} = 4.5/\text{Sin}\,\theta \tag{13}$$

while the second region can be described by Equation (3).

The value of V_o is chosen as the value of V Sin θ at which a mean line drawn through the data intersects the abscissa. The experimental results in the second region are then plotted with log (V Sin θ - V_o) as the abscissa. A curve which gives the least standard deviation on an arithmetic plot is fitted as a

[1]Department of Civil & Hydraulic Engineering, Indian Institute of Science, Bangalore, India.

straight line on the log-log plot shown in Figure 4 and the values of K and α are computed to be 0.178 and 1.50 respectively. With these values for K, V_o and α, Equation (3) reduces to

$$MDPR = 17.8 \times 10^{-2} (V \sin\theta - 1780)^{1.50}/\sin\theta \qquad (14)$$

Table IX shows that the values of MDPR predicted from Equation(14) are much closer to the actual values compared with the values from the equation used by the authors. The standard deviations using Equation (3-A) in these cases are as follows:

	Standard Deviation
With Equation (14)	451 μ/s
With Equation (3)	1192 μ/s

The existence of the two regions where the effects of velocity are very different are also observed in our investigations on cavitation damage (2, 3).

[2] B.C.Syamala Rao, "Cavitation Erosion Studies with Venturi and Rotating Disc in Water", Ph.D. Thesis, Indian Institute of Science, Bangalore, April 1969.

[3] P.K.Kandasami, "Studies on the effect of velocity and Test Duration on Cavitation Damage" M.E. Thesis, Indian Institute of Science, Bangalore, India, August 1969.

TABLE - IX

Comparison of Actual and Predicted MDPR's for Material A-1, Pyroceram, using

$MDPR = 5.34 \times 10^{-5} (V \sin \theta)^{6.27} / \sin \theta$ (3) Authors

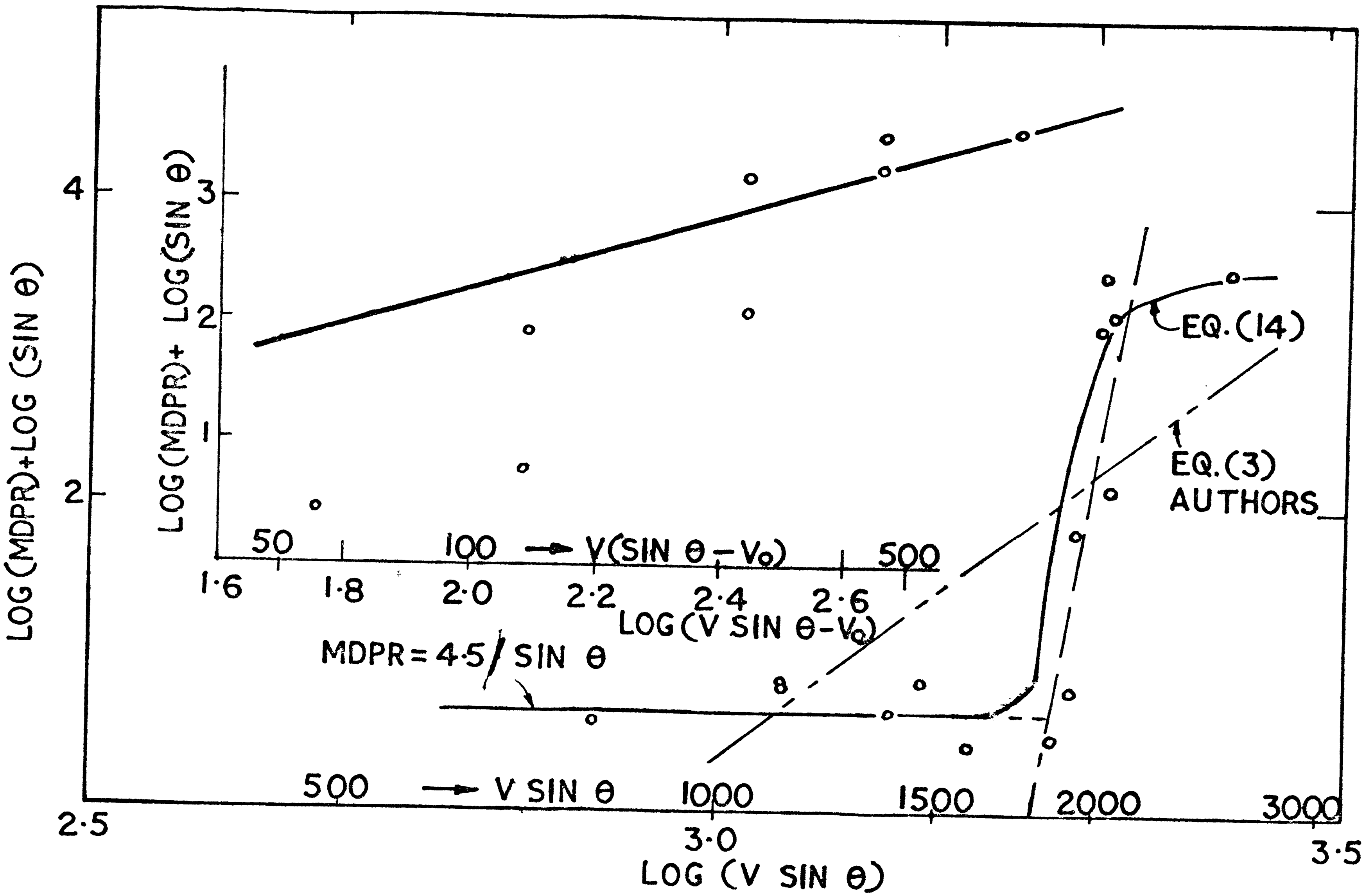
Standard deviation of Equation (3) = 1192 μ/s

$MDPR = 4.5/\sin \theta$ For $V \sin \theta < 1650$ fps (13)

$MDPR = 0.178\ (V \sin \theta - 1780)^{1.50} / \sin \theta$ for $V \sin \theta > 1780$ fps (14)

Standard devitation of Equations (13) and (14) = 451 μ/s

V fps	θ°	MDPR μ/s Predicted by Equation 3 of Authors	Actual	Predicted by Equations (13) and (14) of the discussers
1580	30	.9	7.9	9.0
1580	30	.9	0	9.0
1580	45	5.5	10.5	6.4
1580	45	5.5	10.5	6.4
1580	60	16.1	0	5.2
1580	60	16.1	5.3	5.2
2197	30	6.8	0	9.0
2197	30	6.8	0	9.0
2197	45	43.7	0	6.4
2197	45	43.7	3.6	6.4
2197	60	127.3	7.3	300
2197	60	127.3	80.6	300
2594	30	9.6	0	9.0
2594	30	9.6	0	9.0
2594	45	124.1	0	21.9
2594	45	124.1	4.3	21.9
2594	60	361.4	3,849	2,262
2594	60	361.4	2,240	2,262
2905	30	40.6	0	9.0
2905	30	40.6	14.5	9.0
2905	45	252.4	2,189	1,218
2905	45	252.4	179	1,218
2905	60	735.3	4,465	4,425


LOG (MDPR)+LOG (SIN θ)
4
2
2·5
3·0
3·5
LOG (V SIN θ)
500
→ V SIN θ
1000
1500
2000
3000
LOG (MDPR)+ LOG (SIN θ)
3
2
1
1·6
1·8
2·0
2·2
2·4
2·6
LOG (V SIN θ−Vo)
50
100
→ V(SIN θ − Vo)
500
MDPR = 4·5/ SIN θ
EQ.(14)
EQ.(3)
AUTHORS

FIG·4

Navy 9
2

90°

90	0.9M	0	29.67400	0	0
		500	29.66086	13.14	13.14
		750	29.63865	22.21	35.35
		1000	29.63800	0.65	36.00
		1250	29.63672	1.28	37.28
		1750	29.63570	1.02	38.30
		1750	29.61356		
		2250	29.60941	4.15	42.45
		2250	29.39221		
		3000	29375.96	16.25	58.70