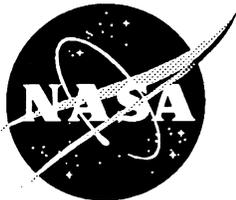


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ISSUES IN NASA PROGRAM AND PROJECT MANAGEMENT



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edited by

Francis T. Hoban

Program Manager

NASA Program and Project Management Initiative



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Issues in NASA Program and Project Management

A Collection of Papers on Aerospace Management Issues

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Summer 1993

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SP-6101(06) *Issues in NASA Program and Project Management* is sixth in a series from NASA's Program and Project Management Initiative. This series is collected and edited by Francis T. Hoban with Dr. William M. Lawbaugh. Statements and opinions are those of the authors and do not represent official policy of NASA or the U.S. Government. Useful and enlightening material is welcome, and diversity of ideas is encouraged.

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But What Will It Cost? The Evolution of NASA Cost Estimating

by Joseph W. Hamaker

Within two years of being chartered in 1958 as an independent agency to conduct civilian pursuits in aeronautics and space, NASA absorbed either wholly or partially the people, facilities and equipment of several existing organizations. These included, most notably, the laboratories of the National Advisory Committee for Aeronautics (NACA) at Langley Research Center in Virginia, Ames Research Center in California, and Lewis Research Center in Ohio; the Army Ballistic Missile Agency (ABMA) at Redstone Arsenal Alabama, for which the team of Wernher von Braun worked; and the Department of Defense Advanced Research Projects Agency (ARPA) and their ongoing work on big boosters.¹

These were especially valuable resources to jump start the new agency in light of the shocking success of the Soviet space probe Sputnik in the autumn of the previous year and the corresponding pressure from an impatient American public to produce some response. Along with these inheritances, there came some existing systems engineering and management practices, including project cost estimating methodologies. This paper will briefly trace the origins of those methods and how they evolved within the Agency over the past three decades.

■ The Origins of the Art

World War II had caused a demand for military aircraft in numbers and in models that far exceeded anything the aircraft industry had even imagined before. While there had been some rudimentary work from time to time² to develop parametric techniques for predicting cost, there was certainly no widespread use of any kind of cost estimating beyond a laborious build-up of work hours and materials. A type of statistical estimat-

ing had been suggested in 1936 by T. P. Wright in the *Journal of Aeronautical Science*.³ Wright provided equations which could be used to predict the cost of airplanes over long production runs, a theory which came to be called the learning curve. By the time the demand for airplanes had exploded in the early years of World War II, industrial engineers were happily using Wright's learning curve to predict the unit cost of airplanes when thousands were to be built (and it's still used today though the quantities involved are more likely to be hundreds instead of thousands).

In the late 1940s the Department of Defense and especially the U.S. Air Force were studying multiple scenarios of how the country should proceed into the new age of jet aircraft, missiles and rockets. The Air Force saw a need for a stable, highly skilled cadre of analysts to help with the evaluation of these alternatives and established the Rand Corporation in Santa Monica, California, as a civilian "think tank" to which it could turn for independent analysis. Rand's work represents some of the earliest and most systematic published studies of cost estimating in the airplane industry.

Among the first assignments given to Rand were studies of first and second generation ICBMs, jet fighters and jet bombers. While the learning curve was still very useful for predicting the behavior of recurring cost, there were still no techniques other than detailed work-hour and material estimating for projecting what the first unit cost might be (a key input to the learning curve equation). Worse still, no quick methods were available for estimating the nonrecurring cost associated with research, development, testing and evaluation (RDT&E). In the defense business in the early to mid-1950s, RDT&E had sud-

Center (GSFC) as another development center. GSFC was assigned responsibility for Earth orbital science satellites and soon had on the drawing board a number of spacecraft for which cost estimates were needed. The Orbiting Astronomical Observatory, the Orbiting Geophysical Observatory and the Nimbus programs were all started early in the 1959-60 period and, like most other projects in the Agency at the time, experienced significant cost growth. GSFC organized a cost group to improve the estimates, first under Bill Mecca, and later managed by Paul Villone. In 1967 Werner Gruhl joined the office where he implemented numerous improvements to the GSFC methods. In later years he joined the Comptroller's office at NASA Headquarters as NASA's chief estimator.

Among the improvements creditable to GSFC during the late 1960s and early 1970s were: 1) spacecraft cost models that were sensitive to the number of complete and partial test units and the quality of the test units; 2) models devoted to estimating spacecraft instruments; and 3) the expansion of the database through the practice of contracting with the prime contractor to document the cost in accordance with NASA standard parametric work breakdown structures (WBS) and approaches.¹⁴

By 1965 most of NASA's contractors were revising their traditional approach to cost estimating, which had relied upon the design engineers to estimate costs, replacing it with an approach that created a new job position—that of trained parametric cost estimators whose job it was to obtain data from the design engineers and translate this information into cost estimates using established procedures.¹⁵ At essentially the same time, cost estimating was being elevated to a separate discipline within NASA Headquarters and at the NASA field centers. This trend toward cost estimating as a specialization was caused by several factors. First, it was unrealistic to expect that the design engineers had the interest, skills and resources necessary to put together good cost esti-

mates. Second, during the preceding three years, the pace of the Gemini and Apollo programs had so accelerated that the Requests for Proposals issued by the government typically gave the contractors only 30 days to respond—only parametricians had any hope of preparing a response in this short amount of time. Third, because of growing cost overrun problems, NASA cost reviews had increased notably and the reviewers were looking for costs with some basis in historical actuals—essentially a prescription for parametric cost estimating.

At both MSC and MSFC, the cost estimating function was placed in an advanced mission planning organization. At MSC, it was embodied within Max Faget's Engineering and Development Directorate,¹⁶ and at MSFC it was within the Future Projects Office headed by Herman Koelle.¹⁷ Faget, an incredibly gifted engineer, had already left his imprint on the Mercury, Gemini and Apollo programs, and was a strong believer in an advanced planning function with strong cost analysis. Koelle, a German engineer who, though not a member of the original team, had later joined von Braun, was also extremely competent and very interested in cost. Koelle had, in fact, along with his deputy William G. Huber, assembled the very first NASA cost methodology in 1960, published first in an inhouse report¹⁸ and then in 1961 as a handbook that Koelle edited for budding space engineers.¹⁹

Out of the eye of the Apollo hurricane for the moment, both the MSFC and the MSC cost personnel now sought to regroup and attempt to make improvements in capability. In 1964 MSFC contracted with Lockheed and General Dynamics²⁰ to develop a more rigorous and sophisticated cost modeling capability for launch vehicle life cycle cost modeling. This effort was led by Terry Sharpe of MSFC's Future Projects Office. Sharpe, an Operations Research specialist interested in improving the rigor of the estimating process, led the MSFC estimating group as they managed the contractor's development of the

model and then brought it in-house and installed the model on MSFC mainframe computers.

Through about 1965 the only computational support in use by NASA estimators was the Freidan mechanical calculator. By the mid-1960s mainframe time was generally available, and by the late 1960s the miracle of hand-held, four-function electronic calculators could be had for \$400 apiece—one per office was the general rule. Throughout the early 1970s the hand-held calculator ruled supreme. By the middle 1970s IMSAI 8080 8-bit microcomputers made their appearance. Finally, by the late 1970s the age of the personal computer had dawned. Estimators, probably more than any other breed, immediately saw the genius of the Apple II, the IBM PC and the amazing spreadsheets: Visicalc, Supercalc and Lotus 1-2-3. Civilization had begun.

The resulting capability was extremely ambitious for the time, taking into account a multitude of variables affecting launch vehicle life cycle cost. The model received significant notoriety, and once the CIA inquired if the MSFC estimators might make a series of runs on a set of Soviet launch vehicles. Busy with their own work, the estimators demurred. The CIA pressed the case to a higher level manager, a retired Air Force colonel. Suddenly the MSFC estimators discovered that they had been mistaken about priorities. The runs were made and the CIA analysts went away happy.

Later in 1964 after a reorganization, management of the MSFC cost office was taken over by Bill Rutledge who went on to lead the MSFC cost group for more than 20 years. Rutledge steadily built the MSFC cost group's strength until it was generally recognized in the late 1960s as the strongest cost organization within the Agency. One of Rutledge's more outstanding innovations was the acquisition of a contractor to expand and maintain an Agency-wide cost database and develop new models. The REDSTAR (Resource Data Storage and Retrieval) database was begun

in 1971 and is still operational today, supporting Agency-wide cost activities. The contract was originally awarded to PRC and, under Rutledge's management, developed numerous models throughout the 1970s and 1980s.

MSFC also established a grassroots cost estimating organization within the MSFC Science and Engineering laboratories. This group was managed by Rod Stewart for a number of years. After his retirement from NASA, Stewart, along with his wife Annie, authored an outstanding series of cost estimating books.²¹ In 1966, MSC, working in parallel to the MSFC activities, contracted with General Dynamics²² and Rand²³ to improve their spacecraft estimating capability. The MSC cost group also significantly improved their capabilities during this period under the very able management of Humboldt Mandell, who was later to play a leading role in the Shuttle, Space Station and Space Exploration Initiative cost estimating activities.

By 1967 both the MSC and MSFC cost estimating organizations were beginning to obtain the first historical data from the flight hardware of the Apollo program. This included cost data on the Saturn IB and Saturn V launch vehicles by stage, and on the Command and Service Module (CSM) and the Lunar Excursion Module (LEM) at the major subsystem level. Fairly shallow data by today's standards, it was considered somewhat of a windfall to the NASA estimators who had been struggling along with two- and three-data point CERs at the total system level. The Project Offices at MSC and MSFC compiled the data between 1967 and 1969 and documented the results in the unpublished "Apollo Cost Study" (preserved today in the JSC and MSFC cost group databases). Eventually this was supplemented by paying the CSM prime contractor to retroactively compile the data in a WBS format useful for parametric cost estimating.²⁴ Despite these improvements, one Rand report in 1967 laments that the number of data points for cost estimating was "depressingly low. . . only one

1972 under contract to the winning prime contractor, North American—though this did not end the debate over the worthiness of the project.³³ All through 1973 NASA was very involved in extensive “capture/cost” analyses to produce data to answer Congressional, GAO and OMB inquiries about the Shuttle’s economic forecasts. These analyses were NASA inhouse extensions of the work done by Mathematica, Lockheed and Aerospace. The studies consumed most of the resources of the MSFC and JSC cost groups as well as Headquarters program office personnel. They compared the discounted life cycle costs of “capturing” the NASA and DOD payloads with the Shuttle versus expendable launch vehicles. The Shuttle case was finally determined to yield a 14 percent internal rate of return and \$14 billion of benefits (in 1972 dollars). This data was used as the final reinforcement of the Shuttle program commitment.

■ Declining Budgets, Rising Costs

Once Shuttle development was safely underway by 1974, most of the estimating talent of the Agency was turned to various kinds of scientific satellite estimating. As NASA’s budget declined in the 1970s, both JPL and GSFC pioneered such economies as the use of the protoflight concept in spacecraft development. Before the 1970s NASA had prototyped most spacecraft (i.e., built one or more prototypes which served as ground test articles) before building the flight article. In the protoflight approach, only one complete spacecraft is built, which serves first as the ground test article and is then refurbished as the flight article. The protoflight approach theoretically saves money. However, these savings must be balanced against the cost of refurbishing the test article into a state ready for flight, the cost of maintaining more rigid configuration control of the ground test article to insure its eventual flight worthiness, and the increased risk of having less hardware.

Other attempts were made to lower cost without much success. Low estimates based on wishful

thinking concerning off-the-shelf hardware and reduced complexity proved unrealistic, and overruns began to breed more overruns as projects underway ate up the funds other projects had expected.

Meanwhile, as NASA Headquarters continued to guide the overall programs, handle the political interfaces, foster other external relations, and integrate and defend the Agency budget, a need was seen to strengthen the Washington cost analysis function.³⁴ Having moved to the Headquarters Comptroller’s Office from GSFC in 1970, Werner Gruhl set up an independent review capability under Mal Peterson, an assistant to the Comptroller. Gruhl aggressively championed the constant improvement of the database. Gruhl and Peterson’s greatest contribution was probably their relentless urging for realistic estimates. They also initiated an annual symposium for all NASA estimators and were instrumental in helping to establish a process for Non-Advocate Reviews (NARs) for potential new projects.

The NAR was instituted as a required milestone in which each major new project had to prove its maturity to an impartial panel of technical, management and cost experts before going forward. As part of the NAR process, Peterson and Gruhl, working with a relatively small staff of one to three analysts, undertook to perform independent estimates of most of the major new candidates for authorization. Peterson largely devoted himself to penetrating reviews of the technical and programmatic readiness, the underpinning of the cost estimate. Gruhl, using mostly models of his own developed from the REDSTAR database, generated his own estimates. Together they were a formidable team and undoubtedly reduced the cost overrun problem from what it would have been without the NAR.

Another significant milestone in cost estimating that occurred during the 1970s was the emergence of the Price Model. First developed within RCA by Frank Freiman, the model began to be market-

ed in 1975 by RCA as a commercially available model. Freiman's brainchild was arguably the single most innovative occurrence in parametric cost estimating ever. His genius was to see hardware development and production costs as a process governed by logical interrelationships between a handful of key variables. Probably feeling his way with intuition and engineering experience more than hard data, Freiman derived a set of algorithms that modeled these relationships. The resulting model could then be calibrated to a particular organization's historical track record by essentially running the model backward to discover what settings for the variables gave the known cost. Once calibrated, the model could be run forward using a rich set of technical and programmatic factors to predict the cost of future projects. While the Price models are applicable to a wide range of industries in addition to aerospace, the model first found use in the aerospace industry. NASA encouraged Freiman to market his invention, and actually provided him with data for calibrating the model after observing its potential in Shuttle cost estimating.³⁵ The success of the Price model inspired the development of several other commercial cost models with application to hardware, software and the life cycle.

By the late 1970s and into the mid-1980s, the cost of NASA projects was a serious problem. It was now obvious that Shuttle payloads cost more, not less, than payloads on unmanned vehicles. Overruns were worse than ever despite better databases, better models, better estimators, and more stringent Headquarters reviews. It seemed that NASA was in danger of pricing itself right out of business.³⁶ At JSC, Hum Mandell, assisted by Richard Whitlock and Kelly Cyr, initiated analyses of this problem. Making imaginative use of the Price model,³⁷ they found that NASA's culture drives cost and that the complexity of NASA projects had been steadily increasing, an idea also advanced by Gruhl. Mandell argued persuasively to NASA management for a change in culture from the exotically expensive to the affordable. At the same time, he argued that estimates of

future projects needed to account for the steadily increasing complexity of NASA projects.

■ Recent Years

Once the Space Shuttle had begun operations, NASA turned its attention once again to defining a Space Station. After Pre-Phase A and Phase A studies had analyzed several configurations, in 1983 NASA ran a Washington-based, multi-center team called the Configuration Development Group (CDG) to lead the Phase B studies. The CDG was led by Luther Powell, an experienced MSFC project manager. For his chief estimator, Powell chose O'Keefe Sullivan, a senior estimator from the MSFC cost group. Sullivan had just completed managing the development of the PRC Space Station Cost Model,³⁸ an innovative model that created a Space Station WBS by cleverly combining historical data points from parts of the Shuttle Orbiter, Apollo modules, unmanned spacecraft and other projects. This model was distributed and used by all four of the Work Package Centers and was probably the most satisfactory parametric cost model ever developed by NASA. Work Package 1 (WP-1) was at MSFC, with responsibility for the Station modules; WP-2 was at JSC with responsibility for truss structures, RCS and C&DH; WP-3 was at LeRC with responsibility for power; and WP-4 was at GSFC with responsibility for platforms. Sullivan used the model to estimate the project at between \$11.8 and \$14 billion (in 1984 dollars). The content of this estimate included the initial capability, eight-person, 75-kilowatt station and space platforms at two different orbital locations, with additional dollars required later to grow the program to full capability.³⁹

Meanwhile, NASA Administrator Jim Beggs had been negotiating with the OMB for support to start the project. Under pressure to propose something affordable, Beggs committed to Congress in September 1983 that a Station could be constructed for \$8 billion, a rather random number in light of the known estimates and the fact that the con-

ceptual design had never settled down to an extent necessary for a solid definition and cost estimate. Nevertheless, the Agency pushed ahead with the Phase B studies and by fall 1987, needing to narrow the options in configurations still being debated between the Centers, established a group called the Critical Evaluation Task Force (CETF), quartered at LaRC and led by LaRC manager Ray Hook. Hook brought Bill Rutledge in from MSFC to lead the cost analysis effort, and Rutledge assembled a team made up of estimators representing the Work Package Centers and Headquarters (Bill Hicks, Richard Whitlock, Tom LaCroix, and Dave Bates). Over a period of a few intense weeks, they generated the cost of the new baseline, which, even after significant requirements had been cut, still totaled at least \$14 billion.

NASA reluctantly took this cost to the OMB. Seeking to inspire a can-do attitude among the CETF team, NASA management passed out buttons containing the slogan "We Can Do It!" One senior estimator, who had seen it all before, modified his button to read "We Can Do It For \$20 Billion!"⁴⁰ Amid great political turmoil, the Space Station was finally given a go-ahead. Despite contractor proposed costs that were more unrealistically optimistic than usual, the source evaluations were completed and contracts were awarded for the four work packages. The project managed to survive several close calls in the FY 1988 through FY 1991 budgets, though with steadily escalating costs and several iterations of requirements cutbacks and redesigns. Like the purchase of a car, the sticker price includes nonrecurring cost only, and this is the cost NASA had always quoted Congress for new projects, including the Space Station. During the long and winding road of gaining Congressional authority for the Station, NASA was asked to include other costs such as Station growth, Shuttle launch costs, operations costs, and various other costs, which led to confusion and charges of even more cost growth than actually occurred.

As this is being written, NASA is actively designing and estimating the cost of several major future programs including the Earth Observation System, the National Launch System and the Space Exploration Initiative, among others. Each of these programs, like most NASA programs before them, is unique unto itself and presents a new set of cost estimating challenges. At the same time, the recent years of growth in budget resources that NASA has enjoyed seems to have run its course. In an era of relatively level budget authority, NASA is seeking ways to maximize the amount of program obtainable. New ideas on this topic abound. Total Quality Management, Design to Cost, Concurrent Engineering and a number of other cultural changes are being suggested as a solution to the problems of high cost. As usual, the NASA estimating community is in the middle. Armed with data from the past, which somehow must be adapted to estimate the future, they attempt to answer the all important question: *But what will it cost?*

So brief a treatment of the history of NASA cost estimating leaves so much unsaid that apologies are in order. Nothing was mentioned of the aeronautical side of NASA, yet they estimate the cost of projects that are no less important to the nation than the space projects focused upon here. The Kennedy Space Center facilities and operations costing was not mentioned, though nothing NASA has sent to space could have been sent without them. Whole projects from which much was learned about cost estimating (Viking, Skylab, Spacelab, Centaur-G, Hubble Space Telescope, Galileo, Magellan, Ulysses and many others) had to be left unexplored. Even when touched upon, many subjects were given only the barest of treatments, the expansion left for other studies. Finally, and worst of all, while this paper unfairly singles out a dozen or so individuals, another few score men and women who have labored hard in the crucial and controversial business of NASA cost estimating will not see their names here. They are saluted anyway.

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Sam II – How We Did It

by Ed Mauldin, Reggie Holloway, Don Hedgepeth and Ron Baker

SAM II is a very successful first-generation atmospheric research experiment developed for the Nimbus 7 observatory by the Langley Research Center. It came into existence within tight resource and short schedule constraints by a core project team of four engineers. Even though SAM II has been in orbit for over 14 years, it continues to meet scientific mission objectives. SAM II was recognized by the American Meteorological Society in January 1991—earning the Principal Investigator, Dr. M. P. McCormick, the Jules G. Charney Award for “. . . outstanding contributions to satellite sensing through development of solar occultation instruments for elucidation of the nature of Polar Stratospheric Clouds” which are central to understanding the heterogeneous chemistry that causes the Antarctic ozone hole.

Today’s spiraling cost, long development schedule, and large resource estimates to develop new spaceflight instruments begs a close review of past concepts to determine if they are applicable today. This paper describes technical approaches and management techniques used during the period from 1973 to 1978, many of which fit within today’s TQM initiatives.

We began relatively inexperienced and none of us knew any of the others when selected for the project. Only half of the team had flight hardware experience. All were GS11/12 engineers. Given the high visibility of the Nimbus program, the risks involved with development of a sophisticated, first-generation instrument, and the limited experience of the project team, Langley management could have micromanaged us to death. Instead, they accepted the risk and let us do our jobs without interference. They gave us the resources and the responsibility and we accepted the accountability for SAM II’s success. They

empowered us to speak with their authority in making real time decisions. Today, this is known as TQM.

Middle line managers helped us with technical advice, but we dealt with the upper managers for all management-related issues. They took a keen personal interest in SAM II and in our efforts. They visited the University of Wyoming (UWY) and Ball Aerospace on many occasions and knew our contractor counterparts. When asked, they even helped us with technical advice. They provided several analyses, including the Aliasing Error Analysis that we used in our instrument error budget.

They probably could have prevented us from making some mistakes, but wisely used restraint and correctly judged that in the final analysis, the experience gained from these mistakes would provide a greater long-term benefit than the temporary setbacks caused by them. And when we made mistakes, we accepted full responsibility without trying to pass the buck, and then worked twice as hard until we had the problem corrected. They offered virtually unlimited support as needed from the line organizations at the Center. The main benefit of the close relationship of the SAM II team with upper management was the tremendous boost in team morale derived from having their trust, confidence, support, and freedom to do things our way without interference.

Each SAM II team member maintained strong relationships with mentors, and each considers this relationship to have had a major positive influence on development of personal and technical skills necessary for SAM II success. We also strongly advocated and succeeded in getting our contractor and subcontractor to provide mentors for their young engineers.

■ Adherence to Phased Project Planning

Although we had no text to follow on phased project planning, we followed all the classical principles and covered all the bases that are contained in today's writings on project life cycles. These include: 1) establishing a clear set of science mission requirements and then converting these into instrument performance requirements during Pre-Phase A; 2) exploring potential instrument concepts that could deliver the necessary performance, lifetime and reliability during Phase A; 3) conducting technology surveys to select the most feasible candidate concepts, performing tradeoff studies to evaluate relative merits of candidate concepts, and evaluating technology state-of-the-art and performing risk analysis of candidate concepts in Phase B; 4) selecting a system and then subsystems concepts and quickly moving to the hardware phase by building an Engineering Model (EM) to do development testing to qualify selected concepts during Phase C; and finally, 5) fabricating, performance testing, and flight qualification testing of hardware during Phase D.

We could have written the current textbook on project life cycle principles. We moved to hardware quickly during Phase C/D and built an all-up Engineering Model that proved to be a major key to our success. The EM was thoroughly evaluated and tested, and many ProtoFlight Model (PFM) design refinements came from unforeseen problems during fabrication and testing or from failure to meet performance requirements during testing. The EM gave us a "test bed" that allowed quick evaluation of potential fixes without endangering flight hardware. All of our significant problems were quickly identified and corrected with permanent solutions using the EM hardware. Analyses are good tools, but the real proof of a design is in hardware performance. Also, the EM permitted testing beyond flight test limits, which helped to evaluate reliability, lifetime, and safety margin. Our EM testing was not constrained by

QA issues, nor the number of test cycles—factors that must be strongly addressed with flight hardware.

Many design flaws were discovered during the EM fabrication and test phase. The most notable failure was that of the elevation gimbal flex pivots during vibration testing. This resulted in an elevation gimbal redesign, including development of an isolation grommet design that has been used by three subsequent solar occultation instruments.

■ Risk Management

Early in the SAM II project, we conducted a survey of the availability and status of technologies that would be required for successful development. We did not take a conservative technical approach and were willing to accept many new and unproven designs and approaches. In retrospect, we used extraordinary engineering judgment in accepting some high-risk approaches that succeeded and in rejecting others that in hindsight would have given us problems. Many of the high-risk designs and approaches that were selected probably would be questioned in today's conservative environment.

We identified and ranked risks and put considerable effort into reducing those that could cause catastrophic problems. For high-risk designs, we aggressively pursued a risk reduction program that usually included fabrication of test articles, qualification testing, life testing, evaluation of results, and assessment of residual risk. Decisions were not based on a "hunch" or even an "educated guess," but were based on doing a lot of risk/payoff homework, identifying the development required, and then conducting the necessary development program. The problems incurred were not as severe as one would expect from a first generation design, and this risk management approach played a major role in the SAM II success.

■ Strong Systems Engineering Approach

Another major key to the SAM II success was effective systems engineering. We believed that with effective systems engineering, the project management process would take care of itself. Ed Mauldin, as Instrument Manager, had the closest role to that of a Project Manager, but this can be described as a part-time job since his full-time responsibility was optics and radiometric engineering. His project management efforts mainly consisted of administrative tasks necessary to keep the books in order; reporting tasks to provide monthly MIC reports and status reviews to Langley Research Center and Goddard Space Flight Center management; and coordination activities such as planning for the design reviews.

Although picked to cover optics, electronics, mechanical, and control system disciplines, we used a systems approach for design and problem solving. Thus, an optimized design was developed that has changed very little in the SAGE, SAGE II, and SAGE III instruments that have followed. This strong systems engineering approach eliminated many potential problems and also formed a “checks and balance” relationship among team members where each forced the other to do homework in order to defend a design or problem solving approach to the other team members. We became interchangeable and looked out for all disciplines when only one of us was in the contractor’s plant to review a design, discuss concerns and problems, or operate the instrument during a test. We were also blessed in having an outstanding systems engineer at Ball Aerospace.

■ Effective Schedule and Cost Control

Much of our success can be attributed to having an excellent working schedule. Although every subsystem of the SAM II instrument had unforeseen problems that were significant schedule drivers, the PFM delivery was shipped only two months after the originally contracted date. This

included recovery from a flex-pivot failure during vibration testing that by itself caused a 30-week delay in the CDR and EM delivery. We actually recovered all but one month of lost PFM schedule, but then lost a month waiting for a high-quality sun in Boulder, Colorado needed to perform the final Baseline Systems Test.

Maintenance of this excellent working schedule can be attributed to Lillian Henry of the University of Wyoming, who developed her PERT and technical skills while working on Project Hawkeye at the University of Iowa. Lillian kept an up-to-date PERT schedule in front of us at all times. PERT was used to provide an efficient guide on how to get from here to there, not to point out that the contractor was failing to keep schedule. We had weekly reviews by teleconference (including Langley, UWY, and Ball teams) in which PERT was used as a tool to review all critical and near-critical path activities and conduct brainstorming to find efficient workarounds to minimize schedule slip when problems arose. For a significant period in the middle of the program, all subsystem paths were parallel critical due to implementation of workarounds. PERT activities were focused, people oriented, and broken out into daily increments until PFM delivery. PERT revisions were frequently made to reflect the current best assessment of the most efficient sequence of activities. We even included the Wyoming and Colorado first week of hunting season as a PERT consideration, since many of our contractors were avid hunters. PERT was used as a daily management tool as opposed to a monthly reporting tool.

Cost control was very simple by today’s computerized Performance Measurement System (PMS) standards—yet very effective. When one of us visited UWY or Ball, one of our first activities was to meet with the Project Manager and review cost and schedule. We wanted to see which activities had been completed since our last review and which were in progress, with the names of individuals attached. We wanted to see how many

hours these individuals had charged to the SAM II account. Then, we personally went to the individual and asked to see results, such as analyses completed, drawings completed, fabrication completed, testing completed, etc. It did not take rocket scientists' skills to find the soft spots, and when these were found, we had meetings with the responsible supervisors and asked for (and received) explanations and remedial action. As shown in the table below, the system worked very effectively. Even without a PMS type system, our cost-to-complete proved reasonably accurate and we did not have any financial surprises.

COST AND SCHEDULE PERFORMANCE	
SAM II PLAN	
• Langley inhouse cost estimate:	\$2.4M
• Original budget (including contingency):	\$3.5M
• Original ProtoFlight Model delivery:	32 months after C/D start
SAM II ACTUALS	
• Contract signed for:	\$2.177M
• Contract cost runout:	\$3.165M
• Contract cost overrun:	\$0.482M
• SAM II instrument total cost (11/9/77):	\$3.250M
• Delivery:	34 months after C/D start

■ Small Core Team

We operated as a small core team backed by technical experts from within the line organizations. Thus, we were a clearinghouse for all line organizations participating on SAM II, which permitted rapid response to contractor technical issues. We often used specialists for ad hoc support, and when we needed support, we were not required to go through line organization channels for approval. Some efforts only took a day or two—other efforts seldom took longer than a week or two. Significant problems, such as the flexpivot failure, were attacked intensively with tiger teams. On these occasions, the focused activities

led to quick and permanent solutions that allowed the project to move forward with a minimum of delay.

Our ad hoc team members, most of whom came out of research organizations, were eager to help us. We never had to persuade any of them to work on SAM II even though on most occasions they were very busy and had to push their regular activities aside. This approach resulted in a considerable manpower savings at the Center and a cost savings to the project.

Although strangers to each other when SAM II began, we immediately developed strong bonds and close personal relationships, and we remain lifelong friends. We spent many hours together professionally and socially. However, the close personal relationships did not stifle strong debates on the issues, for each of us was very outspoken as we aired our concerns in frequent team meetings. And these concerns were always taken as positive critiques as opposed to personal criticism. The fact that a mechanical engineer could grill an optical engineer on optics without the optical engineer taking it personally (and vice versa for other team members) testifies to the strong interpersonal relationships that existed among us.

■ Communication Barriers

Early in the SAM II development, we were faced with a very difficult situation regarding communication channels with UWY and Ball. Contractually, we could not deal directly with Ball since they were a subcontractor of UWY. Moreover, UWY could not perform an adequate subcontract monitoring role because the staff was very “thin” and totally immersed with their work, and UWY did not have experienced engineers covering all the necessary disciplines to perform an adequate monitoring role. To say that we performed a tight rope act would be an understatement.

Our relationship with UWY and Ball employed lessons learned from Project Hawkeye, a satellite developed by Langley with a contract to the University of Iowa with Ball Aerospace as a major subcontractor. First, we aggressively dealt with UWY to define a program that our team could feel comfortable with, and one which provided acceptable confidence of success. This included some tough negotiation in the early days regarding project staffing; we needed to assure ourselves that the UWY team possessed the necessary skills and enthusiasm. After these negotiations were completed, we nurtured our UWY relationships to remove communication barriers and instill mutual cooperation and support. After we had gained their confidence and trust, we followed the same approach with Ball, with the blessing of UWY. We strived from the beginning to develop a clear picture of where we were going and the approach we wanted to use to get there. The emphasis was always on what was best for SAM II. Personal gain was never in the forefront.

On many occasions, we let the contractor take credit for our ideas, which gave them great incentive to work with us, and then we rolled up our sleeves to help get the job completed. Although there were bumps from time to time, the three teams became one united team, all working together to do the best job for SAM II. Differences were resolved by the dedication of each team member and each organization to make the necessary sacrifices to do what would be in the best interest of SAM II.

Often, Government project teams perform a monitoring role in which technical experts are mainly used to evaluate contractor performance and to serve as consultants to help solve problems. We did not monitor our contractor and subcontractor counterparts, but worked side-by-side to share responsibilities in all phases of the project. We had a significant “hands on” role that included participating in design activities, qualifying hardware at Langley, writing procedures and perform-

ing tests at Ball, performing instrument problem troubleshooting, etc. We spent nearly as many hours in the clean room with the instrument as did the Ball and UWY engineers, and were treated more like Ball employees when in the plant than as “customers.” UWY and Ball team members soon learned to respect us as being technical equals and all “us against them” barriers were removed. Good rapport with UWY and Ball had a great side benefit—we always had up-to-date information.

Using this concept, we were able to develop SAM II for a much smaller cost than would have been incurred if we had used the monitoring approach, since team technical manpower was significantly increased and very little energy was wasted in hiding agendas and playing the traditional Government vs. contractor game of staying at arm’s length. A testimony to the success of our balancing act is the fact that we were able to curtail Ball and UWY feelings of meddling and micromanagement and were never seriously accused of these negative behaviors.

One of the Langley SAM II team members was in the contractor’s plant almost continuously, eliminating the need for an on-site representative. These trips overlapped, so that status, issues and concerns of all instrument subsystems could be relayed by the departing team member to the arriving team member. At least one of us was always present during important events, such as subsystem and system checkout and performance and qualification testing. Don Hedgepeth lived in Boulder nearly the entire summer of 1976 helping with the EM assembly, checkout, performance testing and flight qualification testing. Ed Mauldin was once in Boulder for 30 days, waiting for a high-quality sun to run the final Baseline Systems Test. We also traveled with the contractor and subcontractor to vendor facilities to assess status and perform hardware inspections. This allowed an independent assessment of vendor-related issues.

Regarding change orders, we made clear from the beginning that unless the contractor saw a directive in writing from the Contracting Officer, comments heard in brainstorming sessions were not to be interpreted as directives. All directives went through the Contracting Officer to the prime contractor, UWY. Before a directive was issued, the Langley team reviewed it thoroughly with UWY until we were sure that the directive was clearly understood. We did not direct the subcontractor, Ball Aerospace, but they did receive similar directives from UWY.

■ Complementary Relationship with Principal Investigator

We were fortunate to have a very supportive Principal Investigator in Dr. Pat McCormick. Pat provided a clear and concise set of science objectives during Pre-Phase A. He provided a major support role in instrument concept development, such as data inversion simulations to establish instrument performance requirements. Pat helped us evaluate potential instrument concepts during Phase A and helped us conduct tradeoff studies during Phase B. We kept Pat informed of our progress, issues and concerns. He fully understood the engineering problems we faced, and provided relief when we were up against technology barriers in Phase C. Together, we refined the instrument concept during Phase C from a solar tracking instrument to a solar scanning instrument—a design that simplified the instrument and significantly improved the accuracy of data inversion. Neither the engineering team nor the science team would have arrived at this design independently, but the team's synergism resulted in an optimized instrument concept that is still used in current solar occultation instrument design. Both science and instrument teams had a relationship in which each trusted the other to give SAM II their very best effort. In short, Pat was our greatest supporter.

■ The Langley/Goddard Partnership

Today, new projects take the sister-center relationship between Langley and Goddard for granted. It did not start out that way. Traditionally, Goddard owned all requirements in their programs, including instrument performance requirements. An early Nimbus project manager at Goddard reminded us of the Golden Rule: "He who has the gold, rules." We did not accept this lopsided relationship and insisted on being treated as an equal partner. First, we worked diligently to define a boundary between centers, including explaining our responsibilities, authority, and development role. Then, we aggressively negotiated with Goddard until Langley ownership of the SAM II requirements and development approach was affirmed. Goddard retained ownership of spacecraft interface requirements and top-level mission requirements, such as spacecraft orbit parameters. Eventually, we were treated as peers and the inter-center relationship became unruffled.

Again, this illustrates how we insisted on principles now linked to TQM: those doing the work should be given the responsibility and authority to produce their products. The ground rules established then essentially remain in effect today for inter-center space flight development programs.

■ Spacecraft Interface

In the beginning, the Nimbus observatory consisted of nine sophisticated flight instruments that required a stringent adherence to the initial allocation of the limited spacecraft resources. These initial budgets were established on March 10, 1975, by the Nimbus Project Office at Goddard, which was less than two months after SAM II contract award. The controlling document was the Sensor Interface Requirements Document (SIRD). A five-phase delivery of interface materi-

als was required by the SIRD, with each phase requiring substantiation that an instrument could stay within its allocated resources. After March 10, Goddard required monthly review of all resources.

At one point very early in the program, Goddard had allocated all weight and power resources and had no contingency at all. One instrument was removed from the payload to recover contingency, but adherence to initial budgets continued. Thus, weight and power were major considerations in selection of technologies, design concepts, and parts used in SAM II. Despite not having an instrument configuration when the early resource allocation and tight resource constraints were placed, we were able to deliver SAM II within all spacecraft budgets.

The concepts used by a small core team to successfully develop the SAM II instrument, which has performed in orbit since October 1978, are summarized below. We could have mentioned other concepts, but did not want to risk obscuring the ones we felt were most important. Many of these concepts are now basic principles of TQM, but were chosen at the time because they simply made common sense:

- Management empowerment of the project team
- Adherence to Pre-Phase A, Phase A, Phase B, and Phase C/D principles
- Engineering model as early proof-of-design elements
- Attentive risk management
- Strong systems engineering approach
- Effective schedule and cost control
- Small core team backed by experts in the line organization
- Close-knit project team
- Removal of communication barriers with contractors
- Frequent visits to contractors
- Clear procedures for directing contractors
- Complementary relationship with the principal investigator
- Well-defined Langley/Goddard partnership
- Attentiveness to spacecraft interface
- Use of “lessons learned” from previous projects
- Mentor relationships

Our National Space Science Program: Strategies to Maximize Science Return

by Greg S. Davidson

There is a concern at NASA to learn from the lessons of the past and to respond to the concerns of the space science community. We have been grappling with the problems of:

- Big versus small missions
- Multiple simple spacecraft versus spacecraft servicing
- A culture of risk avoidance
- Unrealistic budget planning
- Institutional and political forces
- Linkage to the manned space program

As we are in a very dynamic time for space astrophysics, our discussion must be placed in the context of current events. The missions of the 1980s have been launched and data is beginning to arrive. More new astrophysics missions were expected to be launched between 1991 and the end of 1993 than in the decade of the 1980s. With the greater emphasis on long-term operations designed into several of these recent missions, the supply of new data should continue to grow. Over the last several years, the growth in funding for space astrophysics has exceeded inflation by roughly 15 percent, and the growth in funding for science and data analysis is keeping up with the growth in science data.

There has also been tremendous criticism of the NASA astrophysics program from the media, Congress, public and some members of the science community. Problems with the Hubble mir-

ror have been fodder for comedians and commentators, while other difficulties such as those with hydrogen leaks on the Space Shuttle, or the rash of problems overcome on the Astro mission, have helped to create a perception of serious problems. It is always appropriate to review our programmatic strategies for conducting space astrophysics in light of experience, and to develop our strategy for the future. So let us briefly note the recent scientific output of our space astrophysics program, and then discuss the areas of concern identified above.

■ Status of Astrophysics in January 1991

Cosmic Background Explorer

(cryogenically cooled mission complete
November 1990)

- A smooth big bang
- No later bangs
- Unrivaled data from the infrared background

Hubble Space Telescope

(checkout complete, beginning science verification and operations)

- Potential black hole in nearby galaxy
- Circumstellar ring around Supernova 1987a
- Storms on Saturn
- High-resolution imagery of Pluto and Charon

Roentgen Satellite

(survey complete - U.S. observation time begun February)

- 80,000 new X-ray sources
- 1,000 new extreme ultraviolet sources

Astro/BBXRT

(mission complete December 1990—data being analyzed)

- Results to come . . .

Recent and Upcoming Science Missions

- Gamma Ray Observatory (1991)
- Array of Low-Energy X-ray Imaging Sensors (1991)
- Extreme Ultraviolet Explorer (1991)
- Solar-A/Soft X-ray Telescope/ISAS (1991)
- ASTRO-SPAS/Orbiting and Retrievable Far and Extreme Ultraviolet Spectrometer/ESA (1992)
- Diffuse X-ray Spectrometer (1992)
- KONUS/WIND (1992)
- Solar, Anomalous, and Magnetospheric Particle Explorer (1992)
- Astro-D/Spectroscopic X-ray Observatory/ISAS (1993)
- HST Wide Field and Planetary Camera II (1993)
- Spectrum-X-Gamma/USSR (1993)
 - All-sky monitor
 - Stellar X-ray Polarimeter

During the next several years, some of the other areas of substantial work will include the Advanced X-ray Astrophysical Facility, the X-ray Timing Explorer, the Shuttle test of the Gravity Probe-B instrument, the Submillimeter Wave Astronomical Satellite, additional HST replacement instruments, instruments for the European X-ray Multi-Mirror mission, support for the Russian Radioastron mission and the Japanese VLBI Space Observatory Program, and definition work on the Space Infrared Telescope Facility, the Stratospheric Observatory for Infrared Astronomy, and the Far Ultraviolet Spectroscopic Explorer. Rocket, balloon and airborne activities will continue, as will data analysis from previously flown missions.

■ Big and Small Space Missions

The scientific rationale for a mix of big, moderate and small missions in a balanced program of astrophysics is that there is no optimal mission size to address the incredible variety of scientific phenomena we wish to investigate. In practice, mission size is scaled down to the lowest level required to fulfill the science goals. The U.S. is unique in its capability to conduct science missions that require the largest observatories, but these observatories are balanced by many smaller efforts in our overall program of space astrophysics research. A diversity of missions also helps to develop and maintain our institutional capability to carry out scientific investigations today and in the future. Diversity in mission size supports a variety of implementation strategies, and it helps establish a broad portfolio of missions that can help weather unanticipated and adverse external events.

The terms we use to describe programs may present a misleading perspective to mission size. In one sense, Congress categorizes the entire physics and astronomy budget, which includes astrophysics, space physics, and Shuttle payload mission management, as a single item in the budget.

research and analysis, guest observer and archival research programs round out the currently available astrophysics opportunities. While the diversity in scale of missions is not entirely under our control, we do believe that this combination meets the wide range of needs of the community as they have been expressed to us. Are we correct in this assessment? Some are asking for a greater emphasis on smaller missions in the future. What type of service, product, opportunity or efficiency should be pursued? We welcome further discussion of this issue.

■ Single vs. Multiple Spacecraft in Conducting Missions

There are cost and benefit tradeoffs associated with the strategy in which missions are designed to be conducted by a single complex spacecraft rather than several simpler spacecraft. The most cogent examples of the single complex spacecraft missions are HST and AXAF, and since they represent half of the Great Observatories, questioning the fundamental strategy they embody could provide enormously important insights.

HST and AXAF are not merely large and complex observatories, but they are also serviceable observatories. They reflect a strategy to provide 15 years of on-orbit lifetime through regular replacement of instruments and other hardware. In contrast, the strategy to provide a similar on-orbit lifetime for the Tracking and Data Relay Satellite System (TDRSS) and the Earth Observing System (EOS) involves development of a series of replacement spacecraft on a regular basis. The intent of the servicing strategy is to optimize costs for long-term missions that require an expensive spacecraft (such as HST or AXAF with their large precise optics), and that can also operate in a Shuttle-accessible orbit. TDRSS requires a geosynchronous orbit and EOS requires a polar orbit. Neither TDRSS nor EOS have structural elements analogous to HST or AXAF optics.

The comparison above points to two strategic reasons to use a single, serviceable spacecraft to achieve long life, and to reap the benefits from sharing critical infrastructure. But at what cost? The complexity of a single mission raises costs and increases the required development time, which increases costs further.

While a single spacecraft approach is more vulnerable to an irreparable system single-point failure, servicing provides a programmatic means to regularly repair subsystem failures. The only previous astrophysics missions on a scale even roughly analogous to HST were the Orbiting Astrophysical Observatory (OAO) and the High Energy Astrophysics Observatory (HEAO) series. Two of the four OAO spacecraft failed, one from a launch failure and another on the second day of the mission from a power problem. All three of the HEAO missions were launched and operated successfully for approximately two years. Of course, it was expensive to build serviceability into HST and to purchase the first set of replacement hardware, but the cost to build, launch and operate in the early 1960s would equal \$2.4 billion in 1993 dollars. The life cycle costs of HST, including six Shuttle flights at \$350 million each, is a little over three times that amount. For comparative purposes, the HEAO lifecycle cost through the two years of operations was \$130 million. Is there disagreement that the expected 15-year scientific return of HST will easily surpass that of HEAO and OAO, even in the context of a much advanced technological state-of-the-art?

Multiple missions provide a certain “safety in numbers” for launch vehicle or other flaws, but numbers provide no easy fix to generic failures. If we had built two simple HSTs, both primary mirrors would likely have been distorted by the same faulty null corrector. Since the most cost-effective way to build multiple spacecraft is to have one roughly one to two years ahead of the other, the second Hubble in this example would have been

essentially completed at the time the problem was identified in orbit. In that scenario, repairing the mirror on the ground could easily take as long and be as expensive as the fixes we are implementing today for the real HST.

We must also be careful in using launch rate as a surrogate measure for scientific productivity. Launching five simpler Hubbles with one instrument each might have resulted in a higher flight rate, but the number of launch vehicles by itself does not increase productivity. Implicit in a strategy of flying single multi-instrument spacecraft rather than many single-instrument spacecraft is a lower launch rate. Since a launch itself does not yield science, we have to look for some other measure of scientific productivity that is correlated with flight rate. What are the appropriate ways to measure scientific value: launch rate, number of instruments, weight of instruments, observation time, data returned, refereed publications, new knowledge?

A problem with slow programs is that we do not reap critical information for many years. While this is of most concern in the scientific arena, it also hinders the expansion of our knowledge of how to conduct space science missions. HST is our first experiment with a planned spacecraft servicing strategy, and as a pathfinder it will wind up costing more than programs that can benefit from Hubble's servicing lessons learned. HST was begun in an environment where almost weekly Shuttle flights were anticipated. Throughout the development period, as we have learned about the Shuttle and what it can do, the HST servicing strategy has shifted and adapted. After less than a year on-orbit, we have a small but real database on actual mission events and the programmatic flexibility servicing provides to accommodate them. Servicing will enable key fixes to HST solar arrays and optics, but are these advantages enough to justify the extra expenses? Adopting an empirical approach, let's see the data, let's discuss it, and let's see what we can learn from it.

■ Risk-taking and Risk Avoidance

Can we change the environment to support a level of risk-taking that will increase the long-term efficiency of our space science expenditures? It would be almost impossible to make state-of-the-art spacecraft so reliable that we could be 100 percent certain that there are no technical risks, and even if we could do this, the last bit of reliability would probably cost a lot. A cheaper and more practical approach that NASA has employed is to design our difficult missions to provide additional capability or flexibility that enables us to survive unanticipated problems. If we then can build that mission cheaper, we are getting more science for the dollar.

However, a strategy that includes some risk-taking has one critical implication—sometimes failures happen. The problem with risk-taking strategies is that NASA, Congress, the science community, and the general public are usually unwilling to accept failures. NASA provides a symbol of American technological excellence; thus, NASA successes and failures have a context that exceeds science return for the dollar. NASA receives the budget that it does partly because of this symbolism in the minds of members of Congress and their constituents, but NASA's stature also complicates our simple cost-benefit analysis. Imagine an airline that decided that the strategy to yield the most cost-effective transportation for the dollar would be to reduce safety to the expected fatality level associated with driving a car. Even if this decision could be implemented, what would happen after the first crash?

The Hubble mirror aberration was tragic, but it was also typical of many spacecraft failures in that it was from an utterly unexpected source. But unlike previous missions, the HST program strategy was failure-resilient. On-orbit servicing provides the programmatic flexibility through which even this utterly unexpected technical problem can be addressed and corrected. From a program

perspective, we will have an observatory that will be less than expected for three years, but for the remaining 12 years should live up to its full potential. Nevertheless, the failure in the mirror fabrication has had a profound and fundamental impact on the science community, the public, the Congress and on all of us here.

It is still possible to have a strategy that involves risks, and NASA does designate payloads in one of several categories depending on the level of risk deemed appropriate. Planetary missions are Class A, with requirements to use only the best possible parts and the greatest level of redundancy. Recently, the AXAF spacecraft (with a few exceptions) was deemed to be a Class B mission, in part because of the additional flexibility provided by servicing. The recent Astro flight on the Shuttle Columbia, like many other Shuttle-attached payloads, was developed as a Class C mission.

We agree that failure-resilient strategies should be pursued. So how are we going to change the environment so that there will be support for these strategies? In the abstract, few would disagree. The challenge is to look for ways to enlist and maintain the support for programmatic flexibility and risk-taking even after a failure occurs. If we cannot accept failures of any sort, the cost of missions will inevitably rise.

Budget "Realism" and Strategies to Optimize Financial Risk

How do you estimate the cost of something that has never been done before? NASA starts out with several simultaneous approaches. NASA and contract engineers develop what is known as a "grassroots estimate," in which the working level people estimate their own effort required, and these estimates are aggregated. Although it is critical to have the input from the people who will actually do the job, there are also some problems inherent in a grassroots estimate. Those people do

not yet know how they are going to overcome the unique challenges associated with the missions that yield the state-of-the-art science we are usually pursuing. There is usually some optimism on the part of the engineers, and it is difficult for a grassroots estimate to properly account for the aggregate effect of complex interactions of separate groups working on difficult tasks. So in parallel with a grassroots estimate, a parametric estimate is made using statistical inference based on previous mission experience. By using factors such as subsystem weight or complexity and mission type (such as cryogenically cooled, super lightweight planetary probe, or low Earth orbiting instrument platform), a budget estimate is developed.

The grassroots and parametric estimates are then compared, the information from both estimates is presented, and a single budget estimate is developed for the project. This budget estimate (along with the associated technical and scientific plan for accomplishing the mission) are then reviewed by a team of "non-advocates" who scrutinize the plans and assumptions of the new project, as well as the grassroots and parametric estimates underlying the assumptions.

Given all of this knowledge, some of it contradictory, what budget estimate should be sent to the Office of Management and Budget? If NASA requests a very high budget, we increase the chance that we will look good later, because the chance of overrun is reduced. There is less stress on NASA managers when you have a lot of money for your project. At the same time, there is probably some price at which a program is too expensive to be funded (although it is hard to know what that really is). Another problem is that NASA budgets are a matter of public knowledge, and so all of the contractors know your program's funding. A comfortably large budget can become a tempting target, and so you may find the effort on your mission growing to fit the available budget.

The balance to be struck is to propose a budget which is achievable without being comfortable, and then to monitor and track all changes from this original baseline. Over time, your initial baseline (based on grassroots and parametric estimates) will be modified by contractor bids and negotiated settlements, design reviews, experience with fabrication, assembly and test, and all of the other activities associated with conducting a space science mission. Increases in cost will be of two sorts. Where we have misestimated costs, we request what is known at NASA as a “reprice”—more money to do the same work. Contractor overruns are a subset of repricings, because programs tend to budget for more than the dollar value of the contract to protect against overrun. Only when these reserves are depleted will the program request a repricing to cover a contractor overrun. Sometimes in the development of a mission, we learn that a new activity is required to accomplish the mission, or that spending additional funds to develop a new capability may yield sufficient return to justify the investment. Additional funds to do new work is referred to as an augmentation or as additional scope.

Twice every year NASA formally reviews the budgets of all of our missions to anticipate pending problems, to assess problems that have been identified, and to look for areas where new scope might bring large benefits. It is not entirely a zero-sum competition between these programs for additional funds, but the pressure to make tradeoffs is always there. Within some programs the tradeoffs are internal, and no additional funds are requested. If this is not possible, we must prioritize any annual requests for additional funds. It might look better if there were never any requests for additional funding—hypothetically, NASA would quote a price and come back years later with a spacecraft. But how would we know what price to quote? We could keep eliminating parts of the program to fit within the initial estimate, or we could ask for a high enough budget that we could afford anything. The way the process works now, we make those choices, but we do so incre-

mentally over the life of the missions. Every year our information gets better on what each mission needs and what is possible to accomplish. Most of our effort, and that of our contractors, is dedicated towards learning about the mission and the hardware that can accomplish it—the materials, fabrication and assembly of spacecraft are a minor part of our expenses. If our management at NASA Headquarters is to be based on the science and engineering fundamentals of the missions we are conducting, our management and budgeting must also be a continuous and incremental process. Of course, as our projects and contractors will tell you, this does not mean that we treat budget growth kindly. We must treat an increase in one area as if it were a cut to another, because sometimes that is exactly what we have to do.

We must also counter the tendency towards a focus on the short term, an orientation which is shared by the stock market and indeed with much of our current national character. Congress votes NASA its budget one year at a time. Unlike the private sector, we cannot borrow money from a bank even if it will yield an enormous benefit downstream. The only source of funds in a given year for a new requirement in one mission is to take the money from another. Consequently, everyone’s concern is drawn towards the current year’s budget (which we are spending), and the next year’s budget (which is at OMB or Congress where tradeoffs are being considered). Funding for the next four years beyond that is controlled at OMB, but there is a tendency not to focus on these “outyears.” Unfortunately, our overall scientific productivity depends on choices made throughout the life cycle of our missions. When we develop missions for 15 years of operational life, and 30 years of data analysis to follow, a short-term perspective will not work.

We have a sign on the wall around here: “If everyone keeps saying ‘screw the outyears,’ eventually we will all live in outyears that someone else has screwed.” For long-term missions, this means we must expand our vision even

beyond the five-year horizon typically used for planning. The science return on our missions must be weighed against life cycle costs, and if we only focus on the narrow window of the development period, we are likely to make trade-offs that optimize costs in the short run but are a net loss overall. We have learned the value and insights that come from this perspective in planning the 15-year Hubble operational lifetime, and we plan on implementing the same scope of vision on AXAF. Today's estimates for AXAF operations in the period from 1998 to 2013 must necessarily be soft, but by attempting to track the implications of today's decisions across a mission lifetime, we hope to make decisions that will look as good in retrospect as they do in formulation.

I would also like to raise the controversial premise that part of the "realism" of budget planning depends on an assessment of appropriate financial risk-taking. The problem is analogous to the question of optimizing insurance coverage. If we could reduce the number of programs under development and thereby increase the reserves on each, we could reduce the odds of an overrun on each of our missions. Indeed, the incentive on the individual managers who are responsible for a single project is to be as conservative as possible—to take no financial risks. But at Headquarters our job is different. Our goal is to maximize science return for the dollar. This creates a natural dynamic: the project manager is looking to optimize on behalf of a specific mission, while Headquarters makes tradeoffs between missions and levels of risk. If we are always cutting and delaying every program, the level of risk is too high. If we never have to make a tradeoff between programs, the level of risk we are taking is probably too low. This raises the question: Are we taking the wrong level of financial risk?

Unfortunately, while the downside costs of taking risks are very visible (project cuts or slips), the benefits are not as easily traceable. The quick response to Supernova 1987a was funded by stripping funding flexibility and thus taking

financial risks across the board. In 1988, while AXAF was sent as a new start in the proposed budget to Capitol Hill, we turned down a request from the HST project for \$50 million of additional reserves on development activity. We took what we felt was an appropriate level of risk on HST, independent of concerns for the pending AXAF decision. If we had insisted on having the extra reserves as insurance, it may well have prevented us from starting AXAF that year. We were correct in our assessment that HST development effort did not require the extra reserves to accommodate the problems they were concerned with at that time, but the benefit to astrophysics and space science from this type of risk-taking is usually not as visible as the costs.

One negative aspect of a risk-sharing strategy is that the severe problems in one mission spread across a range of programs. A defining attribute of the Explorer program is that individual projects have reserves that are much lower than usual for other NASA flight programs, and that problems are accommodated within queues. The mission development and launch vehicle problems of the mid-1980s have stretched out the Delta-class Explorer queue to the point where the next mission under development, the X-ray Timing Explorer, was selected 14 years ago. Is this delay acceptable? If not, should we begin to emphasize flight rate more strongly above science performance in Delta-class developments? Should we also wait longer before starting Explorers to increase the likelihood that stable funding will be available?

A particular fear concerning this risk-sharing strategy is that problems in one big project can decimate many other small projects. Put differently, risk-sharing is not appealing to the many if there is one elephant and a lot of mice. However, the existence of several observatories at different stages of their life cycles creates a separate field for elephants, so sensible risk-sharing is now possible. In FY 1991 Congress provided an additional \$30 million for HST, but also levied a similar

reduction that was borne by AXAF. It was deeply disturbing to have to upset the AXAF baseline; we are aware of the inefficiency that funding changes can cause. Nevertheless, we cannot afford to regularly carry reserves to insure against major unanticipated crises such as the HST spherical aberration. If we had carried an extra \$30 million of reserves for Hubble over the past three or four years, these funds would have remained wastefully idle (or worse yet, they would have been spent inefficiently merely because they were available). We had low reserves on Hubble but sufficient funding to support several years of AXAF mirror definition and development work.

Were these the right choices? At the time our assessment of financial risk appeared sound. We also recognize that this is not a clear-cut case because, as events unfolded, we were hit by a more pessimistic scenario than we had planned for. Nevertheless, in retrospect we believe that the overall science productivity has been increased by these choices. In terms of the broader inquiry of operating strategies, the more fundamental questions are whether such financial risk-based strategies are appropriate, and if so, are there further principles or guidance to improve the process?

The problem of financial risk-sharing, as with any risk-based strategy, is that it is difficult to take a broad perspective. There is a cognitive bias in human judgment of risks which has been empirically demonstrated. Negative events resonate far more loudly than positive ones. McCray and Stern ["NASA's Space Science Program: The Vision and the Reality" (1991)] express a concern that the cost of accommodating spherical aberration on HST "may raid small, individual investigator groups of development funds." This fear has a basis in the memory of the so-called "slaughter of the innocents" in 1983 and 1984 when Hubble development problems were solved by cuts primarily from small mission efforts in astrophysics and other science disciplines. These

were truly tragic cuts that caused real damage to individual scientists and teams. Psychologically, the impact of these cuts resonates very deeply. The actual level of reduction was 8 percent of non-Hubble astrophysics in 1983 and 1984, and there has not been a hit on small missions caused by big ones since, but the concern remains because the "slaughter of the innocents" is such a powerful and psychological force in shaping our cognition of risks.

We believe that by establishing one risk pool for large missions and another for small missions, we make it possible to make efficient use of risk-sharing, which yields the maximum amount of science productivity without threatening the "innocents." Inside a single risk pool, a major unanticipated problem such as the HST aberration threatens AXAF, but future AXAF problems may also be weighed against HST funding. It is natural to have a general concern that Hubble will continue to need more and more funding, because that has happened on several occasions. At the same time, the HST Science Institute was specifically created as part of a strategy to counter the institutional tendency of NASA to underinvest in operating missions in favor of new development activity. In general, we want to provide sufficient reserves to our programs so that they can accommodate a nominal range of problems. In our assessment, it is not efficient to provide insurance in the form of reserves to cover very pessimistic scenarios. Since pessimistic scenarios do occur occasionally, we will sometimes be forced to trade off priorities between missions in the same risk pool. We intend to make these tradeoffs in the context of the priorities established (and regularly reiterated) with the science community.

Should we be more risk averse? Holding higher reserves means responding less quickly to opportunities and starting fewer missions. Remember, our risk posture is not the only factor that can influence funding. These have been our choices to date, but we welcome dialogue on this issue.

■ Institutional and Political Constraints/Forces

Institutional constraints sometimes prevent NASA from achieving the best possible science for the dollar. While we do not have the authority to change many of the rules under which we operate, it is worthwhile to discuss the institutional setting of NASA space science because solutions may exist; we may just not see them.

The budget process takes about two years, of which more than half is activity outside NASA by Congress and the Administration. Space science experience has shown that reliable cost estimates are frequently difficult to make until you have already invested about 10 percent of the development cost in definition. However, if we wait until that level of definition and then begin the budget process, we are adding a substantial delay to the program. And once a program is inserted into the budget process, that is no guarantee it will emerge successfully. We have adopted and implemented a strategy embodied in the OSSA Strategic Plan which tries to prioritize and sequence major new starts for a period of several years in order to focus our resources on a few key mission candidates and reduce the complexity of tradeoffs once missions are proposed within the budget process. This strategic planning avoids some types of inefficiency, but inevitable time lags remain in the system.

National political forces sometimes favor highly visible—thereby large—space mission. As public and private individuals pursuing government-supported space science, we are in a bind. The Executive and Congressional process by which the U.S. approves scientific investigations provides the fundamental legitimacy we have to do our jobs. At the same time, institutions and processes can tend towards certain results by virtue of their structure. If in fact there is an institutional predisposition towards large missions in the space mission approval process, then the sources of that structural preference must be

specifically identified and countered. Otherwise, the science administrator who proposes a program of space research that includes fewer large missions and more small missions is likely to lose in the competitive budget arena to others who cater to the existing bureaucratic and political tendencies.

While the current complement of space science missions presents a diversity of large and small science, it is possible that it is not the right mix. The institutional and political process is shaped by the actions and contributions of both public and private space scientists, engineers and managers. The process begins with mission proposals from the scientific community and ends with Congressional approval. If the system has a bias towards bigness, what specific changes can we make or promote to get the system to support the optimal size diversity for space missions? What actions can we take today? How shall we plan to address this issue over time?

■ Linkage to the Manned Space Program

There is a major role in space science for unmanned missions, and we take advantage of the opportunities available. Smaller Explorers have always used expendable launch vehicles, as do even smaller rocket experiments. As experience teaches us more about the capabilities of the Space Shuttle, science mission strategies have been shaped to optimize their mission within the envelope of possibilities. The experience of Challenger and the evolution of the space launch arena since that time have taught us that the Shuttle is generally not an appropriate launcher for larger Explorers. COBE and EUVE had to be redesigned for launch on expendable vehicles, at a significant cost. XTE is planned for Shuttle launch and on-orbit installation on EUVE's Explorer Platform; based on our current investments and options, the servicing strategy embodied in a reusable platform is still the most cost-effective way to pursue the XTE mission. FUSE

is further downstream, and the continuation of this Explorer Platform strategy should be carefully assessed, based on our experience to date and the existence of alternatives. Other Explorers are planned for unmanned vehicles.

Despite the changes in the Shuttle program since the initiation of HST development, that on-orbit observatory is poised to take full advantage of the manned space program in pursuing its 15-year science mission. The Space Infrared Telescope Facility (SIRTF) was originally designed to be a Shuttle attached payload. When Shuttle flight experience indicated the existence of an orbital phenomenon (atomic oxygen glow), the Shuttle Infrared Telescope Facility was changed into a free-flying spacecraft to fulfill the mission needs. Later study has revealed that a 100,000-kilometer orbit optimizes the SIRTF mission return, and so now the current baseline is for launch on a Titan IV unmanned vehicle. There was no institutional resistance to this change, and we intend to continue making launch vehicle choices on the basis of science priorities and cost effectiveness.

Current budgeting policy does not charge differential costs to account for the differences between expendable launch vehicles and the Space Shuttle. Unfortunately, the very fact that the Shuttle is not expendable makes it very hard to calculate the appropriate cost of a single flight. In terms of the expendable fuels used and flight-specific effort, the cost of flying six Shuttle flights in a year instead of five is very small (closer to \$40 million than the \$600 million cost estimate that some members of the science community have used). In economic terms, the variable costs are insignificant when compared to the fixed costs. A greater share of the cost of using a Shuttle flight is due to another economic concept: opportunity cost. If we were to change our minds and launch SIRTF inside a Shuttle, we would have to push some other payload off the manifest. (Of course, some payloads require only partial use of the pay-

load bay and mission timeline.) The value of opportunity cost would depend on the importance of the payload to be replaced. We are also not amortizing the development of the Shuttle in our costs above (nor that of ICBMs upon which our fleet of expendable vehicles is based), because the final economic principle which we are pursuing is that sunk costs should not be considered in making today's choices: we want the most cost-effective way of accomplishing the science missions we are pursuing.

The dialogue should not stop here. We are attempting to pursue better operating strategies to address some of the important problems that have been raised by members of the space astrophysics community. As these efforts progress, we will want to examine their effectiveness, adopt and improve what is successful, and change what is not working. Other key issues remain unanswered, and while some of the institutional problems appear inevitable and unchangeable, we should be wary of complaisance. If we are doing something that can be done better another way, we should try the better way. America's space science program yields benefits to all of us, and it is the duty of those entrusted with conducting this exploration of the universe to grapple with our common challenges and surmount them.

Efforts to understand and to maximize science return have continued. Over the past two and a half years since this was written, the strategy for both XTE and AXAF has been changed from the Shuttle servicing mode, with AXAF split into two smaller, cheaper spacecraft. HST remains on schedule for Shuttle servicing missions in December 1993 and March 1997. Institutional factors now appear to be shifting towards small missions over large ones. Hopefully, this trend will not be simply an exchange of one inappropriate bias for another, but rather an opening of a wider variety of alternatives from which we can pursue the optimum.

Human Needs, Motivation, and the Results of the NASA Culture Surveys

by Mario H. Castro-Cedeno

An organization is defined by Mondy et al. (p. 198) as two or more people working together in a coordinated manner to achieve group results. When run well, an organization will provide synergism to the activities and efforts of its members. Through division of labor and specialization, the members can contribute their individual skills, expertise and effort toward accomplishing goals that are far beyond the capability of any single individual. Modern research and engineering projects would not be possible without large organizations because both specialization and cooperation are essential in addressing the complex and interdisciplinary problems of the modern world.

Unfortunately, the act of organizing can inhibit or limit individual behavior, working conditions and job satisfaction. Sometimes the limitations are easy to see or discover and are not difficult to understand. That is the case with working hours and office space. In other cases, such as status and rank, they are much more difficult to interpret because they are the result of complex cultural interactions. Wage scales and the apportionment of fringe benefits are examples of limitations that have both a cultural and an economic origin.

The limitations that an organization member encounters in the workplace may come from experience that has been codified and formalized into policies and procedures. Or they may be part of the unwritten corporate culture and folklore. Limitations may sometimes arise when responsibilities are transferred from the corporate entity to its representatives. In all effective organizations, members voluntarily give up some of their individuality and freedom for the common good.

The limitations that an organization imposes on its members can cause dissatisfaction and may

produce unhappy employees who will not participate to the extent of their potential in achieving the organization's goals and mission. This loss of interest is called "demotivation" (Mondy, 300). If the dissatisfaction pervades an organization, the inefficient use of the human resources will lead to poor organizational performance. Thus, a project manager must understand and minimize these demotivators.

But even eliminating all the demotivators, or sources of dissatisfaction, may not result in motivated employees. Motivation, which is defined as the desire to put forth effort in pursuit of organizational objectives (Mondy, 292), is a higher goal than avoiding dissatisfaction. What causes motivation must also be understood because maintaining employee morale and motivation is an important project management duty.

■ Scientific Management

The systematic study of the factors that enhance workplace efficiency is called scientific management. It had its origins in the work of Frederick Taylor at the beginning of this century. He and his followers advocated systematizing efficient work procedures by using the scientific method to analyze management problems and situations.

In a classical application of scientific management, Taylor studied the pig iron operation of Bethlehem Steel Company (Taylor, 41-47). He used what are now known as time-and-motion studies to determine that the average worker output was 12.5 tons per person per day. He then prescribed more efficient work methods and standardized rest periods. The result was that average output rose to 48 tons per person per day. The additional efficiency, combined with a new incen-

tive pay system that he also proposed, increased the worker's daily pay from \$1.15 to \$1.85. Thus, both the organization and its members benefited from Taylor's work.

Taylor's followers embraced his methods and techniques, and for some time it was thought that scientific management was all that was needed to improve the efficiency of any organization, especially those involved in manufacturing. They performed time-and-motion studies to develop efficient work procedures and labor-saving tools, and then directed employees in a rational and scientific way.

Beginning in 1924, Elton Mayo and others performed a series of studies at the Western Electric Company's plant in Hawthorne, on Chicago's west side (Mondy, 68). One study looked at how lighting affects worker productivity. Illumination was first increased to extreme brightness, and then it was reduced in stages to the point where materials could hardly be seen. Workers maintained or even exceeded their original output. Similar results were obtained for wage incentive, supervision styles, length and frequency of rest periods, and length of the work week.

The Hawthorne study led Mayo to speculate that something other than the variables under investigation was having an effect on worker productivity. While observing and interviewing the workers, he noticed that merely by participating in the experiment they felt special. Their morale improved and that caused productivity to go up. The influence that researchers can have on the behavior of the people they study is now known as the Hawthorne effect. It is proof that morale and motivation are at least as important as the physical environment and the tools available to workers.

■ Motivation Theories

Managers may attempt to motivate their workers by using rewards, punishment, and charisma, or

by exercising authority. The method used will depend on their beliefs about the causes of motivation. By widening their knowledge of this subject, managers can use the appropriate motivating technique and will make their workers and the organization more productive and efficient.

What causes motivation and what diminishes it have been the subjects of much research. Most theories are based on observations of human nature. Table 1 lists some theories widely accepted within the management science community. Keep in mind that human behavior is complex and impossible to generalize. It varies from person to person and depends on the particular situation. No single theory will be valid all the time.

Douglas McGregor's Theory X and Theory Y propose that people either dislike work and responsibility (Theory X) or enjoy self-direction and achievement (Theory Y). Chris Argyris calls Theory Y behavior "mature behavior." He proposes that only immature people are passive and lack initiative. Both authors believe that most people conform to Theory Y assumptions in a healthy work environment.

Argyris sees an unhealthy work environment as characterized by overspecialization that limits self-expression, by a rigid chain of command, or by an overpowering leader. In such an environment workers have little control over their work day. They are expected to be passive and subservient and must have a short time perspective. According to the theory, an unhealthy work environment will cause the worker to cope by escaping (e.g., leaving the firm or seeking promotion or transfer), by fighting (e.g., joining a union or seeking a way of exerting pressure on the organization), or by adapting and developing an attitude of apathy, indifference, or cynicism. Flight, fight or fatigue, Argyris judges the last option to be the worst choice for the worker's mental health.

Theory X and Theory Y do not provide guidelines for all situations. They do not explain situa-

Table 1. Motivation Theories

Theory	Description
Theory X	People dislike work and need to be coerced or bribed to do their jobs.
Theory Y	People enjoy work, will self-direct if allowed, and will strive to succeed in the workplace.
Self-fulfilling theory	People will attempt to fulfill their leader's expectations.
Reinforcement theory	People become motivated or demotivated when faced with situations similar to past experiences.
Needs theories	People become motivated when they attempt to fulfill their unmet needs.
Equity theory	People become demotivated when, in their assessment, other employees are being rewarded beyond their contributions to the organization.
Expectancy theory	People become motivated when there is a high probability of achieving desirable goals.

tions where good leadership can change the performance of a worker or an organization. One explanation for the change in behavior from Theory X to Theory Y is the self-fulfilling theory of human behavior (Cf. J. L. Single). This is the idea that positive or negative expectations will significantly influence worker behavior. Thus, according to the theory, a unique characteristic of superior leaders and managers is their ability to create high performance expectations that the workers fulfill.

Another theory based on innate human behavior is the reinforcement theory. It proposes that people's behavior can be explained in terms of positive or negative past outcomes. Thus, by rewarding desired behavior and punishing what is not wanted, managers can supposedly control the behavior of their workers. Psychologist B.F. Skinner even suggests that by making use of punishment and rewards over a period of years, people can be controlled and shaped while still feeling free. Although this theory is strong justification for managers practicing organizational behavior modification, it has been criticized as being manipulative and autocratic (Mondy, 296). It also assumes that motivation comes from the

environment and is external to the person, overlooking the simple fact that people are rational, thinking entities who control their own actions.

Some theories attempt to explain motivation as the drive to satisfy personal needs. They are called needs theories of motivation. Table 2 compares four of these theories. These theories propose that motivation occurs when a person attempts to satisfy the lowest unsatisfied need. For example, if workers perceive their jobs as dangerous, they will attempt to satisfy the need for safety and thus will be motivated to change their environment to make it safer. They will concentrate their efforts in activities that satisfy their unfulfilled need for a safer environment (the lowest unsatisfied need) before attempting to fulfill any higher need for creativity. Most U.S. workers, according to Abraham Maslow, have satisfied the two lower needs (physiological and safety) to the point where their focus has shifted to the higher needs (belongingness, self-esteem, and self-actualization).

According to Frederick Herzberg's needs theory of motivation, human needs can be grouped into hygiene needs and motivators. Hygiene needs do

Table 2. Comparison of Needs Theories of Motivation

Frederick Herzberg's	Abraham Maslow's	Clayton Alderfer's	David McClelland's
Hygiene(demotivators) <ul style="list-style-type: none"> • Pay • Status • Working conditions • Fringe benefits • Policies and regulations • Interpersonal relations 	Physiological <ul style="list-style-type: none"> • Air, water, food, etc. Safety and security <ul style="list-style-type: none"> • Danger and job security Belongingness and love <ul style="list-style-type: none"> • Group acceptance 	Existence <ul style="list-style-type: none"> • Air, water, food, and safety Relatedness <ul style="list-style-type: none"> • Interpersonal relations 	Need for affiliation <ul style="list-style-type: none"> • Friendship and social activities
Motivators <ul style="list-style-type: none"> • Meaningful and challenging work • Recognition for accomplishments • Feeling of achievement • Increased responsibility • Opportunity for growth and advancement 	Self-esteem <ul style="list-style-type: none"> • Achievement recognition, and status Self-actualization <ul style="list-style-type: none"> • Use of creative talents 	Growth <ul style="list-style-type: none"> • Promotions, salary, and autonomy 	Need for achievement <ul style="list-style-type: none"> • Challenge and goal oriented Need for power <ul style="list-style-type: none"> • Influence and domination

not motivate, but they can create dissatisfaction and can thus be strong demotivators. Managers must be constantly alert to ensure that these are not hurting the organization. On the other hand, motivators can encourage the superior performance that will result in organizational synergism. The leader or manager is also responsible for using these motivators to benefit the organization.

In McClelland's needs theory of motivation, everyone has three needs: achievement, affiliation and power. But for each person, one of these needs is relatively stronger than the others. Entrepreneurs and salespeople, for example, have a high need for achievement, whereas the best managers have more moderate achievement needs (Cf. M. J. Stahl). A strong need for achievement may actually impede effective delegation of tasks. Also, needs may be cultural, as in Japanese workers having a stronger need for affiliation than U.S. workers.

The relevance of McClelland's theory is that, depending on a person's needs, incentives may be effective or ineffective. For example, a strong need for achievement may require more auto-

my, but a preference for affiliation would dictate team involvement. Hence, if a brilliant researcher with high achievement needs is required to participate in committee work, he or she may not see such a request as beneficial or desirable even if the committee's function is important to the organization. Similarly, a strong team player may feel out of place in a position of team leadership with responsibility for difficult personnel actions such as firing and performance evaluation. In both cases persons with different needs may eagerly pursue those responsibilities.

Nevertheless, in addition to theories based on human needs, other explanations for motivation have been proposed. The equity theory, credited to J. Stacy Adams, states that people base their performance on the correctness of their perceived situations. They do this by comparing their performance and rewards with those of others.

Thus, a worker may decide to stop working "hard" because someone else may get similar or greater rewards with less effort. This inequity may or may not be real, but it is the person's perception that motivates or demotivates. Hence, it is important for an organization to have fair and

open reward and promotion systems. It is also important to communicate to all employees the specific reasons for promotions and rewards.

Another theory of motivation known as expectancy theory was developed by Victor Vroom and modified by Barry Staw. It has dominated research in this field since the early 1970s. The theory states that people are motivated by the probability of achieving desirable goals. To explain motivation, expectancy theorists use the formula:

$$\text{Motivation} = E \times V \times I$$

where E denotes expectancy, the probability that effort will lead to performance; V denotes valence, the desirability of the predicted outcome; and I denotes instrumentality, the perception that rewards are tied to performance.

The expectancy theory gives managers useful guidelines for improving the motivation of their workers. First, training may be used to increase expectancy. Second, any of the needs theories listed in Table 2 will provide guidelines for increasing valence. For example, people with high security needs will value pension plans and job security guarantees, whereas those with self-actualization needs may require challenging assignments or a creative environment. Finally, to maintain instrumentality at a high level, the reward system must be fair and open, with good communication between management and workers.

Today's high-technology professionals have been characterized as highly educated, autonomy seeking, and career motivated rather than company dedicated (Cf. Glinow). Their allegiances are suspect, and they are quick to change employers in search of technical challenge or more autonomy in their work (Bailyn and Raelin). They expect to be rewarded for their work and expertise, and they abide by ethics dictated by their professional groups and not by their employers. In short, their

ties to their professional peers are stronger than those to their employers.

These professionals are motivated by different needs than those of their organizational counterparts, including managers and other support personnel (see Table 3). Numerous surveys have found that technical professionals get the most satisfaction from challenging work, autonomy, and variety of work assignments but that managers are challenged primarily by the opportunity for promotion (Cf. Resnick). Managers, by training and personality traits, prefer predictability and control in their areas of responsibility, but technologists thrive in a challenging and changing technical environment.

Table 3. Motivators: Rewards Most Valued By High Technology Professionals

Reward	Motivator
Professional	Opportunity to work with top-flight professionals Freedom to make own decisions Intellectually stimulating work environment Forward-looking organizational goals Ability to affect national goals and policy
Job content	Productive atmosphere Flexible work hours Long-term project stability Opportunity to address important human needs Patriotic projects Projects of altruistic nature
Career	Work for a leading-edge company Diverse opportunities for personal growth and advancement Opportunity for self-expression Opportunity to play a role in the company's future Opportunity to participate in technological breakthroughs
Social status or prestige	Desirable location Open-door management Recreational facilities
Financial	Twice-yearly salary reviews Compensation for unused leave Cash bonuses

■ Motivators and Demotivators for Scientists and Engineers

The rewards listed in Table 3 address motivators as defined by Herzberg. Demotivators or hygiene factors related to security and affiliation needs have been identified by Resnick-West and Von Glinow and are listed in Table 4. Demotivators arise because the needs of the organization sometimes conflict with the needs of the professional. If a proper balance between these two diverging sets of needs is not found, both the organization and the professionals will suffer.

Table 4. Demotivators: Culture Clashes Between Professionals and Organizations

Category	Organization	Professional
Experts clash	Hierarchical control ("The boss is right")	Expert control ("Let experts decide")
Standards clash	Company policies/rules	Professional standards
Ethics clash	Company secrecy	Dissemination of information
Commitment clash	Company loyalty	Loyalty to profession
Autonomy clash	Organizational decision-making	Desire for autonomy

Donald C. Pelz conducted research to determine what made researchers productive. He concluded that some degree of creative tension between sources of stability and security and sources of disruption was needed to raise researchers' productivity. Table 5 summarizes the eight creative tensions that he identified. These tensions allow researchers to question and gauge the usefulness of their work in the real world. Data supporting the influence of these tensions on researcher productivity confirms this.

Pelz's research shows that scientists and engineers increase their technical contributions when each performs more than one task simultaneously

and has more than one area of specialization. With multiple specialties, the enhanced performance is directly proportional to the number of specialties. With multiple tasks the enhanced performance continues until the researcher has four simultaneous functions or projects. Additional tasks may result in over commitment and inefficiencies that will be detrimental to performance. Similar results plot the performance of scientists and engineers as a function of the decision-making sources including supervisor, project managers, peers, and upper management. Effectiveness correlates strongly to the number of decision-making sources the researcher has to consider.

This appears to contradict the theory that researchers will perform best when isolated from distractions. Apparently, the cross-fertilization of ideas and interpersonal relationships that are possible when a researcher is involved in a limited number of projects with more than one source of direction, before making a decision, has a positive and desirable influence on his or her output. This synergism should be the goal of any research organization.

Additional research by Pelz illustrates the nature of goal-setting synergism. Performance is higher for scientists when the goals are set by the scientist in conjunction with their supervisors than when they are set by the supervisor alone or by scientists alone. For engineers, effectiveness is maintained even when working alone or only with peers. This result may reflect the more product-oriented work performed by engineers. Two lessons appear evident from such research. First, when the goal is clear, motivated workers will achieve it with or without the help of management. Second, when the goal is not clear, the best results are achieved when both the manager and the worker jointly define the task. More research shows that although too little autonomy is not conducive to high productivity, complete independence is not the optimum either. Again, this reinforces the theory that interaction is a neces-

Table 5. Eight Creative Tensions

SECURITY	CHALLENGE
Tension 1- Multiple Tasks	
(None listed in the literature)	Effective scientists and engineers in both research and development laboratories did not limit their activities either to pure science or to application but spent some time on several kinds of R&D activities, ranging from basic research to technical services.
Tension 2 - Interaction	
Effective scientists were intellectually independent their or self-reliant; they pursued their own ideas and valued freedom.	But they did not avoid other people; they and their colleagues interacted vigorously.
Tension 3 - Multiple Skills	
(a) In the first decade of work, young scientists and engineers did well if they spent a few years on one main project.	But young non-Ph.D.s also achieved if they had several skills, and young Ph.D.s did better when they avoided narrow specialization.
(b) Among mature scientists, high performers had greater self-confidence and an interest in probing deeply.	At the same time, effective older scientists wanted to pioneer in broad new areas.
Tension 4 - Autonomy	
(a) In the loosest departments having minimum coordination, the most autonomous individuals with maximum security and minimum challenge were ineffective.	More effective were those persons who experienced stimulation from a variety of external or internal sources.
(b) In departments having moderate coordination it seems likely that individual autonomy permitted a search for the best solution to important problems faced by the organization.
Tension 5 - Influence and Goal Setting	
Both Ph.D.s and engineers contributed most when they strongly influenced key decision-makers but also when persons in several other positions had a voice in selecting their goals.
Tension 6 - Interaction	
High performers named colleagues with whom they shared similar sources of stimulation (personal support) but they differed from colleagues in technical style and strategy.
Tension 7 - Teams	
R&D teams were of greatest use to their organizations at that "group age" when interest in narrow specialization had increased to a medium level but interest in broad pioneering had not yet disappeared.
Tension 8 - Interaction	
In older groups that retained vitality the members preferred each other as collaborators yet their technical strategies differed and they remained intellectually combative.

sary ingredient in achieving high productivity in a research environment.

These results seem to agree with Rensis Likert's research in service organizations. He concluded that high performers felt that they had a great deal of influence in setting their own goals but that the higher echelons had much influence also. He noticed that there was more total influence on high performers than on low performers. This observation conflicts with some older theories of organizational behavior that assume a fixed quantity of influence to be shared between management and workers. These older theories propose that if workers have more influence, management has less. Likert and Pelz propose that more total influence is possible and desirable.

The clear message in these observations is that in research organizations, higher performance requires interaction between members of the organization. Additional research repeats the message and shows clearly that daily interactions are better than less frequent interactions. This conclusion also applies to projects, especially in the early stages of concept definition. Finally, the research shows that the best interactions are consensus and influence as opposed to autocratic management, where the manager alone determines the goals of the workers.

Interaction between organization members can be encouraged by promoting participation in committees and project teams. The practice of concurrent engineering, for example, requires teams that include representatives from research, marketing, engineering, production, and others, according to Carter and Baker. The fast and unconstrained interaction by these specialists in a small work group allows quick identification of key issues and agreement on the best solutions. The result is reduced development time for new products (Cf. Sprague et al.).

Further research shows that work groups tend to become less effective with the passage of time.

Their performance decreases because interaction decreases. As group members get to know each other, interactions become predictable, reducing the need for consultation and idea exchange. Old groups may run out of new ideas. Management should be on the lookout for teams and committees that need overhauling.

These results are consistent with the research by Vollmer et al. Their work is summarized in Figures 1 and 2 for an aerospace industry research laboratory and for a government defense research laboratory, respectively. The vertical axis in the chart, general job satisfaction, contains the hygiene factors; the horizontal axis, professional productivity, contains the motivation factors. The charts are constructed so that issues can be evaluated for their effect on satisfaction (hygiene) and productivity (motivation). For example, in both cases, productivity and satisfaction are associated with freedom to influence the choice of research assignment. Adequate salary is not a factor in productivity but may be a factor in job satisfaction. An inadequate salary will cause dissatisfaction, but salary in excess of that which causes satisfaction will not produce more satisfaction. Clearly, salary is a hygiene factor.

■ Motivation of NASA Employees

To study the validity of the motivation theories discussed previously, the results of two culture surveys of NASA employees were analyzed. The responses in the surveys were compared with the theories to determine which theories best explain the results. The first survey was performed in December 1986 and the second in the spring of 1989. In the interim, Agency management implemented new procedures to change the NASA culture in a positive way.

The results from the surveys are included in Figures 3 to 11. Figure 3 describes the rating system for the questions. A maximum of 5 was possible for each question. A rating of 1 means that the statement is not perceived as true by the per-

PROFESSIONAL PRODUCTIVITY		
G E N E R A L J O B	IS ASSOCIATED WITH	IS NOT ASSOCIATED WITH
	<ul style="list-style-type: none"> • Freedom of choice in research assignments • Consultation with management on research decisions • Opportunity for promotion in own research field 	<ul style="list-style-type: none"> • Opportunity to do interdisciplinary research • Opportunity to do research with members of own discipline • Opportunity to do basic research • Adequate salary • Adequate technical assistance • Opportunity for promotion into management positions • Freedom in day-to-day research activities
	None	<ul style="list-style-type: none"> • Opportunity to do applied research • Opportunity to keep up-to-date on scientific developments • Adequate funds to support individual research interests • Adequate laboratory equipment • Opportunity to persuade sponsor to support research • Recognition by name in connection with research • Opportunity to attend professional meetings • Opportunity to publish research findings
Figure 1. Incentive in Relation to Professional Productivity and General Job Satisfaction for Applied Researchers		
S A T I S F A C T I O N	IS ASSOCIATED WITH	IS NOT ASSOCIATED WITH
	<ul style="list-style-type: none"> • Freedom of choice in research assignments • Opportunity to do basic research 	<ul style="list-style-type: none"> • Opportunity to do applied research • Recognition by name in connection with research • Opportunity to attend professional meetings • Opportunity for promotion into management positions • Freedom in day-to-day activities
	None	<ul style="list-style-type: none"> • Opportunity to do interdisciplinary research • Opportunity to do research with members of own discipline • Consultation with management on research decisions • Opportunity to keep up-to-date on scientific developments • Adequate technical assistance and laboratory equipment • Adequate funds to support individual research interests • Adequate salary • Opportunity to publish research findings • Opportunity for promotion in own research field
Figure 2. Incentive in Relation to Professional Productivity and General Job Satisfaction for Staff Scientists		

son answering the question. A rating of 5 means that the statement is perceived as completely true. The aggregate responses in the figures represent such a large number of responses that small variations are significant and meaningful. For example, a response of 4.3 is more true than 4.2.

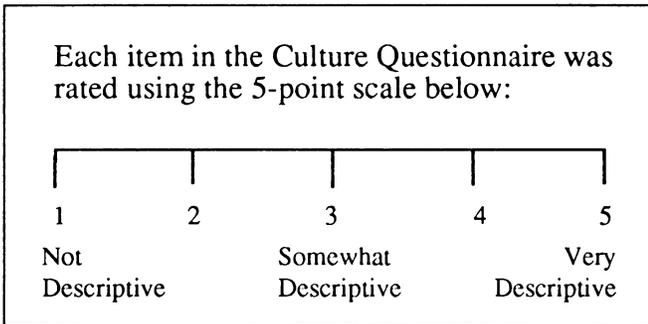


Figure 3. Culture Surveys Rating System

Figures 4 to 6 present the ratings for questions about work satisfaction, work unit climate, and NASA culture. Figure 4 shows that NASA employees are very proud to work for the Agency. The rating is 4.4 out of a possible 5.0. But the responses are not as high for the Center, the work unit and the job. Although the ratings are significantly higher than “somewhat descriptive,” there is a steady decline from NASA to the Center, to the work unit, to the job.

This situation represents an opportunity and a challenge to Agency management. The goal should be to raise the level of employee satisfaction with the job, the work unit, and the Center to the level of satisfaction and pride resulting from association with NASA. This is possible because employees are favorably disposed to work for the Agency. The solution is to make unit managers aware of the situation and to give them the skills to fulfill their workers’ expectations. If the issue of satisfaction is addressed at the work unit level, a successful outcome will be felt in increased satisfaction with the job, the Center and NASA.

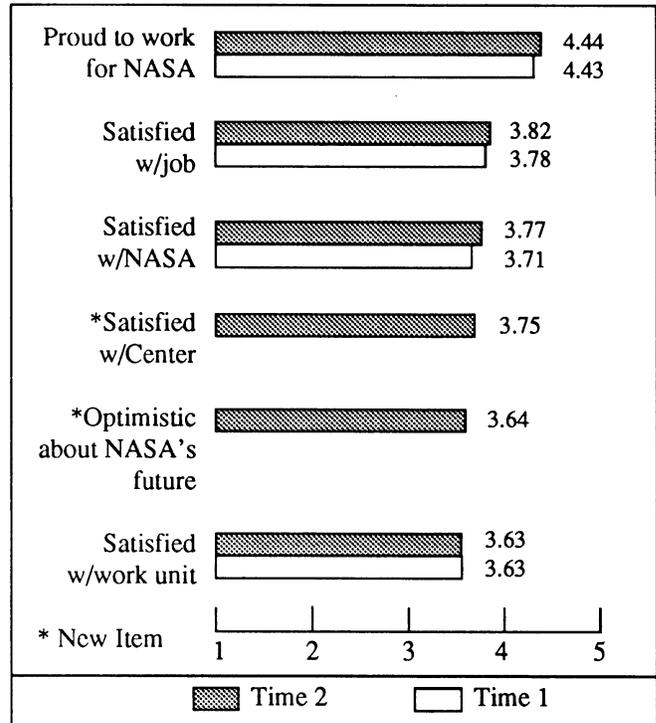


Figure 4. Survey Results: Work Satisfaction

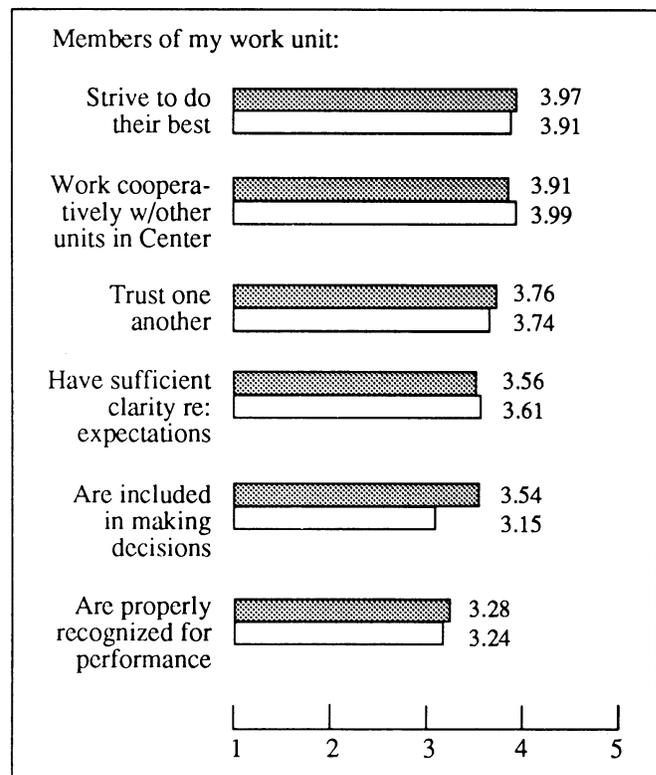


Figure 5. Survey Results: Work Unit Climate

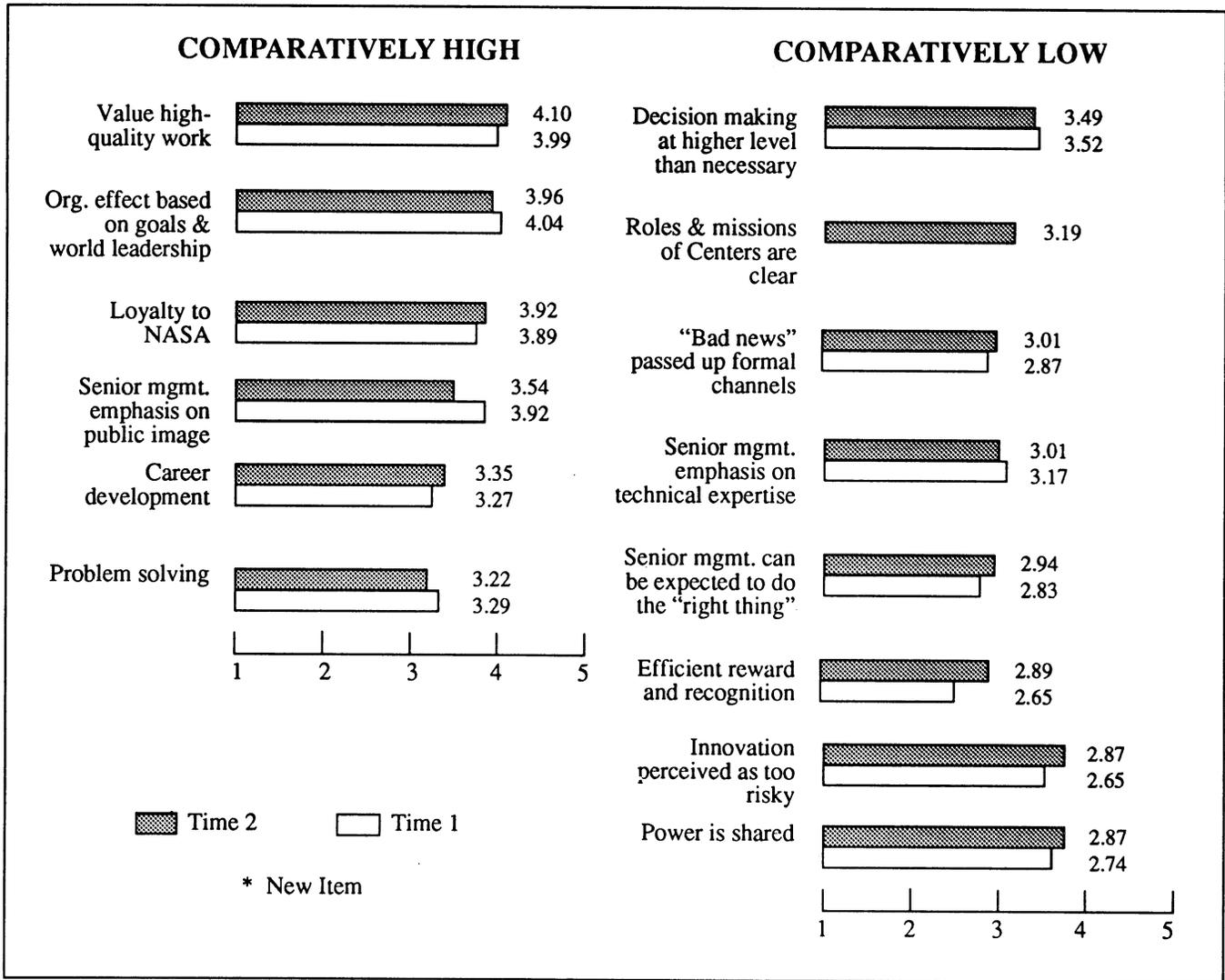


Figure 6. Survey Results: NASA Culture

Both surveys indicate that employee satisfaction is high and that employees are optimistic about the future. A picture emerges of a work force that is materially satisfied, all things considered, and feels secure in its jobs. Figure 5 shows that teamwork is accepted, and that people trust and respect their coworkers but that management is not communicating goals and expectations with clarity. Also, unit members would like to get more recognition for their work. Note that on the question "Members of my work unit are included in making decisions that affect their work" the rating was 3.15 in the first survey and 3.54 in the second, a large improvement.

Apparently, the efforts to change the NASA culture after the Challenger accident were successful and have resulted in more low-level participation in the decision-making process.

Figure 6 deals with perceptions of the NASA culture. It is not surprising that most responses agree that the Agency values high-quality work and world leadership. Loyalty to NASA is also perceived as being part of the NASA culture. A significant drop is noted for "career development" and an even lower rating was recorded for "sufficient reward and recognition." Therefore, these two areas are not perceived as being important in

the NASA culture. Both areas address the self-esteem and self-actualization needs of the employees and offer an opportunity for NASA management to motivate the work force.

The significant increase in participative management could be improved further. Responses to "power is shared" yielded a 2.87 rating, which is less than "somewhat descriptive." This is corroborated by the perception that decision-making takes place at higher levels than necessary, a 3.49 rating, which is "somewhat descriptive."

Figures 7 to 11 compare what NASA employees perceive to what they think should be. Under the column "what is," the high quadrant lists responses with ratings higher than 3.5 and the low quadrant lists items ranked lower than 3.5. The "what should be" high quadrant gives the percentage of responses that listed that item. For the NASA culture (Figure 9), the "what should be" includes three self-esteem and self-actualization motivators and two standards-clash issues. The respons-

	WHAT IS:	WHAT SHOULD BE:
HIGH	Value high quality work	Value high-quality work - 66%
	Value excellence	Maintain expertise within NASA - 60%
	Expect long NASA career	Value excellence - 57%
	Base effectiveness on image as world leader	Sufficient individual reward/recognition - 50%
LOW	Employees very loyal to NASA	Clear roles/missions of NASA installation - 43%
	Sufficient individual reward and recognition	
	Agency senior management can be expected to do the right thing	
	People willing to share power	
	Clear roles and missions of NASA installations	

Figure 7. NASA Culture Comparison

es value "high-quality work" and "value excellence" demonstrate pride in the work done and address fulfillment of self-actualization needs. "Sufficient individual reward and recognition" is a self-esteem issue. "Maintain expertise within NASA" probably refers to the practice of subcontracting certain tasks. Subcontracting is an Agency policy and can be classified as a standards clash. "Clear roles" is also a standards clash. Both standards clashes are demotivators.

The "what is" responses "value high-quality work" and "value excellence" in Figure 7 are in agreement with "what should be." However, the "what is" column also includes some hygiene needs, such as "expect long NASA career" (safety and security) and loyalty to NASA (belongingness). That these needs do not appear in the "what should be" column indicates that hygiene needs have ceased to concern NASA employees. A disconnect in Figure 7 is the importance of "sufficient individual reward and recognition." It is ranked low in the "what is" and high in the "what should be." This self-esteem need apparently is not being met and would be a strong motivator. This same message is repeated in Figure 10.

Figure 8, which addresses decision-making, shows a strong demotivator, an experts clash. The "what is" column includes budget and scheduling, typical management concerns. But the highest ranked "what should be" is "decisions based on research not politics," which is ranked low in the "what is" column. This clash can be addressed by delegating to technical personnel the authority and accountability for meeting budget and schedule constraints.

Figure 9, which addresses power sharing, repeats the experts clash observed in the decision-making process: "people (presumably management) quietly hold onto their power," and "authority is highly centralized." This clash too can be addressed by delegating authority and accountability to lower levels of the organization. Apparently, NASA management is not delegating

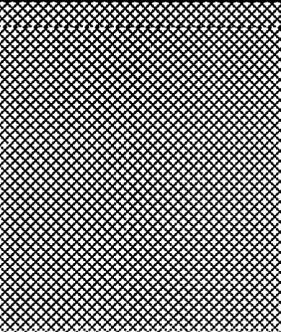
	WHAT IS:	WHAT SHOULD BE:
HIGH	Budget pressures greatly affect decisions	Decisions based on research, data, technical criteria; not politics - 39% Decisions based on open discussion/debate - 19% Implementors involved in decisions - 14% Mgmt. communicates decisions and rationale to employees - 9%
	Schedule pressures greatly affect decisions	
LOW	Decisions delegated to lowest possible level	
	Decisions based on research, not politics	
	Management communicates decisions and rationale to employees	
	Decisions based on open discussion and debate	

Figure 8. Decision-making Comparison

enough to satisfy the high expectations of the work force even though progress was made during the time between surveys.

Figure 10 covers the subject of rewards, a self-esteem need. The “what is,” stating that rewards are political, stands in sharp contrast with “what should be,” which is recognition for individual and work unit performance. Clearly, NASA management can enhance the morale and motivation of the work force and encourage better performance by upgrading the reward system.

Another disconnect that appears in Figure 10 is the statement that “people orientation is important for advancement.” This is ranked high in the “what should be” and low in the “what is.” This may explain the previous finding that people are

less satisfied with the unit and the Center than with NASA. It appears that the supervisor-employee interaction is one of demotivation. The implication is that unit managers must be sensitized to the human needs of their employees.

A significant unmet need is career satisfaction. In Figure 11 the “what should be” responses present the message that clearly defined career paths are expected. These expectations are not always satisfied. The following two disconnects are present: “managers take time to discuss career planning” and “there are viable career paths for non-supervisory employees” are both ranked high in the “what should be” and low in the “what is.” The third disconnect, “higher level manager taking personal interest,” can also be explained as a reflection of the same desire for formal and clear career paths.

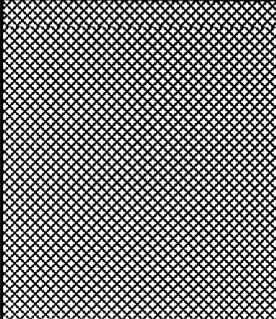
	WHAT IS:	WHAT SHOULD BE:
HIGH	People with technical knowledge can get things done	People are willing to share their power - 39%
	People quietly hold onto their power and authority	People with technical knowledge can get things done - 23%
	Authority is highly centralized	Employees are treated fairly and equitably - 21%
LOW	Employees are treated fairly and equitably	
	People are willing to share their power	

Figure 9. Power-sharing Comparison

	WHAT IS:	WHAT SHOULD BE:
H I G H	Real reward is work itself	For individual performance there is recognition and reward - 40%
	Getting rewarded is political	People orientation is important for advancement - 17%
		For work unit performance there is recognition and reward - 14%
		Real reward is work itself - 12%
L O W	People orientation is important for advancement	
	For individual performance there is recognition/reward	
	For work unit performance there is recognition/reward	
	PA system provides useful discussion of work performance	

Figure 10. Rewards Comparison

Conclusions

Table 6 lists some lessons that have been learned from this research. First, it can be concluded that the needs theories of motivation, especially Herzberg's and Maslow's, agree with the results of the NASA culture surveys. The responses to the surveys appear to indicate that NASA employees are satisfied in their hygiene needs and are striving to satisfy self-esteem and self-actualization needs. The most significant observation is that the need for belonging is satisfied. NASA employees are proud to be part of the Agency and have a high opinion of their coworkers. With their belonging needs satisfied, NASA employees enjoy a greater degree of employment satisfaction than the general population (Cf. Mondy, 298). The consequence is that to motivate their employ-

	WHAT IS:	WHAT SHOULD BE:
H I G H	Higher level manager taking personal interest is important for advancement	Career management is shared responsibility of employee and manager - 39%
	Managers are encouraged to attend formal development activities	Managers take time to discuss career planning with their people - 19%
	Career management is shared responsibility of both employee and manager	There are viable career paths for non-supervisory managerial employees - 14%
L O W	Managers take time to discuss career planning with their people	
	There are viable career paths for non-supervisory/managerial employees	
	There are people at the Center who provide career guidance and counsel	

Figure 11. Career Satisfaction

ees, NASA managers must address self-esteem and self-actualization needs. Two possibilities are recognizing accomplishment and establishing better and clearer career growth paths. The first addresses self-esteem and the second, self-actualization. More consistent use of these motivators would result in a more productive organization.

In the two culture surveys NASA employees sent a clear message that not enough is being done in the areas of recognition and career planning. This deficiency should be remedied because recognition and career growth are the most important sources of satisfaction and motivation for older and more experienced workers. Career growth need not mean a move into management. The dual-ladder option, where opportunities for promotion to higher grades are available to non-supervisors, is a good alternative. What is important is that employees know that they are moving toward a desirable career goal.

Table 6. Motivation of Employees: Lessons for NASA Managers

THEORY AND SURVEY RESULTS	LESSON
<p>1. Needs theories describe the behavior of NASA employees: NASA employees have satisfied their hygiene needs (safety, security, and affiliation).</p> <p>NASA employees strive to satisfy self-image and self-actualization needs. Managers should not confuse hygiene and motivation needs</p>	<p><i>Continue present practices in areas of safety, job security, and team building.</i></p> <p><i>Address self-image with awards; address self-actualization with career planning. Note that pay and performance appraisals are hygiene factors and will not motivate. Use professional growth as a motivator.</i></p>
<p>2. Needs theories give an indication of valence V (i.e., desirability of the outcome) in the expectancy theory.</p> <p>Motivation = E x V x I</p>	<p><i>Identify and address the needs of employees.</i></p>
<p>3. Employee training is important because of expectancy E (i.e., the effort leads to performance).</p> <p>Motivation = E x V x I</p>	<p><i>Continue and expand training programs.</i></p>
<p>4. Communication is important because of instrumentality I (i.e., rewards are tied to performance).</p> <p>Motivation = E x V x I</p>	<p><i>Use newsletters and awards ceremonies to celebrate significant accomplishments.</i></p>
<p>5. NASA employees are dissatisfied with the lower levels of the organization. Employees want people-oriented managers.</p>	<p><i>Make unit managers more sensitive to the needs of employees. Develop training programs.</i></p>
<p>6. NASA has made progress in implementing participative management, but practices are still below employee expectations.</p>	<p><i>Expand manager's awareness and training.</i></p>
<p>7. Work groups are desirable because they promoted interaction. But groups in existence for long period of time lose effectiveness.</p>	<p><i>Use concurrent engineering, quality circles, and teamwork. Reorganize teams and committees periodically and add new members.</i></p>
<p>8. More outside influence is better than complete autonomy.</p>	<p><i>Negotiate goals and objectives between manager and employee. Review periodically.</i></p>

An important consequence of accepting the needs theories of motivation is a reduced dependence on salary as a motivator. Of course money is important, and workers that are not compensated fairly for their efforts can be unhappy and demotivated. As stated by the equity theory of motivation, the perception of fairness in compensation can be an important factor in demotivation. But after acceptable compensation is reached, other factors can be more effective in promoting superior per-

formance and excellence. Thus, reliance on “pay for performance” as a motivator overlooks more effective approaches. Similarly, performance reviews are not motivators. Some good reasons for having performance reviews include the opportunity for the manager to communicate to the employee the goals of the organization and for the employees to state their own. But it is unreasonable to expect the performance appraisal process to be a source of motivation.

The expectancy theory of motivation is an important extension to the needs theory. By proposing expectancy E, valence V, and instrumentality I as the causes of motivation, the theory gives practicing managers a good indication of what needs to be done to motivate subordinates. In addition to addressing the basic human needs, managers must support training and be fair and open when awarding promotions and rewards.

The responses to the surveys show that NASA employees are unhappy with the lower levels of the organization. Specifically, they want unit managers to be more people oriented. Unit managers must do a better job of career counseling and they must do more to make participative management a reality. These issues can and should be addressed through training programs for supervisors.

■ Lessons for NASA Project Managers

Finally, work groups are good and should be encouraged as much as possible. The same is true for all kinds of interactions, such as project and task reviews and staff meetings. They should be used by management to promote interaction between employees.

Projects are high-intensity, goal-oriented endeavors. In the course of day-to-day activities the project manager and staff must continuously rank all the demands on the limited resources available to the project. In such an environment it is easy to rank employee needs and motivation below other more immediate concerns, such as schedules and cost targets. This is not done on purpose, and the assumption usually is that the sacrifices are temporary and needed to achieve a short-term goal.

Unfortunately, designating employee needs and morale as issues of secondary importance is detrimental to the project's host organization. An example of this was reported in *The Soul of a New Machine* (Kidder, 1981). The book records

the design and development of a new computer. Although all the technical goals were achieved in record time, the feat was accomplished at great cost to the organization because one year after the new computer was introduced, all the members of the design team had left the company.

Although this may be an extreme example, anyone with project experience can give examples of poorly motivated people working well below their capabilities. If management truly believes that employees are the organization's most valuable resource, this situation is not acceptable.

The use of surveys allows project managers to track the status of their team, the human portion of their system. The important results of these surveys are the trends, and, therefore, surveys must be repeated periodically. The survey is analogous to the feedback signal in a control system. By continuously monitoring the attitudes and motivation of a team, a project manager can take a proactive approach to problem solving. An example of an attitude survey is included in Figure 12; it is kept short to compensate for the frequency of survey repetition.

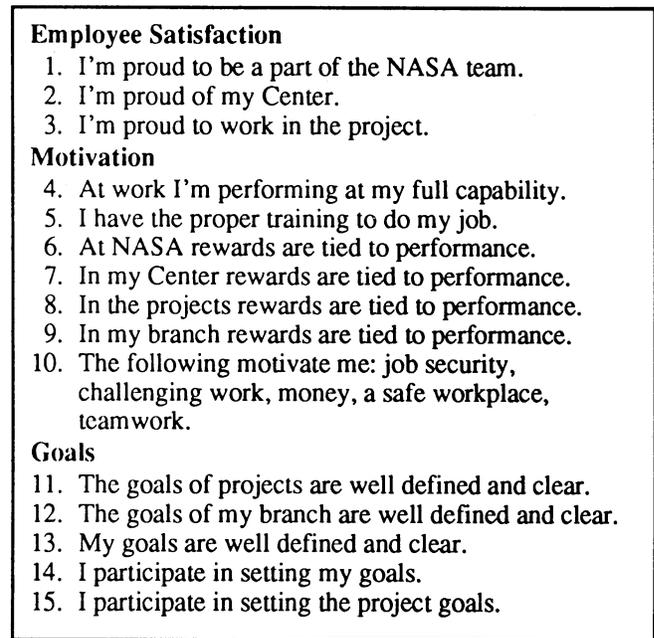


Figure 12. Sample Survey

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Where are the Real Engineers?

by G. Harry Stine

Recently, I have been involved as a consultant on an engineering project. I'd prefer not to mention names because this column is likely to get a little rough on some of the people I worked with. In any event, the purpose of this column isn't to point fingers but to reveal a disturbing trend. Maybe something can be done about it.

The goal of the project was to build something and test it. The device did not exist, although a lot of studies and many technical papers have been written about it in the last 40 years. The first goal was to design, build and operate a proof-of-principle prototype. This would be a cheap and dirty off-performance piece of ironmongery using off-the-shelf technology and hardware. It would be used to check out some of the questionable approaches to the solution, find out if the approach was really workable, discover the items that are always overlooked even in the best designs, and then allow the company to proceed with the pre-production device with a higher degree of confidence and a lowered level of risk.

Furthermore, it had to be done on a total budget that was embarrassingly small and on a time schedule that was impossibly short.

Briefly, this approach is standard, old-hat, everyday engineering that you use when you are trying to do something new and different. No big deal, right? Wrong!

In this particular industry, no one had been allowed to make a mistake in the last 30 years. Everything had to work perfectly the first time. Everything had to be a success when the switch was flipped or the button pushed.

It has been a fascinating experience to watch the way both experienced old-time engineers (who

are now managers) and fresh-caught engineers tackled the project.

The old-time engineers had to battle two decades of on-the-job experience. Tattooed on their brains was the dictum: "Thou shalt not fail, it must work the first time, and thou hast no room for error."

Well, that attitude can be handled because these older engineers remember the time when it wasn't that way. It's not too difficult for them to shift mental gears and get back to the old method that amounts to: "Well, hell, let's just whomp up a boilerplate test model of this puppy and see if it passes the smoke test when we plug it in!" That's what engineering used to be all about, and it's one of the factors that made it fun.

Engineering used to operate on the principle, "Experience gained is directly proportional to the amount of equipment ruined."

Then you could forge ahead to design and build stuff that would not bust. Prototypes were not worth a damn unless you busted them. Otherwise, you would underestimate yourself and did not need the prototypes at all.

Once that ancient principle was reestablished in the minds of the engineering management, the project became fun. But it did not make it any less stressful. The lack of big money and the short deadlines kept the pressure on. I could see the gradual metamorphosis of the older engineers (of which "I are one," too).

The real problems came with the young engineers who had recently (within the last ten years) received their engineering degrees. The young engineers were brilliant when it came to design work. They knew how to run computer analyses

until the floor was covered with printouts. They were whizzes with CAD.

But they had never “bent tin.” They had never been responsible for designing something that could be built and was supposed to do something.

This puzzled me at first. Then I figured out what had happened.

Fifteen years ago, my son decided he wanted to be an engineer so he could become a product designer. So we went to several colleges and universities to see what their engineering curricula, facilities, and teaching staff amounted to.

Turns out that something had changed in Engine School.

Two career paths existed (and still exist) for engineers.

An engineering degree now consists of an extremely strong emphasis on scientific theory, mathematics, and computer technology. And practically no hands-on laboratory work! The venerated engineering degree has been converted into a degree in applied science!

On the other hand was the path leading to a bachelor’s degree in “engineering technology.” Upon close investigation, I discovered that this poor stepchild of modern undergraduate study was indeed the sort of hands-on, practical engineering curriculum that I was familiar with back at mid-century. But it no longer turned out “engineers.” It graduated lowly “engineering technologists.”

Aha! No wonder that some of the modern products of engineering seemed to be less than elegant in their design, construction, and operation! They have, essentially been designed by scientists, not engineers! The real grubby-handed engineers, now called “engineering technologists,” have come along after the “engineers” are finished.

The engineering technologists are the ones who have had to make the damned product work after the design has been approved!

(I have nothing against scientists. In fact, my degree is in physics, not engineering. Scientists are needed to explain why something works after inventors conceive it and engineers make it work. Yes, some modern products have sprung from the science lab. But far more of them have come from inventors.)

Robert A. Heinlein, an engineer himself as well as an eminently practical scientist, put it very well in *The Rolling Stones* in 1962: “Fiddle with finicky figures for months on end—and what have you got? A repair dock. Or a stamping mill. And who cares?” Hazel Stone missed one of the existential joys of engineering: Pride in making it work the way it is supposed to.

Dr. Wernher von Braun was one of the best real engineers I have ever known. I saw him do engineering right out on the test stands with the technicians. When I read his biography, I understood why.

Von Braun studied at Charlottenburg Institute of Technology, Germany’s equivalent of MIT and Cal Tech. As part of his education, he was apprenticed to the Borsig Werk. There, an old foreman handed him a chunk of iron about as large as a child’s head. He also gave von Braun a file and pointed to a bench vise. He was told, “Here are your tools. Make this into a perfect cube. Make every angle a right angle, every face perfectly flat and smooth, and every side equal.”

Five weeks later, von Braun had filed the chunk of iron into the required perfect cube that had become the size of a walnut. *But size had not been specified!* Borsig then put him to work on a lathe, on a shaper, in the foundry, in the forge, and finally in the locomotive assembly sheds. Von Braun later recalled that he had gotten more

insight into practical engineering during that apprenticeship period than he had in any semester in the university.

Today, von Braun would have received a degree in engineering technology, not engineering.

And the engineers involved today in the project I used as a nameless example are learning the hard way what engineers used to learn in undergraduate work and their first few years in the field. They are having to bend tin against a schedule. They are having to make do with what they can get off the shelf. They do not have one thin dime for R&D. They are learning to read *Thomas' Register*. They are learning how to scrounge through junk yards to find something cheap that will do the job. They are facing a world where good enough is the enemy of the best, where an adequate solution today is far more important than a perfect solution tomorrow.

I am convinced both the old hands and the young pups will do just fine on the project. I expect them to destroy the prototypes but also to learn from that. And they are going to come out of the project as one of the best damned engineering teams in the industry. The company they work for has a long and proud history of building gadgets that work, making money for the company and the customer, and staying in service for decades. I will not have to tell you who they are; you will know.

Now, what are we going to do about this dichotomy of engineers and engineering technologists?

If I wanted again the challenge of putting together an engineering team to do things, I think I would be partial to hiring engineering technologists. One of our problems in the United States is our penchant to study things to death before risking our careers on the real possibility of a failure, regardless of whether it is an engineering job or a business deal. Yes, we have got to use our resources

wisely and do our best to succeed rather than fail. Be we have been studying things too much. Time to build and bust some prototypes.

We have got to stop studying things to death. We have got to be willing to bust prototypes. We have to get out there in the world, make things that work, *and produce them!*

I had a friendly controversy going on with Arthur C. Clarke, whom I had known for more than 40 years. I kept telling him, "Arthur, we are not all going to sit around in front of our computer terminals being creative and communicating with one another in the global village. Someone is still going to have to milk the cows!" (Or attach, remove, clean, and repair the milking machines.)

Another friend of mine, L. Sprague de Camp, begins his excellent book, *The Ancient Engineers*, thusly: "Civilization, as we know it today, owes its existence to the engineers. These are the men who, down the long centuries, have learned to exploit the properties of matter and the sources of power for the benefit of mankind."

We do not need to educate more scientists in America. We need more engineers. I think it is time we ended the experiment of calling educated applied scientists "engineers" and transitioned back to what we know works: Educating more grubby-handed "engineers with hairy ears and long and woolly britches," as the old and unprintable ditty goes.

Maybe we also need to adopt the European custom of permitting a real engineer to place before his/her name the honorific, "Ing." Then turn them loose to continue changing the world as they have for centuries.

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Resources for NASA Managers

by William M. Lawbaugh

■ What's New in the Program/Project Management Library Collection

Following is a list of books that have most recently been added to the PPM Library Collection. All of the materials may be borrowed through interlibrary loan from your Center Library. Call Jeffrey Michaels at (202) 358-0172 for further information.

Leadership Jazz

by Max Depree

Call number: HD57.7.D47 1992

Beyond Race and Gender

by R. Roosevelt Thomas

Call number: HF5549.5 .M5 T46 1991

Zapp! The Lightning of Empowerment

by William C. Byham

Call number: HD58.8 .H362 1989

The Age of Unreason

by Charles Handy

Call number: HD58.8 .H362 1989

The Goal

by Eliyahu Goldratt

Call number: PR9510.9 .G64 G6 1986

A Great Place to Work

by Robert Levering

Call number: HP5549.2 .U5 L385 1988

Enlightened Leadership

by Ed Oakley

Call number: HD57.7 .023 1991

A Whack on the Side of the Head

by Roger von Oech

Call number: BF408 .V58 1983

A Kick in the Seat of the Pants

by Roger von Oech

Call number: BF408 .V579 1986

Total Quality Training

by Brian Thomas

Call number: HF5549.5 .T7 T46 1992

The Wisdom of Teams

by Jon Katzenbach

Call number: HD66 .K384 1993

TQM Field Manual

by James Saylor

Call number: HD62.15 .S29 1991

Still More Games Trainers Play

by Edward E. Scannell

Call number: HM133 .S314 1991

100 Training Games

by Gary Kroehnert

Call number: T65.3 .K76 1991

Guide to Quality Control

by Kaoru Ishikawa

Call number: TS156 .G82 1982

Completeness: Quality for the 21st Century

by Philip B. Crosby

Call number: HD62.15 .C76 1992

Benchmarking: A Practitioner's Guide for Becoming and Staying Best of the Best

by Gerald J. Balm

Call number: HD58.9 .B345 1992

Continuous Improvement and Measurement for Total Quality

by Dennis C. Kinlaw

Call number: HD62.15 .K56 1992

Mining Group Gold

by Thomas Kayser

Call number: HD66 .K39 1990

Managing the Total Quality Transformation

by Thomas Berry

Call number: HF5415.157 .B47 1991

Are You Communicating?

by Donald Walton

Call number: P90 .W24 1989

Beyond the Hype: Rediscovering the Essence of Management

by Robert Eccles

Call number: HD31 .E27 1992

■ Book Reviews

Peak Performers:**The New Heroes of American Business**

by Charles Garfield (New York: William Morrow & Co., 1986)

“To me, Apollo 11 represented a temporary end to a peculiar form of discrimination,” says the author of *Peak Performers*, “the discrimination against being the best you can be.”

Charles A. Garfield worked for Grumman Aerospace on Long Island in 1967 when “going to work there every morning was like signing up for one of the great adventures on Earth.” As a novice computer programmer, he helped design and build the Lunar Excursion Module for Apollo 11. There and then Garfield decided to study “missions that motivate” and find out more about the high-level achievement he saw and experienced in the Apollo program.

After Apollo, Garfield left his career in mathematics and pursued a Ph.D. in clinical psychology. Along the way he interned in the cancer ward of a California medical center, thinking these patients were the exact opposite of the peak performers he met at Grumman and NASA. Wrong.

The courage to stay alive he saw there convinced him that the “focus of control” for peak performers was not external but internal.

In 1979 he had a chance meeting in Milan, Italy, with Soviet-bloc physiologists, doctors and research psychologists who changed the whole nature of his study. There experts from East Germany, Bulgaria and the Soviet Union told Garfield about the “psychophysics” used by their Olympic athletes to access their hidden reserves and “actualize their human potential.” They explained how peak performance can be learned—deliberately, systematically and predictably. “My heart started to pound,” says Garfield.

After some relaxation techniques, Garfield engaged in visualization exercises, imagining himself lifting 365 pounds of barbells confidently. He then actually did it, with their encouragement and to his amazement. He experienced the same exhilaration he felt during Apollo.

Over the years as a clinical psychologist, Garfield isolated six factors that constitute peak performance in athletics, business, government and the arts.

1. **Missions that motivate.** As JFK did for Apollo, someone gives the call to action that pulls people together for a common achievement. Some call it “inspiration.”
2. **Results in real time.** Intangible rewards along the route to the goal of a mission, such as meaning, satisfaction or a sense of improvement.
3. **Self-management.** Self control and self mastery towards a clearly defined goal. Some call it “discipline.”
4. **Team building.** Empowering others after self-mastery. Some simply call it “teamwork,” a buzzword of the 1990s.

5. **Course correction.** Finding and navigating a critical path. Change is not only inevitable but anticipated by peak performers.

6. **Change management.** This involves lifelong learning, expectations of success, mental rehearsals and constant renewal.

These six factors are perhaps not the only attributes of peak performance, but they are the ones researched and confirmed by Garfield as he studied people at their best. He gives examples and anecdotes to describe each factor in each successive chapter.

In the end he says peak performers know who they are instinctively, and that the condition is dynamic, not static. He quotes psychologist Carl Rogers: "The good life is not any fixed state . . . nor contentment, nor nirvana, nor happiness," thus debunking a host of American dreams. Rather, "The good life is a *process*, not a state of being. It is a direction, not a destination." So, too, is the attitude of a peak performer.

Peak Performers was published in 1986. Since then much has happened in the world, especially in the former Soviet Union. We can chuckle when Garfield praises People's Express airline or quotes *Jane's Spaceflight*, for example: "The U.S. is developing a new breed of military astronauts, because generals fear that superpower skirmishing in space is 'almost inevitable' in the next 25 years."

Nevertheless, Garfield's book is still quite relevant and enjoys a renewed level of interest. It is light and clear enough as a beach read, but most readers get the idea that the author is in search of something magical and elusive. He's on to something, but whether he has captured it or put it in a bottle is doubtful. That he has tried, however, does seem very important.

The Seven Habits of Highly Effective People: Restoring Character Ethic

by Stephen R. Covey

(New York: Simon & Schuster, 1989)

Jim Fletcher says this book "suggests a discipline for our personal dealing with people which would be undoubtedly valuable if people stopped to think about it." Charles Garfield calls it "a wonderful contribution." He adds: "Dr. Covey has synthesized the habits of our highest achievers and presented them in a powerful way." Lavish praise for what has become the most widely read success book of the 1990s.

It all began when the Brigham Young University management professor took a sabbatical to Oahu, Hawaii. At the college library, "my eyes fell upon a single paragraph that powerfully influenced the rest of my life." The unquoted paragraph "basically contained the single idea that there is a gap or a space between stimulus and response, and that the key to both our growth and happiness is how we use that space." In other words, it doesn't matter what happens to us, good or bad—what really matters is how we react to the events in our lives. They either build us up or break us down.

The BYU professor notes that the first 150 years of "success literature," beginning with Ben Franklin's autobiography, centered on what he calls "Character Ethic." The past 50 years or so centered on "Personality Ethic" which Covey finds superficial, clearly manipulative, intimidating and even deceptive.

These later success books tried to change outward behavior and style, but Covey says the only real change is internal, "inside-out." He calls for a "principle-centered paradigm shift" (echoing the buzzword of the 1980s) from "get rich quick" schemes and "wealth without work" to self-evi-

dent principles derived from “natural laws” such as fairness, service, quality and integrity.

Yet, when Covey lists and describes the seven synthesized habits or regularized principles, they differ little if any from the Dale Carnegie–Earl Nightingale–Peter Drucker–Tom Peters–Charles Garfield success literature the author describes. Certainly the seven habits will not hurt anyone trying to succeed. The reader is not convinced, however, that these are the top seven, much less that only seven habits are needed to provide “a holistic, integrated, principle-centered approach for solving personal and professional problems.” Nevertheless, they are interesting and useful, and they include:

1. **Be Proactive.** “Between stimulus and response, man [and woman] has the freedom to choose,” says Covey. Proactive people do not blame others or make excuses, but rather choose deliberately and turn failures into learning opportunities. Eleanor Roosevelt once said: “No one can hurt you without your consent.”
2. **Begin with the End in Mind.** This is the book’s longest chapter, perhaps because it is the most derivative. Covey here asks every potential leader to write a personal family and work group mission statement and then affirm and visualize it. “One of the main things his research showed,” says Covey, referring to Charles Garfield, “was that almost all of the astronauts and other peak performers are visualizers. They see it; they feel it; they experience it before they actually do it. They begin with the end in mind.”
3. **Put First Things First.** In the previous chapter Covey quoted Drucker and Bennis: “Management is doing things right; leadership is doing the right things.” In this chapter, he advises: “Organize and execute around priorities, and discipline to say no or delegate adroitly.”
4. **Think Win/Win.** This involves a shift in thinking from a paradigm of competition (I win, you lose) to one of cooperation where “agreements or solutions are mutually beneficial, mutually satisfying.” Covey is speaking of cooperation in the workplace, leaving All-American Competition for the marketplace.
5. **Seek First to Understand, Then to Be Understood.** Although Covey does not attribute this habit, it derives from the “Prayer of Peace” of Francis of Assisi. Covey calls it “empathic listening,” from the term empathy. Since oral communication is only 10 percent by words, 30 percent by sounds and 60 percent by body language, this empathic listening calls for listening with your ears, your eyes and your heart or soul. Then you really connect.
6. **Synergize.** Another ’80s buzzword, like “paradigm” or “empower”. As described, synergism is the third alternative of two opposing views—not the dichotomous either/or stance, but both/and, or as Covey would have it, win/win. His business associates, through synergistic free association and brainstorming, came up with their mission statement: “Our mission is to empower people and organizations to significantly increase their performance capability,” which throughout the book is abbreviated as P.C.
7. **Sharpen the Saw.** Principles of balanced self-renewal. Here Covey tries to synthesize the six habits into a seventh but actually produces the best chapter of the book. His marvelous Four Dimensions of Renewal can stand alone: physical (invigorating exercise), spiritual (meditation as a source of power), mental (read a classic a week, keep a journal) and social/emotional (in service to others for true happiness).

This chapter should be read first, especially by those who are interested in TQM. Over all, *The Seven Habits of Highly Effective People* is nicely

written but hardly original. The documentation is poor, and some lists and charts are bewildering. (There's even an 800 number where you can call for even more charts, plus a catalog from the Covey Leadership Center and a list of upcoming Covey seminars, retreats and newsletters.) Some readers swear by this book; others look at it as just another success book. A lot of people are still buying it, hoping to become better leaders and managers.

Readings in Systems Engineering

Ed. by Francis T. Hoban and
William M. Lawbaugh
NASA SP-6102 Washington, 1993

The core of this collection of 17 widely divergent approaches to systems engineering consists of specially commissioned papers from the NASA Alumni League. Owen Morris, Chuck Mathews, John Hodge, John Naugle, Kranz and Kraft, Yardley and Wensley, and Bob Aller are all represented here, along with people who made their mark on systems engineering in government and industry.

The collection begins with the classic formulation of systems engineering given in 1969 by Robert A. Frosh. His common sense approach sets the tone for the next dozen or so analyses and slants on a difficult subject. Not just successful tools and techniques are described and discussed, but failures as well, in particular Skylab 1 and the 1978 Seasat mission. The book ends with a jovial reaction to this discipline as it was being introduced in one of the NASA Centers 25 years ago.

Readings in Systems Engineering is 218 pages, but readable and clearly presented. Designed primarily for the next generation of systems engineers, the book shows the richness of diversity in an increasingly important emerging management discipline.

NASA Systems Engineering Handbook

By Robert Shisko and
Robert G. Chamberlain, et al.
PPMI Publication—Draft September 1992

“This handbook was written to bring the fundamental concepts and techniques of systems engineering to NASA personnel in a way that recognizes the nature of NASA systems and the NASA environment,” the authors say. That’s no easy task, but the 120-page handbook is amply illustrated and well written.

As the authors indicate, the content as well as the style of the *NASA Systems Engineering Handbook* shows a teaching orientation. That’s because the book covers many of the topics taught in NASA’s Project Management and Program Control courses.

The handbook consists of four main sections and three helpful appendices. Part one includes definitions and descriptions of systems engineering, while the second section takes the NASA Project Cycle from Phase A through Phase F, plus funding and product development. The material on the project cycle is drawn from the work of the Inter-Center Systems Engineering Working Group, which met periodically in 1991.

The third and lengthiest section covers “Management Issues in Systems Engineering,” including the Systems Engineer Management Plan (SEMP) the WBS, scheduling, resource planning, risk management, baseline management, reviews and reports. Section four is called “Systems Analysis and Modeling Issues.” It includes the Trade Study Process, cost modeling, effectiveness measures and handling uncertainty.

The appendices consist of the inevitable acronym list, a unit called “Use of the Metric System” with a handy conversion table and, best of all, a set of

eight systems engineering templates and examples, including three techniques of functional analysis.

Currently the handbook is being tested out in NASA's Program/Project Management Initiative and is under review from experts in the field. The *NASA Systems Engineering Handbook* is not a substitute for a Center handbook, but the two are, or should be, complementary. No footnotes are used to clutter the narrative; sidebars are used instead. Two foldout charts in the second section are referred to in the discussion of the NASA Project Cycle.

The NASA Mission Design Process

An Engineering Guide to the Conceptual Design, Mission Analysis, and Definition Phases
NASA Engineering Management Council
December 1992

Shortly after the NASA Engineering Management Council (EMC) was formed in 1991, the need emerged for a clear, compact definition of the mission design process. This slight, 64-page document is described as a "reference compendium of proven approaches to be used by those knowledgeable and experienced in NASA projects and aerospace technology."

It is a handy document, consisting of six sections, including a detailed glossary in the introduction, a compact list of acronyms at the end, and several detailed charts and tables to display the flow of design activities.

"Implementing and Managing the Study Process" shows how a study team is formed and offers guidelines of the necessary, thorough study effort. The authors suggest "6-10 percent of the development costs" be allocated to mission design. They say cost problems during Phase C/D will be minimized, but if less is spent, "larger margins and contingencies must be maintained" until proper definition of requirements and systems.

The biggest section is devoted to "Mission Design Activities" and covers the basic activities and tools from requirements to technical performance measurement for familiar and new technologies. In discussing cost, schedule, performance and risk, the authors make distinctions between robustness (the ability of a system to absorb changes) and flexibility (design features that permit workarounds in problems on orbit). They emphasize the obvious, but often forgotten: "Any schedule extensions will always result in a cost increase."

The subsequent sections cover Pre-Phase A (conceptual design) studies, Phase A (mission analysis) and Phase C/D (execution) but not launch nor mission operations, often described as Phase E and F. A brief closing section on "Conducting a Compressed Study" suggests that communications lines be shortened, decision-making streamlined, design meetings held daily and the list of activities be prioritized. They also advise "maximum utilization of existing designs and hardware" for a compressed study.

The NASA Mission Design Process is available from the EMC or from Dr. Michael G. Ryschkewitsch, Code 704, Goddard Space Flight Center.

Video Reviews

International Ultraviolet Explorer

Lessons Learned and Experiences Shared
in NASA Project Management
1992: 30 min.

On January 26, 1978, a three-stage Delta rocket carried what the narrator, Carter Dove, calls "the world's most productive astronomical satellite" into a low elliptical orbit. IUE was one of the first general-purpose research facilities launched by NASA.

Dr. Albert Boggess, Project Scientist, attributes much of the success to good communications among scientists, engineers and managers on the project. Project Manager Gerry Longanecker explained how managers would alternate meetings between the U.S. and Europe while Britain built the four onboard cameras and ESA supplied the solar arrays. Weekly conference calls were placed at a prescribed time to England.

Charles Freschsel, IUE Operations Manager, points to the “complete and unambiguous requirements” and “thorough testing” as key ingredients, while Kenneth Sizemore, Spacecraft Manager, describes IUE’s unique onboard computer for attitude control. He says that fixed-price contracts were used for off-the-shelf items while cost-plus was better for the gyros, which needed substantial development.

The big management challenges were Delta weight restrictions and delays from the British telescope manufacturers. A dedicated engineer resolved IUE’s hydrazine temperature problems early in the project. Tradeoffs and workarounds saved the day for IUE.

This half-hour video was produced under the auspices of PPMI Program Manager Edward Hoffman and is available from the NASA Headquarters Library.

The International Sun Earth Explorer-3 Mission

Lessons Learned and Experiences Shared
in NASA Project Management
March 1991: 30min.

In 1978, ISEE-3 was launched on a Delta rocket from Cape Canaveral Air Force Station on a 930,000-mile mission to study the effect of solar wind on Earth’s magnetosphere. Its flexible design and contingencies gave ISEE-3 a much longer life than the expected three years.

Project Manager Jerry Madden is an advocate for MBWA, management by walking around or wandering about. “You’ve got to know the base of the pyramid,” you can miss an engineer or two, but these technicians “make it work.”

Deputy Project Manager Dr. Stephen Paddock explains the teamwork that brought ISEE-3 in on schedule and under budget. John Hraster wore two hats as Systems Manager and Mission Operations Manager, while Experiment Manager Martin Davis describes the flexibility needed to coordinate 12 instruments from Goddard Space Flight Center, other NASA Centers, the European Space Agency, industry and academia. Spacecraft Manager Don Miller, the rookie on the team, tells how the cost-plus award fee contract took more time but was suitable for the integration and testing of subsystems manufactured inhouse.

Jerry Madden has lots of advice for younger project managers. First of all, delegate authority and give others plenty of resources but “never tell them how to do it.” Also, no surprises or shocks for Headquarters: inform them, warn them in advance of any potential problems, and invite them to open, major meetings. An unexpected request for much more money for your projects means “you hurt some other project . . . someone else has to pay for your mistakes.”

Like the IUE video, the ISEE-3 ends with a list of about a dozen “lessons.” The narrator, Carter Dove, indicates that a subsequent PPMI video will feature the follow-on project, ICE, the International Cometary Explorer, which evolved from this one. The PPMI Librarian at NASA Headquarters Library can provide this and other PPMI video productions through Center librarians.

