SECTION III

NASA AND EXTERNAL RELATIONS

INTRODUCTION

In achieving its mission over the last 50 years and in pursuit of a variety of goals, NASA has had complex interactions with a large number of external groups. This section discusses three of the most important: the aerospace industry, the Department of Defense, and the international space community. With a few notable exceptions, historians have often submerged these relationships as they concentrated on the internal problems, achievements, and themes of the Agency itself. NASA's relations with any one of these entities would be an enormous topic in its own right; each author in this section has adopted particular case studies that illuminate key issues.

In the first paper, Philip Scranton aims to enhance our understanding of the often contentious interaction between NASA and industry, which has been crucial in designing, testing, and building the hardware necessary to achieve the Agency's mission.[1] This essay gives a vivid accounting of the complexity of the space enterprise at a level that few people outside the space community contemplate. This complexity involves not only the operational relationships between NASA and its prime contractors, but also those among the primes and their thousands of subcontractors, among the subcontractors and the "sub-subs," and so on down the line, all part of the aerospace industry at increasingly diffuse, but real, levels. Scranton points out that while there was (and is) much contention among those in the contracting community, historically all stood together against what they perceived as excessive NASA meddling and oversight. Yet somehow, it all worked (usually) in the end. Drawing on his own work on the fabrication of the Mercury spacecraft; on Bart Hacker and James Grimwood's history of the Gemini program, *On the Shoulders of Titans*;[2] and on Joan Bromberg's *NASA and the Space Industry*, Scranton shows the astonishing array of questions that arise when one considers concrete historical cases.

Beyond his analysis of the problems, Scranton suggests five frameworks for research that might increase our understanding of the relations between NASA and industry, technology and organization, practice and process, and design and production. Two existing frameworks are Stephen Johnson's study of the systems management approach in *The Secret of Apollo* and Howard McCurdy's sociological approach to organizational culture exemplified in *Inside NASA*.[3] Scranton also

1. NASA has sponsored one study of the Agency's relationship with the aerospace industry, but there is considerably more work to be done on the subject. See Joan L. Bromberg, *NASA and the Space Industry* (Baltimore, MD: Johns Hopkins, 1999).

2. Barton C. Hacker and James M. Grimwood, *On the Shoulders of Titans: A History of Project Gemini* (1977; reprint, Washington, DC: NASA SP-4203, 2002).

3. See Howard E. McCurdy, *Inside NASA: High Technology and Organizational Change in the U.S. Space Program* (Baltimore, MD: Johns Hopkins, 1993); Stephen B. Johnson, *The Secret of Apollo: Systems Management in American and European Space Programs* (Baltimore, MD: Johns Hopkins, 2002).

proposes that analytical tools be used from the fields of social construction of technology, management theory, and anthropology to attack these problems.

Scranton hopes for a shift in the writing of NASA history in what he sees as a long-overdue direction: the little-understood world of production for NASA. "Retelling NASA stories from the drafting room and shop floor outwards, from the bottom up," he concludes, "has the potential to reorient a universe of NASA-centric histories." He formulates a large number of questions that constitute a research program to this end.

Scranton's essay does not address the Department of Defense, but since the 1980s, DOD has funneled even more money into the space industry than NASA (their respective space budgets were on the order of $19 billion versus $14 billion in 2003). Even before NASA was formed in 1958, DOD, with its growing stock of ballistic missiles, realized the importance of space for military reconnaissance. In the interservice competition to create a scientific satellite for the International Geophysical Year (IGY, 1957–58), the Navy's Vanguard program was given the go-ahead, but it was the Army, with a modified Jupiter C ballistic missile, that launched Explorer 1 on 31 January 1958, the first successful American satellite in the wake of Sputnik. The opening of the Space Age was accompanied by intense discussion as to whether the nation's space program should be military or civilian. NASA's birth signaled the decision for a civilian agency, but the proper role for military and civilian space programs has been debated ever since.

Peter Hays, a policy analyst with 25 years of service in the Air Force, focuses on three key issues and time periods to illuminate NASA-DOD relations. In the first issue, organizing to implement the American space vision in the 1950s, he finds three major activities with bureaucratic interests that endure today: moving the Army Ballistic Missile Agency (ABMA) into NASA, consolidating DOD space activities under the Air Force, and establishing the National Reconnaissance Office (NRO). Once the ABMA was transferred to Marshall Space Flight Center in Huntsville, Alabama, in September 1960, after a protracted struggle, the Army was officially out of the space business; DOD space activities were concentrated in the Air Force. Not trusting reconnaissance satellites to the Air Force, however, President Eisenhower formed what is now known as the NRO in late 1960. DOD and NRO activities became increasingly classified under President Kennedy, a situation that led to widely divergent public and congressional perceptions of the NASA and military space programs and also made the writing of military space history dependent on declassification.

Hays's second issue is the rationale for human spaceflight in the early space program, in particular the competition between NASA and the Air Force for human spaceflight missions. In this competition, NASA was decidedly the winner; the Air Force was rebuffed on its Dyna-Soar effort by the end of 1963 and its Manned Orbiting Laboratory by 1969 (after $1.4 billion in expenditures).

These early interactions among NASA, DOD, and NRO provide deep background for Hays's third issue, the development of the Space Shuttle, which provided "the most focused, longest running, and most intense interplay among these organizations . . . the single most important factor in shaping their interrelationships." As Hays shows and others have suggested before him, in selling the Shuttle project to Congress and the President, and especially once the decision was made that the Shuttle was to be the nation's primary launch vehicle, NASA needed DOD support and DOD needed NASA to launch its large spy satellites.[4] The Air Force component of DOD was essential in determining Shuttle payload and performance criteria and is credited with saving the program during the Carter administration when Vice President Mondale and the Office of Management and Budget tried to cut it. It was the Air Force that successfully argued that four Shuttles were needed. The price exacted from NASA was mission priority for DOD. Yet, because it did not control the Space Shuttle program, the Air Force was never very enthusiastic about it. And in the aftermath of *Challenger*, the Space Transportation Policy underwent a seismic shift, with the Air Force and NRO once again returning largely to expendable launch vehicles. For historians and policy analysts, the Space Shuttle program provides an unparalleled window on the relations among NASA, DOD, and NRO. Hays concludes that it is "an excellent illustration of the general Air Force ambivalence over the military potential of space and military man-in-space as well as evidence of the lack of clear and accepted doctrinal guidance on these issues."

In the third chapter in this section, John Krige asks an intriguing question: why does the most powerful nation on Earth for the last 50 years want or need international space cooperation? As he points out, some have argued that space cooperation was used in the Cold War era and should continue to be used now, under changed circumstances, as an instrument of foreign policy in which to foster and gain allies. But, he notes, blind international cooperation exacts a price: there is a tension among sharing technology, not compromising national security, and remaining industrially competitive. He argues that sharing technology in the interests of international cooperation makes no sense, historically or practically, unless one opens the "black box" of the interaction of technology and foreign policy: "It is crucial to focus on what specific technologies might be available for sharing in the pursuit of specific foreign policy objectives, rather than—as so often happens—to simply lump technology and foreign policy into an undifferentiated whole." Historians must study international collaboration at this fine-grained level, he insists, if the analysis is to be robust.

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4. See Dwayne A. Day, "Invitation to Struggle: The History of Civil-Military Relations in Space," in John M. Logsdon, gen. ed., *Exploring the Unknown: Selected Documents in the History of the U.S. Civil Space Program*, vol. 2, *External Relationships* (Washington, DC: NASA SP-4407, 1996), esp. pp. 263–270.

In his essay, Krige takes his own advice by analyzing a particular case of attempted technology transfer: the mid-1960s desire by the Johnson administration to collaborate with Western Europe, particularly with the European Launcher Development Organisation (ELDO), on a civilian satellite launcher. This desire was based on the belief that such cooperation would strengthen European unity, close the technology gap between the United States and Europe, and divert ELDO resources from the technology of nuclear weapons delivery by using them in space instead. NASA and the State Department particularly argued the last point: that by sharing launch technology with ELDO, including documentation on the Atlas-Centaur upper stage that would allow European satellites to reach geosynchronous orbit, they would discourage other nations from applying resources to national military programs. In opposition to this desire for cooperation were American national security and business interests. In particular, some felt that American technology transfer might actually benefit the French nuclear weapons program in terms of its delivery system. Others pointed out that the technology transfer might confer commercial advantage to certain countries in terms of competition with INTELSAT, the worldwide communications satellite consortium under U.S. control via COMSAT. Although NASA and the State Department argued for a finer analysis and a case-by-case study rather than the blunt instrument of national security memoranda, in the end, the argument for relaxing constraints on technology transfer lost. Krige explains the reasons, which are deeply rooted in historical events.

Krige suggests that historically, the protection of national security and national industry interests always prevails over foreign policy considerations. His insights into the connections between space and foreign policy open up a new direction in space history and the history of this component of foreign policy.

By no means do the aerospace industry, the Department of Defense, and international relations exhaust even the general categories of NASA's external activities. Other interagency activities, such as interactions with the State Department and the National Oceanic and Atmospheric Administration (NOAA); university relations, as championed by former NASA Administrator James Webb and some of his successors; public and community relations, always important to NASA's image; and congressional relations, so essential to funding, raise their own unique questions as subjects of historical analysis. Nevertheless, taken together, this section highlights how multifaceted NASA history is, as well as how very much remains to be done in a large number of areas and from a variety of new perspectives.

CHAPTER 6

NASA AND THE AEROSPACE INDUSTRY: CRITICAL ISSUES AND RESEARCH PROSPECTS

Philip Scranton

The X-15 was [Harrison] Storms' airplane as much as it was anybody else's airplane. A lot of other people could lay claim to it. The theorists at NACA [National Advisory Committee for Aeronautics] had actually laid out the basic lines and drawn up the specifications. Some of these people thought of [North American's] Storms and his ilk as "tin benders," lowly contractors who simply hammered out the hardware to match the vision of the scientists. But this wasn't hardware. This was jewelry.

—Mike Gray, *Angle of Attack*

As costs rose, schedules slipped. One source of delay was attempted improvements . . . . The Gemini Program Office was less than happy with the course of events . . . . Not only was GPO being bypassed in the process that approved changes Lockheed wanted to make, but the project office was not always even told what those changes were.

—Bart Hacker and James Grimwood, *On the Shoulders of Titans*

[Reassignment to] Spacecraft Assembly and Test brought me totally down to reality—down and dirty with the thousands of physical details that had to be perfectly crafted, installed, verified, and documented, and face to face with the earnest, hard-working men and women who strove to do their very best to build a spacecraft that would land men on the Moon and bring them back safely . . . . I had seen the effort and concentration by hundreds of skilled craftsmen that was needed to make engineering orders or program decisions take shape in fact, not just on paper.

—Thomas J. Kelly, *Moon Lander*

In concluding his 1999 essay review of recent works in NASA history, Northeastern University's W. D. Kay noted that however thorough these studies, they "wind up saying very little about the behavior of the private contractors who actually built the rockets, probes, and satellites. With rare exceptions that almost always involve catastrophes . . . the internal workings of the nation's aerospace contractors never receive anywhere near the level of scrutiny routinely accorded to NASA." Tipping his hat to Roger Bilstein's *Stages to Saturn* as a "happy exception" to this pattern, he added his concern that silences on the industrial front obstructed assessment of credit, blame, and "accountability." In this regard, Kay hoped that aerospace companies would disclose the sources that would document their "role(s) in shaping the U.S. space program,"[1] but at least for Mercury, Gemini, and Apollo, mountains of industry documents have been preserved in NASA files and NARA archives, awaiting our attention. Perhaps this essay will encourage scholars to plunge into them bearing questions and agendas that will enrich our appreciation for the business of building space technologies.

During its first years, NASA reluctantly discarded the NACA's "we build it here" philosophy, abandoning its predecessor's approach for an emphasis on design and supervision, project management, and performance review.[2] Rapidly, then durably, the Agency paid out 90 percent of its budget allocations to contractors, chiefly private-sector firms, for engineering, fabrication, testing, redesign, certification, and shipment.[3] These industrial enterprises and their hundreds, perhaps thousands, of subcontractors, constituted the aerospace industry, which commenced in the 1950s chiefly as a series of projects, then divisions, within well-known aircraft companies: North American, Martin, Lockheed, Boeing, Douglas, and McDonnell, supplemented by specialists in electrical or chemical technologies and products (GE, Thiokol).[4] Given the NASA History Office's charge to research Agency plans, programs, and performance, it is understand-

1. W. D. Kay, "NASA and Space History," *Technology and Culture* 40 (1999): 120–127. A number of titles partly addressing Kay's concerns appeared later than his January 1999 publication; some of them will be discussed below.

2. George Mueller, NASA's Apollo director, indicated that in the 1950s, NACA depended on the Air Force to do fabrication contracting for them, thus beginning the shift to externalization (NASM Oral History Project, Mueller Interview No. 4, 15 February 1988, p. 13, available at *http://www.nasm.si.edu/research/dsh/TRANSCPT/MUELLER4.HTM*).

3. Howard McCurdy, *Inside NASA: High Technology and Organizational Change in the U.S. Space Program* (Baltimore: Johns Hopkins, 1993), p. 39. Some of this was interagency transfer, I presume, as ABMA built some launch vehicles and assembled others, but the bulk of it was funding to private enterprises.

4. Over time, the number of prime contractors shrank decisively through a series of mergers and acquisitions, notably the creation of McDonnell Douglas (1967) and its amalgamation with North American Rockwell's Aerospace Division in a Boeing-led merger during the 1990s. Martin acquired American Marietta in the 1960s, then merged a generation later with Lockheed, yielding Lockheed Martin in 1994. The rising cost of aerospace projects (and of military aircraft development) and the uncertainty of profitability made failure on a multimillion-dollar bid extremely painful and made

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able that histories to date have fostered far greater appreciation for NASA's managerial, political, and mission-related achievements and conflicts than for its contractors' struggles to fabricate and qualify spaceflight technologies. Hence the epigraphs aim to evoke multiple dimensions of manufacturing for NASA—the tensions between Agency managers/designers and onsite corporate program directors; the extravagant demands spaceware placed on engineering and production capabilities ("jewelry"); the perennial need for improvements and fixes; that work's impact on costs, schedules, and communication; and the substantive gap between management/engineering plans and the grinding detail work on shop floors and in clean rooms across America.[5]

To rephrase this somewhat, an enhanced understanding of industrial practice in relation to NASA projects could benefit from sustained attention to four core but interrelated themes: 1) initial designing and building of technological artifacts; 2) testing, redesigning, and reworking/refabricating such artifacts; 3) alliances among and contests between contractors, as well as contractors' collaboration with or challenges to NASA units; and 4) approaches to conceptualizing complex contracting and managerial relationships in the production of "edge" technologies. Exploring these will help expose their layers and nested problem sets as this discussion moves toward sketching examples which illuminate recurrent situations, some elements of change over time, and key persistent features of the environment for fabricating aerospace innovations. In addition, this essay will briefly review aspects of the literature concerning aerospace production for NASA, will mention preliminary findings from my work with Mercury spacecraft fabrication records, and will close by offering a set of potential research questions in this area.

NASA AND INDUSTRY: FOUR CORE ISSUES

1) Initially designing and building aerospace artifacts.

The iconic NASA artifacts were launch vehicles and their payloads (manned capsules, satellites, observatories, etc.), yet a significant class of artifacts never experienced the rigors of the extraterrestrial environment (launch apparatus, testing and simulation devices, ground support and tracking/communications equipment, and much more). While being integral to NASA's ability to reach

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consolidations gradually more attractive. See Joan Bromberg, *NASA and the Space Industry* (Baltimore: Johns Hopkins, 1999), pp. 12–13.

5. The epigraphs reference what Howard McCurdy terms the "first generation" of NASA, the era through 1970. That's the only era about which I can profess anything like detailed knowledge, principally as a result of serving as the Lindbergh Chair at NASM (2003–04) and doing archival research at NARA's Fort Worth branch and at NASA Headquarters on the design and fabrication of the Mercury spacecraft.

space and, not infrequently, reusable,[6] they stood earthbound. Ground equipment, whatever its complexity, arguably faced fewer "unknowns" than that which was launched, suggesting two distinct lines of design and production dynamics. Moreover, as will be indicated below, some aerospace technologies were "merely" complex, whereas others severely "stretched" technological capabilities, another line of differentiation which could profitably be cross-compared with the launched and the grounded artifacts' development.

Nonetheless, virtually all these technological artifacts were custom-designed and purpose-built, although NASA leaders at times urged contractors to use "off-the-shelf" components or items proven in use during earlier projects. The design process was intricate and NASA-led in the early years, at times contentious, and staggeringly demanding in engineering effort and precision. Building was likewise intricate but was contractor-led (with the exception of the Army Ballistic Missile Agency rockets and a few others) and NASA-supervised/-critiqued, while being staggeringly complex in project management, quality control, and shop-floor detail—and yes, often contentious as well.

Moreover, beneath the level of large-object systems (rockets, capsules, launch sites, etc.), complexities in design and building animated the production of components, the parts for components, and the spatial/operational strategies for assembly and integration of components into functional systems (electrical power, fuel delivery, instrumentation) before the further integration of those systems into the large objects. Occasions for error abounded, as all historians of NASA know well, and the challenges of detecting errors' causes varied dramatically—from simply identifying a faulty fuse to reassembling the shattered parts of an exploded Redstone.

The engineering implications of failures were plain: "whenever something broke, we redesigned it."[7] The managerial implications were more ambiguous, for NASA officials, contractors' personnel, subcontractors, veteran Air Force project managers (much involved in NASA efforts), as well as for advocates and critics of the space program, in and out of government. Parts, component, and large-object failures were expected, yet they could (and did) derange budgets, stall schedules, initiate blame games, and hazard careers. Tom Kelly's transfer to Spacecraft Assembly, noted in the third epigraph, was a stark demotion triggered by a dismaying array of leaks in the first Moon lander Grumman had proudly delivered to Cape Kennedy, a shock that led him to a fresh learning curve[8] and leads us to theme two.

6. Unlike everything launched before the Shuttle era. On the Shuttle as the first reusable space vehicle, see Diane Vaughan, "The Role of the Organization in the Production of Techno-Scientific Knowledge," *Social Studies of Science* 29 (1999): 919.

7. *Inside NASA*, p. 32.

8. Thomas J. Kelly, *Moon Lander: How We Developed the Apollo Lunar Module* (Washington, DC: Smithsonian, 2001), pp. 165–171. This demoralization is noted by Stephen Johnson in *The Secret of Apollo* (Baltimore: Johns Hopkins, 2002), pp. 145–146.

2) Redesigning, testing, and reworking aerospace artifacts.

In aerospace design and fabrication, three "rules" might be regarded as near universals: a) "the distance between paper and product is greater than you think," b) "nobody gets it right the first time," and c) "learn that failure is your friend." These are applicable in part because space manufacturing has to meet more demanding environmental tests than any other category of production.[9] Zero gravity, temperatures verging on absolute zero, the vacuum of space, launch vibrations and postlaunch rocket oscillations (pogo-ing), combustion instability, the complex interdependencies of functional systems, and the impossibility of most in-mission fixes combined with other hazards to render manufacturing for NASA launches a high-risk, high-stress task. Testing, particularly of components and subsystems, routinely revealed shortcomings in materials, workmanship, capability, or durability, mandating redesign, indeed often multiple redesigns.[10] "Fixes" themselves could create new problems—e.g., a redesigned part impinging more on a nearby component than the prior version, now radiating vibrations that unsettle its neighbors' instrumentation. Recognized insufficiencies in a system could trigger a higher-order redesign (classically, realization that fuel cell reliability was uncertain, yielding a shift to batteries),[11] which then entailed rethinking system integration. Occasionally, interprogram redesigns affected the large objects, which tended to present a stable exterior appearance. For example, the Mercury capsule's system components were largely located in the interior space of the "tin can," crowding one another and the astronaut. They were maddening to adjust or repair (getting at a failed part in one system usually involved removing elements of another, adding possibilities for error and failure). However, in the larger Gemini capsules, designers modularized functional systems (all key parts located together, insofar as was possible) and removed them outside the astronauts' operating space, making them accessible from the exterior of the capsule for maintenance.[12]

9. The "rules" are of my devising, derived from (not quoted from) primary sources. Likewise, the "more demanding" claim is arguable, though not pursued here. Comparable, but somewhat less demanding, environments for production, in my view, involve nanotechnologies, biotechnologies, deep underwater artifacts (nuclear submarines), and cryogenic or Arctic/Antarctic processes/places. At the press conference observing the Mercury Project's closure, McDonnell's Walter Burke asserted: "The problem of designing and making work this complex group of systems is one which [required] and did get a degree of attention to detail far surpassing [any] that has ever been evident in any industrial effort up to date." A newsman thoughtfully countered that Admiral Rickover might challenge that claim (transcript, Mercury Project Summary Conference, box 1, "Mercury Final Conference," September–October 1963, entry 196—Subject Files, NASA, Johnson Space Center Files, NARA RG255).

10. As Mission Control's Gene Kranz summarized, "If you were successful, the concept was labeled brilliant, and you could focus your energies on the next step, the next set of unknowns. If you had problems, you found them early and somehow made time to fix them while keeping on schedule. If you failed, a lot of expensive hardware was reduced to junk and the schedule shattered" (Gene Kranz, *Failure Is Not an Option*, New York: Simon & Schuster, 2000, p. 210).

11. Kelly, *Moon Lander*, pp. 83–84.

12. Barton Hacker and James Grimwood, *On the Shoulders of Titans: A History of Project Gemini* (Washington, DC: NASA SP-4203, 1977), pp. 33–34.

In this context, experienced contractors understood that NASA's or their own engineers' blueprint designs represented a preliminary set of parameters for manufacturing, given the multiple uncertainties of testing and use and the unknown unknowns (unk-unks) that could wreak havoc at any point.[13] Thousands of engineering design changes would flow through every large-object project, ripping holes in budgets, but ironically reinforcing the confidence of NASA staff and contractors' engineering and production teams. "As a part of their culture, NASA employees came to believe that risk and failure were normal" and that the anticipation of failure led to its avoidance.[14] Hence the salience of acknowledging the long road from sketch to artifact, the necessity of iterative design and testing, and the value of welcoming failures (though obviously not fatalities).

3) Contests and alliances between/among contractors and NASA units.

One could hardly do better for a starting point in thinking about managerial relationships in high-performance technological production and operation than to revisit W. R. Scott's classic formulation of three central issues:

> We expect *technical complexity* to be associated with structural complexity or performer complexity (professionalization); *technical uncertainty* with lower formalization and decentralization of decision making; and *interdependence* with higher levels of coordination. Complexity, uncertainty, and interdependence are alike in at least one respect: each increases the amount of information that must be processed during the course of a task performance. Thus as complexity, uncertainty, and interdependence increase, structural modifications need to be made that will either 1) reduce the need for information processing, for example by lowering the level of interdependence or lowering performance standards; or 2) increase the capacity of information processing systems, by

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13. A concise evocation of the "unk-unks" (famously referenced in a 12 February 2002 press conference by Defense Secretary Donald Rumsfeld) can be found in Tom Kelly's analysis of the Apollo Lunar Excursion Module's (LEM) history. Having completed a preliminary design study for Grumman, Kelly's partner Tom Sanial opined: "'I'll bet the real Apollo won't look like any of the vehicles we've studied.' . . . 'Why do you say that? Don't you think we've done a good job,' I challenged. [Sanial replied,] 'Our study was okay as far as it went, but I'm sure we've just probed the obvious. There's still so much we don't know about how to fly to the Moon.' I had to agree with that. 'You're right. We don't even know yet what we don't know'" (Kelly, *Moon Lander*, p. 16).

14. McCurdy, *Inside NASA*, pp. 62–65. For me, at least, it is not clear, in practice, with what reliability anticipation of failure does lead to its avoidance, or indeed how one would know/measure/analyze this. This may be one of those rarely voiced articles of faith that I have elsewhere referred to as "fabrications." See Philip Scranton, "Cold War Technological Complexities: Building American Jet Engines, 1942–60" (unpublished paper presented at SHOT Annual Meeting, Amsterdam, October 2004).

> increasing the [flow and carrying] capacity of the hierarchy or by legitimating lateral connection among participants.[15]

Todd La Porte and Paula Consolini appropriated this conceptualizing statement as foundational for their studies of "high-reliability organizations," working a counterpoint to the normalization of complex technology/system failures evident in Charles Perrow's analyses.[16] Having done workplace studies, they argued that with enough attention to detail, procedure, and training, complex organizations can and do manage to handle high-risk situations without catastrophic consequences. Yet the situations their air traffic controllers and aircraft carrier landing technicians mastered were characterized by long-term stable technologies, high-volume repetitions, and thus a restricted, known set of risk-enhancing conditions and emergency-inducing variables (chiefly technical failures and cascading climate problems). Though they partook of Scott's three core features, NASA production and operations did not fit this high-reliability stabilization framework, for these were nearly unique phenomena, lacked technological stability, lacked mastery-inducing repetitions, and thus confronted hazard conditions and variables that could not be fully comprehended, much less defended against by backups and redundancies.[17]

One implication of this difference was that for technological, economic, organizational, and cultural reasons, contracts proved blunt instruments for regulating the production and operational relationships between NASA and its contractors, much less among NASA and primes on one hand and thousands of subcontractors (and sub-subs) on another.[18] Technically, the

15. W. R. Scott, *Organizations: Rational, Natural, and Open Systems*, 2nd ed. (Englewood Cliffs, NJ: Prentice Hall, 1987), quoted in Todd La Porte and Paula Consolini, "Working in Practice But Not in Theory: Theoretical Challenges of "High-Reliability Organizations," *Journal of Public Administration Research and Theory* 1 (1991): 30.

16. Charles Perrow, *Normal Accidents*, rev. ed. (Princeton: Princeton University Press, 1999).

17. Vaughan points out that although the Shuttles were reusable, thus superficially identical among existing craft and from mission to mission, in actuality, "no two shuttles were alike; after each mission, the several NASA/contractor work groups made hundreds of changes, so the technical artifact was different for each launch" (Vaughan, "Role," p. 919).

18. In a heroic but doomed effort to "predict changes in NASA satellite contracts," two management analysts secured a NASA grant in the early 1970s and profiled the contract changes for 21 satellite projects. Seeking a predictive formula, they ignored engineering changes below the contract change level (Engineering Change Requests, or ECRs, versus Contract Change Proposals, or CCPs [CCPs were often large-scale shifts in design, whereas ECRs usually were changes in individual components]), identified mean change costs as $100 K–$300 K, and struggled to find something to regress. Yet they did offer an empirical table that suggests the economic foundation for contests and alliances. Focused on 21 contracts between 1959 and 1968, it showed that in the course of the first 10 contracts (1959–62), final costs were 5.1 times initial contract figures on average, though in the final 10 contracts (1964–68), this multiplier fell to 2.1. However, final costs were estimated in half the latter 10, as perhaps cost data

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endless Engineering Change Requests that testing and use generated meant routine contests both over the need for and design of reconfigured components, checkout routines, etc., and over who would bear the costs. Economically, as well, changes (due to incapacities or aimed at improving capabilities) escalated program expenses and generated NASA-corporate alliances between firms when both faced congressional appropriations hurdles. Primes and subs fought over late deliveries and defective products yet stood shoulder to shoulder against persistent NASA "meddling," "intrusive oversight," or "policing."[19]

Varied patterns of clashing cultures stretched back to the space program's earliest days, when, in the course of new and massive contracting for Mercury spacecraft, the inheritance by the Army Ballistic Missile Agency (ABMA) and NASA of "management by detail" from NACA/Peenemünde ran head-on into McDonnell's pride in engineering creativity and independence. Long a principal Air Force aircraft supplier, McDonnell expected a continuation of the arm's-length, consultative style of contract relations crafted over two decades. Instead, NASA designers and managers, who had never held responsibility for a major technologically novel project, locked horns repeatedly with industry specialists who *had* done so.[20] Later, when NASA Administrator James Webb geared up for Apollo in 1963 by reorganizing the Agency's top management, those he brought in had substantial experience in Air Force ballistic missile program management and industrial military contracting (George Mueller, Air Force Generals Samuel Phillips and Edmund O'Connor, and the legendary Joseph Shea).[21] Webb evidently recognized that at NASA, "nobody knew how to do program management or work with industry on large programs."[22]

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remained incomplete at the time of their article's composition. The decline in the overrun due to contract changes does suggest better specifications in the latter period. See William Stephenson and Bruce Berra, "Predicting Changes in NASA Satellite Contracts," *Management Science* 21 (1975): 626–637, table on p. 629. Regarding Apollo, "what began as a $400 million contract would top out at $4.4 billion a decade later. But everybody knew this going in. All of the Apollo bids were smoke and mirrors, because [in 1962] nobody knew what they were talking about" (Gray, *Angle of Attack*, p. 120).

19. Regarding the Shuttle booster, Vaughan observes that NASA saw "Marshall engineering's role" as "policing Thiokol; to find fault, to identify mistakes, to make sure the contractor abided by the contract" (Vaughn, "Role," p. 920). The issue is not that this was not appropriate, but that it was inadequate and ineffectual.

20. Joan Bromberg indicated that NASA core leaders feared loss of design control and shoddy work by companies given too much authority. See Bromberg, *NASA and the Space Industry*, pp. 40, 43. See also McCurdy, *Inside NASA*, pp. 38–42, which includes this gem on p. 41: "In one celebrated instance, contract workers at what became the Kennedy Space Center went out on strike because the von Braun team would not let them alone. The workers were accustomed to Air Force practice, which involved little direct supervision."

21. Shea took personal responsibility for the Apollo fire disaster and resigned from NASA in July 1967 (Kelly, *Moon Lander*, p. 161).

22. McCurdy, *Inside NASA*, p. 92.

McCurdy's judgment on the results of this reorientation is clear: "NASA's success in achieving the goals of the Apollo program was due in large measure to the tension between the Air Force approach to program management and NASA's traditional technical culture."[23]

Organizational structures did create platforms for alliances, however fraught with tension, as well as for clashes. Industry and Agency engineers with similar specialties and backgrounds worked through problem sets in spaces far distant from policy-making and budget authorizations. For example, Space Task Group and McDonnell collaborated in depth to create Project Orbit, the huge vacuum chamber in which an entire Mercury capsule could be tested in as close to space conditions as was then feasible. Later, on the Lunar Lander project, NASA and Grumman co-staffed the Change Control Board to assess modifications and manage configuration (modeled on Air Force practice).[24]

4) Conceptualizing contracting relations and production on technology's edges.

Although these first three items hardly exhaust the potential list of themes linking NASA and industry, technology and organization, practice and process, design and production, it is worth pausing here for a moment to consider the possible conceptual tools and theoretical frameworks with which scholars can map this terrain in ways that increase our understanding. Two existing frameworks stand out, at least in my view: Stephen Johnson's close analysis of systems management's rise to dominion in NASA programs, drawing on Weber, Drucker, and the literature of "knowledge management," and Howard McCurdy's sociological approaches to organizational culture at NASA and its transformations. Johnson's work focuses closely on the struggle to achieve rational control over projects and heighten reliability through devising and enforcing rigorous procedures. McCurdy reaches into the extrarational world of the beliefs and assumptions that underlie (and at

23. Ibid. See also Mike Gray, *Angle of Attack: Harrison Storms and the Race to the Moon* (New York: Norton, 1992), pp. 50–52. On p. 50, for example: "Most of [NASA's] key people were creative iconoclasts like Maxime Faget, conceptual thinkers used to a hands-on approach in which they personally supervised every detail . . . . Now they were being asked to create the largest technical organization of all time."

24. Johnson, *Secret*, p. 128; Kelly, *Moon Lander*, p. 102. By contrast, the Apollo program's "powerful Change Control Board," created in 1967 after the astronauts' deaths, seems to have been entirely NASA-staffed, with George Low making final decisions on "changes proposed by NASA or the prime contractors" (Kelly, *Moon Lander*, p. 163). Johnson discusses the collaborative style of early NASA-industry management more fully in *Secret*, pp. 116–120. Superficially, that is, without specific research into the issue, it appears to me that collaborative NASA-industry design and engineering waned and NASA surveillance/policing increased over time, perhaps a shift triggered by the January 1967 deaths of White, Grissom, and Chaffee, as might be inferred from Johnson's review of the postaccident managerial shifts and conflicts (*Secret*, pp. 146–150). If there was such a shift, was it confined to manned space issues, or did it generalize across all NASA projects?

times undermine) practices, offering a dramatically different perspective. Both focus primarily on the Agency, as would be expected, leaving ample room for pursuing questions about the industry and production side of the spacefaring equation.[25]

Three other perspectives, which grapple with practice at the "local" level, strike me as potentially valuable, particularly in thinking about industrial matters:

1) Adapting the social construction of technology (SCOT) framework to encompass ways in which emergent organizations, much like "unruly" technologies, can become "uncertainty multipliers," a notion Diane Vaughan has applied convincingly to "the NASA/contractor organization" for the Shuttle.[26]

2) Exploring management theorists' conceptualization of the interplay between rationality and irrationality within organizations, and its relation to collateral inquiries into organizational disorder and its implications.[27]

3) Developing research questions in relation to work and technology, based on anthropologists' concern for "situated practice" and "communities of practice."[28]

The provocative potential of Vaughan's perspective can be quickly sensed in her opening remarks to a recent discussion paper on organizations and techno-scientific knowledge:

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25. Johnson, *Secret*, pp. 1–3; McCurdy, *Inside NASA*, pp. 163–164. Johnson also includes an instructive comparison with the European space agencies (European Space Research Organisation [ESRO]/ELDO, *Secret*, chaps. 6 and 7) but does not appear to have delivered on one significant point. He ends chap. 5 (speaking of the period around 1970) with "The disadvantages of systems management would become apparent later . . ." (pp. 152–153), but so far as I can tell, no discussion of disadvantages appears in the remaining sections of his study. There may be other theoretical frameworks well exemplified in NASA literature, but I'm not yet familiar with them. Both McCurdy and Johnson undertake the explanation of NASA's "decline" and the resurgence of mission failures/disasters two decades after Apollo.

26. Vaughan, "Role," pp. 916–919. Vaughn's inspirations flowed from Clifford Geertz, Charles Perrow, and the "situated action" group (n. 27), as well as from the STS and science studies literatures (see "Role," pp. 935–936, nn. 2–5, 17).

27. Nils Brunsson, *The Irrational Organization: Irrationality as a Basis for Organizational Action and Change* (New York: Wiley, 1985); Massimo Warglien and Michael Masuch, *The Logic of Organizational Disorder* (Berlin: deGruyter, 1996), esp. the editors' introduction and chapters by Bruno Bernardi, Erhard Friedberg, and Nils Brunsson.

28. Lucy Suchman, *Plans and Situated Actions* (Cambridge: Cambridge University Press, 1987); John Seely Brown and Paul Duguid, *The Social Life of Information* (Boston: Harvard Business School Press, 2000); Julian Orr, *Talking About Machines* (Ithaca: Cornell University Press, 1996); Christian Heath and Paul Luff, eds., *Technology in Action* (Cambridge: Cambridge University Press, 2000); Etienne Wenger, *Communities of Practice: Learning, Meaning and Identity* (Cambridge: Cambridge University Press, 1998).

> I begin by drawing on organization theory to illustrate the central paradox of organizations: namely, that the characteristics usually associated with the bright side of organizations—the structures and processes designed to assure certainty, order knowledge, and stabilize operations, thereby making coordinated activity possible—also have their dark side—the capacity to generate uncertainty, disordered knowledge, instability and unanticipated outcomes . . . . [T]his paper targets the conjunction of organization and technology that affected the production of knowledge and knowledge claims on a routine basis [at NASA]. The paradox is illustrated by showing the variable effect of the NASA organization on the production of techno-scientific knowledge: 1) the production of disordered and uncertain knowledge on a daily basis; and 2) the fact-hardening mechanisms in place to convert disorder to order when a collective decision was necessary.[29]

Where Johnson sees systems management as generating reliability and certainty, by tracing *Challenger* and other failures to a relaxation of detail discipline,[30] Vaughan sees the ghost as inherent in the great machine and penetrates deeply enough into the everyday life of techno-science to establish that "disordered knowledge is a byproduct of the very organizational mechanisms designed to control it." "Structure creates pockets of meaning systems—distinctive local knowledges . . . —that are by definition contradictory . . . . Structure [also] obscures, so that actions occurring in one part of an organization cannot, for the most part, be observed by people in other parts." Her work echoes in organizational/knowledge terms Perrow's critique of technical complexity, urging that scholars acknowledge that everyday practices and relations have dangerously ambivalent implications for organizational and technical outcomes.[31]

If so, recognizing that nonrational dimensions to organizational and technical practice are routinely yet unevenly present in all action situations can be a valuable step. Nils Brunsson has memorably underscored the presence and significance of nonrational dimensions of organizational practice, especially in regard to innovation. From his perspective, planning creativity is as fruitless as creating a random search for a technical fault, precisely because different modalities of thought and practice inform decision-making versus action-

29. Vaughan, "Role," pp. 914–915.

30. Johnson, *Secret*, pp. 228–229, and n. 9, pp. 275–276. McCurdy debits such disasters in fair measure to the attrition of NASA's classic high-performance "technical culture," rising risk aversion, and a politicized intolerance for failure (*Inside NASA*, chaps. 5 and 6).

31. Vaughan, "Role," p. 916, both quotations; Perrow, *Normal Accidents*.

taking. Agents need perennially to be aware that overreliance on rationality can generate stalemates, just as overreliance on intuition and enthusiasm can yield chaos. One central insight Brunsson's exploration of the "irrational organization" offers is that agreement on goals makes conflict difficult to understand in complex environments, whereas failed conflict resolution (organization change) can generate "social deadlock," the outcome when "a group of people have arrived at a situation which satisfies none of them but which they are unable to change."[32] The relevance of these conceptualizations to analyzing patterns of and changes in NASA-contractor relations is hard to miss.[33]

Third, in their anthropology of work and practice, Julian Orr, Lucy Suchman, and their colleagues undertake to reemphasize the importance of informal structures and relations, and of the knowledge and routines they generate, to organizational activity. As Scott noted, even conceptualizations of organization-technology relations that stress contingency, hence situation/place and history "overlook the importance of informal structures as a response to uncertainty and complexity." These are bottom-up processes or, perhaps better, integrative linkages:

> Rather than augmenting hierarchies, they minimize vertical distinctions, and rather than creating new, specialized lateral roles and relations, they encourage more direct, face-to-face communications among any or all participants as required. Decision making and the exercise of control become more decentralized, and organizational roles less formalized.[34]

32. Brunsson, *Irrational Organization*, pp. 27, 97, 111. By bringing the irrational into the picture of "normal action," Brunsson generates an array of striking (and testable) insights, namely, "efficiency seldom goes hand in hand with flexibility" (p. 4); it is "important to recognize that decisions can exist without actions and actions without decisions" (p. 21); and that in high-risk situations, those undertaking to reduce uncertainty are "speculators in success" and those trying to lower the stakes at risk are "speculators in failure" (p. 52). The *psychological* dimensions of organizational action are key for Brunsson, and these cannot be reduced to rational propositions.

33. Here's one minor story that shows the power of the nonrational in NASA-business relationships. In early 1963, NASA and North American representatives met 15 hours a day, six days a week in Houston to "hammer out a specific agreement on what North American was going to build and what NASA was going to pay for" in the Apollo program. Yet the NASA team was woefully underexperienced in negotiating contracts. As a NASA designer reflected, "We ought to have known better at the very outset . . . . Not any one of [our] technical guys knew a damn thing about costing. They had no basis to negotiate anything. We locked them up in these rooms [with North American managers and lawyers] and *most of them came out mortal enemies. That set a feeling that lasted a long time*" (Gray, *Angle of Attack*, p. 144, emphasis added).

34. W. R. Scott, *Organizations: Rational, Natural and Open Systems*, 3rd ed. (Englewood Cliffs, NJ: Prentice Hall, 1992), pp. 248–249, both quotations. An excellent ethnography based on this approach is Julian Orr's *Talking About Machines*. For a broader perspective, see Robert J. Thomas, *What Machines Can't Do: Politics and Technology in the Industrial Enterprise* (Berkeley: University of California Press, 1994), and Thomas Davenport, Susan Cantrell, and Robert Thomas, "The Art of Work," *Outlook Journal*, January 2002, *http://www.accenture.com/xd/xd.asp?it=enweb&xd=ideas%5Coutlook%5C1.2002%5Cart.xml.*

In American corporations and state agencies, uncertainty generates managerial hunger for top-down control, but few managers can master the massive knowledge requirements for its exercise, especially in situations where knowledge is emergent and distributed widely, as in complex contracting/subcontracting environments. Moreover, as Vaughan emphasized, the compression/reduction of vast bodies of information and the structural inability of capturing situated practice can readily transform control over uncertainty into a generator of illusion and disorder.[35]

NASA AND INDUSTRY: TWO KEY STUDIES

In identifying the themes and conceptual packages just outlined, both the insights and the silences of previous research bearing on production for NASA proved crucial. Thus far, works by Johnson, Kelly, McCurdy, and Vaughan have been emphasized; here, I'd like to consider the legacy of studies by Bart Hacker and Jim Grimwood (Gemini) and Joan Bromberg (NASA and space industries). First, however, a visit to the shop floor from Mike Gray's and Roger Bilstein's Saturn booster studies will set the stage for underscoring the extravagant technical demands and necessities for innovation that infused production for NASA.

The Apollo program's Saturn artifacts were the largest rockets fabricated in the U.S. in the 1960s (perhaps ever). Yet creating their components was enormously difficult; consider, for example, the propellant tanks for the rocket's lightweight S-2 first stage. Huge (reportedly three railway freight cars could be placed inside them) yet fragile (they couldn't be fabricated horizontally, but had to be built upright), they presented unprecedented challenges in welding. "At a time when a flawless weld of a few feet was considered miraculous, the S-2 called for a half mile of flawless welds." Moreover, the components for the tank's dome—"immense pie-shaped wedges of aluminum eight feet wide at the bottom and twenty feet from there to the apex"—were elaborate spatial forms, "a spherical curve from side to side and a complex double ellipsoid from the base to the apex." Given that no techniques existed for accurately machining such shapes, called gores, North American used explosive forming. Technicians placed the alloy blank on a forming die at the bottom of a 60,000-

35. Vaughan, "Role," pp. 926–934. This involves what Vaughan terms "fact-hardening," and the procedures for achieving it here rely substantially on the exclusion of qualitative information. As she notes, "Indeterminacy creates a closure problem." This is resolved by generating quantitatively structured documents and public consensus. "The documents . . . assert consensus through the matter-of-fact tone of the formal mode of discourse, affirming the reality they assert to both the audience and the author. An additional factor that binds people to their actions is 'going public.' When a person participates in and is identified publicly with a decision, that person will resolve inconsistencies to produce attitudes consistent with that choice." Quotations are from pp. 929 and 930.

gallon water tank, then set off a cluster of carefully placed charges on the surface. In an instant the force carried through/by the water pressed the blank into the die-form (trimming followed).[36] These segments in turn were welded by "a new kind of a machine":

> [T]he assemblers . . . were looking at a seam that followed a constantly changing curve over a twenty foot run, and the junction between the [gores] would have to match precisely to within a hundredth of an inch . . . . [T]he ultimate solution looked a little like a Japanese footbridge—a heavily reinforced bow-shaped truss that spanned the width of the dome and carried beneath it a precision track on which the welding machine traveled. The gear-driven welding head, its speed controlled by mathematical formulae, rolled ever so slowly up these rails carrying a tungsten electrode that precisely melted the metal on either side of the joint.[37] [See photo opposite; the footbridge welder is visible at the upper left.]

Thus were intricate demands addressed. Routinely for builders, no obvious means lay available to satisfy the interactive realities of technical complexity, technical uncertainty, and component interdependencies in production for NASA, thus propelling organizational frustration and technological creativity. This pattern is evident in each of the two other studies noted above, to which we now turn.

Industry-NASA relationships are especially prominent in the first 10 chapters of *On the Shoulders of Titans*, the segment authored by Bart Hacker. Like a number of jet engine projects a decade earlier, Gemini was the result of an effort to redesign an existing complex technological artifact, the Mercury capsule. By early 1961, James Chamberlain, Space Task Group's head of Engineering and Contract Administration, determined largely on his own initiative that the Mercury spacecraft needed a redesign "from the bottom up," and thus spent part of February in St. Louis going over possible revisions with McDonnell engineers. Modularizing systems that in Mercury "had been stacked like a layer cake" such that "components of [any] one system had to be

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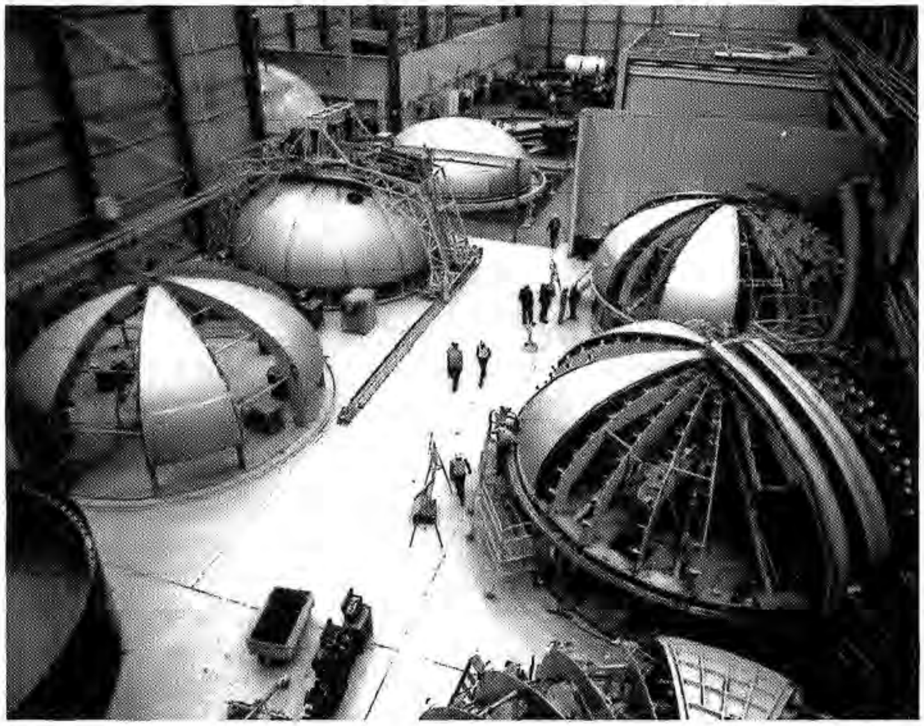
36. Gray, *Angle of Attack*, pp. 154–155.

37. Ibid., p. 156. This sequence is also carefully reported by Bilstein in considerably greater detail. See Roger Bilstein, *Stages to Saturn* (Washington, DC: NASA SP-4206, 1980; reprint, Gainesville: University Press of Florida, 2003), pp. 212–222 (page citations are to the reprint edition). For several of the hard-core technological issues, see W. J. Reichenecker and J. Heuschkel, *NASA Contributions to Joining Metal* (Washington, DC: NASA Technology Utilization Division, NASA SP-5064, 1967). This publication includes references to a number of North American reports, as well as reports from Marshall, Pratt & Whitney, Kaiser Aluminum, and others. The figure is drawn from Bilstein, *Stages*, p. 221.



Gores being welded to bulkheads for the S-II stage of the Saturn V. *(Source: Roger Bilstein,* Stages to Saturn *[Washington, DC: NASA SP-4206, 1980; reprint, Gainesville: University Press of Florida, 2003], p. 221)*

scattered about the craft" would "reduce manufacturing and checkout time," Chamberlain argued. Yet as Hacker summarized, "making it better meant making it over." Once Chamberlain and McDonnell's William Blatz collated the redesign elements, they went before the Capsule Review Board, which "seemed staggered by the scope of the changes presented to them" in June.[38] As in jets, what started as a fix, or more accurately, a vector for refining the artifact, morphed into a largely new device, yet here still a one-man capsule.

McDonnell engineers, led by Walter Burke, were the agents who outlined and pushed for the two-man spacecraft, however, as it was the builders who "were pressing for a more radical effort." Indeed, in undertaking the preliminary design work, "McDonnell had not felt obliged to wait until its contract had been amended to provide the extra funds. The company spent its own money," which generated "a good deal of respect in NASA circles." As major spacecraft contract changes arose in order to expand its size, handle

38. Hacker, *Titans*, p. 33–45.

modularization, and create a docking system and (initially) ejections seats, expectations for reusing Mercury technologies in the new developmental trajectory faded as steadily as the project drove forward. This momentum and focus on industry relations were aided by an organizational arrangement which provided the Gemini Project Office and Chamberlain "a degree of autonomy," enabling them "to deal directly with McDonnell and Air Force Space Systems Division" for capsules and boosters respectively. Chamberlain reported only to Marshall Space Flight Center Director Gilruth, chiefly providing him work in process reviews and discussions from coordination meetings, "Gemini's central management device."[39] Thus far, an organizational device giving Chamberlain singular authority (how unusual? with what exact options? how evaluated by Headquarters and by McDonnell?) and decisive redesign innovations from industry engineers and engineering managers facilitated Gemini's emergence.[40]

However, a series of technological disappointments, cost escalations, and budget controversies soon caused massive headaches. In some measure, these derived from the fact that McDonnell "developed and built only the spacecraft structural shell and electrical system"; all else had been subcontracted. Thousands of components made by hundreds of firms flowed into St. Louis; if Gemini mirrored Mercury in this respect, an unknown, sizable subset of those devices would fail on test, fail to meet specifications, or fail to integrate effectively, and thus would need to be redesigned or replaced.[41] In a retrospective overview, Hacker reflected, "Although the precise nature of

39. Ibid., pp. 49–82, 95. Even as expectations faded that technical apparatus from Mercury could be duplicated in Gemini, major continuities in personnel between the two programs proved a strength, from Faget, Gilruth, Chamberlain, and McDonnell's Walter Burke down to the shop level, where, for example, NASA plant representative Wilbur Gray shifted gradually from Mercury to Gemini. Gray's memos and reports are a marvelous source for reconstituting, in part, the informal relations and emergent communities of practice mentioned earlier in the essay. Chamberlain's autonomy may have been modeled on the direct relationship NASA's Max Faget and McDonnell's John Yardley had in making "thousands of detailed design decisions" on the Mercury capsules. See Loyd Swenson, "The 'Megamachine' Behind the Mercury Spacecraft," *American Quarterly* 21 (1969): 210–227, quotation from p. 222.

40. This approach in no way intends to overlook issues and pressures *external* to the Gemini project, such as the uncertainties about Apollo's developmental trajectory, funding, and schedule, or the cultural/political pressure to keep performing launches as Mercury was beginning to wind down.

41. Archivists at NARA–Fort Worth indicated that the boxes on technical testing and subcontractor relations I was using in my NASM/Lindbergh-supported research had not previously been pulled. Swenson's *This New Ocean* understandably did not penetrate to this level of source material, some of which, it appears, had not yet been archived or declassified at the time of its writing. NASM's Michael Neufeld suggested to me that the view among space historians is that Gemini was a much less troublesome project than Mercury, due to technological and organizational learning. This is a position that might merit further probing, although Hacker did drive more deeply into industry/production documents than did Swenson (Hacker cites telexes, letters to contractors, and activity, status, and "tiger team" reports, for example).

Gemini's problems could not have been predicted, they did arise *where* they were expected—in those systems that demanded the greatest advances beyond current technology."[42] This is such a basic point that it is worth reinforcing—*innovation generates disorder, and dramatic innovation entails error, failure, and conflict across a broad front.* In some technological environments, a stabilization follows, both of knowledge and technology. When additional requirements are promulgated, extensions of capability are feasible on the basis of retained learning and scalable technique, though the achievements usually are hard-won. In other situations, workable innovations do not provide a foundation for enhancing capabilities, which is to say that stabilization proves illusory and learning less than readily applicable to upgrading. These often involve nonscalable technologies, which are the home for hordes of unk-unks and the sources of persistent frustration and failure in large technological projects.

Two Gemini examples merit recounting: the fuel cell innovation and the recurrent issues surrounding thrusters—both involving subcontractors, here General Electric and Rocketdyne. Fuel cells had the potential to replace batteries as the source of on-board electricity, at a major savings in weight. However, in Gemini, the array of problems cropping up "seemed to suggest that theory had outrun practice." GE researchers knew scientifically that the reaction of hydrogen and oxygen could generate power, and they had devised a clever "solid, ion-exchanging membrane" that dramatically simplified both the device and its operation. Unfortunately, this science-led technology did not operate successfully—the membrane leaked, weakening output, and once this fault was corrected, the cell exhibited "degraded performance" once activated. Technicians traced this to the shortcomings of a fiberglass component and replaced it with a Dacron substitute, which triggered new troubles. Other test failures derived from the cracking of the cell's titanium tubing; these were replaced with a titanium-palladium alloy. Further problems appeared, but they "were never conceptual . . . . The rub came in trying to convert [the] concept into hardware to meet the Gemini specifications." After two years' work, NASA canceled the effort in January 1964, resumed work on battery development, and spent $600,000 to retrofit two capsules outfitted with fuel cells. The same pattern recurred soon after, with the Apollo Moon lander's fuel cell program (this time handled by Pratt & Whitney) canceled early in 1965 following two years of trials and failures, with reversion again to batteries.[43]

Thrusters presented an enduring difficulty. Twice in the Mercury program, their fragility and unreliability caused serious concern. In January 1962, McDonnell was testing Capsule No. 2's Reaction Control System when the

42. Hacker, *Titans*, p. 162.

43. Ibid., pp. 103–104, 148–152. For the LEM story, see Kelly, *Moon Lander*, pp. 82–84.

base of the spacecraft caught fire due to leaking thruster propellant, which, when it combusted, caused further leaks, more combustion, and quite a bit of damage to the artifact and to the designers' confidence.[44] Just a month later, during John Glenn's orbital flight, the Automatic Stability Control System, which coordinated the thrusters to maintain proper attitude, went for a walk over Mexico. Glenn explained:

> The capsule started drifting to the right in yaw and it would drift over to about 20 degrees, instead of the normal 30 degree limit, and then the high thruster would kick on and bat it back over to the left. It would overshoot and then it would hunt and settle down again somewhere around zero. The spacecraft would then drift again to the right and do the same thing repeatedly.[45]

Glenn put the system into manual control (then into fly-by-wire), which saved fuel, but the capsule began to yaw to the left, and it was soon apparent that "there was no left low thrust."[46] Glenn discussed how he dealt with the inoperability problem:

> When the fly-by-wire one-pound thruster was not actuating in yaw, I was using a real fast flip of the high thruster in the mode that the one-pound thruster was not operating to control. I couldn't control this as accurately as you can with the one-pound thruster, . . . so what I did several times was, when I would overshoot in rate with the 24-pounder, I would use my one pounder on the other side to bring it back to zero . . . I wouldn't call this desirable.[47]

Unsurprisingly, attention to thruster testing and possible design flaws increased sharply.

With the more ambitious Gemini program's development, thruster problems became more acute and challenging. The smaller of the two propulsion units on Gemini was roughly the size of Mercury's larger unit (25 pounds of thrust), whereas Gemini's big pusher was to yield three times that power (85

44. R. H. Lilienkamp, Senior Engineer, McDonnell, "Investigation of the Capsule No. 2 Incident, 9 January 1962," 16 January 1962, MAC Technical Reports, box 27, entry 198C, NASA-JSC, NARA RG 255.

45. R. B. Voas, "Memorandum for Those Concerned, MA-6 Pilots Debriefing," pp. 13–14, Contract Administration Files, box 31, entry 198E, NASA-JSC, RG255.

46. Ibid.

47. Ibid., p. 61.

pounds). The Mercury components had simply managed attitude control; in Gemini, they had to handle spacecraft maneuvering and in-orbit rendezvous. Third, the Gemini fuel was different—monomethylhydrazine and nitrogen tetroxide, which combusted on contact, versus Mercury's simple hydrogen peroxide, which expanded radically on release under pressure. Last, and most troublesome, whereas the Mercury thrusters operated for a few seconds at a time, Gemini's would need to burn steadily for 5 minutes or more, as well as to pulse repeatedly.

The bad news came in waves. Tests early in 1963 showed that the 25-pound Geminis tended to "char through their casings" when run continuously. A redesign at first seemed to remedy this, but pulse testing proved half again more destructive to the casings, and a series of "expedients . . . could only alleviate, not solve, the problem." Most troubling, the nonscalability gremlin soon surfaced, as "new tests revealed that the larger maneuvering thrusters could not be simply enlarged versions" of the 25-pound engines. Therefore, a separate design and testing program for them had to be devised. In October, the hammer dropped—mission simulations showed that astronauts used their thrusters far more than had been anticipated—thus, "thruster life would have to be doubled or tripled."[48]

Rework lasted well into 1964, with the result that Rocketdyne fell far behind schedule and had spent more than double its allotted $30 million. NASA soon demanded a "full scale" audit, which revealed a "badly managed program," for the company had "grossly underestimated the magnitude and complexity" of its engine subcontract. Fewer than half the engines slated for delivery by November 1964 had been received, and McDonnell was far from confident in the thrusters' reliability. Still, by mid-1965, Rocketdyne had reorganized the engine division, recovered its momentum, and begun to meet or exceed schedule expectations.[49] The facts that different-sized and differently purposed engines could not be scaled up or down from existing, workable models and that elaborate fueling and combustion systems were inadequately understood meant that propulsion surprises would continue to arise.[50]

Technological problems solved for a mission having certain requirements did not necessarily spill over to later missions with more demanding require-

48. Hacker, *Titans*, pp. 83–84, 154–157. The upgraded demands settled at over 9 minutes for the small thrusters and over 13 minutes for the large.

49. Ibid., pp. 210–211. This happy outcome did not prevent thruster problems from arising on three missions—Gemini V, VII, and VIII. See ibid., pp. 259–260, 292, 314–315.

50. One of the key dilemmas here was combustion instability, which arose when flows of fuels (and oxidizers) failed to generate a steady, focused flame thrust, whether due to cavitation, component performance problems, or other factors. Correcting such instability once it occurred seemed impossible, for the effects were dramatic and instantaneous on missile attitude and trajectory, nor was the science of fluid dynamics sufficiently developed to model these flows mathematically and continuously.

ments. The organizational approaches effective for solving first-generation dilemmas would not assuredly suffice for next-generation challenges. As well, the insufficiencies of science regarding critical, complex phenomena (combustion and fluid dynamics, materials performance under zero gravity, etc.) meant that workable engineering outcomes could not be stripped of their anxiety dimensions, for, as with Mercury, components that worked 10 times could (and did) fail on the 11th, without warning and without obvious (or remediable) cause.[51] In this light, it would perhaps be worthwhile for researchers to explore those domains in which basic science guided NASA technical practice, those where NASA practice extended scientific knowledge and theory, and those where the two remained disconnected in specific situations or for longer periods.

Moving to the industry-NASA relationships depicted in Joan Bromberg's pioneering overview entails a shift in focus, for her work undertakes a long-term analysis. This essay is anchored in thinking through technology and production issues, whereas after its opening sections, *NASA and the Space Industry* (*NSI*) moves toward the second of its two themes—space and the marketplace, for satellites, Shuttle usage, et al.—if you will, the consumption side of NASA. Nonetheless, *NSI*'s first theme, "the innovation process," is clearly germane. Here, Bromberg delineates production for NASA's crucial background conditions, identifies core tensions, and offers two detailed case studies of innovation—satellites at Hughes and Apollo at North American.[52]

Four background items Bromberg highlights are particularly rich with implications:

1) Lockheed's science crisis in the mid-'50s "over whether scientists on a project should have control over advanced development." The firm said no; 15 top scientists left, frustrated that their demand to direct work for which "the skill and technical knowledge [was] beyond the state of the art" had been rejected. Science-engineering and scientist-manager relations are a subplot in NASA-industry relations, though, as a novice, it's not clear to me how much these have been investigated.

51. As Hugh Dryden stated in the closing Project Mercury Conference, "We learn how to build things to last longer by trying to build them, by operating them in space, finding out what goes wrong, correcting, learning more about the environment . . . . These are things that we learn by going into space and working there, not from some theory in the laboratory" ("Mercury Final Conference," pp. 1–2, box 1, E196, RG255).

52. At the outset, Bromberg refers to technical professionals' "community of practice" but does not seem to be aware of the communities of practice in literature and research approaches noted here in the section on conceptual frameworks. In a discussion with NASM's Martin Collins (13 January 2005), I came to appreciate that oral history interviewing below the executive level (planned but never completed)—interviewing of design, test, and production engineers, for example—would, in framing novel questions, profit substantively from familiarity with the work of Orr, Suchman, and Lave, and also from thinking closely about Karl Weick's challenging *Sensemaking in Organizations* (Thousand Oaks, CA: Sage, 1995), especially in relation to puzzles, failures, and conflicts over knowledge, interpretation, and practice.

Here, did those resigning create their own firm; move to universities; seek research unit jobs at Mellon, Battelle, or RAND; hire on to other industrial firms; or what? Did such confrontations appear on aerospace's technological edge with some frequency, or was this a rare moment?[53] After all, the role of science and scientists in NASA work is not so obvious at it might seem, given the huge holes in scientific understanding of space environments in this era.

2) The Air Force's creation of Ramo-Wooldridge as a systems engineering and technical management firm (1954). To be sure, this laid the foundation for "weapons system" development and for TRW, but to what extent did valorizing this cluster of sophisticated experts create a template helpful for defining NASA's differences from NACA? Clearly the Air Force was already a contested model in terms of innovation management, so was NASA, in a slightly twisted organizational-lineage sense, Ramo-Wooldridge's unacknowledged or ungrateful offspring?[54]

3) The mid-'50s conflict between the Naval Research Lab and Martin, which prefigured scores of subsequent contretemps. In Project Vanguard, Martin argued that it should be provided "full [technical/managerial?] responsibility," while the NRL demanded the inverse. Martin claimed that the Lab was full of busy fault-finders, "always promoting the 'better' at the expense of the 'good enough,'" whereas the NRL asserted that Martin didn't "grasp how much they were dealing with unknowns, nor the importance of reliability . . . ." This contest, arrayed in just about these exact terms, would be replayed for several decades in NASA-industry relations, so what are we to learn from this early incidence? Was it *that* early, that is, was this just an extension of Navy "control-freakish" patterns, inverse to Air Force (and Army Air Force) delegation of project responsibilities to contractors? Was this "divide" a structural fault in post-war military/space programming, and was it ever resolved? If so, how? If not, with what implications? Or is this whole scenario just an outsider's confused view of the unfolding game?[55]

4) The Army's arsenal system (after its separation from the new Air Force) could not run all its ballistic missile projects inside von Braun's shop, simply because "it did not have the manpower." So was the arsenal system chiefly a managerial/operations framework and, in fair measure,

53. Bromberg, *NSI*, p. 25.
54. These relationships are sketched in Mueller Interview No. 4. See also Bromberg, *NSI*, pp. 26–28.
55. Bromberg, *NSI*, pp. 26–28.

> a hollow production system? Did shortcomings in securing adequate manpower (engineering, production, testing?) preview the complexities of producing for NASA? Did contractors learn from ABMA that they needed to resist control moves from their funders in order to protect opportunities for enhancing their own engineers' capabilities? Did "the enmity between the Army and the aircraft industry" bleed through to the space industry–NASA relationship, and if so, to what extent and with what consequences?[56]

Bromberg also details key drawbacks and advantages for companies undertaking production for NASA. On the downside were the small numbers of artifacts ordered, the necessity for expensive experimental development and research (some of which would be self-funded), demands for higher precision than usual in aeronautical engineering and fabrication, and the need to find and hire ever more engineers (and high-skill shop workers). Still, the pluses were substantial, if somewhat more vague: the "chance to learn technologies, develop skills and install production tooling that they could use for other projects," possible spillovers into commercial products, and the excitement of joining the space-race culture.[57]

She also shows that the bases for strain were quite concrete. If industry representatives in the 1950s saw "NACA engineers . . . as researchers, people whose aim was the production of papers and books," the incoming NASA leadership was equally critical. Given the necessity of contracting, Headquarters feared the loss of design control, shoddy work by contractors given too much leeway, and the loss of collective memory (and identity) as project teams formed and disbanded. Specifically in the Mercury capsule case, "Langley engineers mistrusted industry's ability to design something as novel as a spacecraft," whereas "industry and the military were convinced they knew more about space flight than NASA did."[58] This last item, the industry-military connection, reinforced NASA's uncomfortable position as the national novice in major project development and operations. Max Faget may well have had an advantage in being able to conceptualize a blunt-body spacecraft, but McDonnell's Walter Burke and his Air Force Material Command colleagues had learned firsthand how to fabricate complex aerospace technologies, as had von Braun and ABMA. Last, NASA might have considered industry folks immature and arrogant, but, as Bromberg so neatly puts it, "arrogance in proposals is also one of the channels by which creative ideas flow from industry to government."[59]

56. Ibid., p. 29.
57. Ibid., pp. 38–39.
58. Ibid., pp. 32, 43.
59. Ibid., p. 43.

When introducing the first of her two case studies (Hughes and satellites), Bromberg poses seven questions which articulate the chief concerns and boundaries of the study, "the relation between U.S. industry and the federal government."[60] Except by inference, none of these questions spotlight the technologies themselves, their design, prototyping, testing, redesign, fabrication, plus the consequent interfirm and contractor-government linkages. One technological-process moment appears when the failure of the first Syncom satellite was traced to a ruptured "gas tank," a problem "corrected" after a "search for a stronger material." The second Syncom "functioned brilliantly," but further questions that might have probed this failure and correction fell outside the study's scope.[61] This set-aside resonates with W. D. Kay's concern about the literature's silences on "the internal workings of the nation's aerospace contractors."[62] It remains for future scholars to address how Hughes designed and built its first three satellites; what the firm learned thereby and through what process; what innovations it embedded in the following four INTELSAT IIs; what machinery, materials, engineers, workers, consultations, conflicts, and compromises were involved.[63]

Similarly with North American, Bromberg's analysis works at the level of policy and program, though the secondary sources drawn on (especially Bilstein) yield a greater frequency of references to technical competencies and fabrication challenges. Thus the confrontation between Air Force General Sam Phillips (working for NASA) and North American leaders over "inadequate engineering, poor fabrication quality, faulty inspections, and cost escalations," all leading to delays and rework, is concisely reviewed, yet the underlying reasons for these multiple failures are not divined. As Bilstein, Kelly, and, to a degree, Mike Gray (*Angle of Attack*) demonstrate, in-depth technical review, appropriately contextualized, generates complex, contingent, and real-time analyses of innovation, critical insights and errors, integration, and technological and organizational learning.[64] This is, however, very difficult without

60. The questions are, "How much of the research for the commercial communications satellites would be financed, directed or done by government, and how much by the private sector? Would a private industry arise to launch the satellites or would they be launched by government? Would industry or government own and operate the systems? . . . What private firms would enter into the manufacture and the operation of commercial satellites (comsats)? What strategies would they use to gain market share? How would government policies and actions affect the market positions of private companies? How would these policies and actions affect the technology that was chosen?" (ibid., p. 46).

61. Ibid., p. 53.

62. Kay, "NASA," p. 127.

63. Five years ago, I did an online database search for articles in scholarly and technical journals on the design and fabrication of satellites, which then yielded fewer than a dozen hits. I expect a repeat these days would do much better, although the silences on building aerospace technologies may continue to include these devices.

64. An exceptional source in this regard is Martin Collins's series of interviews with North American Aviation's Lee Atwood, which document the critical role of NASA's detailed oversight in generating

continued on the next page

archival research, which, given its parameters and resources, was not plausible for this study.

Nonetheless, Bromberg skillfully reviews the fabrication and engineering practice changes that followed the Apollo fire deaths: separate managers for each spacecraft, heightened attention to quality control, frequent shop-floor visits (including during night shifts), tightened change controls, along with some of the dilemmas their introduction created. "All changes now had to be funneled first through the program officer at Houston, and then through the manager of that particular spacecraft at NA Rockville. North American engineers were made to adhere rigorously to agreed-on procedures, without any creative flourishes." Moreover, NASA's increased surveillance and micromanagement necessitated hiring hundreds of inexperienced technical managers who knew far less about their programs than those they were overseeing, which in turn led to mechanical rule-following and conflicts, very much on the pattern that Vaughn's conceptualizations outline. Pursuing these issues deeply into archival materials, especially those surrounding the astronauts' deaths and their aftermath, could provide valuable understandings of a critical transition in America's space program.[65]

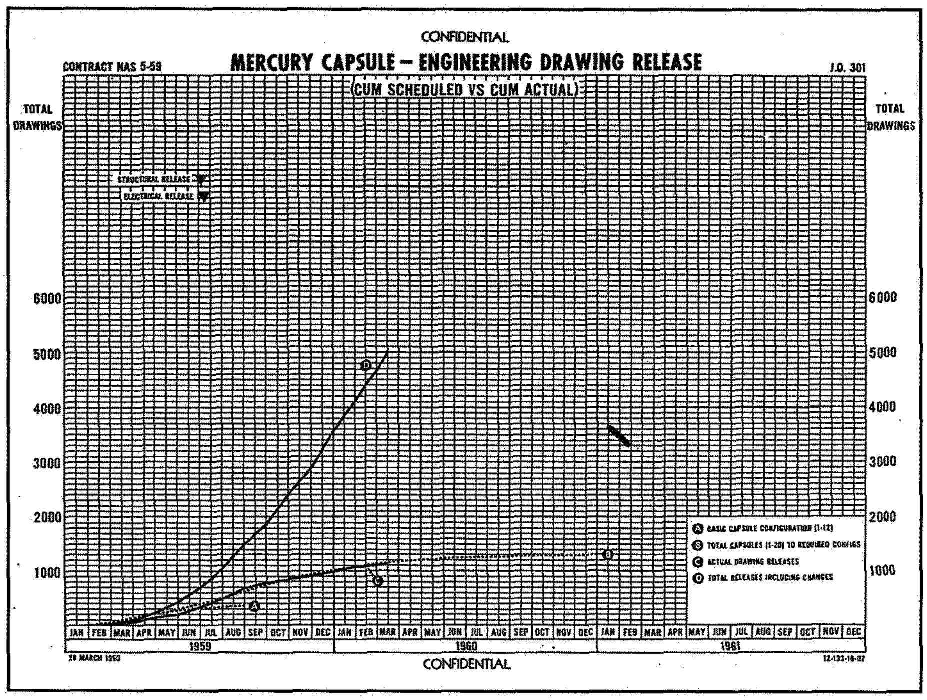
INDUSTRY AND NASA: MERCURY MOMENTS AND CLOSING QUESTIONS

Scattered about earlier pages are some items derived from my archival work with NASA Mercury sources. I'll mention just two others here focusing on a single matter, engineering changes, and will end by offering questions on other issues which may take on a fresh significance when researched from the contractors' technology and organization viewpoint. These items and issues may have more significance to historians of technology and enterprise (who

continued on the next page

masses of change orders and consequent delays and establishes the distinction between projects that were just complex (such as the Apollo Command Module) and those that involved "technological stretching," which ventured into the unknown. (See NASM Oral History Project, Atwood Interviews, no. 4, pp. 3, 10–11; no. 5, pp. 12, 14; no. 6, p. 3; available at *http://www.nasm.si.edu/research/dsh/TRANSCPT/ATWOOD4.HTM*, *http://www.nasm.si.edu/research/dsh/TRANSCPT/ATWOOD5.HTM*, and *http://www.nasm.si.edu/research/dsh/TRANSCPT/ATWOOD6.HTM*.) It appears that this is the only interview with a contractor official. It would be valuable were someone or some institution to take up Collins's plan for interviews with contractor engineers (and perhaps shop workers) before it is too late to target these sources of work and technology information.

65. Bromberg, *NSI*, pp. 70–73, quotation from 71. NASM's Alan Blinder is currently researching the Apollo 204 fire. For the industry perspective here, Bromberg cites a pamphlet by John L. "Lee" Atwood, NAA president, from NASM's Oral History Working File. Deeply interesting is the extensive oral history interview itself, done by NASM's Martin Collins, noted above. (The first segment is at *http://www.nasm.si.edu/research/dsh/TRANSCPT/ATWOOD1.HTM*; links at each section's end take the reader to the next segment.)



Engineering drawing release for the Mercury capsule, March 1960. *(Source: NASA Contract Administration Files, Procurement Division, box 22, entry 100, RG 255, NARA-Southwest)*

very much need to integrate public-sector innovations and organizations into their private-sector worlds) than for NASA history purposes, unless/until the scope and conceptualization of NASA history shifts in the years ahead.

The figure on this page is a simple graph documenting the engineering drawing releases for the Mercury spacecraft project, from inception through 15 March 1960. Lines A and C indicate that based on component counts, McDonnell had estimated that roughly 1,200 drawings would be needed through early 1961, 500 for the basic configuration and another 700 to include different capsules' mission-specific requirements (e.g., an orbital spacecraft versus one for a ballistic flight). Yet in response to the flow of engineering changes inside the project's first year, the actual number of drawings released reached 5,000 (line D). What significance this volume of redesigns had for project development is evident in Lee Atwood's reflections on Apollo:

> Once your engineering output of drawings and specifications gets ragged as far as the schedule is concerned, everything else

> gets ragged . . . . An engineering change is really a recall of something that's been released. You stop it, recall your drawing, you get an instruction to change it, bring it back, and the shop is full of that . . . . The things that are most apparent are usually picked up [in] a couple of weeks' surveys, because everybody has some kind of a schedule. Are you on it? Are you not? Well, of course you're not, and the whole place looked like a wreck. It was stop orders, hold orders, missing parts, material procurement had to be modified in many cases.[66]

Change orders were also lightning rods for NASA-industry arm wrestling, as was plainly the case with the Apollo Command Module:

> [The CSM] commanded the attention of so many astronauts and so many other people, engineers from Houston and all that. They all had their ideas of how things should be arranged, how controls should be set up, and an awful lot of brouhaha over the actual arrangement [resulted] . . . . One of the astronauts said, in connection with that, "You know, we have a pretty strong union." And they really did. They really did. And Dale [Myers] had to face the problem of arrangement [changes,] plus electrical changes, which came from other parts of the stack and from the ground equipment itself . . . . So there were just infinite refinements and changes, more than the S-II, which was fundamentally structural, a weight problem, . . . whereas the impact on the command module was almost screw by screw, and estimate by estimate and switch by switch.[67]

Researching the dynamics, the politics, the language, and the practices regarding engineering changes, which had pervasive implications for scheduling, cost, and program/artifact reliability and success, demands moving deep within both NASA and contractor organizations, following plant representatives like Wilbur Gray from Mercury to Gemini, chasing the origins and resolutions of

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66. Atwood Interviews, no. 5, pp. 10–11.

67. Ibid., p. 12. Elsewhere, Atwood added: "Your ideal is to engineer something, put it in the shop, get it built efficiently, and then inspect it carefully and get it out the door and operate. We had an environment that required us to do all those things at once, with much backtracking to make changes. The changes were almost overwhelming. So this was part of the problem of the organization, and it was far from normal. In fact, as Sam Phillips noted, it was to a considerable degree out of control. Parts had to go back for re-engineering, redesign, again and again, re-release, new material, supply and manufacturing and tooling. Yes, it was a struggle" (Atwood Interviews, no. 7, p. 3).

issues that surfaced briefly in configuration control committee minutes, and reconstituting the scale and significance of conflicts over payment for extra work, rework, redesign, supplementary testing and such. Only in this way will historians begin to understand the sadness behind Atwood's crisp aphorism: "If things are done well, NASA succeeded; if things are done poorly, the contractor failed."[68]

A chart issued on the same date as the drawings release graph accounted for the sources of engineering changes through mid-March 1960. I have not yet tallied the total of engineering changes with any precision, as there evidently were several levels of and procedures for requesting and reporting these. However, there were approximately 340 major "contract change orders" in roughly 30 months and at least 6,000 changes to the capsule components and configurations. Key dilemmas included communicating change implementations, authorizing changes, testing implications of changes on other components, identifying failure sources, and updating specifications to reflect changes.

The figure shows that nearly half the ECRs (Engineering Change Requests) emerged from deficiencies detected in testing, here components. A different class of failures, "interferences," was noted under "Manufacturing Coordination," and at that date, my sense is that these were still physical impingements due to the "spaghetti" style of packing in capsule system components. When full capsule testing commenced, a third sort of testing deficiency appeared—system integration and interface problems. These took on yet further ramifications when capsules connected to boosters and to launch-related ground equipment displayed higher-order integration deficiencies. Together, tests and coordination problems represented nearly two-thirds of the ECRs, with improvements, including the famous astronauts' demand for a window, another one-fifth. Engineering studies, the work closest to scientific research, were handled both by NASA Centers and by McDonnell. What significance and impact these studies had on the project is not yet clear, nor do summary documents provide cost figures for the four classes. Still, this simple chart suggests that, from the beginning, waves of engineering changes flowed through manned space projects from multiple directions, generating specialized knowledge, urgent workarounds and overtime labor, unpredictable cost and schedule implications, and fluctuating currents of disorder.[69] In sum, retelling NASA stories from the drafting room and shop floor outwards, from the bottom up, has the potential to reorient a universe of NASA-centric histories.

68. Atwood Interviews, no. 4, p. 11.

69. Originals of these two figures may be found in CCP Status Reports, box 20, NAS 5-59, Contract Administration Files, entry 100, NASA-Mercury, RG255.

If such a scheme were to be activated, questions and issues like these, some of which reiterate points sounded earlier, would be tabled, all considering change over time, 1950s–1970s, at least:

1) How were relationships between design revisions and manufacturing practice articulated, in the dual-pressure contexts for extensive changes on one count and design freezes and standardization on another?

2) What implications did NASA contracts have for manufacturers' recruitment, training, and retention of highly skilled workers—engineers, shop-floor workers, and managers—for manufacturers' procurement of machinery and facilities?

3) Considering relationships between primes and subcontractors, what patterns and variations in knowledge exchange, mentoring and monitoring, financial management, etc., emerged in NASA contracts? How were these different from such patterns in military contracts? In commercial contracts? How did they differ when technological stretching was at issue, beyond "routine" complexity?

4) What spatial patterning eventuated in early NASA prime and subcontracts, and did this change? If so, how/when/why? What factors conditioned these outcomes (labor supply, proximities and networks, politics)? How did technological change in communications, creating virtual proximities, affect the spatiality of producing for NASA?

5) How did NASA's fabricators frame practices for identifying/processing/testing new materials, including a) uses in prototyping, b) developing supply lines (titanium being a classic case), and c) adapting existing or creating novel manufacturing procedures? What prior experiences with materials substitution (alloy metals, synthetics) conditioned this process versus what new trajectories of technical knowledge-seeking did the devising of aerospace materials articulate?

6) What historically tested production skills and practices were installed/modified/rejected as shop-floor experience in producing for NASA developed? What occasions for technological learning proved crucial to overcoming obstacles to fabrication, precision, or quality? (Consider candidates like chemical milling, explosive forming, numerically controlled tooling, et al.) What implications for further manufacturing practice did these adaptations/adoptions have, and to what degree were they realized? What conflicts between contractor managers and engineers resulted, between managers/engineers and workers, with what outcomes, including strikes? (N.B.: aircraft/aerospace manufacturing had one of the highest union densities in U.S. manufacturing, 1950–1990.)

7) What would be the breakdown of sources for delays and cost overruns; how would these differ among projects, and why? What links and learning trajectories can be established among projects from the contractors' side—evidence for and significance of knowledge-sharing among aerospace rivals—in terms of materials, electronics, or fabrication shifts? What internal and networked transfers of know-how among projects took place, and how significant were they?[70]

8) What arrays of managerial techniques did contractors deploy in efforts to comprehend and influence fabrication projects that, as Atwood testified, threatened to spin out of control? How did firms assess internally the competence of their production efforts, and to what degree did these evaluations correspond with those authored by NASA overseers? How did such Venn diagrams differ among projects, both over time and across artifact classes?

9) How did primes and subcontractors integrate producing for NASA into their enterprises' overall operations, and how was this integration (or lack of it) evidenced by corporate planning processes, capital funds allocations, career tracks, etc.?

10) What informal practices did contractors' employees devise, at each locus and level of institutional activity, to deal with (make sense of) the persistence of insufficient knowledge, the nonlinearity of testing and performance outcomes, the ubiquity of uncertainty, the stresses of complexity, and the nonrational character of creativity? To what degree were such practices formalized in training procedures or, alternatively, concretized, either spontaneously or in a planned way? Most broadly in this arena, how can we assess the human cost of aerospace innovation to individuals, families, and communities (both of practice and of residence)? How do these practices, trainings, outbursts, quits, and implications compare and contrast with those which materialized in commercial-market enterprises and institutions? Ultimately, how (and to what extent) can producing for NASA be integrated into the experience of American business in the

70. Weick makes a provocative comment regarding Westrum's "fallacy of centrality" (the phenomenon of discounting new information because if it were important the individual/organization would already have heard about it): "It is conceivable that heavily networked organizations might find their dense connections an unexpected liability, if this density encourages the fallacy of centrality. 'News' might be discounted if people hear it late and conclude that it is not credible because, if it were, they would have heard it sooner. This dynamic bears watching because it suggests a means by which *perceptions* of information technology might undermine the ability of that technology to facilitate sensemaking. The more advanced the technology is thought to be, the more likely are people to discredit anything that does not come through it. [Thus] the better the information system, the less sensitive it is to novel events" (*Sensemaking*, p. 3, emphasis in original).

Cold War decades, the social life of organizations, the construction of knowledge, and the history of technologies?

These, and surely other, open questions flow from this very partial review of literature and documents concerning NASA-industry relations. Along with the foregoing thoughts on key issues, plausible conceptual frameworks, and implications drawn from that literature, they are offered for reflection and reaction. Perhaps they will encourage what seems a long-overdue vector for research into the distinctive, little-understood world of production for NASA, which exemplifies the intensities, urgencies, joys, and miseries of high-tech, high-pressure, state-sponsored innovation.