of design freedom in IMSC form, 2) the practical value of a nonlinear quantized control, which is neither optimal nor simply implementable, must be questioned, 3) the influence of actuator locations on control law realization dictates that actuator placement is indeed important, and 4) the assertion of guaranteed controllability via IMSC is meaningless. Finally, and possibly more importantly, 5) the IMSC methods suffer a lack of meaningful guidelines in the choice of independent design parameters such as $\bar{R}_r$, since these parameters are related to the magnitudes of the abstract modal controls rather than actual control inputs. This was detailed here for the case of linear optimal control; it is a deficiency of the nonlinear control technique as well.

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Comment on "A Comparison of Control Techniques for Large Flexible Systems"

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THIS paper[1] presents a control method for large space structures essentially based on dynamic decoupling strategies developed by Wonham and Morse in the late 1960's.[2] Although these ideas provide insight into the understanding of LSS behavior, they do not constitute a practical basis for control (e.g., Bryson and Hall.[3]) In addition to this lack of historical perspective on their proposed method (the contribution of the paper is not clearly defined), the authors have ignored the basic difficulties which make the LSS control problem interesting, to wit: 1) measurement noise and errors, 2) actuator location error, 3) modeling errors, 4) coupling with rigid body "modes," and the plethora of practical mechanization constraints such as actuator location and number. It has been demonstrated in numerous experiments and high-order analytical examples that control design which correctly manages such model error problems is necessary to achieve robust LSS control.[4] In this regard, the example selected as a basis for the comparison suggested in the paper's title is not sufficiently complex or representative of actual LSS control problems to justify the strength of the conclusions reached by the authors. In fact, for this problem many control design methods (including some extremely nonrobust mechanizations) can be made to work. Clearly, this is not the point of LSS controls research. The paper raises some interesting questions, but more thinking is needed on the IMSC method.

Received Sept. 22, 1983.

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Reply by Authors to M.A. Floyd, R.E. Lindberg, and M.G. Lyons

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THE three Technical Comments are answered individually. Because some of the arguments and references were the same, it was deemed appropriate to group the individual replies into a single one.

Reply to M.A. Floyd

1) The statement on p. 305 of Ref. 1 is not an allegation but a demonstrable fact. If the author of the Technical Comment believes that pole allocation can be used to design controls for high-order systems, say 100, then he owes it to the technical community to show the results. The same task is almost trivial if the independent modal-space control (IMSC) method is used. Indeed, as soon as the closed-loop poles have been selected, the modal gains can be obtained by means of Eq. (28), clearly a very simple computation. The fact that in IMSC the closed-loop eigenvectors are the same as the open-loop eigenvectors is not a restriction but the natural way of designing controls (see Ref. 2). Note that to change the system eigenvectors, in addition to the system eigenvalues, it takes energy, and this energy is simply wasted. Indeed, Fig. 2 of Ref. 1 shows that to control a cantilever beam by the pole allocation method, IMSC requires appreciably less energy than coupled control, although the closed-loop poles are the same in both cases. Hence, because coupled control requires more energy than IMSC for the same job, the extra energy must be wasted.

2) On page 306 of Reference 1 it is stated that if $Q=\text{diag}(\omega_1^2\ \omega_1^2\ \omega_2^2\ \omega_2^2 \ldots \omega_n^2\ \omega_n^2)$, then $w_C^T Q w_C$ represents the Hamiltonian, which is ample justification. To a person

Received Nov. 22, 1983; revision received Jan. 30, 1984.

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familiar with dynamics, this choice is very logical. If the system admits rigid-body modes, the cost function can be modified with ease through a redefinition of the auxiliary variable $v_r(t)$, Eq. (15). After all, the Hamiltonian is defined for systems admitting rigid-body modes.

3) We were not aware of Ref. 3 (Ref. 2 of the Technical Comment) when we wrote the paper. In fact, Refs. 1 and 3 were submitted for publication on the same date.

On p. 54, Ref. 3 states in connection with on-off control in coupled form: "But that problem is very complicated and a general solution for the optimal cost function seems out of reach. Therefore, toward our objective of an approximation to that optimal cost function, we make two assumptions which are drastic but which lead to usable results."

Our statement concerning the impossibility of on-off control for multi-input, multi-output systems was made in the context of an exact, and not an approximate, solution. This does not mean that future missions should not consider on-off jets. Indeed, on-off jets can be used in clusters so as to yield quantized controls, as suggested by IMSC. In fact, such a control scheme has already been implemented in a laboratory experiment simulating a space structure (Ref. 4), which, incidentally, includes rigid-body modes.

4) The IMSC method does, indeed, lead to globally optimal control, as demonstrated mathematically in Ref. 2. Note that the form of the operator R given in Eq. (30b) of Ref. 2 is the only one leading to independent modal controls, and, hence, to unconstrained control, thus permitting globally optimal control. Any technique causing coupling of the modal controls represents constrained control, so that control cannot be globally optimal but only locally optimal. Quite often in the current practice this implies optimal for a given number and location of actuators. In controlling a finite number n of modes, the IMSC method prescribes as many actuators as the number of controlled modes, thus preserving the unconstrained nature of the modal controls. This amounts to simply letting $R_r=0$ $(r=n+1,\ n+2,\ldots)$ in Eq. (30b) of Ref. 2. If the number of actuators is smaller than n, then the modal controls can no longer be designed independently. This results in constrained control, and, hence, in suboptimal control. Another view of the same problem can be obtained by considering the relation $\boldsymbol{f}=\boldsymbol{BF}$ between the modal control vector $\boldsymbol{f}$ and the actual control vector $\boldsymbol{F}$, where $\boldsymbol{B}$ is the modal participation matrix. In IMSC, both $\boldsymbol{f}$ and $\boldsymbol{F}$ are n-dimensional, so that $\boldsymbol{B}$ is square and of full rank, provided it is nonsingular, which is very likely (see Item 2 later in this Reply). In coupled control dim $\boldsymbol{F}<n$, so that $\boldsymbol{B}$ cannot be of full rank. In fact, if dim $\boldsymbol{F}\ll n$ and/or for given actuator locations, difficulties in controlling n modes can be anticipated, as certain states may be uncontrollable or certain closed-loop poles may be unattainable. That "the work done to control the controlled modes by IMSC does not depend on the actuators' locations" is not a claim but a fact. It was proved in Ref. 35 of Ref. 1 mathematically and the mathematical development was verified via a numerical example. The results are summarized by Eqs. (44) and (45) of Ref. 1. The Technical Comment contends that the above is true "regardless of the control method used, not just in the case of IMSC as stated." The author of the Technical Comment must provide mathematical proof (backed up by a numerical example) before this contention can be regarded as more than a mere claim. Of course, the discussion centers around a multi-input, multi-output and not a single-input, single-output system.

5) The statement in the Technical Comment that "The IMSC method involves a cost function of a very particular form. This cost function automatically adjust itself so that changes in actuator locations do not cause changes in cost." is a complete misreading of IMSC. The cost function in IMSC is independent of the actuators' locations. This is a desirable feature, as the same closed-loop poles are obtained regardless of the actuators' placement. One may wish to place the actuators at certain locations, but it is nice to know that the closed-loop poles will not be affected by that choice.

6a) A cluster of on-off actuators, producing quantized control, represents nonlinear control. Quantized control seems to be considerably easier to implement with thrusters than linear control, which may require variable-thrust jets.

6b) IMSC is ideally suited for implementation by microprocessors. The computation of the modal control forces can be carried out in parallel (see Fig. 3 of Ref. 4), thus reducing the computation time as opposed to coupled control. The actual control forces are obtained from the modal control forces via a simple matrix multiplication. IMSC is also ideal for on-board implementation, as it has the flexibility to change closed-loop poles with ease by simply altering the modal gains in the microprocessors associated with the modes whose poles are to be changed.

6c) The claim that "...the relationship between changes in vibration energy and work is independent of the control method used," cannot be substantiated. Indeed, it is shown in Ref. 1 (Fig. 2) that IMSC requires the least amount of energy. This can be explained by the fact that in coupled control the modal controls are not independent and energy is wasted through spillover from one controlled mode into another, thus degrading the control system performance. The Technical Comment implies that IMSC uses inefficient actuators and coupled control uses efficient actuators. If this is indeed the implication, then one must wonder why IMSC cannot avail itself of the same efficient actuators as coupled control.

7) The authors are glad that the author of the Technical Comment appreciates that a larger number of actuators lowers the cost function. Indeed, IMSC not only "will always appear better" but it will be better. For many years people were arguing the opposite.

8) IMSC was proved superior in the case of the quadratic performance index (Refs. 1 and 2) and the minimum-fuel cost function (Ref. 5), which are used almost exclusively by control engineers. Now the Technical Comment would like to see "real-world" performance criteria used, predicting that "IMSC would certainly fare very poorly if a term which was a function of the number of actuators (increased number of actuators = increased complexity and cost) was included in the cost function." But equating an increased number of actuators to increased complexity and cost is refuted by Table 3 of Ref. 1. At any rate, the technical community would be better served by a mathematical analysis using a "real-world" performance index rather than mere speculation. In this regard, the authors would welcome an investigation by the author of the Technical Comment using a "real-world" performance index. But to make this index as realistic as possible, he may wish to include in the index the cost and weight of the on-board computer, the number and weight of the fuel tanks, the cost of fuel for the duration of the mission, etc.

As far as the "Additional practical problems..." perceived by the Technical Note are concerned, once again the facts are not there to support the contentions:

1) Actuator failure is critical to any control scheme, including IMSC. But it is less of a problem in the case of IMSC. In the case of failure of one actuator, one can control one mode less. This may result in some degradation in the system performance, but definitely not in instability. On the other hand, in coupled control one must solve a new Riccati equation for "optimal control" (which is really constrained optimal control) and a solution is not guaranteed. It is easy to see that if two actuators are used and one fails, stability cannot be guaranteed. Moreover, in the case of redundant actuators, in IMSC one simply recomputes B^{-1}, but in coupled control one must recompute the gains. The latter is likely to require significant computational effort, perhaps exceeding the on-board computer capability.

2) In the case of IMSC, the matrix B is generally well behaved. One would have to go out of his (her) way to choose actuators' locations to render it nearly singular. Consider the example of a one-dimensional system in which ten modes are to be controlled by ten actuators. The tenth mode has only nine nodes, so it is impossible for B to be singular. As a rule of thumb, the actuators should be spaced evenly, which yields a very well behaved matrix B. Note that in coupled control, in which a small number of actuators is used for the same task, the matrix B (which appears there too) can have an entire row equal to zero, in which case the corresponding mode is uncontrollable. In the case of a single actuator, in which the matrix B is a vector, a single zero component renders that mode uncontrollable.

3) IMSC has already been implemented in laboratory experiments (see closing remark).

In the last paragraph, the Technical Comment questions the basic idea of IMSC. Perhaps unwittingly it questions a large segment of mathematical analysis, namely the transformation theory. In effect, the paragraph warns against attempting to solve a set of simultaneous differential equations by transforming them into a set of independent equations via a linear transformation. The Comment states that, "Modal equations of motion are merely a mathematical representation (an approximation, in fact) of the internal behavior of the system," which implies that the modal equations are not even accurate. The modal equations are as accurate as the simultaneous equations. In addition, modal coordinates and velocities are not more "internal" than any other set of coordinates and velocities. They are, however, natural coordinates and velocities (see Ref. 2), which implies a preferred status. Natural coordinates, also known as principal coordinates, do indeed have a preferred status, as they are the only coordinates for which the equations of motion become decoupled. It is strange that the Comment questions the use of modal coordinates and not the use of natural frequencies.

As a final remark, it should be noted that IMSC has been already implemented successfully in the context of multi-input, multi-output control in laboratory experiments (Refs. 4 and 6). The results corrobate many of the statements made in Ref. 1 and should answer most of the questions raised in the Technical Comment.

Reply to R.E. Lindberg

Reference 1 compares independent modal-space control (IMSC) with coupled control. Whereas there are many ways of designing coupled control, they all have one thing in common: the control is constrained (see Item 4 in the Reply to M.A. Floyd). Hence, the classification used is fully justified.

Pole Allocation

Contrary to the opinion expressed in the Technical Comment, using fewer actuators than controlled modes does not provide freedom, but it does place constraints on the control. Complete independence can be achieved only with IMSC. As pointed out in the Reply to M.A. Floyd (Item 4), because a full set of actuators is used in IMSC, the modal participation matrix B is square and unlikely to be singular, no matter where the actuators are placed. On the other hand, in coupled control the matrix B is not of full rank and the designer has less freedom to choose the actuator locations. This point can be dramatized by considering a single actuator. The designer will have difficulties in placing the actuator sufficiently removed from all of the nodes if a large number of modes are to be controlled. Some of the difficulties inherent in the pole allocation technique are discussed in Ref. 1 (pages 304, 305, and 308).

For some other points, see Item 1 in the Reply to M.A. Floyd.

Linear Optimal Control

There are two questionable things in the argument of the Technical Comment in favor of coupled optimal control. In the first place, the coordinates are not independent, so that the assumption of diagonal R_1 must be justified. In the second place, even accepting the assumption of diagonal R_1, what justification is there for giving different weights to different actuators if the actuators are all of the same type? Of course, if R_1 is not diagonal, then the analyst has a problem in assigning weights, as he must select a weight for each pair of actuators. The norm of R_1 can hardly serve as a design guideline.

The situation is considerably better in IMSC. Using R_2 as a diagonal matrix is not a restriction, but a natural consequence of the fact that modal controls are independent. Indeed, the weight assigned to a pair of distinct modal controls must be zero automatically. Here there is genuine freedom in selecting the modal weights $\bar{R}_r$, as the weights can be tailored according to how critical a given mode is. For more critical modes, the weights $\bar{R}_r$ can be made smaller, and vice versa. No such insight is available to the analyst in coupled control.

Nonlinear On-Off Control

The statement that time or fuel optimization cannot be achieved by IMSC is incorrect. The second paragraph merely repeats an assertion of Ref. 1, namely, that the modal controls are on-off and that the actual controls are quantized. There is no reason, however, to believe that modal on-off controls cannot lead to minimum-fuel control. Indeed, if saturation of the actuators takes place at low levels of force, then the solution can be only suboptimal (Ref. 5). But if saturation is unlikely, as is quite often the case, then the solution is optimal. Note that in the case of Ref. 4, the actuators were operating at levels well below saturation. Incidentally, quantized control can be achieved by means of a cluster of on-off devices. Does the Technical Comment mean to imply that variable-thrust control is easier to implement than quantized control?

Control Implementation

Controllability is required for successful implementation of any control technique. In the case of IMSC, controllability is guaranteed if the matrix B is nonsingular, which is the case in one-dimensional systems by definition. For an explanation of this statement, see the *second* Item 2 in the reply to Floyd.

Conclusions

1) On the contrary, IMSC has complete design freedom, as it can achieve virtually any closed-loop system. 2) This statement is "a great exaggeration," as quantized control was already implemented on hardware (Ref. 4). 3) Reference 35 of Ref. 1 demonstrates the exact opposite. 4) The assertion of guaranteed controllability for IMSC reflects a fact. The matrix B is by definition nonsingular for one-dimensional structures (see *second* Item 2 in the Reply to M.A. Floyd) and the control engineer would have to go out of his way to render it singular for two-dimensional structures. 5) For a control engineer with dynamics background, the natural modes are meaningful indeed. The choice of $\bar{R}_r$ is directly related to how critical a given mode is.

Reply to M.G. Lyons

The IMSC is not based on decoupling strategies developed by Wonham and Morse. IMSC uses plant decoupling, whereas Wonham and Morse advocate input-output decoupling. IMSC does, indeed, provide insight into the understanding of large space structures behavior. But, contrary to the opinion expressed in the Technical Comment, IMSC does provide a practical basis for control, and it was, in fact, implemented in laboratory experiments (see Refs. 4 and

6): The authors have not ignored "basic difficulties." In fact, they addressed most of these problems: 1) measurement noise and errors were treated in Refs. 30 and 31 of Ref. 1, in which the concept of modal Kalman filters was introduced, 2) actuator location error was not investigated, but in view of the insensitivity of IMSC to actuators locations, this seems to be a trivial problem, 3) modeling errors were considered in Ref. 26 of Ref. 1. The conclusion was that controls designed by IMSC are insensitive to errors in the open-loop eigenvalues, and 4) rigid-body modes present no particular problem. They were included in Refs. 4 and 5.

When the authors use the term "large flexible system" they refer to the order of the system and not to the physical size. But IMSC reduces a system of any order, including infinite order associated with distributed systems, to a set of independent second-order systems. Hence, many of the complexities associated with large-order systems disappear if IMSC is used but persist if coupled control is used. As far as the complexities listed above, the IMSC theory is much further along than the author of the Technical Comment seems to realize.

In conclusion, the authors welcome the opportunity to discuss the merits of IMSC vis-a-vis coupled control. A great many of the points in the three Technical Comments were presented as facts when in actuality they are only personal opinions. Before they can be regarded as facts, they require mathematical substantiation. The various papers by these and other authors, including some of the more recent ones in which experimental work is presented, provide ample mathematical and experimental substantiation of the IMSC method.

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Errata

Effects of Time Delays on Systems Subject to Manual Control

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[J. Guidance, 7, 416-421, (1984)]

THE following legend was inadvertently omitted from Fig. 11.

Model parameters:

$K_e = 4.2$	$k = 2$
$K_I = 1.0$	$\omega_n = 6.0$ rad/s
$K_2 = 5.0$	$\zeta_n = 0.75$
$T_I = 2.5$ s	$\tau_0 = 0.20$ s
$T_2 = 2.5$ s	

Received July 27, 1984.