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Distributed Parameter Modelling of Flexible Spacecraft: Where's the Beef?

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Abstract

This presentation discusses various misgivings concerning the directions and productivity of Distributed Parameter System (DPS) theory as applied to spacecraft vibration control. We try to show the need for greater cross-fertilization between DPS theorists and spacecraft control designers. We recommend a shift in research directions toward exploration of asymptotic frequency response characteristics of critical importance to control designers.

Distributed Parameter System Theory:

A knife without a blade for which the handle is missing?

or

We used to worry about DPS theory's relevance to Space Structure Control but we're ok now!

D. C. Hyland
Harris Corporation

Some time ago, Larry Taylor asked me to give the initial presentation for this workshop. Larry encouraged me to share some of my misgivings on Distributed Parameter System (DPS) theory as an area of ongoing research activity in hopes of arousing controversy and stimulating discussions during the meeting.

Having agreed to help with the workshop, I found it quite a struggle to arrive at an appropriate title. First, a broken arm and leg suffered early last year put me into a rather futile mood, and gave rise to the melancholic thought expressed by the first title. But then, after my broken limbs began to mend, I devised the second title, reflecting a mood of recovery and optimism. Finally, backing away from undue optimism (we're not okay yet!), I settled on the title indicated on the first page. This title strikes a better balance between futility and enthusiasm. "Where's the beef?" means "What is the substantive contribution?" At least implicitly, the question admits the possibility that there is substance. Indeed, I approach this field as a worried friend, concerned to find precisely those areas in which DPS theory can truly contribute.

- A Few Preliminary Observations -

* Distributed Parameter System theory is a necessary part of our engineering culture - should be widely taught and learned.

* There are so many ways in which the usual lumped parameter models differ from the actual system. A knowledge of DPS theory heightens our awareness of these crucial differences.

* Provides unifying framework for understanding -e.g. connections between modal dynamics and wave propagation.

* Crucial for settling matters of general principle - e.g., existence questions, controllability, stability guarantees, etc.

In any case the criticisms voiced here have nothing to do with the intrinsic merit of the DPS field as a valuable body of mathematical knowledge, but are concerned with where it has been and is going as an unfolding research enterprise. We should take particular care to establish how DPS theory fits in (or whether or not it fits in) to design practice. Unfortunately, many people who build working systems consider DPS research as a form of "middle-class welfare." To counter this perception we need to honestly identify the aspects of DPS theory that are truly essential to control engineering.

First, it is reasonable to observe the intrinsic merits of DPS as a body of knowledge, apart from its *direct* relevance to applications. These merits are listed in the panel. The reader will note many papers in the Workshop that develop these crucial areas of value.

Having said all this, the problem with the DPS research enterprise can be stated in terms of pins and angels. Recall the medieval theological controversy: "How many angels can fit on the end of a pin?" If you are a theologian, then it's quite appropriate to argue this question. On the other hand, if you are a pin manufacturer, the question is irrelevant and it is your duty to worry about other aspects of pins. The trouble comes when theology is mixed in with manufacturing!

First, one is witnessing theology (not engineering) when one hears claims of *universal, infallible* truth. An example is the common argument for adopting a DPS theoretical setting, namely that DPS models are the only models that truly capture the underlying physical reality of aerospace structures. In the panel we list two of the many ways in which this claim is refuted. Indeed, as are all other models, DPS models are also inherently approximate.

In fact, the claim considered here is essentially a claim to guru-hood -i.e., the unique possession of arcane, transcendent knowledge.

Distributed Parameter System Models are Superior Because They Capture the Underlying Physical Reality of Aerospace Structures

* Quantum Mechanics (not continuum mechanics) prevails at small scales; at sufficiently high frequency there are no modes.

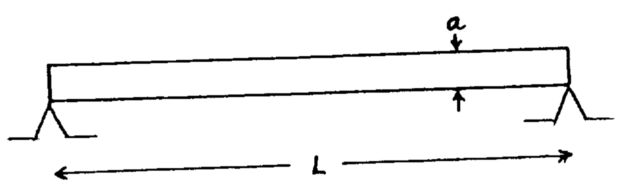
* Real Sensors (for feedback control) have limited resolution
  ☞ observable closed-loop system is necessarily finite (albeit large) dimensional.

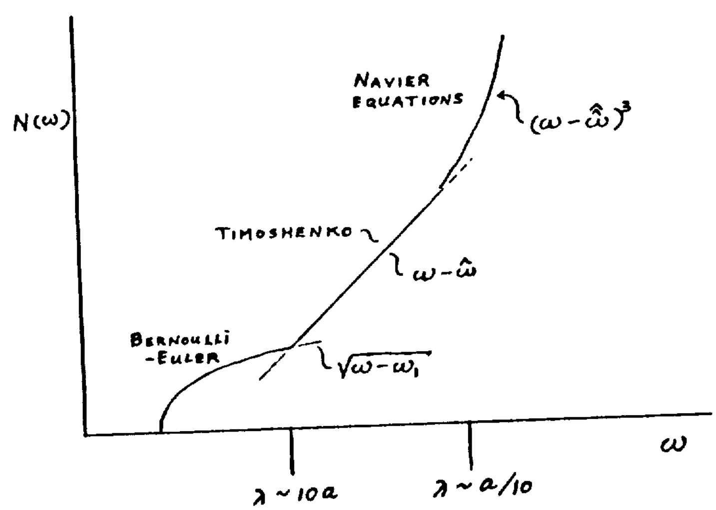
The trouble is, the claim of transcendent wisdom is a very heavy burden. The more extreme the pretension, the more severe the embarrassment. One of the most obvious pretensions is that DPS theory can model infinitely many modes.

The panel sketches the behavior of the "mode count," N(ω) (number of modes below a given frequency) as a function of frequency for a "simple," simply-supported beam. The mode count function gives at least a rough idea of the frequency spacing of adjacent modes - a significant characteristic for control design considerations. It is obvious from the N(ω) chart that the vast majority of DPS work that postulates classical Bernoulli-Euler models for beams, succeeds in modelling infinitely many modes completely erroneously!

Distributed Parameter System Theory can Model Infinitely Many Modes ...

... with infinitely many errors:





For example, even the gross number of modes per octave band may be completely wrong.

Of course, the essence of guru-hood is the claim to secret, esoteric knowledge, without which the engineering problems can not be solved. An often implied, subliminally repeated message is that DPS theory is an absolute prerequisite to successful vibration control design. On the contrary, numerous successful control designs *have* been arrived at without the use of DPS theory (but using control theory) and have been verified experimentally. Indeed, we have yet to see an experimental result that has used DPS theory in a truly substantive way for control *design*.

Of course there are interesting DPS theoretical results that pose qualitative warnings to the designer -e.g., the nonconvergence of LQG design if system dissipation is neglected, the inherent instability of infinite-dimensional systems under certain types of feedback when transport delay is introduced, etc. However, most of these qualitative warnings that are relevant to design could have been formulated without DPS theory. In place of the DPS postulate one could use the hypothesis that the plant is a finite, but arbitrary large dimensional system.

At this point, enough said about theology. Let us consider DPS from the point of view of pin manufacturers (make 'em good and cheap). Let us honestly discuss the aspects of DPS modelling that *are* pertinent to vibration control design. To begin such a discussion, I think we need to return to some elementary *control design* concepts, e.g., the concepts of phase stabilization, gain stabilization and robust performance. These items are now discussed in turn.

DPS Theory is an Absolute Prerequisite to Successful Vibration Control Design!

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* Numerous successful vibration control designs have been arrived at without use of DPS theory (but using *control theory*) & verified experimentally - see e.g., NASA CSI Guest Investigator Program, Phase I.

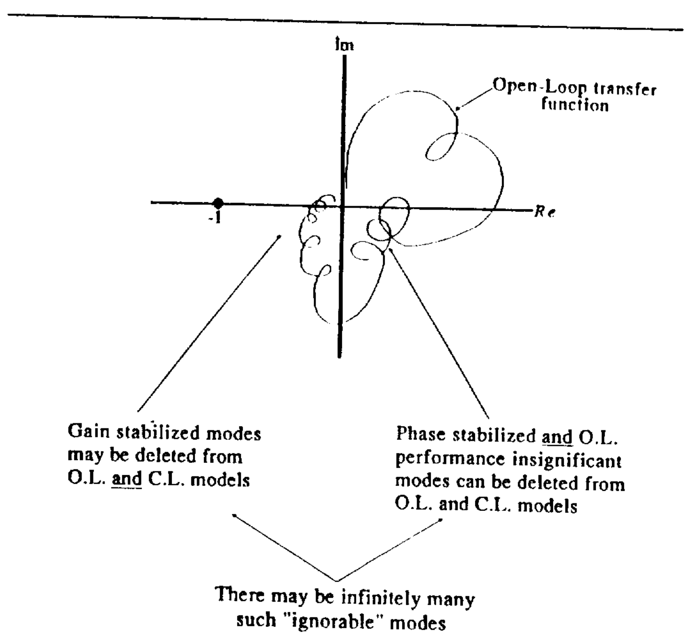
* Try to identify experimental results that have used DPS theory in a substantive way for control *design!*

* What aspects of DPS modelling *are* pertinent to vibration control design?

  To answer this, we need to get back to some elementary *control design* concepts, e.g.:
  - Phase Stabilization
  - Gain Stabilization
  - Robust Performance

First recall the Nyquist diagram - that simple but comprehensive way of visualizing the structure/control interaction and the basic design problem. As sketched here, the Nyquist diagram (assuming rate sensing) is a sequence of loops. Where the loops are large, one tries to shape the phase so that they fall into the 1[st] or 4[th] quadrant (phase stabilization). Where phase is bad, one tries to shape gain so that the magnitude is small, thereby avoiding -1 (gain stabilization). These considerations provide a guide to modelling fidelity and simplification. For example, it is clear that structural modes that have insignificant performance impact in the open-loop *and* are phase stabilized can safely be deleted from both open and closed-loop models. The same can be said for gain stabilized modes outside the controller bandwidth. There may be (and perhaps are) infinitely many such ignorable modes. For practical fidelity, design models should include the modes contributing most to open-loop performance degradation and the modes near the unity gain cross over points or in the band over which cross overs occur frequently. The size of such practical models is usually quite modest. Thus, ignoring elementary control design insights can grossly exaggerate the dimensionality problem.

Remember Mr. Nyquist?

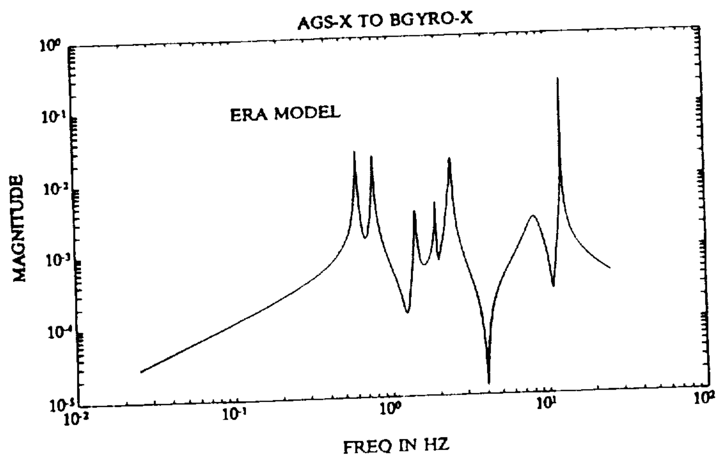


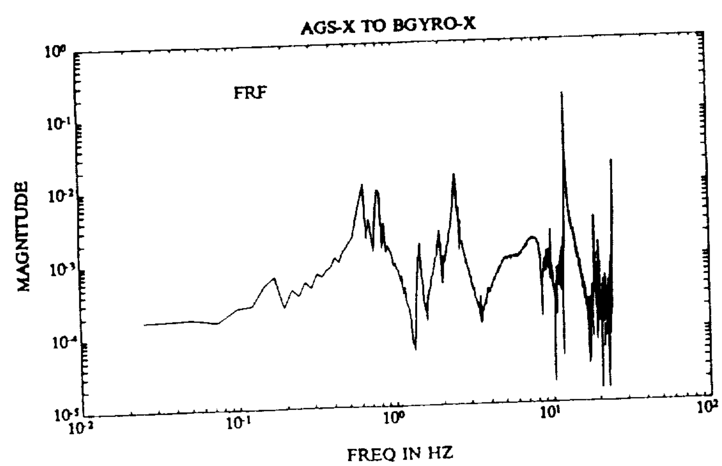
In practice control design models must include:

1. O.L., performance-significant modes
2. modes near (unity gain) cross over

* Size of such models is usually modest.
* Ignoring *control design* insights can grossly exaggerate the dimensionality problem.

To illustrate the occurrence of numerous ignorable modes consider frequency response test data and modelling for the NASA/MSFC ACES test bed*. This test bed structure actually has over 40 modes below 10 Hz, as determined via modal survey. But, as shown in this frequency response function (FRF) and corresponding Eigensystem Realization Algorithm (ERA) model, relatively few of the modes show up in the actuators-to-sensors transfer functions that contain the information pertinent to control design. This occurs because most modes are insignificant to performance and control. In fact, by appropriate control design, we manage to phase stabilize these modes so that they are ignorable in the closed-loop.





The ERA model for the AGS-X to BGYRO-X loop closely resembles the FRF generated fro test data.

* E. G. Collins, Jr., D. J. Phillips, and D. C. Hyland, "Design and Implementation of Robust Decentralized Control Laws for the ACES Structure at Marshall Space Flight Center," NASA Contractor Report 4310, Langley Research Center, July 1990.

For the reasons discussed above, real life design models are of modest dimensions. This is illustrated here by tabulation of the dimensions of models used in our NASA CSI Guest Investigator Program*. As can be seen for ACES, one can often break the problem into decentralized pieces; the size of the models for each piece may be very low indeed.

To repeat: The dimensionality required of models is best judged using *control design* insights.

A Compendium of Dimensions for Harris NASA CSI GIP Phase I Models & Controllers

Test Article	Controller	Model Order	Controller Order
ACES (Has > 40 Modes under 10 Hz)	AGS-X to BGYRO-X	17	4
	AGS-Y to BGYRO-Y	19	6
	IMC-X to DET-Y	4	3
	IMC-Y to DET-X	4	3
	Total, Decentralized	44	16
Mini-MAST	Decentralized	40	24
	Centralized	54	33

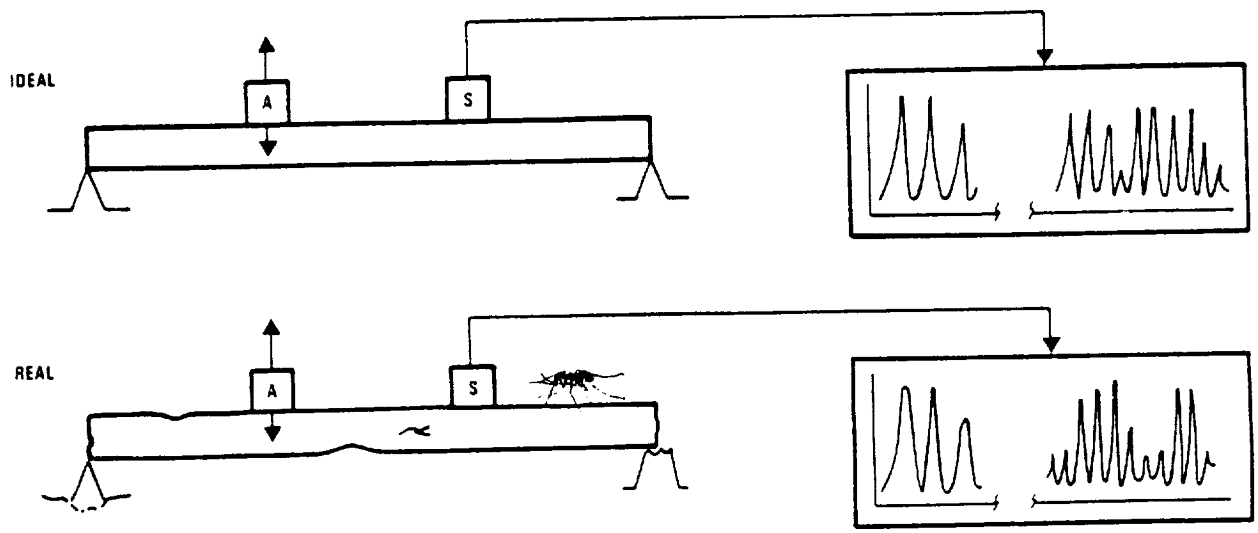
* See:

E. G. Collins, Jr., J. A. King, D. J. Phillips and D. C. Hyland, "High Performance Accelerometer-Based Control of the Mini-Mast Structure," *AIAA J. Guid. Contr. Dyn.*, Vol. 15, pp. 885-892, July 1992.

E. G. Collins, Jr., D. J. Phillips, and D. C. Hyland, "Robust Decentralized Control Laws for the ACES Structure," *Contr. Sys. Mag.*, Vol. 11, pp. 62-70, April 1991.

Next, let us discuss stability robustness and performance robustness. The motivation for a concern with robustness is illustrated here. Real structures differ from their idealizations in numerous ways, including nonuniformities in stiffness and inertia, nonideal boundary conditions, etc. Even when such errors appear to be insignificantly small, there may be a very significant impact on sufficiently high frequency dynamics. Thus we need control system robustness to deal with the sensitivity of structural model characteristics to modelling errors. But robustness with respect to what?

SENSITIVITY TO MODELLING DATA



- SLIGHT ERRORS IN PHYSICAL MODELLING – LARGE ERRORS IN HIGH ORDER MODES
- DPS MODELS CAN ENCOMPASS ONLY LIMITED INFORMATION
  - ARE SUCH MODELS MEANINGFUL WITHOUT CHARACTERIZATIONS OF UNCERTAINTY?
- WHAT INFORMATION MUST DPS MODELS REFLECT?
  - SURELY THOSE FEATURES THAT REMAIN "SHARP" DESPITE ERRORS IN DETAIL

The usual concern is robustness with respect to stability. But even when one presumes collocated actuators and sensors and, as in the reference cited in the panel, one adopts an LQG design that is positive real (hence inherently stable), one does not resolve all robustness questions. This is because robust stability does not imply robust performance, and it is reliable performance that we must ultimately secure.

To illustrate the above point, the positive real LQG design recommended in the cited reference was applied to a single mode (with a nominal value of 10 Hz for the resonant frequency). The chart at the bottom of the panel shows, for various cases, the magnitude of the transfer function from the disturbance to the structural velocity. When the model frequency assumes the 10 Hz value used in the design model, it is seen that the controller greatly attenuates the open loop response. However, a second pair of curves show the open and closed-loop frequency response magnitudes for an off-nominal value (11.5 Hz) of the frequency. In this case, the closed-loop performance is little better than the open-loop behavior. Thus, although the system remains stable, the system performance is very sensitive to modelling error. To achieve practical results that produce substantial and reliable performance benefits from active structural control, we need to secure robustness with respect to performance. This need has been appreciated for some time and some responsiveness on the part of DPS theorists is overdue.

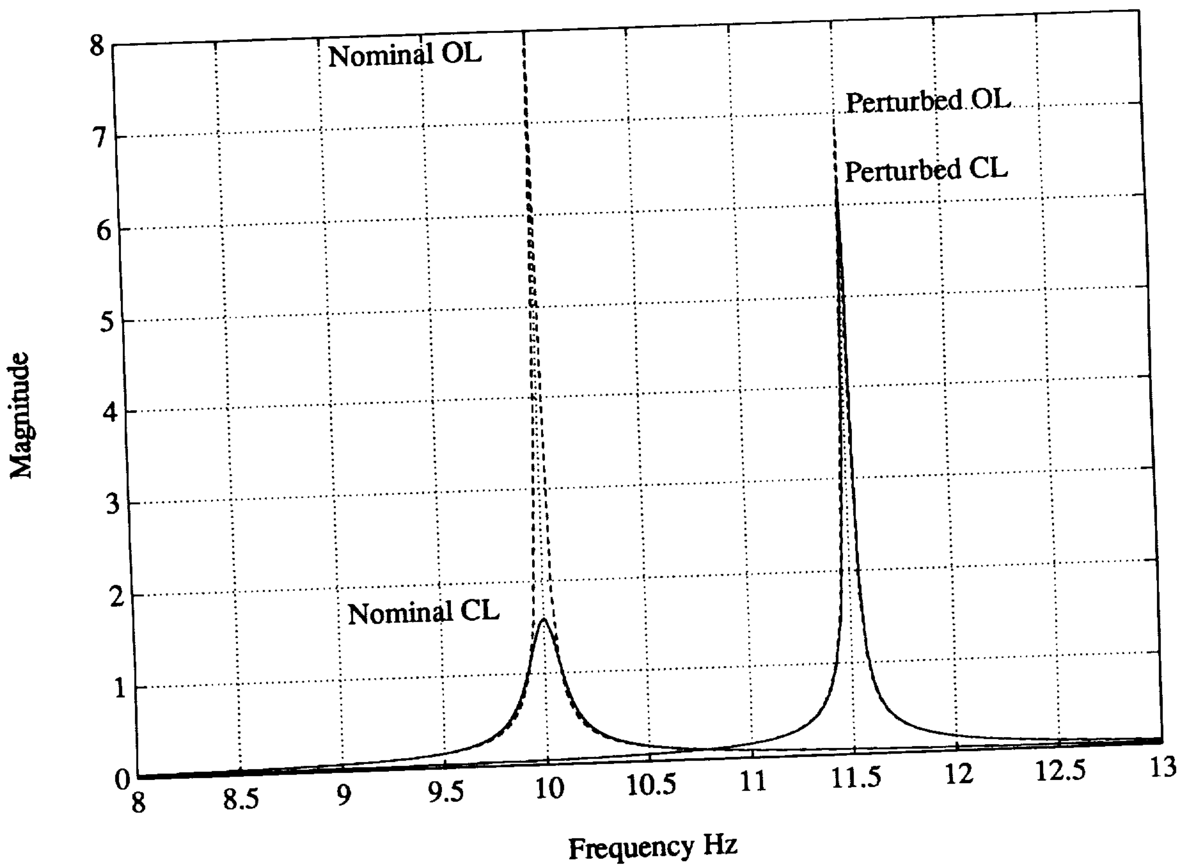
Robust Performance $\neq$ Robust Stability

Example: Positive Real LQG Control

Explicit LQG Solution (One Mode) From:

A.V. Balakrishman, *Proc. 5th NASA/DoD CSI Technology Conference,* Lake Tahoe, Nevada, March 1992.

Transfer function from disturbance to structure velocity



Note that LQR or LQG designs have been (and remain) the controls paradigms for DPS developments. Unfortunately, it has been known for quite a long time that LQG design is *not* robust* and that the complexity (dimension) of LQG controllers is often prohibitive for implementation. For these and many other reasons, control theory has moved far beyond LQG (μ-synthesis, Ω-bounds, multivariable Popov synthesis, etc). We recommend that DPS control developments need to more fully acknowledge the evolution of *control theory* over the past decade.

Model of "DPS Control Theory Results" Generation

Finite-Dimensional Setting		Semi-Group Theory, ∞-dimensional Hilbert Space
LQR or LQG design result #1	⟹ Translate assumptions & notation ⟹	DPS control result #1
LQR or LQG design result #2	⟹	DPS Control result #2
. . .		. . .

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* LQG design is *not* robust

* Complexity of LQG controllers often prohibitive

* For these and many other reasons, *control theory* has moved far beyond LQG

* For the famous counterexample, see:

J. C. Doyle, "Guaranteed Margins for LQG Regulations," *IEEE Trans. Autom. Contr.*, Vol. AC-23, August 1978, pp. 756-757.

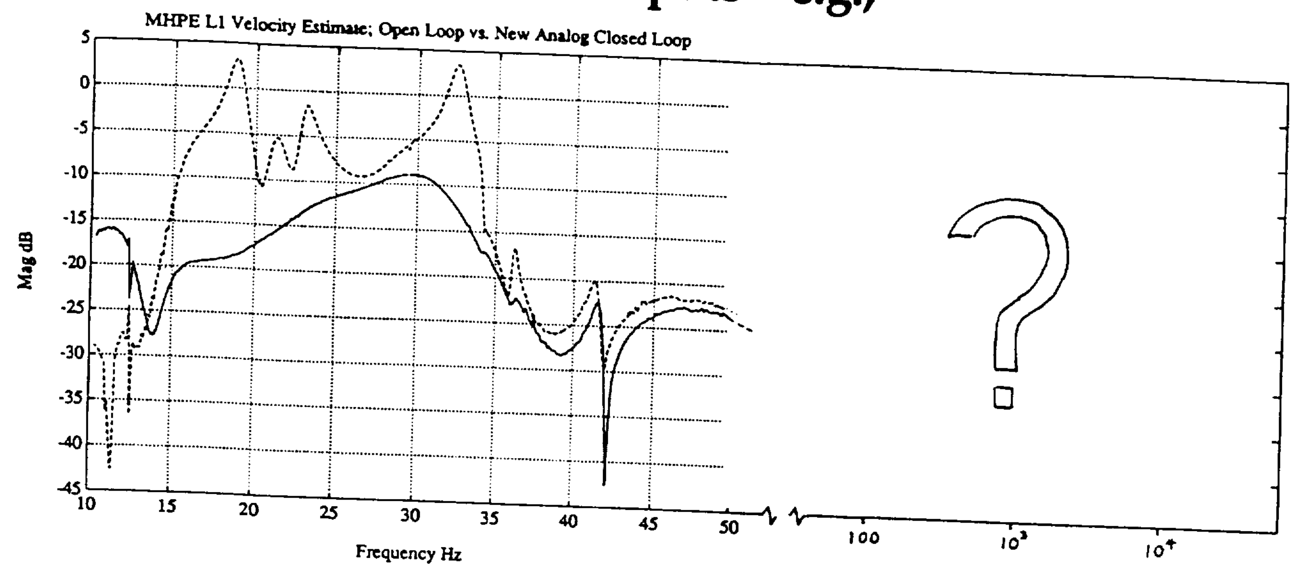
For nonrobust LQG performance in connection with realistically complex systems, see:

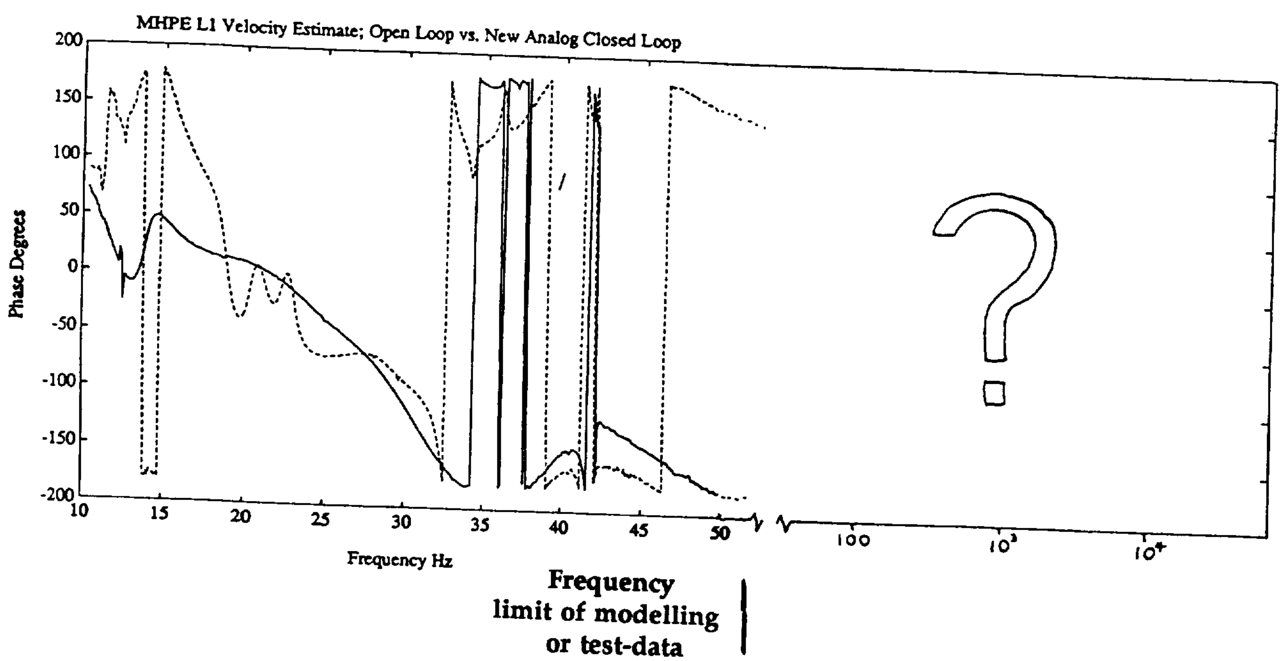
D. S. Bernstein and S. W. Greeley, "Robust Controller Synthesis Using the Maximum Entropy Design Equations," *IEEE Trans. Autom. Contr.*, Vol. AC-31, pp. 362-364, 1986.

Lest it be thought, at this point, that I would bury DPS theory, let me point out that it's a friend's part to rebuke a friend's errors. We firmly believe that there are aspects of DPS modelling that *are* pertinent to vibration control design. When all is said and done, there are people facing *real* problems in controlling real distributed parameter systems.

To link up more fully with the real world, we need to acknowledge that in actual practice, a control design model is tantamount to a complete set of transfer functions. This is rigorous if all performance variables are also sensed variables and is approximately true otherwise. Control designers want DPS theory to provide them the tools for modelling the external (frequency domain) representation of DPS. In particular, we need the capability to estimate or over-bound certain key aspects of the high frequency phenomena. The information needed is not the details of all modes but just a few critical parameters. As is clear from the following discussion, these critical high frequency parameters pertain to phenomena entirely beyond the reach of lumped-parameter models and can only be addressed via DPS theory.

Control-Design Model is Equivalent to the set of transfer functions from all actuator commands to all sensor outputs - e.g.,





We need the ability to estimate certain key aspects of the high frequency phenomena (not details of all modes but just a few critical parameters).

The most critical design information sought is: Where and how to roll-off? In this panel, we highlight specific requirements. G denotes the compensator gain matrix and b_κ and c_κ are the actuation and sensing "signature vectors" for the κ^{th} mode. In other words the vector b_κ contains the actuator modal influence coefficients for the κ^{th} mode.

The graphs show, in histogram form, various components of closed-loop modal dampings. The closed-loop modal dissipation has an inherent component, d_κ, and a control component given as $Re(b_\kappa^T G c_\kappa)$ from a small gain asymptotic approximation. At high frequency, phase goes bad (due to instrumentation, communication delay, etc.) and one needs to roll off $\|G\|$. As illustrated in the charts sketched here, the design challenge is to get from the large gain, phase-stabilizing G at in-band modes to low gain, gain-stabilizing F on out-of-band modes. The lower chart in the panel shows when this is properly done. "Rolling-off" the controller to guarantee the stability of high frequency dynamics requires key information on all modes above cross-over that can only be provided by distributed parameter models. In particular, the minimum frequency separation is needed to determine "how fast" to roll-off, while the minimum open-loop dissipation and maximum modal signature gains are essential to knowing how small $\|G\|$ must be to gain-stabilize.

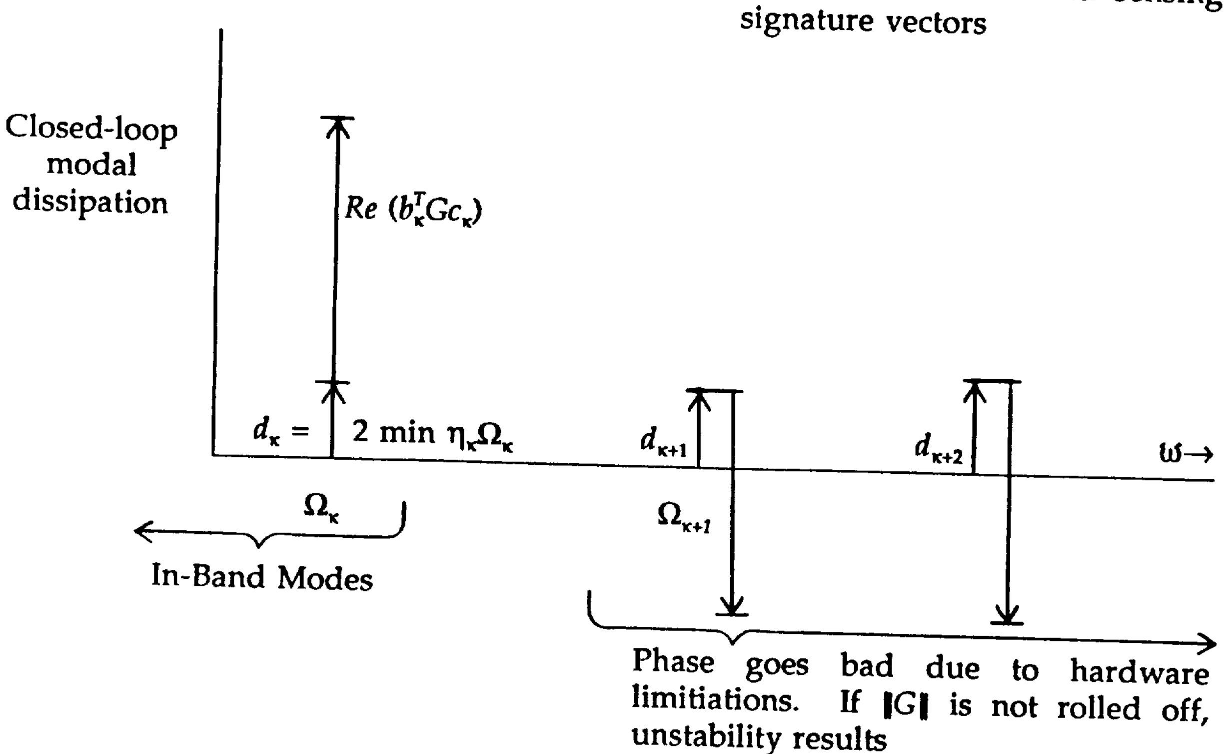
If DPS theory can respond to the challenge of illuminating key high frequency characteristics of the types described above, then a truly substantive and practically useful contribution will have been made.

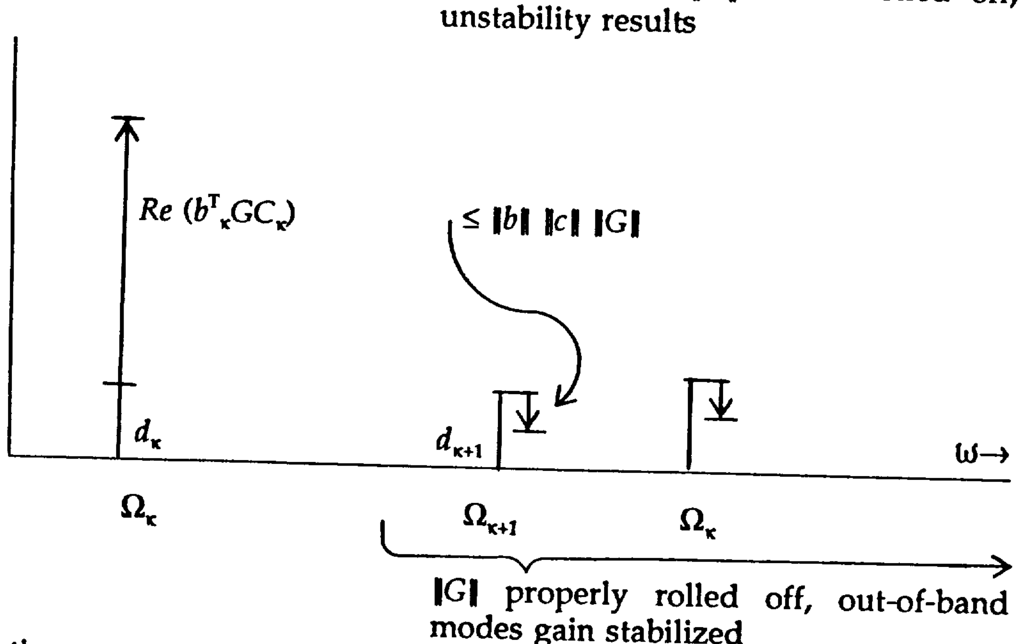
Most critical design information:

Where and how to roll-off

G ▲ compensator gain matrix (including actuator & sensor dynamics)

b_κ, c_κ ▲ κ^{th} mode actuation and sensing signature vectors

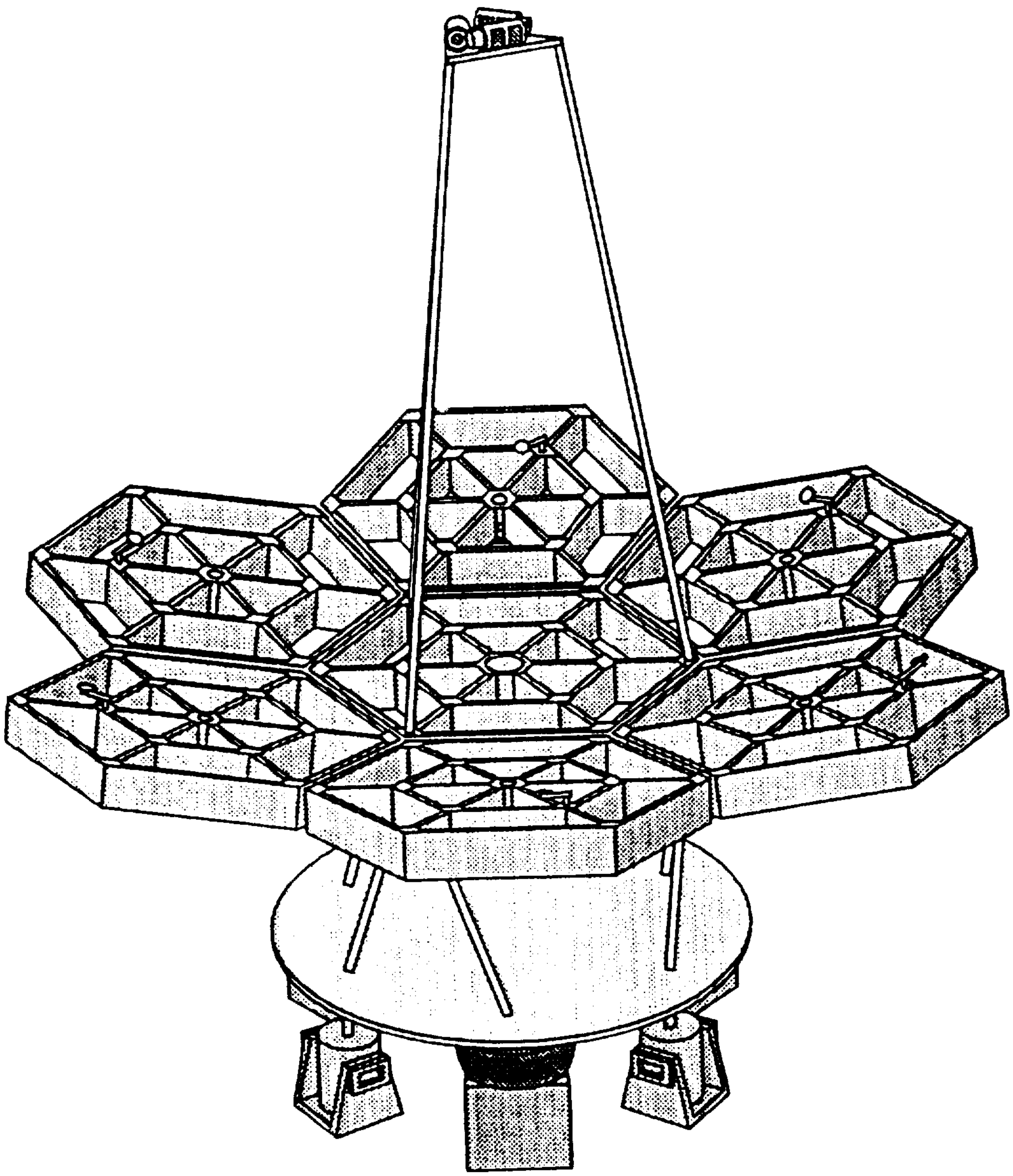




Key DPS Information:

Minimum O.L. dissipation	:	Min $\eta_\kappa \Omega_\kappa$
Minimum frequency separation	:	Min $\lvert\Omega_{\kappa+1}-\Omega_\kappa\rvert$
Maximum modal signature gains	:	Max $\|b_\kappa\|$, Max$\|c_\kappa\|$

In closing, I fling down the gauntlet! Here's the Multi-Hex Prototype Experiment (MHPE). This is one of the most "traceable" vibration control test beds. The MHPE has been operational for the past four years at Harris and is open to guest researchers. If you disagree with my criticisms, show how MHPE may be *better* modelled and/or controlled *specifically* by virtue of application of DPS theory!



THE MULTI-HEX PROTOTYPE