pends primarily on the frequency spacing, and a good rule-of-thumb for the number of iterations n_p required to converge to the p-th mode with frequency ω_p is

$$n_p \sim N/4 \log_{10}(\omega_{p+1}/\omega_p)$$

where N is the number of significant digits desired in the frequency. For example, in the third example of Ref. 1, giving vibration modes of a quite general composite shell, the number of iterations required to obtain frequencies to four digit accuracy was $n_1 = 4$, $n_2 = 11$ for the axisymmetric modes and $n_1 = 5$, $n_2 = 4$ for the first harmonic modes. Furthermore, my experience with a variety of other shells is that the number of iterations required for convergence is relatively insensitive to the initial guess.

2) It was stated in Ref. 1 that ten modes can be obtained and no tape storage is used. The number ten was chosen arbitrarily for the purpose of dimensioning certain quantities in the program. In fact, there are 3600 unused locations in the 32 K core, which if employed, at a rate of 400 locations per mode could store nine additional modes, if desired. However, as explained below, it is doubtful that storage of more than nine modes currently retained will ever be necessary. It was also stated in Ref. 1 that no significant deterioration of accuracy has been observed for the higher modes. The reason for this is simply that, in contrast to the usual laborious hand computation for higher modes of beams using Stodola's method, the orthogonalization with respect to lower modes is made after each iteration; furthermore, this calculation is based on an "inner product" subroutine which is generally accurate to five or six significant digits, considerably more significance than required in modes themselves.

In addition, the method can be modified slightly so that the calculation for a given mode can be made not to depend on previous modes, thereby eliminating any practical limit on the number of modes obtainable. In the mode method an approximation for the next higher eigenvalue (i.e. ω^2) is readily available by virtue of a property of the Rayleigh quotient; namely, the ratio of successive differences of the sequence of square frequency estimates given by Eq. (27) of Ref. 1 converges to the fourth power of the ratio of the next higher frequency to the one being obtained. By the amount of this approximation, the eigenvalue spectrum may be shifted to make the next desired eigenvalue the minimum (in absolute value) of the modified system of equations. As a result, convergence to the corresponding mode no longer depends on the orthogonalization with respect to previously obtained modes. It is also noted that the rate of convergence after such a shift is greatly improved, since the frequency ratio is enlarged by virtue of the desired frequency being now close to zero†. The trade-off in applying such a procedure is that after the eigenvalue shift the differential operator is altered, thereby requiring one recomputation of the complementary solutions (as is necessary in each step of the frequency method). Since roughly five to eight iterations can be made in the time required to compute the complementary solutions, the desirability of applying this procedure for each mode is not clear-cut.

In practice, however, as a result of applying the shifting procedure for very high modes, orthogonalization with respect to only the previous nine modes is probably sufficient to obtain an arbitrary number of modes. It is only necessary that after the shift the next two desired modes have eigenvalues smaller in absolute value than the shifted eigenvalues of previous modes with respect to which no orthogonalization is made. It should be clear from the foregoing that the mode method is not limited in the number of modes obtainable.

3) Since an application of an eigenvalue shift can make any desired mode have the minimum eigenvalue of the modified system of equations, it is clearly unnecessary to obtain all the lower modes of the unmodified system prior to obtaining the desired mode. For example, if a given frequency interval is to be examined, one can shift the middle of this interval to the zero of the frequency axis.

Finally, in reference to Kalnins' last comment, it should be mentioned that the purpose of the first example of Ref. 1 was simply to illustrate the application of the mode method to a shell which had been previously studied in detail by use of Legendre functions[3] and by the frequency method.[2] Since in Ref. 3 seven frequencies are presented for this shell and in Ref. 2 three frequencies are presented, none of which contain the third frequency presented in Ref. 1, the natural conclusion was that the third mode had apparently been overlooked.

† This technique was brought to my attention by G. A. Greenbaum in a private communication.

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Comments on "Effect of Microparticle Impact on the Optical Properties of Metals"

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IN the introduction as well as in other sections of Ref. 1, the authors have stated that the motivation for their efforts was the lack of information regarding "the effect of micrometeoroid bombardment on the surface finish of a space vehicle." Hence they felt the need "to simulate the effect of micrometeoroid impacts on candidate skin materials and to determine the resulting change in optical properties." Although Ref. 1 is an adequate description of a multifaceted study, it is unfortunate indeed that the authors were unaware, apparently, of the work of Refs. 2–5. For, if they had been aware of it, they could have presented the results they obtained in a more meaningful fashion, and they would have immediately understood that the projectiles they used in their experiments (100-μ diam W particle) are inappropriate for the purpose intended. Such particles can hardly be used to determine damage to the optical properties of satellite skin materials by micrometeoroids in space since by the highest flux estimates of Refs. 5 and 6, it would require $\sim 10^4$ yr for just one such particle to strike an area the size of the disc used in their experiments (~ 1 cm^2). It is possible to use particles somewhat different from those causing the surface damage in space ($< 10^{-10}$g); however, extrapolating the effects of laboratory exposures to the actual degradation of surface reflectance caused by microparticles in space at least involves the scaling laws presented and discussed in Ref. 4, and is not a matter of simply presenting measured reflectances.

Although very carefully presented by the authors as observed results only, the reflectance data are used to point out that high velocity impacts caused more degradation than did low velocity impacts. This conclusion is really not too helpful, for the simplest possible analysis would show that

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greater exposure (more hits at higher velocities) would be expected to cause greater degradation. A velocity effect that is of interest is that at constant total kinetic energy of exposure a higher particle velocity actually causes less surface damage.[4]

Although their argument for using a value of zero for crater reflectance R_c seems satisfactory, considering the depth-to-diameter ratios they encountered for their craters, it is certainly not a general result. We have made measurements at NASA Lewis Research Center of R_c for metal targets impacted by a number of projectile materials 1 to 15 μ in diameter and find R_c to vary from 0.3 to 0.5, the amount of variation depending on the target as well as on the projectile.

With regard to presenting surface degradation normalized to damaged area or number of craters, it should be pointed out that a simple expression can be written[4] for the ratio of these two quantities. It is:

$$\frac{(\Delta R/R)/A}{(\Delta R/R)/N} = \left\{\frac{2\,E_{cr}}{3\pi^{1/2}(m_p v_p{}^2)/2}\right\}^{2/3}$$

where E_{cr} is cratering energy density of the target material and $m_p v_p{}^2/2$ is the kinetic energy of a single projectile. Hence the ratio of the two quantities would be expected to vary with target material for a given projectile and impact speed. Comparing these quantities as presented in Tables 2 and 3 of Ref. 1 causes some doubt regarding their measurements for aluminum and gold (for which this ratio rises instead of falling as it should, and as do the other such values with projectile speed).

Perhaps it is most important to point out that conclusions regarding the effect of exposure on α/ϵ of SS 304 should not be made from the data obtained in Ref. 1. It should be clear from the data in their Fig. 9 that, of all the materials exposed, only their SS 304 showed an α/ϵ before exposure less than 3. Obviously, the exposure could not be expected to reduce α/ϵ much further. In Ref. 4 the effect of exposure on SS 304 was as expected. The results there showed the α/ϵ of SS 304 to fall with exposure from 3.86 to 2.0. However, the equilibrium temperature of such a disk remained constant with this change in α/ϵ when the disk was placed in a simulated space environment. This was shown to be a thermal interaction between the disk and the simulated satellite on which it was mounted and from which it can not be perfectly isolated thermally.[7]

In conclusion, we must point out that although a great deal of work has been done and reported in Ref. 1, the results of this work cannot be used to determine the effect of micrometeoroid impact on satellite skin materials in space. This is especially true because the authors had no quantitative idea of the type, size, and mass of the projectiles causing the damage they recorded. For a single nominal projectile size, the range of size of craters formed in the target was so great (most are smaller than the original projectile) as to indicate that the greater part of the damage to optical properties was caused by incidental particles, making any kind of quantitative analysis impossible.

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Reply by Authors to H. Mark and M. J. Mirtich

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The objective of the work described in the subject article[1] was to investigate the change in optical properties of metals after being damaged by a process that simulated micrometeorite impingement. Since the experiments were not designed to correlate the change in optical properties with the impacting or cratering energy of the impinging particles, it is doubtful that the information given in Refs. 2–4 (were they all available) would have altered the presentation of the data. Reference 2 was not presented at the Fifth Symposium on Thermal Radiation of Solids in March of 1964, but was included only in the proceedings of the Symposium which were published in the summer of 1965, ten months after our paper was submitted to this journal. Similarly, Ref. 4 was not presented until four months after our paper was submitted. Reference 3, which was available at the time our paper was submitted, proposes a correlation between cratering energy and reflectivity, a problem beyond the scope of Ref. 1. To support our position that Refs. 2, 3, and 4 are not pertinent to the problem treated in our paper, we wish to point out that the authors of Refs. 2, 3, and 4, realizing this fact, did not deem it necessary to refer to our previous work (Refs. 5, 6, and 7) which was published as early as 1962.

Concerning the size of the impinging particles (the subject of Ref. 8), it appears necessary to restate that the major damage to the targets was caused by fine dust particles (<20 μ diam) and not by 100μ projectiles. The 100μ diam was merely the size of the original projectiles. As explained in the original paper,[1] the projectiles disintegrated into fine dust particles upon being accelerated to velocities as high as 23,000 fps. It was these fine dust particles that caused the surface damage. The comments of Mark and Mirtich concerning the impingement of 100μ projectiles are therefore without foundation.

It may have appeared unnecessary to the commentators to indicate that high velocity impacts caused more degradation than low velocity impacts. We felt, however, that in the case of a scientific area that is in a recognized state of disagreement, any agreement between experimental results and theory should be pointed out.

It is not surprising that the data presented does not agree totally with the ballistics expression given by Mark and Mirtich. In fact, it is safe to say, based on the present lack of agreement of the various ballistic theories, that there are several other expressions which the commentators could cite,

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