Technical Comments

Comment on "Orthogonality Check and Correction of Measured Modes"

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AT some point (usually quite early) in the career of every Structural Systems Identifier, he indulges in some wishful thinking. "Wouldn't it be nice if the eigenvalues of the nonorthogonal generalized mass matrix were really the generalized masses and the eigenvectors would lead to the real vibration modes?" He soon dismisses this mathematical novelty because it is obviously irrelevant to the physics of the Ground Vibration Test (GVT). However, we have just seen a strange variation of this mathematical curiosity advanced in Ref. 1 as a serious proposal for correcting measured modes. Rather than using the eigenvectors of the generalized mass matrix as a postmultiplying correction matrix to the measured modes, Targoff suggests the inverse square root of the generalized matrix as the correction matrix. Targoff observes that, "unfortunately, an infinitude of modal sets can be found, each of which will satisfy the orthogonality check perfectly." This, of course, is true, but there is more to System Identification than that (see, e.g., the survey by Flannelly and Berman[2]). If Targoff had attempted an experimental correlation with the data correlated by McGrew,[3] rather than merely criticizing McGrew's choice of the Gram-Schmidt method for mathematical rather than physical reasons ("the assumption that all of the modal errors occur with the same signum disturbs our concepts of the randomness of the errors in the experimental process,") he would have discovered the wishfulness of his mathematics. McGrew's data[4] had two rigid body modes and four vibration modes. Here is the dilemma in Targoff's hypothesis: shall we "corrupt" the rigid body modes by making all of the modes orthogonal, or shall we orthogonalize the vibration modes among themselves and accept the nonorthogonality with the rigid body modes? Since there is no escaping between the horns of the dilemma, Targoff's hypothesis is reduced to absurdity.

Taking the latter horn of the dilemma, i.e., using the correct rigid body modes with the vibration modes made orthogonal among themselves, a calculation was made based on the data of Ref. 4. The deflection influence coefficient at the wing-tip was found to be 784×10^{-6} in./lb by Targoff's hypothesis, which compares unfavorably to McGrew's 798×10^{-6} in./lb by the Gram-Schmidt method and the experimental value of 875×10^{-6} in./lb. In an earlier calculation Rodden[5] had obtained a value of 788×10^{-6} using Gravitz'[6] influence coefficient averaging procedure, and he noted that much of the discrepancy arises from the truncation error from having only four measured modes. Clearly, Targoff's proposal does not lead to any improvement in the correlations obtained to date.† There is no need to make the calculation‡ impaled on the first horn of the dilemma; it must be agreed that GVT crews can determine rigid body modes without "corrupting" them.

Since Targoff did not refute McGrew's physical argument for the Gram-Schmidt method, the justification bears repeating[3]: "The method is based on the following assumptions: 1) measured frequencies constitute the most accurate test data, 2) modal amplitude and phasing errors increase with increasing frequency, and 3) structural damping effects are small, but tend to cause higher modes to excite lower modes. Therefore, each successively higher-frequency mode shape consists of the "true" modes plus a linear combination of all preceding modes, including rigid-body modes in the case of free-free vibration." The correlation McGrew achieved is a point in favor of the Gram-Schmidt method and, perhaps, it is to be preferred for the physical reasons stated, but it also achieves the required orthogonlity among the rigid and flexible modes.

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†There is a slight improvement over our original wishful thinking: the eigenvectors of the generalized mass matrix lead to an influence coefficient of 776×10^{-6} in./lb.

‡How can we? The calculation is *not* invariant with the choice of coordinate system!

References

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[4]Kordes, E. E., Kruszewski, E. T., and Weidman, D. J., "Experimental Influence Coefficients and Vibration Modes of a Built-Up 45° Delta-Wing Specimen," NACA TN-3999, May 1957.

[5]Rodden, W. P., "A Method for Deriving Structural Influence Coefficients from Ground Vibration Tests," *AIAA Journal*, Vol. 5, May 1967, pp. 991-1000.

[6]Gravitz, S. I., "An Analytical Procedure for Orthogonalization of Experimentally Measured Modes," *Journal of the Aerospace Sciences*, Vol. 25, Nov. 1958, pp. 721-722.

Reply by Author to W. P. Rodden

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TO any serious reader of Targoff's paper,[1] Rodden's remarks must seem strange indeed. He completely disregards the main thrust of the paper, setting up his own straw men which he proceeds to attack with great vigor. Let us list the points he tries to make: 1) a simple alternative to a complex method is obviously wrong; 2) modal survey tests of "free-free" structures are always performed with *perfectly* unconstrained end conditions; 3) Targoff[1] did not attempt to refute McGrew's[4] argument; ergo, it is correct; 4) two (or more) processes may be evaluated comparatively to the order of 1% using an inaccurate data basis, by passing their results through an unproven filter. Response will be made to each of these points, in turn.

1) Rodden makes derisive reference to the fact that the assumption of only-symmetric errors leads to use of "the inverse square root of the generalized matrix as the correction

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matrix" and implies this is too simple to be taken "as a serious proposal." Actually, this is the complicated part of the paper. The simple part, which Rodden ignores, is the proof that, for small errors, [OR]† $=I+2\alpha$. Thus, for small errors (as in the example used by Rodden,[2] Gravitz,[3] and McGrew[4] which is based on the data of Ref. 5), α may be taken directly as half the off-diagonal generalized-mass terms. Now, Targoff[1] pointed out that the Schmidt procedure is equivalent to the assumption of

$$\beta_{ij}=\alpha_{ij} \quad i<j \; ; \quad \beta_{ij}=-\alpha_{ij} \quad i>j$$

Then, with $\delta \equiv \alpha+\beta$

$$\delta_{ij}=2\alpha_{ij} \quad i<j \; ; \quad \delta_{ij}=0 \qquad i>j$$

So, for small errors the corruption matrix is identically equal to the normalized orthogonality matrix with all terms below the main diagonal set to zero. Now, what can be simpler than that!

When corrections to the measured modes are made in accordance with the above described process, they are found to check McGrew's results almost exactly! (For later use call this Method A.)

Suppose we go back and consider only the symmetric error. But first, like Gravitz[3] let's eliminate the "rigid body" modes. By now the reader knows how to do all this *directly by inspection* of the orthogonality matrix. When corrections to the measured modes are made in accordance with this process, the modes are found to be almost identical to those computed by Gravitz! (Call this Method B.)

2) A requirement to *simulate* free-end conditions for large structures, which may have several modes under 10 Hz, is one of the most difficult the experimentalist faces. In all cases, it is *de rigueur* to measure the "rigid body" modes so that proper correction can be made for inaccuracies in the simulation. Conceptually, one first makes slight adjustments to the modes as measured—constrained by the suspension—so that the full set of modes, including the suspension modes, are orthogonal. Only then can the free-free modes be obtained, by mathematically removing the suspension constraints from this full set.

In the case at hand, while no data are presented, the model undergoing test is so stiff that the suspension constraint should have had essentially no effect on the higher modes. In passing, it should be noted that inspection of the figures in Ref. 5 shows that the plunge-pitch modes are, themselves, highly coupled by the suspension and that only two permissible symmetric coordinates, in fact, exist. If these modes had been isolated during the experiment and measured, no doubt some small errors would have been introduced in the recorded data.

In the present case, the off-diagonal terms in the [OR] matrix, representing coupling with the "rigid body" modes, are quite small, and it is of interest for purposes of comparison to use the all-up symmetric correction procedure under the assumption that the "measured" constraint modes are the pure, uncoupled, rigid body modes. This results, of course, in small "corrections" to these assumed measured constraint modes. The resulting elastic modes (Method C) are sufficiently similar to those of Gravitz and Method B to indicate that the method of treatment of the rigid body modes is not of great significance here. However, it is to be expected when testing large, full scale structures under simulated free-free conditions that some elastic deformation will be present, unavoidably, in the so-called rigid body modes.

3) Targoff did not refute McGrew's arguments because the purpose of Ref. 1 was to reveal certain important limitations of the orthogonality check and then to point out some significant implications of these deficiencies, not to present a "method" nor to criticize anyone else's method. However, the concept that all measured modes of any given structure always have their errors apportioned in rigorous conformity to their modal frequencies remains an unproven hypothesis. In particular, large spacecraft which may have as many as fifty modes in the frequency range of usual interest, i.e., below 50 Hz, very often have these modes occur in groupings of two or more within a frequency band of no more than one or two Hz. To say, for example, that the measured coupling errors of two modes, of an almost-axisymmetric structure, distinct only in that the modes represent small departures from cylindrical symmetry, should be attributed to the mode, say, at 27.1 Hz, while the mode at, say, 26.9 Hz is assumed pure may seem inappropriate to some analysts.

Reference 5 presents numerical modal data for the half-wing only, with the presumption of symmetry or antisymmetry as noted. However, inspection of the modal patterns actually determined during the testing reveals clearly the presence of cross-coupling between both categories of modes. The "symmetric" mode at 122.8 Hz, for example, shows clear evidence of dissymmetry in its modal pattern, while the "antisymmetric" mode at 131 cps shows little such evidence. If full sets of measurements had been taken over the entire wing, it appears safe to assume that coupling terms would have appeared in the orthogonality matrix. In this case, perhaps, it would then be more appropriate to assume all the error lay in the measurement of the lower frequency mode. In any event, Ref. 1 states: "... if for some reason the analyst wishes to make other assumptions regarding symmetry and asymmetry, his specification of these conditions permits the determination of a C matrix satisfying the orthogonality conditions."

McGrew makes comparisons between his and Gravitz's methods, but presents no figures-of-merit. He states his method "... matches the test modes more closely for the first two modes than does the procedure of Gravitz. The correlation breaks down for the third and fourth modes, as may be expected from the large amount of coupling between the first three modes and the fourth..." One normal procedure for comparing the deviation of a surface from an ideal is to compute its rms error. The rms deviation of the corrected modes from the measured modes at the 15 measurement points is listed in Table 1.

The differences are indeed small for the first three modes and probably not meaningful because, really, the original coupling terms are quite small. In any event, it is clear, by this criterion at least, that McGrew's results cannot be claimed superior to those of Method C. It is submitted, however, that the large difference in the 4th mode was to be expected in that McGrew has forced all the required correction into that mode.

4) It is now the writer's turn to ask: Does Rodden seriously believe that the difference between 784×10^{-6} and 798×10^{-6} is meaningful when compared to an experimental value of 875×10^{-6}? Does Rodden know what the effect would be of inclusion of the coupling with the antisymmetric mode? Might it not reverse the order of the correlations? How are the results influenced by the fact that the calculations have been based on Rodden's interpolated mass matrix, a truncated set of modes and a relatively complex transformation to SIC's? As Rodden, himself, said in Ref. 2, "It is difficult to assess the degree of correlation achieved because of the numerous sources of error."

Table 1 Comparison of rms deviations

	Mode			
Method	1	2	3	4
McGrew	.015	.022	.018	.114
A	.015	.022	.018	.115
Gravitz	.019	.022	.016	.055
B	.020	.021	.015	.059
C	.016	.020	.009	.058

†Notation of Ref. 1 is used throughout.

It is perhaps best to conclude with another quote from Ref. 2: "We shall not editorialize further on the correlation, or lack thereof, ... but shall invite the reader to make his own judgment."

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[2]Rodden, W. P., "A Method for Deriving Structural Influence Coefficients from Ground Vibration Tests," *AIAA Journal*, Vol. 5, May 1967, pp. 991-1000.

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Comment on "Structure of Turbulent Shear Flows: A New Look"

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THE probability density distributions reported for the first time in this paper by Roshko[1] for the nonuniform density mixing layer are of very great interest as they are of considerable importance in the theory of chemical reactions in turbulent flow.[2,3] The author remarks on the asymmetry of the distribution in the mixing layer with large density difference, a feature not nearly so marked in the constant-density layer. A large part of this asymmetry, however, appears to be associated with the choice of concentration in mole fraction as the variable of interest rather than mass fraction. Figure 1a shows the probability density distributions replotted using the mass fraction of helium, ξ, as the independent variable. The distributions now appear skewed the other way but this is mostly because the mean mass fractions turn out to be mainly on the N_2 side of 0.5. It would be interesting to see data for higher values of $\bar{\xi}$.

Mass fractions are of more interest theoretically than mole fractions since the conservation equations for chemical species and mixture fraction are usually written in terms of mass fractions. In variable-density turbulent flows such equations are best written in Favre form[4,5] and these give rise to Favre probability density distribution functions[5] such as $\tilde{p}_\xi(\xi)$ where

$$\tilde{p}_\xi(\xi) \equiv \frac{1}{\bar{\rho}}\int_0^\infty \rho\, p_{\rho\xi}(\rho,\xi)\,\mathrm{d}\rho$$

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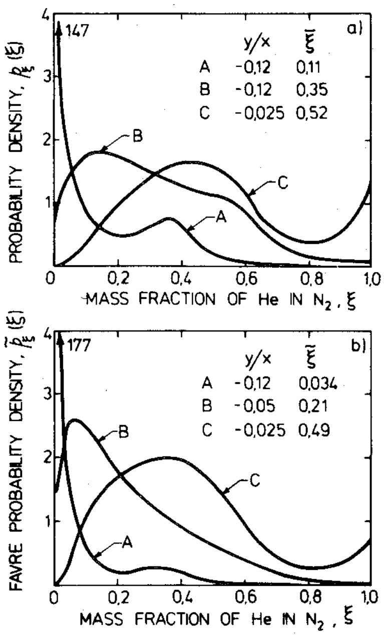

Fig. 1 Probability density distributions of mass fraction of helium in nitrogen in a mixing layer. Data of Roshko.[1] a) Probability density; b) Favre probability density.

In the present case where the density ρ is a function only of ξ the joint probability density function $p_{\xi\rho}(\rho,\xi)$ can be avoided so that

$$\tilde{p}_\xi(\xi) = (\rho/\bar{\rho})p_\xi(\xi)$$

A hypothesis[5] which is gaining support[6] is that it is $\tilde{p}_\xi(\xi)$ and not $p_\xi(\xi)$ which can be modeled in variable density flow in the same way as in uniform density flow. Figure 1b shows the mixing-layer data plotted in a Favre probability density form. These are also skewed to the N_2 side but now all the data is for Favre averages $\tilde{\xi} < 0.5$

$$\tilde{\xi} = \int_0^1 \xi\tilde{p}(\xi)\,\mathrm{d}\xi = \overline{\rho\xi}/\bar{\rho}$$

It would be of interest to see some data for higher values of $\tilde{\xi}$ so that an evaluation of the aforementioned hypothesis can be made.

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