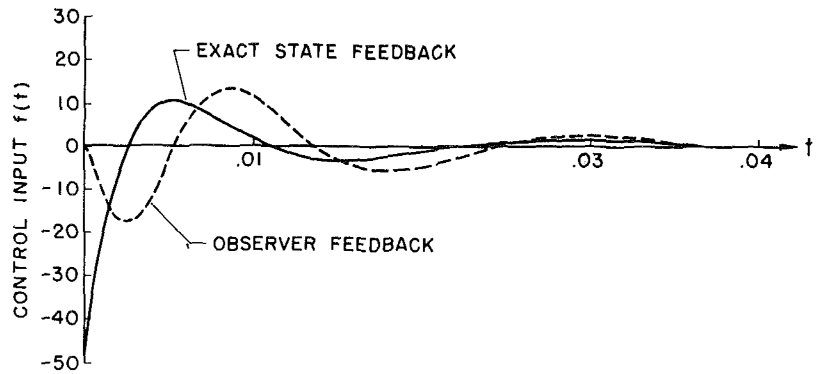

Fig. 6 Control input for Example 2

We see that $x_\sigma = 0$, $1 \leq \sigma \leq n$, if the functions $(PL)^{k_\sigma - 1} P\phi_{j_\sigma}$ are linearly independent. Restating this result, the pair (A, B) in equation (8) is completely controllable if and only if there exist n linearly independent functions in the set

$$P\phi_1, (PL)P\phi_1, \ldots, (PL)^{n-1}P\phi_1, \ldots, P\phi_r, \ldots, (PL)^{n-1}P\phi_r.$$

It is straightforward to show that, if the above condition on P, L, and $\{\phi_j\}_1^r$ is satisfied, the n-dimensional Galerkin approximation to the partial differential equation

$$\frac{\partial^\nu}{\partial t^\nu} X = LX + \sum_{i=1}^{r} \phi_i(z) u_i(t)$$

is completely controllable for any finite positive integer ν.

A similar result holds for observability, as well. Referring to (8), the n-dimensional row vectors of the matrix

$$M = \begin{bmatrix} C^* \\ \cdot \\ \cdot \\ \cdot \\ C^* \quad A^{n-1} \end{bmatrix}$$

are given by

$$\mathbf{c}^*_i A^{k-1} = (< v_i, (PL)^{k-1}\omega_1 >, \ldots, < v_i, (PL)^{k-1}\omega_n >)$$

where $\mathbf{c}^*_i$ is the ith row of C^* and

$$v_i \equiv \sum_{k=1}^{n} \omega_k(\eta_i)\omega_k(z) = P\delta(z - \eta_i).$$

Observability of (8) is equivalent to the condition: rank $M = n$. Since

$$< v_i, (PL)^{k-1}\omega_j > = < \delta(z - \eta_i), (PL)^{k-1}\omega_j > = [(PL)^{k-1}\omega_j](\eta_i),$$

it follows that the set of *vectors*

$$\omega(\eta_1), [(PL)\omega](\eta_1), \ldots, [(PL)^{n-1}\omega](\eta_1), \ldots, \omega(\eta_m), \ldots, [(PL)^{n-1}\omega](\eta_m)$$

contains a maximal linearly independent subset if and only if equation (8) is completely observable. Further, if the foregoing condition on P, L, and $\{\eta_i\}_1^m$ is satisfied for the distributed system

$$\frac{\partial^\nu}{\partial t^\nu} X = LX + \sum_{i=1} \phi_i(z) u_i(t)$$

$$y_k = [PX](\eta_k, t), \qquad k = 1, 2, \ldots, m,$$

then the corresponding Galerkin approximation is completely observable.

DISCUSSION

R. E. KLEIN[7]

The authors, P. A. Orner and A. M. Foster, are to be congratulated for their well thought out work and its clarity of exposition. The control of distributed parameter systems is recognized to be a very difficult question, and consequently, an attitude of caution is important when one is considering general methods. Below are several comments which consider this point.

In the paper by Orner and Foster, several fundamental questions remain unanswered. The paper is applicable to, if one is to believe the title, a particular class of distributed systems. However, the restrictions which define the admissible class are not explicitly stated. Since the method is constructive, one has little, if any, criterion to ascertain in advance if the method will be successful for a given distributed system. Also, the degree of success of the method, if measured in terms of the controllable accuracy of the true distributed system solution, is dependent upon numerous factors including, in particular, the order and convergence properties of the approximation basis, and the number of measurement transducers and zone controllers. Again, there is little indication of how one might rationally prescribe values of n, r, and k or select the shapes of the ϕ_i and $\omega(z)$. In fact, the selection of the Galerkin basis functions constitutes a central issue and one has no guarantee that any suitable basis even exists for many problems.

The method and approach given by the authors is applicable only to those distributed parameter systems which possess solutions which can be reasonably expressed by a finite set of basis functions (of z) weighted by time functions $x_j(t)$, respectively. Unfortunately, a nonnegligible number of industrial distributed parameter systems *do not* fit directly into the Galerkin framework. For example, consider

$$\frac{\partial X(z, t)}{\partial t} + \frac{\partial X(z, t)}{\partial z} = -BX^2(z, t)$$

$$0 < z < l, 0 < t < T$$

where B is given positive scalar. This constitutes a first order nonlinear problem typical of chemical flow reactors for which the characteristic theory clearly allows one to conclude that any assumed mode approximation of the type suggested in the Orner-Foster paper would be inappropriate.

A more serious difficulty concerns the assumption $\mathbf{y} \approx C^*\mathbf{x}$

[7]Assistant Professor of Mechanical Engineering, University of Illinois at Urbana-Champaign, Urbana, Ill.

in equation (8) on which the balance of the paper is ultimately dependent. The L_2 integrability of $X(z, t)$ in no way guarantees the foregoing assumption. It is (or should be) obvious that no pointwise bound $X(z, t) - X_n(z, t)$ is implied since for example, at the measurements points, the measurements $\mathbf{y}(t)$ may depart significantly from $C^*\mathbf{x}$ while the L_2 integrability bound is still satisfied. Should the assumption of $\mathbf{y} \approx C^*\mathbf{x}$ be valid for a given problem, then the balance of the approximation approach in Orner-Foster is, primarily, a straightforward application of ordinary differential equation techniques.

This discusser recognizes the value of the Orner-Foster work; however, he feels that it should also be accompanied with sufficient warnings to the unwary. Specifically, it is not an all powerful method but rather an approach that may or may not work, depending on factors which are often beyond the user's realm of information. A list of detailed remarks and questions follow.

1 Equation (2) is not a first order canonical form as stated by the authors since the operators $L_{2i}, \ldots, L_{tv}$ may be of order greater than one.

2 How general are the zone functions? Can we not admit a single boundary control in certain examples?

3 The selection of the basis functions is the crucial question. What criteria might one use in their selection? Do the basis functions have to independently satisfy the boundary conditions?

4 The term "state estimation error $\mathbf{e}$" is misleading. As the authors use it, $\mathbf{e}$ fails to account for the error induced by $\mathbf{y} \approx C^*\mathbf{x}$.

5 With regard to the above, the bound $\|K + P\|$ on J_n also fails to account for the errors induced by $\mathbf{y} \approx C^*\mathbf{x}$.

6 It seems restrictive to allow for set points in the form of a vector. Isn't the physical problem (often) one of having a distributed setpoint for the entire spatial domain?

7 Readers interested in an application of modal approach techniques may benefit by examining the excellent work by Wiberg.[8]

[8]Wiberg, D. M., "Optimal Control of Nuclear Systems," *Advances in Control Systems*, Leondes, C. T. (ed.), Vol. 5, Academic Press, New York, 1967, pp. 301-388.

Authors' Closure

We thank Professor Klein for his interest and kind remarks. We fully agree that the control of distributed systems is a hard problem and make no claim to an "all powerful" approach, but rather one of engineering utility. The technical comments and questions will be addressed in order.

The "admissible class" of systems is taken as those with states $X(\cdot, t)$ in $L_2(0, 1)$ for each $t \geq 0$, as stated prior to equation (1). The latter property has been established for many (mathematical) distributed systems, and can be argued on physical grounds for virtually all physical systems and approximations thereto. Accordingly, there is always an X_n such that $\|X - X_n\|_{L_2}$ is arbitrarily small for sufficiently large n. Whether the (coefficients defining) X_n *as given by the Galerkin method* will indeed converge to the (coefficients defining the) true X is another matter, and is dependent on a "suitable basis" for the system at hand. A "guarantee" that a suitable basis exists is tantamount to an explicit convergence proof for the general Galerkin method. To the authors knowledge, such a proof does not exist except for certain specific operators (cf. parabolic as in [10]).[9] On the other hand, it would seem that any problem amenable to a finite difference method defines a suitble basis for the Galerkin approach, since it has been shown that the former is a subclass of the latter [11]. It is felt that the lack of convergence proofs should not be allowed to detract from the very real engineering relevance of the computational methods proposed.

The interesting example presented by Professor Klein is equivalent to the linear system

$$\frac{\partial}{\partial t}\begin{pmatrix} u \\ v \end{pmatrix} = \begin{bmatrix} -\partial/\partial z & 0 \\ B & -\partial/\partial z \end{bmatrix}\begin{pmatrix} u \\ v \end{pmatrix}$$

by the transformation $u(z, t) = X(z, t)v(z, t)$. The operator $\frac{\partial}{\partial t} + \frac{\partial}{\partial z}$ generally does not admit a discrete spectral expansion in $L_2(0, 1)$, but this does not rule out the applicability of the Galerkin method per se. There is a difference between lacking an L_2 solution and lacking a discrete spectrum. It should prove interesting to examine the computational behavior of the solution to such equations.

The statement (equation (8)) that $\mathbf{y} \approx C^*\mathbf{x}$ is a working hypothesis, and proved out quite well for the examples examined. It was indeed a main intent of the paper to demonstrate that "a straightforward application of ordinary differential equation techniques" could be fruitful in the practical solution of distributed optimal control problems. While it appears possible, by special basis choices for certain problems, to guarantee that $y_k(t) = X_n(\eta_k, t)$, the pragmatic significance at this point is not clear.

Addressing the enumerated remarks and questions:

1 Equation (2) is in the form of a first order equation in time.

2 The zone functions can be any $L_2(0, 1)$ elements. Boundary control was used in Example 2.

3 One can always convert a problem with non-homogeneous boundary conditions to an equvalent one with homogeneous boundary conditions. The basis can then be chosen to independently satisfy the homogeneous boundary conditions. Alternatively, in the spirit of computational approximation, one could treat a boundary condition discrepancy (between basis and solution) as an error residual in the same way the range error in equation (4) is treated. The choice of specific basis functions is still an open question, as indicated in the Conclusions.

4, 5 It is correct that $\mathbf{e}$ accounts only for the *state estimation error* in the finite dimensional model. As stated earlier, quantification (or elimination) of the error in the approximation $\mathbf{y} \approx C^*\mathbf{x}$ is considered a separate problem.

6 The development in the paper addresses the infinite time interval regulator problem wherein the (distributed) set point is null. A non-null distributed set point can be introduced into the finite dimensional optimal control problem either *directly* as the desired state at certain selected spatial points, or *indirectly* by expansion in the Galerkin basis (with the coefficients defining a desired finite dimensional $\mathbf{x}_d$).

[9]Numbers in brackets designate Additional References at end of Closure.

Additional References

10 Green, J. W., "An Expansion Method for Parabolic Partial Differential Equations," *Journal Research N.B.S.*, Vol. 51, No. 3, Sept. 1953, pp. 127-132.

11 Swartz, B., and Wendroff, B., "Generalized Finite Difference Schemes," *Math. Comp.*, Vol. 23, No. 105, 1969, pp. 37-49.