History of Key Technologies

Spacecraft Attitude Dynamics and Control—A Personal Perspective on Early Developments

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Introduction

WITH some trepidation I accepted the invitation to join the ranks of better authorities who have chronicled the history of key technologies in a series[1-8] published in the *Journal of Guidance, Control, and Dynamics*. My experience is more limited than that of the other contributors to this interesting collection of articles, and my narrative should be recognized as no more than the personal perspective of one observer and participant in the evolution of the field of spacecraft attitude dynamics, stabilization, and control.

The Beginnings: Spin Stabilization

In retrospect, we can see the analytical beginnings of spacecraft attitude dynamics and control in the spinning body dynamics of Euler[9] (1758) or the lunar dynamics of Lagrange[10] (1764). But for each of us personally there was another beginning, and mine was the launching of Explorer I (see Fig. 1) in 1958, when I was an impressionable twenty-one-year-old pursuing a master's degree in civil engineering at MIT. Of course, by that time, Sputnik had been in orbit for more than three months, but neither the general public nor the engineers preparing Explorer I at Jet Propulsion Laboratory knew very much about the rotational dynamics of the pioneering Russian satellite. So it was from Explorer I that we learned our first lessons in attitude stability of spinning spacecraft. We had to learn the hard way, by trying to figure out why Explorer I was unstable and unable to sustain the pattern of spin about its axis of symmetry, which was approximated in its initial conditions after injection into orbit. Within ninety minutes it was essentially tumbling end over end, and the engineers at JPL were scrambling to explain why.[11]

I discovered many years later through personal correspondence that the lessons of Explorer I need not have been discovered so painfully; we were the victims of our own security system (again!). Professor Ronald Bracewell, a radio astronomer at Stanford, had deduced from Sputnik's signals that it was in a stable spin about an axis of symmetry and, from his understanding of arguments derived initially for the rotational dynamics of galaxies, he concluded that Sputnik must be spinning about an axis of maximum moment of inertia (since this condition minimizes kinetic energy for a prescribed angular momentum). Bracewell telephoned JPL to assure himself that those responsible for the spin stabilization of Explorer I realized that it would be unstable unless its spin axis offered the maximum moment of inertia, but the cloak of secrecy surrounding the project kept Bracewell and the JPL engineers from sharing experiences. Only after the signals returning from Explorer I indicated rotational instability did Willard Wells and others at the Jet Propulsion Laboratory discover the heuristic analytical arguments that were to persuade us all for the better part of a decade that a spacecraft could spin stably only about its principal axis of maximum momentum of inertia.[11] Bracewell's deductions were published[12] in September of 1958, too late to help with Explorer I, but he (together with Owen Garriott, who much later became an astronaut) was the first in the open literature on this subject.

Actually both Bracewell and the JPL engineers were anticipated in their deductions by an old-timer at RCA named Vernon Landon, who established the same criterion for spin stabilization without benefit of data from either Sputnik or Explorer. Landon was one of those astonishingly good engineers of bygone times who missed the opportunity for even a bachelor's degree. Perhaps because he read the classical results in dynamics more critically than most college students, he realized that the textbook analyses for free rigid bodies required careful interpretation before application to spacecraft, which were of course neither wholly free of external torque nor rigid. Landon's laboratory notes reveal that he recognized in early 1957 that internal energy dissipation due to spacecraft flexibility violated the assumptions of the classical analyses of Euler and Poinsot, destroying the classical stability of spin

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about a principal axis of minimum moment of inertia. So Vernon Landon could have spared us all the embarrassing instability of Explorer I, but he made no attempt to publish and nobody asked.

The American aerospace community learned the lessons of Explorer I's instability, perhaps too well. Subsequent spin-stabilized satellites, such as the Tiros weather satellites[13] and the Syncom communications satellites,[14] were designed to spin stably about a principal axis of maximum inertia (see Fig. 2), and it was generally believed that spin stabilization about a principal axis of least inertia was impossible. Although it eventually became clear that by 1962 Vernon Landon knew that such spin stabilization was possible if enough angular momentum was stored in a relatively rigid rotor directed along the spin axis of the vehicle, his efforts to publish were thwarted by skeptical reviewers, and his insights were largely unrecognized.

In remembering our clumsy, iterative progress toward the understanding of the dynamics of rotating spacecraft, we should pause to note the reasons for what might otherwise seem to be the errors of ineptitude. We should remember that we couldn't solve the equations of motion of realistic mathematical models of spacecraft in literal form then, and we can't solve them now. Of course we could integrate the equations numerically, but that required a specific mathematical model, not a generic representation of all spinning spacecraft. Unless the analyst had the insight to postulate those characteristics of the spacecraft mathematical model that produced the surprising behavior, he had no way to discover it by digital simulation. The JPL engineers used the best results in the literature, but these results weren't good enough.

It might appear that the mathematical methods of stability analysis[15] should have provided the necessary insights, and eventually these techniques did prove somewhat useful. Perhaps the engineers working on these projects before about 1966 knew too little of the mathematical theory to apply it in useful ways, but the sufficient conditions for stability then available to the theoreticians in the form of Liapunov theorems[16] would not have been terribly useful anyway. Real progress was made only when engineers in the 1960s learned enough about the mathematics of stability analysis to extend the theory for the special case of pervasively damped mechanical systems, for which both necessary and sufficient conditions can be established.[17-20]

In the absence of generic analyses of formal validity, engineers at JPL and elsewhere devised approximate methods of analysis known generally as "energy-sink" methods.[11,12] The approach was to use the solutions appropriate for free rigid bodies (which possess an "energy integral" reflecting the constancy of kinetic energy), while simultaneously recognizing that in fact mechnical energy was being dissipated because of flexibility or internal moving parts in the spacecraft. Approximations were used to estimate the rate of dissipation of energy, and this result was used to modify the rigid body solutions. This technique was used extensively to design the internal energy dissipators ("nutation dampers") on Syncom and later satellites.[14] Although it was surely recognized that Newton's laws were being compromised, the usefulness of the result was repeatedly confirmed by digital simulations for specific cases, and later by flight experience. As a negative consequence of the pervasive use of these informal energy-sink methods, however, engineers began to accept uncritically the inference that spin-stabilization about a principal axis of least inertia was impossible.

Because the space environment is not entirely free of torque, the utility of spin-stabilized satellites was limited until means were devised to apply compensating external torques to reorient the spacecraft angular momentum vector, to which for stable configurations the spin axis converges as internal energy is dissipated. Important contributions were made by Williams and others at Hughes Aircraft Company[14] and by Fryklund, Grasshoff, and others at RCA[13] for application to the Syncom and Tiros satellite series, respectively. Syncom employed gas jets fired for brief bursts at intervals established by the spin speed, and Tiros used internal electrical currents to set up electromagnetic fields producing interaction torques with the Earth's field. Both of these torquing techniques represented good engineering solutions to original problems, illustrating very nicely the roles of ingenuity and conceptual thinking in combination with good science and the clever use of mathematical approximations to solve engineering problems.

Dual-Spin Spacecraft

Because spinning satellites provided only scanning coverage of objects of interest for communications or scientific observation, it was a natural extension to attach a "despun platform" to a spin-stabilized satellite, using a closed-loop control system with a torque motor to maintain the desired relative rotation rate of "rotor" and "platform." The first of these satellites was the Orbiting Solar Observatory (see Fig. 3), which was conceived at Ball Brothers in 1959 with the

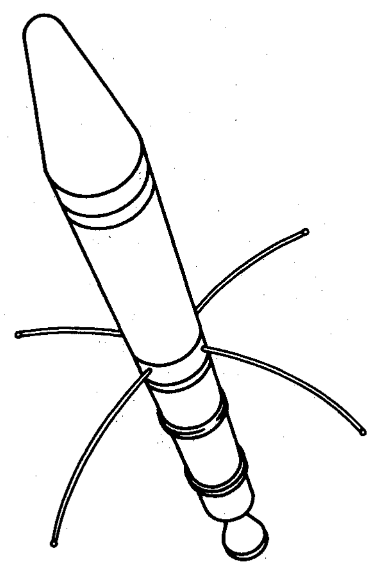

Fig. 1 Explorer I.

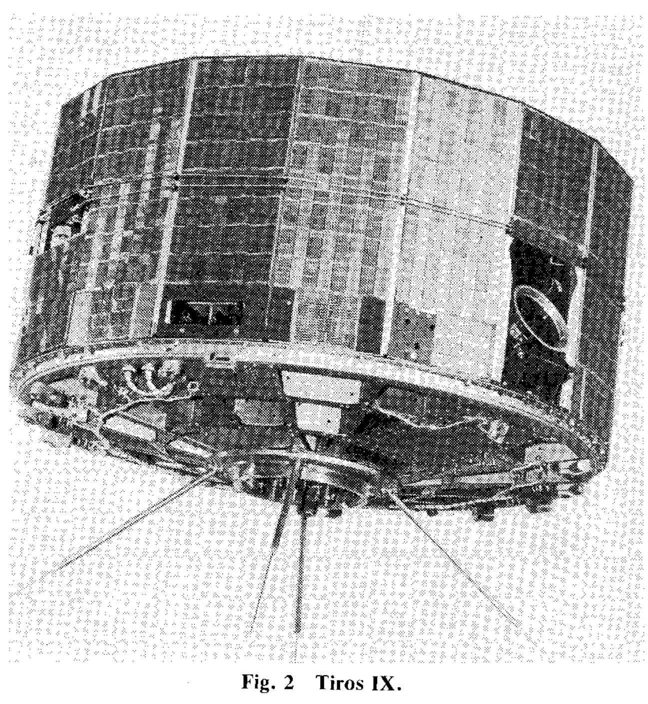

Fig. 2 Tiros IX.

maximum-inertia spin-axis rule very much in mind. (In Fig. 3 the upper half of the vehicle faces the sun while the lower half spins; the spherical tanks increase the spin-axis inertia to the maximum value for the total spacecraft. The "nutation damper" required to attenuate departures from pure spin is located on the despun platform. During part of the mission, the entire spacecraft rotated as though it were a single body.)

Of course simulations predicted the stability of the OSO configuration before launch, and flight experience confirmed this expectation. It was not until 1966 that Tom Spencer of Ball Brothers ran a digital computer simulation for an alternative that was stable despite its violation of the maximum inertia spin-axis rule, and by that time Tony Iorillo of Hughes Aircraft Company was already on record[21] with an analysis that explained the Spencer simulations.

The original impetus for introducing a despun platform at Hughes was to permit a directional antenna to be used on a spinning satellite; there was no immediate objective of circumventing the maximum inertia spin-axis rule and, indeed, in the original 1964 Hughes Interdepartmental Correspondence[22] describing his analysis, Tony Iorillo concludes with a sentence that reaffirms this constraint. Iorillo's initial analysis permitted energy dissipation or "damping" on either spinning rotor or despun platform, and only in his concluding remarks did he assume the rotor location for the damping mechanism. Iorillo soon realized that, by putting the damper on the platform, he could spin-stabilize a spacecraft of any inertia distribution, and he recognized the significance of this result for the spacecraft industry. In 1965 Iorillo presented an approximate analysis[21] that accommodates energy dissipation on both rotor and platform, clearing another major hurdle. He was ready to fly.

And where was Vernon Landon, whose initial paper on this subject was rejected in 1962? Landon had teamed with Brian Stewart to present a different approximate analysis leading to the same result, and their paper[23] was published, to scant attention, in 1964. The formulation was more restricted than Iorillo's 1965 paper, but probably that's not why Iorillo's work received more attention. The management at Hughes decided to use Iorillo's concept in a proposal for a Tactical Communications Satellite, TACSAT 1 (see Fig. 4), and suddenly everyone noticed that there was a controversial new concept of attitude stabilization in the competition.

Iorillo's arguments were intelligent but heuristic in character, and the Air Force was a skeptical customer. Hughes launched a major effort to validate the concept, with formal analysis, digital computer simulations, and physical simulations in the laboratory. Tony Iorillo handled the laboratory tests, John Velman developed the massive simulations and, as a consultant from the UCLA faculty, I tried to strengthen Tony's analytical arguments.[24] The Aerospace Corporation assigned D. L. Mingori to the stabilization concept evaluation, and his contributions augmented the work at Hughes in important ways.[25] Incidentally, in my report[26] I coined the term "dual-spin" to describe this class of spacecraft, and the term stuck. The Air Force held a symposium on dual-spin spacecraft in 1967 and, as general chairman, I brought Vernon Landon out of retirement in Florida to claim the recognition he had been denied.[27] In 1969 TACSAT 1 was launched as the first of many dual-spin spacecraft with the spin axis corresponding to the vehicle's minimum moment of inertia.

Dual-spin spacecraft have provided some of the most intriguing dynamics and control problems in my experience. Very nice work has been done by Longman and Roberson[28] and by others on the gyrostat model, which assumes that both bodies are rigid, and fascinating results emerged for nonlinear damping models on the two bodies in a study conducted by Mingori, Tseng, and myself[29,30] in an effort to explain TACSAT 1 flight anomalies.[31] Coupling between the spin-axis control system and the passive attitude stabilization system may occur for certain inertia distributions or for flexible vehicles, as demonstrated by Velman[32] and others at Hughes. Mark Scher at TRW was a central player in an amazing story of the rescue and recovery of a dual-pin satellite that lost its despun platform control in orbit.[33] A lovely book could be written on this subject if a writer could be found who had a free year and no concerns about the number of people who would read it. Unfortunately, such books are seldom published today. Nor is this journal the place for such an excursion.

Gravity-Stabilized Satellites

I have no firsthand experience with Earth-pointing satellites stabilized in that orientation by the gravity field, but the subject has too many intriguing aspects to be entirely ignored in this little review.

Of course, the first Earth satellite in this class is the moon, and it had been studied extensively long before man launched his first artificial Earth satellite. In 1764, Lagrange[10] conducted an analysis of the rotational dynamics of the moon and concluded from the stability of its Earth-pointing orientation (rotating in inertial space once per orbit) that it must have a principal axis of maximum moment of inertia normal to the plane of its nearly circular orbit. (Incidentally, in solving this problem, Lagrange developed the basic equations of dynamics we know today as Lagrange's Equations and established the foundations on which Liapunov and others erected a theory for stability analysis of differential equation solutions.)

But not many of us were reading Lagrange in the 1950s in America, so much of his work was created anew. I'm told that the original proposals for Earth-pointing satellites relying upon gravity torques for their orientation are buried in the classified literature of the 1950s; work by Breakwell and others at the old North American Aviation (now Rockwell International) is often cited informally. It would be interesting to hear from these pioneering engineers about their early analyses and motivations, but I can shed no light.

I have the impression that it took us a long time to catch up with Lagrange, because the publication[34] in 1961 of a linearized stability analysis for Earth-pointing orientations of rigid

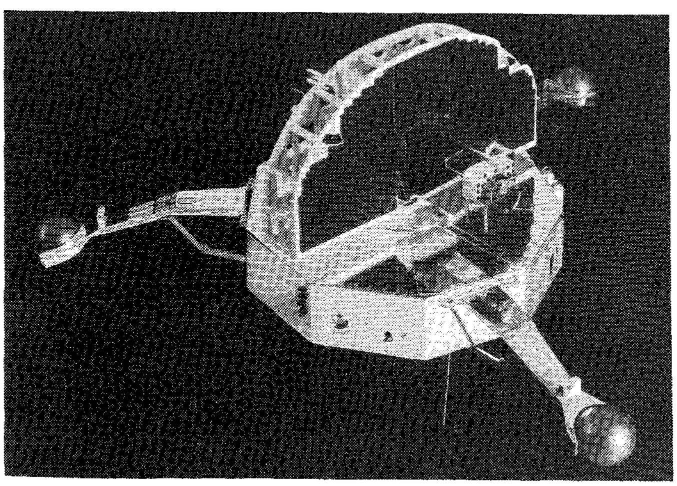

Fig. 3 Orbiting Solar Observatory OSO-1.

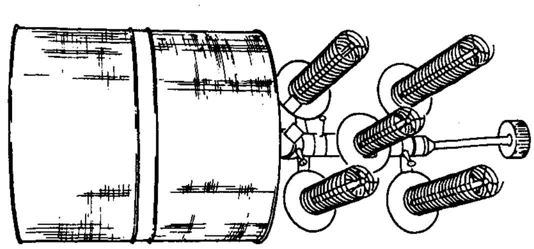

Fig. 4 TACSAT.

bodies in circular orbit demonstrated that we had not yet matched Lagrange's understanding of the problem.

I think it's fair to say that, in the early 1960s, very few people with doctorates in the mechanically oriented disciplines of engineering understood the limitations of linearization as a technique in motion stability analysis; the fundamental work on stability theory by Liapunov[16] and others was familiar in this country only to those working in certain areas of mathematics and control theory. (The Russians[35] seemed better prepared in this respect.) The flood of conceptually new problems introduced by space exploration caught many of us with educational deficiencies that we scrambled to repair, relying sometimes on the classical literature in dynamics and sometimes on more contemporary literature in such disciplines as mathematics and controls.

Of course, World War II had a similar impact on engineering education; physicists and mathematicians were often better prepared to meet the engineering challenges of that era than we were in the engineering disciplines. I hope we've made progress in engineering education since that time.

As often happens, the most exciting problems in the gravity-stabilized satellite field were discovered through unhappy experience. Our capacity to anticipate the behavior of a satellite is limited principally not by deficiencies in our understanding of science and mathematics but by our powers of imagination and insight. Thanks to the digital computer, we can simulate mathematically the behavior of any dynamical system that we can model properly, but special talents are required to incorporate the salient features of a satellite in the preflight simulation models. Rarely do we succeed entirely, and that's when the excitement begins.

A desirable dynamical feature of a gravity-stabilized satellite is obtained by incorporating large disparities in size among the three principal axis moments of inertia of the vehicle. As a practical matter, this characteristic is most readily achieved by deploying appendages from a spacecraft to alter (and greatly enlarge) certain inertias. The mechanical device of choice for such deployment was an open-section tube of elastic metal that could be flattened out and stored during launch on a spool. Figure 5 illustrates a gravity-stabilized satellite (OV1-10) with six such tubes or "booms" which, when deployed to their full length, utterly dominate the rotational inertias of the satellite.

The original analyses of gravity-stabilized satellites were for rigid bodies in circular orbit in an inverse square gravity field. The only torques on the satellite were those due to the gravity gradient (so these are often called "gravity gradient satellites").

Gravity-stabilized satellites such as OV1-10 were extremely flexible. Moreover, they were designed in such a way that rotational vibrations (librations) of the satellite would induce relative motions of satellite components called libration dampers and consequent energy dissipation for the attenuation of oscillations. The mathematical modeling of such satellites for preflight simulation presented major challenges, particularly if the anticipated deflections of the booms exceeded the limitations of linearized small deflection theory or if the orbit was noncircular.

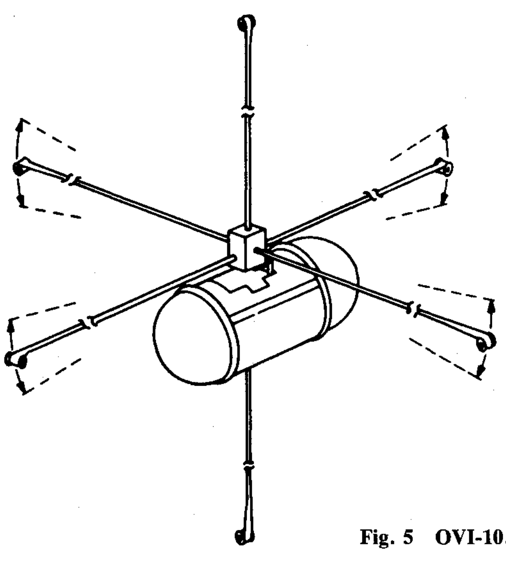

Fig. 5 OVI-10.

An enormous amount of effort was invested in developing simulation programs for nonrigid spacecraft with discrete damping and large relative motion of components,[36,37] and the initial stimulus for this work came in part from the challenges of the gravity-stabilized satellite.[38,39] (In fact I'm still working in this area.[40]) But these programs failed to anticipate the behavior of OV1-10 and its brethren, which surprised us all.

After apparently beginning to settle down into its desired Earth-pointing orientation, the satellite OV1-10 became so excited in its rotations that it "flipped over," rotating 180 degs. Again its oscillations began to decay, but then it seemed to come to life, repeating its pattern of alternating violent and quiescent motions.

Other satellites with deployable open-section booms manifested similar instabilities, including the actively controlled Orbiting Geophysical Observatory OGO III (see Fig. 6). It finally became apparent that the problem was the booms, not the gravity-stabilization concept.

The explanation of these several anomalies in flight performance came to my view at the conference on "gravity gradient" satellites sponsored by the Air Force in 1968. I remember most clearly the hypothesis then advanced by Henry Hoffman of Goddard and subsequently developed by Harry Frisch of Goddard[41] and by Gary Connell and Val Chobotov of the Aerospace Corporation.[42]

It seems that thermal deformations of the open-section booms induced by the sun were producing not only benign planar bending of the booms but also twisting rotations of the booms, as one must expect from the bending-torsion coupling associated with open sections. Rotational motions at the boom tips caused counterrotations of the satellite core body and changing aspect to the sun. The resulting "thermal flutter" caused the observed instabilities.

This is a complicated phenomenon to model for simulation and, before that job was done, the engineering problem was solved: the deployable booms were redesigned so that the flexible metal on the spool came together in closed-section tubes (called "zippered booms"). Much later I saw the report by Frisch[41] that described successful efforts to model the open-section boom for thermal flutter analysis.

A wholly different but equally interesting feature of gravity-stabilized satellites stimulated a series of papers in the literature but, to my knowledge, no flight experience. Tom Kane[43] discovered, by a numerical approach to stability analysis of rigid satellite librations, that even satellites that are stable in an Earth-pointing orientation may be unstable for small librations. John Breakwell and Ralph Pringle[44] interpreted this result in terms of nonlinear resonance, as did Gordon Reiter[45] independently. I had hoped that practical use could be found for such resonances in libration attenuation,[46]

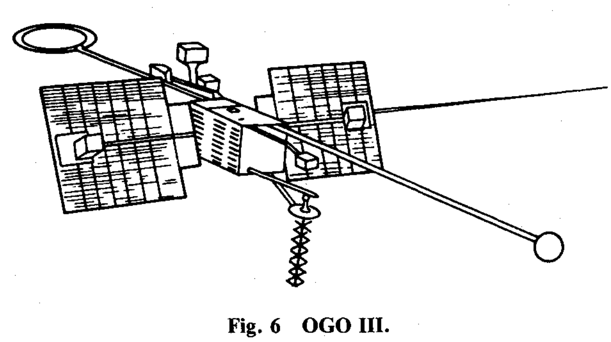

Fig. 6 OGO III.

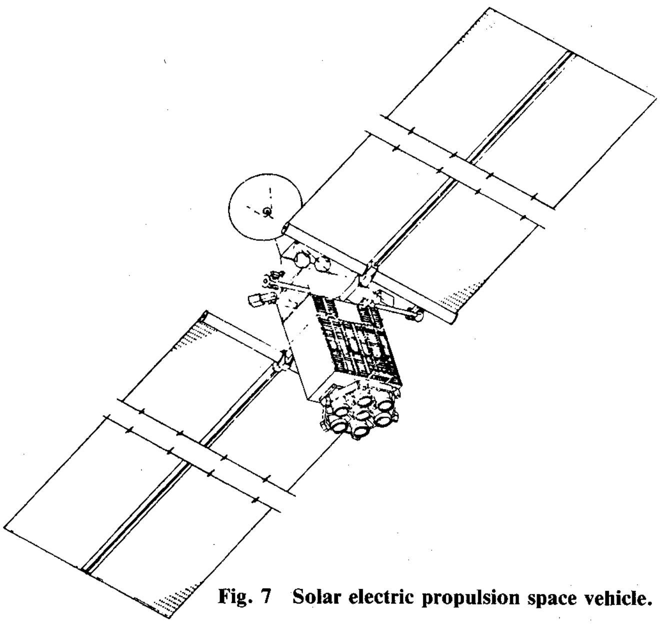

Fig. 7 Solar electric propulsion space vehicle.

but I am not aware of any flight applications of this analytically sophisticated idea.

Gravity stabilization as the sole means of satellite orientation is probably too primitive a concept for the future, but this idea may be useful for future satellites, such as the space station, in periods of limited activity.

Actively Controlled Spacecraft

Because of the bias that comes from my background in dynamics, I have little to contribute to the discussion of spacecraft control problems amenable to representation as the control of three uncoupled planar rotations of rigid bodies about principal axes. This kind of a plant model was good enough for many of the early spacecraft subject to "three-axis control" by gas jets or reaction wheels, and the control system engineers on those projects concentrated on the control logic and its implementation with sensors and actuators. Attitude control responsibilities then typically resided with electrical engineers, and they performed very well on their own until the dynamics of the spacecraft became too complex for their $1/Is^2$ plant transfer functions. As spacecraft became more complex, with moving parts and significant flexibility, attitude control (and sometimes "shape control") became the responsibility of teams of engineers representing structures, dynamics, and control. It is to this class of problem that I will limit these remarks.

Actually my first personal experience with flexible spacecraft control involved a dual-spin spacecraft,[47] which has active control about only one axis as required for pointing the "despun platform." (Refer again to Fig. 4.) Although there is but one control axis, and hence a single scalar motor torque to prescribe, the system is complex dynamically if the platform flexibilities, damping, or inertia characteristics couple the actively controlled rotation of the platform with the passively stabilized rotations transverse to the rotor axis. This is a relatively simple control problem for a relatively complex physical plant, and that was a good place for this erstwhile civil engineer to start learning about controls.[48]

Of course, civil engineers generally work with nonrigid structures that can be treated as elastic bodies, with no discrete dampers or components subject to large relative translations or rotations. Except for the superposition of large translations and rotations in inertial space, most rockets, missiles, and airplanes can be treated in the same way. Engineers in these fields can work with linearized equations having certain properties of symmetry that facilitate transformation to distributed or modal coordinates describing "normal modes" of the entire physical system. For the flexible dual-spin spacecraft, many of these advantages are lost.

I won't attempt to reconstruct here my own learning process or the parallel evolution of the spacecraft control community. I learned largely from and with such colleagues at JPL as Gerald Fleischer and Bert Marsh and such student/colleagues at UCLA as Bob Skelton and Vic Larson. But all that matters to posterity is the reminder that we adapted our analytical methods to the evolving spacecraft, not the other way around.

When spacecraft were proposed that combined distributed flexibility of some components with discrete dampers and large relative rotations of other components, we devised ways to describe the system dynamics in terms of combinations of the discrete coordinates familiar in controls and the distributed coordinates familiar in structures to obtain a "hybrid coordinate" formulation.[47] This approach proved useful for the Viking, for example.[49]

When we could no longer incorporate component flexibility by modifying transfer functions for the application of "classical" control theory, we shifted to state equations and "modern" control theory. We used the solar electric propulsion spacecraft to test this approach, but it was never launched[50] (see Fig. 7).

When we realized that the modern controls texts assumed that the plant model could be incorporated into the control system logic in the flight computer, while this was impossible in our application, we struggled to extend the theory in order to reduce its sensitivity to modeling errors or omissions.[51,52]

Our spacecraft attitude control task is by no means complete; in many ways the challenges looming now seem larger than ever, and more critical to mission performance. But *my* task is simply to tell the story of the past as I see it. And that task is done.

Comment: Implications for Engineering Education

In thinking about the lessons of experience in this story of the evolution of spacecraft attitude controls technology, I realize that there are subtle and even conflicting messages for engineering education.

Progress in this field has occasionally been impeded by deficiencies in the education of the engineers responsible for new developments. The transfer of knowledge from one field of specialization to another has often proved inadequate, and this has been particularly troublesome in the flow between the various branches of engineering and the underlying branches of mathematics. Certainly I would encourage a more open and receptive attitude toward the movements of thought and people across the boundaries that separate the many disciplines of science and engineering.

While this story reminds us that some of our stumbling progress would have been facilitated by better sharing of existing knowledge, we see even more clearly that most of the lessons gained by painful experience could *not* have been learned more easily with more and better science and mathematics. Most of our costly mistakes are the results of conceptual deficiencies reflected in inadequate mathematical models of physical systems, not in improper applications of physical laws or mathematical methods. We are the victims of our deficiencies of imagination and conceptualization more often than of failures in our powers of analysis and computation. There are implications for engineering education here, too.

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